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September 11, 2016

The Honorable Sylvia Mathews Burwell, Secretary
U.S. Department of Health and Human Services
200 Independence Avenue, SW
Washington, DC 20201

Re: Healthy Indiana Program 2.0 § 1115 Demonstration Waiver – Non-Emergency Medical Transportation (NEMT)

Dear Secretary Burwell,

We appreciate the opportunity to comment on Indiana's proposal to extend its waiver of NEMT for the Healthy Indiana Program (HIP) 2.0. The National Health Law Program (NHeLP) protects and advances the health rights of low income and underserved individuals. The oldest non-profit of its kind, NHeLP advocates, educates and litigates at the federal and state level.

While we support states providing Medicaid coverage to low-income adults, we ask CMS to deny Indiana's proposal to extend this waiver of NEMT for its HIP 2.0 demonstration. The evidence the state provides in support of the extension is flawed, incomplete and fails to justify extending the waiver for this important service.

Indiana acknowledges that its initial NEMT evaluation evidence is flawed. The state has published one relatively small NEMT evaluation, conducted by the Lewin group in February 2016. That study acknowledges significant shortcomings, including a small sample size and more fundamental methodological problems due to the absence of an appropriate comparison group. The Lewin group acknowledges that the state plan population with access to NEMT differs substantially from the HIP population in terms of income, health need and in other key demographic features likely to impact access and need for NEMT. The report goes so far to say that: "these populations are very different; a direct comparison of their proportions is not advisable."[1] The differences are large enough to render cross-group comparisons more misleading than informative. The very implausible result that individuals with no access to NEMT reported fewer missed appointments due to transportation barriers (6%) than individuals who do have an NEMT benefit (11%) suggests either key unmeasured variables or a fundamental flaw in the comparative approach as a justification for an NEMT waiver.[2]

The state notes it may have found a slightly better comparison group because one of the three HIP MCOs offers its own NEMT benefit (not funded by the state). However, the data presented does not include key demographic data for the MCO subgroups (health status, age, gender) to show similarities (or differences) between the MCO populations. Factors like selection bias could lead to substantial differences. Results from a second, larger survey conducted in June 2016 address some, but not all these shortcomings. Unfortunately, the state has not made the full results and methodology of the second survey publicly available. This perplexing omission renders it impossible for stakeholders to assess whether the results from the second study indeed support the state's claims.

Other important shortcomings of the evaluation design are not acknowledged in either survey:

- **Both surveys conducted by the Lewin group focus narrowly on missed appointments, which ignores individuals who have no access to transportation and thus make no appointments or avoid care altogether.** The appropriate unit of analysis should be to measure unmet need for care due to lack of transportation. The federal evaluation of Indiana's HIP demonstration will survey unmet care needs due to transportation and may find quite different results than Lewin Group did.

- **The published Lewin Group evaluation does not discuss or address potential response bias in its survey.** Iowa's evaluation found that survey respondents skewed older, whiter and more female than the actual population.[3] Indiana's analysis does not include any data on response rates or on demographic discrepancies between respondents and the general HIP population. It is plausible that isolated individuals who lack adequate access to transportation may be systematically less likely to respond to the survey (e.g. individuals with limited English proficiency).

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[1] The Lewin Group, Indiana HIP 2.0: Evaluation of Non- Emergency Medical Transportation (NEMT) Waiver, 21 (Updated March 2016).

[2] Indiana Family & Social Services Administration (FSSA), NEMT Waiver Amendment Request to the Healthy Indiana Plan (HIP) 2.0 Medicaid Section 1115 Demonstration Waiver, 3 (August 2016).

[3] Suzanne Bentler, et al., University of Iowa Public Policy Center, Non-Emergency Medical Transportation and the Iowa Health and Wellness Plan, 26 (Mar. 2016), http://ppc.uiowa.edu/sites/default/files/nemt_report.pdf.

- **Neither survey really addresses the quality and accessibility of Indiana's existing NEMT benefit.** An alternative, equally plausible interpretation of the state's presented evidence is that Indiana's NEMT benefit is poorly understood or difficult to access for beneficiaries who can access it. The state suggests that it has added questions on awareness of the NEMT benefit in the larger Lewin study, but does not detail what proportion of members with NEMT know about the service (let alone how to access it.) CMS could not justify approving a renewal of the waiver of the NEMT benefit for the HIP expansion if the reason for "comparable" results is due to an ineffective current NEMT delivery system.

- **Indiana's NEMT evaluation fails to address the potential disparate impact of its NEMT waiver on people of color and individuals with significant health care needs.** Iowa's most recent NEMT evaluation found that people of color are significantly more likely to report unmet care needs due to transportation (83% higher odds for Blacks, 31% for Hispanics). People in relatively poorer health (58% higher odds), with multiple physical ailments (63%) or who have any functional deficit (245%) are also *much* more likely to report unmet transportation needs.[4] This evidence strongly suggests that waiving the NEMT benefit disproportionately impacts these groups. While Indiana's evaluation does stratify by income and gender, it does not include any data on racial or ethnic differences or primary language. Given the recent findings from Iowa, health equity issues should factor heavily into CMS's evaluation of the proposed waiver extension. CMS must not approve a continued waiver of this benefit because it likely exacerbates long-standing healthcare disparities for populations that have been historically underserved.

Given these shortcomings, and in light of the upcoming federal evaluation of HIP 2.0 that includes more appropriate questions related to transportation access, CMS should not approve an extension to Indiana's NEMT waiver.

NEMT may not be necessary for everyone, but it is crucial for some of the most vulnerable people and likely helps reduce health disparities. In many ways, the comparative evaluation structure entirely misses the mark by highlighting the relatively small proportion of the general population that needs NEMT. NEMT is most commonly used by individuals who may not be able to drive themselves, may not have access to a car or public transportation, or may have other challenges that make it difficult to get around, such as a disability. Depending on social networks to satisfy medical transportation needs can be unreliable and presents a real barrier to accessing needed care. While most beneficiaries can find ways to get to a provider when they need care most of the time, the NEMT benefit is intended to ensure that *all* beneficiaries, including the most vulnerable, can obtain needed care.

Indiana's data, for all its flaws, clearly shows that many people across the state still have problems getting to a doctor when they need it. Projected over the whole HIP population without access to state-sponsored NEMT (~144,000 individuals), the Lewin

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[4] Id. at 22.

survey results indicate that nearly 9000 HIP 2.0 members (6%) miss an appointment due to transportation barriers every six months.[5] This is not a trivial number. Even if NEMT only reaches a fraction of that group, it would substantially improve access to care for thousands of individuals, especially groups that are historically underserved. Indiana's evaluation and request to extend this waiver focuses on the majority of users while not acknowledging or addressing the expressed needs of a sizeable minority that disproportionately include key protected classes.

Conclusion

The broader point here is that a waiver of NEMT does not promote the objectives of the Medicaid program and likely contributes to persistent health disparities. While we support the continuation of Medicaid expansion coverage, we urge CMS to reject Indiana's request for an NEMT waiver extension. Thank you for considering our comments. If you have any questions or need any further information, please contact David Machledt (machledt@healthlaw.org; 202-384-1271), Policy Analyst, at the National Health Law Program.

Sincerely,

Jane Perkins,
Legal Director

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[5] The Lewin Group, *supra* note 1, at 5 &13. This represents more than a third of the individuals in this population who reported missing an appointment during the survey period (16%). Id. at 21.

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March 17, 2017

VIA ELECTRONIC SUBMISSION

The Honorable Thomas Price, Secretary
U.S. Department of Health and Human Services
200 Independence Avenue, S.W. Washington, DC 20201

Re: Healthy Indiana Plan 2.0 Extension Proposal

Dear Secretary Price:

The National Health Law Program (NHeLP) is a public interest law firm working to advance access to quality health care and protect the legal rights of low-income and under-served people. We support the comments submitted as part of a national group sign-on letter we joined, and we appreciate the opportunity to provide these additional comments to the Healthy Indiana Plan 2.0 (HIP 2.0) extension request.

We support Indiana's decision to accept federal funds to provide coverage to low-income adults. We are encouraged that the extension request proposes to increase outreach and education around tobacco cessation and strengthen its benefit package to help address substance use disorders. Such proposals recognize Medicaid's important role in addressing these public health problems.

However, NHeLP recommends that HHS not approve the HIP 2.0 application as currently requested. This application includes new and continuing waiver requests that do not satisfy the requirements for an 1115 demonstration. We have described our position on existing waivers (Premiums, lockouts, waiting periods, nonemergency transportation and emergency department copays, etc.) in prior comments (dated Sept. 9, 2014). The State has yet to provide sufficient evidence that demonstrates these components promote the objectives of the Medicaid program. We urge HHS work with Indiana officials to bring the proposals into a legally approvable form that also better serves HIP enrollees.

A. Required Premiums & Lockouts

HIP 2.0 is premised on monthly contribution systems. Indiana requests these monthly contributions to implement Personal Wellness and Opportunity (POWER) accounts. We do not think these waivers should ever have been approved, and extensions should not be granted until the State provides clear and convincing evidence that its premiums are not depressing enrollment. Data provided thus far do not support that conclusion.

There is no authority in the Medicaid Act for HHS to approve "lockouts" after termination or require prepayment to begin coverage. These provisions undermine continuity of care. The State has still not presented a plausible argument that delaying enrollment into Medicaid for numerous months helps furnish medical assistance.

The State has been serving parts of this population through a § 1115 demonstration for years. The previous HIP demonstration *already established* that even a premium below $5 a month depresses enrollment rates for very low income individuals.[1] Moreover, interim reports and evaluations from HIP 2.0 raise substantial concerns about the impacts of premiums and cost-sharing on HIP participation and access to care:

- **Both the State's and Lewin Group's evaluations report disenrollment figures based on enrollees who initiated payments but later stopped paying, which only partially describes who may actually be deterred by premium barriers.** Namely, this does not include another set of individuals who apply and are found eligible, but never make their first payment. None of the available data shows how large this group is. Based on monthly reports, roughly 4,000 to 5,000 individuals with incomes above the FPL are "conditionally eligible" in a given month, but none of the evaluations detail how many never pay. Lewin's evaluation design included a survey of "never members" to track people who miss that first payment, but out of a targeted 121 respondents, the surveyor collected only a single completed survey.[2] This cleaving of a potentially large segment of individuals who never enroll from enrollees who fail to continue payments creates the impression of a smaller participation barrier due to premiums.

- **The State reported 67% more disenrollments for nonpayment in HIP 2.0's first year than the Lewin Group.[3]** The Lewin study reported 2,677 disenrollments for failure to pay a premium in the first demonstration year, or roughly 6% of all enrollees it found were subject to disenrollment.[4] By contrast, the Indiana HIP 2.0 Annual Report found that 4,486 members were disenrolled for nonpayment over the

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[1] Ind. Family & Social Servs. Admin. ("FSSA"), *Healthy Indian Plan 2.0 1115 Waiver Application*, 28, (July 2, 2014).

[2] LEWIN GROUP, *Indiana Healthy Indiana Plan 2.0: Interim Evaluation Report*, 16 (July 6, 2016), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/Healthy-Indiana-Plan-2/in-healthy-indiana-plan-support-20-interim-evl-rpt-07062016.pdf. Presumably, the survey will be included in the final evaluation, but that will not come for several years.

[3] Author's calculations based on Lewin and FSSA reports.

[4] LEWIN GROUP, *supra* note 2, at 63.

same time period.[5] The latter number would approach 10% of the 45,607 people ever enrolled who were subject to this policy (and does not include "never enrolled" members described above).[6] Neither Lewin nor the State explained this discrepancy in their reports.

- **Disenrollments appear to have increased precipitously during 2016.** During the third quarter of 2016, the state report 4,621 disenrollments for failure to pay--approximately 10.5% of the roughly 45,000 monthly enrollees potentially subject to disenrollment for nonpayment.[7] This is an 89% spike from the prior quarter, and brings the total disenrollment in the second demonstration year to 9,384 through only three quarters.[8] Lewin reported 2,677 closures in year one, meaning the State disenrolled 73% more people for nonpayment in the third quarter of 2016 than they did in the entire first year of the HIP demonstration.[9]

- **Participation in HIP 2.0 has not met projections.** The state's proposal says that it has exceeded enrollment expectations, but that claim is not supported by the state's most recent published data. In early 2015, Milliman's 1115 budget analysis projected that *average* monthly HIP enrollment would be 518,000 in 2016.[10] Earlier, more modest Milliman projections suggested monthly HIP enrollment would reach 421,000 by October 2016.[11] Actual enrollment of individuals receiving services only reached 389,205 by October 2016.[12] Until better data is available to compare Indiana's participation rates – particularly for the income range subject to disenrollment – against similar states that have expanded Medicaid with no lockout or prepayment requirement, it is difficult to fully evaluate the extent to which premiums may be inhibiting enrollment.

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[5] FSSA, *Healthy Indiana Plan Annual Report*, 22 (Apr. 29, 2016), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/Healthy-Indiana-Plan-2/in-healthy-indiana-plan-support-20-annl-rpt-feb-jan-2016-04292016.pdf. The state claims that this represents only 6.3% of the ever-enrolled over 100% of FPL, but the state's total includes the medically frail, Native Americans, pregnant women and individuals on transitional Medicaid who were not subject to disenrollment.

[6] LEWIN GROUP, *supra* note 3, at 63.

[7] In October 2016, fewer than 45,081 individuals were potentially subject to disenrollment due to non-payment. Individuals in Basic (not subject to disenrollment) and individuals with incomes above 138% FPL, who are likely in TMA, are not subject to disenrollment. Some of the 45,081 Plus members are likely medically frail, or pregnant or young adults under 21 who are also not subject to disenrollment, so the share closed for nonpayment likely exceeds 10.5% of average enrollment. FSSA, *Healthy Indiana Plan Demonstration Quarterly Report, Demonstration Yr 2, Qtr. 3*, 4 (Dec. 30, 2016). The state did not publish an ever-enrolled figure for the third quarter.

[8] The state corrected its quarter 2 total down to 2,442 closures. FSSA, *Healthy Indiana Plan Demonstration Section 1115 Quarterly Report Demonstration Yr. 1, Qtr. 4*, 8 (Mar. 31, 2016).

[9] Percentages based on author's calculations using state data.

[10] MILLIMAN, *1115 Waiver – Healthy Indiana Plan Expansion Proposal: Budget Neutrality Projections*, 1 (June 23, 2014). This budget neutrality analysis uses total member months, but assumes that 518,000 individuals would be enrolled in each of the 12 months.

[11] MILLIMAN, *1115 Waiver – Healthy Indiana Plan Expansion Proposal: Budget Neutrality Projections – 3 Year*, 5 (Feb. 27, 2015).

[12] FSSA, *supra* note 7, at 4.

- **Health care access for individuals locked out from coverage has not been evaluated.** The interim evaluation postpones this aspect of the demonstration to the final evaluation. Without evidence to show a positive impact, no extension should be approved.

HHS should not approve extensions for waivers until the State has explained the full breadth of what it has tested with respect to the previous demonstration population, the results of those tests, how the lessons learned from that project have been incorporated the new proposal, and what new experiments will be conducted regarding this population during the extension. Those lessons must be based on accurate and relevant data.

B. Copayments for Non-Emergent Emergency Department (ED) Use

Indiana's experiment to charge heightened copays of $25 per visit for non-emergent use of the ED cannot be extended. Federal regulations already provide states with generous flexibility to charge as much as $8 for non-emergent ED visits for populations below 150% FPL, but no more than that.[13] To receive approval to waive cost sharing limits set forth in federal law, a cost sharing pilot must meet the tightly circumscribed requirements of § 1916(f), which include very clear requirements that any experiment have a control group and may not exceed two years. HHS granted Indiana a waiver of this statutory limit in February 2015. HHS has no legal authority to approve an extension of any cost sharing waiver under the terms of 1916(f).

The state notes it did not secure approval for the ED copay protocol until February 2016, and CMS later changed the "effective date" of the ED copay waiver authority to that date (Feb 4, 2016). The experiment will now end January 31, 2018.[14] However, HHS has no legal authority to extend it further under the terms of 1916(f). An extension would no longer be a "unique and previously untested use of copayments."[15]

In its proposal, the state claims that the long approval process also delayed implementation of the graduated copayment policy until February 2016.[16] It cites the delay to explain why no data comparing the control and experimental groups, or the number of people charged $25 copays, has yet been included in published evaluations. The state also claims early success for its ED copay policies by citing an Anthem report that found that people transferring from traditional Medicaid into HIP reduced their ED utilization by 30%. That Anthem study used data from February to September 2015.[17] If the state did not implement the graduated copay prior to 2016, then the reductions

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[13] 42 C.F.R. § 447.54.

[14] Eliot Fishman, CMS, *Letter to Joseph Moser setting new date for HIP ED copay experiment*, (Sept. 16, 2016), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/Healthy-Indiana-Plan-2/in-healthy-indiana-plan-support-20-er-room-copay-ext-req-09162016.pdf.

[15] 42 U.S.C. § 1396o(f)(1).

[16] FSSA, *Healthy Indiana Plan (HIP) Section 1115 Waiver Extension Application*, 30 (January 31, 2017), http://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/in-healthy-indiana-plan-support-20-pa4.pdf.

[17] Anthem Public Pol'y Inst., *Healthy Indiana Plan 2.0: Enhanced Consumer Engagement and Decision-Making Are Driving Better Health*, 8 and at FN 47 (July 2016).

Anthem found must not be related to the State's ED copay policy. If it did indeed start charging enrollees $25 copays in 2015 – or informing enrollees that they faced $25 copays – then the ED copay experiment should already be over. In any case, the state must present evidence from the analysis of its controlled experiment before HHS considers approval of *any* other cost sharing waiver for higher nonemergency ED copays. In prior comments, we have addressed the questionable validity of Indiana's claim to the effectiveness of its ED copay policy in the original HIP.[18]

More broadly, nonemergency ED copayments in the Medicaid context have been tested before, and found to be largely ineffective at reducing nonemergency use of the ED.[19] CMS's own bulletin on best practices to reduce unnecessary ED use points to the potential effectiveness of strategies like expanding access to primary care or providing health homes for frequent ED users, but suggests that increased copays for nonemergency use are problematic.[20] For additional information, *see* David Machledt, *Reducing Medicaid Emergency Department Use: Increase Accessibility, Not Copays* (Oct. 2014), http://www.healthlaw.org/publications/search-publications/reducing-medicaid-emergency-department-use.

Finally, any evaluation of this experiment must include careful monitoring of compliance with statutory requirements that, prior to charging any copay for non-emergent use of the ED, there must be an "actually available and accessible" alternate care option and that the facility must provide notice that the care to be provided is non-emergent care subject to additional charges, identify the alternative care option, and provide the enrollee with a referral.[21]

C. Reduce Complexity in POWER Account Incentive Structure

One major stated goal for HIP 2.0 is to incentivize cost-conscious health care seeking behaviors, including incentivizing preventive care. The state claims success toward this goal, but data presented in Indiana's proposal appears to overstate the impact of Indiana's complex POWER account incentive structure associated in achieving these goals. Other evaluation data paint a picture of considerable confusion, misunderstanding, and administrative complexity in HIP 2.0. This is not simply an issue of rolling out a new demonstration. The original HIP, which had a similar POWER

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[18] NHeLP Comments to Indiana's HIP 2.0 Demonstration Proposal, Appendix 1 (Sept 19, 2014), http://www.healthlaw.org/publications/search-publications/nhelp-federal-comments-to-health-indiana-1115-demonstrations.

[19] Mona Siddiqui, et al., *The Effect of Emergency Department Copayments for Medicaid Beneficiaries Following the Deficit Reduction Act of 2005*, 175 JAMA INT. MED. 393 (2015); Karoline Mortensen, *Copayments Did Not Reduce Medicaid Enrollees' Nonemergency Use of Emergency Departments*, 29 HEALTH AFF. 1643 (2010); David J. Becker et al., *Co-payments and the Use of Emergency Department Services in the Children's Health Insurance Program*, 70 MED. CARE RES. REV. 514 (2013).

[20] CMS, *Reducing Nonurgent Use of Emergency Departments and Improving Appropriate Care in Appropriate Settings* (Jan. 16, 2014), http://www.medicaid.gov/Federal-Policy-Guidance/downloads/CIB-01-16-14.pdf; see *also* Wash. State Health Care Authority, *Emergency Department Utilization: Assumed Savings from Best Practices Implementation* (2013).

[21] SSA § 1916A(e)(1).

account and rollover structure, was plagued by poor understanding of the incentives even five years after implementation.[22] HIP 2.0 does not appear to have addressed these issues; awareness is actually worse in the new iteration. In short, the available data do not support the conclusion that the State's incentive structure improves health awareness and access to preventive care compared to a standard Medicaid expansion. We are concerned that the state wishes to add on new incentive programs before adequately resolving issues with the current structure.

One of the criticisms of health accounts tied to high deductible plans is that they add unnecessary complexity that enrollees do not understand. Lewin's HIP 2.0 evaluation found that **more than half** of surveyed HIP enrollees believed they would pay for preventive services from their POWER accounts just like they do other services.[23] Less than 10% reported (correctly) that preventive services have no impact on their POWER account balance.[24] The finding that a majority of HIP enrollees incorrectly report that no-cost preventive services actually cost them suggests that Indiana's POWER account structure may actually discourage seeking preventive care.

HIP 2.0 also included an incentive intended to make members more cost conscious and to reward healthy behaviors like obtaining preventive care. The incentive allows enrollees to roll over some of their remaining POWER account funds to reduce their premiums in the second year, provided they stay enrolled for well over 12 months. Early evidence suggests very poor understanding of the rollover program, which was also poorly understood in the original HIP program. Fully 52% of HIP Plus members (65% of Basic) reported they had never heard of or did not have a POWER account (See chart). Another 20% of Plus members (15% of Basic) never checked their account at all.[25] This directly contradicts the state's claim in its proposal that 60% of Plus members check their POWER account balance, with 40% checking monthly.[26] With so few enrollees aware of the basic structure of Indiana's system, it is hard to imagine that the rollover, with its delayed reward, is positively influencing healthy behaviors. Basic members responded to a true/false question on the structure of the rollover in nearly equal proportions for each answer – no better than random guessing. Plus members had slightly higher awareness that getting preventive services allows them to double rollover, but still only 52% found the true/false statement true.[27]

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[22] In demonstration year 5 of the original HIP, more members were aware of POWER accounts (76.5%), but fully 72% believed incorrectly that preventive screenings were deducted from their accounts. Sixty percent were not sure or did not know the connection between preventive services and rollover. FSSA, *Healthy Indiana Plan 2013 Annual Report & Interim Evaluation Report*, 58, 61-62 (October 2014). Note: Lewin cited this rollover connection data in their evaluation, contending that awareness has improved in HIP 2.0, but the question on the prior survey was structured differently, making a direct comparison problematic. LEWIN GROUP, supra note 2, at 68.

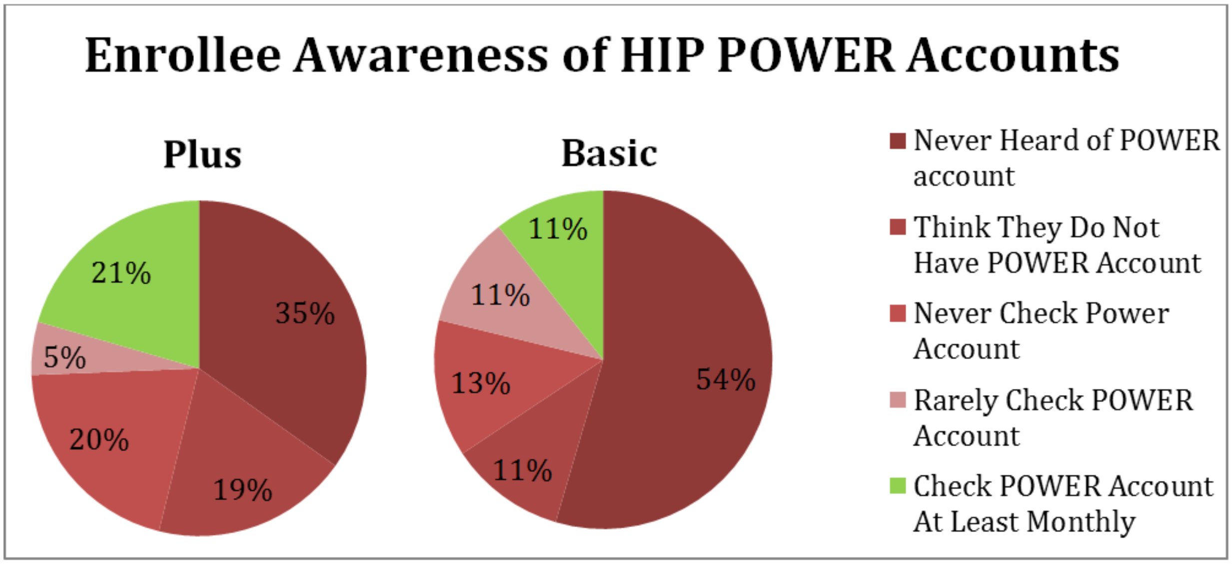
[23] LEWIN GROUP, *supra* note 2, at 66.

[24] Id.

[25] Author's calculations based on results in Lewin evaluation. *See* id.

[26] FSSA, *Healthy Indiana Plan (HIP) Section 1115 Waiver Extension Application*, 12 (Jan. 31, 2017), http://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/in-healthy-indiana-plan-support-20-pa4.pdf.

[27] Id. at 67.



Charts based on data from Lewin Interim Evaluation, at 66.

The state also claims evidence of success in promoting preventive services with the rather banal "finding" that the longer people are enrolled, the more likely they are to receive preventive services.[28] The state also points out that more Plus members received preventive and primary care services compared to Basic members. But Plus members are much older and generally sicker overall, making this an apples-to-oranges comparison.[29] And as the Lewin study points out: "It would be expected that sicker members be more active users of preventive and primary care services."[30] That Basic members are less inclined to seek care or adhere to medications may also be a consequence of the $4 copays they are charged for most services and the widespread misunderstanding about no-cost preventive services.[31] Cost sharing is a widely understood to reduce service utilization. Their higher use of the ED may result from delaying primary care; a point also supported by prior research. In sum, the state's claims of a successful incentives program may actually suggest added barriers to care for lower income Basic members.

Finally, the HIP 2.0 evaluations reveal a number of insights about the effects of administrative complexity on program participation. Two of the top five reasons for a HIP 2.0 closure during the first year related to such administrative issues.[32] In Lewin's small survey of individuals who left HIP 2.0, 9% left due to affordability or nonpayment, but an additional 9% experienced problems with administration or processing.[33] A

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[28] FSSA, *Response to Center on Budget and Policy Priorities Article*, 4 (Sept. 2, 2016), https://www.in.gov/fssa/hip/files/Solomon%20Response%209-2-16.pdf

[29] State data shows that in January 2016, 46% of Basic members were under 30, while only 8% were 50+. Twenty-seven percent of Plus members were under 30, while 25% were 50+. FSSA, *supra* note 10, at 28.

[30] LEWIN GROUP, *supra* note 7, at 104.

[31] For a review of research linking higher cost-sharing with reduced medication adherence, see Jane Perkins and David Machledt, NAT'L HEALTH LAW PROGRAM, *Medicaid Premiums & Cost Sharing* (Mar. 2014), http://www.healthlaw.org/publications/browse-all-publications/Medicaid-Premiums-Cost-Sharing.

[32] LEWIN GROUP, *supra* note 2, at 21.

[33] LEWIN GROUP, *supra* note 2, at 25.

different sample of Basic members found that two-thirds attributed their non-payment of premiums to confusion about payments, plan type or some other administrative issue.[34] These data all suggest that the bifurcation of plan types in HIP 2.0 is creating unnecessary red tape rather than a functional incentive to promote cost conscious decision-making.

D. No Employment-related Incentive Programs

We are generally skeptical of incentive-based healthy behavior programs and oppose programs with punitive elements, such as higher premiums for smokers. Partly, this is because their cost-effectiveness has not been clearly demonstrated. Partly, we find that these programs often fail to accommodate and include people with disabilities and other populations with special needs. Should HHS allow Indiana to implement broad wellness incentives, we urge the State to ensure that all programs are designed to provide equal access and opportunity for all enrollees, including people with disabilities and those with limited English proficiency. Engaging such groups in the planning process is a vital component of a successful design.

Secondly, Medicaid's objectives are to furnish medical assistance. While medical assistance can often help individuals stay healthy enough to work or provide needed supports so individual with a disability can maintain work in the community, Medicaid is not a jobs program. We do not believe employment-related incentives should be tied in any way to Medicaid benefits.

Conclusion

In summary, we have numerous concerns with the legality of Indiana's § 1115 demonstration application, as proposed. Please know that we fully support the use of § 1115 of the Social Security Act to implement true experiments. We strongly object, however, to any efforts to use § 1115 to skirt essential provisions that Congress has placed in the Medicaid Act to protect Medicaid beneficiaries and ensure that the program operates in the best interests of the population groups described in the Act. We urge HHS to address our concerns prior to issuing any approval. If you have questions about these comments, please do not hesitate to contact me (machledt@healthlaw.org) or Jane Perkins (perkins@healthlaworg). Thank you for consideration of our comments.

Sincerely,

David Machledt
Senior Policy Analyst

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[34] An additional 16% cited affordability issues. LEWIN GROUP, *supra* note 2, at 45.

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Marc Fleischaker
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July 7, 2017

VIA ELECTRONIC SUBMISSION

The Honorable Thomas Price, Secretary
U.S. Department of Health and Human Services
200 Independence Avenue, S.W.
Washington, DC 20201

Re: Amendment Request to Healthy Indiana Plan (HIP) § 1115 Waiver Extension Application

Dear Secretary Price:

The National Health Law Program (NHeLP) is a public interest law firm working to advance access to quality health care and protect the legal rights of low-income and under-served people. We commented on a prior version of this extension request on March 17, 2017 and also joined a national group sign-on letter submitted at that time. We incorporate our earlier positions and appreciate the opportunity to provide these additional comments to the amended Healthy Indiana Plan 2.0 (HIP 2.0) extension request.

We support Indiana's decision to accept federal funds to cover low-income adults through Medicaid. We are encouraged that the extension request proposes to increase outreach and education around tobacco cessation and strengthen the benefit package for substance use disorders. Such proposals recognize Medicaid's essential role in addressing these public health problems.

However, NHeLP recommends that HHS <u>not</u> approve the HIP 2.0 extension as requested. This amendment includes new and continuing waiver requests that do not satisfy the 1115 demonstration requirements. We stated our position on existing waivers (premiums, lockouts, waiting periods, nonemergency transportation and emergency department copays, etc.) in prior comments (dated Sept. 9, 2014 and incorporated). The State has yet to provide sufficient evidence showing these components promote Medicaid's objectives, and recent evidence from HIP 2.0 evaluations suggests that several features have created substantial enrollment barriers. We urge HHS to work with Indiana officials to bring the extension proposal into a legally approvable form.

Concurrent comment periods violate 1115 public process regulations

As noted in a separate letter dated July 3, 2017, the comment periods have not complied with the law. State officials submitted this amendment to the extension to CMS shortly after opening the state comment period on it. CMS issued a Letter of Completeness and opened the federal comment period while the State comment period was still open, and without waiting for the State to respond to the comments it received. This process does not comply with 42 C.F.R. §§ 431.408 and 431.416. As requested in our July 3d letter, we are asking that CMS: (1) rescind the Letter of Completeness until such time as the State submits all of the information required by 42 C.F.R. § 431.412(a)(1) and (2) begin the federal comment period required by § 431.416(a) and (b) only after the State submits that information and a new Letter of Completeness is issued.

New Data Shows HIP 2.0 Premiums, Lockouts, and Waiting Periods Create Significant Barriers

We have previously expressed our concern that the premiums included in this waiver have no demonstration value and do not promote the objectives of Medicaid, so they are inappropriate for an 1115 demonstration. Prior studies and literature reviews repeatedly show that premiums create substantial enrollment barriers for low-income populations.[1] The latest data from Lewin, Indiana's chosen evaluator, reinforces this conclusion yet again. That study, released after the comment period closed on Indiana's initial extension application in March, shows that premiums are causing substantial barriers both when enrollees sign up for coverage and later for continuing coverage. Given this new data verifying the chilling effect premiums have on the ability to obtain health coverage and care, CMS should deny a waiver to continue to allow Indiana to charge mandatory premiums in the HIP demonstration.

Nearly three out of ten times a low-income Hoosier faced a barrier due to a required HIP premium payment to start coverage or to remain enrolled, he or she could not overcome the barrier.[2] At least 2,537 Hoosiers were stymied by premiums twice (or more) – once at the front end to begin coverage and again later when they missed a payment.[3] The front-end enrollment barrier presented the bigger obstacle, with nearly one in four (23%) not making the initial payment to start benefits.[4] Although these people could reapply, only about half ever did so successfully.[5] This suggests that

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[1] See, *e.g.*, Samantha Artiga et al., KAISER FAM. FOUND., *The Effects of Premiums and Cost Sharing on Low-Income Populations: Updated Review of Research Findings* (June 1, 2017), http://www.kff.org/medicaid/issue-brief/the-effects-of-premiums-and-cost-sharing-on-low-income-populations-updated-review-of-research-findings/. David Machledt & Jane Perkins, NAT. HEALTH LAW PROGRAM, *Medicaid Premiums & Cost Sharing and Premiums* (March 2014), http://www.healthlaw.org/publications/browse-all-publications/Medicaid-Premiums-Cost-Sharing.

[2] In all, 57,189 of roughly 195,000 who ever faced a required premium were disenrolled or not enrolled due to nonpayment at least once. LEWIN GROUP, *Indiana HIP 2.0: POWER Account Contribution Assessment*, ii (Mar. 31, 2017), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/Healthy-Indiana-Plan-2/in-healthy-indiana-plan-support-20-POWER-acct-cont-assesmnt-03312017.pdf.

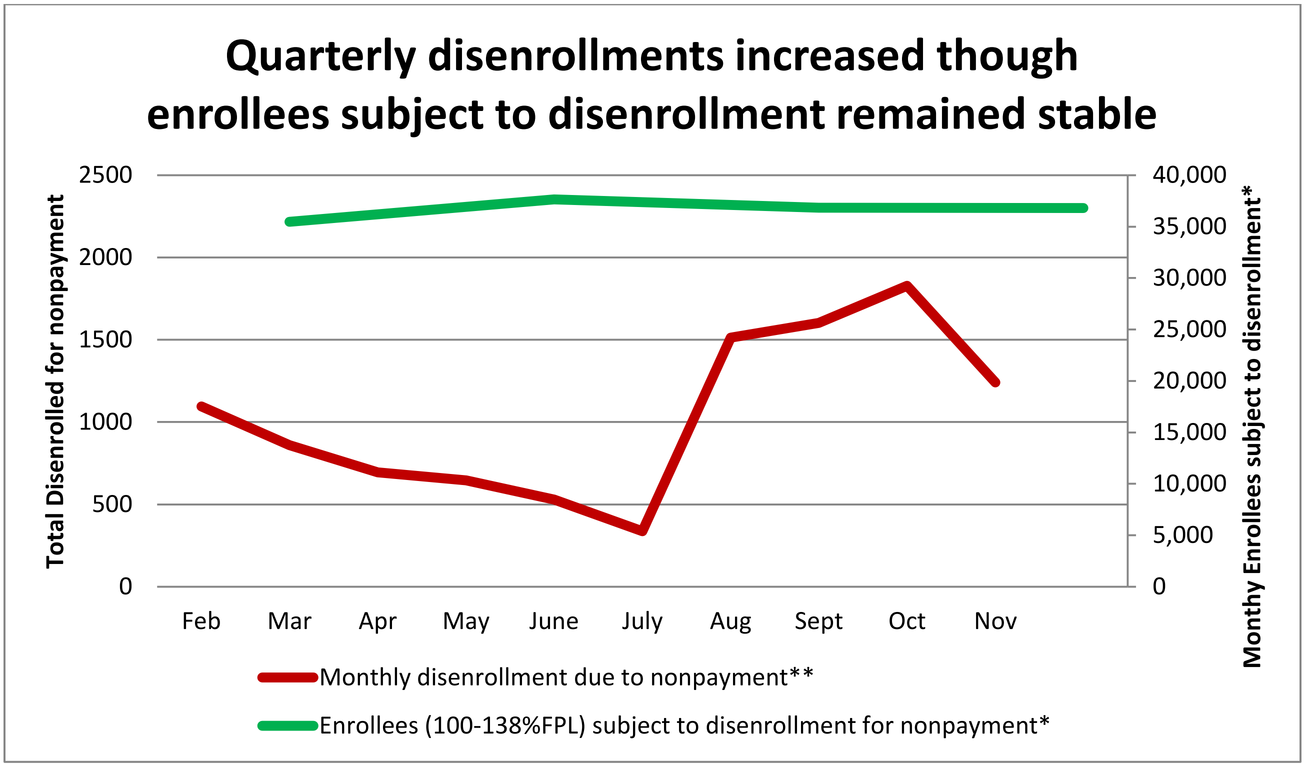
[3] *Id*.

[4] *Id*.

[5] *Id*. at 12.

HIP's "payment before benefits" provision *alone* kept 11.5% of the otherwise eligible applicants in that income group from ever participating in HIP 2.0. Overall, 55% of Hoosiers who applied and were found eligible for HIP 2.0 missed a premium payment at some point, resulting either in failure to begin coverage, disenrollment, or shift to a plan with higher copays and/or fewer benefits.[6]

The rate of disenrollment for non-payment also clearly accelerated in the second half of 2016 (see chart).[7] Lewin's report offers no explanation for this increase. Of 13,550 disenrolled for nonpayment over the 22-month reporting period, 6,183 (46%) occurred in the final four months. Total HIP enrollment increased only 12% over the course of 2016, and monthly enrollment for enrollees subject to disenrollment for nonpayment remained stable throughout the year.[8] This indicates that Lewin's overall average disenrollment rate (6%) substantially understates current the disenrollment rate due to nonpayment of premiums, suggesting that enrollment barriers may have increased.[9]


Quarterly disenrollments increased though enrollees subject to disenrollment remained stable

*Figures based on monthly enrollment for individuals with incomes 100-138% FPL in HIP Plus Plan. Excludes enrollees with state plan benefits, enrollees with incomes above 138% FPL (likely TMA or on appeal), and enrollees in Basic plans because these individuals are not subject to disenrollment for nonpayment. Source: Demonstration quarterly reports.
** Source: Lewin Group POWER Accounts report, at 11.

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[6] *Id*. at 8.

[7] *Id*. at 11.

[8] *Id*.; Ind. Fam. Soc. Servs. Admin. ("FSSA"), *Section 1115 Quarterly Report, DY 2: Qtr.1*, 5 (Mar. 31, 2016); *Section 1115 Quarterly Report, DY 2: Qtr.2*, 6 (June 30, 2016); *Section 1115 Quarterly Report, DY 2: Qtr.3*, 4 (Sept. 30, 2016); *Section 1115 Quarterly Report, DY 2: Qtr.4*, 6 (Jan. 31, 2017).

[9] Lewin Group, *supra* n. 2, at ii. These accelerated disenrollment rates have continued through January 2017, based on the State's most recent quarterly report. FSSA, *Section 1115 Quarterly Report, DY2: Qtr. 4*, *supra* n. 8, at 5.

The Lewin Group also conducted a survey of individuals who never fully enrolled ("never members") or left the program due to nonpayment ("leavers"). Unfortunately, several aspects of the methodology increase the likelihood that the survey results may not accurately reflect the actual experience of people facing these enrollment barriers.[10] Even so, the survey results suggest that, in addition to substantial affordability concerns, many enrollees are confused or face red tape with the payment process for premiums. Fully 30% of never member respondents reported they were confused or unaware of the payment process, while another 22% could not afford the premium.[11] Leavers were more likely to report unaffordability (44%), with another 18% confused or unaware.[12] More than three in four Basic members (who may not be disenrolled) cited unaffordability (34%), confusion (17%), or unawareness (25%) about the premium payment.[13] These numbers suggest significant barriers and poor outreach and notice in HIP 2.0's design and implementation.

Taken together, these new reports suggest that HIP 2.0 premiums are causing substantial access to care barriers for low-income Hoosiers. CMS should not reapprove premiums, required prepayment of premiums, disenrollments and lockouts for this Medicaid expansion population when these aspects of the waiver have hindered access to medical assistance.

Finally, we previously expressed concerns regarding Indiana's emergency department (ED) copayment proposal. Indiana's most recent HIP quarterly report claims that data show a "continued decrease in inappropriate ER usage by HIP members."[14] The tables do show a steady reduction in the proportion of ED visits deemed non-emergency over the prior four quarters.[15] But if the copays were responsible for this reduction, one would also expect an <u>overall</u> reduction in ED visits. But the report indicates that overall ED use has remained stable and has actually increased for Plus members. This must mean that either: (1) there has been a marked increase in emergent visits to the ED for an unexplained reason (delayed care?); or (2) ED providers have been changing their definition of what counts as a non-emergent visit over time. Either way, this evidence should not be interpreted to support the finding that HIP's ED copay is "working."

Work Requirements

The amended extension seeks to impose a mandatory "Gateway to Work" employment program. Once implemented, the work/work search requirements, if not satisfied, would result in suspension of Medicaid coverage. Work search requirements represent an illegal condition of eligibility beyond the Medicaid eligibility criteria.[16]

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[10] Lewin Group, *supra* n. 2, at 4 & D-1. The response rates for the survey were extremely low (3 to 8%). The survey does not include demographic information comparing the sample population (respondents) against the overall population that could reveal potential response bias. For example, no evidence suggests accommodations were made for limited English speaking individuals. Also, several errors in the sampling process due to misclassifications affected the final sample.

[11] Lewin Group, *supra* n. 2, at 20.

[12] *Id*.

[13] *Id*. at 19

[14] FSSA, *Section 1115 Quarterly Report, DY 2: Qtr.4*, 8-9 (Jan. 31, 2017).

[15] *Id*.

[16] *See generally* SSA § 1902.

Conditioning Medicaid eligibility on work requirements will reduce enrollment and does not promote the objectives of the Medicaid program. Medicaid's stated purpose is to provide medical assistance to low-income individuals and to furnish medical assistance and services to help these individuals attain or retain the capacity for independence and self-care.[17] A mandatory work requirement is not medical assistance; it is not a service provided to Medicaid beneficiaries. Work requirements applied to health coverage get it exactly backwards. They block access to necessary care that individuals need to be able to work.

The Medicaid Act establishes the requirements for coverage, and courts have held additional eligibility requirements are illegal.[18] Section 1115 cannot be used to short circuit these protections, because, as CMS has acknowledged when consistently denying previous state requests to impose work requirements through section 1115, conditioning Medicaid eligibility on a work requirement creates barriers to care and does not promote the objectives of the Medicaid program.[19]

Rather than providing medical assistance to low-income populations, this proposal would lead to thousands of low-income adults, including working enrollees, losing eligibility. The State's actuary, Milliman, estimates that 25% of enrollees subject to the requirement would have their eligibility suspended.[20] That does not include thousands more who would lose coverage under this policy due to the red tape required to verify their exemption or work activities. Milliman estimates that students, people with disabilities, and other exempted populations, constitute fully 70% of all HIP 2.0 enrollees, and each would have to document how they fulfill this requirement, often on a monthly basis.[21] This would require new systems to apply different requirements based on length of enrollment, accurate tracking and documentation of employment hours and caregiving hours, and effective screening process for disability and temporary conditions, all on an individual level. In short, this would require tremendous investment of resources and administrative costs and, as CMS has previously concluded, would create dangerous barriers to enrollment and care.

The sheer number of exemptions in Indiana's proposal is a tacit acknowledgment that most low-income enrollees who are not already working have a good reason not to be working.[22] A work

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[17] *See* 42 U.S.C. §§1396-1, 1396d(a).

[18] E*.g.*, *Camacho v. Texas Workforce Comm'n,* 408 F.3d 229, 235 (5th Cir. 2005), *aff'g*, 326 F. Supp. 2d 803 (W.D. Tex. 2004).

[19] *See*, *e.g.*, Ctrs. for Medicare & Medicaid Servs., AHCCCS 1115 Demonstration Extension (Sept. 30, 2016), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/az/Health-Care-Cost-Containment-System/az-hccc-demo-ext-09302016.pdf.; Letter from Vikki Wachino, Dir., CMS, to Jeffrey A. Meyers, Comm'r, N.H. Dep't of Health & Human Servs. (Nov. 1, 2016); *see* Kaiser Fam. Found., *Medicaid Expansion in Pennsylvania: Transition from Waiver to Traditional Coverage* (Aug 3, 2015), http://kff.org/medicaid/fact-sheet/medicaid-expansion-in-pennsylvania/#footnote-159781-6.

[20] Rober M. Damler et al., MILLIMAN, *1115 Waiver – Healthy Indiana Plan*, 4 (May 24, 2017), attached to HIP 2.0 application.

[21] *Id*.

[22] The State claims in its proposal that 244,000 HIP enrollees are unemployed, but it provides no citation to show where that number comes from. FSSA, *Amendment Request to Healthy Indiana Plan (HIP) Section 1115 Waiver Extension Application*, 7 (May 24, 2017).

requirement would only add to their considerable burdens by requiring verification of their exemption or compliance. Studies of TANF have shown that work-related sanctions and regulations are often unevenly applied and fail to distinguish between procedural/administrative issues, like missing an appointment, and actual noncompliance.[23]

Work requirements have been widely tested in other safety net programs and have not been very effective. The Temporary Assistance for Needy Families (TANF) program is the most widely known example of the application of work requirements to public safety net programs. TANF, created in 1996, was initially credited with temporarily helping increase employment rates for low-income mothers and reduce caseloads for cash assistance, but the magnitude of that effect is difficult to tease from a booming economy, a minimum wage increase (1997), and other contemporaneous changes to the safety net, including expansions of the Earned Income Tax Credit (mid-1990s) and the CHIP program (1997).[24] More recent research suggests that the effects of TANF's work requirement were modest and faded over time, and that time limits and work requirements may increase the incidence of extreme poverty when families have neither employment income nor cash assistance.[25] At any rate, it is not clear what Indiana is proposing to demonstrate that has not already been assessed through the TANF work waivers and requirements.

The Gateways to Work program would be extremely expensive and burdensome to implement, but also likely does not offer enough support to actually improve employment. Indiana says its work search program would cost $90/month to administer and run per enrolled member.[26] This would represent a huge shift of funds away from providing health care and into a new bureaucracy designed to require work or stop coverage. Medicaid's purpose is not to fund job training, but to provide medical assistance. The with-waiver estimated monthly cost to cover an adult in the new adult group is $567 in Demonstration year 5, meaning the work search would constitute close to 16% of the coverage cost for enrolled members.[27] The State does not specify how much would be spent on training and employment supports relative to the cost of administering and enforcing the requirement. Both would require enormous investments likely in excess of Milliman's estimates.

The proposal presents no evidence that the Gateway to Work program has the capacity to effectively scale up to enrollment of at least 75,000. The current voluntary program received fewer

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[23] Vicki Lens, *Welfare and Work Sanctions: Examining Discretion on the Front Lines."* 82 SOC. SERVS. REV. 199 (2008); Andrew Cherlin et al., *Operating within the Rules: Welfare Recipients' Experiences with Sanctions and Case Closings* 76 SOC. SERV. REVIEW 387 (2002).

[24] Sandra K. Danziger et al., *From Welfare to a Work-Based Safety Net: An Incomplete Transition*, 35 *J. Policy Analysis & Management* 231 (2016); Younghee Lim, *The Mid-1990s Earned Income Tax Credit Expansion: EITC and Welfare Caseloads*, 32 SOC. WORK RES. 46 (2008).

[25] Sandra K. Danziger et al., *supra* n. 24, at 234; Ladonna Pavetti, CTR. ON BUDGET & POL'Y PRIORITIES, *Work Requirements Don't Cut Poverty, Evidence Shows* (June 7, 2016), http://www.cbpp.org/sites/default/files/atoms/files/6-6-16pov3.pdf. Pamela Loprest & Austin Nichols, URBAN INSTIT., *The Dynamics of Being Disconnected from Work & TANF* (2011), http://www.urban.org/research/publication/dynamics-being-disconnected-work-and-tanf.

[26] Robert Damler et al., *supra* n. 20, at 4. It is not clear if this statement refers to $90 per member enrolled in the Gateway program or per member enrolled in HIP 2.0.

[27] *Id*. at 15.

than 3300 calls and conducted only 550 orientations in HIP's first year.[28] This anemic participation, despite some 300,000 letters sent to enrollees, suggests that the program offers too few actual supports to be of real value to enrollees. And the HIP evaluations provide no documentation that those who have participated have had any more success finding and sustaining work than enrollees who did not participate. Without evidence of prior success or metrics to ensure the program is delivering a quality service, the risk and potential harm of ramping up such a small program so quickly would be astronomical.

In fact, the proposal is not very clear what Gateway to Work actually purports to do. One description states the program will "connect unemployed and under-employed HIP members to available job training, work search, and employment programs," which suggests that those programs are not actually part of (funded by) Gateway to Work.[29] The FSSA website description suggests that the program provides job search assistance, such as case management, job skills training, job search assistance, and in limited cases, some training or educational supports.[30] The proposal fails to mention key components of any successful employment program, such as child care supports and opportunities for supported employment for people with disabilities. By comparison, a training voucher provided through workforce development programs averages approximately $3,500 or more.[31] Support for affordable child care – vital for many working families – would also far exceed $90 per month.

Rather than condition eligibility on participation in a work program that is destined to serve as a benefit cut, Indiana should invest in meaningful job training and affordable child care for HIP participants. We wholeheartedly support efforts by Indiana and other states to create well-funded, independent and voluntary employment supports for lower income individuals. Accessible employment supports are services that our clients have sought and been denied for decades.

Premium Tiering

In the same proposal that would vastly increase the red tape and administrative burden of the HIP 2.0 program by instituting a work requirement, the State simultaneously proposes changes to simplify the premium structure to "ease administrative burden on the State."[32]

Tiering premiums may make the system easier to operate for plans or Medicaid administrators, but tiers neither cure the harm that premiums represent nor address the problems with their legality.

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[28] FSSA, *Healthy Indiana Plan Demonstration Annual Report*, 23 (April 20, 2016).

[29] FSSA, *Amendment Request to Healthy Indiana Plan (HIP) Section 1115 Waiver Extension Application* 5 (May 24, 2017).

[30] *Gateway to Work*, FSSA, http://www.in.gov/fssa/hip/2466.htm (last visited July 1, 2017).

[31] Sheena McConnell et al., MATHEMATICA, *Providing Public Workforce Services to Job Seekers: 15-Month Impact Findings on the WIA Adult and Dislocated Worker Programs* (May 2016), https://www.mathematica-mpr.com/our-publications-and-findings/publications/providing-public-workforce-services-to-job-seekers-15-month-impact-findings-on-the-wia-adult.

[32] FSSA, *Amendment Request to Healthy Indiana Plan (HIP) Section 1115 Waiver Extension Application* (May 24, 2017).

And while the State bills this proposal as a simplification, it includes several changes that will expose enrollees to higher costs and increase their risk of losing coverage:

- **The proposal would charge 50% more for parents and two-person households.** In the face of clear evidence from its own evaluator that premiums are causing enrollment barriers, Indiana proposes a 50% spousal surcharge on premiums, a departure from current approved terms and conditions that limit total household premiums to 2% of monthly income, including all eligible members. This change would increase overall premiums for some households above the 2% monthly threshold, in the face of clear evidence from the State's own evaluator that premiums cause enrollment barriers at current amounts (see above). For example, a couple making just over the Federal Poverty Level would be charged $30/month under this new proposal, versus just over $27 under current policy. CMS should not approve any premiums, let alone an increase in premiums on some enrollees.

- **Restricting access to Transitional Medical Assistance will expose more parents and families to lesser coverage and disenrollment for non-payment.** The State proposes a "technical revision" to its program, but this technical revision has negative policy effects for parents. TMA provides parents transitional coverage when their income increases or other changes cause them to lose eligibility for the traditional § 1931 Parents/Caretakers group (income threshold roughly 18% FPL).[33] The State now proposes that parents whose income increase does not exceed 138% FPL (the Medicaid expansion limit) would no longer qualify for TMA and would instead be covered under regular HIP 2.0 coverage. Parents currently in TMA receive full state plan benefits (including dental, vision and non-emergency Medical transportation) regardless of whether they pay monthly HIP 2.0 premiums and cannot be disenrolled from the program for nonpayment of premiums. If this change were approved, parents whose income rises only slightly (to between 19% FPL and 138% FPL), *would* lose services if they cannot not afford premiums, and those whose income rose to 100-138% FPL would be subject to disenrollment for nonpayment. Furthermore, two-parent households would be subject to the spousal surcharge described above.

Conclusion

Thank you for consideration of our comments. If you have any questions, please contact David Machledt, Sr. Policy Analyst (machledt@healthlaw.org) or Jane Perkins, Legal Director (perkins@healthlaw.org).

Sincerely,

Jane Perkins
Legal Director

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[33] CMS, Medicaid and CHIP Eligibility Levels, https://www.medicaid.gov/medicaid/program-information/medicaid-and-chip-eligibility-levels/ (last visited June 29, 2017).

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August 23, 2017

VIA ELECTRONIC SUBMISSION

The Honorable Thomas Price, Secretary
U.S. Department of Health and Human Services
200 Independence Avenue, S.W.
Washington, DC 20201

Re: Amendment Request to Healthy Indiana Plan (HIP) § 1115 Waiver Extension Application

Dear Secretary Price:

The National Health Law Program (NHeLP) is a public interest law firm working to advance access to quality health care and protect the legal rights of low-income and under-served people. We commented on a prior version of this extension request on March 17, 2017 and also joined a national group sign-on letter submitted at that time. We also submitted comments on July 6, 2017 and signed onto another national group sign-on. We incorporate all these earlier positions and appreciate the opportunity to provide these additional comments to the amended Healthy Indiana Plan 2.0 (HIP 2.0) extension request.

We support Indiana's decision to accept federal funds to cover low-income adults through Medicaid. We are encouraged that the extension request proposes to increase outreach and education around tobacco cessation and to strengthen the benefit package for substance use disorders. Such proposals recognize Medicaid's essential role in addressing these public health problems.

However, NHeLP continues to recommend that HHS <u>not</u> approve the HIP 2.0 extension as requested. This amendment includes new and continuing waiver requests that do not satisfy the 1115 demonstration requirements, and the State's response to public comment does not resolve the potential harm to low-income Hoosiers that the policies laid out in these amendments will cause. We stated our position on existing waivers (premiums, lockouts, waiting periods, nonemergency transportation and emergency department copays, etc.) in prior comments (dated Sept. 9, 2014 and incorporated). The State has not provided sufficient evidence showing that these components promote

Medicaid's objectives, and recent evidence from HIP 2.0 evaluations suggests that several features have created substantial enrollment barriers. New proposals to add in a costly and burdensome work requirement and implement a lockout for not completing renewal forms on time would only exacerbate access barriers and cause thousands of Hoosiers to lose coverage. We urge HHS to work with Indiana officials to bring the extension proposal into a legally approvable form.

Work Requirements

The amended extension seeks to impose a mandatory "Gateway to Work" employment program. Once implemented, the work/work search requirements, if not satisfied, would result in suspension of Medicaid coverage. Work search requirements are not a condition of eligibility authorized by the Medicaid Act.[1]

The State's response to public comment, submitted July 20, added two new qualified work activities: Accredited ESL education and participation in work requirements for SNAP. The State also added several new exemptions: members of the Pokagon Band of the Potawatomi, TANF recipients, former foster children under 26, and chronically homeless individuals. Members who gain an exception while already suspended, such as women who become pregnant, will also be able to regain coverage immediately (if they are aware of this possibility). These limited additions do not address the central flaws that render Indiana's proposal unapprovable, that the proposed work requirements will:

- reduce Medicaid coverage (through suspensions);
- vastly increase administrative burden for the State and for enrollees; and
- add no or insufficient resources for child care, job training and transportation that are critical to create an effective dent in low-income employment.

In short, the work requirements are not likely to promote the objectives of the Medicaid Act and instead would create new barriers to care.

For example, the State clarifies that a person suspended due to non-compliance would be reinstated if he or she became eligible for an exception, such as when a woman becomes pregnant. But that means the State will have to create yet another process to ensure that enrollees are properly notified of this possibility; and the State really has no way to uncover whether a suspended person newly meets an exception. The inevitable result of this policy will be more paperwork and thousands of individuals who *should* have coverage due to an exception remaining suspended simply because they are unaware of their exception.

The minor changes suggested by the State do not come close to curing these harms. The State's lengthening list of exceptions actually reinforces the fact that the vast majority of enrollees in the Medicaid expansion do not need further "incentives" to find ways to improve their incomes. Rather, those able to look for work need solutions to the common barriers that prevent success. The current failure of the Gateways to Work program is likely less due to its voluntary nature than

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[1] *See generally* SSA § 1902.

to its inability to offer meaningful resources and solutions that actually facilitate steady employment. But the State's proposal does nothing to increase child care supports, suggesting only that it will better coordinate existing resources. Nor does the State propose a clear increase in funding for job training or education (activities which fall largely outside the scope of Medicaid funding anyway.) Instead, Indiana "solves" the unemployment problem by simply requiring participation in an under resourced program. Requiring work without providing true opportunities will only make it harder for low-income families to succeed.

With this work requirement proposal, the State has mistaken correlation for causation, assuming that, because employment is generally associated with better health outcomes, forcing more people to work will somehow lead to better health. The State claims that longitudinal studies found that unemployment has an independent effect on deteriorating health outcomes. For this point, the State cites a Robert Woods Johnson issue brief. The only reference to longitudinal studies in that brief refers to two studies conducted in Great Britain and Sweden.[2] Those studies found that the independent negative health effects of recent unemployment tend to exacerbate another well understood phenomenon – that ill health increases the risk of unemployment.[3] In other words, ill health and unemployment exist in a kind of vicious cycle where poor health begets unemployment, which causes more stress, which worsens health, which begets more unemployment. The research cited by the State does not support its conclusion that its punitive work requirement will do anything to break this cycle; rather, threatening enrollees' Medicaid coverage will likely add to enrollees' stress and reinforce the downward spiral.

Instead of conditioning eligibility on participation in a work program that is destined to serve as a benefit cut, Indiana should recognize that Medicaid provides access to needed treatment that can help break the cycle. Investing in meaningful job training and affordable child care for HIP participants might also help. We wholeheartedly support efforts by Indiana and other states to create well-funded, independent and voluntary employment supports for lower income individuals. Our clients have sought accessible employment support services for decades.

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[2] ROBERT WOODS JOHNSON FOUND., *Issue Brief 4: Work and Health – Work Matters for Health*, 9 (Dec. 2008), http://www.commissiononhealth.org/PDF/0e8ca13d-6fb8-451d-bac8-7d15343aacff/Issue%20Brief%204%20Dec%2008%20-%20Work%20and%20Health.pdf.

[3] Tomas Korpi, *Accumulating Disadvantage: Longitudinal Analyses of Unemployment and Physical Health in Representative Samples of the Swedish Population,* 17 EUROPEAN SOCIOLOGICAL REV. 255, 269 (2001); Scott M. Montgomery et al., *Unemployment Pre-dates Symptoms of Depression and Anxiety Resulting in Medical Consultation in Young Men*, 28 INTN'L J. EPIDEMIOLOGY 95 (1999).

Conclusion

Thank you for considering these additional comments. If you have questions, please contact David Machledt, Sr. Policy Analyst (machledt@healthlaw.org) or Jane Perkins, Legal Director (perkins@healthlaw.org).

Sincerely,

Jane Perkins

Jane Perkins
Legal Director

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November 19, 2019

VIA ELECTRONIC SUBMISSION

The Honorable Alex Azar, Secretary
U.S. Department of Health and Human Services
Hubert H. Humphrey Building
200 Independence Ave., S.W.
Washington, D.C. 20201

Re: Healthy Indiana Plan Section 1115 Demonstration HIP Workforce Bridge Amendment (Project No. 11-W-00296/5)

Dear Secretary Azar:

The National Health Law Program (NHeLP) is a public interest law firm working to advance access to quality health care and protect the legal rights of low-income and under-served people. We appreciate the opportunity to provide these comments on Indiana's § 1115 Healthy Indiana Plan amendment application.

Indiana's amendment proposes a new Workforce Bridge program that would provide a limited cash benefit to certain individuals who lose Medicaid eligibility. It also requests additional exemptions from Indiana's previously approved work requirement, which went into effect earlier this year.

The proposed amendments do not address the major flaws we have previously identified with the waiver project as a whole. These two additional exemptions to the work requirement are relatively small in scope and do not fix the legal and policy problems of a Medicaid work requirement. The State's own projections suggest that 25 percent of HIP enrollees subject to the work requirement will lose coverage.[1] These amendments would only exempt a small fraction of this population.[2] The Bridge program, for its part, does nothing to prevent individuals from losing coverage for failure to comply with the work requirement.

In fact, it is ironic that the State now proposes a very limited program to reduce churn and uninsurance when it has already imposed waiting periods, lockouts for failure to pay premiums, and work requirements, which have undoubtedly increased (or, in the case of work requirements, would increase) uninsurance and churn on a far greater scale.[3] The State now seeks to improve insurance affordability for a few individuals at higher incomes after it has already imposed premiums on low-income HIP enrollees that, according to the State's contracted evaluators, caused 29 percent of otherwise eligible applicants subject to mandatory premiums to either fail to begin coverage or to later lose coverage for nonpayment.[4]

In short, rather than approving these limited-scope amendments that affect few HIP enrollees, HHS should heed the evidence from Arkansas, New Hampshire, and Indiana's own projections and withdraw its approval of Indiana's work requirement before thousands more lose coverage in December 2019.

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[1] Ind. Family & Soc. Servs. Admin. [hereinafter "FSSA"], *Amendment Request to Healthy Indiana Plan (HIP) Section 1115 Waiver Extension Application (Project Number 11-W-00296/5)*, Attachment A, 11 (July 20, 2017), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/Healthy-Indiana-Plan-2/in-healthy-indiana-plan-support-20-demo-app-07202017.pdf.

[2] As of July 2019, Indiana reported 97,116 non-exempt HIP enrollees subject to the work requirement. The proposal estimates that only 5,500 caregivers would be exempt under the new amendment. FSSA, *Healthy Indiana Plan Annual Public Forum*, 12 (July 30, 2019), https://www.in.gov/fssa/files/HIP_Updated_Annual_Public_Forum_July_19.pdf; FSSA, *Application for Healthy Indiana Plan Section 1115 Demonstration HIP Workforce Bridge Amendment (Project No. 11-W-00296/5)*, 20 (July 25, 2019), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/in-healthy-indiana-plan-support-20-pa6.pdf [hereinafter "*FSSA Application*"].

[3] Indiana has also received approval for additional lockouts for individuals who do not return their renewal application forms on time.

[4] The Lewin Group, Inc., *Healthy Indiana Plan 2.0: POWER Account Contribution Assessment*, ii (Mar. 31, 2017), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/Healthy-Indiana-Plan-2/in-healthy-indiana-plan-support-20-POWER-acct-cont-assesmnt-03312017.pdf. Lewin's evaluation estimates that 23 percent of all eligible applicants required to pay premiums to begin coverage never made the first payment to enroll fully. Seven percent enrolled but were later terminated for nonpayment. The total share (29 percent) is slightly under the sum because some individuals experienced both barriers. *See also* MaryBeth Musumeci et al., Kaiser Family Foundation, *An Early Look at Medicaid Expansion Waiver Implementation in Michigan and Indiana* (2017), https://www.kff.org/medicaid/issue-brief/an-early-look-at-medicaid-expansion-waiver-implementation-in-michigan-and-indiana/ (documenting reports of confusion about and problems affording premiums).

Adding two new limited exemptions will not prevent HIP's work requirement from causing widespread harm and loss of coverage

As mentioned in our previous comments, also incorporated here, HHS does not have the authority to approve work requirements under Section 1115.[5] They are contrary to the objectives of the Medicaid program and do not serve a valid experimental purpose.

The State requests additional exemptions to its work requirement, known as Gateway to Work, for members of federally recognized tribes and for parents and caretakers with dependents from 6 up through 12 years old. As noted above, these requests for limited exemptions due not cure the fundamental harm caused by work requirements. In short, thousands of individuals will still lose Medicaid coverage every year for failure to comply with Gateway to Work. Arkansas included an exemption from its work requirement for parents and caretakers with dependents up to 18, but this did not prevent over 18,000 individuals from losing coverage within five months.[6] More recently, New Hampshire delayed implementation of its Medicaid work requirement when evidence showed that two thirds of the nearly 25,000 enrollees subject to the work requirement were at risk of losing their eligibility for non-compliance.[7] Not surprisingly, state officials reported challenges with successful outreach to inform enrollees of the changes and reporting process.[8]

There is little reason to believe Indiana's Gateway to Work will fare any better than New Hampshire or Arkansas. The State has long struggled to inform its enrollees of key program features. A 2012 evaluation of HIP 1.0 found that, even 4 years after implementation, roughly

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[5] *See*, National Health Law Program, *Re: Amendment Request to Healthy Indiana Plan (HIP) § 1115 Waiver Extension Application* (July 7, 2017) [attached]; National Health Law Program, *Re: Amendment Request to Healthy Indiana Plan (HIP) § 1115 Waiver Extension Application*, (August 23, 2017) [attached].

[6] Robin Rudowitz, MaryBeth Musumeci, and Cornelia Hall, Henry J. Kaiser Family Found., *February State Data for Medicaid Work Requirements in Arkansas* (Mar. 25, 2019), https://www.kff.org/medicaid/issue-brief/state-data-for-medicaid-work-requirements-in-arkansas/.

[7] New Hampshire Department of Health and Human Services, *DHHS Community Engagement Report: June 2019* (June 2019), https://www.dhhs.nh.gov/medicaid/granite/documents/ga-ce-report-062019.pdf. Notably, after the first month of implementation, the New Hampshire legislature directed the Medicaid agency to expand the exemption for parents and caretakers to include those with dependent children from 6 through 12 years old. The agency estimated this would only apply to 2000 additional individuals. *See* Letter from Jeffrey A. Meyers, Comm'r, N. H. Dep't of Health & Human Servs. to Governor Christopher T. Sununu, 2 (July 8, 2019), https://www.dhhs.nh.gov/medicaid/granite/documents/ga-ce-findings.pdf.

[8] *Id. See also,* Harris Meyer, Modern Healthcare, *New Hampshire Delays, Alters its Medicaid Work Requirement* (July 8, 2019), https://www.modernhealthcare.com/medicaid/new-hampshire-delays-alters-its-medicaid-work-requirement.

one in four respondents had never heard of the POWER account, long the central component of Indiana's model.[9] A 2016 report on HIP 2.0 showed even poorer results, with nearly 53 percent of PLUS members (65 percent of Basic) reporting they never heard of or did not have a POWER account.[10] Fewer than one in ten enrollees correctly reported that preventive care did not count against their POWER account balance.[11] This poor track record on outreach and education does not bode well for the work requirement.

Indiana's work requirement will likely trip up even more enrollees than Arkansas, because the State requires more individuals to actively report their monthly work activities. Arkansas, for all its problems, found over 25,000 enrollees compliant based on income data already available to the state. This meant that only about four in ten non-exempt individuals had to actively report their activities to comply.[12] Yet still over 18,000 of this active-reporting group lost coverage in six months.[13] According to Indiana's July data, a far higher proportion (71 percent) of non-exempt enrollees must actively report their activities to comply.[14]

Given Indiana's poor record on beneficiary awareness of HIP policies and incentives and higher rate of active reporting, the addition of these two exemptions – well after the work requirement has been implemented – cannot be expected to prevent substantial coverage losses at the end of 2019.

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[9] FSSA, *Healthy Indiana Plan Demonstration Section 1115 Annual Report: Demonstration Year 5*, 55-56 (2013), http://www.in.gov/fssa/hip/files/2012_HIP_Annual_Report.pdf; The Lewin Group, Inc., *Indiana Healthy Indiana Plan 2.0: Interim Evaluation Report*, 66 (July 6, 2016), https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/in/Healthy-Indiana-Plan-2/in-healthy-indiana-plan-support-20-interim-evl-rpt-07062016.pdf.

[10] The Lewin Group, *supra* note 9, at 66.

[11] *Id.*, at 67. Over five in ten incorrectly reported that preventive services would be deducted from their accounts, while 40 percent reported they did not know.

[12] Ark. Dep't. of Human Servs., *Arkansas Works Program September 2018 Report*, (2018), https://humanservices.arkansas.gov/images/uploads/newsroom/101518_AWreport.pdf.

[13] Robin Rudowitz, MaryBeth Musumeci, and Cornelia Hall, Henry J. Kaiser Family Found., *February State Data for Medicaid Work Requirements in Arkansas* (Mar. 25, 2019), https://www.kff.org/medicaid/issue-brief/state-data-for-medicaid-work-requirements-in-arkansas/.

[14] FSSA, *Healthy Indiana Plan Annual Public Forum*, 12 (July 30, 2019), https://www.in.gov/fssa/files/HIP_Updated_Annual_Public_Forum_July_19.pdf. The slide shows that as of July 22, 2019, 69,408 non-exempt members had to actively report, while 27,708 met the requirement based on data previously reported to the state ("Reporting met" category).

The Workforce Bridge: Rickety and Poorly Designed

Indiana's Workforce Bridge proposal would also provide up to $1,000 over 12 months for health expenses for HIP enrollees who lose eligibility due to increased income. The State estimates that some 27,000 individuals would qualify for this limited benefit annually.[15] The State intends the Bridge program to "support[] continuity of coverage and address[] the coverage cliff between HIP and commercial coverage."[16] Admirable goals notwithstanding, the Workforce Bridge's extremely limited benefit, poorly thought through logistics, and ill-conceived research design do not meet the requirements of an 1115 demonstration.

Practically, the program would prove difficult to access and would do little to promote true affordability or coverage continuity over the long term. Key facets of program operations remain unexplained while others appear inappropriate. For example, the proposed financing appears to exceed the bounds of what HHS has the authority to waive under 1115 and raises budget neutrality questions. Moreover, it is unclear how the State will ensure that key Medicaid protections in areas including, but not limited to, cost-sharing, due process, provider access, and program integrity, can be maintained when beneficiaries seek care from private market providers or through commercial health plans not associated with Medicaid.

If the state really wants to improve insurance affordability and coverage for individuals just above the Medicaid eligibility threshold, the Affordable Care Act includes a Basic Health Program (BHP) for this very purpose. The BHP, which has been implemented successfully in New York and Minnesota, offers comprehensive benefits with lower cost sharing to individuals with incomes from 133 to 200 percent of Federal Poverty Level (FPL). Alternatively, the state could elect the state option to expand full Medicaid benefits to adults with incomes over 133 percent of FPL.

The proposed funding mechanism is fatally flawed

Indiana cannot receive enhanced federal match to fund services to individuals who are not covered under the adult Medicaid expansion category. Yet Indiana proposes to pay for the Bridge benefit from "unused" funds that currently go to funding the State's portion of participants' POWER accounts.[17] To the extent that HIP enrollees actually use these funds over the course of a year for Medicaid services, Medicaid reimburses the State at an enhanced match rate of 90 percent (for "newly eligible adults"). However, "unused" funds are

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[15] *FSSA Application*, at 8.
[16] *Id.*
[17] *Id.* The funding estimates clearly suggest the state expects to receive a 90 percent FMAP.

not savings the State can simply shift for another purpose. They are not matchable at all unless used to pay for Medicaid-covered services.[18] Moreover, enhanced match only applies to individuals "newly eligible" through the adult Medicaid expansion.[19] Indiana proposes to enroll Workforce Bridge participants through a truncated version of the optional adult Medicaid eligibility group for adults with incomes over 133 percent of FPL. That group cannot qualify for enhanced match.[20] CMS does not have the authority to approve a waiver for enhanced match because the 1115 statute only permits it to waive provisions in § 1396a. The enhanced match is described in § 1396d.[21]

The Workforce Bridge creates a temporary, limited-access, limited benefit Medicaid eligibility category for a population already described in the Medicaid Act.

The State proposes to waive Medicaid requirements to impose different conditions of eligibility on the existing state plan option to cover adults with incomes over 133 percent FPL. Not only would Indiana limit eligible adults in this category to up to 12 months of coverage, but it would set no upper income limit and only enrolls individuals who can show they lost HIP coverage due to increased income. This limited enrollment, limited benefit program would set a dangerous precedent that invites states to create their own Medicaid eligibility categories and benefit packages outside the parameters already set by Congress. Indiana's proposal also ignores key Medicaid rights and guarantees laid out in statute.

The Workforce Bridge appears to waive key Medicaid cost-sharing protections

Indiana suggests that the $1,000 cash benefit is available for cost-sharing assistance, but that healthcare costs above that limit would be assigned to the individual. This does not appear consistent with Medicaid cost sharing statute, which sets per-service and aggregate limits on the cost sharing that can be charged to an individual for services rendered. For comparison, the Medicaid statute allows states to establish premium assistance programs that provide wrap-around services and cost sharing for Medicaid beneficiaries who are enrolled in a private market or employer-sponsored plans. In that case, Medicaid makes up the difference between what the plan pays a provider and Medicaid's limit for a given service. It also covers enrollee cost sharing that exceeds the Medicaid limits. So if a plan paid $130 for generic insulin and left the individual with a $30 copay, but the Medicaid cost sharing limit is a $4 copay, then Medicaid would pay $26.

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[18] 42 U.S.C. § 1396a(a)(10)(A)(i)(VIII).
[19] 42 U.S.C. § 1396d(y).
[20] 42 U.S.C. § 1396a(a)(10)(A)(ii)(XX).
[21] 42 U.S.C. § 1396d(y).

In contrast, the Bridge program as proposed does not maintain Medicaid cost sharing limits because a single service, such as an inpatient stay or expensive surgery, could easily exceed the $1,000 benefit even if it was the first service accessed. This would leave the individual with bills for that service that exceeded both the per-service and perhaps even the aggregate Medicaid cost sharing limits.

Section 1396o(f) of the Medicaid Act forbids HHS from approving any waiver of Medicaid cost sharing protections unless the proposal meets specific additional conditions listed in that provision. Indiana's proposal does not address these additional conditions, including limits on experimental duration, research design with control groups, risk mitigation for enrollees, and others. In fact, the proposal does not request the needed cost sharing waiver at all. Consequently, the proposal, as written, is not approvable under 1115.

Beyond the legal issues raised above, the limited cash benefit introduces numerous implementation questions that are not explained in the proposal. For example, the State proposes that Bridge funds could be used for direct provider-billed claims, such as a physician visit. This would presumably be for individuals not enrolled in other insurance, but who needed services. The State suggests such services will be paid using its standard Medicaid claims process and "run against allowable services under Medicaid and subject to the same benefit limitations and payment as Medicaid."[22] In short, providers will not be reimbursed more than the typically low standard Medicaid rate. Moreover, if the cash benefit does not cover the full service, the remaining cost would accrue to the beneficiary. This raises numerous questions. Will providers be willing to serve Bridge members knowing that Medicaid may only pay part of its already low rate? And how will the State agency ensure that providers will accept Medicaid payment as payment in full – as regulations require -- and will not seek to balance bill beneficiaries at a higher rate once their benefit runs out?[23] The State has not requested a waiver of this provision.

The proposal also allows Bridge participants to use their limited benefit to pay for premiums and cost sharing reductions. While this could help to temporarily reduce costs for employer-sponsored or Marketplace coverage, the program would likely do little to reduce churn. Rather, it simply sets up a new coverage cliff right after the $1000 is spent or the year has passed. Moreover, the proposal allows participants to use Bridge funds to purchase limited benefit short-term or association health plans. Using Medicaid dollars to pay for less than comprehensive coverage exposes participants to enormous financial risks that would likely exacerbate, rather than alleviate, churn.

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[22] *FSSA Application*, at 8.
[23] 42 C.F.R. § 447.15.

Indiana's proposal fails to specify how protections, like due process, would apply to Bridge participants

Beyond cost sharing, Medicaid includes other Constitutional and statutory protections, such as a right to due process, protections from discrimination, and access to providers, among others. The State does not request waivers from these provisions – some of which it could not waive in any case. But the proposal fails to specify how they would apply to services and plans that are administered outside the Medicaid program.

For example, how would FSSA ensure that providers and plans that accepted Medicaid payments under this program were compliant with federal non-discrimination provisions? If a Bridge participant was enrolled in a Marketplace plan, would they only be able to use their Bridge funds if they went to a Medicaid provider? How would Bridge participants access Medicaid fair hearings for denials of care, problems with reimbursement, or other adverse actions by a non-Medicaid provider or plan? How will individuals know which are Medicaid services for which they can use their Bridge funds?

The proposal leaves all these questions unanswered and more. The administrative burden of providing these fundamental protections of the Medicaid program in outside contexts would be enormous and would likely overwhelm the program's extremely limited benefit and cause considerable confusion among providers and participants.

The hypotheses and evaluation design suggest that this program is not designed as a true or effective experiment.

The research design for this proposed project is poor. Indiana's first stated hypothesis for the Bridge program posits that giving participants $1000 will help reduce their out-of-pocket costs. We do not need an experiment to know the answer to this question. Another, more legitimate hypothesis – whether the Bridge account will increase enrollments in Marketplace or employer-sponsored coverage – includes no description of how the State will establish a baseline to allow viable comparisons over time. A third hypothesis posits that the Bridge account will reduce uninsurance, but suggests a methodology that compares coverage rates between people who leave HIP due to earnings increase against people who leave HIP for other reasons. These groups obviously comprise quite different populations in different life circumstances. It would be exceedingly difficult to draw meaningful conclusions about the independent effects of the Workforce Bridge program when comparing data from such disparate groups. In short, the State has not established the experimental value of these amendments, and the proposed evaluation methodology is inadequate.

Conclusion: A Bridge on Shaky Ground

In summary, while the stated goals of reducing churn and improving health care affordability for individuals above 133 percent of FPL are worthy, these proposed amendments would do little to help. A limited and temporary benefit for the small fraction of individuals who leave HIP due to increased income is not likely to do much to reduce churn or improve long-term affordability. The Bridge program also undermines key Medicaid protections and blurs the line of what it means to be a Medicaid beneficiary. These aspects of the proposed program, coupled with the lack of detail in the application, do not satisfy the requirements for a viable 1115 project.

Rather than looking to amend around the edges of a fundamentally flawed program, the right solution here is for HHS to withdraw its approval for Indiana's work requirements, required premiums, and other components of the HIP demonstration that constitute barriers to coverage.

We appreciate your consideration of our comments. If you have questions about these comments, please contact David Machledt (machledt@healthlaw.org) or Catherine McKee (mckee@healthlaw.org).

Sincerely,

Jane Perkins

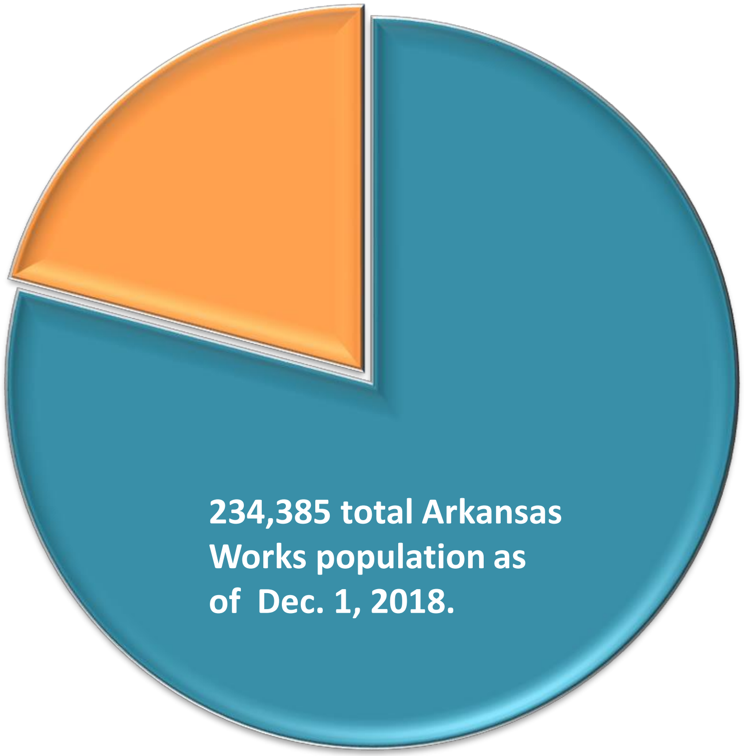
Jane Perkins
Legal Director

Arkansas Works Program

December 2018

Just over 60,600 Arkansas Works enrollees were subject to the work requirement in December. Most are already meeting the requirement through work, school, or other life situations that made them exempt from reporting. Numbers below are a point-in-time snapshot of the requirement and some fluctuate daily.

60,680 subject to work requirement in December*



4,776
Did not meet requirement

55,904
Met requirement due to work, training, or other activity. Most of these individuals are exempt from reporting activities because DHS already has the information showing they are in compliance.

Months of Not Meeting Requirement

One month	Two months	Three months (closed)
0**	**0****	**1,232**

**Numbers as of Jan. 7, 2019. Months reset at end of each calendar year.

*Enrollees ages 30-49 are subject to the requirement in 2018. Those 19-29 will phase in from January to June 2019.

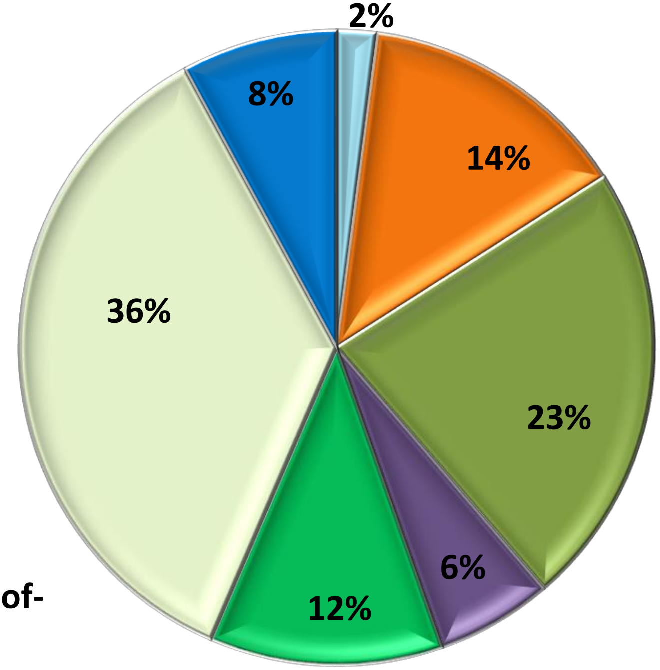
Arkansas Works Program

December 2018

Every Medicaid program has what is known as “churn,” cases that close for various reasons. It is not uncommon for those individuals to take action and come back on a program after receiving a closure notice. The total number of Arkansas Works cases closed in December was 15,981. Of those, only 1,232 closed due to not meeting the requirement.

Of the enrollees whose coverage ended in 2018 due to not meeting the requirement, 966 have applied for and gained coverage in 2019. Of those, 963 are in Arkansas Works.

- 🟦 **Non-compliance (8%)**
- 🟦 **Incarceration (2%)**
- 🟧 **Household increased income (14%)**
- 🟪 **Death (currently 0.1%)**
- 🟩 **Other (23%)**
- 🟪 **Enrollee requested closure (6%)**
- 🟩 **Unable to locate client or moved out-of-state (12%)**
- 🟩 **Failed to return requested information (36%)**



Outreach Efforts
April – December 2018

includes DHS, AFMC, insurance carriers, and DWS

Phone Calls	**230,307**
Letters	**592,102**
Emails	**311,934**
Text Messages	**38,766**
Social Media Posts	**918**

Arkansas Works Clients -
Subject to the Work and Community Engagement Requirement

Reporting Period	Met the Requirement	Did Not Meet the Requirement	Total	% Who Met the Requirement
June 2018	18,351	7,464	25,815	71%
July 2018	31,072	12,722	43,794	71%
August 2018	43,655	16,357	60,012	73%
September 2018	56,509	16,757	73,266	77%
October 2018	56,913	12,128	69,041	82%
November 2018	56,317	8,426	64,743	87%
December 2018	55,904	4,776	60,680	92%

Arkansas Works Clients - Months Not Meeting Requirement in 2019

As of January 7, 2019

One Month	Two Months
-	-

Arkansas Works Clients - Met the Requirement

Reporting Period	Employed >80 hours/month	Dependent Child in Home	Pregnant	Medically Frail	Currently Exempt in SNAP	Caring for Incapacitated Person	Short-Term Incapacitated
June 2018	8,375	2,731	15	2,208	3,480	128	164
July 2018	13,951	4,192	21	4,282	5,780	264	385
August 2018	19,391	5,717	40	6,273	7,776	534	776
September 2018	25,368	7,432	51	8,020	9,705	781	1,113
October 2018	25,425	7,147	54	8,271	9,913	846	1,246
November 2018	25,149	6,765	47	8,393	9,984	849	1,286
December 2018	25,089	6,469	45	8,424	10,244	781	1,180

Reporting Period	Receives Unemployment Benefits	Education and Training	Alcohol or Drug Treatment	American Indian/Alaska Native*	Tea Cash Assistance	Reported Activities	Total
June 2018	187	24	79	515	-	445	18,351
July 2018	310	56	155	832	-	844	31,072
August 2018	444	129	207	1,150	-	1,218	43,655
September 2018	556	242	230	1,479	-	1,532	56,509
October 2018	479	283	239	1,485	-	1,525	56,913
November 2018	398	324	224	1,470	-	1,428	56,317
December 2018	370	333	221	1,437	-	1,311	55,904

Clients may fit into more than of the above groups. However, they are only counted in one group each month.

**Clients who are American Indian / Alaska Native will be part of a future phase in.*

Good Cause Requests Completed

Month	Granted	Denied	Not a Good Cause Issue	Total
June 2018	-	-	-	-
July 2018	3	1	-	4
August 2018	45	4	6	55
September 2018	140	32	74	246
October 2018	182	16	93	291
November 2018	101	4	47	152
December 2018	106	7	43	156
Total	**577**	**64**	**263**	**904**

Good Cause requests are reviewed on a case-by-case basis. Clients who have another exemption reason are counted in this report where appropriate.

Clients Who Met Requirement Through Reporting - Types of Activities

Clients can report more than one type of activity

		Number of clients who reported the activity						
Reporting Period	**Clients Who Reported Activities**	**Work**	**Education and Training**	**Volunteer**	**Job Search**	**Job Search Training**	**Health Education Class**	**Currently Meeting SNAP Requirement**
June 2018	445	73	8	27	18	1	1	351
July 2018	844	145	20	63	40	4	0	639
August 2018	1,218	279	42	120	90	6	3	828
September 2018	1,532	372	42	152	93	5	5	1,025
October 2018	1,525	401	37	181	106	2	3	968
November 2018	1,428	371	34	170	97	4	3	907
December 2018	1,311	313	22	158	84	1	3	849

December 2018 Reporting Period

Clients Who Met Requirement Through Reporting - Types of Activities Reported by Hours

Total Clients Who Met Requirement Through Reporting: 1,311

Clients can report more than one type of activity.

Activity*	Clients Who Met Requirement Through Reporting - Hours Reported					# of Clients Reported	Total Hours Reported
	1-20 Hrs	21-40 Hrs	41-60 Hrs	61-80 Hrs	81+ Hrs		
Work	7	14	26	38	228	313	53,353
Education and Training	2	2	2	8	8	22	2,546
Volunteer	4	9	26	73	46	158	12,341
Job Search	17	25	24	7	11	84	4,800
Job Search Training	0	1	0	0	0	1	39
Health Education Class	3	0	0	0	0	3	30
Currently Meeting SNAP Requirement	--	--	--	--	--	849	N/A

**While there is no limit to the number of hours a client can report, some activity types limit the number of hours clients can receive credit for:*

-- Job Search and Job Search Training - Clients may count up to 39 total hours from these activities combined each month.

-- Health Education Class - Clients may count up to 20 hours each year from this activity.

Clients Who Did Not Meet Requirement - Types of Activities

Clients can report more than one type of activity

				Number of clients who reported the activity					
Reporting Period	**Clients Who Did Not Meet Requirement**	**Reported No Activities**	**Reported Some Activities**	**Work**	**Education and Training**	**Volunteer**	**Job Search**	**Job Search Training**	**Health Education Class**
June 2018	7,464	7,392	72	27	20	5	23	2	1
July 2018	12,722	12,587	135	49	20	12	73	1	1
August 2018	16,357	16,132	225	78	50	19	98	4	1
September 2018	16,757	16,535	222	69	54	23	97	5	1
October 2018	12,128	11,966	162	46	35	17	83	2	1
November 2018	8,426	8,308	118	42	20	14	50	3	0
December 2018	4,776	4,703	73	29	2	7	44	2	0

December 2018 Reporting Period
Clients Who Did Not Meet Requirement - Types of Activities Reported by Hours

Total clients who did not meet requirement:	4,776
Reported No Activities:	4,703
Reported Some Activities:	73

Clients can report more than one type of activity

Activity*	Clients Who Did Not Meet Requirement by Hours Reported					# of Clients Reported	Total Hours Reported
	1-20 Hrs	21-40 Hrs	41-60 Hrs	61-80 Hrs	81+ Hrs		
Work	7	7	9	6	0	29	1,220
Education and Training	1	1	0	0	0	2	35
Volunteer	3	3	1	0	0	7	188
Job Search	13	14	8	0	9	44	4,398
Job Search Training	0	0	0	1	1	2	320
Health Education Class	0	0	0	0	0	0	0

**While there is no limit to the number of hours a client can report, some activity types limit the number of hours clients can receive credit for:*
-- Job Search and Job Search Training - Clients may count up to 39 total hours from these activities combined each month.
-- Health Education Class - Clients may count up to 20 hours each year from this activity.

Arkansas Works Closures

Reporting Period	Total Closures	Closures Due to Not Meeting Requirement	Closures by Reason							
			Incarceration	Household Increased Income	Death	Enrollee Requested Closure	Unable to Locate Client or Moved Out-Of-State	Failed to Return Requested Information	Did Not Meet Requirement	Other
August 2018	18,057	4,353	2%	11%	0.01%	3%	5%	33%	24%	22%
September 2018	15,276	4,109	3%	15%	0.05%	5%	6%	26%	27%	18%
October 2018	15,081	3,815	2%	13%	0.10%	5%	5%	28%	25%	22%
November 2018	20,494	4,655	1%	10%	0.10%	5%	13%	25%	23%	24%
December 2018	15,981	1,232	2%	14%	0.10%	6%	12%	36%	8%	23%

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SPECIAL REPORT

Medicaid Work Requirements — Results from the First Year in Arkansas

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In recent years, policymakers have introduced unprecedented changes to Medicaid. As of April 2019, nine states have received approval by means of a federal waiver to implement work requirements in Medicaid, and six have applications pending.[1] According to the Centers for Medicare and Medicaid Services, work requirements — also known as community engagement requirements — may promote better health and help beneficiaries escape poverty.[2] However, critics dispute these claims[3,4] and warn that the policy could lead to large coverage losses.[5] Work requirements have been used previously in programs such as the Supplemental Nutrition Assistance Program and the Temporary Assistance for Needy Families program. Studies of those programs showed that work requirements produced modest, short-term increases in employment but no increases in income.[6,7] The effects of work requirements in a health insurance program are unclear.

In June 2018, Arkansas became the first state to implement work requirements in Medicaid. Medicaid beneficiaries 30 to 49 years of age were notified by the state (by mail and informational fliers) that they were required to work 80 hours per month, participate in another qualifying community engagement activity such as job training or community service, or meet criteria for an exemption such as pregnancy or disability.[8] Three months of noncompliance or nonsubmission of monthly online reports within a year led to removal from Medicaid. By December, nearly 17,000 adults were notified by mail that they had been removed from Medicaid.[9] In March 2019, a federal judge halted the program owing to concerns about its effect on coverage. Although several analyses have predicted various results of Medicaid work requirements,[10-15] data from independent assessments since the policy took effect have been limited. Our objective was to assess early changes in insurance coverage and employment after implementation of the work requirements in Arkansas.

METHODS

STUDY DESIGN AND SETTING

We conducted a telephone survey to compare changes in outcomes before and after implementation of the work requirements in Arkansas among persons 30 to 49 years of age, as compared with Arkansans 19 to 29 years of age and those 50 to 64 years of age (who were not subject to the requirement in 2018) and with adults in three comparison states — Kentucky, Louisiana, and Texas. Kentucky, like Arkansas, expanded Medicaid under the Affordable Care Act (ACA) in 2014 and planned to introduce work requirements in 2018, but the requirements were blocked by a federal judge before implementation. Neither Louisiana (which expanded Medicaid in 2016) nor Texas (which has not expanded Medicaid) has implemented work requirements. All four study states are in the Southern census region and have poverty rates in the highest quartile of the United States. We used baseline data from 2016 (before the implementation of work requirements) for these states from a previous survey conducted by our team that has been validated against government data sources.[16-18] This project was approved by the institutional review board of the Harvard T.H. Chan School of Public Health.

SAMPLE AND SURVEY

Our survey was conducted by means of cellular and landline telephones, in English or Spanish,

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between November 8 and December 30, 2018. The sample comprised U.S. citizens 19 to 64 years of age who reported family incomes in 2017 below 138% of the federal poverty level (e.g., $16,600 for a single adult or $33,900 for a family of four), which corresponds to the income limit for the ACA Medicaid expansion. This inclusion criterion was based on the respondent's income in the previous year in order to prevent any potential employment response to the policy from biasing the sample composition.

We contacted potential survey participants in Arkansas, Kentucky, Louisiana, and Texas primarily by means of random-digit dialing. The study also included respondents from different surveys that had been previously conducted by our survey vendor who were recontacted for this survey; this facilitated oversampling in the age group subject to work requirements in Arkansas. We combined the 2018 data with baseline data from November and December 2016, which had been obtained from a different set of respondents.[17,18] Further details on the survey design are provided in the Methods section in the Supplementary Appendix, available with the full text of this article at NEJM.org.

OUTCOMES

Our study had three primary outcomes: the percentage of respondents with Medicaid, the percentage of respondents who were uninsured, and the percentage of respondents reporting any employment. Secondary outcomes were the number of hours worked per week, the percentage of respondents satisfying any category of community engagement requirement (described below), the percentage of respondents with employer-sponsored insurance, and two measures of access to care — the percentages of respondents having a personal physician and reporting any cost-related delays in care. We also examined Arkansas respondents' experience with work requirements: whether they had heard "a lot," "a little," or "nothing" about the requirements; whether they thought they were (or would be) subject to the requirements; and their reporting activities to the state.

Health insurance was categorized into mutually exclusive categories (see the Methods section in the Supplementary Appendix). The 2014 expansion in Arkansas used Medicaid funds to purchase ACA marketplace plans for most newly eligible adults (sometimes called the "private option").[19] In contrast, most low-income adults in the other expansion states in our study (Kentucky and Louisiana) were eligible for Medicaid but not ACA marketplace plans. Because of the blurred boundary between Medicaid and marketplace coverage in Arkansas, coverage with Medicaid alone or marketplace coverage alone in Arkansas as compared with the other states would be misleading. Accordingly, we combined Medicaid and marketplace coverage into a single category.

Activities meeting the Arkansas work requirements included 80 hours per month of employment, job search, job training, or community service. Populations of adults who were eligible for exemptions included pregnant women, persons with disabilities or medical frailty, full-time students, persons caring for a child or other household member, and anyone receiving treatment for substance abuse. Since our baseline survey did not assess employment-related activities, our 2018 survey asked respondents about their activities 12 months earlier (during 2017) and then assessed their current activities. The survey questions are shown in the Supplementary Appendix; the 2018 survey questions used identical wording to our baseline survey whenever possible.

STATISTICAL ANALYSIS

Our approach was a difference-in-difference-in-differences (or triple-difference) model, which used comparisons according to year, state, and age group to identify changes in outcomes associated with the policy. Our model tested whether the change among respondents 30 to 49 years of age in Arkansas, relative to the change in other age groups in Arkansas, was larger than the comparable relative changes in other states. This method filters out time trends common to all four states and any state-specific factors influencing employment and coverage in Arkansas that were not due to work requirements. For instance, the waiver in Arkansas increased cost sharing and premiums for some enrollees in addition to work requirements, but these features were not specific to age.[20] We implemented this model with adjustment for state, year, and age group (19 to 29 years, 30 to 39 years, 40 to 49 years, 50 to 59 years, and 60 to 64 years) and with pairwise

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interaction terms between those variables. The policy estimate came from the three-way interaction among indicator variables for Arkansas, the 30-to-49–year-old age group, and the year 2018; the regression equations are shown in the Supplementary Appendix.

For outcomes regarding insurance coverage and health care access, which were measured in separate samples from 2016 and 2018, we used a linear model with standard errors clustered according to age group and state (20 state–age group clusters); we used linear models for ease of interpretation of interaction terms, as is standard practice in difference-in-differences analyses.[21] For community engagement outcomes, which were measured in the 2018 sample on the basis of questions regarding activities in the previous year and current year, we used a multilevel mixed model with random effects for age groups in each state and for each respondent.

All models adjusted for sex, respondent-reported race and ethnic group, educational level, interview language (English or Spanish), marital status, and residence area (urban or rural). All analyses used survey weights to reflect the target population in each state (see the Supplementary Appendix).

To assess awareness of and experiences with work requirements in Arkansas, we calculated survey-weighted means. We estimated a multivariate logistic model to identify demographic predictors of awareness of work requirements.

We conducted several sensitivity analyses: a difference-in-differences model that was limited to respondents 30 to 49 years of age, comparing Arkansas with the other states; models for community engagement that adjusted directly for baseline employment (before the implementation of a work requirement), with the use of a single observation per person; and an analysis of the U.S. Census Bureau American Community Survey for 2016 and 2017 to test whether trends in coverage and employment were similar across our study states and age groups before the implementation of work requirements. We report P values (unadjusted and post hoc family-wise adjusted; see the Supplementary Appendix) only for our three primary outcomes, and we report results with 95% confidence intervals (without adjustment for multiple comparisons) for the primary and secondary outcomes.

RESULTS

STUDY SAMPLE AND DESCRIPTIVE STATISTICS

The overall sample included 5955 respondents (3004 respondents from the 2018 survey, and 2951 from the 2016 baseline data[17,18]). Approximately half the 2018 sample was from Arkansas. Most respondents (90.3%) were recruited by means of random-digit dialing; the remainder consisted of respondents from previous surveys conducted by our survey vendor who were recontacted. A total of 14% of the persons who were contacted for the survey completed it.

Table 1 presents summary statistics for the study sample according to state (Arkansas vs. others) and age (30 to 49 years vs. others). In all four groups, the majority of the respondents were non-Hispanic white, and approximately one quarter of the respondents were black; Hispanic ethnicity was more common in the comparison states than in Arkansas. Respondents in Arkansas disproportionately lived in rural areas.

HEALTH INSURANCE COVERAGE

Figure 1 and Table S1 in the Supplementary Appendix present unadjusted rates of insurance coverage according to year, age, and state. The share of Arkansans 30 to 49 years of age who had Medicaid or ACA marketplace coverage went from 70.5% in 2016 to 63.7% in 2018, a decline of 6.8 percentage points. Meanwhile, the levels of Medicaid or marketplace coverage in the other age groups in Arkansas and among non-Arkansas residents showed smaller changes, ranging from an increase of 3.9 to a decrease of 1.3 percentage points. The percentage of uninsured respondents among Arkansans 30 to 49 years of age increased from 10.5% in 2016 to 14.5% in 2018, with smaller or no changes in the other groups. The percentage of Arkansans 30 to 49 years of age with employer-sponsored coverage increased slightly, from 10.6% to 12.2%.

Table 2 presents regression estimates of our primary insurance coverage outcomes. The model indicated that the percentage of respondents with Medicaid or marketplace coverage declined by 13.2 percentage points (95% confidence interval [CI], −23.3 to −3.2) more among Arkansans 30 to 49 years of age relative to other age groups in the state than the comparable age-based difference in the control states (P=0.01). The analogous esti-

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Table 1. **Characteristics of the Study Sample of Low-Income Adults in Arkansas and in the Control States of Kentucky, Louisiana, and Texas.***

Characteristic	Arkansas, 30–49 Yr of Age (N=804)	Arkansas, Other Ages (N=1430)	Control States, 30–49 Yr of Age (N=1295)	Control States, Other Ages (N=2426)
	percentage of respondents			
Age group				
19–29 Yr	0	50.8	0	49.3
30–39 Yr	57.0	0	54.5	0
40–49 Yr	43.0	0	45.5	0
50–59 Yr	0	31.0	0	31.4
60–64 Yr	0	18.2	0	19.3
Race or ethnic group†				
White non-Hispanic	61.9	64.9	53.1	53.9
Hispanic	4.7	5.8	16.3	17.0
Black non-Hispanic	27.2	23.7	27.1	24.2
Other	6.2	5.7	3.5	4.9
Educational level				
No high school diploma	18.9	18.5	22.7	23.2
High school diploma or equivalent	45.0	42.7	41.9	38.1
Some college or college degree	36.1	38.8	35.3	38.7
Female sex	58.6	54.6	60.1	55.6
Married or living with a partner	44.6	37.8	47.1	35.0
Interview conducted in Spanish	0.7	0.1	4.0	3.2
Resident in rural area	50.2	54.8	35.8	31.1

* Data are from a telephone survey involving low-income adults (income <138% of the federal poverty level), 19 to 64 years of age, who were U.S. citizens. The survey was conducted in November and December 2016 and in November and December 2018. The target group comprised respondents 30 to 49 years of age in Arkansas. For the comparison groups, other ages were respondents 19 to 29 years of age and those 50 to 64 years of age. All estimates are survey-weighted. Percentages may not total 100 because of rounding.

† Race and ethnic group were reported by the respondent.

mate of changes in the percentage of respondents who were not insured was an increase of 7.1 percentage points (95% CI, 0.5 to 13.6; P=0.04). Models that were limited to respondents 30 to 49 years of age showed a pattern of coverage changes associated with the Arkansas work requirements that were similar to those in our primary model. There were no significant changes associated with work requirements in the percentage of respondents with employer-sponsored insurance or the two access measures (Table S2 in the Supplementary Appendix).

EMPLOYMENT AND COMMUNITY ENGAGEMENT

Figure 2 and Table S3 in the Supplementary Appendix present unadjusted estimates of employment and community engagement according to year, age, and state. In all groups, the percentages of respondents who were employed at least 20 hours per week declined from 2017 to 2018, and the percentage of respondents reporting disability increased. Employment declined from 42.4% to 38.9% among Arkansans 30 to 49 years of age, a change of −3.5 percentage points. The three comparison groups had similar decreases, ranging from −2.9 to −5.7 percentage points. Overall, more than 92% of the respondents in all four groups — and nearly 97% of the respondents 30 to 49 years of age in Arkansas — were already meeting the community engagement requirement

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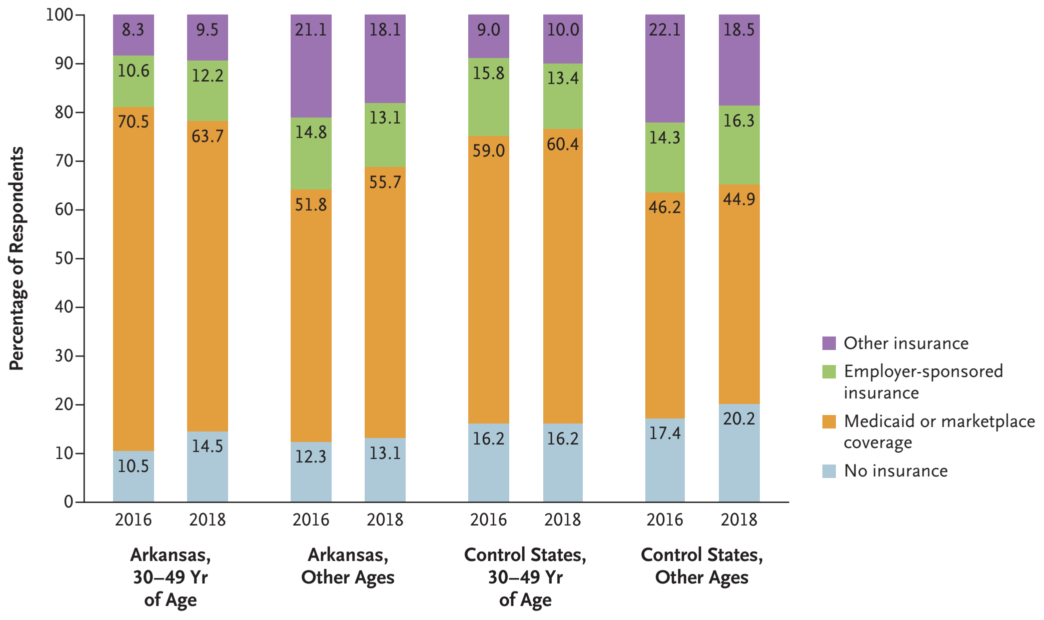

Figure 1. Health Insurance Status According to Year, State, and Age Group.

Data are from a telephone survey involving 5955 low-income adults 19 to 64 years of age that was conducted in November and December 2016 and in November and December 2018. All estimates are survey-weighted. Medicaid and Affordable Care Act marketplace coverage were combined because the Medicaid expansion in Arkansas was implemented with the use of a private insurance expansion, in which most (but not all) expansion enrollees were placed in subsidized marketplace coverage rather than traditional Medicaid. Coverage types were mutually exclusive and categorized according to an insurance hierarchy (see the Methods section in the Supplementary Appendix). The target group comprised respondents 30 to 49 years of age in Arkansas. “Other ages” were the groups of respondents 19 to 29 years of age and those 50 to 64 years of age. Kentucky, Louisiana, and Texas were the control states. Percentages may not total 100 because of rounding.

or should have been exempt before the policy took effect. The share of respondents who were not meeting the requirement increased from 3.3% in 2017 to 3.9% in 2018 in the Arkansas target age group (an increase of 0.6 percentage points), whereas other groups had changes ranging from −1.0 to +0.2 percentage points.

Table 3 presents the regression results for employment. Table S4 in the Supplementary Appendix shows secondary outcomes of hours worked and any community engagement. Neither the main model nor analyses involving only respondents 30 to 49 years of age indicated any significant changes in these outcomes. Similar results were seen in alternative models that adjusted for employment in the previous year (Table S5 in the Supplementary Appendix).

EXPERIENCE WITH WORK REQUIREMENTS

Table S6 in the Supplementary Appendix describes Arkansas residents’ awareness of and experience with work requirements. A total of 32.9% of the adults 30 to 49 years of age who had Medicaid or marketplace coverage had not heard anything about the policy. Multivariate analysis indicated that Medicaid or marketplace enrollees were more likely to know about the policy than those with other coverage (Table S7 in the Supplementary Appendix). Adults 19 to 29 years of age, men, and those without a high-school diploma were less likely to know about the requirements than respondents 30 to 49 years of age, women, and respondents with some college or a college degree, respectively.

Nearly half the target population was unsure whether the requirements applied to them (Table S6 in the Supplementary Appendix). Among Arkansans 30 to 49 years of age who had Medicaid or marketplace coverage or no insurance, only 21.8% thought that they were (or would be) sub-

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Table 2. Regression Estimates of Changes in Health Insurance Associated with Medicaid Work Requirements in Arkansas.*

Outcome and Analysis	Arkansas		Control States		Adjusted Difference in Change (95% CI)†	P Value‡
	2016 (N=733)	2018 (N=1501)	2016 (N=2218)	2018 (N=1503)		
	percent of respondents				*percentage points*	
Respondents with Medicaid or marketplace coverage						
Difference-in-differences analysis involving respondents 30–49 yr of age	70.5	63.7	59.0	60.4	−10.4 (−18.5 to −2.4)	0.02
Difference-in-differences analysis involving respondents in control age groups of 19–29 yr and 50–64 yr of age	51.8	55.7	46.2	44.9	4.0 (−3.9 to 11.9)	0.29
Triple-difference analysis of target age group vs. control age groups and of Arkansas vs. control states	—	—	—	—	−13.2 (−23.3 to −3.2)	0.01
Respondents with no insurance						
Difference-in-differences analysis involving respondents 30–49 yr of age	10.5	14.5	16.2	16.2	5.9 (0.4 to 11.4)	0.04
Difference-in-differences analysis involving respondents in control age groups of 19–29 yr and 50–64 yr of age	12.3	13.1	17.4	20.2	−1.5 (−6.0 to 2.9)	0.46
Triple-difference analysis of target age group vs. control age groups and of Arkansas vs. control states	—	—	—	—	7.1 (0.5 to 13.6)	0.04

* The study sample was from a telephone survey involving 5955 low-income adults 19 to 64 years of age conducted in November and December 2016 and in November and December 2018, minus item nonresponse for each study outcome. All estimates are survey-weighted. Standard errors were clustered according to age group in each state. Coverage types were mutually exclusive and categorized according to an insurance hierarchy. All models were adjusted for sex, respondent-reported race and ethnic group, education, language of the interview (English or Spanish), marital status, and urban or rural residence, as well as by age group, state, and year. Details and full regression equations are provided in the Methods section in the Supplementary Appendix. CI denotes confidence interval.

† The adjusted change associated with work requirements is the policy estimate. Results are from difference-in-differences analysis for the first two rows of each outcome and for the difference-in-difference-in-differences (or triple-difference) analysis for the third row of each outcome.

‡ Family-wise P values, with adjustment for two primary outcomes within the family of coverage outcomes, yield the following P values (in the same order as in the table): 0.03, 0.54, 0.03, 0.03, 0.49, and 0.04 (see the Methods section in the Supplementary Appendix).

ject to the new work requirements; 44.2% were unsure. A total of 14.4% of the respondents outside this age group incorrectly believed that they were subject to the requirements in 2018, and 50.2% were unsure. Among the respondents who had been told by the state that they needed to report community engagement activities, only 49.3% were doing so regularly. The most common reason for not reporting was a belief that they were not meeting the requirement (40.4%), but their other responses indicated that all 22 of these respondents did satisfy the requirements. Other reasons for not reporting to the state were lack of Internet access (32.3% of respondents) and confusion about reporting (17.8%).

TRENDS IN OUTCOMES BEFORE WORK REQUIREMENTS

Table S8 in the Supplementary Appendix shows analysis of data from the American Community Survey from 2016 and 2017. These results revealed no significant differential changes in coverage or

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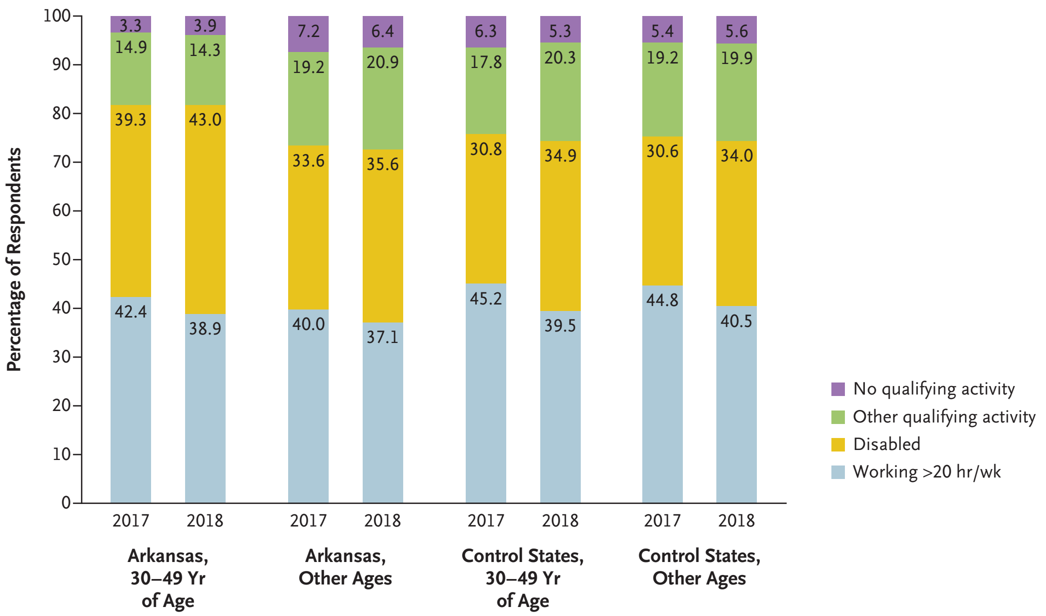

Figure 2. Employment and Community Engagement Activities According to Year, State, and Age Group.

Shown are the results from a telephone survey involving 3004 low-income adults 19 to 64 years of age that was conducted in November and December 2018. Respondents were asked about these outcomes for the current year and for the 12 months before the survey (in 2017). All estimates are survey-weighted. Other qualifying activity included full-time student status, participation in job training, actively seeking work, community service, pregnancy, or caring for a child or household member who could not care for himself or herself. Outcomes were assessed in a mutually exclusive hierarchy, from the bottom to top in each bar (e.g., if a person was working >20 hours a week, we did not assess whether the person was disabled or met another category of activity). The target group comprised respondents 30 to 49 years of age in Arkansas. "Other ages" were the groups of respondents 19 to 29 years of age and those 50 to 64 years of age. Kentucky, Louisiana, and Texas were the control states. Percentages may not total 100 because of rounding.

employment according to state or age group before 2018.

DISCUSSION

Using a timely survey involving low-income adults in Arkansas and three comparison states, we found that implementation of the first-ever work requirements in Medicaid in 2018 was associated with significant losses in health insurance coverage in the initial 6 months of the policy but no significant change in employment. Lack of awareness and confusion about the reporting requirements were common, which may explain why thousands of persons lost coverage even though more than 95% of the target population appeared to meet the requirements or qualify for an exemption.

Our findings regarding coverage are consistent with the official report from Arkansas that nearly 17,000 adults were removed from Medicaid between October and December 2018.[9] We estimate from the American Community Survey that our sampling frame corresponds to 140,000 low-income adults 30 to 49 years of age in Arkansas. Taken together, these numbers imply a reduction in Medicaid enrollment of 12 percentage points, which is well within our confidence intervals. Our results show that this loss of Medicaid coverage was accompanied by a significant increase in the percentage of adults who were uninsured, indicating that many persons who were removed from Medicaid did not obtain other coverage. Although point estimates suggest a potential increase in the use of employer-sponsored insurance, confidence intervals for this measure in-

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Table 3. Regression Estimates of Changes in Employment Associated with Medicaid Work Requirements in Arkansas.*

Outcome and Analysis	Arkansas		Control States		Adjusted Difference in Change (95% CI)†	P Value
	2017 (N=1501)	2018 (N=1501)	2017 (N=1503)	2018 (N=1503)		
	percent of respondents				*percentage points*	
Respondents reporting employment						
Difference-in-differences analysis involving respondents 30–49 yr of age	46.9	42.2	50.6	44.0	1.6 (−5.0 to 8.2)	0.63
Difference-in-differences analysis involving respondents in control age groups of 19–29 yr and 50–64 yr of age	45.0	43.5	49.3	45.2	2.7 (−1.7 to 7.1)	0.23
Triple-difference analysis of target age group vs. control age group and of Arkansas vs. control states	—	—	—	—	−1.1 (−8.7 to 6.5)	0.78

* The study sample was from a telephone survey involving low-income adults 19 to 64 years of age. Respondents in 2018 were asked about their activities for the previous year (12 months earlier in 2017) and about the current year. Thus, each sample contains two observations per person. The model used random effects at the individual and age-group (per state) levels to account for repeated observations. All estimates are survey-weighted. Standard errors were clustered according to age group in each state. All models were adjusted for sex, respondent-reported race and ethnic group, educational level, language of the interview (English or Spanish), marital status, and urban or rural residence, as well as for age group, state, and year. The full regression equations are provided in the Methods section in the Supplementary Appendix.

† The adjusted change associated with work requirements is the policy estimate. Results are from difference-in-differences analysis for the first two rows and for the difference-in-difference-in-differences (or triple-difference) analysis for the third row.

cluded no effect. We did not detect any meaningful changes in the percentages of respondents having a personal physician or cost-related delays in care in the first 3 months of disenrollment; longer-term assessment will be essential.[22-25]

We did not find any significant change in employment (one of our three primary outcomes) or in the related secondary outcomes of hours worked or overall rates of community engagement activities. Although our confidence intervals are wide enough that policy-relevant changes cannot be ruled out, nearly everyone who was targeted by the policy already met the requirements, so there was little margin for the program to increase community engagement. This finding is consistent with analyses predicting that most Medicaid beneficiaries already satisfy work requirements in one way or another.[10,11,14,15,26]

Our descriptive results indicate that the implementation of this policy was plagued by confusion among many enrollees, a finding consistent with qualitative research.[27,28] Lack of Internet access was also a barrier to reporting information to the state, although in late December 2018 Arkansas added a telephone option.[29] To reduce the administrative burden on beneficiaries, state officials used existing data sources when possible to confirm employment or disability status, which exempted two thirds of enrollees from the reporting requirement.[30] Nonetheless, major barriers remain. One third of persons who were subject to the policy had not heard anything about it, and 44% of the target population was unsure whether the requirements applied to them. Levels of awareness were worse among persons with less education and among adults 19 to 29 years of age, who became subject to the Arkansas requirements in January 2019.[20] Although Medicaid has always struggled with high turnover owing in part to legally required annual eligibility redeterminations,[31] our findings suggest that work requirements have substantially exacerbated administrative hurdles to maintaining coverage.

Our study has several limitations. First, our response rate was lower than that of government surveys. However, our approach of combining

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random-digit dialing telephone surveys with demographic weighting for nonresponse has been used to provide timely evidence regarding Medicaid and the ACA, with results similar to those produced by subsequent analyses of government data.[32-36]

Our analysis is based on survey data regarding a policy that created substantial confusion, which makes it difficult to attribute any single respondent's loss of coverage directly to work requirements as opposed to other factors, such as income changes or incompletion of renewal paperwork. Questions about employment may suffer from social desirability bias, leading to greater reporting of employment this year among persons who were subject to the new work requirement. Our lack of baseline data on employment meant that we had to ask respondents about employment activities in the current and previous years, which raises the possibility of recall bias. However, this phenomenon is likely to be similar across states and age groups and would be filtered out by our study design.

The study was limited to a single state implementing work requirements and approximately 6000 respondents overall. Survey questions about experiences reporting work hours to the state applied only to small numbers of respondents. In addition, details regarding work requirements in other states vary and could produce different results.[10] Finally, our study was nonrandomized, and unmeasured time-varying confounders could bias the results. However, our use of both within-state and out-of-state control groups reduces this possibility.

In conclusion, in its first 6 months, work requirements in Arkansas were associated with a significant loss of Medicaid coverage and rise in the percentage of uninsured persons. We found no significant changes in employment associated with the policy, and more than 95% of persons who were targeted by the policy already met the requirement or should have been exempt. Many Medicaid beneficiaries were unaware of the policy or were confused about how to report their status to the state, which suggests that bureaucratic obstacles played a large role in coverage losses under the policy.

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Precarious Work Schedules among Early-Career Employees in the US: A National Snapshot

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August 27, 2014

Abstract

This research brief presents an overview of work schedules among a representative sample of early-career adults (26 to 32 years old) in the United States. Based on an analysis of new items included in the National Longitudinal Survey of Youth 1997 (NLSY97), the brief describes the distribution of three dimensions of work schedules—advance schedule notice, fluctuating work hours, and schedule control—across early-career workers in hourly and non-hourly jobs, overall and separated by gender, regular work hours (full-time/part-time), race, and occupation. In addition, the brief gives special consideration to selected groups of hourly workers, including parents, women, workers of color, and workers in low-pay, high-growth occupations, who are at particular risk of precarious work schedules and economic insecurity. Finally, the brief suggests some implications of these descriptive findings for public policy and future research.

Acknowledgements

We are grateful to Rupa Datta at the National Opinion Research Center and to members of the Bureau of Labor Statistics NLSY97 design team for the invitation to work with them to develop the measures of advance notice and fluctuating work hours and to refine the response categories for the measure of schedule control.

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The authors of this research brief are solely responsible for its content.

Cover photos: Jocelyn Augustino, Petteri Sulonen, and Getty Images.

Precarious Work Schedules among Early-Career Employees in the US: A National Snapshot

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Introduction

This research brief presents an overview of work schedules among a representative sample of early-career adults (26 to 32 years old) in the United States. Harriet Presser's (2003) early research on nonstandard timing made clear that work schedules in many US jobs hold important implications for worker and family well-being. New items included in the National Longitudinal Survey of Youth 1997 Cohort (NLSY97) allow us to analyze three additional dimensions of work schedules: (1) advance schedule notice, (2) fluctuating work hours, and (3) schedule control. This is the first time a measure of advance notice has been included in a US national survey and the first opportunity to gauge the prevalence and magnitude of weekly work-hour fluctuations across the US labor market. Modifications to an existing NLSY97 question about schedule control also make it possible to differentiate between workers whose schedules are set by their employers without their input and those workers who have at least some input into the timing of their work. The unusual detail and breadth of these data provide a valuable picture of the prevalence of these work schedule dimensions and how they intersect to place certain occupational and demographic groups at risk of work schedules that are unpredictable, unstable, or unwanted—in a word, precarious.

This is the first time a measure of advance notice has been included in a US national survey and the first opportunity to gauge the prevalence and magnitude of weekly work-hour fluctuations across the US labor market.

This brief begins with an examination of how each of these three dimensions of work schedules varies among early-career workers in hourly and non-hourly jobs, overall as well as separated by gender, regular work hours (full-time/part-time), race, and occupation. We then take a closer look at selected groups of hourly workers including parents, women, workers of color, and workers in low-pay, high-growth occupations, namely retail, food service, home care, and building-cleaning occupations. We conclude with some thoughts about the implications of these early results for public policy and further scholarly research.

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[1] *Susan J. Lambert and Julia R. Henly are associate professors in the School of Social Service Administration. Peter J. Fugiel is a doctoral candidate in the Department of Sociology.*

Precarious schedules and worker well-being

Work schedules can facilitate or hinder the ability of workers to arrange caregiving, pursue education, secure a second job, and earn an adequate income. Scholars have documented the difficulties posed by nonstandard timing (Dunifon, Kalil, Crosby, & Su, 2013; Han, 2004; Heymann, 2000; Joshi & Bogen, 2007; Presser, 2003) and lack of schedule control (Grzywacz, Carlson, & Shulkin, 2008; Kleiner & Pavalko, 2010; Kelly, Moen, & Tranby, 2011; Lyness, Gornick, Stone, & Grotto, 2012; Major, Klein, & Ehrhart, 2002) for family routines, marital quality, child well-being, worker health, and job performance. Recent research from case studies of firms in various industries suggests that fluctuating hours and schedule unpredictability can also undermine the health and well-being of employees and can make it difficult to secure a second job or attend school (Clawson & Gerstel, 2014; Haley-Lock, 2011; Henly & Lambert, 2014; Henly, Shaefer, & Waxman, 2006). Moreover, eligibility for many social programs depends on the number and stability of work hours. For example, although not required by federal law, states commonly tie work hours and child care subsidies closely together, making it difficult for workers with scheduling challenges to get help paying for child care or use formal child care providers (Ben-Ishai, Matthews, & Levin-Epstein, 2014; Sandstrom, Henly, Claessens, & Ros, 2014). Work-hour requirements are based on the assumption that workers decide how many hours they work, yet because hours are a key component of labor costs, corporate policies often restrict their availability. Conditioning receipt of social benefits on work hours means that workers who experience an unwanted drop in hours can be placed in double-jeopardy as they risk being denied social benefits at the very time they need supports most (Lambert & Henly, 2013).

Prior measures of precarious schedules

Many national surveys originated during an earlier period characterized by widespread standard employment, in other words, full-time jobs with stable schedules. Survey items were deliberately designed to smooth rather than reveal variations in work hours. Most national surveys that address employment continue to ask respondents to report their usual hours of work or the number of hours they worked last week. If respondents volunteer that they cannot answer the usual-hours questions because their hours vary too much, some surveys allow for a variable-hours code. For example, pooling Current Population Survey (CPS) data from 2000 through 2002, 6.4 percent of workers were coded as "hours vary" (Lambert, Haley-Lock, & Henly, 2012). The problem with this approach is that even workers whose hours vary a great deal are likely to offer a numeric response to the usual-hours question rather than volunteer that their hours vary, resulting in an underreporting of hour variation. In a survey of 293 retail employees, in which respondents were randomly assigned to receive either the question "How many hours do you typically work each week?" or one that continued with the option "or do your hours vary too much to say?" only 2 percent of respondents volunteered without prompting that their hours varied as compared with 25 percent of those explicitly given this option.[2] In addition to the usual-hours question, some national surveys include measures of schedule input and nonstandard timing. However, surveys lack the information needed to assess the variability and unpredictability of employees' work schedules. These limitations have, until now, precluded analyses of the intersecting dimensions of precarious schedules on a national scale.

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[2] *Unpublished analyses of data from the University of Chicago Work Scheduling Study; contact authors for more information.*

Measures of precarious scheduling dimensions included in the NLSY97

Recently released data from the National Longitudinal Survey of Youth 1997 Cohort (NLSY97) provide information on multiple dimensions of work schedules among a representative sample of early-career adults. Beginning in 2011 with Round 15 of this ongoing survey, respondents were asked new questions designed to measure advance notice, work-hour fluctuations, and schedule control. The text of these questions follows.

1. Advance notice

How far in advance do you usually know what days and hours you will need to work?

- *One week or less*
- *Between 1 and 2 weeks*
- *Between 3 and 4 weeks*
- *4 weeks or more*

2. Work-hour fluctuations

a. *In the last month, what is the greatest number of hours you've worked in a week at this job? Please consider all hours, including any extra hours, overtime, work you did at home, and so forth.*

b. *In the last month, what is the fewest number of hours you've worked in a week at this job? Please do not include weeks in which you missed work because of illness or vacation.*

3. Schedule control

Which of the following statements best describes how your working hours are decided? By working hours we mean the time you start and finish work, and not the total hours you work per week or month.

- *Starting and finishing times are decided by my employer and I cannot change them on my own.*
- *Starting and finishing times are decided by my employer but with my input.*
- *I can decide the time I start and finish work, within certain limits.*
- *I am entirely free to decide when I start and finish work.*
- *When I start and finish work depends on things outside of my control and outside of my employer's control.*

Overview of the NLSY97 and selected sample

The NLSY97 is a nationally representative[3] survey of people born between 1980 and 1984 who were living in the US in 1997. The National Opinion Research Center (NORC) conducts the survey under the direction of the US Bureau of Labor Statistics (BLS).The NLSY97 was conducted annually through Round 15 (2011-2012) but future rounds will be fielded every other year. Respondents were 26 to 32 years old in Round 15, the first round to include the new questions on advance notice and the source of the data presented in this research brief.[4] The overall response rate for Round 15 is 86.5 percent. For the analyses presented in this research

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[3] *This report uses sampling weights provided by the BLS to adjust statistical estimates for oversampling of youth of color. However, inferences of statistical significance are based on the number of sample observations, not the population N. We use a standard threshold (p<.05) for significance throughout this report.*

[4] *According to estimates from the Current Population Survey, about a third of workers (34 percent of men and 33 percent of women) of what is considered to be prime labor market age (25-54) are between the ages of 25 and 34. This was true in both 2011 when the NLSY97 data were gathered and as recently as June 2014.*

brief, the sample has been narrowed to respondents currently holding civilian jobs in the wage and salaried workforce (N = 3,739). We focus only on respondents' main job, which the NLSY97 defined for these questions as the job of the longest duration.[5]

Overview of respondents. As shown in Table 1, 62 percent of workers in the target population[6] report that they are paid by the hour, 24 percent work part-time (defined as working less than 35 regular hours per week on the main job), 57 percent have no more than a high school education, 58 percent are living with a partner, and 34 percent have a child younger than 6 in their household. The population is equally split on gender, and 69 percent are White, 13 percent Black, and 13 percent Hispanic.

Additional information about the sample

It is important for readers to note that our sample does not include all respondents in the target population (current civilian employees) due to problems with the survey instrument. Some respondents who, according to NLSY97 documentation, should have been asked the new work scheduling questions were erroneously skipped past this section by early versions of the computerized interview guide. Our analyses suggest that this excluded group amounts to 26 percent of eligible Round 15 respondents in the target population. BLS staff responded to our queries about these missing data by documenting patches in survey programming that they implemented to correct skip patterns as problems came to light during the field period.[7] The problematic skip patterns mostly affected respondents not paid by the hour. Approximately 42 percent of eligible respondents in non-hourly jobs were not asked the scheduling questions as compared with 11 percent of those in hourly jobs. Eligible respondents who were living in urban (as compared to rural) locations, were male (as compared to female), or Black (as compared to White) were significantly ($p<.05$) more likely to be skipped past the scheduling questions. Living with a partner or with children was not significantly associated with the probability of being asked the new scheduling questions.

We conduct most of our analyses separately by pay status, that is, we separate respondents paid an hourly wage (hourly) from those paid by some other metric (non-hourly). We find that these groups of employees report quite different scheduling practices, although caution is warranted in interpreting these differences. The NLSY97 infers pay status from the time unit respondents use to report their job earnings.[8] Although all non-hourly employees are asked whether they are paid by the hour, the NLSY97 does not distinguish between salaried employees and other non-hourly workers. Comparative studies suggest that the majority of NLSY97 respondents in the non-hourly group receive a salary (Hamermesh, 2002). Ninety-five percent of non-hourly employees in our sample report weekly, bi-weekly, monthly, quarterly, or annual earnings. But in the absence of explicit confirmation by respondents, we are reticent to interpret these time units as evidence of salaried employment. What we do know is that a small percentage of non-hourly workers (5 percent) report being paid in atypical ways such as by the day, per job, or by commission only.

Given these caveats, readers should have greater confidence in the potential of the data to represent the experiences of early-career workers paid by the hour than those paid by other means. The lack of comparable national data on work schedules means that it is not possible at this time to gauge the biases of this particular sample. Moreover, our sample excludes respondents who said they were self-employed and thus, does not represent the experiences of independent and contract workers who may be at especially high risk for precarious employment, including the types of scheduling practices examined here (Kalleberg, 2011). In sum, this research brief should be viewed as a preliminary, rather than a definitive, estimate of precarious scheduling practices among early-career adults in the US wage and salaried workforce. As more data of this type are collected,[9] understanding of precarious schedules will expand to other groups and improve in precision.

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[5] *For a large majority (87 percent) of the target population, this main job is their only current employee job. Of the 13 percent of workers who held two or more jobs at the time of the survey, most (59 percent of the 13 percent) reported working more hours at their "main" job than at any other job.*

[6] *Once again, this population includes current civilian employees in the US born between 1980 and 1984 who were living in the US in 1997. In order to draw inferences from our sample about this population, we adjust the observed distribution of responses by a set of weights based on respondents' probability of being selected into the sample. Except where otherwise indicated by reference to the "sample" or "respondents," the statistics reported here are population estimates. For the sake of brevity we do not include the number of sample observations in most tables, but these data are reflected in our inferences about statistical significance. We plan to present more detailed tables in a future publication of our main results.*

[7] *We thank Steve McClaskie in particular for his patient and detailed responses to our numerous queries.*

[8] *"For your job with [employer name], what is the easiest way for you to report your total earnings before taxes or other deductions: hourly, weekly, annually, or on some other basis?"*

[9] *The BLS has also included the scheduling items in Round 16 of the NLSY97, which has not yet been released.*

As shown in Table 2, our sample comprises a variety of jobs that span the range of the formal labor market and include both male-dominated and female-dominated occupations. The categories used here are modifications of existing classifications (Goldthorpe, 2000; Mouw & Kalleberg, 2010) that group occupations according to their socioeconomic status, typical employment relationship, and supervisory position.

Table 1: Job and personal characteristics

Group	No. of respondents (unweighted)	Est. % of population (weighted)
Hourly employees	2,394	62
Non-hourly employees	1,344	38
Full-time (35+ hours per week)	2,837	76
Part-time (< 35 hours per week)	890	24
Men	1,842	50
Women	1,897	50
Black, not Hispanic	884	13
Hispanic	821	13
White, not Hispanic	1,905	69
Asian	60	2
Other	69	3
Less than HS	258	6
HS or GED	2,024	51
Some college	305	8
BA or higher	1,146	35
Cohabiting with a spouse/partner	2,039	58
Not cohabiting	1,689	42
Child < 6 years old in HH	1,297	34
Child 6 to 12 years old in HH	424	10
Total Sample	**3,739**	

Table 2. Occupational composition*

	No. of respondents (unweighted)	Est. % of population (weighted)
Elite professionals	88	3
Business staff	317	10
Technical and research staff	179	5
Arts and media occupations	75	2
Office clerks	402	10
Social functionaries	559	16
Service supervisors	236	6
Service workers	1,085	27
Production supervisors	56	2
Skilled trades	296	8
Production workers	412	11
Agricultural occupations+	17	1

**Appendix A contains an overview of the occupations included in each of these categories.*
+Excluded in subsequent analyses broken out by occupation because of the small number of respondents.

Advance notice

Advance notice of one's work schedule is an important source of predictability that can facilitate one's ability to meet both work and personal responsibilities. The further in advance workers know their work schedule, the more time they have to arrange their personal responsibilities in ways that enable them to meet work requirements. Schedule unpredictability, on the other hand, interferes with the ability of workers to plan non-work activities such as scheduling doctor's appointments, socializing with friends, and eating meals with friends or family, contributing to worker stress and work-family conflict (Alexander, Haley-Lock, & Ruan, forthcoming; Henly & Lambert, 2014). For parents, schedule unpredictability can make it difficult to arrange reliable child care and to participate in family routines that experts say are integral to healthy child development, such as monitoring homework and establishing bedtime routines (Henly & Lambert, 2005; Henly, Waxman, & Shaefer, 2006; Miller & Han, 2008). And for employees paid by the hour, an unpredictable work schedule also means unpredictable earnings.

The research cited above on unpredictable work schedules has primarily focused on nonproduction occupations at the lower end of the labor market. The new measure of advance schedule notice in the NLSY97 provides the first data on how advance notice is distributed across the labor market. This allows us to describe schedule unpredictability beyond low-status occupations and offer a fuller picture of which groups do and do not enjoy advance schedule notice.

These differences at the extremes of advance notice demonstrate that work schedules are a source of stratification and inequality in the labor market.

Table 3 summarizes how far in advance employees know what days and hours they will need to work. We estimate that over a third (38 percent) of early career employees overall know their work schedule one week or less in advance. Such short notice is estimated to be significantly more common among workers paid by the hour (41 percent) than by other means (33 percent), among part-time (48 percent) than full-time workers (35 percent), and among workers of color (44 to 45 percent) than among White non-Hispanic workers (35 percent).

In addition to the high rates of short notice among all types of workers, a notable finding highlighted in Table 3 is the bifurcation of responses to this survey question. Although 41 percent of hourly workers report knowing their work schedule only one week or less in advance, a comparable proportion (39 percent) report knowing their work schedule 4 or more weeks in advance. The middle categories (between 1 and 4 weeks) are the least common responses among all the groups considered here. A similar bifurcation is evident for non-hourly workers, despite their overall advantage over hourly workers: one-third of non-hourly workers receive one week or less notice whereas 54 percent of non-hourly workers receive four weeks or more notice, with the middle categories again being least common. These differences at the extremes of advance notice demonstrate that work schedules are a source of stratification and inequality in the labor market.

These data also suggest that short work schedule notice is not just a woman's issue. A significantly larger proportion of men (45 percent) than women (31 percent) report that they know their schedule one week or less in advance. Part-time workers are also at particularly high risk of unpredictable work, regardless of whether they are paid by the hour or not. Over 50 percent of part-time workers in non-hourly jobs and 47 percent of part-time workers in hourly jobs report that they know their work schedule one week or less in advance.

Table 4 shows how advance notice is distributed among occupational groups. Among service workers, production workers, and skilled trades, most employees know their schedule one week or less in advance. Service and production supervisors are equally split between the shortest and longest advance notice categories. In contrast, the majority of professionals, business staff, and providers of social services (for example, school teachers, social workers, and nurses) know their work schedule 4 or more weeks in advance. Schedule notice thus appears to follow status differentials between occupations, with unpredictability the norm among low qualification, closely supervised jobs and predictability the norm among jobs characterized by high educational qualifications and more prestige.

Table 3: Advance notice (percent of hourly, non-hourly, and combined total)*

	1 week or less			between 1 and 2			between 3 and 4			4 or more		
	Hrly	Non	Tot	Hrly	Non	Tot	Hrly	Non	Tot	Hrly	Non	Tot
All employees	41%	33%	38%	13%	9%	12%	6%	4%	5%	39%	54%	45%
Full-time (35+)	39	29	35	12	8	11	5	4	5	44	58	50
Part-time	47	52	48	17	15	16	10	4	8	27	29	28
Men	48	41	45	12	11	12	4	4	4	35	45	39
Women	34	25	31	14	8	12	8	5	7	43	63	51
White	39	30	35	12	8	11	7	4	6	42	57	48
Black	49	33	44	15	13	15	5	5	5	31	50	36
Hispanic	46	43	45	15	8	13	4	4	4	35	45	38

**Estimated proportion of employed cohort population overall and by pay type.*

Table 4. Advance notice by occupation (percent of population)*

	1 week or less	between 1 and 2	between 3 and 4	4 or more
Elite professionals	29%	6%	7%	58%
Business staff	24	10	4	62
Technical and research staff	30	11	4	56
Arts and media occupations	29	15	4	52
Office clerks	26	7	5	62
Social functionaries	18	8	9	65
Service supervisors	37	19	8	36
Service workers	48	17	6	30
Production supervisors	42	14	2	42
Skilled trades	60	10	2	28
Production workers	53	10	3	34

**Estimated proportion of employed cohort population by occupation.*

These data suggest that unpredictability as measured by limited advance schedule notice is a widespread but unevenly distributed feature of work for early-career adults. Part-time employees, skilled tradesmen, and workers in low-status occupations are particularly likely to know their schedule at most a week in advance. Within many demographic and occupational groups, however, employees seem to be divided into two main groups: one with very short notice and one with considerable advance notice. This "predictability gap" is a form of stratification that has not received much attention either from scholars or the public at large.

Work-hour fluctuations

Case studies of workers and firms in an expanding set of occupations and industries demonstrate that the number of hours employees work can vary enormously week to week (Appelbaum, Bernhardt, & Murnane, 2003; Gautié & Schmitt, 2010; Clawson & Gerstel, 2014; Haley-Lock, 2011; Jayaraman, 2013; Lambert, Henly, & Stanczyk, 2014; Luce & Fujita, 2012; Luce, Hammad, & Sipe, 2014). Until now, researchers have not had access to data on the prevalence of work-hour fluctuations across different sectors of the economy because most national surveys focus on estimating usual work hours. The new questions on greatest and fewest hours worked in the prior month included in the NLSY97 thus provide unique and needed information on the prevalence and magnitude of work-hour fluctuations across a representative sample of early career workers, albeit during a one-month period.[10]

Graphs 1 (hourly) and 2 (non-hourly) summarize the distribution of respondents' weekly work hours in the month prior to the survey. In order to show the relation between the range of hours worked and usual weekly hours, we group respondents in 5-, 10-, or 15-hour brackets according to their reported usual hours.[11] Each vertical box displays the range between the median fewest and greatest hours for respondents with usual work hours in a given bracket. The vertical lines, or "whiskers," extend from the 25th percentile of fewest hours to the 75th percentile of greatest hours among this same group. The diagonal trend line connects the median usual hours, marked by a dot, across hour brackets. The use of medians and percentiles rather than means allows us to focus on where the bulk of responses lie and leave out extremely high or low responses.

These graphs show clearly that hour fluctuations are common in our sample and typically quite large. Most of the boxes cover a median range of 10 hours or more, while most of the whiskers extend 5 or more hours beyond this range. The exception is workers who report between 40 and 44 usual hours per week, for whom 40 hours are the median fewest, greatest, and usual hours. This very stable group comprises about 43 percent of hourly employees and 39 percent of non-hourly employees. But for the majority of employees who work fewer than 40 or more than 44 hours in a normal week, hour fluctuations are the norm. Overall, the relationship between usual hours and the magnitude and direction of hour fluctuations is complex, requiring

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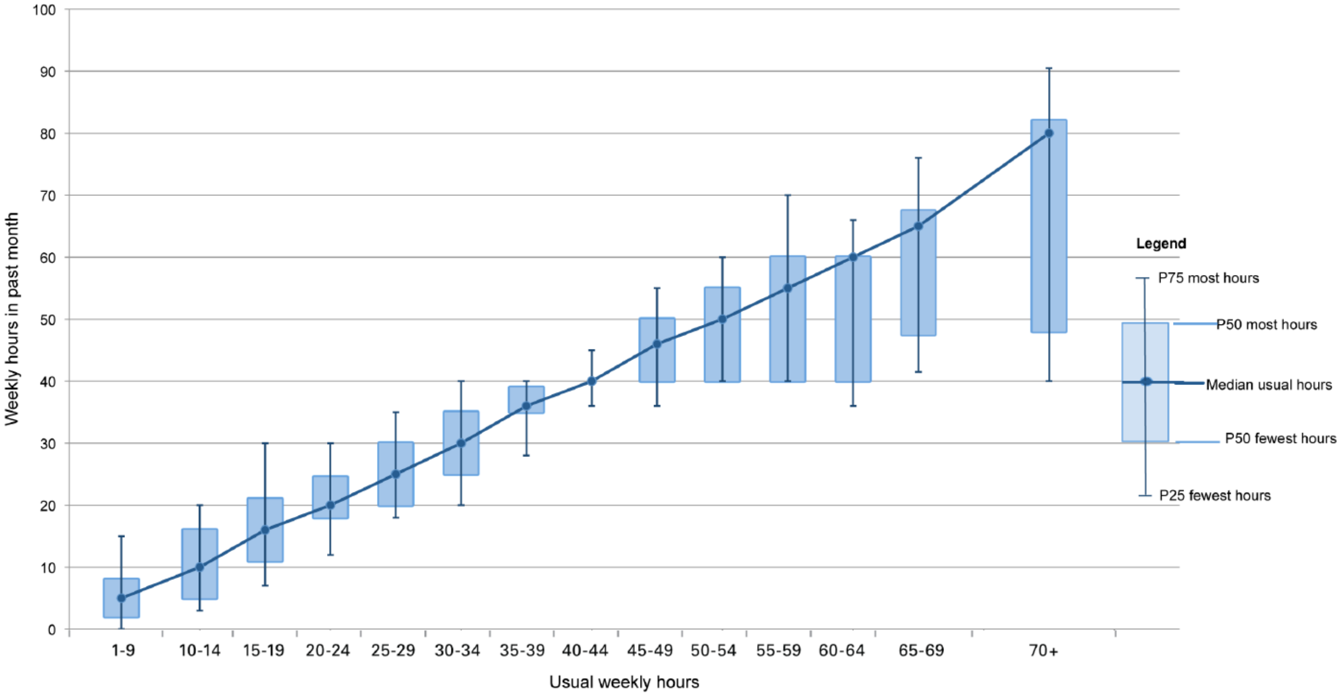
[10] *The NLSY97 also includes questions on usual work hours that predate Round 15. These items do not specify a reference period. Depending on a variety of work characteristics including duration of the job and whether they work overtime, respondents are asked one of the following questions: "How many hours do you work for [employer name] in a normal week? Please include all hours you work whether at your normal work site, at home, or in some other location." "How many hours do you usually work per week at this rate?" As with the new questions about greatest and fewest hours worked, respondents are asked to account for all of the time they spent working in the target job including overtime and work at home.*

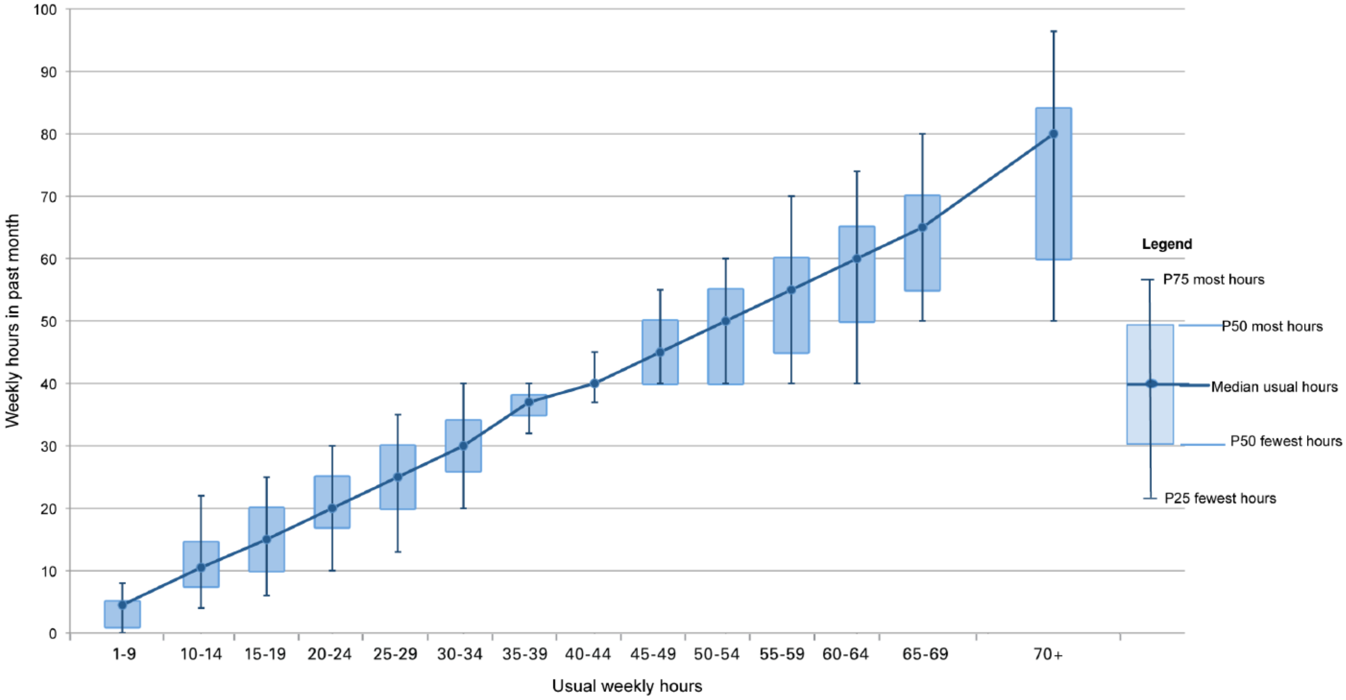
[11] *Workers are grouped into larger categories (wider brackets) at the low and high ends of usual hours due to the smaller number of cases at these extremes.*

close attention to different patterns of work hours. Readers are reminded that the questions on greatest and fewest weekly work hours during the past month ask workers to account for all of the time they spent working in the target job including work at home and overtime.

Graph 1. Hour fluctuations among hourly workers



Graph 2. Hour fluctuations among nonhourly workers



Flexing up or flexing down? Variations in weekly work hours are not necessarily problematic. Rates of involuntary part-time employment have escalated since 2006 (BLS, 2014) and thus, additional hours may be welcomed by some workers, especially those in short-hour jobs paid by the hour. At the other end of the labor market, where over-work is a concern (Golden, 2005; Reynolds, 2005), flexing down toward lower hours may provide a welcome respite from work and additional time to participate in personal and civic life (Jacobs & Gerson, 2004; Schor, 2008).

At the high end of the work-hour distribution, the 40-hour workweek seems to be a minimum rather than the norm.

The above graphs provide some evidence that hour fluctuations may offset low or high usual work hours, but again the picture is complex. Among employees who work 45 or more hours in a normal week, most of the range of work hours lies below respondents' usual hours. This means that it is more common for employees who usually work especially long hours to experience substantial decreases rather than increases in their weekly hours. Less than 25 percent of employees in this group report working fewer than 40 hours in the past month. At the high end of the work-hour distribution, the 40-hour workweek seems to be a minimum rather than the norm.[12]

At the other end of the work-hour distribution, the range of hours worked is more evenly distributed above and below respondents' usual hours. Among respondents working between 10 and 24 hours in a normal week, most report a range in the past month that spans at least 3 hours more and 4 hours less than their usual hours. Fluctuations of nearly a full conventional day of work over the course of a month may be more of a shock to part-time than to full-time employees, since this range represents a larger share of their total hours and, for hourly workers, of their paycheck. Moreover, only the top 25 percent of respondents working between 25 and 34 hours in a normal week reach the level of full-time hours in the past month. For most part-time workers, then, a 40-hour workweek is rare, despite considerable variation in weekly hours.

Prevalence and magnitude of work-hour fluctuations. Absolute fluctuations in work hours provide a concrete measure of work-hour instability, but the shortening or lengthening of a workweek by 8 hours is likely to mean something different to someone usually working 24 hours per week than to someone usually working 48 hours. Hour fluctuations also translate directly into fluctuations in pay for hourly workers, but not necessarily for non-hourly employees who may receive a set salary. It is helpful, therefore, to examine fluctuations relative to usual hours, not simply as a number of hours within discrete brackets, but as a standardized quantity that can be compared across different groups of workers. The following tables present summary statistics on the prevalence and magnitude of fluctuations in weekly work hours by combining responses on fewest, greatest, and usual hours among different demographic and occupational groups.

The columns titled "Any fluctuation" in Table 5 (hourly) and Table 6 (non-hourly) report the estimated share of employees with any work-hour fluctuations during the month, that is, the proportion of workers for whom the fewest hours worked in the past month are not equal to the greatest hours. Approximately 74 percent of employees in both hourly and non-hourly jobs experience at least some fluctuation in weekly hours over the course of a month. The range between the greatest and fewest weekly hours is considerable, amounting to at least one conventional 8-hour workday on average for each group considered here. Even part-time workers

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[12] *Recall that respondents are asked to report their fewest hours worked in the past month excluding weeks in which they "missed work because of illness or vacation."*

experience wide fluctuations in hours, with a mean range of 11 hours. Overall, the mean range is 10 hours among hourly workers as compared with nearly 12 hours among non-hourly workers. Note that there is considerable variation in the fewest and greatest number of hours worked by different groups, even when the range of hours is similar. Non-hourly employees tend to report working more hours than hourly employees and men more than women.

The columns titled "Instability ratio" provide a measure of the magnitude of fluctuations in hours relative to usual work hours, calculated by dividing the hour range by the reported usual hours [(greatest – fewest) ÷ usual]. This measure captures the intuition that a range of 10 hours represents a greater magnitude relative to a 20-hour week (instability ratio = 0.5) than to a 40-hour week (instability ratio = 0.25). As shown in Tables 5 and 6, the average instability ratio is 0.37 among hourly workers overall as compared with 0.32 among non-hourly workers. If we restrict our calculation to just those employees who experience some fluctuation in work hours (i.e., we exclude those with an instability ratio = 0), the average magnitude of work-hour fluctuations rises to 0.43 among non-hourly and 0.49 among hourly workers. We can interpret this last number as suggesting that, among the 74 percent of hourly workers who reported fluctuations in the last month, hours varied by an average of 50 percent of their usual work hours.

Table 5. Hour fluctuations (hourly only)

	Work hour instability*			Weekly hours worked in prior month (means)			
	Any fluctuation	Instability ratio (overall)	Instability ratio (if hrs vary)	Fewest	Usual	Greatest	Hour range
All employees	74%	0.37	0.49	31	37	41	10
Full-time (35+)	70%	0.22	0.32	37	43	47	10
Part-time	83%	0.72	0.87	17	22	28	11
Men	78%	0.36	0.46	33	40	46	12
Women	70%	0.37	0.53	29	33	37	8
White	74%	0.38	0.51	31	36	41	10
Black	73%	0.33	0.45	31	38	42	11
Hispanic	73%	0.35	0.48	33	39	43	10

Table 6. Hour fluctuations (non-hourly only)

	Work hour instability*			Weekly hours worked in prior month (means)			
	Any fluctuation	Instability ratio (overall)	Instability ratio (if hrs vary)	Fewest	Usual	Greatest	Hour range
All employees	74%	0.32	0.43	37	42	48	12
Full-time (35+)	73%	0.24	0.33	40	46	52	12
Part-time	79%	0.75	0.95	15	20	25	11
Men	76%	0.35	0.45	38	45	52	14
Women	71%	0.29	0.40	35	40	45	10
White	76%	0.32	0.42	37	43	49	12
Black	68%	0.34	0.51	34	40	45	11
Hispanic	60%	0.28	0.46	36	41	46	10

**Any fluctuation = share of employees for whom greatest hours > fewest hours. Instability ratio = (greatest – fewest) ÷ usual, or 0 if greatest = fewest. "Overall" refers to the estimated mean among all employees in each group; "if hrs vary" refers to the mean conditional on any fluctuation.*

The prevalence and magnitude of variation in work hours among part-time workers is especially noteworthy. Fully 83 percent of hourly part-time workers and 79 percent of non-hourly part-time workers reported at least some fluctuation in weekly work hours during the prior month (see Tables 5 and 6). The instability ratio among part-time workers whose hours vary is 0.87 for hourly workers and 0.95 for non-hourly workers. Although the range of variation in work hours among part-time workers is only slightly greater on average than among full-time workers (11 hours as compared with 10 hours for hourly jobs), it signifies a much greater magnitude of work-hour instability among workers in part-time than in full-time jobs (0.87 as compared with 0.32 for hourly jobs). Moreover, to the extent that part-time workers rely on the income of their main job to provide financial security, the low average of part-time workers' minimum hours (17 among hourly workers) suggests that fluctuations in work hours may bring financial insecurity.

Fluctuating work hours by occupation. Table 7 reports these same measures of fluctuating work hours for both hourly and non-hourly employees in different occupational groups, revealing a complex distribution of work-hour fluctuations that is not limited to high- or low-status jobs. Hour fluctuations are especially widespread among elite professionals (85 percent) and arts and media occupations (81 percent), whereas they are less common among office clerks (58 percent) and social functionaries (68 percent). The magnitude of fluctuations (instability ratio) among employees whose hours vary, however, is greatest for arts and media workers (0.65), service workers (0.53), and office clerks (0.52). By contrast, service supervisors experience relatively low levels of instability on average (0.24 overall, 0.31 when hours vary). In terms of the average range of weekly hours, employees in the elite professions and skilled trades show the widest fluctuations (17 and 16 hours, respectively), whereas office clerks show the narrowest (7 hours). These patterns do not fit neatly into a contrast between economic sectors or labor market segments, but they do suggest that occupations differ both in the average level of hour instability and the degree of similarity of scheduling practices across employers.

Table 7. Hour fluctuations by occupational groups (hourly and non-hourly combined)

	Work hour instability*			Weekly hours worked in prior month (means)			
	Any fluctuation	Instability ratio (overall)	Instability ratio (if hrs vary)	Fewest	Usual	Greatest	Hour range
Elite professionals	85%	0.39	0.45	37	45	53	17
Business staff	74%	0.28	0.37	38	42	48	10
Technical and research staff	76%	0.27	0.36	38	42	48	10
Arts and media occupations	81%	0.52	0.65	26	31	36	11
Office clerks	58%	0.30	0.52	34	38	41	7
Social functionaries	68%	0.30	0.44	34	39	43	9
Service supervisors	79%	0.24	0.31	37	42	47	10
Service workers	77%	0.41	0.53	28	34	39	11
Production supervisors	65%	0.30	0.47	38	47	51	13
Skilled trades	78%	0.39	0.50	36	45	52	16
Production workers	79%	0.35	0.44	34	41	46	13

**Any fluctuation = share of employees for whom greatest hours > fewest hours. Instability ratio = (greatest – fewest) ÷ usual, or 0 if greatest = fewest. "Overall" refers to the estimated mean among all employees in each group; "if hrs vary" refers to the mean conditional on any fluctuation.*

In sum, the data suggest that hours fluctuate substantially for both hourly and non-hourly workers. Although the 40-hour workweek remains standard for a sizable proportion of early-career workers, the majority of young adults in the labor market work above or below this standard, incurring fluctuations in their work hours that can place them at risk of under-employment or over-work. The pattern of fluctuations across groups is complex and does not reflect a clear high-status/low-status divide. Rather, employees in different occupational groups seem to experience distinct patterns of variation that may be related to the context as well as the content of their work.

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Schedule control

Limited advance schedule notice and hour fluctuations may be especially problematic for employees with limited say over the timing of their work schedules. When workers control their work schedules, variations in the number of hours worked may reflect employee-driven flexibility, a job quality highly valued by today's workers (MacDermid & Tang, 2009; Williams & Huang, 2011). Conversely, without employee control, a lack of variation in work hours—for instance, among employees who usually work 40 hours a week—may reflect rigid job requirements that do not yield when personal matters require attention (McCrate, 2012).

The NLSY97 asks respondents about a key component of schedule control by presenting a range of more employer-driven or more employee-driven descriptions of how starting and finishing times are decided. Table 8 reports the estimated percentage of early-career workers who chose each of the following response options: *Starting and finishing times are decided by my employer and I cannot change them on my own; Starting and finishing times are decided by my employer but with my input; I can decide the time I start and finish work within certain limits; or I am entirely free to decide when I start and finish work.*

Table 8. Schedule control* (percent of hourly, non-hourly, and combined total)

	Employer decides			Employer decides with some input			Employee decides within limits			Employee decides freely		
	Hrly	Non	Tot	Hrly	Non	Tot	Hrly	Non	Tot	Hrly	Non	Tot
All employees	50%	35%	44%	32%	25%	29%	13%	29%	19%	3%	7%	5%
Full-time (35+)	55	36	47	29	24	27	13	29	20	1	6	3
Part-time	39	25	36	37	31	36	13	26	17	7	13	8
Men	54	33	46	29	24	27	12	29	19	2	9	5
Women	46	36	42	34	26	31	13	29	19	4	5	5
White	47	34	42	32	25	29	15	29	21	3	8	5
Black	55	42	51	30	26	29	9	21	13	3	6	4
Hispanic	58	42	53	29	26	28	8	24	13	2	6	3

**The response category "When I start and finish work depends on things outside of my control and outside of my employer's control" is not included in the table. No more than 5 percent of workers in these groups chose this response.*

About 44 percent of workers overall and half of hourly workers say that they do not have any input into when they start and finish work. This employer-driven condition is the most common response for all groups of hourly workers shown in Table 8, ranging from 39 percent of part-time employees to 58 percent of Hispanics. Non-hourly employees are significantly more likely than hourly employees to decide their starting and finishing times within certain limits, though most still report that schedule decisions are employer-driven, with or without their input. Within both hourly and non-hourly groups, full-time workers and workers of color are significantly more likely (as compared with part-time and White workers, respectively) to say their employer decides the timing of their work. Only in part-time non-hourly jobs do more than 10 percent of workers say that they are entirely free to decide starting and finishing times. However, even these workers are more likely to report employer-driven rather than employee-driven schedules (56 percent versus 39 percent). Thus, employer control is clearly the norm, at least when it comes to starting and quitting times.[13]

Table 9. Schedule control* by occupation (percent of hourly and non-hourly combined)

	Employer decides	Employer decides with some input	Employee decides within limits	Employee decides freely
Elite professionals	18%	21%	38%	16%
Business staff	23	27	38	9
Technical and research staff	25	25	42	7
Arts and media occupations	28	22	33	9
Office clerks	42	30	23	4
Social functionaries	59	24	11	3
Service supervisors	27	40	24	6
Service workers	44	36	12	4
Production supervisors	37	33	25	0
Skilled trades	55	27	11	1
Production workers	65	20	9	2

**The response category "When I start and finish work depends on things outside of my control and outside of my employer's control" is not included in the table.*

Although employer-driven scheduling is the norm overall, control varies with occupation in ways that roughly track differences in status and education. Employee-driven scheduling is most prevalent among employees in occupations characterized by high levels of education and prestige, for example, professionals and white-collar workers. As shown in Table 9, elite professionals, business staff, technical employees, and creative workers in the arts and media are among the employees most likely to enjoy control over their starting and finishing times. On the other hand, workers in occupations characterized by more modest levels of education and less prestige, such as in production, the trades, and service industries, are most likely to have little or no control over their work schedule. Within the broad sectors of production and consumer services, supervisors experience significantly greater schedule control than subordinates, and those in high-skill positions have more control than those in low-skill positions. However, there are exceptions to this pattern. The group we term social functionaries, which includes skilled occupations such as secondary school teachers, social workers, and police, reports low levels of schedule control. These patterns suggest that scheduling practices are shaped not only by differences in educational requirements and status, but also by the institutional environment in which jobs are situated.

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[13] *Reporting that the employer sets starting and ending times does not preclude employees from exercising other forms of schedule control. For example, research suggests that being able to take time off during the day to attend to personal responsibilities is a form of flexibility especially valued by hourly workers (Golden, Henly, & Lambert 2013).*

Hour Fluctuations: Flexibility or Instability?

As discussed above, schedule control can make the difference between employees experiencing hour fluctuations as welcome flexibility or unwanted instability. Table 10 (hourly) and Table 11 (non-hourly) show how the extent of schedule control relates to the magnitude of work-hour fluctuations. As before, the magnitude of fluctuations is measured by an instability ratio that norms fluctuations in weekly work hours in relation to the usual number of hours worked. Workers whose hours did not fluctuate in the past month, that is, who gave the same response to the questions on fewest and greatest weekly hours, comprise the zero instability group.

Among hourly workers, there is little relationship between the level of hour instability and schedule control. At best, hourly workers with fluctuating hours are slightly more likely than those with stable schedules to report having some input into the timing of their hours. But regardless of how much hours fluctuate, about half of hourly workers say that their employer determines their work schedule. Thus, for hourly workers, work-hour fluctuations may be better interpreted as instability rather than flexibility.

Table 10. Schedule control by work-hour instability (hourly workers)

Instability ratio*	N	Employer decides	Employer decides with some input	Employee decides (within limits or freely)
0 (stable)	617	57%	28%	15%
>0, < 0.25	650	51%	31%	18%
≥0.25, <0.5	534	50%	36%	13%
≥0.5	593	47%	33%	19%

**Instability ratio = (greatest – fewest) ÷ usual hours or 0 if greatest = fewest.*

Among non-hourly workers, there is a stronger association between the level of instability and schedule control, suggesting that hour fluctuations may actually reflect greater flexibility. The more hours fluctuate, the less likely non-hourly workers are to report that their employer completely controls their schedule and the more likely they are to say that they control the timing of their work, either freely or within limits. We estimate that, among non-hourly workers with the greatest work-hour fluctuations (instability ratio ≥ 0.5), about 1 in 2 (51 percent) have some control over their starting and finishing times, while only 1 in 4 (25 percent) have no input over this aspect of their schedule.

Table 11. Schedule control by work-hour instability (non-hourly workers)

Instability ratio*	N	Employer decides	Employer decides with some input	Employee decides (within limits or freely)
0 (stable)	366	51%	28%	21%
>0, < 0.25	376	36%	26%	37%
≥0.25, <0.5	307	30%	24%	45%
≥0.5	295	25%	22%	51%

**Instability ratio = (greatest – fewest) ÷ usual hours or 0 if greatest = fewest.*

Although fluctuating hours seem more likely to reflect employee-driven flexibility among workers in non-hourly jobs than among those in hourly jobs, there is clearly overlap between the scheduling experiences of hourly and non-hourly workers. About half of hourly workers have at least some input into their schedules, even when their hours fluctuate greatly, and a substantial proportion of non-hourly workers experience instability in work hours and lack of control.

Additional analyses (not shown) suggest that the chance of having short notice increases with increasing work-hour instability, for both hourly and non-hourly workers. Overall, workers with the largest fluctuations in work hours are more than twice as likely as workers with stable schedules to say they know their work schedule one week or less in advance. This exploratory study of precarious work schedules suggests that the interpretation of any one dimension of scheduling is greatly aided by considering its relation to other dimensions.

Precarious scheduling among selected groups in the labor market

In this final section, we provide an overview of the prevalence of the different dimensions of precarious work schedules among groups that are disadvantaged in the labor market or who may be especially vulnerable to the effects of precarious scheduling practices. We also look at occupations that prior research suggests are prime sites for fluctuating and unpredictable work hours (Appelbaum et al., 2003; Haley-Lock, 2011; Jayaraman, 2013; Kalleberg, 2011; Lambert, 2008; Luce & Fujita, 2012). We focus here only on workers paid by the hour.

Table 12 presents estimates of work-hour fluctuations, advance notice, and schedule control among parents of young children, workers of color, workers in hourly low-wage jobs, and women in part-time jobs (regardless of wage rate). What is perhaps most notable about the data presented in this table is that the risk of two or more dimensions of precarious work schedules is quite high among all of these groups.

Among working parents with a child less than 13 years old (44 percent of the total sample), 69 percent of mothers and 79 percent of fathers report that their hours fluctuated in the prior month by an average of approximately 40 percent when compared to their usual hours. For many mothers and fathers, fluctuations in work hours are driven by the requirements of their employer rather than personal preferences. Half of fathers and 46 percent of mothers report that their employer decides their schedule without their input. In combination with the finding that 46 percent of fathers and 32 percent of mothers say they know their work schedule at most one week in advance, these data show a pattern of scheduling practices that are likely to challenge the ability of even the most motivated early-career parent to fulfill responsibilities at work and at home.[14]

69 percent of mothers and 79 percent of fathers report that their hours fluctuated in the prior month by an average of approximately 40 percent when compared to their usual hours.

Short notice and a lack of schedule control are significantly more common among workers of color than among White workers, although they have comparable levels of work-hour

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[14] *These data also show that employed mothers are less likely than fathers to report each of these precarious schedule practices, suggesting that a gendered division of work and family responsibilities may affect scheduling patterns. Of course, the relatively less precarious work schedules of working mothers when compared to fathers should be viewed in the context of the high overall rates of schedule precariousness among both.*

instability. Among workers in low-wage jobs, those in part-time jobs are at particularly high risk of fluctuating work hours (85 percent) which on average amount to 78 percent of their usual hours, whereas full-time workers, even when paid a low wage, report much lower instability in weekly work hours (about 30 percent of their usual hours). Low-paid part-time workers are at higher risk of short notice than low-paid full-time workers, whereas low-paid full-time workers are more likely than low-paid part-time workers to report that their employer controls the timing of their work. Women in part-time hourly jobs commonly experience enormous swings in weekly work hours and a large share report short notice (41 percent) and no schedule input (38 percent).

Overall, these patterns suggest that precarious scheduling can take different forms, as some disadvantaged groups are able to avoid one or more dimension of precariousness while remaining at higher risk along other dimensions.

Table 12. Selected groups of hourly workers

	Any fluctuation	Instability ratio* (if hrs vary)	1 week or less notice	Employer decides timing
Mothers (resident child < 13 years old)	69%	0.45	32%	46%
Fathers (resident child < 13 years old)	79%	0.43	46%	50%
Black	73%	0.45	49%	55%
Hispanic	73%	0.48	46%	58%
White	74%	0.51	39%	47%
Workers in low-wage jobs+				
Full-time	70%	0.30	43%	57%
Part-time	85%	0.78	49%	43%
Women in part-time jobs	81%	0.88	41%	38%

** Instability ratio = (greatest – fewest) ÷ usual hours, averaged across those reporting fluctuating hours.*
+ Wage rate less than $15 per hour

Table 13 presents comparable estimates of precarious schedules within occupations at high risk of fluctuating and unpredictable work hours. These data suggest that concerns for workers in these occupations are warranted. Some 90 percent of food service workers and 87 percent of retail workers report that their hours varied in the past month, with the range of variation amounting to a half or more of their usual work hours on average (48 percent among retail and 68 percent among food service workers). Such large swings in hours and earnings may be compounded by high rates of short notice, as 50 percent of retail workers and 64 percent of food service workers know their schedule a week or less in advance. Janitors and housekeepers experience relatively less instability and unpredictability, but 50 percent report that their employer decides the timing of their work without their input. Among home care workers, by contrast, lack of control is less common, whereas instability and unpredictability are relatively greater.

Table 13. At-risk occupations (hourly and non-exempt)+

	Any fluctuation	Instability ratio* (if hrs vary)	1 week or less notice	Employer decides timing
Janitors and housekeepers	66%	0.43	40%	50%
Food service workers	90%	0.68	64%	39%
Retail workers	87%	0.48	50%	44%
Home care workers	71%	0.62	55%	37%

** Instability ratio = (greatest – fewest) ÷ usual hours, averaged across those reporting fluctuating hours.*
+ Includes hourly workers and non-hourly workers whose low earnings (< $455 week) render them non-exempt from FLSA provisions.

Conclusion

For the first time, national data are available on the prevalence and distribution of distinct dimensions of work schedules among a representative sample of early-career adults (26 to 32 years old) in the United States. The picture painted by these data suggests that workers in occupations across the labor market are at considerable risk of unpredictable, unstable work hours over which they may have little control. At the lower end of the labor market, for example, we estimate that 90 percent of food service workers experienced work-hour fluctuations in the prior month, varying by an average of 68 percent of their usual hours. Half of retail workers know their work schedule one week or less in advance, and half of janitors and housekeepers report that their employer completely controls the timing of their work. But these new data also demonstrate that problematic scheduling practices are not limited to the lower levels of the labor market. Approximately a third of elite professionals, business staff, and technical employees say that their employer solely decides the timing of their work, and over 25 percent of workers in these occupations report knowing their work schedule one week or less in advance. Over 75 percent of early-career workers in these upper-tier occupations report work-hour fluctuations of at least 30 percent during the month, primarily reflecting surges in work hours that place them at risk of over-work.

Problematic scheduling practices are not limited to the lower levels of the labor market.

Perhaps our most striking finding is that short notice, work-hour fluctuations, and lack of schedule control are widespread. Fully 41 percent of early-career workers in hourly jobs overall—47 percent in part-time hourly jobs—report that they know when they will need to work one week or less in advance of the coming workweek. Half of them say that their employer decides the timing of their work hours and 3 in 4 report at least some fluctuations in the number of hours worked in the prior month. On average, hours fluctuate by more than a full, conventional 8-hour day of work (and for hourly workers, pay) in the course of a month.

Beyond these overall statistics, however, we emphasize that different dimensions of scheduling intersect to generate different sorts of experiences for workers. When workers control the timing of their work, fluctuating hours may reflect desired flexibility, but when employers decide schedules, such variations in work hours may introduce unwanted instability into the lives of workers and their families. Similarly, limited advance notice of one's work schedule is likely to be more problematic when work hours fluctuate widely and workers have little say in the timing of their work. Although we have explored relationships between work-hour fluctuations and schedule control and described how patterns of precarious work differ among vulnerable groups, more rigorous analytic approaches are needed to understand the configuration and outcomes of different sorts of schedules. It is difficult, for example, to tease apart differences between groups defined in terms of gender and race from differences between occupations that disproportionately recruit from a particular demographic. Inequality in earnings and other outcomes can often be traced to stratification and sorting of individuals into occupations (Reskin, 2003), and our initial analyses suggest that there is more variation on the dimensions of work schedules observed in this brief by occupation than by personal characteristics.

The first national snapshot of precarious scheduling practices provides a worrisome picture.

Given that this is the first time these measures of advance notice and hour fluctuations have been included in a national survey in the US, there is still much to learn about how these measures compare to other sorts of evidence about work schedules. We remind the reader that it is not possible at this point to gauge potential biases introduced into our estimates by the design and fielding of the NLSY97, especially with respect to non-hourly employees who were less likely to receive the new scheduling questions than employees paid by the hour. Even if our estimates for the population born between 1980 and 1984 were exact, these early-career adults comprise a minority of prime-age workers in the US wage and salaried workforce and do not include the self-employed. Workers' schedules may become more predictable and stable with age, especially if they accumulate seniority with an employer or work experience in an occupation. Nevertheless, members of this younger population are of special interest precisely because they are forging careers and forming families in the aftermath of the Great Recession. The immediate and longer-term well-being of families and communities depends on these young adults succeeding in the labor market which, in turn, depends on the quality of jobs and the practices of today's firms.

In conclusion, the first national snapshot of precarious scheduling practices provides a worrisome picture. Regardless of parenting status, race, gender, and occupation, large proportions of young adults in today's labor market report unpredictable, fluctuating work hours. Not knowing one's work schedule in advance or experiencing fluctuating work hours may not be particularly problematic among workers who schedule their hours themselves, but most early-career employees report having little if any input into the timing of their work. Part-time workers are at particular risk of unpredictable and unstable work schedules. Low usual hours combined with wide fluctuations from week to week and limited advance notice highlight the challenges many part-time workers face in predicting how much they will work and earn.

These data suggest that a substantial proportion of early-career workers in the labor market would stand to benefit from workweek standards that increase advance schedule notice, employee schedule control, and the stability of work hours. It is too risky to depend on the private sector alone to ensure that America's future includes an economy with good jobs that foster the continued and long-term prosperity of firms and families. Legislation that establishes a comprehensive set of standards on scheduling practices is needed to ensure that workers in all occupations and at all levels of the labor market stand a fair chance of thriving at both work and home.

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Appendix A: Occupational classification

Group name	Description	Examples
Elite professionals	elite professionals and corporate executives	corporate executives, lawyers, physicians, architects, postsecondary teachers
Business staff	business and managerial staff	human relations staff, accountants, actuaries, logisticians, education administrators
Technical and research staff	technical, engineering, and research staff	computer programmers, urban planners, economists, psychologists, archivists, pilots
Arts and media occupations	artistic and media-related occupations	actors, photographers, athletes, announcers, editors, public relations specialists
Office clerks	clerical employees and office workers	paralegals, tax preparers, secretaries, bill and account collectors, data-entry workers
Social functionaries	education, medical, and social service paraprofessionals and functionaries	secondary school teachers, clergy, social workers, librarians, nurses, police officers, tax collectors
Service supervisors	consumer and business service supervisors and first-line managers	all non-farm, non-production, private sector first-line supervisors / managers
Service workers	consumer and business service workers and front-line employees	cashiers, cooks, janitors, telemarketers, couriers, child care workers, hairdressers, security guards, taxi drivers
Production supervisors	manufacturing, construction, and transportation supervisors and first-line managers	all manufacturing, construction, and transportation first-line supervisors / managers
Skilled trades	non-farm production, repair, and transportation crafts, skilled trades, and licensed occupations	electricians, roofers, structural iron and steel workers, commercial drivers, sailors, construction painters, machinists, tool and die makers, cabinetmakers
Production workers	non-farm production, repair, and transportation laborers, operators, and helpers	machine setters, operators, and minders; packers, construction laborers, bakers, other metal and plastics workers, painting workers, misc. assemblers and fabricators
Agriculture	farming, forestry, fishing, and related occupations	farmers and ranchers, animal breeders, loggers, conservationists, miscellaneous agricultural workers

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REVIEW

Traveling Towards Disease: Transportation Barriers to Health Care Access

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Abstract Transportation barriers are often cited as barriers to healthcare access. Transportation barriers lead to rescheduled or missed appointments, delayed care, and missed or delayed medication use. These consequences may lead to poorer management of chronic illness and thus poorer health outcomes. However, the significance of these barriers is uncertain based on existing literature due to wide variability in both study populations and transportation barrier measures. The authors sought to synthesize the literature on the prevalence of transportation barriers to health care access. A systematic literature search of peer-reviewed studies on transportation barriers to healthcare access was performed. Inclusion criteria were as follows: (1) study addressed access barriers for ongoing primary care or chronic disease care; (2) study included assessment of transportation barriers; and (3) study was completed in the United States. In total, 61 studies were reviewed. Overall, the evidence supports that transportation barriers are an important barrier to healthcare access, particularly for those with lower incomes or the under/uninsured. Additional research needs to (1) clarify which aspects of transportation limit health care access (2) measure the impact of transportation barriers on clinically meaningful outcomes and (3) measure the impact of transportation barrier interventions and transportation policy changes.

Keywords Healthcare access · Transportation barriers · Medication access · Healthcare barriers

Introduction

Transportation is a basic but necessary step for ongoing health care and medication access, particularly for those with chronic diseases (Fig. 1). Chronic disease care requires clinician visits, medication access, and changes to treatment plans in order to provide evidence-based care. However, without transportation, delays in clinical interventions result. Such delays in care may lead to a lack of appropriate medical treatment, chronic disease exacerbations or unmet health care needs, which can accumulate and worsen health outcomes [1, 2].

Patients with transportation barriers carry a greater burden of disease which may, in part, reflect the relationship between poverty and transportation availability [3]. As a result, understanding the relationship between transportation barriers and health may be important to addressing health in the most vulnerable who live in poverty.

Transportation is often cited as a major barrier to health care access [4–35]. Studies have found transportation barriers impacting health care access in as little as 3 % or as much as

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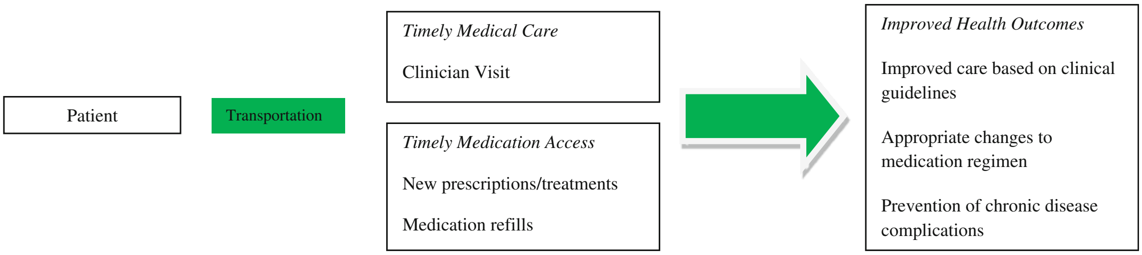

Fig. 1 Model of relationship between transportation, health care access and outcomes

67 % of the population sampled [25, 36]. The wide variability in study findings makes it difficult to determine the ultimate impact that transportation barriers have on health.

This review summarizes and critically evaluates the empirical evidence on transportation barriers to health care access for primary and chronic disease care. For each of the 61 studies reviewed, we evaluated the population characteristics, methods, measures of transportation barriers and results (Table 1). Results are organized into three sections: (1) measurement of transportation barriers, (2) transportation barriers and demographic differences, and (3) measurement of the impact of transportation barriers. Additionally, we define a research agenda based on gaps in the literature and discuss potential intervention opportunities and public policy considerations.

Methods

We searched for peer-reviewed studies that addressed transportation barriers in relation to ongoing health care access. Inclusion criteria were as follows: (1) study addressed access barriers for ongoing primary care or chronic disease care; (2) study included assessment of transportation barriers; and (3) study was completed in the United States. Articles dealing with access to prenatal care, emergency or acute care, or exclusive attention to general screening and prevention were excluded as they may represent a single visit or limited time period of care.

We used PubMed with the following keyword search terms (number of articles returned): *transportation barriers* (963), *transportation barriers clinic* (129), *transportation barriers pharmacy* (13), *transportation barriers hospital* (183), *transportation barriers doctor* (69), *transportation barriers health access* (276), and *transportation barriers chronic disease* (33). Medical Subject Heading (MESH) terms included *health services accessibility AND transportation* (575). Additional background information was found using the terms *transportation barriers health access* to search Web of Science and Psych Info, and *transportation barriers* to search The New York Academy of Medicine Library's Grey Literature Report.

Abstracts were reviewed for inclusion criteria, and if necessary, full text articles were also reviewed. A secondary review of bibliographies was also conducted. In the final review, 61 articles met the inclusion criteria. The search was concluded in December 2012.

Results

Measures of Transportation Barriers

Vehicle Access and Mode of Travel

Nine studies assessed the influence of vehicle access upon access to health care, and all found a positive relationship [24–26, 37–42]. Vehicle access refers to either owning a car or having access to a car through a family member or friend. Arcury et al. [37] studied the relationship of transportation to health care utilization in 1,059 rural Appalachians and found that people who knew someone who regularly provided rides to a member of their family had a greater utilization of health care (Odds Ratio, OR 1.58). Those with a driver's license, independent of other factors, also had greater health care utilization (OR 2.29).

Guidry et al. [26] surveyed 593 cancer patients throughout Texas, and found 38 % of whites, 55 % of African Americans, and 60 % of Hispanics identified poor access to a vehicle as a barrier that could result in missing a cancer treatment.

A study by Salloum et al. [38] looked retrospectively (2000–2007) at 406 cancer patients to see if patients were more or less likely to receive first line chemotherapy based on their demographics. Patients who were significantly less likely to receive first line chemotherapy lived in neighborhoods that had a higher percentage of households without any vehicle. Distance to the nearest chemotherapy facility was not a significant factor.

Rask et al. [40] studied obstacles to care for 3,897 urban, low socioeconomic status (SES) adults in Atlanta and found that walking or using public transportation to receive medical care was an independent predictor of not having a regular source of care (OR 1.44). Patients who did not use private transportation were also more likely to delay care (OR 1.45).

Table 1 Studies on transportation barriers to health care access

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Ahmed et al. [1]	N = 413 adults Urban (Dayton, Ohio), low SES 71 % female, 48 % Black, 42 % Appalachian	Door to door survey on barriers to health care access	"Difficulty finding transportation" (1)	"Hard" or "very hard" time finding transportation (31 %)
Arcury et al. [37]	N = 1,059 adults Rural (North Carolina), mixed SES, 662 female, 948 Whites, 112 Blacks	Retrospective, comparing transportation barriers and health care utilization	"Distance to care for… regular visit… for less serious emergency… for serious emergency" (3) Has a driver's license, any household member has a driver's license, number of vehicles owned in household, days per week spent driving, relative or friend who regularly provides transportation for a family member, knowledge of organizations that provided transportation to health care and use of such transportation (7)	Health care utilization associated with having a driver's license (OR 2.29 more visits) and having a friend or relative who provides transportation (OR 1.58 more visits)
Blazer et al. [14]	N = 4,162 adults, age 65 + Rural/Urban North Carolina), mixed SES, 62 % female, 68 % Non-Black (majority White)	Retrospective cross-sectional survey (1986/87) analyzed for urban/rural variation of health service use, satisfaction, barriers to care	Do you put off or neglect going to the doctor because of "distance or transportation"? (1)	No difference between urban and rural residents in health service use; 7.7 % delayed care due to distance or transportation
Borders et al. [54]	N = 2,097 adults, age 65 + Rural (West Texas), mixed SES 71 % female, 1949 Non-Hispanic, 148 Hispanic	Telephone survey on barriers to health care access	"Always/usually get transportation to doctor's office" (1)	Non- Hispanics (96 %) vs. Hispanics (90 %) could usually get transportation to clinic
Branch et al. [36]	N = 776 adults, age 65 + Massachusetts, 95 % Medicare, 17 % Medicaid, 61 % privately insured, 64 % female Race not reported	Retrospective survey interviews on barriers to health care access	"You did not have a way to travel to the doctor" (1)	Not having a way to get to the doctor (3 %); travel difficulties associated with lower income, being female, living alone, having less education
Call et al. [56][b]	N = 1,853 Minnesota Health Care Plan adult and parent enrollees Minnesota, 65 % female adult enrollees, 47 % female parent enrollees, 1,314 Whites, 539 American Indians	Mailed survey on barriers to health care access	"Difficulties with transportation" (1)	American Indians (39 %) vs. Whites (18 %) have difficulties with transportation
Canupp et al. [49]	N = 163 adults, mean age 26 with spinal cord injuries Birmingham, Alabama, 25 % had income greater than 25,000 dollars, 14 % female, 63 % white	Face to face survey on barriers to follow-up appointments	Obstacles for follow-up included distance to travel and availability of transportation (2)	Non-compliance with appointments associated with distance to travel ($P = 0.004$) and availability of transportation ($P = 0.033$)

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Crain et al. [15][b]	N = 1,376 caretakers of children with asthma 8 metro inner-cities (locations not specified), low SES/74 % Medicaid,36 % female, 73 % Black	Face to face survey on barriers to health care access	“Had no way to get there” (1)	No way to get to clinic for follow-up care (16 %)
Cunningham et al. [17]	N = 2,864 adults with HIV National, mixed SES, 847 females, 1,399 White/959 Black/415 Hispanic	Face to face and telephone surveys on barriers to health care access and reasons for delaying care	“In the last 6 months, have you ever had to go without health care because you didn’t have a way to get there?” (1)	Postponed care because no transportation (15.4 %) (weighted for number of HIV persons in US who were under care from January to March 1996)
Diamant et al. [18]	N = 1,819 adults Los Angeles, California, low SES, 69 % female, 56 % Hispanic/Latino, 23 % Black, 17 % White	Face to face surveys on barriers to health care access	“In the past 12 months, have you ever put off going to the doctor for medical care because you didn’t have a way to get there?” (1)	Delayed care in prior 12 months (33 %) Did not have transportation to get to the doctor (12 %)
Fitzpatrick et al. [60]	N = 4,889 Medicare eligible adults, age 65 + Forsyth County, NC; Sacramento County, CA; Washington County, MD; Allegheny County, PA; mixed SES/70 % with supplemental private insurance, 59 % female, 83 % White	Retrospective surveys on patterns of health care use and barriers to care	“How much [moderate/very much/a whole lot] did each of the following affect your ability to see the doctor…transportation difficulty” (1)	Cited a barrier to seeing the doctor (4 %); Of those citing a barrier, 21 % had transportation difficulties Barriers to care associated with older age, female gender, minorities, lower income, lack of complementary insurance
Flores et al. [24]	N = 203 children’s caretakers Boston, Massachusetts, low SES, Latino	Face to face survey on barriers to health care access	Transportation problems including lack of a car, excessive distance, expense or inconvenience of public transportation (not specified)	Transportation barrier as a reason they had not brought child in for a medical visit (21 %); most cited reason Lack of a car as most frequent transportation difficulty (62 %) Clinics located too far from home (11 %)
Giambruno et al. [25][b]	N = 157 head start health coordinators New York City, New Hampshire, Puerto Rico, US Virgin Islands	Mailed survey on barriers to medical diagnosis and treatment for head start children	Transportation barriers (4) included access to private transportation, access to public transportation, cost of transportation, distance to provider	Barriers to health care access included: Private transportation not available (67 %) Distance to provider (63 %) Cost of transit (63 %) No public transit available (48 %)

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Guidry et al. [26][b]	N = 593 adults with cancer Texas, mixed SES, 56 % female, 42 % White, 40 % Black, 15 % Hispanic	Mailed survey on transportation barriers to cancer treatment	Transportation barriers (4) included distance to treatment center, access to a vehicle, finding someone to drive them to treatment, mode of travel	Barriers to getting cancer treatment were greatest for Hispanics, then Blacks, then Whites: Distance Hispanic (66 %), Black (51 %), White (37 %) Access to a vehicle Hispanic (50 %), Black (46 %), White (19 %) Finding someone to drive them Hispanic (66 %), Black (55 %), White (37 %)
Heckman et al. [44]	N = 226 adults with HIV/AIDS Urban/rural (Wisconsin), 54 % made < $10,000, 19 % female, 69 % White/23 % Black	Mailed survey on barriers to care	Transportation barriers (2) included lack of transportation and long distance to provider with likert scale ranging from 1(no problem at all) to 4 (major problem)	More rural than urban patients cited problems with distance (2.86 vs 1.61 on likert scale; $P = 0.001$), transportation (2.03 vs 1.62 on likert scale; $P = 0.05$)
Hoffman et al. [19]	N = 34 adults with asthma Urban (Pittsburgh), mixed SES, 94 % female, race not reported	Mailed surveys on 10 barriers to compliance with asthma care	"Lack of transportation" (1)	Lack of transportation associated with patients who go to the emergency room for their usual place of care ($P = 0.02$)
Johnson et al. [20]	N = 34,504 honorably discharged veterans, age 18–64 National, mixed SES, American Indian/Alaskan native, White	Analysis of National Health Interview Survey (NHIS) (1997–2006) for health care coverage and reasons for delayed care	Barriers to receiving timely care… transportation problems (not specified)	American Indian/Alaskan Native more likely to delay care than Whites due to transportation problems (OR 2.9)
Kripalani et al. [64]	N = 84 adults Urban (Atlanta), low SES, 41 % female, 88 % Black	Telephone survey on barriers to discharge medication adherence	Level of difficulty visiting pharmacy (not specified)	Patient with difficulty visiting the pharmacy less likely to fill prescription on day of discharge (20 % vs 55 %; $P = 0.002$) Transportation assistance would improve medication use 65 %)
Kruzich et al. [48][b]	N = 102 caregivers of children receiving mental health treatment in residential facility, group home, or psychiatric unit 31 US States, mixed SES, 78 % white	Mailed surveys on barriers to participation in children's mental health treatment	Transportation barriers (3) included distance, cost of transit, lack of transit	Barriers to participation in care identified: Distance from provider (44 %) Cost of transit (28 %) Lack of access to transit (10 %) Distance was the most important barrier (22 %)

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Lamont et al. [52][a]	N = 110 adults Chicago, mixed SES, 26 % female, 67 % white	Retrospective evaluation of association between cancer survival and distance from patient's residence to treating institution	Distance (N/A)	Patients living more than 15 miles from institution had 1/3 hazard ratio for death, and with every 10 miles traveled, hazard of death decreased by 3.2 % Compared with Whites, Blacks had 1/3 the hazard rate of death On average, those traveling more than 15 miles were more often white, male, college educated, had higher family incomes
Levine et al. [67]	N = 5,840 adult stroke survivors age 45 + National, SES not reported, included Blacks and Whites	Retrospective survey using NHIS data (1997–2004) to identify inability to afford medications	Lack of transportation delaying care (1)	Patients who could not afford medications more frequently reported transportation barriers (15 vs. 3 %; $P < 0.001$)
Littenberg et al. [53]	N = 781 adult diabetics, 51 % age 65 + Vermont, 58 % with private insurance/58 % Medicare/20 % Medicaid/5 % military/2 % uninsured, 54 % female, 97 % white	Mailed surveys, face to face interviews, and distance measurements to evaluate the role of travel burden as a barrier to insulin use	Distance (N/A)	Longer driving distance associated with less use of insulin (OR for using insulin for each km of driving distance 0.97)
Malmgren et al. [61]	N = 125 adults, age 62 + Seattle, low SES, 71 % women, 77 % White	Face to face survey on health status and access of health care and unmet needs	"Which of the following have ever made it hard to get care? …no transportation…distance too far" (2)	Problems obtaining care (46 %) No transportation (10 %) Insufficient income to meet personal needs was associated with more financial and structural barriers
Martinez et al. [58][b]	N = 107 HIV youth, age 15-24 Chicago, Newark, New York, Miami, 51 % with stable housing, 64 % female, 73 % Black	Face to face surveys on process of transitioning HIV youth from diagnosis to treatment including needs assessment and barriers to care	Barriers to accessing health care… transportation to health care settings (1)	Transportation to health care was a barrier (40 %)
Musey et al. [27][b]	N = 56 adult diabetics Urban (Atlanta), low SES, 21 women, 100 % Black	Face to face surveys on precipitating causes of diabetic ketoacidosis (DKA)	Lack of money for transportation to pharmacy (1)	DKA caused by cessation of insulin (67 %); 50 % of these patients cited lack of money for insulin or for transportation to the pharmacy
Nemet et al. [51][b]	N = 390 adults, age 65 + Rural (Orleans County, Vermont), income less than 50,000 dollars, gender and race not specified	Mailed surveys on measures of health care association	Distance (N/A)	Distance to doctor was not associated with utilization of health care
Okoro et al. [50][b]	N = 46,659 adults, age 65 + National, mixed income/all insured, 65 % female, 87 % White	Retrospective analysis of behavioral risk factor surveillance system (2002) to explore barriers to care for adults older than age 65	Transportation barriers included no transportation or distance (1)	9 % of those who could not obtain needed medical care cited distance or no transportation as barrier to care

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Pesata et al. [39]	N = 101 children's families Midwestern metropolitan clinic (location not specified), low SES, 54 % Black, 41 % White	Telephone survey on dynamics behind missed appointments	Transportation problems (not specified)	Transportation as the primary reason for missed appointments (no ride or car) (51 %)
Pheley et al. [68][b]	N = 22,703 adult appointments Inner city Minneapolis, low SES, race not reported	Retrospective analysis of change in rate of failed appointments during public bus strike	Rate of failed appointments per 100 scheduled appointments (N/A)	Strike period associated with increased missed visits if visit was with a nurse (RR 1.17, *P* value = 0.01); no impact on doctor's visits
Probst et al. [45]	N = 2,432 households National Household Travel Survey (NHTS), mixed SES, 62 % female, 71 % White	Retrospective analysis of 2001 National Household Travel Survey to determine travel burden to health care by geography and race	Travel burdens measured by distance greater than 30 miles or time greater than 30 min; mode of travel, day and time of trip, driver/passenger status, traffic, region (not specified)	Rural residence associated with higher travel burden by distance (OR 2.67) and time (OR 1.80) Blacks had higher travel burdens by time (OR 3.04) compared to urban residence and Whites
Rask et al. [40]	N = 3,897 adults Urban (Atlanta), low SES, 53 % female, 89 % Black	Face to face survey to determine correlation of obstacles to medical care, lack of care, or delay in care	Lack of transportation (1)	Walking or using public transportation to reach hospital more likely to not have a regular source of care (OR 1.47) If no private transportation, more likely to delay care (OR 1.60) Lack of private transportation was an independent predictor of not having a regular source of care and delaying care
Reif et al. [21][b]	N = 94 HIV case managers Urban/Rural (North Carolina), 86 % female, 59 % White	Mailed survey on barriers to health care for HIV patients	Transportation barriers (2) included lack of adequate transportation and long travel distances to health care services	Lack of transportation was a major barrier for accessing care for clients (41 %) Long travel distances to health care services was a major barrier for accessing care for clients (33 %)
Rittner et al. [62]	N = 1,083, adults, mean age 78 Metro South Florida, low SES, 838 women, 818 White, 252 Hispanic	Group administered survey on health care access barriers in an elderly population who use daytime meal programs and mostly use public transportation	Transportation barriers (not specified)	Lack of transportation was reason for not receiving care in past 6 months (6.1 %)
Rust et al. [22]	N = 30,677 adults National, mixed SES, race varied	Retrospective analysis of 2005 National health interview survey (NHIS) to explore relationship between ED visits and barriers to health care	"No transportation" (1)	Likelihood of ED visit in 1 year if at least 1 barrier vs no barrier: 1 in 3 adults (33 %) vs 1 in 5 (20 %) Of 5 access barriers, "no transportation" was greatest OR (OR 1.88)

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Salloum et al. [38][a]	N = 406 adults with cancer Southeast Michigan, median income 48,000 dollars/patients had to be enrolled in a health care plan in the 1 year preceding cancer diagnosis/12 % did not own cars in household, 41 % female, 69 % White, 29 % Black	Retrospective analysis of factors associated with adherence to chemotherapy guidelines in patients with non-small cell lung cancer (2000–2007)	Transportation barriers (2) included vehicle access and distance to treatment facility	Predictors of chemotherapy underuse included lower vehicle access in the neighborhood(OR 6.96) Distance traveled was not associated with adherence to chemotherapy treatment guidelines No racial differences in the receipt of chemotherapy (neither under or overuse)
Sarnquist et al. [47]	N = 64 adults with HIV Rural (California), majority made less than 20,000 dollars/year, 100 % female	Retrospective face to face surveys on barriers to health care	Transportation barriers (4) included mode to travel, travel time, difficulty traveling, lack of transportation	Most common reason for missed appointments was transportation barriers (37.5 %); as common as "not feeling physically well" Difficulty traveling to appointment 45.3 % Lack of transportation 31.2 % Transportation challenges associated with lower income
Silver et al. [42][b]	N = 698 adults New York City suburb, low SES, 83 % female, 73 % Hispanic	Face to face surveys on transportation barriers to clinic	Transportation barriers (6) included "how did you travel to the clinic today, (if by bus) approximately how much time did you spend on the bust to travel from your home to the clinic, have you ever missed a clinic appointment because of transportation problems, in a typical month how often do you have transportation problems, how often do you use the bus (often/sometimes/never for work/groceries/clinic or hospital/visit family or friends), (if applies) what is the main reason that you don't use the bus more often to travel to the clinic?"	Missed or rescheduled an appointment due to transportation problems (23.5 %) Chronic transportation problems (30 %) Difficulties affording transportation to clinic (nearly 25 %) Bus users twice as likely to report history of missed/rescheduled appointments (40 % vs. 18 % car users; $P < 0.001$)
Skinner et al. [46]	N = 38,866 households of children with special health care needs Urban/Rural (national), low SES, 83 % white, 9 % Black	Retrospective analysis from 2000 to 2002 National Survey of Children with Special Health Care Needs from the National Center for Health Statistics to examine barriers to health care needs for urban and rural special needs children	"transportation/not available in area" (1)	Transportation/service not available in area was a barrier for any kind of care (OR 1.58), for obtaining prescriptions (OR 3.58), for therapy (OR 2.50)
Smith et al. [59][b]	N = 147 caregivers of asthmatic children Urban (St.Louis, Missouri), low SES, race not reported	Scaled survey of pros and cons to primary care follow-up after ED visit for asthma	Finding transportation to get to appointment (1)	Finding transportation was a barrier for parents to obtain follow-up care

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Strauss et al. [54]	N = 973 adult diabetics Vermont/New Hampshire/northern New York, 98 % insured/21 % Medicaid, 55 % female, 97 % White	Analysis of Vermont Diabetes Information System to examine relationship between driving distance and glycemic control	Driving distance (N/A)	Longer driving distances from home to site of primary care was associated with poorer glycemic control with each 22 miles of driving distance associated with a 0.25 % increase in hemoglobin A1c
Tierney et al. [66][a,b]	N = 46,722 all ages (23,015 in 1993; 23,707 in 1994) Indiana, Medicaid recipients, 66 % Black	Cohort study comparing health care utilization of Medicaid patients before (1993) and after(1994) a change in transportation reimbursement policy	Health care utilization (N/A)	Visits to hospital based primary care clinics declined (16 %) Visits to neighborhood health clinics increased (7 %) Emergency and urgent care visits fell (8 %) Visits for medication fills fell (18 %) Hospitalizations increased slightly with no change in number of inpatient days
Wallace et al. [3]	N = 5,000 (2002 National transportation availability and use survey by bureau of transportation statistics- BTS)[c]	Retrospective analysis of BTS, NHIS, MEPS to estimate magnitude of transportation barriers to health care and populations affected nationally	Transportation barriers National health interview survey (1) MEPS (3) National transportation availability and use survey (not specified)	3.6 million Americans (estimated) miss at least one medical trip a year because of transportation and population is more likely to be older, poorer, female, minority, less educated 53.7 % of children who missed care due to transportation live in metro areas of 1 million or more while only 47.5 % of all U.S. children live in these areas
Washington et al. [63]	N = 3,611 adult veterans National, mixed SES, 100 % female, 23 % minorities	Telephone survey of barriers to health care access	Reason for delayed care or unmet need… transportation difficulties (1)	35.7 % of those over age 65 reported transportation difficulties
Weathers et al. [34]	N = 300 adult caretakers of a migrant child less than age 13 4 counties in Eastern North Carolina, 73 % of children lacked insurance, nearly 70 % children were foreign born (62 % from Mexico)	Face to face surveys of factors associated with unmet medical needs for children of migrant workers	Primary reason for the last episode of unmet medical need… lack of transportation (1)	53 % of children had unmet medical need Lack of transportation was primary reason for last episode of unmet medical need (80 %)

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Welty et al. [65]	N = 143 adult members of epilepsy.com with epilepsy Worldwide, (119 US/24 outside US), 75 % female, SES not reported, race not reported	Web survey on impact of limited transportation on medication adherence	Transportation as a barrier to medication access (8) including “approximately how far from your home is your pharmacy located (miles); if you do not drive, are there other forms of transportation available; what best describes your primary mode of transportation, do you ever have trouble picking up prescription medications on time because of transportation problems, how often do transportation problems keep you from getting your prescription medications on time, how often do you ever miss doses of medication because you cannot get to the pharmacy to pick up your medications, do you believe you have seizures because you are not able to pick up your medications on time, do you feel you would miss fewer doses of medication if transportation was not an issue?”	Patients who could not drive vs those who could: Trouble picking up medications on time due to transportation barriers (51 % vs 20 %) Would miss fewer doses if transit not an issue (45 vs. 22 %) Thought had seizures because did not get medications on time (28 vs. 18 %) Living closer and having mail service pharmacies did not improve receipt of on time refills
Wheeler et al. [33]	N = 303 recently hospitalized adults with diabetes Urban (Atlanta), low SES, 46 % female, 91 % Black	Face to face surveys on barriers to follow-up diabetes care after hospital discharge	“no transportation to get to doctor” (1)	Most common barrier to follow-up care was no transportation (60 %)

Table 1 continued

Author	Population	Methods	Measure of transportation barriers (# of items)	Results
Yang et al. [41]	N = 183 caregivers of children attending pediatric clinic Urban (Houston, Texas), low SES/80 % Medicaid, 44 % Hispanic, 28 % African Americain	Face to face and telephone surveys of transportation barriers to keeping appointments	Transportation barriers (16) included "forms of transportation most often used to clinic, form of transportation to last appointment kept, travel time in minutes to clinic for last appointment kept, usual travel time to clinic, number of missed appointments in last 12 months due to transportation problems" and yes/no items included "ever used Houston buses, access to a car, cost of transportation too high in Houston, difficulty getting to last appointment, ever missed appointment due to transportation problem, ever taken a child to a closer clinic, ever late to appointment due to transportation problem, ever miss appointment because auto being used by other person, ever not schedule appointment because of transportation problem, ever miss sick visit because of transportation problem, ever late to an appointment because of lack of parking, ever miss appointment because parking too expensive?"	Overall, 131 missed appointments out of 497 appointments (26.4 %) *Kept appointment:* Access to a car (82 %) Missed in past due to transportation problems (30 %) 31 % of the show group missed appointments in the past due to reasons other than transportation *Did not keep appointment:* Access to a car (58 %) Missed in past due to transportation problems (50 %) 56 % of the no show group missed appointments in the past due to reasons other than transportation
Zullig et al. [23]	N = 954 veterans with colorectal cancer Minnesota, mixed SES, 100 % male, 76 % White	Mailed survey on transportation barriers to cancer treatment and follow-up	"How often was it difficult to get transportation to or from your treatment or follow-up appointment?"	"Often" and "always" difficult (19 %)

[a] All studies are cross-sectional, unless otherwise noted under author. If question stem was available, it was provided in quotations. Focus group studies, review articles and policy papers were not included in this table

[b] Study did not correct for SES by income or insurance, although in some cases this correction may not be applicable

[c] Sample size not reported by article for 2002 National Health Interview Survey-NHIS and 2001 Medical Expenditure Panel Survey-MEPS

Flores et al. studied 203 children's caretakers and found that 21 % of inner-city children faced transportation barriers to timely health care. Of these, 62 % cited lack of a car as the specific barrier, which exceeded other reasons including excessive distance, expense, or inconvenience of public transportation [24].

Two studies reported that 25 % of patients missed an appointment due to transportation problems [41, 42]. Yang et al. [41] studied 183 urban caregivers from Houston and their children's missed appointments, finding that an inability to find a ride resulted in at least one missed appointment for 25 % of the sample. The study also found that 82 % of those who kept their appointments had access to a car, compared to just 58 % of those who did not keep their appointments. Similarly, in a study of 698 low-income adult patients, Silver et al. [42] found that 25 % of missed appointments/rescheduling needs were due to transportation problems and bus users were twice as likely to miss their appointments compared to car users.

One study investigated transit accessibility to health care by either public transit or by foot in various low income counties in the Bay Area [43]. Results revealed that transit accessibility to a hospital, defined as getting to a hospital or clinic in 30 min or less by public transit or ½ mile by foot, varied from 0 to 28 %. Additionally, 55 % of missed appointments or late arrivals were due to transportation problems.

Collectively, these studies suggest that lack or inaccessibility of transportation may be associated with less health care utilization, lack of regular medical care, and missed medical appointments, particularly for those from lower economic backgrounds.

Urban and Rural Geography

Urban and rural locations often differ in transit options, cost of transit, and availability of and distance to health care providers. Despite this, results were mixed in the four studies that compared the impact of transportation barriers on health care access for urban and rural residences [14, 44–46]. Blazer et al. [14] surveyed 4,162 urban and rural adults over 65 in North Carolina to investigate why patients delayed or neglected to see a doctor. The study showed no difference between urban and rural adults in either their use of health services or identification of transportation barriers. Similarly, a study by Skinner et al. [46] included 38,866 households, and found no difference in reports of delayed care between urban and rural parents after controlling for SES.

In contrast, three studies found that rural patients face greater transportation barriers to health care access than their urban counterparts [44–46]. Rural patients reported more problems with transportation and travel distance to health care providers and had a higher burden of travel for health care when measured by distance and time traveled [45]. In a study by Sarnquist et al. [47] that did not make urban comparisons, but included 64 rural, adult HIV patients, 31 % were lacking transportation and 37 % were missing appointments due to transportation problems.

Travel Burden by Time and Distance

Nine studies evaluated distance as a barrier to health care access with mixed results [25, 26, 48–54]. Six found that distance was a barrier to care [25, 26, 48–51]. Of those, five investigated a variation of the question, 'Is distance a barrier to health care access?', to measure the impact of distance [25, 26, 48–50]. The sixth study explored the association between distance to providers and patient reported health care utilization [51]. In contrast, two studies found that distance to a provider was not associated with differences in health care utilization [53, 54]. Surprisingly, one study by Lamont et al. [52] found that a longer distance to one's health care facility was associated with improved health care access. Two studies looked at the relationship of distance to either medication use or clinical outcomes, reporting that longer driving distances from one's physician are associated with less insulin use or poorer glycemic control independent of social, clinical or economic factors [53, 54].

Transportation Barriers and Demographic Differences

Transportation Barriers and Ethnic Differences

Of six studies comparing transportation barriers to health care access across ethnic groups, five found differences [3, 20, 26, 45, 55, 56]. To understand whether ethnic differences independently account for differences in transportation barriers, socioeconomic factors must be considered because they can influence transportation variables [57].

Three studies used national data sets to explore transportation barriers to health care access in minorities, and all controlled for SES [3, 20, 45]. A large secondary analysis of National Health Interview Survey (NHIS) data, Medical Expenditure Panel Survey (MEPS) data, and Bureau of Transportation Statistics (BTS) data, by Wallace et al. [3], estimated that 3.6 million people do not obtain medical care due to transportation barriers. These individuals were more likely to be older, poorer, less educated, female, and from an ethnic minority group. Individuals carrying the highest burden of disease also faced the greatest burden of transportation barriers. In the second study, Johnson et al. [20] analyzed NHIS data from 1997 to 2006 to compare reasons for delayed health care access between 34,504 American Indian/Alaskan Natives and White Veterans, and found that American Indian/Alaskan Natives were more likely to delay care due to transportation problems.

A third study by Probst et al. [45] utilized a cross-sectional household survey, conducted by the US Department of Transportation, to look at ethnic differences in burden of travel for health care. Burden of travel was measured as greater than 30 min or 30 miles to a health care provider. Distance traveled did not vary significantly, but African Americans had higher burdens of travel as compared to Whites even after controlling for mode of travel and SES. In contrast, a study by Borders et al. [55] controlled for SES and found no significant difference in transportation barriers between rural Hispanics and Whites accessing health care in Texas.

Finally, two additional studies found differences by ethnicity, although they did not control for SES. In a study of 593 adults with cancer, Guidry et al. [26] found that Hispanics' transportation barriers to cancer treatment were greater than those of African Americans, and African Americans' barriers were greater than Whites. Transportation barriers included distance to treatment center, access to a vehicle, and finding someone to drive them to treatment. Call et al. [56] contrasted barriers to health care access between 1,853 American Indians and Whites enrolled in the Minnesota Health Care program. The study found that 39 % of American Indians reported transportation barriers compared to 18 % of Whites.

Overall, studies that explored health care access and transportation barriers among members of ethnic minorities and Whites suggested that access is superior for Whites even after controlling for SES.

Special Populations: Children, the Elderly, and Veterans

Certain populations may face unique circumstances with transportation barriers to health care access. For children, significant transportation barriers to health care access have been repeatedly identified [15, 24, 34, 39, 41, 48, 58, 59]. In two separate studies of inner-city children, 18–21 % of respondents cited transportation barriers as the reason for not bringing a child in for needed health care [15, 24]. Among migrant farm workers, 80 % cited lack of transportation as the primary reason for the last episode that their child faced an unmet medical need [34].

The elderly may face a unique combination of access barriers due to disability, illness and likely a greater need for frequent visits to their clinician. Among the elderly reporting any barrier to health care access, 3–21 % reported having transportation barriers, although insurance status and income varied among studies [9, 14, 36, 55, 60–62]. Additional studies of more low-income elderly may be necessary to clarify the role of transportation barriers to health care access.

Two studies examined transportation barriers to health care access for Veterans, a group that often has access to the federal health care system and may receive federally supported transportation assistance. In one study, 19 % of Veterans with colorectal cancer had difficulty with transportation to appointments, and a second study found that 35 % of female Veterans over age 65 had transportation barriers to health care access [23, 63].

Measuring the Impact of Transportation Barriers

Missed Clinic Appointments

Two studies selected patients for research specifically because of missed health care appointments to identify the reasons. In one study of 200 children with a history of missed appointments, 51 % parents identified transportation barriers as the primary reason for missing clinic appointments [42]. In another study, Yang et al. [41] surveyed 183 caregivers of urban children in Texas, and grouped patients based on show rates for a single appointment over a 9-week period. There was a 26 % no show rate overall. For those with a history of missed appointments, 50 % cited transportation problems compared to 30 % of those who kept appointments. Factors associated with missed appointments included not owning a car and not having access to a car.

Pharmacy and Medication Access

Five studies explored the relationship between transportation barriers and medication access with all reporting an inverse association [27, 64–67]. Kripalani et al. [64] studied patterns of discharge medication fills in 84 adults living in urban Atlanta. The study found that following hospital discharge, patients reporting difficulty visiting the pharmacy had lower prescription fill rates than those not reporting difficulty (20 vs. 55 % respectively). Additionally, 65 % of patients felt transportation assistance would improve medication use after discharge. Musey et al. [27] examined the causes for 56 diabetic ketoacidosis [DKA] admissions at Grady Memorial Hospital in Atlanta. He found that 67 % of DKA admissions were related to stopping insulin and 50 % of those patients cited either lack of money for insulin or for transportation to get their medicine.

Welty et al. [65] created an online survey through epilepsy.com to study the relationship between transportation barriers and anti-epileptic use. The study included 143 web site members and found that 45 % of respondents who could not drive said they would miss fewer doses of their medications if transportation was not a problem.

Tierney et al. [66] examined the relationship between transportation policy and health care utilization in a cohort study of 46,722 Medicaid patients, and found that restriction of Medicaid payments for transportation resulted in decreased medication refills. A study by Levine et al. [67] found that transportation barriers were associated with not being able to afford medications, emphasizing that those with low incomes are often the hardest hit by all barriers, including transportation.

Natural Experiments

Two studies have looked at natural experiments to provide real-world insight on the impact of transportation barriers on access to care [66, 68]. One retrospective study by Pheley et al. [68] examined the impact of a 2-week mass transit strike on missed appointments at an inner-city clinic serving a low-income population in Minneapolis. There was no difference in the number of missed appointments between strike and non-strike periods with doctors, but there was an increase of 4.7 failed appointments per 100 scheduled nurse visits (relative risk 1.17).

Another study by Tierney et al. [66] looked at a Medicaid cohort to examine the impact of a policy change that restricted Medicaid payments for transportation on health care utilization. The study focused on the 6-month pre-policy period and the 6-month post-policy period for 46,722 Medicaid patients using an inner-city public hospital and associated clinics. Results revealed that visits to community clinics increased, hospitalizations increased slightly, and visits to hospital based primary care clinics, urgent care clinics, and emergency departments fell.

Discussion

This literature review on transportation barriers and access to health care yielded several important findings. First, patients with a lower SES had higher rates of transportation barriers to ongoing health care access than those with a higher SES (Table 1). Additionally, transportation barriers impacted access to pharmacies and thus medication fills and adherence. Finally, while distance from a patient to a provider would intuitively seem to be a barrier to health care access, the evidence is inconclusive.

Poorer populations face more barriers to health care access in general, and transportation barriers are no exception. In 25 separate studies, 10–51 % of patients reported that transportation was a barrier to health care access (Table 1). This is very significant because when patients cannot get to their health care provider, they miss the opportunity for evaluation and treatment of chronic disease states, changes to treatment regimens, escalation or de-escalation of care and, as a result, delay interventions that may reduce or prevent disease complications (Fig. 1).

Ultimately, transportation barriers may mean the difference between worse clinical outcomes that could trigger more emergency department visits and timely care that can lead to improved outcomes [22]. Since patients who carry the highest burden of disease face greater transportation barriers, addressing these barriers to avoid worsening health seems logical [3]. While there may be differences in transportation barriers based on ethnicity or geography, they may disappear after accounting for socioeconomic factors such as income or insurance. Additionally, studies that reported low rates of transportation barriers to health care access often did not include more vulnerable populations, such as lower income or uninsured patients.

Mixed Evidence

Some aspects of transportation barriers, such as distance, showed mixed evidence regarding the impact on health care access. Distance does not necessarily equate to travel burden and different measures of distance may alter the results. For example, studies that measured the impact of distance subjectively, by asking patients whether distance to the provider was a barrier to health care access or not, concluded it was a barrier [25, 26, 48–50]. However, other studies that objectively measured the distance between homes and health care facilities and subsequent health care utilization found distance was not a barrier [52–54]. A patient may live in a wealthy suburb, own several cars, and have no problem accessing health care, even at a distance. Conversely, a seemingly shorter distance for a patient who has to walk or cannot afford public transit may prove to be too far of a distance, and hence be identified as a barrier by the patient.

Special Populations

Existing studies on the elderly suggest that transportation is a less significant barrier to health care access compared to younger populations. However, these studies lacked inclusion of lower-income elderly populations and did not address concerns that may be more relevant to the elderly, such as safety and disability access. It is possible that the elderly may have fewer competing demands, such as not having to share a car with family members who need a car for work or transporting children. However, additional studies are needed with more representative samples of elderly adults before any conclusions can be drawn about transportation barriers to health care access in this population.

Traveling Forward: Interventions and Public Policy

Collaboration between health policy makers, urban planners, and transportation experts could lead to creative solutions that address transportation barriers to health care access while considering patient health, cost, and efficiency. Such collaboration could also lead to studies in areas that are lacking research, such as research on transportation policy and its impact on health outcomes outside of injury prevention [8]. These collaborations could also use prior research to guide interventions and public policy.

In the studies reviewed, access to a vehicle was consistently associated with increased access to health care even after controlling for SES. Future interventions should consider this link in addition to public transit discounts or medical transportation services. For example, there have been interventions that provide access to cars to improve access to jobs, and these programs could be used as models for providing cars to improve health care access [69].

Additionally, reimbursement for travel should be investigated further to determine the role it plays in keeping appointments and avoiding fragmented care. In Tierney's natural experiment study, which examined the impact of lower Medicaid payments for transportation on health care utilization, several changes occurred in health care utilization rates. These included an increase in community clinic use and hospitalizations, with a decrease in visits to urgent care clinics and emergency departments [66].

New technological innovations such as telehealth may also address transportation barriers by reducing travel needs over time. Telehealth services may include video conferencing, remote monitoring, and other disease management support at a distance. One approach to providing patient-centered care is to evaluate transportation and other barriers to ongoing health care encounters, and provide telehealth services when beneficial and cost-effective. Medication access may also be improved as more services for home medication delivery become available.

Limitations

This review was restricted in scope and had several limitations. Studies with an exclusive focus on screening, prevention, and prenatal and pregnancy care were not evaluated and may have different findings. A majority of the studies used cross-sectional designs thus making cause and effect conclusions difficult (Table 1). The diversity of demographic, geographic, social variables, and outcome measures also make study-to-study comparisons difficult. Efforts to generate a valid measure of transportation barriers for consistent measurement may help to perform future meta-analyses across studies. Prospective studies of local changes in transportation options may also help contribute to the evidence, and although randomized trials would help isolate the impact of transportation interventions they would be impractical to execute [70].

Additionally, the studies on transportation barriers to health care access rely largely on self-report, and lacked an exploration of whether patients were unaware of available services or assistance. While some studies investigated the impact of transportation barriers on objective outcomes such as missed appointments or medication fills, these studies were in the minority. Whether transportation barriers contribute to differences in health outcomes needs to be explored further with objective outcome measures. By demonstrating that transportation barriers lead to missed appointments, poorer medication adherence, and thus poorer diabetes or blood pressure control, transportation barriers could be more strongly linked to health access and outcomes (Fig. 1).

Conclusion

Transportation barriers to health care access are common, and greater for vulnerable populations. The studies reviewed may help guide both the design of interventions that address transportation barriers and the choice of measures used in assessing their effectiveness. Future studies should focus on both the details that make transportation a barrier (e.g., cost, mode of travel, public transit safety, vehicle access) and objective outcome measures such as missed appointments, rescheduled appointments, delayed medication fills, and changes in clinical outcomes. Such studies would help clarify both the impact of transportation barriers and the types of transportation interventions needed. Millions of Americans face transportation barriers to health care access, and addressing these barriers may help transport them to improved health care access and a better chance at improved health [3].

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Operating within the Rules: Welfare Recipients' Experiences with Sanctions and Case Closings

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This article examines the experiences of Temporary Assistance for Needy Families recipients with sanctions and administrative case closings, as reported by respondents in a survey of families in low-income neighborhoods in Boston, Chicago, and San Antonio. Among those who said that their welfare benefits had been reduced or eliminated for noncompliance with the rules, the most common reasons provided were missing an appointment or not filing paperwork. In comparison with other families that had received welfare in the previous 2 years, families that were penalized were more disadvantaged in a number of respects, including lower education and poorer health.

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) altered the welfare system in important ways. The Temporary Assistance for Needy Families (TANF) program, which re-

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placed Aid to Families with Dependent Children (AFDC), gives states a fixed block grant, ends the entitlement of families to welfare benefits, and imposes a 5-year limit on the use of federal funds to provide a family with assistance. Congress intended these restrictions to reduce dependence on government by changing the behavior of welfare recipients along several dimensions, including work, marriage, and childbearing. Yet although caseloads have dropped sharply, relatively few families reached their limits during the first 5 years after PRWORA was enacted (Pavetti and Bloom 2001). Time limits, though, are not the only enforcement mechanism available under PRWORA. According to one estimate, during the first several years of the program, more families had their TANF benefits reduced or eliminated because the welfare office determined that they were not following the rules of the program than had lost benefits as a result of time limits (Goldberg and Schott 2000).

If an adult in a family receiving assistance refuses to engage in required work, states have the authority to reduce the amount of assistance payable to the family, a procedure known as a partial sanction, or to terminate the amount of assistance, a procedure known as a full-family sanction (U.S. Department of Health and Human Services 2000). Sixteen states have chosen to impose only partial sanctions; the rest impose full sanctions, usually after first imposing a partial sanction. States also have the option to impose partial or full sanctions on individuals who fail to cooperate with efforts to establish paternity or obtain child support. In addition, states may impose partial sanctions for other types of noncompliance, such as failure to keep children inoculated against certain illnesses or to maintain children's attendance in school. The U.S. General Accounting Office estimated in 2000 that, in an average month, five times as many families experienced partial sanctions as experienced full sanctions (U.S. General Accounting Office 2000*b*). In the study described in this article, three times as many people report partial sanction as report a full loss of benefits.

Imposing a full-family sanction is not the only way that a state can terminate benefits. States have long had the authority to close cases when recipients do not follow administrative regulations while enrolled in the program. These administrative case closings typically follow procedural violations such as failing to turn in required forms or to meet periodically with caseworkers to determine continuing eligibility for benefits. Administrative case closings differ from case closings due to a change in eligibility status, such as an increase in a recipient's income, a change in marital status, or a youngest child reaching age 18.

Although many researchers have studied administrative case closings from the standpoint of the bureaucracy, fewer have studied the topic from the standpoint of the recipient, for whom administrative case closings and full-family sanctions can be difficult to distinguish. To be sure, there are some potential tangible differences. For example, individuals generally can reapply for benefits immediately if their case is closed for procedural reasons, whereas individuals who are sanctioned may be unable to reapply for a fixed period of time (U.S. Department of Health and Human Services 2000). Yet the rules in the cities we studied allow many sanctioned individuals to reapply as soon as they come into compliance, thus blurring the line between case closings and sanctions.[1] Therefore, full-family sanctions and administrative case closings, both of which terminate TANF benefits for not following rules, can appear very similar to families.

In this article, we provide information on case closings for noncompliance from the point of view of the recipients. In doing so, we consider sanctions and administrative case closings jointly. We asked recipients whether their benefits had been reduced or eliminated because the welfare office said they had not followed the rules. It is possible that some recipients do not correctly understand why they have lost benefits, in part because regulations can be obscure and their implementation difficult to understand. But we argue that a study based on recipients' perceptions of their situations can provide valuable information on their actions and the nature of their difficulties with the TANF system. Drawing on a sample of families from low-income neighborhoods in Boston, Chicago, and San Antonio, we present the most common reasons families reported for losing all or part of their benefits, the characteristics of these families, and their experiences after losing benefits.

Background

Between 1965 and 1970 the AFDC rolls more than doubled and the ensuing administrative difficulties led to an overhaul of state reporting requirements by Congress, resulting in a more formal, rule-bound system that emphasized strict adherence to rules and caseload reduction (Brodkin 1986). The emphasis on decreasing errors and streamlining procedures placed the onus on clients to verify their eligibility and allowed case managers increased authority to penalize them for noncompliance (Handler and Hasenfeld 1991). Observers claimed that case managers sometimes became street-level bureaucrats, overseeing the implementation of rules meant to increase client accountability (Lipsky 1984; Bane and Ellwood 1994; Brodkin 1997). Some contended that caseworkers, under pressure to reduce error rates and enforce compliance, became more concerned with satisfying procedural requirements than with helping clients traverse a system that was becoming increasingly bureaucratic (Brodkin 1986).

Welfare reform strategies during this period employed varying degrees of coercion and persuasion. Programs like the Work Incentive Program (WIN) of 1967 and WIN II of 1971, which were partly coercive in their approach to client conformity, also attempted to encourage client participation by providing incentives such as income disregards for eligible recipients. Both programs allowed sanctions for noncompliance, yet these more punitive measures were seldom used (Handler and Hasenfeld 1991, 1997). Similarly, when Congress passed the Family Support Act of 1988 and created the Job Opportunities and Basic Skills Training (JOBS) Program, the use of sanctions was not widespread, although strict requirements were placed on clients enrolled in the program. Nevertheless, the bureaucratic atmosphere in welfare offices in the decades leading up to PRWORA meant that clients were often unable to distinguish among the various procedural or administrative guidelines case managers could refer to when making decisions about eligibility or compliance.

Evelyn Brodkin's (1986) case studies of AFDC administrative reforms in Massachusetts in the early 1980s demonstrate the inherent contradictions in a system that became increasingly stringent and reliant on regulatory guidelines and yet provided case managers and supervisors with much individual discretion. Frontline workers had the responsibility to interpret the rules according to the complicated and unique circumstances of families' lives. Attempts to resolve ambiguities in a family's application, redetermination, or maintenance of welfare status involved an array of possible resolutions, driven in large part by official guidelines, the welfare office's interpretation of those rules, and the interplay between the caseworker and his or her supervisor and between caseworker and client. Clients were often left confused about official determinations of their cases, in part, because decision makers now had a wider array of options available to them to make such rulings. Case managers, for their part, were also caught in a difficult quandary. At times, they had to choose between what was best for their clients and what was the most appropriate course of action according to the regulations (Brodkin 1986).

The emphasis on work requirements and caseload reduction increased again with the passage of PRWORA. In the early 1990s, the Department of Health and Human Services began to waive some of the federal welfare rules in order to allow states to try new approaches. Some state officials and policy makers were concerned that partial sanctions were not severe enough to encourage families to comply with work requirements, and about 30 states received waivers to implement full-family sanctions. From mid-1993 through 1996, about 18,000 families' benefits were terminated for failure to comply with program requirements (Pavetti et al. 1997).

In addition, under the waiver process, many states began to experiment with reducing or eliminating benefits for other reasons. In part, the reasons involved parental responsibility: under some waivers, AFDC recipients could be sanctioned if they failed to get their children immunized against childhood diseases or to take them to regular medical checkups or if their children did not attend school regularly. States also initiated sanctions if mothers did not provide adequate information about fathers to child support enforcement agencies, although these sanctions were allowed previously and did not require a waiver. Sanctions policies thus evolved as a way to influence the behavior of welfare recipients in several domains.

Under PRWORA, states were required to withhold part of a family's grant—that is, to impose at least a partial sanction—if the adult recipient did not comply with work requirements or failed to cooperate with child support enforcement. Under certain circumstances, states were also required to withhold part of a family's Food Stamps benefit and, optionally, Medicaid coverage for some adults. Moreover, for the first time in federal welfare legislation, states were allowed to impose full-family sanctions on adults in single-parent families for failure to cooperate with work activities. In addition, states were allowed to implement partial or full-family sanctions and to withhold Food Stamps and Medicaid (subject to some limitations) for purposes other than work enforcement.

A report by the U.S. General Accounting Office (2000*b*) estimates that in 1998 about 5 percent of the total average monthly TANF caseload was sanctioned, with substantial variations from state to state. Eighty-three percent were partial sanctions, and 17 percent were full-family sanctions. The numbers in the report reflect full sanctions only in the month the sanction begins, thus underestimating the average number of families under full sanctions each month (U.S. General Accounting Office 2000*a*). In a survey of welfare recipients in an urban Michigan county, 18 percent of the women who received cash welfare benefits said that their benefits were reduced or stopped during the previous year because their welfare worker said they had not followed the rules (Kalil and Seefeldt, in press). As in the study we report, this percentage appears to combine sanctions and administrative case closings.

Few studies provide demographic descriptions of sanctioned cases. But some evidence suggests that many TANF clients who are sanctioned have multiple barriers to self-sufficiency. In their review of other studies, LaDonna Pavetti and Dan Bloom (2001) conclude that although sanctioned families are heterogeneous, hard-to-employ families (e.g., those without GEDs or high school diplomas or those who are long-term welfare recipients) are overrepresented in the sanctioned group. The authors also conclude that often families who are sanctioned experience challenges such as domestic violence, physical and mental health problems, drug or alcohol dependency, or transportation problems that may make it difficult for them to comply with program requirements. These challenges may also extend their time on welfare. Similarly, Heidi Goldberg and Liz Schott (2000), citing many state studies, conclude that sanctioned families, compared with other welfare families, have less education, less work experience, a greater incidence of domestic violence, more disabilities, and more mental and physical health problems and lack support services such as transportation and child care. Ariel Kalil and Kristin Seefeldt (in press) report that in one Michigan county, participants whose benefits were reduced or eliminated were likely to be less educated and younger, but there were no significant differences in race, marital status, or length of time on welfare. The U.S. General Accounting Office (2000*b*) report also notes that sanctioned participants in six states were less educated or faced more barriers to work, such as transportation or child care difficulties or health problems. The authors of some of the studies speculate that low education may limit the ability to understand and comply with complex rules and cause some families to be sanctioned.

Since the main purpose of strengthening the sanctions mechanism in PRWORA was to enforce strict work requirements, it is often assumed that failure to work is the main reason for their imposition. Indeed, reports based on administrative data suggest as much. For example, a U.S. General Accounting Office (2000*b*) report concludes from state studies that 61 percent of sanctions were imposed for noncompliance with work responsibilities, 15 percent for failure to verify immunizations and to ensure school attendance, and 11 percent for failure to cooperate with child support enforcement agencies.[2]

Some studies report that welfare leavers who are sanctioned do not fare as well as those who leave welfare for other reasons. Pavetti and Bloom's (2001) review of a number of leaver studies provides a mixed picture, but they report that many of the families that left welfare in response to sanctions or time limits later had trouble making ends meet and sometimes had insufficient food. However, it is unclear whether sanctioned and time-limited families were worse off than those who left welfare for other reasons. A U.S. General Accounting Office (2000*b*) report concludes that 41 percent of sanctioned adults worked after they left TANF, compared with 68 percent of adults who left welfare for other reasons. Goldberg and Schott (2000) also conclude that sanctioned leavers were less likely to become employed and that those who did become employed had lower earnings than those who left for other reasons. Kalil and Seefeldt's multivariate analysis, which attempts to control for individual differences, suggests that having benefits reduced or eliminated increased the likelihood of encountering economic hardship (Kalil and Seefeldt, in press).

In sum, sanctions and administrative case closings are bureaucratic mechanisms for enforcing compliance with welfare rules and for furthering the goals of welfare policy. Use of the former mechanism has increased since PRWORA, and the latter mechanism has remained common. From a client's perspective, the two mechanisms look almost indistinguishable. A number of studies of sanctions have been carried out using administrative data from states or localities, sometimes supplemented by interviews with welfare administrators and caseworkers. Although these studies can provide estimates of the number of families involved, they often lack information on the experiences of the families. A detailed analysis of the characteristics and experiences of families that say they had their benefits reduced or eliminated can help us to better understand the role that penalties for noncompliance play under TANF.

The Three-City Study

The data used in this article come from a study of low-income families in Boston, Chicago, and San Antonio. For one component of the study, the researchers conducted a household-based, random-sample survey of children and their caregivers in low-income neighborhoods.[3] In households with a child age 0–4 or age 10–14, with a female primary caregiver, and with an income below 200 percent of the federal poverty line, interviewers randomly selected one child and conducted in-person interviews with that child's primary caregiver (a mother in over 90 percent of the cases) and, in the case of 10- to 14-year-olds, the child. Interviews were conducted between March and November of 1999 with 2,402 families, including an oversample of TANF-receiving families. The response rate was 74 percent. Thirty-seven percent of the families were receiving TANF at the time of the interview, and an additional 20 percent had received TANF in the 2 years prior to the interview.[4] All tables and figures reported in this article are based on the random-sample survey.[5]

Of the three states in the study, Massachusetts and Illinois both impose full-family sanctions but implement them in a graduated manner. In Illinois, the initial violation results in a 50 percent reduction of the family's TANF benefit, and subsequent failure to comply for 3 months can result in a full-family sanction. In Massachusetts, the initial violation results in the loss of the adult portion of the family's TANF grant. Subsequent failure to comply with work requirements for 1 month can result in a full-family sanction. So even in the two states in the study that allow full-family sanctions, all noncompliant families receive partial sanctions first. Texas does not impose full-family sanctions. In all three states, noncompliance with work requirements can also result in partial or full reduction in Food Stamps benefits. In these states, as is the case nationwide (Lurie 2001), there appears to be substantial variation at the level of the local welfare office and the caseworker in how frequently sanctions are invoked.

As for administrative case closings, the welfare agencies in all three states have the authority to eliminate families' benefits by closing their cases if the recipients fail to follow a variety of requirements. These include failure to keep an appointment or attend a meeting with a caseworker and failure to produce verification of eligibility for continued benefits. Welfare reform does not appear to have lessened the amount of time that caseworkers must devote to collecting and verifying documents (Lurie 2001).

This article focuses on the 1,262 caregivers in the 1999 survey who said they had received TANF at some point in the previous 2 years. In the survey, interviewers asked every caregiver who had left TANF in the previous 2 years (even those who were back on the rolls at the time of the interview) the following question about the most recent time they left: "Did you go off welfare at that time because the welfare office said you weren't following the rules or was there some other reason?"[6] If the respondent said she left for "some other reason" or if she had received TANF continuously for the previous 2 years, she was asked: "Did the welfare office in [state] reduce your benefits at some point in the past 2 years because they said you were not following the rules?" The answers to these questions were used to calculate the frequency of full loss of benefits (those who said they went off welfare because the welfare office said they were not following the rules) and partial loss of benefits (those who said they had their benefits reduced because the welfare office said they were not following the rules). A partial loss of benefits could only have reflected a partial sanction. A full loss of benefits could have been the result of a sanction or a case closing. The survey did not directly ask which of the two had occurred because it was the investigators' impression that the distinction is often unclear to the recipients who are affected. Nevertheless, when we examined responses to a follow-up question—"Which rules did the welfare office say you were not following?"—we excluded from our measure any recipients who said that their benefits were reduced or eliminated because they got a job, got married, or their income rose (i.e., those whose eligibility status changed).[7] Let us caution that the study did not request access to recipients' administrative records. There may be other recipients in the survey who did not disclose that they had lost benefits as a result of sanctions or case closings or who were not aware that they had lost benefits. And there may be others who mistakenly thought that they were sanctioned.

Reasons for Losing Benefits

Taking all three cities together, we find that 13 percent of caregivers who reported receiving TANF at some point in the previous 2 years also reported experiencing a partial loss of benefits, and another 4 percent reported experiencing a full loss of benefits, because the welfare office said they were not following the rules.[8] Figure 1 displays the results separately for each city. In all three cities, a reported partial loss of benefits was more common than a full loss of benefits. Combining both partial and full losses, the percentage reporting a loss over the previous

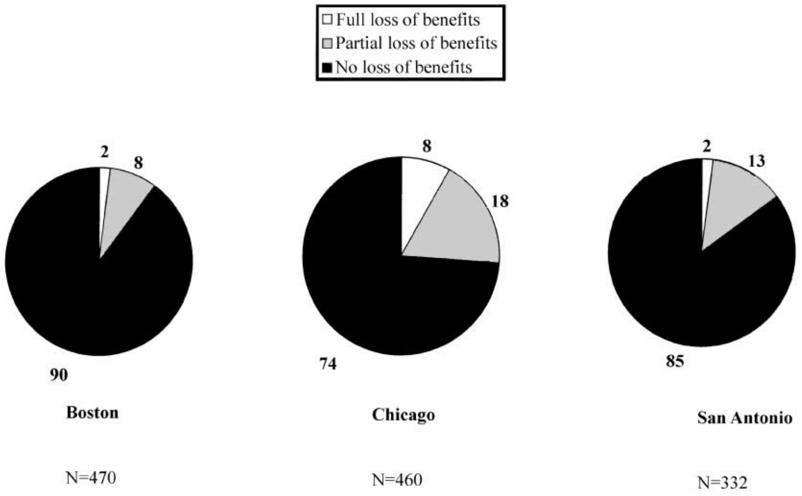
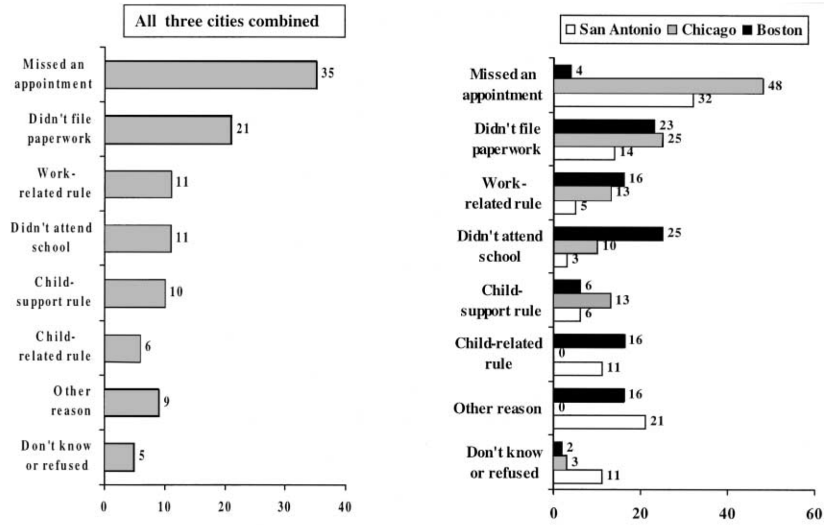


Fig. 1.—Percentage of all individuals who have received welfare in the past 2 years who reported a loss of benefits because the welfare office said they were not following the rules, by city.

2 years was lowest in Boston (10 percent), higher in San Antonio (15 percent), and highest in Chicago (26 percent).

For everyone who reported a partial or full loss of benefits because they were told they were not following the rules, we asked, “Which rules did the welfare office say you were not following?” Figure 2 shows the responses for all three cities together and for each separately. The categories in the chart were not read to the individuals but rather appeared on the interviewer’s laptop computer screen (and in some instances were later combined or recoded by the authors of this report). If an individual gave a reason that did not fit a preassigned category, the interviewer was instructed to enter the response verbatim on the computer. We subsequently examined these responses and recategorized individuals whenever appropriate. (As noted above, we also excluded responses that were clearly about loss of eligibility.) The percentages in figure 2 sum to slightly more than 100 percent because individuals were allowed to state more than one reason, but less than one in 10 did so.

Missed appointment or paperwork problem.—The two largest categories in figure 2 are “missed an appointment” and “didn’t file paperwork.” The survey interviewers did not inquire further about what type of appointment was missed or what kind of paperwork was not filed. Boston had the lowest percentage of caregivers in the survey who said missing an appointment was the reason their benefits were reduced or eliminated.[9] The highest percentage occurred in Chicago (see fig. 2). Interviews with key informants and families in Chicago suggest that efforts to check the



Note: Percentages sum to more than 100 because a small number of individuals gave more than one answer

Fig. 2.—Individuals' reports on which rules the welfare office said they were not following.

continued eligibility of welfare recipients may be the reason for the higher reports of missing a meeting. In 1999, for example, while the survey was underway, some Chicago welfare offices sent redetermination letters to large numbers of recipients in their districts, requiring them all to report to the office on the same day. Those who did not show up had their cases closed. However, the cases could be reopened retroactively if the recipient came to the office within 10 days. Consequently, many of the individuals had their benefits reinstated. Indeed, 62 percent of the Chicago survey respondents in the "missed an appointment" category had their benefits reinstated. Missing an appointment also was the most common reason for losing some or all benefits in San Antonio.

Other reasons.—All of the other categories in figure 2 were reported less often. In 11 percent of the cases, a recipient reported either refusing to work or not showing up for work. These are the adults whose experiences most closely match the common understanding of why sanctions are imposed. They constitute about one-ninth of all the cases reporting partial or full loss of benefits due to rules violations in the survey.

The category "didn't attend school" refers, in part, to rules that require minors who have not finished high school to attend school or GED classes; about one-half of the cases in Boston, where the "didn't attend school" category was highest, involved women age 20 or younger.

The other half of the “didn’t attend school” cases in Boston and most of the small number of cases in the other two cities involved adults; they may have agreed (or been required) to attend English as a second language or job-training classes as part of their TANF responsibilities.

The “child support rules” category refers to the requirement that TANF recipients cooperate with child support enforcement efforts by providing information about the fathers of their children. If they fail to do so, they are subject to sanctions. This type of sanction was present even prior to PRWORA.

The “child-related rules” refer to responsibilities parents must fulfill or else face sanctions. In nearly all cases in the study, these were partial sanctions. Parents were required to get their children immunized against childhood diseases and to have regular medical checkups. They also were held responsible if their children did not attend school regularly. These rules seemed to have an effect on some parents. A San Antonio mother in the ethnography said that it was well known that welfare and Food Stamps benefits would be reduced “if you don’t take them to their checkups, or they’re not updated with their shots.”

The remaining cases fall into the “other” and “don’t know/refused” categories. The former is an amalgam of verbatim responses that we could not understand or could not place in any of the categories. The “don’t know/refused” category is particularly high in San Antonio.

Who Has Benefits Reduced or Cut Off?

We examine which characteristics seem to best predict the likelihood that women would have their TANF benefits reduced or eliminated for noncompliance. Our method is a logistic regression in which the dependent variable is the log-odds of having benefits reduced or eliminated among all caregivers who received TANF in the previous 2 years. The independent variables are characteristics that the study or previous studies suggested might be predictive. They are listed in table 1. Some require explanation:

Poor health: A variable scored one for responses of “fair” or “poor” and zero for responses of “good,” “very good,” or “excellent,” to the question, “In general, how is your health?”

Brief Symptom Inventory: An 18-item scale of anxiety, depression, and somatization, with a higher score indicating more symptoms.[10]

Domestic interference: A scale composed of the number of affirmative responses to four questions asking whether someone in a romantic relationship had interfered with the respondent’s employment, training, or schooling.[11]

Moderate domestic violence: A scale composed of the number of affirmative responses to four questions asking whether someone in a ro-

Table 1

Descriptive Statistics for All Variables Used in Models, for All Women Who Had Been on TANF during the Previous 2 Years (N = 1,122)

	Mean	Standard
Race/ethnicity:		
African American	.490	.496
Hispanic	.479	.496
White (omitted category)	.032	.173
City:		
Boston	.325	.465
Chicago	.331	.467
San Antonio (omitted category)	.344	.471
Completed high school or GED	.624	.480
Age	31.054	9.551
Marital status:		
Married	.177	.378
Cohabiting	.060	.235
Neither married nor cohabiting (omitted category)	.763	.422
Number of minors in household	3.357	1.730
Age of youngest child (in years)	3.636	3.828
Native English speaker (yes = 1)	.756	.426
Months on welfare in past 2 years	18.613	7.969
Months worked in past 2 years	7.078	8.367
Poor health	.266	.438
Brief Symptom Inventory	1.669	1.134
Domestic interference	.134	.277
Moderate domestic violence	.484	.401
Extreme domestic violence	.205	.323
Has phone at home (yes = 1)	.798	.399
Household owns car (yes = 1)	.340	.470
Used marijuana in past 12 months (yes = 1)	.158	.362
Used hard drugs in past 12 months	.031	.171
Neighborhood problems	21.049	6.128

Note.—The numbers presented in this table are weighted.

mantic relationship had ever “threatened to hit you,” “thrown something at you,” “pushed, grabbed, or shoved you,” or “slapped, kicked, bit or punched you.”[12]

Extreme domestic violence: A scale composed of the number of affirmative responses to four questions asking whether someone in a romantic relationship had ever “beaten you,” “chocked or burned you,” “used a weapon or threatened to use a weapon on you,” or “forced you into any sexual activity against your will.”[13]

Neighborhood problems: A scale composed of 11 items measuring common problems.[14]

Our multivariate analysis cannot establish cause and effect. Some correlates of losing benefits, such as lower household income, might precede a penalty or result from it. Indeed, we suspect that many of the characteristics of penalized families preceded the imposition of the penalties. Our intent is not to propose a causal model but rather to identify

Table 2

Odds of Having Benefits Reduced or Eliminated for Noncompliance, for All Women Who Had Been on TANF during the Previous 2 Years ($N = 1{,}122$)

	e^{β}
Demographic characteristics:	
African American	2.452
Hispanic	1.792
Chicago	1.337
San Antonio	1.395
Completed high school or GED	.657*
Age	.971*
Married	1.029
Cohabiting	.661
Number of minors in household	1.049
Age of youngest child	1.012
Native English speaker	1.680
Work/welfare variables:	
Months on welfare in past 2 years	.991
Months worked in past 2 years	.988
Poor health	1.995**
Substantive variables of interest:	
Brief Symptom Inventory	.873
Domestic interference	3.654**
Moderate domestic violence	.962
Extreme domestic violence	.526
Has phone at home	.660*
Household owns car	.520**
Used marijuana in past 12 months	1.755*
Used hard drugs in past 12 months	1.424
Neighborhood problems	1.026

Note.—Coefficients are reported in exponentiated (e^{β}) form.
* $p \leq .05$.
** $p \leq .01$.

a set of characteristics that are typical of the kinds of families that have their benefits reduced or eliminated.

The results of the logistic regression are presented in table 2. Estimated coefficients are presented in exponentiated form as odds ratios. For example, the first coefficient in the table suggests that, controlling for all the other factors in the model, African Americans were 2.45 times as likely to have their benefits reduced for noncompliance as the reference group, non-Hispanic whites. But this difference is not statistically significant, in part due to the small number of non-Hispanic whites in the sample.

Several coefficients were statistically significant: Those who said they were sanctioned or had their cases closed were less likely to have completed high school or to have obtained a GED and were younger, on average.[15] They were also more likely to report being in "fair" or "poor" health, rather than "good," "very good," or "excellent" health. In addition, penalized families scored higher on the scale measuring whether a romantic partner had interfered with their employment, training, or schooling. They also were more likely to report using marijuana during the previous 12 months, although there was no significant difference in use of hard drugs. They were less likely to say that they had a working telephone at home that they can use and less likely to say that anyone in their household owned a car, van, or truck. Finally, penalized families scored marginally ($p < .10$) higher on the neighborhood problems scale.

These findings imply that families that were sanctioned or had their cases closed for procedural reasons were more vulnerable in many respects than other families. Their health tended to be worse, they had lower levels of education, and they tended to have more children to care for in their households. They were more likely to report that a romantic partner interfered with their attempts to work or to attend school. They were less likely to have a telephone; it is, of course, harder for caseworkers and clients to keep in touch when the clients do not have telephones. This communication problem could have contributed to the high percentage of individuals who reported losing benefits as a result of missed appointments. Their lower likelihood of owning an automobile may have made it more difficult for them to find a job or to travel to work.

Coping with Benefit Reductions

In the survey, we asked adults who reported a partial or full loss of benefits for not following the rules whether they had tried to get their benefits reinstated, whether these efforts were successful, and what they did to cope with the loss of income. About two-thirds said that they had tried to get their benefits back. (Among those who tried, about half said that they had started following the rules again, and the other half reported appealing the decision, reapplying, showing proof of good cause, or other strategies.) About half of all respondents who received a full or partial sanction said that they had been able to get their benefits back.[16]

Figure 3 shows the response to a question that was asked of caregivers who reported a partial or full loss of benefits without complete restoration of these benefits: “What did you do to get by when the benefits stopped/after the benefits were cut?” The most common responses (more than one answer was allowed) was “got a job, ” followed by “cut back on necessities” and “got money from friends and family.” Modest numbers reported cutting back on extras, stopping or delaying paying bills, getting more child support from fathers, or obtaining benefits from another program. Only 2 percent mentioned getting help from charity. Less than 1 percent reported obtaining cheaper housing or moving in with others, placing children in someone else’s care, or going to a homeless shelter. It appears that, in addition to getting a job, families

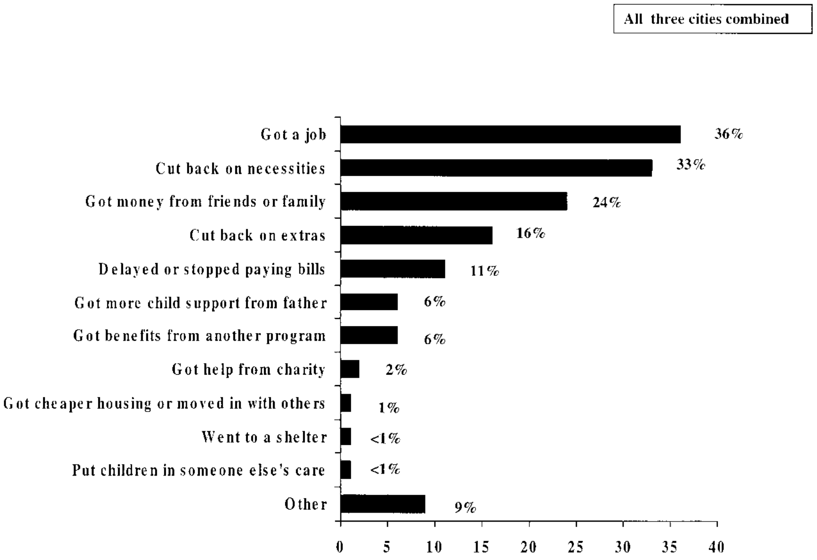


Fig. 3.—What individuals did to get by when benefits were stopped or reduced $N =$ 108 respondents who said they did not get benefits completely restored.

that lose benefits tend to cut spending and rely mainly on friends and kin for support.

Conclusion

In a sample of children and their caregivers in Boston, Chicago, and San Antonio who had received TANF in the 2 years prior to a 1999 interview, 17 percent of the caregivers said that their benefits were reduced (13 percent) or eliminated (4 percent) because the welfare office said they were not following the rules. There were differences among the three cities in the percentage of caregivers who said that their benefits were reduced or eliminated. But these city-to-city variations did not correspond to differences in policies for sanctions and procedural case closings that could be easily measured. For example, Massachusetts and Illinois allow full sanctions to be imposed and Texas does not. Yet the percentage of people who reported penalties in Boston (10 percent) was more similar to San Antonio (15 percent) than to Chicago (26 percent). And the percentages of penalized recipients who said they had their benefits withheld for missing meetings differed greatly in Boston (4 percent) and Chicago (48 percent).

According to the recipients, sanctions and procedural case closings were imposed for a wide range of reasons. Few were imposed because someone directly refused to work or did not show up for a work-related activity. Instead, the most common reasons were bureaucratic: missing a meeting or failing to produce required forms or documents. Some of these reasons were related to failure to provide verification of work, but others were not. Benefit reductions were also imposed for failure to provide enough information about fathers to the child support enforcement system and for many behavioral reasons, such as children's poor school attendance, lack of immunizations, or failure to get children regular medical checkups.

Our data are not sufficient to judge the effectiveness of sanctions and case closings in obtaining compliance with program rules. But we find that sanctions and procedural case closings appeared to ensnare families that were experiencing hardships and possibly to impose more hardships on some of them. Each of the many ways that rules can be violated requires its own verification system and has its own paper trail. For low-income individuals with limited education, daily lives filled with personal turmoil, and employment and family responsibilities to balance, meeting all of these demands may be more than many can handle. Being able to turn in forms on time or to follow up with doctors' offices or employers' personnel offices can be a feat in itself. It requires keeping up with the mail; noticing and adhering to deadlines; and reading, interpreting, and responding to questions—all of this by mothers who may have complex and challenging daily lives. Individuals whose benefits were reduced or cut off tended to have poorer health. They were more likely to report that a romantic partner interfered with their attempts to work or go to school. They were less likely to have a telephone or to own an automobile and were marginally more likely to live in lower-quality neighborhoods.

Our findings, if confirmed by other studies of PRWORA, suggest that welfare agencies may wish to adjust sanctions and case closing procedures to the higher level of disadvantage among many of the affected families and to their difficulties in complying with program rules. In some jurisdictions, for example, social service personnel meet with family members after noncompliance but before a penalty is imposed. They determine whether the family understands what actions are needed to comply, assess what problems the family faces in complying, and provide services to help them comply. In other jurisdictions, social service personnel meet with families after a penalty has been imposed to help them return to compliance (Goldberg and Schott 2000). Throughout this process, it would be useful to identify families' barriers to self-sufficiency and to assist them in making the transition from welfare to steady employment. For some families penalized for noncompliance, steady employment may not be feasible, and exemptions from time limits may be needed.

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Notes

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Coley, Judith Francis, Mark Greenberg, Ron Haskins, Jane Henrici, Laura Lein, Robert Moffitt, Wendell Primus, Howard Rolston, and William Julius Wilson.

1. In Massachusetts, all sanctioned cases can be reinstated as soon as the individual comes into compliance. In Illinois, the first instance of sanctioning reduces benefits that can be reinstated immediately, although the second instance must remain in force for 3 months. In Texas a work-related sanction must remain in force for 1 month for the first violation, 3 months for the second violation, or 6 months for subsequent violations; child-support-related sanctions may be reinstated immediately on compliance. Still, safeguards that apply to sanctions in many states, such as notifying recipients in writing of the impending loss of benefits and providing them an opportunity to demonstrate reasonable cause for noncompliance, do not always apply to case closings.

2. Unfortunately, the Michigan survey did not ask why sanctions were imposed on its subjects.

3. Ninety-three percent of the block groups we selected for our sample covered areas with poverty rates of 20 percent or more.

4. These are unweighted percentages. All other statistics in this report use weights that adjust the statistics to be representative of all the families in the areas of the cities from which we drew our sample. The cities are given equal weight. In weighted terms, 32 percent of the sample was receiving TANF at the time of the interview, and another 16 percent had received TANF in the previous 2 years. The difference between the unweighted and weighted percentages on TANF occurred because we intentionally oversampled families that were likely to be receiving TANF.

5. The 2.5 hour interview with the caregiver covered a wide range of topics. Questions concerning several of the most sensitive topics—domestic violence, work-related domestic harassment, substance use, depression, and anxiety—were asked using the audio computer-assisted self-interview method. Respondents were given a laptop computer and provided with earphones. They saw and heard the questions; no one else in the room could see or hear. They responded to questions by pressing number keys on the computer, as instructed by the program. Studies suggest that this method increases the reporting of sensitive behavior in surveys (Turner et al. 1998).

6. If the respondent indicated that another term such as “public aid” was used to refer to welfare, we substituted that term.

7. It is possible that some recipients whose eligibility changed exited TANF in anticipation of sanctions or administrative case closings.

8. All figures that we report for the three cities combined correspond to average values across the three cities. For example, the 17 percent total for partial and full loss of benefits is an average of 10 percent (Boston), 15 percent (San Antonio), and 26 percent (Chicago).

9. This is not to say that such violations never happen; legal services experts in Boston have told us that they have handled appeals in many such cases. However, there may be some procedural safeguards in Boston that reduce the number of benefit reductions and terminations for missing an appointment.

10. The scale is copyrighted (Derogatis 2000). For each item, response choices range from “not at all” (1) to “extremely” (5). To address skewness in the raw scores, transformed variables were created by adding one to the raw score and taking the natural logarithm. The scale has an alpha reliability of .91 in our sample.

11. The items all begin with the phrase “Has anyone you have been in a romantic relationship with ever . . . .” The selections were as follows: “interfered with your attempts to go to work, training or school?”; “harassed you at work, training, or school?”; “caused you to miss work, school, or training because of their behavior?”; and “ caused you to lose a job because of their behavior?” Respondents received one point for each affirmative answer, and the score was created by taking the mean. The alpha reliability is .83.

12. The items are prefaced by the phrase “Now, think about all of the romantic relationships you have had in your life.” Each item is prefaced by, “Has anyone you have been in a romantic relationship with ever . . . .” Square root transformations of the raw scores are used to correct for skewness of the data. Principal components analysis with promax rotation was performed to ascertain the construct. A mean total composite value was calculated for each individual. The alpha reliability is .85.

13. The scale construction is identical to that for moderate domestic violence. (See n. 12 above.) The alpha reliability is .84.

14. The sequence is prefaced by the statement “For the next questions, please tell me

how much of a problem each of the following is in your neighborhood." The response categories are "not a problem," "somewhat of a problem," and "a big problem." The respondent's answers are added up across the 11 items. The items are "high unemployment," "abandoned houses," "burglaries and thefts," "assaults and muggings," "gangs," "drug dealing in the open," "unsupervised children," "teenage pregnancy," "unsafe streets during the day," "police not being available," and "children that you don't want your [child/children] to associate with." The alpha reliability is .91.

15. Odds ratios of less than 1.0 imply that when the value of a variable increases, the likelihood of having benefits reduced or eliminated decreases.

16. The percentage is only slightly lower (44 percent vs. 47 percent) when the large number of Chicago families who reported being sanctioned for missing an appointment are excluded.

Welfare and Work Sanctions: Examining Discretion on the Front Lines

Vicki Lens
Columbia University

Sanctions are a key tool for enforcing welfare reform's work requirements, but little attention has been paid to how laws, administrative procedures, judicial decisions, and worker discretion interact in the application of sanctions on the front lines. This study analyzes administrative fair hearing decisions and in-depth interviews with sanctioned recipients. The findings suggest that workers interpret and apply sanction rules narrowly, failing to distinguish procedural violations from substantive ones. It also finds that workers are skeptical about claims of good cause exceptions from work rule violations, are strict in the application of the rules governing such exceptions, and overlook rules requiring them to show that a client's action (or inaction) was willful before imposing sanctions. Sanctions are applied across various groups of clients, including those engaged in ongoing work activities, as well as those who are disadvantaged and less willing to work.

Declining caseloads and increased work among recipients have led many scholars and policy makers to herald welfare reform as a success (Rector and Youssef 1999; O'Neill and Hill 2003; U.S. Senate 2003; Haskins 2006). This success frequently is attributed to policies that propel recipients quickly and forcibly into the labor market (Rector and Youssef 1999; O'Neill and Hill 2003; U.S. Senate 2003; Haskins 2006). In welfare reform, employment services approaches that emphasize training and education are replaced by aggressive interventions that require recipients to seek and accept any job or lose their benefits. Sanctions, which are financial penalties for violating welfare's rules, provide welfare caseworkers with a powerful tool for ensuring compliance. Scholars consider the sanctions along with more positive incentives, such as supports and work-related services, to be responsible for welfare reform's perceived success (Rector and Youssef 1999; Mead 2001).

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However, little attention has been paid to how sanctions are applied on the front lines (Pavetti, Derr, and Hesketh 2003). Rather, research tends to focus on sanction rates, characteristics of sanctioned families, and the long-term effect of sanctions on families' well-being (Born, Caudill, and Cordero 1999; Fein and Lee 1999; Edelhoch, Liu, and Martin 2000; Koralek 2000; Westra and Routley 2000; Mancuso and Lindler 2001; Polit, London, and Martinez 2001; Cherlin et al. 2002; Kalil, Seefeldt, and Wang 2002; Hasenfeld, Ghose, and Larson 2004; Pavetti et al. 2004; Wu et al. 2006). For information on sanction policies and procedures, the few implementation studies rely largely on data from administrators, from frontline workers, and to lesser extent, from clients (Fraker et al. 1997; U.S. Department of Health and Human Services 1999; Pavetti et al. 2004). These studies do not fully capture the complex mix of rules, administrative procedures, and worker discretion involved in the application of sanctions.

The current study uses a triangulated research design. The design combines a content analysis of administrative fair hearing decisions on welfare sanctions from a suburban county located in New York with in-depth qualitative interviews of sanctioned recipients, as well as with a review of the laws, regulations, judicial decisions, and administrative directives involving sanctions. Laws and administrative procedures provide structure and guidance for sanction decisions, describing what should happen during the sanctioning process. Fair hearing decisions, which are the product of administrative proceedings initiated by clients to challenge negative agency actions, provide a detailed and individualized administrative account of what occurs from the perspectives of both recipients and workers. Recipient interviews provide a less structured and less bureaucratically confined version of similar events. Together, these data shed light on how laws, administrative procedures, and workers' discretion interact in the application of sanctions.

Background and Context

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (U.S. Public Law 104-193) requires the use of sanctions to address noncompliance with a number of welfare rules, but states have the option of imposing partial or full-family sanctions. Under a partial sanction regime, only a portion of the recipient's welfare grant is reduced when an adult member violates a work rule. Under a full-family sanction regime, the entire family loses its grant for such a violation. The majority of states (36) impose some form of full-family sanction, eliminating the welfare grant to the entire family, including children, either immediately or gradually. Fourteen states, including the two states with the largest populations of welfare recipients (California and New

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York), impose partial sanctions, which eliminate only the noncompliant adult's portion of the grant (Pavetti et al. 2003).

One purpose of sanctions is to encourage compliance with work rules. They are part of what Lawrence Mead (1997, 24) calls the "help and hassle" prescription for influencing recipients' work behaviors. By requiring recipients to engage in work activities, under threat of financial penalties if they do not, self-sufficiency is encouraged rather than dependency. Although welfare reform legitimizes hassle by requiring sanctions, the welfare system has also been reconfigured to help recipients to become self-sufficient. Staff responsibilities have been restructured and new relationships forged with outside service providers, such as workforce boards or labor departments, which are knowledgeable about the labor market (Martinson and Holcomb 2002). Local service offices still make basic eligibility determinations, but they also are encouraged to function more like job centers than like welfare agencies (Lurie 2006). Welfare reform emphasizes (and provides funding for) support services, such as child care and transportation. Many programs use the social work or case management model, which emphasizes flexibility and personalized services (Segal, Gerdes, and Steiner 2004).

Under such a model, one might reasonably presume that sanctions are applied flexibly and that workers consider the client's work behavior in making individualized and holistic evaluations. Supports and sanctions also presumably are balanced; the latter imposed only on clients unwilling to participate in work activities. Both before and since welfare reform, however, scholars have noted that welfare bureaucracies are often more apt to process paper than to process people. Thomas Kane and Mary Jo Bane (1994, 7) coin the term "eligibility-compliance culture" to describe bureaucracies in which workers focus on procedural rules and paperwork rather than on the helping relationship. Before welfare reform, program efforts to encourage work demonstrated that eligibility-compliance cultures are hard to uproot. In her study of the Job Opportunities and Basic Skills program (JOBS), Evelyn Brodkin (1997) finds that agency-performance incentives, including work quotas and federal reimbursement rates, constrain workers' choices. These incentives also encourage workers to ignore clients' preferences and needs. Similarly, Yeheskel Hasenfeld (2000) finds that JOBS workers institutionalize moral assumptions and stereotypes in their daily interactions with clients, choosing to treat clients with suspicion and distrust. He also notes that workers use "highly routinized and bureaucratized" service technologies to assess clients and monitor compliance with work rules (Hasenfeld 2000, 333).

The Temporary Assistance for Needy Families program (TANF) ushered in sweeping organizational and other reforms, but the resistance to change persisted, especially among frontline workers (Meyers, Glaser,

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and MacDonald 1998; Meyers and Dillon 1999; Sandfort 2000; Lurie and Riccucci 2003; Riccucci et al. 2004). In a study that included 11 welfare sites in four states, welfare workers are found to resist organizational reforms that emphasize work-based goals, instead focusing on traditional eligibly determinations (Riccucci et al. 2004; see also Lurie and Riccucci 2003). Workers report that the implementation of TANF changed their jobs little, except for increasing their paperwork. These findings suggest that work-based goals do not replace but are grafted onto preexisting eligibility-compliance cultures. Similarly, Jodi Sandfort (2000) finds that eligibility-compliance practices even surface among some (but not all) private welfare-to-work contractors. For example, one program created elaborate tools for monitoring attendance at classroom sessions, requiring clients to sit and do nothing if classroom sessions ended early or clients completed assigned tasks.

Sanctions are similarly susceptible to routine or resistant practices. Workers verify compliance with work rules, as they do eligibility, often by completing standardized forms and asking clients to provide documents. Clients typically sign written contracts in which they agree to engage in work activities and to be monitored through reports that track, for example, attendance at work programs and Department of Labor (DOL) appointments to help them look for work. Highly scripted encounters may occur within tight administrative rule structures. As a result, sanctions may be less an evaluative task than a clerical one. For example, sanctions may be applied automatically, as in New York City's welfare bureaucracy, where computer systems are programmed to record sanctions automatically if clients do not appear at a work activity.

Discretion may also play a role in how sanctions are applied. As scholars argue, even the most rule bound of bureaucracies provide opportunities for discretion, both positive and negative (Mashaw 1971; Handler 1986; Brodkin 1997; Fording, Soss, and Schram 2007). Particularly in bureaucracies with limited resources, workers may engage in creaming, or helping those they think will succeed while treating harshly those clients they deem to be troublesome (Lipsky 1980). In the context of sanctions, such practices might mean that workers ignore technical violations, for example, a single missed meeting, if the client is perceived as cooperative. Workers may readily accept such clients' reasons for not complying, demanding only minimal proof. In contrast, sanctions may be applied differently among harder-to-serve clients, typically those with multiple problems and barriers to work. Workers may be more skeptical of their explanations and demand higher levels of proof.

There is some indication that harsh treatment from workers correlates with client disadvantage. Studies show that sanctioned recipients are younger, have more children, and are more likely to have never married than are other recipients (Fein and Lee 1999; Edelhoch et al. 2000; Koralek 2000; Westra and Routley 2000; Mancuso and Lindler 2001;

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Cherlin et al. 2002; Kalil et al. 2002; Hasenfeld et al. 2004; Pavetti et al. 2004). Sanctioned recipients are also more likely than other recipients to have health problems, including alcohol and drug problems, and to experience domestic violence (Mancuso and Lindler 2001; Polit et al. 2001; Cherlin et al. 2002; Kalil et al. 2002; Pavetti et al. 2004). Compared with clients who are not sanctioned, sanctioned clients have less human capital. Research also suggests that sanctioned welfare recipients have lower levels of education, less work experience, and longer periods of time on public assistance (Born et al. 1999; Fein and Lee 1999; Edelhoch et al. 2000; Koralek 2000; Westra and Routley 2000; Mancuso and Lindler 2001; Cherlin et al. 2002; Kalil et al. 2002; Hasenfeld et al. 2004; Pavetti et al. 2004; Wu et al. 2006). Logistical problems, such as securing transportation or child care, are also more frequent among sanctioned recipients than among nonsanctioned ones (Mancuso and Lindler 2001; Cherlin et al. 2002; Kalil et al. 2002; Hasenfeld et al. 2004; Pavetti et al. 2004).

These problems and disadvantages are difficult for workers to resolve because the issues are intractable, but progress is also impeded because workers lack necessary resources and skills (Meyers et al. 1998). Sanctions may create a disincentive for helping the hardest-to-serve clients; workers can more easily issue a sanction than provide a support (Bell 2005). In other words, sanctions support the implicit message that failure stems from a client's unwillingness to work, not from an agency that is not helping.

Alternatively, workers may exercise negative discretion broadly, choosing to apply work rules stringently to most or all clients, with little or no attempt to distinguish the unwilling from the unable or the technical violation from a more serious infraction. Enforcement of technical violations (e.g., client failure to notify the correct worker or agency if a work activity is missed) may substitute for more nuanced assessments. In short, local offices may create the welfare-to-work version of an eligibility-compliance culture. In a recent study (Lens 2006) of the implementation of sanctions in Texas, the author finds that transactions between clients and workers are routinized and mechanical. The findings suggest that workers fail to assess clients' work behaviors fully.

In sum, sanctions may be applied in several different ways. They may be issued in a standardized and highly scripted way that is determined by adherence to predetermined administrative rule structures. This may include applying sanction rules more narrowly than the law requires. Or sanctions may be applied more flexibly; workers may make individualized assessments that include an evaluation of clients' work behaviors over time and place. Finally, workers may also pick and choose among clients, letting cooperative clients who violate work rules off the hook while more readily sanctioning troublesome clients. Understanding workers' choices requires an in-depth analysis of sanction laws, admin-

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istrative rules, and procedures, as well as an examination of how such rules and procedures are applied in individual cases.

Data and Method

The front lines of the welfare system can be studied in several different ways. A common approach is to view "administration from the inside out" (Brodkin 1997, 6) by observing how workers do their jobs. Another approach is to look from the outside in, interviewing clients about how workers handle their cases. The current study incorporates elements of both approaches, using administrative fair hearing data, generated from within the bureaucracy, to observe worker behavior indirectly. It also relies on data from interviews conducted with clients in a suburban welfare office in Suffolk County, NY. Further, laws, regulations, judicial decisions, and administrative directives are analyzed to examine the legal and administrative structure for imposing sanctions.

As noted above, fair hearings are adversarial administrative proceedings initiated by clients to challenge negative agency actions, including sanctions. A detailed record emerges from the adversarial process, which includes dictates involving the presentation of evidence by the recipient and agency, cross-examination of witnesses, and issuance of a written decision in which the hearing officer summarizes each side's position, reports findings of fact, and applies relevant provisions of the law.

Hearings also provide the researcher with an administrative perspective that differs from the one gained through site observations and from that obtained in retrospective reporting on interviews or surveys of local actors (e.g., agency staff, administrators, and clients). Hearings capture, without any interference from a researcher, the actions of the parties most interested and involved in the transaction. Thus, they reflect what people did, not what they report doing, recall having done, may do, or may not do if under observation. Hearings also provide a rare opportunity to examine the same case from the perspective of both the worker and the client; they provide a he-said, she-said narrative absent from other administrative data.

As legal proceedings, hearings also provide a record, including the types of required documents, of the bureaucracy's administrative processes. Hearings thus reveal the practicalities and details of implementation. Documents are a bureaucracy's central concern, but some bureaucracies are more demanding than others. As Hasenfeld (2000) notes, bureaucracies serving clients of low social status, such as welfare recipients, frequently use harsh service technologies and treat clients with suspicion. Such bureaucracies are likely to disbelieve clients' reasons for not complying with work rules and to require high levels of proof. Scholars note that bureaucracies characterized by eligibility-compliance practices are particularly likely to make excessive and rigid de-

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mands for documents (Brodkin 1986; Kane and Bane 1994). Hearings contain a detailed record of the types and level of proof required. They thus can reveal the service technologies being used and may enable the researcher to recognize the existence of eligibility-compliance and other practices.

In addition, hearings articulate the bureaucracy's view of how the law should be interpreted and applied. Similar to judicial decisions, hearing decisions resolve individual cases by interpreting and applying laws. Thus, like judicial decisions, hearings can enable the researcher to understand particular laws and their application. Because hearings are part of the state welfare bureaucracy, hearing decisions are official pronouncements by the bureaucracy about the bureaucracy.[1] The decisions, although subject to reversal by courts, are fertile sources for information on how the members of a bureaucracy think and operate at a particular point in time.

One potential limitation of this data source is the fact that cases in the hearing process are not generally representative of New York's welfare cases; few clients appeal.[2] Researchers demonstrate in other contexts that most people do not complain about official's actions, but the complaints of the few who do typically are representative of the mass of unvoiced complaints (Hyman, Shingler, and Miller 1992).[3] Even a small number of hearings can serve as an indicator for error in bureaucracy (Altman, Bardo, and Furst 1979). Even if not wholly representative, such complaints highlight areas of contention within the bureaucracy, providing a more complex understanding of frontline interactions. The research value of hearings thus stems not only from the number of cases considered but also from their purpose and function within the bureaucracy.

The fair hearing data are supplemented here with data from interviews of clients who were sanctioned. Half of these participants reportedly did not appeal their sanctions. The perspective of welfare clients is similar to that of consumers providing feedback about a business, and recipients' perspective differs from that represented in administrative accounts. Recipients view the agency from the outside, and they often have a broader and more detailed knowledge of their circumstances than does their overworked and underresourced worker. They can provide a thicker and more detailed description of client-worker interactions than can be captured in administrative records.

Respondents were identified through purposive sampling.[4] They were referred to me by a nonprofit agency that contracts with the county Department of Social Services (DSS) to assist sanctioned clients in Suffolk County who fail to respond to their sanction by complying with the work rules. In addition, I obtained respondent referrals from several social service agencies in Suffolk County. These organizations include a legal services organization and a nonprofit agency that provides homes for homeless families. Recruitment flyers were also distributed at other

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social service agencies, including nonprofit organizations that run welfare-to-work programs and others that provide crisis intervention services. To increase the proportion of disadvantaged clients in the sample, I recruited respondents from agencies that provide crisis and homeless services. Finally, participants recruited family and friends.

A total of 28 respondents were interviewed in the spring of 2005. Interviews were conducted using a semistructured interview questionnaire. In-depth interviews allow a deep exploration of respondents' sanctioning experience and overall work behavior. The interviews also maximize personal interaction, encouraging familiarity and trust that enhance the validity of the findings (Berg 1998). To ensure respondents' privacy and confidentiality, and to create a more relaxed environment conducive to in-depth interviewing, all but two of the respondents were interviewed in their homes. Two respondents were interviewed in the offices of a nonprofit organization. Interviews lasted a little more than an hour, and respondents were paid $25. All but one of the respondents agreed to allow the interview to be tape-recorded. Respondents were asked a series of open-ended questions on their general experiences with the welfare office, prior and present work experience, and experiences and perceptions of work rules and sanctions. Each interview (except the one that was not recorded) was transcribed verbatim.

The interview sample is a mix of short- and long-term clients. Thirteen clients (46 percent of the sample) report that they have been on welfare for 2 years or less, and 15 (53 percent) report receiving assistance for more than 2 years. The interview sample is split by race as follows: 42 percent are black, 46 percent are white, and 14 percent are Hispanic. All but three sample members are female. In general, the interview sample is more educated than the overall welfare population; 48 percent of all welfare recipients in the national sample have a high school diploma or general equivalency diploma (GED), but 54 percent in the interview sample fall into this category (U.S. Department of Health and Human Services n.d.; see table 1 for sample characteristics).[5] Eleven percent have some postsecondary education.

I also analyzed laws, regulations, administrative directives, and court decisions to understand the legal and administrative structure that governs work rules and sanctions. Judicial cases are included for two reasons. First, court decisions (and especially the appellate court decisions used here) are official and binding sources for how laws should be interpreted and applied. Laws and regulations provide only a partial picture of the law; the judiciary clarifies and interprets broad or vague statutory language. Second, through their review of state administrative hearing decisions, courts determine what errors, if any, administrative agencies have made in the application of laws.[6] They are thus a useful source for exploring how the bureaucracy operates.

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Table 1

SAMPLE CHARACTERISTICS

	National*	Interview Sample	Short Term	Long Term
Race:				
Black	39	42	15	36
White	34	46	69	27
Hispanic	22	14	15	6
Education:				
Less than high school diploma or GED	46	36	31	40
High school diploma or GED	48	54	46	60
Postsecondary education	…	11	23	0
Average age (years)	31	35	33	36

NOTE.—GED = general equivalency diploma. Results are presented in percentages unless otherwise specified. All percentages are rounded. All but three sample members are female.

* National characteristics are drawn from the U.S. Department of Health and Human Services (n.d.).

In sum, multiple data sources, including administrative records, client interviews, and official legal and administrative documents are used to explore the application of sanctions. This use of multiple data sources, referred to as triangulation, enhances the reliability of the findings (Padgett 1998).

In total, 127 fair hearing decisions are analyzed. This represents all decisions on work rule violations in Suffolk County in 2002 and 2004. The decisions are examined using content analysis. First, the following data were extracted verbatim from each decision and copied onto a Microsoft Excel spreadsheet: nature of work rule violation, agency's description of violation, client's reason for not complying, and hearing officer's decision and rationale. Cases were grouped according to clients' reasons for noncompliance. The following categories were used: medical or other exemption, lack of notice of appointment date, family obstacles or situational challenges, administrative error, and scheduling conflicts with work or school. This type of "topical survey," or "manifest content analysis," Margarete Sandelowski and Julie Barroso (2003, 911) explain, "remain[s] close to those data as given" with little or no interpretation.

Each decision was also subjected to what Sandelowski and Barroso (2003, 912) describe as a "thematic survey." This method draws on empirical and theoretical literature to conduct an analysis that is interpretative and thematic. The analyses "convey an underlying or more latent pattern or repetition discerned in the data" (2003, 912) than a topical survey does. Specifically, the decisions were examined for certain organizational forms and practices related to the research questions.

Coding identifies interpretative and thematic patterns in data from the hearing decisions (Miles and Huberman 1994). Provisional codes

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were initially developed by reading through several decisions. Data are coded on two levels. The first level identifies descriptive codes, and the second level identifies thematic ones. Some examples of descriptive codes include “lacking documentation” and “counting the number of required hours worked.” Examples of focused thematic codes include “high proof,” “negative discretion,” and “rigid rule adherence.” The high proof code refers to instances in which clients’ statements were considered insufficient or dubious and third-party documentary evidence was required. The negative discretion code identifies instances in which determination of noncompliance was based on a narrow interpretation of the rules. The rigid rule adherence code is a more refined example of negative discretion. It is defined as close adherence by workers, administrators, or hearing officers to the work rules, irrespective of a client’s overall work effort or the underlying work obstacles he or she faced.

The thematic survey approach was also used to analyze the interview data. This stage of the analysis relied on HyperResearch, a computer software program designed for the analysis of qualitative data. Once again, two levels of codes were used. In the first level, interview transcripts were examined, and a descriptive code was assigned to each line of data. One descriptive code is “making mistakes.” It was used if clients described mistakes or misunderstandings related to sanction rules. In the second level, focused coding was used to examine the line codes assigned in level 1, identifying how often each code was used, assessing the importance of each line code, and choosing the codes that best categorized the emerging themes and patterns (Charmaz 2006). “Bureaucratic skepticism” is an example of a focused code that identifies situations in which the client reports a subjective belief or experience that the agency does not believe. Coding was an iterative process, in which the researcher returned to previously coded transcripts to confirm, refute, or modify codes as they developed.

Traditional legal research methods of case law and statutory analysis are employed to analyze laws, regulations, and court decisions on sanctions. Relevant court decisions on sanctions were identified through LexisNexis, a searchable electronic database of court decisions and other legal documents.

Findings

Legal and Administrative Structure

In accordance with federal and state law, recipients of public assistance in Suffolk County, NY, are required to engage in work activities. Possible work activities include regular appointments at the New York State DOL, job search activities and job fairs, some educational opportunities and

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training (e.g., GED classes), and the Suffolk Work Experience Program (SWEP). In SWEP, clients who do not find employment are required to work off their grant by performing such tasks as cleaning roads or parks or working at nonprofit and government agencies.

Failure to comply with the work rules results in the imposition of a sanction, which is a pro rata reduction of the violator's portion of the grant (New York Social Services Law, sec. 342 [2]). State law specifies a minimum duration for the penalty period associated with a sanction. The sanction period for the first instance of noncompliance is 1 month (or until the client complies, whichever is longer). The minimum duration of a second sanction is 3 months, and subsequent sanctions last for 6 months (New York Social Services Law, sec. 342 [2]). Suffolk County DSS workers impose the sanction based on information provided by the DOL and other work-related providers (e.g., the various SWEP programs). Workers rely on state laws and regulations in imposing sanctions. The local agency does not supplement these regulations with local administrative directives.

State law permits use of sanctions only if violations are "without good cause" (New York Social Services Law, sec. 342 [2] a). The statute explicitly provides that the good cause standard is met if the parent or caretaker of a child can show that child care was unavailable for a child under age 13 (sec. 342 [1]). Additional good cause exemptions are provided. These include exemptions for clients who experience domestic violence, physical limitations, and mental health limitations (sec. 341 [1] a), but the law allows departmental regulations to identify others (sec. 342 [1]). The law also provides a conciliation process for resolving sanctions and requires that failure to comply must be "willful" (sec. 341 [1] a).[7]

No state regulation further defines willful noncompliance, but the regulations provide additional detail on what constitutes good cause and how to determine it: "In determining whether or not good cause exists, *the social services official must consider the facts and circumstances*, including information submitted by the individual subject to such requirements. *Good cause includes circumstances beyond the individual's control, such as, but not limited to*, illness of the member, illness of another household member requiring the presence of the member, a household emergency, or the lack of adequate child care for children who have reached age 6 but are under age 13" (18 New York Codes, Rules, and Regulations, sec. 385.12 [2008]; emphasis added).

Thus, the regulation makes clear that determining good cause necessarily involves discretion; the facts and circumstances of each case must be considered. The regulation also explicitly permits an expansive interpretation by clearly stating that good cause is not limited to the provided examples.

New York State appellate courts have reiterated that the law requires

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that the violation be both without good cause and willful, finding on several occasions that the local agency and the state agency (in its fair hearing decisions) both failed to consider the issue of willfulness (*Benjamin v. McGowan*, 712 N.Y.S.2d 546 [2000]; *Earl v. Turner*, 757 N.Y.S.2d 255 [2003]; *Dost v. Wing*, 792 N.Y.S.2d 105 [2005]).[8] There are several reasons why the requirement of willfulness makes it difficult for the local agency to impose sanctions. First, the agency must verify whether the client's violation was willful before terminating benefits, and this adds an additional level of inquiry (*Benjamin v. McGowan*, *Earl v. Turner*, and *Dost v. Wing*). Second, willfulness and good cause are not equivalent; the willfulness standard expands the range of circumstances in which a sanction may not be imposed. For example, in *Dost v. Wing*, the court found that forgetting an appointment is not a willful act. In that case, the recipient confused the date of the appointment and contacted the agency upon realizing the mistake.

Are Sanction Rules Applied Narrowly?

The law and regulations place good cause determinations at the heart of the sanction process. Such determinations are necessarily fact specific and discretionary. They involve assessing whether the client's reason for not complying is both true and sufficient. The most common good cause explanation, both in the hearing and interview data, involves family and situational obstacles, such as a client or child's temporary illness, transportation and day care problems, and other family emergencies. These types of obstacles are raised as good cause explanations in 55 percent of the hearing cases (70 out of 127) and by 53 percent of the clients interviewed (15 of the 28). Scheduling conflicts with work or school are cited by 20 percent of clients (26 clients) in the fair hearing data and by 17 percent (5 clients) in the interview data. Thirteen (10 percent) clients from the hearing data and five (18 percent) of the interviewed clients claim that they never received notice of their appointment. Seven fair hearing clients (5 percent) and two interviewed clients (7 percent) claim that there was a clerical or other error by the agency. Eight fair hearing clients (6 percent) and four interviewed clients (7 percent) claim that they were exempt from the work rules.[9]

One way that workers and hearing officers limit the application of good cause exemptions is to refuse to take clients' explanations at face value and to demand additional proof. This routinely occurs in the hearing and interview data. In the hearing data, the hearing officer (and, by extension, the worker) almost always rejects clients' statements and testimony as a source of proof.[10] Clients won only 14 of 127 hearings, and in only five hearings was a client's testimony, standing alone, sufficient to establish good cause. To be sure, legal proceedings are designed to focus on proof as a means for ascertaining the truth. However,

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the demand for additional proof is not made because the client's testimony was inconsistent or contradicted by other evidence; this occurred in only 17 out of 127 cases. In spite of this, third-party documentation is routinely required, and clients are subjected to stiffer requirements than may be required in the workplace.

Examples of this can be found in both the interview and the hearing data. These situations often involve clients' claims of their own or a child's illness. In the workplace, routine and temporary illnesses (e.g., stomach viruses or the flu) do not necessarily require medical verification, but such verification is routinely required at the welfare center. Verification requirements work against clients because proof is sometimes not available or not practical. An interviewee named Barbara explains.[11] When she was unable to attend her SWEP assignment because her son had an intestinal virus, the agency refused to accept her statement and pressed her for a physician's note, which she was unable to obtain. Barbara recounts that the agency asked her, "Why didn't you take your baby, run to the doctor? And I said 'cause I didn't have the money to get there. And they was like, 'Well you could have took the bus.' . . . I said no. I said, 'He was sick; he had diarrhea, so I can't go on the bus . . . three buses.'"

Strict demands for documents also create problems for working clients because such demands clash with the realities of their jobs. Rules stipulate that clients must work a set number of hours per week and require verification of wages. However, clients' work hours often fluctuate, and employers sometimes insist that new employees complete a trial period in which they work a limited schedule with fewer than the mandated number of hours. Employees are sometimes paid in cash, and some employers are unwilling to provide verification of work hours.

Despite this, caseworkers refused to accept clients' statements as employment verification when they worked off the books. Other forms of proof are also rejected because they do not fit the bureaucratic mold. For example, fair hearing data include the story of a client whose employer, a restaurant owner, refused to verify her employment. She unsuccessfully tried to reverse her sanction by providing the hearing officer with letters from two people who saw her working and with case notes from the files of a local legal services attorney, who confirmed that the employer would not verify employment.

Workers also routinely interpret ambiguous or incomplete proof in the least favorable light. In a case from the fair hearing data, a client claimed that she was too ill from her arthritis to attend a work assignment. The progress notes that she produced from her physician at a clinic were deemed insufficient because they only indicate that she needed refills on her prescription and do not contain a detailed synopsis of the flare-up of her illness. In another case from the fair hearing data, a physician's note documented the client's bursitis, diabetic reticulo-

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pathy, and uncontrolled diabetes, but the note was rejected because it did not mention the cold and influenza that caused her to miss her work assignment. Although the client may have misinterpreted or exaggerated her illness, an alternative explanation not considered by workers is that short-staffed and busy medical clinics might not provide thorough documentation.

In hearings, bureaucratic records, or the lack thereof, are routinely accepted over clients' statements, testimony, and even documentary proof. The agency's records are typically viewed as incontrovertible, and little or no room is given to prove bureaucratic error. For example, hearing officers consistently find that clients' calls to workers did not occur if the case record did not mention the call.[12] In another example from the hearing data, a client produced a time-stamped document faxed to the agency, but the hearing officer ruled that it was not sufficient proof that the agency received the client's fax.

The requirement that the violation be willful is ignored in the fair hearing data and interviews. Confusion and forgetfulness are routinely interpreted as evasive behavior. A client named Katy describes the agency's response when she allegedly confused a DOL appointment with a GED test date: "'Can I get rescheduled? Am I gonna get in trouble for this?' They said, 'Yes you are; you are gonna get in trouble.' I'm like, I have all the paperwork here. I can go right now and show I've been looking for work, and I have the dates. I have this one; I have that one; my resume is in here; my resume is in there; it's not like I'm not trying."

The hearing data make an even more explicit case that confusion and forgetfulness are not deemed to be valid reasons for missing appointments. Hearing officers routinely reject such explanations as insufficient. In one illustrative example, the hearing officer acknowledged the credibility of the client's testimony that she forgot the appointment but explained, "It does not constitute a valid reason, as appellant is responsible for keeping track of her appointments." In another case, the client received two letters from the agency close in time. She assumed (incorrectly) that the second letter was to notify her of a change in the appointment. When she appeared on the second date and at the wrong office, she was told that she could not be helped because she missed her first appointment, and she was sanctioned. The hearing officer upheld the sanction, noting that the "confusion over two appointments arose from her failure to safeguard agency correspondence."

Similarly, unintentional acts are construed in negative ways. One illustrative example involves a client who overslept on the day of a physical examination that was required as part of her vocational training to become a certified nursing assistant. Although she completed the physical examination 10 days later, she was sanctioned and not permitted by the agency to complete the course. Upholding the agency's action,

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the hearing officer explained that “the reason for the missed appointment, specifically that the appellant overslept, does not constitute good cause.”

Clients’ miscommunications with workers and confusion over administrative procedures are also rejected as valid reasons for failing to comply. In one example from the hearing data, a client believed he was not required to report to the DOL for an employment assessment until his physician completed a required medical assessment form. In another example, a client did not report to the assigned work site because she was not approved to receive a voucher for her child’s child care; she mistakenly assumed that, because she received child care in the past, a new application was not needed. In both cases, the hearing officers rejected these explanations, noting that the clients had been properly notified of their obligation to report.

In sum, although clients may not have reported honestly to their workers or in hearings, it is also the case that common mistakes, confusion, and unintentional acts are interpreted with suspicion and distrust. Some legal requirements, such as that the violation be willful, are ignored. Other legal provisions, specifically good cause exemptions, are construed narrowly. The latter is accomplished by privileging agency records over clients’ accounts, imposing strict standards of proof, routinely requiring third-party documentation, and parsing documents in ways detrimental to the client.

Are Sanctions Applied in a Manner That Subverts Policy Goals?

The goal of sanctions is to ensure client compliance with work rules. The goal of work rules is to help clients achieve self-sufficiency. However, the data suggest that a narrow focus on rules and procedural compliance undermines these goals. Procedural violations are elevated over substantive outcomes, such that the reporting of work-related events takes on more significance than the event itself. As a consequence, sanctions are imposed even if clients are engaged in ongoing work activities.

The reporting rules are specified in documents provided to all clients. These rules require clients to attend work assignments, DOL appointments, and other activities. In addition, clients are required to notify designated agencies of certain events or problems. For example, clients are required to sign a form in which they agree to contact the DOL if they are terminated from a SWEP assignment. They are also required to notify the DOL in advance if they cannot attend an appointment.

The reporting responsibilities invite workers to avoid complex assessments of clients’ willingness to work and instead to ask the much simpler question of whether the proper parties were notified of a missed activity. For example, interview data indicate that several clients abruptly moved to new shelters, some as far as 50 miles away, and were unable

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to attend their SWEP assignments because they lacked transportation. They were sanctioned for not notifying the DOL of the transportation problem. One client explained that the agency moved her to a new shelter, and she therefore assumed (incorrectly) that only her work site supervisor needed to be notified. The agency, however, viewed her failure to notify the DOL as a clear violation of the reporting requirement, and this determination was made despite the fact that the client had a valid reason for not attending.

In another example from the interview data, the agency is reported to have viewed the client's underlying reason for not attending a DOL appointment as less important than whether and to whom she reported it. Jessica, who was pursuing a medical assistant's certificate on her own while attending her SWEP assignment, had a scheduling conflict when her monthly DOL appointment was to take place at the same time as an important test. She chose to take the test, but the DOL had no record that she notified it (a point of contention). She was sanctioned despite her ongoing attendance at the work site and proof from her school of the test date.

Work rules that require a set number of weekly work hours have the anomalous effect of obscuring clients' work efforts. As noted above, the uneven availability of hours that often characterizes low-wage work creates compliance problems for clients. As the fair hearing and interview data reveal, employers may at first employ workers at reduced hours and increase hours over time. Further, home health aide assignments may be canceled, or restaurant hours may fluctuate, leaving clients sometimes short of the hours required. Rather than viewing such circumstances as outside the clients' control, and hence as good cause for not fully complying with work rules, hearing officers adhere strictly to the rules, upholding sanctions if clients fall a few hours short. The work mandate's overall goal, employment, is overlooked or not considered. In one such case, the client was sanctioned when she left her SWEP assignment on a Tuesday to begin a full-time job on the following Sunday. The hearing officer upheld the sanction, noting that the client should have continued in her SWEP assignment until the first day of her new job. That she had secured employment was less relevant than the hours she missed in her transition from SWEP to work.

In sum, a technical, narrow, and often reductive application of the rules obscures clients' work efforts. To be sure, the rules themselves, including rules that require clients to work a certain number of hours per week, direct a certain result. However, rules defining good cause and requiring willfulness permit variation in determining sanctions for individual cases. The choice of rigidity over flexibility when both are possible is characteristic of eligibility-compliance cultures.

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Are Heterogeneous Groups of Clients Sanctioned?

Another mark of an eligibility-compliance culture is a lack of individualized attention to and differentiation among clients (Kane and Bane 1994; Brodkin 1997). Under such a regime, sanctioned clients would likely be diverse, and different types of clients would be caught up in a bureaucratized sanctioning process that treated most, if not all, clients with suspicion and skepticism.

As the interview data reveal (the fair hearing data do not include demographic data), sanction recipients are a diverse mix of short- and long-term clients who have varying levels of work history and education. Two groups of clients emerge: short-term clients who are job ready and willing to work and long-term clients with multiple disadvantages. Contrary to expectations, both types of clients are sanctioned, and there is no discernible distinction between the two. Ann and Carol exemplify this heterogeneity. They have very different backgrounds, routes to welfare, and motivations toward work, but they share similar sanction histories.

Carol, 38 years old, has a long work history that began when she was 16 and continues through the births of her five children. As she says in describing her return to work just 2 weeks after her youngest child's birth, "I never saw her take her first steps. . . . It was what I had to do to take care of her. To survive, that's what you had to do." She worked as a paralegal for over a decade but lost her job when her employer closed his law office. Because she lacks the required college degree, she could not find employment as a paralegal. Unable to pay the rent, she became homeless. She first applied for welfare at age 37. At the time of the interview, she had been on public assistance for a little over a year. She had a clear plan for leaving welfare; she was pursuing a college degree in adolescent psychology and, to comply with the work rules, was also working as a hostess in a catering company. She was sanctioned several times for failing to attend DOL appointments, which often conflicted with her other obligations.

In contrast, Ann applied for welfare as a teenager and, by age 22, had been on assistance for 4 years. She has three children and is pregnant with her fourth. A high school dropout, she earned her GED and became a certified nursing assistant while on public assistance. Despite earning $14.50 an hour as a nursing assistant, she resists more than part-time work and cuts her work hours to remain eligible for welfare. Ann repeatedly fails to attend her work assignment, claiming she is stymied by red tape, such as by problems in obtaining the correct form for a child care referral or by miscommunications with the work site.

Carol and Ann are representative of other clients interviewed. One group, composed typically of short-term or transitory welfare users like

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Carol, works around the work rules, pursuing educational or work opportunities independently. Interview data suggest that their sense of self-efficacy is high and that they have concrete plans. For example, Jessica, a 33-year-old mother with three children, was on assistance for 1 year and has an extensive work history. She pursues her own plan to increase her earning potential by obtaining a medical assistant's certificate. Because the certificate program does not satisfy the work rules, she also has a SWEP assignment to work at a local social service agency. She made an arrangement with her social service agency supervisor to attend her assignment after school, putting in a day that starts early in the morning and ends at 9:00 at night. She was sanctioned when she missed a routine DOL appointment because it conflicted with a test at school.

Another group of clients includes long-term users who have multiple barriers to work. These clients resist their work assignments, are often in crisis, and find compliance difficult. For example, Gladys, a 28-year-old mother with three children, was on welfare for 6 years and dropped out of school in the tenth grade. She refused a work assignment, explaining, "I don't see myself getting up early in the morning to go down there for all them hours, 35 a week, while everybody gets a paycheck and not me. That doesn't make sense." She was sanctioned repeatedly and claims that various barriers prevent her from complying. She described herself as "used to the sanction."

Despite their obvious differences, these short- and long-term clients have similar sanction histories. Missed appointments at the DOL and the failure to attend SWEP assignment are the most common reasons. The heterogeneity of the clients suggests that workers bureaucratize sanctions, applying them without distinguishing among the types of violations or levels of client work efforts.

Limitations

This study, like much implementation research, is limited to the sites being studied. This is an especially important limitation in a study of welfare because states have great flexibility in program design and administration. New York is unlike many states in that it imposes only partial sanctions. Workers in the state may apply sanctions differently than do workers with access to the harsher full sanctions. Also, Suffolk is a suburban county; the findings may not be applicable to rural counties with less complex bureaucracies or to larger and more complex bureaucracies located in urban areas.

This study is limited to examining exercises of negative discretion. Positive discretion is difficult to capture. It occurs in cases where workers have a basis to impose a sanction (e.g., a missed work activity) but do not. Such acts do not generate formal notices or appeals and are not likely to be represented in administrative records. Clients are not nec-

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essarily aware of instances in which workers chose not to sanction them. To be sure, the forms of negative discretion described herein, and their application to a heterogeneous group of clients, suggest a particular type of culture often identified with welfare bureaucracies and, hence, are likely not exceptional. Although this study is useful in establishing the forms that negative discretion can take, further studies are needed. Further research may benefit from a different methodological approach that focuses on the use of positive discretion in sanctioning.

The study's findings on the heterogeneity of the sanctioned population are also limited. As noted above, the findings are based solely on the interview data. Although clients were recruited from diverse sources, the sample size is small. This sample differs from those in other studies, noted above, which indicate that the likelihood of receiving a sanction increases with the client's level of disadvantage. Such heterogeneity may be a result of the way sanctions are applied in Suffolk County, and it may be difficult to generalize findings from studies of individual sites. Further research is needed to determine how sanctions affect different subpopulations of welfare recipients.

Discussion and Implications

The shift in emphasis from cash assistance to work programs raises the potential for fundamental change in the culture of welfare agencies. Kane and Bane (1994) reject an emphasis on eligibility verification and compliance, instead advocating creation of a self-sufficiency culture in which "clients and workers [would engage] in the common tasks of finding work, arranging child care, and so on" (2). However, Mead (1997, 24) aptly describes welfare reform as a mix of "help and hassle." It mixes the potential to help clients with the ability to punish them.

New York's statutory and regulatory framework reflects this dichotomy. Although it requires sanctions, it also leaves ample room for individualizing them. The regulatory language allows for an expansive interpretation of good cause provisions, and the statutory requirements that violations be willful give workers considerable leeway. Discretion, though, can be exercised negatively or positively. As Richard Fording and colleagues (2007, 291) observe, "Discretion may serve as an entry point for unjust and unequal treatment or, alternatively, may permit the tailoring of more equitable and humane responses."

This study's findings identify the forms that negative discretion can take. Both workers and hearing officers hew to a narrow interpretation of good cause while also making that standard difficult to meet. An ingrained skepticism of clients' explanations leads workers and hearing officers to dismiss clients' proof and to see intentionality where others might recognize forgetfulness or confusion. Sanctions are not individualized but applied broadly. In doing so, workers use bureaucratic short-

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cuts that avoid a full assessment of clients' work efforts. Process is exalted over substance, catching a diverse group of clients in the net of sanctions. In sum, workers replicate the eligibility-compliance style of the past and impose the harsh service technologies typically reserved for low-status clients.

There are several possible explanations for the patterns described here. One lies in the enormity of the task of welfare agencies. Helping clients achieve self-sufficiency is difficult; economic conditions and human capital are equally relevant, if not more so, than welfare center practices (Fang and Keane 2004). The hardships endured by the welfare poor, including high rates of illness and disease, poor housing, inadequate educational systems, and the lack of everyday supports that facilitate work, are well documented (see, e.g., Rank 1994; Edin and Lein 1997; Seccombe 2007). Accordingly, an agency focus on documents and procedural violations, the hallmark of an eligibility-compliance culture, is less complicated than a focus on helping clients obtain self-sufficiency. Rather than helping clients to access support services, or acting after supports fail, sanctions can be used to assign blame and to absolve the worker of any responsibility for a client's failure to achieve self-sufficiency.

On an organizational level, the location of employment services at the DOL instead of the DSS may affect how workers exercise their discretion. As Irene Lurie and Norma Riccucci (2003, 674) explain, "Where responsibility for work activities was transferred to a specialized agency, there was little need for the welfare agency to become, in Moynihan's words, an employment and training program that provided income support. Workers in the welfare agency could continue to focus primarily on cash assistance, food stamps, and Medicaid, paying considerable attention to eligibility and compliance." Sanctions become an eligibility task, performed the same way as other eligibility tasks. Process prevails over substance, and standardization over individual assessments.

Despite its emphasis on support services and organizational flexibility, welfare reform may result in less change than is commonly assumed. As Brodkin (1997) observes, workers' choices about whether to apply rules strictly or generously are shaped by agency incentives and procedures. Welfare reform includes such incentives as work participation rates, mandated weekly work hours, and restricted definitions of work activities. These incentives encourage workers to routinize their work. The work-first approach, which calls for immediate labor market attachment, encourages depersonalized service (Anderson 2001). Virtually all of the clients interviewed are engaged in the same trajectory of work activities, from routine DOL appointments to job search activities, to approved jobs, to SWEP assignments. This similarity invites the standardization and bureaucratization of sanctions. It encourages workers to judge the part, not the whole; a missed DOL meeting is a missed

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meeting, regardless of the other work activities of the client. Likewise, if judged by the single standard of a missed meeting, a short-term, job-ready client is no different than a long-term disadvantaged client.

Finally, and perhaps most significantly, workers may be doing what is expected of them, both by the welfare organization and by the policy makers responsible for the bureaucratic structure. As Michael Lipsky (1984) recognized decades ago, there are many ways to disentitle citizens. One way is through legislative schemes that reduce benefits; another is through "bureaucratic disentitlement," or the denial of aid to eligible people through "largely obscure 'bureaucratic' actions and inactions of public authorities" (Lipsky 1984, 3). Lipsky suggests that bureaucratic disentitlement is likely to occur in the absence of legislative change as a way to resolve often hidden societal conflicts concerning the distribution of benefits.

However, disentitlement can also occur in tandem with legislative change and can resolve conflicts within the law. One potential conflict is welfare reform's simultaneous injunctions to help clients with an unprecedented array of supports and also to sanction them. Conflict arises if law and regulations leave unclear which, support or sanction, is appropriate. Likewise, the statutory scheme reinforces and amplifies negative tendencies to view welfare clients homogeneously as unwilling to work, even as the scheme requires individualized and complex judgments of whether work rules violations are willful and without good cause. Thus, although the 1996 welfare reform legislation is aptly described as a punitive and harsh, it also incorporates values of equity and fairness. Courts may uphold provisions designed to ensure fairness (as one did in requiring a New York agency to determine willfulness), but the welfare bureaucracy, both past and present, is highly attuned to the harsh and punitive features of the system. In narrowly interpreting good cause, and even in ignoring the law regarding willfulness, the bureaucracy expands by administrative means welfare reform's harshest provisions.

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Notes

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1. The Office of Hearings and Appeals, a component of the New York State Office of Temporary and Disability Assistance (NYOTDA), has supervisory responsibility over all hearings in the state. The state is divided by regions, consisting of several counties, and hearing officers are assigned to particular counties within these regions. The NYOTDA state commissioner's designee for each region makes the final decision on individual cases. This decision is based on the hearing officer's findings of fact and law, as well as on the officer's recommendations. Because the hearing system is partially decentralized, hearing outcomes may vary and therefore are more representative of the practices of a particular region or county than of those across the entire state.

2. Appeal rates for financial assistance programs are between 1.1 and 3.5 percent in New York State, outside of New York City (Lens and Vorsanger 2005). Although appeal rates are low outside the city, the rates are higher than the sample size used to assess error rates under quality control (1.5–2 percent according to Wrafter [1984]). In any event, the available administrative data do not permit me to estimate what portion of appeals are related to sanctions, but findings from a recent study (Brandwein et al. 2000) suggest that it may be unusually high. The study, based on surveys of 174 current or former welfare recipients who sought assistance at nonprofit agencies in Suffolk County, finds that almost one-third, or 51 recipients, were sanctioned at some point, and 38 (74 percent) of those sanctioned appealed the sanction by requesting a hearing (Brandwein et al. 2000). Although the study's results are biased by the failure of some recipients to complete the survey (and hence perhaps also the likely failure to appeal), the results still demonstrate an unusually high rate of appeal. In the current study, interviews with the sample of clients also indicate that participants appealed at high rates. Of the 28 clients interviewed, 14 (50 percent) report that they appealed a sanction. The sampling was designed to distinguish clients who appealed from those who did not, and the former were surprisingly easy to identify across the range of referral sources used to recruit participants for interviews.

3. As Hyman and colleagues (1992) explain in their study of the complaint behavior of residential utility customers, if organizational and environmental barriers inhibit complaints, "the universe of problems perceived, voiced, and complained will be successively

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smaller than the universe of problems experienced by consumers" (100). The complaints are more likely to be the tip of the iceberg than to be aberrations.

4. Clients were recruited as part of a larger study that focuses on sanctions. The study examines what motivates clients to use the fair hearing system or impedes them from using it. Thus, purposive sampling is used to identify sanctioned clients who had, or had not, used the fair hearing system.

5. Because sampling was not random, the interview sample may not be representative of the general sanction population in Suffolk County. The only available demographic information on the county's sanction population comes from a study conducted jointly in 2003 by Suffolk County DSS and the Education and Assistance Corporation (EAC), a nonprofit agency that provides services to sanctioned clients under an agreement with DSS (Suffolk County Department of Social Services 2003). The study was based on data from the 489 sanctioned clients who were referred by DSS to EAC for assessment and services in 2002. Clients are referred to EAC if they fail to come into compliance after being sanctioned. Thus, the study does not include all sanctioned clients. It focuses on a subpopulation of sanctioned clients that may or may not be more disadvantaged than the overall sanctioned population. Although sanctions may not compel some clients to comply because they have other sources of income and are willing to accept a grant reduction, others may be unable to comply because of multiple barriers. In any event, the sample in this study differs from the sample in the sanction study. The primary differences relate to the level of education. The current study sample reports higher education levels than those reported by the Suffolk County study population (Suffolk County Department of Social Services 2003). In the current study, 54 percent report that they have a high school diploma or GED; the rate was 34 percent in the Suffolk County study. Overall, 42 percent of the current study's sample is black, 46 percent is white, and 14 percent is Hispanic. In the Suffolk County study, 50 percent were black, 28 percent were white, 17 percent were Hispanic, and 5 percent identified themselves as members of some other group. The average age of the current sample is 35; it was 32 in the Suffolk County study.

6. Administrative hearing decisions are reviewable by the state courts through a proceeding filed under article 78 of the New York Civil Practice Law and Rules. The standard of review is whether the decision is based on substantial evidence or is arbitrary, capricious, or an abuse of discretion (New York Civil Practice Law and Rules, sec. 7803).

7. The purpose of conciliations is to provide clients with an additional opportunity to present their reasons for noncompliance before their grant is reduced. The local agency is required to offer conciliations, but participation is voluntary and does not affect a client's right to request a fair hearing.

8. That the failure to consider the issue of willfulness, despite a statutory mandate, was widespread seems to be indicated by a General Information System (GIS) notice sent in September, 2005, by the NYOTDA (2005) to all welfare districts. It advised districts that, in accordance with the court decisions in *Earl v. Turner* and *Dost v. Wing*, they must revise their sanction procedures to include a determination of willfulness. The GIS notice emphasized that such determinations must be made on a case-by-case basis and should include "identifying a pattern of the recipient's failure to take reasonable steps to address issues within the recipient's control that may prevent the recipient from complying with employment requirements" (NYOTDA 2005, 1).

9. In the results presented for noncompliance among the interviewed clients, the sum of the percentages exceeds 100 because several clients had multiple sanctions and therefore provided multiple reasons for noncompliance.

10. The evidence submitted at the hearing is sometimes, but not always, different than the evidence submitted to the agency when the sanction is first imposed. The agency has the option of withdrawing the sanction upon examining the client's evidence at the hearing. No statistics are available on the frequency with which the agency exercises this option in sanction cases, but the agency withdraws its notice in about one-third of all hearing cases in Suffolk County. Both through the initial imposition of the sanction and by not withdrawing, the agency rejects the client's proof. Thus, for the purpose of this analysis, the hearing officer's and the agency's standards of proof are viewed as interchangeable if the sanction is upheld.

11. To protect the confidentiality of the participants, all names in this article are pseudonyms.

12. The hearing officer found otherwise in only one observed case. In that case, the

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recipient submitted personal telephone records indicating that she placed a call to the DOL and further evidence that not all phone calls from clients to the DOL were logged in case records.

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Deciding to Discipline: Race, Choice, and Punishment at the Frontlines of Welfare Reform
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Deciding to Discipline: Race, Choice, and Punishment at the Frontlines of Welfare Reform

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Welfare sanctions are financial penalties applied to individuals who fail to comply with welfare program rules. Their widespread use reflects a turn toward disciplinary approaches to poverty management. In this article, we investigate how implicit racial biases and discrediting social markers interact to shape officials' decisions to impose sanctions. We present experimental evidence based on hypothetical vignettes that case managers are more likely to recommend sanctions for Latina and black clients—but not white clients—when discrediting markers are present. We triangulate these findings with analyses of state administrative data. Our results for Latinas are mixed, but we find consistent evidence that the probability of a sanction rises significantly when a discrediting marker (i.e., a prior sanction for noncompliance) is attached to a black rather than a white welfare client. Overall, our study clarifies how racial minorities, especially African Americans, are more likely to be punished for deviant behavior in the new world of disciplinary welfare provision.

Disciplinary approaches to poverty management are ascendant in the United States today. They are perhaps most visible in the area of criminal justice, where tough new policies have driven incarceration rates to levels that are unprecedented in U.S. history and unrivaled by other nations (Western 2006). Yet mass incarceration is far from an isolated development (Starobin 1998). Welfare policies for the poor have been redesigned in recent years to reflect the idea that the state has a legitimate interest in ensuring that socially marginal groups practice appropriate behaviors (Schram 2006). Today, public aid programs are more directive in setting behavioral expectations, more supervisory in monitoring compliance, and more punitive in responding to infractions (Mead 1997). "New paternalist" welfare programs use a variety of incentives, surveillance mechanisms, and restrictive rules to modify client behaviors. A system for dispensing punishments when incentives and rules prove insufficient serves as a failsafe in this process. Sanctions—penalties that reduce or terminate benefits in

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response to client noncompliance—are the ultimate expression of welfare's disciplinary turn and deserve the greatest credit for the exodus of clients from welfare since the mid-1990s (Rector and Youssef 1999).

This article investigates welfare administrators' decisions to sanction clients, focusing on how the interplay of racial status and character markers shapes such decisions. Our analysis falls into a long tradition of sociological research on the ways that major societal institutions produce racial disparities. Using observational and audit designs, researchers in recent years have revealed stark disparities in the ways black and white Americans are treated in retail transactions (Ayres and Siegelman 1995), the mortgage loan industry (Munnell et al. 1996), the insurance industry (Wissoker, Zimmermann, and Galster 1998), the healthcare sector (Schulman et al. 1999), housing markets (Yinger 1995), and labor markets (Bertrand and Mullainathan 2003; Pager 2007). Despite the broad scope of this research, however, there are few systematic studies of how race matters when government agents use their distinctive authority to punish. The major exception, of course, is the literature exploring the origins and consequences of racial disparities in arrest, sentencing, and incarceration patterns (Pager 2007; Western 2006). It is unclear, however, how findings in the criminal justice domain might generalize to the welfare domain or to other policy areas.

We shift the analytic focus on punishment, in Bourdieu's (1998) terms, from the "right hand of the state" to the "left hand of the state"—or more precisely, from criminal justice to social welfare provision. It is well established that racial attitudes influence public preferences regarding welfare policy (Gilens 1999) and that state-level welfare policy choices closely track the racial composition of welfare caseloads (Soss et al. 2001). But welfare case managers work directly with clients in concrete organizational settings; as a population, they are quite unlike the "average" citizen or state legislator. Their decisions to punish specific individuals and families are a far cry from expressions of general policy preferences or choices among state policy alternatives.

Although there is a significant ethnographic literature on race and welfare case managers (Bonds 2006; Watkins-Hayes 2009), its insights have had little impact on econometric studies of sanctioning, where the actions of frontline workers are bracketed in favor of attention to client and community characteristics (Pavetti, Derr, and Hesketh 2003; Wu et al. 2006). Our study bridges these two streams by asking how client race influences caseworker decisions to apply penalties. We break new ground, first, by presenting rigorous empirical tests of racial bias in welfare sanction decisions and, second, by presenting an explicit cognitive model that explains how client race exerts an influence on officials' decisions to punish.

To theorize the influence of race, we draw on the Racial Classification Model (RCM) of policy choice, which we developed and tested on state-level policy choices elsewhere (Soss, Fording, and Schram 2008). Drawing on models of implicit racism (Quillian 2008), the RCM specifies conditions that can produce policy-based racial disparities even in the absence of racial animus or discriminatory intent. In this article, we test hypotheses derived from the RCM's assertion that racial disparities become more likely when policy targets possess discrediting traits consistent with negative group reputations. Extending the work of Pager (2007), we show how stereotype-consistent markers can provide "expectancy confirmation" (Darley and Gross 1983) and, hence, strengthen the link between racial status and policy treatment (Correll, Benard, and Paik 2007). As Duster (2008) notes, much of the evidence for implicit racism comes from artificial research settings; there is a pressing need for empirical tests in real-world settings and for models that explain how implicit cognitive biases translate into concrete policy outcomes.

To meet these goals, we test propositions from the RCM using both experimental and administrative data. Our experimental data come from a Web-based survey of Florida Welfare Transition (WT) case managers. Respondents were presented with realistic rule-violation vignettes in which key client characteristics were randomly assigned. Case managers were then asked whether they would impose a sanction in response. To our knowledge, no prior sanctioning study has employed this approach, which offers the advantages of causal inference associated with experiments (Kinder and Palfrey 1993) while remaining close to the phenomenon of study: decisions made by actual case managers.

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Despite the advantages of this survey-experimental design, hypothetical vignettes and the limitations of our sample both counsel some humility regarding these data. Accordingly, we triangulate our findings with administrative data from the Florida WT program. While these data offer a weaker basis for causal inference, they have the benefit of reflecting decisions made on actual cases under normal working conditions.

SANCTIONS AND WELFARE REFORM

The use of sanctions to enforce client compliance predates welfare reform. Prior to 1996, however, sanctions were used infrequently and were applied only to the benefits of the adult head of a household, not the entire family (Bloom and Winstead 2002). Sanctions became far more important under the Temporary Assistance for Needy Families (TANF) program because national welfare reform legislation in 1996 specified stricter work requirements, set narrower exemption criteria, made a broader scope of behaviors subject to sanction, and gave states more options in designing penalties (Hasenfeld, Ghose, and Larson 2004). Perhaps most important, state TANF programs now have to meet specific quotas for the percentage of recipients participating in work-related activities. Sanctions are the primary disciplinary tool available to administrators as they seek to motivate client behaviors to meet these quotas. Sanctions are most often applied when recipients fail to complete required hours of participation in countable work activities such as job search, job-readiness classes, vocational education, training, community work, and paid employment.

State sanction policies vary considerably (Bloom and Winstead 2002). To capture this variation, Pavetti and colleagues (2003) offer a simplifying typology. Seventeen states rely on the strictest combination of choices, enforcing an "immediate full family sanction." In these states, the entire TANF family is removed from the rolls at the first instance of noncompliance. Eighteen states use a "gradual full-family sanction," which potentially has the same effect, but only after continued noncompliance. The remaining states enforce a "partial sanction" of benefits (usually reducing only the adult portion of the grant).

There is extensive evidence that sanctions have played a key role in transforming welfare from a system focused on cash benefits to one focused on enforcing work (see Pavetti et al. 2004). Between 1997 and 1999, nearly 500,000 families lost benefits due to sanctions—approximately one quarter of the caseload reduction for that period (Goldberg and Schott 2000). Indeed, the states with the strictest sanctions experienced caseload declines as much as 25 percent greater than those reported by states with the least stringent sanctions (Rector and Youssef 1999). Some suggest that the threat of sanctions may be responsible for even greater numbers of recipients leaving the rolls (Lindhorst, Mancoske, and Kemp 2000). Moreover, studies suggest that being sanctioned significantly increases hardship among recipients (Reichman, Teitler, and Curtis 2005; Stahl 2008).

Together, these studies suggest the vital importance of understanding how sanction decisions actually get made at the frontlines of welfare reform. Direct responsibility for these decisions falls to individual caseworkers who must respond to individual cases. Such workers have long held discretion in dealing with their clients (Lipsky 1980; Maynard-Moody and Musheno 2003), but welfare reform has given them a variety of new powers and responsibilities (Watkins-Hayes 2009). To understand how race matters for punishment in the new world of welfare, one must theorize and investigate case-manager decision making.

THE RACIAL CLASSIFICATION MODEL AND DECISIONS TO PUNISH

Our subject stands in the shadow of a long historical relationship between race and welfare provision in the United States (Lieberman 1998; Quadagno 1994). Many studies cast doubt on the idea that contemporary welfare reform represents a break with this troubled history (Neubeck and Cazenave 2001; Schram, Soss, and Fording 2003); race-coded appeals and racialized public responses played a key role in the national debates leading up to reform (Hancock 2004; Reese 2005). Under devolution, the racial composition of welfare caseloads has been a strong predictor of state choices regarding welfare rules and governance arrangements (Soss et al. 2001; Soss et al. 2008). Implementation studies suggest that client race

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may affect caseworker decisions in sanctioning and other areas (Goldrick-Rab and Shaw 2005; Gooden 2003; Kalil, Seefeldt, and Wang 2002).

But how and why should client racial characteristics influence decisions to sanction welfare recipients? To answer this question, we draw on the RCM. The RCM offers an explanation for how race and ethnicity influence individuals' policy choices in an era in which *de jure* discrimination is outlawed and egalitarian norms are widely endorsed. Drawing on theories of implicit racism (Quillian 2008), the RCM asserts that racial disparities in policy domains can emerge from cognitive biases in decision making even in the absence of conscious racial animus, out-group threat, or in-group favoritism (cf. Blalock 1967; Key 1949).

An extension of the theory of target populations (Schneider and Ingram 1997), the RCM focuses on how social classifications and group reputations guide decisions about how to design and implement policy tools. The model consists of three basic propositions, which we explain in detail elsewhere (Soss et al. 2008). We briefly review them here to clarify the basis of our hypotheses.

1. To be effective in designing policies and applying policy tools to specific target groups, policy actors must rely on salient social classifications and group reputations; without such classifications, actors could not bring coherence to a complex social world or determine appropriate action.

Policy approaches that work for one group might fail for another. Thus, attempts to choose effective policy actions inevitably depend on beliefs about "what kind" of group or individual one is seeking to influence. The first premise of the RCM holds that policy actors rely on social categories to make complex target groups more interpretable and, when making policy choices, draw on social-group reputations as proxies for more detailed information about these targets.

2. When racial minorities are salient in a policy context, race will be more likely to provide a salient basis for social classification of targets and, hence, to signify target differences perceived as relevant to the accomplishment of policy goals.

The salience of race varies across policy domains, time periods, and political jurisdictions. All else being equal, we expect race to become more salient in a policy context as racial minorities figure more prominently in policy-relevant political events, media discourses, and target-group images. Under such conditions, the social classifications that guide policy actions—whether aimed at groups or individuals—are more likely to be based on racial categories. Welfare policy offers a case in point (Gilens 1999; Schram et al. 2003).

3. The likelihood of racially patterned policy outcomes will be positively associated with the degree of policy-relevant contrast in policy actors' perceptions of racial groups. The degree of contrast, in turn, will be a function of (1) the prevailing cultural stereotypes of racial groups, (2) the extent to which policy actors hold relevant group stereotypes, and (3) the presence or absence of stereotype-consistent cues.

It is the contrast between group reputations that allows racial markers to underwrite broad assumptions about individual clients and target groups. The third premise of the RCM suggests that such contrasts depend on differences in racial groups' cultural images, differences in how policy actors internalize these images, and proximate cues that can evoke racial stereotypes and suggest whether a particular policy target fits the profile of a racial-group's reputation.

The RCM offers a clear basis for expectations regarding race and sanction decisions. We conceptualize sanctions as tools for motivating clients, stimulating work efforts, and enforcing responsible behavior. Accordingly, case managers should be more likely to apply sanctions when clients are perceived as less motivated and responsible—that is, when clients are perceived as needing a stronger push to follow rules and to achieve welfare-to-work goals. Client race should thus affect sanction decisions to the extent that group reputations suggest differences in motivation, work effort, and responsibility. Race-based sanction disparities should increase when the contrast between reputations is larger; disparities should be weaker and less consistent when the contrast is smaller. Finally, race-based disparities should be more likely to emerge when proximate cues link clients to group reputations in ways that highlight policy-relevant contrasts.

Like other students of implicit racism, we are particularly interested in the potential for discrediting markers to cue group reputations in ways that produce racial disparities (Quillian 2008). Two individuals seen as belonging to a

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single racial group may nonetheless be associated with quite different group images. Research on intersectionality, for example, emphasizes that the meaning of any category of social identity will likely change when combined with another (e.g., when woman is combined with black as opposed to white [see Crenshaw 1991; Hancock 2007]). Likewise, social cognition research shows that perceivers tend to distinguish "subtypes" of racial groups based on additional characteristics (e.g., "ghetto blacks" versus "black businessmen") and to attribute global group traits to these subtypes to very different degrees (Devine and Baker 1991; Richards and Hewstone 2001). As a result, race-of-target effects are often contingent on additional characteristics that strengthen or weaken an individual's connection to a racial group's prevailing reputation.

Eberhardt and colleagues (2006), for example, find that black defendants convicted of killing white victims are more likely to receive the death penalty if they are perceived as having a "stereotypically black appearance." Likewise, Pager (2007) finds that black job applicants are significantly more disadvantaged than their white counterparts by having a felony conviction on their records. Conversely, in their study of black political candidates, Valentino, Hutchings, and White (2002:86) find that "when the black racial cues are stereotype-inconsistent, the relationship between racial attitudes and the vote disappears.... The effect emerges only when the pairing of the visuals with the narrative subtly reinforces negative stereotypes in the mind of the viewer."

The RCM predicts that when minority clients possess discrediting traits consistent with minority stereotypes, they will be sanctioned significantly more often than (1) clients from all racial groups who lack the discrediting trait and (2) white clients who share the discrediting trait. The RCM also predicts that this effect will be greater for blacks than for Latinos because group stereotypes regarding work effort and personal responsibility are more negative in the case of blacks (Fox 2004; Gilens 1999). To test these hypotheses, we make use of two client traits: one evokes the image of single mothers having children while engaging in long-term welfare dependency (see Hancock 2004); the other evokes images of a preference for living off welfare rather than pursuing the hard work of paid employment (see Gilens 1999).

SURVEY EXPERIMENTAL DESIGN

Our experimental data come from a Web-based survey of Florida Welfare Transition (WT) case managers with sanctioning authority. The Florida WT program relies extensively on sanctions to enforce client compliance, producing one of the highest sanction rates for any state in the nation (Botsko, Synder, and Leos-Urbel 2001). Indeed, a recent analysis of sanctioning in Florida finds that the sanction rate for an entering cohort averages nearly 50 percent after 18 months (Fording, Soss, and Schram 2007). Florida is also one of the most racially diverse states in the country, with sizeable black and Hispanic populations, and the state's TANF population displays even more diversity. Between January 2000 and March 2004, 36.2 percent of TANF adults were black, 33.7 percent were white (non-Hispanic), and 28.5 percent were Hispanic. Given its reliance on sanctions and its diverse client population, Florida is an ideal state for examining the role of race in the sanctioning process.[1]

To ensure anonymity for respondents, the Agency for Workforce Innovation (AWI) distributed the survey link through e-mail to its 24 Regional Workforce Boards (RWBs) for subsequent distribution to caseworkers via e-mail.[2] Case managers completed the surveys during a two-week period at the end of 2006.[3] Prioritizing

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[1] All states must establish formal procedures to ensure fairness when sanctioning clients, and Florida's process is quite similar to that of other states. For a detailed description of Florida's sanction policy, see Appendix; for background on Florida's sanctioning practices, see Fording and colleagues (2007).

[2] AWI requested that the survey be distributed via an official electronic memorandum to RWBs. To meet this request, we used a Web-based survey rather than a mailed, paper survey. RWBs were asked to forward the survey request to their contract agencies so they could encourage case managers to participate. This procedure was designed to motivate participation without case managers feeling any direct pressure from the state government. The authors provided no additional incentives to promote participation in the survey.

[3] All caseworkers shared a survey password to ensure anonymous completion and return. They were sent two follow-up requests for their participation, which extended the time period. We garnered no additional responses after the last follow-up request.

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Table 1. Distribution of Responses by Region

Region Name (Number)	Frequency	Percent
Workforce Central Florida (12)	27	26.0
First Coast Workforce Development, Inc. (8)	22	21.2
Hillsborough County Workforce Board (15)	16	15.4
Pinellas Workforce Development Board (14)	11	10.6
Pasco-Hernando Jobs & Education Partnership Regional Board, Inc. (16)	6	5.8
Brevard Workforce Development Board, Inc. (13)	5	4.8
Southwest Florida Workforce Development Board (24)	5	4.8
Big Bend Jobs and Education Council, Inc. (5)	3	2.9
Citrus Levy Marion Workforce Development Board (10)	2	1.9
Polk County Workforce Development Board, Inc. (17)	2	1.9
North Florida Workforce Development Board (6)	2	1.9
Gulf Coast Workforce Development Board (4)	1	.96
Workforce Development Board of the Treasure Coast (20)	1	.96
Palm Beach County Workforce Development Board (21)	1	.96
Total	104	100.0

respondent anonymity limits our ability to determine whether all WT program officers passed along the survey as requested, as well as the extent to which various regions participated in the survey. We do, however, have regional response data for the subset of case managers who elected to identify themselves by region, as presented in Table 1. This table suggests that responses were spread across a number of regions but clustered in several of the state's larger regions.

We received survey responses from 144 TANF case managers, the vast majority of whom responded to our vignettes.[4] Although Florida officials were unable to provide a precise number for the overall population of WT case managers, estimates ran from 200 to 250. This suggests a rough estimate of between 58 and 72 percent for our response rate. Certain items in the questionnaire, however, including race (N = 98), political party affiliation (N = 103), and recent sanctioning behavior (N = 108), yielded a larger number of nonresponses than most. Table 2 provides a demographic profile of our survey respondents. Although response patterns suggest that some caution should be used in generalizing from this sample, the respondents are quite diverse and offer a sample well suited to the needs of our experimental design.[5] Moreover, by triangulating our experimental results with an analysis of statewide administrative data, we offer an independent safeguard against any biases arising from nonparticipation in the survey.

Our analysis is based on two 2 × 2 experiments embedded in the survey, each of which presented case managers with a vignette and asked them to decide whether to impose a sanction. The 2 × 2 design includes variation on race and a discrediting social marker. Each vignette portrays a hypothetical TANF participant who has arguably fallen out of compliance with program requirements. (For a description of relevant rules and procedures in the WT program, see Appendix.) For the racial dimension of the 2 × 2 design, each vignette makes use of a procedure developed by Bertrand and Mullainathan (2003), who show that by randomly assigning "black-sounding" names and "white-sounding" names to a set of identical resumes, they can significantly influence the rate at which employers contact a fictitious job-seeker. Adapting this procedure, we randomly assigned the client

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[4] A total of 137 caseworkers responded to Vignette 1 and 131 to Vignette 2.

[5] Tests on the full sample, including respondents who did not indicate their race and other personal characteristics, provided baseline responses for our experimental results that are consistent with our multivariate models.

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Table 2. Respondent Characteristics

Respondent Characteristics	Percent of Sample
Sex (N = 114)	
Male	21.1
Female	78.9
Race/Ethnicity (N = 98)	
African American / Black	34.7
European American / White	44.9
Hispanic American / Latino	18.4
Other	2.0
Educational Level (N = 115)	
High school diploma	7.8
Some college or trade school	33.0
4-year college degree	37.4
Some graduate school	13.0
Graduate degree	8.7
Marital Status (N = 115)	
Married	57.4
Divorced/separated/widowed	20.9
Single, never married	15.7
Unmarried couple living together	6.1
Political Party Affiliation (N = 103)	
Democrat or Independent Democrat	60.2
Independent	11.7
Republican or Independent Republican	24.3
Other	3.9
Religious Attendance (N = 113)	
Weekly	33.6
At least once a month	23.9
A few times a year	31.0
Never	11.5
Mean years of welfare services experience (N = 143)	7.0

Note: N = number of caseworkers who responded to survey item.

described in Vignette 1 either a Hispanic-sounding name or a white-sounding name. Similarly, we assigned the client described in Vignette 2 either a black-sounding name or a white-sounding name.[6]

The second dimension of the 2 × 2 experiments manipulates client markers that are commonly associated with (1) images of undeserving welfare clients and (2) negative stereotypes of minority racial/ethnic groups. We based our selection of these client traits on our field interviews, which revealed substantial caseworker attention to these two client "types": the young mother of multiple children and the repeat recipient who has been sanctioned in the past. We present the vignettes below with our experimental manipulations bracketed.[7]

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[6] To guard against confounding effects that might arise from the use of a specific name, we randomly assigned one of three names for each group in each vignette. White-sounding names include Sarah Walsh, Emily O'Brien, and Meredith McCarthy; African American-sounding names include Lakisha Williams, Aisha Jackson, and Tamika Jones; and Hispanic-sounding names include Sonya Perez, Maria Rodriguez, and Luisa Alvarez. To test for name-specific effects, we analyzed responses within each "race condition" to search for significant differences associated with each name. We found no significant differences and thus treat all racial name cues as equivalent.

[7] To minimize the amount of bias potentially introduced by subsequent questions on sanctioning practices, the two vignettes were presented near the beginning of the survey. We tested for order effects,

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VIGNETTE 1

[Emily O'Brien/Sonya Perez] is a 28-year-old single mother with [one child age 7 / four children ages 7 to 11, and who is currently in her fourth month of pregnancy]. She entered the Welfare Transition program six months ago, after leaving her job as a cashier at a neighborhood grocery store where she had worked for nine months. Emily was recently reported for being absent for a week from her assignment for community service work experience. Immediately after hearing that Emily had not shown up for a week of work, Emily's caseworker mailed a Notice of Failure to Participate (Form 2290) and phoned her to ask why she had missed her assignment. Emily was not home when the caseworker called. However, when she responded to the 2290 three days later, she said she no longer trusted the person who was looking after her [child/children], and she did not want to go back to work until she found a new childcare provider. Emily returned to work the next day.

VIGNETTE 2

[Emily O'Brien/Lakisha Williams] is a 26-year-old single mother with two children. She has been in the Welfare Transition program for five months. Lakisha was recently reported for failing to show up for a job interview that had been scheduled for her with a local housecleaning service. Immediately after hearing about the missed interview, Lakisha's caseworker mailed a Notice of Failure to Participate (2290) and phoned her to ask why she had not shown up. Lakisha said she had skipped the interview because she had heard that a better position might open up next month with a home health agency. [She had been sanctioned two months earlier for failure to complete her hours for digital divide.]

The two experiments offer a more likely and a less likely case for finding effects on sanction decision-making.[8] Vignette 1 identifies the client as either white or Hispanic, a contrast associated with smaller stereotype differences than the black–white client contrast used in Vignette 2. The client traits used in the two vignettes reinforce this difference. Vignette 1 focuses on childcare instability, which is a "normal" rather than "deviant" problem for women moving into employment (Loprest 2002). We expect the attribution of numerous children here to cue negative stereotypes related to sexuality and reproduction, but this feature of the vignette also indicates that a comparatively sympathetic group (i.e., children) may suffer hardship as a result of the sanction. As a result, one might expect this cue to produce ambivalence about sanctioning the family. The marker in Vignette 2 is far less equivocal. By stating that the client has previously drawn a sanction, we simultaneously provide case managers with two pieces of information that might cue perceptions of welfare dependency and resistance to achieving self-sufficiency: the client is a repeat recipient who has returned to the program and has a record of at least one previous failure to comply with welfare-to-work rules.[9]

Finally, the two narratives differ in relation to sanction procedures. In Vignette 1, the client was reported for a week's absence from her work experience assignment. Although she was not home when the caseworker called, she responded to the mailed 2290 form well within the 10-day period allotted and reestablished compliance by returning to work the next day. According to both the sanctioning rules and the WT Sanction

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specifically, whether sanctioning decisions in Vignette 1 predict sanctioning decisions in Vignette 2, and find no evidence of such effects.

[8] We developed the questionnaire, including the vignettes, after extensive study of the sanctions process in Florida, including participation on multiple occasions in sanctions training for case managers. We developed the vignettes in consultation with AWI staff responsible for this training. The survey/experiment was pilot tested with other AWI staff who had prior experience as frontline case managers.

[9] Under Florida's WT program, a record of a sanction two months prior to the current failure to comply should not itself increase a client's jeopardy of receiving a sanction for the present offense (see Appendix). The client in the second vignette, however, has a prior sanction and therefore faces a lengthier penalty (one month) for noncompliance than would a client without a prior sanction (10 days). It is possible that a case manager might be less willing to impose a sanction knowing that a client faces a longer penalty and therefore a greater amount of time without cash assistance.

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Flow Chart (see Appendix), the pre-penalty phase for this client should end with compliance following her return to work. In other words, based solely on the vignette, one cannot say that a sanction is the appropriate response. By contrast, although the client in Vignette 2 responded to the case manager's phone call, her reason for not complying with her welfare contract clearly fails to meet requirements for a "good cause" exemption. [10]

The two vignettes present us with an opportunity to test the RCM in strikingly different cases. Each vignette was followed by a question asking respondents to indicate on a four-point scale whether they strongly favor, somewhat favor, somewhat oppose, or strongly oppose requesting a sanction for the client and situation described.[11]

SURVEY EXPERIMENTAL ANALYSIS: RESULTS

As anticipated, fewer caseworkers recommended sanctioning the client in Vignette 1 (34 percent) than in Vignette 2 (79 percent). At the same time, these overall results strongly confirm our expectation of variation in caseworker judgment: roughly a third of caseworkers decided that a sanction was warranted in Vignette 1, despite the fact that WT program rules seem to suggest otherwise, and 21 percent opted not to sanction the client in Vignette 2, despite a clear violation of the rules.

Our baseline results indicate differences in how different clients are treated. The Hispanic-named clients were more likely to be recommended for sanctioning than were the white-named clients in Vignette 1 (40 versus 28.6 percent, $p < .10$); in Vignette 2, the black-named clients were slightly more likely than the white-named clients to be recommended for sanction (82.3 versus 76.8 percent, $p = .22$). Consistent with our hypothesis, the racial disparities widen with the presence of a discrediting marker in each experiment. For Vignette 1, the Hispanic-named clients who are pregnant and have four children were likely to be sanctioned 40 percent of the time, while the non-Hispanic white-named clients were slightly less likely to be sanctioned at 27.3 percent of the time ($p = .11$). For Vignette 2, the black-named clients with a prior sanction were likely to be sanctioned 93.9 percent of the time, compared with 77.4 percent of the time for the previously sanctioned white-named clients ($p < .05$). These simple descriptive results, however, fail to take account of possible differences between managers assigned to each condition of the experiment. To do so, we include relevant covariates in a larger multivariate analysis.

Our multivariate models include dichotomous variables representing three of the four experimental conditions (the white client with a "more deserving" trait serves as the baseline). The models also include measures of selected case manager characteristics that, based on literature on welfare casework, we have reason to believe affect a willingness to impose sanctions: work experience, religiosity, education level, partisan identification, marital status, and racial identity (Dias and Maynard-Moody 2007; Gooden 2003; Watkins-Hayes 2009). We hypothesize that case managers with greater experience in the social welfare field are likely to have either worked under the earlier, more permissive system of welfare or to have witnessed the negative effects of sanctioning; they are therefore less likely to impose sanctions. Case managers with more experience may also be more committed to supporting clients and thus less likely to support sanctioning. Highly religious case managers are likely to have greater commitment to enforcing basic work and family values and thus are possibly more likely to impose sanctions. More educated case managers might be less likely to impose sanctions because they are apt to be more informed about their effects. Democrats are probably less likely to adhere to the new wel-

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[10] Although failure to provide evidence of the childcare problem, or the children being too old under Florida's childcare exemption policy, may preclude a "good cause" exemption, Vignette 1 introduces the idea that "good cause" might apply here because the mother provided some evidence of her childcare problem and came back into compliance within the time allowed.

[11] In Florida, sanctions are all of one type—financial penalties for failure to comply with program rules. Penalties increase with each infraction (see Appendix).

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Table 3. Analysis of Vignette Experiments with Caseworker Characteristics as Covariates

Independent Variables	Vignette 1	Vignette 2
Vignette Condition		
White client, marked	.501	.367
	(.60)	(.51)
Minority client, unmarked	1.383†	–.075
	(1.50)	(–.11)
Minority client, marked	1.693*	2.599**
	(1.96)	(2.28)
Caseworker Characteristics		
Experience	–1.573**	–1.632*
	(–2.73)	(–1.98)
Religiosity	–.314	.018
	(–.56)	(.03)
College education	.134	.424
	(.24)	(.73)
Democrat	.567	–.127
	(.99)	(–.22)
Married	.903	–.152
	(1.48)	(–.25)
Black or Hispanic	.301	.405
	(.48)	(.63)
LR χ2	16.90*	16.90*
Log likelihood	–46.304	–41.482
Pseudo R^2	.154	.169
N	95	94

Note: Entries are coefficients followed by z-scores in parentheses. For Vignette 1, the racial minority is Hispanic and the marked condition is "four children and pregnant," as opposed to one child. For Vignette 2, the racial minority is black and the marked condition is possession of a prior WT sanction, as opposed to no mention of a prior participation spell. The number of cases for each model is lower than the overall sample due to missing data for selected covariates.

† $p \leq .10$; * $p \leq .05$; ** $p \leq .01$ (one-tailed tests).

fare regime's use of sanctions to enforce client compliance. Married case managers might be more likely to support sanctions, perhaps as an expression of a commitment to upholding traditional values. We include racial identity based on the hypothesis that white case managers might be more willing to sanction nonwhites. Furthermore, nonwhite case managers might feel they lack the "race privilege" to sanction whites, while also perceiving special obligations to sanction black clients out of grounds of loyalty to the race or a desire to teach clients of the same race the need to be compliant with established norms.

Table 3 presents the results of logit models for both vignettes. Only one of the covariates emerges as statistically significant, and it does so consistently across experiments. Case managers with more experience (defined as those with more than two years of welfare services experience) emerge here as less likely to impose sanctions. No other covariate achieves statistical significance, and we obtained no significant results in further specification tests of caseworker characteristics not included here.

Caseworkers with more than two years experience are significantly less likely than their less experienced counterparts to choose a sanction in both Vignette 1 ($p < .01$) and Vignette 2 ($p < .05$). Several factors may explain why more experienced case managers are less likely to impose a sanction. For example, case managers with more experience are more likely to have been trained in an earlier era of welfare provision that placed less emphasis on sanctioning (Aid to Families with Dependent Children prior to 1996, or Florida's initial reform program Work and Gain Economic Self-Sufficiency [WAGES] 1996 to 2000). Alternatively, if case managers learn over time that sanctions have negative consequences for clients or for performance ratings, then more experienced case

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managers might become more reluctant to impose sanctions. Another possibility is that experienced case managers develop greater knowledge of how to work with clients and, hence, resort to sanctions less quickly than do novice case managers. Our data do not allow us to arbitrate among these alternatives.[12]

The key findings in Table 3 are indicated by the coefficients for our experimental conditions. The results are striking for their consistency, especially given the considerable differences between our two vignettes. In the first, we find that case managers are no more likely to sanction the white client with multiple children than to sanction the white client with one child. We find borderline results suggesting that case managers may be more likely to sanction the Hispanic client with one child than to sanction the white client with one child (one-tailed test, $p = .067$). By contrast, we find that a Hispanic woman with multiple children is significantly more likely to be sanctioned compared with a white woman with one child.

The results for Vignette 2 tell a similar story, only in stronger form. Here, we find no discernible differences in the odds that a case manager will sanction a white woman with no prior sanction, a black woman with no prior sanction, or a white woman with a prior sanction. Sanction decisions are, from a statistical perspective, invariant across these conditions. When a prior sanction is attributed to a black woman, however, we find a large and statistically significant increase in the possibility of being sanctioned, compared with a white women without a prior sanction.[13] Indeed, under every other combination of race and sanction history in our experiment, the probability of a new sanction being applied is significantly lower than what we observe when the client is identified as a black woman with a prior sanction on her record.

To put these results into perspective, we can calculate the predicted probability of being sanctioned for each of the four client conditions we examine in each vignette by holding all the other variables in the model at their median and allowing the variable in question to vary. Figure 1 provides the predicted probabilities for each of the four types of clients for both vignettes. Based on our model for the first vignette, we find that a Latina mother who is pregnant and already has four children has a .47 predicted probability of being sanctioned, while a white mother in the same condition has only a .21 predicted probability of being sanctioned, and a white mother with only one child has only a .14 probability of being sanctioned. For Vignette 2, a black client with a prior sanction has a .97 predicted probability of being sanctioned, compared with .75 for a white client with a prior sanction and .67 for a white client without a prior sanction.

The consistency of findings across two random-assignment experiments, each with very different conditions, is noteworthy. The key limitation of the evidence, however, is that these findings, like most studies of implicit racism, are based on hypothetical scenarios (Duster 2008). When case managers responded to these vignettes, they were not confronted with a real person: they did not have a detailed case file in hand, they did not have to worry about effects on their performance numbers, and they did not have to contemplate real material hardships that might result. To bring our empirical analysis into line with real-world conditions, we must turn to administrative data generated by the Florida WT program itself. In doing so, we lose certainty about whether clients with different characteristics have equivalent cases and must rely on an imperfect process of specifying control variables. In return, however, we gain the ability to triangulate our survey-experimental findings with data that bear a closer relationship to the real world of administrative practice.

ADMINISTRATIVE DATA: EVENT HISTORY ANALYSIS

The Florida Department of Children and Families (DCF) provided the data for these analyses, which consist of individual-level records for all adults

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[12] In alternate specifications of our models, we find no significant interactions between case manager experience and client race. The effects of case manager experience should thus be seen as independent of the racial patterns presented below.

[13] We tested our models including interactions that combine the race of the caseworker with the race of the client to see if the results could be better explained by intergroup hostility between caseworkers and clients, rather than the logic of the RCM. The interaction terms proved insignificant, suggesting the results are not reflective of intergroup differences between the caseworkers and the clients and that the RCM offers a better explanation.

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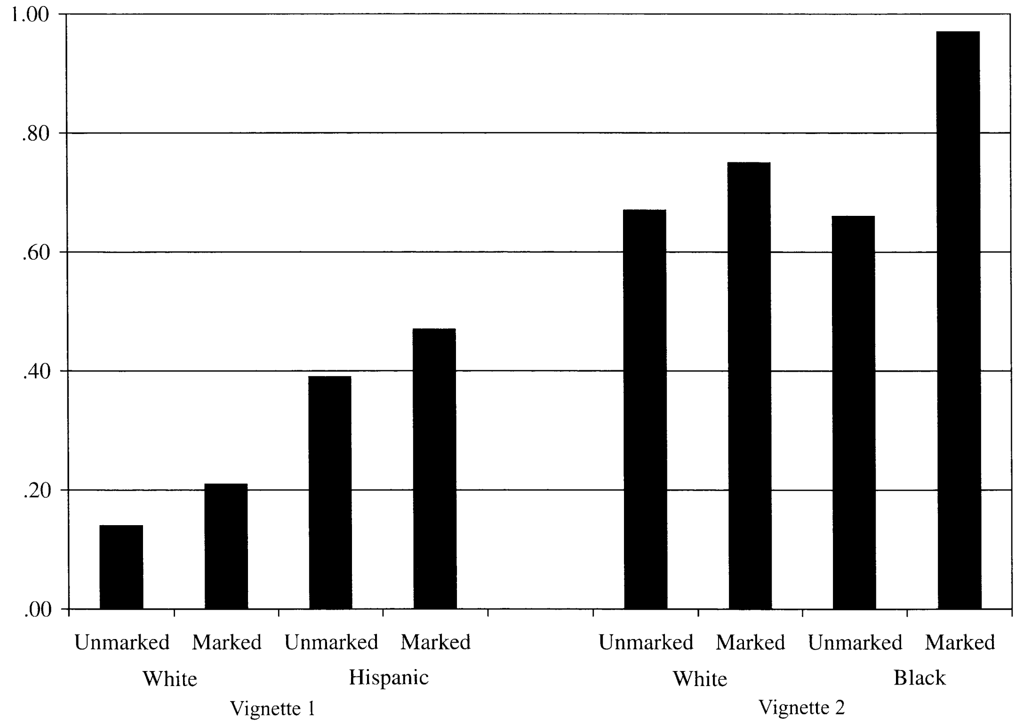

Figure 1. Predicted Probabilities by Race and Condition

who received TANF in Florida between January 2000 and April 2004. Our data set consists of monthly records for reporting case outcomes and the characteristics of TANF clients and their families. Our analytic strategy is to replicate the experimental vignettes as closely as possible by estimating an event history model of sanction initiation, focusing on the effect of a client's race or ethnicity and its interaction with the stigmatizing marker used in each experimental vignette. We begin by examining the joint effects of ethnic identity and family size tested in Vignette 1. We then turn to the joint effects of racial status and sanction history tested in Vignette 2.

Triangulating Vignette 1: Ethnicity and Family Size

For our first analysis, we construct a sample of all new adult clients who entered TANF during the 24-month period from January 2001 through December 2002.[14] In an effort to match Vignette 1 as closely as possible, we then restrict our sample to unmarried, female clients who are either white (non-Hispanic) or Hispanic and who have either one or four children. The dependent variable for our analysis is a dichotomous variable that takes on a value of 1 in the month that a client is sanctioned. We follow each of the 24 cohorts in our sample for up to a maximum of 12 consecutive months, ending our observation at the spell's termination or the 12-month mark, whichever comes first. Clients who exit for reasons other than a sanction, or who are not sanctioned by the 12th month of the spell, are treated as right-censored. For our first analysis, we restrict our attention to the first TANF spell for each individual during this period; we define a spell as continuous months of

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[14] Our selection of cohorts is limited by two factors. First, we define "new" TANF clients as those who have spent at least 12 continuous months without TANF benefits. This precludes the inclusion of clients entering TANF during 2000. In addition, we wish to observe each client cohort for up to 12 months after entering TANF. This forces us to exclude clients who entered TANF during the last year of our observation period.

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TANF receipt.[15] As defined, and accounting for a small percentage of cases for which values of some variables are missing, our total sample size exceeds 6,000 women and nearly 20,000 person-month observations.

We estimate our model using a Cox proportional hazards approach. The advantage of the Cox model is that it allows for flexible, nonparametric estimation of the baseline hazard, or what we might think of as the effect of spell duration on the probability of sanction (Box-Steffensmeier and Jones 2004).[16] Our primary interest lies in the effect of a client's ethnicity and changes in this effect as we move from clients with only one child to clients with four children. To test this interactive hypothesis, we divide our sample into four groups that parallel the experimental groups in Vignette 1.

Based on past research on sanctions and welfare implementation (Hasenfeld et al. 2004; Kalil et al. 2002; Wu et al. 2006), we include a number of independent variables to control for variation in clients' individual and community characteristics.[17] At the individual level, we include variables measuring a client's citizenship status, her age, the age of the youngest child in the TANF family, and two indicators of human capital (income and education). We also control for a variety of community conditions, including local conservatism,[18] percent black, and percent Hispanic,[19] and several measures of employment conditions: the county unemployment rate, the county poverty rate, the level of urbanization as measured by county population, and the annual local wage in food service/drinking establishments.[20] Finally, we include a measure of the county TANF caseload, expressed as a proportion of the county population.[21]

Table 4 shows the results for our event history model. Column I presents the results for a model that estimates the additive effect of ethnicity through the inclusion of a single dichotomous variable (Hispanic) measuring whether a client is white or Hispanic. Column II presents results for a model that tests for an interactive effect between ethnicity and the number of children using the indicator variables described above. For each variable in

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[15] Based on findings from our field research, we do not include the first two months of the first TANF spell in our analysis. In interviews at all levels of the WT program, officials report that sanctions recorded in the first months of a spell often represent a form of "self-sanctioning" that is distinguishable from "true sanctioning" decisions made by case managers. In this scenario, an applicant with some alternative income options enters the official rolls, begins to receive assistance, but then does not return to the local provider after learning what will be required of her and how much cash aid she will receive in return.

[16] We have replicated our findings using other estimation methods as well, including parametric methods (Weibull) and a discrete-time (logit) model.

[17] See Table A1 in the appendix for variable definitions and sources.

[18] Several studies find that local policy implementation is influenced by the local political environment (Cho et al. 2005; Weissert 2000). Based on these studies, we include a measure of local political ideology, which we expect will be positively related to sanction initiation.

[19] Previous studies often find that racial context has a significant effect on racially relevant policy outcomes. The "threat" hypothesis (Blalock 1967; Key 1949) suggests that the higher the percentage of nonwhites, the greater the support for more punitive policies. The "contact" hypothesis (Alport 1954; Fox 2004) indicates that the higher the percentage of nonwhites, the lower the support for punitive policies. There are also possible effects from an increased proportion of the population nonwhite enhancing minority political power (Keech [1968] 1981). Because there is reason to suspect that any of these effects might exist (see Keiser, Mueser, and Choi 2004), we test for effects of community racial composition by including the percentage of the county population that is Hispanic and black, respectively.

[20] Where employment opportunities are relatively numerous and attractive, TANF clients may be more likely to work enough hours to avoid falling out of compliance with TANF rules. Alternatively, local labor market conditions may influence the sanction decisions of case managers, who may be less inclined to sanction clients when job opportunities are less numerous or less attractive.

[21] As the caseload size increases, administrative pressures to reduce caseloads should result in an increase in sanctioning. Additionally, the heightened burden of more cases may increase the chances that caseworkers will rely on race-relevant heuristics. Alternatively, as caseload size increases, if the number of case managers remains fixed, individual case managers may have less time to closely monitor TANF clients for violations of rules, thus resulting in a lower rate of sanctioning.

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Table 4. Cox Proportional Hazard Models of Effects of Minority Status and Number of Children on Sanction Initiation

Independent Variables	I Additive Model	II Interactive Model
Individual Characteristics		
Hispanic client	.960	
White client and one child		[Reference]
White client and four children		.981
Hispanic client and one child		.955
Hispanic client and four children		.982
Age of client	.980**	.980**
Age of youngest child	1.013	1.013
Citizenship status (1 = citizen)	1.276	1.274
Education (reference = > high school)		
Less than high school education	1.371**	1.370**
High school education	1.068	1.068
Income (in 1000s)	1.067**	1.067**
Community Characteristics		
Local conservatism	1.085	1.085
Percent black	.997	.997
Percent Hispanic	.987**	.987**
Annual wage – food service/drinking places	.976	.976
Unemployment rate $_{t-1}$	1.029	1.029
Poverty rate	1.017	1.017
Population (in millions)	1.281**	1.281**
TANF caseload $_{t-1}$	.911	.911
Number of subjects	6,214	6,214
Number of failures	1,792	1,792
Time at risk (person–months)	19,798	19,798

Note: The sample for this analysis consists of all new TANF clients (single-parent, female, Hispanic or white) who entered TANF from January 2001 through December 2002. All clients are observed for a maximum of 12 months (clients who exit without being sanctioned, or who were sanctioned after 12 months, are treated as censored). Cell entries are hazard ratios, with p-values based on robust standard errors (adjusted for error clustering at the county level).
* $p < .05$; ** $p < .01$ (two-tailed tests).

our models, we report the estimated hazard ratio, which reflects the proportional change in the risk of sanction given a one-unit increase in the independent variable of interest. Most of the control variables in the model perform as expected. The risk of being sanctioned is lower among older and more educated clients and among those who reside in a community with a large Hispanic population. Clients are more likely to be sanctioned if they have older children or reside in heavily populated counties. These results are largely consistent with the findings of past studies of sanctioning (Born, Caudill, and Cordero 1999; Fording et al. 2007; Hasenfeld et al. 2004; Kalil et al. 2002; Keiser et al. 2004; Koraleck 2000; Mancuso and Lindler 2001; Wu et al. 2006).[22]

Moving to our main hypotheses of interest, we find no evidence in the administrative data to suggest that Hispanic clients are sanctioned at a greater rate than white clients. This is true regardless of whether we examine the additive effect of ethnicity displayed in column I or its conditional effect in column II. Our first analysis of administrative data thus proves to be

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[22] We also tested for whether black clients with four children were more likely to be sanctioned than whites with one child and the results are not significant.

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inconsistent with the findings for Vignette 1 from our survey of case managers.

TRIANGULATING VIGNETTE 2: RACE AND PRIOR SANCTIONS

We now move to an analysis of administrative data based on Vignette 2, which examined the joint effects of client race (black/white) and sanction history. Once again, we limit our sample to clients with the characteristics described in the vignette—unmarried female clients who are either black or white (non-Hispanic). We also rely on the same general research design by estimating an event history model of sanction initiation. The narrative in Vignette 2, however, presents us with two additional complications for an analysis of administrative data. First, as the stigmatizing condition in Vignette 2 is a prior sanction, we must go beyond the first spell to examine sanctioning outcomes for clients with a prior history of TANF participation. We therefore rely on a sample consisting solely of TANF clients participating in their second spell.[23] Specifically, the sample consists of all TANF clients (unmarried, female, black or white) who entered TANF for the first time between January 2001 and December 2002, and who returned to TANF for a second spell during this same period. As defined, and accounting for some missing data, our sample consists of approximately 19,000 second spell clients, approximately 37 percent of whom experienced a sanction during their second spell.

A second complicating factor arises due to the possibility of sample selection bias. That is, the factors that cause TANF recipients to return to the program for a second spell (and thus enter our sample) may also be related to the outcome we are trying to explain (sanctioning). Because this nonrandom selection may introduce statistical bias, we must control for the selection process that brings some (but not all) first-spell clients into a second spell. To do so, we rely on an estimator introduced by Boehmke, Morey, and Shannon (2006) for continuous-time event-history models with sample selection. The procedure relies on full information maximum likelihood to simultaneously model the selection and event history processes. Like the Heckman model for continuous outcomes, we first estimate a binary model of the selection process—a client's return to TANF after having exited a first spell.[24] We then use the information garnered from the selection model to correct for selection bias in the event history model. Because the estimation procedure is limited to parametric event history models, we use the Weibull distribution to model duration dependence (i.e., the effect of time on sanction initiation).[25]

Once again, we divide our sample into groups analogous to the four experimental groups featured in Vignette 2. We include indicators for three of the groups in the model, excluding white and no prior sanction as the reference group. Because our theory suggests that whites with no prior sanction should be subject to the lowest rate of sanction, we expect that the coefficients representing each of the three indicator variables included in the model will be positive (and the associated hazard ratios > 1.0). In addition, we expect that the variable representing blacks with a prior sanction (black and prior sanction) will display the largest coefficient. Finally, we also include several individual and community-level controls: age of client, age of youngest child, education, income, local conservatism, percent black, annual wage, poverty rate, and TANF caseload.[26]

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[23] While our data allow us to examine sanctioning outcomes beyond the second spell, we limit our analysis to the second spell for at least two reasons. First, because our observation period is fixed, each successive spell necessarily increases the number of right-censored observations. Second, the receipt of a third sanction can result in a TANF client being barred from TANF participation for three months, even if the client comes into compliance with TANF rules. We suspect that this may alter case managers' decision-making processes in ways that render the sanction decision less comparable to the decision prompted by Vignette 2.

[24] We do not present this model in the text, but see Table A2 in the Appendix.

[25] The Weibull distribution allows for a flexible specification of the baseline hazard rate and is appropriate when the baseline hazard rate is monotonically increasing, monotonically decreasing, or flat over time. Based on various diagnostics, we are satisfied that the Weibull model is appropriate for our data.

[26] We do not include *citizenship status, number of children, percent Hispanic,* or *population,* because these variables proved to be highly insignificant in

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Table 5. Weibull Selection Model of Effects of Minority Status and Sanction History on Sanction Initiation during Second TANF Spell

Independent Variables	I Additive Model	II Interactive Model
Individual Characteristics		
Black client	1.190**	
Prior sanction	1.091*	
White client and no prior sanction		[Reference]
White client and prior sanction		1.032
Black client and no prior sanction		1.145*
Black client and prior sanction		1.288**
Age of client	.968**	.968**
Age of youngest child	1.012	1.012
Education (in years)	.979**	.979**
Income (in 1000s)	.975	.976
Community Characteristics		
Local conservatism	1.029	1.028
Percent black	.992*	.992*
Annual wage – food service/drinking places	.973	.973
Poverty rate	1.002	1.002
TANF caseload	.887*	.887*
Rho (error correlation)	–.223**	–.223**
Total N	40,891	40,891
Uncensored N (returning for 2nd spell)	18,827	18,827

Note: The sample for this model consists of all TANF clients (single-parent, female, black or white) who entered TANF from January 2001 through December 2002 and returned for a second TANF spell during this same observation period. All clients are observed during the 2nd spell until they are sanctioned or they exit TANF for other reasons. Clients who exited TANF without being sanctioned, or whose second spell continued beyond the close of our observation window (April 2004), were treated as right-censored observations in the Weibull model. The model was estimated in Stata 10.0 using the DURSEL procedure (Boehmke 2005). Cell entries are hazard ratios, with p-values based on robust standard errors (adjusted for error clustering at the county level).
* $p < .05$; ** $p < .01$ (two-tailed tests).

Table 5 presents the results for our event-history model. The effects of the control variables largely conform to our expectations and look similar to the first-spell results reported in Table 4. The risk of sanction is highest for younger clients who are less educated, and the risk is lower among clients who live in counties with relatively large black populations and high TANF caseloads. The negative effect of percent black is especially interesting given the fact that we found a similar effect for percent Hispanic for the sample that included Hispanic clients. Whether through increased presence within the local welfare bureaucracy, outside pressure on local welfare policymakers, or increased contact with the nonwhite population, it appears that a large minority presence within a community may offset any racial or ethnic biases in sanctioning that could occur within the implementation process.

Moving to the results for our primary hypotheses, we find strong corroboration of our experimental findings. The additive specification in column I tests for the independent effect of race. The hazard ratios reported here indicate that black clients are indeed significantly more likely than white clients to be sanctioned, regardless of their sanction history. The results in column II allow for a test of our interactive hypotheses and suggest that these two characteristics do interact as expected. Indeed, if we begin with the baseline risk of sanction for a white client who has no prior sanction, we find that the positive effect of making this client black is *significantly greater than* the effect of

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preliminary models. Their inclusion in the model does not affect the results reported in Table 5.

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giving the white client a prior sanction. Further, having a prior sanction does not seem to matter much for white clients, but it matters a great deal for black clients. The hazard ratio representing the difference in risk of sanction between whites with and without a sanction is close to 1.0 and is far from being statistically significant ($p = .63$). By contrast, the risk of sanction for black clients with no prior sanction is approximately 14 percent higher than that for white clients without a first-spell sanction ($p < .05$). Among black clients with a prior sanction, this risk is doubled; the risk of sanction for black clients with a prior sanction is 28 percent higher during the second spell than the risk for white clients with no prior sanction ($p < .01$). In summary, even though black clients with no prior sanction are already at a higher risk than white clients of being sanctioned in the second spell, the addition of a prior sanction increases black clients' risk of sanction to a significantly greater degree than for white clients.[27]

DISCUSSION AND CONCLUSIONS

This article confronts, more directly than past research, the question of how and why race influences sanctioning under welfare reform. To do so, we apply a general model developed to explain how racial classifications affect policy choices in diverse domains. The RCM provides clear, testable predictions about where and when racial disparities are likely to emerge in the administration of welfare programs. Our experimental results support these predictions and are corroborated by administrative data indicating how real clients under welfare reform have actually been treated. The results converge to provide striking evidence for both the utility of the model and the enduring power of race in U.S. poverty governance.

Our experiments randomly assigned case managers to vignettes in which clients differed in their race/ethnicity and in the possession of stereotype-consistent discrediting traits. Only one caseworker characteristic emerged as a significant predictor of sanction decisions: case managers with more than two years experience were significantly less likely to impose sanctions in both experiments. Although we discussed several possible explanations for this pattern, our data do not allow us to distinguish among them, thus raising an important puzzle for future research.

In both law and principle, welfare sanctions should be imposed as responses to client behavior. In practice, however, we find that sanctions are also used in response to client characteristics. Despite having identical case narratives, our first vignette finds that a pregnant Latina client with four children is significantly more likely to be sanctioned than a white client with only one child. In the second vignette, we find that a black client with a prior sanction is significantly more likely to be sanctioned than a white client with no prior sanction. The two vignettes are quite different—in the racial/ethnic contrast involved, the nature of the stereotypical trait, and their relation to program rules regarding sanctioning—but the results are largely the same. White clients in these experiments suffer no statistically discernible negative effects when linked to characteristics that hold negative meanings in the welfare-to-work context. As advocates of administrative consistency might hope, case narratives elicit a stable pattern of responses from case managers, regardless of discrediting attributes, *when clients are white.* Minority clients enjoy no such immunity: their odds of being sanctioned rise in the presence of discrediting markers, even when the details of their case do not change a bit.

Our random-assignment vignettes offer a crisp test of causal effects, but this power is purchased at some cost. In the survey setting, case managers are pulled out of their normal organizational environment, client narratives are reduced to a handful of details, and hypothetical sanction decisions involve no cost to case manager time or recipient-family well being. To close this gap, we triangulated our experimental findings with an analysis of administrative data, a source of evidence that provides a weaker basis for causal inference but more faithfully reflects actual practices on the ground. In so doing, we test experimental evidence of implicit racial bias against evidence arising from the actual exercise of government authority (Duster 2008). The results of this analysis do not support our experimental find-

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[27] We tested in the administrative data whether Hispanic clients with a prior sanction were more likely than white clients to be sanctioned again and found this not to be the case.

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ings regarding the attribution of multiple children to a Hispanic woman. In the administrative data, Hispanic clients are no more likely than white clients to be sanctioned, and this null finding holds regardless of number of children. By contrast, we find strong support in the administrative data for our experimental results in Vignette 2. Among second-spell participants, black clients with a prior sanction are more likely than their white counterparts to be sanctioned. White clients suffer no discernible increase in their risk of sanction when they have a prior sanction, while black clients—who are already more likely to be sanctioned than whites—become significantly more likely to be penalized when this discrediting marker appears on their record.

Together, these findings offer powerful evidence that racial status and stereotype-consistent traits interact to shape the allocation of punishment at the frontlines of welfare reform. The lone instance in which our findings do not converge (the treatment of Hispanics in the experiment versus the administrative data) might suggest some inconsistency. Viewed in the context of the RCM, however, it actually conforms to a predictable and repeated pattern. Prior research indicates that the gap between Hispanic and white stereotypes is smaller than the gap between black and white stereotypes (Fox 2004; Gilens 1999). Accordingly, the RCM predicts that disparities will emerge more strongly and consistently in the latter case.

Indeed, we have found this precise pattern of race-based disadvantage in studies of other aspects of welfare reform (Fording et al. 2007; Schram, Fording, and Soss 2008; Soss et al. 2008). This article extends this body of evidence by showing that African Americans are distinctively vulnerable to the presence of discrediting social markers. In the present study, the stigma of deviant behavior attaches most strongly to black clients, more weakly and less consistently to Hispanic clients, and not at all to white clients. This pattern is further supported by our analysis of administrative data showing that Hispanic clients—unlike black clients—are no more likely to be sanctioned when they possess a prior sanction on their record. This pattern closely resembles the results Pager (2007) reports in her landmark study of how race and markers of criminality interact to limit African American men's prospects in the labor market. While Latinas might face other forms of discrimination in the welfare system, including language barriers, our evidence suggests that racial classification leads to greater disparities in sanctioning for blacks than it does for Hispanics.

Our analysis does not directly test whether sanctioning is influenced by racial animus, threat, or loyalty—as opposed to the more cognitive dynamics emphasized by theories of implicit racism (Quillian 2008) and the RCM (Soss et al. 2008). It is worth noting, however, that we do not find evidence in our experiments that white case managers differ from nonwhite case managers in their sanction decisions. White case managers were no more likely to sanction clients overall and no more likely to be influenced by our experimental manipulations of race/ethnicity and client traits. These findings are hard to square with an account that emphasizes white ingroup loyalty or white animus toward blacks. They are far more consistent with models emphasizing how racial classifications operate in implicit ways—without conscious racism—to generate racial disparities (Quillian 2008). Race matters in more subtle ways than overt hostility or loyalty; race is built into the cognitive processes that provide the foundation for decisions about how target groups should be treated in welfare policy settings (Schram 2005).

Sanctioning practices under welfare reform are part of a larger turn toward disciplinary poverty governance in the United States (Wacquant 2001). In this context, it is imperative that social scientists begin to provide some insight into how disciplinary practices operate and how the state's authority to punish may be used in ways that deepen or ameliorate social inequalities. TANF is ostensibly a race-neutral public policy, but it is carried out today in ways that allow preexisting racial stereotypes and race-based disadvantages to produce large cumulative disadvantages (Schram 2005, 2006). Our prior research finds that black TANF recipients, relative to their white counterparts, are more likely to participate in the toughest policy regimes controlled at the most local levels (Soss et al. 2008). Within one such regime, Florida, we find that they are more likely to be sanctioned and their odds of being sanctioned are more likely to rise when they are associated with longer participation spells or participate

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in conservative regions (Fording et al. 2007). While Florida may be distinctive in many ways, studies comparing Florida with other states suggest good reasons to think that the racial dynamics we have uncovered are not unique to welfare implementation in this locale (Shaw et al. 2006). The results presented in this article suggest that welfare sanctions, once imposed, become discrediting markers that make black clients even more vulnerable to a future sanction. Our findings reinforce the conclusion that policy choices not only reflect but also *create* the elements that underpin racial inequality in the U.S. welfare system. Under cover of a policy that is officially race-neutral, welfare systems operate in ways that reflect racial classifications, reproduce racial inequities, and call out for attention from both scholars and reformers.

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APPENDIX: SANCTIONING PROCEDURES IN FLORIDA

Florida's Welfare Transition program is administered by public, nonprofit, and for-profit provider agencies under contract with the State's 24 Regional Workforce Boards (RWBs). In a series of program guidelines issued in February of 2004, Florida's Agency for Workforce Innovation (AWI) and the Department of Children and Families (DCF) clarified the Florida statutes relating to work penalties and pre-penalty counseling, with one goal being to "develop integrated and consistent procedures to implement sanctions" (AWI FG 03-037, 1). According to these guidelines, a first occasion of noncompliance with a work contract results in a full-family termination of temporary cash assistance for a minimum of 10 days or until the individual reestablishes compliance. Second and third instances of noncompliance are attached to periods of termination of one month and three months, respectively. Sanctions that remain in place for more than a 30-day period can be resolved only when the return to compliance has been documented and is accompanied by a new Request for Assistance (RFA) and face-to-face interview (F.S. 445.024). Previous sanctions may be "forgiven" for participants who are compliant with their welfare contracts for a period of six months. Following a six-month compliant period, any sanction levied against a participant is treated as a first occasion (AWI FG 03-037; F.S. 414.065).

The Florida Statutes allow for "good cause" exceptions to the sanctioning policies outlined above. This may represent the point at which the preferences and understandings of individual local actors most clearly come into play (Pavetti et al. 2003). Such exemptions may be granted for instances of noncompliance related to childcare,[28] the current or past effects of domestic

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[28] A childcare exemption is reserved for participants who are single custodial parents "caring for a child under six years of age who can prove they are unable to obtain needed child care within a reason-

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violence, medical incapacity, outpatient mental health or substance abuse treatment, and "circumstances beyond [the participant's] control" (AWI FG 03-037, 5). In each of these instances, with the exception of current or past domestic violence, the individual must "prove to the RWB provider" that she has indeed been rendered unable to work. In the case of a physical or mental health or substance abuse exemption, such proof is, in principle, limited to documentation from a licensed physician or recognized mental health or substance abuse professional. However, the ways in which local actors interpret what constitutes adequate proof may be mediated by their relationships with clients, understandings of their role, and local political and economic contexts. In all instances for which a good cause exemption is thought to be ongoing, participants are required to submit to an Alternative Requirement Plan, with the penalties for failed compliance mirroring those applied to nonexempt program participants. Exactly what these alternative requirements should be is left largely to the discretion of the individual caseworker.

Once a participant has been reported for failure to comply with a work contract requirement, the state requires RWBs to provide notice, both in writing and orally, of the penalties attached to noncompliance prior to actually imposing such penalties. During this phase of contact, the *pre-penalty phase,* participants may still avoid a sanction by either establishing good cause for the noncompliance or returning immediately to satisfactory compliance. Throughout this pre-penalty phase, particular emphasis is placed on caseworkers providing counseling, a service that includes both reminding or warning the participant of the consequences of noncompliance and offering services or supports intended to remedy its causes.

While TANF participants are clearly responsible for establishing proof of the existence of grounds for a good cause exemption, program guidelines emphasize an ongoing relationship between the participant and the RWB representative, most often the individual caseworker. As suggested by interview and narrative response data from our current project, caseworkers interpret the requirements and responsibilities of this relationship in a variety of ways. Moreover, what constitutes pre-penalty counseling may include a range of interventions, from a brief warning or reminder to extensive referral or direct clinical intervention. In both the interpretation and application of sanctioning rules, caseworkers exercise considerable discretion.

Such discretion persists even within the context of extensive provisions for the training of caseworkers in the application of sanctioning rules. Throughout Florida's 24 regions, caseworkers are trained to apply sanctions according to the Welfare Transition (WT) Sanction Flow Chart developed by AWI (see Figure A1). Tracing the various paths that lead to a sanctioning decision suggests some of the ways individual actors might deviate from the planned course.

Should an individual be found noncompliant with the welfare contract, the caseworker is to mail a Notice of Failure to Participate and Possible Sanction (form AWI-WTP 2290), commonly referred to as the "2290," within two days of the first failure. As noted above, an attempt to contact the participant orally is also required, and, even if this oral attempt proves unsuccessful, the participant is allowed 10 days from the date of the 2290 mailing to establish good cause for the noncompliance. A sanction is to be requested only if both attempts at contact fail over a period of 10 days following the written notification. If, however, the participant responds to either form of contact within the allotted time period, the pre-penalty phase can be ended with compliance, provided that one of two conditions is met: (1) the participant establishes good cause based on any of the criteria outlined above; or (2) the participant "agrees to demonstrate satisfactory compliance" (AWI FG 03-037, 7) and remains compliant with her welfare contract for a minimum of 30 days following the first failure.

Immediate, full-family sanctions are considered appropriate when a participant has either (1) failed to respond to the 2290 and oral attempts at contact, (2) failed to establish good cause and refused to demonstrate satisfactory compliance, or (3) failed to follow her welfare contract without good cause for a second time

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able distance from their home or worksite, child care by a relative or others is unavailable or unsuitable, or there is no affordable formal child care" (AWI FG 03-037, 7).

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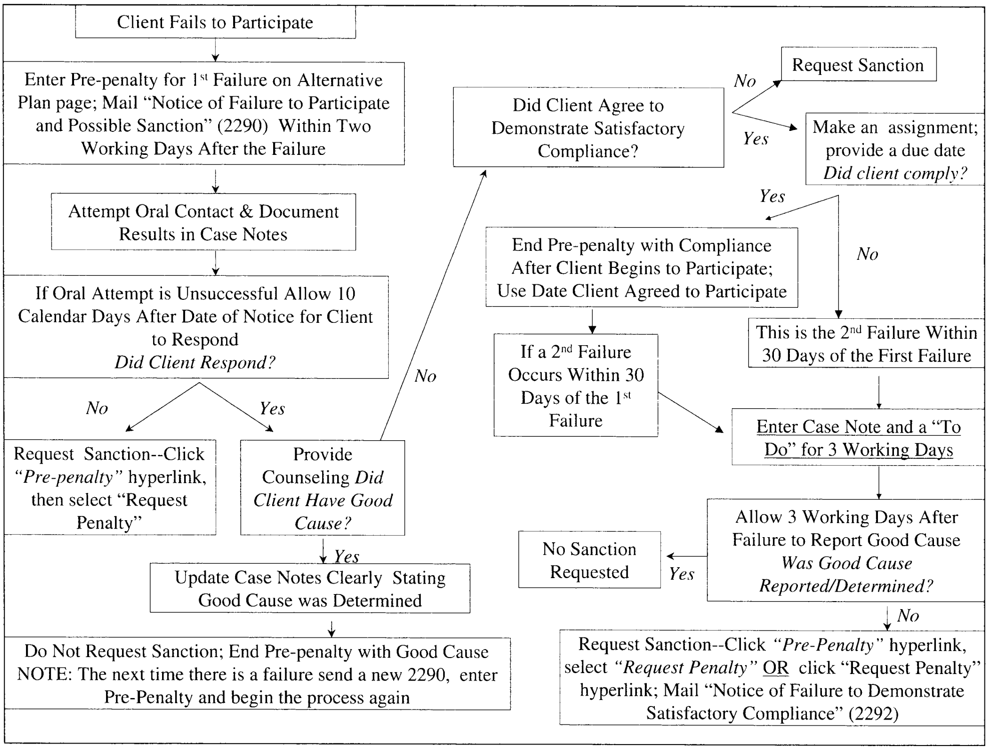

Figure A1. Florida Sanction Flow Chart

Source: Florida Agency for Workforce Innovation (http://www.floridajobs.org/PDG/TrainingPresentations/WT_SancFlowChart061406_070306.ppt).

within 30 days of her first offense. All pre-penalty and sanctioning activities are to be recorded, in accordance with the WT flow chart, in the computerized One Stop Service Tracking (OSST) system. With its system-generated prompts and reminders of what actions are called for and when, the OSST system functions, if not as a check on discretion, then certainly as a guide to when discretion may be most easily and least riskily applied.

While the flow chart provides a basis for suggesting that sanctioning in Florida is a highly structured process, a close reading of the chart shows that substantial opportunities for caseworker discretion remain. How that discretion gets used is the focus of our analysis.

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Table A1. Variable Definitions and Data Sources for Analyses Presented in Tables 4 and 5

Independent Variables	Definition	Minimum-Maximum
Individual Characteristics		
Age of client	Client age (in years)	18–72
Number of children	Number of children in TANF family	0–12
Age of youngest child	Age of youngest child in TANF family	0–17
Income	Earned income in 1,000s	0–200
Education (reference category = more than 12 years):		
Less than high school	1 = more than 12 years, 0 = otherwise	0–1
High school	1 = 12 years, 0 = otherwise	0–1
Black client	1 = black, 0 = otherwise	0–1
Hispanic client	1 = Hispanic, 0 = otherwise	0–1
Political Environment		
County conservatism index	Measure of county political ideology, based on factor analysis of local election results for 18 ideologically-relevant constitutional amendments (see Fording et al. 2007).	–2.5–2.2
Percent black	Percentage of blacks in county of client in 2000 (County and City Data Books 2003).	2.1–57.1
Percent Hispanic	Percentage of Hispanics in county of client in 2000 (County and City Data Books 2003).	1.5–57.3
Socioeconomic Environment		
Annual wage	Average annual income in 1997 for employees in NAICS subsector 722, in 1,000s (County and City Data Books 2003).	7.795–16.674
Unemployment rate	Unemployment rate in county of client, measured each month (Florida Research and Economic Database).	1.7–19.7
Poverty rate	County poverty rate for all persons in 2000 (U.S. Census Bureau Small Area Income and Poverty Estimates).	6.9–24.2
TANF caseload	Number of TANF recipients per 100,000 county residents (calculated by authors).	.142–6.907
Population	Total county population in 2000, in millions (County and City Data Books 2003).	.007–2.253

Source: Data on client characteristics provided by the Florida Department of Children and Families.
Note: TANF = Temporary Assistance for Needy Families; NAICS = North American Industry Classification System. Descriptive statistics are provided for the combined sample and include data for white, black, and Latino welfare clients.

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Table A2. Regression Results for First-Stage Selection Equation

Independent Variables	β
Individual Characteristics	
Age of client	–.0212**
Age of youngest child	–.0212
Black client	.3411**
Citizenship status (1 = citizen)	.2052**
Education	.0358**
Income (in 1000s)	–.0394**
Number of children	.0387**
Prior sanction (1st spell)	.2632**
Community Characteristics	
Annual wage – food service/drinking places	–.0159
Local conservatism	–.0466**
Percent black	–.0038**
Poverty rate	–.0399**
TANF caseload $_{t-1}$	.2085**
Sample size (first spell clients)	40,891
Number of clients returning for 2nd spell	18.827

Note: The sample for this analysis consists of all new TANF clients (single-parent, female, black or white) who entered TANF from January 2001 through December 2002. The dependent variable is a dichotomous variable equal to 1 for clients who returned to TANF for a second spell during our observation period (and 0 otherwise). Cell entries are coefficients generated from an exponential discrete choice model (see Boehmke et al. 2006), estimated using the DURSEL procedure in Stata 10.0 (Boehmke 2005).
* $p < .05$; ** $p < .01$.

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ANALYSIS & COMMENTARY

Work, Health, And Insurance: A Shifting Landscape For Employers And Workers Alike

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ABSTRACT **We examined the complex relationship among work, health, and health insurance, which has been affected by changing demographics and employment conditions in the United States. Stagnation or deterioration in employment conditions and wages for much of the workforce has been accompanied by the erosion of health outcomes and employer-sponsored insurance coverage. In this article we present data and discuss the research that has established these links, and we assess the potential impact of policy responses to the evolving landscape of work and health. The expansion of insurance availability under the Affordable Care Act may have helped reduce the burden on employers to provide health insurance. However, the act's encouragement of wellness programs has uncertain potential to help contain the rising costs of employer-sponsored health benefits.**

The relationship among work, health, and health insurance in the United States is complex, multidimensional, and continually evolving. Employers rely on healthy employees for business operations. They also play an important role in the US health care system through the widespread provision of health insurance and the growing adoption of workplace wellness programs. Changes in workforce demographic composition and labor-market outcomes, such as labor-force participation and part-time and contingent work, affect workforce health. These relationships have crucial implications for health care utilization and spending, the provision of employer-sponsored insurance, and the impact of policy measures such as the Affordable Care Act (ACA).

In this article we discuss the demographic and economic trends that affect relationships among workforce composition, health, and insurance. These trends include the aging of the US population; evolving employment conditions and relative wages; and changes in reliance on employer-sponsored insurance and other programs that affect health and well-being, such as disability insurance. Health patterns in the working-age population have mimicked labor-market patterns to some degree, with stagnation or deterioration in employment conditions and wages mirrored by declining rates of employer-sponsored insurance coverage and rising incidence of work-preventing disability.

We also discuss the impact of the ACA on these developments. Several of its provisions were aimed at addressing the challenges created by demographic and economic trends—for example, by expanding access to health insurance coverage not tied to employment. The future of these and other provisions is clouded by uncertainty over the future of the ACA following the 2016 election.

Whether or not relevant ACA provisions are revised or overturned, employer-sponsored insurance will likely remain a key source of health coverage in the United States. Employers will continue to grapple with rising premium costs and thus will continue to have a strong interest in the health of their workforces. In recent years, they have increasingly embraced incentive-based

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strategies to induce employee lifestyle changes aimed at improving health. The ACA granted employers increased latitude in designing such wellness incentives. However, these approaches face substantial challenges, including limited evidence of financial returns, social and legal obstacles, and the possible repeal of applicable ACA provisions.

In what follows, we summarize key labor-market trends and research results that shed light on these issues, and we identify important open questions for public policy and further research. The demographic and labor-market developments we discuss are associated with reduced workforce health and declining rates of employer-sponsored insurance coverage. Absent public policies that maintain or expand access to affordable alternatives to employer-sponsored coverage, these developments are likely to result in declines in the share of the population with health insurance.

The Changing Workforce

The composition and characteristics of the US labor market have shifted significantly in recent decades, with some labor-market trends continuing and others newly emerging.[1–3]

DEMOGRAPHIC CHANGES Along with much of the rest of the developed world, the United States has experienced population aging that is expected to continue for at least the next decade.[4] Compared to younger workers, older workers have a higher incidence of adverse health conditions and higher health care spending, which poses potential challenges to employers seeking to finance health care for their aging workforces.

The share of individuals in the labor force older than age 55 rose by about 9 percentage points between 2000 and 2015, mostly as a result of growth in the numbers of individuals of traditional working age (ages 55–64), but the size of the group that is past normal retirement age (ages 65 and older) also increased (Exhibit 1). Projections through 2024 show that the labor force will continue to age—mainly through an increase in the share of participants ages 65 and older—but at a slower pace. Population aging has been reinforced by changing labor-force participation rates, with declines for those younger than age 55 and increases for older participants, especially those ages 65 and older.[1]

The racial and ethnic composition of the labor force has been shifting as well, with rising shares of minority groups—especially Hispanics. This trend is projected to continue through 2024, when Hispanics are projected to make up about 20 percent of the labor force, up from about 12 percent in 2000 (Exhibit 2). On average, the US Hispanic population faces different health risks than the white population does, with Hispanics having a notably higher incidence of potentially manageable chronic conditions such as diabetes, high blood pressure, and obesity.[5] Hispanics have long had substantially lower rates of health insurance coverage, owing to their tendency to hold low-wage jobs that do not offer health benefits.[6]

EMPLOYMENT CONDITIONS AND WAGES Demographic shifts have been accompanied by fundamental changes in labor-market characteristics and outcomes. Sustained shifts away from traditional full-time wage and salary employment to part-time employment and contract work, combined with stagnant real wages for workers across most of the earnings distribution, may have affected health outcomes as well as employer-sponsored insurance.

The share of workers doing voluntary part-time work, which reflects workers' stated preference instead of constraints imposed by employers, was largely stable from 2000 to 2015. However, the incidence of involuntary part-time work, which is dictated by the structure of available jobs, rose substantially during the Great Recession of 2007–09 and has remained unusually elevated since then (Exhibit 3). This may reflect permanent changes in the labor market that have increased employers' reliance on part-time work schedules.[7] Because employer-sponsored benefit plans generally can exclude part-time workers, according to Internal Revenue Service and ACA rules, the increased reliance on part-time work may contribute to reductions in employer-sponsored insurance coverage. The stability of multiple job holding over our study period suggests that the increased incidence of involuntary part-time work has not been accompanied by an increased tendency to piece together multiple part-time jobs to achieve full-time hours.

While self-employment has changed little since 2000, the incidence of alternative work arrangements has risen sharply since 2005. As surveyed and analyzed recently by Lawrence Katz and Alan Krueger,[8] this "gig economy" category is characterized by rapidly growing numbers of temporary, on-call, and contract workers. Although statistics are not consistently available over a long period of time, the analysis by Katz and Krueger indicates a small increase in alternative work arrangements between 1995 and 2005, from 9.3 to 10.1 percent, followed by the sharp increase between 2005 and 2015 shown in Exhibit 3 (to 15.8 percent).

Another important long-term trend in the US labor market is the decline in union membership. In 2000, 13.4 percent of workers belonged

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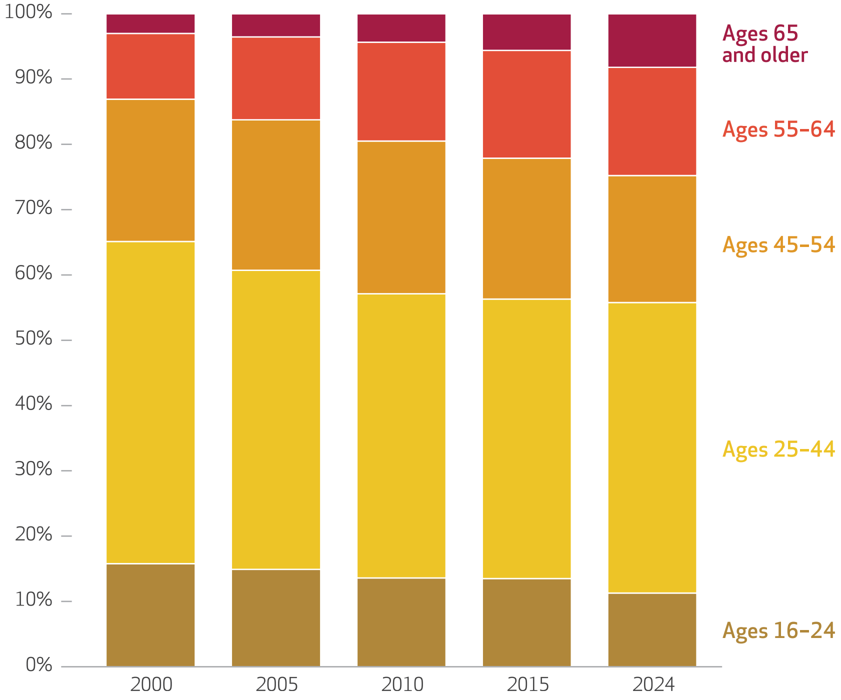
EXHIBIT 1

Composition of the US labor force, by age range, selected years 2000–24



SOURCE Authors' analysis of data for 2000–15 from the Bureau of Labor Statistics via Haver Analytics, and projections for 2024 from Toossi M. Labor force projections to 2024 (see Note 1 in text).

to a union (Exhibit 3), down substantially from one in five in the early 1980s (data not shown). Since 2000, union membership has continued to decline, though at a slower rate: It fell to 11.1 percent in 2015 (Exhibit 3). Comprehensive health benefits have long been a hallmark of union jobs, so declining unionization tends to be associated with an erosion of employer-sponsored insurance.[9]

All of the above labor-market developments likely contributed to the decline in employer-sponsored insurance, which fell by nearly 10 percentage points from 2000 to 2010 (Exhibit 3). This decline was partially offset by a rise in public coverage, mainly Medicaid. The impact of the ACA is evident in the changes between 2010 and 2015, with significant increases in rates of public coverage and individual private health insurance and a corresponding reduction in the fraction of the population with no health insurance.

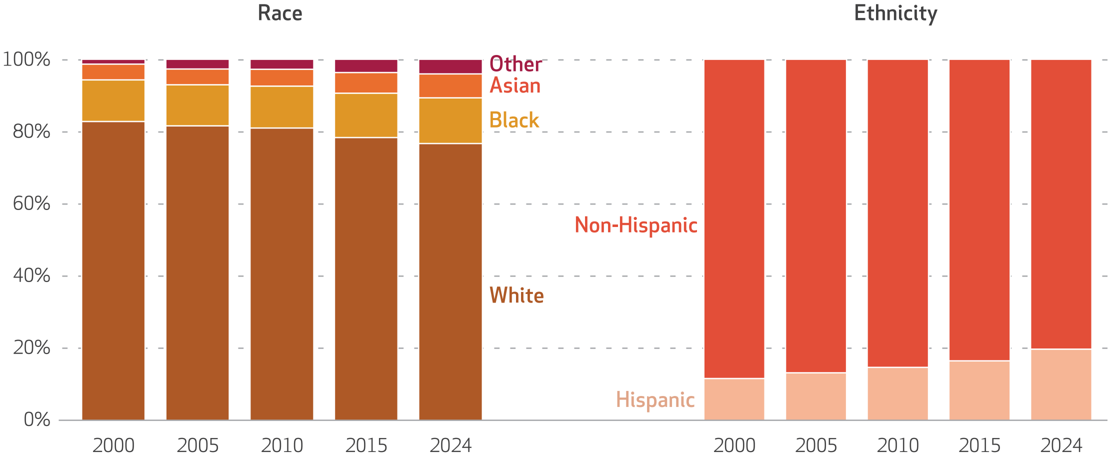
These labor-market trends have been accompanied by a significant increase in permanent (work-preventing) disability claims through Social Security Disability Insurance from 2000 to 2010, followed by a slower rate of increase from 2010 through 2015 (Exhibit 3). We discuss disability claims further in a subsequent section.

Changes in real (inflation-adjusted) wages and the relative wage distribution also are important parts of the labor-market landscape. Real wages have been largely flat or even declined in recent decades for large segments of the working population. From 2000 through 2015, real wages fell slightly for men earning low wages (those at the tenth percentile of the wage distribution) and rose slightly for women earning low wages, as well as for median earners of both genders. By contrast, real wages rose substantially for workers at the ninetieth percentile of the wage distribution.

Research by David Autor[10] and others[2] sug-

EXHIBIT 2

Composition of the US labor force, by race and ethnicity, selected years 2000–24



SOURCE Authors' analysis of data for 2000–15 from the Bureau of Labor Statistics via Haver Analytics, and projections for 2024 from Toossi M. Labor force projections to 2024 (see Note 1 in text).

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EXHIBIT 3

Employment and health insurance characteristics of the US workforce, selected years 2000–15

	2000	2005	2010	2015	Change, 2000 to 2015
EMPLOYMENT CHARACTERISTICS					
Part-time work	16.8%	17.4%	19.7%	18.4%	1.5%
Voluntary	14.3	14.3	13.6	13.9	−0.3
Involuntary	1.5	2.1	5.0	3.5	2.0
Multiple job holding	5.6	5.3	4.9	4.9	−0.7
Self-employed (unincorporated)	7.5	7.4	7.0	6.4	−1.1
Alternative work arrangements[a]	—[b]	10.7	—[b]	15.8	—[b]
Union membership	13.4	12.5	11.9	11.1	−2.3
TYPE OF HEALTH INSURANCE					
Employer-sponsored	65.1%	60.7%	55.3%	55.7%	−9.4%
Other private	10.2	9.9	9.9	16.3	6.2
Public	24.4	27.3	31.2	37.1	12.7
No insurance	13.1	14.6	16.3	9.1	−4.0
DISABILITY BENEFIT RECEIPT					
Social Security Disability Insurance	3.0%	3.7%	4.4%	4.7%	1.6%
Supplemental Security Income	4.0	4.0	4.3	4.3	0.4
WAGES (2015 DOLLARS)					
10th percentile					
All	$8.60	$8.74	$8.70	$9.00	4.6%
Men	9.36	9.23	9.24	9.25	−1.2
Women	8.26	8.50	8.70	8.64	4.6
Median (50th percentile)					
All	$17.20	$17.80	$17.75	$18.00	4.6%
Men	19.56	19.61	19.57	19.71	0.8
Women	15.14	15.78	16.31	16.03	5.8
90th percentile					
All	$38.37	$40.85	$41.81	$44.44	15.8%
Men	43.01	45.52	47.03	49.52	15.1
Women	33.07	35.60	37.10	38.46	16.3

SOURCE Authors' analysis of data from the following sources: on labor force and employment, from the Bureau of Labor Statistics; on type of health insurance, from the Census Bureau; on disability benefits, from the Social Security Administration; on wages, from the Merged Outgoing Rotation Groups of the Current Population Survey; on alternative work arrangements, from Katz LF, Krueger AB. The rise and nature of alternative work arrangements in the United States (see Note 8 in text). **NOTES** Labor-force and employment data are for people ages 16 and older. Disability benefits data are for people ages 20–64. Type of health insurance is based on population shares; percentages sum to more than 100 because of overlapping coverage. Wage data were adjusted by the Consumer Price Index for All Urban Consumers Research Series. The change listed in the final column is affected by rounding errors and may not equal the difference between the 2015 and 2000 values shown. [a]Alternative work arrangements represent the share of all workers employed as temporary help agency workers, on-call workers, contract workers, and independent contractors (2015 numbers correspond to "alternative weight" estimate from the source; see Note 8 in text). [b]Not available.

gests that wage gains at the top of the distribution and relative stagnation in the middle and at the bottom are likely the result of factors related to changing technology, especially the evolving reliance on computers and other forms of information technology, along with changing patterns in international trade. These factors have undermined the demand for the skills needed in traditional middle- and low-wage occupations, which include various blue- and white-collar jobs that involve routine or repetitive tasks that are readily mechanized or outsourced (such as certain types of office and factory work).[2,10] Wage stagnation and corresponding slow growth in living standards may be related to adverse health outcomes for some workers.

Work And Health

A recent article by Andrea Bassanini and Eve Caroli[11] reviewed a number of studies exploring whether work is bad for health. The literature suggests that working long hours undermines health, Bassanini and Caroli concluded. While the studies they reviewed provided limited insight into exact causal mechanisms, it appears that negative effects are strongest in situations where workers face constraints that create gaps between actual and desired work hours.

It is useful to consider these findings in light of the labor-market changes. Some trends, such as the rise in alternative work arrangements, may help enhance workers' health by enabling greater flexibility in hours worked. However, to the

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extent that workers are prompted to work in alternative settings in response to economic stress, this may undermine health.

Bassanini and Caroli's findings are also useful for interpreting the extensive literature about the effects of the business cycle or economic shocks on health. Various studies, including one by Christopher Ruhm,[12] have found that economic downturns tend to improve health outcomes, including reductions in mortality from heart attacks and auto fatalities and general reductions in obesity and in adverse health-related behaviors such as smoking and alcohol consumption. A common interpretation of this relationship is that when the economy is strong, the opportunity cost of healthy behaviors is higher: People work longer hours and have less time to exercise or prepare healthy meals, for example.

In contrast, other studies suggest that adverse economic conditions tend to exacerbate poor mental health[12–14]—particularly for groups of workers with poor economic prospects, including members of minority groups and people with less education.[15] Ruhm recently updated his original study, extending the analysis period to 2010. Consistent with the results of these studies on the relationship between macroeconomic conditions and mental health, he found that deaths from suicides and drug overdoses increase when unemployment rises.[16]

In a related development, the use and misuse of prescription opioids has grown tremendously.[17] In recent work, Anne Case and Angus Deaton argue that this trend is likely a contributing factor to the significant increase in relative mortality among middle-aged white men in the United States, especially less educated men—who have been most adversely affected by the structural changes in the US labor market.[18] Moreover, other research indicates that there are significant and long-lasting increases in mortality resulting from involuntary job loss, with the effects growing based on the size of the associated wage loss.[19] These findings are consistent with Bassanini and Caroli's view that labor-market constraints have adverse health impacts.

This interpretation also finds support in research on the relationship between inequality and health. It has been well established that adverse economic conditions, especially low income, are associated with increased mortality and other indicators of poor health, although causality is likely to run in both directions.[20–22] This relationship is reinforced by unequal access to health insurance, which tends to increase measured inequality in overall income.[23]

The Rise Of Disability

The decline in labor-force participation among adults under age 55 noted above has coincided with an increase in self-reported health problems among nonworkers, especially men. In a recent study, Krueger found that 43 percent of men ages 25–54 who are not in the labor force report their health as fair or poor, and roughly one-third report having a disability. Moreover, many of these male nonparticipants report that they take prescription pain medications daily.[24] As emphasized by Case and Deaton, the rising incidence of work-preventing disability, as reflected in the rise in Social Security Disability Insurance claims over the past few decades, may be related to the adverse labor-market developments documented above. Some research suggests that rising disability claims reflect, in part, the increased value of disability payments relative to real wages.[25] By contrast, other researchers argue that workforce aging largely accounts for the trend in rising disability claims.[26] Given the labor-force trends shown in Exhibits 1–3, this trend is likely to continue.

The ACA And Beyond

The link between work and health is particularly significant in the United States, given the central role that employers play in financing health care. Wage stagnation for middle-income workers, combined with the rapid growth of health care expenditures, contributed to the gradual but steady decline in employer-sponsored insurance coverage over the two decades leading up to the passage of the ACA.[27]

The ACA was designed in part to reverse the decline in insurance coverage by creating new options not tied to employment. The expansion of Medicaid eligibility for low-income childless adults and the availability of federal tax credits and subsidies for purchasing individual and family insurance through the Marketplaces have increased insurance availability for millions of Americans, including low- and middle-wage workers. Some analysts predicted that after passage of the ACA many employers would stop offering health benefits, leaving their employees to obtain coverage on their own.[28] However, others argued that a substantial decline in the availability of employer-sponsored insurance was unlikely, especially for large employers—which account for the bulk of this coverage.[29] Indeed, recent studies have found very little decline in employer-sponsored insurance offerings following the implementation of major ACA coverage provisions in 2014.[30,31]

Our data also show that employer-sponsored insurance coverage remained essentially con-

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stant between 2010 and 2015. In part, this may reflect a steady improvement in labor-market conditions and employment over this period. At the same time, the stabilization of employer-sponsored insurance is similar to the pattern observed in Massachusetts, which enacted health care legislation in 2006 that had many similarities to the ACA.[32]

The future of the ACA remained uncertain in late 2016, when this article was written. Early indications suggest that the new administration and Congress may roll back ACA provisions that expanded insurance access. Unless these changes are offset by other measures that expand coverage, workers in nontraditional employment arrangements and labor-force nonparticipants who do not qualify for Social Security Disability Insurance will encounter renewed financial barriers to obtaining health insurance. The elimination of these ACA provisions would further reinforce the dominance of employer-sponsored insurance at the same time that rising costs would create increasing challenges for employers that wish to maintain their health benefit programs.

Wellness Programs: Changing The Relationship Between Work And Health?

As the dominant source of private health insurance in the United States, employers will continue to grapple with rising health care costs. In recent years, many employers have embraced "wellness" strategies that give employees incentives to make lifestyle changes that could reduce the cost burden of chronic conditions. Common workplace wellness programs feature health risk assessments and physical exams to measure risk factors such as elevated blood pressure and cholesterol, as well as a variety of interventions to address these risk factors. One recent survey suggests that about 80 percent of large firms offer wellness programs.[33]

Increasingly, employers have been using financial incentives in their wellness programs, with rewards based on program participation or achievement of a particular health outcome, such as quitting smoking, exercising, or losing weight. The use of "health-contingent" incentives is constrained by federal regulations designed to prevent discrimination based on health or disability status. However, the ACA granted employers greater latitude in designing such incentives, increasing the maximum permissible incentive from 20 percent to 30 percent of the cost of health coverage, or up to 50 percent for reductions in smoking.

Given the strong links between behavior and costly chronic conditions such as diabetes, hypertension, and cardiovascular disease, the potential for these programs to reduce health care costs seems self-evident to many employers. According to one national survey, more than 90 percent of employers believed they could reduce their health care costs by getting employees to adopt healthier lifestyles.[34]

To reduce costs, however, wellness programs must induce sufficient behavioral modifications to lead to material health improvements that, in turn, translate into reduced use of health services. Employers' widespread reliance on financial incentives reflects low program participation in the absence of a "nudge." Research shows that financial incentives increase participation,[35] though if these incentives come in the form of rewards—as is usually the case—they raise program costs. Thus, any savings arising from behavioral changes must be large enough to offset program costs, which include rewards to employees whose behavior and health status were already in line with program goals. Moreover, from the perspective of an individual employer, wellness programs might not be financially advantageous, because some of the cost reductions due to improved health accrue slowly over time and hence will be shared by a worker's future employers and Medicare.[36,37]

Whether or not wellness incentives lead to cost savings, such strategies raise important questions. Critics argue that some programs violate employees' privacy and can discriminate against workers in poor health.[38] In addition, because there is a strong socioeconomic gradient for the conditions that are typically targeted, the burden of behavioral modifications as well as penalties for not achieving program goals fall disproportionately on lower-income employees.[39]

Even before the new rules created by the ACA, wellness programs were subject to a fragmented federal regulatory regime.[40] And recently these programs have been the subject of litigation, with the Equal Employment Opportunity Commission suing two large employers because—the commission argues—their incentive programs violated the Americans with Disabilities Act of 1990.[41] As a result, the future status of these programs remains uncertain.

Discussion

How will the US labor market evolve in the future? The aging of the workforce is projected to continue (Exhibit 1), as is the increase in the share that is Hispanic (Exhibit 2).[42] Technological change is likely to continue to constrain opportunities for low- and middle-skill workers, which will increase income inequality. The "gig

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economy" and contract work more generally are likely to expand further and may eventually account for a significant share of labor-market activity. The research findings that we have summarized suggest that these labor-market changes may pose increasing challenges for the health of the US working population, while at the same time reducing access to employer-sponsored health insurance.

Health policies cannot reverse broader trends in the labor market, but they can play an important role in mediating the impact of these trends on workers' well-being. Early evidence on the effects of the ACA provides a sense of this potential. Between 2010—when the law was passed—and 2015, the number of Americans lacking health insurance fell by roughly twenty million.[43] Because the gains in coverage have been most pronounced at the lower end of the income distribution, they have offset increases in income inequality.[23] Gains were also greater for ethnic minorities, particularly Hispanics, which reduced disparities in insurance coverage—if only slightly.[44]

Importantly, by creating new health insurance options that are not tied to full-time employment, the ACA strengthened the safety net in a way that should mitigate some of the negative aspects of the evolving labor market, should relevant portions of the law remain in effect. Affordable alternatives to employer-sponsored insurance are not only an important source of insurance for workers in low-wage jobs or alternative work arrangements, but they may also affect applications for disability benefits. The relationship between the availability of health insurance and disability is complex, though recent research on the Massachusetts health reform suggests that it had the effect of reducing applications for disability benefits.[45] All of these considerations suggest that legislative reversals of key ACA provisions, absent the implementation of alternative policies, may intensify the barriers that many working-age people encounter as they seek affordable health care.

For employers that offer health benefits, controlling the growth of spending will remain a critical objective. Incentive-based wellness strategies are the latest innovation in this area. These programs can be seen as the extension of both managed care strategies (such as disease management programs) and consumer-directed health plans (which emphasize demand-side financial incentives). At the same time, the emergence of these programs can be seen as a fundamental shift in the role that employers play in the US health care system—from simply paying for care to attempting to directly affect the health of their employees. Given the high and growing cost burden of chronic conditions, the logic of targeting the health behaviors linked to those conditions is compelling. However, these programs face important legal and practical challenges, and their effectiveness remains uncertain. ■

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Medicaid Versus Marketplace Coverage For Near-Poor Adults: Effects On Out-Of-Pocket Spending And Coverage

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ABSTRACT In states that expanded Medicaid eligibility under the Affordable Care Act, nonelderly near-poor adults—those with family incomes of 100–138 percent of the federal poverty level—are generally eligible for Medicaid, with no premiums and minimal cost sharing. In states that did not expand eligibility, these adults may qualify for premium tax credits to purchase Marketplace plans that have out-of-pocket premiums and cost-sharing requirements. We used data for 2010–15 to estimate the effects of Medicaid expansion on coverage and out-of-pocket expenses, compared to the effects of Marketplace coverage. For adults with family incomes of 100–138 percent of poverty, living in a Medicaid expansion state was associated with a 4.5-percentage-point reduction in the probability of being uninsured, a $344 decline in average total out-of-pocket spending, a 4.1-percentage-point decline in high out-of-pocket spending burden (that is, spending more than 10 percent of income), and a 7.7-percentage-point decline in the probability of having any out-of-pocket spending relative to living in a nonexpansion state. These findings suggest that policies that substitute Marketplace for Medicaid eligibility could lower coverage rates and increase out-of-pocket expenses for enrollees.

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The Affordable Care Act (ACA) expanded eligibility for Medicaid for near-poor nonelderly adults—those with family incomes below 138 percent of the federal poverty level. However, the US Supreme Court's 2012 ruling allowed states to opt out of the Medicaid expansion. In the nineteen states that had chosen not to expand Medicaid as of October 2017, most adults with family incomes of 100–400 percent of poverty[1]—but generally not those with family incomes below 100 percent of poverty—may qualify for tax credits to purchase Marketplace plans if they do not have access to affordable employer-sponsored coverage.

Important differences exist in the cost-sharing provisions applicable to various groups of people with incomes of 100–138 percent of poverty, depending upon Medicaid expansion status. In nonexpansion states, premium tax credits for people in this income range cap premiums for the second-lowest-cost silver plan at 2.0 percent of income, and cost-sharing reduction subsidies increase the actuarial value of a silver plan to 94 percent. In contrast, in expansion states, adults with incomes below 138 percent of poverty typically face no premiums and minimal cost-sharing requirements.[2]

In addition to lower premiums and cost-sharing requirements, Medicaid expansion could also affect consumers' financial situation through higher take-up and coverage eligibility compared to Marketplace coverage. In contrast to subsidized Marketplace coverage, Medicaid enroll-

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ment typically does not require premiums, is available on a retroactive basis, and can occur year-round with no restrictions (that is, there is no open enrollment period)—all of which may contribute to higher take-up of Medicaid than of Marketplace coverage.[3–6] Moreover, in contrast to people with Medicaid, those with access to employer-sponsored coverage with out-of-pocket premiums totaling less than 9.5 percent of their income (adjusted annually) are not eligible for Marketplace subsidies. Thus, fewer people with incomes of 100–138 percent of poverty are eligible for insurance with financial assistance in states that did not expand Medicaid.

While no published research, to our knowledge, has quantified differences in out-of-pocket spending in Medicaid relative to that in Marketplace plans, several studies have evaluated the effects of Medicaid on financial well-being. Adults in Medicaid expansion states experienced larger reductions in the probability of having any out-of-pocket spending compared to Marketplace enrollees but faced greater difficulty in accessing physician care.[7] The Oregon Health Insurance Experiment found that Medicaid coverage reduced the likelihood of borrowing money or skipping bills to pay for medical care by 40 percent and reduced the probability of having a medical debt collection by 25 percent.[8] A national study also found that Medicaid expansion reduced difficulty paying medical bills among low-income parents.[9] Meanwhile, another study found that adults in Kentucky (a traditional Medicaid expansion state) with incomes below 138 percent of poverty experienced a greater reduction in problems paying medical bills than comparable adults in Arkansas, a "private option" expansion state that features the maximum allowable cost sharing under Medicaid rules.[10]

For this study we used data from the Census Bureau's Current Population Survey (CPS) and the American Community Survey (ACS) to analyze out-of-pocket health spending and insurance coverage of near-poor nonelderly adults in Medicaid expansion states compared to near-poor nonelderly adults in nonexpansion states with potential access to subsidized Marketplace plans. Throughout the remainder of this text, we state "Medicaid expansion relative to Marketplace coverage" as shorthand for this comparison. This research is important for states as they consider expansion and make design choices in their Medicaid programs in the coming years. Six states have received section 1115 Medicaid expansion waivers allowing Medicaid to charge premiums for people with incomes of 100–138 percent of poverty.[11] Moreover, Arkansas, which—as noted above—has implemented a "private option" for Medicaid, has submitted a waiver request to lower the eligibility level to 100 percent of poverty from the current 138 percent, while at least five other states have drafted plans to place other limits on existing Medicaid expansions.[11,12] Understanding how out-of-pocket spending and coverage rates differentially changed for nonelderly adults who had incomes of 100–138 percent of poverty and who were eligible for either Medicaid or Marketplace coverage is important to informing pending state and federal policy decisions.

Study Data And Methods

DATA AND SAMPLE We used data for 2011–16 from the CPS Annual Social and Economic Supplement to assess out-of-pocket spending levels in 2010–15. Information covering 2010–13 and 2014–15 provide data for the periods before and after the ACA Medicaid expansion, respectively. The CPS collects individual-level data on income, health insurance coverage, state of residence, and demographic and socioeconomic characteristics. CPS data also include detailed information on out-of-pocket premium and nonpremium medical spending.[13] We refer to nonpremium out-of-pocket medical spending as *cost sharing*. The CPS sample is nationally representative and includes an annual sample of more than 7,000 nonelderly adults with incomes of 100–138 percent of poverty.

We did not use the CPS's insurance information in our main model because of a fundamental redesign of the health insurance questionnaire in 2014 that precludes direct comparisons to estimates from prior years.[14] Instead, we used data for 2010–15 from the ACS to assess the impacts of Medicaid expansion on coverage status in this income group. The ACS surveys approximately three million people each year and, in contrast to the CPS, asked a consistent set of insurance questions over the study period.

We limited our analytic sample to adults ages 19–64 with incomes of 100–138 percent of poverty, and we took into account immigration requirements for eligibility.[15] To approximate ACA-related eligibility for Medicaid and Marketplace coverage, we constructed health insurance units and a measure of Modified Adjusted Gross Income to define income groups. We also imputed documentation status on the CPS for noncitizens using a method developed by the Pew Research Center.[16] For the ACS sample, we excluded noncitizens and people with Medicare.

Our sample for both analyses included forty-four states. We excluded four states that expanded Medicaid after mid-2014 and before 2016 (Alaska, Indiana, New Hampshire, and Pennsyl-

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vania) because post-ACA data for those states would contain a mixture of expansion and nonexpansion periods. We also excluded two states (Massachusetts and Vermont) and the District of Columbia—all of which expanded public coverage to childless adults with incomes of up to 138 percent of poverty before 2014—because they were significantly less affected by the 2014 expansion. As sensitivity tests, we included and excluded various combinations of states. For example, we excluded other states that expanded Medicaid under the ACA before 2014 and states that had expanded Medicaid for some adults before the ACA.

As an alternative specification for the CPS analysis, we use a shorter pre-2014 period (2013 only), because of changes made by the Census Bureau to the income questions on the 2014 survey designed to improve the accuracy of reporting. The change created a split-sample design in which about 30 percent of the sample received the redesigned questions and the remaining 70 percent received the traditional income questions. For our alternative specification test, we included the 2014–15 sample and the portion of the 2013 sample that received the redesigned income questions.

STATISTICAL ANALYSES We estimated difference-in-differences models to compare key coverage and spending outcomes for people with incomes of 100–138 percent of poverty in Medicaid expansion states versus those in nonexpansion states. The key independent variables in each model included an indicator set to 1 for people who lived in Medicaid expansion states (Medicaid), a variable set to 1 for all observations in 2014 or later (Post), and an interaction term (Post*Medicaid) that measured the change in the outcome in expansion states relative to the change in nonexpansion states.

For the ACS coverage analysis, we estimated linear probability difference-in-differences models in which the dependent variables were indicators for being uninsured, being covered by Medicaid, having employer-sponsored insurance (including military coverage), and having direct-purchase coverage (inside or outside the Marketplaces).[17] Given potential concerns about measurement error in the specific type of coverage reported in the ACS,[18] we placed greater credence in our estimates of the impacts on any coverage than in coverage type.

For the CPS out-of-pocket spending analysis, we analyzed three general outcomes: total out-of-pocket spending, out-of-pocket premium spending, and cost sharing. For each of these outcomes, we estimated the following models: an ordinary least squares regression model in which the dependent variable was the person's level of expenses, a linear probability model in which the dependent variable was equal to 1 if the person's family out-of-pocket spending exceeded 10 percent of the family income (high out-of-pocket burden),[19] and a two-part model to account for the large share of zeros in the data. For the two-part model, we estimated linear probability models in the first stage, in which the dependent variable was equal to 1 for people with nonzero expenses, and ordinary least squares models in the second stage, in which the dependent variable was the level of expenses among those with nonzero spending. We adjusted out-of-pocket premium and medical spending for inflation using the Consumer Price Index, and all spending estimates are in 2015 dollars.

For both analyses, each model controlled for several individual and household characteristics—age, sex, race/ethnicity, educational attainment level, work status, citizenship status, and family structure—that could affect coverage or out-of-pocket spending. We also controlled for fixed differences across years (year fixed effects) and geographic areas (state fixed effects for the CPS and Public Use Microdata Area fixed effects for the ACS). For the CPS analysis, we also included an indicator of whether respondents received the traditional or redesigned income questions, to control for changes in the CPS income definition during the analysis period.

We also used various sensitivity tests and subgroup analyses to help identify causal effects and verify the robustness of our models, as further described in the online appendix.[20] We made changes to the income bands to address potential measurement error in income, reestimating the main model to include people with incomes slightly below (75–100 percent of poverty) and slightly above (138–150 percent of poverty) the income band of those in the main model. As a falsification test, we also estimated out-of-pocket spending and coverage impacts among families in higher income bands (150–200 percent and 200–400 percent of poverty), because the ACA coverage provisions for this income group are, for the most part, the same in expansion and nonexpansion states.[21] To formally test for differences in trends, we estimated models in which a 2010–13 linear time trend was interacted with the Medicaid expansion dummy variable.

For the CPS analysis, we used replicate weights designed by the Census Bureau to generate empirically derived standard error estimates. For the ACS analysis, we report robust standard errors clustered at the state level.

LIMITATIONS There were several limitations to this study. First, there was potential for recall error and other forms of measurement error in annual income, as respondents reported multi-

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ple sources of income for themselves and members of their households.[22,23] In part, we addressed this concern by changing the income band definition as a sensitivity test. Similarly, the presence of income "churn" could influence the interpretation of the results, because some people who had full-year incomes of 100–138 percent of poverty may have had incomes below 100 percent or above 138 percent of poverty for part of the year. Since we might have misclassified people's eligibility for subsidized coverage in both expansion and nonexpansion states because of imperfectly measured income and lack of information on offers of affordable employer-sponsored coverage, the net effect of that measurement error would likely be to bias our estimates toward the null (that is, no difference).

Second, between March 2013 and March 2014 there were changes to the CPS in the wording of the questions about out-of-pocket spending and the imputation process for missing responses.[24] The new questions were ordered differently, were shortened to reduce respondent burden, and included a reference to the respondent's employer contribution to the premium, when applicable. We addressed this concern by limiting our sample to data for the period 2013–15, during which the questions on out-of-pocket spending and the imputation process were unchanged.

Third, the 2013 income data for the portion of the sample receiving redesigned CPS income questions can be consistently compared with income data for 2014 and 2015, but not earlier years. We addressed this by estimating a sensitivity model limited to those in the 2013–15 sample who received the redesigned income questions. The concern was also mitigated by the fact that the new income questions were primarily designed to improve the capture of retirement and asset income,[25] changes that were unlikely to have a significant impact on our sample of low-income, nonelderly adults.

Finally, as with any quasi-experimental analysis, time-varying unobservable factors might have biased our estimated effects. For example, Medicaid expansion states might have done a better job with outreach and enrollment efforts, which could have further boosted take-up relative to nonexpansion states. While our falsification tests, pre-2014 trend analyses, and sensitivity analyses were designed to minimize these risks, some potential for bias remains.

Study Results

COVERAGE CHANGES FROM THE AMERICAN COMMUNITY SURVEY Low-income adults experienced unprecedented changes in health insurance coverage in both expansion and nonexpansion states between 2010–13 and 2014–15. The uninsurance rate among adults with incomes of 100–138 percent of poverty declined by 16.4 percentage points in Medicaid expansion states and by 11.7 percentage points in nonexpansion states during this period (exhibit 1 and appendix exhibit A15).[20] The adjusted difference-in-differences estimates show that Medicaid expansion was associated with a 4.5-percentage-point reduction in the probability of being uninsured among sample adults, other things being equal.

This significant decline in the uninsurance rate in expansion states relative to that in nonexpansion states was primarily driven by larger increases in Medicaid coverage in expansion states. Between 2010–13 and 2014–15, the share of sample adults in expansion states covered by Medicaid increased by 11.9 percentage points, while the share covered by Medicaid in nonexpansion states increased by less than 1.0 percentage point. This increase in Medicaid coverage in expansion states was partially offset by a relative decline in private coverage, particularly directly purchased coverage—which is by design. Employer-sponsored insurance and directly purchased private insurance coverage rates increased in both expansion and nonexpansion states during this period, but significantly larger increases occurred in nonexpansion states.

Estimates from sensitivity analyses were generally consistent with the overall findings. First, difference-in-differences estimates from the CPS coverage model were similar to the ACS findings (appendix exhibit A1).[20] Second, the ACS difference-in-differences uninsurance estimates were significantly smaller among people with incomes of 200–400 percent of poverty (appendix exhibit A2).[20] However, we found similar, yet slightly smaller, difference-in-differences estimates among those with incomes of 150–200 percent of poverty, which points to the potential presence of measurement error in income or unmeasured factors correlated with Medicaid expansion that increased take-up among people in that income band beyond differences between Medicaid and the Marketplace. Finally, we found no evidence of differential trends driving the overall coverage findings (appendix exhibit A3).[20]

CHARACTERISTICS OF THE CURRENT POPULATION SURVEY STUDY SAMPLE Appendix exhibit A4 compares sample characteristics from the CPS for people with incomes of 100–138 percent of poverty in expansion states and nonexpansion states in the 2010–13 and 2014–15 periods.[20] People in expansion and nonexpansion states were generally similar in terms of sex, age, work status, family structure, and levels of educational

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EXHIBIT 1

Difference-in-differences in health insurance coverage of adults ages 19–64 with family incomes of 100–138 percent of the federal poverty level in Medicaid expansion versus nonexpansion states, from 2010–13 to 2014–15

	Unadjusted mean		Difference	Difference-in-differences	
Type of coverage	2010–13	2014–15	between periods	Unadjusted	Adjusted
UNINSURED					
Expansion states	0.352	0.188	–0.164	–0.047***	–0.045***
Nonexpansion states	0.429	0.311	–0.117		
MEDICAID					
Expansion states	0.176	0.294	0.119	0.112***	0.111***
Nonexpansion states	0.099	0.106	0.007		
EMPLOYER SPONSORED OR MILITARY					
Expansion states	0.394	0.418	0.024	–0.020***	–0.023***
Nonexpansion states	0.403	0.447	0.044		
DIRECT PURCHASE					
Expansion states	0.078	0.099	0.021	–0.046***	–0.043***
Nonexpansion states	0.069	0.136	0.067		

SOURCE Authors' analysis of data for 2010–15 from the American Community Survey. **NOTES** Medicaid expansion states include those that expanded eligibility for Medicaid in the first half of 2014 or earlier. The estimates exclude states that expanded Medicaid in late 2014 or 2015 (Alaska, Indiana, New Hampshire, and Pennsylvania) and the District of Columbia, Massachusetts, Vermont—all of which expanded Medicaid to childless adults before the ACA was implemented. Adjusted differences-in-differences are estimated controlling for age, sex, race/ethnicity, educational attainment, work status, family structure, urban versus rural residence, activity limitations, and Public Use Microdata Area and year fixed effects. Coverage type estimates are based on the following hierarchy: Medicare, employer-sponsored insurance or military insurance, Marketplace or direct purchase, Medicaid or other public, and uninsured. Regression models are estimated using ordinary least squares. Estimates exclude noncitizens and adults with Medicare or Supplemental Security Income. ***$p < 0.01$

attainment. Adults in expansion states were more likely to be noncitizens and Hispanic and less likely to be non-Hispanic blacks than those in nonexpansion states.

CHANGES IN OUT-OF-POCKET SPENDING In Medicaid expansion states, average total out-of-pocket spending decreased by $42, from $1,014 in 2010–13 to $972 in 2014–15 (exhibit 2 and appendix exhibit A15).[20] In contrast, among the same income group in nonexpansion states, average total out-of-pocket spending increased by $326, from $1,086 to $1,412.[26] Overall, estimates from the regression-adjusted difference-in-differences model show that the Medicaid expansion, relative to Marketplace coverage, reduced average total out-of-pocket spending by $344.

The regression-adjusted difference-in-differences estimates in exhibit 2 also show that relative to available Marketplace coverage in nonexpansion states, Medicaid expansion was associated with a 4.1-percentage-point reduction in the probability of having a high total out-of-pocket spending burden and a 7.7-percentage-point reduction in the probability of having any out-of-pocket spending. These changes were primarily driven by significant increases in these spending outcomes in nonexpansion states. The difference-in-differences estimate for average total out-of-pocket expenses among those with any spending was not significant at the 10 percent level, a finding that is consistent in the remaining exhibits.

The impacts from the total out-of-pocket spending models were generally driven by differential changes in both out-of-pocket premiums and cost sharing in expansion and nonexpansion states. For the first three models (average premium spending, high premium spending burden, and any premium spending), out-of-pocket premium spending increased among sample adults in both expansion and nonexpansion states (exhibit 3). However, these increases were significantly higher in nonexpansion states. The regression-adjusted difference-in-differences estimates show that relative to access to subsidized Marketplace coverage in nonexpansion states, Medicaid expansion was associated with lower average out-of-pocket premium spending (–$125), a lower probability of having a high out-of-pocket premium spending burden (that is, premium spending more than 10 percent of income) (–2.6 percentage points), and a lower probability of having any out-of-pocket premium spending (–7.5 percentage points).

Consistent with the outcomes discussed above, Medicaid expansion was associated with lower average cost-sharing spending (–$218) and a lower probability of having any cost shar-

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EXHIBIT 2

Difference-in-differences in total out-of-pocket spending of adults ages 19–64 with family incomes of 100–138 percent of the federal poverty level in Medicaid expansion versus nonexpansion states, from 2010–13 to 2014–15

	Unadjusted mean		Difference between periods	Difference-in-differences	
	2010–13	2014–15		Unadjusted	Adjusted
AVERAGE OUT-OF-POCKET SPENDING					
Expansion states	$1,014	$972	−$42	−$368***	−$344**
Nonexpansion states	$1,086	$1,412	$326		
HIGH OUT-OF-POCKET SPENDING BURDEN[a]					
Expansion states	0.211	0.212	0.001	−0.048***	−0.041***
Nonexpansion states	0.229	0.278	0.049		
ANY OUT-OF-POCKET SPENDING					
Expansion states	0.593	0.574	−0.019	−0.089***	−0.077***
Nonexpansion states	0.615	0.685	0.070		
AVERAGE OUT-OF-POCKET SPENDING, CONDITIONAL ON ANY OUT-OF-POCKET SPENDING					
Expansion states	$1,711	$1,694	−$17	−$312	−$295
Nonexpansion states	$1,766	$2,061	$295		

SOURCE Authors' analysis of data for 2011–16 from the Current Population Survey's Annual Social and Economic Supplement. **NOTES** Total out-of-pocket spending includes out-of-pocket premium spending and cost sharing (in 2015 dollars). The estimates exclude immigrants imputed as undocumented. Medicaid expansion states include those that expanded Medicaid in the first half of 2014 or earlier. The estimates exclude the states listed in the notes to exhibit 1 and the District of Columbia. Adjusted differences-in-differences are estimated controlling for age, sex, race/ethnicity, educational attainment, work status, citizenship status, family structure, state and year fixed effects, and an indicator of whether the respondent received the traditional or redesigned income questions if they were in the March 2014 sample. Standard errors are calculated using CPS replicate weights. All models are estimated using ordinary least squares. [a] Family out-of-pocket spending exceeded 10 percent of family income. **$p < 0.05$ ***$p < 0.01$

ing (−7.0 percentage points) (exhibit 4). However, the 0.9-percentage-point decline in high cost-sharing spending burdens (that is, cost sharing more than 10 percent of income) was not significant at the 10-percent level.

To summarize, Medicaid expansion (relative to Marketplace access) reduced the uninsurance rate by 4.5 percentage points, the share of people

EXHIBIT 3

Difference-in-differences in out-of-pocket premium spending of adults ages 19–64 with family incomes of 100–38 percent of the federal poverty level in Medicaid expansion versus nonexpansion states, from 2010–13 to 2014–15

	Unadjusted mean		Difference between periods	Difference-in-differences	
	2010–13	2014–15		Unadjusted	Adjusted
AVERAGE OUT-OF-POCKET PREMIUM SPENDING					
Expansion states	$544	$579	$36	−$141***	−$125**
Nonexpansion states	$546	$722	$176		
HIGH OUT-OF-POCKET PREMIUM SPENDING BURDEN[a]					
Expansion states	0.117	0.124	0.007	−0.030***	−0.026**
Nonexpansion states	0.127	0.164	0.037		
ANY OUT-OF-POCKET PREMIUM SPENDING					
Expansion states	0.211	0.253	0.042	−0.081***	−0.075***
Nonexpansion states	0.231	0.354	0.123		
AVERAGE OUT-OF-POCKET PREMIUM SPENDING, CONDITIONAL ON ANY OUT-OF-POCKET PREMIUM SPENDING					
Expansion states	$2,571	$2,289	−$282	$38	$85
Nonexpansion states	$2,359	$2,039	−$320		

SOURCE Authors' analysis of data for 2011–16 from the Current Population Survey's Annual Social and Economic Supplement. **NOTES** Spending is in 2015 dollars. The estimates exclude immigrants imputed as undocumented. Medicaid expansion states include those that expanded Medicaid in the first half of 2014 or earlier. The estimates exclude states listed in the notes to exhibit 1 and the District of Columbia. Adjusted differences-in-differences are estimated controlling for the characteristics listed in the notes to exhibit 2. Standard errors are calculated using successive difference replication methods using CPS replicate weights. All models are estimated using ordinary least squares. [a] Family out-of-pocket premium spending exceeded 10 percent of family income. **$p < 0.05$ ***$p < 0.01$

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EXHIBIT 4

Difference-in-differences in cost sharing of adults ages 19–64 with family incomes of 100–138 percent of the federal poverty level in Medicaid expansion versus nonexpansion states, from 2010–13 to 2014–15

	Unadjusted mean		Difference between periods	Difference-in-differences	
	2010–13	2014–15		Unadjusted	Adjusted
AVERAGE COST SHARING					
Expansion states	\$470	\$393	–\$78	–\$227*	–\$218*
Nonexpansion states	\$540	\$689	\$149		
HIGH COST-SHARING SPENDING BURDEN[a]					
Expansion states	0.091	0.082	–0.008	–0.012	–0.009
Nonexpansion states	0.111	0.115	0.004		
ANY COST SHARING					
Expansion states	0.543	0.500	–0.042	–0.082***	–0.070***
Nonexpansion states	0.555	0.595	0.040		
AVERAGE COST SHARING, CONDITIONAL ON ANY COST SHARING					
Expansion states	\$867	\$785	–\$82	–\$268	–\$274
Nonexpansion states	\$972	\$1,158	\$186		

SOURCE Authors' analysis of data for 2011–16 from the Current Population Survey's Annual Social and Economic Supplement. **NOTES** Spending is in 2015 dollars. Cost sharing includes spending for the person's medical care, such as doctor and dentist visits, hospital visits, diagnostic tests, prescription medicine, glasses and contacts, and medical supplies. The estimates exclude immigrants imputed as undocumented. Medicaid expansion states include those that expanded Medicaid in the first half of 2014 or earlier. The estimates exclude states listed in the notes to exhibit 1 and the District of Columbia. Adjusted differences-in-differences are estimated controlling for the characteristics listed in the notes to exhibit 2. Standard errors are calculated using successive difference replication methods using CPS replicate weights. All models are estimated using ordinary least squares. [a]Family cost-sharing exceeded 10 percent of family income. **p* < 0.10 ****p* < 0.01

with high out-of-pocket spending burdens by 4.1 percentage points, and the share with any out-of-pocket spending by 7.7 percentage points. Additionally, Medicaid expansion was associated with a \$344 decline in average total out-of-pocket spending, a \$125 decline in average out-of-pocket premium spending, and a \$218 decline in average cost-sharing spending, relative to Marketplace access. Relative to 2010–13 means in expansion states, these last three changes represent declines of 33.9 percent, 23.0 percent, and 46.4 percent, respectively.

OUT-OF-POCKET SPENDING SENSITIVITY ANALYSES When we expanded the income band to include people with incomes slightly below (75–138 percent of poverty) or slightly above (100–150 percent of poverty) the Medicaid income eligibility thresholds, the estimated effects were roughly the same or smaller in magnitude compared to those of the main model, as one would expect (appendix exhibit A5).[20] Similarly, the estimated impacts among people in higher-income bands (150–200 percent and 200–400 percent of poverty) were significantly smaller in magnitude compared to those of the main model, and only some of the first-stage linear probability model estimates were significant.

There were similar trends for most spending outcomes in expansion and nonexpansion states before 2013, which offers support for our study design (appendix exhibit A6).[20] We found no evidence of differential trends in the total out-of-pocket spending and premium models. While we did find some evidence of differential trends in average cost sharing, we found no evidence of such trends in any other model. For a further discussion of our sensitivity analyses, see the appendix.[20]

Discussion

We examined the impacts on out-of-pocket spending and health insurance coverage for near-poor adults who gained access to different types of health insurance under the ACA: Medicaid coverage in expansion states and subsidized Marketplace coverage in nonexpansion states.

We found that Medicaid expansion lowered out-of-pocket health spending burdens for people with incomes of 100–138 percent of poverty, relative to not expanding Medicaid. This key finding was likely driven by lower out-of-pocket premiums and cost-sharing requirements in Medicaid, combined with higher overall coverage take-up in expansion states relative to nonexpansion states. While uninsurance rates declined significantly in both expansion and nonexpansion states, the difference-in-differences estimates indicate that, relative to Marketplace coverage, Medicaid expansion was associated with nearly a 5-percentage-point reduction

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in the probability of being uninsured. This finding implies that more restrictive eligibility and enrollment policies, combined with higher premiums for Marketplace coverage relative to Medicaid, were associated with lower take-up rates among people with incomes of 100–138 percent of poverty.

Despite gaining coverage, adults in that group in nonexpansion states experienced significant increases in out-of-pocket spending in 2014–15, while spending declined among people in nonexpansion states. In terms of magnitude, Medicaid expansion was associated with a reduction in average total out-of-pocket spending of $344, high out-of-pocket spending burdens of 4.1 percentage points, and the probability of having any out-of-pocket spending of 7.7 percentage points. The $344 decline in out-of-pocket spending corresponds to 2 percent of the average income for adults with incomes of 100–138 percent of poverty, which is consistent with the amount that low-income people would have to pay out of pocket for a Marketplace plan in nonexpansion states. However, the impact for those who were newly enrolled in Medicaid, relative to those who were newly enrolled in Marketplace coverage, was likely to be much higher—particularly among those with high out-of-pocket expenses before the ACA (for example, high-cost uninsured adults and those with expensive employer-sponsored plans).

Policy Implications

These findings have important implications for state and federal policy makers focused on increasing coverage or lowering out-of-pocket spending burdens among low-income uninsured people. This analysis suggests that nonexpansion states that choose to expand Medicaid under the ACA will see an increase in coverage among people with incomes of 100–138 percent of poverty and a reduction in out-of-pocket spending burdens, particularly if premiums are not included under the expansion. It also suggests that states that drop Medicaid expansion could see an increase in uninsurance and underinsurance for people with incomes of 100–138 percent of poverty, unless the states further subsidize premiums and cost sharing for Marketplace plans. Massachusetts's recent proposed section 1115 waiver does just that, using state funds to subsidize cost sharing for Marketplace enrollees in that income band at a level greater than current federal requirements. Waivers that allow Medicaid to charge premiums for people in this income band could also deter enrollment among the remaining uninsured, while increasing out-of-pocket spending burdens among enrollees.

To increase take-up and lower spending burdens among the population with incomes of 100–138 percent of poverty in both expansion and nonexpansion states, policy makers could reduce or eliminate premium requirements, increase targeted outreach efforts, or increase the value proposition of coverage relative to being uninsured by improving the quality of coverage (for example, by increasing provider participation in Medicaid through higher reimbursements and improving network adequacy in the Marketplace).[27] Future research should focus on the relative effectiveness of these different strategies.

Moving forward, it will be important to consider other factors that could influence coverage take-up and out-of-pocket spending among the population with incomes of 100–138 percent of poverty in expansion and nonexpansion states. These factors include the elimination of cost-sharing reduction subsidies, the availability of zero-premium bronze Marketplace plans in some states, repeal of the individual mandate penalty in the 2017 tax bill, and differences in outreach efforts among late-expansion states compared to those that expanded in 2014. ■

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NOTES

1 In 2017, 100–138 percent of poverty corresponded to $12,060–$16,643 for a single person and $24,600–$33,948 for a family of four. See Department of Health and Human Services, Office of the Assistant Secretary for Planning and Evaluation. U.S. federal poverty guidelines used to determine financial eligibility for certain federal programs [Internet]. Washington (DC): ASPE; [cited 2017 Dec 13]. Available from: https://aspe.hhs.gov/poverty-guidelines

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Foundation; 2017 Jan [cited 2017 Dec 13]. Available from: http://files.kff.org/attachment/Report-Medicaid-and-CHIP-Eligibility-as-of-Jan-2017

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12 McIntyre A, Joseph AM, Bagley N. Small change, big consequences—partial Medicaid expansions under the ACA. N Engl J Med. 2017;377(11): 1004–6.

13 Nonpremium out-of-pocket medical spending includes spending for the individual's medical care, such as doctor and dentist visits, hospital visits, diagnostic tests, prescription medicine, glasses and contacts, and medical supplies. Individuals may have paid out of pocket for this care because it was not a covered service under their health plan or because they were spending toward their deductible, copayments, or coinsurance for the medical care. This measure does not include spending for nonprescription health care products.

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15 Our sample included people in nonexpansion states who had offers of affordable employer-sponsored insurance, since this information is not available in the CPS data. This may have created some biases in our estimates, since these people were not eligible for Marketplace subsidies (for example, including them might have reduced the insurance effect in nonexpansion states, compared to expansion states).

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20 To access the Appendix, click on the Details tab of the article online.

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26 This increase in total out-of-pocket spending in nonexpansion states could be driven by uninsured people moving into Marketplace coverage with some out-of-pocket premium and cost-sharing requirements

27 One potential way to improve network adequacy is through the use of telemedicine. See Ahn S, Corlette S, Lucia K. Can telemedicine help address concerns with network adequacy? Opportunities and challenges in six states [Internet]. Washington (DC): Urban Institute; 2016 Apr 6 [cited 2017 Dec 13]. Available from https://www.urban.org/research/publication/can-telemedicine-help-address-concerns-network-adequacy-opportunities-and-challenges-six-states

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FROM WELFARE TO A WORK-BASED SAFETY NET: AN INCOMPLETE TRANSITION

Sandra K. Danziger, Sheldon Danziger, Kristin S. Seefeldt, and H. Luke Shaefer

INTRODUCTION

The passage of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which we label welfare reform, decisively "ended welfare as we knew it" by eliminating the federal entitlement to cash assistance that Aid to Families with Dependent Children (AFDC) had provided for 60 years. The new cash assistance program, Temporary Assistance for Needy Families (TANF), is a fixed federal block grant that allows states to spend federal and state funds flexibly on cash assistance or a range of other services provided to needy families with children. PRWORA requires only that states enforce participation in work or work-related activities and impose time limits on the receipt of cash assistance from federal funds.

Because it is not an entitlement program, TANF does not require states to provide benefits to all eligible families, nor does it require them to assume responsibility for guaranteeing unemployed recipients who reach their time limit a job if they cannot find one. As a result, the transition to the new "work-based safety net" remains incomplete: neither the federal government nor the states replaced the entitlement to cash assistance with an entitlement to participate in work or work-related activities.

TANF includes incentives that encourage states to minimize their cash assistance caseloads, even when recessions increase the number of eligible families. First, the amount of block grant funds has not changed since the program was enacted. A state's allocation is not responsive to population growth or increased economic hardship. Second, funds not used for cash assistance can be reallocated to other related purposes. Third, states can satisfy the work participation benchmarks either by getting more recipients into work-related activities or jobs (which can be expensive) or simply by keeping caseloads small.

In contrast to this reduction in public responsibility, TANF requires more responsibility from recipients. Indeed, it achieved its goal of eliminating long-term dependence on cash assistance. A recipient can no longer reject a job offer and continue to receive cash assistance. If she does not cooperate with the welfare agency, she can be sanctioned and her family can be removed from the welfare rolls. She can also be removed when she reaches her time limit, even if she is actively seeking work and willing to work for her cash assistance.

TANF's time limits and work requirements did contribute to the post-welfare reform increase in the labor supply of single mothers during the low unemployment years of the late 1990s, even though labor demand issues were not addressed. David Ellwood, whose *Poor Support* (1988) first set out the case for time limits on cash assistance, did address both labor supply and labor demand. Under his plan, cash assistance would end when recipients reached the time limit. However, if they were unable to find work, the welfare agency would not simply close the case as TANF requires, but would offer an opportunity to work in a public or subsidized job.

Because labor demand was so strong in the late 1990s, many analysts and politicians concluded that TANF's labor supply incentives on their own were sufficient to make welfare reform and the transition to a work-based safety net successful. In 2015, however, with two decades of TANF experience, and with the unemployment rate for high school graduates having peaked at 11 percent and remained above 7 percent from November 2008 through December 2013, it is clear that the neglect of the demand side of the labor market calls into question the initial optimistic evaluations of the 1996 reform.

In the next section, we briefly highlight key research findings on the effects of TANF and policy changes related to the 1996 reform. We conclude with several suggestions for policies that would help complete the transition to a work-based safety net.

A BRIEF REVIEW OF THE EVIDENCE

In the immediate aftermath of welfare reform, policy analysts and politicians across the political spectrum gave PRWORA high marks as caseloads plummeted, the employment rate for single mothers increased rapidly, and child poverty fell (Haskins, 2006). Many analysts, however, cautioned that welfare reform was only partially responsible for these successful outcomes (Blank, 2002). For example, the Earned Income Tax Credit (EITC), which provides tax credits to families with children and low earnings, was increased significantly in the early 1990s, the minimum wage was increased in 1997, and access to medical care was expanded by the State Child Health Insurance Program of 1997.

In addition, welfare reform was implemented in a booming economy, in which many employers faced a labor shortage (Holzer, 1999). The national unemployment rate fell to 4 percent in 2000 and the real wages of less-educated workers increased for the first time in decades. Together, these economic and public policy changes increased the financial benefits to a single mother of moving from welfare to work at the same time that TANF's new rules made staying on welfare much more difficult (Danziger et al., 2002).

This created a much stronger work-based safety net for poor parents who were willing to work *and* who could find and maintain employment. Indeed, total spending on poor families with children has increased substantially since welfare reform.

However, this aggregate increase in total spending masks a new divergence—public benefits per poor family have increased *only* for the working poor; they have actually fallen for the nonworking, nondisabled poor (Moffitt, 2015).

Caseloads

Cash assistance caseloads declined more than most observers had predicted when welfare reform was being debated. The national caseload fell from about 5 million families per month in 1994 to about 2.3 million per month in 2000. Caseloads have remained low and are now about 1.6 million families, despite population growth over the past 20 years and recent high unemployment rates. In contrast, the monthly caseload for Supplemental Nutrition Assistance Program, SNAP (formerly Food Stamps) increased from 27.5 to 46.5 million between 1994 and 2014.

TANF serves a much smaller fraction of poor families than AFDC once did—the ratio of the number of poor families with children to the caseload has fallen to 26 percent from 82 percent in 1979 (Center on Budget and Policy Priorities, 2015). In addition, caseloads increased little during the Great Recession (and in some states not at all), a time when the unemployment rate and SNAP caseloads roughly doubled.

Many studies, summarized by Ziliak (2015), emphasize three factors that contributed to the caseload declines: the strong economy, TANF policies that discouraged welfare entry and encouraged exit, and increased public benefits for the working poor such as the increased EITC. In addition, the composition of the caseload is different than it was in 1996, with higher proportions of cases where the mother is exempt from work requirements because she is pregnant or has a child under three months and where the cases contain no adults. Falk (2014) finds that the percentage of "child-only" cases rose from 17.2 to 37.4 percent between 1994 and 2011.

TANF caseloads hardly increased during the Great Recession, but the reasons remain unclear (Ziliak, 2015). One possibility is that many states reallocated their fixed TANF funds to related programs, such as state EITCs, child-care subsidies, and child welfare systems. When the unemployment rate increased, they did not move these funds back to cash assistance because state revenues fell dramatically during the recession. States now spend less than 30 percent of TANF funds for cash assistance (Center on Budget and Policy Priorities, 2015). Another possibility is that increased state discretion and work requirements have led many potential recipients to assume that TANF no longer provides support (Edin & Shaefer, 2015; Seefeldt & Sandstrom, 2015).

Employment and Disconnection from Employment

Moving recipients from welfare to work was a key goal of the 1996 reform. In 1993, 58 percent of low-income mothers were employed at some time during the year, but by 2000, nearly 75 percent were working (Haskins, 2006). The employment of single mothers, unlike TANF caseloads, is responsive to economic conditions—their employment rate declined somewhat after 2000, but has remained above pre-welfare reform levels.

Many who left welfare, however, were unable to work steadily. Whereas three-quarters of former recipients worked at some point in the year following welfare exit, only one-third worked in all four calendar quarters (Acs & Loprest, 2004). The kinds of jobs held by former recipients contribute to this instability. For example, many Wisconsin welfare leavers reported quitting jobs when they or a child became ill or they became pregnant (Collins & Mayer, 2010).

In addition, the decline in the caseload exceeded the increase in employment, suggesting that the implementation of work requirements and time limits, and the failure to provide work opportunities contributed to an increased number of single mothers who had neither earnings nor cash assistance, referred to as the "disconnected." Loprest and Nichols (2011) find that the disconnected represented about one in eight of all single mothers in 1996 and 1997 but about one in five in 2008. Compared with single mothers who have earnings or cash assistance, the disconnected have a greater number of employment barriers, such as health and mental health problems, experiences of domestic violence, and learning disabilities (Turner, Danziger, & Seefeldt, 2006). Some disconnected mothers rely on family members, boyfriends, and the fathers of their children for support, but this help is often unstable (Seefeldt & Sandstrom, 2015).

Poverty

Research on the extent to which welfare reform reduced poverty on its own is mixed, even though child poverty did decline after welfare reform. Some studies report small to substantial income gains for mothers who left welfare for work (Danziger et al., 2002), but others find income losses (Ziliak, 2015). Some former recipients who had stable jobs were reluctant to take promotions at higher wages because they interfere with child care and other family demands (Seefeldt, 2008). Also, even when work paid more than welfare, some single mothers expressed concerns about having less time and energy for their children (London et al., 2004). In his recent review, Ziliak (2015) concludes that "taken together, the results from leaver studies, demonstrations, and from national samples suggest that many women were worse off financially after welfare reform, especially at the bottom of the distribution. But this result becomes clear only if data post 2000 are brought to bear."

The child poverty rate, which reflects many economic, demographic, and policy changes, did fall after welfare reform, from 20.5 percent in 1996 to 16.2 percent in 2000. This progress was lost following the Great Recession as child poverty was 19.9 percent in 2013. The post-welfare reform declines in poverty for black children were also large, from 39.9 percent in 1996 to 31.2 percent in 2000, before rising back to 38.3 percent in 2013.

These child poverty trends include both the children of the working poor, for whom the safety net is now stronger than it was in 1996, and those of the nonworking poor, for whom it is now weaker. Even for those who still receive TANF, benefits have declined either because states have cut them or because they have been eroded by inflation. In most states, real TANF benefits are worth about 20 percent less than in 1996 (Center on Budget and Policy Priorities, 2015). Edin and Shaefer (2015) develop a $2 per person, per day, measure of extreme poverty, adapted from the World Bank's metric of global poverty. In 1996, 1.7 percent of families with children reported cash incomes equivalent to no more than $2 per day, compared to 4.3 percent in 2011. If government noncash-income benefits are added, while the percentage in extreme poverty falls in both years, extreme poverty still increases from 1.1 to 1.6 percent.

Child Well-being

There is relatively little research on the extent to which welfare reform affected child well-being, in part because effects may not be evident for many years. Morris, Gennetian, and Duncan (2005) study the effects of welfare experiments that took place prior to the 1996 reform, such as the New Hope Program, and find that children fared better in programs that improved the economic well-being of welfare recipients. However, the typical TANF program was less generous than the programs they reviewed.

Johnson, Kalil, and Dunifon (2012) examine the relationship between maternal employment patterns of former welfare recipients and the behavioral and academic outcomes of their children. They find detrimental associations between child behavior and academic achievement if the mothers worked in unstable jobs, had fluctuating work hours, or required full-time employment. However, in the minority of cases where former recipients had good jobs, "the negative consequences of long work hours are completely offset when this work experience is in jobs that require cognitive skills that lead to higher wage growth prospects."

SUMMARY AND POLICY OPTIONS

Findings on caseloads, employment, the disconnected, poverty, and child well-being support several conclusions. First, the post-welfare reform safety net that reduced public benefits for the nonworking, nondisabled poor and increased them for the working poor was more successful when jobs were readily available in the late 1990s and much less successful in recent years when unemployment was high. Second, welfare reform and the related policy changes have had heterogeneous effects. Some families are better off financially under the new safety net—particularly those who maintain stable employment across the business cycle. Others, however, are worse off—particularly those who have barriers to employment, such as health and mental health problems and few labor market skills.

The 1996 welfare reform, policy changes such as the increased minimum wage and the EITC expansion, and the booming economy all contributed to declines in cash assistance and increases in employment. But the reform alone contributed to the increase in disconnected mothers and households with extreme low cash incomes. Also, the incentives TANF provided to state governments encouraged them not to expand assistance in response to the Great Recession at the same time that SNAP caseloads roughly doubled.

Evaluating the effects due solely to TANF is difficult. But given the many publications summarized in Ziliak (2015), we conclude that the high marks given to the 1996 reform at its 10-year anniversary are much lower as we approach its 20-year anniversary. It is now time to use what we have learned to complete the transition to a work-based safety net. Here, we offer four suggestions for policies to increase employment and reduce poverty that do not require a return to AFDC's entitlement to cash assistance.

First, the emphasis on "personal responsibility" should be balanced with a "public responsibility" to provide work opportunities to those for whom there is limited employer demand, especially when unemployment rates are high. Those who are willing to work but cannot find employment should be offered the opportunity to work to support their families instead of having their benefits terminated.

The American Recovery and Reinvestment Act of 2009 (the "stimulus") provided \$5 billion in TANF emergency funds. One of the possible uses for these funds was to subsidize jobs in public or nonprofit agencies or with private-sector employers. Among the 39 states plus the District of Columbia that took part, employers created 260,000 jobs with a federal investment of \$1.3 billion (Roder & Elliott, 2013).

About two-thirds of participating employers said they created positions that would not have existed otherwise, and a (nonrandomized) evaluation found that many participants made gains that lasted after the program ended, and that the subsidized jobs garnered "strong support from employers, workers, and state and local officials from across the political spectrum" (Roder & Elliott, 2013, p. 1). Creating a permanent program of work opportunities for those willing to work but unable to find a job would help complete the transition to a work-based safety net.

Second, many former welfare recipients found jobs but had difficulty paying for child care. Ziliak (2014) reports that out-of-pocket child-care costs represent 15 to 25 percent of the earnings of the median single mothers, depending on state of residence. Because the former welfare recipients earn less than the median, their cost burden is even higher. In 2011 only about one in six children who were eligible for federal subsidies received them (U.S. Department of Health and Human Services, Office of the Assistant Secretary for Planning and Evaluation, 2015). Increasing public funds for child-care subsidies would both raise the economic well-being of single mothers, but also contribute to increased labor supply by raising the net gain from work.

Third, TANF should extend cash assistance to a greater percentage of eligible families, with work requirements, time limits, and other restrictions automatically relaxed during recessions. Currently, only the SNAP program provides near-universal access to basic assistance for families with children. Indeed, one lesson of the TANF experience is that attempts to add further work requirements to SNAP without also providing work opportunities would increase both the number of disconnected mothers and the extent of extreme poverty.

Edin and Shaefer (2015) document that many families who need temporary cash assistance have difficulty enrolling in TANF. Reducing barriers to entry requires restraining state discretion to divert TANF funds for other uses. For example, states might be required to spend some fraction of their block grant and maintenance of effort funds on cash assistance and work opportunities. The states could better afford these restrictions if the TANF block grant were finally increased from its 1996 level, with additional automatic increases provided during recessions.

Fourth, the TANF experience revealed that many AFDC recipients had barriers to employment that prevented them from finding steady work even when the economy was booming (Danziger et al., 2000). Expanding work opportunities is one option. But for those whose barriers to work are so severe that they cannot work, but not severe enough to qualify them for disability benefits, Blank and Kovak (2009) propose modifying the Supplemental Security Income (SSI) program that now provides benefits only for permanent disabilities by adding a new part-time or temporary disability benefits program. This allows TANF to focus on those able to work steadily.

The 1996 reform ended the cash-based safety net that had been in place since the 1930s and started the transition toward a work-based safety net. The reform and related policies did increase the economic well-being of working poor families with children. But given the economic conditions in the last 15 years, TANF has decreased the economic well-being of the nonworking, nondisabled poor, as evidenced by the increased number of disconnected and extremely poor families. The modest policy changes proposed here would help us achieve a more effective work-based safety net.

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SUPPLEMENTING TANF'S WORK REQUIREMENT: A COMPROMISE

Ron Haskins

The first point to make about the Danziger et al.'s balanced and fair assessment of TANF on its 20th anniversary is that they focus largely on the work-based safety net. This fact in itself shows how much things have changed for progressives since TANF was enacted in 1996. Now nearly everyone agrees that work should be the major emphasis of the TANF program and that a major role of government is to supplement the income of low-skilled mothers who work but usually earn low wages. Since creation of the Earned Income Tax Credit (EITC) in 1975, Congress has enacted numerous pieces of legislation to strengthen the work-based safety net. The result is that low-income working families with children are eligible for SNAP (food stamps) and other nutrition benefits, the EITC, the Additional Child Tax Credit and their children are covered by Medicaid. In addition, these families are eligible for housing benefits, Head Start, state preschool programs, and day care subsidies, although not all families receive these benefits because Congress and the states do not appropriate enough money to cover all eligible families. Although there's a gap between coverage and need, the work-based safety net still provides an impressive array of benefits to working families with children. Low-income working families with children receive more help from government than ever before—and there is bipartisan agreement that this is good policy.

Report on the 2016 Healthy Michigan Voices Enrollee Survey

June 21, 2017

University of Michigan
Institute for Healthcare Policy & Innovation

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EXECUTIVE SUMMARY

The University of Michigan Institute for Healthcare Policy & Innovation (IHPI) is conducting the evaluation of the Healthy Michigan Plan (HMP) as required by the Centers for Medicare & Medicaid Services (CMS) through a contract with the Michigan Department of Health and Human Services (MDHHS). This report presents selected findings from the responses to the Healthy Michigan Voices (HMV) enrollee survey conducted January-October 2016.

Methods

Sampling for the Healthy Michigan Voices enrollee survey was performed monthly, beginning in January 2016. At time of sample selection, beneficiaries must have had:

- At least 12 months total HMP enrollment in fee for service (FFS) or managed care (MC)
- HMP enrollment (FFS or MC) in 10 of past 12 months
- Have HMP-MC enrollment in 9 of past 12 months
- HMP-MC in the month sampled
- Age between 19 years and 64 years 8 months
- Complete address, phone number, and federal poverty level (FPL) fields in the Data Warehouse
- Michigan address
- Preferred language of English, Arabic, or Spanish

Exclusion in one month of sampling did NOT prohibit inclusion in a subsequent month.

The sampling plan was based on four grouped prosperity regions in the state (Upper Peninsula/North West/North East; West/East Central/East; South Central/South West/South East; Detroit) and three FPL categories (0-35%; 36-99%; ≥100%). In total, 4,090 HMP enrollees participated in the HMV survey, and the weighted response rate for the 2016 Healthy Michigan Voices enrollee survey was 53.7%.

Many items on the survey were drawn from large national surveys. When established measures were not available, items specific to HMP (e.g., items about Health Risk Assessments, understanding of HMP) were developed based on findings from 67 semi-structured interviews with HMP beneficiaries conducted by the evaluation team. New items underwent cognitive testing and pre-testing for timing and flow before being included in the survey instrument. Responses were recorded in a computer-assisted telephone interviewing (CATI) system.

The evaluation team calculated descriptive statistics for responses to all questions with weights calculated and applied to adjust for the probability of selection, nonresponse bias, and other factors. Statistical analyses of bivariate and multivariate relationships were also performed.

Results

Insurance Coverage Prior to HMP

- 57.9% did not have insurance at any time in the year before enrolling in HMP.

Current Health Status/Change in Health with HMP

- 47.8% said their physical health had gotten better since enrolling in HMP.
- 38.2% said their mental and emotional health had gotten better since enrolling in HMP.
- 39.5% said their dental health had gotten better since enrolling in HMP.

Chronic Health Conditions

- 69.2% reported they had a chronic health condition, with 60.8% reporting at least one physical health condition and 32.1% reporting at least one mental health condition.
- 30.6% reported that they had a chronic health condition that was newly diagnosed since enrolling in HMP.
- 18.4% reported they had a functional limitation.

Health Risk Assessment (HRA)

- 49.3% self-reported completing an HRA. While higher than the completion rate in the MDHHS Data Warehouse, this may be due to enrollees completing the patient portion only, recall bias, or misidentifying completion of other forms as completing the HRA.
- 45.9% of those who said they completed an HRA did so because a primary care provider (PCP) suggested it; 33% did so because they received the form in the mail; 12.6% completed it over the phone at time of enrollment.
- Only 0.1% said they completed the HRA to save money on copays and contributions.
- Most of those who reported completing the HRA felt it was valuable for improving their health (83.7%) and was helpful for their PCP to understand their health needs (89.7%). 80.7% of those who said they completed an HRA chose to work on a health behavior.

Health Behaviors and Health Education

- 37.7% of beneficiaries reported smoking or using tobacco in the last 30 days, and 75.2% of these people said they wanted to quit. Of these, 90.7% were working on cutting back or quitting right now.

Regular Source of Care and Primary Care Utilization Prior to HMP

- 73.8% said that in the year before enrolling in HMP they had a place they usually went for health care. Of those, 16.8% said that place was an urgent care center and 16.2% reported the emergency room (ER), while 65.1% reported a doctor's office or clinic.
- 20.6% had not had a primary care visit in five or more years before enrolling in HMP.

Regular Source of Care and Primary Care Utilization with HMP

- 92.2% reported that in the year since enrolling in HMP they had a place they usually went for health care. Of those, 5.8% said that place was an urgent care center and 1.7% reported the emergency room, while 75.2% reported a doctor's office or clinic.
- 85.2% of those who reported having a PCP had a visit with their PCP in the last year. 83.9% of these said it was very easy or easy to get an appointment with their PCP.
- Beneficiaries who were older, white, female, reported worse health, and had any chronic condition were more likely than other beneficiaries to have seen a PCP in the past 12 months.
- Those who reported seeing a PCP in the preceding 12 months were more likely to report improved access to preventive care, completing an HRA, being counseled about health behaviors and being diagnosed with a chronic condition since enrollment.

Foregone Care Prior to and with HMP

- 33% of beneficiaries reported not getting care they needed in the year before enrollment in HMP; 77.5% attributed this to cost concerns. In the year preceding the survey (i.e., since enrolling in HMP), 15.6% reported foregone care; 25.4% attributed that to cost concerns.
- 83.3% agreed or strongly agreed that without HMP they would not be able to go to a doctor.

Changes in Access to Care

- Few beneficiaries (less than 5%) reported their ability to access primary care, specialty care, mental health care, substance use treatment, prescription medication, cancer screening, prevention of health problems and birth control/family planning had worsened since enrolling in HMP; 6.2% reported access to dental care worsened.

Emergency Room Use with HMP

- 28.0% of those who visited the ER in the past year said they called their usual provider's office first. 64% said they were more likely to contact their usual doctor's office before going to the ER than before they had HMP.
- Respondents who used the ER were more likely than those who did not use the ER to report their health as fair/poor (40.1% vs. 23.2%) and to report chronic physical or mental health conditions (79.4% vs. 62.8%).

Impact of HMP on Employment, Education and Ability to Work

- 48.9% reported they were employed/self-employed, 27.6% were out of work, 11.3% were unable to work, and 2.5% were retired.
- HMP enrollees were more likely to be employed if their health status was excellent, very good, or good vs. fair or poor (56.1% vs. 32.3%) or if they had no chronic conditions (59.8% vs. 44.1%).

- Compared to employed enrollees, enrollees who were out of work or unable to work were more likely to be older, male, lower income, veterans, in fair/poor health, and with chronic physical or mental health conditions or limitations.
- Employed respondents missed a mean of 7.2 work days in the past year due to illness. 68.4% said this was the same as before HMP, 17.2% said less and 12.3% said more.
- Among employed respondents, over two-thirds (69.4%) reported that getting HMP insurance helped them to do a better job at work.
- For the 27.6% of respondents who were out of work, 54.5% strongly agreed/agreed that HMP made them better able to look for a job.
- For the 12.8% of respondents who had changed jobs in the past 12 months, 36.9% strongly agreed/agreed that having HMP insurance helped them get a better job.

Knowledge and Understanding of HMP Coverage

- The majority of respondents knew that HMP covers routine dental visits (77.2%), eyeglasses (60.4%), and counseling for mental or emotional problems (56%). Only one-fifth (21.2%) knew that HMP covers name brand as well as generic medications.

Challenges Using HMP Coverage

- Few (15.5%) survey respondents reported that they had questions or problems using their HMP coverage. Among those who did, about half (47.7%) reported getting help or advice, and most (74.2%) of those said that they got an answer or solution.

Out-of-Pocket Healthcare Spending Prior to and with HMP

- 44.7% said they had problems paying medical bills in the year before HMP. Of those, 67.1% said they or their family was contacted by a collections agency.
- 85.9% said that since enrolling in HMP their problems paying medical bills got better.

Perspectives on Cost-Sharing

- 87.6% strongly agreed or agreed that the amount they pay overall for HMP seems fair.
- 88.8% strongly agreed or agreed that the amount they pay for HMP is affordable.

Knowledge and Understanding of HMP Cost-Sharing Requirements

- Only 26.4% were aware that contributions are charged monthly regardless of health care use. Just 14.4% of respondents were aware that they could not be disenrolled from HMP for not paying their bill. Only 28.1% were aware that they could get a reduction in the amount they have to pay if they complete an HRA. 75.6% of respondents were aware that some kinds of visits, tests, and medicines have no copays.

MI Health Account Statement

- 68.2% said they received a MI Health Account statement. 88.3% strongly agreed/agreed they carefully review each statement to see how much they owe. 88.4% strongly agreed/agreed the statements help them be more aware of the cost of health care.

Information Seeking Behaviors

- 71.6% reported being somewhat or very likely to find out how much they might have to pay for a health service before going to get the service.

Perceived Discrimination

- Most respondents did not report feeling judged or treated unfairly by medical staff in the past 12 months because of their race or ethnic background (96.4%) or because of how well they spoke English (97.4%); but 11.6% of respondents felt judged or treated unfairly by medical staff in the past 12 months because of their ability to pay for care or the type of health coverage they had.

Social Interactions

- 67.6% of respondents said that they get together socially with friends or relatives who live outside their home at least once a week; 79.8% said that they amount they engage in social interactions is about the same as before they enrolled in HMP.

Reproductive Health

- Among reproductive age female respondents, 38.4% did not know whether there was a change in their access to family planning services, while 35.5% reported better access and 24.8% reported about the same access. Those with inconsistent health insurance or uninsurance prior to HMP were significantly more likely to report improved access.

Impact on Those with Chronic Health Conditions

- Prior to HMP, 77.2% of those with a chronic physical or mental health condition had a regular source of care, 64.7% of whom said that source of care was a doctor's office or clinic. After HMP, 95.2% had a regular source of care, and 93.1% said it was a doctor's office or clinic.
- In the year prior to HMP enrollment, 58.3% of those with a chronic physical or mental health condition did not have insurance, only 42.1% had seen a PCP, and 51.7% had problems paying medical bills.
- Since HMP enrollment, 89.6% of those with a chronic physical or mental health condition reported seeing a PCP, 64.6% reported their ability to fill prescriptions improved, and 86.3% reported their ability to pay medical bills had improved.
- Respondents with a chronic physical or mental health condition reported overall improvements in their physical (51.9%) and mental health (42.4%) after enrolling in HMP; 7.5% and 6.1% reported their physical and mental health status had worsened.

Impact on Those with Chronic Mood Disorder and Substance Use Disorder

- Since enrollment in HMP, 48.9% of respondents with a self-reported mood disorder (MD) and 50.5% with a self-reported substance use disorder (SUD) reported that their mental health had gotten better.
- Most respondents with a MD reported that having HMP has led to a better life (91.9% strongly agreed/agreed) as did respondents with a SUD (95.8% strongly agreed/agreed).

- Prior to HMP, 37% of respondents who self-reported a SUD used the emergency room as a regular source of care; after at least one year of HMP the emergency room as a regular source of care dropped to 3.6%.

Conclusions

- More than half of respondents, including more than half of those with chronic conditions, did not have insurance at any time in the year before enrolling in HMP. Foregone care, usually due to cost, lessened considerably after enrollment. Most respondents said that without HMP they would not be able to go to the doctor. **HMP does not appear to have replaced employment-based insurance and has greatly improved access to care for underserved persons.**
- The percentage of enrollees who had a place they usually went for health care increased with HMP to over 90%, and naming the ER as a regular source of care declined significantly after enrolling in HMP (from 16.2% to 1.7%). **An emphasis on primary care and disease prevention shifts care-seeking away from acute care settings.**
- A significant majority said since enrolling in HMP their problems paying medical bills had gotten better. Most respondents agreed that **the amount they pay overall for HMP seems fair and is affordable, although monthly contributions affected perceptions of affordability**.
- There were some areas in which **beneficiary understanding of coverage** (e.g., dental, vision and family planning) **and cost-sharing requirements needs to improve**.
- About half of respondents reported completing an HRA, bearing in mind the limits to self-reported data. **Most respondents addressed health risks for reasons other than financial incentives.**
- HMP enrollees with mood disorder or substance use disorder reported improved health, improved access to services and treatment, and were less likely to name the emergency room or urgent care as a regular source of care. Those with substance use disorder still report using the emergency room more often than those with other chronic illnesses.
- Many HMP enrollees reported improved functioning, ability to work, and job seeking after obtaining health insurance through Medicaid expansion. **HMP may help its beneficiaries maintain or obtain employment.**
- Chronic health conditions were common among enrollees in Michigan's Medicaid expansion program, even though most enrollees were under 50 years old. Almost half of these conditions were newly diagnosed after enrolling in HMP. **Enrollees with chronic conditions reported improved access to care and medication, all crucial to successfully managing these conditions and avoiding future disabling complications.** Despite the relatively short term of their enrollment in HMP, almost half of respondents said their physical health had gotten better and nearly 40% said their emotional and mental health and dental health had gotten better since enrolling in HMP, **attesting to the health impact of Medicaid expansion**.

INTRODUCTION

The University of Michigan Institute for Healthcare Policy & Innovation (IHPI) is conducting the evaluation of the Healthy Michigan Plan (HMP) as required by the Centers for Medicare & Medicaid Services (CMS) through a contract with the Michigan Department of Health and Human Services (MDHHS). This report presents findings from responses of the Healthy Michigan Voices (HMV) enrollee survey. From January through October 2016, 4,090 beneficiaries completed the Heathy Michigan Voices survey of current HMP beneficiaries. This is an update to the interim report submitted to CMS in September 2016. Findings from the 2016 Healthy Michigan Voices survey of those who have disenrolled from the Healthy Michigan Plan will be available in late 2017.

METHODS

Sampling for the Healthy Michigan Voices survey was performed monthly, beginning in January 2016. At the time of sample selection, beneficiaries must have had:

- At least 12 months total HMP enrollment in fee for service (FFS) or managed care (MC)
- HMP enrollment (FFS or MC) in 10 of past 12 months
- Have HMP-MC enrollment in 9 of past 12 months
- HMP-MC in the month sampled
- Age between 19 years and 64 years 8 months
- Complete address, phone number, and federal poverty level (FPL) fields in the Data Warehouse
- Michigan address
- Preferred language of English, Arabic, or Spanish

Exclusion in one month of sampling did not prohibit inclusion in a subsequent month. Each month’s sample was drawn to reflect the target sampling plan, proportional to the characteristics of Healthy Michigan Plan beneficiaries as a whole.

The sampling plan was based on four grouped prosperity regions in the state (Upper Peninsula/North West/North East; West/East Central/East; South Central/South West/South East; Detroit) and three FPL categories (0-35%; 36-99%; ≥100%)

Sampling Plan

	Prosperity Region				
	UP/NW/NE	W/EC/E	SC/SW/SE	DET	Total
Federal Poverty Level					
0-35%	7.0%	12.0%	8.0%	12.8%	39.9%
36-99%	6.0%	10.5%	7.0%	11.2%	34.8%
≥100%	4.9%	7.5%	5.0%	8.0%	25.5%

The 4,090 respondents included in this first report of selected findings closely mirror the sampling plan:

Characteristics of the 4,090 HMV Survey Respondents

	Prosperity Region				
	UP/NW/NE	W/EC/E	SC/SW/SE	DET	Total
Federal Poverty Level					
0-35%	288	503	323	486	1,600
	7.0%	12.3%	7.9%	11.9%	39.1%
36-99%	246	467	309	428	1,450
	6.0%	11.4%	7.6%	10.5%	35.5%
≥100%	212	295	205	328	1,040
	5.2%	7.2%	5.0%	8.0%	25.4%
Total N complete	746	1,265	837	1,242	4,090
Total % complete	18.2%	30.9%	20.5%	30.4%	100.00%

HMP beneficiaries selected for the HMV beneficiary survey sample were mailed an introductory packet that contained a letter explaining the project, a brochure about the project, and a postage-paid postcard that could be used to indicate preferred time/day for interview. A toll-free number was provided for beneficiaries who wished to call in at their convenience; otherwise, Healthy Michigan Voices interviewers placed phone calls to sampled beneficiaries between the hours of 9 am and 9 pm. Surveys were conducted in English, Arabic and Spanish; beneficiaries who could not speak one of those languages were excluded from participation.

Survey Design

The survey included measures of demographics, health, access, insurance status and acute care decision making. Many measures were established measures drawn from national surveys, including the National Health and Nutrition Exam Survey (NHANES)[1], the Health Tracking Household Survey (HTHS)[2], the National Health Interview Survey (NHIS)[3], the Behavioral Risk Factor Surveillance System (BRFSS, and MiBRFSS), the Short Form Health Survey (SF-12)[4], the Food Attitudes and Behaviors Survey, the Consumer Assessment of Healthcare Providers and Systems (CAHPS)[5], the Employee Benefit Research Institute Consumer Engagement in Healthcare Survey (CEHCS)[6], the Health Tracking Household Survey, the Commonwealth Fund Health Quality Survey, and the U.S. Census. New items and scales for which established measures were not available, or which were specific to HMP (e.g., items about Health Risk

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[1] NHANES (National Health and Nutrition Exam Survey, CDC)
[2] HTHS (Health Tracking Household Survey)
[3] NHIS (National Health Interview Survey, CDC)
[4] SF-12 (Short Form Health Survey, RAND)
[5] CAHPS (Consumer Assessment of Healthcare Providers and Systems)
[6] Consumer Engagement in Health Care Survey (EBRI: CEHCS)

Assessments, understanding of HMP), were developed based on findings from 67 semi-structured interviews with HMP beneficiaries conducted by the evaluation team. New items underwent cognitive testing, and pre-testing for timing and flow before being included in the survey instrument.

Responses were recorded in a computer-assisted telephone interviewing (CATI) system programmed with the HMV survey.

Survey Response Characteristics

Overall, 9,350 Healthy Michigan Program enrollees were sampled throughout the data collection period. Seven cases with non-mailable addresses were excluded from the population; 100 cases were never mailed or called because data collection goals were achieved; 16 cases were never called because we did not have language-specific interviewers available. Thus, 123 of the original 9,350 were never contacted by phone.

Pre-notification letters were sent to the remaining 9,227 cases, which included a postcard to identify best time/number to call or refusal to participate. Phone calls were made to enrollees who did not refuse by postcard. Some numbers did not work, hence, no contact was established; some numbers worked but no contact was ever established, not allowing us to ascertain eligibility; and other numbers worked and contact was established.
We summarize the results briefly as follows:

Table 1. Call Results to Sampled Individuals

Description	n	Call Result
Total sample	9,350	
Nonmailable (e.g., bad address)	7	n/a
Not included – response goals achieved	100	n/a
Not called	16	n/a
Total sample contact attempted	9,227	
Contact never established		
1) Phone number not working	885	Nonworking number
2) Working but no contact made (e.g., left voicemail but never spoke with a person)	1,360	Unknown eligibility (UN)
Contact established		
3) Enrollee verified not at that number	583	Ineligible
4) Out of state	30	Ineligible
5) Deceased	3	Ineligible
6) Non-HMV language	36	Ineligible
7) Jail/Treatment facility	2	Ineligible
8) Refusal (by mail/phone)	945	Refusal (R)

9) Noncontact with enrollee (Spoke with a person other than enrollee) Other nonresponse (Spoke with an enrollee but did not participate for reasons other than clear refusal)	1,247	Noncontact (NC), Other (O)
10) Full completion	4,090	Interview (I)[7]
11) Partial completion	46*	Partial Interview (P)

*Eighteen cases were originally considered full completion but later recoded to partial completion after the weights were calculated because they had more than 20% of items missing.

There are many ways to calculate response rates as outlined by the American Association for Public Opinion Research (AAPOR, 2016[8]). Response rate formula 3 defined below is one of the common formulas used, particularly for telephone surveys.

$$RR3 = \frac{I}{(I + P) + (R + NC + O) + e \times UN}$$

where e is an estimate eligibility rate for the cases for which we cannot ascertain eligibility and the rest are noted in the table above. One way to estimate e is to use our call results among those we established contacts. As shown above, categories 3) through 7) are deemed ineligible, making 8) through 11) eligible among all contacted. Hence,

$$e = \frac{945 + 1237 + 4090 + 46}{9350 - 7 - 100 - 16 - 885 - 1360} = 90.6(\%)$$

By applying e as estimated above, we obtain the following response rate:

$$RR3 = \frac{4090}{(4090 + 46) + (945 + 1247) + .906 \times 1360} = 54.1(\%)$$

The weighted response rate was calculated to ascertain the response rate that is not subject to the sample design. We used the selection weight ($w_1$in the weighting steps document) to the RR3 formula and used weights applicable for known eligibility cases ($w_3$in the weighting steps document) to e, the estimated eligibility rate. The results are as follows:

$$weighted\ e = 89.9(\%)$$

$$Weighted\ RR3 = 53.7(\%)$$

Thus, the weighted response rate for the 2016 Healthy Michigan Voices enrollee survey was 53.7%.

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[7] NOTE: There was one case that responded to HMV but whose data were over-written due to system issues. This case was considered as a respondent in the response rate calculation but there were no survey data for this case.

[8] The American Association for Public Opinion Research. 2016. Standard Definitions: Final Dispositions of Case Codes and Outcome Rates for Surveys. 9th edition. AAPOR. Access from http://www.aapor.org/AAPOR_Main/media/publications/Standard-Definitions20169theditionfinal.pdf

Analyses

We calculated descriptive statistics for responses to all questions in the survey and these are highlighted in the tables within the body of this report. Weights were calculated and applied to data to adjust for the probability of selection (see Selection Weight, below), nonresponse bias (see Nonresponse Adjustment) and other adjustments (Nonworking Number adjustment, Unknown Eligibility adjustment, Known Eligibility adjustment). **As a result, please note that the proportions included in this report reflect how the results we observed would apply to the eligible population of HMP enrollees** (based on inclusion and exclusion criteria described on page 9). The number of individuals who responded to each survey question is noted in the tables in the report. When N is less than 4,090, this indicates that either some respondents missed that question or the question was part of a skip pattern and was therefore only asked of a subset of respondents according to their previous responses.

For analyses of bivariate and multivariate relationships, the types of analysis, models, variables included and how defined are described in text within this report and are included in the tables in the Appendix of this report. The specific tests are described in the table legends.

In a small number of cases (46), beneficiaries asked to end the survey early or did not follow the intended skip patterns, and their responses were excluded from this analysis. In cases where respondents skipped or refused to answer specific questions, those observations are not included in the analysis for those questions.

Selection Weight

The Healthy Michigan Voices survey sample was drawn each month from January through October 2016 from the HMP enrolled population using stratification which combines FPL and prosperity region. The same stratification sample design determined at the outset of the project was used every month. In each month, the eligible population was defined as HMP enrollees in the Data Warehouse who met the eligibility criteria listed on page 9.
Starting in the second month of sampling, beneficiaries sampled in the previous month(s) were excluded from the population.

Reflecting the sample design, the first step used an inverse of sampling probability and calculated selection weights for sample unit *i* in sampling month *m* in sampling stratum *h* as follows:

$$w_{1,hmi} = \frac{N_{hm}}{n_{hm}}$$

where N_{mh} is the population size and n_{mh} is the sample size.

We made adjustment for nonworking numbers, ineligible cases, unknown eligibility cases and nonresponse (noncontacts and refusal combined) separately as follows.

Nonworking Number Adjustment

Nonworking numbers were considered out of our target population. These numbers were considered out of scope and removed from the sample. We used the following adjustment, $f_{2,hmi}$, factor for this.

$$f_{2,hmi} = \begin{cases} 0, & if\ i\ was\ not\ a\ working\ number \\ \dfrac{\sum_i w_{1,hmi}}{\sum_i I\_WR_i \times w_{1,hmi}}, & if\ i\ was\ a\ working\ number \end{cases}$$

where I_WR_i was a 1/0 indicator for working number status (1: working number, 0: nonworking number). Essentially, $f_{2,hmi}$ removed the nonworking numbers from the scope and weighted up working numbers proportionally within each sampling stratum and month. The resulting weight was:

$$w_{2,hmi} = f_{2,hmi} \times w_{1,hmi}$$

Unknown Eligibility Adjustment

Besides the nonworking numbers, there were working numbers that were never contacted. With these cases, HMV eligibility could not be ascertained. Moreover, the eligibility rate may have differed systematically across strata and some other observed characteristics in the HMP enrollee data. Thus, a new adjustment factor was applied to the weight from the previous stage:

$$f_{3,hmi} = \begin{cases} 0, & if\ eligibility\ is\ unknown\ for\ i \\ \dfrac{\sum_i w_{2,hmi}}{\sum_i I\_UE_i \times w_{2,hmi}}, & if\ eligibility\ is\ known\ for\ i \end{cases}$$

where I_UE_i was a 1/0 indicator for unknown eligibility status (1: known eligibility; 0: unknown eligibility. The resulting weight was:

$$w_{3,hmi} = f_{3,hmi} \times w_{2,hmi}$$

Known Eligibility Adjustment

Among those who were contacted, some may not have been eligible for HMV for various reasons related to the eligibility criteria in Section 1. These cases fell outside of the target population and, hence, were removed through the following:

$$f_{4,hmi} = \begin{cases} 0, & if\ i\ is\ ineligible \\ \dfrac{\sum_i w_{3,hmi}}{\sum_i I\_EL_i \times w_{3,hmi}}, & if\ i\ is\ eligible \end{cases}$$

where I_EL_i was a 1/0 indicator for eligibility status (1: eligible; 0: ineligible). The resulting weight was:

$$w_{4,hmi} = f_{4,hmi} \times w_{3,hmi}$$

Nonresponse Adjustment

Those who are contacted and eligible were retained after the previous step. This did not necessarily mean a direct contact had been made with the enrollee. With some numbers, contact with the sample enrollee was never established. With the remainder, when an interview was solicited, some may have refused or declined participation for various reasons. These were all considered as nonresponse. Overall, there were 6,327 eligible cases; among them, 4,090 were respondents (64.6%).[9]

From the HMV sample frame data, we considered the following characteristics for nonresponse analysis as they were available for both respondents and nonrespondents:

- Sex
- Age (19-34; 35-49; 50-64 years old)
- Race/ethnicity (Hispanic; Non-Hispanic White; Non-Hispanic Black; Non-Hispanic other)
- First HMP month (2 years or more ago; less than 2 years ago)

Additionally, we had the following sampling information available for both respondents and nonrespondents:

- Stratum (FPL x Region)
- FPL
- Region
- Sampling month

Table 2 includes the number of eligible cases by characteristics listed above and the proportion of respondents among eligible cases. Younger and male enrollees were less likely to respond than their counterparts. Based on race/ethnicity, non-Hispanic Black enrollees were most likely to respond, and those in the non-Hispanic other group were least likely to do so. While the proportion of respondents was similar across income levels, among the four regions, Detroit had the lowest proportion. Among 12 strata, UP/NW/NE with 100%+ FPL at 69.5% and W/EC/E with 36-99% FPL at 69.2% had the highest proportion of respondents. Detroit with 36-99% FPL had the lowest proportion at 58.9%. No clear pattern was observed by sampling month. Nonresponse did not occur identically across characteristics as seen in Table 2, which required an adjustment. Following Lee and Valliant (2008)[10], a logistic regression model was used to predict response while controlling for differences in characteristics between respondents and nonrespondents. The predictors included age, sex, race/ethnicity, first month on HMP, sampling strata, sampling month and the interaction between sampling strata and sampling month. The adjustment factor, $f_{5,i}$, was the inverse of response propensity predicted from the logistic regression. The resulting weight was:

$$w_{5,imh} = w_{4,mhi} \times f_{5,i}$$

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[9] There was one case that responded to HMV but whose data were over-written due to system issues. This case was considered as a respondent in the response rate calculation but dropped in the weighting as there were no survey data for this case.

[10] Lee S, Valliant R. 2008. Weighting telephone samples using propensity scores. Advances in Telephone Survey Methodology. 170-183.

Table 2. Proportion of Respondents Among Eligible Cases by Sample Characteristics (for Non-Response Adjustments for Weighting Purpose)

Characteristics	Eligible (n)	Respondents (%)	Characteristics	Eligible (n)	Respondents (%)
Total	6,327	64.9	**Sampling Stratum**		
Age			1. UP/NW/NE, 0-35%	443	65.2
19-35 years old	2,304	60.2	2. UP/NW/NE, 36-99%	385	63.9
36-49 years old	1,755	64.4	3. UP/NW/NE, 100%+	305	69.5
50-64 years old	2,268	70.1	4. W/EC/E, 0-35%	742	68.1
Sex			5. W/EC/E, 36-99%	676	69.2
Female	3,562	67.8	6. W/EC/E, 100%+	464	63.8
Male	2,765	61.2	7. SC/SW/SE, 0-35%	481	67.6
Race/Ethnicity			8. SC/SW/SE, 36-99%	468	66.2
Hispanic	174	64.4	9. SC/SW/SE, 100%+	315	65.1
Non-Hispanic White	4,396	64.4	10. DET, 0-35%	799	61.3
Non-Hispanic Black	1,121	68.8	11. DET, 36-99%	733	58.9
Non-Hispanic Other	636	61.6	12. DET, 100%+	516	63.8
First month on HMP			**Sampling Month**		
Less than 2 yrs ago	3,518	62.6	1	422	61.8
2 yrs or more ago	2,809	67.8	2	576	64.9
FPL			3	698	66.5
0-35%	2,465	65.3	4	735	65.4
36-99%	2,262	64.4	5	701	66.9
100%+	1,600	65.1	6	680	67.8
Region			7	866	68.8
UP/NW/NE	1,133	65.9	8	658	63.2
W/EC/E	1,882	67.4	9	654	57.6
SC/SW/SE	1,264	66.5	10	337	61.7
DET	2,048	61.1			

Post-stratification

The target population of the HMV survey is HMP enrollees ever eligible for HMV (as defined in Section 1) between January and October 2016. There were 384,262 such persons. From the sample frame data we had information about the characteristics of this population. Table 3 compares the population and the sample weighted by nonresponse adjustment weight ($w_{5,imh}$) with respect to age, sex, race/ethnicity, first month enrolled in HMP, sampling stratum, FPL and region. Our weighted sample matched the population reasonably well across most characteristics, except for age, sex and first month on HMP. Compared to the population, our sample overrepresented beneficiaries who were older, females or who enrolled in HMP during the first 3 months of HMP. Hence, this known discrepancy was handled through post-stratification. All the characteristics in Table 3 were controlled for in the post-stratification

using an iterative proportional fitting method (Deville et al., 1993)[11]. This process forced the sample to match the population with respect to the controlled characteristics.
Post-stratification may force the weights to be extreme. These extreme weights increase the variability of estimates and, in turn, lower statistical power. In order to minimize the effect of extreme weights, these weights are trimmed. To address this issue we used the Individual and Global Cap Value (IGCV) method introduced by Izrael et al. (2009)[12]. This method sets thresholds for minimum and maximum adjustment factors in relation to the individual weights and to all weights globally. Specifically, our procedure set the global high cap at 7, the global low cap at 0.12, the individual high cap at 5 and the individual low cap at 0.2. The trimmed weights were normalized to the population total of 384,262. The resulting weight is $w_{6,imh}$. Table 3 includes the sample characteristics weighted by $w_{6,imh}$. When using the post-stratified weight, the sample matched perfectly. However, compared to when using the nonresponse adjustment weight, there was a slight increase in standard error due to variability in weights introduced by post-stratification.

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[11] Deville JC, Särndal CE, Sautory O. 1993. Generalized raking procedures in survey sampling. *Journal of the American Statistical Association*. 88(423):1013-20.

[12] Izrael D, Battaglia MP, Frankel MR. 2009. Extreme survey weight adjustment as a component of sample balancing (aka raking). In Proceedings from the Thirty-Fourth Annual SAS Users Group International Conference.

Table 3. Comparison of Eligible HMP Population and HMV Sample

Characteristics	Population		Sample				
				Weighted by w_5		Weighted by w_6	
	N	%	n	%	SE	%	SE
Total	384,262		4,090				
Age							
19-35 years old	163,071	42.4	1,380	36.9	0.9	42.3	1.0
36-49 years old	113,660	29.6	1,125	28.1	0.8	29.6	0.9
50-64 years old	107,531	28.0	1,585	34.9	0.9	28.1	0.8
Sex							
Female	197,883	51.5	2,409	54.1	0.9	51.6	1.0
Male	186,379	48.5	1,681	45.9	0.9	48.4	1.0
Race/Ethnicity							
Non-Hispanic White	232,688	60.6	2,784	63.1	0.9	60.4	1.0
Non-Hispanic Black	91,208	23.7	807	23.2	0.8	25.8	0.9
Other	60,366	15.7	499	13.7	0.7	13.8	0.7
First month on HMP							
4-6, 2014	158,983	41.4	2,146	49.7	0.9	41.5	0.9
7-12, 2014	89,945	23.4	1,111	27.6	0.8	23.4	0.8
2015	135,334	35.2	833	22.7	0.8	35.2	1.1
Strata							
1. UP/NW/NE, 0-35%	13,282	3.5	288	3.6	0.2	3.5	0.1
2. UP/NW/NE, 36-99%	11,835	3.1	246	3.3	0.2	3.1	0.1
3. UP/NW/NE, 100%+	9,291	2.4	212	2.6	0.2	2.4	0.0
4. W/EC/E, 0-35%	52,224	13.6	503	13.4	0.6	13.6	0.3
5. W/EC/E, 36-99%	33,157	8.6	467	8.8	0.4	8.6	0.2
6. W/EC/E, 100%+	24,248	6.3	295	6.5	0.4	6.3	0.2
7. SC/SW/SE, 0-35%	34,675	9.0	323	8.7	0.5	9.0	0.3
8. SC/SW/SE, 36-99%	20,909	5.4	309	5.5	0.3	5.5	0.2

9. SC/SW/SE, 100%+	15,569	4.1	205	4.0	0.3	4.1	0.2
10. DET, 0-35%	99,024	25.8	486	25.0	1.0	25.7	0.5
11. DET, 36-99%	43,569	11.3	428	11.7	0.6	11.2	0.4
12. DET, 100%+	26,479	6.9	328	6.9	0.4	6.9	0.2
FPL							
0-35%	199,205	51.8	1,600	50.7	0.9	51.8	0.5
36-99%	109,470	28.5	1,450	29.3	0.8	28.4	0.4
100%+	75,587	19.7	1,040	20.0	0.6	19.8	0.3
Region							
UP/NW/NE	34,408	9.0	746	9.4	0.4	9.0	0.2
W/EC/E	109,629	28.5	1,265	28.8	0.8	28.6	0.4
SC/SW/SE	71,153	18.5	837	18.2	0.6	18.6	0.4
DET	169,072	44.0	1,242	43.6	1.0	43.8	0.5

RESULTS

Demographic Characteristics of Respondents

After weighting, demographic characteristics of respondents closely match characteristics of the eligible HMP population as a whole (see Table 3, above).

Table 4. Demographic Characteristics

	%	95% CI
Gender (n=4,090)		
F (n=2,409)	51.6	[49.6,53.5]
M (n=1,681)	48.4	[46.5,50.4]
Age (n=4,090)		
19-34 (n=1,303)	40.0	[38.0,42.0]
35-50 (n=1,301)	34.0	[32.1,35.9]
51-64 (n=1,486)	26.0	[24.5,27.6]
Race (n=4,039)		
White (n=2,784)	61.2	[59.3,63.0]
Black or African American (n=807)	26.1	[24.3,27.9]
Other (n=306)	8.8	[7.7,10.0]
More than one (n=142)	4.0	[3.3,4.9]

Hispanic/Latino (n=4,056)		
Yes (n=188)	5.2	[4.4,6.2]
No (n=3,856)	94.3	[93.3,95.2]
Don't know (n=12)	0.5	[0.2,0.9]
Arab, Chaldean, Middle Eastern (n=4,055)		
Yes (n=204)	6.2	[5.3,7.2]
No (n=3,842)	93.6	[92.5,94.5]
Don't know (n=9)	0.3	[0.1,0.6]
Region (n=4,090)		
Upper Peninsula/Northwest/Northeast (n=746)	9.0	[8.6,9.4]
West/East Central/East (n=1,265)	28.6	[27.8,29.4]
South Central/Southwest/Southeast (n=837)	18.6	[17.8,19.3]
Detroit Metro (n=1,242)	43.8	[42.8,44.9]
FPL (n=4,090)		
0-35% (n=1,600)	51.8	[50.8,52.8]
36-99% (n=1,450)	28.4	[27.6,29.3]
≥100% (n=1,040)	19.8	[19.1,20.4]
Medicaid Health Plan (n=4,088)		
Aetna (n=58)	1.7	[1.2,2.3]
Blue Cross (n=356)	11.6	[10.2,13.1]
Harbor (n=18)	0.7	[0.4,1.3]
McLaren (n=633)	13.0	[11.9,14.2]
Meridian (n=1,265)	29.8	[28.1,31.6]
Midwest (n=3)	0.1	[0.0,0.2]
Molina (n=701)	18.0	[16.5,19.5]
Priority (n=268)	5.9	[5.2,6.7]
Total Health Care (n=85)	2.8	[2.2,3.7]
United (n=443)	13.2	[11.8,14.7]
Upper Peninsula Health Plan (n=258)	3.2	[2.8,3.6]
Employment Status (n=4,075)		
Employed or self-employed (n=2,079)	48.8	[47.0,50.7]
Out of work ≥1 year (n=707)	19.7	[18.1,21.3]
Out of work <1 year (n=258)	7.9	[6.8,9.1]
Homemaker (n=217)	4.5	[3.8,5.3]
Student (n=161)	5.2	[4.3,6.2]
Retired (n=167)	2.5	[2.1,3.0]
Unable to work (n=479)	11.3	[10.1,12.5]
Don't know (n=7)	0.2	[0.1,0.4]
Veteran (n=4,086)		
Yes (n=125)	3.4	[2.7,4.2]
No (n=3,958)	96.5	[95.7,97.2]
Don't know (n=3)	0.1	[0.0,0.5]

Marital Status (n=4,073)		
Married (n=1,008)	20.4	[19.0,21.8]
Partnered (n=185)	4.3	[3.6,5.1]
Divorced (n=865)	18.2	[16.8,19.6]
Widowed (n=147)	2.8	[2.3,3.4]
Separated (n=119)	2.8	[2.3,3.4]
Never Married (n=1,745)	51.6	[49.6,53.5]
Don't know (n=4)	0.1	[0.0,0.2]
Any chronic health condition present (n=4,090)		
Yes (n=2,986)	69.2	[67.3,71.0]
No (n=1,104)	30.8	[29.0,32.7]
At least one physical health condition present (n=4,090)		
Yes (n=2,689)	60.8	[58.8,62.8]
No (n=1,401)	39.2	[37.2,41.2]
At least one mental health condition present (n=4,090)		
Yes (n=1,351)	32.1	[30.3,33.9]
No (n=2,739)	67.9	[66.1,69.7]
Other household enrollee (n=4,082)		
Yes (n=1,592)	35.7	[34.0,37.5]
No (n=2,289)	58.0	[56.1,59.8]
Don't know (n=201)	6.3	[5.3,7.6]

Insurance Coverage Prior to HMP

More than half (57.9%) of survey respondents did not have health insurance at any time in the 12 months prior to HMP enrollment. Of those who reported having health insurance at some point during the 12 months prior to HMP enrollment, the majority (73.8%) had health insurance for all 12 months. Thus, less than one-third (30.2%) of all respondents reported that they had insurance for all 12 months prior to enrolling in HMP. Approximately half (50.8%) of survey respondents who reported having health insurance at any time in the 12 months prior to HMP enrollment had Medicaid, MiChild, or health coverage through another state health program, while a quarter (26.2%) had private insurance through a job or union. Among those who reported private insurance they purchased themselves or someone else purchased (10.2%), approximately one-third (31.5%) purchased the insurance on the healthcare.gov website, and 61.8% of those respondents who purchased health insurance on the healthcare.gov website reported receiving a subsidy.

	%	95% CI
At any time during the 12 months BEFORE you enrolled in the Healthy Michigan Plan, did you have any type of health insurance? (n=4,087)		
Yes (n=1,667)	40.7	[38.8,42.6]
No (n=2,374)	57.9	[55.9,59.8]
Don't know (n=46)	1.4	[1.0,2.1]

[If Yes] Did you have health insurance for all 12 months, 6-11 months, less than 6 months, or not at all? (n=1,667)		
All 12 months (n=1,235)	73.8	[71.1,76.5]
6-11 months (n=245)	15.2	[13.0,17.6]
Less than 6 months (n=129)	7.6	[6.2,9.3]
Don't know (n=58)	3.4	[2.5,4.7]
What type of health insurance did you have?* (n=1,622)		
Medicaid, MiChild, or other state program (n=834)	50.8	[47.7,53.9]
Private insurance provided through a job or union (n=409)	26.2	[23.6,29.0]
Private insurance purchased by you or someone else (n=157)	10.2	[8.3,12.6]
County health plan (n=127)	6.3	[5.2,7.7]
Veterans Health or VA care (n=21)	1.4	[0.8,2.3]
CHAMPUS, TRICARE, other military coverage (n=3)	0.3	[0.1,1.2]
Medicare (n=5)	0.3	[0.1,0.7]
Indian Health Service (n=3)	0.1	[0.0,0.3]
Other (n=83)	5.6	[4.3,7.3]
Don't know (n=23)	1.2	[0.8,1.9]
[If private insurance purchased by you or someone else] Was this insurance purchased on the HealthCare.gov exchange? (n=152)		
Yes (n=59)	31.5	[22.6,41.9]
No (n=75)	55.4	[44.1,66.2]
Don't know (n=18)	13.1	[7.6,21.7]
[If Yes] Did you receive a subsidy? (n=59)		
Yes (n=37)	61.8	[43.9,76.9]
No (n=18)	29.0	[18.1,43.1]
Don't know (n=4)	9.3	[2.2,31.3]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Impact of Prior Year Insurance Status on Improvements in Foregone Care, Access and Health

Respondents who were uninsured all 12 months in the year prior to enrolling in HMP were more likely than those who were insured all 12 months, and those who were insured part of the year, to report foregoing care during that year, and more likely to report foregoing care due to cost concerns (See Appendix Table 1).

Those who were insured all 12 months prior to enrolling in HMP were less likely to report improvements in access to care or improvements in physical, mental or oral health (See Appendix Table 1).

Those who were insured all 12 months prior to HMP agreed less that HMP had reduced stress and they worried less about something bad happening to their health (See Appendix Table 1).

Current Health Status/Change in Health with HMP

More than one-third of respondents rated their health as either excellent or very good (36.3%). Since enrolling in the Healthy Michigan Plan, most respondents reported their physical health had improved (47.8%) or stayed the same (46.1%), their mental health had improved (38.2%) or stayed the same (56.8%) and their dental health had improved (39.5%) or stayed the same (45.5%). About one-third (31.7%) of survey respondents reported losing weight in the past year.

	Mean or %	95% CI
In general, would you say your health is... (n=4,088)		
Excellent (n=337)	9.5	[8.4,10.8]
Very good (n=1,041)	26.8	[25.0,28.7]
Good (n=1,448)	33.8	[32.0,35.7]
Fair (n=931)	22.2	[20.7,23.8]
Poor (n=324)	7.5	[6.6,8.6]
Don't know (n=7)	0.1	[0.0,0.4]
For how many days in the past 30 days was your physical health not good? (n=4,033)		
<14 of past 30 days (n=3,055)	77.2	[75.5,78.7]
≥14 of past 30 days (n=978)	22.8	[21.3,24.5]
For how many days in the past 30 days was your physical health not good? (n=4,033)	Mean 6.8	[6.4,7.2]
Overall, since you enrolled in the Healthy Michigan Plan, would you say your physical health has gotten better, stayed the same, OR gotten worse? (n=4,086)		
Gotten better (n=1,961)	47.8	[45.8,49.8]
Stayed the same (n=1,851)	46.1	[44.2,48.1]
Gotten worse (n=256)	5.5	[4.8,6.4]
Don't know (n=18)	0.5	[0.3,1.0]
For how many days in the past 30 days was your mental health not good? (n=4,002)		
<14 of past 30 days (n=3,226)	80.1	[78.5,81.7]
≥14 of past 30 days (n=776)	19.9	[18.3,21.5]
For how many days in the past 30 days was your mental health not good? (n=4,002)	Mean 6.0	[5.6,6.4]
Overall, since you enrolled in Healthy Michigan Plan, would you say your mental and emotional health has gotten better, stayed the same, OR gotten worse? (n=4,080)		
Gotten better (n=1,550)	38.2	[36.3,40.1]
Stayed the same (n=2,318)	56.8	[54.8,58.7]
Gotten worse (n=186)	4.6	[3.9,5.5]
Don't know (n=26)	0.5	[0.3,0.7]

During the past 30 days, for how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation? (n=4,079)		
0-13 days (n=3,277)	80.6	[79.1,82.1]
14-30 days (n=749)	18.2	[16.8,19.8]
Don't know (n=53)	1.1	[0.8,1.6]
During the past 30 days, for how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation? (n=4,026) [Note: Same as above but excludes "Don't know"]		
<14 of past 30 days (n=3,277)	81.6	[80.0,83.0]
≥14 of past 30 days (n=749)	18.4	[17.0,20.0]
During the past 30 days, for how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation? (n=4,026)	Mean 5.3	[4.9,5.7]
Since you enrolled in the Healthy Michigan Plan, has the health of your teeth and gums gotten better, stayed the same, OR gotten worse? (n=4,084)		
Gotten better (n=1,641)	39.5	[37.6,41.5]
Stayed the same (n=1,809)	45.5	[43.5,47.5]
Gotten worse (n=443)	10.4	[9.3,11.6]
Don't know (n=191)	4.6	[3.9,5.5]
Compared to 12 months ago, how would you describe your weight? (n=4,084)		
Lost weight (n=1,300)	31.7	[29.9,33.6]
Gained weight (n=1,036)	26.4	[24.7,28.2]
Stayed about the same (n=1,732)	41.5	[39.6,43.4]
Don't know (n=16)	0.4	[0.2,0.7]

Chronic Health Conditions

More than two-thirds (69.2%) reported any chronic health condition with 60.8% reporting at least one physical health condition and 32.1% reporting at least one mental health condition. About one-fourth (23.7%) reported having both a physical health condition and a mental health condition. Nearly one-third (30.3%) reported that they had a chronic health condition that was newly diagnosed since enrolling in HMP. Almost one-fifth (18.4%) of respondents reported a functional limitation.

	Col %	95% CI
At least one physical health condition present (n=4,090)		
Yes (n=2,689)	60.8	[58.8,62.8]
No (n=1,401)	39.2	[37.2,41.2]
At least one mental health condition present (n=4,090)		
Yes (n=1,351)	32.1	[30.3,33.9]
No (n=2,739)	67.9	[66.1,69.7]

Any chronic health condition present (n=4,090)		
Yes (n=2,986)	69.2	[67.3,71.0]
No (n=1,104)	30.8	[29.0,32.7]
Any physical health condition AND any mental health condition		
Yes (n=1,054)	23.7	[22.2,25.3]
No (n=3,036)	76.3	[74.7,77.8]
Any new diagnoses since HMP enrollment (n=4,090)		
Yes (n=1,318)	30.6	[28.8,32.4]
No (n=2,772)	69.4	[67.6,71.2]
Functional limitations (n=4,026)		
Yes (n=749)	18.4	[17.0,20.0]
No (n=3,277)	81.6	[80.0,83.0]

The most common chronic conditions reported were hypertension (31.3%), mood disorder (30.4%), and other health conditions (29.2%). Respondents frequently found out about these chronic conditions after enrollment in HMP.

	%	95% CI
Has a doctor or other health professional every told you that you had any of the following?		
Hypertension (n=4,089)		
Yes (n=1,411)	31.3	[29.6,33.1]
No (n=2,661)	68.2	[66.4,69.9]
Don't know (n=17)	0.5	[0.3,0.9]
[If Yes] Did you find out you had [Hypertension] before or after you enrolled in the Healthy Michigan Plan? (n=1,411)		
Before (n=960)	66.6	[63.4,69.7]
After (n=441)	32.4	[29.4,35.6]
Don't know (n=10)	0.9	[0.4,2.0]
Heart disease (n=4,089)		
Yes (n=426)	9.7	[8.6,10.9]
No (n=3,645)	90.0	[88.8,91.1]
Don't know (n=18)	0.3	[0.2,0.5]
[If Yes] Did you find out you had [Heart disease] before or after you enrolled in the Healthy Michigan Plan? (n=426)		
Before (n=290)	65.6	[59.3,71.4]
After (n=135)	34.3	[28.5,40.6]
Don't know (n=1)	0.1	[0.0,0.8]
Diabetes (n=4,089)		
Yes (n=499)	10.8	[9.7,12.0]
No (n=3,574)	88.8	[87.6,89.9]
Don't know (n=16)	0.4	[0.2,0.7]

[If Yes] Did you find out you had [Diabetes] before or after you enrolled in the Healthy Michigan Plan? (n=499)		
Before (n=331)	63.8	[58.1,69.1]
After (n=163)	35.4	[30.1,41.1]
Don't know (n=5)	0.8	[0.3,2.4]
Cancer (non-skin) (n=4,089)		
Yes (n=203)	3.7	[3.2,4.4]
No (n=3,876)	96.0	[95.3,96.6]
Don't know (n=10)	0.3	[0.1,0.6]
[If Yes] Did you find out you had [Cancer (non-skin)] before or after you enrolled in the Healthy Michigan Plan? (n=203)		
Before (n=130)	60.3	[51.8,68.3]
After (n=72)	39.2	[31.3,47.8]
Don't know (n=1)	0.5	[0.1,3.2]
Mood disorder (n=4,084)		
Yes (n=1,288)	30.4	[28.7,32.2]
No (n=2,786)	69.2	[67.4,71.0]
Don't know (n=10)	0.4	[0.2,0.8]
[If Yes] Did you find out you had [Mood disorder] before or after you enrolled in the Healthy Michigan Plan? (n=1,288)		
Before (n=941)	70.9	[67.5,74.0]
After (n=342)	28.8	[25.7,32.2]
Don't know (n=5)	0.3	[0.1,0.9]
Stroke (n=4,089)		
Yes (n=88)	1.9	[1.5,2.5]
No (n=3,997)	97.9	[97.3,98.4]
Don't know (n=4)	0.2	[0.0,0.5]
[If Yes] Did you find out you had [Stroke] before or after you enrolled in the Healthy Michigan Plan? (n=88)		
Before (n=53)	59.8	[46.7,71.7]
After (n=35)	40.2	[28.3,53.3]
Don't know (n=0)	0.0	
Asthma (n=4,088)		
Yes (n=725)	17.1	[15.7,18.6]
No (n=3,353)	82.7	[81.2,84.1]
Don't know (n=10)	0.2	[0.1,0.4]
[If Yes] Did you find out you had [Asthma] before or after you enrolled in the Healthy Michigan Plan? (n=725)		
Before (n=637)	86.6	[83.0,89.5]
After (n=84)	12.9	[10.0,16.4]
Don't know (n=4)	0.6	[0.2,2.0]

Chronic bronchitis, COPD, emphysema (n=4,089)		
Yes (n=479)	10.5	[9.4,11.7]
No (n=3,594)	89.1	[87.9,90.2]
Don't know (n=16)	0.4	[0.2,0.8]
[If Yes] Did you find out you had [Chronic bronchitis, COPD, emphysema] before or after you enrolled in the Healthy Michigan Plan? (n=479)		
Before (n=304)	65.0	[59.5,70.2]
After (n=173)	34.8	[29.6,40.3]
Don't know (n=2)	0.2	[0.0,0.8]
Substance use disorder (n=4,088)		
Yes (n=165)	4.1	[3.4,5.0]
No (n=3,916)	95.7	[94.8,96.4]
Don't know (n=7)	0.2	[0.1,0.5]
[If Yes] Did you find out you had [Substance use disorder] before or after you enrolled in the Healthy Michigan Plan? (n=165)		
Before (n=148)	88.9	[81.6,93.5]
After (n=15)	9.5	[5.3,16.3]
Don't know (n=2)	1.6	[0.4,7.1]
Other chronic condition (n=4,087)		
Yes (n=1,317)	29.2	[27.5,30.9]
No (n=2,759)	70.5	[68.8,72.2]
Don't know (n=11)	0.3	[0.1,0.5]
[If Yes] Did you find out you had [Other chronic condition] before or after you enrolled in the Healthy Michigan Plan? (n=1,317)		
Before (n=829)	63.8	[60.6,67.0]
After (n=451)	33.6	[30.5,36.8]
Don't know (n=37)	2.6	[1.7,3.9]

Health Risk Assessment (HRA)

Approximately half (49.3%) of survey respondents reported that they remembered completing the HRA. This is higher than the completion rate obtained using data from the MDHHS Data Warehouse. One potential explanation for this discrepancy between the self-reported rate and the State reported rate is that some respondents may have completed only the patient portion of the HRA but reported HRA completion in the survey; without also turning in the provider portion of the HRA such partial completions would be marked incomplete in the Data Warehouse. Other potential reasons include recall bias or misunderstanding about the HRA as a special form developed for Healthy Michigan Plan enrollees (e.g., some respondents may be unable to differentiate between the HRA and other health questionnaires they had completed). Among those who reported completing the HRA, the most common reasons for completion were that their primary care provider (PCP) suggested it (45.9%), they got it in the mail (33%), and/or that they completed it during enrollment on the phone (12.6%). Among respondents who reported getting the HRA in the mail, 71.9% said they took the form to their PCP.

	%	95% CI
Do you remember completing the Health Risk Assessment? (n=4,089)		
Yes (n=2,102)	49.3	[47.3,51.2]
No (n=1,681)	42.7	[40.8,44.7]
Don't know (n=306)	8.0	[6.9,9.2]
[If Yes] What led you to complete it?* (n=2,102)		
PCP suggested (n=996)	45.9	[43.2,48.7]
Got it in the mail (n=693)	33.0	[30.4,35.6]
At enrollment on the phone (n=253)	12.6	[10.9,14.6]
Health plan suggested (n=149)	7.3	[6.0,8.9]
To stay on top of my health (n=64)	2.9	[2.1,3.9]
Gift card/money/reward (n=57)	2.5	[1.8,3.4]
To save money on copays/cost-sharing (n=2)	0.1	[0.0,0.3]
Other (n=50)	2.7	[1.8,4.0]
Don't know (n=79)	3.9	[3.0,5.2]
[If 'Got it in the mail'] Did you take the form to your primary care provider? (n=622)		
Yes (n=481)	71.9	[66.5,76.7]
No (n=106)	22.4	[17.8,27.7]
Don't know (n=35)	5.7	[3.7,8.8]

**Respondents were able to provide more than one response for this question. As a result, percentages may exceed 100%.*

A majority of those who reported completing the HRA felt that the HRA was valuable for improving their health (83.7%) and was helpful for their PCP to understand their health needs (89.7%). About one-third (31.5%) of those who said they completed the HRA felt that the HRA was not that helpful because they already knew what they needed to do to be healthy.

	%	95% CI
I think doing the Health Risk Assessment was valuable for me to improve my health. (n=2,100)		
Strongly agree (n=399)	19.0	[16.8,21.3]
Agree (n=1,354)	64.7	[62.0,67.4]
Neutral (n=222)	10.2	[8.7,12.1]
Disagree (n=104)	4.8	[3.8,6.1]
Strongly disagree (n=10)	0.6	[0.3,1.2]
Don't know (n=11)	0.6	[0.3,1.5]

I think doing the Health Risk Assessment was helpful for my primary care provider to understand my health needs. (n=2,099)		
Strongly agree (n=515)	24.9	[22.6,27.4]
Agree (n=1,369)	64.8	[62.1,67.4]
Neutral (n=121)	6.1	[4.9,7.6]
Disagree (n=62)	2.4	[1.8,3.4]
Strongly disagree (n=8)	0.4	[0.2,0.8]
Don't know (n=24)	1.3	[0.8,2.2]
I know what I need to do to be healthy, so the Health Risk Assessment wasn't that helpful. (n=2,100)		
Strongly agree (n=92)	4.5	[3.5,5.7]
Agree (n=567)	27.0	[24.7,29.5]
Neutral (n=308)	16.8	[14.7,19.2]
Disagree (n=1,024)	46.2	[43.5,48.9]
Strongly disagree (n=87)	4.2	[3.2,5.6]
Don't know (n=22)	1.2	[0.7,2.1]

Among those who reported completing the HRA, 80.7% reported choosing to work on at least one health behavior. The most common behaviors that respondents reported selecting were related to nutrition/diet (57.2%) and exercise/activity (52.6%). Among respondents who chose to work on a health behavior, 61.3% said their health care provider or health plan helped them work on this behavior. Some (8%) said there was help they wanted that they did not get.

	%	95% CI
After going through the Health Risk Assessment, or at a primary care visit, did you choose to work on a healthy behavior or do something good for your health? (n=2,100)		
Yes (n=1,690)	80.7	[78.5,82.8]
No (n=393)	18.6	[16.6,20.9]
Don't know (n=17)	0.6	[0.3,1.1]
[If Yes] What did you choose to do?* (n=1,690)		
Nutrition/diet (n=947)	57.2	[54.2,60.2]
Exercise/activity (n=915)	52.6	[49.5,55.7]
Reduce/quit tobacco use (n=317)	18.4	[16.2,20.9]
Lose weight (n=191)	10.1	[8.5,11.9]
Reduce/quit alcohol consumption (n=55)	3.4	[2.5,4.8]
Take medicine regularly (n=32)	2.3	[1.5,3.5]
Monitor my blood pressure/blood sugar (n=33)	1.5	[1.0,2.2]
Flu shot (n=20)	0.9	[0.5,1.4]
Follow-up appointment for chronic disease (n=11)	0.6	[0.3,1.1]
Go to the dentist (n=7)	0.4	[0.2,1.1]
Treatment for substance use disorder (n=3)	0.2	[0.0,0.5]
Other (n=98)	5.4	[4.3,6.8]
Don't know (n=11)	0.8	[0.4,1.7]

Did your health care provider or health plan help you work on this healthy behavior? (n=1,677)		
Yes (n=1,088)	61.3	[58.2,64.4]
No (n=382)	26.3	[23.5,29.3]
NA (n=200)	11.9	[10.1,14.0]
Don't know (n=7)	0.4	[0.2,1.0]
[If Yes or No] Was there help that you wanted that you didn't get? (n=1,470)		
Yes (n=131)	8.0	[6.6,9.7]
No (n=1,313)	90.0	[88.0,91.7]
NA (n=18)	1.2	[0.6,2.3]
Don't know (n=8)	0.8	[0.3,2.0]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Forty percent of survey respondents agreed that information about healthy behavior rewards led them do something they might not have done otherwise. A quarter (26.1%) disagreed, and one-fifth (21.3%) said they did not know.

	%	95% CI
Information about the healthy behavior rewards that I can earn has led me to do something I might not have done otherwise. (n=4,084)		
Strongly agree (n=204)	5.2	[4.4,6.3]
Agree (n=1,431)	35.4	[33.5,37.3]
Neutral (n=487)	12.0	[10.8,13.3]
Disagree (n=969)	24.1	[22.4,25.8]
Strongly disagree (n=75)	2.0	[1.5,2.6]
Don't know (n=918)	21.3	[19.8,22.9]

Health Behaviors and Health Education

More than one-third (36.7%) of survey respondents reported getting a flu shot last fall or winter. Almost one-third (31.9%) of survey respondents reported exercising every day for at least 20 minutes, 48.8% of respondents reported drinking sugary drinks two or fewer days per week, and 37.5% of respondents reported eating three or more servings of fruits or vegetables every day.

	%	95% CI
Did you get a flu shot last fall or winter? (n=4,090)		
Yes (n=1,592)	36.7	[34.8,38.6]
No (n=2,463)	62.4	[60.4,64.3]
Don't know (n=35)	0.9	[0.6,1.5]

In the last 7 days, how many days did you exercise for at least 20 minutes? (n=4,089)		
Every day (n=1,392)	31.9	[30.1,33.7]
3-6 days (n=1,334)	33.5	[31.6,35.4]
1-2 days (n=606)	15.9	[14.4,17.4]
0 days (n=746)	18.4	[17.0,20.0]
Don't know (n=11)	0.3	[0.1,0.6]
In the last 7 days, how many days did you drink sugary drinks, like soda or pop, sweetened fruit drinks, sports drinks, or energy drinks? (n=4,088)		
Every day (n=1,281)	32.4	[30.6,34.3]
3-6 days (n=688)	18.7	[17.2,20.4]
1-2 days (n=886)	21.4	[19.8,23.0]
0 days (n=1,231)	27.4	[25.8,29.2]
Don't know (n=2)	0.1	[0.0,0.3]
In the last 7 days, how many days did you eat 3 or more servings of fruits or vegetables in a day? (n=4,087)		
Every day (n=1,609)	37.5	[35.6,39.4]
3-6 days (n=1,374)	33.6	[31.8,35.5]
1-2 days (n=603)	16.4	[15.0,18.0]
0 days (n=476)	11.8	[10.5,13.1]
Don't know (n=25)	0.7	[0.4,1.1]

About half of respondents reported talking with a health professional about exercise (48.6%) and diet and nutrition (49.8%) in the past 12 months. Among those who reported binge drinking behavior in the past seven days, 30.3% reported talking to a health professional about safe alcohol use.

	%	95% CI
In the last 12 months, has a doctor, nurse, or other health professional talked with you about exercise? (n=4,090)		
Yes (n=2,091)	48.6	[46.7,50.6]
No (n=1,983)	50.9	[48.9,52.9]
Don't know (n=16)	0.4	[0.2,1.0]
In the last 12 months, has a doctor, nurse, or other health professional talked with you about diet and nutrition? (n=4,089)		
Yes (n=2,107)	49.8	[47.8,51.8]
No (n=1,966)	49.7	[47.7,51.7]
Don't know (n=16)	0.5	[0.2,1.1]
In the last 7 days, on how many days did you have 5 or more alcoholic drinks (males) or 4 or more alcoholic drinks (females)? (n=4,087)		
Every day (n=43)	1.1	[0.8,1.6]
3-6 days (n=145)	4.0	[3.3,4.9]
1-2 days (n=556)	14.5	[13.1,16.0]
0 days (n=3,341)	80.3	[78.7,81.9]
Don't know (n=2)	0.1	[0.0,0.4]

[If response other than 0 days] In the last 12 months, has a doctor, nurse, or other health professional talked with you about safe alcohol use? (n=747)		
Yes (n=234)	30.3	[26.3,34.6]
No (n=511)	69.6	[65.2,73.6]
Don't know (n=2)	0.1	[0.0,0.6]

More than one-third (37.7%) of survey respondents reported smoking or using tobacco in the past thirty days. Among those who smoked or used tobacco in the past thirty days, 75.2% reported wanting to quit. Of those who said they would like to quit smoking or using tobacco, 90.7% reported working on cutting back or quitting right now. Among those currently working on quitting or reducing tobacco use, over half (54%) of respondents reported receiving advice or assistance from a health professional or health plan on how to quit in the past 12 months.

	%	95% CI
In the last 30 days, have you smoked or used tobacco? (n=4,089)		
Yes (n=1,533)	37.7	[35.9,39.7]
No (n=2,556)	62.3	[60.3,64.1]
[If Yes] Do you want to quit smoking or using tobacco? (n=1,530)		
Yes (n=1,186)	75.2	[72.0,78.1]
No (n=319)	23.3	[20.4,26.4]
Don't know (n=25)	1.5	[0.9,2.5]
[If Yes] Are you working on cutting back or quitting right now? (n=1,186)		
Yes (n=1,059)	90.7	[88.7,92.4]
No (n=124)	9.1	[7.4,11.1]
Don't know (n=3)	0.2	[0.1,0.8]
In the past 12 months, did you receive any advice or assistance from a health professional or your health plan on how to quit smoking? (n=1,531)		
Yes (n=877)	54.0	[50.8,57.3]
No (n=644)	45.4	[42.2,48.7]
Don't know (n=10)	0.5	[0.3,1.1]

Few (5.9%) survey respondents reported using drugs or medications in the past 30 days to affect mood or aid in relaxation. Among those who reported using drugs or medications for mood or to aid in relaxation, 52.9% used these drugs or medications almost every day. More than one-third (37.1%) of respondents who used these drugs sometimes or every day reported speaking with a health professional about the use of these drugs or medications.

	%	95% CI
In the last 30 days, have you used drugs or medications to affect your mood or help you relax? This includes prescription drugs taken differently than how you were told to take them, as well as street drugs. (n=4,086)		
Yes (n=222)	5.9	[5.1,7.0]
No (n=3,862)	94.0	[92.9,94.9]
Don't know (n=2)	0.1	[0.0,0.3]

[If Yes] How often? Would you say Almost every day, Sometimes, Rarely, or Never? (n=222)		
Almost every day (n=115)	52.9	[44.4,61.2]
Sometimes (n=64)	28.6	[21.6,36.9]
Rarely (n=41)	17.6	[12.0,25.0]
Never (n=2)	0.9	[0.2,3.8]
[If 'Sometimes' or 'Almost every day'] In the last 12 months, has a doctor, nurse, or other health professional talked with you about your use of these drugs or medications? (n=179)		
Yes (n=77)	37.1	[29.2,45.7]
No (n=102)	62.9	[54.3,70.8]

Regular Source of Care and Primary Care Utilization Prior to HMP

In the 12 months prior to HMP enrollment, about three-quarters (73.8%) of survey respondents reported having a place they would usually go for a checkup, when they felt sick, or when they wanted advice about their health and 24% of survey respondents reported not having a regular source of care. Among respondents who reported having a place that they would go for health care in the 12 months prior to HMP enrollment, a doctor's office (47.9%) was the most common place reported, while 16.2% reported the emergency room as their usual place for care. Many (40.1%) survey respondents had not had a primary care visit in the year before HMP enrollment and more than one-fifth (20.6%) had not had a primary care visit in five years or more.

	%	95% CI
In the 12 months before enrolling in the Healthy Michigan Plan, was there a place that you usually would go to for a checkup, when you felt sick, or when you wanted advice about your health? (n=4,084)		
Yes (n=3,051)	73.8	[72.0,75.5]
No (n=955)	24.0	[22.4,25.8]
NA (n=73)	2.1	[1.5,2.8]
Don't know (n=5)	0.1	[0.1,0.4]
[If Yes] What kind of place was it? (n=3,051)		
Doctor's office (n=1,498)	47.9	[45.7,50.2]
Clinic (n=557)	17.2	[15.5,18.9]
Urgent care/walk-in (n=529)	16.8	[15.2,18.6]
Emergency room (n=409)	16.2	[14.6,18.1]
Other place (n=56)	1.8	[1.3,2.4]
Don't know (n=2)	0.1	[0.0,0.2]
Before you enrolled in the Healthy Michigan Plan, about how long had it been since you had a primary care visit? (n=4,086)		
Less than 1 year before HMP (n=1,647)	40.1	[38.2,42.1]
1 to 5 years (n=1,577)	37.8	[35.9,39.7]
More that 5 years (n=813)	20.6	[19.0,22.2]
Don't know (n=49)	1.5	[1.0,2.1]

Regular Source of Care and Primary Care Utilization with HMP

Most (92.2%) survey respondents indicated that in the past 12 months of HMP enrollment there is a place they usually go when they need a checkup, feel sick, or want advice about their health. A doctor's office (75.2%) was the most common place respondents went to for health care in the 12 months enrolled in HMP and just 1.7% reported the emergency room. Among those who usually go to a doctor's office or clinic for health care, 60.6% reported that this is not the same place they went prior to HMP enrollment. Among respondents who reported going to a doctor's office or clinic for their health care, most (96.7%) respondents said this was their primary care provider (PCP) through their HMP coverage. Among the respondents who chose urgent care or the emergency room as their usual place for care while enrolled in HMP, 32.4% said they did not have a PCP through HMP. Among those respondents who used urgent care or the emergency room as their usual place of care and who had a PCP through HMP, about half (49.1%) chose their provider and about half (49.4%) said their plan assigned one.

	%	95% CI
In the last 12 months, is there a place you usually go when you need a checkup, feel sick, or want advice about your health? (n=4,088)		
Yes (n=3,850)	92.2	[90.8,93.4]
No (n=194)	6.2	[5.2,7.4]
NA (n=44)	1.6	[1.0,2.4]
[If Yes] What kind of a place was it? (n=3,850)		
Doctor's office (n=2,934)	75.2	[73.4,77.0]
Clinic (n=640)	16.5	[15.0,18.1]
Urgent care/walk-in (n=181)	5.8	[4.8,6.9]
Emergency room (n=65)	1.7	[1.3,2.2]
Other place (n=29)	0.8	[0.5,1.2]
Don't know (n=1)	0.0	[0.0,0.2]
[If Doctor's Office or Clinic] Is this the same place where you went before you enrolled in Healthy Michigan? (n=3,551)		
Yes (n=1,438)	39.3	[37.3,41.4]
No (n=2,111)	60.6	[58.5,62.6]
Don't know (n=2)	0.1	[0.0,0.3]
[If Doctor's Office or Clinic] And is this your primary care provider for your Healthy Michigan Plan Coverage? (n=3,552)		
Yes (n=3,438)	96.7	[95.8,97.4]
No (n=103)	3.1	[2.4,3.9]
Don't know (n=11)	0.2	[0.1,0.5]
[If the place they usually go for care is NOT their PCP --OR-- usual source of care is urgent care/walk-in clinic or the ER] Do you have a primary care provider through your Healthy Michigan Plan coverage? (n=652)		
Yes (n=418)	63.6	[58.7,68.3]
No (n=208)	32.4	[27.9,37.3]
Don't know (n=26)	3.9	[2.5,6.2]

[If Yes] Did you choose your primary care provider or did your plan assign you to one? (n=216)		
Chose my PCP (n=103)	49.1	[40.3,58.0]
Plan assigned my PCP (n=109)	49.4	[40.5,58.3]
Don't know (n=4)	1.5	[0.5,4.5]

The majority (85.2%) of respondents who reported having a PCP indicated that they saw their PCP in the past 12 months. For survey respondents who reported not seeing their PCP in the previous 12 months while enrolled in HMP, the most common reason given was that they were healthy and did not need to see a provider. Most (91.1%) respondents who had seen their PCP reported talking about things they can do to be healthy and prevent medical problems. Among those who had seen their PCP, 83.9% said it was easy or very easy to get an appointment to see their PCP. For those who said it was difficult or very difficult to schedule an appointment, the most common reason for this difficulty was not getting an appointment soon enough.

	%	95% CI
Have you seen your primary care provider in the past 12 months? (n=3,851)		
Yes (n=3,386)	85.2	[83.5,86.7]
No (n=453)	14.5	[13.0,16.2]
Don't know (n=12)	0.3	[0.2,0.6]
[If Yes] Did you and the primary care provider talk about things you can do to be healthy and prevent medical problems? (n=3,386)		
Yes (n=3,131)	91.1	[89.6,92.3]
No (n=243)	8.5	[7.3,9.9]
Don't know (n=12)	0.4	[0.2,0.9]
In the last 12 months, how easy or difficult was it to get an appointment to see your primary care provider? (n=3,386)		
Very easy (n=1,432)	41.9	[39.8,44.0]
Easy (n=1,443)	42.0	[39.9,44.1]
Neutral (n=274)	8.9	[7.7,10.3]
Difficult (n=166)	4.8	[4.0,5.8]
Very Difficult (n=69)	2.3	[1.7,3.1]
Don't know (n=2)	0.1	[0.0,0.4]
[If Difficult or Very Difficult] What made it difficult? (n=235)		
Couldn't get an appointment soon enough (n=195)	84.0	[77.8,88.8]
Inconvenient hours (n=46)	18.5	[13.3,25.2]
Couldn't get through on the telephone (n=21)	7.7	[4.6,12.7]
Transportation (n=12)	3.7	[1.9,6.9]
Other (n=15)	9.0	[4.8,16.4]

[If No - Have not seen PCP in past 12 months] Why not?* (n=452)		
Healthy/didn't need to see doctor (n=274)	63.4	[57.6,68.8]
Couldn't get appointment (n=37)	7.0	[4.8,10.0]
Transportation difficulties/too far (n=23)	5.5	[3.3,9.1]
See a specialist instead (n=19)	4.2	[2.2,7.6]
Don't like my PCP/staff (n=18)	3.9	[2.3,6.5]
Inconvenient hours (n=10)	3.0	[1.3,6.8]
Don't like doctors in general (n=8)	1.5	[0.6,3.4]
Other (n=149)	30.6	[25.6,36.3]
Don't know (n=3)	0.5	[0.1,1.5]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Primary Care Utilization and Experience

Beneficiaries who were older, white, female, reported worse health, and had any chronic condition were more likely than other beneficiaries to have seen a PCP in the past 12 months. Ethnicity, employment, income and marital status were not associated with likelihood of PCP visit in past 12 months (See Appendix Table 2).

Respondents who reported a PCP visit within the previous 12 months, compared to those who did not, were more likely to report improvement in access to specialty care, help with staying healthy, and cancer screening. Respondents who reported a PCP visit within the previous 12 months, compared to those who did not, were more likely to report completing an HRA, being counseled about exercise, nutrition, tobacco cessation (for those who used tobacco) and being counseled about safe alcohol use (for those who reported unsafe alcohol intake). Respondents who reported a PCP visit within the previous 12 months, compared to those who did not, were more likely to report being diagnosed with a chronic condition since enrollment in HMP (See Appendix Table 3).

Foregone Care Prior to HMP

One-third (33%) of respondents reported not getting the health care they needed in the 12 months prior to HMP enrollment. The most common reasons for not getting the care they needed prior to HMP were being worried about the cost (77.5%) and not having health insurance (67.4%).

	%	95% CI
In the 12 months before enrolling in the Healthy Michigan Plan, was there any time when you didn't get the health care services you needed? (n=4,084)		
Yes (n=1,409)	33.0	[31.2,34.8]
No (n=2,638)	65.9	[64.0,67.7]
Don't know (n=37)	1.1	[0.8,1.7]

[If Yes] Why didn't you get the care you needed?* (n=1,409)		
You were worried about the cost (n=1,121)	77.5	[74.5,80.2]
You did not have health insurance (n=927)	67.4	[64.2,70.4]
Your health plan wouldn’t pay for the treatment (n=105)	7.9	[6.3,9.8]
The doctor or hospital wouldn’t accept your health insurance (n=60)	4.0	[3.0,5.4]
You couldn’t get an appointment soon enough (n=54)	3.5	[2.6,4.8]
You didn’t have transportation (n=36)	2.7	[1.9,4.0]
Other (n=99)	7.3	[5.7,9.4]
Don't know (n=6)	0.5	[0.2,2.0]
Other (write-in): Respondent did not have a doctor (n=24)	1.2	[0.8,1.9]
Other (write-in): Respondent was not satisfied with the care they received (n=19)	1.1	[0.6,1.9]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Foregone Care with HMP

Over one-fifth (22%) of survey respondents reported that there was a time when they needed help or advice when their usual clinic or doctor’s office was closed. Among these respondents, 46.8% said they tried to contact their provider’s office after they were closed to get help or advice. Among those who tried to contact their provider’s office after it was closed, 56.5% said they were able to talk to someone. Among respondents who did not contact their provider’s office when they needed help or advice, the main reason for not contacting them was because the office was closed.

	%	95% CI
In the last 12 months was there a time when you needed help or advice when your usual clinic or doctor's office was closed? (n=4,063)		
Yes (n=916)	22.0	[20.4,23.6]
No (n=3,132)	77.6	[76.0,79.1]
Don't know (n=15)	0.4	[0.2,0.9]
[If Yes] In the most recent case, did you try to contact your provider's office after they were closed to get help or advice? (n=916)		
Yes (n=429)	46.8	[42.8,50.7]
No (n=484)	52.7	[48.7,56.7]
[If Yes] Were you able to talk to someone? (n=428)		
Yes (n=243)	56.5	[50.6,62.2]
No (n=184)	43.0	[37.3,48.9]
Don't know (n=1)	0.5	[0.1,3.2]

[If No-Did not try to contact provider's office] Why didn't you try to contact your provider's office?* (n=488)		
It was closed (n=347)	69.5	[64.2,74.3]
I felt it was an emergency and went to ER/ called 911 (n=78)	15.6	[12.1,19.9]
Decided to wait to see if condition resolved (n=31)	6.5	[4.3,9.8]
Unsure how to contact provider (n=3)	1.2	[0.3,4.5]
Other (n=99)	21.8	[17.5,26.9]
Don't know (n=9)	1.8	[0.8,3.6]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Among all survey respondents, 15.6% said that in the past 12 months there was a time when they did not get the medical or dental care they needed. The most common reasons for not getting the care they needed with HMP were because their health plan would not pay for the treatment (39.6%) and being worried about the cost (25.4%). Those who cited a reason other than the options supplied for not getting the medical or dental care they needed often reported that dental procedures such as crowns and root canals are not covered and indicated that it was difficult to find a dentist who accepted their insurance. Among respondents who did not get needed care because they could not afford it, 63.2% reported dental care as the type of care they wanted.

	%	95% CI
In the last 12 months, was there any time when you didn't get the medical or dental care you needed? (n=4,084)		
Yes (n=629)	15.6	[14.3,17.1]
No (n=3,433)	84.0	[82.5,85.3]
Don't know (n=22)	0.4	[0.2,0.6]
[If Yes] Why didn't you get the care you needed?* (n=629)		
Your health plan wouldn't pay for the treatment (n=251)	39.6	[34.9,44.5]
You were worried about the cost (n=155)	25.4	[21.3,29.9]
The doctor or hospital wouldn't accept your health insurance (n=141)	23.9	[19.8,28.5]
You couldn't get an appointment soon enough (n=73)	11.5	[8.7,14.9]
You did not have health insurance (n=41)	8.5	[5.8,12.4]
You didn't have transportation (n=30)	6.1	[3.9,9.4]
Other (n=199)	29.8	[25.6,34.4]

[If Yes - 'Your health plan wouldn't pay for the treatment', 'You were worried about the cost', 'The doctor or hospital wouldn't accept your health insurance', OR 'You did not have health insurance'] Was there any time in the last 12 months when you needed or wanted any of the following but could not afford it?* (n=393)		
Dental care (including check-ups) (n=252)	63.2	[57.0,69.0]
To see a specialist (n=79)	21.7	[16.8,27.5]
Prescription medication [not over the counter] (n=72)	19.9	[15.3,25.5]
A checkup, physical or wellness visit (n=47)	13.3	[9.6,18.2]
Mental health care or counseling (n=30)	8.9	[5.8,13.3]
Substance use treatment services (n=2)	0.7	[0.2,2.6]
Other (n=49)	13.0	[9.2,17.9]
NONE (n=28)	5.6	[3.8,8.3]
Don't know (n=1)	0.2	[0.0,1.7]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Changes in Access to Care

Many respondents reported greater ability to get prescription medications (59.3%), primary care (57.8%), help staying healthy or preventing health problems (52%), dental care (46.1%), specialist care (44.4%), mental health care (27.5%), and cancer screening (25.7%) after enrolling in HMP compared to before they had HMP coverage. About half (46.7%) of respondents did not know if their ability to get mental health care through HMP was better, worse, or about the same as compared to before enrolling in HMP, though only 2.5% reported that it was worse. The majority (80.7%) of respondents did not know if their ability to get substance use treatment services through HMP was better, worse, or about the same compared to before enrolling in HMP though only 0.2% reported that it was worse. While most (58.6%) respondents did not know if their ability to get cancer screening though HMP was better, worse, or about the same compared to before HMP, 25.7% said it was better. The majority (71%) of respondents also said they did not know if their ability to get birth control/family planning services through HMP is better, worse, or the about the same compared to before HMP.

	%	95% CI
Would you say that your ability to get primary care through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,085)		
Better (n=2,381)	57.8	[55.8,59.7]
Worse (n=93)	2.4	[1.9,3.1]
About the same (n=1,483)	35.9	[34.0,37.8]
Don't know (n=128)	3.9	[3.1,4.9]

Would you say that your ability to get specialist care through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,085)		
Better (n=1,901)	44.4	[42.5,46.4]
Worse (n=177)	4.2	[3.5,5.1]
About the same (n=911)	22.6	[21.0,24.3]
Don't know (n=1,096)	28.7	[26.9,30.6]
Would you say that your ability to get dental care through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,084)		
Better (n=1,930)	46.1	[44.1,48.0]
Worse (n=255)	6.2	[5.4,7.3]
About the same (n=1,138)	29.3	[27.5,31.2]
Don't know (n=761)	18.4	[16.9,19.9]
Would you say that your ability to get mental health care through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,084)		
Better (n=1,077)	27.5	[25.8,29.3]
Worse (n=97)	2.5	[1.9,3.2]
About the same (n=923)	23.3	[21.6,25.0]
Don't know (n=1,987)	46.7	[44.8,48.7]
Would you say that your ability to get substance use treatment services through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,083)		
Better (n=341)	9.8	[8.6,11.1]
Worse (n=9)	0.2	[0.1,0.4]
About the same (n=319)	9.3	[8.1,10.6]
Don't know (n=3,414)	80.7	[79.0,82.3]
Would you say that your ability to get prescription medications through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,085)		
Better (n=2,497)	59.3	[57.4,61.3]
Worse (n=121)	3.1	[2.5,3.9]
About the same (n=1,017)	25.9	[24.2,27.7]
Don't know (n=450)	11.6	[10.4,13.0]
Would you say that your ability to get cancer screening through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,084)		
Better (n=1,156)	25.7	[24.1,27.5]
Worse (n=26)	0.6	[0.4,1.0]
About the same (n=627)	15.0	[13.7,16.5]
Don't know (n=2,275)	58.6	[56.7,60.5]

Would you say that your ability to get help with staying healthy or preventing health problems through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,084)		
Better (n=2,142)	52.0	[50.0,53.9]
Worse (n=48)	1.1	[0.8,1.5]
About the same (n=1,338)	32.5	[30.7,34.3]
Don't know (n=556)	14.5	[13.2,16.0]
Would you say that your ability to get birth control/family planning services through the Healthy Michigan Plan is better, worse, or about the same, compared to before? (n=4,082)		
Better (n=568)	16.1	[14.6,17.7]
Worse (n=16)	0.5	[0.3,0.8]
About the same (n=472)	12.4	[11.1,13.8]
Don't know (n=3,026)	71.0	[69.1,72.8]

Emergency Room Use with HMP

Over one-third (37.6%) of survey respondents reported going to a hospital emergency room (ER) for care in the past 12 months. Of those who went to the ER in the past 12 months, 83.8% felt that the problem needed to be handled in the ER. Over one-quarter (28.0%) of respondents with an ER visit in the past 12 months said they tried to contact their usual provider's office to get help or advice before going to the ER. Among those who tried to contact their provider, 76.6% reported talking to someone. Among those who talked to someone from their provider's office before going to the ER, the most common reason for going to the ER was because the provider said to go (75.7%).

	%	95% CI
During the past 12 months, did you go to a hospital emergency room about your own health (whether or not you were admitted overnight)? (n=4,090)		
Yes (n=1,456)	37.6	[35.7,39.6]
No (n=2,611)	61.8	[59.8,63.7]
Don't know (n=23)	0.6	[0.3,1.0]
[If Yes] Thinking about the last time you were at the emergency room, did you think your problem needed to be handled in the emergency room? (n=1,455)		
Yes (n=1,249)	83.8	[81.1,86.2]
No (n=186)	14.9	[12.6,17.6]
Don't know (n=20)	1.2	[0.8,2.0]
Thinking about the last time you were at the emergency room, did you try to contact your usual provider's office to get help or advice before going to the emergency room? (n=1,456)		
Yes (n=424)	28.0	[25.2,30.9]
No (n=1,025)	71.7	[68.7,74.5]
Don't know (n=7)	0.3	[0.1,0.8]

[If Yes] Did you talk to someone? (n=424)		
Yes (n=319)	76.6	[71.3,81.2]
No (n=105)	23.4	[18.8,28.7]
[If Yes] Why did you end up going to the ER?* (n=319)		
Provider said to go to the ER (n=250)	75.7	[68.9,81.5]
Symptoms didn't improve or got worse (n=36)	14.3	[9.6,20.9]
You could get an appointment soon enough (n=33)	8.0	[5.4,11.8]
Provider advice wasn't helpful (n=12)	3.0	[1.6,5.5]
No response from the provider (n=5)	2.1	[0.7,6.2]
Other (n=51)	16.5	[11.9,22.5]
Don't know (n=2)	0.3	[0.1,1.2]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Among respondents who did not try to contact their provider before going to the ER: 20% arrived to the ER by ambulance, 74.8% went to the ER because it was the closest place to receive care, 18.5% went because they get most of their care at the ER, 64.3% felt the problem was too serious for a doctor's office or clinic, 63.6% reported their usual clinic was closed, and 25.4% said they needed to get care at a time that would not make them to miss school or work.

	%	95% CI
[If No - Did not try to contact usual provider's office before going to the ER] Which of these were true of this particular ER visit? (n=978)		
You arrived by ambulance or other emergency vehicle		
Yes (n=191)	20.0	[17.0,23.3]
No (n=787)	80.0	[76.7,83.0]
You went to the ER because it´s your closest place to receive care		
Yes (n=724)	74.8	[71.4,78.0]
No (n=245)	24.3	[21.2,27.7]
You went to the ER because you get most of your care at the emergency room		
Yes (n=156)	18.5	[15.5,22.0]
No (n=818)	80.8	[77.4,83.9]
Don't know (n=4)	0.6	[0.2,1.8]
The problem was too serious for a doctor's office or clinic		
Yes (n=657)	64.3	[60.3,68.1]
No (n=294)	32.9	[29.2,36.8]
Don't know (n=27)	2.8	[1.6,4.9]
Your doctor´s office or clinic was not open		
Yes (n=628)	63.6	[59.8,67.3]
No (n=297)	30.8	[27.3,34.5]
Don't know (n=52)	5.6	[3.9,7.8]

You needed to get care at a time that would not make you miss work or school		
Yes (n=240)	25.4	[22.1,29.1]
No (n=721)	72.7	[68.9,76.1]
Don't know (n=17)	1.9	[1.1,3.4]

About two-thirds (64.0%) of all respondents said they are more likely to contact their usual provider before going to the ER compared to before HMP.

	%	95% CI
In general, compared to before you had the Healthy Michigan Plan, are you more likely, less likely, or about as likely to contact your usual doctor's office before going to the emergency room? (n=4,081)		
More likely (n=2,722)	64.0	[62.1,65.9]
Less likely (n=289)	8.3	[7.2,9.6]
About as likely (n=910)	23.5	[21.8,25.2]
Don't know (n=160)	4.2	[3.4,5.0]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Impact of HMP on Acute Care Seeking

Respondents who used the ER were more likely than those who did not use the ER to report their health as fair/poor (40.1% vs. 23.2%) and less likely to report excellent/very good health (59.9% vs. 76.8%) (See Appendix Table 4). Respondents who used the ER reported chronic physical or mental health conditions more often than those who did not use the ER (79.4% vs. 62.8%) (See Appendix Table 5).

Impact of HMP on Employment, Education and Ability to Work

While most (78.3%) respondents who were students indicated that the number of days they missed school in the past year was about the same compared to the 12 months before HMP enrollment, 16.5% reported that they missed fewer days in the past year compared to the 12 months before. Among employed or self-employed respondents, 69.4% felt that getting health coverage through HMP helped them do a better job at work. Among respondents who were employed or self-employed, 27.6% reported changing jobs in the past 12 months. Among those who changed jobs in the past 12 months, 36.9% felt that having health coverage through HMP helped them get a better job. For those out of work for less than or more than a year, 54.5% of respondents felt that having HMP made them better able to look for a job.

	Mean or %	95% CI
[If a student] In the past 12 months, about how many days did you miss school because of illness or injury (do not include maternity leave)? (n=159)	Mean 2.9	[1.5,4.3]
Compared to the 12 months before this time, was this more, less, or about the same? (n=160)		
More (n=8)	4.4	[2.0,9.7]
Less (n=27)	16.5	[10.2,25.5]
About the same (n=124)	78.3	[69.1,85.4]
Don't know (n=1)	0.8	[0.1,5.3]
[If employed/self-employed or out of work for less than a year] In the past 12 months, about how many days did you miss work at a job or business because of illness or injury (do not include maternity leave)? (n=2,309)	Mean 7.5	[6.1,9.0]
Compared to the 12 months before this time, was this more, less, or about the same? (n=2,331)		
More (n=299)	12.7	[11.1,14.4]
Less (n=384)	16.6	[14.7,18.6]
About the same (n=1,611)	68.7	[66.2,71.0]
Don't know (n=37)	2.1	[1.3,3.2]
[If employed or self-employed] Has getting health insurance through the Healthy Michigan Plan helped you do a better job at work? (n=2,077)		
Yes (n=1,431)	69.4	[66.8,71.8]
No (n=549)	25.9	[23.6,28.4]
Don't know (n=97)	4.7	[3.7,6.0]
Have you changed jobs in the last 12 months? (n=1,979)		
Yes (n=447)	27.6	[24.9,30.4]
No (n=1,531)	72.3	[69.5,75.0]
Don't know (n=1)	0.1	[0.0,0.6]
[If Yes] Having health insurance through the Healthy Michigan Plan helped me get a better job. (n=447)		
Strongly agree (n=33)	7.7	[5.0,11.6]
Agree (n=123)	29.2	[23.6,35.4]
Neutral (n=103)	21.5	[17.1,26.7]
Disagree (n=150)	33.5	[27.8,39.6]
Strongly disagree (n=30)	6.4	[4.2,9.6]
Don't know (n=8)	1.8	[0.8,4.0]

[If out of work for less than or more than a year] Having healthy insurance through the Healthy Michigan Plan has made me better able to look for a job. (n=957)		
Strongly agree (n=158)	16.2	[13.5,19.3]
Agree (n=389)	38.3	[34.6,42.2]
Neutral (n=185)	19.3	[16.1,22.9]
Disagree (n=143)	17.2	[14.0,20.8]
Strongly disagree (n=35)	3.5	[2.4,5.2]
Don't know (n=47)	5.5	[3.9,7.7]
[If homemaker, retired, or unable to work] In the past 12 months, about how many days were you unable to do your activities because of illness or injury? (n=809)	Mean 135.4	[122.2,148.6]
Compared to the 12 months before this time, was this more, less, or about the same? (n=859)		
More (n=151)	18.6	[15.4,22.2]
Less (n=131)	16.8	[13.7,20.6]
About the same (n=551)	61.2	[56.8,65.3]
Don't know (n=26)	3.4	[2.1,5.5]

Compared to employed enrollees, enrollees who were out of work or unable to work were more likely to be older (27.5% of out of work enrollees and 42.1% unable to work enrollees vs. 20.0% of employed enrollees were aged 51-64), male (57.2% of out of work enrollees and 53.9% of unable to work enrollees vs. 45.5% of employed enrolles were male), lower income (79.1% of out of work enrollees and 73.8% of unable to work enrollees vs. 33.7% of employed enrollees had incomes that were 0-35% FPL), veterans (3.9% of out of work enrollees and 5.9% of unable to work enrollees vs. 2.3% of employed enrollees), in fair/poor health (33.7% of out of work enrollees and 73.4% of unable to work enrollees vs. 19.6% of employed enrollees), and with chronic physical or mental health conditions (65.1% of out of work enrollees and 87.5% of unable to work enrollees vs. 53.8% of employed enrollees had physical health conditions; 35.3% of out of work enrollees and 61.7% of unable to work enrollees vs. 25.2% of employed enrollees had mental health conditions) or limitations (24.4% of out of work enrollees and 68.8% of unable to work enrollees vs. 13.3% of employed enrollees had physical impariments; 25.0% of out of work enrollees and 48.4% of unable to work enrollees vs. 11.6% of employed enrollees had mental impairments) (See Appendix Table 9).

HMP enrollees were more likely to be employed if their health status was excellent, very good, or good vs. fair or poor (56.1% vs. 32.3%) or if they had no chronic conditions (59.8% vs. 44.1%) (See Appendix Tables 11 and 12). Employed respondents missed a mean of 7.2 work days in the past year due to illness. 68.4% said this was about the same as before HMP, 17.2% said less and 12.3% said more (See Appendix Table 13).

Enrollees were 1.7 times more likely to report being out of work if aged 51-64, 1.8 times as likely if male, 1.9 times as likely if African-American, 1.5 times as likely if in fair/poor health, 1.5 times as likely if with mental health conditions, or functional limitations (1.4 times as likely if with physical limitation; 2.0 times as likely if with mental limitation). Enrollees were more likely to report being unable to work if older (2.3 times more likely for 35-50-year-olds, 4.2 times more likely for 51-64-year-olds), 1.9 times as likely if male, 3.5 times as likely if in fair/poor health, 1.7 times as likely if with with chronic physical health conditions, 2.6 times as likely if with chronic mental health condition, or functional limitations (5.1 times as likely if they reported a physical limitation; 2.3 times as likely if they reported a mental limitation) (See Appendix Table 14).

Employed enrollees with improved physical or mental health since HMP enrollment were 4.1 times more likely to report that HMP helped them to do a better job at work (See Appendix Table 15). Enrollees who were out of work with improved physical or mental health since HMP enrollment were 2.8 times more likely to report that HMP made them better able to look for a job. Enrollees who had a recent job change and improved physical or mental health since HMP enrollment were 3.2 times more likely to report that HMP helped them get a better job (See Appendix Table 16).

Impact of HMP on Access to Dental Care and Oral Health

Better access to dental care since HMP was reported by 46.1% of respondents, with students and younger respondents less likely to report better access (See Appendix Table 18). Improved oral health of their teeth and gums was reported by 39.5% of respondents, with students and younger respondents most likely to report no change in their oral health (See Appendix Table 20).

Survey respondents who were aware of their HMP dental coverage were significantly more likely to report improved access to dental care and improved oral health since HMP compared to those who were unaware (See Appendix Table 21). Among survey respondents who reported foregoing needed medical or dental care due to cost since HMP, 63.2% reported foregoing dental care. Foregone care varied by both employment status and region (See Appendix Table 19).

Among those who reported better access to dental care, 51.2% strongly agreed or agreed that HMP helped them to get a better job, 61.5% strongly agreed or agreed that HMP helped them to look for a job; and 77.8% reported doing a better job at work; all of these were significantly greater than responses for those who reported no change or worse access to dental care. Among those who reported better access to dental care, 67.9% reported improved oral health, significantly greater than those who reported no change or worse access to dental care. There was no significant impact of better access to dental care with HMP on ER use in the past year (See Appendix Table 22).

Perspectives on HMP Coverage

The majority of survey respondents agreed that it is very important for them personally to have health insurance (97.4%), that they do not worry as much about something bad happening to their health since HMP enrollment (69%), that having HMP has taken a lot of stress off of them (87.9%), that without HMP they would not be able to go to the doctor (83.3%), and that having HMP has helped them live a better life (89.2%).

	%	95% CI
It is very important for me personally to have health insurance. (n=4,084)		
Strongly agree (n=1,892)	44.6	[42.6,46.5]
Agree (n=2,101)	52.8	[50.8,54.8]
Neutral (n=43)	1.3	[0.9,2.0]
Disagree (n=43)	1.2	[0.8,1.8]
Strongly disagree (n=4)	0.1	[0.0,0.3]
Don't know (n=1)	0.0	[0.0,0.1]
I don't worry as much about something bad happening to my health since enrolling in the Healthy Michigan Plan. (n=4,081)		
Strongly agree (n=700)	17.0	[15.6,18.5]
Agree (n=2,142)	52.0	[50.0,54.0]
Neutral (n=352)	8.8	[7.8,9.9]
Disagree (n=764)	18.8	[17.3,20.3]
Strongly disagree (n=78)	2.2	[1.6,2.8]
Don't know (n=45)	1.3	[0.9,1.9]
Having the Healthy Michigan Plan has taken a lot of stress off me. (n=4,087)		
Strongly agree (n=1,147)	26.0	[24.4,27.7]
Agree (n=2,495)	61.9	[60.0,63.7]
Neutral (n=220)	6.5	[5.5,7.6]
Disagree (n=195)	4.7	[4.0,5.6]
Strongly disagree (n=15)	0.4	[0.2,0.7]
Don't know (n=15)	0.5	[0.3,0.9]
Without the Healthy Michigan Plan, I wouldn't be able to go to the doctor. (n=4,085)		
Strongly agree (n=1,212)	28.2	[26.5,29.9]
Agree (n=2,211)	55.1	[53.2,57.1]
Neutral (n=166)	4.1	[3.4,5.0]
Disagree (n=450)	11.2	[10.0,12.5]
Strongly disagree (n=31)	1.0	[0.7,1.5]
Don't know (n=15)	0.4	[0.2,0.7]
Having the Healthy Michigan Plan has helped me live a better life. (n=4,083)		
Strongly agree (n=1,067)	25.0	[23.4,26.8]
Agree (n=2,609)	64.2	[62.3,66.1]
Neutral (n=255)	6.9	[6.0,8.0]
Disagree (n=119)	3.0	[2.4,3.7]
Strongly disagree (n=13)	0.3	[0.2,0.5]
Don't know (n=20)	0.6	[0.3,1.1]

Knowledge and Understanding of HMP Coverage

There were some gaps in knowledge among survey respondents about the health care services covered by HMP. The majority of respondents knew that HMP covers routine dental visits (77.2%), eyeglasses (60.4%), and counseling for mental or emotional problems (56%). Only one-fifth (21.2%) were aware that HMP covers name brand as well as generic medications.

	%	95% CI
My Healthy Michigan Plan covers routine dental visits. (n=4,086)		
Yes (n=3,170)	77.2	[75.4,78.8]
No (n=175)	3.9	[3.3,4.7]
Don't know (n=741)	18.9	[17.3,20.6]
My Healthy Michigan Plan covers eyeglasses. (n=4,086)		
Yes (n=2,590)	60.4	[58.5,62.4]
No (n=314)	7.8	[6.8,9.0]
Don't know (n=1,182)	31.8	[29.9,33.7]
My Healthy Michigan Plan covers counseling for mental or emotional problems. (n=4,086)		
Yes (n=2,318)	56.0	[54.0,57.9]
No (n=104)	3.1	[2.4,3.9]
Don't know (n=1,664)	40.9	[39.0,42.9]
Only generic medicines are covered by my Healthy Michigan Plan. (n=4,085)		
Yes (n=1,451)	35.8	[33.9,37.7]
No (n=892)	21.2	[19.7,22.9]
Don't know (n=1,742)	43.0	[41.0,44.9]

The majority (83.2%) of respondents reported rarely or never needing help reading instructions, pamphlets, or other written material from a doctor, pharmacy or health plan.

	%	95% CI
How often do you need to have someone help you read instructions, pamphlets, or other written materials from a doctor, pharmacy, or health plan? (n=4,088)		
Never (n=3,031)	72.6	[70.8,74.3]
Rarely (n=413)	10.6	[9.5,12.0]
Sometimes (n=390)	10.6	[9.4,11.9]
Often (n=94)	2.4	[1.8,3.1]
Always (n=157)	3.7	[3.1,4.5]
Don't know (n=3)	0.0	[0.0,0.1]

Challenges Using HMP Coverage

Few (15.5%) survey respondents reported that they had questions or problems using their HMP coverage. Among those who had questions or problems, about half (47.7%) reported getting help or advice. The most commonly reported sources of help were from a health plan hotline, someone at the doctor's office, and an option outside of the provided responses. Among those who reported an option other than the ones provided, common responses were getting help from a case worker or someone at the pharmacy. Most (74.2%) of those who reported receiving help said that they got an answer or solution to their question.

	%	95% CI
Have you had any questions or problems using your Healthy Michigan Plan insurance? (n=4,089)		
Yes (n=632)	15.5	[14.2,17.0]
No (n=3,449)	84.3	[82.8,85.7]
Don't know (n=8)	0.2	[0.1,0.3]
[If Yes] Did anyone give you help or advice? (n=632)		
Yes (n=324)	47.7	[42.8,52.5]
No (n=302)	51.2	[46.4,56.1]
Don't know (n=6)	1.1	[0.4,3.2]
[If Yes] Who helped you?* (n=324)		
Health Plan Hotline (n=100)	32.2	[26.3,38.8]
Someone at my doctor's office (n=83)	22.4	[17.6,28.2]
HMP Beneficiary Hotline (n=46)	14.7	[10.6,20.0]
Helpline (n=39)	13.9	[9.4,20.1]
Friend/Relative (n=9)	2.8	[1.4,5.5]
Community health worker (n=6)	1.4	[0.5,3.6]
Other (n=96)	29.8	[24.2,36.1]
Don't know (n=5)	2.1	[0.8,5.9]
Did you get an answer or solution to your question(s)? (n=324)		
Yes (n=238)	74.2	[68.0,79.5]
No (n=83)	24.7	[19.4,30.8]
Don't know (n=3)	1.1	[0.4,3.5]

**Respondents were able to provide more than one response for this question; As a result, percentages may exceed 100%.*

Out-of-Pocket Healthcare Spending Prior to HMP

In the 12 months prior to HMP enrollment, almost one-quarter (23.3%) of respondents spent more than $500 out of pocket for their own medical and dental care. In the 12 months prior to HMP enrollment, 44.7% of respondents reported having problems paying medical bills. Of those who reported having problems paying their medical bills, 67.1% reported being contacted by a collections agency and 30.7% thought about filing for bankruptcy. Among those who thought about it, 21.4% filed for bankruptcy.

	%	95% CI
During the 12 months BEFORE you were enrolled in HMP, about how much did you spend out-of-pocket for your own medical and dental care? (n=4,082)		
Less than $50 (n=1,696)	42.4	[40.4,44.3]
$51-100 (n=376)	8.9	[7.9,10.1]
$101-500 (n=954)	22.8	[21.2,24.6]
$501-2,000 (n=605)	14.3	[13.0,15.7]
$2,001-3,000 (n=153)	4.0	[3.3,5.0]
$3,001-5,000 (n=119)	2.7	[2.2,3.4]
More than $5,000 (n=91)	2.3	[1.8,3.0]
Don’t know (n=88)	2.5	[1.9,3.3]
In the 12 months before enrolling in the Healthy Michigan Plan, did you have problems paying medical bills? (n=4,085)		
Yes (n=1,869)	44.7	[42.7,46.6]
No (n=2,196)	54.9	[52.9,56.8]
Don't know (n=20)	0.4	[0.3,0.7]
[If Yes] Because of these problems paying medical bills, have you or your family been contacted by a collections agency? (n=1,869)		
Yes (n=1,235)	67.1	[64.4,69.8]
No (n=618)	31.8	[29.2,34.6]
Don't know (n=16)	1.0	[0.5,2.0]
Because of these problems paying medical bills, have you or your family thought about filing for bankruptcy? (n=1,869)		
Yes (n=559)	30.7	[28.1,33.5]
No (n=1,304)	68.9	[66.2,71.6]
Don't know (n=6)	0.3	[0.1,0.8]
[If Yes] Did you file for bankruptcy? (n=559)		
Yes (n=128)	21.4	[17.6,25.9]
No (n=429)	77.7	[73.1,81.8]
Don't know (n=2)	0.8	[0.2,4.4]

Out-of-Pocket Healthcare Spending with HMP

In the past 12 months, the majority (63.2%) of respondents reported spending less than $50 out-of-pocket for their own medical or dental care. Among survey respondents who previously had problems paying their medical bills (in the 12 months prior to HMP), most (85.9%) felt that their problems paying medical bills have gotten better since enrolling in HMP.

	%	95% CI
During the last 12 months, about how much did you spend out-of-pocket for your own medical and dental care? (n=4,076)		
Less than $50 (n=2,540)	63.2	[61.3,65.1]
$51-100 (n=503)	11.8	[10.6,13.1]
$101-500 (n=705)	17.2	[15.7,18.8]
$501-2,000 (n=210)	4.7	[4.0,5.6]
$2,001-3,000 (n=33)	0.8	[0.5,1.3]
$3,001-5,000 (n=15)	0.3	[0.1,0.6]
More than $5,000 (n=10)	0.3	[0.1,0.6]
Don’t know (n=60)	1.6	[1.2,2.3]
[If Yes - Had problems paying medical bills in the 12 months before HMP] Since enrolling in Healthy Michigan, have your problems paying medical bills gotten worse, stayed the same, or gotten better? (n=1,869)		
Gotten better (n=1,629)	85.9	[83.7,87.9]
Stayed the same (n=176)	10.6	[8.9,12.6]
Gotten worse (n=51)	2.6	[1.9,3.7]
Don't know (n=13)	0.9	[0.4,1.8]

Perspectives on Cost-Sharing

The majority (87.6%) of survey respondents agreed that the amount they have to pay for HMP coverage seems fair. Most (88.8%) respondents agreed that the amount they pay for HMP coverage is affordable. Almost three-quarters (72.1%) of respondents agreed that they would rather take some responsibility to pay something for their health care than not pay anything.

	%	95% CI
The amount I have to pay overall for the Healthy Michigan Plan seems fair. (n=4,082)		
Strongly agree (n=1,065)	24.8	[23.2,26.5]
Agree (n=2,568)	62.8	[60.9,64.7]
Neutral (n=145)	4.2	[3.4,5.2]
Disagree (n=153)	4.0	[3.3,4.8]
Strongly disagree (n=28)	0.8	[0.5,1.3]
Don't know (n=123)	3.4	[2.7,4.2]
The amount I pay for the Healthy Michigan Plan is affordable. (n=4,084)		
Strongly agree (n=1,073)	25.1	[23.4,26.8]
Agree (n=2,606)	63.7	[61.8,65.6]
Neutral (n=132)	3.9	[3.2,4.9]
Disagree (n=139)	3.5	[2.9,4.3]
Strongly disagree (n=28)	0.7	[0.4,1.2]
Don't know (n=106)	3.0	[2.4,3.8]

I'd rather take some responsibility to pay something for my health care than not pay anything. (n=4,073)		
Strongly agree (n=653)	14.8	[13.5,16.2]
Agree (n=2,396)	57.3	[55.3,59.2]
Neutral (n=326)	8.7	[7.6,10.0]
Disagree (n=541)	14.6	[13.2,16.0]
Strongly disagree (n=77)	2.1	[1.6,2.8]
Don't know (n=80)	2.5	[1.9,3.3]

Knowledge and Understanding of HMP Cost-Sharing Requirements

Only one-quarter (26.4%) of respondents were aware that contributions are charged monthly regardless of health care use. Approximately one-fifth (20.7%) of respondents were aware that there is a limit or maximum on the amount they might have to pay. Few (14.4%) respondents were aware that they could not be disenrolled from HMP for not paying their bill. Just over one-quarter (28.1%) of respondents were aware that they could get a reduction in the amount they have to pay if they complete a health risk assessment. The majority (75.6%) of respondents were aware that some kinds of visits, tests, and medicines have no copays.

	%	95% CI
Contributions are what I am charged every month for Healthy Michigan Plan coverage even if I do not use any health care. (n=4,081)		
Yes (n=1,149)	26.4	[24.7,28.1]
No (n=986)	23.4	[21.8,25.1]
Don't know (n=1,946)	50.2	[48.3,52.2]
There is no limit or maximum on the amount I might have to pay in copays or contributions. (n=4,083)		
Yes (n=856)	20.7	[19.2,22.3]
No (n=952)	23.0	[21.4,24.7]
Don't know (n=2,275)	56.3	[54.3,58.2]
I could be dropped from the Healthy Michigan Plan for not paying my bill. (n=4,084)		
Yes (n=1,371)	34.2	[32.3,36.1]
No (n=571)	14.4	[13.0,15.8]
Don't know (n=2,142)	51.5	[49.5,53.5]
I may get a reduction in the amount I might have to pay if I complete a health risk assessment. (n=4,081)		
Yes (n=1,161)	28.1	[26.3,30.0]
No (n=438)	10.7	[9.6,12.0]
Don't know (n=2,482)	61.1	[59.2,63.1]
Some kinds of visits, tests, and medicines have no copays. (n=4,084)		
Yes (n=3,176)	75.6	[73.8,77.3]
No (n=161)	4.6	[3.8,5.5]
Don't know (n=747)	19.8	[18.2,21.5]

MI Health Account

The majority (68.2%) of respondents reported that they received a MI Health Account statement.

	%	95% CI
Have you received a bill or statement from the state that showed the services you received and how much you owe for the Healthy Michigan Plan? It's called your MI Health Account Statement. (n=4,090)		
Yes (n=3,011)	68.2	[66.3,70.1]
No (n=951)	28.5	[26.6,30.4]
Don't know (n=128)	3.3	[2.7,4.1]

Among respondents who reported receiving a MI Health Account statement, 88.3% agreed that they carefully review each statement to see how much they owe, 88.4% agreed that the statements help them be more aware of the cost of health care, 30.8% agreed that the information in the statement led them to change some of their health care decisions.

	%	95% CI
I carefully review each MI Health Account statement to see how much I owe. (n=3,005)		
Strongly agree (n=765)	25.3	[23.4,27.4]
Agree (n=1,910)	63.0	[60.8,65.1]
Neutral (n=97)	3.5	[2.8,4.5]
Disagree (n=193)	6.9	[5.8,8.1]
Strongly disagree (n=30)	0.9	[0.6,1.5]
Don't know (n=10)	0.3	[0.2,0.6]
The MI Health Account statements help me be more aware of the cost of health care. (n=3,005)		
Strongly agree (n=654)	22.0	[20.2,24.0]
Agree (n=1,981)	66.4	[64.2,68.5]
Neutral (n=134)	4.4	[3.6,5.4]
Disagree (n=185)	5.6	[4.7,6.7]
Strongly disagree (n=21)	0.5	[0.3,0.8]
Don't know (n=30)	1.0	[0.6,1.5]
Information I saw in a MI Health Account statement led me to change some of my decisions about health care. (n=3,006)		
Strongly agree (n=134)	5.2	[4.2,6.3]
Agree (n=749)	25.6	[23.7,27.6]
Neutral (n=420)	14.9	[13.2,16.7]
Disagree (n=1,513)	48.0	[45.8,50.3]
Strongly disagree (n=104)	3.3	[2.6,4.2]
Don't know (n=86)	3.0	[2.3,4.0]

Information Seeking Behaviors

More than half (58.9%) of all survey respondents agreed that the amount they might have to pay for prescriptions influences their decisions about filling prescriptions.

	%	95% CI
The amount I might have to pay for my prescriptions influences my decisions about filling prescriptions. (n=4,084)		
Strongly agree (n=625)	15.7	[14.3,17.2]
Agree (n=1,736)	43.2	[41.2,45.2]
Neutral (n=282)	7.0	[6.0,8.0]
Disagree (n=1,162)	28.0	[26.3,29.8]
Strongly disagree (n=154)	3.5	[2.9,4.2]
Don't know (n=125)	2.8	[2.2,3.5]

Among all respondents, 71.6% reported being somewhat or very likely to find out how much they might have to pay for a health service before going to get it, 67.9% reported being somewhat or very likely to talk with their doctor about how much different health care options would cost them, 75.3% reported that they were somewhat or very likely to ask their doctor to recommend a less costly prescription drug, and 78.1% reported that they were somewhat or very likely to check reviews or ratings of quality before choosing a doctor or hospital.

	%	95% CI
Find out how much you might have to pay for a health service before you go to get it. (n=4,076)		
Very likely (n=1,816)	45.0	[43.0,46.9]
Somewhat likely (n=1,096)	26.6	[24.9,28.4]
Somewhat unlikely (n=490)	12.1	[10.9,13.4]
Very unlikely (n=589)	14.4	[13.1,15.8]
Don't know (n=85)	2.0	[1.5,2.6]
Talk with your doctor about how much different health care options would cost you. (n=4,076)		
Very likely (n=1,611)	40.8	[38.9,42.8]
Somewhat likely (n=1,135)	27.1	[25.4,28.8]
Somewhat unlikely (n=551)	13.8	[12.4,15.2]
Very unlikely (n=682)	15.9	[14.5,17.3]
Don't know (n=97)	2.4	[1.9,3.1]
Ask your doctor to recommend a less costly prescription drug. (n=4,074)		
Very likely (n=2,153)	50.9	[48.9,52.8]
Somewhat likely (n=990)	24.4	[22.7,26.1]
Somewhat unlikely (n=331)	9.7	[8.4,11.0]
Very unlikely (n=496)	12.8	[11.5,14.1]
Don't know (n=104)	2.4	[1.9,3.0]

Check reviews or ratings of quality before choosing a doctor or hospital. (n=4,074)		
Very likely (n=2,169)	53.8	[51.8,55.7]
Somewhat likely (n=973)	24.3	[22.7,26.1]
Somewhat unlikely (n=344)	8.3	[7.3,9.5]
Very unlikely (n=473)	11.0	[9.9,12.3]
Don't know (n=115)	2.5	[2.0,3.1]

Impact of HMP Premium Contributions on Cost-Conscious Behaviors

Beneficiaries with incomes 100 to 133% of the FPL, and therefore subject to monthly contributions, were no more likely then beneficiaries with incomes 36 to 99% of the FPL who are not subject to monthly premium contributions to agree they carefully review their MI Health Account statements (86.0% vs. 88.7%), inquire about costs of services before getting them (70.4% vs. 72.9%), talk to providers about costs of health services (67.8 vs. 68.6%), or ask for less costly medications (77.0% vs.78.2%) (See Appendix Table 24).

Beneficiaries with incomes 100 to 133% of the FPL were less likely than beneficiaries with incomes 36 to 99% of the FPL without monthly premium contributions to agree their health care payments were affordable (84.9% vs. 90.8%; P = 0.001), but were no more likely to report foregoing needed care due to cost in the previous 12 months of HMP enrollment (10.4% vs. 12.0%) (See Appendix Table 25).

Perceived Discrimination

Most respondents did not report feeling judged or treated unfairly by medical staff in the past 12 months because of their race or ethnic background (96.4%) or because of how well they spoke English (97.4%); however, 11.6% of respondents felt judged or treated unfairly by medical staff in the past 12 months because of their ability to pay for care or the type of health coverage they had.

	%	95% CI
In the last 12 months, have you ever felt that the doctor or medical staff judged you unfairly or treated you with disrespect because of your race or ethnic background. (n=4,076)		
Yes (n=114)	2.9	[2.3,3.6]
No (n=3,928)	96.4	[95.6,97.0]
Don't know (n=34)	0.8	[0.5,1.1]
In the last 12 months, have you ever felt that the doctor or medical staff judged you unfairly or treated you with disrespect because of how well you speak English. (n=4,075)		
Yes (n=64)	1.7	[1.3,2.3]
No (n=3,975)	97.4	[96.6,97.9]
Don't know (n=36)	0.9	[0.6,1.5]

In the last 12 months, have you ever felt that the doctor or medical staff judged you unfairly or treated you with disrespect because of your ability to pay for care or the type of health insurance you have. (n=4,077)		
Yes (n=465)	11.6	[10.4,12.9]
No (n=3,551)	87.0	[85.7,88.3]
Don't know (n=61)	1.4	[1.1,1.9]

Respondents who reported using the emergency room in the past year were more likely than those who did not use the emergency room to report being judged/treated unfairly by race (4.7% vs 1.7%), and ability to pay (15.5% vs. 9.2%) (See Appendix Tables 6 and 7).

Social Interactions

Two-thirds (67.6%) of respondents said that they get together socially with friends or relatives who live outside their home at least once a week. Most (79.8%) respondents reported that the amount they are involved with their family, friends, and/or community is about the same as before they enrolled in HMP.

	%	95% CI
How often do you get together socially with friends or relatives who live outside your home? (n=4,076)		
Every day (n=543)	14.0	[12.7,15.5]
Every few days (n=999)	23.7	[22.0,25.3]
Every week (n=1,217)	29.9	[28.1,31.7]
Every month (n=850)	21.0	[19.4,22.6]
Once a year or less (n=437)	10.9	[9.7,12.2]
Don't know (n=30)	0.6	[0.4,1.0]
Since enrolling in the Healthy Michigan Plan are you involved with your family, friends or community more, less, or about the same? (n=4,077)		
More (n=590)	15.1	[13.7,16.6]
Less (n=184)	4.4	[3.7,5.3]
About the same (n=3,284)	79.8	[78.2,81.4]
Don't know (n=19)	0.6	[0.4,1.1]

Selected Sub-Population Analyses

Reproductive Health

Among reproductive age women respondents age 19-45, 38.4% “did not know” whether there was a change in their access to family planning services, while 35.5% reported better access, 24.8% reported about the same access, and 1.4% reported worse access. Reproductive age women with inconsistent health insurance or that were uninsured in the year prior to HMP coverage were significantly more likely to report improved access to family planning services compared to those who were fully insured in the prior year (See Appendix Table 27).

Impact on Those with Chronic Health Conditions

A total of 68.1% of respondents reported that they had any chronic disease or mood disorder. More than half (59.9%) of respondents reported at least one chronic physical condition (ranging from 9.7% for heart disease to 31.3% for hypertension), 30.9% reported a chronic mental health condition (depression, anxiety, or bipolar disorder), and 22.6% reported both a physical and mental health chronic condition. Forty-four percent (44%) of those reporting a chronic condition reported they were newly diagnosed since enrolling in HMP. About one-third (30.6%) of all respondents were diagnosed with a new chronic physical condition or mood disorder since enrolling in HMP. This ranged from 32.4-35.4% of those with common physical health conditions (hypertension, heart disease, diabetes, COPD), 40.2% of those with stroke, and 28.8% of those with mood disorder.

	%	95% CI
Physical Chronic Disease[13] (n=4,090)		
Yes (n=2,640)	59.9	[57.9,61.8]
No (n=1,450)	40.1	[38.2,42.1]
Mood Disorder or Mental Health Condition (n=4,090)		
Yes (n=1,301)	30.9	[29.1,32.7]
No (n=2,789)	69.1	[67.3,70.9]
Any Chronic Disease or Mood Disorder (n=4,090)		
Yes (n=2,939)	68.1	[66.2,70.0]
No (n=1,151)	31.9	[30.0,33.8]
[If Any Chronic Disease or Mood Disorder] Any New Diagnoses since HMP Enrollment (n=2,939)		
Yes (n=1,297)	44.0	[41.7,46.3]
No (n=1,642)	56.0	[53.7,58.3]
Physical Chronic Disease and Mood or Mental Disorder (n=4,090)		
Yes (n=1,002)	22.6	[21.1,24.2]
No (n=3,088)	77.4	[75.8,78.9]
Any New Diagnoses since HMP Enrollment (n=4,090)		
Yes (n=1,318)	30.6	[28.8,32.4]
No (n=2,772)	69.4	[67.6,71.2]
Functional Limitations (n=4,026)		
Yes (n=749)	18.4	[17.0,20.0]
No (n=3,277)	81.6	[80.0,83.0]

Among those with a chronic physical or mental health condition in the year prior to HMP enrollment, 58.3% did not have insurance, only 42.1% had seen a primary care provider, and 51.7% had problems paying medical bills (See Appendix Table 30). Since HMP enrollment, 89.6% of those with a chronic physical or mental health condition reported seeing a primary

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[13] For these analyses, chronic illness does not include cancer.

care doctor, 64.6% reported their ability to fill prescription medications improved, and 86.3% reported their ability to pay medical bills had improved (See Appendix Tables 31 and 32). Prior to HMP 77.2% of those with a chronic physical or mental health condition had a regular source of care, 64.7% of whom said that source of care was a doctor's office or clinic. After HMP, 95.2% had a regular source of care, and 93.1% said it was a doctor's office or clinic (See Appendix Table 32).

Respondents with a chronic physical or mental health condition reported overall improvements in their physical (51.9%) and mental health (42.4%) status after enrolling in HMP, while 7.5% and 6.1% reported their physical and mental health status had worsened (See Appendix Table 31).

During HMP coverage, 18.4% of those with a chronic physical or mental health condition reported not getting medical or dental care they needed, with perceived health plan non-coverage (38.5%), cost (25.7%) and insurance not accepted (23.7%) the most common reasons (See Appendix Table 32).

Impact on Those with Mood Disorder and Substance Use Disorder

Nearly half (46.2%) of respondents who said they had a mood disorder stated that they had better access to mental health care, however, 20.3% did not know (See Appendix Table 39). Nearly half (48.3%) of respondents with SUD stated that they had better access to treatment, however 33.6% did not know. Most respondents without a self-reported SUD (82.8%) did not know how having HMP impacted their ability to get substance use treatment services (See Appendix Table 40). Since enrollment in HMP, 48.9% of respondents with a self-reported mood disorder (MD) and 50.7% with a self-reported substance use disorder (SUD) reported that their mental health had gotten better (See Appendix Table 41).

Respondents with a mood disorder reported that having HMP has led to a better life (92% strongly agreed or agreed) with more social connection and involvement with family and friends (21% stated more) and at higher rates than all HMP beneficiaries (12.6%). For respondents with a SUD, 95.8% strongly agreed or agreed that having HMP led to a better life and reported HMP led to more social connection and involvement with family and friends (23.2%) at higher rates than among respondents without a substance use disorder at 14.8% (See Appendix Tables 42 and 43).

Prior to HMP, 37% respondents who self-reported a SUD used the emergency room as a regular source of care, while after having HMP coverage, the percentage of those with a self-reported SUD who said they used the emergency room as a regular source of care dropped to 3.6% (See Appendix Tables 34 and 36). However, in the last 12 months (on HMP) those with a mood disorder and those with SUD were more likely to go to the ER than those without a mood disorder or SUD (50.5% MD v. 31.9% without a MD; 60.4% SUD v. 36.6% without a SUD) (See Appendix Table 37).

Respondents with SUD chose the ER due to proximity over other reasons (87.6% with a SUD v. 73.9% without a SUD) (See Appendix Table 44). For ER visits in general, respondents with a SUD have a higher odds of going to the emergency room (odds ratio 2.4) compared to all HMP beneficiaries (See Appendix Table 38).

CONCLUSIONS

- More than half of respondents, including more than half of those with chronic conditions, did not have insurance at any time in the year before enrolling in HMP. More than one-third of respondents reported not getting the care they needed in the year before enrolling in HMP and most respondents reported that their ability to get care had improved since enrolling in HMP. Foregone care, usually due to cost, lessened considerably after enrollment. Over half of respondents reported better access to primary care, help with staying healthy, and cancer screening. **HMP does not appear to have replaced employment-based insurance and has greatly improved access to care for most enrollees.**
- The percentage of enrollees who had a place they usually went for health care increased with HMP to over 90%, and naming the emergency room as a regular source of care declined significantly after enrolling in HMP (from 16.2% to 1.7%). For unscheduled health needs, some HMP beneficiaries sought advice from their regular source of care prior to seeking care, and the majority were referred to the emergency room. Those who used the emergency room had a higher chronic disease burden, and poorer health status. **The HMP emphasis on primary care and disease prevention appears to have shifted much care-seeking from acute care settings to primary care settings.**
- A significant majority of respondents agreed or strongly agreed that without HMP they would not be able to go to the doctor, that HMP helped them live a better life, and since enrolling in HMP their problems paying medical bills had gotten better. Premium contributions did not seem to have initially increased engagement in cost-conscious behaviors or to have increased foregone care due to cost, but did affect the perceived affordability of HMP. **Most respondents agreed that the amount they pay overall for HMP seems fair and is affordable, although enrollees subject to monthly contributions were somewhat less likely to perceive HMP as being affordable**.
- There were some areas in which beneficiaries showed a limited knowledge of HMP and its covered benefits (e.g., dental, vision and family planning) and misunderstanding about the cost-sharing requirements under HMP. A small number of respondents reported questions or problems using their HMP coverage. **These areas provide opportunities to improve beneficiaries' understanding of their coverage.**
- About half of respondents reported completing an HRA, bearing in mind the limits to self-reported data. Most HMP enrollees who completed the HRA believed it was beneficial. They rarely reported completing it because of incentives to reduce their cost-sharing. Most respondents who completed the HRA reported receiving help from their PCP or health plan on a healthy behavior. **Most respondents who recalled completing an HRA found this beneficial and received support to engage in a healthy behavior.**

- Dental coverage for HMP beneficiaries improved access to dental care and improved oral health for many, although many beneficiaries were unaware of dental coverage and were were less likely to report improved access and oral health. **Increasing beneficiary awareness of coverage for dental services has the potential to improve oral and overall health.**
- Many HMP enrollees reported improved functioning, ability to work, and job seeking after obtaining health insurance through Medicaid expansion. HMP enrollees who reported improved physical or mental health since HMP were more likely to report that HMP helped them to do a better job at work, made them better able to look for a job, and helped them get a better job. While many HMP enrollees attributed improvements in employment and ability to work to improved physical, mental and dental health due to covered services, some had ongoing barriers to employment. **HMP may influence beneficiaries' ability to obtain or maintain employment.**
- About half of reproductive-aged women HMP beneficiaries did not know whether there was a change in their access to family planning services compared to before HMP coverage. Those who previously had no or inconsistent health insurance, compared to those with consistent health insurance, reported improved access to family planning services. **Improved dissemination of the family planning services covered by HMP could help beneficiaries better meet their reproductive health needs.**
- Chronic health conditions were common among enrollees in Michigan's Medicaid expansion program, even though most respondents were under 50 years old. Almost half of these conditions were newly diagnosed after enrolling in HMP. Prior to HMP enrollment, a majority of enrollees with chronic illness lacked health insurance and could not access needed care. In particular, HMP enrollees with mood disorder or substance use disorder reported improved health, improved access to services and treatment, and were less likely to name the emergency room or urgent care as a regular source of care. **Enrollees with chronic conditions reported improved access to care and medications, all crucial to successfully managing these conditions and avoiding future disabling complications.**
- Overall, since enrolling in HMP almost half of respondents said their physical health had gotten better, and nearly 40% said their emotional and mental health and their dental health had improved. **These improvements underscore the impact of HMP on enrollees' health and well-being in addition to its effects on their ability to access needed care.**

APPENDIX

Impact of Prior Year Insurance Status on Improvements in Foregone Care, Access, and Health

Table 1. Insurance Status Prior to HMP: Impact on Outcomes

Outcomes[1]	All	Uninsured all 12 months [REF] (n=2,374)	Insured part of 12 months (n=374)	Insured all 12 months (n=1,235)
	Mean or %	% [95% CI]	% [95% CI]	% [95% CI]
Foregone care in 12 months prior to HMP enrollment	33.0	42.2 [39.7,44.7]	31.2 ** [25.7,36.8]	17.3 *** [14.8,19.8]
Foregone care due to cost in 12 months prior to HMP enrollment[2]	25.9	34.4 [31.9,36.8]	24.3 ** [19.2,29.4]	10.6 *** [8.6,12.6]
Improved access to prescription medicines	59.3	67.9 [65.4,70.3]	62.1 [55.9,68.4]	43.0 *** [39.6,46.5]
Improved access to primary care	57.8	68.7 [66.2,71.2]	57.4 ** [51.0,63.8]	37.9 *** [34.3,41.4]
Improved access to help with staying healthy	52.0	60.3 [57.8,62.8]	55.4 [49.0,61.7]	36.2 *** [32.8,39.6]
Improved access to dental care	46.1	54.1 [51.5,56.7]	48.0 [41.6,54.3]	32.3 *** [28.9,35.7]
Improved access to specialist care	44.4	51.8 [49.3,54.4]	44.1 * [37.8,50.4]	31.6 *** [28.2,34.9]
Improved access to mental health care	27.5	32.0 [29.6,34.4]	26.4 [20.4,32.3]	18.5 *** [15.7,21.3]
Improved access to cancer screening	25.7	31.3 [28.9,33.6]	23.4 * [18.2,28.7]	17.2 *** [14.8,19.6]
Improved physical health	47.8	54.3 [51.8,56.9]	50.6 [44.0,57.2]	34.6 *** [31.1,38.0]
Improved mental health	38.2	42.2 [39.6,44.7]	36.3 [30.0,42.7]	30.9 *** [27.3,34.4]
Improved oral health	39.5	44.4 [41.8,47.0]	40.1 [34.0,46.1]	31.5 *** [28.2,34.9]
I don't worry so much…[mean score, 0-4]	Mean 2.64	2.73 [2.67,2.78]	2.71 [2.56,2.86]	2.49 *** [2.41,2.57]
Having HMP has taken a lot of stress off me [mean score, 0-4]	Mean 3.09	3.16 [3.12,3.19]	3.17 [3.09,3.24]	2.99 *** [2.94,3.05]

NOTE: * denotes *P* < 0.05, ** denotes *P* < 0.01, and *** denotes *P* < 0.001.

[1] Results are adjusted for sex, age, income (0-33%FPL, 33-100%, 100-133%) race/ethnicity (NHW, AA, Hispanic, Arab/Chaldean, Others), urbanicity, health status and presence of any chronic condition.

[2] Going without health care because 'you were worried about the cost,' 'you did not have health insurance,' 'the doctor or hospital wouldn't accept your health insurance,' or 'your health plan wouldn't pay for the treatment.'

Primary Care Utilization and Experience

Table 2. Healthy Michigan Plan Beneficiary Characteristics, by PCP Visit in the Past 12 Months

	PCP visit in the past 12 months				
	Yes		No		*P*-value[1]
	Row %	95% CI	Row %	95% CI	
All[2] (n=4,090)	79.3	[77.5,80.9]	20.7	[19.1,22.5]	
Age					<0.001
19-34 (n=1,303)	72.1	[68.8,75.1]	27.9	[24.9,31.2]	
35-50 (n=1,301)	81.0	[78.0,83.7]	19.0	[16.3,22.0]	
51-64 (n=1,486)	88.1	[85.8,90.0]	11.9	[10.0,14.2]	
Gender					<0.001
Male (n=1,681)	73.6	[70.6,76.4]	26.4	[23.6,29.4]	
Female (n=2,409)	84.6	[82.7,86.4]	15.4	[13.6,17.3]	
FPL					0.364
0-35% (n=1,600)	78.7	[75.9,81.3]	21.3	[18.7,24.1]	
36-99% (n=1,450)	81.0	[78.3,83.5]	19.0	[16.5,21.7]	
≥100% (n=1,040)	78.2	[74.9,81.2]	21.8	[18.8,25.1]	
Race					<0.001
White (n=2,784)	82.5	[80.5,84.4]	17.5	[15.6,19.5]	
Black or African American (n=807)	74.4	[70.2,78.3]	25.6	[21.7,29.8]	
Other (n=306)	73.9	[67.4,79.5]	26.1	[20.5,32.6]	
More than one (n=142)	73.4	[62.5,82.0]	26.6	[18.0,37.5]	
Hispanic/Latino					0.331
Yes (n=188)	74.4	[66.4,81.0]	25.6	[19.0,33.6]	
No (n=3,856)	79.5	[77.7,81.3]	20.5	[18.7,22.3]	
DK (n=12)	68.2	[30.8,91.2]	31.8	[8.8,69.2]	
Arab, Chaldean, Middle Eastern					0.387
Yes (n=204)	82.4	[74.6,88.2]	17.6	[11.8,25.4]	
No (n=3,842)	79.0	[77.2,80.8]	21.0	[19.2,22.8]	
DK (n=9)	61.9	[24.4,89.1]	38.1	[10.9,75.6]	
Health status					<0.001
Excellent (n=337)	67.9	[61.3,73.8]	32.1	[26.2,38.7]	
Very good (n=1,041)	71.9	[67.9,75.7]	28.1	[24.3,32.1]	
Good (n=1,448)	81.3	[78.3,84.0]	18.7	[16.0,21.7]	
Fair (n=931)	86.3	[83.3,88.9]	13.7	[11.1,16.7]	
Poor (n=324)	90.7	[86.4,93.8]	9.3	[6.2,13.6]	
Any chronic health condition present					<0.001
Yes (n=2,986)	85.1	[83.2,86.8]	14.9	[13.2,16.8]	
No (n=1,104)	66.2	[62.5,69.8]	33.8	[30.2,37.5]	
Employment status					0.103
Yes (n=2,079)	77.8	[75.2,80.2]	22.2	[19.8,24.8]	
No (n=2,011)	80.7	[78.2,82.9]	19.3	[17.1,21.8]	

Married or partnered					0.102
Yes (n=1,193)	81.6	[78.4,84.5]	18.4	[15.5,21.6]	
No (n=2,880)	78.5	[76.4,80.5]	21.5	[19.5,23.6]	

[1] Pearson chi-square analyses

[2] Overall percentage of enrollees who had a PCP visit in the past year, regardless of whether or not they reported having a PCP

Table 3. Impact of PCP Visit in the Past 12 Months on Access, HRA, Counseling for Healthy Behavior and Diagnosis of New Chronic Condition

NOTE: Reported n is the number of observations in the logistic regression model

	Saw PCP in past 12 months		*P*-value[5]
	Yes (%)	No (%)	
Improved access to help with staying healthy[1] (n=4,004)	55.1 [52.8, 57.3]	40.1 [35.3, 44.9]	<0.001
Improved access to dental care[1] (n=4,011)	47.5 [45.3, 49.8]	41.1 [36.4, 45.9]	0.021
Improved access to specialty care[1] (n=4,012)	46.8 [44.6, 49.0]	35.6 [30.8, 40.4]	<0.001
Improved access to mental health care[1] (n=4,011)	28.0 [26.0, 30.1]	25.1 [20.7, 29.4]	0.242
Improved access to cancer screening[1] (n=3,997)	27.6 [25.7, 29.6]	18.0 [14.3, 21.6]	<0.001
Remembered completing an HRA (n=4,014)	52.8 [50.6, 55.1]	36.4 [31.7, 41.1]	<0.001
Reported being counseled about exercise (n=4,015)	55.4 [53.1, 57.6]	22.3 [18.4, 26.2]	<0.001
Reported being counseled about nutrition (n=4,014)	56.4 [54.1, 58.6]	24.7 [20.6, 28.7]	<0.001
Reported being counseled about tobacco cessation[2] (n=1,506)	61.6 [57.9, 65.2]	27.1 [20.2, 34.0]	<0.001
Reported being counseled about alcohol[3] (n=734)	36.2 [30.9, 41.5]	15.7 [8.4, 23.0]	<0.001
Reported being counseled about drug use[4] (n=173)	40.0 [30.4, 49.6]	30.1 [13.7, 46.5]	0.300
New diagnosis of chronic condition (n=4,015)	32.0 [30.1, 34.0]	22.7 [18.3, 27.0]	<0.001

[1] Participants reported that access to these health care resources had gotten better since enrollment in HMP

[2] Those who reported tobacco use

[3] Those who reported unsafe alcohol intake

[4] Those who reported unsafe drug use

[5] Logistic regression models included covariates age, gender, race, health status, FPL, employment, married/partnered and chronic condition

Impact of HMP on Acute Care Seeking

Table 4. Emergency Room Use in the Past 12 Months, by Health Status

	Health Status				
	Excellent, very good, or good		Fair or poor		*P*-value[1]
	Row %	95% CI	Row %	95% CI	
Any ER visits past 12 months (n=4,081)					<0.001
Yes (n=1,454)	59.9	[56.8,63.0]	40.1	[37.0,43.2]	
No (n=2,604)	76.8	[74.7,78.8]	23.2	[21.2,25.3]	

[1] Pearson chi-square analyses

Table 5. Emergency Room Use in the Past 12 Months, by Presence of Chronic Condition

	Any Chronic Health Condition Present				
	Yes		No		*P*-value[1]
	Row %	95% CI	Row %	95% CI	
Any ER visits past 12 months (n=4,090)					<0.001
Yes (n=1,456)	79.4	[76.4,82.1]	20.6	[17.9,23.6]	
No (n=2,611)	62.8	[60.3,65.2]	37.2	[34.8,39.7]	

[1] Pearson chi-square analyses

Table 6. Emergency Room Use in the Past 12 Months, by Perceived Discrimination Because of Race

	Discrimination: Race/Ethnicity				
	Yes		No		*P*-value[1]
	Row %	95% CI	Row %	95% CI	
Any ER visits past 12 months (n=4,076)					<0.001
Yes (n=1,451)	4.7	[3.5,6.3]	95.0	[93.4,96.3]	
No (n=2,603)	1.8	[1.3,2.5]	97.2	[96.4,97.8]	

[1] Pearson chi-square analyses

Table 7. Emergency Room Use in the Past 12 Months, by Perceived Discrimination Because of Ability to Pay

	Discrimination: Health Insurance/Ability to Pay				
	Yes		No		*P*-value[1]
	Row %	95% CI	Row %	95% CI	
Any ER visits past 12 months (n=4,077)					<0.001
Yes (n=1,452)	15.5	[13.4,17.9]	83.1	[80.6,85.3]	
No (n=2,603)	9.2	[7.8,10.8]	89.4	[87.8,90.9]	

[1] Pearson chi-square analyses

Table 8. Emergency Room Use in the Past 12 Months, by Perceived Discrimination Because of Ability to Speak English

	Discrimination: Ability to Speak English				
	Yes		No		*P*-value[1]
	Row %	95% CI	Row %	95% CI	
Any ER visits past 12 months (n=4,075)					0.003
Yes (n=1,451)	2.3	[1.5,3.4]	97.5	[96.3,98.3]	
No (n=2,602)	1.4	[0.9,2.0]	97.3	[96.3,98.1]	

[1] Pearson chi-square analyses

Table 9. Demographic and Health Characteristics for HMP Enrollees by Employment Status

	All	Employed or self-employed	Out of work, Total	Homemaker	Student	Retired	Unable to work	*P*-value
	% [95% CI]	% [95% CI]	% [95% CI]	% [95% CI]	% [95% CI]	% [95% CI]	% [95% CI]	
Age								
19-34	39.9 [37.9,41.9]	45.8 [43.0,48.6]	34.8 [30.9-38.9]	37.9 [30.1,46.3]	87.5 [81.4,91.8]	0	14.8 [10.6,20.2]	<0.001
35-50	34.0 [32.2,36.0]	34.2 [31.6,36.8]	37.7 [33.8-41.8]	35.1 [27.5,43.6]	8.5 [5.0,14.2]	1.1 [0.3,4.5]	43.1 [37.6,48.8]	
51-64	26.1 [24.6,27.6]	20.0 [18.3,21.9]	27.5 [24.4-30.8]	27.0 [20.7,34.3]	4.0 [2.1,7.7]	98.9 [95.5,99.7]	42.1 [36.8,47.5]	
Male Gender	48.5 [46.5,50.4]	45.5 [42.7,48.3]	57.2 [53.3,61.1]	6.8 [3.7,12.1]	53.3 [43.8,62.4]	51.3 [41.7,60.8]	53.9 [48.3,59.4]	<0.001
Race								
White or Caucasian	61.3 [59.4,63.2]	62.2 [59.5,64.9]	55.2 [51.1-59.2]	66.2 [58.0,73.5]	53.9 [44.3,63.2]	74.3 [63.0,83.1]	70.3 [64.7,75.4]	<0.001
Black or African-American	25.9 [24.2,27.7]	24.2 [21.8,26.8]	34.4 [30.6-38.5]	10.4 [6.3,16.7]	24.8 [17.9,33.4]	16.4 [9.3,27.2]	21.9 [17.3,27.3]	
Other	8.8 [7.7,10.0]	9.4 [7.9,11.2]	5.9 [4.4-7.9]	21.2 [15.3,28.7]	18.3 [11.2,28.6]	5.0 [2.0,11.9]	4.3 [2.5,7.3]	
More than one race	4.0 [3.3,4.9]	4.1 [3.1,5.5]	4.4 [3.0-6.5]	2.2 [1.0,5.1]	3.0 [1.0,8.2]	4.3 [1.1,15.4]	3.6 [2.1,6.1]	
Ethnicity								
Hispanic/Latino	5.2 [4.4,6.2]	6.1 [4.9,7.6]	4.6 [3.1-6.6]	4.9 [2.5,9.3]	6.5 [2.5,15.5]	2.8 [1.2,6.5]	3.3 [1.8,6.0]	0.429
Arab/Chaldean/Middle Eastern	6.2 [5.3,7.2]	7.3 [5.9,9.0]	2.7 [1.7-4.1]	21.1 [14.8,29.1]	14.6 [8.8,23.3]	0	1.2 [0.3,4.9]	<0.001
FPL								
0-35%	51.7 [50.7,52.7]	33.7 [31.3,36.3]	79.1 [76.5-81.5]	27.4 [19.8,36.8]	57.6 [48.4,66.3]	32.2 [23.0,42.9]	73.8 [69.4,77.8]	<0.001
36-99%	28.5 [27.6,29.3]	38.1 [36.1,40.1]	15.0 [12.9-17.3]	46.6 [38.7,54.6]	21.5 [15.5,29.0]	35.4 [26.9,44.9]	13.9 [10.9,17.6]	
≥100%	19.8 [19.2,20.5]	28.1 [26.5,29.8]	5.9 [4.7-7.4]	26.0 [20.0,33.0]	20.9 [14.4,29.3]	32.4 [25.0,40.9]	12.2 [9.6,15.4]	
Veteran	3.4 [2.7,4.2]	2.3 [1.6,3.3]	3.9 [2.6-5.8]	0.5 [0.1,2.0]	3.0 [1.0,8.7]	13.4 [7.6,22.5]	5.9 [3.7,9.2]	0.001
Health Status								
Excellent, very good, or good	70.1 [68.4,71.9]	80.3 [78.1,82.4]	66.1 [62.3-69.6]	77.5 [70.2,83.5]	81.1 [72.5,87.6]	75.9 [67.8,82.5]	26.2 [21.5,31.5]	<0.001
Fair or poor	29.7 [28.0,31.5]	19.6 [17.5,21.9]	33.7 [30.1-37.4]	22.5 [16.5,29.8]	18.9 [12.4,27.5]	24.1 [17.5,32.2]	73.4 [68.1,78.1]	
Chronic Health Condition	69.2 [67.3,71.0]	62.3 [59.5,65.0]	74.0 [69.9-77.6]	66.0 [57.5,73.7]	52.6 [43.1,62.0]	77.8 [67.5,85.6]	94.0 [90.6,96.2]	<0.001
Physical Health Condition	60.8 [58.8,62.8]	53.8 [51.0,56.6]	65.1 [60.9-69.0]	58.4 [49.9,66.3]	40 [31.4,49.3]	76.3 [66.0,84.1]	87.5 [82.6,91.2]	<0.001
Diabetes	10.8 [9.7,12.0]	8.8 [7.5,10.4]	11.4 [9.3-13.9]	9.9 [5.8,16.3]	4.1 [1.8,9.3]	9.3 [5.4,15.6]	22.3 [17.9,27.4]	<0.001
Hypertension	31.3 [29.6,33.1]	24.9 [22.7,27.3]	37.6 [33.8-41.5]	20.6 [15.2,27.2]	10.7 [6.7,16.5]	46.2 [36.7,55.9]	54.2 [48.5,59.8]	<0.001
Cardiovascular Disease	9.8 [8.7,11.0]	7.1 [5.9,8.6]	10.4 [8.2-13.2]	6.6 [4.0,10.6]	3.7 [1.7,7.9]	12.5 [8.2,18.7]	22.9 [18.3,28.2]	<0.001
Asthma	17.1 [15.7,18.6]	14.7 [12.9,16.6]	16.1 [13.5-19.1]	22.8 [16.5,30.8]	21.2 [14.4,30.1]	14.2 [8.0,24.0]	26.6 [21.9,31.9]	<0.001
COPD	10.5 [9.5,11.7]	7.6 [6.2,9.1]	11.2 [9.2-13.6]	10.6 [5.9,18.2]	2.9 [1.2,7.2]	17.4 [11.8,25.0]	23.7 [19.3,28.8]	<0.001
Cancer	3.7 [3.2,4.4]	2.8 [2.1,3.6]	2.7 [1.8-4.1]	5.2 [3.1,8.6]	1.8 [0.5,6.5]	7.6 [4.5,12.5]	10.2 [7.4,14.0]	<0.001
Mental Health Condition	32.2 [30.4,34.0]	25.2 [22.9,27.7]	35.3 [31.7-39.1]	24.2 [18.0,31.5]	30.2 [22.1,39.8]	20.3 [13.3,29.8]	61.7 [56.1,66.9]	<0.001
Mood disorder	30.5 [28.7,32.3]	23.5 [21.2,25.9]	33.7 [30.1-37.4]	23.9 [17.8,31.3]	26.6 [19.1,35.8]	19.9 [12.9,29.5]	59.6 [54.1,65.0]	<0.001
Other	0.8 [0.4,1.3]	0.8 [0.4,1.8]	0.2 [0.0-1.1]	0.3 [0.0,1.8]	3.7 [1.0,12.6]	0.4 [0.1,2.8]	1.2 [0.5,2.8]	0.008

Functional Impairment (≥14 of past 30 days)								
Physical	22.9 [21.3,24.5]	13.3 [11.6,15.3]	24.4 [21.2-27.9]	21.3 [15.0,29.1]	7.6 [4.3,13.1]	24.0 [17.3,32.2]	68.8 [63.2,73.8]	<0.001
Mental	19.9 [18.3,21.5]	11.6 [10.1,13.4]	25.0 [21.7-28.7]	15.1 [9.8,22.4]	16.2 [9.8,25.4]	13.6 [8.8,20.4]	48.4 [42.7,54.1]	<0.001

Table 10. Demographic and Health Characteristics for HMP Enrollees who are Out of Work, ≥ 1 year vs. <1 year

	Out of work ≥ 1 year		Out of work <1 year		Out of work, Total	
	%	[95% CI]	%	[95% CI]	%	[95% CI]
Age						
19-34	28.8	[24.6,33.4]	49.8	[42.2,57.4]	34.8	[30.9-38.9]
35-50	40.0	[35.3,44.9]	32.1	[25.9,39.0]	37.7	[33.8-41.8]
51-64	31.2	[27.4,35.3]	18.1	[13.2,24.3]	27.5	[24.4-30.8]
Male Gender	58.4	[53.7,62.9]	54.5	[46.9,61.9]	57.2	[53.3,61.1]
Race						
White or Caucasian	58.0	[53.2,62.6]	48.2	[40.7,55.8]	55.2	[51.1-59.2]
Black or African-American	31.9	[27.5,36.7]	40.8	[33.1,48.9]	34.4	[30.6-38.5]
Other	6.1	[4.3,8.5]	5.7	[3.2,9.8]	5.9	[4.4-7.9]
More than one race	4.1	[2.5,6.6]	5.4	[2.8,9.9]	4.4	[3.0-6.5]
Ethnicity						
Hispanic/Latino	5.0	[3.2,7.7]	3.5	[1.7,7.2]	4.6	[3.1-6.6]
Arab/Chaldean/Middle Eastern	2.6	[1.6,4.1]	3.0	[1.3,7.2]	2.7	[1.7-4.1]
FPL						
0-35%	81.8	[78.7,84.6]	72.4	[66.6,77.6]	79.1	[76.5-81.5]
36-99%	13.9	[11.4,16.9]	17.6	[13.7,22.3]	15.0	[12.9-17.3]
≥100%	4.3	[3.1,5.8]	10.0	[7.0,14.0]	5.9	[4.7-7.4]
Veteran	4.7	[3.0,7.2]	2.0	[0.8,4.8]	3.9	[2.6-5.8]
Health Status						
Excellent, very good, or good	63.6	[59.1,67.9]	72.2	[65.3,78.2]	66.1	[62.3-69.6]
Fair or poor	36.1	[31.8,40.6]	27.8	[21.8,34.7]	33.7	[30.1-37.4]
Chronic Health Condition	75.9	[71.3,80.0]	69.1	[60.6,76.4]	74.0	[69.9-77.6]
Physical Health Condition	68.2	[63.4,72.6]	57.4	[49.4,65.0]	65.1	[60.9-69.0]
Diabetes	13.8	[11.1,17.1]	5.2	[3.0,8.7]	11.4	[9.3-13.9]
Hypertension	39.8	[35.3,44.5]	32.0	[25.6,39.2]	37.6	[33.8-41.5]
Cardiovascular Disease	11.3	[8.6,14.8]	8.2	[5.1,12.9]	10.4	[8.2-13.2]
Asthma	16.3	[13.2,19.9]	15.6	[11.2,21.3]	16.1	[13.5-19.1]
COPD	12.6	[10.1,15.6]	7.8	[5.0,12.0]	11.2	[9.2-13.6]
Cancer	2.4	[1.5,3.9]	3.5	[1.6,7.2]	2.7	[1.8-4.1]
Mental Health Condition	35.1	[30.8,39.6]	35.9	[29.3,43.0]	35.3	[31.7-39.1]
Mood disorder	33.5	[29.3,38.0]	33.9	[27.5,41.0]	33.7	[30.1-37.4]
Other	0.2	[0.0,1.6]	0		0.2	[0.0-1.1]

Functional Impairment (≥14 of past 30 days)						
Physical	26.2	[22.3,30.5]	19.8	[14.7,26.3]	24.4	[21.2-27.9]
Mental	26.3	[22.3,30.8]	21.8	[16.2,28.7]	25.0	[21.7-28.7]

Table 11. Employment Status Among Healthy Michigan Plan Enrollees, by Health Status

	Health Status						
	Excellent, very good, or good		Fair or poor		Total		*P*-value[1]
	Col %	95% CI	Col %	95% CI	Col %	95% CI	
Employment Status (n=4,059)							<0.001
Employed or self-employed (n=2,076)	56.1	[53.7,58.4]	32.3	[29.1,35.5]	48.9	[47.0,50.8]	
Out of work ≥1 year (n=705)	17.9	[16.0,19.9]	23.9	[21.0,27.0]	19.7	[18.1,21.3]	
Out of work <1 year (n=258)	8.1	[6.8,9.7]	7.4	[5.7,9.4]	7.9	[6.8,9.1]	
Homemaker (n=217)	5.0	[4.2,6.0]	3.4	[2.5,4.7]	4.5	[3.8,5.3]	
Student (n=161)	6.0	[4.9,7.4]	3.3	[2.1,5.1]	5.2	[4.3,6.2]	
Retired (n=167)	2.7	[2.2,3.4]	2.0	[1.5,2.8]	2.5	[2.1,3.0]	
Unable to work (n=475)	4.2	[3.4,5.2]	27.8	[24.8,31.0]	11.3	[10.1,12.5]	

[1] Pearson chi-square analyses

Table 12. Employment Status Among Healthy Michigan Plan Enrollees, by Presence of Chronic Condition

	Any Chronic Health Condition Present						
	Yes		No		Total		*P*-value[1]
	Col %	95% CI	Col %	95% CI	Col %	95% CI	
Employment Status (n=4,068)							<0.001
Employed or self-employed (n=2,079)	44.1	[41.9,46.3]	59.8	[55.9,63.5]	48.9	[47.0,50.8]	
Out of work ≥1 year (n=707)	21.6	[19.7,23.6]	15.4	[12.7,18.5]	19.7	[18.1,21.3]	
Out of work <1 year (n=258)	7.9	[6.7,9.2]	7.9	[5.7,10.8]	7.9	[6.8,9.1]	
Homemaker (n=217)	4.3	[3.6,5.2]	5.0	[3.7,6.7]	4.5	[3.8,5.3]	
Student (n=161)	3.9	[3.1,5.0]	8.0	[6.0,10.4]	5.2	[4.3,6.2]	
Retired (n=167)	2.8	[2.3,3.5]	1.8	[1.1,2.9]	2.5	[2.1,3.0]	
Unable to work (n=479)	15.3	[13.8,17.0]	2.2	[1.4,3.5]	11.3	[10.1,12.5]	

[1] Pearson chi-square analyses

Table 13. Ability to Work Among Healthy Michigan Plan Enrollees Who Are Employed/Self-Employed

	Mean or %	95% CI
[If employed or self-employed] In the past 12 months, about how many days did you miss work at a job or business because of illness or injury (do not include maternity leave)?	Mean 7.2	[5.6,8.7]
Compared to the 12 months before this time, was this more, less, or about the same? (n=2,074)		
More (n=261)	12.3	[10.7,14.1]
Less (n=345)	17.2	[15.2,19.5]
About the same (n=1,437)	68.4	[65.8,70.9]
Don't know (n=31)	2.1	[1.2,3.4]

Table 14. Multivariable Logistic Regression Analysis of Association between HMP Enrollee Demographic and Health Characteristics and being Out of Work or Unable to Work

	Outcomes[1]			
	Out of Work		Unable to Work	
Characteristic	aOR (95% CI)	*P*-value	aOR (95% CI)	*P*-value
Age				
19-34	[ref]	[ref]	[ref]	[ref]
35-50	1.29 (0.99-1.67)	0.056	2.34 (1.45-3.75)	<0.001
51-64	1.67 (1.29-2.17)	<0.001	4.20 (2.64-6.65)	<0.001
Male gender	1.80 (1.45-2.23)	<0.001	1.88 (1.35-2.63)	<0.001
Race				
White or Caucasian	[ref]	[ref]	[ref]	[ref]
Black or African-American	1.93 (1.50-2.49)	<0.001	1.16 (0.76-1.78)	0.483
Other	0.75 (0.50-1.11)	0.148	0.51 (0.25-1.06)	0.072
More than one race	1.25 (0.72-2.18)	0.423	1.02 (0.49-2.15)	0.954
Fair or poor health	1.47 (1.15-1.89)	0.003	3.52 (2.42-5.11)	<0.001
Chronic Health Condition [reference = none]				
Physical	1.11 (0.88-1.42)	0.378	1.73 (1.08-2.79)	0.023
Mental	1.47 (1.16-1.87)	0.001	2.61 (1.82-3.73)	<0.001
Functional Limitation [reference = none]				
Physical	1.43 (1.07-1.92)	0.016	5.10 (3.54-7.33)	<0.001
Mental	1.95 (1.46-2.60)	<0.001	2.29 (1.56-3.37)	<0.001

aOR = adjusted odds ratio; CI = confidence interval

[1] Each column represents a different multivariable logistic regression model.

Table 15. Factors Associated with Employment and Ability to Work, Among Healthy Michigan Plan Enrollees who were Employed/Self-employed

Characteristic	Outcomes[1]			
	Employed or Self-Employed (Weighted N=106,619)		Better Job at Work (Weighted N=75,282)	
	aOR (95% CI)	*P*- value	aOR (95% CI)	*P*-value
Physical or mental health better since HMP enrollment	1.08 (0.89, 1.30)	0.44	4.08 (3.11, 5.35)	<0.001
Age				
19-34	Reference		Reference	
35-50	0.98 (0.78, 1.24)	0.89	0.96 (0.70, 1.31)	0.78
51-64	0.56 (0.45, 0.70)	<0.001	1.10 (0.80, 1.51)	0.57
Female gender	1.00 (0.83, 1.21)	0.98	1.42 (1.08, 1.85)	0.01
Race				
White or Caucasian	Reference		Reference	
Black or African American	0.96 (0.77, 1.21)	0.74	1.55 (1.10, 2.19)	0.01
Other	0.87 (0.61, 1.23)	0.44	1.24 (0.69, 2.21)	0.47
More than one race	1.10 (0.67, 1.82)	0.71	1.70 (0.79, 3.67)	0.18
FPL				
0-35%	Reference		Reference	
36-99%	3.72 (3.02, 4.58)	<0.001	0.79 (0.54, 1.15)	0.22
100-133%	4.40 (3.51, 5.52)	<0.001	0.62 (0.42, 0.90)	0.01
Fair or poor health	0.67 (0.53, 0.83)	<0.001	1.09 (0.76, 1.57)	0.64
Chronic health condition	0.84 (0.67, 1.06)	0.14	1.57 (1.18, 2.09)	0.002
Functional limitation, physical or mental	0.26 (0.19, 0.34)	<0.001	1.20 (0.69, 2.09)	0.53

aOR = adjusted odds ratio; CI = confidence interval; HMP = Healthy Michigan Plan

[1] Each column represents a different multivariable logistic regression model. In the first model, employment status was dichotomized as employed/self-employed vs. all other responses. We checked for collinearity of variables, including health status/chronic condition/function and there was no collinearity in the model.

Table 16. Factors Associated with Job Seeking Ability, Among Healthy Michigan Plan Enrollees who Had a Recent Job Change or were Out of Work

Characteristic	Outcomes[1]			
	Better able to look for job[2] (Weighted N=35,711)		Helped get a better job[3] (Weighted N=9,275)	
	aOR (95% CI)	*P*- value	aOR (95% CI)	*P*-value
Physical or mental health better since HMP enrollment	2.82 (1.93, 4.10)	<0.001	3.20 (1.69, 6.09)	<0.001
Age				
19-34	Reference		Reference	
35-50	1.36 (0.87, 2.11)	0.17	1.01 (0.55, 1.87)	0.97
51-64	1.76 (1.14, 2.72)	0.01	1.30 (0.65, 2.59)	0.46
Female gender	0.73 (0.50, 1.07)	0.10	0.72 (0.41, 1.25)	0.24
Race				
White or Caucasian	Reference		Reference	
Black or African American	0.80 (0.53, 1.22)	0.30	1.31 (0.68, 2.55)	0.42
Other	1.52 (0.73, 3.19)	0.27	1.69 (0.65, 4.41)	0.28
More than one race	0.51 (0.22, 1.23)	0.13	0.46 (0.13, 1.67)	0.24
FPL				
0-35%	Reference		Reference	
36-99%	0.83 (0.53, 1.29)	0.40	0.90 (0.47, 1.73)	0.76
100-133%	0.74 (0.41, 1.36)	0.33	0.60 (0.31, 1.17)	0.13
Fair or poor health	1.17 (0.79, 1.74)	0.42	1.17 (0.56, 2.45)	0.67
Chronic health condition	0.87 (0.54, 1.40)	0.57	1.31 (0.72, 2.36)	0.37
Functional limitation, physical or mental	0.85 (0.56, 1.30)	0.46	1.51 (0.47, 4.89)	0.49

aOR = adjusted odds ratio; CI = confidence interval; HMP = Healthy Michigan Plan

[1] Each column represents a different multivariable logistic regression model.

[2] Strongly agree or agree that "Having health insurance through the Healthy Michigan Plan has made me better able to look for a job."

[3] Strongly agree or agree that "Having health insurance through the Healthy Michigan Plan helped me get a better job."

Table 17. Healthy Michigan Plan Beneficiary Characteristics, by Awareness of Dental Care Coverage

	My Healthy Michigan Plan covers routine dental visits.						
	Yes		No		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Age							0.524
19-34 (n=1,303)	76.9	[73.8,79.8]	4.6	[3.4,6.2]	18.5	[15.8,21.4]	
35-50 (n=1,300)	76.7	[73.6,79.5]	3.4	[2.5,4.6]	20.0	[17.3,23.0]	
51-64 (n=1,483)	78.2	[75.6,80.6]	3.7	[2.7,5.0]	18.1	[15.9,20.6]	
Total (n=4,086)	77.2	[75.4,78.8]	3.9	[3.3,4.7]	18.9	[17.3,20.6]	
FPL							0.016
0-35% (n=1,599)	77.1	[74.3,79.7]	2.9	[2.1,4.1]	20.0	[17.5,22.7]	
36-99% (n=1,448)	78.5	[75.9,80.9]	4.9	[3.7,6.4]	16.6	[14.5,18.9]	
≥100% (n=1,039)	75.3	[72.0,78.3]	5.2	[3.9,7.1]	19.4	[16.7,22.5]	
Total (n=4,086)	77.2	[75.4,78.8]	3.9	[3.3,4.7]	18.9	[17.3,20.6]	
Region							0.087
UP/NW/NE (n=745)	78.6	[75.0,81.7]	2.9	[1.9,4.4]	18.5	[15.5,22.0]	
W/EC/E (n=1,264)	79.0	[76.2,81.5]	3.3	[2.4,4.6]	17.7	[15.3,20.3]	
SC/SW/SE (n=836)	72.5	[68.5,76.2]	4.6	[3.3,6.4]	22.9	[19.3,26.9]	
DET (n=1,241)	77.7	[74.6,80.5]	4.2	[3.1,5.7]	18.1	[15.5,21.0]	
Total (n=4,086)	77.2	[75.4,78.8]	3.9	[3.3,4.7]	18.9	[17.3,20.6]	
Employment status							0.364
Employed or self-employed (n=2,078)	77.9	[75.5,80.2]	4.0	[3.1,5.2]	18.0	[15.9,20.4]	
Out of work ≥1 year (n=705)	74.4	[69.7,78.6]	3.4	[2.0,5.7]	22.2	[18.2,26.8]	
Out of work <1 year (n=258)	78.9	[72.1,84.4]	3.8	[2.1,7.0]	17.3	[12.2,24.0]	
Homemaker (n=217)	79.3	[72.3,84.9]	6.1	[3.1,11.7]	14.6	[10.1,20.6]	
Student (n=161)	75.3	[66.1,82.6]	5.4	[2.9,10.0]	19.3	[12.6,28.5]	
Retired (n=167)	80.1	[72.8,85.8]	3.8	[1.8,7.7]	16.1	[11.0,23.1]	
Unable to work (n=479)	77.1	[72.4,81.2]	2.2	[1.3,3.7]	20.7	[16.7,25.3]	
Don't know (n=7)	53.2	[15.8,87.3]	0		46.8	[12.7,84.2]	
Total (n=4,072)	77.2	[75.4,78.8]	3.8	[3.2,4.6]	19.0	[17.4,20.7]	

[1] Pearson chi-square analyses

Table 18. Healthy Michigan Plan Beneficiary Characteristics, by Perceived Dental Care Access

	Would you say that your ability to get dental care through the Healthy Michigan Plan is better, worse, or about the same, compared to before?								
	Better		Worse		About the same		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Age									<0.001
19-34 (n=1,302)	44.4	[41.1,47.8]	6.4	[4.8,8.4]	35.2	[31.9,38.6]	14.1	[11.9,16.6]	
35-50 (n=1,298)	47.7	[44.3,51.1]	5.9	[4.6,7.6]	26.1	[23.2,29.1]	20.3	[17.5,23.4]	
51-64 (n=1,484)	46.4	[43.3,49.6]	6.5	[5.1,8.3]	24.7	[22.1,27.5]	22.4	[19.9,25.0]	
Total (n=4,084)	46.1	[44.1,48.0]	6.2	[5.4,7.3]	29.3	[27.5,31.2]	18.4	[16.9,19.9]	
FPL									0.104
0-35% (n=1,596)	46.8	[43.7,49.9]	5.3	[4.1,7.0]	28.2	[25.4,31.2]	19.7	[17.3,22.2]	
36-99% (n=1,448)	46.3	[43.2,49.4]	6.8	[5.4,8.7]	29.6	[26.7,32.6]	17.3	[15.0,19.8]	
≥100% (n=1,040)	43.6	[40.2,47.2]	7.8	[6.0,10.1]	32.1	[28.8,35.5]	16.5	[14.0,19.3]	
Total (n=4,084)	46.1	[44.1,48.0]	6.2	[5.4,7.3]	29.3	[27.5,31.2]	18.4	[16.9,19.9]	
Region									0.566
UP/NW/NE (n=746)	48.8	[44.7,52.9]	6.5	[4.9,8.5]	28.0	[24.3,32.0]	16.8	[14.1,19.8]	
W/EC/E (n=1,263)	47.3	[44.2,50.5]	5.9	[4.4,7.8]	28.1	[25.3,31.1]	18.6	[16.2,21.3]	
SC/SW/SE (n=835)	45.4	[41.4,49.5]	5.8	[4.2,8.0]	27.9	[24.1,31.9]	20.9	[17.9,24.3]	
DET (n=1,240)	44.9	[41.5,48.4]	6.6	[5.1,8.5]	31.0	[27.9,34.4]	17.4	[14.9,20.3]	
Total (n=4,084)	46.1	[44.1,48.0]	6.2	[5.4,7.3]	29.3	[27.5,31.2]	18.4	[16.9,19.9]	
Employment status									<0.001
Employed or self-employed (n=2,077)	48.2	[45.5,51.0]	5.5	[4.5,6.7]	30.1	[27.6,32.7]	16.2	[14.3,18.2]	
Out of work ≥1 year (n=704)	45.7	[41.0,50.4]	4.9	[3.1,7.7]	25.3	[21.4,29.6]	24.2	[20.2,28.7]	
Out of work <1 year (n=258)	43.0	[35.8,50.5]	9.0	[4.9,15.8]	28.8	[22.1,36.4]	19.3	[13.8,26.2]	
Homemaker (n=217)	48.0	[39.8,56.3]	5.7	[3.2,9.8]	33.8	[26.5,41.9]	12.6	[8.6,18.1]	
Student (n=160)	32.3	[24.6,41.0]	12.8	[7.6,20.9]	43.8	[34.5,53.6]	11.1	[6.6,18.0]	
Retired (n=167)	48.6	[39.0,58.3]	7.4	[3.8,13.9]	24.8	[17.3,34.3]	19.2	[13.1,27.1]	
Unable to work (n=479)	44.1	[38.6,49.7]	6.8	[4.4,10.4]	27.1	[22.2,32.5]	22.0	[17.8,27.0]	
Don't know (n=7)	58.7	[17.6,90.4]	0		0		41.3	[9.6,82.4]	
Total (n=4,069)	46.1	[44.1,48.0]	6.2	[5.3,7.2]	29.4	[27.6,31.3]	18.3	[16.9,19.9]	

[1] Pearson chi-square analyses

Table 19. Healthy Michigan Plan Beneficiary Characteristics, by Forgone Dental Care

	Forgone dental care due to cost[1]				
	Yes		No		*P*-value[2]
	Row %	95% CI	Row %	95% CI	0.537
Age					
19-34 (n=136)	65.3	[55.1,74.3]	34.7	[25.7,44.9]	
35-50 (n=132)	58.5	[47.9,68.3]	41.5	[31.7,52.1]	
51-64 (n=125)	66.1	[54.1,76.3]	33.9	[23.7,45.9]	
Total (n=393)	63.2	[57.0,69.0]	36.8	[31.0,43.0]	
FPL					0.282
0-35% (n=156)	59.9	[50.6,68.5]	40.1	[31.5,49.4]	
36-99% (n=142)	64.1	[53.2,73.7]	35.9	[26.3,46.8]	
≥100% (n=95)	72.0	[60.8,81.0]	28.0	[19.0,39.2]	
Total (n=393)	63.2	[57.0,69.0]	36.8	[31.0,43.0]	
Region					0.047
UP/NW/NE (n=55)	57.2	[42.3,70.9]	42.8	[29.1,57.7]	
W/EC/E (n=115)	61.1	[50.8,70.6]	38.9	[29.4,49.2]	
SC/SW/SE (n=92)	50.6	[38.9,62.2]	49.4	[37.8,61.1]	
DET (n=131)	70.5	[59.6,79.5]	29.5	[20.5,40.4]	
Total (n=393)	63.2	[57.0,69.0]	36.8	[31.0,43.0]	
Employment status					0.008
Employed or self-employed (n=196)	61.5	[52.6,69.8]	38.5	[30.2,47.4]	
Out of work ≥1 year (n=67)	68.6	[53.9,80.3]	31.4	[19.7,46.1]	
Out of work <1 year (n=26)	82.5	[64.3,92.5]	17.5	[7.5,35.7]	
Homemaker (n=18)	79.2	[52.8,92.8]	20.8	[7.2,47.2]	
Student (n=19)	78.9	[55.9,91.7]	21.1	[8.3,44.1]	
Retired (n=9)	70.3	[31.8,92.3]	29.7	[7.7,68.2]	
Unable to work (n=58)	41.3	[25.6,59.1]	58.7	[40.9,74.4]	
Total (n=393)	63.2	[57.0,69.0]	36.8	[31.0,43.0]	

[1] Going without dental care because 'you were worried about the cost,' 'you did not have health insurance,' 'the doctor or hospital wouldn't accept your health insurance,' or 'your health plan wouldn't pay for the treatment.'

[2] Pearson chi-square analyses

Table 20. Healthy Michigan Plan Beneficiary Characteristics, by Oral Health

	Since you enrolled in the Healthy Michigan Plan, has the health of your teeth and gums gotten better, stayed the same, or gotten worse?								
	Gotten better		Stayed the same		Gotten worse		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Age									<0.001
19-34 (n=1,302)	38.8	[35.6,42.1]	50.1	[46.7,53.6]	8.1	[6.5,10.1]	2.9	[2.0,4.2]	
35-50 (n=1,299)	39.9	[36.6,43.3]	42.1	[38.7,45.5]	12.5	[10.5,14.9]	5.5	[4.1,7.4]	
51-64 (n=1,483)	40.1	[37.1,43.3]	42.9	[39.8,46.0]	11.0	[9.2,13.0]	6.0	[4.7,7.8]	
Total (n=4,084)	39.5	[37.6,41.5]	45.5	[43.5,47.5]	10.4	[9.3,11.6]	4.6	[3.9,5.5]	
FPL									0.198
0-35% (n=1,597)	40.0	[37.0,43.1]	44.0	[40.9,47.2]	11.1	[9.4,13.0]	4.9	[3.8,6.4]	
36-99% (n=1,448)	40.7	[37.7,43.8]	44.9	[41.8,48.0]	9.9	[8.1,12.0]	4.6	[3.4,6.0]	
≥100% (n=1,039)	36.6	[33.3,40.0]	50.3	[46.8,53.9]	9.2	[7.4,11.3]	3.9	[2.7,5.6]	
Total (n=4,084)	39.5	[37.6,41.5]	45.5	[43.5,47.5]	10.4	[9.3,11.6]	4.6	[3.9,5.5]	
Region									0.053
UP/NW/NE (n=745)	40.9	[36.9,45.0]	44.4	[40.3,48.5]	9.3	[7.3,11.8]	5.5	[3.9,7.5]	
W/EC/E (n=1,263)	38.2	[35.2,41.3]	46.9	[43.7,50.1]	9.0	[7.4,10.8]	6.0	[4.5,7.9]	
SC/SW/SE (n=836)	36.4	[32.7,40.4]	46.6	[42.5,50.8]	13.0	[10.5,15.9]	4.0	[2.8,5.6]	
DET (n=1,240)	41.4	[38.0,44.9]	44.4	[40.9,47.9]	10.4	[8.6,12.6]	3.8	[2.7,5.4]	
Total (n=4,084)	39.5	[37.6,41.5]	45.5	[43.5,47.5]	10.4	[9.3,11.6]	4.6	[3.9,5.5]	
Employment status									<0.001
Employed or self-employed (n=2,077)	40.1	[37.4,42.8]	46.9	[44.2,49.7]	9.2	[7.8,10.8]	3.8	[2.9,5.0]	
Out of work ≥1 year (n=704)	35.9	[31.6,40.4]	48.9	[44.2,53.7]	11.3	[8.6,14.7]	3.9	[2.6,5.8]	
Out of work <1 year (n=258)	43.2	[35.8,50.9]	42.0	[34.6,49.8]	9.0	[6.1,13.1]	5.8	[3.2,10.1]	
Homemaker (n=217)	43.3	[35.2,51.7]	45.3	[37.3,53.5]	9.3	[5.9,14.4]	2.2	[0.8,5.6]	
Student (n=161)	34.6	[26.4,43.7]	51.0	[41.5,60.3]	9.4	[5.7,15.0]	5.1	[2.0,12.8]	
Retired (n=167)	44.9	[35.3,54.9]	41.7	[32.7,51.3]	10.1	[5.9,16.7]	3.3	[1.4,7.5]	
Unable to work (n=478)	39.7	[34.3,45.4]	35.6	[30.5,41.1]	15.8	[12.0,20.6]	8.9	[6.0,12.9]	
Don't know (n=7)	27.0	[6.5,66.1]	39.3	[10.5,78.2]	0		33.7	[5.6,81.3]	
Total (n=4,069)	39.4	[37.5,41.4]	45.6	[43.7,47.6]	10.4	[9.3,11.6]	4.6	[3.8,5.5]	

[1] Pearson chi-square analyses

Table 21. Perceived Access to Dental Care, Forgone Dental Care, Dental Health, ER Use, and Missed Work or School, by Awareness of Dental Care Coverage

	Awareness of dental care coverage				
	Yes		No[1]		*P*-value[2]
	Row %	95% CI	Row %	95% CI	
Ability to get dental care					<0.001
Better (n=1,929)	92.6	[90.9,94.0]	7.4	[6.0,9.1]	
Worse (n=255)	63.6	[55.6,70.8]	36.4	[29.2,44.4]	
About the same (n=1,137)	72.3	[68.7,75.6]	27.7	[24.4,31.3]	
Don't know (n=760)	51.0	[46.4,55.6]	49.0	[44.4,53.6]	
Total (n=4,081)	77.2	[75.4,78.8]	22.8	[21.2,24.6]	
Forgone dental care due to cost[3]					0.277
Yes (n=252)	64.9	[57.2,71.9]	35.1	[28.1,42.8]	
No (n=141)	71.6	[61.3,80.1]	28.4	[19.9,38.7]	
Total (n=393)	67.4	[61.3,72.9]	32.6	[27.1,38.7]	
Dental health status					<0.001
Gotten better (n=1,641)	92.3	[90.6,93.8]	7.7	[6.2,9.4]	
Stayed the same (n=1,809)	69.9	[67.0,72.7]	30.1	[27.3,33.0]	
Gotten worse (n=443)	58.9	[53.1,64.5]	41.1	[35.5,46.9]	
Don't know (n=189)	59.5	[50.3,68.0]	40.5	[32.0,49.7]	
Total (n=4,082)	77.2	[75.4,78.8]	22.8	[21.2,24.6]	
Any ER visits past 12 months					0.785
Yes (n=1,455)	77.4	[74.4,80.0]	22.6	[20.0,25.6]	
No (n=2,609)	77.1	[74.9,79.2]	22.9	[20.8,25.1]	
Don't know (n=22)	69.6	[43.6,87.2]	30.4	[12.8,56.4]	
Total (n=4,086)	77.2	[75.4,78.8]	22.8	[21.2,24.6]	
Days of school missed					0.896
None (n=94)	74.3	[62.0,83.7]	25.7	[16.3,38.0]	
1-7 days (n=50)	78.4	[58.7,90.2]	21.6	[9.8,41.3]	
More than 7 days (n=15)	76.0	[48.0,91.6]	24.0	[8.4,52.0]	
Total (n=159)	75.8	[66.4,83.2]	24.2	[16.8,33.6]	

Days of work missed					0.930
None (n=1,180)	78.4	[75.1,81.3]	21.6	[18.7,24.9]	
1-7 days (n=744)	77.9	[73.6,81.6]	22.1	[18.4,26.4]	
More than 7 days (n=384)	77.2	[71.7,82.0]	22.8	[18.0,28.3]	
Total (n=2,308)	78.0	[75.7,80.2]	22.0	[19.8,24.3]	

[1] Includes "Don't know" responses

[2] Pearson chi-square analyses

[3] Going without dental care because 'you were worried about the cost,' 'you did not have health insurance,' 'the doctor or hospital wouldn't accept your health insurance,' or 'your health plan wouldn't pay for the treatment.'

Table 22. Perceived Impact of HMP on Employment, ER Use, and Dental Health, by Perceived Access to Dental Care

	Would you say that your ability to get dental care through the Healthy Michigan Plan is better, worse, or about the same, compared to before?										
	Better		Worse		About the same		Don't know		Total		*P*-value[1]
	Col %	95% CI	Col %	95% CI	Col %	95% CI	Col %	95% CI	Col %	95% CI	
HMP helped me get a better job (n=447)											<0.001
Strongly agree (n=33)	12.0	[7.1,19.5]	4.6	[1.1,17.3]	3.8	[1.5,9.6]	4.0	[1.0,15.3]	7.7	[5.0,11.6]	
Agree (n=123)	39.2	[30.2,49.0]	17.6	[5.5,44.0]	25.6	[17.2,36.2]	10.5	[5.2,20.2]	29.2	[23.6,35.4]	
Neutral (n=103)	17.8	[12.7,24.4]	36.7	[20.0,57.3]	20.0	[12.5,30.5]	31.4	[19.0,47.1]	21.5	[17.1,26.7]	
Disagree (n=150)	24.4	[17.4,33.1]	35.8	[18.5,57.8]	44.6	[34.1,55.6]	35.7	[22.6,51.4]	33.5	[27.8,39.6]	
Strongly disagree (n=30)	5.7	[2.8,11.4]	5.3	[1.2,21.2]	4.9	[2.0,11.3]	12.0	[6.1,22.3]	6.4	[4.2,9.6]	
Don't know (n=8)	0.9	[0.3,2.9]	0		1.1	[0.2,4.9]	6.4	[1.8,20.3]	1.8	[0.8,4.0]	
Better job at work (n=2,075)											<0.001
Yes (n=1,430)	76.8	[73.2,80.0]	56.9	[46.7,66.5]	63.3	[58.2,68.1]	63.1	[56.6,69.0]	69.4	[66.8,71.8]	
No (n=548)	19.2	[16.2,22.6]	34.4	[25.5,44.4]	32.6	[28.0,37.6]	30.3	[24.8,36.5]	25.9	[23.6,28.3]	
Don't know (n=97)	4.0	[2.8,5.8]	8.7	[4.4,16.4]	4.1	[2.4,6.9]	6.6	[4.1,10.5]	4.7	[3.7,6.0]	
HMP helped me look for job (n=955)											<0.001
Strongly agree (n=158)	18.9	[14.8,23.7]	11.0	[4.7,23.3]	11.8	[7.9,17.3]	17.7	[12.0,25.5]	16.3	[13.6,19.4]	
Agree (n=388)	42.6	[37.2,48.3]	17.1	[8.6,31.3]	41.6	[34.0,49.7]	31.2	[24.2,39.1]	38.2	[34.5,42.1]	
Neutral (n=185)	17.0	[12.9,22.0]	7.6	[3.6,15.5]	21.1	[14.8,29.3]	25.2	[18.0,34.0]	19.4	[16.2,23.0]	
Disagree (n=143)	14.1	[10.5,18.7]	51.3	[33.3,69.0]	16.9	[11.7,23.8]	14.7	[8.6,24.1]	17.2	[14.1,20.9]	
Strongly disagree (n=35)	3.8	[2.1,6.9]	4.3	[1.2,14.6]	3.6	[1.7,7.6]	2.8	[1.2,6.2]	3.5	[2.4,5.2]	
Don't know (n=46)	3.6	[2.1,6.2]	8.7	[2.4,27.3]	5.0	[2.5,9.6]	8.4	[4.4,15.6]	5.4	[3.8,7.6]	

Any ER visits past 12 months (n=4,084)											0.474
Yes (n=1,452)	38.5	[35.8,41.3]	43.1	[35.4,51.1]	35.0	[31.5,38.8]	37.0	[32.7,41.5]	37.5	[35.6,39.4]	
No (n=2,609)	60.8	[58.0,63.6]	56.9	[48.9,64.6]	64.4	[60.7,68.0]	62.4	[57.9,66.7]	61.9	[60.0,63.8]	
Don't know (n=23)	0.7	[0.3,1.6]	0		0.5	[0.2,1.3]	0.6	[0.2,1.4]	0.6	[0.3,1.0]	
Dental health status (n=4,081)											<0.001
Gotten better (n=1,641)	67.9	[65.2,70.6]	14.4	[9.2,21.9]	20.9	[18.0,24.1]	7.0	[5.0,9.8]	39.6	[37.7,41.5]	
Stayed the same (n=1,807)	26.6	[24.1,29.3]	33.9	[26.8,41.8]	68.9	[65.4,72.3]	59.5	[55.0,63.9]	45.5	[43.6,47.5]	
Gotten worse (n=443)	4.5	[3.6,5.7]	46.9	[39.2,54.8]	8.8	[7.0,11.0]	15.2	[12.3,18.6]	10.4	[9.3,11.6]	
Don't know (n=190)	1.0	[0.5,1.7]	4.8	[2.6,8.7]	1.4	[0.9,2.3]	18.2	[15.0,22.0]	4.5	[3.8,5.4]	

[1] Pearson chi-square analyses

Impact of HMP Premium Contributions on Cost-Conscious Behaviors

Table 23. Healthy Michigan Plan Beneficiary Characteristics, by Federal Poverty Level

Characteristic[1]	FPL 0-35%		FPL 36-99%		FPL ≥100%		Total		*P*-value[2]
	%	95% CI	%	95% CI	%	95% CI	%	95% CI	
Age									0.035
19-34 (n=1,303)	38.1	[35.0,41.3]	40.5	[37.4,43.7]	44.0	[40.4,47.6]	40.0	[38.0,42.0]	
35-50 (n=1,301)	36.1	[33.1,39.1]	33.6	[30.7,36.6]	29.2	[26.1,32.5]	34.0	[32.1,35.9]	
51-64 (n=1,486)	25.9	[23.5,28.3]	25.9	[23.5,28.5]	26.8	[24.1,29.7]	26.0	[24.5,27.6]	
Gender									<0.001
Male (n=1,681)	57.2	[54.1,60.2]	39.1	[36.0,42.3]	39.0	[35.5,42.6]	48.4	[46.5,50.4]	
Female (n=2,409)	42.8	[39.8,45.9]	60.9	[57.7,64.0]	61.0	[57.4,64.5]	51.6	[49.6,53.5]	
Race/ethnicity									<0.001
White, non-Hispanic (n=2,714)	54.4	[51.4,57.4]	62.9	[59.9,65.9]	66.7	[63.4,69.9]	59.3	[57.3,61.1]	
Black, non-Hispanic (n=800)	32.6	[29.7,35.6]	18.2	[15.8,21.0]	19.3	[16.7,22.1]	25.9	[24.1,27.7]	
Hispanic (n=78)	1.9	[1.2,2.9]	2.4	[1.6,3.5]	2.4	[1.4,4.0]	2.1	[1.6,2.8]	
Other (n=448)	11.2	[9.3,13.3]	16.4	[14.1,19.1]	11.7	[9.5,14.3]	12.8	[11.5,14.2]	
Region									<0.001
UP/NW/NE (n=746)	6.7	[6.2,7.2]	10.9	[10.1,11.7]	12.3	[11.5,13.2]	9.0	[8.6,9.4]	
W/EC/E (n=1,265)	26.2	[25.1,27.5]	30.5	[29.1,31.9]	32.1	[30.4,33.8]	28.6	[27.8,29.4]	
SC/SW/SE (n=837)	17.4	[16.2,18.7]	19.2	[18.2,20.3]	20.6	[19.2,22.1]	18.6	[17.8,19.3]	
DET (n=1,242)	49.6	[48.1,51.2]	39.4	[37.6,41.2]	35.0	[33.3,36.7]	43.8	[42.8,44.9]	

Married or partnered									<0.001
Yes (n=1,193)	13.8	[11.9,16.0]	34.6	[31.7,37.5]	38.7	[35.4,42.2]	24.6	[23.2,26.2]	
No (n=2,880)	86.2	[84.0,88.1]	65.4	[62.5,68.3]	61.3	[57.8,64.6]	75.4	[73.8,76.8]	
Health status									<0.001
Excellent, very good, or good (n=2,826)	64.1	[61.1,66.9]	75.7	[73.1,78.2]	78.6	[75.6,81.3]	70.2	[68.5,72.0]	
Fair or poor (n=1,255)	35.9	[33.1,38.9]	24.3	[21.8,26.9]	21.4	[18.7,24.4]	29.8	[28.0,31.5]	
Any chronic health condition									<0.001
Yes (n=2,986)	72.9	[69.8,75.7]	66.2	[63.1,69.1]	63.9	[60.4,67.2]	69.2	[67.3,71.0]	
No (n=1,104)	27.1	[24.3,30.2]	33.8	[30.9,36.9]	36.1	[32.8,39.6]	30.8	[29.0,32.7]	
Any health insurance in 12 months before HMP enrollment									<0.001
Yes (n=1,667)	35.4	[32.5,38.4]	44.8	[41.7,48.0]	48.6	[45.0,52.1]	40.7	[38.8,42.6]	
No (n=2,374)	62.6	[59.6,65.6]	54.1	[50.9,57.2]	50.9	[47.3,54.4]	57.9	[55.9,59.8]	
Cost-related access barriers in 12 months before HMP enrollment[3]									0.666
Yes (n=1,341)	32.4	[29.6,35.4]	31.2	[28.4,34.2]	30.6	[27.5,33.9]	31.7	[29.9,33.6]	
No (n=2,706)	67.6	[64.6,70.4]	68.8	[65.8,71.6]	69.4	[66.1,72.5]	68.3	[66.4,70.1]	
Carefully review MIHA statements[4]									0.387
Yes (n=2,675)	88.7	[86.2,90.8]	89.1	[86.4,91.3]	86.5	[83.4,89.1]	88.3	[86.8,89.7]	
No (n=330)	11.3	[9.2,13.8]	10.9	[8.7,13.6]	13.5	[10.9,16.6]	11.7	[10.3,13.2]	
Find out about service costs[5]									0.232
Yes (n=2,912)	70.3	[67.4,73.0]	73.5	[70.7,76.1]	72.1	[68.8,75.1]	71.5	[69.7,73.3]	
No (n=1,164)	29.7	[27.0,32.6]	26.5	[23.9,29.3]	27.9	[24.9,31.2]	28.5	[26.7,30.3]	
Talk with doctor about costs[6]									0.736
Yes (n=2,746)	67.3	[64.3,70.1]	68.7	[65.7,71.6]	68.4	[65.0,71.6]	67.9	[66.0,69.7]	
No (n=1,330)	32.7	[29.9,35.7]	31.3	[28.4,34.3]	31.6	[28.4,35.0]	32.1	[30.3,34.0]	
Ask doctor about less costly drug[7]									<0.001
Yes (n=3,143)	71.6	[68.7,74.4]	79.0	[76.4,81.4]	79.3	[76.2,82.0]	75.2	[73.4,76.9]	
No (n=931)	28.4	[25.6,31.3]	21.0	[18.6,23.6]	20.7	[18.0,23.8]	24.8	[23.1,26.6]	
Check reviews or ratings of quality[8]									0.058
Yes (n=3,142)	76.4	[73.7,79.0]	79.6	[77.0,82.0]	80.4	[77.6,82.9]	78.1	[76.4,79.7]	
No (n=932)	23.6	[21.0,26.3]	20.4	[18.0,23.0]	19.6	[17.1,22.4]	21.9	[20.3,23.6]	

Fewer medical bill problems in previous 12 months of HMP enrollment[9]									0.191
Yes (n=1,629)	84.4	[80.9,87.4]	88.3	[84.6,91.2]	86.9	[82.9,90.1]	85.9	[83.7,87.9]	
No (n=240)	15.6	[12.6,19.1]	11.7	[8.8,15.4]	13.1	[9.9,17.1]	14.1	[12.1,16.3]	
Payments affordable for HMP[10]									0.015
Yes (n=3,679)	88.6	[86.4,90.5]	91.1	[88.9,92.9]	85.9	[83.2,88.2]	88.8	[87.4,90.0]	
No (n=405)	11.4	[9.5,13.6]	8.9	[7.1,11.1]	14.1	[11.8,16.8]	11.2	[10.0,12.6]	
Foregone care due to cost in previous 12 months of HMP enrollment[3]									0.589
Yes (n=439)	11.2	[9.3,13.3]	11.8	[9.9,14.1]	10.1	[8.2,12.4]	11.1	[10.0,12.5]	
No (n=3,623)	88.8	[86.7,90.7]	88.2	[85.9,90.1]	89.9	[87.6,91.8]	88.9	[87.5,90.0]	

[1] n does not sum to 4,090 for every characteristic due to skip patterns, "don't know" responses, or non-responses for individual items.

[2] pearson chi-square analyses

[3] Going without health care because 'you were worried about the cost,' 'you did not have health insurance,' 'the doctor or hospital wouldn't accept your health insurance,' or 'your health plan wouldn't pay for the treatment.'

[4] Strongly agree or agree that carefully review MIHA statements.

[5] Very or somewhat likely to find out about the costs of services before receiving them.

[6] Very or somewhat likely to talk with doctors about how much services will cost.

[7] Very or somewhat likely to ask doctors about a less costly prescription drug.

[8] Very or somewhat likely to check quality reviews or ratings before getting care.

[9] Among individuals with problems paying medical bills in the 12 months before enrolling in HMP.

[10] Strongly agree or agree that payments for HMP are affordable.

Table 24. Engagement in Cost-Conscious Behaviors among Subgroups of HMP Beneficiaries

Subgroup[2]	Outcomes[1]														
	Carefully review MIHA statements[3] (n=2,924)			Find out about service costs[4] (n=3,979)			Talk with doctor about costs[5] (n=3,978)			Ask doctor about less costly drug[6] (n=3,978)			Check reviews or ratings of quality[7] (n=3,977)		
	%	95% CI		%	95% CI		%	95% CI		%	95% CI		%	95% CI	
FPL															
0-35%	89.3	87.0	91.5	71.6	68.8	74.4	68.1	65.2	71.0	73.8*	71.0	76.6	77.8	75.2	80.4
36-99% (ref)	88.7	86.0	91.3	72.9	70.0	75.8	68.6	65.5	71.6	78.2	75.4	80.9	79.0	76.3	81.6
100+%	86.0	83.0	89.0	70.4	67.0	73.8	67.8	64.3	71.3	77.0	73.7	80.2	78.4	75.4	81.4
Gender															
Male (ref)	87.4	85.1	89.8	69.7	67.0	72.4	67.2	64.3	70.1	71.5	68.7	74.2	75.0	72.4	77.6
Female	89.2	87.3	91.1	73.6*	71.3	76.0	69.1	66.7	71.5	79.6***	77.3	81.8	81.3***	79.1	83.4

Age															
19-34 (ref)	86.2	83.5	88.9	76.9	74.0	79.8	72.0	68.9	75.1	77.6	74.6	80.6	82.3	79.5	85.0
35-50	88.2	85.5	90.9	67.0***	63.5	70.2	64.8**	61.5	68.2	72.7*	69.5	75.8	75.7**	72.7	78.8
51-64	91.4**	89.3	93.5	70.0**	67.0	73.0	66.6*	63.5	69.7	76.2	73.4	79.0	75.3**	72.6	78.1
Race/ethnicity															
White, non-Hispanic (ref)	89.1	87.3	90.9	72.7	70.2	75.2	68.8	66.2	71.3	78.9	76.5	81.2	78.4	76.1	80.7
Black, non-Hispanic	88.4	85.0	91.8	71.8	67.9	75.7	69.3	65.2	73.4	73.3*	69.4	77.2	81.3	77.9	84.7
Hispanic	83.9	73.3	94.5	51.3**	37.0	65.6	51.9*	37.8	66.0	59.9**	46.0	73.8	64.1*	50.1	78.1
Other	85.5	80.3	90.6	70.2	65.0	75.4	65.6	59.9	71.2	68.0***	62.7	73.3	72.8*	67.3	78.2
Marital status															
Not married or partnered (ref)	88.1	86.3	89.9	71.6	69.5	73.6	67.9	65.8	70.1	74.7	72.7	76.7	77.1	75.1	79.0
Married or partnered	89.4	86.8	92.1	72.2	68.7	75.7	68.9	65.3	72.6	78.3	75.0	81.7	81.6	78.8	84.4
Region															
UP/NW/NE (ref)	86.7	82.9	90.6	68.0	63.8	72.2	66.8	62.6	71.0	76.2	72.2	80.2	70.3	66.2	74.5
W/EC/E	90.2	87.8	92.5	72.2	69.2	75.2	69.6	66.5	72.6	76.7	73.8	79.6	79.8***	77.2	82.4
SC/SW/SE	87.5	84.4	90.7	71.5	67.7	75.3	67.8	64.1	71.5	78.0	74.7	81.4	79.0**	75.9	82.1
DET	88.0	85.3	90.7	72.3	69.1	75.5	67.7	64.3	71.2	73.8	70.6	77.0	78.5**	75.4	81.6
Health status															
Excellent, very good, or good (ref)	89.3	87.5	91.0	72.5	70.3	74.7	68.4	66.1	70.7	76.6	74.4	78.8	79.1	77.0	81.2
Fair or poor	86.1	82.9	89.4	69.9	66.6	73.2	67.7	64.3	71.0	73.1	69.9	76.3	76.3	73.3	79.4
Any chronic health condition															
No (ref)	86.9	83.4	90.4	74.2	70.8	77.6	70.7	67.2	74.3	75.1	71.6	78.6	81.6	78.5	84.7
Yes	89.0	87.3	90.7	70.7	68.4	72.9	67.1	64.8	69.4	75.8	73.6	77.9	76.8*	74.7	78.9
Any health insurance in 12 months before HMP enrollment															
No (ref)	88.9	87.0	90.8	70.8	68.5	73.2	69.1	66.8	71.5	75.5	73.2	77.8	76.7	74.5	78.9
Yes	87.7	85.3	90.1	73.0	70.2	75.8	66.7	63.7	69.8	75.7	72.9	78.5	80.5*	78.0	83.1
Forgone care due to cost in 12 months before HMP enrollment[8]															
No (ref)	89.2	87.5	90.9	70.1	67.9	72.4	67.9	65.6	70.2	74.5	72.4	76.7	77.5	75.4	79.5
Yes	87.0	83.8	89.8	75.0*	72.0	78.0	68.8	65.4	72.1	77.8	74.7	80.9	79.7	76.9	82.6

NOTES: * denotes *P* < 0.05, ** denotes *P* < 0.01, and *** denotes *P* < 0.001.

[1] The columns for each outcome depict marginal estimates from a logistic regression model in which the dependent variable is the respective outcome and the independent variables are all of the characteristics in the table rows.
[2] Subgroups denoted by (ref) are the reference for statistical tests.
[3] Strongly agree or agree that carefully review MIHA statements.
[4] Very or somewhat likely to find out about the costs of services before receiving them.
[5] Very or somewhat likely to talk with doctors about how much services will cost.
[6] Very or somewhat likely to ask doctors about a less costly prescription drug.
[7] Very or somewhat likely to check quality reviews or ratings before getting care.
[8] Going without health care because 'you were worried about the cost,' 'you did not have health insurance,' 'the doctor or hospital wouldn't accept your health insurance,' or 'your health plan wouldn't pay for the treatment.'

Table 25. Health Care Affordability Among Subgroups of HMP Beneficiaries

Subgroup[2]	Outcomes[1]								
	Fewer medical bill problems[3] (n=1,816)			Payments affordable[4] (n=3,982)			Forgone care due to cost[5] (n=3,967)		
	%	95% CI		%	95% CI		%	95% CI	
FPL									
0-35%	84.8	81.7	88.0	89.2	87.1	91.2	10.9	9.0	12.9
36-99% (ref)	88.3	84.7	91.9	90.8	88.7	92.3	12.0	9.7	14.2
100+%	85.3	81.1	89.5	84.9**	82.1	87.7	10.4	8.2	12.7
Gender									
Male (ref)	84.4	81.0	87.8	89.1	87.0	91.1	10.2	8.3	12.2
Female	87.0	84.5	89.6	88.5	86.8	90.3	11.9	10.2	13.6
Age									
19-34 (ref)	83.4	79.2	87.6	88.3	86.0	90.6	13.7	11.2	16.2
35-50	85.3	82.0	88.6	87.9	85.5	90.3	9.9*	8.1	11.8
51-64	89.4*	86.6	92.3	90.8	88.8	92.8	9.2**	7.3	11.1
Race/ethnicity									
White, non-Hispanic (ref)	87.4	84.7	90.1	91.7	90.3	93.2	10.3	8.8	11.8
Black, non-Hispanic	84.8	80.6	89.1	84.0***	80.7	87.3	10.5	7.7	13.3
Hispanic	91.5	79.1	100.0	86.8	87.3	95.3	18.4	7.1	29.7
Other	79.7	71.0	88.4	85.3**	80.8	89.7	14.9*	10.5	19.3

Marital status									
Not married or partnered (ref)	85.7	83.3	88.1	88.9	87.4	90.4	11.1	9.7	12.6
Married or partnered	86.2	81.7	90.6	88.6	86.0	91.3	11.1	8.6	13.6
Sampling Region									
UP/NW/NE (ref)	82.1	76.8	87.3	90.9	87.9	94.0	8.3	6.0	10.6
W/EC/E	87.8*	84.3	91.2	88.6	86.3	90.9	10.8	8.7	12.9
SC/SW/SE	86.4	82.2	90.7	88.9	86.3	91.4	11.3	8.9	13.8
DET	85.1	81.4	88.8	88.6	86.4	90.8	11.9*	9.5	14.2
Health status									
Excellent, very good, or good (ref)	87.4	84.8	90.0	90.0	88.4	91.6	10.2	8.7	11.7
Fair or poor	83.2	79.5	86.8	85.8**	83.0	88.6	13.1*	10.6	15.6
Any chronic health condition									
No (ref)	85.7	80.7	90.7	88.4	85.7	91.0	7.7	5.6	9.8
Yes	85.8	83.4	88.3	89.0	87.4	90.6	12.5**	10.9	14.2
Any health insurance in 12 months before HMP enrollment									
No (ref)	86.9	84.5	89.4	89.8	88.3	91.4	9.7	8.2	11.2
Yes	83.3	79.4	87.3	87.3	84.9	89.6	13.4**	11.2	15.6
Forgone care due to cost in 12 months before HMP enrollment[6]									
No (ref)	83.2	80.2	86.2	89.6	88.1	91.0	8.1	6.8	9.5
Yes	88.8**	85.9	91.7	87.0	84.2	89.8	17.6***	14.8	20.5

NOTES: * denotes $P < 0.05$, ** denotes $P < 0.01$, and *** denotes $P < 0.001$.

[1] The columns for each outcome depict marginal estimates from a logistic regression model in which the dependent variable is the respective outcome and the independent variables are all of the characteristics in the table rows.

[2] Subgroups denoted by (ref) are the reference for statistical tests.

[3] Among individuals with problems paying medical bills in the 12 months before enrolling in HMP.

[4] Strongly agree or agree that payments for HMP are affordable.

[5] Going without health care in the previous 12 months of HMP enrollment because ‘you were worried about the cost,’ ‘you did not have health insurance,’ ‘the doctor or hospital wouldn’t accept your health insurance,’ or ‘your health plan wouldn’t pay for the treatment.’

[6] Going without health care in the 12 months before HMP enrollment because ‘you were worried about the cost,’ ‘you did not have health insurance,’ ‘the doctor or hospital wouldn’t accept your health insurance,’ or ‘your health plan wouldn’t pay for the treatment.’

Reproductive Health

Table 26. Characteristics of Reproductive Age Females

	Col %	95% CI
Age (n=1,168)		
19-34 (n=754)	68.1	[64.8,71.3]
35-45 (n=414)	31.9	[28.7,35.2]
Race (n=1,162)		
White (n=769)	61.7	[58.2,65.2]
Black or African American (n=254)	24.9	[21.9,28.2]
Other (n=90)	8.5	[6.7,10.6]
More than one (n=49)	4.9	[3.4,6.8]
FPL (n=1,168)		
0-35% (n=312)	40.1	[36.8,43.6]
36-99% (n=490)	34.5	[31.8,37.4]
≥100% (n=366)	25.3	[23.0,27.7]
Married or partnered (n=1,166)		
Yes (n=337)	23.7	[21.2,26.4]
No (n=829)	76.3	[73.6,78.8]
Health status (n=1,168)		
Excellent, very good, or good (n=905)	76.5	[73.4,79.4]
Fair or poor (n=263)	23.5	[20.6,26.6]
Health insurance in 12 months before HMP enrollment (n=1,167)		
Insured all 12 months (n=434)	36.4	[33.1,39.9]
Insured less than 12 months (n=129)	12.0	[9.7,14.6]
Not insured (n=570)	48.4	[44.9,52.0]
Don't know (n=34)	3.2	[2.1,4.8]
PCP visit in the past 12 months (n=1,168)		
Yes (n=947)	80.4	[77.5,83.0]
No (n=221)	19.6	[17.0,22.5]

Table 27. Healthy Michigan Plan Beneficiary Characteristics and Ability to Get Birth Control/Family Planning Services

	Would you say that your ability to get birth control/family planning services through the Healthy Michigan Plan is better, worse, or about the same, compared to before?								
	Better		Worse		About the same		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Age									<0.001
19-34 (n=753)	40.9	[36.6,45.3]	1.9	[1.0,3.5]	26.9	[23.3,30.9]	30.3	[26.3,34.6]	
35-45 (n=413)	24.1	[19.4,29.5]	0.3	[0.0,2.4]	20.2	[15.4,26.0]	55.4	[49.3,61.4]	
Total (n=1,166)	35.5	[32.2,39.0]	1.4	[0.7,2.5]	24.8	[21.8,28.0]	38.4	[34.9,41.9]	
Race									0.224
White (n=767)	34.4	[30.4,38.7]	1.9	[1.0,3.6]	23.0	[19.6,26.8]	40.7	[36.4,45.2]	
Black or African American (n=254)	35.3	[28.3,43.0]	0.4	[0.1,3.1]	29.4	[23.1,36.7]	34.8	[27.9,42.3]	
Other (n=90)	48.0	[36.4,59.8]	0		25.7	[16.5,37.5]	26.3	[17.4,37.7]	
More than one (n=49)	32.9	[19.5,49.7]	2.5	[0.4,16.1]	24.7	[11.8,44.7]	39.9	[24.3,57.8]	
Total (n=1,160)	35.7	[32.4,39.2]	1.4	[0.8,2.5]	24.9	[22.0,28.1]	38.0	[34.5,41.5]	
FPL									0.280
0-35% (n=311)	34.8	[28.7,41.4]	1.9	[0.8,4.7]	21.4	[16.1,27.7]	41.9	[35.3,48.8]	
36-99% (n=490)	36.9	[32.0,42.2]	0.5	[0.2,1.8]	26.2	[22.0,30.8]	36.3	[31.6,41.3]	
≥100% (n=365)	34.7	[29.4,40.4]	1.7	[0.7,4.1]	28.2	[23.3,33.6]	35.5	[30.2,41.1]	
Total (n=1,166)	35.5	[32.2,39.0]	1.4	[0.7,2.5]	24.8	[21.8,28.0]	38.4	[34.9,41.9]	
Married or partnered									0.890
Yes (n=337)	34.1	[28.6,40.1]	1.1	[0.4,2.9]	25.3	[20.3,30.9]	39.6	[34.0,45.5]	
No (n=827)	36.1	[32.1,40.2]	1.5	[0.7,3.0]	24.7	[21.2,28.5]	37.8	[33.7,42.1]	
Total (n=1,164)	35.6	[32.3,39.1]	1.4	[0.8,2.5]	24.8	[21.9,28.0]	38.2	[34.8,41.8]	
Health status									0.114
Excellent, very good, or good (n=903)	35.3	[31.6,39.2]	1.0	[0.5,1.9]	26.4	[23.0,30.1]	37.3	[33.4,41.4]	
Fair or poor (n=263)	36.2	[29.1,43.8]	2.6	[0.9,7.3]	19.5	[14.4,25.9]	41.7	[34.7,49.0]	
Total (n=1,166)	35.5	[32.2,39.0]	1.4	[0.7,2.5]	24.8	[21.8,28.0]	38.4	[34.9,41.9]	
Health insurance in 12 months before HMP enrollment									<0.001
Insured all 12 months (n=434)	27.5	[22.3,33.2]	2.5	[1.1,5.5]	35.3	[30.2,40.9]	34.7	[29.4,40.3]	
Insured less than 12 months (n=127)	33.8	[24.4,44.7]	1.0	[0.1,6.5]	21.9	[14.5,31.8]	43.3	[33.0,54.2]	
Not insured (n=570)	42.5	[37.6,47.5]	0.5	[0.2,1.3]	17.9	[14.1,22.6]	39.1	[34.1,44.2]	
Don't know (n=34)	28.2	[11.9,53.2]	3.1	[0.4,19.4]	18.7	[8.5,36.1]	50.0	[29.4,70.6]	
Total (n=1,165)	35.5	[32.2,39.0]	1.4	[0.8,2.5]	24.8	[21.9,28.0]	38.3	[34.9,41.8]	

PCP visit in the past 12 months									0.376
Yes (n=945)	36.8	[33.0,40.7]	1.2	[0.6,2.2]	24.8	[21.5,28.4]	37.2	[33.4,41.2]	
No (n=221)	30.2	[23.6,37.8]	2.1	[0.6,7.7]	24.7	[18.7,31.7]	43.0	[35.4,50.9]	
Total (n=1,166)	35.5	[32.2,39.0]	1.4	[0.7,2.5]	24.8	[21.8,28.0]	38.4	[34.9,41.9]	

[1] Pearson chi-square analyses

Impact on Those with Chronic Health Conditions

Table 28. Functional Limitations Among Those with Chronic Conditions

	Functional Limitations				
	Yes		No		*P*-value[1]
	Row %	95% CI	Row %	95% CI	
Physical Chronic Disease					<0.001
Yes (n=2,590)	24.8	[22.8,26.9]	75.2	[73.1,77.2]	
No (n=1,436)	9.1	[7.2,11.5]	90.9	[88.5,92.8]	
Total (n=4,026)	18.4	[17.0,20.0]	81.6	[80.0,83.0]	
Mood Disorder or Mental Health Condition					<0.001
Yes (n=1,279)	35.3	[32.1,38.7]	64.7	[61.3,67.9]	
No (n=2,747)	10.9	[9.5,12.5]	89.1	[87.5,90.5]	
Total (n=4,026)	18.4	[17.0,20.0]	81.6	[80.0,83.0]	
Any Chronic Disease or Mood Disorder					<0.001
Yes (n=2,885)	24.4	[22.5,26.4]	75.6	[73.6,77.5]	
No (n=1,141)	5.8	[4.1,8.3]	94.2	[91.7,95.9]	
Total (n=4,026)	18.4	[17.0,20.0]	81.6	[80.0,83.0]	

[1] Pearson chi-square analyses

Table 29. Healthy Michigan Plan Beneficiary Characteristics Among Those with Chronic Disease and Among Those with Functional Limitations

	Any Chronic Disease or Mood Disorder		Functional Limitations	
	Col %	95% CI	Col %	95% CI
Age (n=4,090)				
19-34 (n=1,303)	32.5	[30.3,34.8]	23.5	[19.5,28.1]
35-50 (n=1,301)	36.7	[34.5,39.0]	40.2	[35.9,44.7]
51-64 (n=1,486)	30.8	[28.9,32.8]	36.3	[32.2,40.5]

Gender (n=4,090)				
Male (n=1,681)	46.7	[44.4,49.0]	50.6	[46.1,55.1]
Female (n=2,409)	53.3	[51.0,55.6]	49.4	[44.9,53.9]
Race (n=4,039)				
White (n=2,784)	64.4	[62.2,66.6]	63.7	[59.0,68.1]
Black/African American (n=807)	24.8	[22.8,26.9]	23.6	[19.7,28.0]
Other (n=306)	6.8	[5.7,8.0]	8.0	[5.6,11.1]
More than one (n=142)	4.0	[3.1,5.1]	4.8	[3.2,7.0]
Hispanic/Latino (n=4,056)				
Yes (n=188)	4.7	[3.8,5.9]	6.1	[4.0,9.3]
No (n=3,856)	94.7	[93.5,95.7]	93.5	[90.3,95.8]
Don't Know (n=12)	0.6	[0.3,1.2]	0.4	[0.1,2.6]
Arab, Chaldean, Middle Eastern (n=4,055)				
Yes (n=204)	3.8	[3.0,4.8]	3.8	[2.3,6.3]
No (n=3,842)	95.8	[94.8,96.7]	95.9	[93.4,97.5]
Don't Know (n=9)	0.3	[0.2,0.7]	0.3	[0.0,1.9]
Marital status (n=4,073)				
Not married or partnered (n=2,880)	75.6	[73.7,77.3]	78.0	[74.2,81.4]
Married or partnered (n=1,193)	24.4	[22.7,26.3]	22.0	[18.6,25.8]
Health status (n=4,081)				
Excellent (n=337)	4.5	[3.7,5.6]	1.5	[0.7,3.1]
Very good (n=1,041)	19.5	[17.6,21.5]	8.3	[5.7,11.9]
Good (n=1,448)	37.1	[34.9,39.4]	20.9	[17.6,24.7]
Fair (n=931)	28.3	[26.3,30.4]	37.7	[33.4,42.2]
Poor (n=324)	10.5	[9.2,12.0]	31.6	[27.5,35.9]
Physical health not good any days in past 30 days (n=4,090)				
Yes (n=2,082)	58.0	[55.7,60.3]	88.0	[84.5,90.8]
No (n=2,008)	42.0	[39.7,44.3]	12.0	[9.2,15.5]
Mental health not good any days in past 30 days (n=4,090)				
Yes (n=1,635)	49.1	[46.8,51.4]	75.1	[71.2,78.7]
No (n=2,455)	50.9	[48.6,53.2]	24.9	[21.3,28.8]

Table 30. Access to Care Prior to HMP Enrollment Among Those With Chronic Disease

	Any Chronic Disease or Mood Disorder		Physical Chronic Disease		Mood Disorder or Mental Health Condition		Functional Limitations	
	Col %	95% CI	Col %	95% CI	Col %	95% CI	Col %	95% CI
Any health insurance in 12 months before HMP enrollment (n=4,087)								
Yes (n=1,667)	40.8	[38.5,43.0]	40.3	[38.0,42.7]	44.0	[40.6,47.6]	41.1	[36.8,45.7]
No (n=2,374)	58.3	[56.0,60.5]	58.7	[56.4,61.1]	55.0	[51.5,58.5]	57.1	[52.6,61.6]
Don't Know (n=46)	1.0	[0.6,1.5]	1.0	[0.6,1.6]	0.9	[0.5,1.7]	1.7	[0.7,4.3]
Insurance duration before HMP enrollment (n=1,667)								
All 12 months (n=1,235)	74.9	[71.7,77.9]	75.2	[71.9,78.3]	74.5	[69.5,78.9]	66.4	[59.2,72.9]
6-11 months (n=245)	14.4	[12.1,17.2]	14.3	[11.9,17.1]	14.1	[10.8,18.2]	17.6	[12.7,23.8]
Less than 6 months (n=129)	6.7	[5.2,8.5]	6.8	[5.2,8.8]	6.5	[4.4,9.6]	11.0	[6.9,17.0]
Don't know (n=58)	4.0	[2.8,5.8]	3.6	[2.5,5.3]	4.9	[2.9,8.2]	5.0	[2.7,9.3]
Problems paying medical bills before HMP enrollment (n=4,085)								
Yes (n=1,869)	51.7	[49.4,54.0]	52.9	[50.5,55.3]	52.7	[49.2,56.2]	59.4	[54.9,63.8]
No (n=2,196)	47.9	[45.6,50.2]	46.8	[44.4,49.2]	47.0	[43.5,50.5]	40.0	[35.6,44.5]
Don't Know (n=20)	0.4	[0.2,0.7]	0.3	[0.1,0.7]	0.3	[0.1,0.8]	0.6	[0.2,1.7]
Didn't get care needed before HMP enrollment (n=4,084)								
Yes (n=1,409)	38.4	[36.2,40.7]	39.2	[36.8,41.5]	41.8	[38.4,45.2]	47.3	[42.8,51.9]
No (n=2,638)	60.6	[58.4,62.9]	59.8	[57.5,62.2]	57.5	[54.1,60.9]	51.8	[47.3,56.3]
Don't Know (n=37)	1.0	[0.6,1.5]	1.0	[0.6,1.6]	0.7	[0.4,1.3]	0.9	[0.3,2.4]
PCP visit timing before HMP enrollment (n=4,086)								
Less than 1 year before HMP (n=1,647)	42.1	[39.8,44.4]	41.9	[39.6,44.3]	45.6	[42.1,49.1]	40.4	[36.1,44.9]
1 to 5 years (n=1,577)	36.2	[34.0,38.4]	36.0	[33.8,38.4]	35.1	[31.9,38.4]	36.8	[32.6,41.3]
More that 5 years (n=813)	20.4	[18.6,22.5]	20.7	[18.7,22.8]	18.7	[16.0,21.6]	21.5	[17.9,25.6]
Don’t Know (n=49)	1.3	[0.8,2.0]	1.3	[0.8,2.1]	0.7	[0.4,1.3]	1.3	[0.6,2.5]

Table 31. Impact of HMP on Chronic Disease Care Access and Function Among Enrollees With Chronic Illness

	Any Chronic Disease or Mood Disorder		Physical Chronic Disease		Mood Disorder or Mental Health Condition		Functional Limitations	
	Col %	95% CI	Col %	95% CI	Col %	95% CI	Col %	95% CI
Ability to get mental health care (n=4,084)								
Better (n=1,077)	32.2	[30.0,34.4]	29.7	[27.5,32.0]	46.4	[42.9,49.9]	36.2	[31.9,40.7]
Worse (n=97)	3.4	[2.7,4.4]	2.9	[2.2,3.9]	6.2	[4.7,8.2]	8.1	[5.9,11.1]
About the same (n=923)	22.1	[20.2,24.1]	21.4	[19.5,23.4]	27.1	[24.1,30.4]	21.4	[17.9,25.3]
Don't know (n=1,987)	42.3	[40.1,44.6]	46	[43.6,48.4]	20.2	[17.6,23.1]	34.3	[30.2,38.6]
Ability to get prescription meds (n=4,085)								
Better (n=2,497)	64.6	[62.3,66.8]	64.6	[62.3,66.9]	67.6	[64.3,70.7]	66.7	[62.3,70.9]
Worse (n=121)	3.9	[3.0,4.9]	4.0	[3.1,5.2]	4.5	[3.2,6.1]	7.0	[4.9,9.8]
About the same (n=1,017)	24.6	[22.6,26.6]	24.6	[22.6,26.8]	23.5	[20.7,26.6]	22.0	[18.4,26.1]
Don't know (n=450)	7.0	[5.9,8.3]	6.8	[5.6,8.1]	4.4	[3.2,6.1]	4.3	[2.8,6.6]
Ability to pay medical bills (n=1,869)								
Gotten worse (n=51)	3.1	[2.2,4.4]	3.3	[2.3,4.6]	4.2	[2.6,6.6]	5.5	[3.3,9.1]
Stayed the same (n=176)	9.8	[8.0,11.9]	9.7	[7.8,12.0]	9.5	[7.0,12.7]	13.5	[9.6,18.7]
Gotten better (n=1,629)	86.3	[83.8,88.4]	86.6	[84.1,88.7]	85.0	[81.1,88.2]	80.0	[74.4,84.6]
Don't know (n=13)	0.9	[0.4,2.1]	0.5	[0.2,1.1]	1.4	[0.4,4.2]	1.0	[0.3,3.3]
Physical health status (n=4,086)								
Gotten better (n=1,961)	51.9	[49.6,54.2]	52.9	[50.5,55.3]	50.2	[46.7,53.6]	41.5	[37.1,46.0]
Stayed the same (n=1,851)	40.3	[38.0,42.6]	38.5	[36.2,40.8]	39.0	[35.6,42.5]	38.6	[34.2,43.2]
Gotten worse (n=256)	7.5	[6.4,8.6]	8.2	[7.1,9.5]	10.3	[8.6,12.4]	19.1	[16.0,22.6]
Don't know (n=18)	0.4	[0.2,0.7]	0.4	[0.2,0.7]	0.5	[0.2,1.3]	0.8	[0.3,1.9]
Mental health status (n=4,080)								
Gotten better (n=1,550)	42.4	[40.1,44.7]	40.8	[38.4,43.2]	48.7	[45.2,52.2]	34.9	[30.7,39.3]
Stayed the same (n=2,318)	50.9	[48.6,53.2]	52.8	[50.4,55.2]	40.1	[36.7,43.6]	47.0	[42.5,51.6]
Gotten worse (n=186)	6.1	[5.1,7.4]	5.7	[4.7,6.9]	10.8	[8.8,13.2]	17.1	[13.8,20.9]
Don't know (n=26)	0.6	[0.4,0.9]	0.7	[0.4,1.1]	0.4	[0.2,0.8]	1.1	[0.5,2.1]

Table 32. Opportunities for Improvement of Chronic Disease Care in HMP

	Any Chronic Disease or Mood Disorder		Physical Chronic Disease		Mood Disorder or Mental Health Condition		Functional Limitations	
	Col %	95% CI	Col %	95% CI	Col %	95% CI	Col %	95% CI
Foregone care in past 12 months (n=4,084)								
Yes (n=629)	18.4	[16.6,20.3]	17.7	[15.9,19.6]	22.5	[19.8,25.6]	27.8	[23.8,32.1]
No (n=3,433)	81.4	[79.5,83.1]	82.1	[80.1,83.8]	77.2	[74.2,80.0]	72.0	[67.6,76.0]
Don't Know (n=22)	0.2	[0.1,0.4]	0.2	[0.1,0.5]	0.2	[0.1,0.6]	0.2	[0.1,0.7]
Foregone care because worried about cost (n=629)								
Yes (n=155)	25.7	[21.2,30.8]	25.3	[20.6,30.8]	28.8	[22.7,35.7]	26.8	[19.7,35.3]
No (n=474)	74.3	[69.2,78.8]	74.7	[69.2,79.4]	71.2	[64.3,77.3]	73.2	[64.7,80.3]
Foregone care because no insurance (n=629)								
Yes (n=41)	8.9	[5.8,13.3]	6.8	[4.3,10.6]	9.0	[4.8,16.2]	8.8	[4.0,18.2]
No (n=588)	91.1	[86.7,94.2]	93.2	[89.4,95.7]	91.0	[83.8,95.2]	91.2	[81.8,96.0]
Foregone care because insurance not accepted (n=629)								
Yes (n=141)	23.7	[19.1,28.9]	25.1	[20.2,30.9]	24.6	[18.7,31.5]	23.2	[16.4,31.8]
No (n=488)	76.3	[71.1,80.9]	74.9	[69.1,79.8]	75.4	[68.5,81.3]	76.8	[68.2,83.6]
Foregone care because health plan wouldn't pay (n=629)								
Yes (n=251)	38.5	[33.4,43.9]	39.6	[34.2,45.4]	34.9	[28.5,42.0]	37.9	[29.7,47.0]
No (n=378)	61.5	[56.1,66.6]	60.4	[54.6,65.8]	65.1	[58.0,71.5]	62.1	[53.0,70.3]
Foregone care because couldn’t get an appointment soon enough (n=630)								
Yes (n=73)	10.0	[7.4,13.5]	10.4	[7.6,14.1]	11.5	[7.7,16.8]	15.6	[10.2,23.1]
No (n=557)	90.0	[86.5,92.6]	89.6	[85.9,92.4]	88.5	[83.2,92.3]	84.4	[76.9,89.8]
Forgone care because no transportation (n=629)								
Yes (n=30)	6.7	[4.1,10.6]	5.2	[3.2,8.6]	9.9	[5.8,16.5]	9.2	[5.2,15.7]
No (n=599)	93.3	[89.4,95.9]	94.8	[91.4,96.8]	90.1	[83.5,94.2]	90.8	[84.3,94.8]
Foregone checkup due to cost[1] (n=393)								
Yes (n=47)	13.9	[9.7,19.6]	12.9	[9.0,18.3]	16.5	[10.2,25.4]	13.1	[7.7,21.5]
No (n=346)	86.1	[80.4,90.3]	87.1	[81.7,91.0]	83.5	[74.6,89.8]	86.9	[78.5,92.3]
Forgone specialty care due to cost[2] (n=393)								
Yes (n=79)	24.5	[18.7,31.4]	25.7	[19.6,32.9]	26.0	[18.1,35.7]	33.8	[23.0,46.5]
No (n=314)	75.5	[68.6,81.3]	74.3	[67.1,80.4]	74.0	[64.3,81.9]	66.2	[53.5,77.0]

PCP visit in the past 12 months								
Yes (n=3,386)	89.6	[87.8,91.1]	90.5	[88.7,92.0]	90.1	[87.3,92.4]	92.4	[88.8,94.9]
No (n=453)	10.2	[8.7,12.0]	9.3	[7.8,11.0]	9.7	[7.5,12.6]	7.2	[4.7,10.8]
Don't Know (n=12)	0.2	[0.1,0.5]	0.3	[0.1,0.6]	0.1	[0.0,0.5]	0.4	[0.1,1.5]
Regular place of care before HMP enrollment (n=4,084)								
Yes (n=3,051)	77.2	[75.1,79.1]	77.2	[75.0,79.2]	78.3	[75.3,80.9]	75.1	[70.8,78.9]
No (n=955)	21.6	[19.7,23.6]	21.5	[19.5,23.6]	21.2	[18.5,24.1]	22.0	[18.4,26.1]
NA (n=73)	1.1	[0.7,1.7]	1.2	[0.8,1.8]	0.5	[0.2,1.2]	2.6	[1.4,4.9]
Don't know (n=5)	0.1	[0.0,0.4]	0.2	[0.1,0.5]	0.1	[0.0,0.6]	0.3	[0.1,1.4]
Regular place of care before HMP enrollment--location (n=3,051)								
Clinic (n=557)	17.4	[15.5,19.4]	17.5	[15.5,19.6]	16.2	[13.5,19.4]	17.3	[13.3,22.1]
Doctor's office (n=1,498)	47.3	[44.7,49.9]	47.0	[44.3,49.7]	49.9	[45.9,53.9]	46.8	[41.7,51.9]
Urgent care/walk-in (n=529)	16.1	[14.3,18.1]	16.3	[14.4,18.4]	14.5	[12.1,17.3]	13.0	[10.3,16.4]
Emergency room (n=409)	17.3	[15.3,19.5]	17.5	[15.4,19.8]	16.8	[14.0,20.0]	19.9	[16.0,24.5]
Other place (n=56)	1.8	[1.3,2.6]	1.7	[1.1,2.5]	2.5	[1.5,4.0]	3.0	[1.7,5.4]
Don't know (n=2)	0.1	[0.0,0.3]	0.1	[0.0,0.4]	0.1	[0.0,0.7]	0	
Regular place of care past 12 months (n=4,088)								
Yes (n=3,850)	95.2	[93.8,96.3]	96.0	[94.7,97.0]	94.7	[92.4,96.4]	93.2	[89.4,95.7]
No (n=194)	4.1	[3.1,5.4]	3.5	[2.6,4.8]	4.4	[2.9,6.4]	5.0	[2.9,8.3]
NA (n=44)	0.7	[0.4,1.4]	0.5	[0.3,0.9]	0.9	[0.3,2.6]	1.8	[0.7,4.9]
Regular place of care past 12 months--location (n=3,850)								
Clinic (n=640)	16.0	[14.3,17.8]	16.5	[14.7,18.4]	14.4	[12.2,16.9]	17.3	[14.0,21.1]
Doctor's office (n=2,934)	77.1	[75.0,79.0]	76.7	[74.6,78.8]	79.7	[76.8,82.4]	75.9	[71.6,79.8]
Urgent care/walk-in (n=181)	4.8	[3.8,6.0]	4.6	[3.5,5.9]	3.8	[2.6,5.6]	4.1	[2.3,7.0]
Emergency room (n=65)	1.5	[1.1,2.2]	1.6	[1.1,2.3]	1.2	[0.8,2.1]	1.7	[0.8,3.4]
Other place (n=29)	0.6	[0.4,1.0]	0.6	[0.3,1.0]	0.8	[0.4,1.7]	1.1	[0.4,2.8]
Don't know (n=1)			0		0		0	

[1] Going without a checkup because ‘you were worried about the cost,’ ‘you did not have health insurance,’ ‘the doctor or hospital wouldn’t accept your health insurance,’ or ‘your health plan wouldn’t pay for the treatment.’

[2] Going without specialty care because ‘you were worried about the cost,’ ‘you did not have health insurance,’ ‘the doctor or hospital wouldn’t accept your health insurance,’ or ‘your health plan wouldn’t pay for the treatment.’

Impact on Those with Mood Disorder and Substance Use Disorder

Table 33. Regular Source of Care Prior to HMP Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	In the 12 months before enrolling in the Healthy Michigan Plan, was there a place that you usually would go to for a checkup, when you felt sick, or when you wanted advice about your health?								
	Yes		No		NA		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder									0.002
Yes (n=1,287)	78.0	[75.0,80.7]	21.4	[18.7,24.4]	0.5	[0.2,1.2]	0.1	[0.0,0.6]	
No (n=2,781)	71.9	[69.6,74.0]	25.2	[23.2,27.4]	2.7	[2.0,3.7]	0.2	[0.1,0.5]	
Don't know (n=10)	100.0		0		0		0		
Total (n=4,078)	73.8	[72.1,75.5]	24.0	[22.3,25.7]	2.1	[1.5,2.8]	0.1	[0.1,0.4]	
Substance use disorder									0.650
Yes (n=165)	79.6	[70.9,86.3]	20.0	[13.5,28.8]	0.3	[0.0,2.3]	0		
No (n=3,910)	73.5	[71.7,75.2]	24.2	[22.5,26.0]	2.1	[1.6,2.9]	0.2	[0.1,0.4]	
Don't know (n=7)	87.9	[43.9,98.5]	12.1	[1.5,56.1]	0		0		
Total (n=4,082)	73.8	[72.0,75.5]	24.0	[22.4,25.8]	2.1	[1.5,2.8]	0.1	[0.1,0.4]	

[1] Pearson chi-square analyses

Table 34. Type of Regular Source of Care Prior to HMP Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	[If Yes-Regular source of care prior to HMP] What kind of place was it?												
	Clinic		Doctor's office		Urgent care/walk-in		Emergency room		Other place		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder													0.117
Yes (n=1,013)	16.0	[13.3,19.0]	49.9	[45.9,53.9]	14.5	[12.1,17.4]	17.0	[14.2,20.3]	2.5	[1.5,4.1]	0.1	[0.0,0.7]	
No (n=2,026)	17.8	[15.8,20.1]	47.0	[44.2,49.8]	18.0	[15.9,20.3]	15.7	[13.7,18.0]	1.4	[1.0,2.2]	0	[0.0,0.3]	
Don't know (n=10)	3.1	[0.4,20.8]	54.6	[20.1,85.2]	0		42.3	[13.2,78.0]	0		0		
Total (n=3,049)	17.2	[15.5,18.9]	48.0	[45.7,50.3]	16.8	[15.2,18.5]	16.3	[14.6,18.1]	1.8	[1.3,2.4]	0.1	[0.0,0.2]	

Substance use disorder													<0.001
Yes (n=131)	12.2	[7.4,19.5]	32.9	[23.1,44.4]	16.1	[9.6,25.9]	37.0	[27.1,48.1]	1.1	[0.2,4.6]	0.7	[0.1,5.0]	
No (n=2,913)	17.4	[15.7,19.3]	48.6	[46.2,50.9]	16.8	[15.2,18.7]	15.3	[13.6,17.2]	1.8	[1.3,2.5]	0	[0.0,0.2]	
Don't know (n=6)	0		100.0		0		0		0		0		
Total (n=3,050)	17.2	[15.5,18.9]	48.0	[45.7,50.3]	16.8	[15.1,18.5]	16.2	[14.6,18.1]	1.8	[1.3,2.4]	0.1	[0.0,0.2]	

[1] Pearson chi-square analyses

Table 35. Regular Source of Care with HMP Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	In the last 12 months, is there a place you usually go when you need a checkup, feel sick, or want advice about your health?						
	Yes		No		NA		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder							0.028
Yes (n=1,288)	95.2	[93.0,96.7]	3.9	[2.6,5.7]	0.9	[0.3,2.6]	
No (n=2,784)	90.9	[89.1,92.4]	7.3	[6.0,8.9]	1.8	[1.2,2.9]	
Don't know (n=10)	93.9	[64.8,99.2]	0		6.1	[0.8,35.2]	
Total (n=4,082)	92.2	[90.8,93.4]	6.2	[5.2,7.4]	1.6	[1.1,2.4]	
Substance use disorder							0.803
Yes (n=165)	94.0	[85.2,97.7]	6.0	[2.3,14.8]	0		
No (n=3,914)	92.1	[90.7,93.3]	6.2	[5.2,7.5]	1.6	[1.1,2.5]	
Don't know (n=7)	100.0		0		0		
Total (n=4,086)	92.2	[90.8,93.4]	6.2	[5.2,7.4]	1.6	[1.0,2.4]	

[1] Pearson chi-square analyses

Table 36. Type of Regular Source of Care with HMP Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	[If Yes-Regular source of care with HMP] What kind of place was it?												
	Clinic		Doctor's office		Urgent care/walk-in		Emergency room		Other place		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder													0.058
Yes (n=1,245)	14.6	[12.3,17.1]	79.5	[76.6,82.1]	3.9	[2.6,5.6]	1.3	[0.8,2.1]	0.8	[0.4,1.7]	0		
No (n=2,590)	17.4	[15.6,19.4]	73.2	[70.9,75.4]	6.7	[5.4,8.2]	1.9	[1.4,2.6]	0.8	[0.5,1.3]	0	[0.0,0.3]	
Don't know (n=9)	0		96.7	[77.8,99.6]	3.3	[0.4,22.2]	0		0		0		
Total (n=3,844)	16.5	[15.0,18.0]	75.2	[73.4,77.0]	5.8	[4.8,6.9]	1.7	[1.3,2.2]	0.8	[0.5,1.2]	0	[0.0,0.2]	
Substance use disorder													0.815
Yes (n=159)	17.4	[11.0,26.4]	71.2	[61.0,79.6]	5.8	[2.0,15.5]	3.6	[1.4,9.0]	2.0	[0.6,7.3]	0		
No (n=3,682)	16.5	[15.0,18.1]	75.4	[73.5,77.1]	5.8	[4.8,6.9]	1.6	[1.2,2.1]	0.7	[0.5,1.1]	0	[0.0,0.2]	
Don't know (n=7)	6.8	[0.8,39.7]	93.2	[60.3,99.2]	0		0		0		0		
Total (n=3,848)	16.5	[15.1,18.1]	75.2	[73.4,77.0]	5.8	[4.8,6.9]	1.7	[1.3,2.2]	0.8	[0.5,1.2]	0	[0.0,0.2]	

[1] Pearson chi-square analyses

Table 37. Emergency Room Use in Past 12 Months Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	Any ER visits past 12 months						
	Yes		No		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder							<0.001
Yes (n=1,288)	50.5	[47.0,54.0]	48.1	[44.6,51.6]	1.4	[0.7,2.8]	
No (n=2,786)	31.9	[29.7,34.2]	67.9	[65.6,70.1]	0.2	[0.1,0.5]	
Don't know (n=10)	61.5	[23.3,89.4]	38.5	[10.6,76.7]	0		
Total (n=4,084)	37.7	[35.8,39.6]	61.8	[59.8,63.7]	0.6	[0.3,1.0]	
Substance use disorder							<0.001
Yes (n=165)	60.4	[50.7,69.3]	38.7	[29.9,48.4]	0.9	[0.1,5.9]	
No (n=3,916)	36.6	[34.7,38.5]	62.9	[60.9,64.8]	0.6	[0.3,1.0]	
Don't know (n=7)	88.3	[56.5,97.8]	11.7	[2.2,43.5]	0		
Total (n=4,088)	37.7	[35.8,39.6]	61.8	[59.8,63.7]	0.6	[0.3,1.0]	

[1] Pearson chi-square analyses

Table 38. Factors Associated with ER Use Among HMP Enrollees

	Outcome: Emergency Room Visit in Past 12 Months		
	aOR	95% CI	*P*-value
Predictors:			
Age	0.979	[0.9716, 0.98549]	0.001
FPL	0.998	[0.9958, 0.99922]	0.004
Hypertension diagnosis[1]	1.795	[1.485, 2.16907]	0.001
Stroke diagnosis[1]	1.999	[1.1728, 3.40759]	0.011
Asthma diagnosis[1]	1.507	[1.2104, 1.87552]	0.001
COPD diagnosis[1]	2.118	[1.6104, 2.78609]	0.001
Substance use disorder diagnosis[1]	2.395	[1.5293, 3.74951]	0.001

aOR = adjusted odds ratio; CI = confidence interval; HMP = Healthy Michigan Plan

NOTE: The odds ratios presented here represent the results of a single logistic regression model adjusting for age, FPL, and presence or absence of the listed diagnoses.

[1] Diagnoses were dichotomized as not present (0) vs. present (1).

Table 39. Perceived Access to Mental Health Care Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	Would you say that your ability to get mental health care through the Healthy Michigan Plan is better, worse, or about the same, compared to before?								
	Better		Worse		About the same		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder									<0.001
Yes (n=1,287)	46.2	[42.7,49.7]	6.3	[4.8,8.3]	27.2	[24.1,30.5]	20.3	[17.6,23.2]	
No (n=2,781)	19.4	[17.5,21.5]	0.8	[0.5,1.2]	21.6	[19.6,23.7]	58.2	[55.8,60.6]	
Don't know (n=10)	7.2	[1.5,28.4]	0		24.0	[5.0,65.6]	68.8	[31.1,91.5]	
Total (n=4,078)	27.5	[25.8,29.4]	2.5	[1.9,3.1]	23.3	[21.6,25.1]	46.7	[44.8,48.7]	
Substance use disorder									<0.001
Yes (n=165)	46.6	[37.2,56.3]	3.0	[1.2,7.4]	22.8	[16.1,31.2]	27.6	[19.1,38.1]	
No (n=3,910)	26.7	[24.9,28.6]	2.5	[1.9,3.2]	23.2	[21.5,25.1]	47.6	[45.6,49.6]	
Don't know (n=7)	11.7	[2.2,43.5]	0		64.5	[24.6,91.0]	23.8	[4.8,65.8]	
Total (n=4,082)	27.5	[25.8,29.3]	2.5	[1.9,3.2]	23.3	[21.6,25.1]	46.7	[44.8,48.7]	

[1] Pearson chi-square analyses

Table 40. Perceived Access to Substance Use Treatment Among Those with a Substance Use Disorder

	Would you say that your ability to get substance use treatment services through the Healthy Michigan Plan is better, worse, or about the same, compared to before?								
	Better		Worse		About the same		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Substance use disorder									<0.001
Yes (n=165)	48.3	[38.7,58.1]	1.7	[0.4,6.6]	16.4	[11.0,23.7]	33.6	[25.2,43.1]	
No (n=3,909)	8.1	[7.0,9.4]	0.1	[0.1,0.3]	8.9	[7.7,10.3]	82.8	[81.1,84.4]	
Don't know (n=7)	6.8	[0.8,39.7]	0		54.7	[16.4,88.1]	38.6	[9.9,78.2]	
Total (n=4,081)	9.8	[8.6,11.1]	0.2	[0.1,0.4]	9.3	[8.1,10.6]	80.7	[79.0,82.3]	

[1] Pearson chi-square analyses

Table 41. Change in Mental Health Status Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	Overall, since you enrolled in Healthy Michigan Plan, would you say your mental and emotional health has gotten better, stayed the same, or gotten worse?								
	Gotten better		Stayed the same		Gotten worse		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder									<0.001
Yes (n=1,286)	48.9	[45.4,52.4]	39.8	[36.5,43.3]	10.9	[8.9,13.3]	0.4	[0.2,0.9]	
No (n=2,778)	33.3	[31.1,35.6]	64.4	[62.1,66.7]	1.8	[1.3,2.4]	0.5	[0.3,0.9]	
Don't know (n=10)	82.2	[53.9,94.8]	14.7	[3.9,42.7]	3.1	[0.4,20.8]	0		
Total (n=4,074)	38.2	[36.3,40.2]	56.7	[54.7,58.7]	4.6	[3.8,5.4]	0.5	[0.3,0.7]	
Substance use disorder									<0.001
Yes (n=165)	50.7	[41.0,60.3]	40.5	[31.2,50.5]	8.8	[4.6,16.1]	0		
No (n=3,906)	37.6	[35.7,39.6]	57.5	[55.5,59.5]	4.3	[3.6,5.2]	0.5	[0.3,0.8]	
Don't know (n=7)	46.5	[12.1,84.5]	11.7	[1.4,55.1]	41.8	[7.9,85.8]	0		
Total (n=4,078)	38.2	[36.3,40.1]	56.7	[54.8,58.7]	4.6	[3.9,5.5]	0.5	[0.3,0.7]	

[1] Pearson chi-square analyses

Table 42. Perceived Impact of HMP Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	Having the Healthy Michigan Plan has helped me live a better life.												
	Strongly agree		Agree		Neutral		Disagree		Strongly disagree		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder													<0.001
Yes (n=1,286)	32.1	[28.9,35.5]	59.9	[56.4,63.4]	4.3	[3.0,6.0]	2.4	[1.6,3.7]	0.6	[0.3,1.4]	0.6		
No (n=2,781)	21.9	[20.0,23.9]	66.1	[63.8,68.3]	8.1	[6.8,9.5]	3.2	[2.5,4.1]	0.2	[0.1,0.3]	0.6	[0.3,1.2]	
Don't know (n=10)	36.2	[10.5,73.3]	63.8	[26.7,89.5]	0		0		0		0		
Total (n=4,077)	25.1	[23.4,26.8]	64.2	[62.3,66.1]	6.9	[5.9,8.0]	2.9	[2.4,3.7]	0.3	[0.2,0.5]	0.6	[0.3,1.1]	
Substance use disorder													<0.001
Yes (n=165)	35.5	[27.2,44.8]	60.3	[50.7,69.1]	1.6	[0.6,4.4]	2.6	[0.4,13.8]	0		0		
No (n=3,909)	24.6	[22.9,26.3]	64.5	[62.5,66.4]	7.1	[6.1,8.3]	2.9	[2.3,3.6]	0.3	[0.2,0.6]	0.6	[0.4,1.1]	
Don't know (n=7)	34.8	[8.5,75.4]	23.4	[5.3,62.4]	0		41.8	[7.9,85.8]	0		0		
Total (n=4,081)	25.0	[23.4,26.8]	64.2	[62.3,66.1]	6.9	[5.9,8.0]	2.9	[2.4,3.7]	0.3	[0.2,0.5]	0.6	[0.3,1.1]	

[1] Pearson chi-square analyses

Table 43. Change in Frequency of Involvement with Family and Friends Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	Since enrolling in the Healthy Michigan Plan are you involved with your family, friends or community more, less, or about the same?								
	More		Less		About the same		Don't know		*P*-value[1]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder									<0.001
Yes (n=1,287)	21.0	[18.1,24.2]	8.3	[6.5,10.5]	70.0	[66.6,73.2]	0.7	[0.3,1.5]	
No (n=2,774)	12.6	[11.1,14.3]	2.6	[2.0,3.5]	84.2	[82.4,85.9]	0.6	[0.3,1.2]	
Don't know (n=10)	4.6	[0.6,28.5]	25.2	[3.9,73.9]	70.2	[26.1,94.0]	0		
Total (n=4,071)	15.1	[13.7,16.6]	4.4	[3.7,5.3]	79.8	[78.2,81.4]	0.6	[0.3,1.1]	
Substance use disorder									0.001
Yes (n=165)	23.2	[16.0,32.2]	8.3	[4.0,16.4]	67.4	[57.6,75.9]	1.1	[0.2,7.6]	
No (n=3,903)	14.8	[13.3,16.3]	4.2	[3.5,5.1]	80.4	[78.8,82.0]	0.6	[0.3,1.1]	
Don't know (n=7)	23.8	[5.4,63.1]	41.8	[7.9,85.8]	34.4	[8.4,75.0]	0		
Total (n=4,075)	15.1	[13.7,16.6]	4.4	[3.7,5.3]	79.8	[78.2,81.4]	0.6	[0.4,1.1]	

[1] Pearson chi-square analyses

Table 44. Went to ER Because of Proximity Among Those with a Mood Disorder and Among Those with a Substance Use Disorder

	Went to the ER because it's your closest place to receive care[1]						
	Yes		No		Don't know		*P*-value[2]
	Row %	95% CI	Row %	95% CI	Row %	95% CI	
Mood disorder							0.940
Yes (n=398)	75.1	[69.5,80.1]	24.1	[19.3,29.8]	0.7	[0.1,3.6]	
No (n=575)	74.4	[69.9,78.4]	24.6	[20.7,29.1]	1.0	[0.4,2.3]	
Don't know (n=4)	89.8	[45.8,98.9]	10.2	[1.1,54.2]	0		
Total (n=977)	74.8	[71.3,77.9]	24.3	[21.2,27.8]	0.9	[0.4,1.9]	
Substance use disorder							0.035
Yes (n=70)	87.6	[77.6,93.5]	10.1	[5.3,18.5]	2.3	[0.3,14.7]	
No (n=907)	73.9	[70.2,77.2]	25.4	[22.1,29.0]	0.8	[0.3,1.8]	
Don't know (n=1)	0		100.0		0		
Total (n=978)	74.8	[71.4,78.0]	24.3	[21.2,27.7]	0.9	[0.4,1.9]	
Mood or substance use disorder							0.791
No (n=559)	74.3	[69.7,78.3]	25.0	[21.0,29.5]	0.7	[0.3,1.7]	
Yes (n=418)	75.5	[70.0,80.3]	23.4	[18.7,28.8]	1.1	[0.3,3.8]	
Total (n=977)	74.8	[71.3,77.9]	24.3	[21.2,27.8]	0.9	[0.4,1.9]	

[1] Asked of respondents with an ER visit in the past 12 months who said they did not try to contact their usual provider's office to get help or advice before going to the ER

[2] Pearson chi-square analyses

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January 11, 2018

VIA U.S. MAIL AND ELECTRONIC SUBMISSION

Brian Neale, Director
Centers for Medicare & Medicaid Services
Department of Health and Human Services
P.O. Box 8016
Baltimore, MD 21244-8016

Re: State Medicaid Director Letter, “Opportunities to Promote Work and Community Engagement Among Medicaid Beneficiaries”

Dear Director Neale:

The National Health Law Program (NHeLP) is a public interest law firm working to advance access to quality health care and protect the legal rights of low-income and underserved people.

Earlier today, the Centers for Medicare & Medicaid Services (CMS) sent a letter to State Medicaid Directors titled “Opportunities to Promote Work and Community Engagement Among Medicaid Beneficiaries,” which purports to justify imposing punitive work requirements on Medicaid beneficiaries to achieve better health outcomes. The letter suggests that the Secretary would deem such a requirement likely to promote the objectives of the Medicaid Act.

We have grave concerns, both procedural and substantive, regarding this letter. It not only reverses current agency policy that consistently and adamantly rejects work requirements, but it does so without soliciting public comment or feedback. While members of the public have commented on the work requirements proposed by several states in pending section 1115 waiver applications, as advocates, we had no opportunity to respond to the various, specific issues raised in CMS’s letter. As a result, these state-specific comments fall far short of the type of public notice and comment that typically attaches to such a significant about face.

Moreover, CMS’s novel proposition that work requirements are consistent with the objectives of the Medicaid Act comes only after the state and federal comment periods have closed on at least seven state

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proposals that contain work requirements. The timing of CMS’s letter has precluded any opportunity to comment on how or whether various states’ waiver applications address specific requirements and policy issues the letter identifies, such as exception processes, budget neutrality, and evaluation design. By waiting to issue this substantive letter for so long, CMS has effectively undermined stakeholders’ ability to comment meaningfully during these prior comment periods.

Equally troubling, CMS’s rationale in the letter entirely ignores the wealth of literature regarding the negative health consequences of work requirements, which was repeatedly cited by NHeLP and others in those state-specific comments. It appears that CMS has decided on a policy position first and then cherry-picked a small number of studies in an effort to justify this drastic shift in agency policy. However, as the attached and incorporated by reference Statement of Review from LaDonna Pavetti, an expert on work requirements, explains, the studies that CMS cites do not support its conclusion that punitive work requirements are likely to improve health outcomes.

Accordingly, NHeLP urges CMS to re-open or extend the public comment periods for all pending section 1115 waiver applications that seek to impose work requirements as a condition of eligibility, including Arizona, Arkansas, Indiana, Kansas, Kentucky, Maine, New Hampshire, North Carolina, Utah, and Wisconsin. A re-opened comment period will allow all stakeholders a meaningful opportunity to provide input to CMS's newly announced policy.

Thank you for your consideration.

Sincerely,

Jane Perkins

Jane Perkins
Legal Director
perkins@healthlaw.org

Statement of Review

My name is LaDonna Pavetti. I am Vice President for Family Income Support at the Center on Budget and Policy Priorities where I lead our work on the Temporary Assistance for Needy Families (TANF) program and our analysis of poverty trends. I have been doing work on the implementation and effectiveness of TANF since it was created in 1996. Prior to coming to the Center, I worked as a Senior Fellow at Mathematica Policy Research, Inc., one of the nation's top program evaluation firms that has conducted numerous rigorous evaluations of social programs, including TANF. I hold a Ph.D. in public policy from the Kennedy School of Government at Harvard University where I conducted research on movement on and off the Aid to Families with Dependent Children (AFDC) program, which was the precursor to TANF.

One of my areas of expertise is the effect of mandatory work requirements as they have been applied in TANF. I have examined the literature assessing the effectiveness of work requirements extensively and have been asked to present testimony to Congress on this topic multiple times. I also served on the advisory group for a comprehensive synthesis of the impacts of welfare reform on families' employment and earnings.

I have read the Dear State Medicaid Director letter re: Opportunities to Promote Work and Community Engagement Among Medicaid Beneficiaries, issued by the Department of Health and Human Services Centers for Medicare & Medicaid Services on January 11, 2018. CMS cites seven resources to support of its assertion that punitive work requirements are likely to improve health outcomes. I have reviewed the cited materials. As discussed below, those studies do not support CMS's conclusion.

First, as a general matter, none of the articles that CMS has relied upon suggest that *requiring* work as a condition of eligibility is likely to promote health outcomes. In fact, the 2006 literature review from Waddell and Burton actually reports evidence to the contrary. It cites strong research finding that forcing people off public benefit programs has negative consequences. In its summary of research on people who leave public benefits programs, the review finds that "[t]he net result is that interventions which encourage and support claimants to come off benefits and successfully get them (back) into work are likely to improve their health and well-being; interventions which simply force claimants off benefits are more likely to harm their health and well-being."[1]

Despite these findings from its own cited study, CMS has decided that it will permit punitive work requirements that will force beneficiaries to lose benefits, while at the same time refusing to offer states federal funding for the work supports that this and other studies have found actually improve health outcomes. Absent a major infusion of state dollars to bolster such supports – which no state has proposed to do – any mandatory work requirement cannot realistically expect to increase employment rates. To the contrary, such an approach directly contradicts the evidence before the agency and will inevitably force some people off Medicaid and force others to seek low-wage, temporary employment with erratic work schedules to the detriment of their health and well-being.

Second, CMS has entirely ignored the evidence that the quality of work matters, choosing instead to erroneously assert that any and all work will improve health outcomes. But the evidence cited by CMS once again undercuts its position. Both Waddell and Burton and van der Noordt et al. suggest that work can

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[1] Gordon Waddell and A Kim Burton, *Is Work Good For Your Health and Well-Being?*, at 30 (2006). *See also, e.g.* R. Dorsett et al., *Leaving Incapacity Benefit. Department of Social Security Research Report No. 86*. The Stationery Office, London (1998) [summarized in Waddell and Burton]

benefit health, but the quality and sustainability of the job matters.[2] In fact, according to the Waddell and Burton review, individuals who lost social security benefits suffered worse health outcomes partly because they often work in poor-quality, low-wage jobs and have ongoing issues with job security.[3] Decent job options can be scarce for this population and enforcing a work requirement that funnels beneficiaries toward predominantly temporary, dead-end jobs could actually worsen their health outcomes.

Third, CMS failed to even consider or discuss the applicability of basing its policy decisions on studies from the United Kingdom and other European countries that offer universal health coverage. The cited longitudinal analysis involves male workers in England and Wales, and both literature reviews draw heavily on studies from Europe and the UK. Individuals in these countries who lose social security benefits nonetheless maintain their health insurance. The review on which CMS relies to assert that "unemployment is generally harmful to health," therefore, in fact has little bearing on how policies that terminate health coverage will influence health outcomes in the United States. Moreover, it bears repeating that, even in the UK, where individuals do have stable access to health care, "interventions which simply force claimants off benefits are more likely to harm their health and well-being."[4]

Fourth, CMS has failed to acknowledge the important distinction between correlation and causation. For instance, van der Noordt et al. acknowledge that their results could be overstated because they were unable to adequately account for a "healthy worker effect," whereby relatively healthier individuals are also more likely to find a job.[5] Similarly, the letter cites a 2014 Gallup poll, which suggests a correlation between long-term unemployment and depression. But many social determinants correlate with health outcomes and improved mental health. For instance, access to steady housing is associated with improved health outcomes, while homelessness is associated with significantly worse outcomes.[6] Of course, requiring people to have a home to maintain their Medicaid benefits – particularly if a state provided no appreciable extra help – would hardly be expected to improve their health outcomes. It would just kick homeless people off the program and exacerbate their problems.

Likewise, while the Gallup poll shows a correlation between unemployment and depression, it does not automatically follow that increased employment will reduce or treat depression. In fact, the study expressly notes that "[t]he causal direction of the relationship, though, is not clear from Gallup's data," and one explanation is that depression makes it harder to find and maintain a job.[7] Even setting aside this criticism, terminating Medicaid benefits for failing to meet a mandatory work requirement is likely to leave many individuals suffering from depression without access to non-emergency care or treatment—a concern which CMS did not address in its letter to the states.

Fifth, the letter claims that community engagement is associated with improved health outcomes and can lead to paid employment. CMS first cites a health plan survey that appears to have made no adjustments

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[2] Gordon Waddell and A Kim Burton, *Is Work Good For Your Health and Well-Being?*, at 24 (2006).
[3] Gordon Waddell and A Kim Burton, *Is Work Good For Your Health and Well-Being?*, at 29 (2006).
[4] Gordon Waddell and A Kim Burton, *Is Work Good For Your Health and Well-Being?*, at 30 (2006).
[5] Van der Noordt et al., Health effects of employment: a systematic review of prospective studies, at 735 (2014).
[6] "Homelessness & Health: What's the Connection?" National Health Care for the Homeless Council (June 2011) https://www.nhchc.org/wp-content/uploads/2011/09/Hln_health_factsheet_Jan10.pdf (collecting studies).
[7] Steve Crabtree, "In U.S., Depression Rates Higher for Long-Term Unemployed," Gallup (2014) http://news.gallup.com/poll/171044/depression-rates-higher-among-long-term-unemployed.aspx. *See also*, *e.g.*, C. McLean, *Worklessness and Health: What Do We Know about the Causal Relationship, 1st Edition,* Health Development Agency, London, (2005) [summarized in Waddell and Burton].

at all for relative socioeconomic status, health status, or ability to volunteer among its respondents and thus provides little added value. A second citation, a literature review on the effects of volunteering (defined as an act of free will), "did not find any consistent, significant health benefits arising through volunteering."[8] While the review found limited benefits on well-being and mental health among people who volunteer, it relied mostly on study cohorts that are aged 50 and over and notes that improved outcomes "may be limited to older volunteers" and may also decline as hours of volunteering increase.[9] Importantly, the authors also note that the results of the cohort studies were not confirmed by randomized studies which are the gold standard for determining the effectiveness of an intervention. In short, the evidence cited hardly supports, and more likely undermines, the value of state proposals that would mandate substantial "community engagement" as a mechanism to improve health outcomes.

Finally, CMS cites evidence for the largely uncontroversial point that higher income is associated with longer life expectancy. But the study CMS cites cautions that these relationships "should not be interpreted as causal effects of having more money because income is correlated with other attributes that directly affect health."[10] The very fact that people in poor health tend to make less money could easily explain much of the mortality/income gradient. Moreover, CMS offers no evidence or basis for its belief that imposing work requirements would lead to increased employment or higher income. In fact, repeated studies find that access to Medicaid benefits facilitates employment,[11] while evidence from TANF shows that punitive work requirements have little or no lasting effect on income and can actually increase severe poverty.[12]

Dated: January 11, 2018

LaDonna Pavetti

LaDonna Pavetti, Ph.D.

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[8] Jenkinson, et al., *Is volunteering a public health intervention? A systematic review and meta-analysis of the health and survival of volunteers* (2013).

[9] Jenkinson, et al.

[10] R. Chetty, M. Stepner, and S. Abraham et al., *The Association Between Income and Life Expectancy in the United States, 2001-2014,* 315 JAMA 1750, 1764 (2016).

[11] Renuka Tipirneni et al., Institute for Healthcare Policy and Innovation, University of Michigan, *Medicaid Expansion Helped Enrollees Do Better at Work or in Job Searches*, (2017), available at http://ihpi.umich.edu/news/medicaid-expansion-helped-enrollees-do-better-work-or-job-searches; Louija Hou et al., "Working Paper No. 22170: The Effect of the Patient Protection and Affordable Care Act Medicaid Expansions on Financial Well-Being," National Bureau of Economic Research, (2016), available at http://nber.org/papers/w22170; Ohio Dep't of Medicaid, *Ohio Medicaid Group VII Assessment: A Report to the Ohio General Assembly*, (2017), http://medicaid.ohio.gov/Portals/0/Resources/Reports/Annual/Group-VIII-Assessment.pdf.

[12] LaDonna Pavetti, Ctr. on Budget & Pol'y Priorities, *Work Requirements Don't Cut Poverty, Evidence Shows* (2016); Sandra K. Danziger et al., *From Welfare to a Work-Based Safety Net: An Incomplete Transition*, 35 J. Pol'y Analysis & Management 231, 234 (2016); Gayle Hamilton et al., *National Evaluation of Welfare-to-Work Strategies: How Effective Are Different Welfare-to-Work Approaches? Five-Year Adult and Child Impacts for Eleven Programs*, Manpower Demonstration Research Corporation (2001); Stephen Freedman et al., *National Evaluation of Welfare-to-Work Strategies: Two-year Impacts for Eleven Programs*, Manpower Development Research Corporation, (2000) http://www.mdrc.org/publication/evaluatingalternative-welfare-work-approaches.

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Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children (Review)

Gibson M, Thomson H, Banas K, Lutje V, McKee MJ, Martin SP, Fenton C, Bambra C, Bond L

Gibson M, Thomson H, Banas K, Lutje V, McKee MJ, Martin SP, Fenton C, Bambra C, Bond L.
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[Intervention Review]

Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

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ABSTRACT

Background

Lone parents in high-income countries have high rates of poverty (including in-work poverty) and poor health. Employment requirements for these parents are increasingly common. 'Welfare-to-work' (WtW) interventions involving financial sanctions and incentives, training, childcare subsidies and lifetime limits on benefit receipt have been used to support or mandate employment among lone parents. These and other interventions that affect employment and income may also affect people's health, and it is important to understand the available evidence on these effects in lone parents.

Objectives

To assess the effects of WtW interventions on mental and physical health in lone parents and their children living in high-income countries. The secondary objective is to assess the effects of welfare-to-work interventions on employment and income.

Search methods

We searched the Cochrane Central Register of Controlled Trials (CENTRAL), MEDLINE Ovid, Embase Ovid, PsycINFO EBSCO, ERIC EBSCO, SocINDEX EBSCO, CINAHL EBSCO, Econlit EBSCO, Web of Science ISI, Applied Social Sciences Index and Abstracts (ASSIA) via Proquest, International Bibliography of the Social Sciences (IBSS) via ProQuest, Social Services Abstracts via Proquest, Sociological Abstracts via Proquest, Campbell Library, NHS Economic Evaluation Database (NHS EED) (CRD York), Turning Research into Practice (TRIP), OpenGrey and Planex. We also searched bibliographies of included publications and relevant reviews, in addition to many relevant websites. We identified many included publications by handsearching. We performed the searches in 2011, 2013 and April 2016.

Selection criteria

Randomised controlled trials (RCTs) of mandatory or voluntary WtW interventions for lone parents in high-income countries, reporting impacts on parental mental health, parental physical health, child mental health or child physical health.

Data collection and analysis

One review author extracted data using a standardised extraction form, and another checked them. Two authors independently assessed risk of bias and the quality of the evidence. We contacted study authors to obtain measures of variance and conducted meta-analyses where possible. We synthesised data at three time points: 18 to 24 months (T1), 25 to 48 months (T2) and 49 to 72 months (T3).

Main results

Twelve studies involving 27,482 participants met the inclusion criteria. Interventions were either mandatory or voluntary and included up to 10 discrete components in varying combinations. All but one study took place in North America. Although we searched for parental health outcomes, the vast majority of the sample in all included studies were female. Therefore, we describe adult health outcomes as 'maternal' throughout the results section. We downgraded the quality of all evidence at least one level because outcome assessors were not blinded. Follow-up ranged from 18 months to six years. The effects of welfare-to-work interventions on health were generally positive but of a magnitude unlikely to have any tangible effects.

At T1 there was moderate-quality evidence of a very small negative impact on maternal mental health (standardised mean difference (SMD) 0.07, 95% Confidence Interval (CI) 0.00 to 0.14; N = 3352; studies = 2)); at T2, moderate-quality evidence of no effect (SMD 0.00, 95% CI 0.05 to 0.05; N = 7091; studies = 3); and at T3, low-quality evidence of a very small positive effect (SMD −0.07, 95% CI −0.15 to 0.00; N = 8873; studies = 4). There was evidence of very small positive effects on maternal physical health at T1 (risk ratio (RR) 0.85, 95% CI 0.54 to 1.36; N = 311; 1 study, low quality) and T2 (RR 1.06, 95% CI 0.95 to 1.18; N = 2551; 2 studies, moderate quality), and of a very small negative effect at T3 (RR 0.97, 95% CI 0.91 to 1.04; N = 1854; 1 study, low quality).

At T1, there was moderate-quality evidence of a very small negative impact on child mental health (SMD 0.01, 95% CI −0.06 to 0.09; N = 2762; studies = 1); at T2, of a very small positive effect (SMD −0.04, 95% CI −0.08 to 0.01; N = 7560; studies = 5), and at T3, there was low-quality evidence of a very small positive effect (SMD −0.05, 95% CI −0.16 to 0.05; N = 3643; studies = 3). Moderate-quality evidence for effects on child physical health showed a very small negative effect at T1 (SMD −0.05, 95% CI −0.12 to 0.03; N = 2762; studies = 1), a very small positive effect at T2 (SMD 0.07, 95% CI 0.01 to 0.12; N = 7195; studies = 3), and a very small positive effect at T3 (SMD 0.01, 95% CI −0.04 to 0.06; N = 8083; studies = 5). There was some evidence of larger negative effects on health, but this was of low or very low quality.

There were small positive effects on employment and income at 18 to 48 months (moderate-quality evidence), but these were largely absent at 49 to 72 months (very low to moderate-quality evidence), often due to control group members moving into work independently. Since the majority of the studies were conducted in North America before the year 2000, generalisabilty may be limited. However, all study sites were similar in that they were high-income countries with developed social welfare systems.

Authors' conclusions

The effects of WtW on health are largely of a magnitude that is unlikely to have tangible impacts. Since income and employment are hypothesised to mediate effects on health, it is possible that these negligible health impacts result from the small effects on economic outcomes. Even where employment and income were higher for the lone parents in WtW, poverty was still high for the majority of the lone parents in many of the studies. Perhaps because of this, depression also remained very high for lone parents whether they were in WtW or not. There is a lack of robust evidence on the health effects of WtW for lone parents outside North America.

PLAIN LANGUAGE SUMMARY

How do welfare-to-work interventions for lone parents affect adult and child health?

Lone parents in wealthy countries have disproportionately high levels of poverty and ill health. Governments argue that both poverty and health might improve if lone parents started working or worked more, while some researchers think that working at the same time as raising children alone could be stressful and make health worse.

Welfare-to-work interventions (WtW) are designed to either encourage or require lone parents to look for work. Earnings top-ups, stopping or reducing benefits, training, helping to pay for child care and limits on how long benefits are paid have all been used to try

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to increase lone parent employment. In order to understand how requiring lone parents to take part in WtW programmes affects their and their children's health, we systematically reviewed studies that collected information on these effects.

We found 12 studies involving 27,482 participants that compared groups of lone parents in WtW interventions with lone parents who continued to receive welfare benefits in the normal way. All of the studies were at high risk of bias because the staff who collected the data knew when respondents were in the intervention group. In some studies, lone parents who were not in the intervention group were affected by similar changes to welfare policy that applied to all lone parents. We used statistical techniques to combine the results of different studies.These analyses suggest that WtW does not have important effects on health. Employment and income were slightly higher 18 to 48 months after the start of the intervention, but there was little difference 49 to 72 months after the studies began. In a number of studies, lone parents who were not in WtW interventions found jobs by themselves over time. It is possible that effects on health were small because there was not much change in employment or income. Even when employment and income were higher for the lone parents in WtW, most participants continued to be poor. Perhaps because of this, depression also remained very high for lone parents whether they were in WtW or not.

All but one of the studies took place in the United States or Canada before the year 2000. This means it is difficult to be sure whether WtW would have the same effects in different countries at other times.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON *[Explanation]*

Welfare to work for lone parents. Maternal health outcomes
Summaries of all health outcomes reported in the review are provided in Web appendix 1.

Patient or population: lone parents
Settings: high income countries
Intervention: welfare to work

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Control	Welfare to work				
T1 maternal mental health CES-D (mean score)[a]	-	The mean T1 maternal mental health in the intervention groups was **0.07 standard deviations higher** (0.00 to 0.14 higher)	-	3352 (2 studies)	⊕⊕⊕○ **Moderate**[b]	Very small negative effect
T2 maternal mental health CES-D (mean score)[a]	-	The mean T2 maternal mental health in the intervention groups was **0.00 standard deviations higher** (0.05 lower to 0.05 higher)	-	7091 (3 studies)	⊕⊕⊕○ **Moderate**[b]	No effect
T3 maternal mental health CES-D (mean score)[a]	-	The mean T3 maternal mental health in the intervention groups was **0.07 standard deviations lower** (0.15 lower to 0 higher)	-	8873 (4 studies)	⊕⊕⊕○ **Moderate**[b,c]	Very small positive effect

T1 maternal self-rated health % in poor health. Event defined as poor health	**201 per 1000**	**171 per 1000** (109 to 274)	**RR 0.85** (0.54 to 1.36)	311 (1 study)	⊕○○○ **Low**[b,d]	Very small positive effect
T2 maternal self-rated health % in good or excellent health. Event defined as good/ excellent health	**347 per 1000**	**367 per 1000** (329 to 409)	**RR 1.06** (0.95 to 1.18)	2551 (2 studies)	⊕⊕⊕○ **Moderate**[b]	Very small positive effect
T3 maternal self-rated health % in good or very good health. Event defined as good/ excellent health	**664 per 1000**	**645 per 1000** (605 to 691)	**RR 0.97** (0.91 to 1.04)	1854 (1 study)	⊕⊕○○ **Low**[b,e]	Very small negative effect

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: confidence interval; **RR**: risk ratio.
Very small effect: unlikely to be substantively important.
Small effect: may be substantively important.
Modest effect: likely to be substantively important.
See Table 5 for further explanation

GRADE Working Group grades of evidence
High quality: further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: we are very uncertain about the estimate.

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[a] Better indicated by lower values.
[b] All studies were downgraded due to high risk of bias in at least one domain.
[c] Heterogeneity over 50% and no plausible explanation identified.
[d] Confidence interval crosses line of no effect and includes appreciable benefit or harm.
[e] UK ERA was at very high risk of bias due to high levels of attrition amongst most deprived groups.

BACKGROUND

Description of the condition

Rates of lone parenthood have increased across all high-income countries in recent decades. Prevalence of lone parenthood ranges from 9% in Italy to 24% in the USA. A meta-analysis conducted by the Organisation for Economic Cooperation and Development (OECD) found that the children of single parents experienced worse outcomes than children in two-parent households across five domains: academic achievement; behavioural outcomes; depression and anxiety; self-esteem; and social relations. However, the magnitude of these effects varies across countries (OECD 2009).

The UK is one of the most recent countries to implement wholesale reform of welfare benefits for lone parents. As such, reform is a very current policy issue in the UK, and it provides a useful example of the development of welfare-to-work policy for lone parents. Recent estimates suggest that there are approximately 1.9 million lone parents in the UK, with 23% of all dependent children residing in lone-parent families (Evans 2010). Of these, 737,000 were out of work and claiming welfare benefits in 2008 (Department for Work and Pensions 2012). In 2008, 59% of lone mothers were in paid employment, compared to 71% for couple mothers. Lone parents and their children face high levels of poverty both in and out of work, with 66% of lone-parent families occupying the bottom two income quintiles, compared to 23% of two-parent households (Maplethorpe 2010). In addition to increased risk of poverty, lone parents and their children have higher levels of a range of other adverse outcomes. In 2005, the UK Families and Children Survey (FACS) found that lone mothers were twice as likely as couple mothers to describe their health as 'not good' (14% compared to 7%) (Hoxhallari 2007). Incidence of depression among lone parents is nearly three times that of other groups (Targosz 2003). Lone parents and their children in the UK and other European countries also disproportionately experience a range of other adverse outcomes: psychiatric disease; attempted suicide; alcohol and drugs-related disease (Weitoft 2003); and poor educational outcomes (Weitoft 2004). Mechanisms linking lone parenthood to poor health may include poverty (Spencer 2005), lack of support (Brown 1997), and stigma (Benzeval 1998). A focus on poverty as key amongst these has in part contributed to the introduction of policies designed to increase lone parents' participation in the labour market.

Historically, many high-income countries with comprehensive welfare systems have made lone parents eligible for welfare benefits and exempted them from labour market participation. This has changed in a number of countries in recent decades, most notably in the USA, where major welfare reform measures were implemented throughout the 1990s. Concern about the growing cost of welfare led to restrictions in eligibility for benefits and the introduction of time limits on welfare receipt (Blank 1997).

The UK has until recently maintained a relatively generous policy towards lone parents, providing welfare benefits and exempting them from work requirements until their youngest child reaches 16. However, in 2008 the UK Government implemented welfare reform legislation requiring lone parents to be available for work for at least 16 hours per week (latterly increased to 25 hours) when their youngest child reached the age of 12. This age threshold dropped further, to 10 years in 2009, 7 years in October 2010 (Department for Work and Pensions 2008), and subsequently to 5 years in 2011-12. The Summer Budget 2015 included a provision to decrease this age threshold to three as of September 2017. These changes have been accompanied by a range of interventions designed to promote labour market participation, including financial sanctions.

Similar restrictions in many OECD countries on social security benefits for lone parents are typically aimed at promoting employment in order to increase income and reduce poverty. In addition, such policies are believed to influence outcomes such as health, parental and child well-being, and family formation (in the USA - see for instance Jagannathan 2004). In the UK, the introduction of welfare-to-work policies for lone parents rests on a belief that engaging in paid work will alleviate the poverty blamed for poor health outcomes in lone parents, and thus has the potential to tackle inequalities in both income and health (Department of Health 2008). In addition to concerns about the increasing cost of welfare payments, policy makers in the UK justify increased conditionality on the basis that working is health promoting and will benefit both lone parents and their children. The Department for Work and Pensions report setting out the case for welfare reform in the UK stated:

"Helping more lone parents into work is good for their health, boosts self-esteem, promotes independence and lifts children out of poverty. . . Having parents in work also boosts children's self-esteem. When parents leave benefit and move into work, their children become more independent, understand the value of money, and gain from treats and activities. There's a trade-off between time and money, but get the balance right and everyone wins" (Department for Work and Pensions 2007).

However, while controlling for poverty in observational studies explains much (though not all) of the increased risk of adverse outcomes experienced by lone parent families (Benzeval 1998), working does not necessarily lift lone parent families out of poverty (Hoxhallari 2007). With regard to health, there is widespread acceptance that a causal relationship exists between employment and health in the general population (Waddell 2006), although this relationship is mediated by the quality of employment (Siegrist 2009). However, the pathways linking work and health may be more complex in lone parents, with the potential for negative as well as positive impacts. On the one hand, working may increase income (though this is not guaranteed) in lone parent families and alleviate the poverty that is linked to adverse health outcomes (Spencer 2000). Working may also increase parental confidence and self-esteem, leading to improved parenting (Michalopoulos

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2002). Increased use of formal child care may also improve child outcomes (The NICHD Early Child Care Research Network 1998). However, these positive impacts may be mediated by factors such as job quality and hours worked (Morris 2003b). There is some evidence that time poverty, role strain, and parental absence, contingent upon the parent's attempts to fulfil multiple roles simultaneously, may impact negatively on the health of lone parents and their children. For instance, although lone mothers in Sweden have both higher employment rates and lower poverty than in the UK, they continue to experience poorer outcomes in health and other areas relative to the general population. Some authors hypothesise that this is due to time poverty and the stress engendered by combining child rearing with employment (Weitoft 2003; Whitehead 2000). Thus, there are a number of potentially conflicting mechanisms at play that may influence the health of lone parents who participate in welfare-to-work interventions or enter the labour market. In the context of the widespread implementation of such policies, it is important to gain a better understanding of these issues by locating and synthesising existing evidence.

In the USA, where most welfare-to-work evaluations have taken place, welfare reform was a highly politicised and controversial issue. In part due to this controversy, the federal government required that individual states conduct experimental evaluations of their new welfare-to-work programmes during the period of wholesale reform in the 1990s (Page 1997). As a result, a large evidence base of US randomised controlled trials (RCTs) exists, which has the potential to assist in answering such questions.

Description of the intervention

Welfare-to-work interventions are defined for the purpose of the review as government-financed interventions (which can be delivered by public, private or third sector organisations) that encourage or require participants to take up employment, increase economic activity, or increase their employability. These may be either mandatory or voluntary. Many of the programmes are referred to in acronyms, and we have provided a glossary in Appendix 1 for ease of reference.

Interventions can be differentiated in three ways: in terms of their underpinning ethos; the methods they adopt to promote employment; and their individual programme components. Welfare-to-work programmes may adopt either a caseload reduction (CR) approach or an anti-poverty approach (AP). In the former, the focus is on reducing welfare rolls for political or economic reasons, and the aim is to engage people in the labour market regardless of whether labour market participation leads to any improvement in material circumstances. In contrast, in the poverty reduction approach, there is a recognition that many long-term welfare recipients may be unable to secure employment that provides a wage above the level of benefits, and there is a determination that those who become employed should be lifted out of poverty by their efforts (Miller 2008). Interventions also differ in terms of the methods they adopt to promote employment. Labour force attachment (LFA) approaches adopt a 'work first' strategy, based on the idea that rapid engagement in employment is the most effective means of promoting economic independence. Human capital development (HCD) approaches focus on education and training (increasing employability), in the belief that, in the long term, acquiring skills or qualifications is likely to assist welfare recipients in attaining higher-quality or more secure employment. These approaches are not mutually exclusive; for instance, an intervention with the primary aim of caseload reduction may employ either LFA or HCD approaches in pursuit of that aim. It should also be noted that these categories constitute 'ideal types'; it is likely that in practice, interventions contain a mixture of approaches or shift emphasis from one approach towards another during the course of the intervention.

A further means of differentiation between interventions is at the level of individual programme components. Welfare-to-work programmes may include a wide range of components, including: earnings supplements; earnings disregards (i.e. a proportion of earned income is disregarded when calculating benefit entitlement); childcare subsidies; requirements to participate in employment or employment-related activity for a specified number of hours per week in order to qualify for financial and other types of support; mandated participation in assigned jobs in order to receive welfare benefits; lifetime limits on receipt of welfare benefits; sanctions; employment training; health insurance subsidies (usually in the form of an extension of transitional US Medicaid entitlement after starting employment); and case management. Again, it is not possible to definitively assign a particular set of components to a given intervention approach; interventions of any approach may include one or more of these components in varying combinations. Certain components are more likely to appear in some types of intervention: for instance, financial incentives are more likely to appear in anti-poverty interventions, while mandatory employment is more likely to appear in caseload reduction programmes. However, other components, such as case management or sanctions, may be employed in interventions using any approach.

Evaluations of these interventions most often compare the intervention with 'usual care' (in the USA, this comprises receipt of the previous benefit, called Aid to Families with Dependent Children (AFDC), which continued to be provided to control groups in intervention trials), although in some cases studies compare LFA and HCD interventions with each other and usual care.

How the intervention might work

The principle aims of welfare-to-work interventions relate to increasing employment and improving other economic outcomes. Some welfare programme evaluations also assess health indicators as secondary outcomes. By contrast, these are the primary outcomes of interest for this review. There are a number of hypothesised pathways by which interventions aiming to promote labour market participation might impact on the mental or physical health of lone parents and their children. These may vary according to the approach adopted and the components included in the intervention. Evidence from some primary studies suggests that programmes focused on caseload reduction and swift job entry have a negative impact on parental mental health. This is particularly apparent in parents of preschool children and may stem from either increased stress associated with the combination of child care and pressure to start working, or from the tendency to take jobs of low quality when rapid employment take-up is a condition of the intervention (Morris 2008).

Income supplements, in the form of either earnings disregards or financial incentives, might be expected to have a positive impact on health by increasing income. Being subject to benefit time limits or to sanctions for failure to comply with programme requirements could lead to a decrease in income and a concomitant increase in stress. Participating in training and gaining new skills could lead to improved confidence and self-esteem for mothers, with positive effects on parenting and thus on child mental health (Zaslow 2000). Alternatively, requirements to attend training, mandatory employment or other employment preparation activities, while continuing to be solely responsible for child rearing, may place lone parents under increased stress, with negative effects on both parental health and parenting practices (Gennetian 2000). Mothers may have less time to spend with their children, which could lead to decreased interaction and supervision. On the other hand, participating in training or receiving childcare subsidies could involve increased use of formal child care, which may lead to improved educational and social outcomes for children (Morris 2003b). Health insurance subsidies are likely to have a positive health impact by increasing access to health care. The impact of case management may depend on the specific content or tone, which will vary between interventions (Morris 2008).

If the intervention is successful in its primary aim of placing lone parents in employment, a number of other impacts on health may result. If income increases, the parent may be able to provide more or better material and educational resources for their offspring (Gennetian 2000). This could also alleviate stress associated with poverty, thus improving parental health and parenting practices (Morris 2003a). As with participating in training, the time pressure and stress of employment may affect parental mental health and reduce time spent interacting with or supervising children. Potential health benefits that accrue from entering employment are mediated by factors such as job quality/stability, shift patterns and wage levels (Morris 2008). Certain components of an intervention, such as financial incentives/earnings disregards, childcare subsidies and health insurance subsidies, may become available only when the parent is in employment.

The primary focus of this review is the health impact of participating in the intervention, although we are also interested in the impact of changes in employment or income as a result of participating in the intervention. However, in most cases it is not readily apparent whether changes in health outcomes result from participating in the intervention, or from becoming employed as a result of so doing, since health outcomes are not generally disaggregated by employment outcomes.

Why it is important to do this review

Lone parents are a group who experience social and health disadvantage, with a higher prevalence of health problems than the general population. Employment and employment conditions are recognised to be important social determinants of health (Commission on Social Determinants of Health 2008), and lone parents are more likely than other groups to enter jobs with poor pay and conditions (Evans 2004). Therefore, requirements for lone parents to work or to take part in welfare-to-work interventions are likely to impact positively or negatively on the health of this population group, thereby reducing or increasing health inequalities. In the UK context of a very rapid shift in policy, involving revising the child age threshold downward from 16 to 5 in less than five years, it is of crucial importance to investigate the likely health impacts of this shift. In a wider context, many OECD countries have introduced or increased conditionality for lone parents in recent years, with countries including Australia, the Netherlands and Sweden reducing the youngest child threshold or more rigidly enforcing work requirements (Finn 2010). The Economics Task Group of the Strategic Review of Health Inequalities in England (the Marmot Review) highlighted the lack of knowledge regarding this issue and called for more research to increase understanding of the health impacts of welfare-to-work interventions in lone parents (Suhrcke 2009).

A sizeable evidence base on the health impacts of welfare-to-work interventions aimed at lone parents exists, consisting primarily of RCTs conducted in the USA, but there are no systematic reviews or syntheses, nor has consideration been given to its applicability (or not) in other country contexts. A number of non-systematic literature reviews have summarised evaluations of the health impacts of lone parent welfare-to-work interventions (Carnochan 2005; Kissane 2007; Waldfogel 2007). These tend to suggest that there can be adverse impacts on some outcomes, particularly for adolescents (Gennetian 2002a). However, none of these have used systematic review methods to locate, extract, critically appraise and synthesise data from such evaluations. One meta-analysis of welfare-to-work interventions did not conduct a literature search but used an existing database of US and Canadian studies (up to the year 2000) (Greenberg 2005). Another research synthesis did not state what search methods they used (Grogger 2002). Both of these publications included only US and Canadian studies and did not consider adult health outcomes. There is currently no Cochrane Review of the topic. One Cochrane Review assessed interventions that increased income among low-income families (Lucas 2008).

Most of these interventions were welfare-to-work interventions, and many were aimed at lone parents. However, the review did not include studies that did not provide a cash benefit, nor did it include parental health outcomes. Another Cochrane Review focused on the health impacts of in-work tax credits for families, which some consider to be a welfare-to-work intervention. However, in-work tax credits are available to people who are not on welfare, and the associated payments are not time limited, in contrast to the financial incentives provided to participants in this review (Evans 2001).

Given the many pathways by which such interventions might have positive or negative impacts on the health of lone parents and their children, a review of this topic is both timely and relevant to policy. The findings of the review will be useful to policy makers and practitioners in the field of welfare to work. In particular, information about variations in intervention types and health impacts may usefully inform the development of appropriate welfare-to-work interventions.

OBJECTIVES

To assess the effects of WtW interventions on mental and physical health in lone parents and their children living in high-income countries. The secondary objective is to assess the effects of welfare-to-work interventions on employment and income.

METHODS

Criteria for considering studies for this review

Types of studies

Due to the difficulties inherent in evaluating social interventions, it is not common for these to be evaluated using RCT methods. For this reason, systematic reviews of social interventions often include non-randomised studies, such as prospective and retrospective controlled evaluations. Although these are subject to a number of threats to validity, in most cases they constitute the 'best available evidence' (Ogilvie 2005). We identified many non-randomised studies of welfare-to-work interventions. However, we also identified a substantial number of RCTs of welfare-to-work interventions, and as this design is recognised as the most robust method of evaluating interventions, we restricted the review to this study design. We had intended to include quasi-randomised studies (i.e. studies using alternate allocation or allocation by date of birth), but did not identify any studies of this type. Therefore, the studies included in the review are RCTs of welfare-to-work interventions using standard methods of randomisation.

A preliminary search for relevant literature also identified a large number of welfare-to-work intervention studies using qualitative methods, such as face-to-face interviewing and focus groups, to investigate participants' lived experience of interventions. A separate review assesses the qualitative evidence (Campbell 2016).

Types of participants

Lone parents and their dependent children residing in countries defined by the World Bank as 'high-income' (World Bank 2011), with established social welfare systems were the population of interest for the review. In Europe, lone parents are defined as parents living solely with their children or with their children and other adults who are not the parent's partner, spouse, or the other biological parent of the children. However, in the USA the definition is broader, including parents who are cohabiting with, but not married to, either the children's other parent or a new partner. Therefore we included studies on the basis of the group that the interventions explicitly targeted. That is, we included studies if the authors described the intervention as aimed at lone parents and the respondents as lone parents. In addition, study samples often included a proportion of respondents who were married and living with a spouse. We included studies in which most participants were lone parents. We excluded studies with fewer than 60% lone parents that did not report findings by parental status, as it would not be possible to discern whether effects were specific to lone parents. We reported and commented on all relevant demographic information.

Types of interventions

We included welfare-to-work interventions initiated at government level and aimed at adult lone parents exclusively or in combination with couple parents. We provide detailed information about the interventions in Description of the intervention. We included caseload reduction, anti-poverty, labour force attachment and human capital development interventions, consisting of any combination or intensity of the components described previously. We included studies comparing the intervention with usual care (i.e. the standard welfare entitlement and conditions that existed prior to the implementation of the intervention). We also included studies comparIng two or more variants of the intervention with usual care. For instance, if a study compared both labour force attachment and human capital development interventions with usual care, we included the study.

We identified a subset of interventions aimed explicitly at teenage parents, which generally included only lone mothers who gave birth at any age up to 20. However, the primary aims of these interventions were to encourage teen parents to complete high school education and to teach parenting skills. Since their primary outcome was not gaining employment, and teenage parents comprise a discrete subpopulation with specific needs, we excluded interventions aimed at teen parents. A further category of intervention conducted in the USA subsequent to the initial wave of welfare reform evaluated the efficacy of providing additional services to those who had proven hard to place in employment. These 'enhanced services' interventions were often aimed at populations with special needs (e.g. drug addiction, severe health problems) and were provided in addition to the standard welfare-to-work intervention, which forms the experimental condition in the studies we included. We therefore excluded these from the review.

Types of outcome measures

The primary outcomes of interest in this review are health outcomes. We were also interested in economic outcomes as mediators of an intervention's health impacts. Hence, we included studies that reported health outcomes and extracted any available data on economic outcomes. We excluded studies that did not report the health outcomes listed below.

Primary outcomes

We extracted both reported, validated health scales and self-reported health measures.
Table 1 summarises the primary outcomes. We obtained data on parental health assessed by validated measures of parental physical and mental health, such as the Physical Health Scale, the Centre for Epidemiological Studies Depression Scale (CES-D), and the WHO Composite International Diagnostic Interview (CIDI), as well as self-reported physical or mental health. We also extracted data on child health such as parent- or child-reported physical or mental health measures (e.g. the Health Status Scale and the Behavior Problems Index (BPI)). We also included studies reporting only child health outcomes. All included health outcomes are listed below.

Secondary outcomes

We extracted economic outcomes that were reported in addition to the health outcomes outlined above. Economic outcomes included: full- or part-time employment; health insurance coverage, total income; and average earnings. We did not extract economic outcomes where studies reported health outcomes for a subsample but reported economic outcomes only for the main sample.
Although the studies reported many identical outcomes, studies often gave them different names. For ease of comprehension, we standardised these terms.

Search methods for identification of studies

Electronic searches

We searched the following databases in 2011, 2013 and 2016.

- Cochrane Central Register of Controlled Trials (CENTRAL 2016, Issue 4) in the Cochrane Library (searched 5 April 2016).
- MEDLINE Ovid (1948 to 5 April 2016).
- Embase Ovid (1947 to 5 April 2016).
- PsycINFO EBSCO (1806 to 5 April 2016).
- ERIC EBSCO (1964 to 5 April 2016).
- SocINDEX EBSCO (1895 to 5 April 2016).
- CINAHL EBSCO (1982 to 5 April 2016).
- Econlit EBSCO (1969 to 5 April 2016)
- Web of Science ISI (1900 to 5 April 2016).
- Applied Social Sciences Index and Abstracts (ASSIA) Proquest (1987 to 6 April 2016).
- International Bibliography of the Social Sciences (IBSS) ProQuest (1951 to 6 April 2016).
- Social Services Abstracts Proquest (1980 to 6 April 2016).
- Sociological Abstracts Proquest (1952 to 6 April 2016).
- Campbell Library (2000 to 6 April 2016).
- NHS Economic Evaluation Database (NHS EED) CRD York (1994 to 6 April 2016).
- Turning Research into Practice (TRIP) (1997 to 6 April 2016).
- OpenGrey (1997 to 6 April 2016).
- Planex (1984 to 6 April 2016).

We undertook exploratory searches of trials registers but as we did not identify any relevant trials, we did not run full searches. We did not exclude documents on the basis of language or publication date.
Appendix 2 details the full searches. Where available, we used a study design filter to limit searches to randomised trials. For databases without study design filters, we included search terms relating to study design in an effort to increase the specificity of the search. We identified key terms used in welfare policy outside North America to ensure that the search was sensitive to relevant research beyond North America. We examined thesauri in electronic bibliographic databases and used our knowledge of existing relevant publications from outside of North America to inform the search strategy. Due to the volume of literature found, we excluded conference papers and theses.

Searching other resources

We handsearched the bibliographies of both included and highly relevant publications and reviews, and we contacted the authors of included studies in order to locate unpublished or ongoing research. Since independent research organisations and government departments conduct many evaluations of welfare-to-work interventions, we handsearched a large number of relevant websites (see Appendix 3). Websites with search interfaces or searchable databases were searched using terms such as 'lone parent', 'lone parent welfare', 'welfare reform' or 'welfare health'. Otherwise, we screened the relevant publications topic on the website. Where this was possible, we list the number of initial 'hits' from these websites in Appendix 3. We searched a number of websites belonging to research organisations known to conduct research in this area, and in an effort to also locate research conducted outside of North America, we searched websites carrying research sponsored or conducted by the national governments of OECD countries. We only handsearched publications in English.

Data collection and analysis

Selection of studies

Two authors independently screened abstracts and titles of retrieved publications against the inclusion criteria described above. We retrieved the full text of publications appearing to meet the inclusion criteria and independently assessed them for inclusion. We systematically recorded the reasons for exclusion of publications at the second stage of screening. We documented disagreements and resolved them by consensus, with arbitration by a third member of the team if we could not achieve consensus.

Data extraction and management

Five review authors (MG, KB, MJM, VL and SPM) designed a standardised data extraction form, and two (MG and KB) piloted it before full extraction commenced. One author (KB, MG, MJM, SPM, VL) then extracted data using the data extraction form, and another (MG) checked them; we resolved any discrepancies through discussion, involving a third review author if necessary. We extracted data on the topics described in Table 2.

Many evaluations of welfare-to-work interventions have generated multiple publications, which often report on differing subsamples or include the same impact data reported on a number of occasions. To avoid reporting duplicate data, we tabulated the outcomes reported in each publication, including the relevant sample, the specific measures used and the dates for which they were reported. We used these tables to identify unique outcome data and exclude repeated data. Where different publications reported the same outcome data, we compared values for each data point to check for discrepancies. Where any uncertainty remained we contacted study authors for clarification.

Assessment of risk of bias in included studies

Two authors (MG and VL) independently conducted 'Risk of bias' assessments on primary outcomes using the Cochrane 'Risk of bias' tool (Higgins 2011a), adding several domains from the Cochrane Effective Practice and Organisation of Care (EPOC) Group's risk of bias tool (see Appendix 4). We assessed baseline characteristics, baseline outcome measurements and contamination in addition to the standard Cochrane 'Risk of bias' items. In line with the EPOC tool, we did not assess blinding of participants and personnel, as it is typically not possible to blind participants or providers to social interventions (Oakley 2003). We assessed two domains - blinding of outcome assessors and baseline outcome measures - at the level of individual outcomes. We assessed incomplete outcome data at both study and outcome level, since missing outcome data can occur at the level of the study (unit non-response) or at the level of the outcome (item non-response). The review authors conducting the assessment resolved discrepancies through discussion and referred to a third author (HT) for resolution if necessary.

Measures of treatment effect

The studies reported outcomes as both continuous and dichotomous variables. We calculated risk ratios (RR) for dichotomous outcomes and standardised mean differences (SMD) for continuous outcomes. We reported standardised effect sizes where an outcome was only reported by a single study in order to facilitate comparison of effect magnitude across outcomes measuring the same underlying construct. In calculating RRs, we defined the 'event' in the manner in which the outcome was reported. If the prevalence of a 'bad' outcome (such as risk of depression) was reported, we defined this as the event. Similarly, if studies reported the proportion of the sample experiencing a good outcome (such as being in good or excellent health), we defined this as the event. Where the good outcome represented the event, we noted it in the Summary of findings for the main comparison and Summary of findings 2. We grouped outcomes according to type (e.g. parental physical health, parental mental health). Where sufficient data were available, we used Review Manager 5 (Review Manager 2014) to calculate effect sizes.

Unit of analysis issues

Studies implemented and evaluated the included interventions at the level of the individual. Authors generally reported outcomes for the adult participant (i.e. the lone parent) and for one focal child. All but three studies collected data from only one focal child per family. Two of these reported adjusting standard errors to take account of shared variance between siblings. We contacted the authors of the third study to confirm that they had taken appropriate measures, and they reported having applied the Huber-White correction in STATA to account for shared variance.

Combining groups and outcomes

A number of studies included more than one intervention group and did not report aggregate data for these. In addition, a number of studies reported data for subgroups of recipients or by child age subgroups. Where studies included more than one intervention group but only one control group, we combined experimental groups for the primary analysis (Higgins 2011b), ensuring that control group data were entered only once to avoid duplication. Where studies included subgroups defined by location or respondent characteristics, we combined experimental subgroups and, separately, control subgroups as appropriate. In the case of dichotomous outcomes, this was achieved simply by summing the appropriate statistics (Higgins 2011b). For continuous outcomes, we entered group means, standard deviations and Ns into the ‘Calculate based on several groups’ function in Review Manager 5 (Review Manager 2014). Where no measure of variance was available, outcome data were reported narratively and included as ‘other data’ in the Data and analyses section. In these cases, studies only reported significance levels at the 0.01, 0.05 or 0.1 levels. We reported these in the text and in the ’other data’ tables. We derived a number of outcomes from reported data where appropriate. For instance, if number of hours worked per week was reported as a categorical variable with five categories, we summed those below 30 hours to derive a value for part-time employment and those at or above 30 hours to derive a value for full-time employment. These are also reported in meta-analysis footnotes.

Dealing with missing data

Few studies reported measures of variance that would permit the calculation of effect sizes and inclusion of outcomes in meta-analyses. Two studies reported P values for all outcomes, and two further studies reported P values for some outcomes. We contacted the authors of all other studies to request measures of variance and received them from MDRC (formerly Manpower Demonstration Research Corporation) for all studies conducted by that organisation. The author of one study provided pooled standard deviations, and the Social Research and Demonstration Corporation (SRDC) provided standard errors for two further studies. We were unable to obtain measures of variance for the remaining three studies.

We used standard errors and P values to calculate standard deviations in the Cochrane standard deviation calculator tool. Where measures of variance were not available, we reported effects narratively in the text and in ’other data’ tables in Data and analyses.

Where available, we extracted data on attrition and item non-response from publications and included them in the ’Risk of bias’ assessments.

Assessment of heterogeneity

Included studies were relatively homogeneous in terms of design, population and outcome measures, although the interventions varied in terms of approach and components provided. We performed Chi^2 tests and used the I^2 statistic to test for statistical heterogeneity. Where heterogeneity was moderate to high ($I^2 > 60\%$; Deeks 2011), we performed post hoc sensitivity analyses to investigate the effect of excluding obvious outliers, and we formulated exploratory hypotheses for the causes of such heterogeneity. As there are multiple sources of possible variation in complex social interventions such as these, we used random-effects models for all meta-analyses (Deeks 2011).

Assessment of reporting biases

As there were fewer than 10 studies available for any category of outcome included in a meta-analysis, it was not possible to investigate reporting bias using funnel plots or Egger’s test. We used the Cochrane ’Risk of bias’ tool to investigate selective outcome reporting and incomplete outcome data (Higgins 2011a).

Data synthesis

We collected data at all available time points and classified them for analysis purposes in terms of the time elapsed between randomisation and data collection. We created three categories: time point 1 (T1), at 12 to 24 months since randomisation; time point 2 (T2), at 25 to 48 months; and time point 3 (T3), at 49 to 72 months. Although this division differs slightly from the intervals stated in the protocol, we found that after collecting data and establishing the actual distribution of studies and follow-up times, these intervals provided the optimal spread of follow-up times and number of studies within each interval. One study reported partial data at 96 months. We did not include these in the main analysis but summarised them narratively and reported them as ’other data’ in Data and analyses. Two later publications analysed linked mortality data from two other studies at 15 and 17 to 19 years. We report these narratively in the text. Table 3 shows the reported follow-ups and time points.

Many reported outcomes were sufficiently homogeneous to be included in meta-analyses. In addition, we were able to obtain many of the statistics required for meta-analysis from authors. We were therefore able to meta-analyse many outcomes, and where this was not possible, to calculate effect sizes for individual outcomes. In a few cases it was not possible to calculate an effect size. Where no measure of variance was available, we entered data into Review Manager 5 (Review Manager 2014) as ’other data’. If there were sufficient studies that reported standard deviations for an identical continuous outcome, we imputed them for outcomes with no measure of variance. In such cases, we conducted sensitivity analyses to investigate the effects of using different methods to impute the standard deviation (e.g. the average of all reported standard deviations compared to the highest reported value) (Higgins 2011b).

We grouped outcomes into child and adult outcomes and then by type of outcome, that is, we synthesised and analysed adult physical health and adult mental health separately. Employing the approach to summary assessment of risk of bias suggested in Higgins 2011a, we judged all studies to be at high risk of bias; therefore for each time point and category, we entered into the primary analyses all studies for which the necessary data were available.

We used standard mean differences to calculate combined effect sizes for continuous outcomes using Review Manager 5 (Review Manager 2014). This permitted the inclusion in meta-analyses of continuous outcomes measuring the same construct, such as parental depression. Where outcomes were not sufficiently similar

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to be included in meta-analyses, we calculated individual effect sizes and presented them in forest plots. Where data were not available for individual outcomes, we reported these in the text within the appropriate outcome category and also presented them in 'other data' tables.

Subgroup analysis and investigation of heterogeneity

We intended to investigate between-study heterogeneity using subgroup analyses. In particular, we planned to conduct subgroup analyses of studies grouped in terms of the typology of interventions identified in the early stages of the review (i.e. caseload reduction/anti-poverty and labour force attachment/human capital development; see Description of the intervention for a more detailed description of these). However, this was not possible since the number of studies in each category within each time point was insufficient to permit further statistical analysis. In addition, we found that interventions defined by approach or ethos were more similar in practice than expected. We were also unable to conduct other planned subgroup analyses because they lacked either data or sufficient studies; these included studies that differed according to economic contexts, implementation, level of bias, age of child, level of participant disadvantage, ethnicity and whether or not participants became employed. The largest source of variation in the interventions was in terms of the components provided. It was not possible to investigate the effects of this variation systematically, as there were again insufficient studies providing similar combinations of components. We were therefore limited to our planned primary analysis including all studies at each time point. However, where there was evidence of high heterogeneity (> 60%) we conducted post hoc sensitivity analyses to investigate the effect of excluding obvious outliers and suggested possible hypotheses relating to intervention characteristics that might explain such differences. While this is an acceptable method of investigating heterogeneity, since the analyses are not pre-specified it does not produce reliable results and can only be seen as a means of generating hypotheses (Deeks 2011). Where heterogeneity was high and we could identify a plausible hypothesis, we presented impacts from outlying studies separately and discussed the potential role of the identified characteristic.

Sensitivity analysis

As described above, sensitivity analysis was used for post hoc investigation of heterogeneity.

Quality and applicability of evidence

We assessed the quality of the evidence for each outcome using the GRADE approach where an effect estimate was available, either from a meta-analysis or a single study (Schünemann 2011a). After importing all analyses from Review Manager 5 (Review Manager 2014) to GRADEpro GDT 2014, we assessed each outcome for threats to quality from risk of bias, inconsistency, indirectness, imprecision and publication bias. Where it was not possible to calculate an effect estimate, we judged the quality of the evidence to be 'unclear'.

Each outcome domain included outcomes measuring the same construct in different ways. For instance, studies reported parental mental health as both a continuous and a dichotomous variable. Hence, we could not include all of the outcomes in meta-analysis, so each domain included some outcomes that we combined in a meta-analysis and some outcomes for which we could only calculate a single study effect estimate. We graded evidence from each of these separately, but analyses within each domain could vary in quality, hampering the GRADE objective of reaching a judgement on the overall quality of the evidence for any single outcome domain.

In order to facilitate an overall quality assessment for each domain, we developed a domain-level summary assessment. This was based on the assessment of quality for the analyses including the largest sample size. For instance, at T1 five studies reported a measure of parental mental health. We could include data from two of these studies in a meta-analysis and calculated separate effect sizes for the remaining three outcomes. The sample size in the meta-analysis was 3352, and the evidence was of moderate quality. The combined sample size for the remaining three studies was 767. The evidence from two of the single studies was low quality, and from the remaining study the evidence was very low quality. Since the sample size of the moderate-quality evidence in this domain was much larger, we assigned the domain an overall judgement of moderate quality. We included the analyses on which the domain level judgement was based in Summary of findings for the main comparison and Summary of findings 2. Where more than one analysis in a given domain contributed to the domain level assessment, we included the analysis with the largest sample size in the 'Summary of findings' tables.

If we assessed studies included in a meta-analysis as being at high risk of bias, we downgraded the evidence once. Where a study was deemed to be at very high risk of bias, we downgraded the evidence twice (for instance where severe or systematic attrition was present). The only exception to this was where the study contributed 10% or less of the overall weight of a meta-analysis, in which case we did not downgrade for very high risk of bias. We downgraded once for inconsistency if I^2 was greater than 50%, effects were in opposing directions, and we could not identify a plausible explanation for heterogeneity. However, if I^2 was above 50% but all effects were in the same direction, or if we could identify a plausible explanatory hypothesis, we did not downgrade for inconsistency (Schünemann 2011b). We did not downgrade for risk of bias caused by contamination because since it leads to underestimation of impacts, it is deemed to be of less concern than risk of bias in domains likely to cause overestimated impacts (Higgins 2011a). To assess indirectness, we considered the extent to which the population and setting of the included studies was similar to those of interest for the review, and whether any outcome measures used were indirect or proxy measures.

When assessing imprecision, we downgraded continuous outcomes (reported as SMDs) once if the confidence intervals included 0.5 standard deviations on either side of the point estimate and crossed the line of no effect. For dichotomous outcomes, we downgraded once if the confidence intervals included a 25% reduction or increase in the RR (on either side of the point estimate) and also crossed the line of no effect. If the confidence interval crossed the line of no effect but did not include appreciable benefit or harm, according to the above criteria, we did not downgrade for imprecision. However, where the CI crossed null and the effect was very small, we noted that this was unlikely to be an important effect (Ryan 2016). Where there was reason to suspect publication bias, we downgraded once on this criterion. We assigned all health outcomes a 'critical' rating and all economic outcomes an 'important' rating. We present GRADE assessments for the health outcomes used for the domain level GRADE assessment in the Summary of findings for the main comparison and Summary of findings 2. GRADE guidance stipulates that 'Summary of findings' tables should be ordered by problem or population, then by intervention type or comparison, then by outcome (GRADEpro GDT 2014). In this case, there is only one intervention, but the studies report impacts across a wide range of follow-up times. Our synthesis is structured in terms of intervention, then population, then time point (i.e. short, medium and long-term follow-up), then outcome, so we have ordered the 'Summary of findings' tables in the same way.

In reaching conclusions regarding the applicability of evidence, we considered variations in context and culture. We extracted data on implementation and on national and local intervention contexts. We were unable to statistically investigate the role of such factors due to small numbers of studies sharing given characteristics. In addition, we considered the broader context in which most interventions were implemented, that is the USA, during a period of economic expansion, and in a country lacking universal healthcare coverage. We discuss these issues in the section Overall completeness and applicability of evidence.

RESULTS

Description of studies

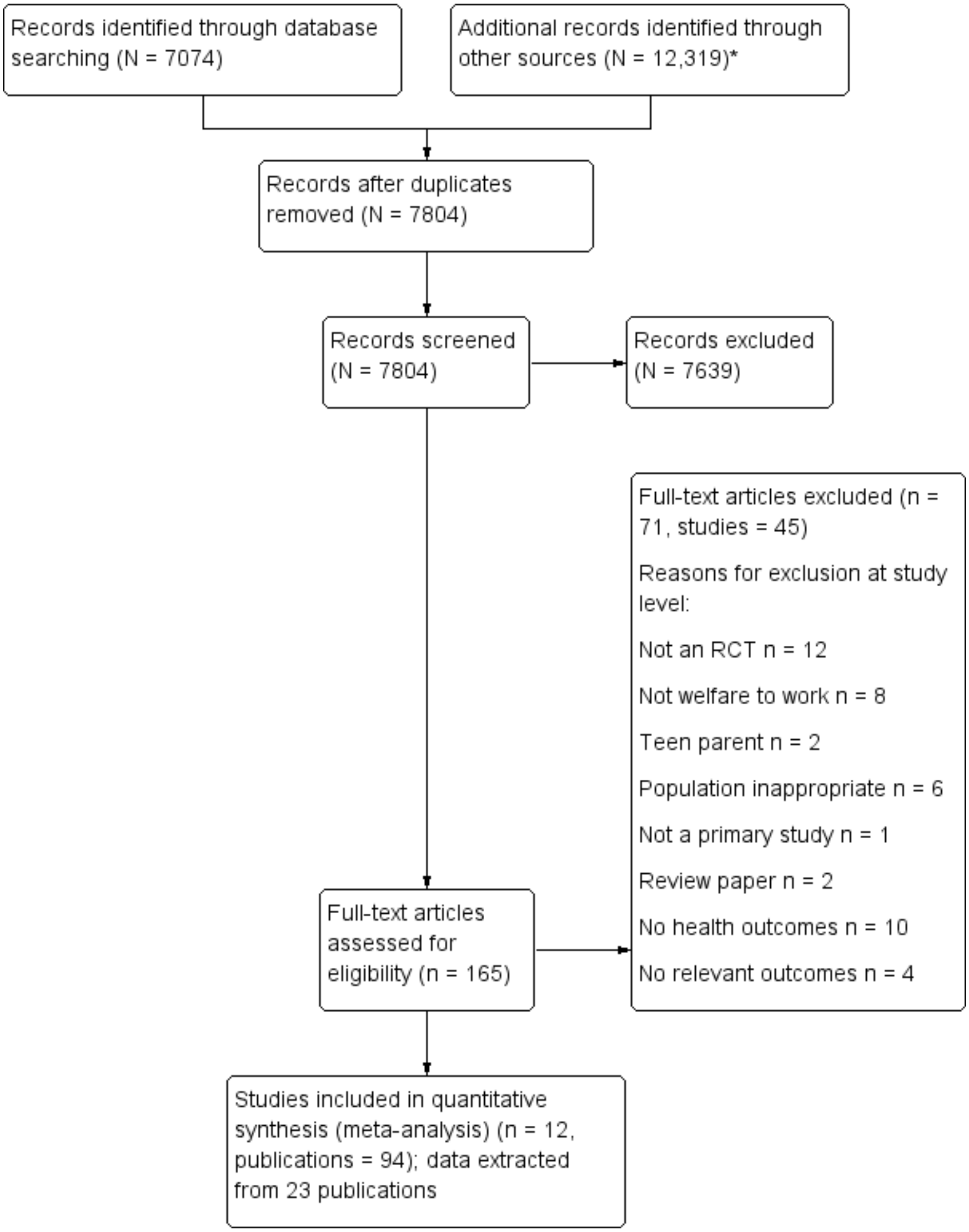
See: Characteristics of included studies; Characteristics of excluded studies.

Results of the search

We conducted database searches in 2011, 2013 and 2016. These yielded a total of 7074 references. We identified a further 12,319 references through an extensive stage of contacting authors, searching websites with searchable interfaces, and handsearching bibliographies (see Appendix 3). Because it was not possible to download the website search results to Endnote 2016, we screened the titles for relevance and identified 1609 potentially eligible records, which we added to the results of the database searches in Endnote 2016 for a total of 8683 records.

We removed 879 duplicates from the combined results of the handsearches and the database searches. This left a total of 7804 references, of which we excluded 7639 on the basis of title or abstract. We assessed 165 full-text articles for eligibility, excluding 71 records reporting on 45 studies: 12 were not RCTs, 10 reported no health outcomes, 8 were not welfare to work, 8 were aimed at teen parents (2 studies) or an otherwise inappropriate population (6 studies), 4 reported health outcomes that were not relevant to this review, 2 were review papers, and 1 was not a primary study. Figure 1 details the progress of citations through the screening process. After full-text screening, we included 12 studies with 94 associated publications.

Figure 1. Study flow diagram.*An initial stage of screening reduced records from all other sources to 1609. The remaining records were then de-duplicated against the Endnote library containing the electronic search results. Note this figure does not include publications found on websites without searchable databases.



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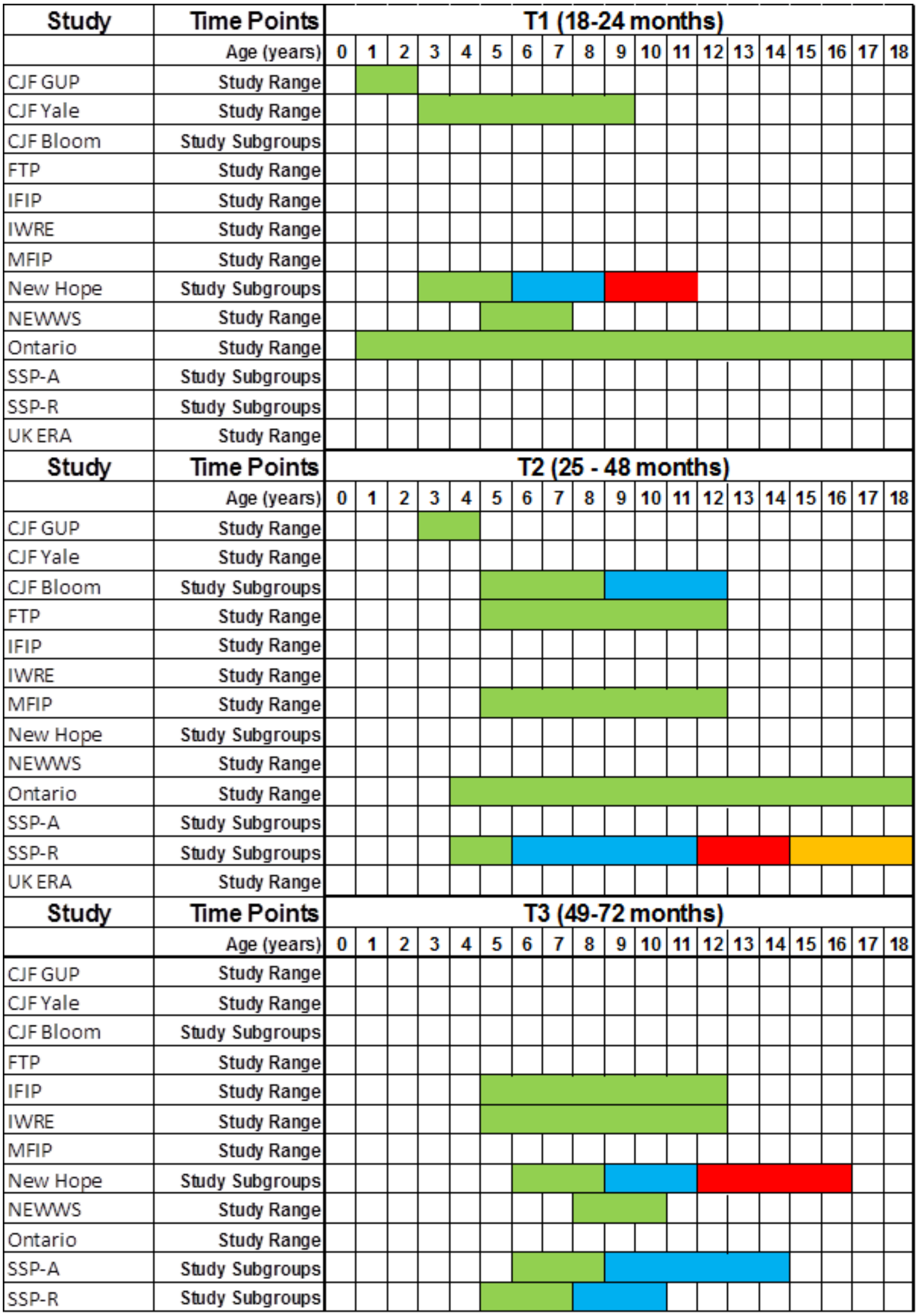
Of the 94 identified publications associated with the 12 included studies, many did not report outcomes relevant to this review. Thirty-four of these publications met all of our inclusion criteria, including reporting relevant outcomes. In some cases authors reported the same outcomes in two or more publications. To prevent double counting, we tabulated all reported outcomes in each publication and cross-checked to ensure that each instance of a given outcome was extracted only once. Where discrepancies in data were identified, we contacted study authors to confirm the correct values. Following this process, we identified 23 publications reporting unique outcome data for the 12 included studies. We reference these 23 publications in the Included studies section. We include all other publications in the Additional references section. Nine of the included records came from the database searches, and we identified the remaining fourteen by handsearching only.

Included studies

Study characteristics

Twelve studies met all of the inclusion criteria for this review. Three independent groups evaluated one intervention, Connecticut Jobs First. The Manpower Demonstration Research Corporation (now known as MDRC) conducted the main evaluation (CJF 2002). However, two further groups of researchers analysed additional independent samples from the same study: Yale University (CJF Yale 2001) and the Growing Up in Poverty project at the University of California at Berkeley (CJF GUP 2000). They selected samples on the basis of the focal child's age that were mutually exclusive, as shown in Figure 2. For ease of description throughout the review, we allocated a separate study ID for each (CJF Yale 2001 and CJF GUP 2000, respectively) and created separate 'Characteristics of studies' tables.

Figure 2.   Age of children at time of data collection by time point



Study	Time Points	T1 (18-24 months)																		
	Age (years)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
CJF GUP	Study Range		🟩	🟩																
CJF Yale	Study Range				🟩	🟩	🟩	🟩	🟩	🟩	🟩									
CJF Bloom	Study Subgroups																			
FTP	Study Range																			
IFIP	Study Range																			
IWRE	Study Range																			
MFIP	Study Range																			
New Hope	Study Subgroups				🟩	🟩	🟩	🟦	🟦	🟦	🟥	🟥	🟥							
NEWWS	Study Range						🟩	🟩	🟩											
Ontario	Study Range		🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩
SSP-A	Study Subgroups																			
SSP-R	Study Subgroups																			
UK ERA	Study Range																			

Study	Time Points	T2 (25 - 48 months)																		
	Age (years)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
CJF GUP	Study Range				🟩	🟩														
CJF Yale	Study Range																			
CJF Bloom	Study Subgroups						🟩	🟩	🟩	🟩	🟦	🟦	🟦	🟦						
FTP	Study Range						🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩						
IFIP	Study Range																			
IWRE	Study Range																			
MFIP	Study Range						🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩						
New Hope	Study Subgroups																			
NEWWS	Study Range																			
Ontario	Study Range					🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩
SSP-A	Study Subgroups																			
SSP-R	Study Subgroups					🟩	🟩	🟦	🟦	🟦	🟦	🟦	🟦	🟥	🟥	🟥	🟧	🟧	🟧	🟧
UK ERA	Study Range																			

Study	Time Points	T3 (49-72 months)																		
	Age (years)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
CJF GUP	Study Range																			
CJF Yale	Study Range																			
CJF Bloom	Study Subgroups																			
FTP	Study Range																			
IFIP	Study Range						🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩						
IWRE	Study Range						🟩	🟩	🟩	🟩	🟩	🟩	🟩	🟩						
MFIP	Study Range																			
New Hope	Study Subgroups							🟩	🟩	🟩	🟦	🟦	🟦	🟥	🟥	🟥	🟥	🟥		
NEWWS	Study Range									🟩	🟩	🟩								
Ontario	Study Range																			
SSP-A	Study Subgroups							🟩	🟩	🟩	🟦	🟦	🟦	🟦	🟦	🟦				
SSP-R	Study Subgroups						🟩	🟩	🟩	🟦	🟦	🟦								

Characteristics of evaluation teams

North American research organisations led or were closely involved in most included studies. MDRC was directly responsible for the evaluation of Connecticut Jobs First (CJF 2002), New Hope (New Hope 1999), the National Evaluation of Welfare to Work Strategies (NEWWS 2001), the Minnesota Family Investment Program (MFIP 2000), California's Greater Avenues for Independence (California GAIN 1994), and the Family Transition Program (FTP 2000). MDRC also collaborated closely with its sister organisation, the Social Research and Demonstration Corporation (SRDC), on the Canadian Self-Sufficiency Program (SSP Applicants 2003; SSP Recipients 2002), and with the UK Department for Work and Pensions (DWP) on the UK Employment Retention and Advancement demonstration (UK ERA 2011). Mathematica Policy Research evaluated the Iowa Family Investment Program (IFIP 2002), and Abt Associates conducted the Indiana Welfare Reform Evaluation (IWRE 2002). An academic team was responsible for only one study, conducted in Ontario (Ontario 2001). In most cases, state-level government departments initiated the studies, often in conjunction with federal government departments such as the US Department of Health and Human Services or the Canadian Department of Human Resources and Skills Development. The UK Department for Work and Pensions launched UK ERA 2011, and the regional level government in Ontario initiated Ontario 2001. Only New Hope 1999 differed in this respect, as it was initiated by a community organisation with a very clear aim of ensuring participants were better off in work.

Objectives of interventions

In all cases, the primary objective of the interventions was to promote labour market participation and increase economic self-sufficiency. Many interventions had supplementary objectives of either reducing welfare rolls or making work pay. We discuss these in further detail below.

Theory of change

Eleven of the 12 included studies included a logic model or a textual description of hypothesised pathways linking the intervention to child outcomes. Only California GAIN 1994 did not report a theory of change in the publication extracted for this review. Studies hypothesised that programme messages regarding employment and training, along with sanctions, case management and other intervention components, might lead to changes in direct, targeted outcomes such as income and/or employment. These might influence intermediate outcomes such as material resources, parental stress and mental health, parenting, and use of formal or informal child care. Each of these may affect children's outcomes either through direct material changes or via changes in parental stress levels. Increased attendance at informal or formal child care could lead to increased exposure to educational experiences and to infectious illnesses. At each stage in the model, from targeted outcomes to effects on children, there is the potential for effects to be either positive or negative. There may also be positive effects on some outcomes and negative effects for others. Effects may also vary depending on level of exposure or interactions between intervention components. An example of a logic model used by study authors is provided in Figure 3.

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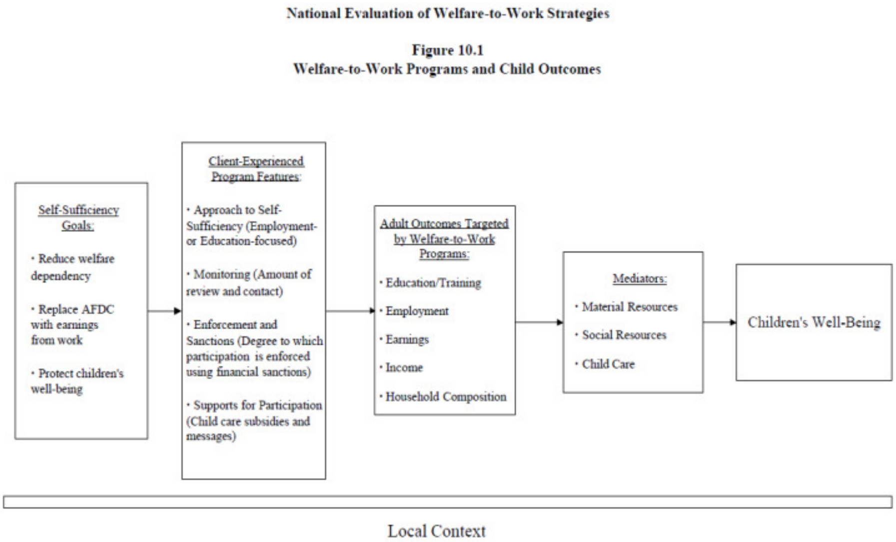
Figure 3. Example of study logic model from NEWWS 2001 Copyright © 2001 MDRC: reproduced with permission.



Sample size

Many of the included studies were large and complex. Total sample sizes ranged from 765 in Ontario 2001 to 66,400 in IWRE 2002. However, in most larger studies, only administrative data were collected for participants, with a subsample (usually defined by age of the focal child) of these surveyed to assess health outcomes. Where this was the case, we extracted economic data only for the relevant subsample. All sample sizes are provided in Characteristics of included studies.

Study design

All included studies were randomised controlled trials. Randomisation was at the level of the individual.

Setting

Of the 12 included studies, 8 took place in the USA (CJF 2002; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; New Hope 1999; NEWWS 2001), 3 in Canada (Ontario 2001; SSP Applicants 2003; SSP Recipients 2002) and 1 in the UK (UK ERA 2011). Most evaluations began between 1991 and 1996. California GAIN 1994 began in 1986 and UK ERA 2011 in 2003. SSP Applicants 2003 and SSP Recipients 2002 reported exclusively using all staff and premises for delivery of the intervention. FTP 2000 reported assigning each client a case manager and an employment and training worker who worked on premises kept apart from the control group. New Hope 1999 reported that 'project representatives' delivered the intervention but did not specify the place of delivery. CJF 2002, NEWWS 2001 and Ontario 2001 reported using standard welfare caseworkers to deliver the intervention. California GAIN 1994, IFIP 2002 and MFIP 2000 described staff as GAIN, IFIP and MFIP caseworkers respectively, but it was unclear whether these were standard personnel or recruited specifically to deliver the intervention. UK ERA 2011 employed specialised Advancement Support Advisers to assist participants post-employment.

Political and economic context

All but one of the included studies took place during periods of increasing public and political opposition to welfare payments as well as reductions in the value of and entitlements to benefits. The economic contexts in which the studies were conducted varied, with eight studies reporting good economic conditions (CJF 2002; IFIP 2002; MFIP 2000; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011), two studies reporting a period of recession or economic restraint (California GAIN 1994; Ontario 2001), and two reporting no information on economic context (FTP 2000; IWRE 2002).

Participants

Participants were lone mothers and their children. Some studies included small percentages of lone fathers but used feminine terminology throughout due to the overwhelming majority of participants being women. Adult ages ranged from 18 to 54, and child ages ranged from 18 months to 18 years. Since the interventions were aimed at lone parents in receipt of welfare, participants in all studies had low socioeconomic status. All studies included both existing welfare recipients and new applicants. Most of the study samples comprised unemployed lone parents, as identified by the study authors. However, in many studies a proportion of the sample were married and living with their spouse at randomisation (range from 0% to 33.9%; 12% or under in 8 of 12 studies. California GAIN 1994, CJF GUP 2000, SSP Applicants 2003 and SSP Recipients 2002 did not report data on the current family structure of respondents), and a proportion of the sample were also working but still receiving welfare at randomisation or in the year prior to the study (range from 1.7% to 69%). We present full population characteristics in the Characteristics of included studies tables.

Study subgroups

A number of studies collected or reported data for subgroups of recipients, defined by intervention status (in multi-arm studies), location, child age and welfare receipt status. These are described below and summarised in Table 4. We describe the manner in which these were included in meta-analyses in Data synthesis.

MFIP 2000 was particularly complex, having a total of 10 intervention subgroups defined by intervention type, location and recipient status. Two interventions were included in MFIP 2000: the full welfare-to-work intervention (MFIP) and MFIP 2000 Incentives Only (MFIP-IO), which provided only financial incentives to those who gained employment but did not involve any compulsion. MFIP-IO was delivered only to urban respondents. Thus, the 10 groups in MFIP 2000 comprised: long-term urban MFIP; long-term urban MFIP-IO; long-term urban control; recent urban MFIP; recent urban MFIP-IO; recent urban control; long-term MFIP rural; long-term rural control; recent rural MFIP and recent rural control. We combined experimental and control groups as appropriate. A number of outcomes were not reported for every subgroup. Where this was the case, we appended the relevant forest plot with an explanatory footnote.

NEWWS 2001 was also a complex study, with two co-interventions delivered at three sites. One intervention group received a labour force attachment (LFA) intervention, intended to place participants in employment of any kind as rapidly as possible, while the other received a human capital development (HCD) intervention, aimed at increasing respondents' employability by enhancing their skills. Thus there were a total of six groups within the NEWWS 2001 study. However, one group (Riverside HCD) differed systematically from the rest of the sample, since the HCD intervention was only available to respondents who lacked basic skills. We therefore excluded this group from the meta-analyses.

For each of the studies listed in Table 4, not all outcomes were reported for each subgroup, so samples included in meta-analyses may vary by outcome within studies. These instances are signalled in the footnotes of each meta-analysis.

Child age ranges and subgroups

All studies collected data on differing age groups of children, with ages ranging from 18 months to 18 years. Figure 2 shows the age groups and subgroups reported by each study at each time point. In some cases, trials reported child outcomes only by subgroups. Data synthesis describes the manner in which these were included in meta-analyses.

At T1, reported age ranges were 18 months to 3 years (CJF GUP 2000), 3 to 10 years (CJF Yale 2001), 5 to 7 years (NEWWS 2001), and 3 to 12 years (New Hope 1999). Ontario 2001 included children aged 2 to 18 years. At T2, SSP Recipients 2002 reported data on children ranging from 3 to 18 years, and CJF 2002, FTP 2000 and MFIP 2000 reported data on children aged 5 to 12 years. Ontario 2001 included children aged 4 to 18, and CJF GUP 2000, children aged 3 to 5 years. At T3, children in NEWWS 2001 were aged 8 to 10 years, and children in New Hope 1999 were aged 6 to 16. IFIP 2002 and IWRE 2002 reported data on children ranging from 5 to 12 years, and SSP Applicants 2003 included children aged 6 to 14 years. SSP Recipients 2002 reported data on children aged 5.5 to 9.5 years.

Intervention characteristics

Ethics

Except for New Hope 1999, all interventions implemented in the USA were compulsory, and investigators did not seek any form of consent for participation in the study (CJF 2002; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; NEWWS 2001). In Canada all interventions were voluntary (Ontario 2001; SSP Applicants 2003; SSP Recipients 2002), as was UK ERA 2011. All of the voluntary studies described a process of obtaining informed consent from participants prior to randomisation.

Length of follow-up

The data we report were collected between 18 months and 18 years after randomisation. Four studies reported relevant health outcomes at two follow-up time points (CJF 2002; NEWWS 2001; Ontario 2001; SSP Recipients 2002). New Hope 1999 reported outcomes at three follow-ups. An independent team of researchers linked data from two studies to mortality data at 15 to 18 years (CJF 2002; FTP 2000). Table 3 shows all follow-up times we report. We describe the manner in which we analyse the follow-ups in Data synthesis.

Exposure to the intervention

At T1 and T2, all data reported were from samples that were still exposed to the intervention. In CJF 2002 and FTP 2000, a proportion of the sample would have reached lifetime limits for welfare receipt and ceased to receive earnings disregards. They would still have been exposed to sanctions, training and case management. At T3, a number of interventions had ended, and sample members were no longer exposed to intervention conditions. These included IFIP 2002; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002 and UK ERA 2011. Interventions were ongoing in IWRE 2002 and NEWWS 2001. NEWWS 2001 had not included financial support at any time, and the time-limited earnings disregards provided by IWRE 2002 would no longer have been available to the intervention group. There was an expectation that impacts would continue after the interventions had ended because early labour market entry would allow respondents to accrue labour market advantage in terms of job quality and earnings, and that this could contribute to a better environment for children, with lasting health benefits.

Ethos and approach

Although the overarching aim of all included interventions was to promote employment among lone parents in receipt of welfare benefits, the motivation or ethos underlying this objective differed, as did the approach to achieving it. We describe these differences in detail in Description of the intervention. Briefly, interventions had one of the following motivations.

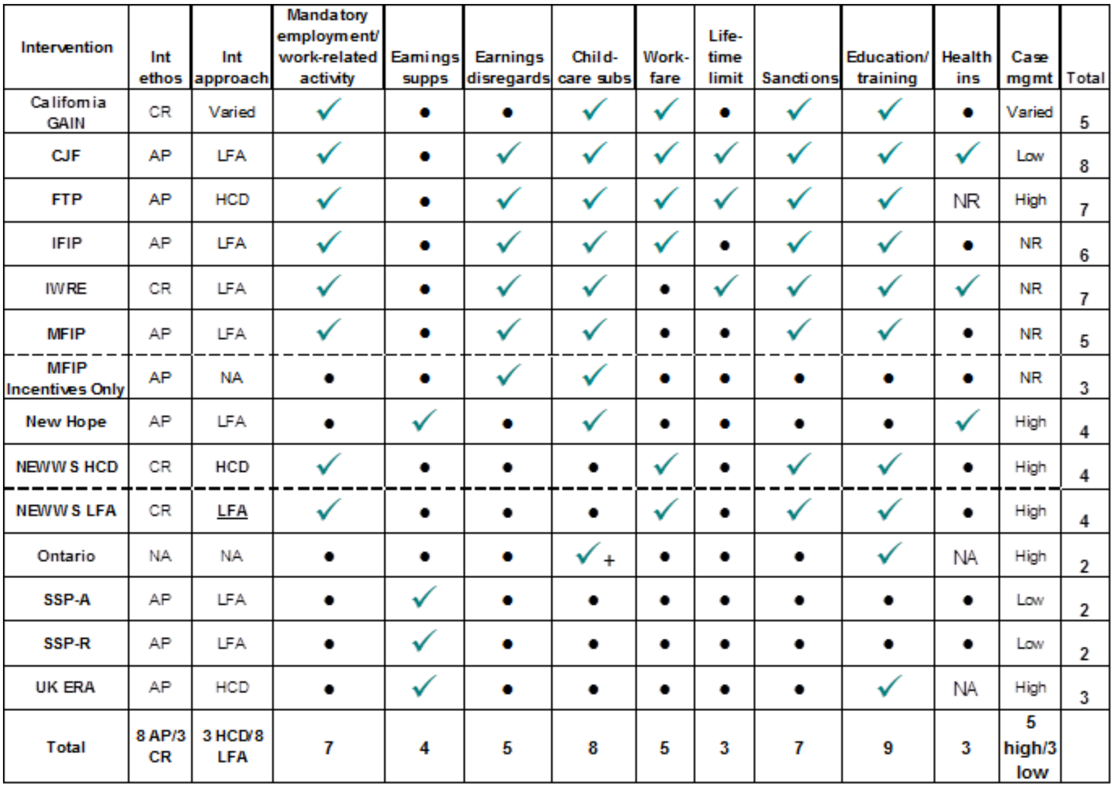
1. Caseload reduction (CR) interventions attempted to move recipients off welfare as quickly as possible, regardless of job quality or in-work income.
2. Anti-poverty (AP) interventions attempted to increase the incomes of former recipients when in employment.

Two approaches were adopted in pursuit of these aims.

1. Labour force attachment (LFA) emphasised rapid integration in the labour market.
2. Human capital development (HCD) aimed to promote skills development in order to secure better quality employment.

Either LFA or HCD approaches could be adopted by CR or AP interventions. Figure 4 provides information about all studies' ethos and approach.

Figure 4.



Intervention	Int ethos	Int approach	Mandatory employment/ work-related activity	Earnings supps	Earnings disregards	Child-care subs	Work-fare	Life-time limit	Sanctions	Education/ training	Health ins	Case mgmt	Total
California GAIN	CR	Varied	✓	●	●	✓	✓	●	✓	✓	●	Varied	5
CJF	AP	LFA	✓	●	✓	✓	✓	✓	✓	✓	✓	Low	8
FTP	AP	HCD	✓	●	✓	✓	✓	✓	✓	✓	NR	High	7
IFIP	AP	LFA	✓	●	✓	✓	✓	●	✓	✓	●	NR	6
IWRE	CR	LFA	✓	●	✓	✓	●	✓	✓	✓	✓	NR	7
MFIP	AP	LFA	✓	●	✓	✓	●	●	✓	✓	●	NR	5
MFIP Incentives Only	AP	NA	●	●	✓	✓	●	●	●	●	●	NR	3
New Hope	AP	LFA	●	✓	●	✓	●	●	●	●	✓	High	4
NEWWS HCD	CR	HCD	✓	●	●	●	✓	●	✓	✓	●	High	4
NEWWS LFA	CR	LFA	✓	●	●	●	✓	●	✓	✓	●	High	4
Ontario	NA	NA	●	●	●	✓+	●	●	●	✓	NA	High	2
SSP-A	AP	LFA	●	✓	●	●	●	●	●	●	●	Low	2
SSP-R	AP	LFA	●	✓	●	●	●	●	●	●	●	Low	2
UK ERA	AP	HCD	●	✓	●	●	●	●	●	✓	NA	High	3
Total	8 AP/3 CR	3 HCD/8 LFA	7	4	5	8	5	3	7	9	3	5 high/3 low	

Eight studies meeting the inclusion criteria were AP interventions (CJF 2002; FTP 2000; IFIP 2002; MFIP 2000; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). Three studies evaluated CR interventions (California GAIN 1994; IWRE 2002; NEWWS 2001). Four studies evaluated HCD interventions (FTP 2000; California GAIN 1994; NEWWS 2001; UK ERA 2011), and eight evaluated LFA interventions (CJF 2002; IFIP 2002; IWRE 2002; MFIP 2000; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; NEWWS 2001 had both an LFA arm and an HCD arm). Ontario 2001 did not fall into any of these categories. There was no apparent relationship within the included studies between aim and approach - AP and CR interventions adopted both LFA and HCD approaches, although interventions that adopted an AP ethos alongside an LFA approach predominated, with six of the included studies adopting this combination (CJF 2002; IFIP 2002; MFIP 2000; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002). However, in practice this typology did not prove as useful as anticipated. Even where study authors stated that the intervention explicitly adopted one of the above approaches, in practice there often seemed to be little variation between interventions of differing types. For instance, a number of LFA interventions offered training, and this did not necessarily differ in level or scope from that offered by HCD interventions.

Implementation

Study authors often reported that implementation of interventions varied widely within studies. This variation occurred both at the level of intervention ethos and approach, and at the level of individual components, as might be expected in complex interventions with multiple components delivered in different sites and settings.

Intervention components

We identified 10 individual components in the interventions (see Figure 4). Except those in UK ERA 2011, control group respondents were also subject to many of these components, such as employment requirements and earnings disregards, to varying degrees. Thus, we describe only those intervention components that represent an incentive, sanction or service over and above what the control group received.

Three studies tested variants of the main intervention with two or more intervention arms. NEWWS 2001 delivered parallel LFA and HCD interventions in three different sites. Ontario 2001 tested the impact of five different approaches to delivering support to single parents. Two groups within the study received employment training and are included in the review. One of these groups also received child care and support from health visitors. MFIP 2000 included an incentive-only arm (MFIP-IO) whose recipients were not subject to mandatory work requirements but received earning supplements and other benefits if they chose to return to work. The only participants who received additional programme benefits such as earnings supplements, childcare subsidies and health insurance were working or engaged in work-related activity for a specified number of hours per week, apart from Ontario 2001, which provided child care regardless of employment status. Intervention components were as follows.

1.Mandatory employment or employment-related activity

Seven interventions featured compulsory job searching, training, work placements or other employment-related activity (California GAIN 1994; CJF 2002; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; NEWWS 2001). Respondents in the intervention group were required to actively seek employment or participate in employment-related activity for a specified numbers of hours per week. Failure to do so could result in financial sanctions involving partial or total cessation of welfare benefits for a specified period of time. In MFIP 2000, respondents in the recent applicant group and in the MFIP-IO arm of the intervention were not required to be available for work; employment requirements applied only to those in the long-term group who had been out of work for 24 months of the previous 36.

2. Earnings supplements

Participants in four studies received top-ups to earned income when they worked for a specified number of hours per week or over a given period (New Hope 1999; SSP Applicants 2003; SSP Recipients 2002 (minimum 30 hours work per week, tapering above a certain level of earnings); UK ERA 2011 (up to six payments of GBP 400 for each period when participants worked 30 or more hours per week for 13 out of 17 weeks)). Supplements were limited to a period of three years. While supplements were being paid, respondents' total income could increase even if their earned income was low.

3. Earnings disregards

Five interventions disregarded a proportion of earned income when calculating welfare entitlement (CJF 2002; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000). Methods of calculating and levels of generosity varied across studies. Where earned income was disregarded, respondents could claim welfare while earning at much higher levels than previously. However, in CJF 2002, FTP 2000 and IWRE 2002, these periods while working and claiming welfare counted towards the respondent's lifetime limit on welfare receipt. While respondents received earnings disregards, total welfare receipt and numbers on welfare were higher. As with supplements, disregards could increase total income even if earned income was low.

4. Childcare subsidies

All but four interventions provided childcare subsidies (NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). Financial contributions toward the cost of child care were made either directly to childcare providers or to parents for a period of one to two years following uptake of employment. Ontario 2001 provided a childcare programme to one arm of the intervention only.

5. Workfare

Five studies featured compulsory work placements, or 'workfare', in order to qualify for benefits (California GAIN 1994; CJF 2002; FTP 2000; IFIP 2002; NEWWS 2001). This differs from requirements to work or to take steps towards work (component 1) in that participants were assigned a specific placement (in the public, private or voluntary sector), which they had to attend for a set number of hours per week in order to continue receiving benefits, and they were not paid at a normal market rate. New Hope 1999 assigned participants who were unsuccessful in finding work to community service jobs, but these were seen as proper employment and paid at the market rate.

6. Lifetime limits

The package of welfare reforms passed in the USA in 1996 included a federal lifetime limit of 60 months of welfare receipt, with individual states retaining the freedom to apply shorter limits. CJF 2002, FTP 2000, and IWRE 2002 included lifetime limits on welfare receipt. IFIP 2002 did not include time limits over and above those applying to the whole sample under a federal waiver granted in 1993. Studies conducted in the UK and Canada did not involve time limits on benefit receipt, but eligibility for supplements or other programme benefits was time limited (Ontario 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). California GAIN 1994 predated the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, and both MFIP 2000 and New Hope 1999 were designed with the expectation that there would be no time limits on welfare receipt. MFIP 2000 was able to maintain this under the intervention conditions, but New Hope 1999 participants were not held back from lifetime limits after the implementation of Wisconsin Works in 1997.

The CJF 2002 time limit was 21 months. FTP 2000 recipients were limited to 24 months of cash assistance in a 60-month period, and IWRE 2002 stopped benefits after 24 months. For recipients who found employment, the period in which they received earnings disregards and other programme benefits counted towards their welfare ’clock’. Thus, there was a transition point where they went from working and receiving many other benefits to relying solely on earned income. Advisors had some discretion in the application of time limits and could grant extensions where they judged recipients to have made a good faith effort or to have been incapacitated through ill health.

7. Sanctions

Seven of the studies used partial or total cessation of welfare benefits for a designated period in response to non-compliance with some aspect of the interventions’ work requirements (CJF 2002; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; NEWWS 2001). Sanctions varied in severity across interventions. For instance, MFIP 2000 removed 10% of an individual’s Temporary Assistance for Needy Families (TANF) payments whilst IFIP 2002 in some cases removed all of a claimant’s benefits for a six-month period. Rates of sanctioning also varied within and between interventions. The MFIP 2000 recent applicant group were not sanctioned for failure to take part in work-related activities until they had been in receipt of welfare benefit for 24 months out of a 36-month period. The MFIP-IO group were not required to seek work and were not sanctioned for failure to do so. As voluntary interventions, New Hope 1999, Ontario 2001, SSP Applicants 2003, SSP Recipients 2002 and UK ERA 2011 did not include sanctions, although as noted above, any earnings supplements or other programme benefits were withdrawn if participants did not meet minimum work-related activity requirements.

8. Education and/or training

Most of the interventions included some form of education, training or both, whether they were explicitly described as HCD or LFA. In some this was limited to job searching skills or short-term courses (CJF 2002; MFIP 2000; Ontario 2001). Others provided a basic training course to bring participants to the level of a US high school graduate, followed by short vocational courses if participants were still unable to find work (California GAIN 1994; IFIP 2002; both the LFA and HCD components of NEWWS 2001). FTP 2000 developed an extensive set of services around training and development, including assigning specific staff to each participant, funding ongoing training for those who found employment, and developing training work placements in conjunction with local employers. New Hope 1999, SSP Applicants 2003 and SSP Recipients 2002 did not provide training but did provide advice and referrals to suitable courses. UK ERA 2011 also provided information, but in addition paid for training and provided bonuses of up to GBP 1000 on completion of training.

9. Health insurance subsidies

Three interventions subsidised participants’ health insurance (CJF 2002; IWRE 2002; New Hope 1999). CJF 2002 provided transitional Medicaid for two years after participants found employment, and IWRE 2002 subsidised health insurance while participants’ incomes remained below the federal poverty level. New Hope 1999 offered a subsidised health insurance scheme to respondents who were not eligible for employment-based health insurance or Medicaid. MFIP 2000 participants were eligible for Minnesota’s subsidised health insurance scheme, but this was not an intervention component. California GAIN 1994, FTP 2000, IFIP 2002, MFIP 2000 and NEWWS 2001 provided no health insurance over and above that available to control group members on gaining employment. Ontario 2001, SSP Applicants 2003, SSP Recipients 2002 and UK ERA 2011 were delivered in countries with universal healthcare systems, so health insurance was not a relevant component.

10. Case management

Case management was the method whereby individual ’cases’ within welfare-to-work programmes were managed and controlled. Case managers were generally responsible for a wide range of tasks, including: client orientation, assessment, transmission of core messages, activity assignment, monitoring and tracking participation and progress, responding to non-compliance, maintaining case files, dealing with outside providers and providing pre- and post-employment advice. In practice, case management differed in terms of levels of contact, flexibility, enforcement and monitoring. The case manager/participant ratio also varied widely across interventions in response to both available resources and programme design. Based on each of these dimensions, we categorised the interventions as having high or low case management. FTP 2000, New Hope 1999, NEWWS 2001, Ontario 2001 and UK ERA 2011 all provided high levels of case management. In CJF 2002, SSP Applicants 2003 and SSP Recipients 2002, levels of case management were low. Case management in California GAIN 1994 varied across the study sites, and IFIP 2002, IWRE 2002 and MFIP 2000 reported insufficient detail to assign a level.

Control condition

For studies conducted in the USA (CJF 2002; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; New Hope 1999; NEWWS 2001), the control condition prior to the passage of PRWORA in 1996 represented ’usual care’, that is, control group members in all interventions were eligible for standard

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welfare benefits under Aid for Families with Dependent Children (AFDC). Following the passage of PRWORA, the intervention condition was in fact 'usual care' as the interventions were rolled out statewide while they were being evaluated. Control group members in CJF 2002, FTP 2000, IWRE 2002 and MFIP 2000 were held back on previous conditions for the purposes of evaluation. IFIP 2002 was terminated after 3.5 years, and all respondents were moved to TANF. Wisconsin Works was introduced in 1997 and affected all respondents in New Hope 1999. Under AFDC, conditions varied to some degree from state to state. Commonly, however, AFDC was not time limited and included: an earned income disregard at a value considerably below that of most interventions; work requirements that commenced when the youngest child was older than those in the interventions; shorter periods of eligibility for transitional Medicaid and childcare assistance, and less severe sanctions for non-compliance. Receipt of welfare benefits was not subject to time limits. In the Canadian studies, control group members continued to be eligible for Income Assistance (Ontario 2001; SSP Applicants 2003; SSP Recipients 2002). Usual care in Canada varied across states and also changed during the course of the interventions. SSP Applicants 2003 and SSP Recipients 2002 took place in New Brunswick and British Columbia. Initially in both states work requirements were minimal. During the intervention, British Columbia introduced a six-month ban for those who left a job without just cause, reduced Income Assistance levels and reduced earnings disregards for receiving it. By contrast, in New Brunswick earnings disregards increased. During the evaluation of the Ontario 2001 intervention, the state administration introduced work requirements for parents of school-aged children. In the UK, usual care for lone parents involved no work requirement other than attending a work-focused interview twice a year until 2008, when lone parents with a youngest child aged 12 or over (2008) and 7 and over (October 2009) were transferred to Jobseeker's Allowance (JSA), which is a conditional out-of-work benefit (UK ERA 2011). There was no time limit on benefit receipt.

Primary outcomes

Studies used a range of measures and formats to report primary and secondary outcomes within and between studies and across different time points. The following provides a summary of which outcomes were reported by each intervention. Appendix 5 includes further details including the time points at which each outcome was reported. Although we searched for parental health outcomes, the vast majority of the sample in all included studies was female. Therefore, we describe adult health outcomes as 'maternal' for the remainder of the review.

Maternal mental health

All 12 studies reported maternal mental health outcomes. Nine studies used the Centre for Epidemiological Studies Depression Scale (CES-D) (CJF 2002; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002), and two used the Composite International Diagnostic Interview (CIDI) (CJF Yale 2001; Ontario 2001). These are both well validated and widely used measures of risk of depression in adults. They were reported both as a continuous measure (mean total score), and as a dichotomous measure (proportion scoring above a cutpoint defined as 'at risk of depression'). Ontario 2001 used an alternative version of the CIDI scale (University of Michigan, Composite International Diagnostic Interview). Two studies used non-validated self-report measures of mental health; California GAIN 1994 asked respondents how often they were unhappy or depressed, and UK ERA 2011 asked how often respondents felt miserable or depressed. These were five-item scales reported as dichotomous outcomes.

Maternal physical health

Five studies reported a measure of maternal physical health using a five-item measure of self-rated health ranging from poor to very good or excellent (CJF Yale 2001; California GAIN 1994; New Hope 1999; Ontario 2001; UK ERA 2011). CJF Yale 2001 reported the percentage of the sample with one or more physical health problems, while California GAIN 1994, Ontario 2001 and UK ERA 2011 reported the percentage in good or very good health, and New Hope 1999 reported the mean score on the five-item scale.

Child mental health

Ten studies reported child mental health measures (CJF 2002; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; New Hope 1999; NEWWS 2001; Ontario 2001; SSP Applicants 2003; SSP Recipients 2002). The widely used Behavior Problems Index (BPI), which provides the score of responses to single items, was reported by CJF 2002, CJF Yale 2001, FTP 2000, IFIP 2002, IWRE 2002, MFIP 2000 and NEWWS 2001. New Hope 1999 used the Problem Behavior Scale (PBS), while SSP Applicants 2003 and SSP Recipients 2002 reported the Behavior Problems Scale (BPS). These score each item from 1 to 3 or 1 to 5 (depending on the age of the child) and calculate the mean of the score for each item in the scale. Other measures reported included the Survey Diagnostic Instrument (SDI; reported by Ontario 2001) and the Child Behavior Checklist (CBC; reported by CJF GUP 2000). Investigators collected all of these measures via parent report. In addition, SSP Recipients 2002 collected one measure of adolescent depression risk (CES-D % at risk) via self-report.

Child physical health

Nine studies reported a measure of child physical health. In five of these, mothers rated their child's health on a five-point scale ranging from poor to very good or excellent (CJF 2002; FTP 2000; IWRE 2002; New Hope 1999; NEWWS 2001). SSP Applicants 2003 and SSP Recipients 2002 reported the mean score across items using a four-item instrument. IFIP 2002 reported the percentage of children with fair or poor health, and MFIP 2000 reported the percentage with good or excellent health. All of these outcomes were collected via parent report.

Secondary outcomes

Employment

Ten studies reported employment outcomes (CJF 2002; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). All employment measures were dichotomous, reporting the percentage of the sample employed or not employed for a given measure. Measures reported were: currently employed (CJF 2002; CJF GUP 2000; CJF Yale 2001; FTP 2000; IFIP 2002; IWRE 2002; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002); currently employed full-time (IFIP 2002; New Hope 1999; NEWWS 2001; SSP Applicants 2003; UK ERA 2011); currently employed part-time (IFIP 2002; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011); ever employed since randomisation (CJF Yale 2001; California GAIN 1994; MFIP 2000; NEWWS 2001); ever employed in the year of data collection (CJF 2002; FTP 2000; New Hope 1999; UK ERA 2011); and ever employed full- or part-time since randomisation (California GAIN 1994; MFIP 2000; SSP Recipients 2002). In some cases we derived these measures by, for instance, summing categorical outcomes that reported hours of work per week in order to calculate values for full- and part-time employment. We defined full-time employment as 30 or more hours per week.

Income and earnings

Nine studies reported measures of income. CJF 2002, FTP 2000, MFIP 2000 and New Hope 1999 reported total average income for the year of data collection. IWRE 2002 reported income for the month prior to the survey annualised to represent the previous year's income. IFIP 2002 and NEWWS 2001 reported average income in the month prior to the survey. SSP Applicants 2003 and SSP Recipients 2002 reported average income per month in the six months prior to data collection. At 60 months, NEWWS 2001 also reported total income for years 1 to 5. Income included earnings, food stamps, supplements provided by the intervention and AFDC/TANF payments. IWRE 2002, New Hope 1999, SSP Applicants 2003 and SSP Recipients 2002 also included earned income tax credit (EITC) in the total income figure.

Eleven studies reported a measure of earnings. CJF 2002, MFIP 2000, New Hope 1999, SSP Applicants 2003, SSP Recipients 2002 and UK ERA 2011 reported average total earnings for the year prior to the survey. IWRE 2002 reported annualised earnings in the month prior to the survey. California GAIN 1994 reported average weekly earnings since randomisation, and IFIP 2002 reported average earnings in the month prior to the survey. NEWWS 2001 reported total earnings for years 1 to 5. FTP 2000 did not report earnings directly, but we calculated this by subtracting income from AFDC/TANF and food stamps from the figure for total income.

Many of the interventions included either an earned income disregard or a financial supplement in order to make work pay and ease the transition from welfare to work. Most of these were time limited, with limits ranging from 21 to 36 months (although extensions were often available for people with particular difficulties). Where earned income was disregarded, respondents could claim welfare while earning at much higher levels than previously. However, the periods while working and claiming welfare counted towards the respondent's lifetime limit on welfare receipt. While supplements or disregards were being paid, respondents' total income could increase even if their earned income was low. Obviously when time limits were reached, this effect ceased. In all cases, time limits were reached during the period defined as T2 (24 to 48 months). A number of studies also reported total earnings. We extracted both measures in order to investigate the relationship between earned and total income.

Welfare receipt

Ten studies used a number of different measures to report welfare receipt (CJF 2002; FTP 2000; IFIP 2002; IWRE 2002; New Hope 1999; NEWWS 2001; Ontario 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). CJF 2002, FTP 2000, IFIP 2002, MFIP 2000, New Hope 1999, SSP Applicants 2003 and SSP Recipients 2002 reported the average amount received in the year prior to the survey. IWRE 2002 reported the average amount received in the month prior to the survey, annualised, and NEWWS 2001 reported the total amount of benefit received between years 1 and 5. UK ERA 2011 reported the average amount of benefits received per week. IFIP 2002, IWRE 2002, NEWWS 2001 and UK ERA 2011 reported the proportion of the sample currently in receipt of benefit. New Hope 1999 and Ontario 2001 reported the proportion of the sample receiving benefits in the year prior to the survey. Since lower levels of total welfare paid and of numbers claiming welfare are the desirable outcomes from policy makers' perspectives, we defined these as positive in the analyses. It should be noted that in a number of interventions, welfare received and the proportion of the sample on welfare would be expected to increase in the short- to medium-term, as higher disregards of earned income in calculating welfare entitlement led to continuing eligibility for welfare while working. This effect would

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be expected to decrease in the medium- to long-term, however, as eligibility for disregards expired.

Health insurance

Six of the studies conducted in the USA reported data on health insurance (CJF 2002; California GAIN 1994; IFIP 2002; MFIP 2000; New Hope 1999; NEWWS 2001). Ontario 2001, SSP Recipients 2002, SSP Applicants 2003 and UK ERA 2011 took place in Canada and the UK, where the state provides universal health coverage. Therefore these studies did not report data on health insurance. All health insurance outcomes were dichotomous and measured in many different ways, precluding meta-analysis. Effect sizes were calculated for all reported measures.

Excluded studies

See Results of the search; Characteristics of excluded studies.

Risk of bias in included studies

All studies had at least one item at high risk of bias, with two studies having four domains at high risk (NEWWS 2001; Ontario 2001). All but two studies were at low risk of bias for allocation concealment and sequence generation, and it is very likely that these two studies conducted these but did not report it (IFIP 2002; IWRE 2002). Blinding of outcome assessment was rare, and only one study reported baseline outcome measurements (Ontario 2001).

All risk of bias judgements are presented in the Characteristics of included studies tables and summarised in Figure 5 and Figure 6. Since all studies were at high risk in at least one domain, the summary judgement was that all the included studies were at high risk of bias.

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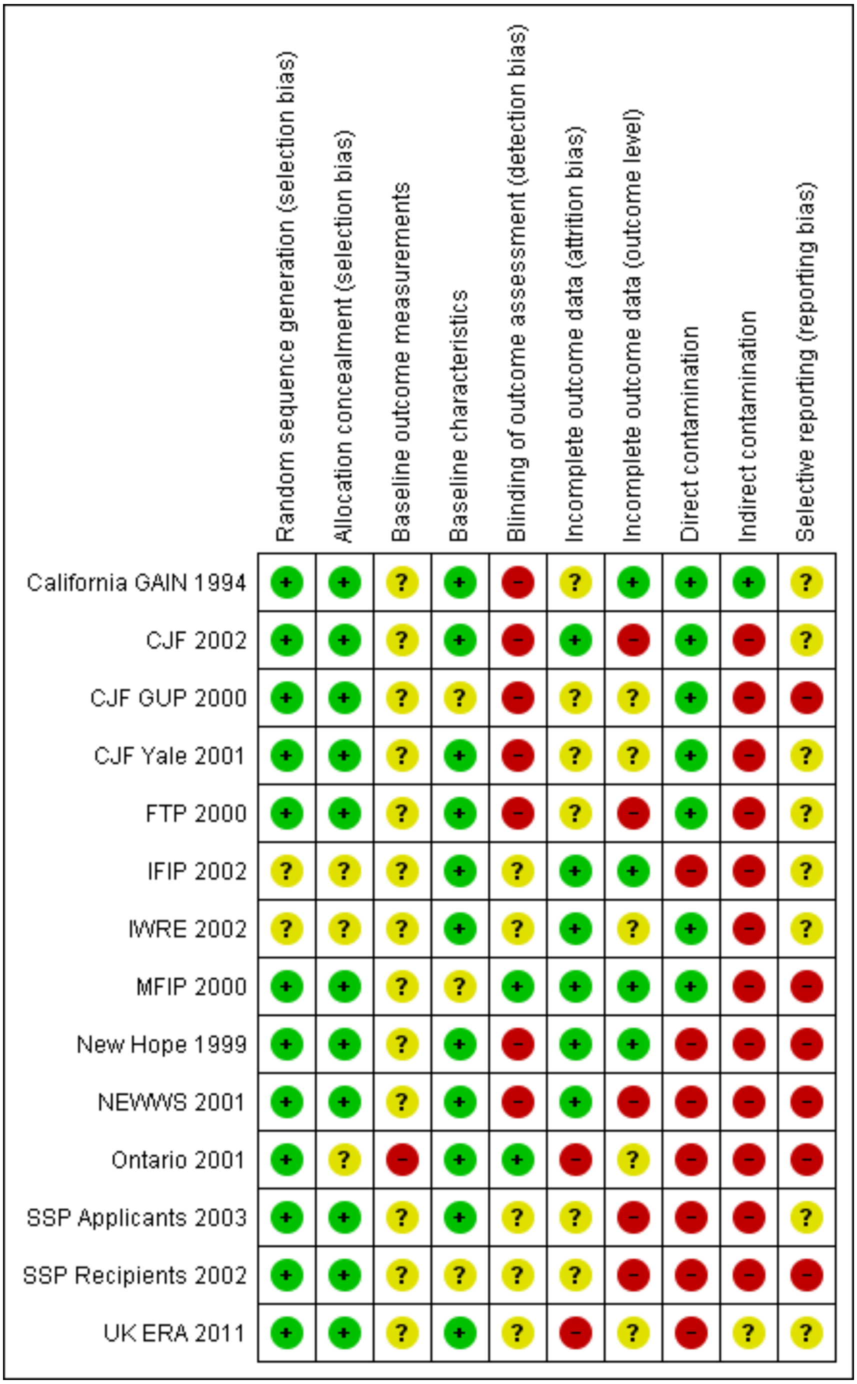
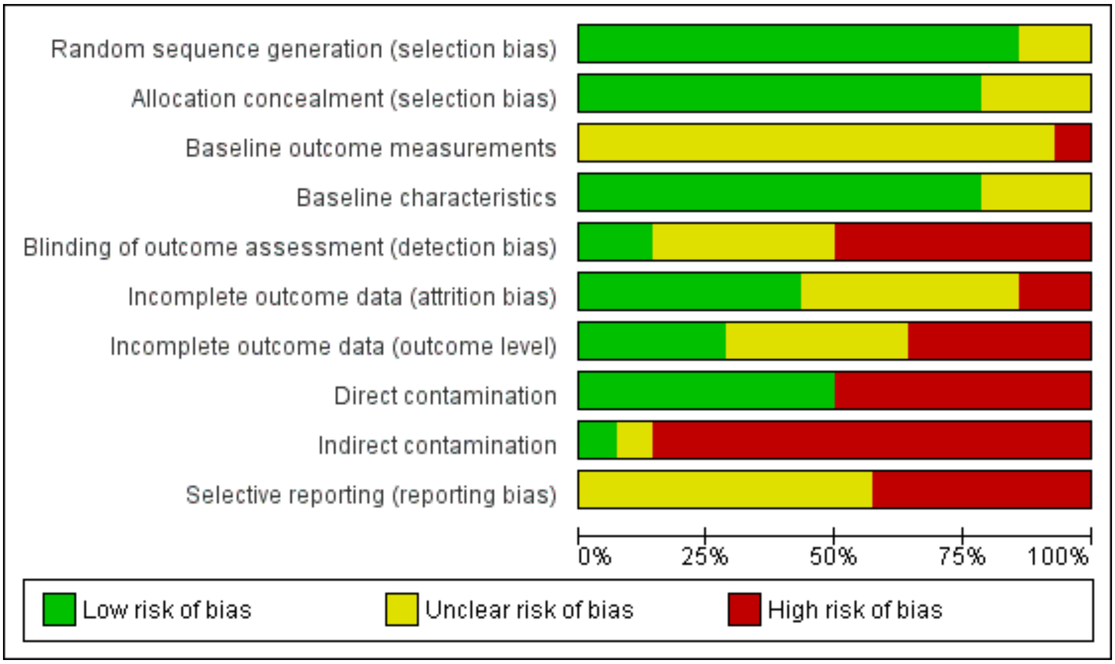
Figure 5. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.



	Random sequence generation (selection bias)	Allocation concealment (selection bias)	Baseline outcome measurements	Baseline characteristics	Blinding of outcome assessment (detection bias)	Incomplete outcome data (attrition bias)	Incomplete outcome data (outcome level)	Direct contamination	Indirect contamination	Selective reporting (reporting bias)
California GAIN 1994	+	+	?	+	−	?	+	+	+	?
CJF 2002	+	+	?	+	−	+	−	+	−	?
CJF GUP 2000	+	+	?	?	−	?	?	+	−	−
CJF Yale 2001	+	+	?	+	−	?	?	+	−	?
FTP 2000	+	+	?	+	−	?	−	+	−	?
IFIP 2002	?	?	?	+	?	+	+	−	−	?
IWRE 2002	?	?	?	+	?	+	?	+	−	?
MFIP 2000	+	+	?	?	+	+	+	+	−	−
New Hope 1999	+	+	?	+	−	+	+	−	−	−
NEWWS 2001	+	+	?	+	−	+	−	−	−	−
Ontario 2001	+	?	−	+	+	−	?	−	−	−
SSP Applicants 2003	+	+	?	+	?	?	−	−	−	?
SSP Recipients 2002	+	+	?	?	?	?	−	−	−	−
UK ERA 2011	+	+	?	+	?	−	?	−	?	?

Figure 6. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies.



Allocation

Allocation concealment and sequence generation

Nine studies were conducted by or in partnership with large North American non-profit research companies, with well-established reputations for conducting good quality research (MDRC, formerly Manpower Demonstration Research Corporation, and its sister organisation, the Social Research and Demonstration Corporation (SRDC)) (CJF 2002; California GAIN 1994; FTP 2000; MFIP 2000; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). As such, they adopt robust procedures for sequence generation; communication with study authors confirmed this. The description of MDRC's sequence generation procedure, as provided by Cynthia Miller of MDRC, is available in Appendix 6. Where reports explicitly describe allocation concealment, it is clearly conducted correctly, as in the following text:

"FTP staff members placed a phone call to MDRC and read a few items from the BIF [background information form] to an MDRC clerk. Using this information, individuals were randomly assigned to either the FTP or the AFDC group by a computer program on site at MDRC" (FTP 2000).

As described in the *Cochrane Handbook of Systematic Reviews of Interventions* (Higgins 2011a), we based our judgement of other studies conducted by the same organisation on our knowledge of FTP 2000, concluding that for all MDRC and SRDC studies, allocation concealment was 'probably done'. One study took place in an academic setting (Ontario 2001). While authors clearly described adequate methods of sequence generation for this study, they provided no information about allocation concealment, leading to a judgement of unclear risk of bias. Private (for-profit) research organisations conducted IFIP 2002 and IWRE 2002 (Mathematica Associates and Abt Associates, respectively). Since the trial reports provided no information, we judged the studies to be at unclear risk for both sequence generation and allocation concealment. However, again these are large and very reputable companies, and it is highly likely that they followed correct procedures.

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Baseline outcome measures

We assessed baseline measures at the level of individual outcomes. We assessed outcomes that were not reported at baseline to be at unclear risk of bias. Where investigators collected and adjusted for baseline measures, or reported them by intervention status with few significant differences, we assessed them to be at low risk. Where studies did not report baseline outcomes by intervention status, or where there were differences between groups at baseline and authors reported no adjustment, we judged them to be at high risk.

Twelve studies reported no health outcomes at baseline, therefore all were judged to be at unclear risk of bias (CJF 2002; CJF GUP 2000; CJF Yale 2001; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). Ontario 2001 reported all baseline outcome measures, but these differed across intervention groups and authors did not describe any adjustment, so we assessed it as being at high risk of bias. NEWWS 2001 reported and adjusted for maternal mental health at baseline but did not collect any other health outcomes at baseline, and we deemed it to be at unclear risk of bias.

Baseline characteristics

We assessed risk of bias in the domain of baseline characteristics at study level. Where studies reported baseline characteristics by intervention group and showed them to have no statistically significant differences, or where they used regression to adjust for baseline differences, we assigned a judgement of low risk of bias. We considered that 11 studies met these criteria (CJF 2002; CJF Yale 2001; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; New Hope 1999; NEWWS 2001; Ontario 2001; SSP Applicants 2003; UK ERA 2011). Three studies were at unclear risk of bias as they did not present baseline characteristics by intervention group and did not report adjusting for all characteristics (CJF GUP 2000; MFIP 2000; SSP Recipients 2002).

Blinding

Blinding of outcome assessment was conducted at the level of individual outcomes. With few exceptions, investigators assessed health outcomes through face-to-face surveys. All mental and physical health outcomes were self-report measures. Six studies reported that data collectors were not blinded, and we assessed them to be at high risk of bias (California GAIN 1994; CJF 2002; CJF GUP 2000; CJF Yale 2001; New Hope 1999; NEWWS 2001). Five studies provided no information on blinding of outcome assessors (IFIP 2002; IWRE 2002; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). Although it is very unlikely that assessors were blinded, we judged studies to be at unclear risk in the absence of further information. MFIP 2000 collected 'sensitive' outcomes via Audio-Enhanced, Computer-Assisted Self-Interviewing and we judged it to be at low risk of bias. In Ontario 2001, although all data were collected face-to-face, outcome assessors were blinded for all outcomes, so we assessed the study to be at low risk of bias.

Incomplete outcome data

We conducted risk of bias assessment for missing outcome data at study level and at outcome level. At study level, we assessed unit non-response (attrition), and at outcome level we assessed item non-response.

At study level, we considered six studies that reported using weighting or imputation to deal with missing data to be at low risk of bias (CJF 2002; IFIP 2002; IWRE 2002; MFIP 2000; New Hope 1999; NEWWS 2001). Six studies reported attrition of up to 29% but did not discuss reasons for attrition, and we judged them to be at unclear risk of bias (CJF Yale 2001; CJF GUP 2000; California GAIN 1994; FTP 2000; SSP Applicants 2003; SSP Recipients 2002). We assessed two studies to be at high risk of bias (Ontario 2001; UK ERA 2011). At two years, response rates for Ontario 2001 varied across groups, ranging from 39% to 58%, and by four years, the response rate had increased to 78.5% of baseline, with no further information provided. UK ERA 2011 reported a response rate at follow-up of 62%, and the authors noted that more disadvantaged respondents were more likely to drop out. Compared to the larger sample that used administrative data, data from the survey overestimated impacts on earnings, and the authors urge caution in interpreting the findings. Thus, we consider that the risk of bias from missing outcome data is particularly high for this study.

We deemed four studies to be at low risk of bias from missing item-level data for all outcomes. California GAIN 1994 and IFIP 2002 reported that item non-response was low, while both MFIP 2000 and New Hope 1999 used multiple imputation to account for missing item data. We assessed five studies that provided no information on missing item level data to be at unclear risk (CJF Yale 2001; CJF GUP 2000; IWRE 2002; Ontario 2001; UK ERA 2011). Authors reported that sample sizes may have varied for individual outcomes in CJF 2002, FTP 2000, NEWWS 2001, SSP Applicants 2003 and SSP Recipients 2002, so we assigned a high risk of bias.

Contamination

We judged all but California GAIN 1994 as being at high risk of bias due to contamination. We could describe contamination in these studies as either indirect, that is, where the control group were likely to have been influenced by changes in social attitudes towards welfare and by awareness of changing rules affecting the majority of the population, or direct, where there was evidence that the control group were actually subject to the treatment condition at some point during the study.

In the USA, following the passage of PRWORA in 1996, welfare policies very similar to those applied to the experimental groups were implemented nationwide. In Canada, restrictions to welfare benefits for lone parents were also implemented in the late 1990s, and in the UK requirements to seek employment were placed on lone parents of successively younger children. As a result, the control group were directly affected by the new policies in a number of studies. New Hope 1999 (T3 data only), NEWWS 2001 (T3 data only), Ontario 2001 (T2 data only), SSP Applicants 2003, SSP Recipients 2002 and UK ERA 2011 all operated during periods when welfare policies changed, and investigators were unable to prevent new requirements applying to control groups. In most cases it is difficult to be sure how much these changes affected controls. NEWWS 2001 reported that 15% of Atlanta and 7% of Grand Rapids controls had some exposure to the intervention at T3. In New Hope 1999 and UK ERA 2011, only control group members in receipt of benefits, and in the case of UK ERA 2011, with a youngest child aged under 12 (2008) or 10 (2009) would have been affected. We judged all of these studies to be at high risk of bias from direct contamination.

Five US studies, known collectively as the Child Waiver Impact Experiments (CWIE), operated after the implementation of welfare reform and intended to maintain experimental conditions for the duration of the study (CJF 2002; FTP 2000; IFIP 2002; IWRE 2002; MFIP 2000). All were successful in this except IFIP 2002, since the intervention was terminated and the control group moved to the new state level policy three and a half years after randomisation. We judged IFIP 2002 to be at high risk of bias from direct contamination and the remainder to be at low risk. Media coverage and publicity, as well as changed attitudes to welfare, accompanied the new policies, and there is evidence that some control group respondents in the CWIE studies believed themselves to be subject to the new rules (Moffitt 2004). However, in most cases the evaluation teams made concerted efforts to minimise contamination and ensure that control groups were aware of conditions pertaining to them, and they argued that substantial treatment-control differences remained (Bloom 1999). We judged all of the CWIE studies to be at high risk of bias from indirect contamination.

It is also likely that New Hope 1999, NEWWS 2001, Ontario 2001, SSP Applicants 2003, SSP Recipients 2002 and UK ERA 2011 were affected by attitude changes and awareness of more restrictive policies. We deemed these to be at high risk of indirect contamination. We believe that only California GAIN 1994, which was conducted prior to the introduction of TANF, was at low risk of bias from indirect contamination.

It is likely that contamination bias would lead to an underestimation of impacts on economic outcomes among the intervention group, as control group members endeavoured to find employment in the mistaken belief that this was now required of them. Underestimation of impacts is not deemed to be as serious as overestimation (Higgins 2011a); however, it is difficult to be sure what effect this type of contamination would have had on health outcomes.

Selective reporting

We assessed selective outcome reporting at study level. Protocols were not available for any of the included studies, and studies that reported data for more than one time point or subgroup rarely reported outcomes consistently across groups or times. We assessed six studies to be at high risk of bias for this reason (CJF GUP 2000; MFIP 2000; New Hope 1999; NEWWS 2001; Ontario 2001; SSP Recipients 2002). NEWWS 2001 reported maternal mental health at T1 but not at T3. We assessed a further eight studies to be at unclear risk because there was no way to ascertain whether they reported all planned outcomes (CJF 2002; California GAIN 1994; FTP 2000; IFIP 2002; IWRE 2002; Ontario 2001; SSP Applicants 2003; UK ERA 2011).

Other potential sources of bias

Government bodies, which arguably had a vested interest in the success of the interventions, funded and participated in all included studies except New Hope 1999. Sources of funding are recognised as potential sources of bias. However, as stated, the evaluations involved highly reputable research organisations that have made major contributions to the development of methods for conducting social experiments in their own right. As such, there is no suggestion that the findings were in any way influenced by the source of funding.

Quality of the evidence

See Summary of findings for the main comparison; Summary of findings 2.

All included studies were at high risk of bias in at least one domain, therefore we downgraded all evidence once for this criterion. As a result, no evidence could attain a quality rating higher than moderate. We judged two studies to be at very high risk of bias - UK ERA 2011 due to high and systematic attrition leading to biased estimates, and Ontario 2001 (at T1) due to severe attrition (> 60%). Where these studies contributed more than 10% of the overall weight to a meta-analysis, we downgraded the evidence twice for the risk of bias criterion. We downgraded much economic evidence at T3 due to the inclusion of UK ERA 2011 in the analyses. However, exclusion of this study had only marginal effects on the estimates. We also downgraded some health outcomes at T3 due to UK ERA 2011's very high risk of bias.

We considered few effects to be at serious risk of inconsistency. If heterogeneity was more than 50%, effect directions differed, and we could not identify any plausible explanation, we downgraded the evidence once for inconsistency. Where heterogeneity was high but there was a plausible explanatory hypothesis, we did not downgrade and presented a post hoc sensitivity analysis in Effects of interventions. Similarly, if I^2 was above 50% but all effects were in the same direction, we did not downgrade for inconsistency. We discuss these instances in Effects of interventions.

In relation to indirectness, the population of interest for this review was lone parents in receipt of welfare benefits in high-income countries. Since the populations of all included studies met these criteria, we did not downgrade for indirectness. None of the outcomes included in the review were indirect measures, so we did not downgrade for indirectness in relation to outcomes.

We did downgrade a number of health outcomes for imprecision due to low event rates. Since we had no reason to suspect that other studies have been conducted but remained unpublished, we did not downgrade any outcomes for publication bias. We assessed outcomes for which an effect size could not be calculated as being of unclear quality.

In most domains, there were a number of measures of the same outcome that we could not include in a meta-analysis. Within each domain, there was often a range of quality assessments for different measures. We based an overall assessment for the domain as a whole on the grade assigned to the analysis or analyses with the largest total sample size. On this basis, of the 12 health domains, we assessed all as moderate quality except T1 maternal mental health (low quality), T3 maternal physical health (low quality) and T3 child mental health (unclear quality). We assessed all T1 and T2 economic domains as moderate quality and all T3 ones as low quality. We report these domain level assessments in the domain summaries in Effects of interventions.

Effects of interventions

See: **Summary of findings for the main comparison** Welfare to work for lone parents. Maternal health outcomes; **Summary of findings 2** Welfare to work for lone parents. Child health outcomes

Although authors explicitly described many of the interventions as adopting a certain ethos or approach (CR/AP, HCD/LFA; see Description of the intervention), we found that in practice, they did not actually differ from one another as much as expected. In addition, there were too few of a given type at each time point to permit grouping them by type for meta-analysis. For this reason, we included all interventions that reported suitable data at each time point in the meta-analyses. The comparison in all cases was with usual care (see Description of the intervention).

As described in Data synthesis, we grouped the interventions by time point (T1 = 18 to 24 months, T2 = 25 to 48 months, T3 = 49 to 72 months) and synthesised the outcomes by time point and domain (e.g. T1 maternal mental health), as this was how studies reported results. In most cases, it was not possible to include all outcomes in a given domain in a single meta-analysis, either because there was a mixture of continuous and dichotomous outcomes, because dichotomous outcomes reported were heterogeneous, or because authors did not report a measure of variance. We reported these outcomes narratively in the text, and where it was possible to calculate an effect size, we presented it in forest plots. Since it can be challenging to comprehend the range of analyses, particularly where there is a mixture of meta-analyses and narrative reporting, tables summarising all of the main analyses conducted are available at Web appendix 1 and Web appendix 2. These are designed to summarise the direction and strength of effects, as well as the quality of evidence available, in a way that readers can apprehend visually. Upward and downward pointing arrows indicate positive and negative directions of effect, respectively, defined in terms of the desirability of the outcome (e.g. an upward pointing arrow is used for a reduction in CES-D, as this indicates better mental health). A single arrow represents a ’very small’ effect, two arrows a ’small’ effect, and three a ’modest’ effect, as defined in Table 5. A ’o’ indicates that there is evidence of no effect. The colour of the arrow denotes the quality: green indicates moderate quality; amber, low quality; and red, very low quality. Where we could not assess quality, we used black.

For dichotomous outcomes, we defined the ’event’ as reported by study authors, whether it was considered a ’good’ or a ’bad’ outcome. For instance, when calculating employment, we defined the good outcome (being employed) as the event, although traditionally the bad outcome is considered the event (Alderson 2009). In some outcomes, risk ratios (RRs) are high because there are so few events; when event and non-event are reversed, the effect size is much smaller. However, we reported the RRs in this way because this is how the original studies reported them. We identify instances where the ’good’ outcome is defined as the event as such in the ’Summary of findings’ tables (Summary of findings for the main comparison; Summary of findings 2).

Effect sizes across virtually all outcomes were small (i.e. SMD 0.20 to 0.49) or trivial (SMD > 0.20) using Cohen’s rule for interpreting SMDs or RRs (Cohen 2013). However, there is debate regarding the utility of these rules for interpreting the effects of population level interventions, since an effect that appears small or even tiny when considered at the level of the individual may be important if replicated across a large population (Kunzli 2000, Siontis 2011). Cohen has stated that effect sizes observed outside laboratory conditions are likely to be small, and that use of his definitions of effect magnitude warrant caution (Cohen 2013). Other authors have also argued that in interventions which affect large populations, an SMD of 0.10 could be important if replicated across the population (Coe 2002). We therefore employed a modified approach to defining effect sizes, taking an SMD of < 0.10 to represent a ’very small’ effect, 0.11 to 0.20 a ’small’ effect, and > 0.20 a ’modest’ effect. ’Very small’ effects are unlikely to be important, particularly where the confidence intervals (CIs) cross the line of null effect (Ryan 2016). We present our definitions in Table 5 alongside those recommended by Cohen. The effect magnitude for RRs below 1 is calculated by subtracting 1 from the RR then multiplying by 100, such that RR 0.80 to 0.50 is equivalent to RR 1.20 to 1.50, and RR 0.81 to 0.99 is equivalent to RR 1.01 to 1.19. These are defined as small and very small effects, respectively.

Primary outcomes

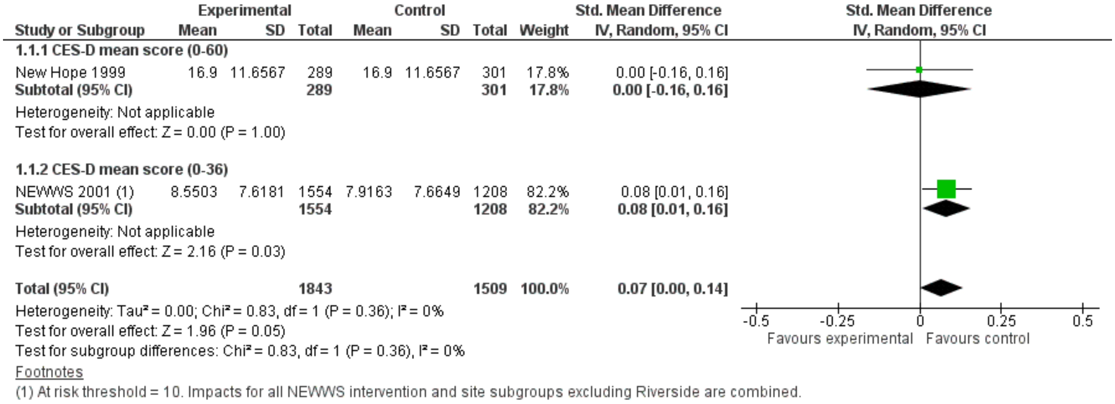
Maternal mental health

Time point 1 (T1): 18 to 24 months since randomisation

All five studies reporting at T1 reported a measure of maternal mental health. New Hope 1999 and NEWWS 2001 reported continuous measures (CES-D scale mean scores 0 to 60 and 0 to 36, respectively), and a further three studies reported dichotomous measures (CJF GUP 2000; CJF Yale 2001; Ontario 2001). We combined the continuous measures in a meta-analysis. However, the dichotomous measures reported differed across studies and were not amenable to meta-analysis.

Meta-analysis of the two continuous outcomes indicated that mental health was worse in the intervention group up to two years after the intervention. Although the evidence was of moderate quality, the effect was very small (SMD 0.07, 95% CI 0.00 to 0.14; N = 3352; 2 studies; Analysis 1.1; Figure 7). Both outcomes from the CJF substudies (CJF GUP 2000: RR 1.21, 95% CI 0.72 to 2.06, N = 308; CJF Yale 2001: RR 1.18, 95% CI 0.80 to 1.74, N = 311) indicated that mental health was better in the control group, while Ontario 2001 reported no effect of the intervention (Analysis 1.2). However, the evidence from CJF GUP 2000 and CJF Yale 2001 was of low quality due to wide confidence intervals that encompassed both no effect and appreciable harm. The evidence from Ontario 2001 was of very low quality for the same reason and due to high attrition.

Figure 7. Forest plot of comparison: 1 Time point 1 Maternal mental health, outcome: 1.1 Maternal mental health continuous.



Study or Subgroup	Experimental Mean	Experimental SD	Experimental Total	Control Mean	Control SD	Control Total	Weight	Std. Mean Difference IV, Random, 95% CI
1.1.1 CES-D mean score (0-60)								
New Hope 1999	16.9	11.6567	289	16.9	11.6567	301	17.8%	0.00 [-0.16, 0.16]
Subtotal (95% CI)			**289**			**301**	**17.8%**	**0.00 [-0.16, 0.16]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 0.00 (P = 1.00)								
1.1.2 CES-D mean score (0-36)								
NEWWS 2001 (1)	8.5503	7.6181	1554	7.9163	7.6649	1208	82.2%	0.08 [0.01, 0.16]
Subtotal (95% CI)			**1554**			**1208**	**82.2%**	**0.08 [0.01, 0.16]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 2.16 (P = 0.03)								
Total (95% CI)			**1843**			**1509**	**100.0%**	**0.07 [0.00, 0.14]**

Heterogeneity: Tau² = 0.00; Chi² = 0.83, df = 1 (P = 0.36); I² = 0%
Test for overall effect: Z = 1.96 (P = 0.05)
Test for subgroup differences: Chi² = 0.83, df = 1 (P = 0.36), I² = 0%

Footnotes
(1) At risk threshold = 10. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Time point 2 (T2): 25 to 48 months since randomisation

All of the six included studies that reported at T2 reported maternal mental health. CJF 2002, CJF GUP 2000, FTP 2000, and SSP Recipients 2002 reported CES-D mean score. CJF GUP 2000 did not report sample sizes for intervention and control groups or measures of variance, so we could not include these data in a meta-analysis. Meta-analysis of the remaining three studies provided moderate-quality evidence of no effect of the intervention on maternal mental health (SMD 0.00, 95% CI −0.05 to 0.05; N = 7091; Analysis 2.1; Figure 8). CJF GUP 2000 reported higher depression scores among the intervention group (CES-D 15.5 versus 13.9; P < 0.10; Analysis 2.2).

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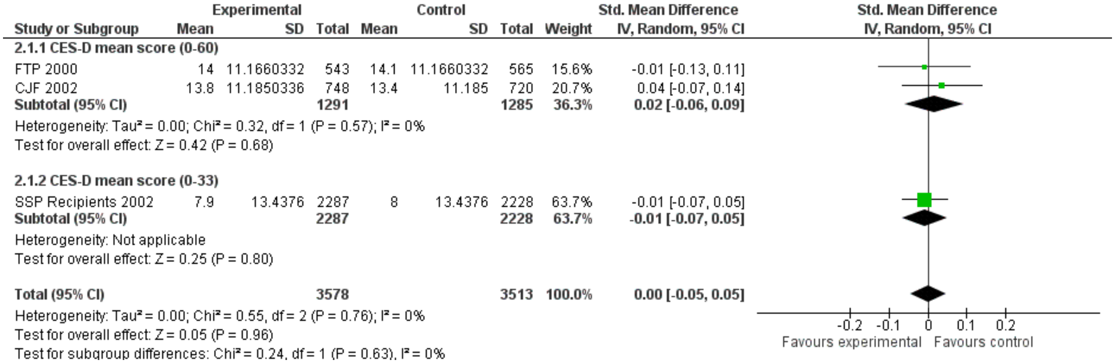
Figure 8. Forest plot of comparison: 2 Time point 2 Maternal mental health, outcome: 2.1 Maternal mental health continuous.



	Experimental			Control				Std. Mean Difference
Study or Subgroup	**Mean**	**SD**	**Total**	**Mean**	**SD**	**Total**	**Weight**	**IV, Random, 95% CI**
2.1.1 CES-D mean score (0-60)								
FTP 2000	14	11.1660332	543	14.1	11.1660332	565	15.6%	-0.01 [-0.13, 0.11]
CJF 2002	13.8	11.1850336	748	13.4	11.185	720	20.7%	0.04 [-0.07, 0.14]
Subtotal (95% CI)			**1291**			**1285**	**36.3%**	**0.02 [-0.06, 0.09]**
Heterogeneity: Tau² = 0.00; Chi² = 0.32, df = 1 (P = 0.57); I² = 0%								
Test for overall effect: Z = 0.42 (P = 0.68)								
2.1.2 CES-D mean score (0-33)								
SSP Recipients 2002	7.9	13.4376	2287	8	13.4376	2228	63.7%	-0.01 [-0.07, 0.05]
Subtotal (95% CI)			**2287**			**2228**	**63.7%**	**-0.01 [-0.07, 0.05]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 0.25 (P = 0.80)								
Total (95% CI)			**3578**			**3513**	**100.0%**	**0.00 [-0.05, 0.05]**
Heterogeneity: Tau² = 0.00; Chi² = 0.55, df = 2 (P = 0.76); I² = 0%								
Test for overall effect: Z = 0.05 (P = 0.96)								
Test for subgroup differences: Chi² = 0.24, df = 1 (P = 0.63), I² = 0%								

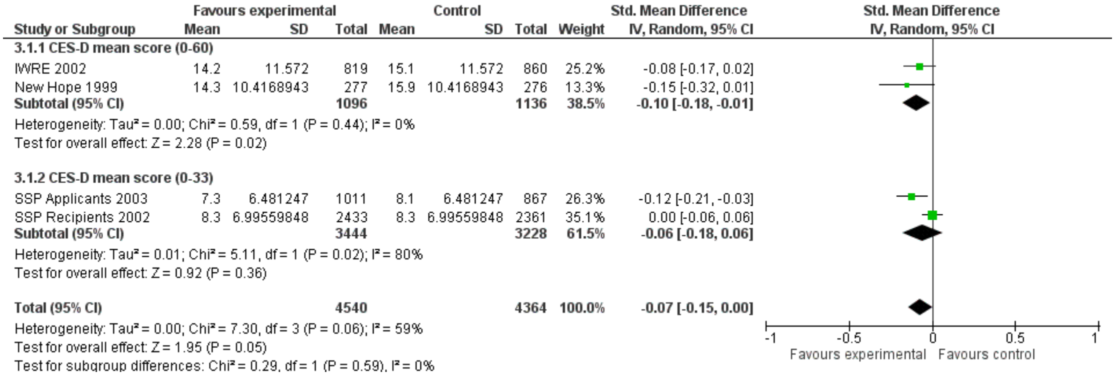
California GAIN 1994 and MFIP 2000 reported different dichotomous measures, precluding meta-analysis. California GAIN 1994 reported the percentage of the sample who said they felt unhappy, sad or depressed, and MFIP 2000 reported the percentage of respondents at high risk of depression on the CES-D scale (≥ 23/60). California GAIN 1994 reported a very small effect in favour of the control group (RR 1.06, 95% CI 0.95 to 1.18; N = 2242), and MFIP 2000 reported no effect on high risk of depression (Analysis 2.3). Evidence from both studies was of moderate quality, although the result from California GAIN 1994 was unlikely to be important as the effect was very small and the CI crossed the line of null effect.

Time point 3 (T3: 49 to 72 months since randomisation)

Six out of seven studies with follow-up at T3 reported a measure of maternal mental health. IWRE 2002, New Hope 1999, SSP Applicants 2003 and SSP Recipients 2002 reported CES-D mean score. IFIP 2002 reported the percentage at high risk of depression on the CES-D scale (≥ 23/60), and UK ERA 2011 reported the percentage who often or always felt miserable or depressed. The two dichotomous outcomes were incommensurate and not amenable to meta-analysis. NEWWS 2001 reported maternal mental health at T1 but not at T3.

Meta-analysis of four continuous outcomes provided moderate-quality evidence of a very small favourable impact on maternal mental health (SMD −0.07, 95% CI −0.15 to 0.00; N = 8873; 4 studies; Analysis 3.1; Figure 9; IWRE 2002; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002). We calculated effect sizes for the two dichotomous outcomes; there was a very small effect in favour of the intervention for high risk of depression in IFIP 2002 (RR 0.94, 95% CI 0.73 to 1.20; N = 813), while UK ERA 2011 reported a small effect in favour of control (RR 1.25, 95% CI 0.98 to 1.59; N = 1365; Analysis 3.2). However, the evidence from these studies was of low and very low quality, respectively, due to wide confidence intervals including both no effect and appreciable benefit in IFIP 2002 or harm in UK ERA 2011, and high attrition in UK ERA 2011.

Figure 9. Forest plot of comparison: 3 Time point 3 Maternal mental health, outcome: 3.1 Maternal mental health continuous.



	Favours experimental			Control				Std. Mean Difference
Study or Subgroup	**Mean**	**SD**	**Total**	**Mean**	**SD**	**Total**	**Weight**	**IV, Random, 95% CI**
3.1.1 CES-D mean score (0-60)								
IWRE 2002	14.2	11.572	819	15.1	11.572	860	25.2%	-0.08 [-0.17, 0.02]
New Hope 1999	14.3	10.4168943	277	15.9	10.4168943	276	13.3%	-0.15 [-0.32, 0.01]
Subtotal (95% CI)			**1096**			**1136**	**38.5%**	**-0.10 [-0.18, -0.01]**
Heterogeneity: Tau² = 0.00; Chi² = 0.59, df = 1 (P = 0.44); I² = 0%								
Test for overall effect: Z = 2.28 (P = 0.02)								
3.1.2 CES-D mean score (0-33)								
SSP Applicants 2003	7.3	6.481247	1011	8.1	6.481247	867	26.3%	-0.12 [-0.21, -0.03]
SSP Recipients 2002	8.3	6.99559848	2433	8.3	6.99559848	2361	35.1%	0.00 [-0.06, 0.06]
Subtotal (95% CI)			**3444**			**3228**	**61.5%**	**-0.06 [-0.18, 0.06]**
Heterogeneity: Tau² = 0.01; Chi² = 5.11, df = 1 (P = 0.02); I² = 80%								
Test for overall effect: Z = 0.92 (P = 0.36)								
Total (95% CI)			**4540**			**4364**	**100.0%**	**-0.07 [-0.15, 0.00]**
Heterogeneity: Tau² = 0.00; Chi² = 7.30, df = 3 (P = 0.06); I² = 59%								
Test for overall effect: Z = 1.95 (P = 0.05)								
Test for subgroup differences: Chi² = 0.29, df = 1 (P = 0.59), I² = 0%								

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Summary

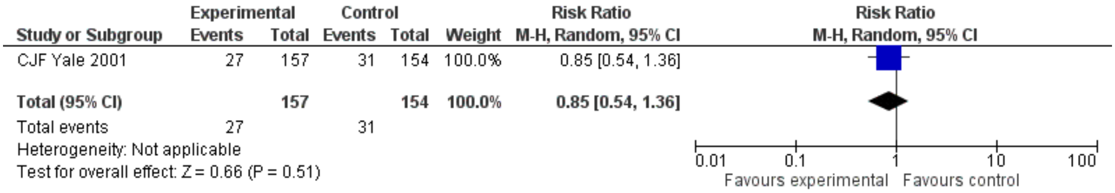
Effects on maternal mental health varied across time points, with moderate-quality evidence of a very small negative impact of the intervention at T1, no effect at T2, and a very small positive effect at T3. At T1 and T3 there were individual studies that reported larger negative effects on maternal mental health, but the evidence was of low or very low quality. One study that reported a very small negative impact at T1 did not report maternal mental health at T3. At all time points, evidence of moderate quality predominated, therefore the overall quality assessment for maternal mental health at each time point was moderate.

Maternal physical health

Time point 1 (T1): 18 to 24 months since randomisation

One study reported the percentage of the sample in fair or poor health at T1, providing evidence of low quality that the intervention group reported better health than control (RR 0.85, 95% CI 0.54 to 1.36; N = 311; Analysis 4.1; Figure 10; CJF Yale 2001). We downgraded this evidence due to imprecision. CJF GUP 2000 also collected a measure of self-reported health but did not report impacts by intervention group.

Figure 10. Forest plot of comparison: 4 Time point 1 Maternal physical health, outcome: 4.1 In poor health (%).



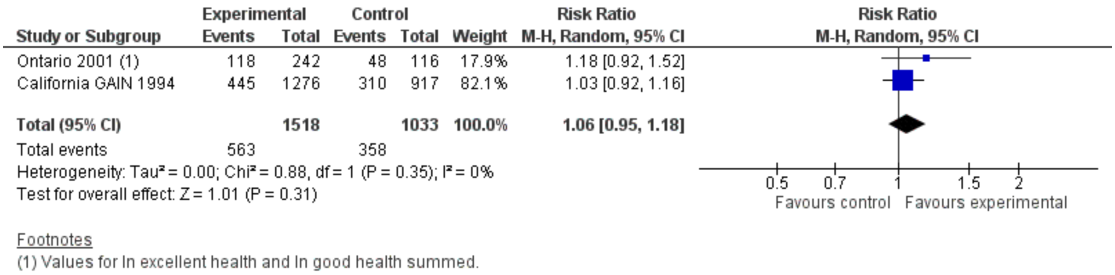
Study or Subgroup	Experimental Events	Experimental Total	Control Events	Control Total	Weight	Risk Ratio M-H, Random, 95% CI
CJF Yale 2001	27	157	31	154	100.0%	0.85 [0.54, 1.36]
Total (95% CI)		**157**		**154**	**100.0%**	**0.85 [0.54, 1.36]**
Total events	27		31			

Heterogeneity: Not applicable
Test for overall effect: Z = 0.66 (P = 0.51)

Time point 2 (T2): 25 to 48 months since randomisation

Two studies reported the percentage of the sample in good or excellent health at T2 (California GAIN 1994; Ontario 2001). Meta-analysis indicated that the intervention group reported better health than control, although this was a very small effect (RR 1.06, 95% CI 0.95 to 1.18; N = 2551; Analysis 5.1; Figure 11). Although the evidence was of moderate quality, the effect is unlikely to be important, as the effect size is very small and the CI crosses the line of null effect.

Figure 11. Forest plot of comparison: 5 Time point 2 Maternal physical health, outcome: 5.1 In good or excellent health %. Event defined as In good or excellent health.



Study or Subgroup	Experimental Events	Experimental Total	Control Events	Control Total	Weight	Risk Ratio M-H, Random, 95% CI
Ontario 2001 (1)	118	242	48	116	17.9%	1.18 [0.92, 1.52]
California GAIN 1994	445	1276	310	917	82.1%	1.03 [0.92, 1.16]
Total (95% CI)		**1518**		**1033**	**100.0%**	**1.06 [0.95, 1.18]**
Total events	563		358			

Heterogeneity: Tau² = 0.00; Chi² = 0.88, df = 1 (P = 0.35); I² = 0%
Test for overall effect: Z = 1.01 (P = 0.31)

Footnotes
(1) Values for In excellent health and In good health summed.

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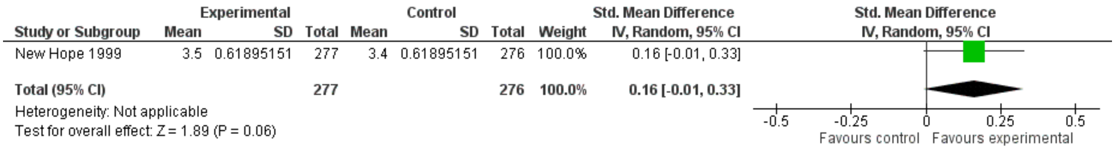
Time point 3 (T3): 49 to 72 months since randomisation

Two studies assessed self-reported physical health at T3. New Hope 1999 reported mean score on the maternal physical health scale, which showed moderate-quality evidence of a small effect in favour of the intervention (SMD 0.16, 95% CI −0.01 to 0.33; N = 553; Analysis 6.1; Figure 12). UK ERA 2011 reported the proportion of the sample with good/very good health. This showed a very small effect in favour of control (RR 0.97, 95% CI 0.91 to 1.04; N = 1854; Analysis 6.2). However, the evidence was of low quality due to high risk of bias from attrition, and the effect was unlikely to be important as it was very small and the CI crossed the line of null effect.

Figure 12. Forest plot of comparison: 6 Time point 3 Maternal physical health, outcome: 6.1 Self-reported health (1-5).



	Experimental			Control				Std. Mean Difference
Study or Subgroup	Mean	SD	Total	Mean	SD	Total	Weight	IV, Random, 95% CI
New Hope 1999	3.5	0.61895151	277	3.4	0.61895151	276	100.0%	0.16 [-0.01, 0.33]
Total (95% CI)			**277**			**276**	**100.0%**	**0.16 [-0.01, 0.33]**

Heterogeneity: Not applicable
Test for overall effect: Z = 1.89 (P = 0.06)

Summary

Only four studies reported measures of maternal physical health, and all but one reported small to very small positive effects. There was moderate-quality evidence of a very small positive effect at T2 and a small positive effect at T3. UK ERA 2011 reported a very small negative effect on maternal physical health at T3, but the evidence was of low quality. The evidence on maternal physical health at T1 and T3 was predominantly of low quality; therefore we assessed evidence at both time points to be low quality overall. At T2, the evidence was of moderate quality.

Child mental health

Time point 1 (T1): 18 to 24 months since randomisation

Four studies reported a measure of child behaviour problems at T1. New Hope 1999 and NEWWS 2001 reported mean scores for the Problem Behavior Scale (PBS) and the Behavior Problems Index (BPI), respectively. Ontario 2001 reported the proportion of the sample with three or fewer behaviour disorders as a categorical variable. We dichotomised the latter variable to create an outcome for the proportion of the sample with two or three behaviour disorders. CJF Yale 2001 reported the proportion of the sample with behaviour problems (measured using the BPI). We could not meta-analyse the dichotomous outcomes, but we calculated effect sizes.

In a meta-analysis including New Hope 1999 and NEWWS 2001, heterogeneity was high ($I^2 = 75\%$, P = 0.05). In a post hoc analysis, we calculated individual effect sizes for the outcomes showing that New Hope 1999 had a small positive impact on the intervention group (SMD −0.17, 95% CI −0.34 to −0.01; N = 563), and NEWWS 2001 had a very small negative effect (SMD 0.01, 95% CI −0.06 to 0.09; N = 2762; Analysis 7.1; Figure 13). We hypothesised that intervention characteristics caused this heterogeneity, as New Hope 1999 was a voluntary anti-poverty intervention that provided a generous earnings supplement, while NEWWS 2001 by contrast was mandatory and offered no earnings supplement. While income showed a small increase in New Hope 1999, there was a very small decrease in NEWWS 2001. Evidence from each study was of moderate quality.

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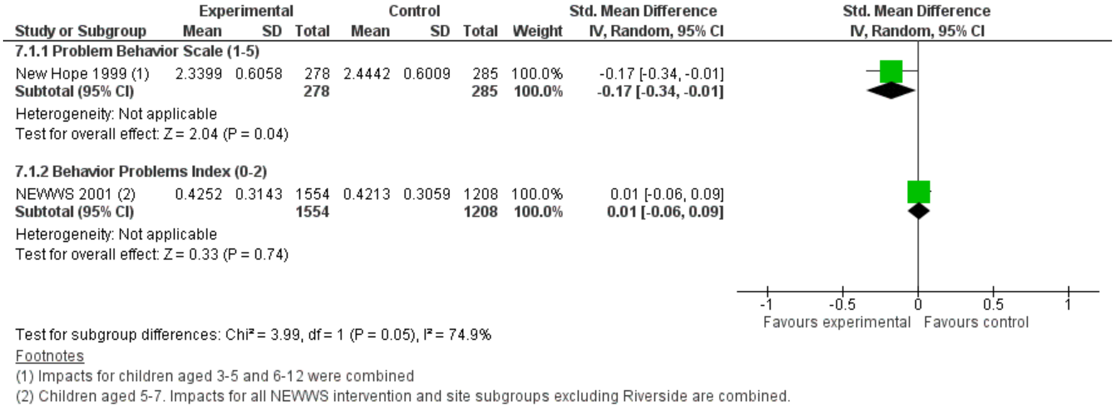
Figure 13. Forest plot of comparison: 7 Time point 1 Child mental health, outcome: 7.1 Child behaviour problems continuous.



Study or Subgroup	Experimental Mean	Experimental SD	Experimental Total	Control Mean	Control SD	Control Total	Weight	Std. Mean Difference IV, Random, 95% CI
7.1.1 Problem Behavior Scale (1-5)								
New Hope 1999 (1)	2.3399	0.6058	278	2.4442	0.6009	285	100.0%	-0.17 [-0.34, -0.01]
Subtotal (95% CI)			**278**			**285**	**100.0%**	**-0.17 [-0.34, -0.01]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 2.04 (P = 0.04)								
7.1.2 Behavior Problems Index (0-2)								
NEWWS 2001 (2)	0.4252	0.3143	1554	0.4213	0.3059	1208	100.0%	0.01 [-0.06, 0.09]
Subtotal (95% CI)			**1554**			**1208**	**100.0%**	**0.01 [-0.06, 0.09]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 0.33 (P = 0.74)								

Test for subgroup differences: Chi² = 3.99, df = 1 (P = 0.05), I² = 74.9%

Footnotes

(1) Impacts for children aged 3-5 and 6-12 were combined

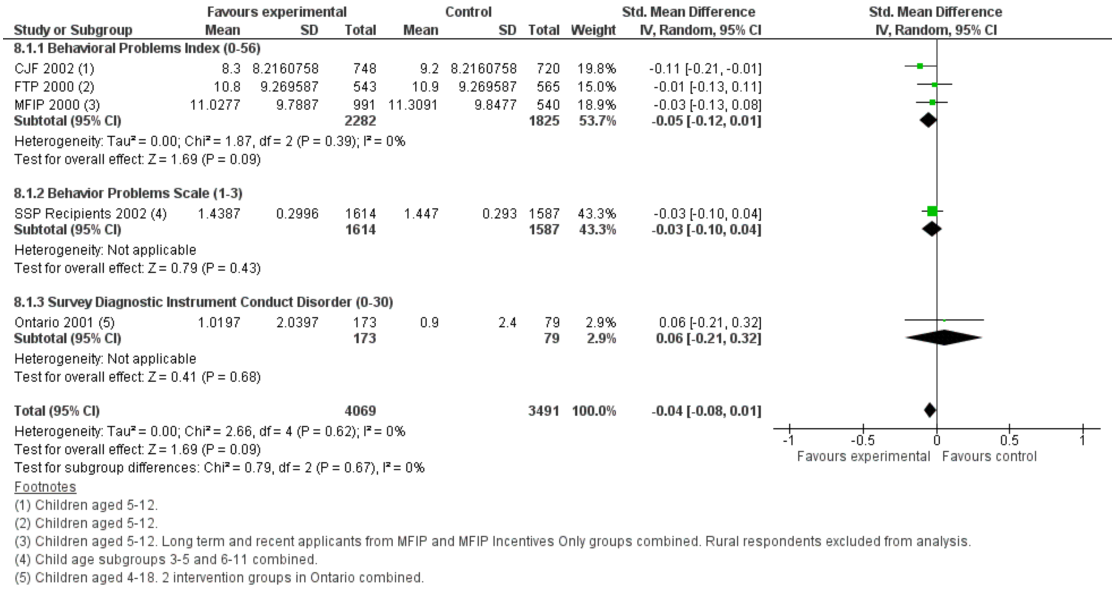
(2) Children aged 5-7. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Individual effect sizes for the dichotomous outcomes showed modest negative effects on behaviour problems in the intervention groups in both Ontario 2001 (RR 1.58, 95% CI 0.48 to 5.24; N = 178) and CJF Yale 2001 (RR 1.58, 95% CI 0.92 to 2.72; N = 311; Analysis 7.2). However, evidence from these outcomes was low quality in CJF Yale 2001 and very low quality in Ontario 2001 due to wide confidence intervals including no effect and appreciable harm and very high risk of bias in Ontario 2001.

Time point 2 (T2): 25 to 48 months since randomisation

A meta-analysis including continuous measures of child behaviour problems from five studies provided moderate-quality evidence of a very small effect in favour of the intervention at T2 (SMD −0.04, 95% CI −0.08 to 0.01; N = 7560; Analysis 8.1 Figure 14; CJF 2002; FTP 2000; MFIP 2000 urban respondents only; Ontario 2001; SSP Recipients 2002). This effect was very small and the CI crossed the line of null effect, so it is unlikely to be important. One further study reported a continuous measure of child behaviour that we could not include in the meta-analysis because there was no reported measure of variance (CJF GUP 2000). This study found a small, statistically non-significant effect in favour of control (Analysis 8.3). SSP Recipients 2002 also reported a measure of adolescent mental health (CES-D ≥ 8/30). This provided moderate-quality evidence of a very small positive effect of the intervention (RR 0.97, 95% CI 0.87 to 1.08; N = 1417; Analysis 8.2), but as this effect was very small and the CI crossed the line of null effect, it is unlikely to be important.

Figure 14. Forest plot of comparison: 8 Time point 2 Child mental health, outcome: 8.1 Child behaviour problems continuous.



	Favours experimental			Control				Std. Mean Difference
Study or Subgroup	**Mean**	**SD**	**Total**	**Mean**	**SD**	**Total**	**Weight**	**IV, Random, 95% CI**
8.1.1 Behavioral Problems Index (0-56)								
CJF 2002 (1)	8.3	8.2160758	748	9.2	8.2160758	720	19.8%	-0.11 [-0.21, -0.01]
FTP 2000 (2)	10.8	9.269587	543	10.9	9.269587	565	15.0%	-0.01 [-0.13, 0.11]
MFIP 2000 (3)	11.0277	9.7887	991	11.3091	9.8477	540	18.9%	-0.03 [-0.13, 0.08]
Subtotal (95% CI)			**2282**			**1825**	**53.7%**	**-0.05 [-0.12, 0.01]**
Heterogeneity: Tau² = 0.00; Chi² = 1.87, df = 2 (P = 0.39); I² = 0%								
Test for overall effect: Z = 1.69 (P = 0.09)								
8.1.2 Behavior Problems Scale (1-3)								
SSP Recipients 2002 (4)	1.4387	0.2996	1614	1.447	0.293	1587	43.3%	-0.03 [-0.10, 0.04]
Subtotal (95% CI)			**1614**			**1587**	**43.3%**	**-0.03 [-0.10, 0.04]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 0.79 (P = 0.43)								
8.1.3 Survey Diagnostic Instrument Conduct Disorder (0-30)								
Ontario 2001 (5)	1.0197	2.0397	173	0.9	2.4	79	2.9%	0.06 [-0.21, 0.32]
Subtotal (95% CI)			**173**			**79**	**2.9%**	**0.06 [-0.21, 0.32]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 0.41 (P = 0.68)								
Total (95% CI)			**4069**			**3491**	**100.0%**	**-0.04 [-0.08, 0.01]**
Heterogeneity: Tau² = 0.00; Chi² = 2.66, df = 4 (P = 0.62); I² = 0%								
Test for overall effect: Z = 1.69 (P = 0.09)								
Test for subgroup differences: Chi² = 0.79, df = 2 (P = 0.67), I² = 0%								

Footnotes

(1) Children aged 5-12.

(2) Children aged 5-12.

(3) Children aged 5-12. Long term and recent applicants from MFIP and MFIP Incentives Only groups combined. Rural respondents excluded from analysis.

(4) Child age subgroups 3-5 and 6-11 combined.

(5) Children aged 4-18. 2 intervention groups in Ontario combined.

Time point 3 (T3): 49 to 72 months since randomisation

Five studies reported a continuous measure of child behaviour problems at T3 (IFIP 2002; IWRE 2002; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002); however, we excluded two from the meta-analysis as no measures of variance were available (IFIP 2002; SSP Recipients 2002). In addition, NEWWS 2001 reported three subscores of the BPI but did not report the summary measure. Meta-analysis of the three remaining studies indicated a very small effect in favour of the intervention (SMD −0.05, 95% CI −0.16 to 0.05; N = 3643; Analysis 9.1; Figure 15). Heterogeneity was high (I^2 = 59%; P = 0.09), with SSP Applicants 2003's negative direction of effect clearly differing from the positive effects of the other studies. Heterogeneity dropped to 7% and the point estimate increased following removal of SSP Applicants 2003 from the analysis (SMD −0.10, 95% CI −0.18 to −0.01; N = 2509; 2 studies; Analysis 9.2). We could identify no plausible hypothesis to explain this heterogeneity. The evidence was of low quality due to this unexplained heterogeneity.

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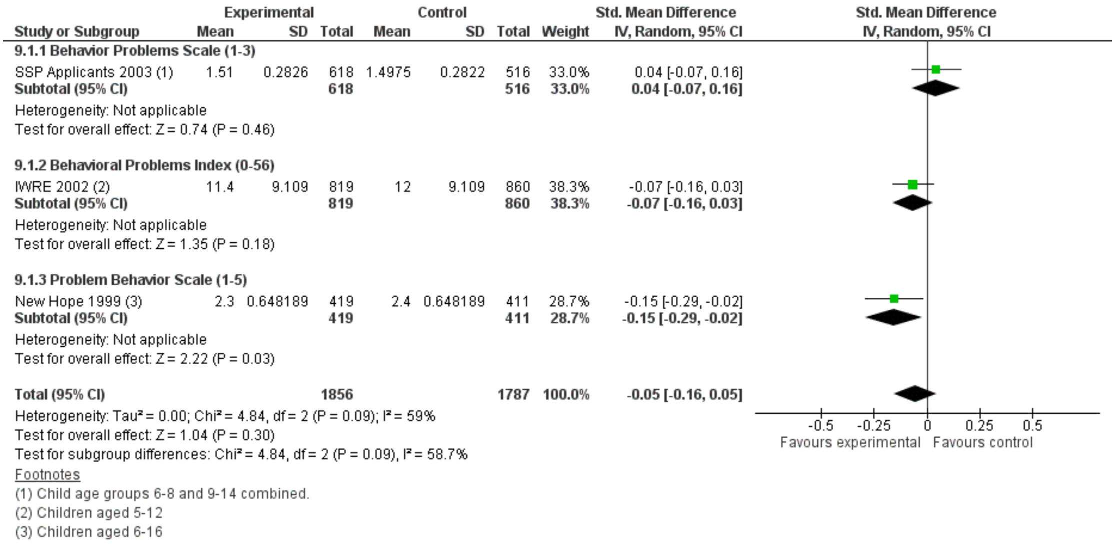
Figure 15. Forest plot of comparison: 9 Time point 3 Child mental health, outcome: 9.1 Child behaviour problems continuous.



Study or Subgroup	Experimental Mean	Experimental SD	Experimental Total	Control Mean	Control SD	Control Total	Weight	Std. Mean Difference IV, Random, 95% CI
9.1.1 Behavior Problems Scale (1-3)								
SSP Applicants 2003 (1)	1.51	0.2826	618	1.4975	0.2822	516	33.0%	0.04 [-0.07, 0.16]
Subtotal (95% CI)			**618**			**516**	**33.0%**	**0.04 [-0.07, 0.16]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 0.74 (P = 0.46)								
9.1.2 Behavioral Problems Index (0-56)								
IWRE 2002 (2)	11.4	9.109	819	12	9.109	860	38.3%	-0.07 [-0.16, 0.03]
Subtotal (95% CI)			**819**			**860**	**38.3%**	**-0.07 [-0.16, 0.03]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 1.35 (P = 0.18)								
9.1.3 Problem Behavior Scale (1-5)								
New Hope 1999 (3)	2.3	0.648189	419	2.4	0.648189	411	28.7%	-0.15 [-0.29, -0.02]
Subtotal (95% CI)			**419**			**411**	**28.7%**	**-0.15 [-0.29, -0.02]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 2.22 (P = 0.03)								
Total (95% CI)			**1856**			**1787**	**100.0%**	**-0.05 [-0.16, 0.05]**
Heterogeneity: Tau² = 0.00; Chi² = 4.84, df = 2 (P = 0.09); I² = 59%								
Test for overall effect: Z = 1.04 (P = 0.30)								
Test for subgroup differences: Chi² = 4.84, df = 2 (P = 0.09), I² = 58.7%								

Footnotes

(1) Child age groups 6-8 and 9-14 combined.

(2) Children aged 5-12

(3) Children aged 6-16

We calculated effect sizes for the three measures reported by NEWWS 2001.The intervention had a small positive effect on externalising behaviour, a very small positive effect on internalising behaviour and a very small negative effect on hyperactivity. SSP Recipients 2002 reported no effects on the Behavior Problems Scale for children aged 5.5 to 7.5 years or 7.5 to 9.5 years (Analysis 9.5). Behaviour problems were very slightly higher among the IFIP 2002 applicant intervention group (intervention 11.3/control 10.9, not statistically significant) and very slightly lower among the ongoing intervention group (intervention 11.8/ control 12.0, not statistically significant; Analysis 9.4).

Summary

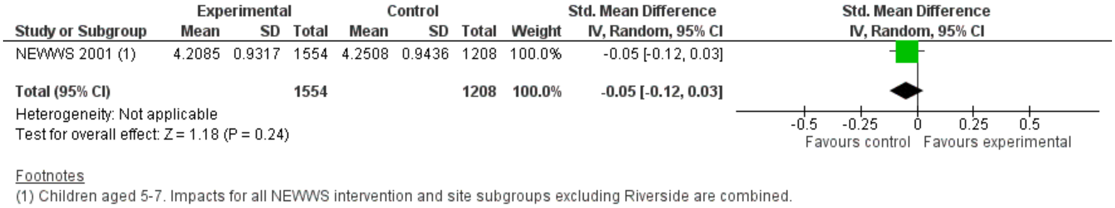
At T1 there was moderate-quality evidence of a small positive effect on problem behaviour in one study and of a very small negative effect in another study. This difference in effect was possibly related to study characteristics. Two further studies reported a modest negative effect, but the evidence was of low and very low quality. There was moderate-quality evidence of very small positive effects at T2. At T3, there was low-quality evidence of a very small positive effect, and conflicting evidence from three studies for which we could not calculate effect sizes. Since the evidence was primarily of moderate quality at T1 and T2, this was the overall assessment for both time points. Most evidence at T3 was of unclear quality, so this was the overall domain assessment.

Child physical health

Time point 1 (T1): 18 to 24 months since randomisation

Only one study reported a measure of child physical health at T1. NEWWS 2001 reported evidence of moderate quality that the intervention had a very small negative effect on the general health rating of children in the intervention group (SMD −0.05, 95% CI −0.12 to 0.03; N = 2762; Analysis 10.1; Figure 16). As this effect was very small and the CI crossed zero, it is unlikely to be important.

Figure 16. Forest plot of comparison: 10 Time point 1 Child physical health, outcome: 10.1 General health rating (1-5).



Study or Subgroup	Experimental Mean	Experimental SD	Experimental Total	Control Mean	Control SD	Control Total	Weight	Std. Mean Difference IV, Random, 95% CI
NEWWS 2001 (1)	4.2085	0.9317	1554	4.2508	0.9436	1208	100.0%	-0.05 [-0.12, 0.03]
Total (95% CI)			**1554**			**1208**	**100.0%**	**-0.05 [-0.12, 0.03]**

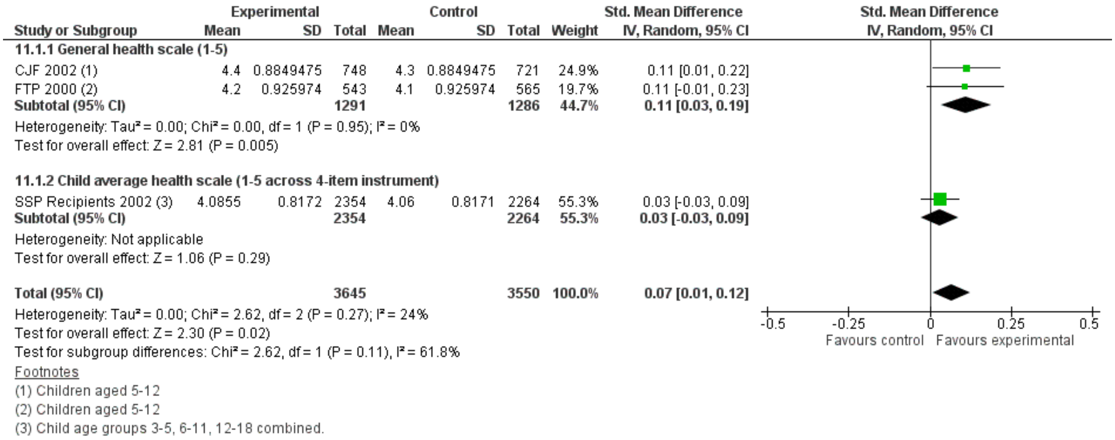
Heterogeneity: Not applicable
Test for overall effect: Z = 1.18 (P = 0.24)

Footnotes
(1) Children aged 5-7. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Time point 2 (T2): 25 to 48 months since randomisation

At T2, three studies reported continuous measures of child physical health (CJF GUP 2000; FTP 2000; SSP Recipients 2002). Meta-analysis found that the intervention had a very small positive impact on child physical health (SMD 0.07, 95% CI 0.01 to 0.12; N = 7195; Analysis 11.1; Figure 17). One study reported the percentage of the sample in good or excellent health (MFIP 2000); this showed a very small effect in favour of control (RR 0.98, 95% CI 0.93 to 1.02; N = 1900; Analysis 11.2). As this effect was very small and the CI crossed the line of null effect, it is unlikely to be important. Evidence for all outcomes was of moderate quality.

Figure 17. Forest plot of comparison: 11 Time point 2 Child physical health, outcome: 11.1 Child physical health continuous.



Study or Subgroup	Experimental Mean	Experimental SD	Experimental Total	Control Mean	Control SD	Control Total	Weight	Std. Mean Difference IV, Random, 95% CI
11.1.1 General health scale (1-5)								
CJF 2002 (1)	4.4	0.8849475	748	4.3	0.8849475	721	24.9%	0.11 [0.01, 0.22]
FTP 2000 (2)	4.2	0.925974	543	4.1	0.925974	565	19.7%	0.11 [-0.01, 0.23]
Subtotal (95% CI)			**1291**			**1286**	**44.7%**	**0.11 [0.03, 0.19]**
Heterogeneity: Tau² = 0.00; Chi² = 0.00, df = 1 (P = 0.95); I² = 0%								
Test for overall effect: Z = 2.81 (P = 0.005)								
11.1.2 Child average health scale (1-5 across 4-item instrument)								
SSP Recipients 2002 (3)	4.0855	0.8172	2354	4.06	0.8171	2264	55.3%	0.03 [-0.03, 0.09]
Subtotal (95% CI)			**2354**			**2264**	**55.3%**	**0.03 [-0.03, 0.09]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 1.06 (P = 0.29)								
Total (95% CI)			**3645**			**3550**	**100.0%**	**0.07 [0.01, 0.12]**

Heterogeneity: Tau² = 0.00; Chi² = 2.62, df = 2 (P = 0.27); I² = 24%
Test for overall effect: Z = 2.30 (P = 0.02)
Test for subgroup differences: Chi² = 2.62, df = 1 (P = 0.11), I² = 61.8%

Footnotes
(1) Children aged 5-12
(2) Children aged 5-12
(3) Child age groups 3-5, 6-11, 12-18 combined.

Time point 3 (T3): 49 to 72 months since randomisation

Six studies reported child physical health at T3. IWRE 2002, NEWWS 2001, New Hope 1999, SSP Applicants 2003 and SSP Recipients 2002 reported continuous measures, and IFIP 2002 reported the percentage of the sample in fair or poor health. No measure of variance was available for SSP Recipients 2002. Since standard deviations for four studies reporting the same outcome were available, we imputed a standard deviation for SSP Recipients 2002 based on the average for the other four studies. Meta-analysis of the continuous outcomes showed moderate quality evidence of

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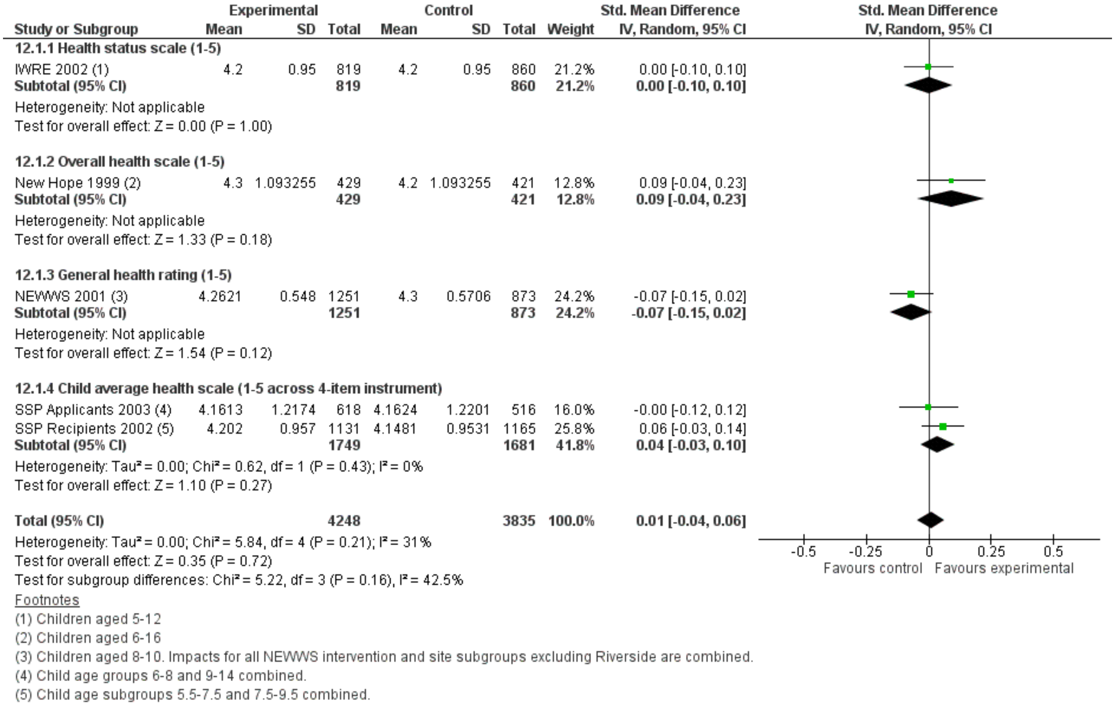
a very small positive effect (SMD 0.01, 95% CI −0.04 to 0.06; N = 8083; 5 studies; Analysis 12.1; Figure 18). Fair/poor health was higher among the IFIP 2002 intervention group, but the quality of the evidence was low due to confidence intervals including both no effect and appreciable harm (RR 1.26, 95% CI 0.73 to 2.14; N = 1475; Analysis 12.2).

Figure 18. Forest plot of comparison: 12 Time point 3 Child physical health, outcome: 12.1 Child physical health continuous.



Study or Subgroup	Experimental Mean	Experimental SD	Experimental Total	Control Mean	Control SD	Control Total	Weight	Std. Mean Difference IV, Random, 95% CI
12.1.1 Health status scale (1-5)								
IWRE 2002 (1)	4.2	0.95	819	4.2	0.95	860	21.2%	0.00 [-0.10, 0.10]
Subtotal (95% CI)			**819**			**860**	**21.2%**	**0.00 [-0.10, 0.10]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 0.00 (P = 1.00)								
12.1.2 Overall health scale (1-5)								
New Hope 1999 (2)	4.3	1.093255	429	4.2	1.093255	421	12.8%	0.09 [-0.04, 0.23]
Subtotal (95% CI)			**429**			**421**	**12.8%**	**0.09 [-0.04, 0.23]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 1.33 (P = 0.18)								
12.1.3 General health rating (1-5)								
NEWWS 2001 (3)	4.2621	0.548	1251	4.3	0.5706	873	24.2%	-0.07 [-0.15, 0.02]
Subtotal (95% CI)			**1251**			**873**	**24.2%**	**-0.07 [-0.15, 0.02]**
Heterogeneity: Not applicable								
Test for overall effect: Z = 1.54 (P = 0.12)								
12.1.4 Child average health scale (1-5 across 4-item instrument)								
SSP Applicants 2003 (4)	4.1613	1.2174	618	4.1624	1.2201	516	16.0%	-0.00 [-0.12, 0.12]
SSP Recipients 2002 (5)	4.202	0.957	1131	4.1481	0.9531	1165	25.8%	0.06 [-0.03, 0.14]
Subtotal (95% CI)			**1749**			**1681**	**41.8%**	**0.04 [-0.03, 0.10]**
Heterogeneity: Tau² = 0.00; Chi² = 0.62, df = 1 (P = 0.43); I² = 0%								
Test for overall effect: Z = 1.10 (P = 0.27)								
Total (95% CI)			**4248**			**3835**	**100.0%**	**0.01 [-0.04, 0.06]**
Heterogeneity: Tau² = 0.00; Chi² = 5.84, df = 4 (P = 0.21); I² = 31%								
Test for overall effect: Z = 0.35 (P = 0.72)								
Test for subgroup differences: Chi² = 5.22, df = 3 (P = 0.16), I² = 42.5%								

Footnotes
(1) Children aged 5-12
(2) Children aged 6-16
(3) Children aged 8-10. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.
(4) Child age groups 6-8 and 9-14 combined.
(5) Child age subgroups 5.5-7.5 and 7.5-9.5 combined.

Summary

One study that reported child physical health at T1 found moderate-quality evidence of a very small negative effect. At T2, there was moderate-quality evidence of a very small positive effect on child physical health. One individual study reported no effect. There was moderate-quality evidence of no effect at T3, while low-quality evidence from one study showed a small negative effect. At each time point, most evidence on child physical health was of moderate quality.

Secondary outcomes

Employment

Time point 1 (T1): 18 to 24 months since randomisation

Three studies reported the proportion of the sample currently in employment at T1 (CJF GUP 2000; CJF Yale 2001; NEWWS 2001). There was moderate-quality evidence of a small positive effect among the intervention group (RR 1.22, 95% CI 1.12 to 1.32; N = 3381; Analysis 13.1). Meta-analysis of three studies that reported the proportion of the sample who had ever been employed since randomisation also found moderate-quality evidence of a very small positive effect on intervention group employment (RR 1.14, 95% CI 1.07 to 1.21; N = 3818; Analysis 13.2; CJF Yale

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2001; New Hope 1999; NEWWS 2001). Heterogeneity was over 50% (I^2 = 53%, P = 0.12); however, we did not downgrade the evidence since all effects were in the same direction.

Time point 2 (T2): 25 to 48 months since randomisation

Two studies reported the proportion of respondents ever employed in the 36 months since randomisation (California GAIN 1994; MFIP 2000), and CJF 2002 and FTP 2000 reported the proportion ever employed in the year of the study. A meta-analysis provided moderate-quality evidence that the intervention had a very small positive effect on ever having been employed (RR 1.12, 95% CI 1.07 to 1.17; N = 7422; 4 studies; Analysis 14.1).

Three studies reported the proportion of the sample ever employed full-time since randomisation (California GAIN 1994; MFIP 2000; SSP Recipients 2002). A meta-analysis provided evidence of moderate quality indicating that the intervention had a small effect on employment (RR 1.20, 95% CI 1.05 to 1.37; N = 9806; Analysis 14.2). Heterogeneity was high (I^2 = 83%; P = 0.002) because the impact of MFIP 2000 on employment was lower than that of the other studies (RR 1.05, 95% CI 0.92 to 1.18). Most of the MFIP 2000 sample were not subject to employment mandates and could receive earnings disregards for lower levels of employment participation, providing a plausible hypothesis to explain this heterogeneity. Excluding MFIP 2000 from the analysis resulted in an RR of 1.29 (CI 1.18 to 1.40; N = 8275; 2 studies) indicating that the intervention had a small effect on full-time employment (Analysis 14.3).

Evidence of moderate quality from two studies showed that intervention group participants were more likely to have been in part-time employment since randomisation than the control group, although the effect was very small (RR 1.14, 95% CI 1.04 to 1.25; N = 4845; California GAIN 1994; MFIP 2000), and the effect was weaker than the effect on full-time employment (Analysis 14.4). SSP Recipients 2002 reported the proportion of the sample currently in part-time employment, with moderate-quality evidence showing a small negative effect on the intervention group for being in part-time employment (RR 0.80, 95% CI 0.69 to 0.93; N = 4852; Analysis 14.4).

Time point 3 (T3): 49 to 72 months since randomisation

At T3, six studies reported the proportion of the sample currently in work (IFIP 2002; IWRE 2002; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). A meta-analysis indicated that there was a very small effect in favour of the intervention (RR 1.03, 95% CI 0.99 to 1.07; N = 14,355; Analysis 15.1). This evidence was of low quality due to high attrition in UK ERA 2011.

Six studies reported the proportion of the sample currently employed full-time (IFIP 2002; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011), and five reported the proportion currently employed part-time (IFIP 2002; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). The quality of evidence for full-time employment was low due to high attrition in UK ERA 2011. Meta-analysis indicated that the intervention had a very small effect on the proportion employed full-time (RR 1.05, 95% CI 1.00 to 1.12; N = 13233; Analysis 15.3). The meta-analysis of studies reporting part-time employment showed that the control group were more likely to work part-time, although the effect was very small (RR 0.93, 95% CI 0.85 to 1.01; N = 12,676; 5 studies; Analysis 15.4). The evidence was of low quality due to high attrition in UK ERA 2011. The effects in Analysis 15.1, Analysis 15.3 and Analysis 15.4 are unlikely to be important, as they are very small and the CI crosses the line of null effect.

Two studies reported the proportion who had ever worked in the fifth year of the study (New Hope 1999; UK ERA 2011), and one study reported the proportion who had ever worked between years 1 and 5 of the study (NEWWS 2001). Meta-analysis of the first two studies showed moderate-quality evidence that the effect of the intervention was close to zero (RR 1.01, 95% CI 0.96 to 1.06; N = 2599; Analysis 15.2). NEWWS 2001 found moderate-quality evidence of a very small effect in favour of the intervention group being employed between years 1 and 5 of the study (RR 1.12, 95% CI 1.08 to 1.17; N = 2124; Analysis 15.2).

Summary

Overall, the intervention showed very small to small positive effects on all measures of employment at T1 and T2 (ranging from RR 1.12 to 1.22). One study requiring full-time employment in order to receive an earnings supplement found that part-time employment was slightly lower in the intervention group. All evidence at T1 and T2 was of moderate quality. At T3 the effects on most measures of employment were close to zero, with similar proportions of the control group in employment at 49 to 72 months. One study reporting the proportion who had ever been employed in years 1 to 5 of the study found moderate-quality evidence of a very small effect in favour of the intervention. There was low-quality evidence that the intervention group were slightly less likely to be in part-time employment than the intervention group. Much of the evidence on employment at T3 was of low quality. At T1 and T2, we assessed most evidence on employment as moderate quality, therefore the domain level quality assessment was also moderate.

Income and earnings

Time point 1 (T1): 18 to 24 months since randomisation

There was evidence of moderate quality from two studies on income effects (New Hope 1999; NEWWS 2001). When we included both studies in a meta-analysis, there was no effect on income. However, heterogeneity was very high (I^2 = 80%; P = 0.02). The direction of effects varied, with New Hope 1999 showing a small positive effect on income (SMD 0.11, 95% CI −0.04 to 0.25; N = 744) and NEWWS 2001 finding a very small negative effect (SMD −0.08, 95% CI −0.15 to −0.00; N = 2762; Analysis 16.1). There were a number of differences between these studies that may have contributed to this, including the lack of any earnings supplement or disregard over and above that received by the control group in the NEWWS 2001 intervention.

New Hope 1999 and NEWWS 2001 also reported earnings at T1. No measure of variance was available for NEWWS 2001 total earnings, so a meta-analysis was not possible. New Hope 1999 reported a very small positive effect on intervention group annual earnings (SMD 0.07, 95% CI −0.08 to 0.21; N = 744; 1 study, moderate quality; Analysis 16.2). However, as this effect was very small and the CI crossed zero, it is unlikely to be important. Across all of the groups included in NEWWS 2001, mean differences in monthly earnings ranged from USD 33 to USD 197 in favour of the intervention. Only two groups reported statistically significant differences (Atlanta HCD and Riverside LFA). Although earnings were slightly higher for the NEWWS 2001 intervention group, income was lower (Analysis 16.3).

Time point 2 (T2): 25 to 48 months since randomisation

Four studies reported a measure of total income at T2 (CJF 2002; FTP 2000; MFIP 2000; SSP Recipients 2002). A meta-analysis including all four studies provided evidence of low quality that income was higher among the intervention group (SMD 0.10, 95% CI 0.02 to 0.17; N = 8934; 4 studies; Analysis 17.1). Heterogeneity was high (I^2 = 62%; P = 0.05) and visual inspection and a post hoc sensitivity analysis indicated this was due to CJF 2002, which showed virtually no effect on income. A possible explanation for this is that earnings disregards had ceased by this point for most CJF 2002 respondents. Both MFIP 2000 and SSP Recipients 2002 were still providing earnings supplements when T2 data were collected, which may account for their stronger positive effects on income. However, although FTP 2000 had also ceased to supplement income, income was higher in the intervention group. With CJF 2002 excluded from the analysis, the point estimate increased to SMD 0.14 (95% CI 0.09 to 0.18; N = 7465; 3 studies), indicating a small positive effect on income (Analysis 17.2).

Four studies reported impacts on earnings in the third year following randomisation (CJF 2002; California GAIN 1994; MFIP 2000; SSP Recipients 2002). We calculated average earnings in year 4 for FTP 2000. No measures of variance were available for FTP 2000, California GAIN 1994 or MFIP 2000, so we could not include these in a meta-analysis. CJF 2002 and SSP Recipients 2002 provided moderate-quality evidence of a very small positive effect on earnings (SMD 0.09, 95% CI 0.04 to 0.13; N = 6321; 2 studies; Analysis 17.3). Mean annual earnings for the MFIP 2000 full-intervention groups ranged from USD 4061 to USD 6817 and for the MFIP 2000 incentives-only groups from USD 3967 to USD 6270. Intervention group earnings exceeded those of control in three groups (long-term urban MFIP, long-term urban MFIP-IO, and long-term rural MFIP). However, control group earnings exceed those of intervention in the remaining three groups (recent urban MFIP, recent urban MFIP-IO and long-term rural MFIP). None of these effects reached statistical significance. Analysis 17.4). California GAIN 1994 reported average weekly earnings only for respondents who were in employment, finding that the intervention group earned slightly more than the control group (intervention USD 204/control USD 190; Analysis 17.5). Study authors did not calculate statistical significance. For the FTP 2000 intervention group, average year 3 earnings were USD 969 higher than control (Analysis 17.6). We could not calculate statistical significance.

Time point 3 (T3): 49 to 72 months since randomisation

At T3, six studies reported a measure of total income (IFIP 2002; IWRE 2002; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002). We could not include IFIP 2002 in meta-analysis as no measure of variance was available. The remaining studies provided moderate-quality evidence of almost no effect on income (SMD 0.01, 95% CI −0.04 to 0.06; N = 11,735; 5 studies; Analysis 18.1). In the IFIP 2002 ongoing sample, intervention group income in the month prior to the survey exceeded that of control (intervention USD 1533/control1451, not significant; Analysis 18.2). However in the IFIP 2002 applicant group, control income exceeded that of the intervention group (Intervention USD 1857/Control USD 2110, $P < 0.05$; Analysis 18.2).

Five studies were included in a meta-analysis of total earnings (IWRE 2002; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011), which indicated that earnings were higher among the intervention group, although the effect was very small (SMD 0.04, 95% CI 0.00 to 0.07; N = 11,501; Analysis 18.3). This evidence was of low quality due to UK ERA 2011's high risk of attrition bias. IFIP 2002 and NEWWS 2001 also reported total earnings, but measures of variance were not available. In the two IFIP 2002 groups, there were small differences in favour of control (ongoing group) and intervention (applicant group). Neither reached statistical significance (Analysis 18.4). All five experimental groups in the NEWWS 2001 study reported that the intervention groups earned more than control in years 1 to 5 of the study. Only one difference was statistically significant (Riverside LFA intervention USD 17342/control USD 10805, P = 0.01; Analysis 18.5).

Summary

Of two studies that reported moderate-quality evidence of effects on income at T1, one study that provided an earnings supplement found a small positive effect and another that did not found a very small negative effect. One study reported moderate-quality evidence of a very small positive effect on earnings. We could not calculate an effect size for the other study that reported slightly higher earnings among five intervention groups, which were statistically significant in two of the groups.

At T2 two meta-analyses provided moderate-quality evidence of a small positive effect on income and a very small positive effect on earnings among the intervention group. We could not calculate an effect size for two studies reporting earnings; one study found no statistically significant differences between intervention and control. Another reported very slightly higher earnings for the intervention group.

At T3, a meta-analysis of five studies found moderate-quality evidence of a very small positive effect on income. One further study for which we could not calculate an effect size showed a statistically significant effect in favour of control among one subgroup of respondents. There was moderate-quality evidence of a very small positive effect on earnings from five studies. We could not calculate effect sizes for two further studies; one found higher earnings among all five intervention groups, although the difference was statistically significant in just one. The other reported no statistically significant differences and slightly higher earnings in one control subgroup. Based on the majority of the evidence at T1 and T2, the domain level assessments of income and earnings were of moderate quality. At T3, the evidence was predominantly of low quality, which was reflected in the domain level assessment.

Welfare receipt

Time point 1 (T1): 18 to 24 months since randomisation

Only one study reported total welfare received at T1 (New Hope 1999), finding evidence of moderate quality that total welfare received was lower in the intervention group (SMD −0.10, 95% CI −0.24 to 0.04; N = 744; 1 study), although the effect size was small (Analysis 19.1). Three studies reported the proportion of the sample in receipt of welfare at T1 (New Hope 1999; NEWWS 2001; Ontario 2001). There was a very small effect in favour of the intervention group (RR 0.88, 95% CI 0.84 to 0.92; N = 3714), and the evidence was of moderate quality (Analysis 19.2).

Time point 2 (T2): 25 to 48 months since randomisation

At T2, four studies reported total welfare received (CJF 2002; FTP 2000; MFIP 2000; SSP Recipients 2002). Combining these in a meta-analysis resulted in very high heterogeneity (I^2 = 98%; $P < 0.001$). Inspection of the forest plot showed that MFIP 2000 had a negative direction of effect whilst the remaining three were positive (MFIP 2000: SMD 0.33, 95% CI 0.22 to 0.43; N = 1531; Analysis 20.1). This was possibly due to the generous earnings disregards MFIP 2000 provided to the intervention group throughout the study, which allowed them to receive welfare benefits while working at higher levels than the control group. Therefore we conducted a post hoc sensitivity analysis excluding MFIP 2000. This provided moderate-quality evidence of a modest positive effect on total welfare received among the intervention group (SMD −0.24, 95% CI −0.33 to −0.15; N = 7429; 3 studies; Analysis 20.2). Heterogeneity was still high (I^2 = 69%; P = 0.04) due to a stronger positive effect of FTP 2000 on total welfare received. Although we could not identify any plausible explanation, we did not downgrade the quality of evidence because all effects were in the same direction.

Ontario 2001 and SSP Recipients 2002 reported the proportion of the sample in receipt of welfare at T2. This indicated that fewer participants in the intervention group were in receipt of welfare (RR 0.87, 95% CI 0.83 to 0.91; N = 5210; Analysis 20.3). The evidence was of moderate quality.

Time point 3 (T3) (49 to 72 months since randomisation

Seven studies reported total welfare received at T3 (IFIP 2002; IWRE 2002; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). We could not include IFIP 2002 and IWRE 2002 in the meta-analysis as no measures of variance were available. The measure reported by NEWWS 2001 differed from that of the other studies (total welfare received in years 1 to 5 rather than in the year prior to data collection), and we therefore analysed it separately.

Meta-analysis of New Hope 1999, SSP Applicants 2003, SSP Recipients 2002 and UK ERA 2011 showed low-quality evidence of a very small positive effect on total welfare received (SMD −0.06, 95% CI −0.11 to −0.00; N = 9822; Analysis 21.1). The evidence was of low quality due to high risk of bias in UK ERA 2011. The effect of NEWWS 2001 on welfare receipt over the four intervention years was considerably stronger (SMD −0.47, 95% CI −0.56 to −0.38; N = 2124), possibly because NEWWS 2001 maintained intervention and control conditions for the duration of the study and did not provide the intervention group with earnings disregards at any time (Analysis 21.2). This evidence was of moderate quality. IWRE 2002 also maintained the AFDC regime for the control group, and while no effect size could be calculated, the difference in welfare payments would appear to be large in absolute terms (annualised welfare received: intervention USD 685/control USD 1082; P < 0.01; Analysis 21.3). In the IFIP 2002 study, where all respondents became subject to TANF after 3.5 years, there were small differences between intervention and control groups. The difference in monthly welfare received was statistically significant for the IFIP 2002 applicants' sample, with the intervention group receiving higher welfare payments than control (intervention USD 56/control USD 34, P < 0.05; Analysis 21.4).

Six studies reported the proportion of the sample in receipt of welfare at T3 (IFIP 2002; IWRE 2002; New Hope 1999; SSP Applicants 2003; SSP Recipients 2002; UK ERA 2011). There was a very small effect in favour of the intervention (RR 0.92, 95% CI 0.86 to 0.99; N = 12,976), although the evidence was of low quality due to systematic attrition in UK ERA 2011 (Analysis 21.5).

Summary

One study reporting effects on total welfare received at T1 showed moderate-quality evidence of a small effect in favour of the intervention (i.e. the intervention group received less welfare than control.) There was also moderate-quality evidence of a very small positive effect on the proportion of the intervention group in receipt of welfare. At T2, there was evidence of a modest positive effect on total welfare received, which was of moderate quality when we excluded one study that had a modest negative impact on total welfare. There was also moderate-quality evidence of a very small positive effect on the proportion of the sample in receipt of welfare.

There was low-quality evidence of a very small positive effect on welfare received in the previous year at T3. One study reported a modest positive effect (moderate quality) on welfare received between years 1 and 5. We could not calculate effect sizes for the amount of welfare received in two further studies. One reported that the intervention group received very slightly more welfare than control, while the other reported a large absolute difference in favour of the intervention. There was low-quality evidence of a very small positive effect on the proportion in receipt of welfare at T3. The majority of the evidence at T1 and T2 was of moderate quality, therefore these domains were assessed as such. At T3, the evidence was predominantly of low quality.

Health insurance

Time point 1 (T1): 18 to 24 months since randomisation

Three studies reported a measure of adult health insurance at T1. CJF GUP 2000 and CJF Yale 2001 reported the proportion of the sample with Medicaid at the time of the survey, New Hope 1999 reported the proportion of the sample that had ever had Medicaid since randomisation, and NEWWS 2001 reported the proportion who ever had health insurance provided by their employer since randomisation. Findings varied across studies: CJF GUP 2000 and CJF Yale 2001 found a very small effect in favour of the intervention (RR 1.16, 95% CI 1.08 to 1.25; N = 606); New Hope 1999 found a very small effect (RR 1.09, 95% CI 1.03 to 1.16; N = 590); and NEWWS 2001 a small effect (RR 1.40, 95% CI 1.16 to 1.69; N = 2762) in favour of the intervention (Analysis 22.1). Only one study reported the proportion of focal children ever having health insurance since randomisation (NEWWS 2001), finding a very small negative effect for the intervention (RR 0.99, 95% CI 0.96 to 1.01; N = 2762; Analysis 22.2). All of the evidence was of moderate quality.

Time point 2 (T2): 25 to 48 months since randomisation

At T2, one study reported the number of adults with Medicaid or other health insurance within 2 to 3 years of randomisation (California GAIN 1994), and one study reported the proportion of children having any health insurance continuously in the previous 36 months (MFIP 2000).

California GAIN 1994 found a very small effect in favour of control (RR 0.97, 95% CI 0.93 to 1.01; N = 2193), while MFIP 2000 found a very small effect in favour of the intervention (RR 1.16, 95% CI 1.08 to 1.24; N = 1531; ; Analysis 23.1). The evidence was of moderate quality in both cases, but the result for California GAIN 1994 is unlikely to be important, as the effect was very small and the CI crossed the line of null effect. CJF GUP 2000 reported the percentage of adult respondents with health insurance (intervention 88%/control 82%) and the percentage of children covered by Connecticut's state programme for children (intervention 95%/control 76%), but it was not possible to calculate effect sizes as studies did not report group Ns (data not entered into RevMan).

Time point 3 (T3): 49 to 72 months since randomisation

At T3 three studies reported four measures of child and family health insurance (IFIP 2002; New Hope 1999; NEWWS 2001). IFIP 2002 and NEWWS 2001 reported the proportion of cases where the whole family was covered by Medicaid or private insurance, finding a very small effect in favour of control (RR 0.98, 95% CI 0.92 to 1.05; N = 3599; Analysis 24.1). The evidence was of low quality due to high unexplained heterogeneity (I^2 = 76%; P < 0.04), with NEWWS 2001 favouring control and IFIP 2002 favouring the intervention, although both effects were very small. New Hope 1999 reported the proportion of respondents with any type of health insurance and the proportion of respondents whose focal child was insured. These provided moderate-quality evidence of very small effects in favour of control. However, in all cases the effects are unlikely to be important as they are very small and the CI crosses the line of null effect.

Summary

At T1 there were very small positive effects on adult health insurance and no effect on child health insurance. At T2, one study found a very small effect in favour of control, while one other found a very small effect in favour of the intervention. Evidence from T1 and T2 was of moderate quality. Effects on health insurance were very small at T3. The evidence at T1 and T2 was all assessed as moderate quality; therefore both domains were assigned a grade of moderate. At T3, most evidence was of low quality.

Data not included in the synthesis

New Hope at 96 months

New Hope 1999 reported data at 96 months for a limited set of outcomes. Since there was only one study that reported partial data at such a long follow-up, we analysed this separately from the main synthesis. By 96 months postrandomisation, the intervention had ended five years prior, and there were few differences between intervention and control in adult CES-D score, adult physical health, or the internalising and externalising subscores of the Problem Behavior Scale (PBS) among girls. Boys in the intervention group fared slightly better in terms of the PBS subscores, with effect sizes of −0.15 for the externalising (P = 0.12) and internalising (P = 0.15) subscores. Notably, maternal CES-D scores for both the intervention and control groups were higher than T3 estimates, and over the threshold for risk of depression (intervention 17.36/control 17.33; Analysis 25.1).

Connecticut Jobs First and Florida Transition Programme at 15 to 18 years

Analyses of linked mortality data for CJF 2002 respondents (15 years postrandomisation) and FTP 2000 respondents (17 to 18 years postrandomisation) found that despite increases in employment, there was a very small, statistically non-significant increase in mortality among the CJF 2002 intervention group (hazard ratio 1.13, 95% CI 0.87 to 1.46) and a small statistically non-significant increase in the FTP 2000 intervention group (hazard ratio 1.26, 95% CI 0.96 to 1.66; data not entered into RevMan).

ADDITIONAL SUMMARY OF FINDINGS *[Explanation]*

Welfare to work for lone parents. Child health outcomes
Summaries of all outcomes reported in the review are provided in Web appendix 2

Patient or population: lone parents
Settings: high-income countries
Intervention: welfare to work

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Control	Welfare to work				
T1 child mental health Behavioural Problems Index (mean score)[a]	-	The mean T1 child mental health in the intervention groups was **0.01 standard deviations higher** (0.06 lower to 0.09 higher)	-	2762 (1 study)	⊕⊕⊕○ **Moderate**[b]	Very small negative effect
T2 child mental health Behavior Problems Index, Behavior Problems Scale, Survey Diagnostic Instrument Conduct Disorder (mean score)[a]	-	The mean T2 child mental health in the intervention groups was **0.04 standard deviations lower** (0.08 lower to 0.01 higher)	-	7560 (5 studies)	⊕⊕⊕○ **Moderate**[b]	Very small positive effect
T3 child mental health Behaviour Problems Index, Behaviour Problems Scale, Problem Behaviour Scale (mean score)[a]	-	The mean T3 child mental health in the intervention groups was **0.05 standard deviations lower** (0.16 lower to 0.05	-	3643 (3 studies)	⊕⊕○○ **Low**[b,c]	Very small positive effect

		higher)				
T1 child health (mother reported) 5-point scale (mean score)[d]	-	The mean T1 child health (mother reported) in the intervention groups was **0.05 standard deviations lower** (0.12 lower to 0.03 higher)	-	2762 (1 study)	⊕⊕⊕○ **Moderate**[b]	Very small negative effect
T2 child health (mother reported) 5 point scale, 4 item instrument (mean score) [d]	-	The mean T2 child health (mother reported) in the intervention groups was **0.07 standard deviations higher** (0.01 to 0.12 higher)	-	7195 (3 studies)	⊕⊕⊕○ **Moderate**[b]	Very small positive effect
T3 child health (mother reported) 5 point scale, 4 item instrument (mean score) [d]	-	The mean T3 child health (mother reported) in the intervention groups was **0.01 standard deviations lower** (0.04 lower to 0.06 higher)	-	8083 (5 studies)	⊕⊕⊕○ **Moderate**[b]	Very small positive effect

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval
Very small effect: unlikely to be substantively important.
Small effect: may be substantively important.
Modest effect: likely to be substantively important.
See Table 5 for further explanation

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[a] Better indicated by lower values.
[b] All studies were downgraded due to high risk of bias in at least one domain.
[c] Heterogeneity over 50% and no plausible explanation identified.
[d] Better indicated by higher values.

DISCUSSION

Summary of main results

This review identified 12 RCTs evaluating the effects of participating in welfare-to-work (WtW) interventions on the health of lone parents and their children. The studies we identified were of highly complex, multi-component and often multi-site interventions. We were able to conduct meta-analyses for most outcomes and to calculate standardised effect sizes for much of the remainder. We synthesised the data across three time points (18 to 24 months, 25 to 48 months and 49 to 72 months) and eight outcome domains: maternal mental health, maternal physical health, child mental health, child physical health, employment, income, welfare receipt and health insurance. However, there were limited numbers of studies in each meta-analysis, and fewer in each predefined subgroup, precluding statistical investigation of the influence of study characteristics via subgroup analysis. We were therefore restricted to our planned primary analyses, which included data from all studies. The typology we set out to investigate using subgroup analysis proved less useful than anticipated, as interventions using apparently different approaches were often similar in terms of content and methods.

Eight of the included studies were conducted in the USA, three in Canada and one in the United Kingdom. The Canadian provinces and US states in which the evaluations took place were diverse in terms of geography, demographics and local labour markets. Most evaluations began between 1991 and 1996. California GAIN 1994 began in 1986 and UK ERA 2011 in 2003. All studies were at high risk of bias in at least one domain, although when we incorporated risk of bias and other factors in the GRADE assessment of quality of evidence, most evidence was of moderate quality, implying that further research "is likely to have an important impact on our confidence in the estimate of effect and may change the estimate" (GRADEpro GDT 2014).

Overall, most effects in this review fell below the conventionally accepted threshold for a small effect. However, as discussed in Effects of interventions, there is some debate regarding the importance of very small effect sizes and suggestions that effect sizes above SMD 0.10 are potentially important when interventions may affect a large population (Coe 2002). Nonetheless, the overwhelming majority of effects on health outcomes in this review were below this size, suggesting that there are unlikely to be tangible impacts on health. While the direction of effect is mostly positive, there is moderate-quality evidence that all but two effect sizes were very small. There is moderate-quality evidence of a small positive effect on child mental health from one study at T1. There is low-quality evidence from single studies of small negative effects on maternal mental health and child mental health at T1, and on maternal mental health and child mental health at T3. There is some suggestion that the effects on maternal mental health varied over time, with a tendency toward negative impacts at T1, no effect at T2 and positive impacts at T3. It is possible that intervention group participants experienced higher stress levels at T1, either because they were actively involved in the intervention at that time, due to a period of adjusting to WtW requirements, or because their children were likely to be younger. However, as the effects are so small, any hypotheses regarding this difference in effects are necessarily speculative.

Most economic outcomes provided moderate-quality evidence of very small effects. There was moderate-quality evidence of small positive effects on income and some measures of employment at T1 and T2, and modest positive effects on total welfare received at T2 and in one study reporting at T3 (although a meta-analysis of four studies at T3 found a very small effect). Many economic outcomes at T2 and T3 are likely to have been affected by direct or indirect contamination, which would have led to underestimated impacts. How this might have affected health outcomes is unclear. Although these analyses included interventions specifically designed to increase income and promote labour market advancement, effects on these outcomes were limited. In spite of higher employment and earnings, effects on income at T1 and T2 were not always positive. In addition, there is evidence that welfare reform led to an increase in lone parents' expenditure on items such as travel and food consumed away from the home, suggesting that any increase in total income may not have boosted respondents' disposable income (Waldfogel 2007). At 5 to 6 year follow-ups, effects on employment and income were very small, although much of this evidence was of low or very low quality. In some studies very small effects were due to control groups voluntarily entering employment at a similar rate to intervention groups.

On this basis, we conclude that WtW interventions are unlikely to improve the health of lone parents and their children. There is some evidence to suggest that there may be small adverse effects on health in some circumstances. Effects on employment and income were perhaps smaller than policy makers might hope or expect. Since economic impacts are hypothesised to mediate health impacts, it is possible that effects on health were very small due to the small economic impacts. These very small effects on maternal and child mental health need to be interpreted against a background of very poor mental health for intervention and control groups at all time points. The control group risk of depression at any time point ranged from 14.4% to 40.7%, compared to an average within-year prevalence of 6.7% for women in the US general population (Pratt 2008). Comparison of effects on income across studies is complicated by variations in tax and transfer systems in different state jurisdictions. However, overall it is clear that effects on income were unlikely to have important substantive effects. Indeed, although we did not extract data on poverty, most studies noted that poverty remained high for all groups.

As noted above, there were insufficient studies possessing similar characteristics to permit statistical subgroup analyses. We were therefore unable to investigate the influence of the intervention ethos (anti-poverty/caseload reduction), approach (labour force attachment/human capital development; fully explained in

Description of the intervention) or population characteristics. Similarly, we could not statistically investigate other intervention characteristics such as whether the intervention was voluntary or mandatory, or whether income was supplemented in any way. However, we used post hoc sensitivity analyses where there was high heterogeneity to generate hypotheses regarding the influence of study or intervention characteristics on effect estimates (Haidich 2010). These post hoc hypotheses suggested that voluntary interventions that lead to increased income may have positive effects on child mental health, while mandatory interventions that increase employment but do not improve income may lead to negative impacts on maternal and child health.

Overall completeness and applicability of evidence

This review has addressed the questions of the health and economic impacts of welfare-to-work interventions for lone parents and their children. However, the evidence is limited geographically and temporally, in that most studies took place in North America during a period of economic expansion in the 1990s. We were unable to investigate the role of economic outcomes as mediators of health impacts due to the small number of studies reporting at each time point.

The applicability of the findings from the included studies to other contexts is also debatable given that the USA lacks a system of universal health care (although most respondents were eligible for Medicaid), and most of the US and Canadian studies were from the 1990s. On the other hand, both the USA and Canada, like other countries currently implementing active labour market policies for lone parents, are high-income countries with developed social welfare systems. Furthermore, while most studies are from only two countries, these are not homogeneous, and economic and political contexts varied across the states and provinces in which studies were conducted. Generalisability may be enhanced by such diversity of contexts (Armstrong 2011).

In terms of transferability, the evaluations were conducted at scale, in real-world settings, indicating that they are practically feasible. Various forms of WtW policies and interventions for lone parents have been or are being implemented across the developed world. However, it is important to be aware that the welfare-to-work interventions currently implemented internationally differ from those evaluated in these studies in many ways. The age of youngest child at which lone parents are required to be available for work varies internationally but is rarely as young as that tested in these studies (often as young as six months). Many interventions do not provide earnings disregards, extensive case management, training opportunities or childcare subsidies. Welfare reform as implemented in the USA also had many important differences from the interventions reviewed here. These include the universal implementation of lifetime limits on welfare receipt (which featured in only three studies reviewed here) and the use of diversion policies to prevent eligible lone parents from claiming welfare at all.

The included interventions consisted of multiple components in varying combinations. Individual participants did not receive every intervention component, but few studies reported data on uptake of discrete components, not to mention duration or intensity. Although most of these reports provided a great deal of detail on intervention content, information on some components (e.g. training) could be limited. Even if they did provide such data, extraction and analysis would be extremely challenging. In the absence of such information, however, it is not possible to investigate the influence of intervention uptake or individual components. However, diversity of components and adherence thereof may enhance applicability (Armstrong 2011). Although some studies reported cost-benefit analyses, extraction and interpretation of these was beyond the scope of this review, limiting our ability to draw conclusions based on intervention cost relative to very small changes in health. However, Greenberg et al synthesised cost-benefit analyses of 28 North American WtW RCTs, including many of the studies included in this review. They reported mixed results from different programme designs, but overall found that gains in employment and earnings did not generally persist beyond 5 years. This was due to the time-limited nature of programme services, and the tendency of control group members to find employment independently. Greenberg et al also note that cost-benefit analyses do not account for non-monetary costs such as loss of participants' time, or labour market displacement effects potentially leading to greater difficulty in finding work for non-participants.

Many studies reported implementation issues that had the potential to affect internal validity. For instance, lack of resources, staff attitudes to welfare reform, cultural differences between sites and caseloads were all mentioned as factors that influenced the nature of the intervention delivered. A number of interventions altered in approach from HCD to LFA or vice versa during the evaluations. In addition, the intervention implemented did not always accord with the explicit ethos or approach. Local economic, social and political contexts also varied. We were unable to statistically investigate the role of implementation issues due to small numbers of studies sharing given characteristics. Implementation and uptake issues, while problematic for internal validity, can be seen to increase external validity, as effects more closely resemble the likely impacts of an intervention when implemented in a real-world setting (Armstrong 2011; Gartlehner 2006). The evidence is that the US-wide implementation of WtW was far from uniform, and the resourcing of interventions was not commensurate with that provided for the evaluations (Muennig 2015). The role of broader economic and political contexts is discussed below.

Population characteristics

The intervention was compulsory in 7 of the 12 included studies, and participants were recruited from the existing population of lone parent welfare claimants. It is very likely that sample popu-

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lations reflected the target population of the intervention in these cases. However, recruitment processes may have reduced generalisabilty as some claimants did not attend study orientation events or found work before the study began; for instance in NEWWS 2001, only 66% of those invited to orientation events actually attended. In five of the included studies, participation was voluntary. It is likely that this also influenced generalisabilty as only those who were more motivated to gain employment would volunteer to participate. In addition, not all of those who volunteered to participate and were randomised to the intervention took up the available services; only 27% of those randomised to treatment in SSP Applicants 2003 took up the offer of generous earnings supplements. In New Hope 1999, study workers recruited participants in community settings, possibly leading to a less representative population. On the other hand, in both cases this may have led to a more realistic approximation of how the interventions might work outside the trial context. In a number of studies, some proportion of the sample were married or living with a partner at randomisation. Although some studies reported data on family formation, we did not extract these, as this was not an outcome considered in the review. However, we know that lone parenthood is frequently not a static state, and it is likely that changes in partnership status among the participants again render them more representative of the wider population of lone parents.

Political and economic context

All but one of the included studies took place during a period of increasing public and political opposition to welfare payments and well-publicised restrictions to benefit entitlements. This may have encouraged those closest to the labour market to enter employment independently, leaving more disadvantaged welfare claimants on the welfare rolls, although a number of studies made efforts to ensure the control groups were aware of their status. The nature of the population receiving welfare would also have been influenced by the prevailing economic contexts. In a buoyant labour market, those who are more job-ready are likely to find employment independently, leaving the more disadvantaged to participate in the study (NEWWS 2001). In a period of economic contraction, even the job-ready would struggle to find work. All of the included studies were affected by one or more of these factors, but the seven US studies conducted after the implementation of welfare reform in 1996 were the most affected. The economy expanded rapidly during this period. In addition, the Earned Income Tax Credit (EITC), which supplements the incomes of low-income workers, was greatly expanded at this time, increasing the attractiveness of employment for lone parents. All of these factors are likely to have decreased the potential for positive effects on economic outcomes. During this period there were large decreases in welfare receipt among lone parents in the US; the total caseload declined from 5 million to 2.1 million between 1994 and 2000 (Grogger 2003a), and employment rates increased rapidly, from 56% to 76% of single mothers between 1995 and 1999 (Pavetti 2015). Analyses of observational evidence suggest that the flourishing economy and the expansion of EITC, rather than welfare reform, were responsible for most of the decline in welfare receipt (Grogger 2003b). The EITC and the economic boom would have affected both intervention and control groups, while the control groups would have been affected by contamination to some extent. Some studies reported that control group respondents left welfare voluntarily in large numbers as a result of the economic conditions, leading to small impacts on employment in the studies. Given that the contribution of welfare reform to increased employment in the general lone parent population (who were exposed to the intervention) is considered relatively small, it seems likely that experiences of welfare reform via contamination were responsible for only a small proportion of the control groups' increase in employment.

Quality of the evidence

The review includes 12 RCTs, conducted in a variety of settings. Numbers of participants in a given analysis range from 148 to 14,355. Most studies included in this review were large, well-conducted RCTs of a highly complex social intervention that aimed to influence a number of upstream socioeconomic determinants of health. They provided experimental evidence of the medium- to long-term health effects of a policy-level intervention. As such, they represent a body of evidence of unusual quality in the field of public health. However, as with any body of evidence, there are some methodological issues that are discussed below.

Using the GRADE approach to assessment, the highest quality attained by any of the evidence was moderate, due to every study being at high risk of bias in at least one domain. Due to the high number of outcome measures within each time point and domain, we developed a domain level GRADE assessment (see Risk of bias in included studies). Using this assessment, we judged that 9 of 12 health domains provided moderate-quality evidence, while 2 domains contributed evidence of low quality and 1 domain of unclear quality. We judged 8 of 12 economic domains to be of moderate quality and the remaining 4 as low quality. It is normally expected that evidence from public health interventions will be of low or very low quality (Burford 2012). There were only two studies in which it was unclear whether random sequence generation was adequate, and three in which allocation concealment was unclear. The most common reasons for high risk of bias were contamination, failure to blind outcome assessors and selective outcome reporting (see Figure 5 and Figure 6).

Contamination is not deemed to be as serious as other sources of bias since it is likely to lead to underestimated impacts (Higgins 2011a). It is difficult to know how much the estimates might have been affected by direct contamination arising from exposure of the control group to the intervention, although it seems that only a small proportion of the control group was directly exposed in most studies. Impacts on economic outcomes were stronger in two studies that maintained intervention and control conditions at 49 to 72 months, but most of the effects were still very small, and this may have been due to other differences between the studies. All but one of the studies was affected by indirect contamination arising from changes in attitudes to welfare and publicity surrounding the introduction of welfare reforms, although efforts were made to maintain experimental-control distinctions in a number of these. Again, it is difficult to know how much this might have affected estimates of economic impacts or health outcomes.

Outcome assessors were not blinded in five studies, and blinding was unclear in a further five. Evidence suggests that this is likely to lead to overestimated impacts (Hróbjartsson 2012). There was evidence of selective outcome reporting in six studies reporting results for multiple subgroups or time points. One study reported maternal mental health at T1 but not at T3. No protocol was available for the remainder of studies.

Some evidence was of low or very low quality. With respect to health outcomes, evidence was usually downgraded due to imprecision caused by low event rates. Very high risk of bias in UK ERA 2011 led to some health outcomes and a number of economic outcomes being downgraded at T3. However, excluding UK ERA 2011 from analyses of economic impacts did not change the effect estimates.

Potential biases in the review process

Despite including a very wide range of terms in our electronic searches, we identified more publications via handsearching (14/23) than electronically. Closer inspection of search results indicated that our searches did not identify some publications that were in databases because the population (i.e. lone parents) was rarely specified in titles, keywords or abstracts. However, a feature of this body of evidence is that evaluations of welfare-to-work interventions were conducted at state level by large, well-known research organisations with comprehensive websites. As described in Searching other resources, we put considerable effort into identifying publications listed on such websites. Further, RCTs of social interventions are conducted extremely rarely outside of North America. Thus, we are reasonably confident that we identified all relevant studies.

In contrast to the many public health and social intervention evaluations that lack sufficient detail on intervention content and components (Hoffman 2014), most studies included in this review reported such information in extensive detail and frequently in extremely large reports designed primarily for policy makers. While of course it is welcome to be able to describe interventions in some detail, the level of detail provided was often overwhelming in the context of a systematic review. In addition, when this level of detail is available it becomes apparent that any one component, which initially seems relatively straightforward, can in fact have multiple variations across studies. This raises the question of what level of detail the review author should attempt to capture, and indeed whether any of the components can actually be seen to be the same thing at all. However, as Petticrew 2013 have argued, it is not essential to describe every level of complexity in a given intervention, and it is useful to answer questions regarding the average effects of interventions with the same underlying purpose on the outcomes of interest. As Petticrew 2013 also observed, even so-called 'simple' interventions are likely to be much more complex than is usually acknowledged. Arguably such complexity is often masked by scant reporting of interventions. As discussed previously, it was not possible to use statistical subgroup analyses to investigate these aspects of complexity here. It may, however, be appropriate to do so in a future narrative synthesis.

Agreements and disagreements with other studies or reviews

We are not aware of any other reviews or meta-analyses of welfare-to-work interventions that include maternal health outcomes. Grogger 2002 conducted meta-analysis of data on child health outcomes from US WtW evaluations, including eight of the studies included in this review (CJF 2002; FTP 2000; IFIP 2002; MFIP 2000; New Hope 1999; NEWWS 2001; SSP Applicants 2003; SSP Recipients 2002; search methods were unclear). Grogger 2002 reported small favourable and unfavourable health effects of welfare reform. Most of the favourable health impacts were associated with increased income, but the authors suggest that different intervention components may have countervailing effects, such that effect estimates were very small.

Greenberg 2005 conducted a meta-analysis of child health data from North American WtW evaluations, including five of the studies included in this review (CJF 2002, FTP 2000, IWRE 2002, NEWWS 2001, MFIP 2000; the studies were identified in an existing database of WtW studies). They describe their findings as highly tentative but, contrary to Grogger 2002, they suggest that increased income was not associated with better child health. However, they argue that impacts on income were so small that they were unlikely to influence health outcomes. The review authors identified financial incentives and time limits as intervention components that appeared to have a negative impact on child mental health.

Lucas 2008 conducted a Cochrane Review of the impact of financial benefits on child health and social outcomes. Most of the included studies were of North American welfare-to-work interventions, including CJF 2002, FTP 2000, IFIP 2002, MFIP 2000, New Hope 1999 and SSP. The review came to no overall conclusion on the health impacts of the intervention due to inconsistent effects. The authors noted that effects on income were very small and again suggested that this may explain the lack of effects on health.

A number of studies have also used observational data, including natural experiments using a difference-in-difference (DiD) approach. There has not been a systematic review of this evidence, but Grogger 2002 also included DiD studies, and a more recent narrative review by Ziliak 2015 included DiD and other robust econometric studies of welfare reform in the USA. The findings of Grogger 2002 in relation to economic outcomes are consistent with those described above for RCTs, but they did not find any econometric studies that reported any of the child health outcomes included in this review.

Ziliak 2015 found that studies analysing adult and child health outcomes were scarce and provided conflicting evidence. Based on the limited available studies, Ziliak 2015 reported that effects on maternal health outcomes were mixed but noted there was some evidence of negative effects on black and Hispanic women. Studies reporting child health outcomes were also mixed, but there was some evidence of negative effects on breastfeeding, birthweight, and child maltreatment from studies using the DiD approach to analyse national survey data. In terms of economic outcomes, Ziliak 2015 concluded that while employment and earnings rose, incomes did not, and poverty increased over the longer term. A more recent DiD study of US cross-sectional data also reported small negative impacts on several measures of lone mothers' health, including days of good mental health and health behaviours, although estimates crossed the line of null effect in several cases (Basu 2016).

A systematic review of qualitative studies conducted by several of the authors of the present review found 16 studies conducted in five high-income countries (Campbell 2016). The findings of the qualitative review indicated that lone parents connected WtW participation with increased stress, depression, anxiety and fatigue, apparently due to conflict between WtW and parental responsibilities, and loss of control over key life decisions. There were reports of more positive impacts for some lone parents. More recent qualitative research has been conducted in the UK since the implementation of employment requirements for parents of much younger children; Johnsen 2016 found evidence of extreme anxiety caused by employment requirements and sanctions, in addition to negative physical health effects resulting from insufficient nutrition.

AUTHORS' CONCLUSIONS

Implications for practice

The evidence in this review suggests that interventions aiming to increase employment among lone parents, either by mandating employment in combination with sanctions and earnings disregards, or by offering additional benefits to those who gain employment voluntarily, are likely to have impacts on health which are generally positive but of a magnitude unlikely to have any tangible effects. Effects on employment and income are likely to be small to very small in the medium to long term. There is some evidence to suggest that small negative health impacts are possible in some circumstances. Even where generous financial assistance was provided, effects on income were small.

The ongoing very high levels of depression risk in both intervention and control groups suggest that, although employment increased for both groups, conditions continued to be very challenging for all respondents, and that these interventions did little to address these issues. Given that many of the interventions failed to reduce poverty, it is perhaps not surprising that there was little impact on mental health. Consideration should be given to policies that aim to address the determinants of the high burden of mental ill health among lone parents. Welfare and employment impacts in some of the reviewed studies were limited in part because many of those in the control groups left welfare voluntarily. On this basis, it seems that many lone parents did enter employment of their own volition when circumstances permitted. This would suggest that demand-side issues may have a greater influence on lone parent employment than the individual characteristics targeted by such interventions.

Implications for research

Governments in a number of countries are introducing or scaling up employment requirements for lone parents. The specific content of these policies and interventions varies between and sometimes within countries. In searching for studies to include in this review, we found only one RCT and very few observational studies of the health impacts of welfare-to-work interventions beyond North America. In this light, there is an urgent need for robust evaluations of the economic and health impacts of welfare-to-work interventions for lone parents implemented in other high income countries. Given the institutional and cultural barriers to experimental evaluation of social interventions outside North America, it is unlikely that randomised trials will be conducted in these countries, but at the very least robust observational studies can and should be used to estimate the health impacts of these policies. There are likely opportunities for natural experiments using secondary analysis of existing survey and routine data in many countries.

Reporting of mean impacts may mask substantial variation in intervention effects (Bitler 2006). Where possible, reviews using individual participant meta-analysis would permit investigation of the influence of participant and intervention characteristics, uptake of different intervention components and whether impacts on economic outcomes mediate health effects (Petticrew 2012; Stewart 2011). This would allow review authors to go beyond questions of effectiveness to consider what works, how it works and for whom (Greenhalgh 2015).

As discussed above, contamination was an issue for a number of the studies, although it is difficult to be sure to what extent the control groups were affected by either direct or indirect contamination. Ideally, control groups would have been insulated from intervention conditions or messages, and researchers made efforts to do this in four of the seven US studies conducted following nationwide welfare reform. This situation illustrates one of the difficulties faced by those attempting to research policy-level interventions; it is rarely within the researchers' power to control the timing of an intervention or to prevent concurrent policy change from affecting the research samples (Bonnell 2011; Craig 2008). In the Child Waiver Impact Experiment studies, researchers were in the unusual position of being able to hold back control groups while reform was implemented state-wide. However, such exceptional control designs are clearly far from ideal when the policy is of a type that is likely to influence the control group via cultural and attitudinal changes. The only realistic way to prevent problems arising from contamination is to evaluate the policy prior to widespread implementation. Researchers have been making the case for some time that evaluation is more effective if it is planned and conducted prior to full-scale implementation of a new policy or intervention, in order to collect baseline data and to maintain a comparison or control group that has not been exposed to the new policy (House of Commons 2009).

A major issue with systematic reviews of complex social or public health interventions is their high degree of complexity, which often leads to such reviews being extremely lengthy, time consuming and both resource- and labour-intensive. Thomson 2013 has suggested a number of ways in which highly complex reviews can be narrowed or simplified in order to expedite more speedy completion. However, if there is a desire or need within the research community to conduct reviews that encompass the complexity of these interventions, it is necessary to find a means of addressing the high burden this places on researchers and academic departments.

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* *Indicates the major publication for the study*

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CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

California GAIN 1994

Methods	Randomised controlled trial, follow-up at 36 months
Participants	Full impact sample (welfare applicants and recipients from 6 counties in California: Alameda, Butte, Los Angeles, Riverside, San Diego, Tulane recruited between 1988 and 1990), N ≥ 33,000; AFDC-FG sample (single parents with school-aged children 6 or older), N = approximately 22,770 **Survey sample (survey conducted in Alameda, Los Angeles, Riverside, San Diego, Tulane), N = 2242** Average age: AL 34.7 years/BU 33.6 years/LA 38.5 years/RI 33.7 years/SD 33.8 years/TU 34.9 years Ethnicity (%) - white, non-Hispanic/Hispanic/black, non-Hispanic/lndochinese/other Asian/other AL: 17.9/7.5/68.6/2.1/0.8/1.6 BU:85.7/5.6/3.5/0.6/2.2/2.0 LA: 11.6/31.9/45.3/9.9/0.7/0.4 RI: 51.2/27.6/15.5/1.3/1.7/2.2 SD: 41.8/25.3/22.5/5.5/0.9/3.1 Employment status - currently employed (%): AL 11.5/BU 5.9/LA 26.3/RI 6.4/SD 18.4/TU 6.9 Family structure - not reported
Interventions	Compulsory intervention with caseload reduction (CR) ethos and an approach that varied over time and across sites I*ntervention group*: mandatory employment; childcare subsidy; workfare; sanctions; education and training; varied case management *Control group*: subject to existing welfare programme *Further details of intervention components:* Mandatory employment: participation in employment, training, job search or unpaid work experience required in order to receive welfare payments and other programme benefits. Childcare subsidy: offered fully subsidised transitional child care for one year after participant left welfare for work up to regional market childcare cost rates Workfare: unpaid work experience in a public or non-profit agency, paid at level of state minimum wage Sanctions: financial sanctions were a last resort. They involved a reduction in welfare grant for 3 or 6 months. Duration depended on level of noncompliance. Education and training: participants without high school diploma or low literacy were deemed “in need of basic education” and given opportunity to attend a basic education class - Adult Basic Education (ABE), General Educational Development (GED) prep, or English as Second Language (ESL) instruction. Could choose job search first but if failed to gain employment, required to enter basic education. Skills training, on-the-job train-

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	ing, vocationally oriented postsecondary education or unpaid work experience were also available. Varied case management: case management varied in level of enforcement, monitoring and quality of case management. Also varying emphasis on personalised attention. Generally small caseloads
Outcomes	*Maternal mental health*: unhappy, sad or depressed very often or fairly often (%) *Maternal physical health*: in good or excellent health (%) *Economic employment*: ever employed since randomisation (36 months) (%); ever employed full-time since randomisation (%); ever employed part-time since randomisation (%) *Economic income*: average weekly earnings since randomisation (USD) *Economic insurance*: respondent has Medicaid or other health insurance within 2-3 yrs of randomisation (%)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	No significant differences in baseline characteristics
Blinding of outcome assessment (detection bias) Health outcomes	High risk	All data from face-to-face survey. Outcome assessors not blinded
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	80% response rate. No reasons for missing data provided
Incomplete outcome data (outcome level)	Low risk	Authors report item non-response low
Direct contamination	Low risk	Control group isolated from GAIN participants for duration of study

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Indirect contamination	Low risk	Predates welfare reform
Selective reporting (reporting bias)	Unclear risk	No study protocol available

CJF 2002

Methods	Randomised controlled trial, follow-up at 18 and 36 months
Participants	Half of all welfare applicants and reapplicants in Manchester and New Haven randomised between January 1996 and February 1997. Full sample N = 4803 **Focal Child Sample (single mothers with a child between the ages of 5 and 12 at the 3-year survey), N = 2069** Age - average age 30.1 years Ethnicity - white non-Hispanic 34.5%; black non-Hispanic 42.5%; Hispanic 22.2%; other 0.45% (averaged across Jobs First and AFDC) Employment status - 25.5% of full sample employed Family structure - 0.4% married, living together
Interventions	Compulsory intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach (moved towards human capital development (HCD) approach in implementation) *Intervention group*: mandatory employment; earnings disregard; childcare subsidy; workfare; time limit; sanctions; education and training; health insurance; low case management *Control group*: subject to previous welfare programme *Further details of intervention components*: Mandatory employment: participation in employment, training, job search or unpaid work experience required in order to receive welfare payments and other programme benefits Earnings supplements: none Earnings disregards: for employed recipients, all earned income disregarded when calculating grants and food stamp benefits as long as below the federal poverty level (USD 1138 per month for family of 3 in 1998) Childcare subsidy: provided childcare assistance for families leaving welfare for work for as long as income was below 75% of state median Workfare: unpaid work experience; no further detail Lifetime limit: 21 cumulative months of cash assistance unless in receipt of exemption or extension. Renewable 6 month extensions available if made a “good-faith effort” to find work and income below welfare payment standard. Many extensions were allowed Sanctions: failing to meet work requirements within 21 months or quitting job without good cause could result in welfare grant being reduced or closed. 1st instance = reduced by 20% for 3 months; 2nd instance = reduced by 35% for 3 months; 3rd instance = grant cancelled for 3 months. Stricter when reached time limit - a “one-strike” policy where one instance of non-compliance during extension could result in permanent discontinuance of grant Education and training: education and training provided for those unable to find a job after 3-6 months job search activities. Adult basic education, GED prep, ESL, vocational

	training. Also job search skills training if independent job search failed. Moved toward greater emphasis on training during intervention Health insurance: provided 2 years of transitional Medicaid for families leaving welfare for work Case management: focus on self-directed job search. Case management generally non-intensive, with low levels of monitoring and interaction. Lack of resources coupled with large caseloads
Outcomes	*Maternal mental health*: T2 CES-D mean score (0-60) *Child mental health*: T2 Behavior Problems Index (0-56) *Child physical health*: T2 general health scale (1-5) *Economic employment*: T2 ever employed in year of study (%) *Economic benefit receipt*: T2 average annual welfare benefit year 3 (USD) *Economic income:* T2 average annual income (benefits, earnings and Food Stamps) years 3-4 (USD) T2 average earnings in year of survey (USD)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	No significant differences in baseline characteristics. Regression used to control for baseline characteristics
Blinding of outcome assessment (detection bias) Health outcomes	High risk	Health outcomes collected by face-to-face survey; outcome assessors not blinded
Incomplete outcome data (attrition bias) All outcomes	Low risk	Response rate: intervention 72%, control 70%. Weighting and regression used to control for treatment group and response differences

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Incomplete outcome data (outcome level)	High risk	Authors report that sample size may vary for all health outcomes
Direct contamination	Low risk	No evidence that control group received intervention treatment
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare; authors state control group likely to have been influenced by welfare reform
Selective reporting (reporting bias)	Unclear risk	No study protocol available

CJF GUP 2000

Methods	Randomised controlled trial, follow up at 18 and 36 months
Participants	Half of all welfare applicants and reapplicants in Manchester and New Haven randomised between January 1996 and February 1997. Full sample N = 4803 **Connecticut Interim Client Survey sample (child aged 12-42 months at the 18-month interview), N = 342** Age - average age at 18-month interview: 25.4 years Ethnicity - Latina 20%; African American 38%; white/Anglo 42% Employment status - 46% of all women had worked in the year prior to randomisation Family structure - 73% mothers never married
Interventions	Compulsory intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach (moved towards human capital development (HCD) approach in implementation) *Intervention group*: mandatory employment; earnings disregard; childcare subsidy; workfare; time limit; sanctions; education and training; health insurance; low case management *Control group*: subject to previous welfare programme *Further details of intervention components*: Mandatory employment: participation in employment, training, job search or unpaid work experience required in order to receive welfare payments and other programme benefits Earnings supplements: none Earnings disregards: for employed recipients, all earned income disregarded when calculating grants and food stamp benefits as long as below the federal poverty level (USD 1138 per month for family of 3 in 1998) Childcare subsidy: provided childcare assistance for families leaving welfare for work for as long as income was below 75% of state median Workfare: unpaid work experience; no further detail Lifetime limit: 21 cumulative months of cash assistance unless in receipt of exemption or extension. Renewable 6 month extensions available if made a “good-faith effort” to find work and income below welfare payment standard. Many extensions were allowed Sanctions: failing to meet work requirements within 21 months or quitting job without

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	good cause could result in welfare grant being reduced or closed. 1st instance = reduced by 20% for 3 months; 2nd instance = reduced by 35% for 3 months; 3rd instance = grant cancelled for 3 months. Stricter when reached time limit - a "one-strike" policy where one instance of noncompliance during extension could result in permanent discontinuance of grant Education and training: education and training provided for those unable to find a job after 3-6 months job search activities. Adult basic education, GED prep, ESL, vocational training. Also job search skills training if independent job search failed. Moved toward greater emphasis on training during intervention Health insurance: provided 2 years of transitional Medicaid for families leaving welfare for work Case management: focus on self-directed job search. Case management generally non-intensive, with low levels of monitoring and interaction. Lack of resources coupled with large caseloads
Outcomes	*Maternal mental health*: T1 CIDI at risk (% threshold not reported) T2 CES-D mean score (0-60) *Child mental health*: T2 Child Behavior Checklist (1-3) *Economic employment*: T1 currently employed (%) *Economic insurance*: T1 respondent has Medicaid (%)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Unclear risk	Baseline characteristics reported for whole sample; no adjustment reported
Blinding of outcome assessment (detection bias) Health outcomes	High risk	Health outcomes collected by face-to-face survey; outcome assessors not blinded
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	78% response rate at 36 months; no reasons for missing data provided

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Incomplete outcome data (outcome level)	Unclear risk	No information on item non-response
Direct contamination	Low risk	No evidence that control group received intervention treatment
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare; authors state control group likely to have been influenced by welfare reform
Selective reporting (reporting bias)	High risk	Outcomes reported at each time point differ. CES-D mean score and Child Behavior Checklist reported at 36 months but not at 18 months. Mother reported general health collected at each time point but not reported

CJF Yale 2001

Methods	Randomised controlled trial, follow up at 18 months
Participants	Half of all welfare applicants and reapplicants in Manchester and New Haven randomised between January 1996 and February 1997. Full sample N = 4803 **Older child subsample (child aged 3-10 years at 18-month interview) N = 311** Age - average age 30.1 years Ethnicity - black, non-Hispanic: 41.31%; Hispanic: 17.70%; Asian/Pacific Islander: 0.69%; white: 39.61%; other: 0.69% Employment status - not reported Family structure - 4.52% living with spouse
Interventions	Compulsory intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach (moved towards human capital development (HCD) approach in implementation) *Intervention group*: mandatory employment; earnings disregard; childcare subsidy; workfare; time limit; sanctions; education and training; health insurance; low case management *Control group*: subject to previous welfare programme *Further details of intervention components*: Mandatory employment: participation in employment, training, job search or unpaid work experience required in order to receive welfare payments and other programme benefits Earnings supplements: none Earnings disregards: for employed recipients, all earned income disregarded when calculating grants and food stamp benefits as long as below the federal poverty level (USD 1138 per month for family of 3 in 1998) Childcare subsidy: provided childcare assistance for families leaving welfare for work for as long as income was below 75% of state median

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	Workfare: unpaid work experience; no further detail Lifetime limit: 21 cumulative months of cash assistance unless in receipt of exemption or extension. Renewable 6-month extensions available if made a "good-faith effort" to find work and income below welfare payment standard. Many extensions were allowed Sanctions: failing to meet work requirements within 21 months or quitting job without good cause could result in welfare grant being reduced or closed. 1st instance = reduced by 20% for 3 months; 2nd instance = reduced by 35% for 3 months; 3rd instance = grant cancelled for 3 months. Stricter when reached time limit - a "one-strike" policy where one instance of non-compliance during extension could result in permanent discontinuance of grant Education and training: education and training provided for those unable to find a job after 3-6 months job search activities. Adult basic education, GED prep, ESL, vocational training. Also job search skills training if independent job search failed. Moved toward greater emphasis on training during intervention Health insurance: provided 2 years of transitional Medicaid for families leaving welfare for work Case management: focus on self-directed job search. Case management generally non-intensive, with low levels of monitoring and interaction. Lack of resources coupled with large caseloads
Outcomes	*Maternal mental health*: T1 = CES-D % at risk ($\geq$ 16/60) *Maternal physical health*: T1 = 1 or more physical health problems (%) *Child mental health*: T1 = Behavior Problems Index (% with problems) *Economic employment*: currently employed (%); ever employed since randomisation (18 months) (%) *Economic insurance*: respondent has Medicaid (%)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	Baseline characteristics presented and compared; few significant differences

CJF Yale 2001 *(Continued)*

Blinding of outcome assessment (detection bias) Health outcomes	High risk	All data collected by face-to-face survey; outcome assessors not blinded
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	20% of the sample lost to follow-up or refused to participate. Reasons for missing data not presented by intervention group status
Incomplete outcome data (outcome level)	Unclear risk	No information on item non-response provided
Direct contamination	Low risk	No evidence that control group received intervention treatment
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare; authors state control group likely to have been influenced by welfare reform
Selective reporting (reporting bias)	Unclear risk	No study protocol available

FTP 2000

Methods	Randomised controlled trial, follow up at 48 months
Participants	Report sample (single parent applicants and a proportion of reapplicants in Escambia County, Florida, randomly assigned between August 1994 and February 1995) N = 2817 **Focal Child Sample: respondents to 4-year survey sample who had a child between 5 and 12 years old, N = 1108** Age - maternal age categories (%) - intervention/control: under 20 years: 8.1/6.0; 20-23 years: 22.8/24.1; 24-33 years: 54.9/54.3; 34-43 years: 13.1/14.3; 44 years or older: 1.1/1.2 Ethnicity (%) - intervention/control: white, non-Hispanic: 44.7/43.3; black, non-Hispanic: 53.3/54.9; other: 2.0/1.8 Employment status - not reported Family structure - married, live together (%): intervention/control 0.6/1.2
Interventions	Compulsory intervention with anti-poverty (AP) ethos and human capital development (HCD) approach (moving towards labour force attachment (LFA) in implementation) *Intervention group*: mandatory employment; earnings disregard; childcare subsidy; workfare; time limit; sanctions; education and training; high case management *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: participation in employment, training, job search or unpaid work experience of at least 30 hours per/week required in order in order to receive welfare payments and other programme benefits Earnings supplements: none

FTP 2000 (*Continued*)

	Earnings disregards: enhanced earned income disregard - first USD 200 plus one-half of remaining earned income disregarded in calculating monthly grant Childcare subsidy: offered subsidised transitional child care for 2 years after participant left welfare for work Workfare: job ready participants assigned to workfare if they did not find employment after 3 weeks of job search Lifetime limit: limited most families to 24 months of cash assistance in any 60-month period ('least job-ready' 36 in 72 months). Allowed up to 2, four-month extensions in some circumstances. Time limit could also be suspended if health problems identified by a doctor Sanctions: first 3 years of implementation, sanctions involved partial benefit termination. Adopted WAGES sanctioning policy in mid-1997 which could result in full termination for repeated noncompliance. Under WAGES: 1st instance = cash assistance closed until compliance; 2nd instance = cash and food stamps case closed until 30 days of compliance; 3rd instance = both closed for at least 3 months Education and training: strong emphasis on training provision, which was well resourced. Provided adult basic education and vocational training. Assigned some participants (lacking high school diploma/low literacy) to community institutions providing maths and reading instruction or GED prep. Created special short-term training programs for those facing time limits which could lead to qualifications such as nursing, machining, office supervision. Strong links with local industry Health insurance: none Case management: individualised, intensive case management delivery with small staff to participant ratios. Provided intensive one-on-one job placement help to those approaching time limit
Outcomes	*Maternal mental health*: CES-D mean score (0-60) *Child mental health*: Behavior Problems Index (0-56) *Child physical health*: general health scale (1-5) *Economic employment*: ever employed in year of study (%) *Economic benefit receipt*: total AFDC/TANF received year 4 (USD) *Economic income:* average total income (benefits, earnings and food stamps) year 4 (USD); average total income from earnings, AFDC/TANF and food stamps year 4 (USD)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences

Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	Baseline characteristics presented and compared; no significant differences
Blinding of outcome assessment (detection bias) Health outcomes	High risk	Health outcomes collected by face-to-face survey; outcome assessors not blinded
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	20% of the sample lost to follow-up or refused to participate; reasons for missing data not presented by intervention group status
Incomplete outcome data (outcome level)	High risk	Authors report that sample size may vary for all health outcomes
Direct contamination	Low risk	No evidence that control group received intervention treatment
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare; authors state control group likely to have been influenced by welfare reform
Selective reporting (reporting bias)	Unclear risk	No study protocol available

IFIP 2002

Methods	Randomised controlled trial, follow up at 60 months
Participants	Full sample (ongoing welfare recipients randomised 10/93, new applicants randomised between 10/93 3/96), N = 17,345. Core survey sample (stratified random sample from full sample) N = 4111 **Child Impact Study sample (respondents from the Core survey that had a child between 5 and 12 years old), N = 1962** Age - average age Ongoing cases: 26.6 years; applicants: 26.1 years Ethnicity - race/ethnicity (%) Ongoing cases: white: 79.8; black: 16.0; Hispanic or other: 3.1 Applicants: white: 78.6; black: 8.0; Hispanic or other: 4.1 Employment status - employed in year prior to randomisation: Ongoing cases: 51.8% Applicants: 69.6% Family structure - married respondents

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	Applicants: 33.9% Ongoing cases: 16.8%
Interventions	Compulsory intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach *Intervention group*: mandatory employment; earnings disregard; childcare subsidy; workfare; sanctions; education and training; case management not reported *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: required to participate in PROMISE JOBS, a programme providing employment and training opportunities. Required to complete 20 hours of work or work-related activities per week in order to receive welfare payments and other programme benefits Earnings supplements: none Earnings disregards: FIP provided earned-income disregards that resulted in a tax rate on earnings of only 40 percent: for every USD 1 a FIP family earned, FIP benefit amount reduced by USD 0.40, so total income increases by USD 0.60 Childcare subsidy: financial assistance to pay for child care while receiving cash welfare and for up to 2 years after their cash welfare case closed because of earnings or employment. The parent made a modest co-payment based on family income and size, and IFIP paid the remaining cost of child care, up to the provider's regular fee for private-paying families or the state's maximum payment rate, whichever was lower Workfare: unpaid work experience and community service mentioned; no further detail Lifetime limit: no time limit mentioned Sanctions: failure to comply with programme requirements led to assignment to the Limited Benefit Plan. Initially this provided 3 months of full FIP cash benefits, then 3 months of reduced benefits and then 6 months of no benefits for the whole family. Revised in 1996 to 3 months of reduced benefits followed by 6 of no benefits. For second failure benefits terminated fully and immediately for 6 months. Revised in 1999 to full termination for first instance of non-compliance. Benefits restored immediately on compliance Education and training: placed little weight on developing skills and more on rapid entry into employment. However, did require mothers under 18 to obtain a high school diploma or GED Health insurance: none Case management: little detail provided
Outcomes	*Maternal mental health*: CES-D % at high risk ($\geq$ 23/60) *Child mental health*: Behavior Problems Index (0-56) *Child physical health*: in fair or poor health (%) *Economic employment*: currently employed (%): currently employed full-time (%); currently employed part-time (%) *Economic benefit receipt*: average welfare received month prior to survey (USD);

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	currently receiving Family Independence Payment (%) *Economic income:* household income month prior to survey (USD); average earnings month prior to survey(USD) *Economic insurance*: family has health insurance (%)
Notes	-

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	No information provided
Allocation concealment (selection bias)	Unclear risk	No information provided
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	No significant differences in baseline characteristics; regression used to control for differences
Blinding of outcome assessment (detection bias) Health outcomes	Unclear risk	All outcomes collected by face-to-face survey; no information on blinding
Incomplete outcome data (attrition bias) All outcomes	Low risk	Response rate: intervention 75.8%; control 74.0%. Weights used to account for survey non-response and attrition
Incomplete outcome data (outcome level)	Low risk	Authors report item non-response low
Direct contamination	High risk	Control conditions terminated during intervention; all participants moved to TANF at 3.5 years
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare
Selective reporting (reporting bias)	Unclear risk	No study protocol available

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IWRE 2002

Methods	Randomised controlled trial, follow up at 60 months
Participants	Full sample (all Indiana single-parent welfare recipients randomly assigned between May 1995 and April 1996), N = 66,440 5-year survey (stratified random sample from full population), N = 3360 **Focal Child sample (families who completed 5-year survey with a child aged 5 to 12),** N =**1679.** Age - under 25 years: 48%; 25-34 years: 42.7%; 35 + years: 9.3% Ethnicity - non-white: 44.7% Employment status - quarters worked in the 5 quarters before randomisation: 38.3% none; 35.8% between 1-3 quarters; 25.9% between 4-5 quarters 25.9% Family structure - never married: 43.8%; separated: 10.5%; divorced or widowed: 22.5%; married and living with spouse: 23.2%
Interventions	Compulsory intervention with caseload reduction (CR) ethos and labour force attachment (LFA) approach *Intervention group*: mandatory employment; earnings disregard; childcare subsidy; time limit; sanctions; education and training; health insurance; case management not reported *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: required to participate in work activities (primarily working or looking for employment) for 25 hours per week in order to receive welfare payments and other programme benefits Earnings supplements: none Earnings disregards: TANF grant fixed at level of recipients' initial earnings for some time after they entered employment Childcare subsidy: subsidy provided but no detail given Workfare: none Lifetime limit: 24-month lifetime limit on TANF receipt. Affected only adults' portion of the grant; children continued to receive assistance Sanctions: for first violation, TANF grant reduced by adult's portion for 2 months, for second and third violation, reduced by same amount for 12 and 36 months, respectively. No full family sanction Education and training: training is referred to but no detail is provided. Main activity is described as "unsubsidized employment and job search" Health insurance: none Case management: Little detail provided
Outcomes	*Maternal mental health*: CES-D mean score (0-60) *Child mental health*: Behavior Problems Index (0-56) *Child physical health*: health status scale (1-5) *Economic employment*: currently employed (%) *Economic benefit receipt*: TANF receipt month before survey, annualised year 5 (USD); currently receiving TANF (%)

	Economic income: total household income month prior to survey, annualised (USD); earnings month prior to survey, annualised (USD)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Conducted by Indiana State; no information provided
Allocation concealment (selection bias)	Unclear risk	Conducted by Indiana State; no information provided
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	No significant differences in baseline characteristics; regression used to control for differences
Blinding of outcome assessment (detection bias) Health outcomes	Unclear risk	All outcomes collected by face-to-face survey; blinding of outcome assessors unlikely; maternal depression was self-administered
Incomplete outcome data (attrition bias) All outcomes	Low risk	Overall response rate 70%; reports statistically significant difference between intervention and control groups. Weights used to adjust for attrition
Incomplete outcome data (outcome level)	Unclear risk	No information on item non-response provided
Direct contamination	Low risk	No evidence that control group received intervention treatment
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare; authors state control group likely to have been influenced by welfare reform
Selective reporting (reporting bias)	Unclear risk	No study protocol available

MFIP 2000

Methods	Randomised controlled trial, follow up at 36 months
Participants	Total sample; welfare applicants and recipients from April 1994 to March 1996 in 3 urban and 4 rural Minessota counties), N = 14,639 **Child Study Survey Sample (random subset of families who entered programme between April 1994 and October 1994 with at least one child between 2 and 9 years old), N = 2639** Age - average age: 28.9 among long-term recipients, 30.1 among recent applicants Ethnicity - % long-term recipients/% recent applicants: White, non-Hispanic: 46.4/63.5 Black, non-Hispanic: 40.9/27.9 Hispanic: 2.2/2.2 Native American/Alaskan Native: 8.8/5.3 Asian/Pacific Islander: 1.7/1.2 Employment status - 12.8% among long-term recipients, 22.3% among recent applicants Family structure - married, living with spouse: 0.5% among long-term recipients, 0.6% among recent applicants
Interventions	Compulsory intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach (intervention group 1) *Intervention group*1 (MFIP): mandatory employment; earnings disregard; childcare subsidy; sanctions; mandatory education and training; case management not reported *Intervention group*2 (MFIP-incentives only): earnings disregard; childcare subsidy; voluntary education and training; case management not reported *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: mandatory participation in employment-focused activities for long-term welfare recipients. MFIP required mothers who were unemployed for 24 months out of the previous 36 to work 30 hours at least per week if not participating in employment services or 20 hours if had child under age of 6 in order to receive welfare payments and other programme benefits. Short-term recipients and MFIP-IO group were not required to participate in work related activities but received programme benefits if they did Earnings supplements: none Earnings disregards: recipients eligible for welfare until income reached 140% of the poverty level. Those already working received additional income for no extra hours of work Childcare subsidy: child care subsidies paid directly to provider if recipient working while on welfare. Amounts paid did not differ from control group, but intervention group also given child care for attending counselling, drug programmes etc. to tackle barriers to work Workfare: none Lifetime limit: none Sanctions: failure to comply with the programme requirements led to sanction involving monthly welfare payments reduced by 10% Education and training: employment and training participation required if receiving assistance for 24 of past 36 months. Provided job search, short-term training, and educational activities Health insurance: none

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	Case management: case management role to monitor and give guidance but level of monitoring or time spent with clients not detailed; staff-to-participant ratio not mentioned
Outcomes	*Maternal mental health*: CES-D % at high risk (≥23/60) *Child mental health*: Behavior Problems Index (0-56) *Child physical health*: in good or excellent health (%) *Economic employment*: ever employed since randomisation (36 months) (%); ever employed full-time since randomisation (%); ever employed part-time since randomisation (%) *Economic benefit receipt*: average annual welfare benefit year 3 (USD) *Economic income*: average annual income (benefits and earnings) year 3 (USD); average annual earnings years 1-3 (USD) *Economic insurance*: children have health insurance continuously past 36 months (%)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Unclear risk	Baseline characteristics not presented separately for intervention groups; some baseline characteristics are controlled for, but not clear which ones
Blinding of outcome assessment (detection bias) Health outcomes	Low risk	Health outcomes collected by Audio-CASI; outcome assessors blind to response
Incomplete outcome data (attrition bias) All outcomes	Low risk	Response rate: intervention 80.3% control 75%.Regression used to control for differences between groups

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MFIP 2000 *(Continued)*

Incomplete outcome data (outcome level)	Low risk	Values for health outcomes imputed to account for item non-response
Direct contamination	Low risk	No evidence that control group received intervention treatment
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare; authors state control group likely to have been influenced by welfare reform
Selective reporting (reporting bias)	High risk	Maternal CES-D scale, health insurance and employment outcomes not reported for rural subgroups

New Hope 1999

Methods	Randomised controlled trial, follow up at 24, 60 and 96 months
Participants	Total sample: low-income adults aged ≥ 18 years living in 2 inner city areas of Milwaukee, randomly assigned from August 1994 through December 1995. Recruited by community workers in community settings. N = 1357 **Child and Family Study (CFS) Sample (families with at least one child between ages 1 and 10 at baseline), N = 745** Age - average age T1: 29.4 years Ethnicity - T1: African American: 55.0%, Hispanic: 29.3%, white: 12.5%, Native American/Alaskan Native: 3.2% Employment status - employed at randomisation: 36.5% Family structure - married, living with spouse: 10.5%
Interventions	Voluntary intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach *Intervention group*: earnings supplement; childcare subsidy; health insurance; high case management *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: voluntary programme. Required to work full time (at least 30 hours a week) in order to receive earnings supplements and other programme benefits Earnings supplements: New Hope offered monthly earnings supplements to participants who worked at least 30 hours per week but whose earnings left their household below 200% of the poverty line. Earnings supplements were adjusted upward for household size, up to a maximum of 2 adults and 4 children Earnings disregards: none Childcare subsidy: financial assistance to cover child care expenses for children under age 13 when parent worked at least 30 hours per week. Participants paid a portion of the cost, based on income and household size; New Hope covered the remainder. For participants to qualify for New Hope subsidies, the child care had to be provided in

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	state-licensed or county-certified homes or child care centres Workfare: none. Community service jobs were available to those who could not find employment independently, but these were voluntary and paid at market rates Lifetime limit: no limit on receipt of welfare payments. 3-year limit on supplement payments Sanctions: none Education and training: education or training activities were not provided but New Hope staff provided advice and signposting to training Health insurance: provided for those working at least 30 hours per week and not covered by employers' health insurance or Medicaid. Required to contribute toward premium on a sliding scale that took into account their income and household size; New Hope subsidised the remainder Case management: intensive case management with high-quality staff services, individualised attention, flexibility and frequent contact. Voluntary so focus on engagement through support rather than sanctions
Outcomes	*Maternal mental health*: T1 CES-D mean score (0-60); T3 CES-D mean score (0-60) *Maternal physical health*: T3 physical health scale (1-5) *Child mental health*: T1 Problem Behavior Scale (1-5); T3 Problem Behavior Scale (1-5) *Child physical health*: T3 overall health scale (1-5) *Economic employment*: T1 ever employed year 2 (%); T3 ever employed year 5 (%); T3 currently employed full-time (%) *Economic benefit receipt*: T1 total AFDC received year 2 (USD); T1 ever received AFDC/TANF year 2 (%); T3 total AFDC/TANF receipt year 5 (USD); T3 ever received AFDC/TANF year 5 (%) *Economic income*: T1 total income year 2 (USD); T1 average annual earnings year 2 (USD); T3 total income year 5 (USD); T3 average earnings year 5 (USD) *Economic insurance*: T1 respondent ever had Medicaid since randomisation (24 months) (%); T3 respondent has health insurance (%); T3 all focal children have health insurance (%)
Notes	-

Risk of bias

New Hope 1999 *(Continued)*

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	No significant differences in baseline characteristics. Weighting used to control for differences
Blinding of outcome assessment (detection bias) Health outcomes	High risk	Health outcomes collected by face-to-face survey; outcome assessors not blinded
Incomplete outcome data (attrition bias) All outcomes	Low risk	Response rates (%): At 2 years intervention 79.7, control 79 At 5 years intervention 77, control 73.5 Unit and item non-response addressed using multiple imputation
Incomplete outcome data (outcome level)	Low risk	Unit and item non-response addressed using multiple imputation
Direct contamination	High risk	Wisconsin Works implemented state-wide in 1997, a year before New Hope intervention ended. It is unclear how much New Hope participants were affected but it is likely that year 5 data are affected by contamination bias
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare
Selective reporting (reporting bias)	High risk	Maternal physical health not reported at T1. Child overall health not reported at T1. Total behaviour problems not reported at 96 months

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NEWWS 2001

Methods	Randomised controlled trial, follow-up at 24 and 60 months
Participants	Full impact sample (welfare applicants or recipients randomly assigned June 1991 to December 1994 in Atlanta, Grand Rapids, Riverside, Columbus, Detroit, Oklahoma City, Portland), N = 41,715 **Child Outcomes Study sample (single parent with child aged 3 to 5 years at randomisation, in Atlanta, Grand Rapids or Riverside. Randomly selected from respondents to 2-year Survey), N = 3018** Age - T1 mean age of mother: 29.0 years in Atlanta, 26.7 years in Grand Rapids, 29.3 years in Riverside Ethnicity - T1 % in Atlanta/Grand Rapids/Riverside White, non-Hispanic: 3.6/52.7/46.3; Hispanic: 0.7/6.0/31.4; black, non-Hispanic: 95.2/39.1/19.6; black Hispanic: 0.1/0.2/0.0; American Indian/Alaskan: 0.2/1.1/1.3; Asian/Pacific Islander: 0.1/0.2/1.5; other: 0.1/0.8/0.0 Employment status - T1 % in Atlanta/Grand Rapids/Riverside employed at baseline 9.1%/11.5%/9.7% Family structure - T1 % in Atlanta/Grand Rapids/Riverside married, living with spouse: 0.9%/2.1%/2.2%
Interventions	Compulsory intervention with caseload reduction (CR) ethos, human capital development (HCD) approach (intervention group 1) and labour force attachment (LFA) approach (intervention group 2) *Intervention group 1 (HCD)*: mandatory employment; workfare; sanctions; education and training; high case management *Intervention group 2 (LFA)*: mandatory employment; workfare; sanctions; education and training; high case management *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: required to engage in a JOBS welfare-to-work programme requiring mandated participation in education, training and/or employment activities for an average of 30 hours per week, including at least 20 hours in actual work or job search, in order to receive welfare payments and other programme benefits Earnings supplements: none Earnings disregards: none Childcare subsidy: none Workfare: participants could be assigned to 3 types of work experience positions: unpaid work in the public or private sector (in exchange for their welfare grant), on job training in private sector and paid work. More common in LFA programmes Lifetime limit: none Sanctions: sanctions in place for non-participation in work mandates. Grand Rapids LFA in particular frequently issued sanctions, while other programmes gave clients more chances to comply. Adult welfare grant was reduced by approximately 20%, depending on the site. Penalty continued until sanctioned individual complied with participation mandate. Minimum sanction length of 3 months for 2nd 'offence' and 6 months for third offence (no minimum length for first offence) Education and training: HCD groups initially assigned to some type of skill-building activity (GED prep, ESL, adult basic skills classes). LFA programmes assigned most enrollees to job club as first activity. Education and training available after if necessary or in addition to work

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	Health insurance: none Case management: most sites described as 'high enforcement' with close monitoring and sanctions applied for non-participation. Suggests intensive case management. Seems HCD programmes more flexible, though varied across sites
Outcomes	*Maternal mental health*: T1 CES-D mean score (0-36) *Child mental health*: T1 Behavior Problems Index (0-2); T3 BPI Externalising subscore (0-18); T3 BPI Internalising subscore (0-24); T3 BPI Hyperactivity subscore (0-18) *Child physical health*: T1 general health rating (1-5); T3 general health rating (1-5) *Economic employment*: T1 currently employed (%); T1 ever employed since randomisation (24 months) (%); T3 currently employed (%); T3 ever employed years 1-5 (%); T3 currently employed full-time (%); T3 currently employed part-time (%) *Economic benefit receipt*: T1 currently receiving AFDC (%); T3 total welfare payments years 1-5 (USD) *Economic income*: T1 total net household income in prior month (USD); T1 average earnings previous month (USD); T3 total income years 1-5 (USD); T3 average earnings years 1-5 (USD) *Economic insurance*: T1 respondent ever had employer-provided health insurance since randomisation (24 months) (%); T1 child health insurance (%); T3 family has health insurance (%)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Probably done, since other reports from the same investigators clearly describe use of random sequences
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6

Baseline outcome measurements Unclear risk	Unclear risk	Maternal CES-D collected at baseline and controlled for, but no other health outcomes collected at baseline
Baseline characteristics	Low risk	Response rates at 5 years (%): Atlanta LFA: 82.8 Atlanta HCD: 77.6 Atlanta control: 79.9 Grand Rapids LFA: 84.5 Grand Rapids HCD: 80.3 Grand Rapids control: 85.9 Riverside LFA: 62.9 Riverside HCD: 67.3 Riverside control: 64.9 Weights and regression used to control for differences in baseline characteristics
Blinding of outcome assessment (detection bias) Health outcomes	High risk	Health outcomes by face-to-face survey; outcome assessors not blinded
Incomplete outcome data (attrition bias) All outcomes	Low risk	Weights and regression used to correct/control for differences in background characteristics. Authors state differences in response rates and characteristics were not sufficient to bias the impacts
Incomplete outcome data (outcome level)	High risk	Authors report that sample size may vary for all health outcomes
Direct contamination	High risk	Some control group members in Atlanta and Grand Rapids were required to participate in WtW programmes after year 3. Data at 5 years may therefore suffer from contamination bias
Indirect contamination	High risk	All studies conducted after 1996 affected by change in attitudes to welfare
Selective reporting (reporting bias)	High risk	Maternal CES-D scale and summary Behavior Problems Index only reported at 24 months

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Ontario 2001

Methods	Randomised controlled trial, follow up at 24 and 48 months
Participants	Full sample (all new single parent applicants approved to receive welfare benefit in 2 areas of Ontario) N (eligible) = 1739; N (recruited) = 765 **Full intervention group, employment training group and control group N = 459** Age (%): 15-19 years: full Intervention (FI) 2.5/employment retraining (ER) 4.1/self-directed (SD) 6.9; 20-24 years: FI 16.5/ER 19/SD 16.4; 25-29: FI 21.5/ER 23.1/SD 19; 30-34 years: FI 19/ER 21.5/SD 20.7; 35-39 years: FI 21.5/ER 16.5/SD 19.8; 40 and over years: FI 19/ER 15.7/SD 17.2 Ethnicity - not reported. Employment status - %: full-time work: FI 5/ER 5.8/SD 1.7; part-time work: FI 15.8/ER 10/SD 14.7; unemployed: FI 16.7/ER 15.8/SD 20.7 Family structure - marital status (n/%) Married or remarried or common law: 12/1.6; separated 343/ 44.8; divorced or annulled 171/22.4; widowed 10/1.3; never married 22.9/29.9
Interventions	Voluntary intervention *Full Intervention group*: childcare subsidy; education and training; high case management *Employment training group*: employment training only *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: not required to participate in programme activities in order to receive welfare payments or other programme benefits. No sanctions or supplements attached to non/participation Earnings supplements: none Earnings disregards: none Childcare subsidy: group 1 (comprehensive approach) received subsidised after school recreation/child care twice a week for 4 years Workfare: none Lifetime limit: none Sanctions: none Education and training: group 1 received up to 6 employment skills focused sessions with an employment counsellor Health insurance: NA Case management: case management involved home visits and intensive contact and support. Flexible/personalised case management focused on problem solving, engagement and empowerment
Outcomes	*Maternal mental health*: T1 presence of mood disorders (University of Michigan, Composite International Diagnostic Interview) (%) *Maternal physical health*: T2 in good or excellent health % *Child mental health*: T1 1 or more behaviour disorders (Survey Diagnostic Instrument) (%); T2 Survey Diagnostic Instrument Conduct Disorder (0-30) *Economic benefit receipt*: T1 received social assistance in last 12 months (%); T2 social assistance/unemployment insurance receipt year 4 (%)

Notes	-	
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	"Subjects eligible and receiving income maintenance were randomly allocated to one of five treatment strategies using a computerized randomization schedule which blocked randomly after every 5th or 10th subject (household) to ensure equal numbers in all treatment groups."
Allocation concealment (selection bias)	Unclear risk	No information provided
Baseline outcome measurements Unclear risk	High risk	Outcome measures presented for all groups, but differ by group; no mention of adjustment
Baseline characteristics	Low risk	Many baseline characteristics are presented; there are few significant differences between groups
Blinding of outcome assessment (detection bias) Health outcomes	Low risk	Outcome assessors blind to intervention status
Incomplete outcome data (attrition bias) All outcomes	High risk	Very high. Overall response rate at randomisation 44%. Of 1739 eligibles, 700 refused and 274 were not contactable. At 2 years, response rate varied across groups from 38%-58%. Overall response rate at 4 years was 78.5% of randomisation sample; no reasons for missing data provided
Incomplete outcome data (outcome level)	Unclear risk	No information on item non-response
Direct contamination	High risk	Ontario Works introduced in 1996, although single parents of children under school age often exempt. Also earnings disregards increased. Difficult to assess how much this would have affected this sample
Indirect contamination	High risk	Attitudes to welfare became increasingly negative during this period

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Selective reporting (reporting bias)	High risk	All outcomes reported at each time point differ

SSP Applicants 2003

Methods	Randomised controlled trial, follow up at 72 months
Participants	**Applicant study sample; single parents 19 years or older who had recently applied for Income Assistance (and remained on it for 12 out of 13 months in order to receive supplement), assigned randomly between February 1994 and March 1995. Randomly selected from all adult single parents applying for IA in selected areas of British Columbia. N = 3,315** Age - under age 25 (%): intervention 15.5, control 14.3 Ethnicity - First Nations ancestry (%) intervention 7.2, control 8.7 Employment status - worked in month before randomisation (%): intervention 24.0, control 23.1 Family structure - never married (%): intervention 21.6, control 25.1
Interventions	Voluntary intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach *Intervention group*: earnings supplement; low case management *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: not required to participate in employment in order to receive welfare payments. Required to work at least 30 hours per week to be eligible for supplement payments Earnings supplements: financial supplement paid to parents who worked 30 or more hours per week an amount equal to half the difference between their actual earnings and a 'benchmarked' level of earnings. During the first year of operations, the benchmark was CAD 30,000 in New Brunswick and CAD 37,000 in British Columbia. Had to remain on Income Assistance for 12 months to qualify for supplement payments Earnings disregards: none Childcare subsidy: none Workfare: none Lifetime limit: no limit on receipt of welfare payments. 3-year limit on supplement payments Sanctions: none Education and training: education or training activities were not provided but SSP staff provided advice and signposting to training Health insurance: NA Case management: generally non-intensive with limited contact
Outcomes	*Maternal mental health*: CES-D mean score (0-33) *Child mental health*: Behavior Problems Scale (1-3) *Child physical health*: child average health scale (1-5)

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	Economic employment: currently employed (%); currently employed full-time (%); currently employed part-time (%) *Economic benefit receipt*: average Income Assistance received year 6 (CAD); currently receiving income assistance (%) *Economic income*: total monthly individual income at 72 months (CAD); average earnings year 6 (CAD)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Randomly chosen from lists of IA recipients
Allocation concealment (selection bias)	Low risk	"Immediately after the baseline interview, each of these . . . single parents was randomly assigned to one of the research groups of the SSP study. Each sample member had 50-50 odds of being assigned to the program group or the control group."
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	Significant differences in some baseline characteristics, but regression adjusted estimates did not differ from unadjusted estimates. Unadjusted estimates presented throughout
Blinding of outcome assessment (detection bias) Health outcomes	Unclear risk	Health outcomes collected by face-to-face survey; no information on blinding
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	72% response rate at 72 months; no reasons for missing data provided
Incomplete outcome data (outcome level)	High risk	Authors report that sample size may vary for all outcomes
Direct contamination	High risk	Direct - welfare conditions became increasingly restrictive during the course of the study

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SSP Applicants 2003 *(Continued)*

Indirect contamination	High risk	Attitudes to welfare became increasingly negative during this period
Selective reporting (reporting bias)	Unclear risk	No study protocol available

SSP Recipients 2002

Methods	Randomised controlled trial, follow-up at 36 and 54 months
Participants	**Recipient study sample; single parents 19 years or older who had received Income Assistance payments in the current month and at least 11 of the prior 12 months. Randomly selected from all adult single parents applying for IA in selected areas of British Columbia and New Brunswick between November 1992 and March 1995** **N = 5739** Age 19-24 years (only age group reported) %: total sample/British Columbia/New Brunswick: 21.7/17.3/26.5 Ethnicity - % total sample/British Columbia/New Brunswick: First Nations ancestry 9.7/13.1/6.0; not born in Canada 13.0/22.5/ 2.4 Employment status - 19% employed in total at baseline Family structure - never married % total sample/British Columbia/New Brunswick 48.9/43.7/54.6
Interventions	Voluntary intervention with anti-poverty (AP) ethos and labour force attachment (LFA) approach *Intervention group*: earnings supplement; low case management *Control group*: subject to existing welfare programme *Further details of intervention components*: Mandatory employment: not required to participate in employment in order to receive welfare payments. Required to work at least 30 hours per week to be eligible for supplement payments Earnings supplements: financial supplement paid to parents who worked 30 or more hours per week an amount equal to half the difference between their actual earnings and a 'benchmarked' level of earnings. During the first year of operations, the benchmark was CAD 30,000 in New Brunswick and CAD 37,000 in British Columbia. Had to find a full-time job within 12 months to qualify for supplement payments Earnings disregards: none Childcare subsidy: none Workfare: none Lifetime limit: no limit on receipt of welfare payments. 3-year limit on supplement payments Sanctions: none Education and training: education or training activities were not provided but SSP staff provided advice and signposting to training Health insurance: NA Case management: generally non-intensive with limited contact

Outcomes	*Maternal mental health*: T2 CES-D mean score (0-33); T3 CES-D mean score (0-33) *Child mental health*: T2 Behavior Problems Scale (1-3); T2 adolescent CES-D at risk (%≥8/30); T3 Behavior Problems Scale (1-3) *Child physical health*: T2 child average health scale (1-5); T3 child average health scale (1-5) *Economic employment*: T2 ever employed full-time since randomisation (%); T2 employed FT at 33 months (%); T2 currently employed part-time (%); T3 currently employed (%); T3 currently employed full-time (%); T3 currently employed part-time (%) *Economic benefit receipt*: T2 average Income Assistance year 3 (CAD); T2 Income Assistance receipt year 3 (%); T3 average Income Assistance received year 5 (CAD); T3 cCurrently receiving Income Assistance (%); *Economic income*: T2 total monthly individual income 6 months prior to 3 year survey (CAD); T2 average earnings in year of survey (CAD); T3 total monthly individual income (average in 6 months prior to month 54 (CAD)); T3 monthly earnings year 5, quarter 18 (CAD)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Randomly chosen from lists of IA recipients
Allocation concealment (selection bias)	Low risk	"Immediately after the baseline interview, each of these . . . single parents was randomly assigned to one of the research groups of the SSP study. Each sample member had 50-50 odds of being assigned to the program group or the control group."
Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline

Baseline characteristics	Unclear risk	Baseline characteristics not reported by intervention status; no adjustment reported
Blinding of outcome assessment (detection bias) Health outcomes	Unclear risk	Health outcomes collected by face-to-face survey; no information on blinding
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	80% response rate at 2 years. 72% response rate at 5 years; no reasons for missing data provided
Incomplete outcome data (outcome level)	High risk	Authors report that sample size may vary for all outcomes: "Sample sizes reflect the largest sample of all measures shown. However, sample sizes vary largely across the measures, ranging from 235 to 1,111 in the program group."
Direct contamination	High risk	Direct - welfare conditions became increasingly restrictive during the course of the study
Indirect contamination	High risk	Attitudes to welfare became increasingly negative during this period
Selective reporting (reporting bias)	High risk	Child health outcomes and subgroups reported at T1 and T3 differ

UK ERA 2011

Methods	Randomised controlled trial, follow up at 60 months
Participants	Main study sample N = 16,384. New Deal for Lone Parents Sample (lone parents receiving welfare benefits and attending a Job Centre, randomised between October 2003 and December 2004, from 6 sites in the UK) N = 6787 **Customer survey sample N = 1854** Age - (%:) under 30 years, 41.3; 30-39 years, 39.7; 40 years or older, 19.0 Ethnicity - ethnic minority 14.8; white 85.2 Employment status - number of months worked in 3 years prior to randomisation (%) none, 49.6; 1-12 months, 23.1; ≥ 13 months, 27.3 Family structure - marital status (%) single, 71.6; divorced, 14.7; separated, 11.6; widowed, 1.2; living together, 0.0; married, 0.6
Interventions	Voluntary intervention with anti-poverty (AP) ethos and human capital development (HCD) approach *Intervention group*: earnings supplement; education and training; high case management *Control group*: subject to existing welfare programme *Further details of intervention components*:

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	Mandatory employment: not required to participate in employment in order to receive welfare payments. Required to work at least 30 hours per week in 13 weeks out of 17 week period to be eligible for supplement payments. Unique postemployment 'in work' phase lasting approximately 2 years Earnings supplements: paid an employment retention bonus of GBP 400, 3 times a year for 2 years for staying in full-time work (at least 30 hours per week for 13 out of every 17 weeks) Earnings disregards: none Childcare subsidy: none Workfare: none Lifetime limit: no limit on receipt of welfare payments. 33-month limit on supplement payments Sanctions: none Education and training: provided financial support for training and completion bonuses - assistance for training courses up to GBP 1000 while employed and a bonus up to GBP 1000 for completing training when employed. Helped to identify appropriate education or training courses Health insurance: NA Case management: supportive case management. Flexible with regular, intensive post employment support. Generally small caseloads, however substantial variation across offices
Outcomes	*Maternal mental health*: miserable or depressed often or always (%) *Maternal physical health*: long-standing illness, disability or infirmity (%); in good or very good health (%) *Economic employment*: currently employed (%); ever employed year 5 (%); currently employed full-time (%); currently employed part-time (%) *Economic benefit receipt*: average Income Support received per wk (GBP); currently receiving Income Support or Jobseeker's Allowance (GBP) *Economic income*: average earnings year 5 (GBP)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Computerised algorithm used
Allocation concealment (selection bias)	Low risk	Central allocation; see Appendix 6

Baseline outcome measurements Unclear risk	Unclear risk	No health outcomes collected at baseline
Baseline characteristics	Low risk	Regression used to control for differences in background characteristics
Blinding of outcome assessment (detection bias) Health outcomes	Unclear risk	Health outcomes collected by face-to-face survey; no information on blinding
Incomplete outcome data (attrition bias) All outcomes	High risk	Very high. 62% of randomisation sample responded to 60-month survey (64% of intervention group and 60% of control group). Most disadvantaged more likely to drop out. Administrative data showed that survey data overestimated impact on earnings, although estimate for ever employed in year 5 was not biased. Weighting attempted but not successful; authors state findings should be treated with caution
Incomplete outcome data (outcome level)	Unclear risk	No information on item non-response
Direct contamination	High risk	5-year data were collected between October 2008 and January 2009. During this period, lone parents with a youngest child aged ≥ 12 years (2008) and ≥ 7 years (October 2009) were transferred to Jobseekers' Allowance, which is a conditional out-of-work benefit. They were therefore required to prove that they were actively seeking work. It is not clear what proportion of the sample were affected by these changes
Indirect contamination	Unclear risk	Attitudes to welfare changed during this period
Selective reporting (reporting bias)	Unclear risk	No study protocol available

AFDC: Aid to Families with Dependent Children; **AP**: anti-poverty; **audio-CASI**: audio-enhanced, computer-assisted self-interviewing; **BPI**: Behavior Problems Index; **CES-D**: Center for Epidemiologic Studies Depression Scale; **CIDI**: Composite International Diagnostic Interview; **CR**: caseload reduction; **ESL**: English as a second language; **FIP**: family independence payment; **GED**: general education development; **HCD**: human capital development; **IA**: income assistance; **LFA**: labour force attachment; **MFIP**: Minnesota Family Investment Program; **NA**: not applicable; **NDLP**: New Deal for Lone Parents; **TANF**: Temporary Assistance for Needy Families; **WtW**: welfare to work.

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Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
ABC 1999	No relevant outcomes
Action Emploi 2011	Not a randomised control trial
ARIZONA WORKS 2003	No health outcomes
Bembry 2011	Not a randomised control trial
BIAS 2014	Not welfare to work
BIAS Next Generation 2016	Not welfare to work
Bloom 2016	Not welfare to work
Callahan 1995	Not welfare to work
Cook 2009	Not a randomised control trial
CWEP 1986	No health outcomes
Danziger 2000	Not a randomised control trial
Dockery 2004	No health outcomes
Duncan 2004	Not a randomised control trial
EMPOWER 1999	No health outcomes
ERA 2007	Inappropriate population
Farrell 2013	Inappropriate population
FLORIDA PI 1994	No health outcomes
Fuller 2002	Not a randomised control trial
Grogger 2009	Review
Horton 2002	Not a randomised control trial
HPOG 2014	Not welfare to work
JOBS 1993	Aimed at teenage parents
JOBS 1995	Not a randomised control trial

(Continued)

JOBS 1ST GAIN 1999	No relevant outcomes
Limoncelli 2002	Not a randomised control trial
Maynard 1979	No health outcomes
Meckstroth 2006	Low proportion of lone parents
MFSP 1991	No health outcomes
MICHIGAN FAMILIES 1997	No health outcomes
Michigan Work First 2000	Not a randomised control trial
Morris 2005	Not a primary study
New Jersey FDP 1998	Not a randomised control trial
Opportunity NYC Family Rewards 2013	Not lone parents
Opportunity NYC Work Rewards 2015	No health outcomes
PACE 2014	Not welfare to work
SIME/DIME 1983	Not welfare to work
STED 2015	Not welfare to work
SUPPORTED WORK 1979	No health outcomes
SWIM 1989	No relevant outcomes
TEEN JOBS 1993	Aimed at teenage parents
The SNAP Employment and Training Evaluation 2014	Not lone parents
TPD 1989	Aimed at teenage parents
TWRW 2003	Population unclear
VERMONT WRP 1998	No relevant outcomes
Walker 2005	Not a randomised control trial
Weil 2002	Not a randomised control trial
Zaslow 2002	Review

DATA AND ANALYSES

Comparison 1. Time point 1 Maternal mental health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Maternal mental health continuous	2	3352	Std. Mean Difference (IV, Random, 95% CI)	0.07 [0.00, 0.14]
1.1 CES-D mean score (0-60)	1	590	Std. Mean Difference (IV, Random, 95% CI)	0.0 [-0.16, 0.16]
1.2 CES-D mean score (0-36)	1	2762	Std. Mean Difference (IV, Random, 95% CI)	0.08 [0.01, 0.16]
2 Maternal mental health dichotomous	3		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 Presence of mood disorders (University of Michigan, Composite International Diagnostic Interview) (%)	1	148	Risk Ratio (M-H, Random, 95% CI)	0.99 [0.50, 1.99]
2.2 CES-D at risk (% ≥ 16/ 60)	1	311	Risk Ratio (M-H, Random, 95% CI)	1.18 [0.80, 1.74]
2.3 CIDI at risk (% threshold not reported)	1	308	Risk Ratio (M-H, Random, 95% CI)	1.21 [0.72, 2.06]

Comparison 2. Time point 2 Maternal mental health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Maternal mental health continuous	3	7091	Std. Mean Difference (IV, Random, 95% CI)	0.00 [-0.05, 0.05]
1.1 CES-D mean score (0-60)	2	2576	Std. Mean Difference (IV, Random, 95% CI)	0.02 [-0.06, 0.09]
1.2 CES-D mean score (0-33)	1	4515	Std. Mean Difference (IV, Random, 95% CI)	-0.01 [-0.07, 0.05]
2 CJF GUP CES-D mean score (0-60)			Other data	No numeric data
3 Maternal mental health dichotomous	2		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
3.1 Unhappy, sad or depressed very often or fairly often (%)	1	2242	Risk Ratio (M-H, Random, 95% CI)	1.06 [0.95, 1.18]
3.2 CES-D at high risk (% ≥ 23/60)	1	1900	Risk Ratio (M-H, Random, 95% CI)	1.00 [0.85, 1.18]

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Comparison 3. Time point 3 Maternal mental health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Maternal mental health continuous	4	8904	Std. Mean Difference (IV, Random, 95% CI)	-0.07 [-0.15, 0.00]
1.1 CES-D mean score (0-60)	2	2232	Std. Mean Difference (IV, Random, 95% CI)	-0.10 [-0.18, -0.01]
1.2 CES-D mean score (0-33)	2	6672	Std. Mean Difference (IV, Random, 95% CI)	-0.06 [-0.18, 0.06]
2 Maternal mental health dichotomous	2		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 CES-D at high risk (% ≥ 23/60)	1	1475	Risk Ratio (M-H, Random, 95% CI)	0.94 [0.73, 1.20]
2.2 Miserable or depressed often or always (%)	1	1365	Risk Ratio (M-H, Random, 95% CI)	1.25 [0.98, 1.59]

Comparison 4. Time point 1 Maternal physical health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 In poor health (%)	1	311	Risk Ratio (M-H, Random, 95% CI)	0.85 [0.54, 1.36]

Comparison 5. Time point 2 Maternal physical health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 In good or excellent health (%)	2	2551	Risk Ratio (M-H, Random, 95% CI)	1.06 [0.95, 1.18]

Comparison 6. Time point 3 Maternal physical health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Self-reported health (1-5)	1	553	Std. Mean Difference (IV, Random, 95% CI)	0.16 [-0.01, 0.33]
2 In good or very good health (%)	1	1854	Risk Ratio (M-H, Random, 95% CI)	0.97 [0.91, 1.04]

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Comparison 7. Time point 1 Child mental health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Child behaviour problems continuous	2		Std. Mean Difference (IV, Random, 95% CI)	Subtotals only
1.1 Problem Behavior Scale (1-5)	1	563	Std. Mean Difference (IV, Random, 95% CI)	-0.17 [-0.34, -0.01]
1.2 Behavior Problems Index (0-2)	1	2762	Std. Mean Difference (IV, Random, 95% CI)	0.01 [-0.06, 0.09]
2 Child behaviour problems dichotomous	2		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 One or more behavior disorders (Survey Diagnostic Instrument) (%)	1	178	Risk Ratio (M-H, Random, 95% CI)	1.58 [0.48, 5.24]
2.2 Behavior Problems Index (% with problems)	1	311	Risk Ratio (M-H, Random, 95% CI)	1.58 [0.92, 2.72]

Comparison 8. Time point 2 Child mental health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Child behaviour problems continuous	5	7560	Std. Mean Difference (IV, Random, 95% CI)	-0.04 [-0.08, 0.01]
1.1 Behavioral Problems Index (0-56)	3	4107	Std. Mean Difference (IV, Random, 95% CI)	-0.05 [-0.12, 0.01]
1.2 Behavior Problems Scale (1-3)	1	3201	Std. Mean Difference (IV, Random, 95% CI)	-0.03 [-0.10, 0.04]
1.3 Survey Diagnostic Instrument Conduct Disorder (0-30)	1	252	Std. Mean Difference (IV, Random, 95% CI)	0.06 [-0.21, 0.32]
2 Adolescent mental health dichotomous	1		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 Adolescent CES-D at risk (% $\geq$ 8/30)	1	1417	Risk Ratio (M-H, Random, 95% CI)	0.97 [0.87, 1.08]
3 Child Behavior Checklist (1-3)			Other data	No numeric data

Comparison 9. Time point 3 Child mental health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Child behaviour problems continuous	3	3643	Std. Mean Difference (IV, Random, 95% CI)	-0.05 [-0.16, 0.05]
1.1 Behavior Problems Scale (1-3)	1	1134	Std. Mean Difference (IV, Random, 95% CI)	0.04 [-0.07, 0.16]
1.2 Behavioral Problems Index (0-56)	1	1679	Std. Mean Difference (IV, Random, 95% CI)	-0.07 [-0.16, 0.03]
1.3 Problem Behavior Scale (1-5)	1	830	Std. Mean Difference (IV, Random, 95% CI)	-0.15 [-0.29, -0.02]
2 Child behaviour problem continuous excluding SSP Applicants	2	2509	Std. Mean Difference (IV, Random, 95% CI)	-0.10 [-0.18, -0.01]
2.1 Behavioral Problems Index (0-56)	1	1679	Std. Mean Difference (IV, Random, 95% CI)	-0.07 [-0.16, 0.03]
2.2 Problem behavior scale (1-5)	1	830	Std. Mean Difference (IV, Random, 95% CI)	-0.15 [-0.29, -0.02]
3 NEWWS 2001 Child mental health	1		Std. Mean Difference (IV, Random, 95% CI)	Subtotals only
3.1 BPI Externalising subscore (0-18)	1	2124	Std. Mean Difference (IV, Random, 95% CI)	-0.12 [-0.21, -0.03]
3.2 BPI Internalising subscore (0-24)	1	2124	Std. Mean Difference (IV, Random, 95% CI)	-0.04 [-0.13, 0.04]
3.3 BPI Hyperactivity subscore (0-18)	1	2124	Std. Mean Difference (IV, Random, 95% CI)	0.03 [-0.06, 0.12]
4 IFIP Behavioral Problems Index (0-56)			Other data	No numeric data
5 SSP-R T3 Behavior Problems Scale (1-3)			Other data	No numeric data

Comparison 10. Time point 1 Child physical health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 General health rating (1-5)	1	2762	Std. Mean Difference (IV, Random, 95% CI)	-0.05 [-0.12, 0.03]

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Comparison 11. Time point 2 Child physical health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Child physical health continuous	3	7195	Std. Mean Difference (IV, Random, 95% CI)	0.07 [0.01, 0.12]
1.1 General health scale (1-5)	2	2577	Std. Mean Difference (IV, Random, 95% CI)	0.11 [0.03, 0.19]
1.2 Child average health scale (1-5 across 4-item instrument)	1	4618	Std. Mean Difference (IV, Random, 95% CI)	0.03 [-0.03, 0.09]
2 Child physical health dichotomous	1		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 In good or excellent health	1	1900	Risk Ratio (M-H, Random, 95% CI)	0.98 [0.93, 1.02]

Comparison 12. Time point 3 Child physical health

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Child physical health continuous	5	8083	Std. Mean Difference (IV, Random, 95% CI)	0.01 [-0.04, 0.06]
1.1 Health status scale (1-5)	1	1679	Std. Mean Difference (IV, Random, 95% CI)	0.0 [-0.10, 0.10]
1.2 Overall health scale (1-5)	1	850	Std. Mean Difference (IV, Random, 95% CI)	0.09 [-0.04, 0.23]
1.3 General health rating (1-5)	1	2124	Std. Mean Difference (IV, Random, 95% CI)	-0.07 [-0.15, 0.02]
1.4 Child average health scale (1-5 across 4-item instrument)	2	3430	Std. Mean Difference (IV, Random, 95% CI)	0.04 [-0.03, 0.10]
2 Child physical health dichotomous	1		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 In fair or poor health (%)	1	1475	Risk Ratio (M-H, Random, 95% CI)	1.26 [0.73, 2.14]

Comparison 13. Time point 1 Employment status

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Currently employed (%)	3	3381	Risk Ratio (M-H, Random, 95% CI)	1.22 [1.12, 1.32]
2 Ever employed (%)	3	3818	Risk Ratio (M-H, Random, 95% CI)	1.14 [1.07, 1.21]
2.1 Ever employed since randomisation (18 months) (%)	1	311	Risk Ratio (M-H, Random, 95% CI)	1.17 [1.03, 1.34]
2.2 Ever employed year 2 (%)	1	745	Risk Ratio (M-H, Random, 95% CI)	1.09 [1.03, 1.15]
2.3 Ever employed since randomisation (24 months) (%)	1	2762	Risk Ratio (M-H, Random, 95% CI)	1.17 [1.11, 1.24]

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Comparison 14. Time point 2 Employment status

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Ever employed (%)	5	12274	Risk Ratio (M-H, Random, 95% CI)	1.13 [1.08, 1.19]
1.1 Ever employed since randomisation (36 months)	2	4845	Risk Ratio (M-H, Random, 95% CI)	1.14 [1.03, 1.26]
1.2 Average employment year of study	2	2577	Risk Ratio (M-H, Random, 95% CI)	1.10 [1.03, 1.17]
1.3 Employed at 33 months	1	4852	Risk Ratio (M-H, Random, 95% CI)	1.19 [1.10, 1.28]
2 Ever employed full-time since randomisation (%)	3	9806	Risk Ratio (M-H, Random, 95% CI)	1.20 [1.05, 1.37]
3 Ever employed full-time excluding MFIP (%)	2	8275	Risk Ratio (M-H, Random, 95% CI)	1.29 [1.18, 1.40]
4 Employed part-time (%)	3		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
4.1 Ever employed part-time since randomisation	2	4845	Risk Ratio (M-H, Random, 95% CI)	1.14 [1.04, 1.25]
4.2 Currently employed part-time	1	4852	Risk Ratio (M-H, Random, 95% CI)	0.80 [0.69, 0.93]

Comparison 15. Time point 3 Employment status

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Currently employed (%)	6	14355	Risk Ratio (M-H, Random, 95% CI)	1.03 [0.99, 1.07]
2 Ever employed (%)	3		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 Ever employed year 5	2	2599	Risk Ratio (M-H, Random, 95% CI)	1.01 [0.96, 1.06]
2.2 Ever employed years 1-5	1	2124	Risk Ratio (M-H, Random, 95% CI)	1.12 [1.08, 1.17]
3 Currently employed full-time (%)	6	13233	Risk Ratio (M-H, Random, 95% CI)	1.05 [1.00, 1.12]
4 Currently employed part-time (%)	5	12676	Risk Ratio (M-H, Random, 95% CI)	0.93 [0.85, 1.01]

Comparison 16. Time point 1 Income

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total income	2		Std. Mean Difference (IV, Random, 95% CI)	Subtotals only
1.1 Total income year 2 (USD)	1	744	Std. Mean Difference (IV, Random, 95% CI)	0.11 [-0.04, 0.25]
1.2 Total net household income in prior month (USD)	1	2762	Std. Mean Difference (IV, Random, 95% CI)	-0.08 [-0.15, -0.00]
2 Earnings	1		Std. Mean Difference (IV, Random, 95% CI)	Subtotals only

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2.1 Average annual earnings year 2 (USD)	1	744	Std. Mean Difference (IV, Random, 95% CI)	0.07 [-0.08, 0.21]
3 NEWWS T1 Average earnings previous month (USD)			Other data	No numeric data

Comparison 17. Time point 2 Income

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total income	4	8934	Std. Mean Difference (IV, Random, 95% CI)	0.10 [0.02, 0.17]
1.1 Average annual income (benefits, earnings and food stamps) years 3-4 (USD)	1	1469	Std. Mean Difference (IV, Random, 95% CI)	-0.01 [-0.11, 0.09]
1.2 Average total income (benefits, earnings and food stamps) year 4 (USD)	1	1108	Std. Mean Difference (IV, Random, 95% CI)	0.08 [-0.04, 0.20]
1.3 Average annual income (benefits and earnings) year 3 (USD)	1	1531	Std. Mean Difference (IV, Random, 95% CI)	0.14 [0.03, 0.24]
1.4 Total monthly individual income (average from all sources in 6 months prior to 3-year survey) (CAD)	1	4826	Std. Mean Difference (IV, Random, 95% CI)	0.15 [0.10, 0.21]
2 Total income excluding CJF	3	7465	Std. Mean Difference (IV, Random, 95% CI)	0.14 [0.09, 0.18]
2.1 Average total income from earnings, AFDC/TANF and food stamps year 4 (USD)	1	1108	Std. Mean Difference (IV, Random, 95% CI)	0.08 [-0.04, 0.20]
2.2 Average annual income welfare/earnings year 3 (USD)	1	1531	Std. Mean Difference (IV, Random, 95% CI)	0.14 [0.03, 0.24]
2.3 Total monthly individual income 6 months prior to 3-year survey (CAD)	1	4826	Std. Mean Difference (IV, Random, 95% CI)	0.15 [0.10, 0.21]
3 Average earnings in year of survey (USD)	2	6321	Std. Mean Difference (IV, Random, 95% CI)	0.09 [0.04, 0.13]
4 MFIP Average annual earnings years 1-3 (USD)			Other data	No numeric data
5 GAIN Average weekly earnings since randomisation (USD)			Other data	No numeric data
6 FTP Average earnings in year of study (USD)			Other data	No numeric data

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Comparison 18. Time point 3 Income

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total income	5	11745	Std. Mean Difference (IV, Random, 95% CI)	0.01 [-0.04, 0.06]
1.1 Total household income month prior to survey, annualised (USD)	1	1679	Std. Mean Difference (IV, Random, 95% CI)	-0.03 [-0.13, 0.06]
1.2 Total income year 5 (USD)	1	745	Std. Mean Difference (IV, Random, 95% CI)	0.06 [-0.09, 0.20]
1.3 Total income years 1-5 (USD)	1	2124	Std. Mean Difference (IV, Random, 95% CI)	0.05 [-0.04, 0.14]
1.4 Total monthly individual income at 72 months (CAD)	1	2371	Std. Mean Difference (IV, Random, 95% CI)	0.07 [-0.01, 0.15]
1.5 Total monthly individual income (average in 6 months prior to month 54 (CAD)	1	4826	Std. Mean Difference (IV, Random, 95% CI)	-0.04 [-0.09, 0.02]
2 IFIP household income month prior to survey (USD)			Other data	No numeric data
3 Total earnings	5	11501	Std. Mean Difference (IV, Random, 95% CI)	0.04 [-0.00, 0.07]
3.1 Average earnings year 5 (USD)	1	745	Std. Mean Difference (IV, Random, 95% CI)	0.05 [-0.09, 0.20]
3.2 Average earnings year 6 (CAD)	1	2371	Std. Mean Difference (IV, Random, 95% CI)	0.08 [0.00, 0.16]
3.3 Average earnings year 5 (GBP)	1	1854	Std. Mean Difference (IV, Random, 95% CI)	0.06 [-0.04, 0.15]
3.4 Earnings month prior to survey, annualised (USD)	1	1679	Std. Mean Difference (IV, Random, 95% CI)	0.01 [-0.09, 0.11]
3.5 Monthly earnings year 5, quarter 18 (CAD)	1	4852	Std. Mean Difference (IV, Random, 95% CI)	0.01 [-0.05, 0.07]
4 IFIP Average earnings month prior to survey (USD)			Other data	No numeric data
5 NEWWS T3 Average earnings years 1-5 (USD)			Other data	No numeric data

Comparison 19. Time point 1 Welfare receipt

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total AFDC received year 2 (USD)	1	744	Std. Mean Difference (IV, Random, 95% CI)	-0.10 [-0.24, 0.04]
2 Proportion of sample receiving welfare (%)	3	3714	Risk Ratio (M-H, Random, 95% CI)	0.88 [0.84, 0.92]
2.1 Received social assistance in last 12 months	1	207	Risk Ratio (M-H, Random, 95% CI)	0.85 [0.76, 0.97]
2.2 Currently receiving AFDC	1	2762	Risk Ratio (M-H, Random, 95% CI)	0.87 [0.83, 0.92]

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2.3 Ever received AFDC/ TANF year 2	1	745	Risk Ratio (M-H, Random, 95% CI)	0.94 [0.84, 1.06]

Comparison 20. Time point 2 Welfare receipt

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Average annual welfare benefit	4	8960	Std. Mean Difference (IV, Random, 95% CI)	-0.11 [-0.36, 0.15]
1.1 Average annual welfare benefit year 3 (USD)	1	1469	Std. Mean Difference (IV, Random, 95% CI)	-0.22 [-0.32, -0.11]
1.2 Average Income Assistance year 3 (CAD)	1	4852	Std. Mean Difference (IV, Random, 95% CI)	-0.18 [-0.24, -0.13]
1.3 Average annual welfare benefit year 3 (USD)	1	1531	Std. Mean Difference (IV, Random, 95% CI)	0.33 [0.22, 0.43]
1.4 Total AFDC/TANF received year 4 (USD)	1	1108	Std. Mean Difference (IV, Random, 95% CI)	-0.35 [-0.47, -0.23]
2 Average annual welfare benefit excluding MFIP	3	7429	Std. Mean Difference (IV, Random, 95% CI)	-0.24 [-0.33, -0.15]
2.1 Average annual welfare benefit year 3 (USD)	1	1469	Std. Mean Difference (IV, Random, 95% CI)	-0.22 [-0.32, -0.11]
2.2 Total AFDC/TANF received year 4 (USD)	1	1108	Std. Mean Difference (IV, Random, 95% CI)	-0.35 [-0.47, -0.23]
2.3 Average Income Assistance year 3 (USD/CAD)	1	4852	Std. Mean Difference (IV, Random, 95% CI)	-0.18 [-0.24, -0.13]
3 Proportion of sample receiving welfare	2	5210	Risk Ratio (M-H, Random, 95% CI)	0.87 [0.83, 0.91]
3.1 Social assistance/ unemployment insurance receipt year 4 (%)	1	358	Risk Ratio (M-H, Random, 95% CI)	0.90 [0.71, 1.14]
3.2 Income Assistance receipt year 3 (%)	1	4852	Risk Ratio (M-H, Random, 95% CI)	0.87 [0.83, 0.90]

Comparison 21. Time point 3 Welfare receipt

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total welfare benefit received	4	9822	Std. Mean Difference (IV, Random, 95% CI)	-0.06 [-0.11, -0.00]
1.1 Total AFDC/TANF receipt year 5 (USD)	1	745	Std. Mean Difference (IV, Random, 95% CI)	0.01 [-0.14, 0.15]
1.2 Average Income Assistance received year 6 (CAD)	1	2371	Std. Mean Difference (IV, Random, 95% CI)	-0.12 [-0.20, -0.04]
1.3 Average Income Support received per wk (GBP)	1	1854	Std. Mean Difference (IV, Random, 95% CI)	0.0 [-0.09, 0.09]

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1.4 Average Income Assistance received year 5 (CAD)	1	4852	Std. Mean Difference (IV, Random, 95% CI)	-0.07 [-0.12, -0.01]
2 Total welfare payments years 1-5 (USD)	1	2124	Std. Mean Difference (IV, Random, 95% CI)	-0.55 [-0.63, -0.46]
3 IWRE TANF receipt month before survey, annualised year 5 (USD)			Other data	No numeric data
4 IFIP Average welfare received month prior to survey (USD)			Other data	No numeric data
5 Proportion of sample receiving welfare	6	12976	Risk Ratio (M-H, Random, 95% CI)	0.92 [0.86, 0.99]
5.1 Currently receiving TANF (%)	1	1679	Risk Ratio (M-H, Random, 95% CI)	0.79 [0.67, 0.93]
5.2 Ever received AFDC/ TANF year 5 (%)	1	745	Risk Ratio (M-H, Random, 95% CI)	0.91 [0.64, 1.29]
5.3 Currently receiving Income Support or Jobseeker's Allowance (%)	1	1854	Risk Ratio (M-H, Random, 95% CI)	0.98 [0.87, 1.11]
5.4 Currently receiving Family Independence Payment (%)	1	1475	Risk Ratio (M-H, Random, 95% CI)	1.09 [0.89, 1.33]
5.5 Currently receiving Income Assistance (%)	1	2371	Risk Ratio (M-H, Random, 95% CI)	0.84 [0.72, 0.98]
5.6 Currently receiving Income Assistance (%)	1	4852	Risk Ratio (M-H, Random, 95% CI)	0.94 [0.89, 0.99]

Comparison 22. Time point 1 Health insurance

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Respondent has health insurance (%)	4		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
1.1 Respondent has Medicaid	2	606	Risk Ratio (M-H, Random, 95% CI)	1.16 [1.08, 1.25]
1.2 Respondent had any health insurance since randomisation (24 months)	1	590	Risk Ratio (M-H, Random, 95% CI)	1.09 [1.03, 1.16]
1.3 Respondent ever had employer-provided health insurance since randomisation (24 months)	1	2762	Risk Ratio (M-H, Random, 95% CI)	1.40 [1.16, 1.69]
2 Child health insurance (%)	1	2762	Risk Ratio (M-H, Random, 95% CI)	0.99 [0.96, 1.01]

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Comparison 23. Time point 2 Health insurance

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Health insurance (%)	2		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
1.1 Respondent has Medicaid or other health insurance within 2-3 years of randomisation	1	2193	Risk Ratio (M-H, Random, 95% CI)	0.97 [0.93, 1.01]
1.2 Children have continuous health insurance for past 36 months	1	1531	Risk Ratio (M-H, Random, 95% CI)	1.16 [1.08, 1.24]

Comparison 24. Time point 3 Health insurance

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Health insurance	3		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
1.1 Family has health insurance (%)	2	3599	Risk Ratio (M-H, Random, 95% CI)	0.98 [0.92, 1.05]
1.2 Respondent has health insurance (%)	1	561	Risk Ratio (M-H, Random, 95% CI)	0.97 [0.91, 1.04]
1.3 All focal children have health insurance (%)	1	561	Risk Ratio (M-H, Random, 95% CI)	0.95 [0.89, 1.02]

Comparison 25. New Hope 96 months

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Maternal and child health outcomes			Other data	No numeric data

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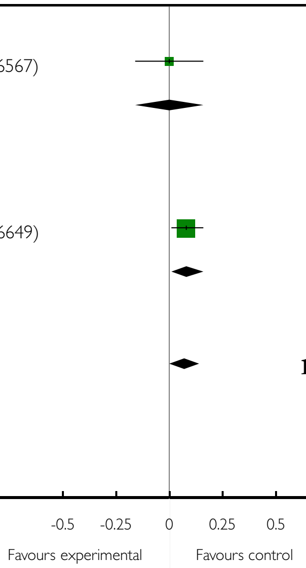
Analysis 1.1. Comparison 1 Time point 1 Maternal mental health, Outcome 1 Maternal mental health continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 1 Time point 1 Maternal mental health

Outcome: 1 Maternal mental health continuous

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 CES-D mean score (0-60)							
New Hope 1999	289	16.9 (11.6567)	301	16.9 (11.6567)		17.8 %	0.0 [-0.16, 0.16]
Subtotal (95% CI)	**289**		**301**			**17.8 %**	**0.0 [-0.16, 0.16]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.0 (P = 1.0)							
2 CES-D mean score (0-36)							
NEWWS 2001 (1)	1554	8.5503 (7.6181)	1208	7.92 (7.6649)		82.2 %	0.08 [0.01, 0.16]
Subtotal (95% CI)	**1554**		**1208**			**82.2 %**	**0.08 [0.01, 0.16]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.16 (P = 0.031)							
Total (95% CI)	**1843**		**1509**			**100.0 %**	**0.07 [0.00, 0.14]**
Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.83$, df = 1 (P = 0.36); $I^2 = 0.0\%$							
Test for overall effect: Z = 1.96 (P = 0.050)							
Test for subgroup differences: $Chi^2 = 0.83$, df = 1 (P = 0.36), $I^2 = 0.0\%$							



(1) At risk threshold = 10. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

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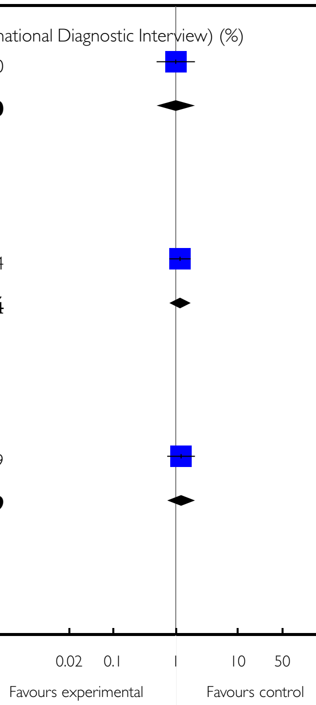
Analysis 1.2. Comparison 1 Time point 1 Maternal mental health, Outcome 2 Maternal mental health dichotomous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 1 Time point 1 Maternal mental health

Outcome: 2 Maternal mental health dichotomous

Study or subgroup	Favours experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 Presence of mood disorders (University of Michigan, Composite International Diagnostic Interview) (%)					
Ontario 2001 (1)	16/88	11/60		100.0 %	0.99 [0.50, 1.99]
Subtotal (95% CI)	**88**	**60**		**100.0 %**	**0.99 [0.50, 1.99]**
Total events: 16 (Favours experimental), 11 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.02 (P = 0.98)					
2 CES-D at risk (% ≥ 16/60)					
CJF Yale 2001	42/157	35/154		100.0 %	1.18 [0.80, 1.74]
Subtotal (95% CI)	**157**	**154**		**100.0 %**	**1.18 [0.80, 1.74]**
Total events: 42 (Favours experimental), 35 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.82 (P = 0.41)					
3 CIDI at risk (% threshold not reported)					
CJF GUP 2000	25/149	22/159		100.0 %	1.21 [0.72, 2.06]
Subtotal (95% CI)	**149**	**159**		**100.0 %**	**1.21 [0.72, 2.06]**
Total events: 25 (Favours experimental), 22 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.72 (P = 0.47)					



(1) Values for 2 intervention groups in Ontario combined.

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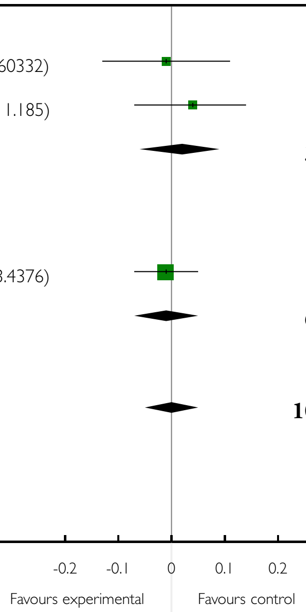
Analysis 2.1. Comparison 2 Time point 2 Maternal mental health, Outcome 1 Maternal mental health continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 2 Time point 2 Maternal mental health

Outcome: 1 Maternal mental health continuous

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 CES-D mean score (0-60)							
FTP 2000	543	14 (11.1660332)	565	14.1 (11.1660332)		15.6 %	-0.01 [-0.13, 0.11]
CJF 2002	748	13.8 (11.1850336)	720	13.4 (11.185)		20.7 %	0.04 [-0.07, 0.14]
Subtotal (95% CI)	**1291**		**1285**			**36.3 %**	**0.02 [-0.06, 0.09]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.32, df = 1 (P = 0.57); I^2 =0.0%							
Test for overall effect: Z = 0.42 (P = 0.68)							
2 CES-D mean score (0-33)							
SSP Recipients 2002	2287	7.9 (13.4376)	2228	8 (13.4376)		63.7 %	-0.01 [-0.07, 0.05]
Subtotal (95% CI)	**2287**		**2228**			**63.7 %**	**-0.01 [-0.07, 0.05]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.25 (P = 0.80)							
Total (95% CI)	**3578**		**3513**			**100.0 %**	**0.00 [-0.05, 0.05]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.55, df = 2 (P = 0.76); I^2 =0.0%							
Test for overall effect: Z = 0.05 (P = 0.96)							
Test for subgroup differences: Chi^2 = 0.24, df = 1 (P = 0.63), I^2 =0.0%							



Analysis 2.2. Comparison 2 Time point 2 Maternal mental health, Outcome 2 CJF GUP CES-D mean score (0-60).

CJF GUP CES-D mean score (0-60)

Study	Intervention	Int n	Control	Cont n	Total n	Sig
CJF GUP 2000	15.5	Not reported	13.9	Not reported	187	< 0.10

Analysis 2.3. Comparison 2 Time point 2 Maternal mental health, Outcome 3 Maternal mental health dichotomous.

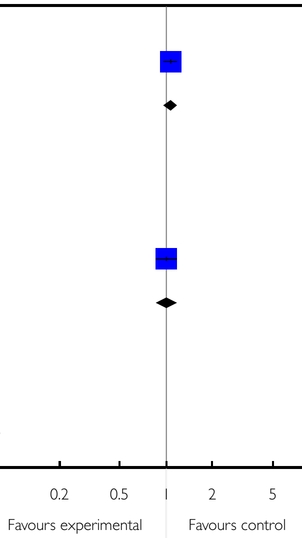
Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 2 Time point 2 Maternal mental health

Outcome: 3 Maternal mental health dichotomous

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 Unhappy, sad or depressed very often or fairly often (%)					
California GAIN 1994	490/1302	335/940		100.0 %	1.06 [0.95, 1.18]
Subtotal (95% CI)	**1302**	**940**		**100.0 %**	**1.06 [0.95, 1.18]**
Total events: 490 (Experimental), 335 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.96 (P = 0.33)					
2 CES-D at high risk (% $\geq$ 23/60)					
MFIP 2000 (1)	292/1180	178/720		100.0 %	1.00 [0.85, 1.18]
Subtotal (95% CI)	**1180**	**720**		**100.0 %**	**1.00 [0.85, 1.18]**
Total events: 292 (Experimental), 178 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.01 (P = 0.99)					

Test for subgroup differences: $\text{Chi}^2 = 0.29$, df = 1 (P = 0.59), I^2 =0.0%



(1) Rural and urban long term and recent applicants from MFIP and MFIP-IO groups combined.

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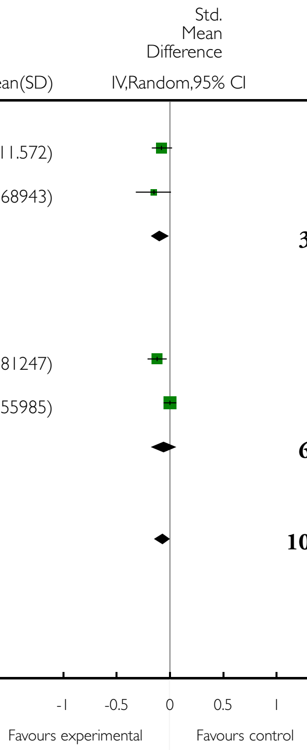
Analysis 3.1. Comparison 3 Time point 3 Maternal mental health, Outcome 1 Maternal mental health continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 3 Time point 3 Maternal mental health

Outcome: 1 Maternal mental health continuous

Study or subgroup	Favours experimental N	Mean(SD)	Control N	Mean(SD)	Weight	Std. Mean Difference IV,Random,95% CI
1 CES-D mean score (0-60)						
IWRE 2002	819	14.2 (11.572)	860	15.1 (11.572)	25.2 %	-0.08 [-0.17, 0.02]
New Hope 1999	277	14.3 (10.4168943)	276	15.9 (10.4168943)	13.3 %	-0.15 [-0.32, 0.01]
Subtotal (95% CI)	**1096**		**1136**		**38.5 %**	**-0.10 [-0.18, -0.01]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.59, df = 1 (P = 0.44); I^2 =0.0%						
Test for overall effect: Z = 2.28 (P = 0.023)						
2 CES-D mean score (0-33)						
SSP Applicants 2003	1011	7.3 (6.481247)	867	8.1 (6.481247)	26.3 %	-0.12 [-0.21, -0.03]
SSP Recipients 2002	2433	8.3 (6.9955985)	2361	8.3 (6.9955985)	35.1 %	0.0 [-0.06, 0.06]
Subtotal (95% CI)	**3444**		**3228**		**61.5 %**	**-0.06 [-0.18, 0.06]**
Heterogeneity: Tau^2 = 0.01; Chi^2 = 5.11, df = 1 (P = 0.02); I^2 =80%						
Test for overall effect: Z = 0.92 (P = 0.36)						
Total (95% CI)	**4540**		**4364**		**100.0 %**	**-0.07 [-0.15, 0.00]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 7.30, df = 3 (P = 0.06); I^2 =59%						
Test for overall effect: Z = 1.95 (P = 0.051)						
Test for subgroup differences: Chi^2 = 0.29, df = 1 (P = 0.59), I^2 =0.0%						



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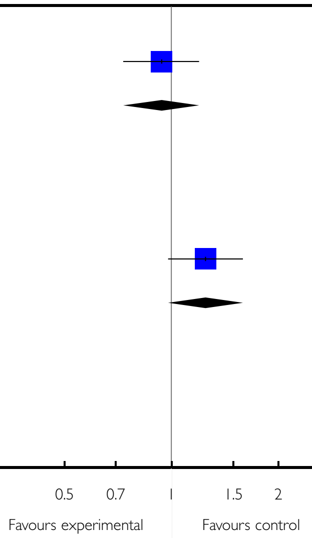
Analysis 3.2. Comparison 3 Time point 3 Maternal mental health, Outcome 2 Maternal mental health dichotomous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 3 Time point 3 Maternal mental health

Outcome: 2 Maternal mental health dichotomous

Study or subgroup	Favours experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 CES-D at high risk (% ≥ 23/60)					
IFIP 2002 (1)	149/982	80/493		100.0 %	0.94 [0.73, 1.20]
Subtotal (95% CI)	**982**	**493**		**100.0 %**	**0.94 [0.73, 1.20]**
Total events: 149 (Favours experimental), 80 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.53 (P = 0.60)					
2 Miserable or depressed often or always (%)					
UK ERA 2011 (2)	128/713	94/652		100.0 %	1.25 [0.98, 1.59]
Subtotal (95% CI)	**713**	**652**		**100.0 %**	**1.25 [0.98, 1.59]**
Total events: 128 (Favours experimental), 94 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.76 (P = 0.078)					
Test for subgroup differences: Chi^2 = 2.59, df = 1 (P = 0.11), I^2 =61%					



(1) Applicants and recipients groups combined.

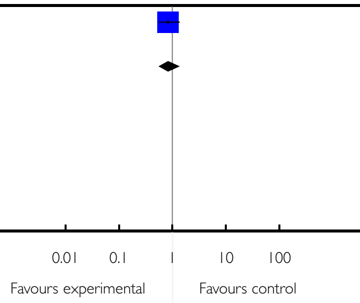
(2) Respondents with at least one child under 16 yrs. Percentage who report feeling miserable or depressed always or often.

Analysis 4.1. Comparison 4 Time point 1 Maternal physical health, Outcome 1 In poor health (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 4 Time point 1 Maternal physical health
Outcome: 1 In poor health (%)

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
CJF Yale 2001	27/157	31/154		100.0 %	0.85 [0.54, 1.36]
Total (95% CI)	**157**	**154**		**100.0 %**	**0.85 [0.54, 1.36]**

Total events: 27 (Experimental), 31 (Control)
Heterogeneity: not applicable
Test for overall effect: Z = 0.66 (P = 0.51)
Test for subgroup differences: Not applicable

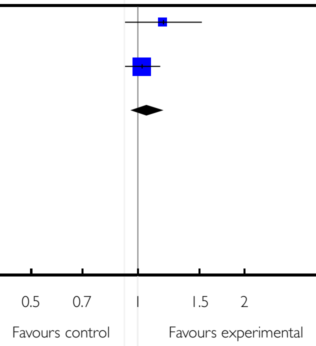


Analysis 5.1. Comparison 5 Time point 2 Maternal physical health, Outcome 1 In good or excellent health (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 5 Time point 2 Maternal physical health
Outcome: 1 In good or excellent health (%)

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
Ontario 2001 (1)	118/242	48/116		17.9 %	1.18 [0.92, 1.52]
California GAIN 1994	445/1276	310/917		82.1 %	1.03 [0.92, 1.16]
Total (95% CI)	**1518**	**1033**		**100.0 %**	**1.06 [0.95, 1.18]**

Total events: 563 (Experimental), 358 (Control)
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.88, df = 1 (P = 0.35); I^2 =0.0%
Test for overall effect: Z = 1.01 (P = 0.31)
Test for subgroup differences: Not applicable



(1) Values for In excellent health and In good health summed.

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Analysis 6.1. Comparison 6 Time point 3 Maternal physical health, Outcome 1 Self-reported health (1-5).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 6 Time point 3 Maternal physical health

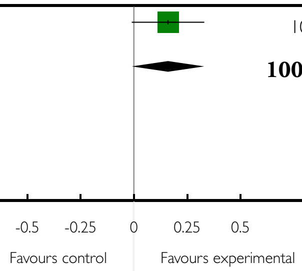
Outcome: 1 Self-reported health (1-5)

Study or subgroup	Experimental N	Experimental Mean(SD)	Control N	Control Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
New Hope 1999	277	3.5 (0.6189515)	276	3.4 (0.6189515)		100.0 %	0.16 [-0.01, 0.33]
Total (95% CI)	**277**		**276**			**100.0 %**	**0.16 [-0.01, 0.33]**

Heterogeneity: not applicable

Test for overall effect: Z = 1.89 (P = 0.058)

Test for subgroup differences: Not applicable



Analysis 6.2. Comparison 6 Time point 3 Maternal physical health, Outcome 2 In good or very good health (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 6 Time point 3 Maternal physical health

Outcome: 2 In good or very good health (%)

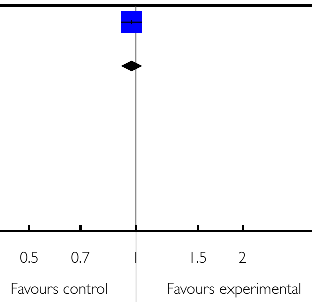
Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
UK ERA 2011	614/951	600/903		100.0 %	0.97 [0.91, 1.04]
Total (95% CI)	**951**	**903**		**100.0 %**	**0.97 [0.91, 1.04]**

Total events: 614 (Experimental), 600 (Control)

Heterogeneity: not applicable

Test for overall effect: Z = 0.85 (P = 0.39)

Test for subgroup differences: Not applicable



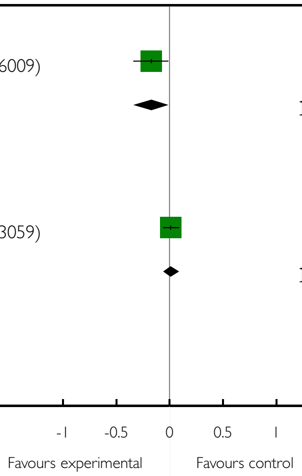
Analysis 7.1. Comparison 7 Time point 1 Child mental health, Outcome 1 Child behaviour problems continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 7 Time point 1 Child mental health

Outcome: 1 Child behaviour problems continuous

Study or subgroup	Experimental		Control		Std. Mean Difference	Weight	Std. Mean Difference
	N	Mean(SD)	N	Mean(SD)	IV,Random,95% CI		IV,Random,95% CI
1 Problem Behavior Scale (1-5)							
New Hope 1999 (1)	278	2.3399 (0.6058)	285	2.44 (0.6009)		100.0 %	-0.17 [-0.34, -0.01]
Subtotal (95% CI)	**278**		**285**			**100.0 %**	**-0.17 [-0.34, -0.01]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.04 (P = 0.041)							
2 Behavior Problems Index (0-2)							
NEWWS 2001 (2)	1554	0.4252 (0.3143)	1208	0.42 (0.3059)		100.0 %	0.01 [-0.06, 0.09]
Subtotal (95% CI)	**1554**		**1208**			**100.0 %**	**0.01 [-0.06, 0.09]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.33 (P = 0.74)							
Test for subgroup differences: Chi^2 = 3.99, df = 1 (P = 0.05), I^2 =75%							



(1) Impacts for children aged 3-5 and 6-12 were combined

(2) Children aged 5-7. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

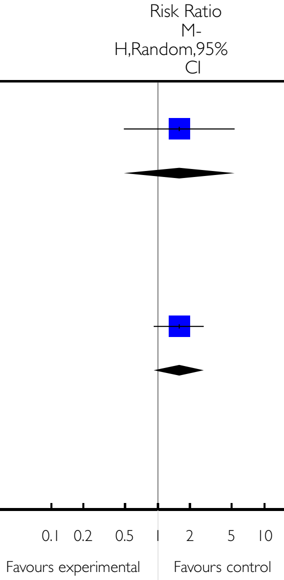
Analysis 7.2. Comparison 7 Time point 1 Child mental health, Outcome 2 Child behaviour problems dichotomous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 7 Time point 1 Child mental health

Outcome: 2 Child behaviour problems dichotomous

Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 One or more behavior disorders (Survey Diagnostic Instrument) (%)				
Ontario 2001 (1)	14/133	3/45	100.0 %	1.58 [0.48, 5.24]
Subtotal (95% CI)	**133**	**45**	**100.0 %**	**1.58 [0.48, 5.24]**
Total events: 14 (Experimental), 3 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 0.75 (P = 0.46)				
2 Behavior Problems Index (% with problems)				
CJF Yale 2001 (2)	29/157	18/154	100.0 %	1.58 [0.92, 2.72]
Subtotal (95% CI)	**157**	**154**	**100.0 %**	**1.58 [0.92, 2.72]**
Total events: 29 (Experimental), 18 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 1.65 (P = 0.099)				



(1) Children aged 1.5-18. 2 intervention groups in Ontario combined. Values for 1, 2 and 3 behaviour disorders summed to produce single outcome.

(2) Children aged 3-10.

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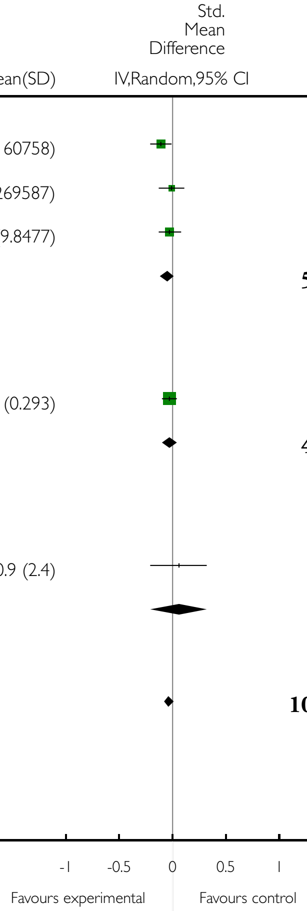
Analysis 8.1. Comparison 8 Time point 2 Child mental health, Outcome 1 Child behaviour problems continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 8 Time point 2 Child mental health

Outcome: 1 Child behaviour problems continuous

Study or subgroup	Favours experimental N	Mean(SD)	Control N	Mean(SD)	Weight	Std. Mean Difference IV,Random,95% CI
1 Behavioral Problems Index (0-56)						
CJF 2002 (1)	748	8.3 (8.2160758)	720	9.2 (8.2160758)	19.8 %	-0.11 [-0.21, -0.01]
FTP 2000 (2)	543	10.8 (9.269587)	565	10.9 (9.269587)	15.0 %	-0.01 [-0.13, 0.11]
MFIP 2000 (3)	991	11.0277 (9.7887)	540	11.31 (9.8477)	18.9 %	-0.03 [-0.13, 0.08]
Subtotal (95% CI)	**2282**		**1825**		**53.7 %**	**-0.05 [-0.12, 0.01]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 1.87, df = 2 (P = 0.39); I^2 =0.0%						
Test for overall effect: Z = 1.69 (P = 0.092)						
2 Behavior Problems Scale (1-3)						
SSP Recipients 2002 (4)	1614	1.4387 (0.2996)	1587	1.45 (0.293)	43.3 %	-0.03 [-0.10, 0.04]
Subtotal (95% CI)	**1614**		**1587**		**43.3 %**	**-0.03 [-0.10, 0.04]**
Heterogeneity: not applicable						
Test for overall effect: Z = 0.79 (P = 0.43)						
3 Survey Diagnostic Instrument Conduct Disorder (0-30)						
Ontario 2001 (5)	173	1.0197 (2.0397)	79	0.9 (2.4)	2.9 %	0.06 [-0.21, 0.32]
Subtotal (95% CI)	**173**		**79**		**2.9 %**	**0.06 [-0.21, 0.32]**
Heterogeneity: not applicable						
Test for overall effect: Z = 0.41 (P = 0.68)						
Total (95% CI)	**4069**		**3491**		**100.0 %**	**-0.04 [-0.08, 0.01]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 2.66, df = 4 (P = 0.62); I^2 =0.0%						
Test for overall effect: Z = 1.69 (P = 0.092)						
Test for subgroup differences: Chi^2 = 0.79, df = 2 (P = 0.67), I^2 =0.0%						



(1) Children aged 5-12.

(2) Children aged 5-12.

(3) Children aged 5-12. Long term and recent applicants from MFIP and MFIP Incentives Only groups combined. Rural respondents excluded from analysis.

(4) Child age subgroups 3-5 and 6-11 combined.

(5) Children aged 4-18. 2 intervention groups in Ontario combined.

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Analysis 8.2. Comparison 8 Time point 2 Child mental health, Outcome 2 Adolescent mental health dichotomous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 8 Time point 2 Child mental health

Outcome: 2 Adolescent mental health dichotomous

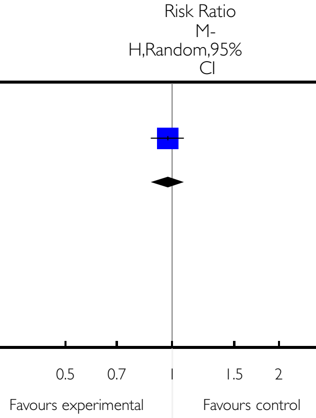
Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Adolescent CES-D at risk (% ≥ 8/30)				
SSP Recipients 2002 (1)	338/740	319/677	100.0 %	0.97 [0.87, 1.08]
Subtotal (95% CI)	**740**	**677**	**100.0 %**	**0.97 [0.87, 1.08]**

Total events: 338 (Experimental), 319 (Control)

Heterogeneity: not applicable

Test for overall effect: Z = 0.54 (P = 0.59)

Test for subgroup differences: Not applicable



(1) Child age subgroups 12-14 and 15-18 combined.

Analysis 8.3. Comparison 8 Time point 2 Child mental health, Outcome 3 Child Behavior Checklist (1-3).

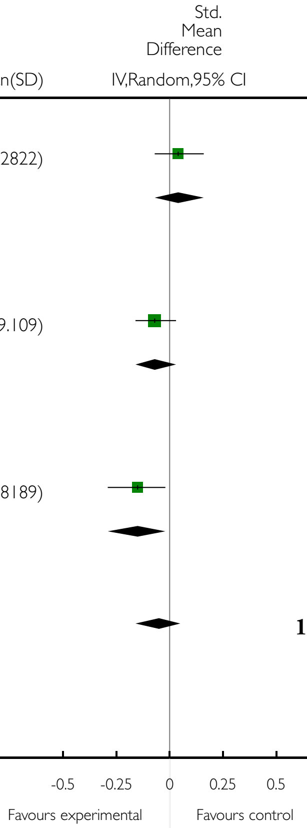
Child Behavior Checklist (1-3)

Study	**Intervention**	**Int n**	**Control**	**Cont n**	**Total N**
CJF GUP 2000	1.7	Not reported	1.6	Not reported	182

Analysis 9.1. Comparison 9 Time point 3 Child mental health, Outcome 1 Child behaviour problems continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 9 Time point 3 Child mental health
Outcome: 1 Child behaviour problems continuous

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Weight	Std. Mean Difference IV,Random,95% CI
1 Behavior Problems Scale (1-3)						
SSP Applicants 2003 (1)	618	1.51 (0.2826)	516	1.5 (0.2822)	33.0 %	0.04 [-0.07, 0.16]
Subtotal (95% CI)	**618**		**516**		**33.0 %**	**0.04 [-0.07, 0.16]**
Heterogeneity: not applicable						
Test for overall effect: Z = 0.74 (P = 0.46)						
2 Behavioral Problems Index (0-56)						
IWRE 2002 (2)	819	11.4 (9.109)	860	12 (9.109)	38.3 %	-0.07 [-0.16, 0.03]
Subtotal (95% CI)	**819**		**860**		**38.3 %**	**-0.07 [-0.16, 0.03]**
Heterogeneity: not applicable						
Test for overall effect: Z = 1.35 (P = 0.18)						
3 Problem Behavior Scale (1-5)						
New Hope 1999 (3)	419	2.3 (0.648189)	411	2.4 (0.648189)	28.7 %	-0.15 [-0.29, -0.02]
Subtotal (95% CI)	**419**		**411**		**28.7 %**	**-0.15 [-0.29, -0.02]**
Heterogeneity: not applicable						
Test for overall effect: Z = 2.22 (P = 0.027)						
Total (95% CI)	**1856**		**1787**		**100.0 %**	**-0.05 [-0.16, 0.05]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 4.84, df = 2 (P = 0.09); I^2 =59%						
Test for overall effect: Z = 1.04 (P = 0.30)						
Test for subgroup differences: Chi^2 = 4.84, df = 2 (P = 0.09), I^2 =59%						



(1) Child age groups 6-8 and 9-14 combined.
(2) Children aged 5-12
(3) Children aged 6-16

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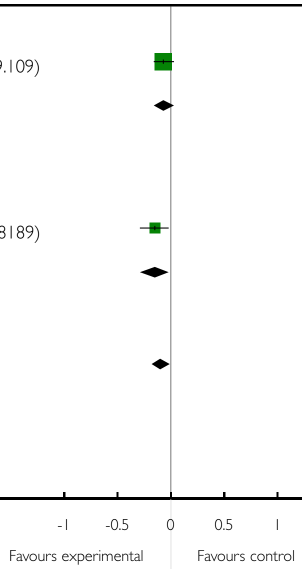
Analysis 9.2. Comparison 9 Time point 3 Child mental health, Outcome 2 Child behaviour problem continuous excluding SSP Applicants.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 9 Time point 3 Child mental health

Outcome: 2 Child behaviour problem continuous excluding SSP Applicants

Study or subgroup	Favours [experimental] N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Behavioral Problems Index (0-56)							
IWRE 2002 (1)	819	11.4 (9.109)	860	12 (9.109)		65.7 %	-0.07 [-0.16, 0.03]
Subtotal (95% CI)	**819**		**860**			**65.7 %**	**-0.07 [-0.16, 0.03]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.35 (P = 0.18)							
2 Problem behavior scale (1-5)							
New Hope 1999 (2)	419	2.3 (0.648189)	411	2.4 (0.648189)		34.3 %	-0.15 [-0.29, -0.02]
Subtotal (95% CI)	**419**		**411**			**34.3 %**	**-0.15 [-0.29, -0.02]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.22 (P = 0.027)							
Total (95% CI)	**1238**		**1271**			**100.0 %**	**-0.10 [-0.18, -0.01]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 1.08, df = 1 (P = 0.30); I^2 =7%							
Test for overall effect: Z = 2.29 (P = 0.022)							
Test for subgroup differences: Chi^2 = 1.08, df = 1 (P = 0.30), I^2 =7%							



(1) Children aged 5-12

(2) Children aged 6-16

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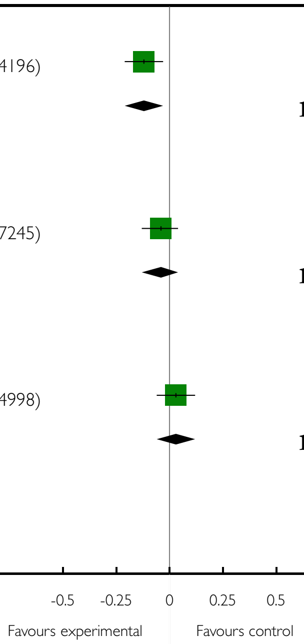
Analysis 9.3. Comparison 9 Time point 3 Child mental health, Outcome 3 NEWWS 2001 Child mental health.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 9 Time point 3 Child mental health

Outcome: 3 NEWWS 2001 Child mental health

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 BPI Externalising subscore (0-18)							
NEWWS 2001 (1)	1251	4.5353 (2.3803)	873	4.82 (2.4196)		100.0 %	-0.12 [-0.21, -0.03]
Subtotal (95% CI)	**1251**		**873**			**100.0 %**	**-0.12 [-0.21, -0.03]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.71 (P = 0.0067)							
2 BPI Internalising subscore (0-24)							
NEWWS 2001 (2)	1251	8.1623 (2.7027)	873	8.28 (2.7245)		100.0 %	-0.04 [-0.13, 0.04]
Subtotal (95% CI)	**1251**		**873**			**100.0 %**	**-0.04 [-0.13, 0.04]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.97 (P = 0.33)							
3 BPI Hyperactivity subscore (0-18)							
NEWWS 2001 (3)	1251	6.2985 (2.4166)	873	6.22 (2.4998)		100.0 %	0.03 [-0.06, 0.12]
Subtotal (95% CI)	**1251**		**873**			**100.0 %**	**0.03 [-0.06, 0.12]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.68 (P = 0.50)							
Test for subgroup differences: Chi² = 5.76, df = 2 (P = 0.06), I² =65%							



(1) Children aged 8-10

(2) Children aged 8-10

(3) Children aged 8-10

Analysis 9.4. Comparison 9 Time point 3 Child mental health, Outcome 4 IFIP Behavioral Problems Index (0-56).

IFIP Behavioral Problems Index (0-56)

Study	Group	Intervention	Int n	Control	Cont n	Sig
IFIP 2002	Ongoing recipients	11.8	540	12.0	273	NS
IFIP 2002	Applicants	11.3	442	10.9	220	NS

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Analysis 9.5. Comparison 9 Time point 3 Child mental health, Outcome 5 SSP-R T3 Behavior Problems Scale (1-3).

SSP-R T3 Behavior Problems Scale (1-3)

Study	Child age	Intervention	Int n	Control	Cont n	Sig.
SSP Recipients 2002	5.5-7.5 years at follow up	1.3	554	1.3	605	NS
SSP Recipients 2002	7.5-9.5 years at follow up	1.3	577	1.3	560	NS

Analysis 10.1. Comparison 10 Time point 1 Child physical health, Outcome 1 General health rating (1-5).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 10 Time point 1 Child physical health

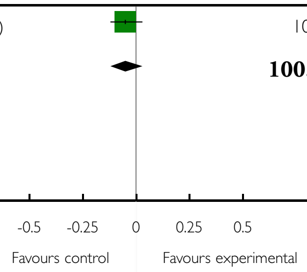
Outcome: 1 General health rating (1-5)

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
NEWWS 2001 (1)	1554	4.2085 (0.9317)	1208	4.25 (0.9436)		100.0 %	-0.05 [-0.12, 0.03]
Total (95% CI)	**1554**		**1208**			**100.0 %**	**-0.05 [-0.12, 0.03]**

Heterogeneity: not applicable

Test for overall effect: Z = 1.18 (P = 0.24)

Test for subgroup differences: Not applicable



(1) Children aged 5-7. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

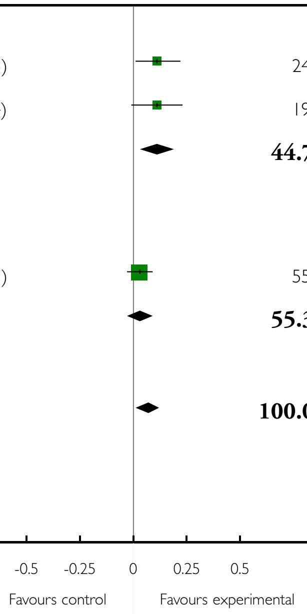
Analysis 11.1. Comparison 11 Time point 2 Child physical health, Outcome 1 Child physical health continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 11 Time point 2 Child physical health

Outcome: 1 Child physical health continuous

Study or subgroup	Experimental N	Experimental Mean(SD)	Control N	Control Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 General health scale (1-5)							
CJF 2002 (1)	748	4.4 (0.8849475)	721	4.3 (0.8849475)		24.9 %	0.11 [0.01, 0.22]
FTP 2000 (2)	543	4.2 (0.925974)	565	4.1 (0.925974)		19.7 %	0.11 [-0.01, 0.23]
Subtotal (95% CI)	**1291**		**1286**			**44.7 %**	**0.11 [0.03, 0.19]**
Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.00$, df = 1 (P = 0.95); I^2 =0.0%							
Test for overall effect: Z = 2.81 (P = 0.0050)							
2 Child average health scale (1-5 across 4-item instrument)							
SSP Recipients 2002 (3)	2354	4.0855 (0.8172)	2264	4.06 (0.8171)		55.3 %	0.03 [-0.03, 0.09]
Subtotal (95% CI)	**2354**		**2264**			**55.3 %**	**0.03 [-0.03, 0.09]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.06 (P = 0.29)							
Total (95% CI)	**3645**		**3550**			**100.0 %**	**0.07 [0.01, 0.12]**
Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 2.62$, df = 2 (P = 0.27); I^2 =24%							
Test for overall effect: Z = 2.30 (P = 0.022)							
Test for subgroup differences: $Chi^2 = 2.62$, df = 1 (P = 0.11), I^2 =62%							



(1) Children aged 5-12

(2) Children aged 5-12

(3) Child age groups 3-5, 6-11, 12-18 combined.

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Analysis 11.2. Comparison 11 Time point 2 Child physical health, Outcome 2 Child physical health dichotomous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 11 Time point 2 Child physical health

Outcome: 2 Child physical health dichotomous

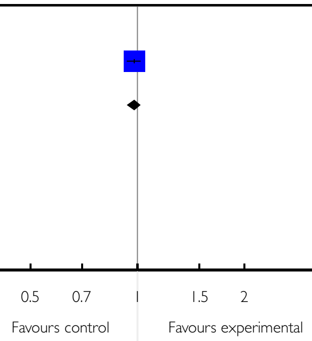
Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 In good or excellent health					
MFIP 2000 (1)	926/1180	578/720		100.0 %	0.98 [0.93, 1.02]
Subtotal (95% CI)	**1180**	**720**		**100.0 %**	**0.98 [0.93, 1.02]**

Total events: 926 (Experimental), 578 (Control)

Heterogeneity: not applicable

Test for overall effect: Z = 0.95 (P = 0.34)

Test for subgroup differences: Not applicable



(1) Children aged 5-12. MFIP and MFIP-IO long term and recent urban and rural groups combined.

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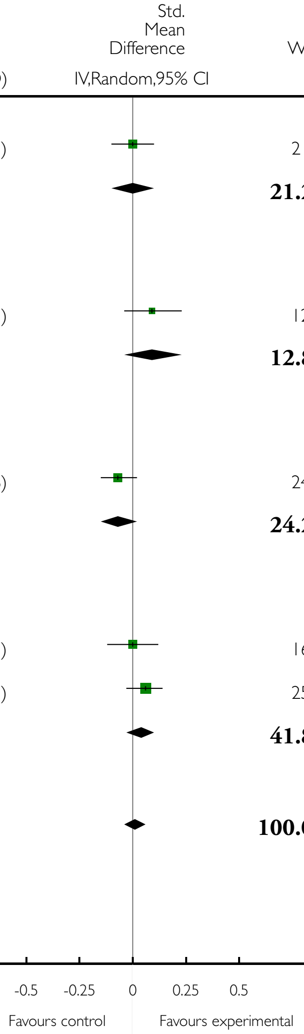
Analysis 12.1. Comparison 12 Time point 3 Child physical health, Outcome 1 Child physical health continuous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 12 Time point 3 Child physical health

Outcome: 1 Child physical health continuous

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Weight	Std. Mean Difference IV,Random,95% CI
1 Health status scale (1-5)						
IWRE 2002 (1)	819	4.2 (0.95)	860	4.2 (0.95)	21.2 %	0.0 [-0.10, 0.10]
Subtotal (95% CI)	**819**		**860**		**21.2 %**	**0.0 [-0.10, 0.10]**
Heterogeneity: not applicable						
Test for overall effect: Z = 0.0 (P = 1.0)						
2 Overall health scale (1-5)						
New Hope 1999 (2)	429	4.3 (1.093255)	421	4.2 (1.093255)	12.8 %	0.09 [-0.04, 0.23]
Subtotal (95% CI)	**429**		**421**		**12.8 %**	**0.09 [-0.04, 0.23]**
Heterogeneity: not applicable						
Test for overall effect: Z = 1.33 (P = 0.18)						
3 General health rating (1-5)						
NEWWS 2001 (3)	1251	4.2621 (0.548)	873	4.3 (0.5706)	24.2 %	-0.07 [-0.15, 0.02]
Subtotal (95% CI)	**1251**		**873**		**24.2 %**	**-0.07 [-0.15, 0.02]**
Heterogeneity: not applicable						
Test for overall effect: Z = 1.54 (P = 0.12)						
4 Child average health scale (1-5 across 4-item instrument)						
SSP Applicants 2003 (4)	618	4.1613 (1.2174)	516	4.16 (1.2201)	16.0 %	0.00 [-0.12, 0.12]
SSP Recipients 2002 (5)	1131	4.202 (0.957)	1165	4.15 (0.9531)	25.8 %	0.06 [-0.03, 0.14]
Subtotal (95% CI)	**1749**		**1681**		**41.8 %**	**0.04 [-0.03, 0.10]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.62, df = 1 (P = 0.43); I^2 =0.0%						
Test for overall effect: Z = 1.10 (P = 0.27)						
Total (95% CI)	**4248**		**3835**		**100.0 %**	**0.01 [-0.04, 0.06]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 5.84, df = 4 (P = 0.21); I^2 =31%						
Test for overall effect: Z = 0.35 (P = 0.72)						
Test for subgroup differences: Chi^2 = 5.22, df = 3 (P = 0.16), I^2 =42%						



(1) Children aged 5-12

(2) Children aged 6-16

(3) Children aged 8-10. Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

(4) Child age groups 6-8 and 9-14 combined.

(5) Child age subgroups 5.5-7.5 and 7.5-9.5 combined.

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Analysis 12.2. Comparison 12 Time point 3 Child physical health, Outcome 2 Child physical health dichotomous.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 12 Time point 3 Child physical health

Outcome: 2 Child physical health dichotomous

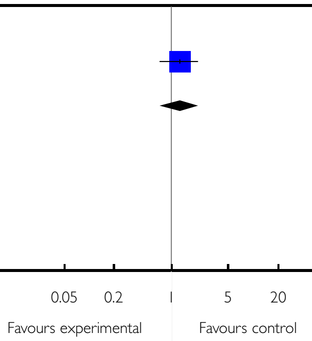
Study or subgroup	Favours experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 In fair or poor health (%)					
IFIP 2002 (1)	45/982	18/493		100.0 %	1.26 [0.73, 2.14]
Subtotal (95% CI)	**982**	**493**		**100.0 %**	**1.26 [0.73, 2.14]**

Total events: 45 (Favours experimental), 18 (Control)

Heterogeneity: not applicable

Test for overall effect: Z = 0.83 (P = 0.41)

Test for subgroup differences: Not applicable



(1) Children aged 5-12. Applicants and recipients groups combined.

Analysis 13.1. Comparison 13 Time point 1 Employment status, Outcome 1 Currently employed (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 13 Time point 1 Employment status

Outcome: 1 Currently employed (%)

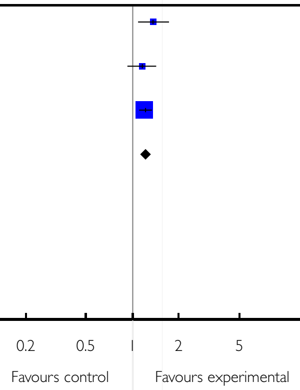
Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
CJF GUP 2000	83/149	65/159		11.9 %	1.36 [1.08, 1.72]
CJF Yale 2001	89/158	75/153		14.6 %	1.15 [0.93, 1.42]
NEWWS 2001 (1)	673/1554	432/1208		73.5 %	1.21 [1.10, 1.33]
Total (95% CI)	**1861**	**1520**		**100.0 %**	**1.22 [1.12, 1.32]**

Total events: 845 (Experimental), 572 (Control)

Heterogeneity: Tau^2 = 0.0; Chi^2 = 1.18, df = 2 (P = 0.56); I^2 =0.0%

Test for overall effect: Z = 4.78 (P < 0.00001)

Test for subgroup differences: Not applicable

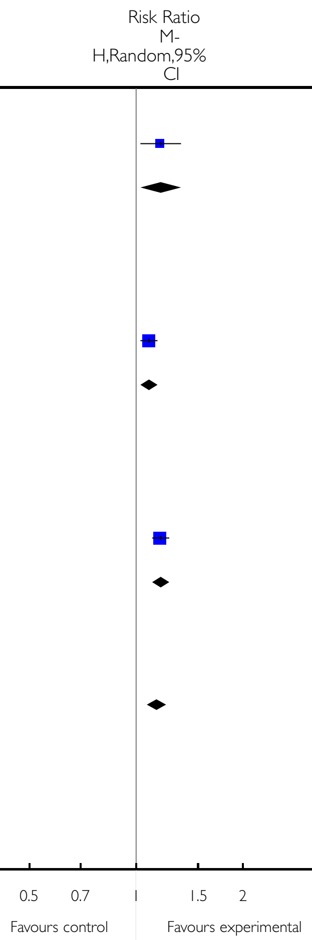


(1) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Analysis 13.2. Comparison 13 Time point 1 Employment status, Outcome 2 Ever employed (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 13 Time point 1 Employment status
Outcome: 2 Ever employed (%)

Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Ever employed since randomisation (18 months) (%)				
CJF Yale 2001	127/158	105/153	16.5 %	1.17 [1.03, 1.34]
Subtotal (95% CI)	**158**	**153**	**16.5 %**	**1.17 [1.03, 1.34]**
Total events: 127 (Experimental), 105 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 2.35 (P = 0.019)				
2 Ever employed year 2 (%)				
New Hope 1999	329/366	313/379	41.6 %	1.09 [1.03, 1.15]
Subtotal (95% CI)	**366**	**379**	**41.6 %**	**1.09 [1.03, 1.15]**
Total events: 329 (Experimental), 313 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 2.88 (P = 0.0039)				
3 Ever employed since randomisation (24 months) (%)				
NEWWS 2001 (1)	1085/1554	718/1208	41.9 %	1.17 [1.11, 1.24]
Subtotal (95% CI)	**1554**	**1208**	**41.9 %**	**1.17 [1.11, 1.24]**
Total events: 1085 (Experimental), 718 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 5.54 (P < 0.00001)				
Total (95% CI)	**2078**	**1740**	**100.0 %**	**1.14 [1.07, 1.21]**
Total events: 1541 (Experimental), 1136 (Control)				
Heterogeneity: Tau^2 = 0.00; Chi^2 = 4.29, df = 2 (P = 0.12); I^2 =53%				
Test for overall effect: Z = 4.07 (P = 0.000048)				
Test for subgroup differences: Chi^2 = 3.65, df = 2 (P = 0.16), I^2 =45%				



(1) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

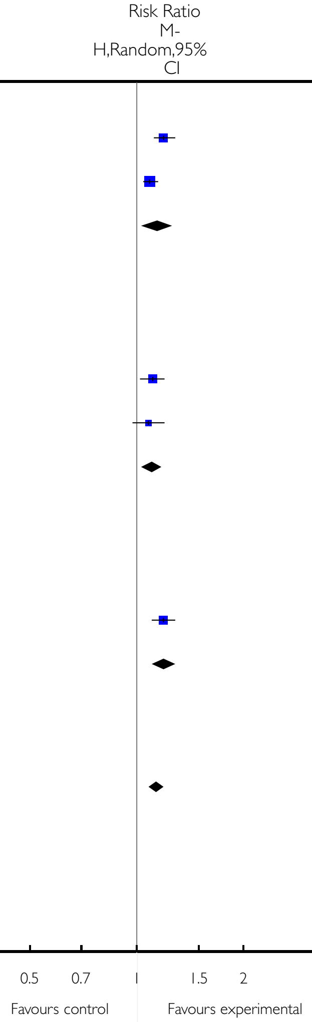
Analysis 14.1. Comparison 14 Time point 2 Employment status, Outcome 1 Ever employed (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 14 Time point 2 Employment status

Outcome: 1 Ever employed (%)

Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Ever employed since randomisation (36 months)				
California GAIN 1994	1076/1925	650/1389	21.0 %	1.19 [1.12, 1.28]
MFIP 2000 (1)	881/991	439/540	28.2 %	1.09 [1.04, 1.15]
Subtotal (95% CI)	**2916**	**1929**	**49.2 %**	**1.14 [1.03, 1.26]**
Total events: 1957 (Experimental), 1089 (Control)				
Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 5.88$, df = 1 (P = 0.02); $I^2 = 83\%$				
Test for overall effect: Z = 2.56 (P = 0.010)				
2 Average employment year of study				
CJF 2002	495/748	431/721	18.4 %	1.11 [1.02, 1.20]
FTP 2000	315/543	303/565	13.1 %	1.08 [0.97, 1.20]
Subtotal (95% CI)	**1291**	**1286**	**31.5 %**	**1.10 [1.03, 1.17]**
Total events: 810 (Experimental), 734 (Control)				
Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.12$, df = 1 (P = 0.73); $I^2 = 0.0\%$				
Test for overall effect: Z = 2.90 (P = 0.0037)				
3 Employed at 33 months				
SSP Recipients 2002	972/2460	794/2392	19.3 %	1.19 [1.10, 1.28]
Subtotal (95% CI)	**2460**	**2392**	**19.3 %**	**1.19 [1.10, 1.28]**
Total events: 972 (Experimental), 794 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 4.55 (P < 0.00001)				
Total (95% CI)	**6667**	**5607**	**100.0 %**	**1.13 [1.08, 1.19]**
Total events: 3739 (Experimental), 2617 (Control)				
Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 8.53$, df = 4 (P = 0.07); $I^2 = 53\%$				
Test for overall effect: Z = 5.28 (P < 0.00001)				
Test for subgroup differences: $Chi^2 = 2.63$, df = 2 (P = 0.27), $I^2 = 24\%$				



(1) MFIP and MFIP-IO long term and recent urban groups combined. Not reported for rural groups.

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Analysis 14.2. Comparison 14 Time point 2 Employment status, Outcome 2 Ever employed full-time since randomisation (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 14 Time point 2 Employment status

Outcome: 2 Ever employed full-time since randomisation (%)

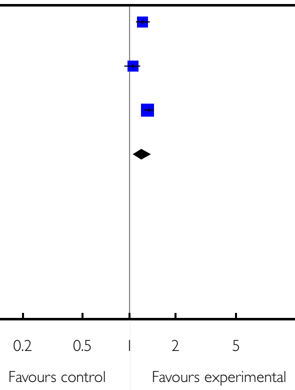
Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
California GAIN 1994	699/1925	414/1389		32.9 %	1.22 [1.10, 1.35]
MFIP 2000 (1)	424/991	221/540		29.9 %	1.05 [0.92, 1.18]
SSP Recipients 2002	1291/2503	952/2458		37.2 %	1.33 [1.25, 1.42]
Total (95% CI)	**5419**	**4387**		**100.0 %**	**1.20 [1.05, 1.37]**

Total events: 2414 (Experimental), 1587 (Control)

Heterogeneity: Tau^2 = 0.01; Chi^2 = 12.10, df = 2 (P = 0.002); I^2 =83%

Test for overall effect: Z = 2.72 (P = 0.0065)

Test for subgroup differences: Not applicable



(1) MFIP and MFIP-IO long term and recent urban groups combined. Not reported for rural groups.

Analysis 14.3. Comparison 14 Time point 2 Employment status, Outcome 3 Ever employed full-time excluding MFIP (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 14 Time point 2 Employment status

Outcome: 3 Ever employed full-time excluding MFIP (%)

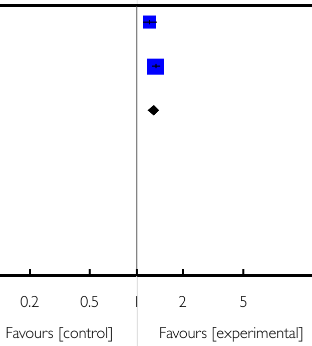
Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
California GAIN 1994	699/1925	414/1389		40.1 %	1.22 [1.10, 1.35]
SSP Recipients 2002	1291/2503	952/2458		59.9 %	1.33 [1.25, 1.42]
Total (95% CI)	**4428**	**3847**		**100.0 %**	**1.29 [1.18, 1.40]**

Total events: 1990 (Experimental), 1366 (Control)

Heterogeneity: Tau^2 = 0.00; Chi^2 = 2.21, df = 1 (P = 0.14); I^2 =55%

Test for overall effect: Z = 5.72 (P < 0.00001)

Test for subgroup differences: Not applicable



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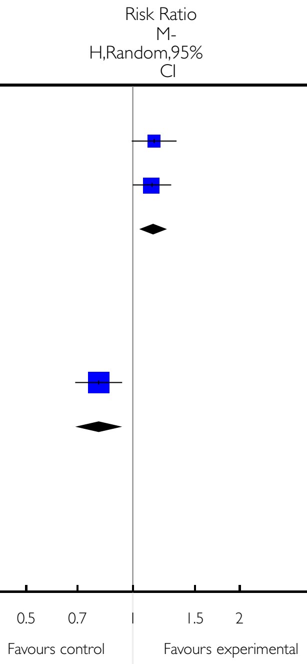
Analysis 14.4. Comparison 14 Time point 2 Employment status, Outcome 4 Employed part-time (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 14 Time point 2 Employment status

Outcome: 4 Employed part-time (%)

Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Ever employed part-time since randomisation				
California GAIN 1994	373/1925	235/1389	40.8 %	1.15 [0.99, 1.33]
MFIP 2000 (1)	454/991	218/540	59.2 %	1.13 [1.00, 1.28]
Subtotal (95% CI)	**2916**	**1929**	**100.0 %**	**1.14 [1.04, 1.25]**
Total events: 827 (Experimental), 453 (Control)				
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.01, df = 1 (P = 0.92); I^2 =0.0%				
Test for overall effect: Z = 2.70 (P = 0.0069)				
2 Currently employed part-time				
SSP Recipients 2002 (2)	273/2460	332/2392	100.0 %	0.80 [0.69, 0.93]
Subtotal (95% CI)	**2460**	**2392**	**100.0 %**	**0.80 [0.69, 0.93]**
Total events: 273 (Experimental), 332 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 2.92 (P = 0.0034)				
Test for subgroup differences: Chi^2 = 15.32, df = 1 (P = 0.00), I^2 =93%				



(1) MFIP and MFIP-IO long term and recent urban groups combined. Not reported for rural groups.

(2) Calculated from categorical data for hours worked per week (hours per week < or = 30).

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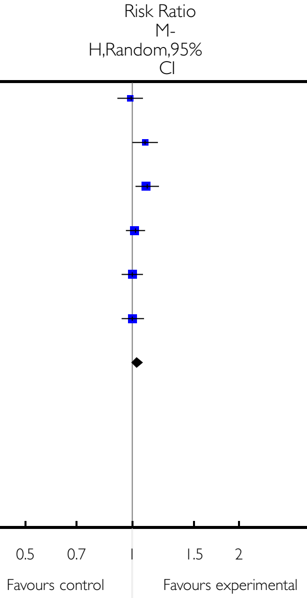
Analysis 15.1. Comparison 15 Time point 3 Employment status, Outcome 1 Currently employed (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 15 Time point 3 Employment status

Outcome: 1 Currently employed (%)

Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
IFIP 2002 (1)	626/982	317/493	14.8 %	0.99 [0.91, 1.07]
IWRE 2002	478/819	462/860	13.8 %	1.09 [1.00, 1.18]
NEWWS 2001 (2)	765/1251	485/873	16.9 %	1.10 [1.02, 1.19]
SSP Applicants 2003 (3)	710/1186	693/1185	19.5 %	1.02 [0.96, 1.09]
SSP Recipients 2002 (4)	1028/2460	1002/2392	19.7 %	1.00 [0.93, 1.07]
UK ERA 2011	542/951	514/903	15.3 %	1.00 [0.93, 1.08]
Total (95% CI)	**7649**	**6706**	**100.0 %**	**1.03 [0.99, 1.07]**



Total events: 4149 (Experimental), 3473 (Control)

Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 6.89$, $df = 5$ ($P = 0.23$); $I^2 = 27\%$

Test for overall effect: $Z = 1.68$ ($P = 0.093$)

Test for subgroup differences: Not applicable

(1) Applicants and recipients groups combined..

(2) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

(3) All hours per week categories summed.

(4) Monthly employment rate Yr 5, quarter 2.

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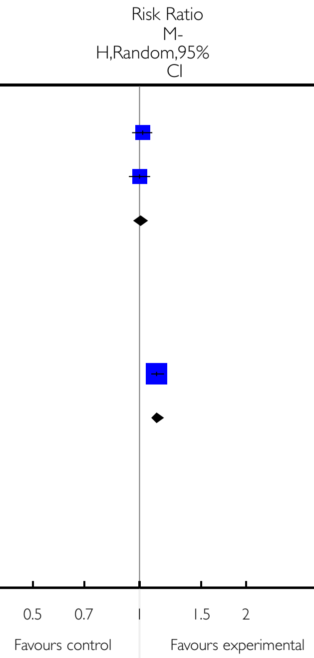
Analysis 15.2. Comparison 15 Time point 3 Employment status, Outcome 2 Ever employed (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 15 Time point 3 Employment status

Outcome: 2 Ever employed (%)

Study or subgroup	Favours control n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Ever employed year 5				
New Hope 1999	297/366	303/379	46.4 %	1.02 [0.95, 1.09]
UK ERA 2011	625/951	595/903	53.6 %	1.00 [0.93, 1.07]
Subtotal (95% CI)	**1317**	**1282**	**100.0 %**	**1.01 [0.96, 1.06]**
Total events: 922 (Favours control), 898 (Control)				
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.14, df = 1 (P = 0.71); I^2 =0.0%				
Test for overall effect: Z = 0.22 (P = 0.82)				
2 Ever employed years 1-5				
NEWWS 2001 (1)	1114/1251	693/873	100.0 %	1.12 [1.08, 1.17]
Subtotal (95% CI)	**1251**	**873**	**100.0 %**	**1.12 [1.08, 1.17]**
Total events: 1114 (Favours control), 693 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 5.78 (P < 0.00001)				
Test for subgroup differences: Chi^2 = 12.00, df = 1 (P = 0.00), I^2 =92%				



(1) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

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Analysis 15.3. Comparison 15 Time point 3 Employment status, Outcome 3 Currently employed full-time (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 15 Time point 3 Employment status

Outcome: 3 Currently employed full-time (%)

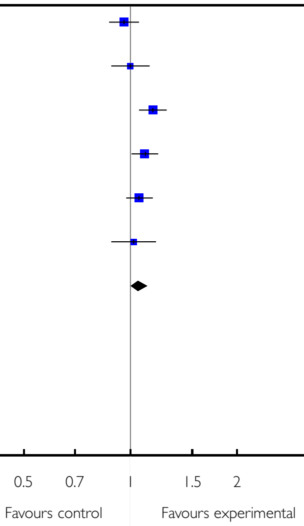
Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
IFIP 2002 (1)	523/982	273/493		17.9 %	0.96 [0.87, 1.06]
New Hope 1999	178/281	175/276		13.4 %	1.00 [0.88, 1.13]
NEWWS 2001 (2)	635/1251	382/873		19.0 %	1.16 [1.06, 1.27]
SSP Applicants 2003 (3)	556/1186	505/1185		19.7 %	1.10 [1.01, 1.20]
SSP Recipients 2002 (4)	686/2460	629/2392		19.1 %	1.06 [0.97, 1.16]
UK ERA 2011 (5)	263/951	245/903		10.8 %	1.02 [0.88, 1.18]
Total (95% CI)	**7111**	**6122**		**100.0 %**	**1.05 [1.00, 1.12]**

Total events: 2841 (Experimental), 2209 (Control)

Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 9.17$, $df = 5$ ($P = 0.10$); $I^2 = 45\%$

Test for overall effect: $Z = 1.80$ ($P = 0.072$)

Test for subgroup differences: Not applicable



(1) Applicants and recipients groups combined.

(2) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

(3) Hours worked per week 30 - over 40; categorical variables summed.

(4) Hours worked per week 30 - over 40; categorical variables summed.

(5) Hours worked per week > 30.

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Analysis 15.4. Comparison 15 Time point 3 Employment status, Outcome 4 Currently employed part-time (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 15 Time point 3 Employment status

Outcome: 4 Currently employed part-time (%)

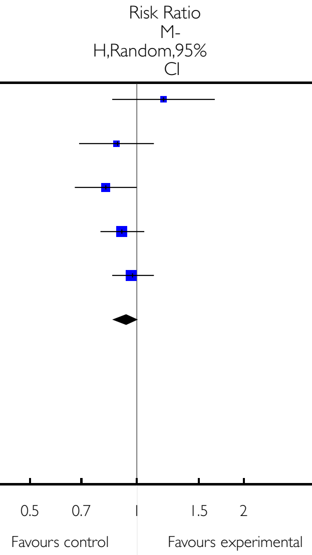
Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
IFIP 2002 (1)	104/982	44/493	6.3 %	1.19 [0.85, 1.66]
NEWWS 2001 (2)	130/1251	103/873	11.7 %	0.88 [0.69, 1.12]
SSP Applicants 2003	154/1186	188/1185	17.5 %	0.82 [0.67, 1.00]
SSP Recipients 2002	325/2460	347/2392	32.3 %	0.91 [0.79, 1.05]
UK ERA 2011 (3)	277/951	270/903	32.2 %	0.97 [0.85, 1.12]
Total (95% CI)	**6830**	**5846**	**100.0 %**	**0.93 [0.85, 1.01]**

Total events: 990 (Experimental), 952 (Control)

Heterogeneity: Tau^2 = 0.00; Chi^2 = 4.32, df = 4 (P = 0.36); I^2 =7%

Test for overall effect: Z = 1.78 (P = 0.075)

Test for subgroup differences: Not applicable



(1) Applicants and recipients groups combined.

(2) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

(3) Hours per week 1-15 and 16-29 summed.

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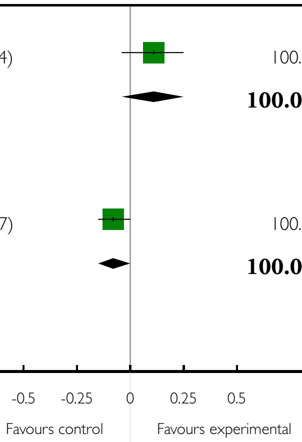
Analysis 16.1. Comparison 16 Time point 1 Income, Outcome 1 Total income.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 16 Time point 1 Income
Outcome: 1 Total income

Study or subgroup	Favours control N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Total income year 2 (USD)							
New Hope 1999	13808 (6675.2942604)	366	378	13086 (6675.2942604)		100.0 %	0.11 [-0.04, 0.25]
Subtotal (95% CI)	**366**		**378**			**100.0 %**	**0.11 [-0.04, 0.25]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.47 (P = 0.14)							
2 Total net household income in prior month (USD)							
NEWWS 2001 (1)	1554	1240.982 (847.4572)	1208	1309.58 (889.9447)		100.0 %	-0.08 [-0.15, 0.00]
Subtotal (95% CI)	**1554**		**1208**			**100.0 %**	**-0.08 [-0.15, 0.00]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.06 (P = 0.039)							

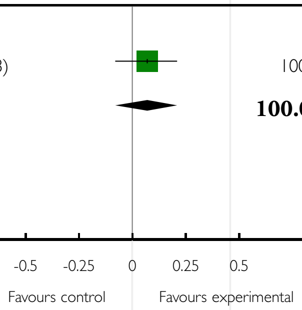


(1) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Analysis 16.2. Comparison 16 Time point 1 Income, Outcome 2 Earnings.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 16 Time point 1 Income
Outcome: 2 Earnings

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Average annual earnings year 2 (USD)							
New Hope 1999	366	8310 (6418.7328)	378	7886 (6418.7328)		100.0 %	0.07 [-0.08, 0.21]
Subtotal (95% CI)	**366**		**378**			**100.0 %**	**0.07 [-0.08, 0.21]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.90 (P = 0.37)							
Test for subgroup differences: Not applicable							



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Analysis 16.3. Comparison 16 Time point 1 Income, Outcome 3 NEWWS T1 Average earnings previous month (USD).

NEWWS T1 Average earnings previous month (USD)

Study	Intervention group	I nt	n	C ont	n	Sig
NEWWS 2001	Atlanta Human Capital Development	343	520	289	506	.0.1
NEWWS 2001	Atlanta Labour Force Attachment	326	396	293	506	NS
NEWWS 2001	Grand Rapids Human Capital Development	336	205	341	216	NS
NEWWS 2001	Grand Rapids Labour Force Attachment	392	225	345	216	NS
NEWWS 2001	Riverside Labour Force Attachment	337	208	197	486	0.001

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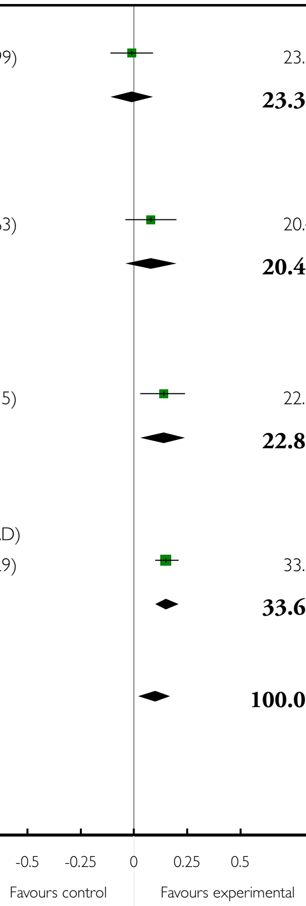
Analysis 17.1. Comparison 17 Time point 2 Income, Outcome 1 Total income.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 17 Time point 2 Income

Outcome: 1 Total income

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Average annual income (benefits, earnings and food stamps) years 3-4 (USD)							
CJF 2002	748	12397 (7400.99)	721	12465 (7400.99)		23.3 %	-0.01 [-0.11, 0.09]
Subtotal (95% CI)	**748**		**721**			**23.3 %**	**-0.01 [-0.11, 0.09]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.18 (P = 0.86)							
2 Average total income (benefits, earnings and food stamps) year 4 (USD)							
FTP 2000	543	7965 (6469.63)	565	7432 (6469.63)		20.4 %	0.08 [-0.04, 0.20]
Subtotal (95% CI)	**543**		**565**			**20.4 %**	**0.08 [-0.04, 0.20]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.37 (P = 0.17)							
3 Average annual income (benefits and earnings) year 3 (USD)							
MFIP 2000 (1)	11457.0484 (5050.5876)	991	540	10764.97 (5227.6915)		22.8 %	0.14 [0.03, 0.24]
Subtotal (95% CI)	**991**		**540**			**22.8 %**	**0.14 [0.03, 0.24]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.53 (P = 0.012)							
4 Total monthly individual income (average from all sources in 6 months prior to 3-year survey) (CAD)							
SSP Recipients 2002	2453	1405 (884.7729)	2373	1270 (884.7729)		33.6 %	0.15 [0.10, 0.21]
Subtotal (95% CI)	**2453**		**2373**			**33.6 %**	**0.15 [0.10, 0.21]**
Heterogeneity: not applicable							
Test for overall effect: Z = 5.29 (P < 0.00001)							
Total (95% CI)	**4735**		**4199**			**100.0 %**	**0.10 [0.02, 0.17]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 7.79, df = 3 (P = 0.05); I^2 =62%							
Test for overall effect: Z = 2.55 (P = 0.011)							
Test for subgroup differences: Chi^2 = 7.79, df = 3 (P = 0.05), I^2 =62%							



(1) Long term and recent urban applicants from MFIP groups combined. Rural respondents excluded from analysis due to missing measures of variance.

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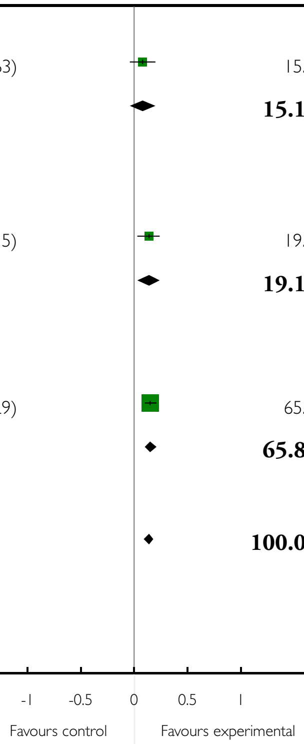
Analysis 17.2. Comparison 17 Time point 2 Income, Outcome 2 Total income excluding CJF.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 17 Time point 2 Income

Outcome: 2 Total income excluding CJF

Study or subgroup	Experimental N	 Mean(SD)	Control N	 Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Average total income from earnings, AFDC/TANF and food stamps year 4 (USD)							
FTP 2000	543	7965 (6469.63)	565	7432 (6469.63)		15.1 %	0.08 [-0.04, 0.20]
Subtotal (95% CI)	**543**		**565**			**15.1 %**	**0.08 [-0.04, 0.20]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.37 (P = 0.17)							
2 Average annual income welfare/earnings year 3 (USD)							
MFIP 2000 (1)	11457.0484 (5050.5876)	991	540	10764.97 (5227.6915)		19.1 %	0.14 [0.03, 0.24]
Subtotal (95% CI)	**991**		**540**			**19.1 %**	**0.14 [0.03, 0.24]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.53 (P = 0.012)							
3 Total monthly individual income 6 months prior to 3-year survey (CAD)							
SSP Recipients 2002	2453	1405 (884.7729)	2373	1270 (884.7729)		65.8 %	0.15 [0.10, 0.21]
Subtotal (95% CI)	**2453**		**2373**			**65.8 %**	**0.15 [0.10, 0.21]**
Heterogeneity: not applicable							
Test for overall effect: Z = 5.29 (P < 0.00001)							
Total (95% CI)	**3987**		**3478**			**100.0 %**	**0.14 [0.09, 0.18]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 1.11, df = 2 (P = 0.57); I^2 =0.0%							
Test for overall effect: Z = 5.93 (P < 0.00001)							
Test for subgroup differences: Chi^2 = 1.11, df = 2 (P = 0.57), I^2 =0.0%							



(1) Long term and recent urban applicants from MFIP groups combined. Rural respondents excluded from analysis due to missing measures of variance.

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Analysis 17.3. Comparison 17 Time point 2 Income, Outcome 3 Average earnings in year of survey (USD).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 17 Time point 2 Income

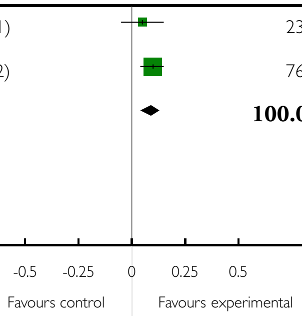
Outcome: 3 Average earnings in year of survey (USD)

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
CJF 2002	748	8072 (6826.1)	721	7721 (6826.1)		23.3 %	0.05 [-0.05, 0.15]
SSP Recipients 2002	2460	4640 (8706.182242)	2392	3805 (8706.182242)		76.7 %	0.10 [0.04, 0.15]
Total (95% CI)	**3208**		**3113**			**100.0 %**	**0.09 [0.04, 0.13]**

Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.56, df = 1 (P = 0.46); I^2 =0.0%

Test for overall effect: Z = 3.40 (P = 0.00068)

Test for subgroup differences: Not applicable



Analysis 17.4. Comparison 17 Time point 2 Income, Outcome 4 MFIP Average annual earnings years 1-3 (USD).

MFIP Average annual earnings years 1-3 (USD)

Study	Group	MFIP	MFIP-IO	Control	MFIP n	MFIP-IO n	Cont n	Sig
MFIP 2000	Long-term urban recipients	4657	3,967	3906	306	292	281	NS
MFIP 2000	Recent urban recipients	6817	6,270	7438	258	135	259	NS
MFIP 2000	Long-term rural recipients	4061	NA	4139	92	NA	105	NS
MFIP 2000	Recent rural recipients	6530	NA	5854	97	NA	75	NS

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Analysis 17.5. Comparison 17 Time point 2 Income, Outcome 5 GAIN Average weekly earnings since randomisation (USD).

GAIN Average weekly earnings since randomisation (USD)

Study	Intervention	Int n	Control	Cont n	Sample	Sig
California GAIN 1994	204	1076	190	648	Employed respondents only	No test conducted

Analysis 17.6. Comparison 17 Time point 2 Income, Outcome 6 FTP Average earnings in year of study (USD).

FTP Average earnings in year of study (USD)

Study	Intervention mean	Intervention n	Control mean	Control n
FTP 2000	6177	543	5208	565

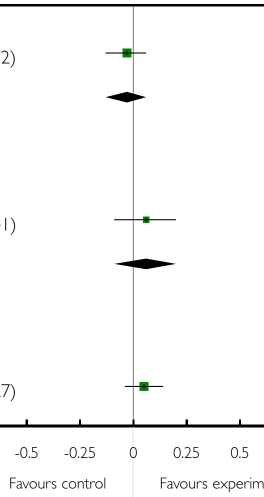
Analysis 18.1. Comparison 18 Time point 3 Income, Outcome 1 Total income.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 18 Time point 3 Income

Outcome: 1 Total income

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Total household income month prior to survey, annualised (USD)							
IWRE 2002 (1)	819	19923 (13706.272)	860	20390 (13706.272)		17.6 %	-0.03 [-0.13, 0.06]
Subtotal (95% CI)	**819**		**860**			**17.6 %**	**-0.03 [-0.13, 0.06]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.70 (P = 0.49)							
2 Total income year 5 (USD)							
New Hope 1999	366	14329 (9479.241)	379	13777 (9479.241)		9.6 %	0.06 [-0.09, 0.20]
Subtotal (95% CI)	**366**		**379**			**9.6 %**	**0.06 [-0.09, 0.20]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.79 (P = 0.43)							
3 Total income years 1-5 (USD)							
NEWWS 2001 (2)	47155.3589 (22147.6627)	1251	873	46025.89 (22808.0227)		20.0 %	0.05 [-0.04, 0.14]



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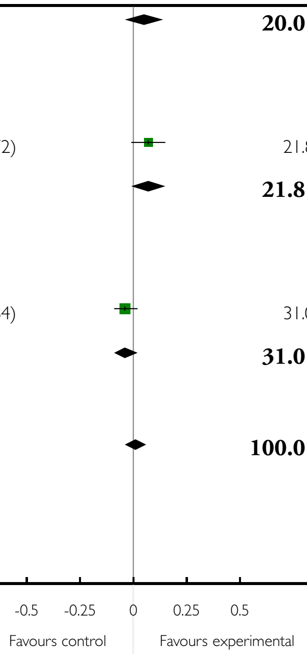
Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
Subtotal (95% CI)	**1251**		**873**			**20.0 %**	**0.05 [-0.04, 0.14]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.14 (P = 0.25)							
4 Total monthly individual income at 72 months (CAD)							
SSP Applicants 2003	1186	1921 (1349.421772)	1185	1832 (1349.421772)		21.8 %	0.07 [-0.01, 0.15]
Subtotal (95% CI)	**1186**		**1185**			**21.8 %**	**0.07 [-0.01, 0.15]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.60 (P = 0.11)							
5 Total monthly individual income (average in 6 months prior to month 54 (CAD)							
SSP Recipients 2002	2453	1311 (778.0964)	2373	1340 (778.0964)		31.0 %	-0.04 [-0.09, 0.02]
Subtotal (95% CI)	**2453**		**2373**			**31.0 %**	**-0.04 [-0.09, 0.02]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.29 (P = 0.20)							
Total (95% CI)	**6075**		**5670**			**100.0 %**	**0.01 [-0.04, 0.06]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 6.58, df = 4 (P = 0.16); I^2 =39%							
Test for overall effect: Z = 0.50 (P = 0.62)							
Test for subgroup differences: Chi^2 = 6.58, df = 4 (P = 0.16), I^2 =39%							



(1) Measure of variation unobtainable from authors

(2) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Analysis 18.2. Comparison 18 Time point 3 Income, Outcome 2 IFIP household income month prior to survey (USD).

IFIP household income month prior to survey (USD)

Study	Group	Intervention	Int n	Control	Cont n	Sig
IFIP 2002	Ongoing	1533	540	1451	273	NS
IFIP 2002	Applicant	1857	442	2110	220	0.05

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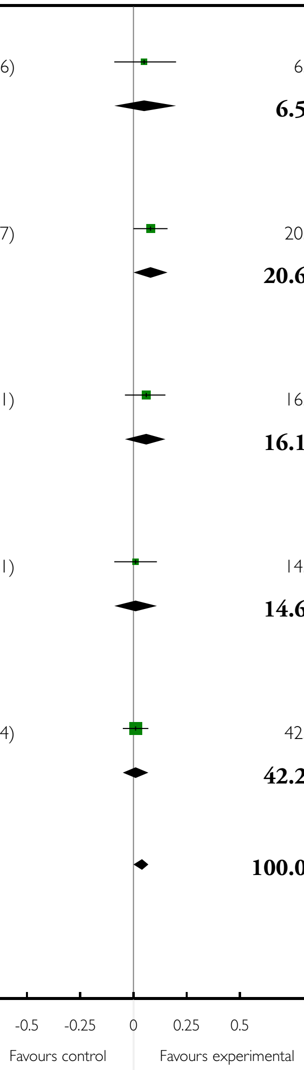
Analysis 18.3. Comparison 18 Time point 3 Income, Outcome 3 Total earnings.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 18 Time point 3 Income

Outcome: 3 Total earnings

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Average earnings year 5 (USD)							
New Hope 1999	366	11324 (9354.036)	379	10824 (9354.036)		6.5 %	0.05 [-0.09, 0.20]
Subtotal (95% CI)	**366**		**379**			**6.5 %**	**0.05 [-0.09, 0.20]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.73 (P = 0.47)							
2 Average earnings year 6 (CAD)							
SSP Applicants 2003	1186	14033 (15752.1567)	1185	12727 (15752.1567)		20.6 %	0.08 [0.00, 0.16]
Subtotal (95% CI)	**1186**		**1185**			**20.6 %**	**0.08 [0.00, 0.16]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.02 (P = 0.044)							
3 Average earnings year 5 (GBP)							
UK ERA 2011	951	6406 (8137.311)	903	5952 (8137.311)		16.1 %	0.06 [-0.04, 0.15]
Subtotal (95% CI)	**951**		**903**			**16.1 %**	**0.06 [-0.04, 0.15]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.20 (P = 0.23)							
4 Earnings month prior to survey, annualised (USD)							
IWRE 2002	819	8140 (9480.391)	860	8040 (9480.391)		14.6 %	0.01 [-0.09, 0.11]
Subtotal (95% CI)	**819**		**860**			**14.6 %**	**0.01 [-0.09, 0.11]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.22 (P = 0.83)							
5 Monthly earnings year 5, quarter 18 (CAD)							
SSP Recipients 2002	2460	496 (835.7934)	2392	488 (835.7934)		42.2 %	0.01 [-0.05, 0.07]
Subtotal (95% CI)	**2460**		**2392**			**42.2 %**	**0.01 [-0.05, 0.07]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.33 (P = 0.74)							
Total (95% CI)	**5782**		**5719**			**100.0 %**	**0.04 [0.00, 0.07]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 2.66, df = 4 (P = 0.62); I^2 =0.0%							
Test for overall effect: Z = 1.88 (P = 0.060)							
Test for subgroup differences: Chi^2 = 2.66, df = 4 (P = 0.62), I^2 =0.0%							



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Analysis 18.4. Comparison 18 Time point 3 Income, Outcome 4 IFIP Average earnings month prior to survey (USD).

IFIP Average earnings month prior to survey (USD)

Study	Group	Intervention	Int n	Control	Cont n	Sig
IFIP 2002	Ongoing	816	540	808	273	NS
IFIP 2002	Applicant	1053	442	1117	220	NS

Analysis 18.5. Comparison 18 Time point 3 Income, Outcome 5 NEWWS T3 Average earnings years 1-5 (USD).

NEWWS T3 Average earnings years 1-5 (USD)

Study	Group	int	N	cont	N	Sig
NEWWS 2001	Atlanta Human Capital Development	22,961	367	20,516	311	NS
NEWWS 2001	Atlanta Labour Force Attachment	23,063	289	20,516	311	NS
NEWWS 2001	Grand Rapids Human Capital Development	23,975	196	23,340	214	NS
NEWWS 2001	Grand Rapids Labour Force Attachment	26,625	214	23,340	214	NS
NEWWS 2001	Riverside Labour Force Attachment	17,342	185	10,805	348	0.01

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Analysis 19.1. Comparison 19 Time point 1 Welfare receipt, Outcome 1 Total AFDC received year 2 (USD).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 19 Time point 1 Welfare receipt

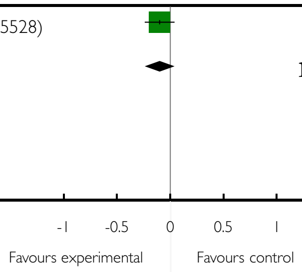
Outcome: 1 Total AFDC received year 2 (USD)

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
New Hope 1999	366	1978 (2280.5528)	378	2207 (2280.5528)		100.0 %	-0.10 [-0.24, 0.04]
Total (95% CI)	**366**		**378**			**100.0 %**	**-0.10 [-0.24, 0.04]**

Heterogeneity: not applicable

Test for overall effect: Z = 1.37 (P = 0.17)

Test for subgroup differences: Not applicable



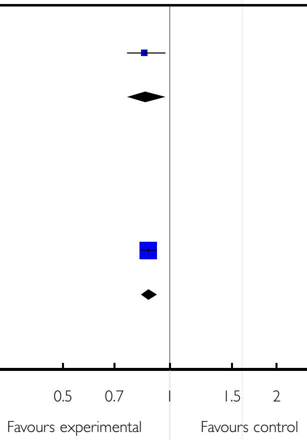
Analysis 19.2. Comparison 19 Time point 1 Welfare receipt, Outcome 2 Proportion of sample receiving welfare (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 19 Time point 1 Welfare receipt

Outcome: 2 Proportion of sample receiving welfare (%)

Study or subgroup	Favours experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 Received social assistance in last 12 months					
Ontario 2001 (1)	113/147	54/60		13.9 %	0.85 [0.76, 0.97]
Subtotal (95% CI)	**147**	**60**		**13.9 %**	**0.85 [0.76, 0.97]**
Total events: 113 (Favours experimental), 54 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 2.53 (P = 0.012)					
2 Currently receiving AFDC					
NEWWS 2001 (2)	948/1554	845/1208		70.8 %	0.87 [0.83, 0.92]
Subtotal (95% CI)	**1554**	**1208**		**70.8 %**	**0.87 [0.83, 0.92]**
Total events: 948 (Favours experimental), 845 (Control)					



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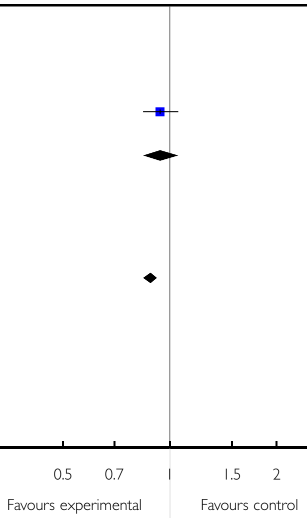
Study or subgroup	Favours experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
Heterogeneity: not applicable					
Test for overall effect: Z = 4.94 (P < 0.00001)					
3 Ever received AFDC/TANF year 2					
New Hope 1999	214/366	235/379		15.3 %	0.94 [0.84, 1.06]
Subtotal (95% CI)	**366**	**379**		**15.3 %**	**0.94 [0.84, 1.06]**
Total events: 214 (Favours experimental), 235 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.98 (P = 0.32)					
Total (95% CI)	**2067**	**1647**		**100.0 %**	**0.88 [0.84, 0.92]**
Total events: 1275 (Favours experimental), 1134 (Control)					
Heterogeneity: Tau^2 = 0.0; Chi^2 = 1.72, df = 2 (P = 0.42); I^2 =0.0%					
Test for overall effect: Z = 5.49 (P < 0.00001)					
Test for subgroup differences: Chi^2 = 1.68, df = 2 (P = 0.43), I^2 =0.0%					



(1) 2 intervention groups in Ontario combined.

(2) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

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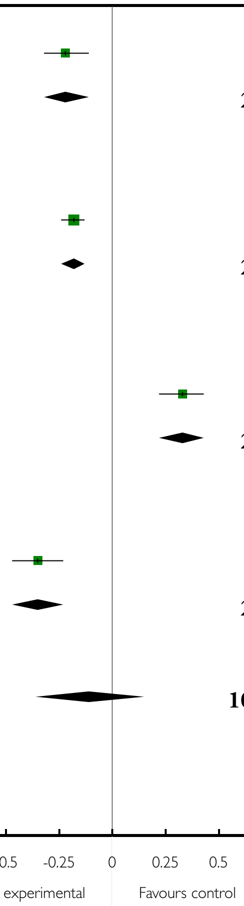
Analysis 20.1. Comparison 20 Time point 2 Welfare receipt, Outcome 1 Average annual welfare benefit.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 20 Time point 2 Welfare receipt

Outcome: 1 Average annual welfare benefit

Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Average annual welfare benefit year 3 (USD)							
CJF 2002 (1)	748	2461 (2301.58)	721	2961 (2301.58)		24.9 %	-0.22 [-0.32, -0.11]
Subtotal (95% CI)	**748**		**721**			**24.9 %**	**-0.22 [-0.32, -0.11]**
Heterogeneity: not applicable							
Test for overall effect: Z = 4.15 (P = 0.000034)							
2 Average Income Assistance year 3 (CAD)							
SSP Recipients 2002	2460	6186 (4979.936)	2392	7090 (4979.936)		25.7 %	-0.18 [-0.24, -0.13]
Subtotal (95% CI)	**2460**		**2392**			**25.7 %**	**-0.18 [-0.24, -0.13]**
Heterogeneity: not applicable							
Test for overall effect: Z = 6.31 (P < 0.00001)							
3 Average annual welfare benefit year 3 (USD)							
MFIP 2000 (2)	991	6237.221 (3266.929)	540	5169.71 (3303.9375)		24.9 %	0.33 [0.22, 0.43]
Subtotal (95% CI)	**991**		**540**			**24.9 %**	**0.33 [0.22, 0.43]**
Heterogeneity: not applicable							
Test for overall effect: Z = 6.05 (P < 0.00001)							
4 Total AFDC/TANF received year 4 (USD)							
FTP 2000	543	317 (1054.06)	565	689 (1054.06)		24.6 %	-0.35 [-0.47, -0.23]
Subtotal (95% CI)	**543**		**565**			**24.6 %**	**-0.35 [-0.47, -0.23]**
Heterogeneity: not applicable							
Test for overall effect: Z = 5.82 (P < 0.00001)							
Total (95% CI)	**4742**		**4218**			**100.0 %**	**-0.11 [-0.36, 0.15]**
Heterogeneity: Tau^2 = 0.06; Chi^2 = 91.08, df = 3 (P<0.00001); I^2 =97%							
Test for overall effect: Z = 0.83 (P = 0.41)							
Test for subgroup differences: Chi^2 = 91.08, df = 3 (P = 0.0), I^2 =97%							



(1) Data from CJF 2002 published in Gennetian and Morris 2003. It is unclear whether 'welfare benefit' includes Food Stamps.

(2) Long term and recent urban MFIP and MFIP IO groups combined. Rural group excluded from analysis due to missing measures of variance.

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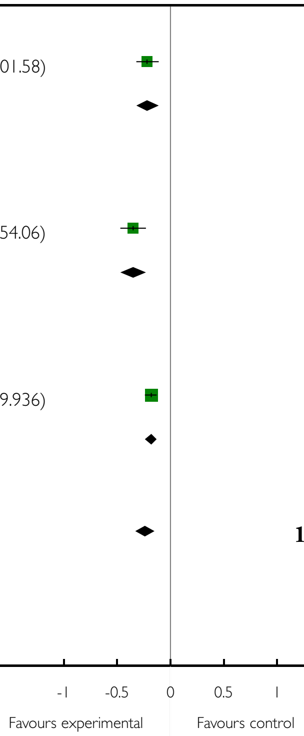
Analysis 20.2. Comparison 20 Time point 2 Welfare receipt, Outcome 2 Average annual welfare benefit excluding MFIP.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 20 Time point 2 Welfare receipt
Outcome: 2 Average annual welfare benefit excluding MFIP

Study or subgroup	Experimental N	Experimental Mean(SD)	Control N	Control Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
1 Average annual welfare benefit year 3 (USD)							
CJF 2002 (1)	748	2461 (2301.58)	721	2961 (2301.58)		31.0 %	-0.22 [-0.32, -0.11]
Subtotal (95% CI)	**748**		**721**			**31.0 %**	**-0.22 [-0.32, -0.11]**
Heterogeneity: not applicable							
Test for overall effect: Z = 4.15 (P = 0.000034)							
2 Total AFDC/TANF received year 4 (USD)							
FTP 2000	543	317 (1054.06)	565	689 (1054.06)		27.6 %	-0.35 [-0.47, -0.23]
Subtotal (95% CI)	**543**		**565**			**27.6 %**	**-0.35 [-0.47, -0.23]**
Heterogeneity: not applicable							
Test for overall effect: Z = 5.82 (P < 0.00001)							
3 Average Income Assistance year 3 (USD/CAD)							
SSP Recipients 2002	2460	6186 (4979.936)	2392	7090 (4979.936)		41.5 %	-0.18 [-0.24, -0.13]
Subtotal (95% CI)	**2460**		**2392**			**41.5 %**	**-0.18 [-0.24, -0.13]**
Heterogeneity: not applicable							
Test for overall effect: Z = 6.31 (P < 0.00001)							
Total (95% CI)	**3751**		**3678**			**100.0 %**	**-0.24 [-0.33, -0.15]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 6.52, df = 2 (P = 0.04); I^2 =69%							
Test for overall effect: Z = 4.96 (P < 0.00001)							
Test for subgroup differences: Chi^2 = 6.52, df = 2 (P = 0.04), I^2 =69%							



(1) Data from CJF 2002 published in Gennetian and Morris 2003. It is unclear whether 'welfare benefit' includes Food Stamps.

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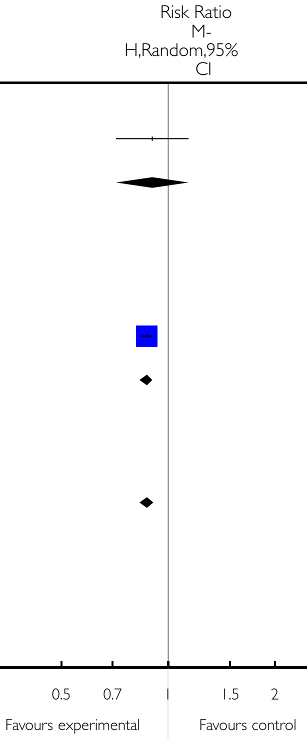
Analysis 20.3. Comparison 20 Time point 2 Welfare receipt, Outcome 3 Proportion of sample receiving welfare.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 20 Time point 2 Welfare receipt

Outcome: 3 Proportion of sample receiving welfare

Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Social assistance/unemployment insurance receipt year 4 (%)				
Ontario 2001 (1)	105/242	56/116	2.9 %	0.90 [0.71, 1.14]
Subtotal (95% CI)	**242**	**116**	**2.9 %**	**0.90 [0.71, 1.14]**
Total events: 105 (Experimental), 56 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 0.88 (P = 0.38)				
2 Income Assistance receipt year 3 (%)				
SSP Recipients 2002	1498/2460	1677/2392	97.1 %	0.87 [0.83, 0.90]
Subtotal (95% CI)	**2460**	**2392**	**97.1 %**	**0.87 [0.83, 0.90]**
Total events: 1498 (Experimental), 1677 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 6.72 (P < 0.00001)				
Total (95% CI)	**2702**	**2508**	**100.0 %**	**0.87 [0.83, 0.91]**
Total events: 1603 (Experimental), 1733 (Control)				
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.08, df = 1 (P = 0.78); I^2 =0.0%				
Test for overall effect: Z = 6.77 (P < 0.00001)				
Test for subgroup differences: Chi^2 = 0.08, df = 1 (P = 0.78), I^2 =0.0%				



(1) 2 intervention groups in Ontario combined.

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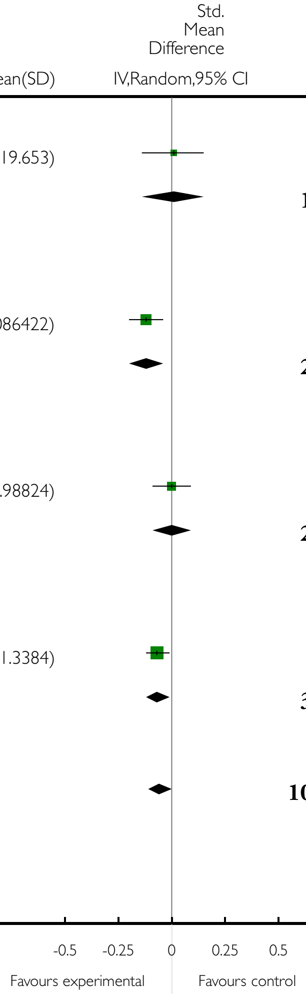
Analysis 21.1. Comparison 21 Time point 3 Welfare receipt, Outcome 1 Total welfare benefit received.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 21 Time point 3 Welfare receipt

Outcome: 1 Total welfare benefit received

Study or subgroup	Experimental N	Experimental Mean(SD)	Control N	Control Mean(SD)	Weight	Std. Mean Difference IV,Random,95% CI
1 Total AFDC/TANF receipt year 5 (USD)						
New Hope 1999 (1)	366	476 (1419.653)	379	466 (1419.653)	11.1 %	0.01 [-0.14, 0.15]
Subtotal (95% CI)	**366**		**379**		**11.1 %**	**0.01 [-0.14, 0.15]**
Heterogeneity: not applicable						
Test for overall effect: Z = 0.10 (P = 0.92)						
2 Average Income Assistance received year 6 (CAD)						
SSP Applicants 2003	1186	1825 (3871.086422)	1185	2280 (3871.086422)	26.6 %	-0.12 [-0.20, -0.04]
Subtotal (95% CI)	**1186**		**1185**		**26.6 %**	**-0.12 [-0.20, -0.04]**
Heterogeneity: not applicable						
Test for overall effect: Z = 2.86 (P = 0.0043)						
3 Average Income Support received per wk (GBP)						
UK ERA 2011	951	21 (0.98824)	903	21 (0.98824)	22.6 %	0.0 [-0.09, 0.09]
Subtotal (95% CI)	**951**		**903**		**22.6 %**	**0.0 [-0.09, 0.09]**
Heterogeneity: not applicable						
Test for overall effect: Z = 0.0 (P = 1.0)						
4 Average Income Assistance received year 5 (CAD)						
SSP Recipients 2002	2460	4934 (4701.3384)	2392	5245 (4701.3384)	39.7 %	-0.07 [-0.12, -0.01]
Subtotal (95% CI)	**2460**		**2392**		**39.7 %**	**-0.07 [-0.12, -0.01]**
Heterogeneity: not applicable						
Test for overall effect: Z = 2.30 (P = 0.021)						
Total (95% CI)	**4963**		**4859**		**100.0 %**	**-0.06 [-0.11, 0.00]**
Heterogeneity: Tau^2 = 0.00; Chi^2 = 4.51, df = 3 (P = 0.21); I^2 =33%						
Test for overall effect: Z = 2.15 (P = 0.032)						
Test for subgroup differences: Chi^2 = 4.51, df = 3 (P = 0.21), I^2 =33%						



(1) For all outcomes, the closest equivalent benefits have been included in the analysis.

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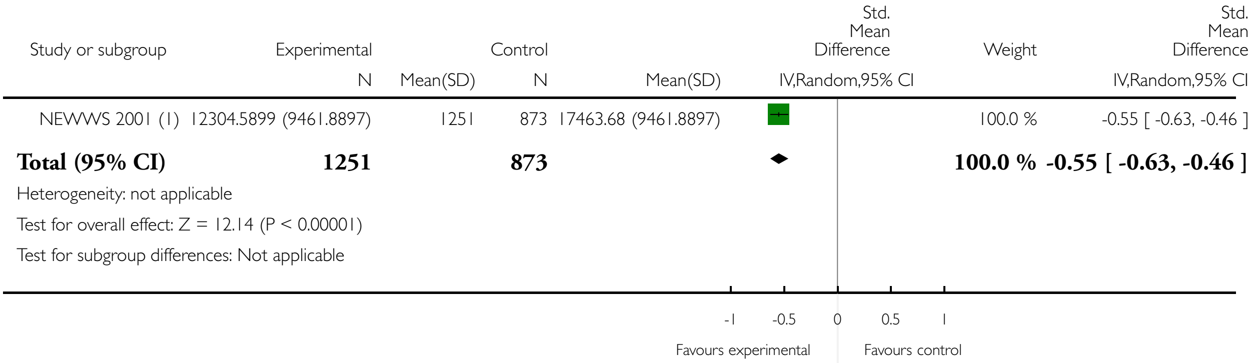
Analysis 21.2. Comparison 21 Time point 3 Welfare receipt, Outcome 2 Total welfare payments years 1-5 (USD).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 21 Time point 3 Welfare receipt
Outcome: 2 Total welfare payments years 1-5 (USD)



Study or subgroup	Experimental N	Mean(SD)	Control N	Mean(SD)	Weight	Std. Mean Difference IV,Random,95% CI
NEWWS 2001 (1)	1251	12304.5899 (9461.8897)	873	17463.68 (9461.8897)	100.0 %	-0.55 [-0.63, -0.46]
Total (95% CI)	**1251**		**873**		**100.0 %**	**-0.55 [-0.63, -0.46]**

Heterogeneity: not applicable
Test for overall effect: Z = 12.14 (P < 0.00001)
Test for subgroup differences: Not applicable

(1) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Analysis 21.3. Comparison 21 Time point 3 Welfare receipt, Outcome 3 IWRE TANF receipt month before survey, annualised year 5 (USD).

IWRE TANF receipt month before survey, annualised year 5 (USD)

Study	**Intervention**	**Int n**	**Control**	**Cont n**	**Total n**	**Sig.**
IWRE 2002	685	819	1082	860	1679	< 0.01

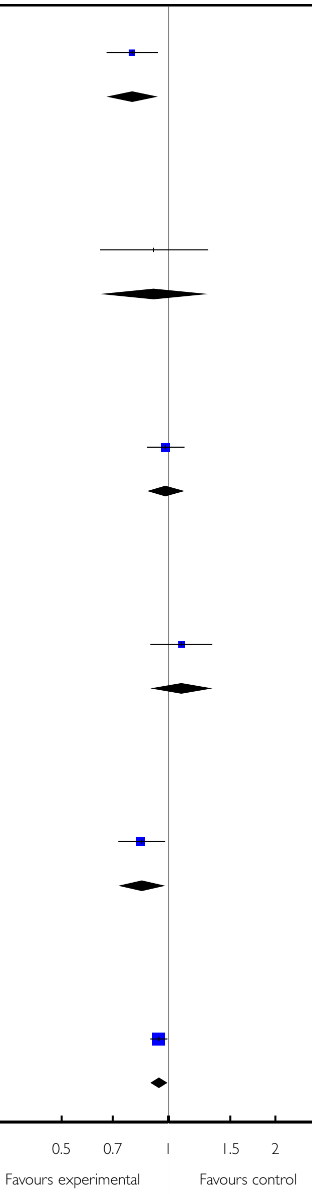
Analysis 21.4. Comparison 21 Time point 3 Welfare receipt, Outcome 4 IFIP Average welfare received month prior to survey (USD).

IFIP Average welfare received month prior to survey (USD)

Study	**Group**	**Intervention**	**Int n**	**Control**	**Cont n**	**Sig.**
IFIP 2002	Ongoing	111	540	103	273	NS
IFIP 2002	Applicant	56	442	34	220	< 0.05

Analysis 21.5. Comparison 21 Time point 3 Welfare receipt, Outcome 5 Proportion of sample receiving welfare.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children
Comparison: 21 Time point 3 Welfare receipt
Outcome: 5 Proportion of sample receiving welfare



Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 Currently receiving TANF (%)					
IWRE 2002	187/819	248/860		14.1 %	0.79 [0.67, 0.93]
Subtotal (95% CI)	**819**	**860**		**14.1 %**	**0.79 [0.67, 0.93]**
Total events: 187 (Experimental), 248 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 2.79 (P = 0.0052)					
2 Ever received AFDC/TANF year 5 (%)					
New Hope 1999	50/366	57/379		4.1 %	0.91 [0.64, 1.29]
Subtotal (95% CI)	**366**	**379**		**4.1 %**	**0.91 [0.64, 1.29]**
Total events: 50 (Experimental), 57 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.54 (P = 0.59)					
3 Currently receiving Income Support or Jobseeker's Allowance (%)					
UK ERA 2011 (1)	331/951	321/903		19.9 %	0.98 [0.87, 1.11]
Subtotal (95% CI)	**951**	**903**		**19.9 %**	**0.98 [0.87, 1.11]**
Total events: 331 (Experimental), 321 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.33 (P = 0.74)					
4 Currently receiving Family Independence Payment (%)					
IFIP 2002 (2)	235/982	108/493		10.5 %	1.09 [0.89, 1.33]
Subtotal (95% CI)	**982**	**493**		**10.5 %**	**1.09 [0.89, 1.33]**
Total events: 235 (Experimental), 108 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.86 (P = 0.39)					
5 Currently receiving Income Assistance (%)					
SSP Applicants 2003	232/1186	275/1185		15.2 %	0.84 [0.72, 0.98]
Subtotal (95% CI)	**1186**	**1185**		**15.2 %**	**0.84 [0.72, 0.98]**
Total events: 232 (Experimental), 275 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 2.16 (P = 0.031)					
6 Currently receiving Income Assistance (%)					
SSP Recipients 2002	1299/2460	1344/2392		36.3 %	0.94 [0.89, 0.99]
Subtotal (95% CI)	**2460**	**2392**		**36.3 %**	**0.94 [0.89, 0.99]**

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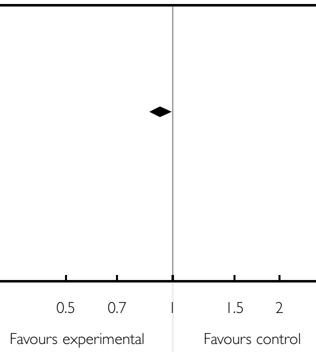


Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
Total events: 1299 (Experimental), 1344 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 2.36 (P = 0.018)				
Total (95% CI)	**6764**	**6212**	**100.0 %**	**0.92 [0.86, 0.99]**
Total events: 2334 (Experimental), 2353 (Control)				
Heterogeneity: Tau^2 = 0.00; Chi^2 = 8.60, df = 5 (P = 0.13); I^2 =42%				
Test for overall effect: Z = 2.10 (P = 0.035)				
Test for subgroup differences: Chi^2 = 8.55, df = 5 (P = 0.13), I^2 =42%				

(1) Proportion receiving IS and JSA reported separately in study and summed by review authors.

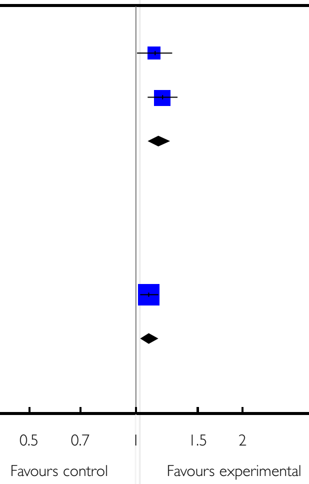
(2) Applicants and recipients groups combined.

Analysis 22.1. Comparison 22 Time point 1 Health insurance, Outcome 1 Respondent has health insurance (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 22 Time point 1 Health insurance

Outcome: 1 Respondent has health insurance (%)



Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Respondent has Medicaid				
CJF GUP 2000	122/144	113/151	41.7 %	1.13 [1.01, 1.27]
CJF Yale 2001	144/157	119/154	58.3 %	1.19 [1.08, 1.31]
Subtotal (95% CI)	**301**	**305**	**100.0 %**	**1.16 [1.08, 1.25]**
Total events: 266 (Experimental), 232 (Control)				
Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.38, df = 1 (P = 0.54); I^2 =0.0%				
Test for overall effect: Z = 3.98 (P = 0.000068)				
2 Respondent had any health insurance since randomisation (24 months)				
New Hope 1999	270/289	257/301	100.0 %	1.09 [1.03, 1.16]
Subtotal (95% CI)	**289**	**301**	**100.0 %**	**1.09 [1.03, 1.16]**
Total events: 270 (Experimental), 257 (Control)				

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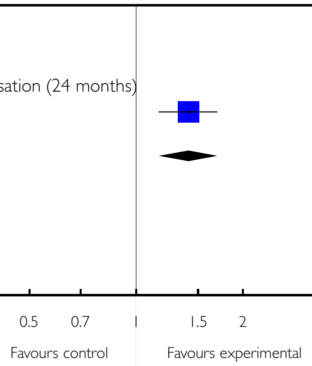
Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
Heterogeneity: not applicable					
Test for overall effect: Z = 3.16 (P = 0.0016)					
3 Respondent ever had employer-provided health insurance since randomisation (24 months)					
NEWWS 2001 (1)	259/1554	144/1208		100.0 %	1.40 [1.16, 1.69]
Subtotal (95% CI)	**1554**	**1208**		**100.0 %**	**1.40 [1.16, 1.69]**
Total events: 259 (Experimental), 144 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 3.47 (P = 0.00052)					



(1) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

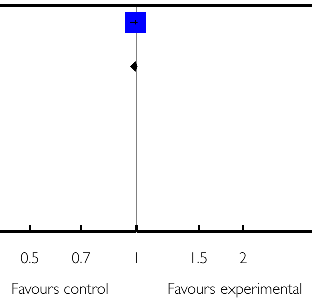
Analysis 22.2. Comparison 22 Time point 1 Health insurance, Outcome 2 Child health insurance (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 22 Time point 1 Health insurance

Outcome: 2 Child health insurance (%)

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
NEWWS 2001 (1)	1395/1554	1098/1208		100.0 %	0.99 [0.96, 1.01]
Total (95% CI)	**1554**	**1208**		**100.0 %**	**0.99 [0.96, 1.01]**
Total events: 1395 (Experimental), 1098 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.00 (P = 0.32)					
Test for subgroup differences: Not applicable					



(1) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

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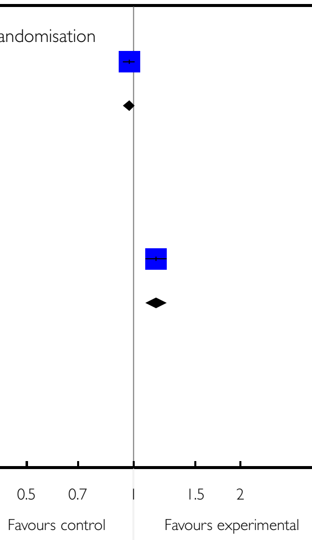
Analysis 23.1. Comparison 23 Time point 2 Health insurance, Outcome 1 Health insurance (%).

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 23 Time point 2 Health insurance

Outcome: 1 Health insurance (%)

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Weight	Risk Ratio M-H,Random,95% CI
1 Respondent has Medicaid or other health insurance within 2-3 years of randomisation					
California GAIN 1994	1041/1276	773/917		100.0 %	0.97 [0.93, 1.01]
Subtotal (95% CI)	**1276**	**917**		**100.0 %**	**0.97 [0.93, 1.01]**
Total events: 1041 (Experimental), 773 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.68 (P = 0.093)					
2 Children have continuous health insurance for past 36 months					
MFIP 2000 (1)	744/991	351/540		100.0 %	1.16 [1.08, 1.24]
Subtotal (95% CI)	**991**	**540**		**100.0 %**	**1.16 [1.08, 1.24]**
Total events: 744 (Experimental), 351 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 3.95 (P = 0.000079)					
Test for subgroup differences: $\mathrm{Chi}^2 = 18.26$, df = 1 (P = 0.00), I^2 =95%					



(1) MFIP and MFIP-IO long term and recent urban groups combined. Not reported for rural groups.

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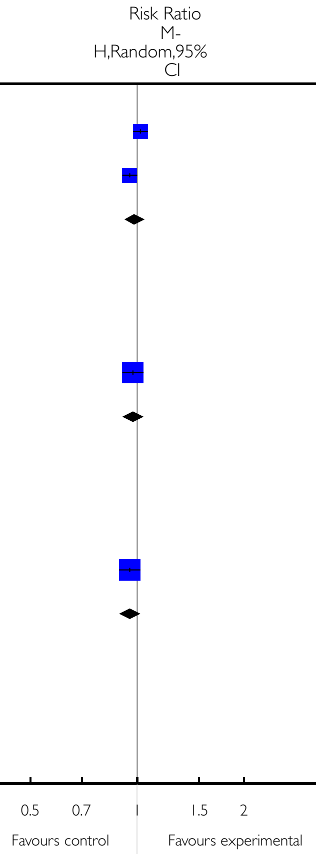
Analysis 24.1. Comparison 24 Time point 3 Health insurance, Outcome 1 Health insurance.

Review: Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children

Comparison: 24 Time point 3 Health insurance

Outcome: 1 Health insurance

Study or subgroup	Experimental n/N	Control n/N	Weight	Risk Ratio M-H,Random,95% CI
1 Family has health insurance (%)				
IFIP 2002 (1)	824/982	406/493	49.6 %	1.02 [0.97, 1.07]
NEWWS 2001 (2)	933/1251	686/873	50.4 %	0.95 [0.91, 1.00]
Subtotal (95% CI)	**2233**	**1366**	**100.0 %**	**0.98 [0.92, 1.05]**
Total events: 1757 (Experimental), 1092 (Control)				
Heterogeneity: Tau^2 = 0.00; Chi^2 = 4.23, df = 1 (P = 0.04); I^2 =76%				
Test for overall effect: Z = 0.48 (P = 0.63)				
2 Respondent has health insurance (%)				
New Hope 1999	242/282	246/279	100.0 %	0.97 [0.91, 1.04]
Subtotal (95% CI)	**282**	**279**	**100.0 %**	**0.97 [0.91, 1.04]**
Total events: 242 (Experimental), 246 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 0.83 (P = 0.41)				
3 All focal children have health insurance (%)				
New Hope 1999	237/282	246/279	100.0 %	0.95 [0.89, 1.02]
Subtotal (95% CI)	**282**	**279**	**100.0 %**	**0.95 [0.89, 1.02]**
Total events: 237 (Experimental), 246 (Control)				
Heterogeneity: not applicable				
Test for overall effect: Z = 1.41 (P = 0.16)				
Test for subgroup differences: Chi^2 = 0.41, df = 2 (P = 0.81), I^2 =0.0%				



(1) Applicants and recipients groups combined.

(2) Impacts for all NEWWS intervention and site subgroups excluding Riverside are combined.

Analysis 25.1. Comparison 25 New Hope 96 months, Outcome 1 Maternal and child health outcomes.

Maternal and child health outcomes

Study	Outcome	Group	Intervention	Int n	Control	Cont n	Total n	P value	Effect size
New Hope 1999	Physical health (mean score) (1-5)	Parents	3.2	NR	3.22	NR	595	0.82	**−0.02**

New Hope 1999	CES-D mean score (0-60)	Parents	17.36	NR	17.33	NR	595	**0.98**	**0.00**
New Hope 1999	Problem Behavior Scale Externalising subscore	Boys	2.34	NR	2.45	NR	570	**0.107**	**−0.15**
New Hope 1999	Problem Behavior Scale Externalising subscore	Girls	2.34	NR	2.3	NR	531	**0.615**	**0.05**
New Hope 1999	Problem Behavior Scale Internalising subscore	Boys	2.29	NR	2.39	NR	570	**0.148**	**−0.15**
New Hope 1999	Problem Behavior Scale Internalising subscore	Girls	2.32	NR	2.35	NR	531	**0.664**	**−0.04**

ADDITIONAL TABLES

Table 1. Primary outcome measures

Primary outcomes	**Reported measures**
Parental**mental health**	Center for Epidemiologic Studies Depression Scale (CES-D), Composite International Diagnostic Interview (CIDI), University of Michigan Composite International Diagnostic Interview (UM-CIDI). Currently unhappy, sad or depressed ’very often’ or ’fairly often’. Miserable or depressed ’often’ or ’always’
Parental**physical health**	5-item self-report health measures; ≥ 1 physical health problem(s)
Child mental health	Behavior Problems Index (BPI), Problem Behavior Scale of the Social Skills Rating System (PBS), the Survey Diagnostic Instrument of the Ontario Child Health Survey (SDI), Child Behavior Checklist

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Table 1. Primary outcome measures (*Continued)*

	(CBCL), Behavior Problems Scale
Child physical health	5-item measures of parent reported health, except for the Self-Sufficiency Project for Applicants (SSP Applicants 2003) and for Recipients (SSP Recipients 2002), which used a 4-item measure with answers given on a 5-point scale and averaged across the 5 items

Table 2. Data extracted in standardised data extraction form

Intervention	Bodies initiating and evaluating intervention
	Hypothesis for mechanisms linking intervention to health
	Location
	Dates
	Political and economic context
	Intervention (and co-intervention if applicable) approach (i.e. HCD/LFA, anti-poverty/caseload reduction)
	Intervention (and co-intervention if applicable) components
	Other implementation or contextual information
Population	Sample demographics (family composition, age, ethnicity)
	Socioeconomic factors (employment status)
	Sample size
Study information	Study duration
	Length of follow-up
	Attrition and non-response
	Final sample size
	Method of adjusting for confounders
	Statistical tests used
	Study limitations
Outcomes	Outcome measures used
	Data collection times

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Table 2. **Data extracted in standardised data extraction form** *(Continued)*

Results	Impacts on outcomes at each follow-up (including all data on statistical tests)
	Impacts on relevant subgroups
Other information	Authors' orientation
	Authors' conclusions
	Policy and research recommendations
	Reviewers' comments

Table 3. **Data collection time points**

Time point	**T1: 18-24 months**		**T2: 25-48 months**		**T3 49-72 months**			**Narrative synthesis**	
Study	**18 months**	**24 months**	**36 months**	**48 months**	**54 months**	**60 months**	**72 months**	**96 months**	**15-17 years**
CJF 2002	-	-	X	-	-	-	-	-	X
CJF GUP 2000	X	-	X	-	-	-	-	-	-
CJF Yale 2001	X	-	-	-	-	-	-	-	-
FTP 2000	-	-	-	X	-	-	-	-	X
California GAIN 1994	-	-	X	-	-	-	-	-	-
IFIP 2002	-	-	-	-	-	X	-	-	-
IWRE 2002	-	-	-	-	-	X	-	-	-
MFIP 2000	-	-	X	-	-	-	-	-	-
New Hope 1999	-	X	-	-	-	X	-	X	-
NEWWS 2001	-	X	-	-	-	X	-	-	-

Table 3. Data collection time points *(Continued)*

Ontario 2001	-	X	-	X	-	-	-	-	-
SSP Applicants 2003	-	-	-	-	-	-	X	-	-
SSP Recipients 2002	-	-	X	-	X	-	-	-	-
UK ERA 2011	-	-	-	-	-	X	-	-	-
Studies (k)	k = 5		k = 6		k = 7			k = 1	k = 2

Table 4. Reported subgroups

Study	Type of subgroup	Subgroup
IFIP 2002	Welfare receipt status	Ongoing/applicant
MFIP 2000	Location	Urban/rural
MFIP 2000	Welfare receipt status	Long-term/recent
MFIP 2000	Intervention	Full intervention/incentives only
NEWWS 2001	Intervention	LFA/HCD
NEWWS 2001	Location	Grand Rapids, Riverside, Atlanta
New Hope 1999	Child age	T1 3-5 years, 6-8 years, 9-12 years
Ontario 2001	Intervention	Full intervention/employment training only
SSP Applicants 2003	Child age	6-8 years, 9-14 years
SSP Recipients 2002	Child age	T2: 3-5 years, 6-11 years, 12-18 years; T3: 5.5-7.5 years, 7.5-9.5 years

Table 5. Definitions of effect magnitude

Cohen's standards	SMD	Odds ratio	Modified approach	SMD	RR
Trivial	< 0.20	< 1.50	**Very small**	< 0.10	1.01-1.19
Small	0.20-0.49	1.50-2.49	**Small**	0.10-0.20	1.20-1.50
Medium	0.50-0.79	2.50-4.29	**Modest**	> 0.20	> 1.50

APPENDICES

Appendix 1. Glossary

AFDC	Aid to Families with Dependent Children (1996 changed to TANF)
AP	Anti-Poverty
California GAIN	California Greater Avenues for Independence
CJF	Connecticut Jobs First
CR	Caseload Reduction
CWIE	Child Waiver Impact Experiments
EITC	Earned Income Tax Credit
FTP	The Family Transition Program
HCD	Human Capital Development
IFIP	Iowa Family Investment Programme
IWRE	The Indiana Welfare Reform Evaluation
JOBS	Job Opportunities and Basic Skills
LFA	Labour Force Attachment
MDRC	Manpower Demonstration Research Corporation (now MDRC)

(*Continued)*

MFIP	Minnesota Family Investment Program
MFIP-IO	Minnesota Family Investment Program (Incentives Only)
New Hope	New Hope for families and children
NEWWS	National Evaluation of Welfare-to-Work Strategies
PRWORA	Personal Responsibility and Work Opportunity Reconciliation Act (1996)
SRDC	The Social Research and Demonstration Corporation
SSP-A	The Self-Sufficiency Project for Applicants
SSP-R	The Self-Sufficiency Project for Recipients
TANF	Temporary Assistance for Needy Families (previously AFDC)

Appendix 2. Search strategy for all databases searched

1. Medline Ovid MEDLINE(R) 1946 to March Week 4 2016 15.4.16

1. (never married adj2 (mother* or father* or parent*)).ab,ti.
2. (separated adj2 (mother* or father* or parent*)).ab,ti.
3. exp Single parent/
4. exp Single-parent-family/
5. fatherless famil*.ab,ti.
6. fragile famil*.ab,ti.
7. lone father*.ab,ti.
8. Lone mother*.ab,ti.
9. Lone parent*.ab,ti.
10. motherless famil*.ab,ti.
11. One parent*.ab,ti.
12. single father*.ab,ti.
13. Single mother*.ab,ti.
14. Single-parent*.ab,ti.
15. sole father*.ab,ti.
16. sole mother*.ab,ti.
17. Sole parent*.ab,ti.
18. sole registrant*.ab,ti.
19. unmarried father*.ab,ti.
20. unmarried mother*.ab,ti.
21. unwed father*.ab,ti.
22. Unwed Mother*.ab,ti.
23. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 13 or 14 or 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22
24.“Canada Health and Social Transfer”.ab,ti.
25.“Personal Responsibility and Work Opportunity Reconciliation Act”.ab,ti.
26.“Active labo?r market polic*”.ab,ti.
27.“Active labo?r market program* ”.ab,ti.

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28. ADFC.ab,ti.
29. “Agenda 2010”.ab,ti.
30. “Aid to Families with Dependent Children”.ab,ti.
31. “Allocation Parent Isole”.ab,ti.
32. “ALMP”.ab,ti.
33. “America Works”.ab,ti.
34. (API and (work* or job* or employ* or train* or vocation*)).ab,ti.
35. “Back-to-work”.ab,ti.
36. “cash benefit*”.ab,ti.
37. “cash incentive*”.ab,ti.
38. “child care assistance”.ab,ti.
39. “child care provision*”.ab,ti.
40. “child care subsid*”.ab,ti.
41. “child care support”.ab,ti.
42. CHST.ab,ti.
43. Community Wage.ab,ti.
44. “Domestic Purposes Benefit”.ab,ti.
45. “Employment Tax Deduction”.ab,ti.
46. “earning disregard*”.ab,ti.
47. employability.ab,ti.
48. Employment.ab,ti.
49. “Employment Program*”.ab,ti.
50. ETD.ab,ti.
51. exp income/
52. exp Public assistance/
53. exp Social security/
54. exp Social welfare/
55. “Family Program*”.ab,ti.
56. “Family Transition Program*”.ab,ti.
57. “financial benefit*”.ab,ti.
58. “financial incentive*”.ab,ti.
59. “financial sanction*”.ab,ti.
60. “Financial support”.ab,ti.
61. Financial support/
62. Financing, Government/
63. FTP.ab,ti.
64. “government intervention*”.ab,ti.
65. “Government program*”.ab,ti.
66. Government Programs/
67. “health care provision*”.ab,ti.
68. “health care subsid*”.ab,ti.
69. “health insurance provision*”.ab,ti.
70. “health insurance subsid*”.ab,ti.
71. “Hilfe zum Arbeit”.ab,ti.
72. “Hilfe zum Lebensunterhalt”.ab,ti.
73. “human capital development”.ab,ti.
74. “income benefit*”.ab,ti.
75. “income incentive*”.ab,ti.
76. “income supplement*”.ab,ti.
77. “Income support”.ab,ti.
78. “Individual Re-integration Agreement”.ab,ti.
79. IRO.ab,ti.
80. Job.ab,ti.

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81. Jobbskatteavdraget.ab,ti.
82. Jobless*.ab,ti.
83. “labo?r force attachment*”.ab,ti.
84. “labo?r force participation”.ab,ti.
85. “Labo?r market activation”.ab,ti.
86. “mandatory employment”.ab,ti.
87. MFIP.ab,ti.
88. “Minnesota Family Investment Program”.ab,ti.
89. “monetary benefit*”.ab,ti.
90. “monetary incentive*”.ab,ti.
91. “monetary support”.ab,ti.
92. ((childcare or child care) adj allowance*).ab,ti.
93. “National Evaluation of Welfare-to work Strategies”.ab,ti.
94. NDLP.ab,ti.
95. “New Deal for Lone Parents”.ab,ti.
96. “New Hope Project”.ab,ti.
97. “Newstart allowance”.ab,ti.
98. NEWWS.ab,ti.
99. “Ontario Works”.ab,ti.
100. Poverty.ab,ti.
101. PRWORA.ab,ti.
102. “public welfare reform*”.ab,ti.
103. (Retrain* or Re-train*).ab,ti.
104. RMI.ab,ti.
105. sanctions.ab,ti.
106. “Self-Sufficiency Project”.ab,ti.
107. “Revenu Minimum d’Insertion”.ab,ti.
108. “Social assistance”.ab,ti.
109. SSP.ab,ti.
110. TANF.ab,ti.
111. “Temporary Assistance for Needy Families”.ab,ti.
112. “time limit*”.ab,ti.
113.Training.ab,ti.
114. Unemployment.ab,ti.
115. Vocation*.ab,ti.
116. Welfare.ab,ti.
117. “work first strateg*”.ab,ti.
118. “Work for your dole”.ab,ti.
119. work*.mp.
120. “Working For Families”.ab,ti.
121. “tax credit*”.ab,ti.
122. 24 or 25 or 26 or 27 or 28 or 29 or 30 or 31 or 32 or 33 or 34 or 35 or 36 or 37 or 38 or 39 or 40 or 41 or 42 or 43 or 44 or 45 or 46 or 47 or 48 or 49 or 50 or 51 or 52 or 53 or 54 or 55 or 56 or 57 or 58 or 59 or 60 or 61 or 62 or 63 or 64 or 65 or 66 or 67 or 68 or 69 or 70 or 71 or 72 or 73 or 74 or 75 or 76 or 77 or 78 or 79 or 80 or 81 or 82 or 83 or 84 or 85 or 86 or 87 or 88 or 89 or 90 or 91 or 92 or 93 or 94 or 95 or 96 or 97 or 98 or 99 or 100 or 101 or 102 or 103 or 104 or 105 or 106 or 107 or 108 or 109 or 110 or 111 or 112 or 113 or 114 or 115 or 116 or 117 or 118 or 119 or 120 or 121
123. randomized controlled trial.pt.
124. controlled clinical trial.pt.
125. randomized.ab.
126. placebo.ab.
127. drug therapy.fs.
128. randomly.ab.
129. trial.ab.

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130. groups.ab.
131. or/123-130
132. exp animals/ not humans.sh.
133. 131 not 132
134. 23 and 122 and 133

2. Embase 1947-Present, updated daily 15.4.16

1. never married adj2 (mother* or father* or parent*)).ab,ti.
2. (separated adj2 (mother* or father* or parent*)).ab,ti.
3. exp Single parent/
4. fatherless famil*.ab,ti.
5. fragile famil*.ab,ti.
6. lone father*.ab,ti.
7. Lone mother*.ab,ti.
8. Lone parent*.ab,ti.
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12. Single mother*.ab,ti.
13. Single-parent*.ab,ti.
14. sole father*.ab,ti. 1
15. sole mother*.ab,ti.
16. Sole parent*.ab,ti.
17. sole registrant*.ab,ti.
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19. unmarried mother*.ab,ti.
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54. “Family Transition Program*”.ab,ti.
55. “financial benefit*”.ab,ti. 1111
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57. “financial sanction*”.ab,ti.
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112. Unemployment.ab,ti.
113. Vocation*.ab,ti.
114. Welfare.ab,ti.
115. “work first strateg*”.ab,ti.
116. “Work for your dole”.ab,ti.
117. work*.mp.
118. “Working For Families”.ab,ti.
119. “tax credit*”.ab,ti.
120. exp “lowest income group”/
121. exp “social care”/
122. “Public assistance”.ab,ti.
123. or/23-122
124. Random*.ab,ti.
125. Factorial*.ab,ti.
126. Crossover*.ab,ti.
127. cross over*.ab,ti.
128. cross-over*.ab,ti.
129 .placebo*.ab,ti.
130. (doubl* adj blind*).ab,ti.
131. (singl* adj blind*).ab,ti.
132. assign*.ab,ti.
133. allocate*.ab,ti.
134. volunteer*.ab,ti.
135 .exp crossover-procedure/
136. exp double-blind procedure/
137. exp randomized controlled trial/
138. exp single-blind procedure/
139. or/124-138
140. 22 and 123 and 139

3. Psycinfo (EBSCOhost) 15.4.16

S1. (DE “Single Parents” OR DE “Single Fathers” OR DE “Single Mothers”) or TX (never married n2 mother*) or TX (never married n2 father*) or TX (never married n2 parent*) or TX (separated n2 mother*) or TX (separated n2 father*) or TX (separated n2 parent*) or TI fatherless famil* or AB fatherless famil* or TI fragile famil* or AB fragile famil* or TI lone father* or AB lone father* or TI Lone mother* or AB Lone mother* or TI Lone parent* or AB Lone parent* or TI never married parent* or AB never married parent* or TI “motherless famil*” OR AB “motherless famil*” OR TI “One parent*” OR AB “One parent*” OR TI “single father*” OR AB “single father*” OR TI “Single mother*” OR AB “Single mother*” OR TI “Single-parent*” OR AB “Single-parent*” OR TI “sole father*” OR AB “sole father*” OR TI “sole mother*” OR AB “sole mother*” OR TI “Sole parent*” OR AB “Sole parent*” OR TI “sole registrant*” OR AB “sole registrant*” OR TI “unmarried father*” OR AB “unmarried father*” OR TI “unmarried mother*” OR AB “unmarried mother*” OR TI “unwed father*” OR AB “unwed father*” OR TI “Unwed Mother*” OR AB “Unwed Mother*”)

S2. DE “Income Level” OR DE “Lower Income Level” OR DE “Middle Income Level” OR DE “Upper Income Level” OR DE “Social Security” or TI “Social security” or AB “Social security” or TI (“Canada Health and Social Transfer”) or AB (“Canada Health and Social Transfer”) or TI (“Personal Responsibility and Work Opportunity Reconciliation Act”) or AB (“Personal Responsibility and

Work Opportunity Reconciliation Act") or TI "Active labo?r market polic*" or AB "Active labo?r market polic*" or TI ADFC or AB ADFC or TI "Agenda 2010" or AB "Agenda 2010" or TI "cash benefit*" or AB "cash benefit*" or TI "cash incentive*" or AB "cash incentive*" or TI "child care assistance" or AB "child care assistance" or TI "child care provision*" or AB "child care provision*" or TI "child care subsid*" or AB "child care subsid*" or TI "child care support" or AB "child care support" or TI "child care support" or AB "child care support" or TI CHST or AB CHST or TI "Community Wage" or AB "Community Wage" or TI "Domestic Purposes Benefit" or AB "Domestic Purposes Benefit" or TI "Employment Tax Deduction" or AB "Employment Tax Deduction" or TI "earning disregard*" or AB "earning disregard*" or TI employability or AB employability or TI Employment or AB Employment or TI "Employment Program*" or AB "Employment Program*" or TI ETD or AB ETD or TI "Family Program*" or AB "Family Program*" or TI "Family Transition Program*" or AB "Family Transition Program*" or TI "financial benefit*" or AB "financial benefit*" or TI "financial incentive*" or AB "financial incentive*" or TI "financial sanction*" or AB "financial sanction*" or TI "Financial support" or AB "Financial support" or TI Financing, Government or AB Financing, Government or TI FTP or AB FTP or TI "government intervention*" or AB "government intervention*" or TI "Government program*" or AB "Government program*" or "Government Programs*" or "Government Programs*" or TI "health care provision*" or AB "health care provision*" or TI "health insurance subsid*" or AB "health insurance subsid*" or TI "Hilfe zum Arbeit" or AB "Hilfe zum Arbeit" or TI "Hilfe zum Lebensunterhalt" or AB "Hilfe zum Lebensunterhalt" or TI "Hilfe zum Lebensunterhalt" or AB "Hilfe zum Lebensunterhalt" or TI "income benefit*" or AB "income benefit*" or TI "income incentive*" or AB "income incentive*" or TI "income supplement*" or AB "income supplement*" or TI "Income support" or AB "Income support" or TI "Individual Re-integration Agreement" or AB "Individual Re-integration Agreement" or TI IRO or AB IRO or TI Job or AB Job or TI Jobbskatteavdraget or AB Jobbskatteavdraget or TI Jobless* or AB Jobless* or TI "labo?r force attachment*" or AB "labo?r force attachment*" or TI "labo?r force participation" or AB "labo?r force participation" or TI "Labo?r market activation" or AB "Labo?r market activation" or TI "mandatory employment" or AB "mandatory employment" or TI MFIP or AB MFIP or TI "Minnesota Family Investment Program" or AB "Minnesota Family Investment Program" or TI "monetary benefit*" or AB "monetary benefit*" or TI "monetary incentive*" or AB "monetary incentive*" or TI "monetary support" or AB "monetary support" or TI "childcare allowance*" or AB "childcare allowance*" or TI "child care allowance*" or AB "child care allowance*" or TI "National Evaluation of Welfare-to work Strategies" or AB "National Evaluation of Welfare-to work Strategies" or TI NDLP or AB NDLP or TI "New Deal for Lone Parents" or AB "New Deal for Lone Parents" or TI "New Hope Project" or AB "New Hope Project" or TI "Newstart allowance" or AB "Newstart allowance" or TI NEWWS or AB NEWWS or TI "Ontario Works" or AB "Ontario Works" or TI Poverty or AB Poverty or TI PRWORA or AB PRWORA or TI "public welfare reform*" or AB "public welfare reform*" or TI Retrain* or AB Retrain* or TI Re-train* or AB Re-train* or TI RMI or AB RMI or TI sanctions or AB sanctions or TI "Self-Sufficiency Project" or AB "Self-Sufficiency Project" or TI "Revenu Minimum d'Insertion" or AB "Revenu Minimum d'Insertion" or TI "Social assistance" or AB "Social assistance" or TI SSP or AB SSP or TI TANF or AB TANF or TI "Temporary Assistance for Needy Families" or AB "Temporary Assistance for Needy Families" or TI "time limit*" or AB "time limit*" or TI Training or AB Training or TI Unemployment or AB Unemployment or TI Vocation* or AB Vocation* or TI Welfare or AB Welfare or TI "work first strateg*" or AB "work first strateg*" or TI work* or AB work* or TI "Working For Families" or AB "Working For Families" or TI "tax credit*" or AB "tax credit*" Or TI "Public assistance" or TI "Social welfare" Or AB "Public assistance" or AB "Social welfare"

S3. quasi-random* or randomi?ed controlled trial or controlled clinical trial or clinical trial or trial or random*

S4. S1 AND S2 AND S3

4. ERIC (EBSCOhost) 15.4.16

S1. SU("one parent family") OR ("fatherless family") OR TI("single parent*") OR AB("single parent*") OR TI("unmarried father*") OR AB("unmarried father*") OR TI("never married father*") OR AB("never married father*") OR TI("never married mother*") OR AB("never married mother*") OR TI("never married parent*") OR AB("never married parent*") OR TI("separated mother*") OR AB("separated mother*") OR TI("separated father*") OR AB("separated father*") OR TI("Single-parent-family") OR AB("Single-parent-family") OR TI("fatherless famil*") OR AB("fatherless famil*") OR TI("fragile famil*") OR AB("fragile famil*") OR TI("lone father*") OR AB("lone father*") OR TI("Lone mother*") OR AB("Lone mother*") OR TI("motherless famil*") OR AB("motherless famil*") OR TI("One parent*") OR AB("One parent*") OR TI("single father*") OR AB("single father*") OR TI("Single mother*") OR AB("Single mother*") OR TI("Single-parent*") OR AB("Single-parent*") OR TI("sole father*") OR AB("sole father*") OR TI("sole mother*") OR AB("sole mother*") OR TI("Sole parent*") OR AB("Sole parent*") OR TI("sole registrant*") OR AB("sole registrant*") OR TI("unmarried father*") OR AB("unmarried father*") OR TI("unmarried mother*") OR AB("unmarried mother*") OR TI("unwed father*") OR AB("unwed father*") OR TI("Unwed Mother*") OR AB("Unwed Mother*")

S2. SU("welfare services") OR SU(income) OR SU(" family income ") OR SU(" guaranteed income ") OR SU(salaries) OR SU(" teacher salaries ") OR SU(" merit pay ") OR SU(wages) OR SU(" minimum wage ") OR SU(" low income") OR TI("Canada Health and Social Transfer") OR AB("Canada Health and Social Transfer") OR TI("Personal Responsibility and Work Opportunity Reconciliation Act") OR AB("Personal Responsibility and Work Opportunity Reconciliation Act") OR TI("Active labo?r market polic*") OR AB("Active labo?r market polic*") OR TI("Active labo?r market program*") OR AB("Active labo?r market program* ") OR TI(ADFC)

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OR AB(ADFC) OR TI(“ Agenda 2010”) OR AB(“Agenda 2010”) OR TI(“Aid to Families with Dependent Children”) OR AB(“Aid to Families with Dependent Children”) OR TI(“Allocation Parent Isole”) OR AB(“Allocation Parent Isole”) OR TI(ALMP) OR AB(ALMP) OR TI(“America Works”) OR AB(“America Works ”) OR TI(API) OR AB(API) OR TI(“ Back-to-work”) OR AB(“Back-to-work”) OR TI(“cash benefit*”) OR AB(“cash benefit*”) OR TI(“cash incentive*”) OR AB(“cash incentive*”) OR TI(“child care assistance”) OR AB(“child care assistance”) OR TI(“child care provision*”) OR AB(“child care provision*”) OR TI(“child care subsid*”) OR AB(“child care subsid*”) OR TI(“child care support”) OR AB(“child care support ”) OR TI(CHST) OR AB(CHST) OR TI(Community Wage) OR AB(“Community Wage”) OR TI(“ Domestic Purposes Benefit”) OR AB(“Domestic Purposes Benefit”) OR TI(“Employment Tax Deduction”) OR AB(“Employment Tax Deduction”) OR TI(“earning disregard*”) OR AB(“earning disregard* ”) OR TI(employability) OR AB(employability) OR TI(Employment) OR AB(Employment) OR TI(“ Employment Program*”) OR AB(“Employment Program* ”) OR TI(ETD) OR AB(ETD) OR TI(income) OR AB(income) OR TI(Public assistance) OR AB(Public assistance) OR TI(Social security) OR AB(Social security) OR TI(Social welfare) OR AB(Social welfare) OR TI(“ Family Program*”) OR AB(“Family Program*”) OR TI(“Family Transition Program*”) OR AB(“Family Transition Program*”) OR TI(“financial benefit*”) OR AB(“financial benefit*”) OR TI(“financial incentive*”) OR AB(“financial incentive*”) OR TI(“financial sanction*”) OR AB(“financial sanction*”) OR TI(“Financial support”) OR AB(“Financial support ”) OR TI(Financial support) OR AB(Financial support) OR TI(Financing, Government) OR AB(Financing, Government) OR TI(FTP) OR AB(FTP) OR TI(“ government intervention*”) OR AB(“government intervention*”) OR TI(“Government program*”) OR AB(“Government program* ”) OR TI(Government Programs) OR AB(Government Programs) OR TI(“ health care provision*”) OR AB(“health care provision*”) OR TI(“health care subsid*”) OR AB(“health care subsid*”) OR TI(“health insurance provision*”) OR AB(“health insurance provision*”) OR TI(“health insurance subsid*”) OR AB(“health insurance subsid*”) OR TI(“Hilfe zum Arbeit”) OR AB(“Hilfe zum Arbeit”) OR TI(“Hilfe zum Lebensunterhalt”) OR AB(“Hilfe zum Lebensunterhalt”) OR TI(“human capital development”) OR AB(“human capital development”) OR TI(“income benefit*”) OR AB(“income benefit*”) OR TI(“income incentive*”) OR AB(“income incentive*”) OR TI(“income supplement*”) OR AB(“income supplement*”) OR TI(“Income support”) OR AB(“Income support”) OR TI(“Individual Re-integration Agreement”) OR AB(“Individual Re-integration Agreement ”) OR TI(IRO) OR AB(IRO) OR TI(Job) OR AB(Job) OR TI(Jobbskatteavdraget) OR AB(Jobbskatteavdraget) OR TI(Jobless*) OR AB(Jobless*) OR TI(“ labo?r force attachment*”) OR AB(“labo?r force attachment*”) OR TI(“labo?r force participation”) OR AB(“labo?r force participation”) OR TI(“Labo?r market activation”) OR AB(“Labo?r market activation”) OR TI(“mandatory employment”) OR AB(“mandatory employment ”) OR TI(MFIP) OR AB(MFIP) OR TI(“ Minnesota Family Investment Program”) OR AB(“Minnesota Family Investment Program”) OR TI(“monetary benefit*”) OR AB(“monetary benefit*”) OR TI(“monetary incentive*”) OR AB(“monetary incentive*”) OR TI(“monetary support”) OR AB(“monetary support”) OR TI(“childcare allowance*”) OR AB(“childcare allowance*”) OR TI(“child care allowance*”) OR AB(“child care allowance*”) OR AB(“National Evaluation of Welfare-to work Strategies”) OR AB(“National Evaluation of Welfare-to work Strategies”) OR TI(NDLP) OR AB(NDLP) OR TI(“New Deal for Lone Parents”) OR AB(“New Deal for Lone Parents”) OR TI(“New Hope Project”) OR AB(“New Hope Project”) OR TI(“Newstart allowance”) OR AB(“Newstart allowance”) OR TI(NEWWS) OR AB(NEWWS) OR TI(“Ontario Works”) OR AB(“Ontario Works”) OR TI(Poverty) OR AB(Poverty) OR TI(PRWORA) OR AB(PRWORA) OR TI(“public welfare reform*”) OR AB(“public welfare reform*”) OR TI(Retrain*) OR TI(Re-train*) OR AB(Retrain*) OR AB(Re-train*) OR TI(RMI) OR AB(RMI) OR TI(sanctions) OR AB(sanctions) OR TI(“Self-Sufficiency Project”) OR AB(“Self-Sufficiency Project”) OR TI(“Revenu Minimum d’Insertion”) OR AB(“Revenu Minimum d’Insertion”) OR TI(“Social assistance”) OR AB(“Social assistance”) OR TI(SSP) OR AB(SSP) OR TI(TANF) OR AB(TANF) OR TI(“Temporary Assistance for Needy Families”) OR AB(“Temporary Assistance for Needy Families”) OR TI(“time limit*”) OR AB(“time limit*”) OR TI(Training) OR AB(Training) OR TI(Unemployment) OR AB(Unemployment) OR TI(Vocation*) OR AB(Vocation*) OR TI(Welfare) OR AB(Welfare) OR TI(“work first strateg*”) OR AB(“work first strateg*”) OR TI(“Work for your dole”) OR AB(“Work for your dole”) OR TI(work*) OR AB(work*) OR TI(“Working For Families”) OR AB(“Working For Families”) OR TI(“tax credit*”) OR AB(“tax credit*”)

S3. TI(“quasi-random*”) OR TI(“randomi?ed controlled trial”) OR TI(“controlled clinical trial”) OR AB(“quasi-random*”) OR AB(“randomi?ed controlled trial”) OR AB(“controlled clinical trial”) OR TI(“clinical trial”) OR TI(trial) OR TI(random*) OR AB(“clinical trial”) OR AB(trial) OR AB(random*)

S4. S1 AND S2 AND S3

5. Socindex (EBSCOhost) 15.4.16

S1. ((DE “PUBLIC welfare” OR DE “ALMSHOUSES” OR DE “ASYLUMS” OR DE “CHILD welfare” OR DE “COMMUNITY organization” OR DE “FAITH-based initiative (Government program)” OR DE “FOOD stamps” OR DE “FRESH-air charity” OR DE “INCOME maintenance programs” OR DE “INSTITUTIONAL care” OR DE “LEGAL assistance to the poor” OR DE “MATERNALISM (Public welfare)” OR DE “MILITARY social work” OR DE “NATIONAL service” OR DE “SOCIAL medicine” OR DE “SOCIAL service, Rural” OR DE “TRANSIENTS, Relief of” OR DE “WELFARE fraud” OR DE “WELFARE state”) OR (DE “SOCIAL security” OR DE “WORKERS’ compensation”)) OR (DE “FAMILY policy” OR DE “CHILD welfare”) or TI “Social

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security” or AB “Social security” or TI (“Canada Health and Social Transfer”) or AB (“Canada Health and Social Transfer”) or TI (“Personal Responsibility and Work Opportunity Reconciliation Act”) or AB (“Personal Responsibility and Work Opportunity Reconciliation Act”) or TI “Active labo?r market polic*” or AB “Active labo?r market polic*” or TI ADFC or AB ADFC or TI “Agenda 2010” or AB “Agenda 2010” or TI “cash benefit*” or AB “cash benefit*” or TI “cash incentive*” or AB “cash incentive*” or TI “child care assistance” or AB “child care assistance” or TI “child care provision*” or AB “child care provision*” or TI “child care subsid*” or AB “child care subsid*” or TI “child care support” or AB “child care support” or TI “child care support” or AB “child care support” or TI CHST or AB CHST or TI “Community Wage” or AB “Community Wage” or TI “Domestic Purposes Benefit” or AB “Domestic Purposes Benefit” or TI “Employment Tax Deduction” or AB “Employment Tax Deduction” or TI “earning disregard*” or AB “earning disregard*” or TI employability or AB employability or TI Employment or AB Employment or TI “Employment Program*” or AB “Employment Program*” or TI ETD or AB ETD or TI “Family Program*” or AB “Family Program*” or TI “Family Transition Program*” or AB “Family Transition Program*” or TI “financial benefit*” or AB “financial benefit*” or TI “financial incentive*” or AB “financial incentive*” or TI “financial sanction*” or AB “financial sanction*” or TI “Financial support” or AB “Financial support” or TI Financing, Government or AB Financing, Government or TI FTP or AB FTP or TI “government intervention*” or AB “government intervention*” or TI “Government program*” or AB “Government program*” or “Government Programs*” or “Government Programs*” or TI “health care provision*” or AB “health care provision*” or TI “health insurance subsid*” or AB “health insurance subsid*” or TI “Hilfe zum Arbeit” or AB “Hilfe zum Arbeit” or TI “Hilfe zum Lebensunterhalt” or AB “Hilfe zum Lebensunterhalt” or TI “Hilfe zum Lebensunterhalt” or AB “Hilfe zum Lebensunterhalt” or TI “income benefit*” or AB “income benefit*” or TI “income incentive*” or AB “income incentive*” or TI “income supplement*” or AB “income supplement*” or TI “Income support” or AB “Income support” or TI “Individual Re-integration Agreement” or AB “Individual Re-integration Agreement” or TI IRO or AB IRO or TI Job or AB Job or TI Jobbskatteavdraget or AB Jobbskatteavdraget or TI Jobless* or AB Jobless* or TI “labo?r force attachment*” or AB “labo?r force attachment*” or TI “labo?r force participation” or AB “labo?r force participation” or TI “Labo?r market activation” or AB “Labo?r market activation” or TI “mandatory employment” or AB “mandatory employment” or TI MFIP or AB MFIP or TI “Minnesota Family Investment Program” or AB “Minnesota Family Investment Program” or TI “monetary benefit*” or AB “monetary benefit*” or TI “monetary incentive*” or AB “monetary incentive*” or TI “monetary support” or AB “monetary support” or TI “childcare allowance*” or AB “childcare allowance*” or TI “child care allowance*” or AB “child care allowance*” or TI “National Evaluation of Welfare-to work Strategies” or AB “National Evaluation of Welfare-to work Strategies” or TI NDLP or AB NDLP or TI “New Deal for Lone Parents” or AB “New Deal for Lone Parents” or TI “New Hope Project” or AB “New Hope Project” or TI “Newstart allowance” or AB “Newstart allowance” or TI NEWWS or AB NEWWS or TI “Ontario Works” or AB “Ontario Works” or TI Poverty or AB Poverty or TI PRWORA or AB PRWORA or TI “public welfare reform*” or AB “public welfare reform*” or TI Retrain* or AB Retrain* or TI Re-train* or AB Re-train* or TI RMI or AB RMI or TI sanctions or AB sanctions or TI “Self-Sufficiency Project” or AB “Self-Sufficiency Project” or TI “Revenu Minimum d’Insertion” or AB “Revenu Minimum d’Insertion” or TI “Social assistance” or AB “Social assistance” or TI SSP or AB SSP or TI TANF or AB TANF or TI “Temporary Assistance for Needy Families” or AB “Temporary Assistance for Needy Families” or TI “time limit*” or AB “time limit*” or TI Training or AB Training or TI Unemployment or AB Unemployment or TI Vocation* or AB Vocation* or TI Welfare or AB Welfare or TI “work first strateg*” or AB “work first strateg*” or TI work* or AB work* or TI “Working For Families” or AB “Working For Families” or TI “tax credit*” or AB “tax credit*” Or TI “Public assistance” or TI “Social welfare” Or AB “Public assistance” or AB “Social welfare”

S2. TX (never married n2 mother*) or TX (never married n2 father*) or TX (never married n2 parent*) or TX (separated n2 mother*) or TX (separated n2 father*) or TX (separated n2 parent*) or TI fatherless famil* or AB fatherless famil* or TI fragile famil* or AB fragile famil* or TI lone father* or AB lone father* or TI Lone mother* or AB Lone mother* or TI Lone parent* or AB Lone parent* or TI never married parent* or AB never married parent* or TI “motherless famil*” OR AB “motherless famil*” OR TI “One parent*” OR AB “One parent*” OR TI “single father*” OR AB “single father*” OR TI “Single mother*” OR AB “Single mother*” OR TI “Single-parent*” OR AB “Single-parent*” OR TI “sole father*” OR AB “sole father*” OR TI “sole mother*” OR AB “sole mother*” OR TI “Sole parent*” OR AB “Sole parent*” OR TI “sole registrant*” OR AB “sole registrant*” OR TI “unmarried father*” OR AB “unmarried father*” OR TI “unmarried mother*” OR AB “unmarried mother*” OR TI “unwed father*” OR AB “unwed father*” OR TI “Unwed Mother*” OR AB “Unwed Mother*”

S3. quasi-random* or randomi?ed controlled trial or controlled clinical trial or clinical trial or trial or random*

S4. S1 AND S2 AND S3

6. CINAHL (EBSCOhost) 15.4.16

S1. (MH “Income”) or (MH “Public Assistance+”) or (MH “Social Welfare+”) or TI “Social security” or AB “Social security” or TI (“Canada Health and Social Transfer”) or AB (“Canada Health and Social Transfer”) or TI (“Personal Responsibility and Work Opportunity Reconciliation Act”) or AB (“Personal Responsibility and Work Opportunity Reconciliation Act”) or TI “Active labo?r market polic*” or AB “Active labo?r market polic*” or TI ADFC or AB ADFC or TI “Agenda 2010” or AB “Agenda 2010” or TI “Social security” or AB “Social security” or TI (“Canada Health and Social Transfer”) or AB (“Canada Health and Social

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Transfer") or TI ("Personal Responsibility and Work Opportunity Reconciliation Act") or AB ("Personal Responsibility and Work Opportunity Reconciliation Act") or TI "Active labo?r market polic*" or AB "Active labo?r market polic*" or TI ADFC or AB ADFC or TI "Agenda 2010" or AB "Agenda 2010" or TI "cash benefit*" or AB "cash benefit*" or TI "cash incentive*" or AB "cash incentive*" or TI "child care assistance" or AB "child care assistance" or TI "child care provision*" or AB "child care provision*" or TI "child care subsid*" or AB "child care subsid*" or TI "child care support" or AB "child care support" or TI "child care support" or AB "child care support" or TI CHST or AB CHST or TI "Community Wage" or AB "Community Wage" or TI "Domestic Purposes Benefit" or AB "Domestic Purposes Benefit" or TI "Employment Tax Deduction" or AB "Employment Tax Deduction" or TI "earning disregard*" or AB "earning disregard*" or TI employability or AB employability or TI Employment or AB Employment or TI "Employment Program*" or AB "Employment Program*" or TI ETD or AB ETD or TI "Family Program*" or AB "Family Program*" or TI "Family Transition Program*" or AB "Family Transition Program*" or TI "financial benefit*" or AB "financial benefit*" or TI "financial incentive*" or AB "financial incentive*" or TI "financial sanction*" or AB "financial sanction*" or TI "Financial support" or AB "Financial support" or TI Financing, Government or AB Financing, Government or TI FTP or AB FTP or TI "government intervention*" or AB "government intervention*" or TI "Government program*" or AB "Government program*" or "Government Programs*" or "Government Programs*" or TI "health care provision*" or AB "health care provision*" or TI "health insurance subsid*" or AB "health insurance subsid*" or TI "Hilfe zum Arbeit" or AB "Hilfe zum Arbeit" or TI "Hilfe zum Lebensunterhalt" or AB "Hilfe zum Lebensunterhalt" or TI "Hilfe zum Lebensunterhalt" or AB "Hilfe zum Lebensunterhalt" or TI "income benefit*" or AB "income benefit*" or TI "income incentive*" or AB "income incentive*" or TI "income supplement*" or AB "income supplement*" or TI "Income support" or AB "Income support" or TI "Individual Re-integration Agreement" or AB "Individual Re-integration Agreement" or TI IRO or AB IRO or TI Job or AB Job or TI Jobbskatteavdraget or AB Jobbskatteavdraget or TI Jobless* or AB Jobless* or TI "labo?r force attachment*" or AB "labo?r force attachment*" or TI "labo?r force participation" or AB "labo?r force participation" or TI "Labo?r market activation" or AB "Labo?r market activation" or TI "mandatory employment" or AB "mandatory employment" or TI MFIP or AB MFIP or TI "Minnesota Family Investment Program" or AB "Minnesota Family Investment Program" or TI "monetary benefit*" or AB "monetary benefit*" or TI "monetary incentive*" or AB "monetary incentive*" or TI "monetary support" or AB "monetary support" or TI "childcare allowance*" or AB "childcare allowance*" or TI "child care allowance*" or AB "child care allowance*" or TI "National Evaluation of Welfare-to work Strategies" or AB "National Evaluation of Welfare-to work Strategies" or TI NDLP or AB NDLP or TI "New Deal for Lone Parents" or AB "New Deal for Lone Parents" or TI "New Hope Project" or AB "New Hope Project" or TI "Newstart allowance" or AB "Newstart allowance" or TI NEWWS or AB NEWWS or TI "Ontario Works" or AB "Ontario Works" or TI Poverty or AB Poverty or TI PRWORA or AB PRWORA or TI "public welfare reform*" or AB "public welfare reform*" or TI Retrain* or AB Retrain* or TI Re-train* or AB Re-train* or TI RMI or AB RMI or TI sanctions or AB sanctions or TI "Self-Sufficiency Project" or AB "Self-Sufficiency Project" or TI "Revenu Minimum d'Insertion" or AB "Revenu Minimum d'Insertion" or TI "Social assistance" or AB "Social assistance" or TI SSP or AB SSP or TI TANF or AB TANF or TI "Temporary Assistance for Needy Families" or AB "Temporary Assistance for Needy Families" or TI "time limit*" or AB "time limit*" or TI Training or AB Training or TI Unemployment or AB Unemployment or TI Vocation* or AB Vocation* or TI Welfare or AB Welfare or TI "work first strateg*" or AB "work first strateg*" or TI work* or AB work* or TI "Working For Families" or AB "Working For Families" or TI "tax credit*" or AB "tax credit*"

S2. TX (never married n2 mother*) or TX (never married n2 father*) or TX (never married n2 parent*) or TX (separated n2 mother*) or TX (separated n2 father*) or TX (separated n2 parent*) or TI fatherless famil* or AB fatherless famil* or TI fragile famil* or AB fragile famil* or TI lone father* or AB lone father* or TI Lone mother* or AB Lone mother* or TI Lone parent* or AB Lone parent* or TI never married parent* or AB never married parent* or TI "motherless famil*" OR AB "motherless famil*" OR TI "One parent*" OR AB "One parent*" OR TI "single father*" OR AB "single father*" OR TI "Single mother*" OR AB "Single mother*" OR TI "Single-parent*" OR AB "Single-parent*" OR TI "sole father*" OR AB "sole father*" OR TI "sole mother*" OR AB "sole mother*" OR TI "Sole parent*" OR AB "Sole parent*" OR TI "sole registrant*" OR AB "sole registrant*" OR TI "unmarried father*" OR AB "unmarried father*" OR TI "unmarried mother*" OR AB "unmarried mother*" OR TI "unwed father*" OR AB "unwed father*" OR TI "Unwed Mother*" OR AB "Unwed Mother*"

S3. MH "Quantitative Studies" or MH "Clinical Trials+" or MH "Placebos" or MH "randomisation" or TX allocat* random* or TX placebo* or TX random* allocat* or TX randomi* control* trial* or TX (singl* n1 blind*) or TX (singl* n1 mask*) or TX (doubl* n1 blind*) or TX (doubl* n1 mask*) or TX (tripl* n1 blind*) or TX (tripl* n1 mask*) or TX (trebl* n1 blind*) or TX (trebl* n1 mask*)

S4. S1 AND S2 AND S3

7. Econlit (EBSCOhost) 15.4.16

S1. (ZU "social security") or (ZU "social security and public pensions") or (ZU "welfare and poverty: general") or (ZU "welfare and poverty: government programs; provision and effects of welfare programs") or (ZU "welfare and poverty: other") or (ZU "welfare economics: general") or TI "Social security" or AB "Social security" or TI ("Canada Health and Social Transfer") or AB ("Canada Health and Social Transfer") or TI ("Personal Responsibility and Work Opportunity Reconciliation Act") or AB ("Personal Responsibility

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and Work Opportunity Reconciliation Act”) or TI “Active labo?r market polic*” or AB “Active labo?r market polic*” or TI ADFC or AB ADFC or TI “Agenda 2010” or AB “Agenda 2010” or TI “cash benefit*” or AB “cash benefit*” or TI “cash incentive*” or AB “cash incentive*” or TI “child care assistance” or AB “child care assistance” or TI “child care provision*” or AB “child care provision*” or TI “child care subsid*” or AB “child care subsid*” or TI “child care support” or AB “child care support” or TI “child care support” or AB “child care support” or TI CHST or AB CHST or TI “Community Wage” or AB “Community Wage” or TI “Domestic Purposes Benefit” or AB “Domestic Purposes Benefit” or TI “Employment Tax Deduction” or AB “Employment Tax Deduction” or TI “earning disregard*” or AB “earning disregard*” or TI employability or AB employability or TI Employment or AB Employment or TI “Employment Program*” or AB “Employment Program*” or TI ETD or AB ETD or TI “Family Program*” or AB “Family Program*” or TI “Family Transition Program*” or AB “Family Transition Program*” or TI “financial benefit*” or AB “financial benefit*” or TI “financial incentive*” or AB “financial incentive*” or TI “financial sanction*” or AB “financial sanction*” or TI “Financial support” or AB “Financial support” or TI Financing, Government or AB Financing, Government or TI FTP or AB FTP or TI “government intervention*” or AB “government intervention*” or TI “Government program*” or AB “Government program*” or “Government Programs*” or “Government Programs*” or TI “health care provision*” or AB “health care provision*” or TI “health insurance subsid*” or AB “health insurance subsid*” or TI “Hilfe zum Arbeit” or AB “Hilfe zum Arbeit” or TI “Hilfe zum Lebensunterhalt” or AB “Hilfe zum Lebensunterhalt” or TI “Hilfe zum Lebensunterhalt” or AB “Hilfe zum Lebensunterhalt” or TI “income benefit*” or AB “income benefit*” or TI “income incentive*” or AB “income incentive*” or TI “income supplement*” or AB “income supplement*” or TI “Income support” or AB “Income support” or TI “Individual Re-integration Agreement” or AB “Individual Re-integration Agreement” or TI IRO or AB IRO or TI Job or AB Job or TI Jobbskatteavdraget or AB Jobbskatteavdraget or TI Jobless* or AB Jobless* or TI “labo?r force attachment*” or AB “labo?r force attachment*” or TI “labo?r force participation” or AB “labo?r force participation” or TI “Labo?r market activation” or AB “Labo?r market activation” or TI “mandatory employment” or AB “mandatory employment” or TI MFIP or AB MFIP or TI “Minnesota Family Investment Program” or AB “Minnesota Family Investment Program” or TI “monetary benefit*” or AB “monetary benefit*” or TI “monetary incentive*” or AB “monetary incentive*” or TI “monetary support” or AB “monetary support” or TI “childcare allowance*” or AB “childcare allowance*” or TI “child care allowance*” or AB “child care allowance*” or TI “National Evaluation of Welfare-to work Strategies” or AB “National Evaluation of Welfare-to work Strategies” or TI NDLP or AB NDLP or TI “New Deal for Lone Parents” or AB “New Deal for Lone Parents” or TI “New Hope Project” or AB “New Hope Project” or TI “Newstart allowance” or AB “Newstart allowance” or TI NEWWS or AB NEWWS or TI “Ontario Works” or AB “Ontario Works” or TI Poverty or AB Poverty or TI PRWORA or AB PRWORA or TI “public welfare reform*” or AB “public welfare reform*” or TI Retrain* or AB Retrain* or TI Re-train* or AB Re-train* or TI RMI or AB RMI or TI sanctions or AB sanctions or TI “Self-Sufficiency Project” or AB “Self-Sufficiency Project” or TI “Revenu Minimum d’Insertion” or AB “Revenu Minimum d’Insertion” or TI “Social assistance” or AB “Social assistance” or TI SSP or AB SSP or TI TANF or AB TANF or TI “Temporary Assistance for Needy Families” or AB “Temporary Assistance for Needy Families” or TI “time limit*” or AB “time limit*” or TI Training or AB Training or TI Unemployment or AB Unemployment or TI Vocation* or AB Vocation* or TI Welfare or AB Welfare or TI “work first strateg*” or AB “work first strateg*” or TI work* or AB work* or TI “Working For Families” or AB “Working For Families” or TI “tax credit*” or AB “tax credit*” Or TI “Public assistance” or TI “Social welfare” Or AB “Public assistance” or AB “Social welfare” or TI income or AB income

S2. TX (never married n2 mother*) or TX (never married n2 father*) or TX (never married n2 parent*) or TX (separated n2 mother*) or TX (separated n2 father*) or TX (separated n2 parent*) or TI fatherless famil* or AB fatherless famil* or TI fragile famil* or AB fragile famil* or TI lone father* or AB lone father* or TI Lone mother* or AB Lone mother* or TI Lone parent* or AB Lone parent* or TI never married parent* or AB never married parent* or TI “motherless famil*” OR AB “motherless famil*” OR TI “One parent*” OR AB “One parent*” OR TI “single father*” OR AB “single father*” OR TI “Single mother*” OR AB “Single mother*” OR TI “Single-parent*” OR AB “Single-parent*” OR TI “sole father*” OR AB “sole father*” OR TI “sole mother*” OR AB “sole mother*” OR TI “Sole parent*” OR AB “Sole parent*” OR TI “sole registrant*” OR AB “sole registrant*” OR TI “unmarried father*” OR AB “unmarried father*” OR TI “unmarried mother*” OR AB “unmarried mother*” OR TI “unwed father*” OR AB “unwed father*” OR TI “Unwed Mother*” OR AB “Unwed Mother*”

S3. quasi-random* or randomi?ed controlled trial or controlled clinical trial or clinical trial or trial or random*

S4. S1 AND S2 AND S3

8. The Cochrane Central Register of Controlled Trials (CENTRAL) 15.4.16

#1. (“single parent*” or “unmarried father*” or “never married father*” or “never married mother*” or “never married parent*” or “separated mother*” or “separated father*” or “Single-parent-family” or “fatherless famil*” or “fragile famil*” or “lone father*” or “Lone mother*” or “lone parent” or “motherless famil*” or “One parent*”or “single father*” or “Single mother*”or “Single-parent*” or “sole father*” or “sole mother*” or “Sole parent*” or “sole registrant*” or “unmarried father*” or “unmarried mother*” or “unwed father*” or “Unwed Mother*”):ti,ab,kw

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#2. (income or "Public assistance" or "Social security" or "Social welfare" or "Canada Health Social Transfer" or "Personal Responsibility Work Opportunity Reconciliation Act" or "Active labo?r market polic*" or "Active labo?r market program*" or ADFC or "Agenda 2010" or "Aid to Families with Dependent Children" or "Allocation Parent Isole" or ALMP or "America Works" or API or "Back-to-work" or "cash benefit*" or "cash incentive*" or "child care assistance" or "child care provision*" or "child care subsid*" or "child care support" or CHST or "Community Wage" or "Domestic Purposes Benefit" or "Employment Tax Deduction" or "earning disregard*" or employability or Employment or "Employment Program*" or ETD or "Family Program*" or "Family Transition Program*" or "financial benefit*" or "financial incentive*" or "financial sanction*" or "Financial support" or "Government Financing" or FTP or "government intervention*" or "Government program*" or "Government Programs" or "health care provision*" or "health care subsid*" or "health insurance provision*" or "health insurance subsid*" or "Hilfe zum Arbeit" or "Hilfe zum Lebensunterhalt" or "human capital development" or "income benefit*" or "income incentive*" or "income supplement*" or "Income support" or "Individual Re-integration Agreement" or IRO or Job or Jobbskatteavdraget or Jobless* or "labo?r force attachment*" or "labo?r force participation" or "Labo?r market activation" or "mandatory employment" or MFIP or "Minnesota Family Investment Program" or "monetary benefit*" or "monetary incentive*" or "monetary support" or "childcare allowance*" or "child care allowance*" or "National Evaluation of Welfare to work Strategies" or NDLP or "New Deal for Lone Parents" or "New Hope Project" or "Newstart allowance" or NEWWS or "Ontario Works" or Poverty or PRWORA or "public welfare reform*" or Retrain* or Re-train* or RMI or sanctions or "Self-Sufficiency Project" or "Revenu Minimum d'Insertion" or "Social assistance" or SSP or TANF or "Temporary Assistance for Needy Families" or "time limit*" or Training or Unemployment or Vocation* or Welfare or "work first strateg*"or "Work for your dole" or work*or "Working For Families" or "tax credit*") :ti,ab,kw
#3. #1 and #2

9. Web of Science (all databases) 15.4.16

TOPIC: (income or welfare or work* or train* or Social security or Public assistance or financ* or allowance or polic* or Retrain* or Back-to-work or employability or Employment or job or poverty or sanctions) *AND* TOPIC: (single parent* or unmarried father* or never married father* or never married mother* or never married parent* or separated mother* or separated father* or Single-parent-family or fatherless famil* or fragile famil* or lone father* or Lone mother* or motherless famil* or One parent* or single father* or Single mother* or Single-parent* or sole father* or sole mother* or Sole parent* or sole registrant* or unmarried father* or unmarried mother* or unwed father* or Unwed Mother*) *AND* TOPIC: (quasi-random* or randomi?ed controlled trial or controlled clinical trial or quasi-random* or randomi?ed controlled trial or controlled clinical trial or clinical trial or trial or random*)

10. Applied Social Sciences Index and Abstracts (ASSIA) (Proquest) 16.4.16

((SU(income) OR SU("social welfare") OR SU("economic welfare") OR SU("red cross") OR SU("social security") OR SU("attendance allowances") OR SU("child benefit") OR SU("disability allowances") OR SU("disability living allowance") OR SU("domestic assistance allowances") OR SU("energy allowances") OR SU("familyy allowances") OR SU("familyy credit") OR SU("furniture allowances") OR SU("housing benefits") OR SU("housing grants") OR SU("incapacity benefit") OR SU("independent living fund") OR SU("industrial injury benefits") OR SU("invalidity benefit") OR SU("maternity benefits") OR SU(medicaid) OR SU(medicare) OR SU("mobility allowances") OR SU("national provident funds") OR SU("severe weather payments") OR SU("sickness benefits") OR SU("social fund") OR SU("emergency social funds") OR TI("Canada Health and Social Transfer") OR AB("Canada Health and Social Transfer") OR TI("Personal Responsibility and Work Opportunity Reconciliation Act") OR AB("Personal Responsibility and Work Opportunity Reconciliation Act") OR TI("Active labo?r market polic*") OR AB("Active labo?r market polic*") OR TI("Active labo?r market program*") OR AB("Active labo?r market program*") OR TI(ADFC) OR AB(ADFC) OR TI("Agenda 2010") OR AB("Agenda 2010") OR TI("Aid to Families with Dependent Children") OR AB("Aid to Families with Dependent Children") OR TI("Allocation Parent Isole") OR AB("Allocation Parent Isole") OR TI(ALMP) OR AB(ALMP) OR TI("America Works") OR AB("America Works") OR TI(API) OR AB(API) OR TI("Back-to-work") OR AB("Back-to-work") OR TI("cash benefit*") OR AB("cash benefit*") OR TI("cash incentive*") OR AB("cash incentive*") OR TI("child care assistance") OR AB("child care assistance") OR TI("child care provision*") OR AB("child care provision*") OR TI("child care subsidy*") OR AB("child care subsidy*") OR TI("child care support") OR AB("child care support") OR TI(chest) OR AB(chest) OR TI("Community Wage") OR AB("Community Wage") OR TI("Domestic Purposes Benefit") OR AB("Domestic Purposes Benefit") OR TI("Employment Tax Deduction") OR AB("Employment Tax Deduction") OR TI("earning disregard*") OR AB("earning disregard*") OR TI(employability) OR AB(employability) OR TI(Employment) OR AB(Employment) OR TI("Employment Program*") OR AB("Employment Program*") OR TI(ETD) OR AB(ETD) OR TI(income) OR AB(income) OR TI("Public assistance") OR AB("Public assistance") OR TI("Social security") OR AB("Social security") OR TI("Social welfare") OR AB("Social welfare") OR TI("familyy Program*") OR AB("familyy Program*") OR TI("familyy Transition Program*") OR AB("familyy Transition Program*") OR TI("financial benefit*") OR AB("financial benefit*") OR TI("financial incentive*") OR AB("financial incentive*") OR TI("financial sanction*") OR AB("financial sanction*") OR TI("Financial support") OR AB("Financial support") OR TI("Financial support") OR AB("Financial support") OR TI("Financing, Government") OR AB("Financing, Government") OR TI(FTP) OR AB(FTP) OR TI("government intervention*") OR AB("government intervention*") OR TI("Government program*") OR AB("Government program*") OR TI("Government Programs") OR AB("Government Programs") OR TI("health care provision*") OR AB("health care provision*") OR TI("health care subsidy*") OR AB("health care subsidy*") OR TI("health insurance provision*") OR AB("health insurance provision*") OR TI("health insurance subsidy*") OR AB("health insurance subsidy*") OR TI("Hilfe zum Arbeit") OR AB("Hilfe zum Arbeit") OR TI("Hilfe zum Lebensunterhalt") OR AB("Hilfe zum Lebensunterhalt") OR TI("human capital development") OR AB("human capital development") OR TI("income benefit*") OR AB("income benefit*") OR TI("income incentive*") OR AB("income incentive*") OR TI("income supplement*") OR AB("income supplement*") OR TI("Income support") OR AB("Income support") OR TI("Individual Re-integration Agreement") OR AB("Individual Re-integration Agreement") OR TI(IRO) OR AB(IRO) OR TI(Job) OR AB(Job) OR TI(Jobbskatteavdraget) OR AB(Jobbskatteavdraget) OR TI(Jobless*) OR AB(Jobless*) OR TI("labo?r force attachment*") OR AB("labo?r force attachment*") OR TI("labo?r force participation") OR AB("labo?r force participation") OR TI("Labo?r market activation") OR AB("Labo?r market activation") OR TI("mandatory employment") OR AB("mandatory employment") OR TI(MFIP) OR AB(MFIP) OR TI("Minnesota familyy Investment Program") OR AB("Minnesota familyy Investment Program") OR TI("monetary benefit*") OR AB("monetary benefit*") OR TI("monetary incentive*") OR AB("monetary incentive*") OR TI("monetary support") OR AB("monetary support") OR TI("childcare allowance*") OR AB("childcare allowance*") OR TI("child care allowance*") OR AB("child care allowance*") OR AB("National Evaluation of Welfare-to work strategyies") OR AB("National Evaluation of Welfare-to work strategyies") OR TI(NDLP) OR AB(NDLP) OR TI("New Deal for Lone Parents") OR AB("New Deal for Lone Parents") OR TI("New Hope Project") OR AB("New Hope Project") OR TI("Newstart allowance") OR AB("Newstart allowance") OR TI(news) OR AB(news) OR TI("Ontario Works") OR AB("Ontario Works") OR TI(Poverty) OR AB(Poverty) OR TI(PRWORA) OR AB(PRWORA) OR TI("public welfare reform*") OR AB("public welfare reform*") OR TI(Retrain*) OR TI(restrain*) OR AB(Retrain*) OR AB(restrain*) OR TI(RMI) OR AB(RMI) OR TI(sanctions) OR AB(sanctions) OR TI("Self-Sufficiency Project") OR AB("Self-Sufficiency Project") OR TI("Revenu Minimum d'Insertion") OR AB("Revenu Minimum d'Insertion") OR TI("Social assistance") OR AB("Social assistance") OR TI(SSP) OR AB(SSP) OR TI(TANF) OR AB(TANF) OR TI("Temporary Assistance for Needy Families ") OR AB("Temporary Assistance for Needy Families ") OR TI("time limit*") OR AB("time limit*") OR TI(Training) OR AB(Training) OR TI(Unemployment) OR AB(Unemployment) OR TI(Vocation*) OR AB(Vocation*) OR TI(Welfare) OR AB(Welfare) OR TI("work first strategy*") OR AB("work first strategy*") OR TI("Work for your dole") OR AB("Work for your dole") OR TI(work*) OR AB(work*) OR TI("Working For Families ") OR AB("Working For Families ") OR TI("tax credit*") OR AB("tax credit*")) *AND* (SU("single parent Families ") OR SU("single mothers") OR SU("low income single mothers") OR SU("single adolescent mothers") OR TI("unmarried father*") OR AB("unmarried father*") OR TI("never married father*") OR AB("never married father*") OR TI("never married mother*") OR AB("never married mother*") OR TI("never married parent*") OR AB("never married parent*") OR TI("separated mother*") OR AB("separated mother*") OR TI("separated father*") OR AB("separated father*") OR TI("Single-parent-familyy") OR AB("Single-parent-familyy") OR TI("fatherless family*") OR AB("fatherless family*") OR TI("fragile family*") OR AB("fragile family*") OR TI("lone father*") OR AB("lone father*") OR TI("Lone mother*") OR AB("Lone mother*") OR TI("motherless family*") OR AB("motherless family*") OR TI("One parent*") OR AB("One parent*") OR TI("single father*") OR 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AB("unmarried father*") OR TI("unmarried mother*") OR AB("unmarried mother*") OR TI("unwed father*") OR AB("unwed father*") OR TI("Unwed Mother*") OR AB("Unwed Mother*")) *AND* (TI("quasi-random*") OR TI("randomi?ed controlled trial") OR TI("controlled clinical trial") OR AB("quasi-random*") OR AB("randomi?ed controlled trial") OR AB("controlled clinical trial") OR TI("clinical trial") OR TI(trial) OR TI(random*) OR AB("clinical trial") OR AB(trial) OR AB(random*))

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Limits: Peer reviewed

11. International Bibliography of the Social Sciences (IBSS) (ProQuest) 16.4.16

(SU(income) OR SU(“agricultural income”) OR SU(“ disposable income”) OR SU(“familyy income”) OR SU(“ farm income”) OR SU(“farmers income”) OR SU(“ household income”) OR SU(“income elasticity”) OR SU(“ industrial income”) OR SU(“low income”) OR SU(“ minimum income”) OR SU(“guaranteed minimum income”) OR SU(“ national income”) OR SU(“public revenuee”) OR SU(“public property revenuee”) OR SU(“wages national income ratio”) OR SU(“ per capita income”) OR SU(“permanent income”) OR SU(“ real income”) OR SU(“tariff revenuees”) OR SU(“ tax revenuee ”) OR SU(“ social welfare”) OR SU(“community care”) OR SU(“social security”) OR SU(“familyy allowances”) OR SU(“health insurance”) OR SU(“housing allowances”) OR SU(“maternity benefits”) OR SU(“means testing”) OR SU(“redistributive social security”) OR SU(“social security financing”) OR SU(“social security funds”) OR SU(“social services”) OR SU(“social support”) OR SU(“benefit plans”) OR SU(“disability benefit”) OR SU(“disabled rehabilitation”) OR SU(“social workers ”) OR TI(“Public assistance”) OR AB(“Public assistance”) OR TI(“ Canada Health and Social Transfer”) OR AB(“Canada Health and Social Transfer”) OR TI(“ Personal Responsibility and Work Opportunity Reconciliation Act”) OR AB(“Personal Responsibility and Work Opportunity Reconciliation Act”) OR TI(“ Active labo?r market polic*”) OR AB(“Active labo?r market polic*”) OR TI(“ Active labo?r market program* ”) OR AB(“Active labo?r market program*”) OR TI(ADFC) OR AB(ADFC) OR TI(“ Agenda 2010”) OR AB(“Agenda 2010”) OR TI(“ Aid to Families with Dependent Children”) OR AB(“Aid to Families with Dependent Children”) OR TI(“ Allocation Parent Isole”) OR AB(“Allocation Parent Isole”) OR TI(“ ALMP”) OR AB(“ALMP”) OR TI(“ America Works”) OR AB(“America Works”) OR TI(API) OR AB(API) OR TI(“ Back-to-work”) OR AB(“Back-to-work”) OR TI(“ cash benefit*”) OR AB(“cash benefit*”) OR TI(“ cash incentive*”) OR AB(“cash incentive*”) OR TI(“child care assistance”) OR AB(“child care assistance”) 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TI(” income benefit* “) or AB(” income benefit* “) or TI(” income incentive* “) or AB(” income incentive* “) or TI(” income supplement* “) or AB(” income supplement* “) or TI(” Income support “) or AB(” Income support “) or TI(” Individual reintegration Agreement “) or AB(” Individual reintegration Agreement “) or TI(IRO) Or AB(IRO) Or TI(Job) Or AB(Job) Or TI(Jobbskatteavdraget) Or AB(Jobbskatteavdraget) Or TI(Jobless*) Or AB(Jobless*) Or TI(” labo?r force attachment* “) or AB(” labo?r force attachment* “) or TI(” labo?r force participation “) or AB(” labo?r force participation “) or TI(” Labo?r market activation “) or AB(” Labo?r market activation “) or TI(” mandatory employment “) or AB(” mandatory employment “) or TI(MFIP) Or AB(MFIP) Or TI(” Minnesota familyy Investment Program “) or AB(” Minnesota familyy Investment Program “) or TI(” monetary benefit* “) or AB(” monetary benefit* “) or TI(” monetary incentive* “) or AB(” monetary incentive* “) or TI(” monetary support “) or AB(” monetary support “) or TI(” childcare allowance* “) Or AB(” childcare allowance* “) or TI(” child care allowance* “) or AB(” child care allowance* “) Or AB(” National Evaluation of Welfare-to work strategyies “) or AB(” National Evaluation of Welfare-to work strategyies “) or TI(NDLP) Or AB(NDLP) Or TI(” New Deal for Lone Parents “) or AB(” New Deal for Lone Parents “) or TI(” New Hope Project “) or AB(” New Hope Project “) or TI(” Newstart allowance “) or AB(” Newstart allowance “) or TI(NEWWS) Or AB(NEWWS) Or TI(” Ontario Works “) or AB(” Ontario Works “) or TI(Poverty) Or AB(Poverty) Or TI(PRWORA) Or AB(PRWORA) Or TI(” public welfare reform* “) or AB(” public welfare reform* “) or TI(Retrain*) or TI(Re-train*) Or AB(Retrain*) or AB(Re-train*) Or TI(RMI) Or AB(RMI) Or TI(sanctions) Or AB(sanctions) Or TI(” Self-Sufficiency Project “) or AB(” Self-Sufficiency Project “) or TI(” revenue Minimum d’Insertion “) or AB(” revenue Minimum d’Insertion “) or TI(” Social assistance “) or AB(” Social assistance “) or TI(SSP) Or AB(SSP) Or TI(TANF) Or AB(TANF) Or TI(” Temporary Assistance for Needy Families “) or AB(” Temporary Assistance for Needy Families “) or TI(” time limit* “) or AB(” time limit* “) or TI(Training) Or AB(Training) Or TI(Unemployment) Or AB(Unemployment) Or TI(Vocation*) Or AB(Vocation*) Or TI(Welfare) Or AB(Welfare) Or TI(” work first

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strategy* “) or AB(” work first strategy* “) or TI(” Work for your dole “) or AB(” Work for your dole “) or TI(work*) Or AB(work*) Or TI(” Working For Families “) or AB(” Working For Families “) or TI(” tax credit* “) or AB(” tax credit* “) *AND* (SU(”unmarried mothers“) OR TI(”single parent*“) OR AB(”single parent*“) OR TI(”unmarried father*“) OR AB(”unmarried father*“) OR TI(”never married father*“) OR AB(”never married father*“) OR TI(”never married mother*“) OR AB(”never married mother*“) OR TI(”never married parent*“) OR AB(”never married parent*“) OR TI(”separated mother*“) OR AB(”separated mother*“) OR TI(”separated father*“) OR AB(”separated father*“) OR TI(”Single-parent-familyy“) OR AB(”Single-parent-familyy“) OR TI(”fatherless family*“) OR AB(”fatherless family*“) OR TI(”fragile family*“) OR AB(”fragile family*“) OR TI(”lone father*“) OR AB(”lone father*“) OR TI(”Lone mother*“) OR AB(”Lone mother*“) OR TI(”motherless family*“) OR AB(”motherless family*“) OR TI(”One parent*“) OR AB(”One parent*“) OR TI(”single father*“) OR AB(”single father*“) OR TI(”Single mother*“) OR AB(”Single mother*“) OR TI(”Single-parent*“) OR AB(”Single-parent*“) OR TI(”sole father*“) OR AB(”sole father*“) OR TI(”sole mother*“) OR AB(”sole mother*“) OR TI(”Sole parent*“) OR AB(”Sole parent*“) OR TI(”sole registrant*“) OR AB(”sole registrant*“) OR TI(”unmarried father*“) OR AB(”unmarried father*“) OR TI(”unmarried mother*“) OR AB(”unmarried mother*“) OR TI(”unwed father*“) OR AB(”unwed father*“) OR TI(Unwed Mother* ”) or AB(“ Unwed Mother* ”) *AND* (TI(“quasi-random*”) OR TI(“randomi?ed controlled trial”) OR TI(“controlled clinical trial”) OR AB(“quasi-random*”) OR AB(“randomi?ed controlled trial”) OR AB(“controlled clinical trial”) OR TI(“clinical trial”) OR TI(trial) OR TI(random*) OR AB(“clinical trial”) OR AB(trial) OR AB(random*)
Limits: Peer reviewed

12. Social Services Abstracts (Proquest) 16.4.16

(SU(“single fathers”) OR SU(“single mothers”) OR TI(“single parent*”) OR AB(“single parent*”) OR TI(“unmarried father*”) OR AB(“unmarried father*”) OR TI(“never married father*”) OR AB(“never married father*”) OR TI(“never married mother*”) OR AB(“never married mother*”) OR TI(“never married parent*”) OR AB(“never married parent*”) OR TI(“separated mother*”) OR AB(“separated mother*”) OR TI(“separated father*”) OR AB(“separated father*”) OR TI(“Single-parent-family”) OR AB(“Single-parent-family”) OR TI(“fatherless family*”) OR AB(“fatherless family*”) OR TI(“fragile family*”) OR AB(“fragile family*”) OR TI(“lone father*”) OR AB(“lone father*”) OR TI(“Lone mother*”) OR AB(“Lone mother*”) OR TI(“motherless family*”) OR AB(“motherless family*”) OR TI(“One parent*”) OR AB(“One parent*”) OR TI(“single father*”) OR AB(“single father*”) OR TI(“Single mother*”) OR AB(“Single mother*”) OR TI(“Single-parent*”) OR AB(“Single-parent*”) OR TI(“sole father*”) OR AB(“sole father*”) OR TI(“sole mother*”) OR AB(“sole mother*”) OR TI(“Sole parent*”) OR AB(“Sole parent*”) OR TI(“sole registrant*”) OR AB(“sole registrant*”) OR TI(“unmarried father*”) OR AB(“unmarried father*”) OR TI(“unmarried mother*”) OR AB(“unmarried mother*”) OR TI(“unwed father*”) OR AB(“unwed father*”) OR TI(“Unwed Mother*”) OR AB(“Unwed Mother*”)) *AND* (SU(income) OR SU(profits) OR SU(“social security”) OR SU(“social welfare) or TI(” Public assistance “) or AB(” Public assistance “) OR TI(” Canada Health AND Social Transfer “) OR AB(” Canada Health AND Social Transfer “) OR TI(” Personal Responsibility AND Work Opportunity Reconciliation Act “) OR AB(” Personal Responsibility AND Work Opportunity Reconciliation Act “) OR TI(” Active labo?r market police* “) OR AB(” Active labo?r market police* “) OR TI(” Active labo?r market program* “) OR AB(” Active labo?r market program* “) OR TI(ADFC) OR AB(ADFC) OR TI(” Agenda 2010 “) OR AB(” Agenda 2010 “) OR TI(” Aid to families with Dependent Children “) OR AB(” Aid to families with Dependent Children “) OR TI(” Allocation Parent sole) OR AB(“ Allocation Parent sole ”) OR TI(ALMP) OR AB(ALMP) OR TI(“ America Works ”) OR AB(“ America Works ”) OR TI(API) OR AB(API) OR TI(“ Back-to-work ”) OR AB(“ Back-to-work ”) OR TI(“ cash benefit* ”) OR AB(“ cash benefit* ”) OR TI(“ cash incentive* ”) OR AB(“ cash incentive* ”) OR TI(“ child care assistance ”) OR AB(“ child care assistance ”) OR TI(“ child care provision* ”) OR AB(“ child care provision* ”) OR TI(“ child care subsidy* ”) OR AB(“ child care subsidy* ”) OR TI(“ child care support ”) OR AB(“ child care support ”) OR TI(chest) OR AB(chest) OR TI(“ Community Wage ”) OR AB(“ Community Wage ”) OR TI(“ Domestic Purposes Benefit ”) OR AB(“ Domestic Purposes Benefit ”) OR TI(“ Employment Tax Deduction ”) OR AB(“ Employment Tax Deduction ”) OR TI(“ earning disregard* ”) OR AB(“ earning disregard* ”) OR TI(employability) OR AB(employability) OR TI(Employment) OR AB(Employment) OR TI(“ Employment Program* ”) OR AB(“ Employment Program* ”) OR TI(ETD) OR AB(ETD) OR TI(income) OR AB(income) OR TI(“ Public assistance ”) OR AB(“ Public assistance ”) OR TI(“ Social security ”) OR AB(“ Social security ”) OR TI(“ Social welfare ”) OR AB(“ Social welfare ”) OR TI(“ family Program* ”) OR AB(“ family Program* ”) OR TI(“ family Transition Program* ”) OR AB(“ family Transition Program* ”) OR TI(“ financial benefit* ”) OR AB(“ financial benefit* ”) OR TI(“ financial incentive* ”) OR AB(“ financial incentive* ”) OR TI(“ financial sanction* ”) OR AB(“ financial sanction* ”) OR TI(“ Financial support ”) OR AB(“ Financial support ”) OR TI(“ Financial support ”) OR AB(“ Financial support ”) OR TI(“ Financing, Government ”) OR AB(“ Financing, Government ”) OR TI(FTP) OR AB(FTP) OR TI(“ government intervention* ”) OR AB(“ government intervention* ”) OR TI(“ Government program* ”) OR AB(“ Government program* ”) OR TI(“ Government Programs ”) OR AB(“ Government Programs ”) OR TI(“ health care provision* ”) OR AB(“ health care provision* ”) OR TI(“ health care subsidy* ”) OR AB(“ health care subsidy* ”) OR TI(“ health insurance provision* ”) OR AB(“ health insurance provision* ”) OR TI(“ health insurance subsidy* ”) OR AB(“ health insurance subsidy* ”) OR TI(“ Hilfe zum Arbeit ”) OR AB(“ Hilfe zum Arbeit ”) OR TI(“ Hilfe zum Lebensunterhalt ”) OR AB(“ Hilfe zum Lebensunterhalt ”) OR TI(“ human capital development ”) OR AB(“ human capital

development ”) OR TI(“ income benefit* ”) OR AB(“ income benefit* ”) OR TI(“ income incentive* ”) OR AB(“ income incentive* ”) OR TI(“ income supplement* ”) OR AB(“ income supplement* ”) OR TI(“ Income support ”) OR AB(“ Income support ”) OR TI(“ Individual Re-integration Agreement ”) OR AB(“ Individual Re-integration Agreement ”) OR TI(IRO) OR AB(IRO) OR TI(Job) OR AB(Job) OR TI(Jobbskatteavdraget) OR AB(Jobbskatteavdraget) OR TI(Jobless*) OR AB(Jobless*) OR TI(“ labo?r force attachment* ”) OR AB(“ labo?r force attachment* ”) OR TI(“ labo?r force participation ”) OR AB(“ labo?r force participation ”) OR TI(“ Labo?r market activation ”) OR AB(“ Labo?r market activation ”) OR TI(“ mandatory employment ”) OR AB(“ mandatory employment ”) OR TI(MFIP) OR AB(MFIP) OR TI(“ Minnesota family Investment Program ”) OR AB(“ Minnesota family Investment Program ”) OR TI(“ monetary benefit* ”) OR AB(“ monetary benefit* ”) OR TI(“ monetary incentive* ”) OR AB(“ monetary incentive* ”) OR TI(“ monetary support ”) OR AB(“ monetary support ”) OR TI(“ childcare allowance* ”) OR AB(“ childcare allowance* ”) OR TI(“ child care allowance* ”) OR AB(“ child care allowance* ”) OR AB(“ National Evaluation of Welfare-to work strategyies ”) OR AB(“ National Evaluation of Welfare-to work strategyies ”) OR TI(NDLP) OR AB(NDLP) OR TI(“ New Deal for Lone Parents ”) OR AB(“ New Deal for Lone Parents ”) OR TI(“ New Hope Project ”) OR AB(“ New Hope Project ”) OR TI(“ Newstart allowance ”) OR AB(“ Newstart allowance ”) OR TI(news) OR AB(news) OR TI(“ Ontario Works ”) OR AB(“ Ontario Works ”) OR TI(Poverty) OR AB(Poverty) OR TI(PRWORA) OR AB(PRWORA) OR TI(“ public welfare reform* ”) OR AB(“ public welfare reform* ”) OR TI(Retrain*) OR TI(“ restrain* ”) OR AB(Retrain*) OR AB(“ restrain* ”) OR TI(RMI) OR AB(RMI) OR TI(sanctions) OR AB(sanctions) OR TI(“ Self-Sufficiency Project ”) OR AB(“ Self-Sufficiency Project ”) OR TI(“ Revenu Minimum d’Insertion ”) OR AB(“ Revenu Minimum d’Insertion ”) OR TI(“ Social assistance ”) OR AB(“ Social assistance ”) OR TI(SSP) OR AB(SSP) OR TI(TANF) OR AB(TANF) OR TI(“ Temporary Assistance for Needy families ”) OR AB(“ Temporary Assistance for Needy families ”) OR TI(“ time limit* ”) OR AB(“ time limit* ”) OR TI(Training) OR AB(Training) OR TI(Unemployment) OR AB(Unemployment) OR TI(Vocation*) OR AB(Vocation*) OR TI(Welfare) OR AB(Welfare) OR TI(“ work first strategy* ”) OR AB(“ work first strategy* ”) OR TI(“ Work for your dole ”) OR AB(“ Work for your dole ”) OR TI(work*) OR AB(work*) OR TI(“ Working For families ”) OR AB(“ Working For families ”) OR TI(“ tax credit* ”) OR AB(“ tax credit* ”)) *AND* (TI(“quasi-random*”) OR TI(“randomi?ed controlled trial”) OR TI(“controlled clinical trial”) OR AB(“quasi-random*”) OR AB(“randomi?ed controlled trial”) OR AB(“controlled clinical trial”) OR TI(“clinical trial”) OR TI(trial) OR TI(random*) OR AB(“clinical trial”) OR AB(trial) OR AB(random*))
Limits Peer reviewed

13. Sociological Abstracts (Proquest) 16.4.16
(SU(“single fathers”) OR SU(“single mothers”) OR SU(“single parent family”) OR TI(“single parent*”) OR (“single parent*”) OR TI(“unmarried father*”) OR AB(“unmarried father*”) OR TI(“never married father*”) OR AB(“never married father*”) OR TI(“never married mother*”) OR AB(“never married mother*”) OR TI(“never married parent*”) OR AB(“never married parent*”) OR TI(“separated mother*”) OR AB(“separated mother*”) OR TI(“separated father*”) OR AB(“separated father*”) OR TI(“Single-parent-family”) OR AB(“Single-parent-family”) OR TI(“fatherless family*”) OR AB(“fatherless family*”) OR TI(“fragile family*”) OR AB(“fragile family*”) OR TI(“lone father*”) OR AB(“lone father*”) OR TI(“Lone mother*”) OR AB(“Lone mother*”) OR TI(“motherless family*”) OR AB(“motherless family*”) OR TI(“One parent*”) OR AB(“One parent*”) OR TI(“single father*”) OR AB(“single father*”) OR TI(“Single mother*”) OR AB(“Single mother*”) OR TI(“Single-parent*”) OR AB(“Single-parent*”) OR TI(“sole father*”) OR AB(“sole father*”) OR TI(“sole mother*”) OR AB(“sole mother*”) OR TI(“Sole parent*”) OR AB(“Sole parent*”) OR TI(“sole registrant*”) OR AB(“sole registrant*”) OR TI(“unmarried father*”) OR AB(“unmarried father*”) OR TI(“unmarried mother*”) OR AB(“unmarried mother*”) OR TI(“unwed father*”) OR AB(“unwed father*”) OR TI(“Unwed Mother*”) OR AB(“Unwed Mother*”)) *AND* (SU(“income”) OR SU(“profits”) OR SU(“social security ”) OR SU(“social welfare”) OR TI(“Public assistance”) OR AB(“Public assistance”) OR TI(“Canada Health and Social Transfer”) OR AB(“Canada Health and Social Transfer”) OR TI(“Personal Responsibility and Work Opportunity Reconciliation Act”) OR AB(“Personal Responsibility and Work Opportunity Reconciliation Act”) OR TI(“Active labo?r market polic*”) OR AB(“Active labo?r market polic*”) OR TI(“Active labo?r market program*”) OR AB(“Active labo?r market program*”) OR TI(ADFC) OR AB(ADFC) OR TI(“Agenda 2010”) OR AB(“Agenda 2010”) OR TI(“Aid to Families with Dependent Children”) OR AB(“Aid to Families with Dependent Children”) OR TI(“Allocation Parent Isole”) OR AB(“Allocation Parent Isole”) OR TI(ALMP) OR AB(ALMP) OR TI(“America Works”) OR AB(“America Works”) OR TI(API) OR AB(API) OR TI(“Back-to-work”) OR AB(“Back-to-work”) OR TI(“cash benefit*”) OR AB(“cash benefit*”) OR TI(“cash incentive*”) OR AB(“cash incentive*”) OR TI(“child care assistance”) OR AB(“child care assistance”) OR TI(“child care provision*”) OR AB(“child care provision*”) OR TI(“child care subsidy*”) OR AB(“child care subsidy*”) OR TI(“child care support”) OR AB(“child care support”) OR TI(chest) OR AB(chest) OR TI(“Community Wage”) OR AB(“Community Wage”) OR TI(“Domestic Purposes Benefit”) OR AB(“Domestic Purposes Benefit”) OR TI(“Employment Tax Deduction”) OR AB(“Employment Tax Deduction”) OR TI(“earning disregard*”) OR AB(“earning disregard*”) OR TI(employability) OR AB(employability) OR TI(Employment) OR AB(Employment) OR TI(“Employment Program*”) OR AB(“Employment Program*”) OR TI(ETD) OR AB(ETD) OR TI(income) OR AB(income) OR TI(“Public assistance”) OR AB(“Public assistance”) OR TI(“Social security”) OR AB(“Social security”) OR TI(“Social welfare”) OR AB(“Social welfare”) OR TI(“family Program*”) OR AB(“family Program*”) OR

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TI(“family Transition Program*”) OR AB(“family Transition Program*”) OR TI(“financial benefit*”) OR AB(“financial benefit*”) OR TI(“financial incentive*”) OR AB(“financial incentive*”) OR TI(“financial sanction*”) OR AB(“financial sanction*”) OR TI(“Financial support”) OR AB(“Financial support”) OR TI(“Financial support”) OR AB(“Financial support”) OR TI(“Financing, Government”) OR AB(“Financing, Government”) OR TI(FTP) OR AB(FTP) OR TI(“government intervention*”) OR AB(“government intervention*”) OR TI(“Government program*”) OR AB(“Government program*”) OR TI(“Government Programs”) OR AB(“Government Programs”) OR TI(“health care provision*”) OR AB(“health care provision*”) OR TI(“health care subsidy*”) OR AB(“health care subsidy*”) OR TI(“health insurance provision*”) OR AB(“health insurance provision*”) OR TI(“health insurance subsidy*”) OR AB(“health insurance subsidy*”) OR TI(“Hilfe zum Arbeit”) OR AB(“Hilfe zum Arbeit”) OR TI(“Hilfe zum Lebensunterhalt”) OR AB(“Hilfe zum Lebensunterhalt”) OR TI(“human capital development”) OR AB(“human capital development”) OR TI(“income benefit*”) OR AB(“income benefit*”) OR TI(“income incentive*”) OR AB(“income incentive*”) OR TI(“income supplement*”) OR AB(“income supplement*”) OR TI(“Income support”) OR AB(“Income support”) OR TI(“Individual Re-integration Agreement”) OR AB(“Individual Re-integration Agreement”) OR TI(IRO) OR AB(IRO) OR TI(Job) OR AB(Job) OR TI(Jobbskatteavdraget) OR AB(Jobbskatteavdraget) OR TI(Jobless*) OR AB(Jobless*) OR TI(“labo?r force attachment*”) OR AB(“labo?r force attachment*”) OR TI(“labo?r force participation”) OR AB(“labo?r force participation”) OR TI(“Labo?r market activation”) OR AB(“Labo?r market activation”) OR TI(“mandatory employment”) OR AB(“mandatory employment”) OR TI(MFIP) OR AB(MFIP) OR TI(“Minnesota family Investment Program”) OR AB(“Minnesota family Investment Program”) OR TI(“monetary benefit*”) OR AB(“monetary benefit*”) OR TI(“monetary incentive*”) OR AB(“monetary incentive*”) OR TI(“monetary support”) OR AB(“monetary support”) OR TI(“childcare allowance*”) OR AB(“childcare allowance*”) OR TI(“child care allowance*”) OR AB(“child care allowance*”) OR AB(“National Evaluation of Welfare-to work strategyies”) OR AB(“National Evaluation of Welfare-to work strategyies”) OR TI(NDLP) OR AB(NDLP) OR TI(“New Deal for Lone Parents”) OR AB(“New Deal for Lone Parents”) OR TI(“New Hope Project”) OR AB(“New Hope Project”) OR TI(“Newstart allowance”) OR AB(“Newstart allowance”) OR TI(news) OR AB(news) OR TI(“Ontario Works”) OR AB(“Ontario Works”) OR TI(Poverty) OR AB(Poverty) OR TI(PRWORA) OR AB(PRWORA) OR TI(“public welfare reform*”) OR AB(“public welfare reform*”) OR TI(Retrain*) OR TI(restrain*) OR AB(Retrain*) OR AB(restrain*) OR TI(RMI) OR AB(RMI) OR TI(sanctions) OR AB(sanctions) OR TI(“Self-Sufficiency Project”) OR AB(“Self-Sufficiency Project”) OR TI(“Revenu Minimum d’Insertion”) OR AB(“Revenu Minimum d’Insertion”) OR TI(“Social assistance”) OR AB(“Social assistance”) OR TI(SSP) OR AB(SSP) OR TI(TANF) OR AB(TANF) OR TI(“Temporary Assistance for Needy Families ”) OR AB(“Temporary Assistance for Needy Families ”) OR TI(“time limit*”) OR AB(“time limit*”) OR TI(Training) OR AB(Training) OR TI(Unemployment) OR AB(Unemployment) OR TI(Vocation*) OR AB(Vocation*) OR TI(Welfare) OR AB(Welfare) OR TI(“work first strategy*”) OR AB(“work first strategy*”) OR TI(“Work for your dole”) OR AB(“Work for your dole”) OR TI(work*) OR AB(work*) OR TI(“Working For Families ”) OR AB(“Working For Families ”) OR TI(“tax credit*”) OR AB(“tax credit*”)) *AND* (TI(“quasi-random*”) OR TI(“randomi?ed controlled trial”) OR TI(“controlled clinical trial”) OR AB(“quasi-random*”) OR AB(“randomi?ed controlled trial”) OR AB(“controlled clinical trial”) OR TI(“clinical trial”) OR TI(“trial”) OR TI(“random*”) OR AB(“clinical trial”) OR AB(trial) OR AB(random*))

Limits Peer reviewed

14. Campbell Library

Parent* OR welfare*

15. NHS Economic Evaluation Database (NHS EED)

(parent*) OR (welfare*)

16. Turning Research Into Practice (TRIP);

“one parent* or ”lone Parent*“ or ”single parent*“ or ”single mother*“) and welfare

17. Open Grey

”one parent“ and work*

18. Planex

(”one parent families“ or ”lone parent“ or ”single parent*“ or ”single mother*“) and (employment or welfare*)

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Appendix 3. Websites searched

Institution/Project	References found	Studies identified for detailed screening
Abt Associates Inc.	245	1
Administration for Children and Families Office of Planning, Research & Evaluation	226	13
Australian Government Employment and Workplace Relations	NA	0
Australian Institute of Family Studies	NA	0
British Library- welfare reform on the web	118	3
Brookings Institution	50	0
Canadian Social Research Links	152	0
Cato Institute	NA	0
Center for Quality Assurance and Policy Studies	NA	0
Center for Social Services Research	2	0
Center on Budget and Policy Priorities	2432	0
Centre for Economic and Social Inclusion	NA	0
Centre for Market and Public Organisation (Bristol)	NA	0
Chapin Hall (Chicago)	40	0
Department for Work and Pensions	223	7
Department of Social and Family Affairs, Ireland	NA	0
Department of Social and Policy Sciences, Bath	NA	0
Does 'Work for the dole' work?	NA	0
Employment Research Institute	NA	0
Gerald R. Ford School of Public Policy	90	5

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(*Continued*)

Government of Western Australia, Department of Health	NA	0
Heritage Foundation	NA	0
Human Capability and Resilience research project	NA	0
Human Resources and Skills Development Canada	NA	0
Institute for Fiscal Studies	NA	0
Institute for Policy Research	215	7
Institute for Public Policy Research	96	0
Institute for Research on Poverty	732	55
Institute of Economic Affairs	26	0
Joseph Rowntree Foundation	NA	0
Manhattan Institute	NA	0
Manpower Demonstration Research Corporation/MDRC	174	56
Maryland Institute for Policy Analysis and Research	NA	0
Mathemetica Policy Research Inc.	103	1
Ministry of Social Development, New Zealand	NA	0
National Bureau of Economic Research	31	12
National Centre for Social Research	NA	0
National Evaluation of Welfare to Work strategies	27	22
National Poverty Center (Michigan)	73	15
New South Wales Office for Women	NA	1
Norwegian Government	NA	0

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(*Continued)*

One Family	NA	0
Pioneer Institute	NA	0
Policy Library	NA	0
Policy Studies Institute	NA	0
RAND Corporation	523	8
Ray Marshall Centre	104	2
Research Connections	823	19
Robert Wood Johnson Foundation	29	0
Social Policy Digest	NA	0
Social Research and Demonstration Corporation, Canada	90	8
Statistics Norway	47	0
The Centre for Analysis of Social Exclusion	NA	0
The Institute for Employment Studies	56	0
The Institute for Labour Market Policy Evaluation, Sweden	102	0
The Research Forum	54	51
The Urban Institute	1219	6
US Government Accountability Office	580	3
WE Upjohn Institute	35	0
Total	8717 E	275

Websites with search interfaces or searchable database were searched using terms such as 'lone parent' 'lone parent welfare' 'welfare reform' or 'welfare health'. Otherwise the relevant publications topic in a website was screened. Where this was possible, the number of initial 'hits' is listed. NA = no searchable interface. Total is an estimate due to websites without searchable interfaces.

Appendix 4. Risk of bias for studies with a separate control group

Was the allocation sequence adequately generated?

Score ”Yes“ if a random component in the sequence generation process is described (e.g. referring to a random number table). Score ”No“ when a nonrandom method is used (e.g. performed by date of admission). Controlled clinical trials (CCTs) and controlled before-and-after studies (CBAs) should be scored ”No“. Score ”Unclear“ if not specified in the paper.

Was the allocation adequately concealed?

Score ”Yes“ if the unit of allocation was by institution, team or professional and allocation was performed on all units at the start of the study; or if the unit of allocation was by patient or episode of care and there was some form of centralised randomisation scheme, an on-site computer system or sealed opaque envelopes were used. CBAs should be scored ”No“. Score ”unclear“ if not specified in the paper.

Were baseline outcome measurements similar?

Score ”Yes“ if performance or patient outcomes were measured prior to the intervention, and no important differences were present across study groups. In RCTs, score ”Yes“ if imbalanced but appropriate adjusted analysis was performed (e.g. analysis of covariance). Score ”No“ if important differences were present and not adjusted for in analysis. If RCTs have no baseline measure of outcome, score ”Unclear“.

Were baseline characteristics similar?

Score ”Yes“ if baseline characteristics of the study and control providers are reported and similar. Score ”Unclear“ if it is not clear in the paper (e.g. characteristics are mentioned in text but no data were presented). Score ”No“ if there is no report of characteristics in text or tables or if there are differences between control and intervention providers. Note that in some cases imbalance in patient characteristics may be due to recruitment bias whereby the provider was responsible for recruiting patients into the trial.

Were incomplete outcome data adequately addressed?

Score ”Yes“ if missing outcome measures were unlikely to bias the results (e.g. the proportion of missing data was similar in the intervention and control groups or the proportion of missing data was less than the effect size i.e. unlikely to overturn the study result). Score ”No“ if missing outcome data was likely to bias the results. Score ”Unclear“ if not specified in the paper (do not assume 100% follow up unless stated explicitly).

Was knowledge of the allocated interventions adequately prevented during the study?

Score ”Yes“ if the authors state explicitly that the primary outcome variables were assessed blindly, or the outcomes are objective, e.g. length of hospital stay. Primary outcomes are those variables that correspond to the primary hypothesis or question as defined by the authors. Score ”No“ if the outcomes were not assessed blindly. Score ”unclear“ if not specified in the paper.

Was the study adequately protected against contamination?

Score ”Yes“ if allocation was by community, institution or practice and it is unlikely that the control group received the intervention. Score ”No“ if it is likely that the control group received the intervention (e.g. if patients rather than professionals were randomised). Score ”unclear“ if professionals were allocated within a clinic or practice and it is possible that communication between intervention and control professionals could have occurred (e.g. physicians within practices were allocated to intervention or control).

Was the study free from selective outcome reporting?

Score ”Yes“ if there is no evidence that outcomes were selectively reported (e.g. all relevant outcomes in the methods section are reported in the results section). Score ”No“ if some important outcomes are subsequently omitted from the results. Score ”unclear“ if not specified in the paper.

Was the study free from other risks of bias?

Score ”Yes“ if there is no evidence of other risk of biases. If some primary outcomes were imbalanced at baseline, assessed blindly or affected by missing data and others were not, each primary outcome can be scored separately. If ”Unclear“ or ”No“, but there is sufficient data in the paper to do an adjusted analysis (e.g. baseline adjustment analysis or intention to treat analysis) the criteria should be rescored to ”Yes“.

Appendix 5. All reported outcomes by study

1. Maternal mental health				
Study	**CES-D mean**	**CES-D % at risk**	**CIDI % at risk**	**Self-report**
Timepoint 1				
CJF GUP 2000			threshold NR	
CJF Yale 2001		≥16/60		
New Hope 1999	x			
NEWWS 2001	x			
Ontario 2001			threshold NR	
Timepoint 2				
CJF 2002	x			
CJF GUP 2000	x			
FTP 2000	x			
California GAIN 1994				x
MFIP 2000	x	≥23/60		
Ontario 2001				
SSP Recipients 2002	x			
Timepoint 3				

(*Continued*)

IFIP 2002		≥23/60		
IWRE 2002	x			
New Hope 1999	x			
NEWWS 2001				
SSP Applicants 2003	x			
SSP Recipients 2002	x			
UK ERA 2011				x

2. Maternal physical health			
Study	**In poor health (%)**	**In good health (%)**	**Physical health scale**
Timepoint 1			
CJF GUP 2000			
CJF Yale 2001	x		
New Hope 1999			
NEWWS 2001			
Ontario 2001			
Timepoint 2			
CJF 2002			
CJF GUP 2000			
FTP 2000			
California GAIN 1994		x	
MFIP 2000			
Ontario 2001		x	
SSP Recipients 2002			

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(*Continued*)

Timepoint 3			
IFIP 2002			
IWRE 2002			
New Hope 1999			x
NEWWS 2001			
SSP Applicants 2003			
SSP Recipients 2002			
UK ERA 2011		x	

3. Child mental health

	Behavior problems total score (mean)	Behavior problems (% with problems)	Number of behavior problems	% at risk for depression
Timepoint 1				
CJF GUP 2000				
CJF Yale 2001		x		
New Hope 1999				
NEWWS 2001	x			
Ontario 2001			x	
Timepoint 2				
CJF 2002	x			
CJF GUP 2000				
FTP 2000	x			
California GAIN 1994				
MFIP 2000	x			
Ontario 2001				

(*Continued)*

SSP Recipients 2002				x
Timepoint 3				
IFIP 2002	x			
IWRE 2002	x			
New Hope 1999				
NEWWS 2001	x			
SSP Applicants 2003	x			
SSP Recipients 2002	x			
UK ERA 2011				

4. Child physical health		
	Mother reported health (mean score)	**Good/excellent or fair/poor health (%)**
Timepoint 1		
CJF GUP 2000		
CJF Yale 2001		
New Hope 1999		
NEWWS 2001	x	
Ontario 2001		
Timepoint 2		
CJF 2002	x	
CJF GUP 2000		
FTP 2000	x	
California GAIN 1994		
MFIP 2000		x

(Continued)

Ontario 2001		
SSP Recipients 2002	x	
Timepoint 3		
IFIP 2002		x
IWRE 2002	x	
New Hope 1999	x	
NEWWS 2001	x	
SSP Recipients 2002	x	
SSP Applicants 2003	x	
UK ERA 2011		

5. Employment status

	Currently employed (%)	**Currently employed FT (%)**	**Currently employed PT (%)**	**Ever employed since RA/in year of study (%)**	**Ever employed FT since RA/ in year of study (%)**	**Ever employed PT since RA/in year of survey (%)**
Timepoint 1						
CJF GUP 2000	x					
CJF Yale 2001	x			x		
New Hope 1999				x		
NEWWS 2001	x			x		
Ontario 2001						
Timepoint 2						
CJF 2002				x		
CJF GUP 2000						
FTP 2000				x		

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(*Continued*)

California GAIN 1994				x	x	x
MFIP 2000				x	x	x
Ontario 2001						
SSP Recipients 2002		x	x		x	x
Timepoint 3						
IFIP 2002	x	x	x			
IWRE 2002	x					
New Hope 1999		x		x		
NEWWS 2001	x	x	x	x		
SSP Applicants 2003	x	x	x			
SSP Recipients 2002	x	x	x			
UK ERA 2011	x	x	x	x		

6. Income

	Total income (USD/CAD)	**Earnings (USD/CAD/GBP)**
Timepoint 1		
CJF GUP 2000		
CJF Yale 2001		
New Hope 1999	x	x
NEWWS 2001	x	x
Ontario 2001		
Timepoint 2		

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(Continued)

CJF 2002	x	x
CJF GUP 2000		
FTP 2000	x	x
California GAIN 1994		x
MFIP 2000	x	x
Ontario 2001		
SSP Recipients 2002	x	x
Timepoint 3		
IFIP 2002	x	x
IWRE 2002	x	x
New Hope 1999	x	x
NEWWS 2001	x	x
SSP Applicants 2003	x	x
SSP Recipients 2002	x	x
UK ERA 2011		x

7. Welfare Receipt		
	Total benefit received (USD/CAD/GBP)	**Receiving benefits currently or in year of study (%)**
Timepoint 1		
CJF GUP 2000		
CJF Yale 2001		
New Hope 1999	x	x
NEWWS 2001		x
Ontario 2001		x

(Continued)

Timepoint 2		
CJF 2002	x	
CJF GUP 2000		
FTP 2000	x	
California GAIN 1994		
MFIP 2000	x	
Ontario 2001		x
SSP Recipients 2002	x	
Timepoint 3		
IFIP 2002	x	x
IWRE 2002	x	x
New Hope 1999	x	x
NEWWS 2001	x	
SSP Applicants 2003	x	x
SSP Recipients 2002	x	x
UK ERA 2011	x	x

8. Health insurance

	Adult Health Insurance	Child Health Insurance	Family Health Insurance
Timepoint 1			
CJF GUP 2000	x		
CJF Yale 2001	x		
New Hope 1999	x		
NEWWS 2001	x	x	

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(*Continued*)

Ontario 2001			
Timepoint 2			
CJF 2002	x		
CJF GUP 2000			
FTP 2000			
California GAIN 1994	x		
MFIP 2000		x	
Ontario 2001			
SSP Recipients 2002			
Timepoint 3			
IFIP 2002			x
IWRE 2002			
New Hope 1999	x	x	
NEWWS 2001			x
SSP Applicants 2003			
SSP Recipients 2002			
UK ERA 2011			

Appendix 6. MDRC sequence generation procedure

Provided by Cynthia Miller, MDRC 16/9/11

”Like comparable research organizations, MDRC’s random assignment process is regulated by a control file consisting of the sequence of assignment values. Simple random assignment is not generally used at MDRC because social programs often have quotas related to how many individuals can be served in a given site during a given time period. Therefore, MDRC goes to great lengths to make control files as unpredictable as possible while at the same time avoiding localized ‘bad draws’ that could adversely affect program operations. MDRC’s random assignment process is regulated by a ’“sequence”’ file consisting of the ordering of assignment values. Each project gets its own sequence file - they are never reused. The sequence files are constructed by defining blocks of assignments, each block made up of different sizes configured to approximate the intended random assignment ratio. The size of the blocks generally average over 20 assignments; the assignments within each block are randomized using an available random number generator with a uniform distribution. We generally use 9 different block sizes, and the order of block sizes is also randomized, with each block size occurring exactly once within a ‘superblock’ of the 9 block sizes. Each superblock is calculated to generate precisely the targeted random assignment ratio. Within a superblock a given block may not necessarily exactly match the target random assignment ratio, but any deviance from

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the target in one block will be compensated for in another block. This is especially necessary when an odd-sized random assignment ratio is specified (e.g., 55:45).

Each time a control file is generated, we produce many more versions than we need. For each version we calculate an entropy measure that reflects the distribution among configurations of possible subpatterns within the control file. We use this entropy measure to gauge the extent to which a given sequence file could reveal information from the pattern of past assignments to help anticipate future assignments. For the research study we will choose a sequence file from among the ones generated that contain the highest overall entropy; that is, they tend to have a more uniform distribution of distinct subpatterns. The higher the entropy the more likely it is that subpatterns are equally likely and therefore are unpredictable. For example, within the constraints of the overall average block size, maximizing the entropy of the sequence would mean that a subpatterns like "ECECEC", "EEEEEE", "CECCCE" and "EEECCC" would all tend to be equally likely within the overall sequence file."

WHAT'S NEW

Last assessed as up-to-date: 16 April 2016.

Date	Event	Description
14 February 2018	New citation required but conclusions have not changed	Republished to allow open access. No changes to the July 2017 published text

HISTORY

Protocol first published: Issue 5, 2012

Review first published: Issue 8, 2017

Date	Event	Description
12 January 2017	Feedback has been incorporated	Responses to second external reviewer's comments incoporated
8 November 2016	Feedback has been incorporated	Responses to first external reviewer's comments incorporated
3 August 2016	Feedback has been incorporated	Internal reviewers' amendments following responses to first round of comments have been incorporated
24 May 2016	Feedback has been incorporated	Responses to internal reviewers' comments incorporated.

CONTRIBUTIONS OF AUTHORS

Conceived study: MG, HT, CB, LB.

Secured funding: MG, HT, CB, LB.

Drafted the protocol: MG, KB, HT, LB, CB.

Developed and implemented the search strategy: CF, MG.

Selected studies: MG, KB, VL.

Extracted and interpreted data from studies: MG, KB, MJM, VL, SPM.

Assessed risk of bias: MG, VL.

Assessed quality (GRADE): MG, HT.

Entered data into RevMan: MG, MJM, SPM.

Carried out the meta-analysis: MG.

Interpreted the analysis: MG, HT.

Draft the final review: MG.

Methodological advice: HT, LB, CB.

Comments on review: HT, LB, CB, KB, MJM, VL, SPM.

Disagreement resolution: CB, HT.

DECLARATIONS OF INTEREST

MG: none known.

HT: none known.

KB: none known.

VL: none known.

MJM: none known.

SPM: none known.

CF: none known.

CB: none known.

LB: none known.

SOURCES OF SUPPORT

Internal sources

- Chief Scientist Office, Scottish Government Health and Social Care Directorates, UK.

Core funding to Evaluating the Health Effects of Social Interventions Programme, MC˙UU˙12017/4 (to June 2015)

- UK Medical Research Council/Chief Scientist Office, UK.

Core funding to Informing Healthy Public Policy Programme, MC˙UU˙12017-15 (from June 2015)

External sources

- Chief Scientist Office, Scottish Government Health and Social Care Directorates, UK.

Grant code CZG/2/422: Grant funding to £50,000

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

The original title of the review was ”Welfare to work interventions and their effects on health and well-being of lone parents and their children“. This title was developed at an early stage of the review, when the intention was to include a wide range of psychosocial outcomes. The range of outcomes was subsequently restricted to measures of mental and physical health. We have therefore changed the title to ”Welfare-to-work interventions and their effects on the mental and physical health of lone parents and their children“. We have also amended the primary objective to reflect this change.

The review has been focused on high-income countries since the outset, but this was not originally reflected in the objectives. We have amended the primary objective to state that the focus is on interventions conducted in high-income countries.

We excluded studies with fewer than 60% lone parents.

We did not develop a more detailed typology of interventions because we found that in practice, interventions of different types did not necessarily differ from each other.

We did not calculate or assess welfare dependency, as we deemed that it would not be a useful outcome measure.

The time points used to analyse and present data differ slightly from the intervals stated in the protocol, as having collected data and established the actual distribution of studies and follow-up times, the division of intervals used provided the optimal spread of follow-up times and number of studies within each interval.

The protocol stated that I^2 above 75% would trigger a decision to conduct narrative synthesis. Greater understanding of meta-analysis methods led to the decision to employ a threshold of 60% for post hoc sensitivity analysis investigating intervention characteristics or components as potential explanatory factors where there was an obvious outlier. We chose 60% as it is the upper end of the range defined as moderate in Higgins 2011a.

We did not use sensitivity analysis to investigate decisions made during the review. The decisions specified in the protocol concerned participant characteristics (all studies included employed and couple parents) and level of bias (all studies were at high risk of bias). Sensitivity analysis was used post hoc to investigate the influence of intervention characteristics on effects.

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INDEX TERMS

Medical Subject Headings (MeSH)

*Child Health [ethics]; *Health Status; *Maternal Health [ethics]; *Mental Health; Employment [economics; ethics; legislation & jurisprudence; *psychology]; Income; Insurance, Health [statistics & numerical data]; Poverty; Randomized Controlled Trials as Topic; Single Parent [*psychology]; Social Welfare [ethics; legislation & jurisprudence; *psychology]

MeSH check words

Adolescent; Adult; Child; Child, Preschool; Humans; Infant

DATAGRAPHIC

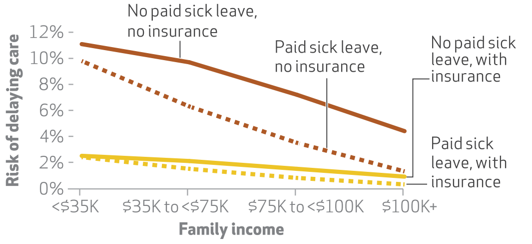
DOI: 10.1377/hlthaff.2016.1580

Workforce Health and Productivity

Work and health are inextricably linked. Most American adults spend nearly half of their waking hours at work. Poor physical and social working conditions are associated with chronic illnesses such as hypertension, and work-related accidents and illnesses claim billions of dollars in health care costs and lost productivity annually. Racial and ethnic minorities disproportionately work in low-paying jobs and face increased risks of work-related injury or disability. Policies and benefits such as paid sick leave and unemployment compensation are associated with improved health outcomes. Employment is also the source for most nonelderly Americans' health insurance coverage. Some companies promote healthy lifestyle choices through wellness and other programs, understanding that healthy, productive workers contribute positively to the bottom line.

Sick leave and medical care

Risk of delaying medical care in the last 12 months due to cost,by income levels, 2013

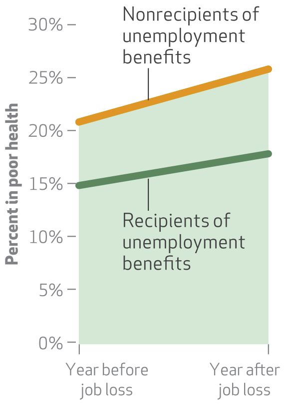


70% of the U.S. civilian population working full time (but only 19% of part-time workers) have paid sick leave benefits

See DeRigne et al, *Health Affairs*, March 2016

Unemployment benefits and health

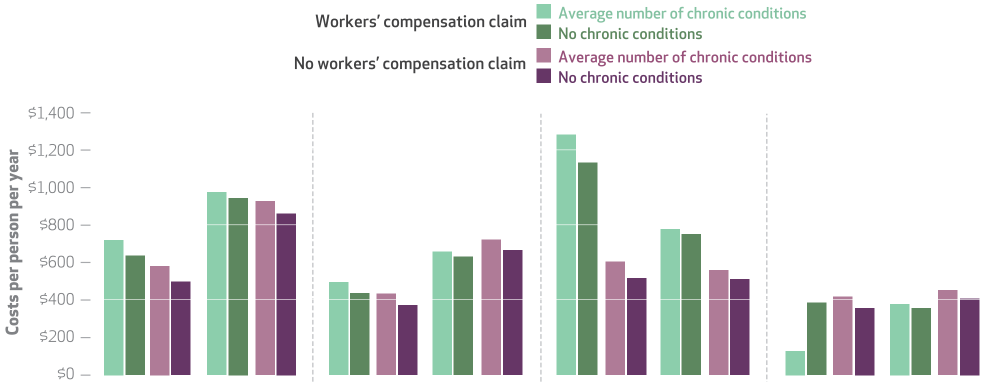
Percentages of individuals reporting poor health, before and after job loss



See Cylus and Avendano on page 293

Absenteeism and presenteeism costs

In a study of nearly 17,000 workers at 314 diverse companies in Colorado, absenteeism costs were highest among those who reported having only physical job difficulty. Presenteeism (job performance) costs were greatest among employees who reported having both cognitive and physical job difficulty, regardless of their chronic health conditions or compensation claim history.



See Jinnett et al. on page 241

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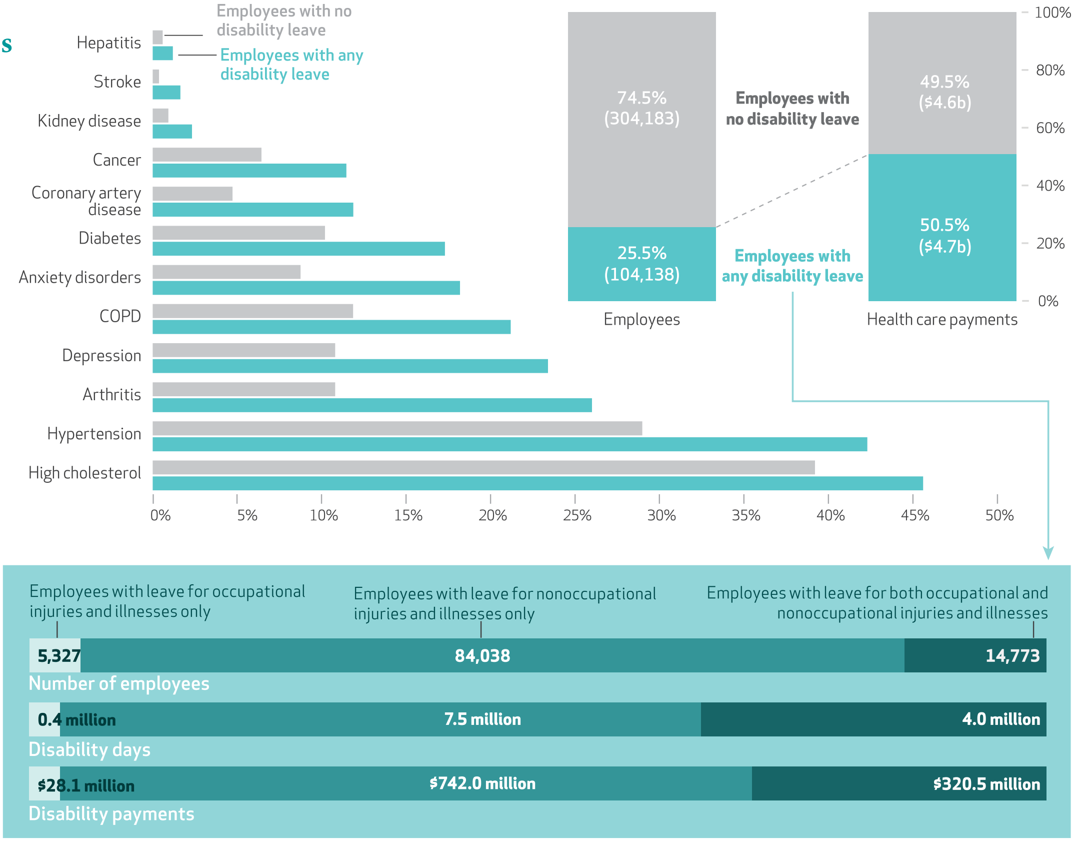
Health care cost and disability leaves

2008–12

About one in four employees in a cohort of 408,000 US workers took a temporary leave of absence due to illness or injury in 2008–12. They accounted for more than half of the total medical and pharmaceutical payments made through their employer's health insurance plan. Those who took disability leaves had a relatively higher prevalence of chronic conditions such as high cholesterol, hypertension, arthritis, and depression compared to those with no disability leaves.

$1.8b

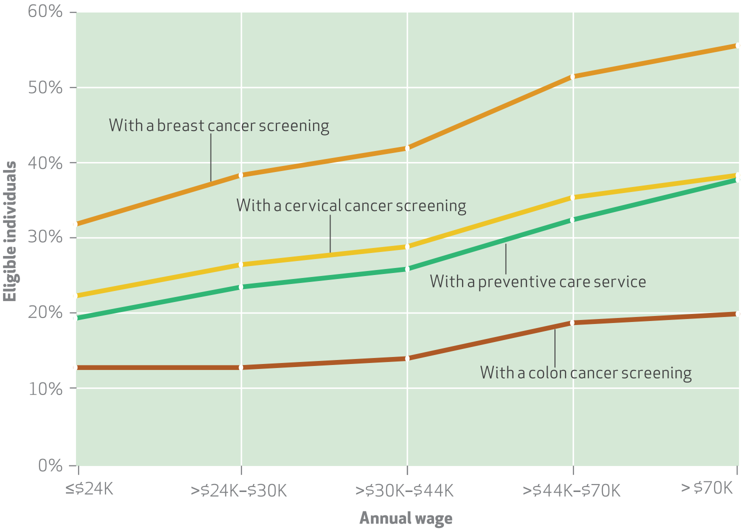
Lost productivity from disability leaves



See Gifford on pages 245–8

Preventive care use by wage

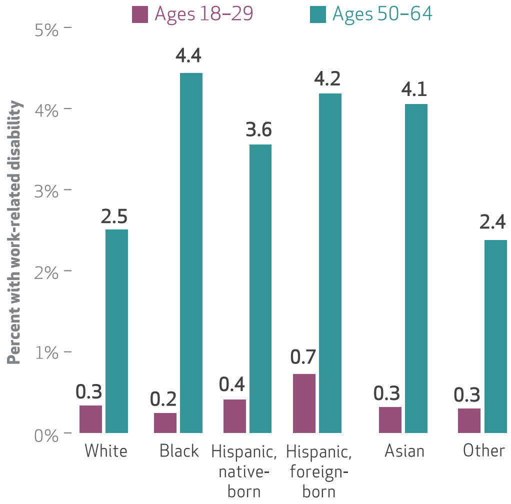
In a study of nearly 43,000 employees with commercial insurance in 2014, those in the lowest wage group have significantly lower rates of preventive care utilization and recommended cancer screenings than those in the highest wage group. Despite having health insurance, low-wage workers still face numerous obstacles to obtaining needed care.



See Sherman et al. on page 255

Work-related disabilities by age and race

Occupational injuries and illnesses lead to significant health care costs and productivity losses. Compared to non-Hispanic whites, ethnic and racial minorities have higher percentages of work-related disability.



See Seabury et al. on page 270

For a full list of sources, click on the Appendix link in the box to the right of the article online.

5W Infographics

Health Affairs

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Campbell *et al. BMC Public Health* (2016) 16:188
DOI 10.1186/s12889-016-2880-9

BMC Public Health

RESEARCH ARTICLE Open Access

Lone parents, health, wellbeing and welfare to work: a systematic review of qualitative studies

Mhairi Campbell*, Hilary Thomson, Candida Fenton and Marcia Gibson

Abstract

Background: Lone parents and their children experience higher than average levels of adverse health and social outcomes, much of which are explained by high rates of poverty. Many high income countries have attempted to address high poverty rates by introducing employment requirements for lone parents in receipt of welfare benefits. However, there is evidence that employment may not reduce poverty or improve the health of lone parents and their children.

Methods: We conducted a systematic review of qualitative studies reporting lone parents' accounts of participation in welfare to work (WtW), to identify explanations and possible mechanisms for the impacts of WtW on health and wellbeing. Twenty one bibliographic databases were searched. Two reviewers independently screened references and assessed study quality. Studies from any high income country that met the criteria of focussing on lone parents, mandatory WtW interventions, and health or wellbeing were included. Thematic synthesis was used to investigate analytic themes between studies.

Results: Screening of the 4703 identified papers and quality assessment resulted in the inclusion of 16 qualitative studies of WtW in five high income countries, USA, Canada, UK, Australia, and New Zealand, covering a variety of welfare regimes. Our synthesis found that WtW requirements often conflicted with child care responsibilities. Available employment was often poorly paid and precarious. Adverse health impacts, such as increased stress, fatigue, and depression were commonly reported, though employment and appropriate training was linked to increased self-worth for some. WtW appeared to influence health through the pathways of conflict and control, analytical themes which emerged during synthesis. WtW reduced control over the nature of employment and care of children. Access to social support allowed some lone parents to manage the conflict associated with employment, and to increase control over their circumstances, with potentially beneficial health impacts.

Conclusion: WtW can result in increased conflict and reduced control, which may lead to negative impacts on mental health. Availability of social support may mediate the negative health impacts of WtW.

Key words: Lone parent, Welfare to work, Health, Wellbeing, Systematic review, Qualitative synthesis, Welfare reform

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Background

Lone parents and their children have poor health and social outcomes, disproportionately experiencing depression [1, 2], psychiatric disease, attempted suicide, alcohol and drugs-related disease [3], poor educational outcomes [4], and school behaviour problems [5]. Lone mothers in the UK are twice as likely as partnered mothers to describe their health as 'not good' (13 % compared to 7 %) [5]. Much of these adverse outcomes can be attributed to high rates of poverty among lone parents [6–9]. In 2014, 42 % of children in UK lone parent households were poor compared to 23 % in couple households [10]. In many high income countries, employment rates are lower for lone parents than couple parents [9]. In the UK, 63 % of lone parents were in employment compared to 72 % of partnered mothers in 2014 [11]. Governments around the world have attempted to address the issue of lone parent poverty by implementing policies designed to promote employment.

Government policies to promote employment include making receipt of welfare benefits conditional upon efforts to find work. Beginning in the United States in the 1990s, eligibility restrictions, often based on the age of the youngest child, have been introduced in many other Organisation for Economic Cooperation and Development (OECD) countries [12]. The age of youngest child when lone parents are expected to seek employment varies between countries, from under one year old in some US states, and is currently 5 years in the UK [12]. Such welfare to work interventions (WtW) require benefit claimants to prove that they are actively seeking employment, or to participate in training programmes intended to improve employability. Failure to comply with these requirements can lead to financial sanctions. In addition to poverty reduction, rationales for these policies include reducing public expenditure [13] and improving health [14]. Employment may promote increased income, improved parental confidence and consequently enhanced parenting [15]. However, available evidence suggests that employment does not necessarily reduce poverty among lone parents [16]. Despite high employment among lone mothers in Sweden, lone mothers have worse self-reported health than partnered mothers [17], and participating in welfare to work in the USA has been found to reduce cases of anxiety but increase those of depression, with variance among subpopulations [18]. A substantial body of experimental studies on the impacts of WtW on lone parents and their children is currently being synthesised in a systematic review [19]. Twelve randomised controlled trials are included in the review and a preliminary synthesis indicates that impacts on economic outcomes and on measures of adult and child health are small but mostly positive [20].

The impacts of participation in WtW on the health and wellbeing of lone parents and their children, and the mechanisms involved, are unclear. This study contributes to understanding of these by systematically reviewing qualitative studies reporting lone parents' accounts of participating in WtW, focussing on identifying mechanisms linking their participation with health, wellbeing, and socio-economic determinants of health. Evidence from qualitative studies can provide insights into the mechanisms linking interventions with health and wellbeing [21], and into respondents' experiences of the intervention. It can also further understanding of the influence of contexts and personal characteristics on individual responses to the intervention [22].

Methods

For this systematic review, the inclusion criteria were studies that included lone parents; who were participating in WtW programmes; and reported data on health or wellbeing. The review included studies of lone parents and their dependent children living in OECD countries with established social welfare systems. As definitions of lone parents and dependent children can vary slightly between countries and interventions, the review included studies involving lone parents and their children as defined by the study authors. Studies were excluded where there was a mix of lone and couple parents or where the co-habitation status of the parent was unclear. Mandatory WtW interventions were included; studies where participation in the WtW initiative was voluntary and there was no link with benefit eligibility were excluded. Studies were included if there was reference to health or wellbeing (as defined by the study author). Particular areas of interest were experiences and accounts of WtW interventions in relation to the health and wellbeing of participants and their children, and to social determinants of health. Health and wellbeing were conceptualised broadly to encompass stress levels, energy, impact on relationships, managing everyday tasks, and confidence, in addition to physical or mental health conditions. Research from any discipline or theoretical tradition that used recognised qualitative methods of data collection and analysis was included. In accordance with good practice for systematic reviews, the study protocol is available [23], and PRISMA reporting guidelines were used [24].

Literature search and screening

The search strategy was developed by CF, an information scientist, with contribution from MG. CF conducted the searches in 2009 and 2013. Key search terms were selected to source literature on 'lone parents' and 'welfare to work'. Additional file 1 provides an example search strategy, search terms, and a full list of databases searched. A full search strategy for each electronic database is available from the authors. Twenty one electronic bibliographic databases of peer reviewed articles and grey literature were searched with no date or language limits. For non-English language texts, we were able to ascertain either by the title or English language abstract whether articles were relevant to the review. Two reviewers (MG and MC) independently screened the search results by title, then by abstract. The full text was then screened to establish inclusion decisions. Disagreements were resolved by discussion within the review team.

Quality assessment of qualitative studies

The quality assessment criteria for qualitative studies were based on those developed by Dixon-Woods et al. [25]. The criteria focus on the transparency and appropriateness of methods used (see Additional file 2). Each study was assessed independently by MG and MC to ascertain whether the research questions, sampling, data collection and analysis were clearly reported and suited to qualitative enquiry, claims made were supported by sufficient evidence, and the paper made a useful contribution to the review question. The results were compared and any differences re-examined and resolved through discussion. Studies were excluded if they did not report any qualitative data, did not use qualitative methods for analysis, or did not make a useful contribution to the review question as assessed by the reviewers.

Extraction and synthesis

The full text of included studies was imported into NVivo software. Analysis of the extracted data drew on thematic synthesis, a methodology designed to enhance the transparency of synthesising qualitative data and facilitate the construction of new analytical themes from the collated data [26]. Each reviewer (MC, MG, HT) independently assessed three included papers then discussed initial thoughts on broad descriptive coding themes. Line-by-line coding by MC on the findings and discussion sections of six papers identified 30 codes. These codes were organised into five broad descriptive themes, based on the content of the codes and the authors' knowledge of socio-economic determinants of health. These were then used by MC to conduct line-by-line coding of the remaining included papers. The reviewers met regularly to discuss and agree coding as it developed. A summary of the coded text was collated by MC. This summary was then used by MC, MG and HT to identify analytical themes emerging from the descriptive themes across the included studies, in accordance with the interpretive stage of thematic analysis [26].

Results

The searches identified 4703 papers. Following screening, we identified 19 articles reporting 16 studies of compulsory WtW interventions or programmes (Fig. 1), which met the inclusion criteria for the review. Seven were conducted in the USA, three in Canada, three in the UK, two in Australia and one in New Zealand, totalling 724 participants. While the studies met the quality criteria, the recruitment processes of several were ambiguous, and there was variation in the depth of useful information. The results of the quality assessment are shown in Online Appendix B. Studies focused on a variety of aspects of the experience of lone parents involved in WtW. While not all studies reported on every aspect of interest to the review, all presented data for the synthesis. Table 1 provides an overview of the research questions, focus, and methods of the included studies. Many participants moved frequently between WtW and employment. Therefore, respondents and study authors often did not distinguish between the impacts of participating in WtW and being in employment. Further, for many WtW participants the demands of WtW and employment were similar, again leading to a lack of differentiation between the two scenarios. Nonetheless, we aimed to maintain clarity on whether any impacts described were attributed to WtW or subsequent employment.

Contextual information describing the respondents' experiences of being a lone parent and dependent on welfare benefits was provided by all of the included studies. Several studies noted that lone parents were at higher risk of role strain than two parent families [27–30], as they had less support with their domestic role, parenting duties and coping with the effects of poverty [27–31]. Combinations of circumstances, including health problems, care of extended family members, dangerous neighbourhoods, violence, frequent enforced residential moves, homelessness and domestic violence meant many lone parents struggled to cope with domestic obligations, and made trying to find and maintain work extremely difficult [29, 30, 32, 33]. Some North American studies noted that few respondents had formal qualifications [28–30, 32]. There was limited information on the age of participants across the studies. In general, the age of participants tended to range from early twenties to over 50 years. While four of the studies did not give information on the age of participants, there was no overall emphasis in the other 12 studies on young lone mothers. As Oliker observed, teenage lone mothers are usually guided towards education programmes [30].

We identified five broad themes relating to lone parents' experiences of participating in WtW: domestic role; the WtW system; employment; economic circumstances; and health and wellbeing. The themes we identified were overlapping and at times mutually reinforcing. Insights relating to each of these key themes are presented below.

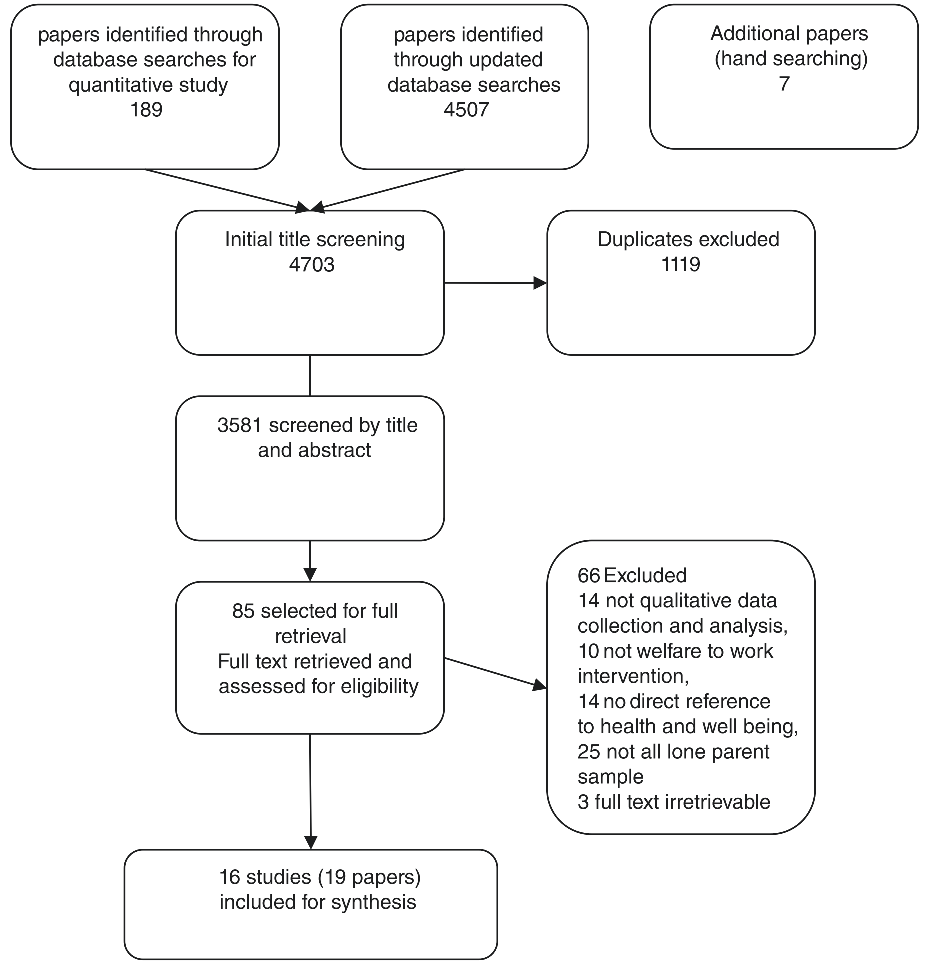

Fig. 1 Flow chart of literature search

Domestic role

Respondents' domestic role entailed having sole responsibility for caring and providing for their children, managing their household, and organising childcare during WtW and employment activities. When WtW requirements conflicted with sole responsibility for parenting, such as lack of childcare during WtW activities, caring obligations usually took priority [27, 29, 30, 34–37]. This need to prioritise care of children could impact on participants' ability to maintain work [28, 35], resulting in absences and financial sanctions or loss of wages [38, 39]. Within the broad theme of domestic role, there were issues of 'parenting', i.e. care and safety of children, which was distinct from 'childcare', i.e. the supervision of children by others when the parent was involved in WtW activities. These sub-themes, along with social support, are described in more detail below.

Parenting

There were mixed reports within studies on how WtW impacted on the participant's role as a parent. Participation in WtW made some respondents feel they were a good role model for their children and facilitated more positive parenting [27, 33]. However, several studies noted that gaining employment could lead to conflict; while the parent could gain money and self-worth, less time was available to spend with children [27, 30, 33, 37, 39]. "*I may have more money but I don't have more time and time is important because you can be skint and be a wonderful mother..*"([37], page 62). Exhaustion could lead to harsher parenting [27] and inability to supervise children "*There were times I came home from work and fallen asleep when she's in a tubful of water.*" ([30], page 183).

Parents also had concerns about the safety of their children due to the requirements of WtW or subsequent employment conflicting with available childcare [30, 37, 40]. This sometimes led to older children looking after younger siblings [28, 30]. One US study reported that a participant had to leave her five year old child supervising her three year old for an hour every morning [30]. Lack of supervision for younger teenagers was a concern [29, 30], particularly when

Table 1 Included studies of lone parents' experience of compulsory welfare to work

Study papers	Country	Year	Data collection	Recruitment	Sample no.	Focus of paper(s)
Baker 2002 [44], 2004; Baker & Tippin 2002 [31]	New Zealand	2001	Face to face interviews	All eligible claimants in study area invited	120	2002, 2004: impact of poor health on gaining and maintaining employment 2002: demands of meeting parenting, welfare and work requirements
Breitkreuz et al. 2010 [27]	Canada	2001, 2002	Face to face interviews	Via social service agencies, employability programmes and snowballing	17	Impact of unpaid domestic duties and employment for welfare to work lone parents
Critelli et al. 2010 [38]	USA	Prior to 2007	Telephone interviews	Eligible claimants on foster agency lists	100	Impact of welfare to work policies on lone foster parents
Good Gingrich 2010 [39]	Canada	2006 - 2010	Face to face interviews (peer)	Purposive sampling	42	"lone mothers' experiences of the design, delivery, and enforcement of workfare"
Grahame & Marston 2012 [40]	Australia	2008, 2009	Interviews	Purposive sampling from eligible participants of welfare to work records	21	Wellbeing of welfare to work lone parents: dependency and development of autonomy
Haux et al. 2012 [37]	UK	2009, 2010	Face to face interviews (peer)	Single Parent Action Network participants, Citizens Advice and Job Centre Plus invite, social network sites	50	Experience of welfare to work assistance and implications for wellbeing
Hildebrandt 2002 [34]; Hildebrandt & Kelber 2005 [28]	USA	1999 - 2000	Face to face interviews (peer)	Snowball sampling	34	2002: Effect of welfare to work on lone parents' health and wellbeing 2005: Perceptions of lone parents of their health and wellbeing while on welfare to work
Hildebrandt 2006 [29]	USA	2000	Face to face interview	Purposive sampling from participants in work-based welfare programme, snowballing	31	Barriers to maintaining welfare to work participation
Hildebrandt & Ford 2009 [32]	USA	2007 - 2009	Face to face interviews	Community based purposive sampling	41	Barriers to success when lone parents are removed from welfare after the 5 year time limit
Lane et al. 2011 [33]	UK	2011	Interviews	Welfare to work records	60	Experience of welfare to work
McArthur et al. 2013 [41]	Australia	2009	Telephone interviews, focus groups	Social security social workers invite potential eligible participants	48	Lone parents' encounters with welfare to work process, in particular the most in need feeling under greatest scrutiny
McPhee &Bronstein 2003 [36]	USA	1999	Face to face interviews	All participants of (un-named) welfare to work programme	39	"Effect of welfare reforms on lone parents' perceived ability to care for themselves and their family"
Oliker 1995 [30]	USA	1987 - 1992	Face to face interviews, observation	Participants of job search, job training programmes	30	How welfare to work lone parents make decisions about work in relation to their domestic obligations
Peacey 2009 [42]	UK	2009	Telephone interviews	Callers to helpline/participants of employability programme/internet site	34	Experience of lone parents as they move from non-conditional welfare benefits to welfare to work
Pollack and Caragata 2010 [43]	Canada	2005 - 2009	Face to face interviews	Adverts in social services offices, snowballing, referrals from welfare workers	42	"how lone mothers construct their own subjectivity" in relation to workfare
Selekman and Ybarra 2011 [35]	USA	? 2006	Face to face interviews	Random selection of participants from larger study who had increased income	15	The facilitators for welfare to work lone parents who gain paid employment

the only affordable housing was in neighbourhoods where it was unsafe for children to play outside or travel to school, and they risked coming into contact with gangs [30, 32].

Childcare

Difficulties finding formal or informal childcare that was affordable and safe exacerbated the challenges involved in complying with WtW requirements or subsequent employment [30, 38]. Participants experienced problems finding childcare that was: reliable and regular [27, 30, 37]; affordable [28, 30, 33, 37]; local, and flexible in order to accommodate short notice changes to hours, extra shifts, or school holidays [37]. Some respondents required specialised childcare for children with developmental or behavioural conditions [37, 38]. Lack of suitable childcare was a barrier to gaining employment [27, 30, 35, 37, 38].

Social support

Strong social support from family or friends, often in the form of informal childcare, was important in aiding participants to move successfully into paid employment [27, 30, 33, 35, 37]. Informal childcare was essential for some respondents, and was the only way to cope with combining unpredictable demands such as a child's illness, with WtW or employment [30]. However, the level of social support available to respondents varied between individuals and over time [30, 35–37], with some participants having no access to social support [28, 30, 33, 39]. Even when available, informal social support could be unreliable, as the provider's circumstances were often as unpredictable as those of the respondent [30, 33, 35].

Welfare to work system

Several studies noted that WtW staff did not recognise the implications of being a lone parent [31, 36, 39, 41]. For example, a lone parent without child care provision was not allowed to bring her children to appointments [29, 39]. Participants often felt staff treated them with a lack of respect [29, 32, 33, 36, 37, 42, 43]. Welfare staff not fully understanding the implications of receiving various benefits caused problems as many respondents received intricate, interconnected benefits relating to their lone parent status [39]. This was exacerbated by lack of staff continuity which required respondents to explain their circumstances afresh at every appointment [37, 41, 42], short appointment slots [33], and difficulties contacting welfare staff outside regular appointments [29, 36].

There was often an emphasis on quick placement into poor quality employment [29, 30, 32, 34, 39]. One study noted a lack of appreciation of participants' relevant skills (e.g. knowledge of children, caring roles) [40]. Training programmes that helped respondents gain basic level education [28], or computer skills [39, 42] were reported to increase respondents' confidence. Some programmes addressed the broader problems many lone parents experienced by including methods of coping with stress [39], while others offered routes to assistance for domestic abuse [28]. However, frequently training did not lead to recognised higher qualifications and was too basic to be useful [39, 42]. Two studies reported that rather than encouraging participants to take control of their circumstances, the emphasis was on compliance with WtW requirements [39, 43]. Some respondents had little control over which WtW activities they attended [39, 41, 42].

Employment

For some respondents, employment led to increased income [27, 37] and confidence [27, 33, 37]. Some study participants expressed ambitions for the future and a desire to work [31, 32, 36, 39, 40], *"I think I could be a social worker, a nurse, a dental assistant. I think I could do anything that involves helping people and making sure that people are happy."*([39], page 114). However, the employment opportunities available to respondents were typically: at or near the minimum wage [30, 37]; physically demanding [30]; lacking autonomy [37]; and had limited potential for career development [33]. Many jobs involved working atypical hours outside those of regular formal childcare, inconsistent shift patterns, and long hours [27, 30, 35, 37]. Jobs were often short term [35], resulting in frequent repetition of WtW procedures including benefit applications, job searching and the upheaval of reorganising domestic arrangements to accommodate a new job [30].

Support offered by employers or co-workers could be as important as the level of pay [35], and an important factor in the sustainability of employment [37, 44]. Such support included understanding respondents' circumstances and offering some flexibility for family related events [35, 37]. However, one study found some participants hid their lone parent status to avoid employer prejudice that lone parents were unreliable employees [31].

Economic circumstances

Some studies reported that low income from welfare benefits caused financial insecurity for some recipients [29, 37, 39]. Routine discretionary decisions by case managers and benefit payment errors could result in sudden and unpredictable changes to essential income sources [35, 39, 41]. Low income from WtW benefits or poorly paid employment led to arrears in utility bills [42], rent payment [33, 37, 42], eviction [30], and restrictions on the family food budget [29, 31, 37, 39].

Several studies reported that even where respondents achieved full-time employment they experienced financial insecurity [27, 30, 33, 34], often relying on associated welfare benefits to meet employment incurred costs (e.g. childcare) [35]. For some, the cost of formal childcare was too high for a minimum-wage job to be economical [28, 33, 37].

Successfully achieving part-time employment that paid enough for participants to feel 'better off' was positive [27]. Associated in-work benefits (such as Working Tax Credit in the UK) were helpful to participants in maintaining employment [33, 37]. However, small increases in earnings could cross eligibility thresholds for other essential benefits (e.g. housing), leading to reductions in total income [30, 35, 37]. Many participants lacked any financial safety net and so were vulnerable to negative economic impacts if they lost employment or were removed from WtW [30]. The authors of two studies raised concern that inadequate income from WtW could force some respondents to turn to criminal acts to support their families [36], prevent women from leaving abusive relationships, or force participants into unsuitable relationships to obtain accommodation [29].

Health and wellbeing

A high proportion of respondents or their children suffered from ill health which restricted their ability to take part in WtW or employment [27–29, 32, 37, 38, 41, 44]. Mental illness, depression [33, 44]; and children's behaviour problems [30, 44] were barriers to successful participation in WtW and subsequent employment.

For some, involvement in WtW and employment exacerbated ill health [27–30, 33, 34, 37, 39, 41, 44]. While studies mentioned both physical and mental health, few studies elaborated on the effects of WtW on physical health.

Many respondents reported that participation in WtW increased stress [29, 33, 37, 39, 41]. The combined pressures of domestic obligations, involvement in WtW, employment requirements and financial insecurity were linked to poor mental health (stress, anxiety, panic attacks) [27, 33, 39, 41, 44], depression [28, 29, 34, 37, 39, 44], and fatigue [27, 30]. *"My health before work-based welfare was all right, but now…my health is not on the good side. I do be getting depressed and I am going to see a therapist for it."* ([34], page 366).

Participation in WtW could contribute to low self-esteem and low self efficacy, the attributes respondents often required to improve their chances of gaining employment and independently supporting themselves and their families [43]. For many, WtW was experienced as stigmatising [31, 33, 37, 39, 40, 43, 44], and questions could be perceived as humiliating and intrusive [36, 39, 41, 43, 45].

There were a small number of reports of beneficial effects. WtW increased some participants' self-worth [27, 28, 33, 37], and for some led to increased confidence in their ability to gain employment [28, 37], particularly, in one study, for those who had previous employment experience [37].

Overarching issues of conflict and control

Across each of the descriptive themes identified, analytical themes of conflict and control emerged from participants' reported experiences of participating in WtW and attempting to gain and maintain employment in combination with their parenting and domestic obligations.

Within the descriptive theme of domestic role, there was conflict between participants' obligations to provide care for their children and requirements to participate in WtW activities, away from their children. Control over decisions regarding care of children was removed from respondents and dictated by WtW programmes [37, 40, 42].

Within WtW systems there was often conflict between the type of training available to respondents and what respondents required or aspired to [32, 39, 41, 42]. Respondents frequently lacked control over the type of employment applied for, with the expectation that they apply for any employment, regardless of suitability [30, 34, 37].

The nature of employment generally available conflicted with the flexibility required when raising children alone. The jobs most likely to be available to respondents (who often had few educational or vocational qualifications) offered little control over shift times and days worked, and little autonomy within the job role [27, 33–35, 37].

Problems arising from low income were frequently exacerbated by fluctuations beyond their control resulting from WtW processes. Inadequacy and fluctuation in income conflicted with participants' need to provide adequately for their children [33, 34, 37].

The poor health and wellbeing of many respondents and their children conflicted with the requirements of WtW [27, 29, 32, 34, 37, 38, 41, 44]. This was compounded when involvement in WtW impacted on respondents' health [27–30, 33, 34, 37, 39, 41, 44]. Trying to cope with combining both welfare activities and domestic duties could result in health issues, such as stress, fatigue and depression.

Some participants tried to overcome these conflicts and establish as much control as they were able to over their circumstances. Their priority was care of their children and they tried to fit WtW and employment commitments around their children's needs, for example by trying to arrange WtW appointments within school hours, and seeking work suited to school hours and within easy travel of home, school, and childcare [27, 33, 37].

Discussion

This systematic review of qualitative data provides insight into how lone parents' involvement in mandatory WtW impacts on health and wellbeing. The potential health impacts of WtW, an upstream determinant of health, on a population vulnerable to health inequality are of international significance with the implementation of WtW policies in many high income countries. This evidence synthesis included studies from five high income countries, covering a variety of welfare regimes.

We analysed data on the experiences of lone parents to uncover explanations of how participating in compulsory WtW may improve or worsen health and wellbeing. The directly conflicting demands of WtW activity and caring for children, and the loss of control over decisions regarding employment, childcare, and training, were reported to lead to stress, fatigue and poor mental health. While the majority of findings were about negative impacts of WtW, some respondents found participation in WtW a positive experience, benefiting from training and experiencing increased self-esteem.

Strengths and limitations

The review followed a protocol and rigorous review methods, with a PRISMA checklist [24] used to guide reporting (see Additional file 3). As with all reviews, publication bias may exist; studies reporting equivocal findings may not have been published. This review included studies from five high income countries, and thus may have a relevance to other higher income countries with similar welfare programmes. However, context, such as the particular circumstances of the lone parents and the training and support provided by WtW programmes, is important to qualitative studies and should be taken into consideration when interpreting the conclusions. Several of the included studies did not detail methods of recruiting respondents, therefore we cannot rule out the possibility of selection bias through recruitment of either more disadvantaged or more successful participants. However, within the review there were diverse experiences of WtW, including participants who had succeeded in gaining employment and participants who had struggled in WtW. These diverse experiences strengthened the synthesis and interpretation of conflict and control in relation to WtW for lone parents.

Work-family conflict for lone parents in WtW

Previous research has reported that a lack of work-life balance is associated with poor health [46], and Greenhaus and Beutells' work-family conflict theory may help to frame the findings of our review [47]. Work-family conflict theory proposes three mechanisms through which an individual's employment role can impact on their family role: time devoted to work; strain from participation in work; and particular behaviours required by work [47]. It has been suggested that lone mothers participating in WtW experience work-family conflict in similar ways to working mothers. This can occur when the requirements of WtW: conflict with care of children due to long or atypical hours; cause fatigue, stress, or overwhelm the participant; or impede family duties [48]. In this review, there was evidence of each of these work-family conflict mechanisms, and these were found to impact on health and wellbeing by contributing to stress, fatigue and poor mental health.

WtW and control

Many aspects of WtW reduced participants' ability to exercise control, particularly relating to care of children, training and employment. Lack of educational qualifications and employment experience, in addition to domestic obligations, meant participants often had little control over the type of employment available to them. This meant that many lone parents in these studies could only access precarious employment, now proposed as a social determinant of health [49]. Lack of control in these areas may link WtW participation with poor health and wellbeing. Constraints on welfare claimants' levels of control have been found for lone parents [50] and in other welfare populations [51]. When experiencing employment insecurity, the ability of individuals to make positive changes which could improve their health can be affected by their perceived control as well as structural factors [52]. Lack of perceived control of circumstances has been connected to poor health through psychological and biological pathways [53–55]. Lack of control may trigger chronic stress, leading to negative emotions and depression [53–55]. These conditions may also lead to negative biological impacts on the immune and cardiovascular systems [56].

This review found that many participants were unable to gain control of their circumstances and reported poor wellbeing, particularly stress. For some respondents, taking control of their lives involved removing themselves, temporarily or long term, from WtW, as they could not maintain sufficient care for their child [30, 36]. This is consistent with evidence of increasing 'disconnection' from work or welfare in the United States. In 2011/12, over five million children in the US lived in disconnected families [57], that is, with parents who are neither in work nor in receipt of welfare, and have no known source of income. There is some evidence that this is beginning to occur in the UK [58].

For lone parents who benefitted from WtW, higher control was facilitated through enhanced skills or qualifications, increased confidence in their employability, accessing employment that was compatible with caring responsibilities, and earnings sufficient to improve their standard of living. It may be that lone parents with positive experiences of WtW have less conflict to manage and greater control of their circumstances. Social support allowed some participants to manage conflicts between WtW and bringing up children alone. Positive social support might contribute towards a reduced level of conflict. Observational studies have found evidence that social support can assist in managing work-family conflict [59], and that supportive workplace practices increase perceived control and reduce conflict, resulting in lower rates of depression, blood cholesterol and other complaints [60]. However, social support can have negative impacts, expectations of reciprocal support (see review [61]), and as found in this review, many lone parents do not have a consistent support network.

Conclusion

This synthesis of the experiences of lone parents in mandatory WtW suggests that WtW participation may do little to improve lone parents' health and wellbeing or economic circumstances, often only leading to low paid, precarious employment. Conflict and control appear to be mechanisms that link lone parents' participation in WtW with health. The demands of parenting alone and employment are frequently in direct conflict, and lone parents are often denied control over major life decisions and everyday routines by WtW obligations.

While WtW may have potential to contribute towards improving health and wellbeing for lone parents, contextual mediating factors may act to counter this potential. In particular, unavailability of suitable employment, welfare assistance, childcare, and social support, may lead to WtW being counterproductive with respect to health and wellbeing. As employment requirements for lone parents in receipt of welfare are implemented internationally, increased awareness of the adverse impacts for many, and the potential for negative impacts on health and wellbeing due to the conflicts inherent in combining employment with raising children alone, may help to develop more effective interventions. WtW programmes which do not provide adequate training, emphasise placement in any available job, and do not recognise individual circumstances such as health problems are unlikely to lead to improved economic security and may be counterproductive for the health of lone parents. Therefore, while acknowledging the limitations discussed above, our recommendation based on the findings of this review are for further research on the health and wellbeing implications for lone parents of participating in mandatory WtW. In particular, there should be further investigation of how this vulnerable population can gain greater control of their circumstances, and how conflicts between lone parenthood and mandatory WtW can be resolved.

Additional files

Additional file 1: Bibliographic databases searched, search terms and example search. (DOCX 16 kb)

Additional file 2: Quality assessment. (DOCX 36 kb)

Additional file 3: PRISMA checklist. (DOC 63 kb)

Competing interests

The authors declare that they have no competing interests.

Authors' contributions

MG, HT and MC designed the review methodology. CF developed the search strategy with contribution from MG. CF conducted the literature search. MC and MG undertook study selection and assessed study quality. MC, MG and HT extracted and analysed data. MC wrote the first draft. All authors participated in the writing of the final manuscript.

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Medicaid and Mortality: New Evidence from Linked Survey and Administrative Data
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ABSTRACT

We use large-scale federal survey data linked to administrative death records to investigate the relationship between Medicaid enrollment and mortality. Our analysis compares changes in mortality for near-elderly adults in states with and without Affordable Care Act Medicaid expansions. We identify adults most likely to benefit using survey information on socioeconomic and citizenship status, and public program participation. We find a 0.13 percentage point decline in annual mortality, a 9.3 percent reduction over the sample mean, associated with Medicaid expansion for this population. The effect is driven by a reduction in disease-related deaths and grows over time. We find no evidence of differential pre-treatment trends in outcomes and no effects among placebo groups.

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The Medicaid program is the largest health insurance provider for low income individuals in the United States. Established in 1965, Medicaid currently covers over 72 million enrollees and represents over $500 billion in government spending annually (Centers for Medicare & Medicaid Services, 2019a,b). However, despite the size and scope of this program, we know relatively little about whether Medicaid coverage actually improves the health of its beneficiaries. This is particularly true for low income adults who gained Medicaid eligibility under the Affordable Care Act (ACA), and who are the focus of nearly all of the ongoing policy debate surrounding the program. Studies of the health effects for this group tend to be inconclusive due to small sample sizes (Baicker et al., 2013; Finkelstein et al., 2012), or due to the lack of available data that links information on Medicaid eligibility to objective measures of health such as mortality (Black et al., 2019). The inconclusive nature of these results has led to skepticism among some researchers, policymakers, and members of the media as to whether Medicaid has any positive health impacts for this group.[1]

Understanding what types of public programs, if any, are effective at improving the health of low-income individuals is especially important given that they experience dramatically higher mortality rates and worse health outcomes on a number of dimensions than the general population. For example, the annual mortality rate for individuals ages 55 to 64 in households earning less than 138 percent of the Federal Poverty Level (FPL) is 1.6 percent, almost 2.3 times higher than the 0.7 percent rate experienced by higher-income individuals of the same age.[2] This low-income group also experiences higher risks of dying from diabetes (by 432%), cardiovascular disease (238%), and respiratory disease (213%) relative to those in higher income households; all of these diseases are believed to be at least somewhat amenable to drug therapy. These higher rates of death translate to dramatic differences in life expectancy across income groups. For example, Chetty et al. (2016) find that men at the bottom of the income distribution live on average nearly 15 years less, and women over 10 years less, than those at the top of the income distribution conditional on surviving to age 40. While data from nearly all countries show a positive correlation between income and health, this correlation is stronger in the United States than other high income countries (Semyonov et al., 2013).

Medicaid could play a crucial role in reducing these disparities if it improves access to effective medical care that beneficiaries would not otherwise receive, and recent research suggests this is likely to be the case. For example, Ghosh et al. (2019) find a substantial increase in prescription drug utilization under the ACA Medicaid expansions, including medications for the management of diabetes, treatments for HIV and Hepatitis C, and drug therapies for cardiovascular disease. These particular types of prescription drugs are among those demonstrated to reduce mortality.[3] Changes in access to these

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[1] Flagged as an example of this by Sommers et al. (2017), Congressman Raul Labrador stated that "nobody dies because they don't have access to health care" during a discussion of Medicaid (Phillips, 2017). Also, Goodman-Bacon et al. (2017) provide a review of media discussion and some academic research suggesting that Medicaid may in fact be harmful to health.

[2] Authors' calculations using death rates from 2008 to 2013 derived from the publicly-available National Health Interview Survey Linked Mortality File (National Center for Health Statistics, 2019) for adults with incomes below 138% FPL and those with incomes 400% FPL or greater. We chose these two income cutoffs since adults with incomes below 138% FPL qualify for Medicaid in states that expanded their programs to include low-income adults under the ACA; also, adults with incomes below 400% FPL qualify for subsidies for private insurance coverage.

[3] Systematic reviews and meta-analyses of randomized, controlled trials find significant decreases in all-cause and cardiovascular mortality for adults who receive statins (Chou et al., 2016) and decreased all-cause mortality for Type 2 diabetics receiving glucose-lowering drugs (Zheng et al., 2018). In addition, systematic reviews of observational studies indicate decreased mortality among HIV-infected adults initiating anti-retroviral therapy (Chou et al., 2005), as well as

medications are likely to be particularly important for this population given their higher prevalence of chronic disease (Karaca-Mandic et al., 2017). Medicaid coverage may also affect health if it leads to earlier detection and treatment of life-threatening health conditions. Existing research has documented increased screening of treatable cancers such as breast and cervical cancer with expanded Medicaid coverage (Finkelstein et al., 2012; Sabik et al., 2018), as well as the detection of cancer both overall and at an early stage (Soni et al., 2018) and improved access to cancer surgery (Eguia et al., 2018). Furthermore, Medicaid coverage increases the number of hospitalizations, procedures performed in the hospital, and the number of emergency department visits for conditions that require immediate care (Duggan et al., 2019; Finkelstein et al., 2012; Taubman et al., 2014), all of which are likely to be associated with serious medical issues that require treatment. In addition to increasing the provision of these types of ostensibly high value services, Medicaid also increases the use of a variety of other types of medical care such as routine screening for chronic illnesses, outpatient physician visits, use of prescription drugs that aid in smoking cessation, and dental care which also have the potential to improve health over the longer term.[4]

In this paper, we provide new evidence of the impact of Medicaid on health by using administrative mortality data linked to large-scale, individual survey records. We use this novel dataset to examine the impact of a sizeable Medicaid eligibility expansion that occurred in some states as the result of the ACA. In 2014, the ACA expanded eligibility for the Medicaid program to include all adults in families with incomes under 138 percent of the Federal Poverty Level (FPL). Previously only pregnant women, adults with disabilities, and very low income parents tended to qualify for Medicaid coverage. Although intended to apply to all states, a 2012 Supreme Court decision made the Medicaid eligibility expansion optional. As a result, only 29 states and the District of Columbia expanded coverage in 2014, with 7 additional states electing to expand over the next several years. Despite non-universal adoption, the ACA Medicaid expansions still represent a historic expansion in insurance coverage. Approximately 13.6 million adults gained Medicaid coverage under the ACA (Medicaid and CHIP Payment and Access Commission, 2018); for comparison, Medicare enrolled about 19 million elderly beneficiaries after its creation in 1965 (Bureau of the Census, 1969). We take advantage of variation in state adoption of this large expansion in coverage to compare changes in mortality among individuals in expansion states and non-expansion states.

In contrast to prior research that relies on death certificate data with limited information on individual characteristics, our data include detailed survey measures collected from the 2008 to 2013 years of the American Community Survey (ACS). This large-scale national survey contains approximately 4 million respondents in each year and allows us to observe information on specific characteristics that determine Medicaid eligibility including income, citizenship status, and the receipt of other social assistance. With this information, we are able to identify individuals who were most likely to benefit from the ACA Medicaid eligibility expansions and, in this way, overcome the inherent limitations present in existing studies that rely only on aggregate death records. Following Black et al. (2019), we focus on those in this group who were between the ages of 55 and 64 in 2014, who are at greater risk of mortality, although we also present results for all non-elderly adults. We follow individuals in our sample over time

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indirect evidence of decreased mortality linked to cured infection under antiviral treatment for Hepatitis C (Moyer, 2013).

[4] See Finkelstein et al. (2012); Nasseh and Vujicic (2017); Semyonov et al. (2013).

to examine changes in mortality associated with Medicaid expansion by linking them to the Census Numident file, which contains administrative records on the date of death for all individuals with Social Security Numbers (SSNs) who die in the United States. This file allows us to observe mortality rates for our sample through 2017, four years after the initial ACA Medicaid eligibility expansions. Despite the high-quality of the death information in the Census Numident file, it does not include cause of death information. In supplemental analyses, we further examine changes in mortality by the underlying cause of death using data from the Mortality Disparities in American Communities (MDAC) project, which links the 2008 year of the ACS to death certificate records using the National Death Index.

Our analysis shows that the ACA Medicaid expansions reduced mortality among this targeted group. Prior to the expansions, individuals in our sample residing in expansion and non-expansion states had very similar trends in both Medicaid coverage and mortality. At the time of the expansion, the trajectories of these two groups diverged significantly, with expansion state residents seeing increases in Medicaid coverage and decreases in the probability of being uninsured, and decreases in annual mortality rates. In the first year following the coverage expansion, the probability of mortality declined by about 0.09 percentage points, or 6.4 percent relative to the sample mean. The estimated impact of the expansions increases over time, suggesting that prolonged exposure to Medicaid results in increasing health improvements. By year 4, residents of expansion states have an annual mortality rate that is 0.2 percentage points lower than their non-expansion state counterparts. In our supplemental analysis using the MDAC data, we find evidence that healthcare amenable and internal causes of death were reduced by the expansions, but no evidence that deaths due to external causes, such as car accidents, fell. We also conduct several placebo tests to assess the validity of our analysis including examining the impact of the expansions on those age 65 or older in 2014 who did not gain Medicaid eligibility; examining the effect on individuals in higher income households who were less likely to be affected; and, restricting the analysis sample to the pre-ACA period. We find no relative change in coverage or mortality across expansion and non-expansion states among the elderly or in the pre-ACA period, settings in which no Medicaid expansion occurred. Among those in higher income households, we find small but statistically significant increases in Medicaid coverage and similarly small decreases in mortality, consistent with a causal impact of Medicaid on mortality.

Our analysis provides new evidence that Medicaid coverage reduces mortality rates among low-income adults. Our estimates suggest that approximately 15,600 deaths would have been averted had the ACA expansions been adopted nationwide as originally intended by the ACA. This highlights an ongoing cost to non-adoption that should be relevant to both state policymakers and their constituents.

1 Background

Many studies have shown that Medicaid coverage increases access to and use of health care and reduces financial burden for low-income adults,[5] but evidence as to whether it improves their health remains limited. Studies that do examine health tend to rely on self-reported health measures from survey data. The evidence from these studies spans from estimated large or modest improvements in reported health associated with Medicaid expansion (Cawley et al., 2018; Simon et al., 2017; Sommers et al.,

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[5] See, e.g., Abramowitz, 2018; Allen et al., 2017; Baicker et al., 2013; Brevoort et al., 2019; Buchmueller et al., 2016; Caswell and Waidmann, 2017; Courtemanche et al., 2017; Finkelstein et al., 2012; Gallagher et al., 2019; Ghosh et al., 2019; Hu et al., 2018; Miller and Wherry, 2017, 2019; Simon et al., 2017; Sommers et al., 2015, 2017.

2017), to no effects (Courtemanche et al., 2018a,b; Sommers et al., 2015; Wherry and Miller, 2016) or even small but marginally significant negative effects (Miller and Wherry, 2017).

One concern with self-reported health data is that it may not accurately measure changes in physical health. In the Oregon Health Insurance Experiment (OHIE), low-income adults selected by a lottery to apply for Medicaid coverage reported near immediate improvements in their health compared to the controls, despite experiencing no significant differences yet in their health care utilization (Finkelstein et al., 2012).[6] The researchers concluded that the change in reported health may at least partly capture a general sense of improved well-being, or "winning" effects resulting from individuals' lottery selection. There is also the risk that changes in self-reported health may reflect increasing awareness of health problems or interactions with the health care system, rather than actual changes in physical health. One example would be increased contact with health providers leading to new information about a previously undiagnosed illness and, as a consequence, a worsened self-perception of health. This could bias downwards estimates of the effect of public health insurance on health.[7] Finally, in general, the reliability of self-reported health measures for U.S. adults and their association with objective health measures are documented to be worse among lower socioeconomic status groups (Dowd and Zajacova, 2007, 2010; Zajacova and Dowd, 2011).

In addition to offering the first experimental evidence on the effects of expanded Medicaid, the Oregon Health Insurance Experiment (OHIE) covered new ground by collecting data on clinical health measures among its participants. The researchers did not observe significant effects on any of the collected measures, however, which were blood pressure, cholesterol, and blood sugar levels. Using administrative data, they also found no evidence that Medicaid coverage led to a reduction in mortality during the 16 months following coverage gain. Their estimate suggested a 16 percent reduction in mortality associated with acquiring Medicaid, but with a large confidence interval that could not rule out sizeable changes in either direction.[8]

As the data become available, researchers are beginning to evaluate the mortality effects of the ACA Medicaid expansions.[9] Two current studies use population-level mortality data to estimate changes in adult mortality in expansion states compared to non-expansion states. In contrast to Oregon, the ACA Medicaid expansions affected a much larger number of people (13.6 million vs. under 11,000) (Medicaid and CHIP Payment and Access Commission, 2018; Finkelstein et al., 2012). However, the authors rely on death certificate data without the information on individual income needed to identify the policy's target population. As a consequence, it can be difficult to detect effects at the population-

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[6] The researchers found an improvement in self-reported health for the treatment group during their initial survey, which was conducted, on average, about one month after gaining coverage, that was about two-thirds of the size of their main effect estimated using survey data collected more than a year later.

[7] See Currie and Gruber (1995) for more discussion.

[8] Another relevant randomized social experiment provided Medicare to newly entitled Social Security Disability Insurance (SSDI) beneficiaries (as opposed to them being subject to a 2-year waiting period for coverage). The evaluation of this experiment found no reductions in mortality up to 3 years later but the sample sizes were too small to be able to detect effects (Weathers and Stegman, 2012).

[9] A separate but related literature has examined the relationship between public health insurance and child mortality using variation in exposure tied to the introduction of Medicaid and later expansions in public coverage under Medicaid and the Children's Health Insurance Program. For the most part, these studies have found significant declines in mortality associated with expanded coverage for infants and children both in the short-term (e.g. Currie and Gruber, 1996a,b; Goodman-Bacon, 2018b; Howell et al., 2010) and long-term (Brown et al., 2018; Goodman-Bacon, 2016; Wherry and Meyer, 2016).

level, particularly when Medicaid coverage is estimated to have increased by as little as 1 percentage point among all nonelderly adults (Black et al., 2019). The two studies examining the effects of the ACA Medicaid expansions in this manner reach different conclusions, detecting no (Black et al., 2019) and sizeable effects on adult mortality (a 3.6% reduction) (Borgschulte and Vogler, 2019).In addition, research on pre-ACA expansions in Medicaid that also relies on aggregated data finds larger effects on adult mortality;[10] Sommers et al. (2012) and Sommers (2017) find a 6 percent reduction in nonelderly adult mortality in pre-ACA Medicaid expansions in New York, Maine, and Arizona measured over a five-year period.The absence of conclusive evidence on whether Medicaid improves the objective health of adult beneficiaries is a major omission given that Medicaid is a public health program that aims to improve access to and use of efficacious health care.

All of these studies rely on changes in survival for the Medicaid eligible to translate into overall mortality effects observable at the population (or state) level. However, at least two studies suggest that a focus on subgroups most at risk for mortality may increase the likelihood of detecting effects. Swaminathan et al. (2018) examine the impact of the ACA Medicaid expansions on the one-year survival rate of patients with end stage renal disease initiating dialysis. The authors find a significant 8.5 percent reduction in mortality for individuals with this chronic condition, driven primarily by a decrease in deaths due to causes considered health care amenable. More recently, Khatana et al. (2019) find evidence of a decrease in rates of cardiovascular disease among adults ages 45-64 associated with state adoption of the ACA Medicaid expansions.

For this reason, it is likely that the primary impediment to analyzing the impact of Medicaid on mortality has been data availability. Data from death certificate records contain very little socioeconomic information on the decedent; in particular, they contain no information on the decedent's income, whether he or she previously had health insurance coverage, or other characteristics that might affect Medicaid eligibility. Without data that links information on individual Medicaid eligibility and mortality, researchers must rely on eligibility changes over larger population groups – for example, residents of certain states or counties – which contain many individuals who would not be affected by Medicaid policy. This decreases the power to detect changes in mortality of a plausible magnitude, leading some researchers to conclude that "it will be extremely challenging for a study [on the ACA Medicaid expansions] to reliably detect effects of insurance coverage on mortality unless these data can be linked at the individual level to large-sample panel data" (Black et al., 2019).

This finding of mortality effects for certain subgroups that may not be detectable in larger aggregations of data is consistent with existing work on the effects of Medicare on health. Card et al. (2004) and Finkelstein and McKnight (2008) find little evidence of an effect of Medicare on mortality using death certificate records. However, among those who are hospitalized and severely ill, Card et al. (2009) find a significant 1 percentage point (or 20 percent) reduction in mortality following admission that persists for at least 9 months following discharge. This analysis notably identifies these effects by comparing patients just below and above the Medicare-eligible age of 65 when admitted, which is just above the age range considered in our analyses.

It is also worth noting that, at the time of these studies, Medicare did not provide coverage for

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[10] In addition, an analysis of the mortality effects of insurance expansion under Massachusetts's 2006 health reform by Sommers et al. (2014) finds a significant 2.9 percent reduction in all-cause mortality over four years of follow-up; among deaths from "heath-care amenable" conditions, the authors find a 4.5 percent decline.

prescription drugs. Recent papers studying the introduction of prescription drug coverage under the Medicare Part D program are finding evidence of mortality declines. Huh and Reif (2017) focus on those age 66 and find that insurance coverage for prescription drugs reduces mortality in this group by about 0.16 percentage points annually (about 9.6 percent). Dunn and Shapiro (2019) find slightly larger effects in an analysis that incorporates individuals with older ages. For both papers, reductions in mortality are driven by a decline in deaths due to cardiovascular disease. Using data for a subset of Medicare beneficiaries, Kaestner et al. (2017) find no significant effect on mortality but do document reductions in hospitalization admissions for heart disease, respiratory disease, and diabetes under the program. Importantly, sizeable increases in the use of prescription drugs that treat these particular diseases have been documented under the ACA Medicaid expansions (see Ghosh et al., 2019).

2 Data and Outcomes

To conduct our analysis, we use data from two sources. First, we select respondents from the 2008 to 2013 waves of the American Community Survey who, based on their pre-ACA characteristics, were likely to benefit from the ACA Medicaid expansions. We include only individuals who either are in households with income at or under 138 percent of the FPL or who have less than a high school degree. Since we only have information on income captured at one point in time, the latter criterion is used to identify individuals who are of low socioeconomic status but might not meet the income cutoff at the time of the ACS interview. We exclude non-citizens, many of whom are not eligible for Medicaid, and those receiving Supplemental Security Income (SSI), who are likely to be Medicaid eligible even without the expansions.[11] We restrict our primary analysis to individuals who were age 55 to 64 in 2014. This higher age group has relatively high mortality rates, and is also consistent with the sample criteria used in Black et al. (2019). We present results for all non-elderly adults in a supplementary analysis. We also exclude residents of 4 states and DC that expanded Medicaid to low-income adults prior to 2014.[12] There are approximately 566,000 respondents who meet our sample criteria.[13]

Descriptive statistics for the sample by state Medicaid expansion status are reported in Table A1. The average age of the respondents in the two groups is similar. However, individuals in expansion states are slightly better off with higher average income (147% of the FPL vs 140%) and educational attainment (45.3% with less than high school education vs 46.8%), as well as lower baseline rates of coverage (32.6% uninsured vs 37.3%), than individuals in non-expansion states. In addition, individuals in expansion states are more likely to be white or Hispanic, while a higher share of those in non-expansion states are black.

These data are linked to the Census Numident file. The Census Numident file is derived from the Social Security Administration (SSA) Numerical Identification file, which includes information on date and county of birth and date of death (if it has occurred) for individuals with a Social Security Number (SSN). These data have been used in, e.g., Brown et al. (2018); Chetty et al. (2011, 2016); Dobbie and Song (2015); Sullivan and von Wachter (2009), and other research relying on death information from

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[11] SSI recipients are automatically eligible for Medicaid coverage in most states.

[12] DE, MA, NY, and VT all expanded coverage to individuals with incomes at least to the poverty line prior to the ACA; DC received approval to implement its ACA Medicaid expansion early with enrollment starting in 2011.

[13] Note that Census disclosure rules prohibit the disclosure of exact sample sizes and require rounding. All sample sizes reported in this paper are therefore rounded according to these rules.

tax records. Total deaths reported in the SSA file by age and year closely track the numbers reported by the National Center for Health Statistics (Chetty et al., 2016). In addition to this death information from the SSA, the Census Bureau also has information on date of death from the National Death Index (NDI) for some individuals and years, which it incorporates into its date of death measure when available.[14] The Census Bureau receives the SSA Numident file each year and formats this information so that there is a single record per individual reflecting the most accurate and up-to-date information at that point in time. We use the most recently available version of the Census Numident, which captures date of death through the second quarter of 2018. Because we observe only a partial year in 2018, we limit our analyses to deaths occurring in 2017 and earlier.

The Census Numident and ACS data are linked via the Census Bureau's Personal Identification Validation System (PVS). This system assigns individuals in each dataset a protected identification key (PIK), an anonymized identifier that allows Census to track individuals across datasets. Approximately 94 percent of all ACS respondents are successfully assigned a PIK using available information on name, address, and date of birth, with a slightly higher match rates for citizens (95 percent) (Wagner and Layne, 2014). The assignment of a PIK allows respondents in the ACS to be matched to the Census Numident file. PIKS for the Census Numident file are assigned using social security numbers (SSNs). Since our analysis is restricted to older citizens, and since nearly all American citizens have SSNs assigned by the time they reach adulthood (see Bernstein et al., 2018), we expect to have nearly full coverage of deaths in the Numident file.

Once these data are linked, we observe the vital status of each individual during the year they respond to the ACS and then each subsequent year. For example, an individual who responds to the 2008 ACS has his or her vital status observed in 2008 and each subsequent year through 2017, whereas an individual who responds to the 2013 ACS has his or her vital status observed in 2013 through 2017. We construct our outcome measure to represent mortality during each calendar year. If the individual is alive in a given year, the outcome variable takes a value of 0; if that individual died in that year it takes a value of 1. Once an individual has died, he or she is removed from the sample for subsequent years. In this way, we will be able to measure changes in the annual probability of death among individuals who were alive at the beginning of the year.

Annual mortality is about 1.4 percent for our sample on average across all years, and approximately 1.3 percent among respondents in expansion states during the year just prior to expansion.[15] Note that because we have a fixed sample that ages in each period, mortality rates increase over time (i.e., our sample is oldest in the last year, 2017).

While our data offer the opportunity to link mortality and economic variables at the individual level, there are also several important limitations. First, we observe the economic characteristics of individuals (income and educational attainment, receipt of social services, and citizenship status) at the time they respond to the ACS, between 2008 and 2013. These are time-varying characteristics and may not accurately reflect economic characteristics at the time of the Medicaid expansions for some members of our sample. For example, an individual in a low-income household in 2008 may be in a higher-income household by 2014, at the time the expansions occurred. Similarly, individuals may

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[14] The NDI collects detailed information on deaths from state vital statistics offices. Respondents to the 2008 ACS were linked to the NDI for the years 2008-2015, as part of the Mortality Disparities in American Communities project.

[15] These annual averages are calculated excluding mortality rates for individuals during their year of ACS interview.

migrate to different states between the time they responded to the ACS and the time the expansions occurred, resulting in our misclassification of whether that individual was exposed to the eligibility expansion.[16] In general, we expect that this type of misclassification will bias our estimates towards zero.

A second limitation is that our data do not include information on the cause of death. The death information in the Census Numident is derived primarily from the Social Security Administration death records, which contain only date of death. We therefore supplement our main analysis with an exploration of data from the 2008 year of the ACS, which was linked to death certificate records from 2008 to 2015 as part of the Mortality Disparities in American Communities (MDAC) project. While this drastically reduces both the sample size and follow-up period, it does allow us to investigate changes in mortality based on the underlying cause of death as reported on the death certificate.

3 Empirical Strategy

Our empirical strategy looks at changes in annual mortality in the expansion states relative to the non-expansion states before and after the implementation of the ACA Medicaid expansions. We estimate this using an event-study model that allows us to assess the evolution of relative outcomes while controlling for fixed differences across states and national trends over time. We estimate:

$$Died_{isjt} = Expansion_s \times \sum_{\substack{y=-6 \\ y\neq -1}}^{3} \beta_y I(t - t^*_s = y) + \beta_t + \beta_s + \beta_j + \gamma \mathbf{I}(j = t) + \epsilon_{isjt}. \quad (1)$$

As described earlier, our data is constructed at the individual (i) by year (t) level. Each individual responds to the ACS during a survey wave (j) and reports their state of residence (s) at that time. The dependent variable $Died_{isjt}$ denotes death during each year t among individuals who were alive at the beginning of year t. We only observe mortality for part of the year in the year the individual is surveyed (j), since that individual had to be alive in order to complete the survey. To account for this, we include an indicator variable that year t is the year that the individual responded to the ACS (i.e., that $j = t$).[17] In this equation, β_s denotes state fixed effects and β_j denotes fixed effects associated with survey wave. β_t denotes calendar year fixed effects, which will account for general trends in mortality for all individuals in our sample including their gradual aging over time.[18]

The variable $Expansion_s$ equals 1 if, at the time they responded to the ACS, individual i was living in a state that opted to expand Medicaid eligibility between 2014 and 2017, and zero otherwise. Indicator variables $I(t - t^*_s = y)$ measure the time relative to the implementation year, t^*_s, of the expansion in that state, and are zero in all periods for non-expansion states.[19] While most states expanded in the beginning of 2014, some states expanded later in the year or in subsequent years. If a state expanded on or after July 1 of a given year, we code it as having expanded in the subsequent

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[16] Note that it does not appear that migration decisions are correlated with a state's decision to expand Medicaid, see Goodman (2017).

[17] If we drop the observations for which we observe less than a full year of mortality our results are unchanged. Note that we do not have information on the date of the ACS interview.

[18] Results are also virtually identical in a model that includes controls for gender, race, and single year of age.

[19] We group together $y \leq -6$ into a single indicator variable interacted with expansion status since we only observe $y < -6$ for late expander states.

year.[20] The omitted category is $y = -1$, the year prior to the expansion. Therefore, each estimate of β_y provides the change in outcomes in expansion states relative to non-expansion states during year y, as measured from the year immediately prior to expansion. If expansion and non-expansion states were trending similarly prior to the ACA, we would expect that indicators associated with event times $y = -6$ to $y = -2$ would be small and not statistically significant. We estimate equation (1) with a linear probability model and report heteroskedasticity-robust standard errors that are clustered at the state level. All analyses use ACS survey weights.

In addition to the event study analyses, we also present difference-in-differences estimates as a summary of the effect across all post-expansion years. These are estimated using the same equation except the event study coefficients are replaced with a single variable indicating the individual i was in an Expansion state after the expansion had occurred ($Expansion_s \times Post_t$).

4 Results

4.1 Impact of ACA Expansions on Medicaid Eligibility and Enrollment

We first estimate the impact of the ACA Medicaid expansions on Medicaid eligibility and coverage for individuals similar to those in our sample. We consider changes in eligibility for Medicaid in addition to enrollment changes since eligible individuals are "conditionally covered" by the program, in the sense that they may choose to remain uninsured and enroll only when they become ill. This concept of conditional coverage was first discussed by Cutler and Gruber (1996) in their study of historic Medicaid expansions for pregnant women and children; it may be even more relevant in our context, however, given another change under the ACA designed to provide a new pathway for the uninsured to gain immediate access to Medicaid-funded services. For the first time, the federal government required states to implement presumptive eligibility programs under their Medicaid programs. Specifically, the ACA granted hospitals the ability to make presumptive eligibility determinations for Medicaid for certain groups covered in their state, including the non-elderly ACA expansion population (Caucci, 2014).[21] This means that if patients appear to have incomes low enough to qualify for Medicaid, hospitals may grant temporary Medicaid enrollment. Recipients of this temporary enrollment status may immediately receive health services and providers are guaranteed reimbursement for those services. In addition to presumptive eligibility programs, federal law directs states to provide retroactive coverage for new enrollees by covering medical bills incurred up to 3 months prior to their application date if they met the eligibility criteria during that time.[22] By not requiring an individual to first enroll in Medicaid prior to receiving Medicaid-funded care, these policies reinforce the notion that all eligible individuals are effectively covered by the program even if not actually enrolled.

Since we only observe our sample in the ACS during the pre-expansion years, we do not have

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[20] In our analyses, states that expanded Medicaid in 2014 are AR, AZ, CA, CO, CT, HI, IL, IA, KY, MD, MI, MN, NJ, NM, NV, ND, OH, OR, RI, WA, and WV. Michigan implemented their expansion in April 2014 with the remainder of states expanding in January 2014. States that we considered to have 2015 expansions are NH (implemented August 15, 2014), PA (January 1, 2015), and IN (February 1, 2015). We consider AK (September 1, 2015) and MT (January 1, 2016) to be 2016 expansion states and LA (July 1, 2016) to be a 2017 expansion state.

[21] Previously presumptive eligibility programs were optional for states and limited to pregnant women and children. States also had discretion over what types of providers could grant presumptive eligibility for these groups.

[22] A handful of states (AR, IA, IN, NH) had federal waivers to waive retroactive coverage for the expansion population, or other existing Medicaid eligibility groups, during our study period (Musumeci and Rudowitz, 2017).

information on their economic characteristics or coverage decisions during the post-expansion period. However, we are able to estimate model (1) using respondents in the 2008 to 2017 waves of the ACS who were age 55 to 64 in 2014, and otherwise meet the same sample restrictions as in our main analyses. While this analysis does not completely mirror that used to study mortality, it allows us to provide an estimate of the changes in eligibility and coverage similar to those likely experienced by our sample.[23] We impute income eligibility for Medicaid using information on family structure and income and state-specific eligibility criteria over this time period.[24] In addition to changes in Medicaid eligibility, we also examine changes in Medicaid coverage and overall insurance status using respondent reports about current health insurance coverage at the time of the ACS survey.

The results are presented in Figure 1 and in the first three columns of Table 1. We find a large increase in Medicaid eligibility associated with the ACA Medicaid expansions with gains of between 41 and 46 percentage points during each post-expansion year, as compared to the year just prior to expansion. Consistent with many other studies of this policy,[25] we also find significant increases in Medicaid coverage and decreases in uninsurance associated with the decision to expand Medicaid eligibility. Reported Medicaid coverage increases by 7.3 percentage points in the first year and by 9.9 percentage points four years after the expansion relative to the year prior to expansion, while uninsurance decreases by 3.8 percentage points in the first year and 3.9 percentage points four years after the expansion. The estimates for years 2 and 3 are larger than those for year 4, which likely reflects the increasing share of the sample that is aging into Medicare over the study period.

It is important to note that the increases in Medicaid coverage observed in the survey data are most likely smaller than total enrollment changes for several reasons. First, Medicaid coverage is notoriously underreported in survey data. Boudreaux et al. (2015) link the 2009 ACS to administrative data on Medicaid and Children Health Insurance Program (CHIP) enrollment and find that 23 percent of Medicaid/CHIP enrollees do not report this source of coverage. Rates of underreporting are higher for adults and minority groups; in addition, these groups are more likely to report no insurance coverage than other sources of coverage. Second, by asking about coverage only at the time of the survey, the ACS does not capture information on Medicaid coverage for individuals enrolled in Medicaid during other times during the year. Given that there is tremendous churn among adults in the Medicaid program,[26] these estimates, therefore, likely underrepresent the total share of adults gaining any Medicaid coverage during each year.

We conducted our own analysis of underreporting for individuals meeting our sample criteria using data available from the 2008 to 2012 National Health Interview Survey (NHIS) for respondents linked to administrative data on Medicaid enrollment.[27] We found that while 14.3 percent of the sample reported

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[23] There is one additional difference in the setup of this analysis. To avoid having multiple samples disclosed from the restricted-use data, we use the public-use ACS files for this "first-stage" analysis. The public-use file is a two-thirds random sample of the restricted-use file and will therefore result in nearly identical results, but with slightly larger confidence intervals.

[24] We consider eligibility for low-income parents under Medicaid Section 1931 criteria in each state, as well as expanded eligibility for parents and childless adults under waiver programs that offered comparable coverage to the ACA Medicaid expansions. Please see Appendix Section B for additional details about the eligibility imputation.

[25] E.g., Buchmueller et al. (2016); Cawley et al. (2018); Courtemanche et al. (2017); Miller and Wherry (2017, 2019); Sommers et al. (2015)

[26] See, for example, analyses in Sommers (2009) and Collins et al. (2018).

[27] These data are available from the National Center for Health Statistics for NHIS respondents who consent to the linkage. Due to an unfortunately timed change in the way CMS collects enrollee-level Medicaid administrative records,

being enrolled in Medicaid at the time they completed the survey, 19.3 percent were enrolled at some point during that year according to the CMS administrative records; this suggests an undercount based on survey data of approximately 35 percent.

Because this analysis is based on the reporting behavior of Medicaid enrollees prior to the ACA, it may not necessarily reflect the degree of underreporting among those gaining Medicaid coverage under the ACA expansions. Therefore, we also estimate by how much we might be undercounting the change in total Medicaid enrollment under the ACA by comparing the "first stage" we obtain from the ACS with a "first stage" obtained from different CMS administrative data reports on total Medicaid enrollment during the study period. The two different administrative sources used for this analysis offer different definitions of enrollment and have different information in terms of the years and states available, as well as the ages for which information on enrollment is collected. Depending on the data source used, we find estimates of undercount ranging from somewhat smaller (18%) to considerably larger (exceeding 100%) than the estimate arrived at with the NHIS-CMS linked data. Since the NHIS-CMS data analysis allowed us to create an analytic sample most similar to that used in this paper, we apply the 35 percent undercount estimates when discussing treatment effects in the section that follows. Additional details on the analysis of underreporting in the NHIS-CMS data, as well as the analyses involving the CMS data reports may be found in Appendix Section C.

4.2 Impact of ACA Expansions on Mortality

Our estimates of equation (1) are presented in Figure 2 and in the fourth column of Table 1. Prior to the ACA expansion, mortality rates trended similar across the two groups: pre-expansion event study coefficients are close to zero and not statistically significant. Starting in the first year of the expansion, we observe mortality rates decrease significantly among respondents in expansion states relative to non-expansion states. The coefficient estimated in the first year following the expansion indicates that the probability of dying in this year declined by about 0.09 percentage points. In years 2 and 3, we find reductions in the probability of about 0.1 percentage points and, in year 4, a reduction of about 0.2 percentage points. All estimates are statistically significant.

In the difference-in-differences model, we estimate an average reduction in mortality of about 0.13 percentage points (top panel of Table 1).[28] We can combine this estimate with the estimates of the first stage to provide information on the treatment effect of Medicaid coverage on the group that actually enrolled.[29] Our analysis of the ACS suggested that Medicaid enrollment increased by about 10.1 percentage points in our sample. However, as noted above, we estimate that survey measures are likely to underreport actual take-up by about 35 percent (see Appendix Section C). Incorporating this underreport into our first stage estimates indicates that the true first stage is likely closer to 15.5

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data are unavailable for most states after 2012.

[28] If we average the post-expansion event study indicators, rather than estimating a two way fixed effects difference-in-differences coefficient, the estimate is nearly identical. This suggests that any potential bias introduced in the DID estimate from using earlier implementation states as controls for later implementation states during their post-period if there are time-varying treatment effects is likely small (see Goodman-Bacon, 2018a).

[29] One can further scale up this estimate to arrive at the local average treatment effect of gaining any coverage by incorporating estimated crowd out. However, for the interpretation to be valid it must be the case that Medicaid coverage is equivalent to the private coverage purchased when Medicaid is unavailable. This is unlikely to be the case; for example, beneficiaries who switch to Medicaid from private insurance typically will not pay a premium and have minimal cost sharing, and could thus potentially benefit financially. For this reason, we focus on the treatment effect of Medicaid in this discussion.

percent (i.e., $\frac{0.101}{(1-0.35)}$). Our estimates therefore suggest that the treatment effect of Medicaid coverage on mortality is about a 0.8 percentage point ($= \frac{0.13}{0.155}$) reduction.

It is important to note that even this re-scaled first stage only considers the immediate, or short-term, effects of Medicaid coverage on mortality. To the extent that there are longer-term effects on health, it is not clear that the average annual change in coverage is the correct first stage. For instance, individuals who gained coverage in 2014 but not later years may still experience health benefits that translate into reduced mortality in subsequent years. This is particularly relevant for the age group we study, as part of the sample ages in to Medicare over our analysis period. These individuals might still experience reduced mortality after enrollment in Medicare due to long-run health gains from receiving Medicaid at ages 62 to 64. Results in recent work examining the long-term effects of public insurance expansions for children document health improvements that manifest years later.[30] A more appropriate first stage, if the data were available, might be the change in the proportion of the sample with any exposure to Medicaid at the time of each post-expansion year, which will necessarily be larger than the estimates presented here.

4.3 Placebo Tests and Additional Analyses

To assess the validity of our empirical approach, we conduct several "placebo" tests. In these tests, we investigate whether we observe effects of the Medicaid expansions in populations that we expect to be unaffected or less affected by the policy change.

Our first placebo tests uses individuals who were age 65 and older at the time of the ACA expansions. These individuals had near universal coverage through the Medicare program and should not have been directly affected by the coverage expansions.[31] To conduct this test, we estimate equation (1) but use a sample of individuals who were 65 years old or older in 2014. The results are presented in the first panel of Figure 3. As predicted, we observe no effect of the Medicaid expansions on Medicaid coverage for this group (panel a). We also see no effect of the ACA on mortality rates for this group.

A second placebo tests shifts our analysis sample back in time to the pre-ACA period. This test can assess whether any elements of our sample construction, such as drawing the ACS sample only in the pre-expansion period, might lead to spurious results. We construct the data in the same fashion as our main analysis, but use mortality data from 2004 to 2013 for ACS respondents from 2004 to 2009 (rather than mortality data from 2008 to 2017 for 2008-2013 ACS respondents). We construct a variable indicating that a state expanded that corresponds to $Expansion_s$ in equation (1), but behaves as if the expansions occurred in 2010 rather than 2014, with states expanding t years after 2014 treated as if they expanded in 2010+t. The results of this placebo test using the pre-ACA period is presented in the second row of Figure 3.[32] As expected, we find no effect on Medicaid coverage or mortality in expansion states during this pre-ACA period.

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[30] Boudreaux et al. (2016) and Goodman-Bacon (2016) document better later life adult health among children who gained exposure to Medicaid under its rollout in the 1960s. Brown et al. (2018); Currie et al. (2008); Miller and Wherry (2018); Thompson (2017); Wherry and Meyer (2016) and Wherry et al. (2017) find evidence of better long-term health for children benefiting from later expansions in Medicaid and CHIP.

[31] Prior work has documented some spillover effects on the health care utilization of this population under pre-ACA state Medicaid expansions, but analyses of the ACA Medicaid expansions have found no evidence of such spillovers and are able to rule out very small effects (Carey et al., 2018).

[32] Since the ACS only began collecting data on health insurance in 2008, the analysis for Medicaid coverage is limited to the 2008-2013 years.

Finally, we examine individuals age 55 to 64 in households earning 400% FPL or greater at the time of the ACS interview. This group should be less affected than our main sample of low income or low education respondents. However, they may still gain Medicaid coverage under the expansions due to changes in income over time, or if their income is reported with error. As seen in the third row of Figure 3, we do find small but statistically significant increases in Medicaid enrollment corresponding with the expansions among this group. We also see small but, for some years, statistically significant reductions in mortality for this group. However, these mortality reductions are quite small, between 15 and 20% of the size observed in our primary sample. The sample for the higher income group is also nearly three times as large as our main sample, resulting in much tighter confidence intervals. Taken together, all three placebo tests support our empirical design.

In addition to these placebo tests, we also conduct several additional analyses to further understand the impact of the Medicaid expansions. First, we examine changes in death rates by the underlying cause of death using the MDAC. These analyses rely on a much smaller sample and shorter follow-up period, and so we consider this analysis to be exploratory in nature. We examine deaths due to non-disease related (i.e. ”external”) and disease-related (i.e. ”internal”) causes separately. A subset of deaths caused by internal factors are considered to be “health care amenable” (Nolte and McKee, 2003), which we also examine separately. These results are presented in Table A2. We observe similar patterns for internal mortality and health care amenable mortality as we do in our main results, with relative decreases beginning in the first year after the expansions occur. Individual year effects are not statistically significant for health care amenable mortality, and are significant at the $p < 0.10$ level for deaths from internal causes; however, we find highly significant reductions in deaths related to internal causes and marginally ($p < 0.10$) significant reductions in deaths from health care amenable causes under the difference-in-differences model. In contrast, mortality from external causes, which may be less affected by insurance coverage, does not appear to decrease after the expansions. The point estimates on the individual year effects are not statistically significant and the estimate on the pooled year effect is only significant at the 10% level. The estimate is also positive, although we note that there is a slight upward pre-trend in these deaths in the expansion states relative to non-expansion states.

We further probe cause of death analysis by conducting an analysis using the ICD code groupings by body region. We emphasize that this exercise is meant to be exploratory only with the hope that it will provide guidance for future work should better data become available. The results are reported in Table A3. For most diseases, we observe negative coefficients; the largest negative point estimates are observed for deaths related to neoplasms (cancer), endocrine and metabolic diseases (primarily diabetes), cardiovascular disease, and respiratory diseases. Two of these (cardiovascular and endocrine/metabolic) are marginally significant at the 10% level. We also see a small negative but statistically significant impact on diseases related to the skin and subcutaneous tissue. However, this significant effect would not survive a correction for the many tests conducted.

A second additional analysis uses our main data source but examines changes in mortality for different populations. Our main analysis is limited to individuals age 55 to 64 at the time of the Medicaid expansions, a group with higher mortality rates that has been the focus of other work on this topic (e.g. Black et al., 2019). In column (1) of Table A4, we also estimate the impact of Medicaid expansion on mortality for individuals who meet our sample inclusion criteria but are age 19 to 64 in 2014. As with the 55-64 year old group, we find that mortality rates trended very similarly in the two groups of states prior to the expansions, with the event study coefficients for the pre-expansion years very close to zero (except for $y = -6$). Beginning in the first year of expansion, we see relative declines in mortality in the expansion states, although the estimates are much smaller in magnitude than those observed for the 55-64 age group and only statistically significant in the second year following implementation. In that year, we find a reduction in the probability of death of about 0.02 percentage points. Interestingly, when combined with the first stage for this group (a 13.4 percentage point gain in Medicaid coverage; these results available from the authors), the associated treatment effect is very close to that reported in the Oregon Health Insurance Experiment (although not statistically significant): about a 0.15 percentage point reduction in the probability of mortality, compared to their estimate of 0.13 percentage points (LATE estimate in Table IX in Finkelstein et al., 2012).

Another additional analysis limits the main sample of 55 to 64 year olds to approximately a 30 percent subset who reported being uninsured at the time of the survey. These results are presented in the second column of Table A4. As this group is somewhat younger, the mean annual mortality rate is slightly lower than in the overall sample, at 1.1% mortality per year. This subsample also has fewer observations – 180,000 individuals (or 1.3 million individual by year observations) – resulting in wider confidence intervals. Nevertheless, we observe the same pattern of no pre-ACA changes and a relative decrease in mortality beginning at the time of expansion. The point estimates indicate somewhat larger decreases in mortality for this group of 0.15 percentage points (or 13.6% of the sample mean) compared to the reduction in the main sample of 0.13 percentage points (or 9.3% of the sample mean).

5 Interpreting the Estimates and Comparisons to Past Work

The above results present consistent evidence of a decrease in all-cause mortality among low socioeconomic status, older adults under the ACA Medicaid expansions. Our point estimate indicates an average decrease in annual mortality of 0.13 percentage points during the four-year post period, or a treatment effect of Medicaid coverage among those who enroll of 0.8 percentage points. To interpret the magnitude of this estimate, we must consider the mortality rate in the absence of Medicaid expansion. The average annual mortality rate in our sample is about 1.4 percentage points. However, baseline mortality among those who actually enrolled in Medicaid (i.e., the "compliers," see Imbens and Angrist, 1994) is potentially much higher. This will be the case if those in worse health are more likely to enroll in Medicaid. The literature indicates that such adverse selection does tend to occur (e.g. Kenney et al., 2012; Marton and Yelowitz, 2015); this may also be exacerbated by policies designed to provide immediate coverage to those in need, as discussed earlier (i.e. presumptive eligibility and retroactive coverage). Data from the 2014 National Health Interview survey linked mortality files indicate that Medicaid enrollees in the 55-64 age range have a 2.3 percentage point chance of of dying in the following year.[33] We may therefore expect the mean mortality rate among the compliers to fall somewhere in the 1.4 to 2.3 percent range. Combined with our estimated treatment effect of an 0.8 percentage point reduction in mortality, this indicates that Medicaid reduces mortality by between 35%

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[33] Note that this is similar to the 2.3 percentage point probability of dying observed in the Oregon Health Insurance Experiment control group for participants in this age group over the approximately 16 month period over which deaths were observed (as calculated by the authors from the public-use replication kit, see Table A5).

and 57%. Naturally, the uncertainty about both the size of the first stage and the baseline mean among the compliers results in a fairly large range of possible treatment effects. For this reason, we believe the focus should be primarily on the reduced-form estimates of the change in mortality for our overall sample, which was selected based on their likely eligibility for Medicaid, rather than these "back of the envelope" treatment effect calculations.

Nevertheless, we further assess the plausibility of our estimates by comparing the treatment effect estimate to that reported in the OHIE. We use the public-use replication kit to examine the effect of the experiment on participants who were ages 55-64 at the time of the experiment to derive estimates comparable to those presented here. Among this group, receiving Medicaid reduced the probability of mortality over a 16 month period by about 1.6 percentage points, or a decline of 70% relative to the control mean; this estimate is associated with a p-value of 0.128 (reported in Table A5). We scale this effect by 12/16th to arrive an annual effect of Medicaid on mortality of about 1.2 percentage points. This is comparable, but larger, than the 0.8 percentage point treatment effect estimated here.

Our estimated change in mortality for our sample translates into sizeable gains in terms of the number of lives saved under Medicaid expansion. Since there are about 3.7 million individuals who meet our sample criteria living in expansion states,[34] our results indicate that approximately 4,800 fewer deaths occurred per year among this population, or roughly 19,200 fewer deaths over the first four years alone. Or, put differently, as there are approximately 3 million individuals meeting this sample criteria in non-expansion states, failure to expand in these states likely resulted in 15,600 additional deaths over this four year period that could have been avoided if the states had opted to expand coverage.[35]

6 Conclusion

There is robust evidence that Medicaid increases the use of health care, including types of care that are well-established as efficacious such as prescription drugs and screening and early detection of cancers that are responsive to treatment.[36] Given this, it may seem obvious that Medicaid would improve objective measures of health. However, due to data constraints, this relationship has been difficult to demonstrate empirically, leading to widespread skepticism that Medicaid has any salutary effect on health whatsoever. Our paper overcomes documented data challenges by taking advantage of large-scare federal survey data that has been linked to administrative records on mortality. Using these data, we show that the Medicaid expansions substantially reduced mortality rates among those who stood to benefit the most.

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[34] Authors' calculation using the public-use ACS.

[35] This relies on the assumption that effects of expansion in the non-expansion states would be similar to those observed in the expansion states.

[36] (E.g. Finkelstein et al., 2012; Ghosh et al., 2019; Soni et al., 2018).

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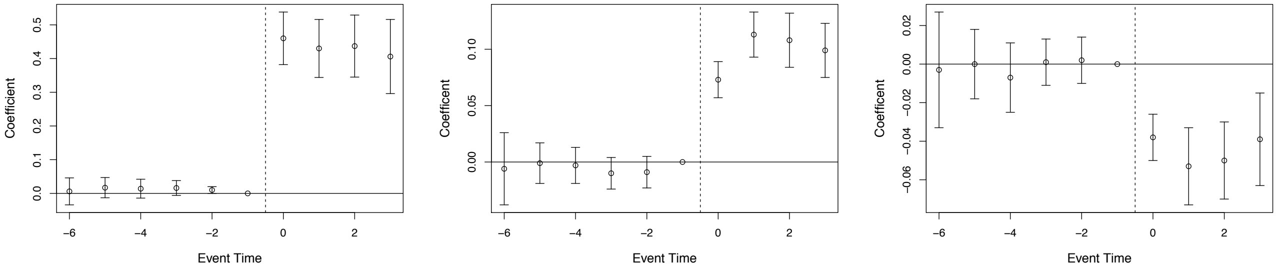
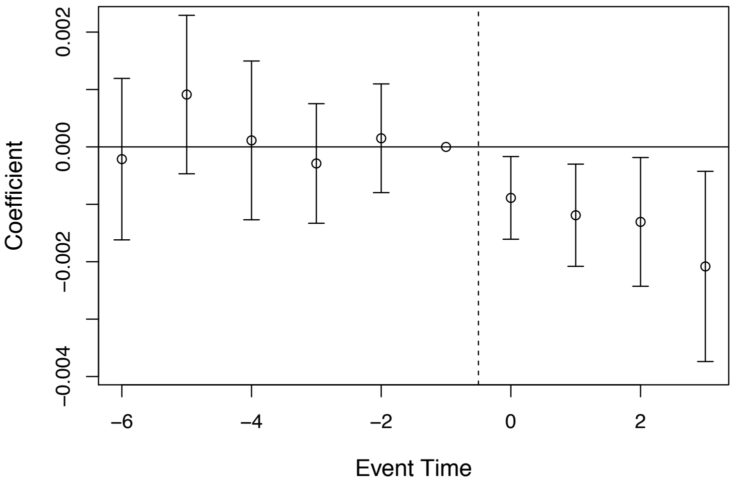
Figure 1: Effect of the ACA Medicaid Expansions on Eligibility and Coverage



(a) Medicaid Eligibility (b) Medicaid Coverage (c) Uninsured

Note: These figures report coefficients from the estimation of equation (1) for the outcomes of Medicaid eligibility, Medicaid coverage, and uninsurance in the 2008-2017 American Community Survey. The coefficients represent the change in outcomes for expansion states relative to non-expansion states in the six years before and four years after expansion, as compared to the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 19-64 in 2014 who are not SSI recipients and who have either less than a high school degree or household income below 138% FPL. See Appendix Section B for detailed information on Medicaid eligibility determination.

Figure 2: Effect of the ACA Medicaid Expansions on Annual Mortality



Note: This figure reports coefficients from the estimation of Equation 1 for annual mortality. The coefficients represent the change in mortality for expansion states relative to non-expansion states in the six years before and four years after expansion, as compared to the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 19-64 in 2014 observed in the 2008-2013 American Community Survey who are not SSI recipients and who have either less than a high school degree or household income below 138% FPL.

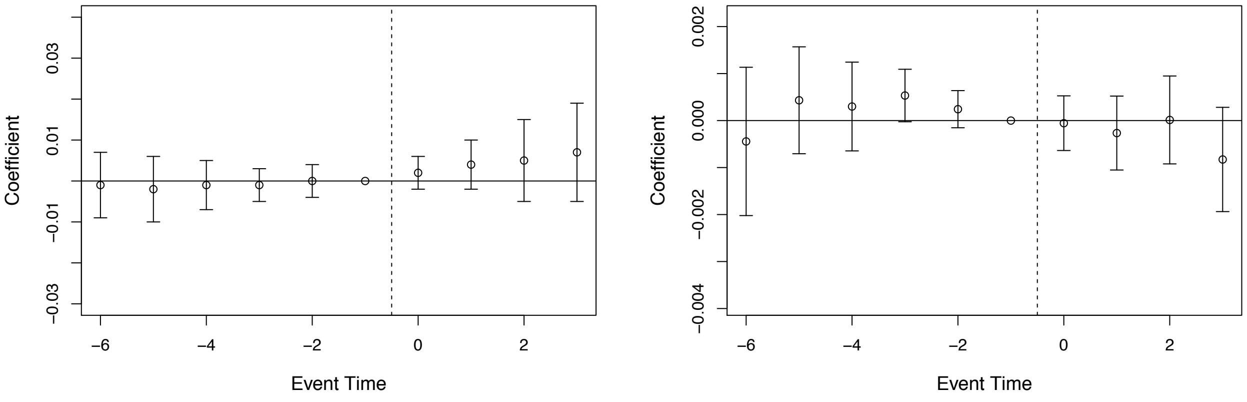
Table 1: Impact of the ACA Expansions on Coverage and Mortality: Difference-in-Differences Estimates

	Medicaid Eligibility	Medicaid Coverage	Any Insurance Coverage	Died in Year Main Sample
Difference-in-Differences Model:				
Expansion × Post	0.428 (0.05)***	0.101 (0.012)***	-0.044 (0.010)***	-0.001320 (0.000497)**
Event Study Model:				
Year 3	0.406 (0.055)***	0.099 (0.120)***	-0.039 (0.012)***	-0.002082 (0.0008284)**
Year 2	0.437 (0.046)***	0.108 (0.012)***	-0.050 (0.010)***	-0.001306 (0.0005606)**
Year 1	0.430 (0.043)***	0.113 (0.010)***	-0.053 (0.011)***	-0.001190 (0.0004449)***
Year 0	0.460 (0.039)***	0.073 (0.008)***	-0.038 (0.006)***	-0.000888 (0.0003600)**
Year -1 (Omitted)	0	0	0	0
Year -2	0.010 (0.005)*	-0.009 (0.007)	0.002 (0.006)	0.0001502 (0.0004735)
Year -3	0.016 (0.011)	-0.010 (0.007)	0.001 (0.006)	-0.0002885 (0.0005306)
Year -4	0.014 (0.014)	-0.003 (0.008)	-0.007 (0.009)	0.0001134 (0.0006915)
Year -5	0.017 (0.015)	-0.001 (0.009)	0.000 (0.009)	0.0009119 (0.0006901)
Year -6	0.006 (0.020)	-0.006 (0.016)	-0.003 (0.015)	-0.0002132 (0.0007031)
N (Individuals x Year)	714673	714673	714673	4030000
N (Individuals)	714673	714673	714673	566000

Note: This table displays the event study coefficient estimates of equation (1). Columns 1-4 show results for the main sample, citizens age 55 to 64 in 2014 who do not receive SSI and are either in low income households or have less than a high school degree education. Column 5 shows a placebo test using all individuals age 65 and older in 2014. For models based on restricted-use data, sample sizes are rounded following Census disclosure rules. See text for more details. Significance levels: *=10%, **=5%, ***=1%.

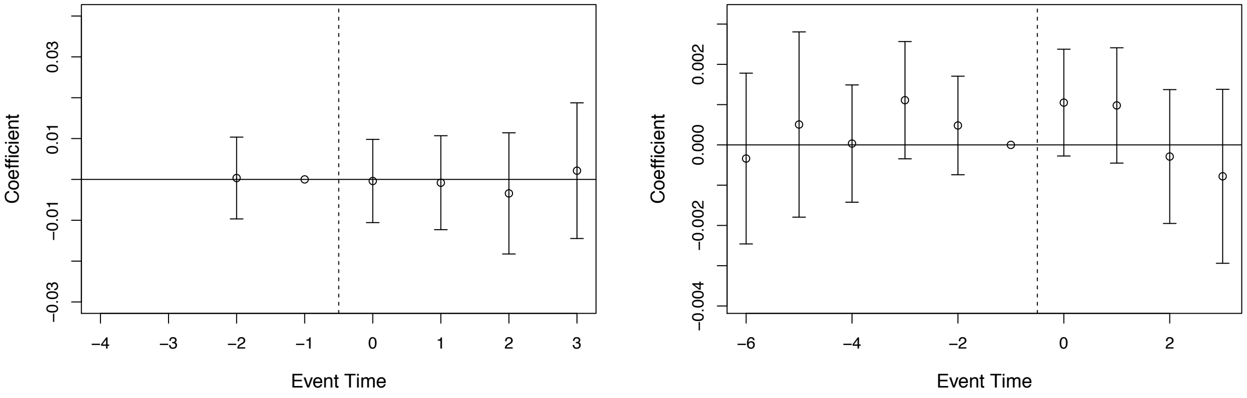
Figure 3: Placebo Tests

Age 65+ in 2014



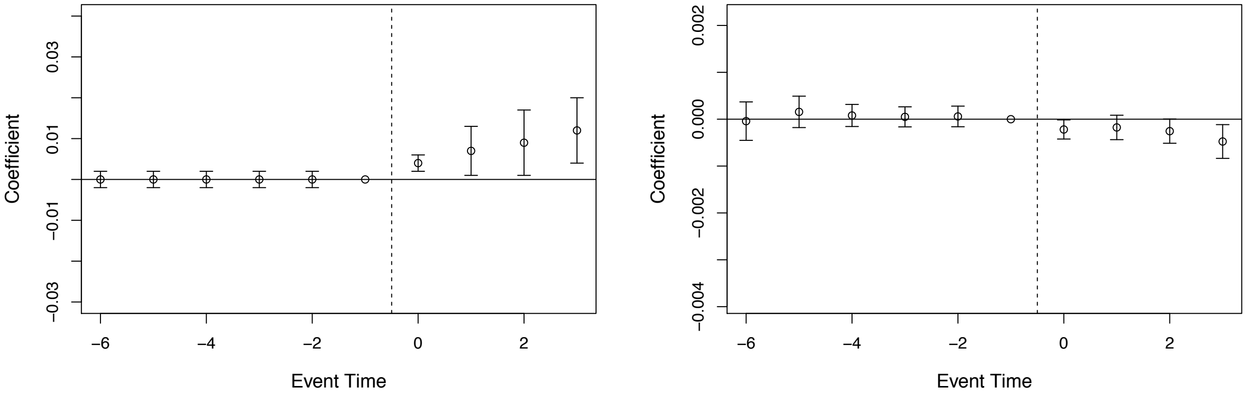
(a) Medicaid Coverage (b) Annual Mortality

Pre-ACA Years



(c) Medicaid Coverage (d) Annual Mortality

Income 400%FPL +



(e) Medicaid Coverage (f) Annual Mortality

Note: These figures plot coefficients from equation (1) for those age 65 and older in 2014 who would not have been affected by the Medicaid expansion due to their eligibility for the Medicare program (Row 1) and for those in higher income households who were likely less affected (Row 3). Row 2 plots the coefficients from (1) but uses pre-ACA years as a placebo test (see text for details).

Medicaid and Mortality: New Evidence from Linked Survey and Administrative Data

Appendix

Sarah Miller Sean Altekruse Norman Johnson Laura R. Wherry

A Additional Results

We present additional tables discussed in the main text in this section in Tables A1-A4. See the main text for further discussion of these results.

B First Stage Eligibility Estimates

To estimate the change in Medicaid eligibility associated with the ACA Medicaid expansions, we use the 2008-2017 ACS downloaded from IPUMS USA (Ruggles et al., 2019) and impute eligibility for our sample using state eligibility rules for each year. We consider eligibility for low-income parents under Medicaid Section 1931 criteria in each state, as well as expanded eligibility for parents and childless adults under waiver programs that offered comparable coverage to the ACA Medicaid expansions. We do not consider expanded programs that cover a more limited set of services and follow documentation from the Kaiser Family Foundation (KFF) to make this determination.

Information on state eligibility thresholds for coverage for adults were compiled from the sources listed in Table A6. The notes column in the table provides a record of any decisions made in applying the eligibility rules or to reconcile inconsistencies across different sources. KFF documentation on eligibility thresholds over time, which were used as our primary source, take into account state rules on earnings disregards when applicable. We defined the family unit for eligibility determination using the health insurance unit definition prepared by the State Health Access State Assistance Center, see details in State Health Access Data Assistance Center (2012). Following Medicaid rules for countable income (Centers for Medicare & Medicaid Services, 2016), we did not include family income from the Temporary Assistance for Needy Families or SSI programs in the calculation of total family income.

C Evaluating Survey Undercount of Medicaid Enrollment

To explore the extent to which survey measures undercount the number of individuals in our sample who were enrolled in Medicaid at any point during the survey year, we undertake several different analyses. Survey measures may undercount yearly enrollment because of respondents misreporting coverage or because a respondent correctly reports non-enrollment at the time of the survey but enrolls at a different point during the year. To examine this type of undercount, we take advantage of linked survey and administrative data on Medicaid coverage through the National Health Interview Survey via the public use NCHS-CMS Medicaid Feasibility Files. For each eligible respondent in the NHIS, these feasibility files state whether the respondent is present in the CMS MAX Person Summary (PS) file in each year.[37] All Medicaid enrollees are included in the PS file if they were enrolled at any point

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[37] Respondents were eligible for linkage if they were age 18 or older at the time of the survey and if they consented to have their administrative data linked.

during that year, even if they were enrolled for only a partial year. We can therefore compare presence in the PS file to self-reported Medicaid coverage in the NHIS for individuals meeting our sample criteria (i.e., citizens, not receiving SSI, age 55 to 64 in 2014 and either in households earning under 138% FPL or having less than a high school education).

Ideally we would perform this calculation during the 2014 to 2017 years. However, a change in the way CMS collected administrative data from state Medicaid offices occurred in 2013 and resulted in far fewer states providing the necessary administrative data for linkages. Since the public use NHIS file does not contain state identifiers, we limit our analysis to years in our sample period during which all states were available, i.e. the 2008 to 2012 waves of the NHIS.

Our results are presented in Table A7. We see that while that 14.3 percent of NHIS respondents meeting our sample criteria reported being enrolled in Medicaid when asked as part of the survey, 19.3 percent were found to be enrolled at some point during the year in the administrative records. Enrollment would therefore be undercounted by approximately 35 percent ($\frac{19.3}{14.3} \approx 1.35$) relying on survey data alone, motivating our re-scaling of the survey first stage estimates.

We also supplement this analysis by using administrative enrollment data published in two sets of CMS reports. We calculate the difference-in-differences estimate from each set of reports and compare it to an estimate derived from survey reports for a similar population in the ACS. The first set of reports come from administrative enrollment data published by CMS and compiled by the Kaiser Family Foundation (KFF). Beginning in July 2013, CMS has published monthly total enrollment numbers in their Medicaid and CHIP Application, Eligibility Determination, and Enrollment Data reports. The KFF has compiled these monthly reports and calculated pre-ACA average monthly Medicaid/CHIP enrollment during the period July-September 2013, as well as average Medicaid/CHIP monthly enrollment for each month during the post-expansion period (Kaiser Family Foundation, 2019c). These totals refer to the total number of unduplicated individuals enrolled in Medicaid and CHIP.

We combine these administrative totals with state population estimates from each year of the ACS to create enrollment rates. Using the average monthly enrollment rates for 2013 and the monthly enrollment rates for 2014-2017, we then estimate a difference-in-differences model that includes state, year, and month fixed effects. We follow the same definition and timing of Medicaid expansion, as well as exclude the 5 early expander states, as in our main mortality data analyses. We use population weights and cluster the standard errors at the state level. We then compare these estimates to those acquired using only ACS survey data over the same period.

The results are reported in Table A8. The estimates using CMS data show a larger rate of Medicaid participation at baseline and a larger increase in participation under the ACA Medicaid expansions when compared to the estimates using ACS data. The change associated with Medicaid expansion is 23% larger when estimated with the administrative data.

The second analysis uses the MAX validation reports, which report the total number of Medicaid enrollees by state as well as the percent of enrollees in the 45 to 64 age range. These data have two advantages over the KFF monthly reports: they report the total number of individuals ever enrolled during the year and they are available for a population closer in age to the group examined in the main study. However, there are two major disadvantages to these reports: they are only available through 2014, and only for 16 states.[38] Using these data, and the corresponding sample from the ACS, we conduct a similar comparison. These results are reported in columns 3 and 4 of Table A8. For this age group and set of states, we find a dramatically larger effect of the ACA expansions using the enrollment rates based on the administrative data – about an 8.6 percentage point increase in enrollment – compared to those derived from the ACS – an increase of only 2.6 percentage points.

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[38] These states are CA, GA, ID, IA, LA, MI, MN, MS, MO, NJ, PA, SD, TN, UT, WV, and WY.

Table A1: Descriptive Statistics of Main Sample by State Expansion Status

	Expansion State	Non-expansion State
% White	70.9	68.7
% Black	14.9	24.2
% Hispanic	15.3	12.2
% Uninsured	32.6	37.3
% Medicaid	20.5	16.2
% Less than High School Education	45.3	46.8
Average Age in 2014	55.8	55.9
Average Income relative to FPL	1.47	1.40
N	231,200	190,448

Note: This table displays weighted means for residents in expansion and non-expansion states meeting the sample criteria described in the text. These statistics were calculated using publicly-available 2008-2013 ACS data rather than the restricted version used in the main analysis.

Table A2: Impact of the ACA Expansions on Coverage and Mortality: Cause of Death

	Deaths from Internal Causes	Deaths from Health Care Amenable Causes	Deaths from External Causes
Difference-in-Differences Model:			
Expansion × Post	-0.002351 (0.006754)***	-0.0009907 (0.0005043)*	0.0003831 (0.0001998)*
Event Study Model:			
Year 1	-0.002207 (0.001262)*	-0.0004100 (0.0008170)	0.0000954 (0.0003947)
Year 0	-0.002090 (0.001081)*	-0.001029 (0.0007480)	0.0002501 (0.0003154)
Year -1 (Omitted)	0	0	0
Year -2	-0.0005340 (0.0008272)	0.0006530 (0.0005310)	-0.0000658 (0.0003380)
Year -3	0.0008772 (0.001038)	0.0001387 (0.0007171)	-0.0000658 (0.0004400)
Year -4	-0.0004416 (0.001118)	-0.0000797 (0.0008195)	-0.0003190 (0.0003844)
Year -5	0.0007490 (0.0009490)	0.0004741 (0.0007390)	-0.0002190 (0.0003696)
Year -6	0.0007098 (0.001062)	0.0002333 (0.0006164)	-0.0006014 (0.0003489)
N (Individuals x Year)	683000	683000	683000
N (Individuals)	88500	88500	88500

Note: This table displays the event study coefficient estimates of equation (1) using the MDAC. Sample sizes are rounded following Census disclosure rules. See text for more details. DRB Disclosure Approval #: CBDRB-FY19-310. Significance levels: *=10%, **=5%, ***=1%.

Table A3: Impact of the ACA Expansions on Mortality: Impact by ICD Grouping

	Infectious Disease	Neoplasms	Diseases of the blood and blood-forming organs	Endocrine, nutritional and metabolic diseases	Mental/Behavioral
Expansion × Post	-0.0000671 (0.0001273)	-0.0005512 (0.0004556)	0.0000337 (0.0000345)	-0.0004314 (0.0002277)*	-0.0000465 (0.0001100)
Mean	0.004121	0.02718	0.0002675	0.005279	0.001676
	Nervous System	Circulatory System	Respiratory	Digestive	Skin and Sub-cutaneous Tissue
Expansion × Post	-0.0000131 (0.00011.62)	-0.0008861 (0.0004804)*	-0.0003801 (0.0002758)	-0.0000046 (0.000243)	-0.00002550 (0.0000119)**
Mean	0.002392	0.02504	0.008223	0.006589	0.00008866
	Musculoskeletal system	Genitourinary system	Other		
Expansion × Post	0.0001148 (0.0000706)	-0.0001297 (0.0001101)	0.0003175 (0.0001910)		
Mean	0.0004495	0.002094	0.07006		

Note: This table displays the difference-in-differences coefficient estimates using the MDAC. Each entry is the result from a different regression. Rates are reported under coefficient estimates. Sample sizes are rounded following Census disclosure rules. DRB Approval Number: CBDRB-FY19-400. See text for more details. Significance levels: *=10%, **=5%, ***=1%.

Table A4: Impact of the ACA Expansions on Coverage and Mortality: Additional ACS Analyses

	Died in Year Ages 19-64	Died in Year, Uninsured at Time of ACS
Difference-in-Differences Model:		
Expansion × Post	-0.0001900 (0.0001680)	-0.001500 (0.0006590)**
Event Study Model:		
Year 3	-0.0003183 (0.0002191)	-0.002517 (0.0008802)**
Year 2	-0.0001270 (0.0001702)	-0.001425 (0.0009531)
Year 1	-0.0002309 (0.0001124)**	-0.001437 (0.0006387)**
Year 0	-0.0001541 (0.0001034)	-0.0008643 (0.0008343)
Year -1 (Omitted)	0	0
Year -2	-0.0000201 (0.0001146)	0.0001791 (0.0008579)
Year -3	0.0000318 (0.0001471)	-0.0005425 (0.0007373)
Year -4	-0.0000049 (0.0001663)	0.0001925 (0.0009295)
Year -5	0.0000138 (0.0001890)	0.001409 (0.001057)
Year -6	-0.0002170 (0.0002160)	0.0001959 (0.001126)
N (Individuals x Year)	23630000	1280000
N (Individuals)	3160000	180000

Note: This table displays the event study coefficient estimates of equation (1) using the ACS. Sample sizes are rounded following Census disclosure rules. See text for more details. DRB Disclosure Approval #: CBDRB-FY19-310. Significance levels: *=10%, **=5%, ***=1%.

Table A5: Results from the Oregon Health Insurance Experiment for participants age 55-64 in 2008

	Control Group Mean	Reduced Form	2SLS	p-value
Alive	0.977	0.0042	0.016	0.128
N	6550 (Control)	4240 (Treatment)		

This table uses the public-use replication kit of the Oregon Health Insurance Experiment to estimate the impact of Medicaid on individuals who were between the ages of 55 and 64 at the time of the experiment. The data and code were downloaded from `https://www.nber.org/oregon/4.data.html`.

Table A6: Sources for Parent and Adult Medicaid Eligibility Rules by Year

Year	Sources	Notes
2008-2010	Adults: NGA Center for Best Practices (2010), Table 9	We follow the criteria reported in Heberlein et al. (2011), Table 4 to determine whether programs described in NGA Center for Best Practices (2010) meet the full coverage criteria. We turned to additional sources to reconcile other differences with the program details reported in Kaiser Family Foundation (2019a). Specifically, we added a program for AZ following National Conference of State Legislatures (2009), a DC program based on Meyer et al. (2010), and altered HI and VT program details using Indiana Legislative Services Agency (2011).
2011-2017	Adults: Kaiser Family Foundation (2019a)	We consider eligibility rules to be in place as of the date of the relevant KFF survey. To be consistent with our definition of implementation of the ACA Medicaid eligibility expansions, we consider the expansion in Indiana to take place in 2015.
2008-2017	Parents: Kaiser Family Foundation (2019b)	We consider eligibility rules to be in place as of the date of the relevant KFF survey with the exception of the December 2009 survey for parents eligibility, which we apply to the 2010 year. To be consistent with our definition of implementation of the ACA Medicaid eligibility expansions, we consider the expansion in Indiana to take place in 2015.

Table A7: Undercount Estimates from the NHIS-CMS Linked Feasibility Files

% Reported Enrolled in Survey	0.143 (0.008)
% Reported Enrolled in Administrative Data	0.193 (0.009)
N	2,267

Note: This table displays the fraction of NHIS respondents meeting sample inclusion criteria who reported being enrolled in Medicaid in the NHIS (first row) versus those who were shown to be enrolled in Medicaid in the CMS administrative data (second row). Standard errors are in parentheses.

Table A8: Comparison of Medicaid Coverage Estimates: CMS vs. ACS

	All Ages and States, 2013-2017		Age 44-64, 17 States, 2012-2014	
	Enrollment Based on CMS Enrollment Reports	Enrollment Based on ACS Data	Enrollment Based on MAX Validation Reports	Enrollment Based on ACS Data
Expansion x Post	0.0382*** (0.0093)	0.0309*** (0.0049)	0.0862*** (0.0248)	0.0258*** (0.0065)
Baseline Mean in Expansion States	0.197	0.172	0.120	0.108
Number of Observations	2,103	14,323,891	48	2,423,253

Note: The first two columns in this table display the difference-in-differences estimates for analyses using monthly enrollment rates constructed from CMS enrollment reports and self-reported enrollment from the ACS for all ages, respectively, for the years 2013-2017. All regressions include state and year fixed effects and the regression with CMS data also includes month dummies. The second two columns display the DID estimates for analyses using total number of adults ages 45-64 ever enrolled in each year during 2012-2014 from the MAX validation reports from 16 states, as well as the estimates derived from a comparison ACS sample for those years. The regressions include state and year fixed effects. For all regressions, robust standard errors are clustered by state. The regressions with administrative data use state population estimates as weights, while the analyses with ACS data use survey weights. See text in Appendix Section C for more details on the data. Significance levels: *=10%, **=5%, ***=1%.

Leighton Ku
Teresa A. Coughlin

Sliding-Scale Premium Health Insurance Programs: Four States' Experiences

As publicly funded health insurance shifts more toward coverage of working families of low and moderate incomes, there has been growing interest in beneficiary cost sharing, in the form of sliding-scale premiums. In the 1990s, Hawaii, Minnesota, Tennessee, and Washington initiated expansion programs that used sliding-scale premiums for working-class families. The experience in these states indicates that it is feasible to require cost sharing of premiums, but there are a number of design and operational complexities. A preliminary analysis indicates that, as expected, higher out-of-pocket premium shares were associated with lower participation rates.

An important goal for the nation has been to reduce the number of uninsured Americans, particularly those in working-class families. One major policy option is helping lower-income families purchase insurance by offering government subsidies. The 1997 creation of the Children's Health Insurance Program (CHIP) signals the nation's continuing commitment to expand coverage using public programs. Earlier in the decade, many states began initiatives to offer insurance coverage to families whose incomes were above the traditional limits of eligibility for programs like Medicaid. These initiatives included state-funded health insurance programs like Washington's Basic Health Plan and Minnesota's MinnesotaCare, as well as Medicaid Section 1115 demonstration projects like Tennessee's TennCare or Hawaii's QUEST (Wooldridge et al. 1996; Coughlin et al. 1997; Nichols et al. 1997; Lipson and Schrodel 1996; Diehr et al. 1996; Call et al. 1997). These programs required that some participants pay premiums on a sliding-scale basis. The enabling legislation for CHIP also let states require that enrollees pay a modest share of the premiums for CHIP coverage.

In contrast, Medicaid is free to participants. Since the greatest numbers of uninsured people are in working households of low to moderate income (Hoffman 1998), initiatives to expand coverage to people with incomes above the poverty level have led to the question of whether the recipients should bear responsibility for paying a share of the cost of insurance.

Policy discussions about cost sharing raise many issues. Proponents note that sliding-scale premiums target subsidies toward lower-income people and provide less governmental assistance to those with more income. Some believe that cost sharing promotes personal responsibility and eases some of the political and social stigma associated with Medicaid. Premium sharing makes the government assistance more like private health insurance, in which cost sharing is the norm, and may serve as a better bridge between public and private health insurance. Similarly, requiring people to pay part of the premiums

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might reduce "crowd out," a practice in which some people drop private coverage to take advantage of the free (or less expensive) public benefits (Cutler and Gruber 1997; Call et al. 1997; Dubay and Kenney 1996, 1997). Finally, cost sharing reduces governmental outlays, because beneficiaries shoulder some of the expenses and participation tends to be lower.

However, requiring that beneficiaries pay part of the premium also has disadvantages. Most important, this cost sharing reduces participation among the target population. Many families may choose to use their income to pay for food, rent, clothing or other goods rather than buy health insurance. Some families, especially those below the poverty level, may lack the discretionary income to buy insurance, no matter how cheap. Premiums can lead to adverse selection, because sicker people may be more likely to buy in than the healthy. This problem may be exacerbated if people choose to pay premiums in the months when they need medical care, but not when they are healthy.[1] The net result is that people covered might have higher-than-average medical needs, leading to higher medical expenditures per beneficiary. However, an early study of Washington's Basic Health Plan failed to find evidence of adverse selection (Diehr et al. 1993). Lastly, premiums increase programs' administrative complexity, requiring a billing system and development of policies on handling delayed payment or nonpayment of premiums.

Because states did not charge premiums until recently, there has been little information about how cost-sharing programs are designed or administered, or how they affect participants. To understand these programs, we interviewed state officials and reviewed state documents and data, including the premium schedules and participation counts. This paper reviews a number of the issues concerning sliding-scale premium schedules and the experience of four states—Hawaii, Minnesota, Tennessee, and Washington—that initiated such programs. We focus on policies and experiences in 1995, although we briefly discuss programmatic changes since that time. First, we provide a brief background on each of these four state programs. Next, we discuss how they structured their premium programs. We then provide some preliminary analyses of participation rates and the relationship of price and participation. We conclude by discussing the policy implications of our findings.

What Happened in the Four States?

Four states served as laboratories for understanding how sliding-scale premiums work for low- to middle-income families. Hawaii, Minnesota, Tennessee, and Washington developed relatively large, subsidized insurance programs in the early 1990s. Each had ambitious goals to reduce the number of uninsured people and each wanted to cover uninsured working families. At the same time, state policymakers felt it was appropriate to require that higher-income participants contribute to the cost of insurance. As a result, the states designed cost-sharing systems, including sliding-scale premiums charged to beneficiaries. An additional element in these states' reforms was the development of managed care systems. Hawaii, Tennessee, and Washington required that participants join capitated managed care plans, and Minnesota required joining a health plan in 1996.

While they all were state-initiated, the programs' origins varied. Washington and Minnesota began programs using only state funds. While this meant that they had more flexibility, it also meant that budgets and benefit packages were tighter. Tennessee's and Hawaii's programs were funded jointly by the state and the federal governments as Section 1115 Medicaid demonstration programs. Thus, the federal government needed to approve the policies and some Medicaid legislative requirements still applied. Tennessee's TennCare and Hawaii's QUEST had broad, Medicaid-like benefit packages and were barred from charging premiums to those who were previously eligible for Medicaid.

TennCare

In January 1994, Tennessee implemented one of the most expansive subsidized insurance programs in the nation. Initially, all uninsured people, if they were uninsured on a given date before application, were eligible to join, but subsidies were available only for those with family incomes up to 400% of the poverty level. While TennCare was free to those below poverty, premiums gradually rose above that level. TennCare recipients with incomes above poverty also were subject to deductibles and copayments. Because of budget constraints, TennCare stopped enrolling new uninsured people in January 1995. It continued to enroll people covered under regular Medicaid eligibility rules and those who were uninsurable due to special medical conditions. The uninsured people already participating in TennCare were grandfathered and continued to get insurance.

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In 1997, the program was reopened to displaced workers and children under the state's CHIP.

In the beginning, Tennessee's program had some serious administrative weaknesses. Because of its mail application system, applicants usually did not know how much they owed until *after* they were enrolled. Further, the state failed to send out premium billing notices for six months and did not send another billing notice until December 1994. When participants finally received billing notices for back-owed premiums, many were unable or unwilling to pay. The state dropped more than 60,000 participants for nonpayment during 1995, while thousands of others covered their debts by paying under an installment plan.

QUEST

Hawaii's program began in August 1994. It served nondisabled, nonelderly people who had incomes up to 300% of the poverty level and were not covered by the state's employer-mandated private health insurance. In 1994, people with incomes between 133% and 300% of poverty paid sliding-scale premiums and were subject to nominal copayments. Due to fiscal problems and a class action lawsuit, Hawaii undertook a series of changes to reduce caseloads and spending.[2] In 1995, the state raised recipients' share of premiums. In 1996, it imposed an assets test, charged full premiums to those above 100% of poverty and imposed a limited moratorium on enrolling new applicants. In 1997, QUEST eligibility was limited to those with incomes below poverty (except for pregnant women and infants with family incomes up to 185% of poverty and children ages 1 to 5 with incomes up to 133% of poverty).

Basic Health Plan (BHP)

Washington began its BHP in 1989 as a pilot program that was administratively separate from Medicaid. Under the program, adults and children had different rules. For example, in 1995, children who were enrolled in BHP and had family incomes below 200% of poverty were counted under a Section 1902(r)(2) amendment, were eligible for federal match payments under Medicaid, and received the broad Medicaid benefits package. By contrast, adults had a more restricted benefits package that was closer to private insurance than Medicaid (e.g., no prescription drug coverage, deductibles for hospital stays). Sliding-scale premiums applied to adults at all income ranges; those with no income were charged $10 per month for an individual or $20 for a family.[3] The premium subsidies declined to zero for people with incomes above 200% of poverty, although people with higher incomes could enroll. In early 1996, the state reduced premium levels in a successful attempt to boost participation. Later in 1996, the state capped the number of adults admitted because the program reached its funding limits.

MinnesotaCare

In 1992, Minnesota created its subsidized insurance program, MinnesotaCare. It served uninsured families with incomes below 275% of poverty, as well as single adults and childless families with low incomes (up to 135% of poverty in 1996). Participants had to have been uninsured at least four months before applying and could not have had access to employer-paid (i.e., employer pays more than half the premium) insurance within the previous 18 months. Like BHP, it had a narrower benefit package (e.g., a deductible and $10,000 limit for inpatient care) than Medicaid and was administered separately. Premiums were based on a sliding scale, except that children in families with incomes under 150% of poverty (who were not otherwise Medicaid eligible) paid a flat $4 per month. Premiums were charged for adults with incomes above the maximum Medicaid income eligibility level. In 1995, MinnesotaCare operated as a fee-for-service insurance program, but it shifted to capitated managed care the next year. In 1996, benefits for pregnant women and children under age two with incomes below 275% of poverty, and other Medicaid-eligible people (e.g., 11-year-olds under 100% of poverty) who chose MinnesotaCare, became eligible for federal financial matching under a Section 1115 demonstration program.

How Were the Premiums Structured?

A key aspect to subsidized premium programs is the design of the premium schedule. All four states created premium structures that were progressive from no income to 200% of poverty; that is, the price (as a percentage of family income) rose for those with higher incomes. Each state charged full premiums to families at the top end of the income range. Thus, prices were quite high at the top of the income range—more than 5% of family income. Tables 1, 2, 3, and 4 provide data about the premium levels in 1995 for a single adult and for a family of four.

There were some interesting differences in how states structured their programs. Minnesota and Washington charged small amounts even to those with incomes below the poverty line (e.g., a family

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Table 1. Premium levels for participants in Hawaii QUEST, early 1995

	Individual premium share			Family of four premium share		
Income as % of poverty	Monthly payment ($)	% of full premium[a]	% of income	Monthly payment ($)	% of full premium[a]	% of income
0%	0	0	0	0	0	0
50%	0	0	0	0	0	0
100%	0	0	0	0	0	0
150%	13	7	1.4	53	7	2.8
200%	38	20	3.0	150	20	6.0
250%	113	60	7.2	451	60	14.3
300%	188	100	10.1	752	100	19.8
350%	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible
400%	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible

Note: No cost sharing at or below 133% of poverty or for pregnant women and children under 185% of poverty.
[a] Assumes a premium of $188 per person, single or family. Every additional person is added at the rate of a single person, up to a family size of five. The premium shares are based on the percentage of poverty, but the actual costs vary with the island and the plan selected by the client. Includes medical and dental costs.

with no income would pay $12 per month in Minnesota or $20 in Washington), while those below poverty in Tennessee and Hawaii were not charged anything. Minnesota and Hawaii capped eligibility to those with incomes below 275% or 300% of poverty, respectively, while people with incomes beyond the subsidy limit could enroll in TennCare or BHP, but had to pay full premiums.

States also varied in the relative price of premiums for individuals and families. Minnesota and Washington had similar or lower prices for families (measured as a percentage of family income) than for single adults.[4] To do this, both states provided higher subsidies (as a percentage of the full premium) for families than individuals. In contrast, Tennessee and Hawaii used similar subsidies for individuals and families, expressed as a percentage of the full premium; the net effect was that families paid a larger share of family income than individuals. In Hawaii, higher-income families could owe as much as 20% of family income at the upper range of income. To keep relative premium levels similar for families and individuals, states need to offer higher subsidy rates for families.[5]

The programs varied in how they calculated the amount beneficiaries were charged. MinnesotaCare's fee-for-service premium was fixed and TennCare set the capitation rates for all plans. Thus, the out-of-pocket premium schedules were uniform for people with equivalent incomes. In comparison, Hawaii and Washington paid health plans different prices and provided percentage-based subsidies. Thus, there was a modest incentive to pick a less expensive plan. For example, if a person who owed

Table 2. Premium levels for participants in MinnesotaCare, late 1995

	Individual premium share			Family of four premium share		
Income as % of poverty	Monthly payment ($)	% of full premium[a]	% of income	Monthly payment ($)	% of full premium[a]	% of income
0%	4	4	Not applicable	12	4	Not applicable
50%	5	5	1.6	12	4	1.9
100%	14	13	2.2	29	9	2.3
150%[a]	Not eligible	Not eligible	Not eligible	72	23	3.8
200%[a]	Not eligible	Not eligible	Not eligible	149	48	5.9
250%[a]	Not eligible	Not eligible	Not eligible	278	89	8.8
300%	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible
350%	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible
400%	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible	Not eligible

Note: MinnesotaCare is a state-funded program with a limited benefit package. Under a 1902(r)(2) rule, children and pregnant women can be enrolled in MinnesotaCare and also be covered by Medicaid.
[a] Income eligibility up to 275% of poverty for families with children or up to 135% for childless adults. Individual members of families with children may join, in which case they pay premiums equal to 3.9% of income at 150% of poverty, 5.9% of income at 200% of poverty, and 6.7% of income at 250% of poverty. Families do not pay for more than three members. Assumes full premiums of $104 per month for an individual and $312 for a family of four.

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Table 3. Premium levels for participants in TennCare, early 1995

	Individual premium share			Family of four premium share		
Income as % of poverty	Monthly payment ($)	% of full premium[a]	% of income	Monthly payment ($)	% of full premium[a]	% of income
0%	0	0	0	0	0	0
50%	0	0	0	0	0	0
100%	3	2	.4	7	2	.5
150%	11	8	1.2	27	8	1.4
200%	55	40	4.4	137	40	5.4
250%	74	54	4.7	184	54	5.8
300%	95	70	5.1	238	70	6.3
350%	109	80	5.0	273	80	6.2
400%	137	100	5.5	342	100	6.8

[a] The premiums for those at or above 200% of poverty are based on the "low deductible" option. Using the "high deductible" option would lead to lower premiums, but higher deductibles. There are no copayments for those below poverty or Medicaid eligibles. Between 101% and 199% of poverty, the copayments range from 2% to 8% and are set at 10% above 200% of poverty. Assumes full premiums are $137 per month for one person and $342 for a family of four.

a 20% premium share could choose two plans that differed in full premiums by $10, that person would have to pay $2 more to get the higher-cost plan. Neither state used a defined contribution approach, which sets a fixed-dollar subsidy at a given poverty level and makes the beneficiary responsible for any difference between the full premium and the fixed subsidy.

Although this paper focuses on the 1995 experiences of the four states, there were changes in premium schedules over time. Two states purposefully used premiums as a caseload management tool, with mixed success. Hawaii initially increased premiums in order to reduce QUEST participation levels. Preliminary data indicate that the caseloads did not change noticeably until the state made much stricter changes in eligibility (e.g., imposing a moratorium on new cases and adding an assets test). On the other hand, Washington state was concerned that BHP had low participation, and greatly reduced BHP premiums in early 1996. Participation in the program roughly doubled after the prices were lowered.

What Were Participation Rates?

The share of the premium that people must pay may affect participation levels and, consequently, determine the extent to which public subsidy programs achieve their goals of lowering the number of uninsured people. Premium shares are relevant to program budgeting, since they may affect both the number of people participating and the amount of

Table 4. Premium levels for participants in the Washington Basic Health Plan (BHP), late 1995

	Individual premium share			Family of four premium share		
Income as % of poverty	Monthly payment ($)[a]	% of full premium[b]	% of income	Monthly payment ($)[a]	% of full premium[b]	% of income
0%	10	8	Not applicable	20	5	Not applicable
50%	10	8	3.2	20	5	3.2
100%	23	19	3.7	46	11	3.6
150%	52	44	5.6	104	25	5.5
200%	123	104	9.9	246	59	9.7
250%	123	104	7.9	246	59	7.8
300%	123	104	6.6	246	59	6.5
350%	123	104	5.6	246	59	5.6
400%	123	104	4.9	246	59	4.9

Note: BHP is a state program with a limited benefit package, although children may participate in BHP Plus, which is under Medicaid.

[a] Includes a minimum of $10 per individual and $20 per family. Also, for those at or above 200% of poverty, includes a $5 per person administrative fee, which adds another 4%.

[b] Actual premiums vary by plan, although consistent premium shares are used. Assumes monthly premiums of $118 for single adults and $419 for family of four. Because of Medicaid tie-in, children under 18 under 200% of poverty are not charged premiums.

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government subsidy per participant. In this section, we estimate 1995 program participation rates for three of the four states.

We used the following strategy to estimate participation rates. Each state provided data about the number of participants at varying income levels in 1995, which corresponded to differing premium levels paid by beneficiaries. We excluded data from Tennessee because it appeared that many of those who joined TennCare did not know how much they would pay when they signed up and, because of billing problems, many never paid their premiums anyway.[6] Thus, the TennCare experience did not always reflect people's willingness to buy coverage.

We estimated the size of the eligible population in each state by using a merged three-year sample of the 1991–1993 Current Population Surveys (CPS). The CPS data were edited to adjust for Medicaid undercounts using the Urban Institute's TRIM2 microsimulation model (Winterbottom, Liska, and Obermaier 1995). We estimated the number of uninsured people in each state in income cohorts, where each cohort is defined by a range of 25 percentage points of the federal poverty level (i.e., 101% to 125%, 126% to 150%, etc.). We "aged" these estimates forward to 1995 levels. Using state administrative data on the number of participants as numerators, and CPS estimates of the eligible population as denominators, we computed participation

Table 5. Premium levels and estimated participation rates by poverty range, 1995

Income range as % of poverty	Median premium for two as % of income	Estimated participation as % of uninsured
Hawaii QUEST—early 1995		
133–149%	1.4	42
150–174%	2.4	47
175–199%	3.5	45
200–224%	5.3	33
225–249%	6.9	11
250–274%	11.9	10
275–300%	13.6	3
MinnesotaCare—late 1995		
100–124%	2.3	45
125–149%	3.1	55
150–174%	3.8	29
175–199%	4.8	21
200–224%	5.9	8
225–249%	7.4	5
250–275%	8.8	3
Washington BHP—late 1995		
25–49%	3.2	32
50–74%	1.9	21
75–99%	3.1	33
100–124%	3.0	24
125–149%	3.7	24
150–174%	4.4	11
175–200%	5.1	6

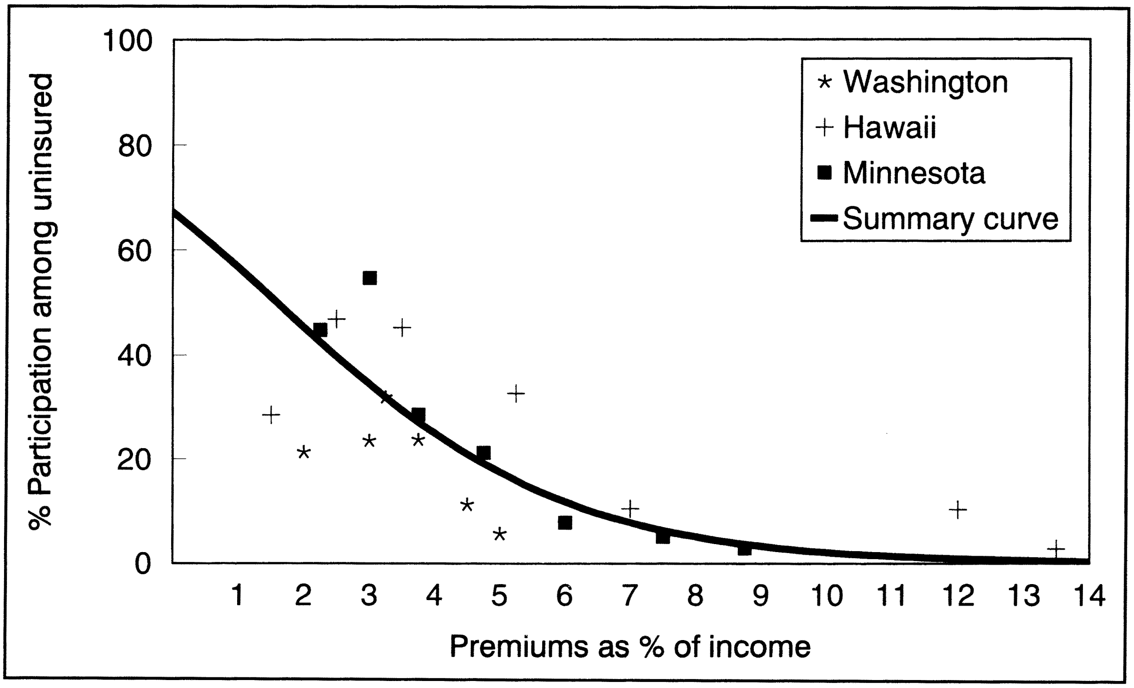

Figure 1. Estimated level of participation, based on premium levels in three states, 1995

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rates in each income cohort. Finally, we computed the median monthly cost of premiums for two people in a two-person family (an adult and a child) to represent the premiums paid in each income bracket (Ku and Coughlin 1997). A two-person family corresponds to typical insurance units in the programs and includes a higher- (adult) and lower- (child) cost beneficiary.

The estimates for the three states are summarized in Table 5 and shown graphically in Figure 1. Within each state, the general relationship showed—as expected—that participation fell as the amount that people had to pay rose. The figure shows a curve that summarizes the relationship of premiums as a percentage of family income and participation rates for the three states, pooled together. The method of estimating this relationship is shown in the notes section.[7] We chose to pool the data for the three states because there was a very limited number of observations and because pooling reduces the impact of sampling error in any given observation or in any given state.

The summary curve helps illustrate the general relationship of price and participation. Three insights also can be drawn from the summary curve (and also hold true for each state individually). First, average participation rates fall as the relative price (premium share as a percentage of income) rises. Second, even when the premium share is relatively high, a few people still participate. Third, if we extrapolate the curve, it is apparent that many eligible people will not participate even when programs are free.

More specifically, the summary curve estimates that raising premium shares from 1% to 3% of family income decreases expected participation rates from about 57% to 35% among the uninsured. When the premium is 5% of income, participation is about 18%. This model suggests that a useful way to analyze the effect of cost sharing is by measuring the premium share as a percentage of family income.

The data suggest that participation levels were somewhat lower in Washington state than Minnesota or Hawaii. As mentioned earlier, Washington officials also were concerned about poor participation, and lowered premiums to increase participation in 1996. The reason for Washington's lower participation rate in 1995 is not clear, but we can offer a few possible explanations. BHP offered a limited benefit package, which might have been less attractive than a richer Medicaid-type benefit. On the other hand, MinnesotaCare's benefits also were limited, but that program had higher participation. BHP required that families with incomes below poverty pay premiums, but MinnesotaCare did so as well. It appeared that each of the three programs had a lot of publicity and outreach, but it is possible that public awareness was lower in Washington, although this is hard to measure. A host of other factors, such as the employment, cultural, or health care environment in each state also may affect participation rates.[8] We were not able to control for these factors in this analysis.

This simple and preliminary analysis indicates the general relationship of premium share to income and participation rates. Many important issues that might affect participation remain unresolved. For example, is participation higher in states with more generous benefit packages? How do operational factors, such as outreach or ease of application, affect participation rates? More research is needed to answer these questions.

Discussion

The experiences of Hawaii, Minnesota, Tennessee, and Washington demonstrate that sliding-scale premium programs for low-income individuals and families are feasible. There are some administrative complexities, however. Implementing such a system requires that states carefully design premium share schedules, establish administrative policies for problems such as what to do when people do not pay their premiums, and arrange for either the state agency or health plans to administer bill collection.

More recent interest in premiums has been fueled by the creation of CHIP programs. Federal legislation lets states charge premiums or copayments, particularly when the CHIP programs are independent of Medicaid. For example, children in families with incomes above 150% of poverty cannot bear total costs greater than 5% of family income. Preliminary information indicates that the actual CHIP premiums tend to be very low (e.g., $5 to $10 per child per month) and generally have just one or two levels (Riley and Pernice 1998). While we believe that the experiences reported here are relevant to CHIP programs, they are not identical. Most important, CHIP programs are only for children, while the programs in the four study states were for adults as well as children. It is possible that the willingness to buy insurance differs for situations in which only children or the whole family are covered.

States likely will continue to test ways to expand health insurance coverage using subsidy programs

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for working-class families. The experience of these four states illustrates the possibilities and limitations of such programs. However, these findings are not generalizable to all possible subsidy arrangements: they reflect a handful of states with specific types of programs. Another commonly mentioned policy option is subsidizing the purchase of private insurance using tax credits or vouchers. This option poses a different set of operational and conceptual issues and the findings of this study are not applicable.

In designing and implementing subsidized insurance programs, there is an inherent tension between the goals of lowering governmental cost or encouraging personal responsibility (leading to higher premium shares) and maximizing participation (leading to lower premium shares). The data from these states suggests that, while many low-income people would pay premiums to purchase subsidized health insurance, their willingness to pay is limited. If, for example, the government subsidizes half the full cost of insurance for a family of four with an income of 200% of poverty, the family's out-of-pocket price still would be relatively high (about 7% of income). This study suggests that few, perhaps less than 10% of the uninsured, would participate if they had to pay 7% of their income for health insurance. Surveys consistently show that a major reason that people lack health insurance is because they say they cannot afford it (Thorpe and Florence 1999). It seems likely that future efforts to expand insurance coverage will require premium sharing by some beneficiaries. In designing and implementing these new initiatives, policymakers need to be careful in balancing budget resources with the goal of reducing the number of uninsured people.

Notes

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1 Programs can create some safeguards to minimize the extent of drop-out/drop-in. For example, if people voluntarily drop out of MinnesotaCare or are terminated for nonpayment of premiums, they are barred from re-entering the program for four months, with certain hardship exceptions. This is intended to prevent adverse selection.

2 The plaintiffs' attorney argued that since QUEST was only for nondisabled people, those with disabilities faced more restrictive eligibility criteria. They sought relief under the Americans with Disabilities Act. In light of the lawsuit and the state's fiscal problems, Hawaii made QUEST income eligibility much tighter.

3 Children under 200% of poverty and a few others are not charged premiums.

4 The family cost in Washington was lower than for a single adult, because children under 200% of poverty were free due to their Medicaid status.

5 A key reason for this discrepancy is that there are different economies of scale used in poverty measures and the pricing of insurance premiums. For example, poverty scales assume that a four-person family needs twice as much income as one person, while insurance costs for a four-person family are typically about three times as high as for a single adult.

6 Even so, participation rates for TennCare were similar to the other three programs'.

7 To generate the summary curve shown in Figure 1, we used a grouped logit model (Greene 1990) of the form:

$$\ln [p_i/(1 - p_i)] = \beta + \beta X_i + \epsilon_i,$$

where p_i is the participation rate for a given income poverty "cohort" in a state, X_i is the premium level and other related income measures, βs are estimated coefficients and ϵ_i is the error term. Weights were based on the number of people in each income bracket, normalized to average 1.0. There are 21 observations, shown in Table 2, each representing hundreds or thousands of people in each state who fall into a given income cohort. Because the range of eligibility differs in each state, the number of cohorts and income range represented varied from state to state.

We tested alternative model specifications including income (as a percentage of poverty), premiums expressed in dollar terms (rather than as a percentage of poverty) and interactions of income and premiums (Appendix Table). We included income terms and interactions of premiums and income in light of the concern that insurance might be a superior good and those with higher incomes are more willing to purchase it. The estimated models yielded consistent results that, as expected, higher premiums are associated with lower participation rates. In models that included income, income generally was not significant at a .05 level, but the non-significant trend suggested that higher incomes were associated with greater participation. The interactions of premium levels and income were not significant, but that is not surprising given the limited sample sizes. The simplest model (model 1), which used premiums as a percentage of income as the only independent variable, explained just about as much variance as the model which also included income. Addition of more variables did not substantially improve model fit or modify the coefficient for premiums. The curve shown in Figure 1 is based on the simple model 1.

We also estimated the models under a different ap-

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Appendix Table. Main and alternative models of the relationship of premiums and participation rates in Hawaii, Minnesota, and Washington, 1995

	Model 1 (main model)		Model 2		Model 3		Model 4		Model 5		Model 6	
	Coefficient	*t*	Coefficient	*t*	Coefficient	*t*	Coefficient	*t*	Coefficient	*t*	Coefficient	*t*
Using premiums as % of income												
Intercept	.7239	1.803	.4633	1.054	−.3678	−.293						
Premiums as % of income	−.4555	−4.056	−.6353	−3.642	−.3343	−.727						
Income as % of poverty			.0065	1.329	.0105	1.394						
Interaction premium and income					−.0015	−.709						
Adjusted *R*-squared		.436		.458		.442						
Using premiums in $ per month												
Intercept							−.1444	−.581	−1.0356	−1.720	−.8381	−1.404
Premiums in $							−.0161	−3.314	−.0294	−3.091	−.1002	−2.080
Income as % of poverty									.0108	1.612	.0192	2.240
Interaction premium and income											.0002	1.498
Adjusted *R*-squared								.333		.384		.425

Note: Dependent variable: $\ln[p/(1-p)]$ where p = probability of participation among the uninsured.

proach, using bootstrap resampling methods with random resampling of the 21 cases with replacement to create 50 bootstrap data sets. We used the 50 data sets to examine the possibility that the model may be biased because the underlying population parameters are not normally distributed or that extreme values in one or two observations may skew results (Mooney and Duval 1993; Veall 1998). The bootstrap coefficients were quite similar to the single model and the standard errors were marginally larger, suggesting that bias was minimal. For example, the single model estimates that participation with a premium share at 1% of income would be 57%, while the pooled bootstrap estimate is 55%, virtually the same.

We acknowledge that this analysis is simplistic and is sharply limited by data availability. A better data set would include large representative surveys of low-income people in each state, measuring program participation, income and premiums paid, as well as factors that also might affect the demand for insurance, including health status, family composition, employment status, and perceptions of the program, including awareness of the insurance program, ease of application, and perceived generosity of benefits. However, we are unaware of a better data set for this purpose.

8 Lastly, the difference might be an artifact of sampling error. Because of limited sample sizes in the CPS (even using a three-year merged file), there is uncertainty in the estimates of the denominators. In this event, the apparently low participation rates in Washington may be caused by random noise and might have been found in one of the other two states if different samples were drawn.

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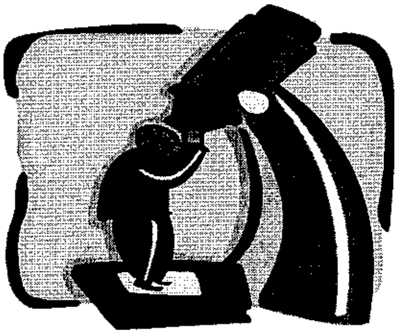
Utah Primary Care Network (PCN)

Disenrollment Report

July – August 2003

PCN Disenrollees



August 2004

Utah Department of Health
Center for Health Data
Office of Health Care Statistics

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Executive Summary

Utah's Primary Care Network (PCN) is the first Medicaid 1115 waiver program in the nation to provide publicly funded primary care coverage with donated hospital and specialty care. PCN is a limited benefits program for low income adults, 19-64 years of age.

Since July 2002, the Office of Health Care Statistics (Utah Department of Health) has conducted several surveys in fulfillment of reporting requirements outlined in the demonstration waiver. The results in this report are from PCN enrollees who disenrolled from the program, after one year of membership, in either July or August 2003.

- According to numbers obtained from PCN enrollment files and PCN quarterly reports submitted to the Centers of Medicare and Medicaid Services, 1,709 of 6,275 (27.2%) PCN enrollees left the program during July-September 2003.
- A total of 896 surveys were delivered to persons identified as disenrolled from the Primary Care Network. Usable surveys were returned from 517 individuals yielding a response rate of 61%. Respondents were more likely to be female (60% vs. 40% male), reside in urban areas (63%) rather than rural (37%). 76% were between the ages of 19 and 44.
- Over one-third of PCN disenrollees now have health insurance through another source.
- Nearly half of disenrollees report they are still eligible for the PCN program
- 29% of respondents listed 'finances' as a reason for disenrollment. Of these 63% could not afford the $50 enrollment fee and 78% reported not having health insurance after exiting the PCN program.
- 26% of respondents indicated they disenrolled from the program because it failed to meet their health care needs; 62% of this group reported that PCN did not offer needed services.
- Nearly half (47%) of survey respondents reported not seeing a health care provider in the previous twelve months.
- Two-thirds (66.7%) of those who needed prescription medication and nearly two-thirds (62.8%) of those who needed medical care reported that they were able to get it.
- Personal doctor/nurse satisfaction was rated high by 70% of survey respondents. However, PCN program satisfaction was rated high by only 41% of those surveyed.

Educational tools designed to enhance the importance of using primary care among PCN enrollees might be helpful. Because emergency room care is the second most utilized health service, enrollees might benefit from increased awareness of access options for less costly immediate care. Program satisfaction might improve by better communicating to PCN enrollees which services are covered versus which are not in the program.

Acknowledgements

The Primary Care Network Disenrollment Report was developed to meet program evaluation requirements outlined in the Utah 1115 Medicaid Demonstration Waiver. Numerous individuals made important contributions to this report's development.

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Introduction

The Primary Care Network (PCN) program was developed to provide primary care with donated hospital and specialty care for the uninsured, low-income adult population, aged 19 to 64 years. Since the beginning of PCN in July of 2002, the Office of Health Care Statistics (OHCS) has been gathering data on enrollees to examine the program's impact. Several studies are being conducted examining changes in health status and utilization. The purpose of the current report is to describe findings from a survey that assessed those who disenrolled from the PCN program after one year of membership.

Methods

Survey Instrument

OHCS and PCN staff collaborated to develop a 43-question survey instrument to be administered to disenrollees. The instrument contained questions in the following categories:

- Reasons for disenrollment
- Satisfaction with the program
- Health care utilization (past 6 months)
- Current health status (SF-12®)[1]

Utilization questions and a sub-set of the satisfaction questions were identical to those used in the Year 1 and Year 2 health assessment surveys[2]. This allowed comparisons between the various groups of PCN enrollees.

Note

Year 1 health assessments were conducted July-December 2002 and completed by each PCN applicant during the orientation process. Year 2 health assessments were conducted July-December 2003 through a questionnaire mailed to 3,000 re-enrollees.

Participants

Primary Care Network enrollees are required to pay an annual $50 enrollment fee for membership in the program. Each enrollee who has remained eligible for the entire year receives a notice by mail informing them of re-enrollment procedures. After sending back the

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[1] The SF-12© survey is widely used to help determine individual physical and mental health status. It is primarily comprised of 2 questions concerning physical functioning; 2 questions on role limitations because of physical health problems; 1 question on bodily pain; 1 question on general health perceptions; 1 question on vitality (energy/fatigue); 1 question on social functioning; 2 questions on role limitations because of emotional problems; and 2 questions on general mental health (psychological distress and psychological well-being). More information can be found at: http://www.qualitymetric.com.

[2] PCN Self-Health Evaluation Quarterly Report One (July-September 2002) and PCN Self-Health Evaluation Quarterly Report Two (July-December 2002), Utah Department of Health, Office of Health Care Statistics, 2003 <http://www.health.utah.gov/hda>.

necessary forms and the $50 annual fee, a PCN caseworker reviews the re-enrollment application and then determines eligibility for the coming year.

According to numbers obtained from PCN enrollment files and PCN quarterly reports submitted to the Centers of Medicare and Medicaid Services, 1,709 of 6,275 (27.2%) PCN enrollees left the program during July-September 2003.

Reasons for disenrollment from PCN include:

Reason	Percentage
Other/Miscellaneous	23%
Transferred to another Medicaid Program	17%
Enrolled in another insurance plan	17%
Moved out of state	9%
Failure to provide requested information	7%
Has access to other insurance	6%
Did not complete renewal	6%
Unable to locate	5%
Request of enrollee	4%
Income exceeds allowable limit	3%
Moved to a Public institution	2%
Death	1%
Not a Utah resident	1%

Source: PCN quarterly report to the Centers for Medicare and Medicaid Services, September 2003.

The Division of Workforce Services (DWS) provided the OHCS with a list of all 1,131 members who did not renew in July or August of 2003. From this population, we eliminated people who did not have a valid address (n=110) and those who were currently on Medicaid or were otherwise not eligible for the disenrollment survey (n=125).

Survey instruments, along with a cover letter and free gel pen, were mailed to all 896 disenrollees eligible for this study. The first wave of surveys was mailed on November 12, 2003. This was followed by a reminder postcard, a second survey, a second reminder, and finally a third survey. The last wave of mailings took place in early December 10, 2003.

A total of 544 surveys were returned by the cut-off date for a 61% response rate. Nearly all of the data entry was performed by Utah Correctional Industries (UCI), with the OHCS staff doing data entry on some surveys that arrived later. The enrollment status of each respondent was verified with Medicaid's eligibility files. 517 respondents had not re-enrolled by the cut-off date and were included in the following analysis.

Demographic Distribution of PCN Disenrollees

Characteristics	Sampling Frame	Valid Respondents
Number of persons	896	517
Gender		
Female	55%	60%
Male	45%	40%
Age		
19-44	76%	76%
45-64	24%	24%
Location		
Urban	63%	63%
Rural	37%	37%
Race/Ethnicity		
White / Non-Hispanic	88%	89%
Hispanic	6%	5%
Black / Non-Hispanic	2%	3%
Asian / Pacific Islander	2%	1%
American Indian	1%	1%
Other / Unknown	1%	1%

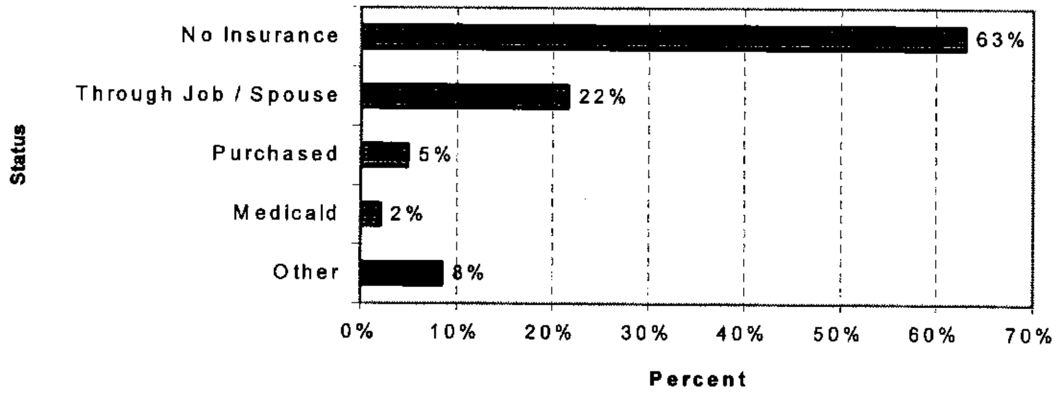
Results

The main purpose of the analysis was to gain a better understanding of why respondents chose not to renew their membership in the PCN. As stated above, the purpose of the PCN program was to provide low-income, uninsured adults with access to preventive and primary care. It was expected that some members would move on to full-time employment and then be eligible for commercial insurance. These would be members well served by the PCN program. However, PCN staff was also interested in examining what problems, if any, members had in accessing services in the PCN so that these problems could be addressed in the future.

Other Insurance Coverage

As shown in Figure 1, the majority of respondents (63%; n = 283) indicated that they had not obtained insurance from another source since disenrolling from the PCN. Of those who reported that they have obtained health coverage, 58% (n = 97) indicated that they now had coverage through their job. Another 23% (n = 38) of the respondents marked "other." The remainder was divided between those who purchased their own insurance (13%) and those on Medicaid (5%).

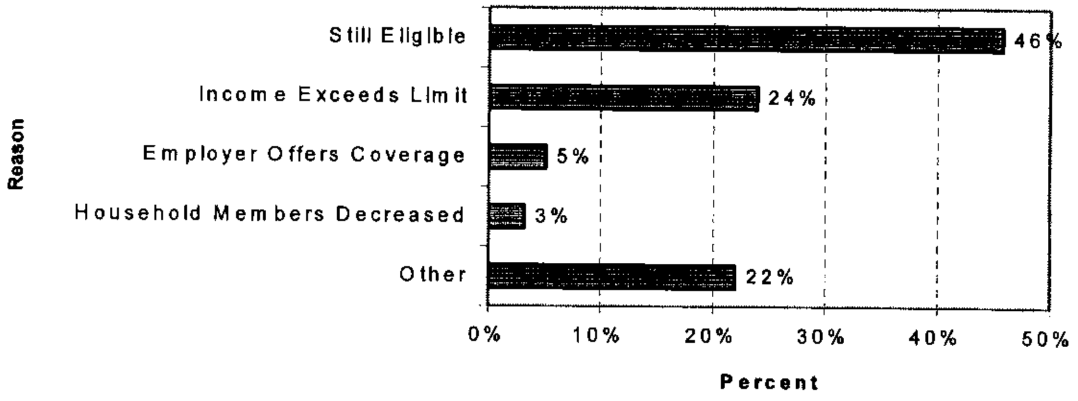
Figure 1. Current Insurance Status of PCN Disenrollees



PCN Eligibility

Respondents were asked whether they believe, or were told, that they no longer qualify for the PCN. Nearly half of PCN disenrollees indicate that they believe they are still eligible for the program (see Figure 2). Of those who indicated they were not eligible, the most common reason (n = 117, or 52% of those no longer eligible) cited was that the respondent now exceeds the income requirements. "Other" was the next most frequent category (n = 108, or 48% of ineligibles). Respondents were asked to specify the "other" reason they were no longer eligible. Of those who wrote in an answer, 23 indicated that they were full-time students. Finally, 11% indicated that they don't meet eligibility requirements because their employer offers health insurance. Note that respondents were instructed to mark all of the categories that applied to them, so some respondents may be represented in more than one category.

Figure 2. Reasons for PCN Ineligibility



Financial Barriers

Respondents were asked to indicate whether finances were the reason they did not re-enroll in the PCN. Only 29% of the respondents indicated financial barriers to re-enrollment (see Figure 3). The majority (n = 82 or 63%) of those who indicated financial barriers to their re-enrollment indicated that they could not afford the $50 re-enrollment fee. 26% indicated an inability to pay the co-pays (with 25 respondents marking both options).

A follow-up analysis shows that 78% of those who indicated a financial barrier to re-enrollment did not currently have health insurance. These respondents represent a possible burden of uncompensated care in the future since they have no access to primary or preventive care.

Meeting Health Needs

Respondents were also asked whether they did not re-enroll in the PCN because it failed to meet their health care needs. 26% of respondents (n=130) gave a "yes" response to this question. Of these respondents 62% indicated that PCN did not cover services that they needed (see Figure 3). The next most common reason cited was that the respondent was healthy and didn't use the services (22%) followed by an inability to pay co-pays (20%), and an inability to find a provider (16%). Note that respondents could mark more than one option and 14% of respondents did not mark any of the options.

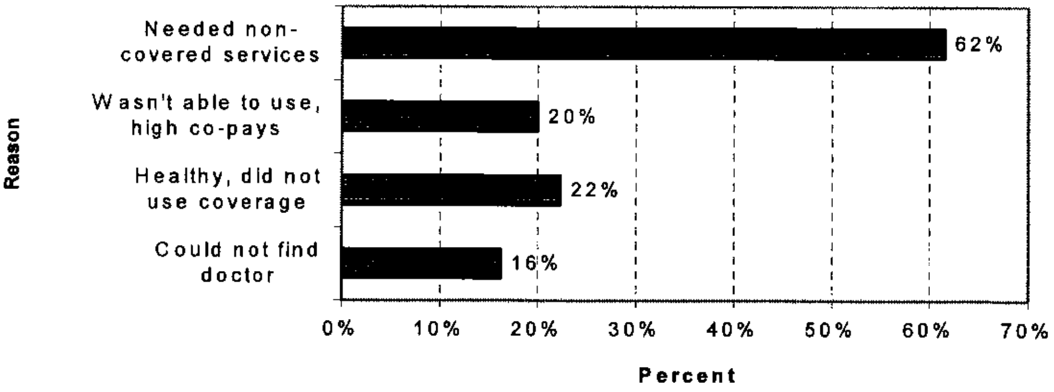


Figure 3. Ways in Which PCN Did Not Meet Respondents' Health Care Needs (n = 130)

Needed Care

Respondents were asked about the types of health care services that they needed in the past six months. The majority of respondents (65%) indicated that they needed medical care in the past 6 months (see Figure 4). The next most frequent category of need care was prescription medication, with 57% of respondents. 56% of the respondents indicated that they needed dental care in the past 6 months.

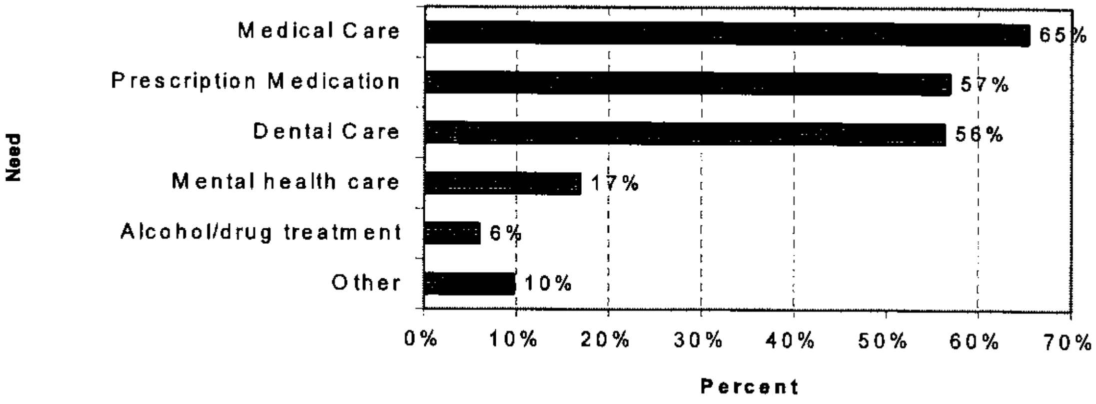


Figure 4. Reported Health Care Needs in the Past 6 Months

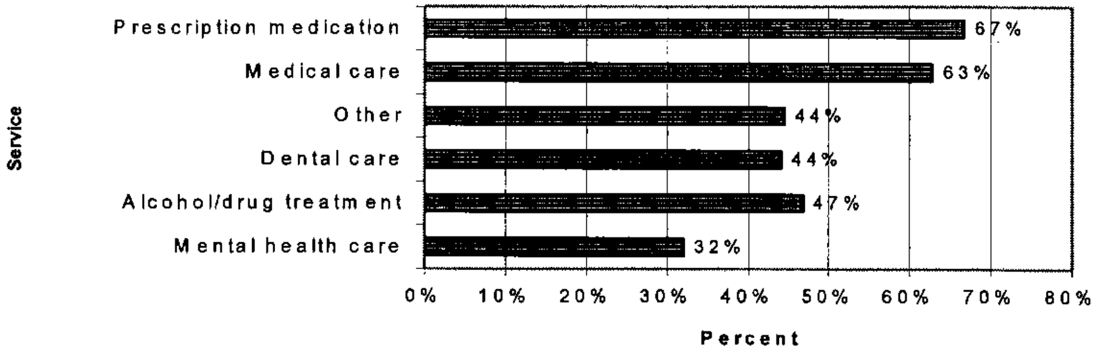
In a separate question, respondents were asked if they needed care "right away from a doctor's office, clinic or emergency room". 36% of respondents (n = 161) indicated that they needed this type of care at least once in the past 6 months.

Barriers to Care

Respondents who reported that they needed a specific type of care were then asked whether they were able to get that care. Figure 6 shows that many PCN disenrollees report difficulty in getting needed health care services. (In the calculation of each percentage, the denominator is the number of respondents who indicated that they needed that type of care). Thirty-two percent of those who needed mental health care reported that they were able to get it. A similar pattern is shown for alcohol/drug treatment, and dental services. Two-thirds (66.7%) of those who needed prescription medication and nearly two-thirds (62.8%) of those who needed medical care reported that they were able to get it.

Note: The PCN provides only limited coverage for many of these services. For example, there is a four prescription limit for pharmacy, and specialist medical care is not covered.

Figure 5. Percentage of Respondents Able to Get Needed Health Care Services in the Last 6 Months



Several questions on the PCN disenrollment survey were taken from NCQA's CAHPS®[3] instrument. One of these questions assesses how much problem the respondent had in getting the care that he or she needed. Nearly half (49%) of the respondents indicated that getting care was "not a problem" (see Figure 7). However, the remaining respondents indicated that getting needed care was either a "small problem" (22%) or a "big problem" (29%).

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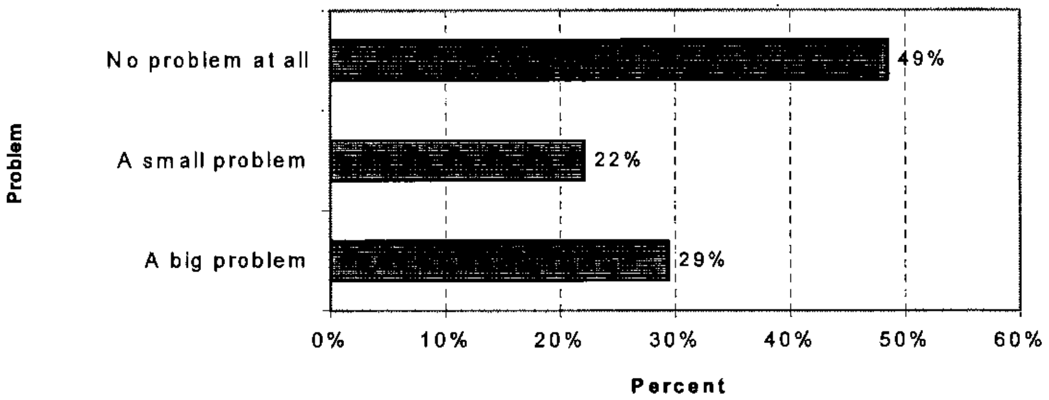
[3] CAHPS® is a registered trademark of the National Committee for Quality Assurance (NCQA). CAHPS® is an easy-to-use kit of survey and report tools that provides reliable and valid information to help consumers and purchasers assess and choose among health plans. Since 1996, the Utah Department of Health has conducted a CAHPS® satisfaction survey of Utah HMO enrollees. More information can be found at http://www.cahps-sun.org.

Figure 6. Percentage of Respondents Reporting Problems in Getting Needed Care

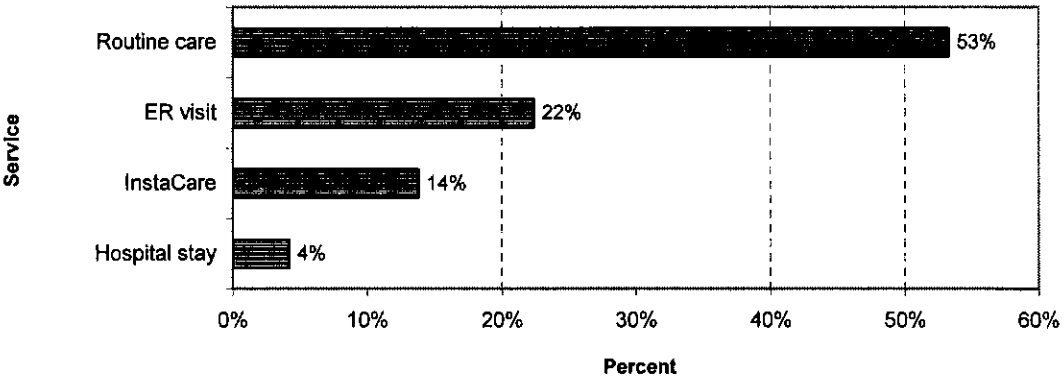


Use of Services

76% of respondents (n=392) reported needing one or more health care services in the past six months. Figure 5 shows the percentage of respondents reporting at least one visit to four different types of health care providers. 53% of the respondents reported visiting a health care provider for routine care in the last 6 months. Of these, 42% reported 1 visit, 21% reported 2 visits, and 38% reported 3 or more visits to a health care provider in the last 6 months.

Emergency department utilization was the next most common type of health care visit with 22% (n = 100) reporting at least one visit to the ER in the last 6 months. An additional 62 respondents (14%) reported at least one visit to an InstaCare clinic. Finally, 19 respondents (4%) reported an overnight hospital stay (not including maternity care).

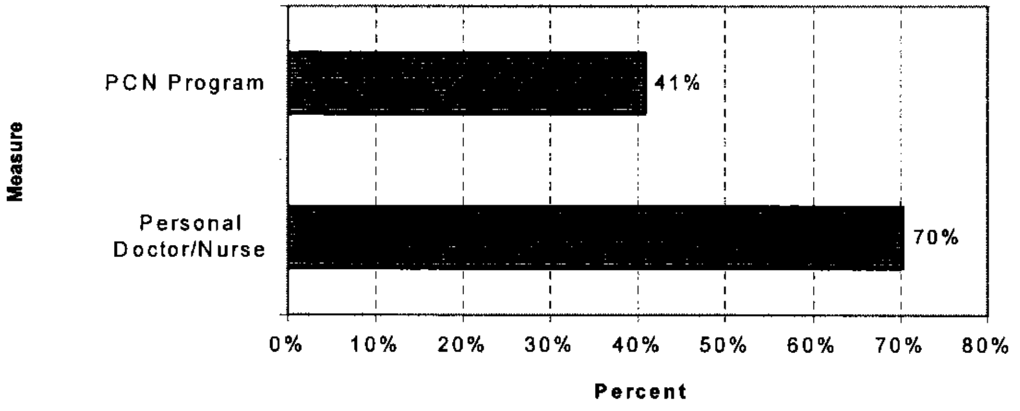
Figure 7. Percentage of Respondents Reporting Uses of Health Care Services in the Past 6 Months



Satisfaction With Care

Two questions assessed the respondents overall satisfaction with their personal doctor or nurse and with the PCN program in general. On both of these questions, the respondent gave a rating from 0 to 10 with 0 being “the worst possible” and 10 being “the best possible”. Figure 8 shows the percentage of respondents who rated either their personal doctor/nurse or the PCN program as an 8, 9 or a 10. A far greater percentage of disenrollees gave these high ratings to their doctor/nurse than to the PCN program. Both ratings are lower than those seen in the comparable satisfaction surveys for the Utah Medicaid program in 2003.

Figure 8. Percentage of Respondents Giving an 8, 9 or 10 Rating to Their Personal Doctor/Nurse or the PCN Program



Major Findings and Conclusions

The purpose of the present study was to gain information about members who chose not to renew their membership in the PCN after one year of enrollment. Respondents were asked about their reasons for disenrollment, their utilization of health care, and their overall satisfaction with the program. Major findings from this study are as follows:

- Over one-third of PCN disenrollees now have health insurance through another source.
- Nearly half of disenrollees report they are still eligible for the PCN program
- Most PCN members are able to get access to services covered by the plan (routine care and prescription medication)
- More than half of the respondents had seen a doctor for routine care in the last 6 months
- Nearly half report “no problem” in getting needed care
- Satisfaction with personal doctor is relatively high, while satisfaction with the PCN program is relatively low

Based on these findings, we have some recommendations for further study and program activities:

- Additional education of members about covered vs. non-covered services. *A major issue found in the survey is that respondents are dissatisfied that needed specialist services are not covered. It is possible that members were not completely aware of the restrictions in the program or of efforts to provide specialist services through other sources.*

- Continued education of members about the importance of primary care. *Nearly half of the enrollees did not have a primary care visit in the last 6 months.*

- Continued education of members regarding health care access options. *Emergency room care is the second most utilized health care.*

Utah Department of Health

Primary Care Network (PCN) Survey

For candidates who did not renew their membership for Year 2

11/14/03

The PCN program would like your help. Our records show that you were enrolled in the Primary Care Network for the past year but did not renew your membership for this year. We're trying to understand the reasons why people do not re-enroll in this program. We also want to ask some questions about your health and the health care you have received after being with the PCN for one year. This survey will help us to improve our services. ***We will not give your personal information or answers to anyone.*** Any information that would let someone identify you or your family will be kept private. You may choose to answer this survey or not. Your participation is completely voluntary.

SURVEY INSTRUCTIONS:

- Answer each of the questions by ***completely filling*** in the circle to the left of your answer.
  - ● Yes
  - ○ No
- You are sometimes told to skip over some questions in this survey. When this happens you will see an arrow with a note that tells you what question to answer next, like this:
  - ○ Yes ➔ Go to Question 3
  - ○ No

START HERE ⬇

1. When your first year of membership in the PCN was ending, we sent you a renewal notice. At this time, we have not received your enrollment fee for a second year of coverage. Is this correct?

○ Yes ➔ Go to Question 3
○ No ➔ Go to Question 2

2. If this is not correct, which of the following describes your situation?

○ I did not receive a renewal notice, but I am going to apply for renewal
○ I received the renewal notice and I am going to reapply
○ I have been approved for the 2nd year but have not sent the $50 enrollment fee yet
○ I have already renewed my membership in the PCN and paid the $50 enrollment fee
○ Other ______________________________

> *If you answered **NO** to Question 1 and provided an answer to Question 2, you do not need to complete this questionnaire. Please go to the end of survey and follow the instruction for mailing it back to us (page 4).*

3. Have you obtained health coverage from another source?

○ Yes ➔ Go to Question 4
○ No ➔ Go to Question 5

4. Which of the following is your new source of health coverage?

○ I now have health coverage through my job
○ I now have Medicaid
○ I have turned 65 and now have Medicare
○ I purchased my own health coverage
○ Other ______________________________

5. Do you believe (or has someone told you) you are no longer eligible for PCN?

○ Yes ➔ Go to Question 6
○ No ➔ Go to Question 7

6. What is the reason you no longer qualify for PCN? Mark all that apply.

○ My household income now exceeds the PCN eligibility limits
○ My employer offers health insurance (although I haven't signed up for it)
○ The number of individuals in our household has decreased
○ Other ______________________________

7. Did you decide not to re-enroll in the PCN for financial reasons?

○ Yes ➔ Go to Question 8
○ No ➔ Go to Question 9

8. Which of the following describes your financial situation? (related to not re-enrolling in PCN). Mark all that apply.

- ○ I cannot afford the $50 enrollment fee
- ○ I cannot afford the co-payments for doctor visits and/or prescriptions
- ○ Other ______________________

9. Did you decide not to enroll because the PCN program did not meet your health needs?

- ○ Yes ➔ Go to Question 10
- ○ No ➔ Go to Question 11

10. In what way(s) did the PCN program fail to meet your health needs? Mark all that apply.

- ○ I could not find a doctor or provider in my area who accepts PCN
- ○ I was healthy and did not use the coverage
- ○ The co-pays for doctor visits and prescriptions were too high so I wasn't really able to use the PCN when I needed it
- ○ I needed too many services that were not covered by the PCN

11. If there are other reasons you have decided not to re-enroll in the PCN, please describe them below:

____

____

____

____

12. In general, how would you rate your overall health now? Mark one.

- ○ Excellent
- ○ Very Good
- ○ Good
- ○ Fair
- ○ Poor

13. In the last 6 months have you needed any of the following kinds of health care? Mark all that apply.

- ○ Medical care
- ○ Dental care
- ○ Mental health care
- ○ Alcohol/drug treatment
- ○ Prescription medication
- ○ Other ______________________
- ○ None ➔ Go to Question 15

14. Were you able to get the health care you needed in the last 6 months? If No, what was the reason? Mark all that apply.

Type of Care	**Yes**	**No**	**If No, Why?**
Medical Care	○	○	________
Dental care	○	○	________
Mental health care	○	○	________
Alcohol/drug treatment	○	○	________
Prescription medication	○	○	________
Other	○	○	________

YOUR HEALTH CARE

Routine health care includes illness or injuries that do not require care right away, such as colds and sprains, and preventive care such as a check up, breast exam, Pap smear, prostate exam, blood pressure check, blood sugar test, etc.

15. In the last 6 months (not counting times you went to an emergency room), how many times did you go to a health provider to get ***routine care*** for yourself?

- ○ None
- ○ 1
- ○ 2
- ○ 3
- ○ 4
- ○ 5 to 9
- ○ 10 or more times

16. In the last 6 months, how much of a problem, if any, was it to ***get the care you or a doctor believed necessary***?

- ○ A big problem
- ○ A small problem
- ○ Not a problem
- ○ I did not need health care in the past 6 months

17. In the last 6 months, did you have an ***illness or injury that needed care right away*** from a doctor's office, clinic, or emergency room?

- ○ Yes ➔ Go to Question 18
- ○ No ➔ Go to Question 19

18. How often did you get this care as soon as you wanted?

- ○ Never
- ○ Sometimes
- ○ Usually
- ○ Always

19. In the last 6 months, how many times did you go to an ***emergency room*** to get care for yourself?

- ○ None ➔ Go to Question 21
- ○ 1 ➔ Go to Question 20
- ○ 2 or 3 ➔ Go to Question 20
- ○ 4 or more times ➔ Go to Question 20

20. What was the ***reason*** you went to the ***emergency room instead of somewhere else*** for health care? Mark ***all*** that apply.

- ○ I needed emergency care
- ○ Doctor's office/clinics were closed
- ○ I do not have a personal doctor
- ○ I could not afford to pay the co-pay to see a doctor
- ○ I could not get an appointment to see a doctor soon enough
- ○ I needed a prescription drug
- ○ I did not know where else to go
- ○ Other ______________________________

21. In the last 6 months, how many times did you go to an ***urgent care clinic***, such as InstaCare to get care for yourself?

- ○ None
- ○ 1
- ○ 2 or 3
- ○ 4 or more times

22. In the last 6 months, how many times did you have to ***stay over night in a hospital***? "Overnight" means you stayed in a hospital at least one night, and perhaps more than one night, because you were ill or injured. ***If you are a woman, do not include staying in the hospital while giving birth.***

- ○ None
- ○ 1
- ○ 2 or 3
- ○ 4 or more times

23. A personal doctor or nurse is the health provider who knows you best. This can be a general doctor, specialist doctor, a nurse practitioner, or a physician assistant. Do you have one person you think of as your personal doctor or nurse?

- ○ Yes ➔ Go to Question 24
- ○ No ➔ Go to Question 26

24. Using any number from 0 to 10, where 0 is the worst personal doctor or nurse possible, and 10 is the best personal doctor or nurse possible, what number would you use to rate your personal doctor or nurse?

Worst personal doctor or nurse possible

Best personal doctor or nurse possible

0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

25. Did you have the same personal doctor or nurse before you signed up for the PCN?

- ○ Yes ➔ Go to Question 27
- ○ No ➔ Go to Question 26

26. Since you signed up for the PCN, how much of a problem, if any, was it to get a personal doctor or nurse you are happy with?

- ○ A big problem
- ○ A small problem
- ○ Not a problem

Specialists** are doctors like surgeons, heart doctors, allergy doctors, skin doctors, and others who specialize in one area of health care. When you answer the next questions,* ***DO NOT INCLUDE DENTAL VISITS.

27. In the last 6 months, did you or a doctor think you ***needed to see a specialist***?

- ○ Yes ➔ Go to Question 28
- ○ No ➔ Go to Question 30

28. How much of a problem, if any, was it to get a ***referral to the specialist*** that you needed to see? (***Referral*** can be a slip of paper, verbal instruction, or an appointment.)

- ○ A big problem
- ○ A small problem
- ○ Not a problem

29. In the last 6 months, did you ***see a specialist***?

- ○ Yes
- ○ No

YOUR HEALTH

30. The next few questions ask about activities you might do during a typical day. Does ***YOUR HEALTH NOW LIMIT YOU IN MODERATE ACTIVITIES***, such as moving a table, pushing a vacuum cleaner, or bowling? Would you say you are limited a lot, a little, or not at all?

- ○ A lot
- ○ A little
- ○ Not at all

31. How about ***climbing several flights of stairs***? Would you say your health limits you a lot, a little, or not at all?

- ○ A lot
- ○ A little
- ○ Not at all

During the past 4 WEEKS**, how much of the time have you had any of the following problems with your work or regular daily activities* ***AS A RESULT OF YOUR PHYSICAL HEALTH?

32. How much of the time have you ***accomplished less*** than you would like?

- ○ All of the time
- ○ Most of the time
- ○ Some of the time
- ○ A little of the time
- ○ None of the time

33. How much of the time were you limited in ***the kind*** of work or other activities you could do?

- ○ All of the time
- ○ Most of the time
- ○ Some of the time
- ○ A little of the time
- ○ None of the time

***During the past 4 WEEKS**, how much of the time have you had any of the following problems with your work or other daily activities **AS A RESULT OF ANY EMOTIONAL PROBLEMS**, such as feeling depressed, or anxious?*

34. How much of the time have you ***accomplished less*** than you would like?
    - ○ All of the time
    - ○ Most of the time
    - ○ Some of the time
    - ○ A little of the time
    - ○ None of the time

35. How much of the time did you have trouble doing work or other activities as ***CAREFULLY*** as usual?
    - ○ All of the time
    - ○ Most of the time
    - ○ Some of the time
    - ○ A little of the time
    - ○ None of the time

36. During the past 4 weeks, how much did ***PAIN*** interfere with your normal work, including both work outside the home and housework?
    - ○ Not at all
    - ○ A little bit
    - ○ Moderately
    - ○ Quite a bit
    - ○ Extremely

*The next four questions ask about how you feel and how things have been with you **during the past 4 WEEKS**.*

37. How much of the time during the past 4 weeks have you ***FELT CALM AND PEACEFUL?***
    - ○ All of the time
    - ○ Most of the time
    - ○ Some of the time
    - ○ A little of the time
    - ○ None of the time

38. How much of the time during the past 4 weeks did you ***have a lot of energy***?
    - ○ All of the time
    - ○ Most of the time
    - ○ Some of the time
    - ○ A little of the time
    - ○ None of the time

39. How much of the time during the past 4 weeks have you ***felt downhearted and depressed***?
    - ○ All of the time
    - ○ Most of the time
    - ○ Some of the time
    - ○ A little of the time
    - ○ None of the time

40. During the past 4 weeks, how much of the time have your ***physical health or emotional problems*** interfered with your social activities, like visiting with friends, relatives, etc.?
    - ○ All of the time
    - ○ Most of the time
    - ○ Some of the time
    - ○ A little of the time
    - ○ None of the time

41. Do you now smoke or use chew tobacco every day, some days or not at all?
    - ○ Every day
    - ○ Some days
    - ○ Not at all

42. What is the highest year or grade of education you have completed?
    - ○ Some high school or less
    - ○ High school graduate/GED
    - ○ Some college or vocational school
    - ○ Tech./Vocational school grad/Assoc. degree
    - ○ College graduate (4-year college degree)
    - ○ Some postgraduate courses
    - ○ Postgraduate/Professional degree

About PCN Program

43. Using any number from 0 to 10, where 0 is the worst program possible and 10 is the best program possible, what number would you use to rate the PCN Program?

Worst Program Possible — ***Best*** Program Possible

0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

End of Survey:

Please use the postage paid return envelope to mail in your survey. Keep the gel pen!
We will send your prepaid 60-minute phone card within 2 weeks.

THANK YOU!

If you have any questions about this health assessment, please call the Office of Health Care Statistics at (801) 550-7182 or send an email to ***mikemartin@utah.gov.***

For enrollment or general information about the Primary Care Network, please call ***1-888-222-2542***

Genevieve Kenney
R. Andrew Allison
Julia F. Costich
James Marton
Joshua McFeeters

Effects of Premium Increases on Enrollment in SCHIP: Findings from Three States

This study examines the effects of new and higher premiums on SCHIP enrollment in Kansas, Kentucky, and New Hampshire—three states that implemented premium changes in 2003. We used state administrative enrollment records from 2001 to 2004–2005 to track changes in total caseloads, new enrollments, and disenrollment timing in premium-paying categories of SCHIP before and after the premium changes were implemented. Premium hikes were associated with lower caseloads in all three states and with earlier disenrollment in Kentucky and New Hampshire. Premium increases appeared to have greater disenrollment effects for lower-income children in New Hampshire and for nonwhite children in Kentucky.

The State Children's Health Insurance Program (SCHIP) was created under the Balanced Budget Act of 1997 to provide health insurance coverage to uninsured low-income children whose family incomes were too high to qualify for Medicaid. States have latitude over numerous design aspects of their SCHIP programs, including whether to expand their existing Medicaid program, use a non-Medicaid program, or create some combination of the two approaches. Over two-thirds of the states (35 in all) expanded coverage with a separate non-Medicaid program or a combination program. Under the cost-sharing provisions governing separate SCHIP programs, out-of-pocket spending for premiums and copayments may not exceed 5% of family income for children whose family incomes are above 150% of the federal poverty level (FPL). Such spending is limited to substantially lower proportions of family income for children whose family incomes are below 150% of the federal poverty level. In addition, cost-sharing amounts (either absolute dollar amounts or proportions of household income) may not be greater for lower-income families than for higher-income families. Premiums are still rare in Medicaid programs for children, limited to states that have waivers from the Centers for Medicare and Medicaid Services (CMS).

In response to budget pressures brought on by decreases in state tax revenues and increasing enrollment levels in the early 2000s, a growing number of states introduced or increased premiums for public coverage, primarily under SCHIP. Between 2002 and 2004, 16 states raised premium

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levels on children's coverage. As of 2004, over three-quarters of the 35 states with separate SCHIP programs charged premiums, and five states charged premiums under Medicaid waivers for some eligibility groups. While some premium increases in SCHIP have been driven by the rising costs of providing care, other states have looked to premiums as a tool for constraining public outlays (Hill, Courtot, and Sullivan 2005; Fox and Limb 2004).

There is a growing empirical literature that aims to document the extent to which premiums affect enrollment in public programs.[1] A recent report provides a synthesis of findings with respect to the effects of cost-sharing in public programs (Artiga and O'Malley 2005). The study concludes that new or increased premiums in public insurance coverage can be barriers to enrollment and may increase disenrollment substantially in the months after implementation.

Most of the studies to date have found decreased caseloads and increased disenrollment rates following premium hikes and some evidence attributing higher disenrollment to premium nonpayment. However, no prior study has controlled for other state-specific factors that could confound the estimated effects of the premium increase. For example, other research indicates that changes in the unemployment rate affect enrollment in public programs (Cawley and Simon 2005). Thus, changes in the underlying economy may affect access to employer-sponsored coverage and enrollment in public programs.

In this paper, we examine the effects of premium changes in Kansas, Kentucky, and New Hampshire, three states that implemented higher premiums in 2003 for some or all of the children enrolled in their separate SCHIP programs. Premium levels were increased in Kansas and New Hampshire, while premiums were imposed for the first time in Kentucky.[2] We chose to study these three states to take advantage of differences in their program structures (such as eligibility limits), their federal SCHIP matching rates (ranging from 65% to 79%), and their premium changes (such as the magnitude of the changes). In addition, these three states represent different regions of the country and have different racial and ethnic enrollee mixes.

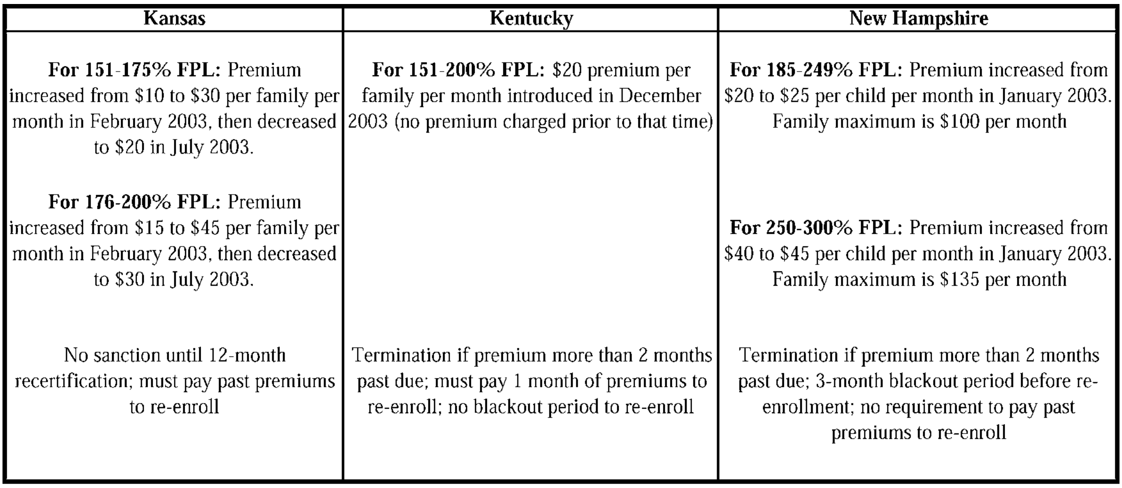
Gaining a better understanding of the impacts of public premiums is important in light of a recent National Governors Association (NGA) proposal to give states more latitude over cost-sharing in public programs (NGA 2005). Moreover, this information could help states guide their SCHIP programs as they search for ways to maintain program enrollment in the face of financing strains.

Because changes in premiums can affect both the number of children who enter the program and how long they remain enrolled, we examine the extent to which premium increases affect enrollment in, and disenrollment from, premium-paying SCHIP coverage. We first examine caseload changes following the premium change in the premium-paying categories, both descriptively and in a multivariate context. We also examine the impact of premium changes on new enrollments using multivariate methods. We then use duration analysis to assess the extent to which disenrollment timing seems to have been affected by the imposition of higher premiums. The following sections of the paper provide background information on the premium policies in the three states, a discussion of data and methods, findings, and policy implications.

Background

Figure 1 provides background information on the three states in the study. Premiums were charged as part of the SCHIP programs in Kansas and New Hampshire from the outset; while they also were included in Kentucky's enabling legislation, they were not implemented until December 2003. The number of enrolled children in the premium-paying category is small relative to the total number of children enrolled in either Medicaid or SCHIP in each state. As of July 2003, caseloads in the premium-paying categories in Kansas and New Hampshire were 10,524 and 5,818, respectively. In Kentucky, the caseload in July 2003 was 19,625 in the 151% to 200% FPL group (which was charged a premium beginning in December 2003). The premium-paying enrollment constituted 5.8%, 5.1%, and 9% of total pubilc program enrollment among children in Kansas, Kentucky, and New Hampshire, respectively.

In Kansas, the separate SCHIP program has two premium-paying categories: one covers children with family incomes between 151% to 175% of the FPL and the other covers children with family incomes between 176% and 200%



Kansas	Kentucky	New Hampshire
For 151-175% FPL: Premium increased from $10 to $30 per family per month in February 2003, then decreased to $20 in July 2003.	**For 151-200% FPL:** $20 premium per family per month introduced in December 2003 (no premium charged prior to that time)	**For 185-249% FPL:** Premium increased from $20 to $25 per child per month in January 2003. Family maximum is $100 per month
For 176-200% FPL: Premium increased from $15 to $45 per family per month in February 2003, then decreased to $30 in July 2003.		**For 250-300% FPL:** Premium increased from $40 to $45 per child per month in January 2003. Family maximum is $135 per month
No sanction until 12-month recertification; must pay past premiums to re-enroll	Termination if premium more than 2 months past due; must pay 1 month of premiums to re-enroll; no blackout period to re-enroll	Termination if premium more than 2 months past due; 3-month blackout period before re-enrollment; no requirement to pay past premiums to re-enroll

Figure 1. Premium policies in three states, 2003 (Information from the figure is drawn from multiple sources: Kansas – Smith and Rousseau 2003, Kansas Family Medical Assistance Manual 2005; Kentucky – Kentucky Children's Health Insurance Program Web site 2005; New Hampshire – New Hampshire Healthy Kids Corporation Web site, 2005, Smith and Rousseau 2003, T. Brooks, personal communication 2005.)

of the FPL (some children with family incomes between 100% and 150% of the FPL also are covered under SCHIP, with no premiums).[3] The federal SCHIP match rate for Kansas was between 72% and 73% during the study period. Monthly premiums had been $10 per family for children with incomes between 151% and 175% of the FPL and $15 per family for children with incomes between 176% and 200% of the FPL. They were increased to $30 and $45, respectively, per family per month, in February 2003. Premiums subsequently were decreased to $20 and $30, respectively, per family per month in July 2003.

The decision in the fall of 2002 to increase premiums in Kansas came at the fiscal low point of a recession and was part of a multi-agency package of budget reductions. The premium increases were partially reversed just five months later by a new governor after the state fiscal situation began to improve. There was little publicity about either of the premium changes and premium-paying families were notified of both changes only through their normal monthly bill. Nonpayment of premiums does not result in termination of enrollment in Kansas until recertification, which occurs at 12-month intervals after initial enrollment. Children whose enrollment is terminated due to nonpayment must pay all past due premiums to be recertified and re-enroll, but there is no enrollment blackout period. Because premium nonpayment does not lead to disenrollment until recertification, and because premiums were tripled for just a five-month period, the higher premium may have had a more diffuse disenrollment effect in Kansas than in the other two states.

Like Kansas, Kentucky covers SCHIP-eligible children with family incomes between 101% and 150% of the federal poverty level through a Medicaid expansion without any premiums.[4] The federal SCHIP match rate for Kentucky was 79% during the study period. In December 2003, Kentucky introduced a premium of $20 per month per family for children covered by a separate SCHIP program and with family incomes between 151% and 200% of the FPL. The premium was part of the SCHIP implementing legislation, but it had been interpreted as optional rather than mandatory until a combination of legislative pressure and unexpectedly high enrollment prompted reconsideration of the issue. In Kentucky, enrollees have an eligibility period of 12 months, but children are terminated from the program if premiums are more than two months past due and re-enrollment requires payment of at least one past-due premium. There is no blackout period before a child is eligible to re-enroll.

In New Hampshire, the Medicaid program

has higher eligibility levels than in Kansas or Kentucky, covering children up to age 18 with family incomes reaching 185% of the FPL. A SCHIP Medicaid expansion covers infants with family incomes between 185% and 300% of the FPL with no premium, and a separate SCHIP program charges premiums for children ages 1 through 18 whose family incomes are between 185% and 300% of the FPL. The federal SCHIP match rate in New Hampshire was 65% at the time of this study. Premiums were increased by $5 per month per child in these two eligibility categories in January 2003, from $20 to $25 (for the 185% to 250% FPL group) and from $40 to $45 (for the 251% to 300% FPL group). The premium increases in New Hampshire, the first since the inception of SCHIP, were implemented due to rising program costs and the fact that premiums had been declining in real terms over the years.

New Hampshire imposes premiums at the child level, in contrast with the family-level premiums used in Kansas and Kentucky. New Hampshire's monthly premiums are capped at $100 and $135 per family per month for the lower- and the higher-income groups, respectively. Children have a 12-month eligibility period, but enrollment is terminated if premiums are more than two months past due. There is no requirement to pay past-due premiums to re-enroll in New Hampshire, but there is a three-month lockout period. If a recipient's family has a legitimate reason for not paying premiums, a charity program can cover most of the cost of the premiums for up to three months. However, this assistance program is small because charitable funds for this purpose are limited and relatively few families ask for help.[5]

All three states had recertification points at 12-month intervals, but the definition of the starting point varied across the three states. In Kansas and Kentucky, recertification occurs approximately 12 months after the initial enrollment date, whereas in New Hampshire it occurs 12 months after the application date (which can be as long as two months before the enrollment date).

In 2004, Kentucky collected a total of $2.26 million in SCHIP premiums and spent an estimated $446,000 on administrative costs collecting the premiums; Kansas collected a total of $1.36 million in premiums in 2004 and spent approximately $270,000 on administrative costs. New Hampshire collected a total of $2.31 million in premiums in 2004 and spent an estimated $140,000 to $150,000 for premium collection. The premiums collected and the administrative costs reported here include both the state and federal shares. Taking the total premiums collected net the administrative costs associated with collecting premiums, suggests that premiums accounted for 2.5% of total state and federal SCHIP outlays in Kansas, 2.8% in Kentucky, and 19.8% in New Hampshire.

Data and Methods

The analysis draws primarily on state administrative records on individual enrollment from 2001 to 2004–2005 for the premium-paying categories of the separate SCHIP programs in Kansas, Kentucky, and New Hampshire.[6] We have not included data from the start-up years of each SCHIP program since they may not be indicative of enrollment and disenrollment patterns at a more mature point in the program's history. The study population was defined as: enrollees ages 1 through 18 with family incomes between 151% and 200% of poverty in Kansas and Kentucky; enrollees under 1 with family incomes between 151% and 200% of poverty in Kansas and family incomes between 186% and 200% of poverty in Kentucky; and enrollees ages 1 through 18 in New Hampshire with family incomes between 185% and 300% of poverty.

Individual monthly enrollment files were combined to create a single data set for each state covering the entire study period. Since changes in caseloads are determined by changes in new enrollments and by changes in disenrollments, we analyzed total caseloads and its component parts separately. We used time-series methods to examine changes in caseloads and new enrollments and duration analysis to examine changes in disenrollment rates.

Where possible, comparable measures were defined in each state. Children who were enrolled at any point in a given month were considered current enrollees (which in nearly all instances meant that they were enrolled as of the first day of the month); new enrollees were defined as those who were in a premium-paying category in a given month, but who had not been enrolled in a premium-paying category the prior month; and disenrollees were defined as children enrolled in premium-paying SCHIP for at least one month, but not enrolled in premium-paying SCHIP the subsequent month.[7]

Over the study period, a small percentage of disenrollees from the premium-paying category were subsequently re-enrolled in a premium-paying category after a one-month gap in coverage: .8% of disenrollees in Kansas, 1.8% of disenrollees in New Hampshire, and 3.1% in Kentucky. The implications of brief enrollments for children, families, and SCHIP programs vary significantly by state and by the length of the disenrollment. For the broadest indication of the potential impact of the premium changes, our analysis included one-month gaps as disenrollments. While using a one-month gap may increase the counts of disenrollees and new enrollees on a monthly basis, it is unlikely that the estimated effects of the premium changes will be affected given the small share of disenrollees who re-enroll after one month.

The administrative files contain limited demographic information on the children enrolled in the premium-paying categories. In all three states, information was provided on the age, gender, and county of residence. Kansas and Kentucky data also included information on the child's race/ethnicity. In New Hampshire, the file contained information on household size, whereas in Kentucky and Kansas we estimated a child's number of siblings by using the family's identification number to match enrolled siblings. In Kentucky, the file contained information on managed care enrollment and enrollment status (i.e., previously enrolled in Medicaid, previously enrolled in the non-premium-paying category of SCHIP, or not enrolled previously in any type of public coverage) of children prior to enrollment in the premium-paying category.

Kansas's data also included information on enrollment status and monthly records of a family's income relative to the federal poverty level, which was used to estimate premium payment status. This method of estimating premium payment status was validated through comparisons with official caseload counts and with the portion of premium payment records that could be matched at the family level. New Hampshire provided monthly family income from the initial application or latest recertification, along with the number of people in the child's household, which we used to calculate income as a proportion of the federal poverty level.[8] Monthly data on state-level unemployment rates for all three states were obtained from the Bureau of Labor Statistics.

Multivariate time-series analyses of changes in monthly caseload were performed using the following specification:

$$C(t) = a + b^{*}T + c^{*}\,T \text{ squared} + d^{*}\,\text{Premium}\,(t) + f^{*}\text{UE}\,(t) + g^{*}\,M\,(t) + u\,(t) \qquad (1)$$

where C (t) is the total caseload in the premium-paying category in month t; T is a time trend that takes the value 1 in the first month of the analysis, 2 in the second month of the analysis, etc.; T squared is the time trend squared (to allow for a nonlinear time trend); Premium (t) is a dummy variable that takes the value 1 in the months following the premium increase; UE (t) is the unemployment rate reported for month t; M is a set of monthly dummy variables; and u(t) is an error term. Monthly dummy variables were included to control for possible seasonal variation in caseloads and because of annual redefinitions in federal poverty levels that lead some children to transfer from premium-paying into non-premium-paying categories.[9] We tested for auto-correlation in the error term and, when indicated, we adjusted the estimates for auto-correlation using a Cochrane-Orcutt transformation. We also estimated multivariate models on new enrollments using the same structure as specified in equation 1 to assess the extent to which new enrollment in premium-paying categories appeared to be affected by higher premiums.

Analyses of individual-level disenrollment patterns were conducted using a Cox proportional hazards model based on cohorts of children entering the key program enrollment categories between 2001 and 2004. Other research has used a Cox proportional hazards model to measure the effects of premium changes on disenrollment patterns (Shenkman et al. 2002; Herndon et al. 2006). Our Cox proportional hazards model used time-varying covariates, with the following specification:

$$h(t) = h_0(t) \times e^{\beta_1 \text{Recert\_1}_i + \beta_2 \text{Recert\_2}_i + \beta_3 \text{Recert\_3}_i + \beta_4 \text{Post\_Premium}_i + \beta \text{X}_i} \qquad (2)$$

where $h(t)$ is the hazard rate for disenrolling from the premium-paying category; $h_0(t)$ is the baseline hazard function; β_1, β_2, and β_3 are the hazard rates associated with recertification at 12, 24, and 36 months, respectively;[10] β_4 is the hazard rate associated with the premium increase (i.e., the premium variable is a time-varying variable that takes the value 0 before the premium increase and the value 1 after the premium increase); and βX_i is a vector of hazards associated with covariates such as the child's age, the child's gender, and family size.[11] As with the time-series analysis, separate models were estimated for each state.

We examined the disenrollment experience of the cohorts of children in each of the three states who enrolled in a premium-paying SCHIP category at some point between 2001 and 2004. By relying on cohort data for our hazard model, we avoided the problem of having left-censored enrollment spells in the premium-paying category. We treated as right-censored all children in these cohorts who were still enrolled at the end of the study period for each state and those who turned 19 (i.e., who aged out of the program).

Disenrollments in states with active recertification are typically concentrated in or around the expected time of recertification. Observation of monthly exit hazards (not shown) suggests that the period of time between children's enrollment in a premium-paying category and their first recertification varied across the study states. These variations appear to be caused by differences in the process of establishing an administrative marker for the date of first recertification. As noted earlier, New Hampshire sets an administrative marker 12 months after the date an application is received; therefore, most children reach their first recertification after 10 to 11 months of being enrolled in the program. In Kansas, most children in the study cohorts have their first recertification after 12 months; in Kentucky, most children appear to reach their first recertification at around 13 or 14 months, which is driven by the experience of children who had public coverage prior to enrolling in the premium-paying category.

Findings

Caseload Changes

Figures 2 through 4 show changes in premium-paying caseloads over the study period. In all three states, caseload growth rates in the six months prior to the premium increase were consistently higher than those in the six months after the premium increase. The most dramatic change in caseload occurred in Kentucky, where the premium-paying caseload decreased by 16.4% (3,194 children) in the three months following introduction of the premium. The caseload stabilized in February 2004, but had not returned to pre-premium levels nine months after the premium was introduced. In both Kansas and New Hampshire, small declines in the caseload occurred immediately following the premium hike, but the caseload resumed growing three to five months following the premium increase, though at lower rates than observed before the increase.

The caseload changes in the premium-paying category differ substantially from those observed in the other categories of public coverage in the six-month period following the premium increase (Table 1). While caseload changes in premium-paying SCHIP ranged from an increase of 1% to a decline of 18.2% across the three states over that period, Medicaid caseloads for children increased by 2.8% to 3.3%. Enrollment in non-premium-paying SCHIP coverage in Kansas grew by 4.9% and stayed flat in Kentucky (no trend data is presented for the very small non-premium-paying SCHIP category of infants in New Hampshire). It appears that while caseloads declined or stayed flat in the premium-paying categories following the premium increases, caseloads for other categories of public coverage were rising.

Time-Series Analyses

Table 2 shows findings from time-series models on premium-paying caseloads. In all three states, a negative premium effect was found ($p < .10$, one-tailed test [Kansas]; $p < .05$, one-tailed test [Kentucky and New Hampshire]). The implied average effect of the premium was to reduce monthly caseloads by 421 children (4.1%) as of December 2004 in Kansas; by 3,262 children (18.1%) as of April 2005 in Kentucky; and by 201 children (3.7%) as of November 2004 in New Hampshire. Other specifications also were estimated, modeling the policy effect and the time trends in different ways. While the magnitude and precision of the policy effects vary under alternative specifications,[12] the pattern of findings consistently points to premium increases having a negative effect on caseloads in premium-

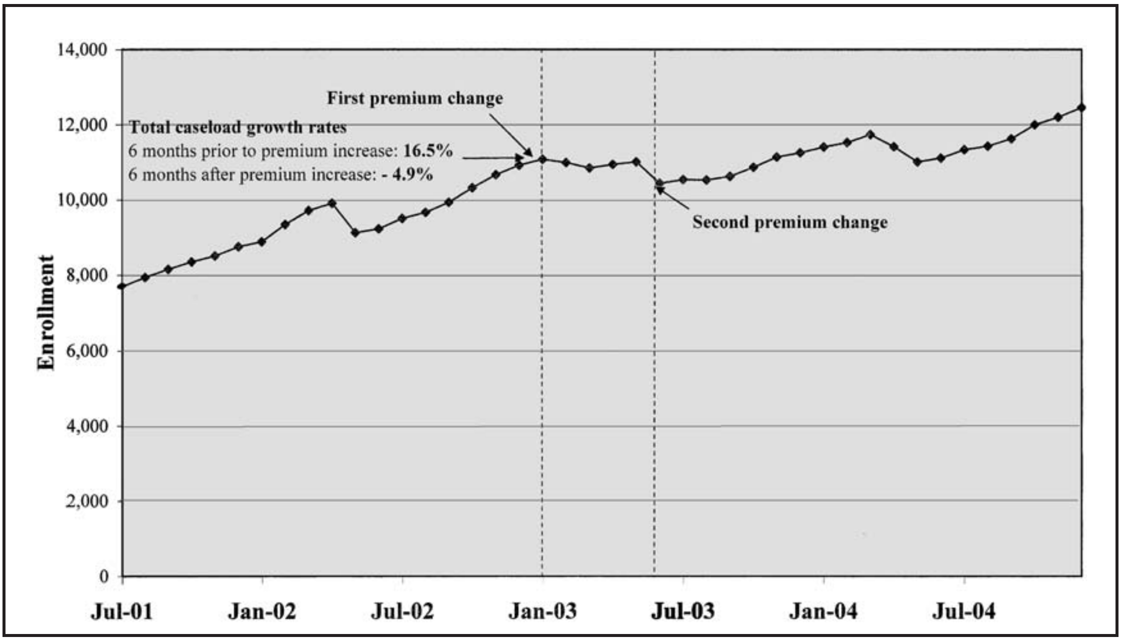


Figure 2. Changes in total caseloads in the premium-paying categories of Kansas' Healthwave XXI Program (In February 2003, premiums were tripled to \$30/month/family for children 150% to 175% FPL and \$45/month/family for children 175% to 200% FPL. In July 2003, premiums were reduced to \$20/month/family for children 150% to 175% FPL and \$30/month/family for children 175% to 200% FPL. Source: Linked monthly administrative enrollment data for Kansas from July 2001 to December 2004.)

paying categories. We found that including the unemployment rate affected the estimated impact of premiums in New Hampshire, but not in the other two states.[13]

For new enrollment in premium-paying SCHIP, a negative premium effect was found ($p < .10$, one-tailed test) for Kansas and New Hampshire, but no significant effect of the introduction of a premium was found in Kentucky. The implied average effect of the premium was to reduce new enrollment by 180 and 71 children per month, or by 10.1% and 17.7%, in Kansas and New Hampshire, respectively.

Hazard Analyses

Table 3 provides descriptive information for the hazard analyses in each state. Table 4 provides estimates from the hazard models, which indicate how the higher premium levels and other factors affected disenrollment from the premium-paying category of SCHIP. All results presented in this section are significant at a .05 level unless specified otherwise. Our findings suggest that the higher premium levels led children to disenroll earlier than they would have otherwise in both Kentucky and New Hampshire. The strongest positive effect on the disenrollment hazard was found in Kentucky, where the hazard rate was 1.3 times higher following introduction of the premium. A positive, but smaller effect that was more sensitive to model specification was found in New Hampshire, indicating that the disenrollment hazard rate was 1.1 times higher following the premium increase.[14] We may be observing a smaller disenrollment effect in New Hampshire than in Kentucky because the premium increase in New Hampshire was smaller or because the premium was applied to a higher-income group than in Kentucky.

In contrast, the results for Kansas indicate that disenrollment rates did not increase in the months following the premium increase that took place in February 2003—if anything, the hazard rate for disenrollment appeared to be lower over that period. The contrary findings for Kansas may be due to a number of factors, including the lack of sanctions for premium nonpayment until the

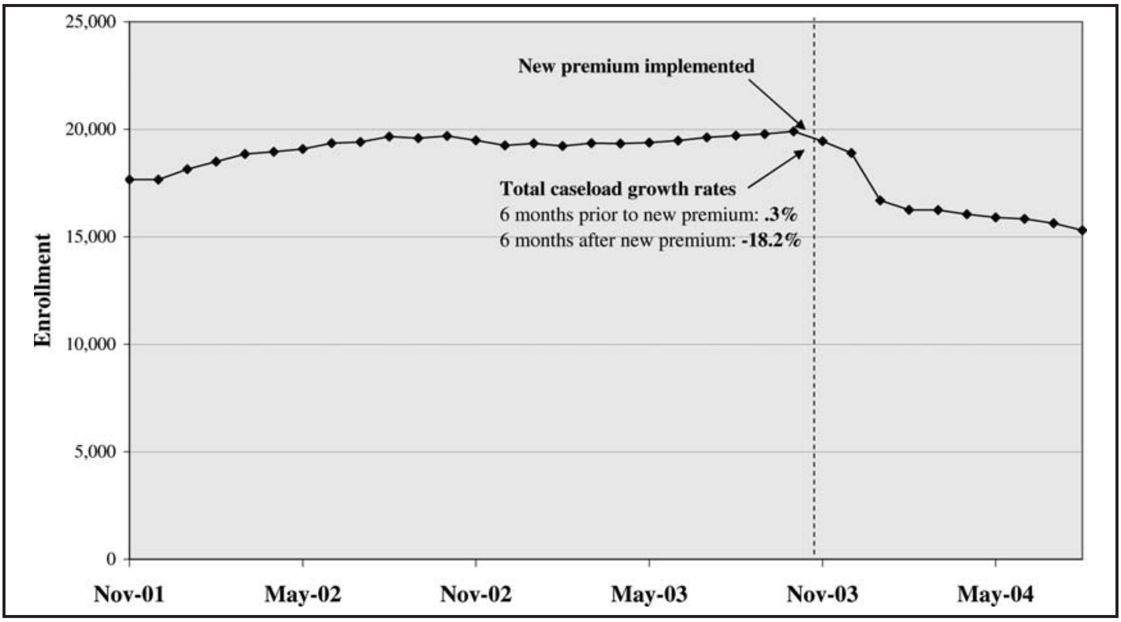


Figure 3. Changes in total caseloads in the premium-paying category of Kentucky's KCHIP program (In December 2003, a new premium of $20/family/month was implemented for children 151% to 200% FPL. Source: Linked monthly administrative enrollment data for Kentucky from November 2001 to August 2004.)

recertification point, the fact that the premium was increased and then decreased within a relatively short period of time, or unmeasured differences in the population of SCHIP eligibles or enrollees over time.

Some groups of children in Kentucky and New Hampshire appeared to be affected by the premium change more than others. In Kentucky, a model specification that included interaction terms between the policy change variable and all the other explanatory variables found a hazard rate of 1.20 on the interaction term for nonwhite children (data not shown). This result implies that disenrollment rates increased more for nonwhite children after the introduction of the premium than for white children,[15] which suggests that nonwhite children were affected more than other children by the introduction of the premium. This finding may be due to unobserved differences in characteristics, such as family income, of children in different racial subgroups.

In New Hampshire, the effect of the premium increase on the disenrollment hazard appeared to differ for children in the two different premium-paying categories (data not shown). Children in the 185% to 250% FPL group had a disenrollment hazard rate that was 1.1 times higher than the baseline hazard measured before the premium increase, while children in the 251% to 300% FPL group had a disenrollment hazard rate that was .9 times ($p < .10$) the rate before the premium increase. It appears that the increased hazard rate for disenrollment that occurred following the premium increase in New Hampshire was limited to children in the 185% to 250% FPL group.

Factors other than the premium increase also affected the disenrollment hazard rate. In all three states, both the first and second recertification points were associated with a higher disenrollment hazard rate. For example, in New Hampshire, at first recertification, the disenrollment hazard rate was 1.6 times greater than the baseline hazard, and after the second recertification the disenrollment hazard rate was 4.6 times greater. For the relatively few children who reached their third recertification, the disenrollment hazard rate was much lower (.2 times the disenrollment hazard rate). Similarly, results indicate that transferring into the premium-paying category of SCHIP from Medicaid or from the non-premium-paying category of SCHIP was associated with larger disenrollment hazards.

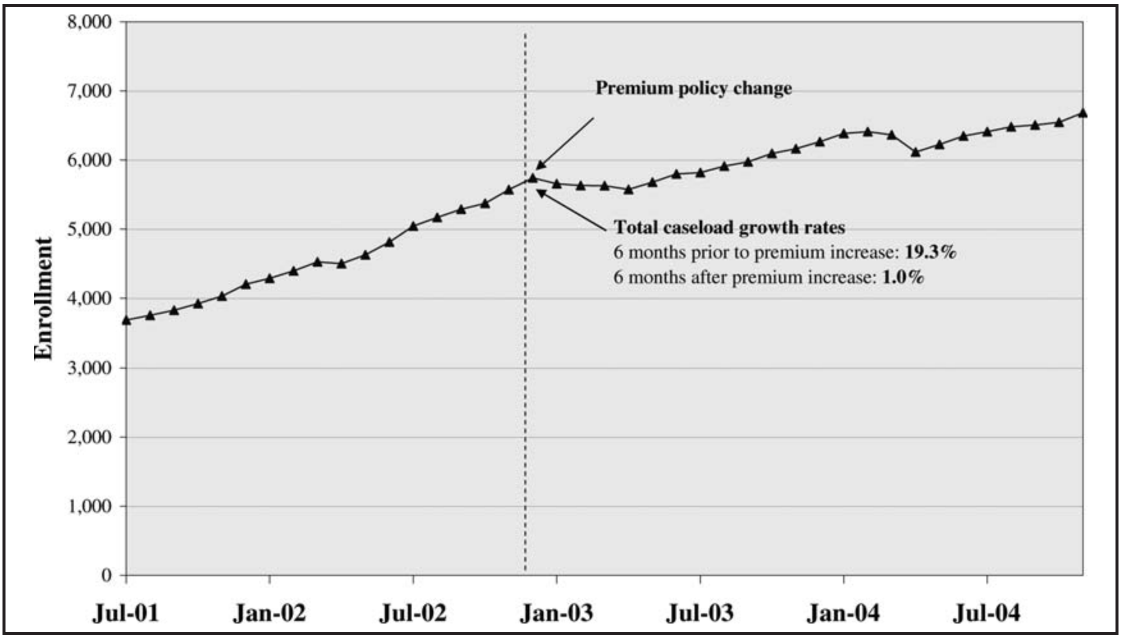


Figure 4. Changes in total caseloads in the premium-paying categories of New Hampshire's Healthy Kids Silver Program (In January 2003, the premium was increased to $25/month/child for children 185% to 249% FPL. For children 250% to 300% FPL, the premium was increased to $45/month/child. Source: Linked monthly administrative enrollment data for New Hampshire from July 2001 to November 2004.)

In Kentucky, for example, children transferring from Medicaid into premium-paying SCHIP had a disenrollment hazard rate 1.6 times greater than those without public coverage prior to enrolling in premium-paying SCHIP.

In terms of the demographic characteristics of children, gender does not seem to have had a differential impact on disenrolling in any of the three states. Other demographic characteristics had inconsistent impacts in the three study

Table 1. Caseload growth rates six months prior to and six months after the premium change in Kansas, Kentucky, and New Hampshire

		SCHIP	
	Medicaid (%)	No premium (%)	Premium-paying (%)
Kansas[a]			
6 months prior to premium change	5.74	7.01	16.55
6 months after premium change	3.06	4.94	−4.88
Kentucky			
6 months prior to premium change	2.41	3.53	.32
6 months after premium change	2.79	−.23	−18.24
New Hampshire			
6 months prior to premium change	5.26	N.A.[b]	19.33
6 months after premium change	3.32	N.A.[b]	.98

Source: Monthly administrative enrollment data from Kansas, Kentucky, and New Hampshire, 2001 to 2004–2005.

[a] Caseload growth rates are measured from the first premium change in Kansas.

[b] N.A. = not applicable.

Table 2. Time-series estimates of caseload changes for premium-paying enrollment categories in Kansas, Kentucky, and New Hampshire, 2001 to 2004–2005

	Kansas	Kentucky	New Hampshire
Time	185**	170**	160**
Time squared	−2*	−3.7**	−1.8**
After premium change	−421*	−3,262**	−201**
Unemployment rate	−151	−37	7
Average monthly premium effect	−421	−3,262	−201
Premium effect as a percent of premium-paying caseload	−4.06	−18.12	−3.69

Source: Monthly administrative enrollment data from Kansas, Kentucky, and New Hampshire, 2001 to 2004–2005.
Note: Premium variable takes the value "1" after premium was increased and is "0" otherwise. The premium effect is interpreted as the average monthly change in caseload due to the premium increase.
* Indicates significance at .10 level.
** Indicates significance at .05 level.

states. Being covered under managed care (hazard rate 1.22) increased the odds of disenrolling in Kentucky. Enrollment spans of younger children, ages 1 to 5, ended earlier than the enrollments of teenagers in Kentucky (hazard rate 1.09) and in New Hampshire (hazard rate 1.14), but lasted longer in Kansas (hazard rate .93). Income also had a negative association with the disenrollment hazard rate—in New Hampshire, it appears that children with incomes between 251% and 300% of the FPL had a substantially lower hazard rate relative to children with incomes between 185% and 250% of the FPL.

Policy Implications

Despite differences in SCHIP premium and eligibility policies in Kansas, Kentucky, and New Hampshire, the premium increases had negative effects on premium-paying caseloads in all three states based on changes in actual caseloads, caseload growth rates, and time-series analyses of caseload changes. Our analysis also suggests

Table 3. Descriptive statistics for hazard analyses

	Kansas	Kentucky	New Hampshire
Number of new enrollment spans[a]	35,939	51,649	16,422
Premium category (%)			
Kansas HealthWave high premium	34	N.A.	N.A.
Kansas HealthWave low premium	66	N.A.	N.A.
Kentucky KCHIP III	N.A.	100	N.A.
New Hampshire 185% to 250% FPL	N.A.	N.A.	80
New Hampshire 251% to 300% FPL	N.A.	N.A.	20
Age (%)			
1 to 5	41	36	34
6 to 12	37	37	40
13 to 18	22	27	26
Gender (%)			
Male	51	52	51
Female	49	48	49
Number of siblings	1.31	1.16	N.A.
Household size	N.A.	N.A.	3.79

Source: Linked monthly administrative enrollment data for Kansas, Kentucky, and New Hampshire from July 2001 to December 2004.
Note: N.A. = not applicable.
[a] An enrollment span is the period of time a child is continuously enrolled in premium-paying SCHIP. A span starts when a child enrolls in premium-paying SCHIP and ends when the child disenrolls from premium-paying SCHIP.

Table 4. Estimated coefficients from hazard analyses in Kansas, Kentucky, and New Hampshire

	Kansas[a]		Kentucky[b]		New Hampshire[c]	
Variable	Hazard ratio	*p*-value	Hazard ratio	*p*-value	Hazard ratio	*p*-value
Premium change						
After	.95	.007	1.28	.000	1.11	.000
Before	1.00	N.A.	1.00	N.A.	1.00	N.A.
Premium category						
Kansas HealthWave high premium	1.36	.000	N.A.	N.A.	N.A.	N.A.
Kansas HealthWave low premium	1.00	N.A.	N.A.	N.A.	N.A.	N.A.
Kentucky KCHIP III	N.A.	N.A.	1.00	N.A.	N.A.	N.A.
New Hampshire 185% to 250% FPL	N.A.	N.A.	N.A.	N.A.	1.02	.407
New Hampshire 251% to 300% FPL	N.A.	N.A.	N.A.	N.A.	1.00	N.A.
First recertification						
Yes	3.05	.000	2.77	.000	1.65	.000
No	1.00	N.A.	1.00	N.A.	1.00	N.A.
Second recertification						
Yes	4.17	.000	2.25	.000	4.56	.000
No	1.00	N.A.	1.00	N.A.	1.00	N.A.
Third recertification						
Yes	N.A.	N.A.	N.A.	N.A.	.19	.006
No	N.A.	N.A.	N.A.	N.A.	1.00	N.A.
Child's characteristics						
Gender						
Female	1.00	.788	1.00	.744	1.02	.315
Male	1.00	N.A.	1.00	N.A.	1.00	N.A.
Race/Ethnicity						
Nonwhite	1.00	.932	1.14	.000	N.A.	N.A.
White	1.00	N.A.	1.00	N.A.	N.A.	N.A.
Age						
1 to 5	.93	.001	1.09	.000	1.14	.000
6 to 12	.87	.000	.95	.000	.98	.328
13 to 18	1.00	N.A.	1.00	N.A.	1.00	N.A.
Household size	N.A.	N.A.	N.A.	N.A.	.99	.240
Number of siblings	.97	.000	1.07	.000	N.A.	N.A.

Source: Linked monthly administrative enrollment data for Kansas, Kentucky, and New Hampshire from July 2001 to December 2004.

Note: N.A. = not applicable.

[a] Model also includes controls for household's most recent reported income as a percentage of the FPL, region, and whether the child was enrolled in Medicaid or non-premium-paying SCHIP prior to enrollment.

[b] Model also includes controls for region, managed care enrollment, and whether the child was enrolled in Medicaid or non-premium-paying SCHIP prior to enrollment.

[c] Model also includes controls for household's most recent reported income as a percentage of the FPL and region.

that raising premiums led to reductions in new enrollment in Kansas and New Hampshire and to faster disenrollment in both Kentucky and New Hampshire. Effects on disenrollment were larger in Kentucky than in New Hampshire and no disenrollment effects were observed in Kansas. In Kentucky, larger disenrollment effects were found for nonwhite children relative to white children, while in New Hampshire disenrollment effects were concentrated among children in the lower-income eligibility group (185% to 250% of the FPL).

Our findings suggest that states that raise premium levels will experience lower caseloads in premium-paying categories in the months following a premium increase. The findings from Kentucky indicate that states introducing premiums for the first time may experience substantial disenrollment effects compared to states making small changes to their premium levels. Our research also suggests that premium increases may have greater disenrollment effects when they are applied to lower-income families. This finding is consistent with new research from Florida (Herndon et al. 2006), which has found that disenrollment rates increased more following a premium increase in Florida for children with family incomes between 100% and 149% of the FPL than for children with family incomes between 150% and 200% of the FPL. It is also consistent with findings from a national analysis of premium impacts, which indicated that public premiums had more pronounced effects on families with incomes between 100% and 200% of the FPL than on higher-income families (Kenney, Hadley, and Blavin 2006/2007).

The results from Kansas indicate that other state policies related to premium nonpayment may affect the observed impacts of premium increases. In particular, to the extent that premium nonpayment results in termination only at recertification points, premium increases may have delayed impacts on program disenrollment. The mode of premium payment also may determine responses to premium increases. Findings from focus group studies and separate findings from Rhode Island indicate that families value having multiple options for premium payment, and that more payment options, including a payroll deduction, may minimize disenrollment due to premium nonpayment (Kannel and Pernice 2005).[16]

There are a number of limitations to this analysis. While we used local unemployment rates to control for changes in the underlying economy, which could affect enrollment in public programs, we were not able to include all the factors that could affect demand for public coverage. In particular, we could not control for changes in health care premiums for employer-sponsored coverage, which were rising at high rates over the study period. In addition, we did not have accurate measures of the number of children who were eligible for coverage in the premium-paying categories and how that number was changing over the time period for analysis. Thus, there may have been other changes occurring in these states at the time the premium changes were made that confounded our ability to isolate the effects of premium increases on enrollment and disenrollment. Finally, the length of the post-premium periods varied from state to state, ranging from nine months for the disenrollment analysis and 17 months for the time-series analysis in Kentucky to 23 months in Kansas and New Hampshire for both analyses. Using a shorter time period after the policy change in Kentucky may have affected the impact estimates, although our policy estimates from the other states were not affected when we included fewer months following the policy change in our analysis.

With the exception of New Hampshire, which has higher SCHIP eligibility thresholds and higher premium levels, it appears that SCHIP premiums account for a relatively small fraction of total (i.e., federal and state) SCHIP spending in these states. The extent to which premiums generate savings depends on the state's SCHIP matching rate—premium collections will generate relatively more savings in outlays for states that have lower federal SCHIP matching rates. In addition to the state's matching rate and the administrative costs associated with collecting premiums, the effect of a premium increase on state outlays will depend on the magnitude of the premium change and the effect of the premium change on caseloads. The time-series models estimated here suggest that states will experience some reductions in premium-paying caseloads in the short run, which should lower state outlays under SCHIP.

However, estimating the savings to states resulting from premium increases also requires factoring in other potential costs. For example, premium increases or the introduction of new premiums may lead to positive spillover enrollment effects for non-premium-paying categories of SCHIP or for Medicaid (Kenney et al. 2006). For example, if a premium change or new premium increased the likelihood that families would notify the state of changes in family circumstances that make them eligible for non-premium-paying SCHIP or for Medicaid, it could end up costing a state more, especially if there were a shift from SCHIP to Medicaid (with the accompanying decline in the federal matching rate). It is also possible that new or higher SCHIP premiums could dissuade families with incomes in the Medicaid or SCHIP eligibility range from applying for coverage, which would reduce state outlays for Medicaid and SCHIP but could increase costs for uncompensated care.

To the extent that higher premiums lead to greater cycling on and off public coverage, administrative costs may be higher, which increases state costs. Likewise, to the extent that healthier enrollees are more likely than other children to disenroll in response to premium increases (Herndon et al. 2006), higher premiums could change the case mix of enrollees to include more children in fair or poor health and raise the average cost per enrollee. At the same time, however, charging premiums may make it easier for states to implement a passive redetermination process for the families that continue paying premiums, which in turn appears to reduce disenrollment levels (Dick et al. 2002). There may be nonfinancial impacts as well. It is possible that new or higher premiums can change voter and provider perceptions of SCHIP, making it appear more like private coverage and potentially enhancing long-term support for the program.

A number of changes to public health insurance programs for children are being contemplated, including providing states with greater latitude to use cost-sharing in Medicaid. A National Governors Association plan would allow states to charge premiums as high as 5% of family income for children with family incomes between 100% and 150% of the FPL (NGA 2005). Currently, only nominal premiums are permitted in that income group and other research suggests that even these very low premium levels can lead to reductions in enrollment levels (Herndon et al. 2006; Kenney et al. 2006). Moreover, this analysis suggests that among children with family incomes above 150% of the FPL, premiums that are well below the levels permissible under current law can reduce enrollment levels. It will be critical for states that impose significantly higher premiums to understand how they affect rates of coverage for children and to assess the subsequent impacts on children's coverage status and their access to health care.

Notes

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At the time this paper was written, R. Andrew Allison was director of health care finance and organization at the Kansas Health Institute.

1 Three national studies (Kenney, Hadley, and Blavin 2006/2007; Hadley et al. 2006/2007; and Kronebusch and Elbel 2004) find a negative relationship between public premiums and enrollment in public health insurance programs for children. Two state-specific studies examined premium changes in Florida using a Cox proportional hazards model. The first study indicated that the likelihood of disenrollment increased for enrollees who experienced premium increases, while the reverse was observed for those experiencing premium decreases (Shenkman et al. 2002). The more recent study (Herndon et al. 2006) found that premium hikes were associated with an increased likelihood of disenrollment, with larger effects observed among children whose family incomes were between 100% and 150% of the federal poverty level than among more affluent families.
The Maryland Department of Health and Mental Hygiene (2004) reported that among parents of children disenrolled for nonpayment of a $37 premium for children with family incomes between 185% and 200% of the federal poverty level, 63% felt the premium was affordable and 35% did not. One-fifth of all families that were disenrolled indicated that the new premium was a factor leading to their child's disenrollment. The Vermont Department of Prevention, Assistance, Transition, and Health Access (2004) found an ambiguous impact on enrollment following an increase in premiums from $20 to $25 per family per month for children with family incomes between 185% and 225% of the federal poverty level, and from $50 to $70 per family per month for children with family incomes between 225% and 300% of the federal poverty level.

2 Premiums were tripled in Kansas for a period of five months and then reduced to double the original amount.

3 Kansas children are eligible for Medicaid with household incomes at or under 150% of the FPL under age 1, 133% of the FPL for ages 1–5, and 100% of the FPL at ages 6 and older.

4 Kentucky children are eligible for Medicaid with household incomes at or under 185% of the FPL under age 1, 133% of the FPL for ages 1–5, and 100% of the FPL at ages 6 and older.

5 Tricia Brooks, New Hampshire Healthy Kids president and CEO. Personal communication, February 2005.

6 For Kansas, the time period of the data used for both the time-series and hazard models was July 2001 to December 2004. For Kentucky, the time frame for the time-series model was November 2001 to April 2005, while the time frame for the Cox proportional hazard model was December 2001 to August 2004. For New Hampshire, the time period of the data used for both the time-series and hazard models was July 2001 to November 2004.

7 This implies that a transfer into Medicaid or non-premium-paying SCHIP is considered an exit from premium-paying SCHIP.

8 The income data were highly correlated with the premium-paying category of the child and, on average, children in the 251% to 300% FPL enrollment category had higher incomes than those in the 185% to 250% FPL category.

9 This adjustment occurs in May in Kansas and in April for Kentucky and New Hampshire. The total caseload models were estimated with 42 monthly observations for Kentucky and Kansas and with 41 monthly observations for New Hampshire.

10 The hazard rates associated with the 36-month recertification point were estimated for both Kansas and New Hampshire.

11 The following describes the covariates included in the hazard analyses in each state. In Kansas, the model included a premium change variable, recertification variables (first, second, or third), SCHIP program category (151% to 175% FPL vs. 176% to 200% FPL), age, gender, race/ethnicity, and the number of siblings. In Kentucky, the model included a premium change variable, recertification variables (first or second), enrollment in public coverage prior to enrolling in the premium-paying category, participation in managed care, age, gender, race, number of siblings, and region of residence. In New Hampshire, the model included a premium change variable, recertification variables (first, second, or third), SCHIP program category (185% to 250% FPL and 251% to 300% FPL), income, age, gender, and household size.

12 In alternative specifications, we modeled the policy effect as: 1) an interaction with the time trend without the time trend squared variable; 2) interactions with both the time trend and the time trend squared without a dummy variable for the period after the premium increase was in effect; and 3) a dummy variable for the period after the premium increase was in effect without the time trend squared variable.

13 The unemployment rate had no effect on caseloads that was statistically significant at conventional levels in any of the three states. However, the effect of the premium change in New Hampshire was affected by the inclusion of the unemployment rate, due to the fact that changes in the unemployment rate were correlated with the increase in the premium.

14 The premium effects estimated in the hazard models are sensitive to the inclusion of income information in New Hampshire and to the inclusion of unemployment data in both Kentucky and New Hampshire. While the direction of the premium effects was consistent across most alternative specifications, the magnitude of the effect did vary across models. In addition, the estimated effects of the unemployment rate varied across states; it was found to have a positive effect on the disenrollment hazard in New Hampshire, while it was found to have a negative effect on the disenrollment hazard in Kentucky.

15 Similarly, the coefficient on the managed care indicator in the Kentucky model was 1.19 (significant at a .10 level) and the interaction term between the policy change and managed care was 1.07 and significant at a .01 level, implying that an enrollment spell for a child in managed care was more likely to result in a disenrollment after the introduction of the premium relative to children not enrolled in managed care. Since managed care is concentrated in just a few areas of the state, this finding may reflect other differences between the children enrolled in managed care and those in a fee-for-service setting.

16 Trish Leddy, administrator, Center for Child and Family Health, State of Rhode Island. Personal communication, November 2005.

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Review

The use of financial incentives in promoting smoking cessation

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A R T I C L E I N F O

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A B S T R A C T

Objective: Cigarette smoking is the leading cause of preventable death in the United States and world. Despite the availability of numerous therapies for smoking cessation, additional efficacious interventions are greatly needed. We provide a narrative review of published studies evaluating financial incentives for smoking cessation and discuss the parameters important for ensuring the efficacy of incentive interventions for smoking cessation.

Methods: Published studies that evaluated the impact of incentives to promote smoking cessation and included an appropriate control or comparison condition were identified and reviewed.

Results: Incentives are efficacious for promoting smoking abstinence across the general population of smokers as well as substance abusers, adolescents, patients with pulmonary disease, patients with serious mental illness and other challenging subgroups. To develop and implement an effective incentive treatment for smoking, special attention should be paid to biochemical verification of smoking status, incentive magnitude and the schedule of incentive delivery.

Conclusion: Consistent with the extensive literature showing that incentives are effective in reducing illicit drug use, a large body of evidence supports their effectiveness in reducing smoking. Continued efforts are warranted to further develop and disseminate incentive-based treatments for smoking cessation across clinical settings and populations.

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Objective

Cigarette smoking is the leading cause of preventable death worldwide, with a current annual mortality of 5 million and a projected increase to 8 million by 2030 (Mathers and Loncar, 2006). In the U.S., smoking causes over 443,000 premature deaths each year, accounting for 30% of all cancer deaths and costing over $96.8 billion annually (American Cancer Society, 2011; Centers for Disease Control, 2008). Despite the many therapies available for smoking cessation (Fiore et al., 2008; Public Health Service, U.S. Department of Health and Human Services, 2000), most patients relapse within the months following a quit attempt (Ahluwalia et al., 2002; Brigham et al., 1991; Fiore et al., 1994; Hughes et al., 2004; Hurt et al., 1997). Meta-analyses evaluating the effectiveness of the various smoking treatments have shown that estimated abstinence rates tend to be relatively modest (e.g., 6-month abstinence rates of 8–28%; Fiore et al., 2008). Many smokers also experience significant difficulty initiating abstinence. While up to 80% of smokers express some desire to quit, only about 30% actually attempt to quit per year (Eisenberg et al., 1999). Of those who try to quit, at least 75% relapse within 2–3 days following an initial quit attempt (Hughes et al., 1992). Especially problematic are “hard-core” smokers who may be difficult to help with existing treatments (Emery et al., 2000; Seidman and Covey, 1999). These smokers typically have a heavy pattern of smoking (≥15 cigarettes/day), infrequent and unsuccessful quit attempts, and a low motivation to quit (Emery et al., 2000). Data also suggest that socioeconomic disadvantage is associated with poor response to cessation treatments (Higgins and Chilcoat, 2009). Taken together, the available data show that the vast majority of smokers fail to initiate abstinence or have relapsed within six months of treatment (Fiore et al., 1994).

The limitations of current interventions, as well as the difficulties reaching hard-core smokers, suggest the need for innovative and more intensive approaches (Fagerstrom et al., 1996; Henningfield, 2000; Ranney et al., 2006; Stitzer, 1999). We review research on the systematic use of financial incentives to promote abstinence from smoking. Incentive-based interventions, also known as contingency management (CM), are empirically-supported behavioral treatments with demonstrated effectiveness across a wide range of populations and drugs. This approach typically involves a tangible reward delivered contingent upon the patient meeting a predetermined therapeutic target (Higgins et al., 2008). In the most common approach, patients earn voucher-based incentives for biochemically-verified abstinence from drug use. In the area of smoking cessation, smoking abstinence is typically the primary target behavior, with incentives contingent upon negative breath carbon monoxide or urine cotinine levels.

The aims of the present report are two-fold: first, we aim to provide the reader with an understanding of the experimental research on incentives for smoking cessation. We present a narrative review of the published experimental studies evaluating financial incentives for smoking cessation, ranging from the early studies demonstrating initial efficacy to more recent replications across populations and platforms. As several quantitative reviews have been published previously on this topic (Cahill and Perera, 2011; Lussier et al., 2006; Prendergast et al., 2006), we will not repeat that exercise here but rather will direct the reader to those reviews for greater details when appropriate. Second, we aim to inform the reader on how to thoughtfully design and implement an incentive program for smoking cessation, with an understanding of the parameters most likely to optimize effectiveness.

Criteria for inclusion

Literature searches were conducted using PubMed, PsychINFO and Cochrane Database of Systematic Reviews, using the terms ‘vouchers’, ‘contingency management’, and ‘incentives’ both alone and paired with the terms ‘smoking’ and ‘nicotine’. Reference sections of published articles were also reviewed, and investigators in this area of research were consulted to ensure that all relevant articles were included (White, 1994). November 2011 represented the last publication month for which studies were included. Studies were included if they met the following criteria: (1) involved contingent financial incentives for smoking abstinence, (2) were published in a peer-reviewed journal; (3) used a research design wherein treatment effects could be attributed to the incentive intervention. This last criterion included randomized trials that included comparison conditions where subjects either did not receive financial incentives or received them noncontingently, as well as studies that utilized a within-subjects design wherein effects of the incentive condition could be compared to a baseline comparison condition. Publications from two areas (i.e., Quit and Win contests, workplace interventions) were not included in this review. Quit and Win contests typically involve verification of smoking status prior to participant entry, a pledge to quit on a unified date for a predetermined duration of time, and a grand prize (e.g., family vacation) for a randomly-selected abstinent participant (Lando et al., 1994). Workplace incentive interventions typically involve an employer-organized intervention wherein smokers are reinforced individually or in teams for evidence of a cessation attempt, change in self-reported tobacco use, or participation in the program. While both types of interventions represent creative and important approaches, the published studies vary widely in methodology and are generally less rigorous than the interventions reviewed here (i.e., less frequent clinical contact, biochemical monitoring and contingent reinforcement opportunities). For additional information about both approaches, we direct the reader to several recent and very thorough reviews (Cahill and Perera, 2008, 2011; Cahill et al., 2008; Leeks et al., 2010).

Incentives for smoking cessation in the general population

In this section we review the studies evaluating incentives for promoting smoking abstinence in the general population of smokers. We begin by briefly discussing the early studies which demonstrated the initial feasibility and efficacy of incentives for promoting smoking abstinence, followed by more recent efforts to extend incentives to broader populations of smokers.

Seminal studies

The seminal demonstrations of the feasibility of using incentives to reduce smoking were conducted by researchers at Johns Hopkins University (Stitzer and Bigelow, 1982, 1983, 1984, 1985; Stitzer et al., 1986b). These studies generally involved non-treatment-seeking adult smokers, rigorous experimental methods, brief intervention durations (mean: 2.3 weeks, range: 1–4 weeks), frequent study visits (range: 1–3×/day), CO monitoring and moderate incentive magnitudes (mean: $37.25/week; range: $5–84/week). They provided persuasive demonstrations that incentives, delivered contingent upon biochemically-verified abstinence, can reduce smoking. Overall, these studies provided the first, and most rigorous, demonstrations of the efficacy of incentive interventions in reducing smoking among healthy adult smokers.

Using incentives to examine other questions related to smoking

Subsequent studies conducted over the next two decades replicated the finding that contingent incentives can promote smoking abstinence. Many have also used incentives as a tool to investigate larger scientific questions related to smoking. For example, early abstinence plays a crucial role in determining the likelihood of success of a quit attempt, with smoking abstinence during first two weeks of a quit attempt strongly predicting longer-term success (Garvey et al., 1992; Gourlay et al., 1994; Higgins et al., 2006; Kenford et al., 1994; Lamb et al., 2004a; Yudkin et al., 1996). Several studies demonstrated that a period of experimentally-produced abstinence (or lapses) can directly influence subsequent efforts to abstain from smoking and identified reductions in nicotine withdrawal, craving and relative reinforcing effects of smoking as potential mechanisms underlying this effect (Alessi et al., 2004; Chornock et al., 1992; Heil et al., 2004; Lussier et al., 2005). In the strongest demonstration, Yoon et al. (2009) used incentives to experimentally manipulate the amount of abstinence achieved (i.e., 1 vs. 14 days) and demonstrated that the relationship between early and later abstinence is causal rather than correlational. Studies also have used incentives to experimentally manipulate smoking abstinence and evaluate nicotine withdrawal (Alessi et al., 2004; Heil et al., 2003). These provided important information characterizing the time-course and severity of nicotine withdrawal among smokers. Incentives have also been used to produce abstinence and examine how abstinence may influence individuals' self-efficacy and readiness to quit. Contrary to earlier assertions that incentives may decrease these measures (Curry et al., 1991), studies have shown that both self-efficacy (Alessi et al., 2004; Heil et al., 2004; Lamb et al., 2005) and readiness to quit (Lamb et al., 2005) increase during incentive-produced abstinence. Incentives also have been used as an evidence-based platform to evaluate the contribution of pharmacotherapies for smoking cessation (Gray et al., 2011; Hanson et al., 2003; Mooney et al., 2005; Perkins et al., 2008; 2010; Tidey et al., 2002, 2011). Finally, as discussed below, studies have used incentives to investigate the procedural details that influence the efficacy of incentive treatments, including schedule, immediacy and incentive magnitude (e.g., Roll and Higgins, 2000; Roll and Howard, 2008; Roll et al., 1996, 2000). In summary, incentives have served as an effective tool for investigating a variety of questions related to smoking cessation and relapse. While these studies were conducted with smokers, their findings likely hold generality to other forms of drug use as well.

Using technology to facilitate implementation

Internet, smartphone and other technologies hold potential for increasing access to incentive treatments, particularly for smokers who may have difficulty accessing treatment. For example, Dallery and colleagues have developed an internet-based incentive program for promoting smoking abstinence (Dallery and Glenn, 2005; Dallery et al., 2007; Glenn and Dallery, 2007; Meredith et al., 2011; Reynolds et al., 2008; Stoops et al., 2009). Smoking status is verified using a web camera to observe participants providing CO samples, which are then time stamped, preserved as video recordings and emailed to clinic staff twice daily. A website provides an individualized home page for each participant and other resources. As incentive holdings accumulate, the participant requests goods and services by email, which are then delivered to them in person or by mail. Results suggest that this approach is feasible and effective in promoting smoking abstinence (see Dallery and Raiff, 2011 for a recent review). Other forms of web-assisted tobacco interventions (WATIs) are also receiving increasing attention and could incorporate incentive components for abstinence (e.g., Feil et al., 2003; Meis et al., 2002; Norman et al., 2008; Riley et al., 2002; Woodruff et al., 2001).

Using shaping procedures to aid hard-to-treat smokers

While incentives are effective in promoting abstinence, there is often a subset of hard-to-treat smokers who do not encounter the available incentives because they never meet the abstinence requirement (Lamb et al., 2004a). Shaping procedures may help surmount this problem by increasing the likelihood that early abstinence will be reinforced. Using a behavior-shaping technique with percentile schedules, a participant can receive rewards for demonstrating gradual reductions in CO levels over consecutive visits until he or she eventually achieves CO levels that are below the qualitative abstinence cutoff (Galbicka, 1994). Lamb et al. (2004a, 2004b, 2005, 2007) have conducted a series of experimental studies demonstrating that CO contingencies set by percentile schedules produce significant reductions in CO levels, even among smokers without plans to quit. These findings suggest that less stringent shaping procedures may aid individuals who are difficult to treat with typical incentive (or other) approaches.

Larger-scale applications of incentives for smoking cessation

While incentives can clearly produce robust effects on smoking, most of the above studies utilized limited sample sizes and tight experimental control. An important question is whether incentive-based smoking cessation can be implemented on a larger scale. Several recent studies suggest that this approach can be scaled up and still retain efficacy in promoting abstinence. One randomized trial, for example took place at a Veterans Affairs Medical Center with 179 smokers (Volpp et al., 2006). The incentive group received financial incentives for attending smoking classes and for evidence of abstinence 30 days after treatment (defined as a self-report of no smoking during the past seven days and urine cotinine <500 ng/ml). The control group was simply offered the five smoking classes. Attendance rates were higher in the incentive vs. control group. While these abstinence criteria were less stringent and the levels of smoking abstinence somewhat lower than many of the above studies, this trial demonstrated that modest financial incentives can increase enrollment and short-term quit rates in a community-based clinical setting. Another study was conducted with 878 employees of a large, multinational company (Volpp et al., 2009). Smokers in the incentive group received information about smoking-cessation programs available in the community plus financial incentives for completing a program and for evidence of smoking abstinence. Smokers in the control group simply received information about available smoking-cessation programs. Significantly more incentive participants participated in and completed smoking-cessation programs compared to controls. Additionally, significantly more incentive participants achieved biochemically-verified abstinence and these differences persisted 6 months after discontinuation of the incentive program. This study provided the first empirical data showing that incentives, implemented on a large scale, can produce significant treatment effects that persist after the program is discontinued.

Incentives for smoking cessation in special populations

Over the past two decades, incentives have also been used to target smoking in a variety of challenging smoker subgroups. Below we briefly review the published studies that have evaluated the efficacy of incentive-based approaches in special populations of smokers.

Substance abusers

Nine studies have evaluated the use of incentives for promoting smoking abstinence among illicit drug abusers (Alessi et al., 2008; Dunn et al., 2008, 2010; Hunt et al., 2010; Robles et al., 2005; Schmitz et al., 1995; Shoptaw et al., 1996, 2002; Wiseman et al., 2005). Most of this work has been done with patients receiving methadone or buprenorphine maintenance treatment for opioid dependence. Prevalence of smoking in these patients is estimated at 84–94% and associated with increased morbidity and mortality (Clemmey et al., 1997; Hser et al., 1994; Hurt et al., 1996; Nahvi et al., 2006; Richter et al., 2001). The majority of patients recognize the serious health risks associated with smoking and express interest in quitting (Clemmey et al., 1997; Frosch et al., 1998; Kozlowski et al., 1989; Richter et al., 2001; Sees and Clark, 1993). Further, the opioid treatment modality is uniquely situated for implementing smoking-cessation interventions as many patients are stable, achieve prolonged periods of abstinence from illicit drug use and remain in treatment for extended periods. This lends itself to frequent and, if necessary, prolonged clinical contact to ensure success with smoking cessation. Additionally, methadone and buprenorphine programs adhere to a relatively uniform set of state and federal regulations. Thus, development of an effective intervention in these patients holds substantial potential for impacting tobacco use in this group throughout the country.

Of the five studies investigating incentives for smoking cessation in opioid-maintained patients, four have showed a significant treatment effect (Dunn et al., 2008, 2010; Shoptaw et al., 1996, 2002). These were randomized trials wherein methadone- or buprenorphine-maintained smokers could earn voucher-based incentives contingent upon smoking-abstinent breath or urine samples. They generally involved frequent study visits (range: daily–3×/week), moderate incentive magnitudes (mean: $104.51/week; range: $18.25–$181.25/week) and an escalating schedule of reinforcement for consecutive negative samples (cf. Higgins et al., 1991). These studies provided clear support for the efficacy of incentives in promoting smoking abstinence in opioid-dependent patients. In contrast to the above studies, no treatment effect was seen in an early report by Schmitz et al. (1995), which used a within-subject design and involved relatively low-frequency visits (2×/week), modest incentive magnitudes ($10/week) and no escalating schedule. As noted previously, magnitude and schedule of reinforcement can be important determinants of the efficacy of incentive interventions (Kirby et al., 1998; Roll and Higgins, 2000; Roll et al., 1996; Silverman et al., 1999; Stitzer and Bigelow, 1983, 1984). The remaining studies with non-opioid substance abusers have also used relatively frequent study visits (range: 3×/day–2×/week) and moderate incentive magnitudes (range: $50–$149.50/week) and also produced significant effects (Alessi et al., 2008; Hunt et al., 2010; Robles et al., 2005; Wiseman et al., 2005). Overall, incentives are effective at promoting smoking abstinence in substance-abusing patients and offer potential for wide dissemination to treatment programs throughout the country.

Adolescents and young adults

Another growing area of research is with younger smokers. More than 80% of established adult smokers began smoking before age 18 years and, in 2009, 8.2% of middle school and 23.9% of high school students reported current tobacco use (Centers for Disease Control, 2010). Applying innovative and efficacious treatments to this population seems particularly important. Ten experimental studies have been published evaluating the efficacy of incentives in promoting smoking abstinence in adolescents (Corby et al., 2000; Gray et al., 2011; Krishnan-Sarin et al., 2006; Reynolds et al., 2008; Roll, 2005; Tevyaw et al., 2007) and college students (Correia and Benson, 2006; Irons and Correia, 2008; Kassaye, 1984; Tevyaw et al., 2009). Of these, eight showed a significant treatment effect (Corby et al., 2000; Correia and Benson, 2006; Irons and Correia, 2008; Kassaye, 1984; Krishnan-Sarin et al., 2006; Reynolds et al., 2008; Roll, 2005; Tevyaw et al., 2009). They generally involved brief durations (range: 1–4 weeks), a wide range of visit frequencies (range: 1×/week–3×/day), and moderate incentive magnitudes (mean: $47.27/week; range: $17.49–$94.50/week). These studies provided clear support for the efficacy of incentives in promoting abstinence in adolescent smokers. In contrast, no significant treatment effect was seen in reports by Tevyaw et al. (2007) or Gray et al. (2011). While these studies used generally similar parameters, possible reasons for the lack of significant effects may include the unusually high abstinence rate during baseline (Tevyaw et al., 2007) and infrequent study visits (Gray et al., 2011). In addition, while Gray et al. (2011) did not show a main effect of vouchers, they did find a significant interaction wherein bupropion + incentives demonstrated superior abstinence over either bupropion or incentives alone.

Taken together, a growing literature is demonstrating the efficacy of incentives for reducing smoking in young people. School settings may offer a particularly good setting in which to implement smoking cessation, as most students attend the school or college frequently and for extended periods. While not experimental evaluations, several additional publications have reported on important issues related to this topic. These have included examinations of spending behavior among adolescents receiving incentives for smoking abstinence (Cavallo et al., 2010), efforts to combine incentives with other treatment components such as nicotine replacement and cognitive–behavior therapy (Cavallo et al., 2007; Hanson et al., 2003), examination of predictors of treatment response among adolescent smokers (Krishnan-Sarin et al., 2007) and more general reviews of this treatment approach (Stanger and Budney, 2010).

Patients diagnosed with pulmonary disease

While smoking produces a wide range of health problems, respiratory illness is one of the most common smoking-related consequences. Chronic obstructive pulmonary disease (COPD) is one respiratory illness that includes chronic bronchitis, emphysema and other significant comorbidities (Agusti, 2005; GOLD, 2010; van Weel and Schellevis, 2006). An estimated 90% of all COPD deaths in the U.S. are attributable to smoking, and smoking cessation is identified as the single most effective intervention to reduce the incidence and progression of COPD (GOLD, 2010; Mannino et al., 2003; NCCCC, 2004; U.S. Department of Health and Human Services, 1984, 2004). Two studies have been conducted with COPD-diagnosed smokers (Crowley et al., 1991, 1995). These involved lottery tickets as the incentives, daily study visits, moderate incentive magnitudes (range: 7–140 tickets/week) and an adaptive reinforcement schedule dependent upon smoking abstinence. Both studies produced significant treatment effects, providing support for the efficacy of incentives in promoting abstinence in this challenging group of smokers.

Other special smoker populations

Incentives have been used to promote smoking abstinence among other special subgroups of smokers at particularly-elevated risk for specific health implications, including patients with serious mental illness (e.g., Gallagher et al., 2007; Kollins et al., 2010; Roll et al., 1998; Tidey et al., 2002, 2011) and pregnant women (e.g., Donatelle et al., 2000, 2004; Gadomski et al., 2011; Heil et al., 2008; Higgins et al., 2004, 2010; Ker et al., 1996). Detailed information on the use of incentives for smoking cessation with these important groups is covered elsewhere in this Special Issue.

Parameters of effective incentive interventions

Our second aim is to provide the reader with an understanding of the procedural details that are central to developing and implementing an effective incentive program for smoking cessation.

Biochemical monitoring

There are two common approaches to monitoring recent smoking: measuring breath carbon monoxide (CO) levels and measuring cotinine levels found in urine, plasma or saliva. Breath CO levels are typically measured using hand-held meters, which are simple to use and require little technical training. CO is generally considered the best way to verify smoking abstinence during the initial days of cessation (Jarvis et al., 1987). As the elimination half-life of CO averages about 4 h, abstaining smokers can achieve a level in the non-smoker range within 12–24 h (SRNT Subcommittee on Biochemical Verification, 2002). Thus, breath CO monitoring can provide a sensitive method for detecting and reinforcing abstinence in the early days following a quit attempt when relapse risk is at its highest. In designing a treatment, it is important to choose a CO cutoff that is low enough to detect low rates of ongoing smoking but not so low that second-hand smoke or another environmental source of CO creates a false-positive reading. Recent studies suggest that cutoffs of 3–6 ppm provide an appropriate level of sensitivity (Higgins et al., 2004, 2007a; Javors et al., 2005; Middleton and Morice, 2000; SRNT Subcommittee on Biochemical Verification, 2002). When breath CO is used as the primary measure, the schedule of monitoring also becomes important. Given the short elimination half-life, light smokers can rapidly achieve a negative CO within several hours of their last cigarette. Thus, interventions need to include at least daily monitoring in order to be confident of continuous abstinence. Even with daily monitoring, smokers may be able to engage in low levels of smoking that go undetected (Javors et al., 2005). As a result, some incentive studies have required CO monitoring to take place multiple times per day (e.g., Alessi et al., 2004; Heil et al., 2003).

Cotinine is a metabolite of nicotine that has a half-life of approximately 20 h and is an especially sensitive biomarker for verifying smoking status (Benowitz, 1996; Jarvis et al., 1987; Rebagliato, 2002). Cotinine testing requires several continuous days of abstinence for smokers to achieve levels in the non-smoker range (SRNT Subcommittee on Biochemical Verification, 2002). Levels can be obtained from a range of bodily fluids (e.g., urine, plasma, saliva) and are typically analyzed using enzyme immunoassay testing (EMIT) or gas chromatography (GC). More recently, semi-quantitative test strips (e.g., NicAlert), which require no specialized equipment or training, have been demonstrated reliable for measuring cotinine (Gaalema et al., 2011). This approach may provide a cost- and time-sensitive alternative to EMIT or GC. An advantage of cotinine is that a monitoring frequency of 2–3 times per week should be sufficient to detect recent smoking. However, because of its relatively long half-life, basing incentive delivery on cotinine during the early days of a quit attempt can be problematic. For example, an individual smoking 20 cigarettes/day would need approximately 5 days to meet the commonly-used cutoff of 80 ng/ml, which would significantly delay the first opportunity to reward this early abstinence (Higgins et al., 2007a; SRNT Subcommittee on Biochemical Verification, 2002). Considerations for choosing an appropriate cotinine cutoff are similar to those outlined above for CO (e.g., balancing risks of both sensitivity and specificity). Currently, there are no clear published guidelines about the sensitivity of different cotinine cutoffs, though a urinary cutoff of 50 ng/ml is generally recommended when using GC to classify smoking status in the general population of smokers (SRNT Subcommittee on Biochemical Verification, 2002). A final consideration is that other sources of nicotine, such as nicotine replacement therapies, will elevate cotinine levels and confound testing. This can be circumvented by using non-nicotine (e.g., varenicline, bupropion) rather than nicotine-containing medications. Overall, considering the above issues, an effective intervention might strategically combine both methods in order to maximize the opportunity to detect and reinforce smoking abstinence. For example, breath CO could be used during the initial days of the cessation effort, followed by less frequent monitoring using urinary cotinine later in the intervention (Dunn et al., 2008, 2010; Heil et al., 2008; Higgins et al., 2004). This procedure maximizes confidence that the person is not engaging in low levels of undetected smoking while minimizing the chance that early efforts at abstinence go unrewarded.

Incentive magnitude

A consistent finding in the incentive literature is that larger-magnitude incentives are more effective than smaller magnitudes (Lussier et al., 2006). In an early study, for example, Stitzer and Bigelow (1983) directly examined the effects of payment amount on the reduction of breath CO levels using a between-groups design in which participants were required to reduce their CO level to half their baseline level to earn an incentive. They found that success at meeting the CO criterion increased as the payment amount increased. These results have been replicated in subsequent studies (Correia and Benson, 2006; Lamb et al., 2004b; Stitzer and Bigelow, 1984). Overall, there is an orderly relationship between the amount of the incentive and amount of smoking abstinence produced and this is consistent with the larger literature on incentives and illicit drug use (e.g., Silverman et al., 1999).

Schedule of incentive delivery

Another important feature of an effective treatment is the schedule used for delivering incentives. Escalating payment schedules, for example, reward an individual for achieving continuous abstinence by increasing the value of the incentives earned with each consecutive negative (Higgins et al., 1991). Rather than promoting only brief durations of abstinence, they are specifically designed to reinforce the continuous periods of sustained abstinence that are important for longer-term success. To further promote continuous abstinence, they often include a reset component wherein voucher values following an instance of drug use are reset back to the initial value. In studies directly comparing escalating to fixed payment schedules, escalating schedules with a reset contingency are more effective in promoting longer durations of abstinence than fixed conditions (Roll and Higgins, 2000; Roll et al., 1996). Indeed, escalating payments are now a standard feature of many incentive programs for drug abuse (e.g., Dunn et al., 2008, 2010; Heil et al., 2008; Higgins et al., 1993; Shoptaw et al., 2002; Silverman et al., 1996).

Challenges

While three decades of science have demonstrated the efficacy of incentives for smoking, barriers to their widespread adoption have included their perceived cost, complexity and staff burden (Ledgerwood, 2008; Stitzer and Kellogg, 2008). In this final section we discuss some of the challenges encountered thus far, as well as efforts taken to surmount them.

Cost

One commonly-cited challenge is the perceived cost of incentive treatments. There are a variety of potential solutions to help defray or minimize these costs. As discussed above, capitalizing on technology may help to reduce some of the costs of implementation (Dallery and Raiff, 2011). Donations could be solicited from businesses, health care groups and foundations to help cover the incentives themselves (Amass and Kamien, 2004, 2008; Donatelle et al., 2000). While monetary incentives are among the most highly valued rewards (Amass et al., 1996; Roll et al., 2005), one also can consider using other naturally-occurring, often non-monetary forms of reinforcement to promote positive behavior change in challenging clinical populations. Methadone clinics have for decades effectively provided access to clinic privileges (e.g., methadone take-home doses, scheduling flexibility) contingent upon the patient's demonstration of drug abstinence or compliance with treatment (Stitzer et al., 1977, 1986a). Further, considering particularly high smoking rates and poor treatment response among disadvantaged individuals (Graham et al., 2007; Higgins and Chilcoat, 2009), there is potential for creatively using Medicaid or other funds as incentives for changes in smoking or other health-related behaviors that increase risk for chronic disease. Indeed, incentive-based policies for a range of health behaviors are being increasingly adopted by health maintenance organizations and policymakers in the U.S. and internationally (Higgins, 2010; Pear, 2009; Rosenberg, 2008). Finally, other creative approaches for mitigating incentive costs include fee rebates, deposit contracting and delivering incentives on an intermittent or random schedule (Amass and Kamien, 2008; Cahill and Perera, 2008, 2011; Elliott and Tighe, 1968; Kaper et al., 2005; Knealing et al., 2008; Paxton, 1980, 1981, 1983; Petry and Alessi, 2008; Winett, 1973). For example, deposit contracting typically requires that the patient makes an initial monetary deposit which can then be recouped based on smoking abstinence. While most of these studies have not used a research design wherein treatment effects could be attributed to the incentive intervention, results do suggest that this approach may offer a cost-effective way to deliver incentives for smoking abstinence (Amass and Kamien, 2008; Dallery et al., 2008; Elliott and Tighe, 1968; Lando, 1977; Paxton, 1980, 1981, 1983; Spring et al., 1978; Winett, 1973). However, one note of caution is to remember that reward magnitude is an important determinant of the effectiveness of incentive interventions and that at some point reductions in the amount of the reward could inadvertently dilute the efficacy of the intervention (Dallery et al., 2001; Lussier et al., 2006; Petry et al., 2004; Silverman et al., 1999). Worth noting is that incentive interventions for smoking, as well as for other behaviors, actually represent a relatively cost-effective approach. Incentive costs are generally proportional to the degree of behavior change they produce, with costs increasing when the intervention produces the desired results (which are accompanied by their own health-related improvements and cost savings) and decreasing when the individual fails to respond to treatment. Considering that tobacco use results in $96 billion in health care costs each year (American Cancer Society, 2011), cost and benefit assessments of incentive approaches for smoking cessation are favorable (see contribution by Dr. Sindelar in this Special Issue).

Sustaining abstinence after the intervention

Another challenge is whether incentive-produced abstinence can be sustained after the intervention itself is discontinued. There is some evidence that incentive programs can have lasting effects on smoking following treatment discontinuation (Heil et al., 2008; Higgins et al., 2004; Volpp et al., 2009). That said, as with other chronic health conditions (e.g., diabetes, hypertension), relapse to smoking may occur post-treatment and longer-term or ongoing support may be necessary to sustain treatment effects. As the data suggest that the key to producing longer-term success is increasing during-treatment abstinence (Higgins et al., 2007b; Yoon et al., 2009), efforts to further improve rates of initial abstinence are reasonable ways to drive up post-treatment success (e.g., increase incentive magnitude, increase frequency of monitoring, add pharmacotherapy). A second possibility is the use of incentive programs that can be in place for an extended period. Examples of these already exist for using incentives to treat illicit drug use (e.g., Silverman et al., 1996, 1999, 2007) and it is not inconceivable to do the same for smoking. Web-based or other technology platforms could be also used to support the long-term use of incentives for smoking abstinence. Finally, a hybrid approach could include an intensive incentive intervention for the initial months following cessation, followed by a carefully-constructed maintenance phase involving less frequent or perhaps randomly-scheduled biochemical monitoring and high-magnitude rewards for abstinence.

Dissemination

Efforts to extend incentives into widespread practice have also been slow, though the studies by Volpp et al. (2006, 2009) demonstrate the significant potential for implementing this approach on a larger scale. The continued use of innovative technology will likely be important for supporting the dissemination of incentives for smoking and other health behaviors, as is evidenced by the exciting work to move incentives to internet-based platforms for smoking (Dallery and Raiff, 2011). As noted above, health maintenance organizations and state and federal policymakers in the U.S. and elsewhere are beginning to incorporate incentive-based policies for a range of health behaviors (e.g., Higgins, 2010; Pear, 2009; Rosenberg, 2008), which will further promote the use of this approach on a large scale. Overall, evidence of the widespread dissemination of incentives for smoking and other behaviors is building.

Conclusion

Tobacco use remains a significant public health problem. Despite the availability of numerous treatments (Fiore et al., 2008), most patients relapse and additional efficacious interventions are sorely needed. Incentives are effective in promoting smoking abstinence across the general population of smokers as well as substance abusers, adolescents, patients with pulmonary disease, patients with serious mental illness and other challenging subgroups. To develop and implement an effective incentive treatment for smoking, special attention should be paid to biochemical verification of smoking status, incentive magnitude and the schedule of incentive delivery. Challenges thus far to the large-scale use of incentives have included perceived cost, the need to sustain the abstinence following discontinuation of incentives, and the need for innovative ways to support implement incentive treatments on a larger scale. Overall, continued efforts are warranted to further develop and disseminate incentive-based treatments for smoking cessation.

Conflict of interest statement

The authors declare that there are no conflicts of interest.

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ARIZONA HEALTH CARE COST CONTAINMENT SYSTEM

FISCAL IMPACT OF IMPLEMENTING COST SHARING AND BENCHMARK BENEFIT PROVISIONS OF THE FEDERAL DEFICIT REDUCTION ACT OF 2005

December 13, 2006

Janet Napolitano
Governor

Anthony D. Rodgers
Director

REPORTING REQUIREMENTS

Arizona Session Laws 2006, Chapter 344, § 3 (HB 2863) requires the Arizona Health Care Cost Containment System (AHCCCS) to submit to the Joint Legislative Budget Committee (JLBC) a report on the fiscal impact of implementing certain provisions in the federal Deficit Reduction Act of 2005 (Public Law 109-171). The report must address the fiscal impact associated with:

1. Enacting the maximum amount of cost sharing subject to the federal limitations that aggregate cost sharing and premiums cannot exceed five percent of household income.
2. Cost sharing for prescription drugs that are not preferred drugs within a class.
3. Cost sharing for nonemergency care provided in a hospital.
4. Cost sharing for an alternative benefit package that is actuarially equivalent to federal benchmark benefit packages. As required in a September 1, 2006, letter to JLBC, AHCCCS reported that it selected the state employee health benefit plan as a benchmark.

SUMMARY AND FINDINGS

1. The Agency identified $8,865,334 in potential premiums or $8,069,380 in potential copayments. The total premiums and copayments cannot be implemented simultaneously due to the 5% limit on cost sharing under the DRA. After accounting for the 66.47% federal share, this would yield $2,972,547 in potential premiums and $2,705,663 in potential copayments. The administrative costs of implementing these cost sharing methods would be $15,838,100.
2. The Agency identified up to $1,902,678 in potential premiums or $4,373,473 in potential copayments for ALTCS members receiving home and community based services; however, imposing additional cost sharing on ALTCS members may have an adverse fiscal impact on the state. Members unable to pay cost sharing may need to forego necessary medical services, leading to poorer health and costlier care. Others may choose to move into nursing facilities to avoid going without needed services. The fiscal impact for the state could be substantial because the cost of institutional care is more than three times greater than the cost of HCBS. In addition, cost sharing would be inconsistent with Arizona's new Medicare copayment subsidy program for many of these individuals.
3. While the static analysis presented in this document potentially identifies millions of dollars in cost sharing options, it should be noted that the Agency has identified considerable infrastructure investments would be needed for implementation. The analysis does not take into account any shift in services as a result of new cost sharing.
4. New premiums may increase disenrollment, resulting in more uninsured citizens, more uncompensated care for the state's hospitals, and further increase challenges facing emergency rooms.
5. Excluding behavioral health services, AHCCCS may incur costs by implementing the state employee benefit plan instead of the AHCCCS benefit plan, due to broader services under the state employee plan and the DRA requirement to provide comprehensive wrap-around services for children.

6. Managed care is intended to shift health care from an illness-based approach to one emphasizing disease prevention and maintenance of health; cost sharing on general office visits may be inconsistent with this objective. Premiums can lead to high member turnover, which can decrease the ability of plans to manage care; therefore, longer enrollment can lead to better health outcomes.

DRA COST SHARING

DRA COST SHARING. Social Security Act § 1916A, established by §§ 6041 to 6043 of the federal Deficit Reduction Act of 2005 (DRA; Public Law 109-171), permits states to require Medicaid members to pay cost sharing that was previously prohibited by federal law. Below are DRA premium and cost sharing requirements.

1. Types of cost sharing. The DRA permits states to require premiums, enrollment fees, copayments, and similar fees, including higher cost sharing for non-preferred drugs and non-emergency use of the emergency room.
2. Return of federal share. The state must return to the federal government the federal share (66.47%) of any premiums or copayments imposed on Medicaid members.
3. Inapplicable to other cost sharing. These provisions apply only to cost sharing imposed under § 1916A. They have no effect on AHCCCS' current cost sharing.
4. Most AHCCCS members ineligible. Section 1916A does not apply to Medicaid-eligible individuals with family income at or below 100% Federal Poverty Level (FPL), or to individuals who are only eligible for Medicare cost sharing, KidsCare, or HIFA parents.
5. Denial of eligibility and services. A state may deny Medicaid eligibility or terminate eligibility for failure to pay a DRA premium, and a state may permit providers to refuse services to members who do not pay DRA copayments.
6. Exempt individuals (premiums). The following individuals cannot be charged premiums or enrollment fees:
    a. Certain mandatory groups of children under 18, including children receiving TANF or SSI-cash, SOBRA children, and foster children.
    b. Recipients of adoption or foster care assistance.
    c. Pregnant women.
    d. Hospice patients.
    e. Institutionalized individuals who are required to pay a share of cost.
    f. Women in the breast and cervical cancer treatment program.
7. Exempt services (copayments). The following services may only be subject to nominal copayments (up to $3.00) for prescription drugs and non-emergency use of the emergency room. No other cost sharing may be imposed.
    a. Services provided to exempt individuals.
    b. Preventive services for children under 18.
    c. Family planning services and supplies.

   d. Emergency services.

8. Five percent aggregate cap. The aggregate amount of all premiums and cost sharing for all family members may not exceed 5% of the family's income.
9. Variable limits. Cost sharing limits depend on family income, unless the individual or service is exempt. The DRA cost sharing maximums are presented in Table 1.

Table 1. DRA Cost Sharing Maximums

Family Income or Exempt Status	**5% Cap**: The aggregate amount of premiums and cost sharing imposed on all family members cannot exceed 5% of family income			
	Premiums	**General Cost Sharing**	**Cost Sharing: Drugs**	**Cost Sharing: Non-emergency ER**
0 - 100% FPL	$0	$0	$0	$0
100% - 150% FPL	$0	10% of service cost	$3.00	$6.00
Above 150% FPL	Unspecified	20% of service cost	20% of drug cost	Unspecified
Exempt individuals and services (regardless of family income)	$0	$0	$3.00	$3.00

DRA COST SHARING ANALYSIS

Groups impacted. In analyzing the fiscal impact of implementing DRA cost sharing for AHCCCS members, the following groups were excluded.

- Groups to which DRA cost sharing provisions do not apply.
- Groups with fewer than 1,120 members, pursuant to actuarial guidelines for achieving credible projections and because the cost of administering cost sharing for small coverage groups can be disproportionate to fiscal savings.
- Institutionalized members of the Arizona Long Term Care System (ALTCS), who already contribute all but a small portion of their income to the cost of care.
- Non-institutionalized children in ALTCS, due to DRA exemptions and because AHCCCS was mandated to implement new cost sharing for developmentally disabled children with family income at or above 400% FPL.

DRA cost sharing can, however, be applied to the following groups. See Table 2 for applicable limits.

- Transitional Medical Assistance (TMA) families with income over 100% FPL. Federal law requires the state to provide temporary Medicaid coverage as families transition away from TANF and poverty. Eligible families are those whose employment income is between 100% FPL and 185% FPL. AHCCCS eligibility ends after two six-month periods. DES determines eligibility.
- SOBRA children with income over 100% FPL. Federal law requires the state to cover children:
    i. Under age one with family income up to 140% FPL,
    ii. Age one to five with family income up to 133% FPL, and
    iii. Age six to 18 with income up to 100% FPL.

  SOBRA children under age six with income between 100% FPL and 140% FPL can be subject to DRA cost sharing.

- Non-institutionalized adult ALTCS members with income over 100% FPL. ALTCS covers long term care services for individuals who are institutionalized or at risk of institutionalization. Non-institutionalized members receive home and community based services (HCBS).

  The two ALTCS groups include individuals who are elderly or physically disabled (EPD) or developmentally disabled (DD). AHCCCS analyzed each group separately due to significant differences in service utilization. As stated above, this analysis excludes children and institutionalized members. Adult ALTCS members receiving HCBS with income over 100% FPL can be subject to DRA cost sharing.

Table 2. Cost Sharing Maximums for Groups Analyzed.

Coverage Group or Exempt Status	**5% Cap**: The aggregate amount of premiums and cost sharing imposed on all family members cannot exceed 5% of family income			
	Premiums	**General Cost Sharing**	**Cost Sharing: Drugs**	**Cost Sharing: Non-emergency ER**
TMA 100% - 150% FPL (unless exempt)	$0	10% of service cost	$3.00	$6.00
TMA 150% - 185% FPL (unless exempt)	Unspecified	20% of service cost	20% of drug cost	Unspecified
SOBRA Children 100% - 140% FPL	$0	$0	$3.00	$3.00
ALTCS HCBS 100% - 150% FPL (unless exempt)	$0	10% of service cost	$3.00	$6.00
ALTCS HCBS Over 150% (unless exempt)	Unspecified	20% of service cost	20% of drug cost	Unspecified
Exempt individual or service (regardless of income or group)	$0	$0	$3.00	$3.00

The following services, which account for the vast majority of expenditures, were included in this fiscal impact analysis.

- Inpatient hospital.
- Outpatient hospital.
- Physician visits.
- Prescription drugs.
- Non-emergency use of the emergency room.

Fiscal analysis: premiums and copayments. AHCCCS established standard copayments by applying the DRA cost sharing maximums to the average cost for that service. See Table 4.

- This is a static analysis. It does not account for decreases in utilization and enrollment resulting from imposition of premiums or cost sharing. Nor does this analysis calculate the financial impact of other consequences of cost sharing. Members' inability to afford cost sharing may result in higher costs of care, such as increased hospitalization or use of emergency services.
- The federal share (66.47%) of all amounts collected must be returned to the federal government; this is not reflected in Table 4.

Fiscal analysis: administration. These are preliminary estimates. If new copayments or premiums are implemented, AHCCCS will need to further refine these estimates. See Table 3.

- AHCCCS' current premium billing system is operating at capacity; the system must be replaced or completely redesigned to expand capacity to include new programs. AHCCCS estimates that it would take up to three years to develop such a system, at a cost of up to $5 million, excluding hardware costs.
- DES does not currently have a premium billing system. If premiums are imposed on groups for which DES determines eligibility, DES may need to build a similar premium billing system, with costs up to an additional $5 million, excluding hardware and ongoing support and maintenance.
- AHCCCS currently does not have a system for tracking copayments to ensure that the Agency complies with the DRA requirement that cost sharing not exceed 5% of family income. Building such a system for AHCCCS and each health plan may cost up to $2 million, excluding ongoing support and maintenance.
- AHCCCS could not estimate personnel costs associated with a copayment system.
- AHCCCS estimates new premiums would add approximately 15 minutes to the eligibility determination process for calculation and management of premiums, requiring 25 new FTE at a total fund cost of $923,900.

Table 3. Administrative Expenses Associated with Implementing New Premiums and Copayments

Expense	Cost
AHCCCS Premium Billing System	Up to $5,000,000
DES Premium Billing System	Up to $5,000,000
Copayment Tracking System	Up to $2,000,000
Hardware	$100,000
Ongoing support	Up to $2,444,000
Premium billing staff (one-time costs)	$26,800
Premium billing staff (ongoing costs)	$343,400
Eligibility Determination	$923,900
Total	Up to $15,838,100 (total funds)

Table 4. AHCCCS Analysis of DRA Premiums and Copayments. See Table 3 for the breakdown of up to $15.8 million in administrative expenses. Also, note that AHCCCS must reimburse the federal government for the federal share (66.47%) of premiums and cost sharing.

	TMA		SOBRA	HCBS EPD		HCBS DD	
	100% - 150% FPL	**Over 150% FPL**	**Over 100% FPL**	**100% - 150% FPL**	**Over 150% FPL**	**100% - 150% FPL**	**Over 150% FPL**
	11,717 households	4,837 households	19,919 children	5,153 households	1,654 households	1,059 households	149 households
Maximum Premium							
Amount Per Household Per Month	$0.00	$119.97	$0.00	$0.00	$83.62	$0.00	$136.12
Amount Per Household Per Year (PHPY)	$0.00	$1,439.59	$0.00	$0.00	$1,003.41	$0.00	$1,633.39
Total Collected for Premiums	$0.00	$6,962,656	$0.00	$0.00	$1,659,369	$0.00	$243,309
Maximum Copayment for Prescriptions							
Amount PHPY	$23.64	$68.33	$7.20	$27.36	$113.97	$32.40	$122.95
Total Collected for Prescriptions	$276,996	$330,462	$143,417	$140,977	$188,483	$34,303	$18,315
Maximum Copayments for Services							
Inpatient amount PHPY	$58.47	$116.95	$0.00	$277.11	$554.22	$62.26	$124.51
Outpatient amount PHPY	$25.09	$50.18	$0.00	$113.23	$226.47	$11.65	$23.30
Physician amount PHPY	$54.12	$108.23	$0.00	$72.02	$144.04	$27.90	$55.81
Maximum PHPY for Services	$137.68	$275.36	$0.00	$462.36	$924.73	$101.81	$203.62
Maximum Copayments Prescriptions and Services							
Maximum Per Household Per Year for Prescriptions and Services	$161.32	$343.69	$7.20	$489.72	$1,003.41	$134.21	$326.57
Total Collected for Drugs and Services	$1,890,235	$1,662,255	$143,417	$2,523,366	$1,659,369	$142,092	$48,646

DRA BENCHMARK BENEFIT PLANS

DRA BENCHMARK BENEFITS

Social Security Act § 1937, established by DRA § 6044, permits states to create mandatory "benchmark" benefit plans for certain healthy children and adults. Below are requirements for DRA benchmark benefit plans.

1. Type of benefits. Instead of the traditional Medicaid benefits, a state may require certain Medicaid members to enroll in alternative benchmark benefit plans or employer-sponsored insurance that meets the benchmark standard.
2. Most AHCCCS members ineligible. A state may not make participation in a benchmark benefit plan mandatory for ALTCS members, pregnant women, Medicare eligible members, many low-income families, members receiving only family planning or emergency services, spend-down populations, women in the cancer treatment program, and most elderly, disabled or other special needs members. Some excluded members may be eligible to participate on a voluntary basis.
3. Benchmarks. Each of the following is considered a benchmark.
    - The standard Blue Cross/Blue Shield benefit plan offered under the Federal Employees Health Benefit Program.
    - The state employee benefit plan.
    - The benefit plan offered by the largest commercial, non-Medicaid, health maintenance organization in the state.
    - Any other appropriate benefit plan approved by CMS.

    In the alternative, a state may develop a benefit plan that is substantially actuarially equivalent to one of the benchmarks.
4. EPSDT wrap-around for children under 19. While some children may be enrolled in mandatory benchmark benefit plans, the state must provide a wrap-around benefit of full services available under the Early Periodic Screening, Diagnosis, and Treatment (EPSDT) program. EPSDT includes any Medicaid covered service, whether or not the service is covered under Arizona's Medicaid State Plan.
5. Variable benefits. The state may vary benefits based on member characteristics or geographic location.
6. Cost sharing. The DRA cost sharing limits under § 1916A continue to apply to members enrolled in benchmark plans.

DRA BENCHMARK BENEFIT ANALYSIS

Benchmark selected. AHCCCS developed this analysis using the state employee benefit plan as the benchmark. This selection is appropriate because, at times, the AHCCCS benefit plan has been viewed as more extensive than the state employee benefit plan. In addition, data and benefits associated with the state employee benefit plan are readily available, ensuring a more accurate, comprehensive, efficient, and timely comparison.

Actuarial analysis. AHCCCS contracted with Milliman USA to determine the actuarial value of the AHCCCS benefit plan compared to the state employee benefit plan. Milliman analyzed both benefit packages and, using the current AHCCCS per member per month rate (PMPM; the monthly amount paid to contractors for each enrolled member), determined the change in the

PMPM if AHCCCS provided the state employee benefit plan instead of the AHCCCS benefit plan.

Two AHCCCS benefits are more generous than the benefits available to state employees: prescription lenses for children and non-emergency transportation. Several state employee benefits are more generous than those available to AHCCCS members, particularly notable are chiropractic and infertility treatment. The change in the PMPM resulting from these differences is represented in Table 5.

Table 5. Actuarial comparison of state employee and AHCCCS plans, excluding EPSDT. The average PMPM is $250.

Change in AHCCCS PMPM if State Employee Benefits Replace AHCCCS Benefits			
Benefit	Range of Impact		EPSDT
	Low	High	
Chiropractic	+ $0.25	+ $0.30	X
Hearing Aids	+ $0.05	+ $0.08	X
Infertility Treatment	+ $0.13	+ $0.26	
Mammography	+ $0.09	+ $0.13	
Occupational/Speech Therapy	+ $0.08	+ $0.13	X
Orthotics	+ $0.00	+ $0.04	X
Prescription Lenses for Children	– $0.27	– $0.35	X
Non-Emergency Transportation	– $0.45	– $0.60	X
Total	– $0.12	– $0.01	

AHCCCS did not quantify the impact of the DRA requirement to provide EPSDT wrap-around benefits for children under 19. EPSDT requires AHCCCS to provide prescription lenses and non-emergency transportation for children, even if they are not provided under the benchmark plan. Therefore, AHCCCS could not save the $0.27 to $0.35 PMPM for prescription lenses and a portion of the PMPM for non-emergency transportation. Likewise, a small portion of the increased cost associated with such services as chiropractic, hearing aids, and occupational and speech therapy would not be incurred because these services are currently available to AHCCCS children as required by EPSDT. With these adjustments, it appears that implementing the state employee health plan would result in a modest increase in the PMPM for children.

Milliman also analyzed AHCCCS behavioral health benefits. Because respite services (short term care of a member to provide necessary relief for the member's caregiver) and residential mental health benefits are not included in the state employee benefit plan, the AHCCCS plan costs $6.46 PMPM more than the state employee benefit plan. As discussed above, AHCCCS would not realize the full savings under the benchmark plan because EPSDT requires the state to cover these services for children. Due to its role as a safety net for individuals with a range of disabling and chronic conditions, AHCCCS likely covers a disproportionate number of individuals with chronic behavioral health conditions requiring more intensive treatment when compared to workers enrolled in the state employee benefit plan. Without these services, the health of members with chronic behavioral health conditions may deteriorate, leading to greater costs for the state.

It must be noted that changes to AHCCCS behavioral health services are subject to court oversight as a result of two pending suits, *Arnold v. Sarn* and *J.K v. Eden*. This oversight could impact the state's ability to eliminate these services for some populations.

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STATE MEDICAID EXPANSIONS AND MORTALITY, REVISITED: A COST-BENEFIT ANALYSIS

BENJAMIN D. SOMMERS

ABSTRACT

Previous research found that Medicaid expansions in New York, Arizona, and Maine in the early 2000s reduced mortality. I revisit this question with improved data and methods, exploring distinct causes of death and presenting a cost-benefit analysis. Differences-in-differences analysis using a propensity score control group shows that all-cause mortality declined by 6 percent, with the most robust reductions for health-care amenable causes. HIV-related mortality (affected by the recent introduction of antiretrovirals) accounted for 20 percent of the effect. Mortality changes were closely linked to county-level coverage gains, with one life saved annually for every 239 to 316 adults gaining insurance. The results imply a cost per life saved ranging from $327,000 to $867,000 which compares favorably with most estimates of the value of a statistical life.

KEYWORDS: Medicaid, health insurance, mortality
JEL CLASSIFICATION: I13, I18, I38, D31

I. Introduction

The major expansion of Medicaid eligibility under the Affordable Care Act (ACA)—and the subsequent flexibility granted to states by the Supreme Court to opt out—has prompted a renewed debate about the value of Medicaid coverage. While evidence from the Oregon Health Insurance Experiment (OHIE) and other recent studies have demonstrated the program's value in reducing financial risk to beneficiaries and increasing access to health care services (Baicker et al. 2013; DeLeire et al. 2013; Finkelstein et al. 2012; Sommers and Oellerich 2013), the literature is more ambiguous about the impact of Medicaid on health. This, in turn, has fueled significant debate about the relative economic merits of spending billions of additional public dollars on the program. With nearly 20 states still debating whether to expand Medicaid under the ACA, the relevance of these issues is clear.

In the landmark OHIE, which studied the effects of a lottery that randomly offered approximately 10,000 low-income adults the opportunity to enroll in Oregon's Medicaid program, acquiring Medicaid led to significant improvements in self-reported health and mental health, but no statistically significant changes in several physiologic measures. Meanwhile, in a much larger but nonrandomized analysis of three state Medicaid

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expansions in the early 2000s, Sommers, Baicker, and Epstein (2012) used a differences-in-differences framework and found improved self-reported health and a significant decline in population-level mortality over a five-year follow-up period for nonelderly adults, compared with four neighboring states without expansions. The OHIE results—along with several methodological concerns about the Sommers et al. paper—have left some analysts to conclude that Medicaid offers no health benefits to beneficiaries (Roy 2013).

Here, I revisit the Medicaid expansions in three states—New York, Arizona, and Maine—which extended Medicaid eligibility to a similar population of low-income adults as the OHIE and ACA expansions,[1] with several important methodological improvements. Using restricted access microdata (1997–2007) from the Centers for Disease Control and Prevention (CDC), which captures detailed information on every death occurring in the United States each year, I develop a more ideal control group for these expansion states, matching counties in those states based on pre-expansion mortality trends, as well as county-level demographic and economic indicators. I also use the more detailed data to explore specific causes of death, including health-care amenable causes of death—which previous research suggests may be more responsive to better access to medical care (Nolte and McKee 2003; Sommers, Long, and Baicker 2014)—and deaths related to HIV, since the introduction of highly aggressive antiretroviral therapy for HIV in the late 1990s led to well-documented declines in mortality that partially coincided with the Medicaid expansions in these states. Finally, I link these mortality data to county-level changes in the uninsured rate from before and after the Medicaid expansion, to test for the marginal impact of the coverage expansion and conduct a cost-benefit analysis of the mortality changes.

The primary findings are threefold. First, using more detailed data and a stronger identification strategy, I find robust evidence of an impact of Medicaid expansions on mortality similar to previous research. After Medicaid expansions in these three states, the population death rate for 20- to 64-year-old adults declined by 6 percent, or roughly 20 deaths per 100,000 when compared with adults living in demographically and economically similar counties in non-expanding states.

Second, mortality changes varied by cause of death. Declines in mortality were most robust for deaths from so-called health-care amenable causes, which declined by 6.7 percent, while deaths from other causes showed a smaller decline. While results for New York (the largest state with the largest proportional expansion) drove the all-cause mortality declines, analyses excluding New York suggest that health-care amenable mortality may have declined after expansions in Maine and Arizona as well. Meanwhile, HIV-related mortality was declining quite rapidly during this period, and HIV was much more prevalent at baseline in expansion states than in the control group. HIV-related mortality declines accounted for 20 percent of the overall mortality effect in this study. However, the relative

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[1] New York expanded eligibility to childless adults with incomes up to 100 percent of the federal poverty level and parents with incomes up to 150 percent of the federal poverty level in September 2001. Arizona expanded eligibility to childless adults with incomes below 100 percent of the federal poverty level in November 2001 and to parents with incomes up to 200 percent of the federal poverty level in October 2002. Maine expanded eligibility to childless adults with incomes up to 100 percent of the federal poverty level in October 2002.

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decline in HIV-related mortality was nearly twice as large in Medicaid-expanding states as in non-expanding states, suggesting that expanded insurance worked in tandem with new antiretrovirals to produce larger health impacts.

Third, mortality changes were closely linked to county-level changes in insurance coverage, supporting the conclusion that expansion of health insurance rather than other contemporaneous changes were driving the changing death rates. A differences-in-differences-in-differences (DDD) model using elderly adults produced similar findings, further supporting the expansion of insurance coverage to working age adults as the most likely causal mechanism. Point estimates suggest that for every 239 to 316 adults gaining health insurance, one death was prevented each year. Using data from the OHIE of increased per-person health-care spending from acquiring Medicaid, this suggests a societal cost of $327,000 to $867,000 per life saved (in 2007 dollars), depending on assumptions about the deadweight loss of public financing.

The paper is organized as follows. Section II reviews the literature on the relationship between mortality and health insurance in general, and the impact of Medicaid in particular. Section III describes the data and methods. Section IV presents the results for all-cause mortality and analyses by cause of death. Section V presents additional specifications, including a DDD model with elderly adults and a model linking changes in mortality with county-level reductions in the uninsured rate. Section VI presents a cost-benefit analysis and concludes.

II. Previous Literature

There is a lack of consensus in the literature about the impact of health insurance on mortality. Studies of Medicare among the elderly have found mixed results: analysis of the 1965 introduction of Medicare did not find any mortality change (Finkelstein and McKnight 2008), while a regression discontinuity analysis of more recent data detected a reduction in in-hospital deaths at age 65 (Card, Dobkin, and Maestas 2009). Longitudinal analyses of private insurance have found large unadjusted differences in survival between insured and uninsured adults, but studies differ as to whether adjustment for underlying health differences fully explain the survival gap (Black et al. 2013; Kronick 2009) or still leave a residual mortality benefit of insurance (Wilper et al. 2009). Most recently, an analysis of Massachusetts' 2006 health reform found a significant reduction in mortality—particularly for health-care amenable causes of death—compared with a propensity score–defined control group (Sommers, Long, and Baicker 2014).

With regard to Medicaid more specifically, a large body of research demonstrates that the program improves access to care and financial risk protection, compared with being uninsured. These studies include cross-sectional analyses using state eligibility as an instrument for Medicaid coverage (Long, Coughlin, and King 2005), differences-in-differences analyses of Medicaid eligibility expansions (Currie and Gruber 1996a, 1996b), and the recent randomized trial in Oregon (Finkelstein et al. 2012). However, evidence on Medicaid's impact on health and mortality has been mixed. Currie and Gruber (1996a, 1996b) showed that expansions of Medicaid eligibility to pregnant women and children in the 1980s led to reductions in infant mortality and child mortality, though others (Epstein

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and Newhouse 1998; Howell 2001) found little or no effect. In another recent paper, the initial implementation of Medicaid in the late 1960s was linked to a 40 percent individual-level reduction in infant and child mortality for nonwhites (Goodman-Bacon 2013).

Meanwhile, among adults a potentially more definitive study exists because of Oregon's fortuitous use of a lottery to randomly select low-income adults from a waitlist for the chance to enroll in Medicaid. The OHIE showed that gaining Medicaid led to significant and rapid improvements in self-reported health, major reductions in depression, and increased use of recommended services such as cancer screening and medication for diabetes. But over a mean follow-up of 18 months, there were no statistically significant changes in blood pressure, cholesterol, or glycated hemoglobin (Baicker et al. 2013; Finkelstein et al. 2012). However, these latter three measures are far from the only pathway through which health care might reduce mortality, and some have argued that OHIE was underpowered to detect changes in these measures given the relative small number of participants who had elevated blood pressure, cholesterol, or glucose at the outset (Frakt 2013). While OHIE did examine mortality at one year, with no significant changes detected (and a point estimate corresponding to a 16 percent relative mortality reduction), the confidence interval was extremely wide and could not rule out very large individual-level mortality changes, with a 95 percent confidence interval ranging from −82 percent to +50 percent.[2]

In the absence of any randomized trial of adequate size to settle this question, the largest quasi-experimental analysis of Medicaid's impact on adult mortality comes from Sommers, Baicker, and Epstein (2012). Using population-level vital statistics and survey data, the researchers showed that large state Medicaid expansions in 2001 and 2002 in New York, Arizona, and Maine led to a 3.2 percentage point decrease in the uninsured rate, gains in access to care and self-reported health, and a mortality decline of 20 deaths per 100,000 nonelderly adults, relative to adults in neighboring states without expansions. However, there are several potential threats to the study's identification strategy. First, different demographic features and pre-expansion trends between treatment and control states may have biased the results (Kaestner 2012). Second, the results were largely driven by New York, which has several unique features that distinguish it from other states in the original sample. For instance, New York experienced a major increase in mortality in 2001 because of the terrorist attacks of September 11. Third, the Medicaid expansions—implemented in 2001 and 2002—were concurrent with the ongoing diffusion of antiretroviral medications, which led to a dramatic decline in HIV-related mortality beginning in 1996 (Duggan and Evans 2008; Palella et al. 2006).[3] Combined with New York's high

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[2] Finkelstein et al. (2012) used an instrumental variables analysis of the lottery for Medicaid eligibility to identify the local average treatment effect (LATE) of acquiring Medicaid. They measured a 0.0013 percentage point decrease in the likelihood of death compared with the control group, with a 95 percent confidence interval of [−0.0066, 0.0040], from a baseline mortality rate of 0.008. Thus, their estimates imply a confidence interval for an individual-level mortality change of −82 percent to +50 percent.

[3] National age-adjusted HIV-related mortality for nonelderly adults peaked at 23.8 per 100,000 in 1995 and had declined to 8.8 by 1997 and 5.3 by 2007, according to CDC official statistics (https://wonder.cdc.gov).

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HIV rates, this raises the possibility that new medications rather than insurance may have caused the observed mortality decline. Finally, the authors' county-level mortality analysis was subject to the ecological fallacy and did not include information on insurance coverage at the individual or even county level.

Here, I explore the impact of these three states' large Medicaid expansions with an improved data source and special attention to the main threats to validity from the original analysis: the comparability of pre-expansion trends and control state demographic features, the potential biases of September 11 and HIV antiretroviral therapy, and the lack of a direct link between insurance coverage and the observed mortality reductions. Finally, I use the estimates of deaths prevented per person insured to present a cost-benefit analysis of these state Medicaid expansions.

III. Data and Methods

A. DATA

The primary data come from national vital statistics collected by the Centers for Disease Control and Prevention (CDC). The analysis by Sommers, Baicker, and Epstein (2012) used the publicly available Compressed Mortality File, which reports annual mortality rates by county age-race-sex cells. However, in the public use file, the CDC suppressed all cells with death counts between one and five, and Sommers et al. had to impute these suppressed values for 5 percent of the weighted sample. These suppression rules and imputation may have introduced bias and precluded more detailed analyses by causes of death.

To address these limitations, I obtained access to the complete US Vital Statistics data set. This data set is available only via direct application to the CDC,[4] and contains the following information on every US death occurring each year: age group, sex, race, cause of death, and county of residence.[5] The CDC pairs this information with the corresponding population denominator by county, age, sex, and race (from Census data) to allow for the calculation of an annual death rate. Causes of death are based on the International Classification of Diseases, versions 9 (1997–98) and 10 (1999–2007).

Since differential changes in the economy in expansion versus control states may threaten the identification strategy, the mortality data were then merged with several county-year-specific economic indicators reported in the Area Resource File.[6] These measures were the unemployment rate (from the Bureau of Labor Statistics' Local Area

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[4] CDC data use policies prohibit the author from directly sharing this data set, but the application for data access is available at https://naphsis-public.sharepoint.com/programs/vital-statistics-data-research-request-process.

[5] Location of death is based on the individual's residence. If a person dies in a county other than where they live, his/her death is counted in mortality statistics for their home county.

[6] Interestingly, it is unclear what direction the potential bias would be from differential economic changes. While numerous studies document a major detrimental effect of lower socioeconomic status on survival, others have found that mortality decreases during recessions (Ruhm 2000, 2005). More recent research suggests that adjusting for unemployment as well as poverty rates and median income may be necessary to fully capture macroeconomic influences on mortality (Gordon and Sommers 2016).

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Unemployment Statistics) and median income and poverty rates (from the Census Bureau's Small Area Income and Poverty Estimates). In addition, I obtained county-level measures of the uninsured rate among the 19- to 64-year-old age group from the Census Bureau's Small Area Health Insurance Estimates.

B. METHODS AND CONTROL GROUP

The study design was a differences-in-differences analysis comparing annual mortality in county race-sex-age group cells in three expansion states—New York, Arizona, and Maine—with a control group of counties without Medicaid expansions. The control group was created by matching the subset of US counties that most resembled the counties in the expansion states based on pre-expansion mortality trends, baseline demographic features, and economic conditions. The study period was 1997–2007, with the pre-expansion baseline period defined as 1997–2000, and 2001 omitted from the sample to avoid any bias from the attacks of September 11.[7] The approach of using a propensity score–based control group of counties is similar to Sommers, Long, and Baicker's (2014) analysis of Massachusetts' health reform. It resembles the synthetic control group method (Abadie, Diamond, and Hainmueller 2015) by matching on pre-intervention trends in the outcome, but it additionally incorporates information on key covariates that are important predictors of the outcome and that differ significantly across counties. The trade-off of adding additional elements for matching is that the precision of the match for any one dimension may decrease as other factors are used in the propensity match. Thus, pre-expansion trend testing and placebo testing are still critical for assessing the identifying assumption.

The propensity score model was fit using the following county-level logistic regression for the whole United States in the pre-expansion period, weighted by each county's population size:

$$\begin{aligned} ExpansionCounty_c = {} & \beta_0 + \beta_1 UnemploymentRate_c + \beta_2 PovertyRate_c + \beta_3 MedianIncome_c \\ & + \beta_4 \boldsymbol{X_c} + \beta_5 MortalityRate97_c + \beta_6 MortalityRate98_c \\ & + \beta_7 MortalityRate99_c + \beta_8 MortalityRate00_c + \varepsilon_c \qquad (1), \end{aligned}$$

where c indexes the county. *ExpansionCounty* was a binary variable equal to one for counties in New York, Arizona, and Maine, and zero otherwise. $\boldsymbol{X_c}$ is a vector of demographic traits (percentages of the county's population in each age group, race/ethnicity, and sex), based on the population of adults ages 20–64.[8] All economic and demographic variables (including the county annual employment rate, poverty rate, and median household

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[7] The time frame differs slightly here from Sommers, Baicker, and Epstein (2012). In that paper, the sample was limited to five years before and after each state's expansion, which means that the study period was 1997–2006 for New York and Arizona, and 1998–2007 for Maine. Here, I use the full overlapping time period to be consistent across all states (1997–2007), with year fixed effects to capture any national mortality trends. The first full year of coverage after each expansion is considered the first post-expansion year.

[8] Nineteen-year-olds are classified in the mortality data as part of the 15- to 19-year-old age group, which is why the sample for working age adults for this analysis begins at age 20.

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income) were averaged over the 1997–2000 pre-expansion period. Coefficients β_5 through β_8 captured the annual mortality pattern (*MortalityRate97* for the year 1997, for instance) for this same period. Then the model was used to generate predicted values (propensity scores) for each county, indicating its similarity to the expansion counties' overall population and mortality pattern over time.

In traditional propensity score matching, these propensity scores are then used to select specific matches to serve as controls for each treatment county. However, the distinctive nature of the CDC mortality data set introduces several challenges for this approach: the counties have widely different population sizes that have to be taken into account in the final regression models to produce meaningful population estimates, and the overall number of counties in the treatment states is small—just 93 overall, which may lead to a lack of power particularly for subgroup analyses. Thus, following previous work analyzing this data set (Sommers, Long, and Baicker 2014), I use the propensity score to describe a composite synthetic control group defined as the quartile of counties (weighted by population size) with the highest propensity scores—that is, those most closely resembling the overall population of the three expansion states. This approach resulted in control counties being identified in all remaining 47 states but not the District of Columbia, yielding a final sample containing 50 state clusters. As a sensitivity analysis, I also tested more traditional propensity score matching approaches, using 5:1, 3:1, and 2:1 propensity score matches (without replacement) for each expansion county, incorporating a caliper of 0.2 times the standard deviation of the logit propensity score to confirm reasonable matches (Austin 2011). These analyses produce similar findings to the main model.

Online Appendix Table A1 (see http://www.mitpressjournals.org/doi/suppl/10.1162/ajhe_a_00080) shows the results from the logistic regression model for equation 1. The strongest county-level predictors of expansion status (as measured by the test statistic) were the age distribution (with expansion counties having an older population on average than the nation as a whole) and poverty rate (with expansion counties having a higher poverty rate on average). Other significant predictors were a higher proportion of women, fewer Latinos, and lower mortality in 1998 and 2000. Mortality rates for 1997 and 1999 are nonsignificant because of multicollinearity; in models only considering a single year mortality rate at a time, each of the four years are significant predictors ($p < 0.05$) in equation 1.

To determine whether this approach yielded a control group with similar pre-expansion mortality trends, I first tested for differential linear trends for the years 1997–2000 in the control states versus the expansion states. I then conducted a falsification test for any divergence in mortality trends in 2000 (the last full year before the first expansions began), using only the 1996–2000 pre-expansion data and treating 2000 as a placebo expansion year. The results of these analyses, reported in Section IV, support the identifying assumption of similar pre-expansion mortality trends. Equation 2 describes the linear trend testing, and equation 3 describes the placebo test, both of which were analyzed using the pre-expansion data (1997–2000) and weighted by population size:

$$\begin{aligned} MortalityRate_{ijkct} = {} & \beta_0 + \beta_1 \boldsymbol{X_{ijk}} + \beta_2 \boldsymbol{County\text{-}Level\ Factors_{ct}} + \beta_3 Time\ Trend_t \\ & + \beta_4 Expansion\ State_s \times Time\ Trend_t + \Omega\ County_c + \varepsilon_{ijkct} \quad (2), \end{aligned}$$

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where *i* indexed age, *j* race, *k* sex, *c* county, *s* state, and *t* year. The dependent variable is the mortality rate per 100,000 adults in each age-race-sex-county-year observation. $\boldsymbol{X_{ijk}}$ was a vector of demographic traits (age group, race, and sex). ***County-Level Factors*** included county-year-specific poverty rate, median income (in 2007 inflation-adjusted dollars), unemployment rate, and percentage of the population that is Latino.[9] Ω is a vector of county fixed effects. β_3 is a linear time trend, and β_4 measures any differential time trend for the expansion states, compared with the control group.

$$\begin{aligned} \mathit{MortalityRate}_{ijkct} = {} & \beta_0 + \beta_1 \boldsymbol{X_{ijk}} + \beta_2 \boldsymbol{\mathit{County\text{-}Level\ Factors}_{ct}} + \beta_3 \mathit{Expansion\ State}_s \\ & \times \mathit{Yr2000}_{st} + \mu\, \mathit{Year}_t + \Omega\ \mathit{County}_c + \varepsilon_{ijkct} \qquad (3). \end{aligned}$$

Equation 3, which represents the placebo test for a differential mortality effect in the last pre-expansion year, replaces the linear time trend with μ, a vector of year fixed effects. Then β_3 captures the effect of living in an expansion state in the year 2000, the last year before any of the state Medicaid expansions. The remaining terms are defined as in equation 2. The regression used Huber-White robust standard errors clustered at the state level ($n = 50$).

C. ANALYSES OF ALL-CAUSE MORTALITY

The primary outcome was all-cause mortality, expressed as deaths per 100,000 adults. The unit of analysis was the year-specific age-sex-race cell within each county. Equation 4 shows the primary regression model:

$$\begin{aligned} \mathit{MortalityRate}_{ijkct} = {} & \beta_0 + \beta_1 \boldsymbol{X_{ijk}} + \beta_2 \boldsymbol{\mathit{County\text{-}Level\ Factors}_{ct}} + \beta_3 \mathit{Expansion\ State}_s \\ & \times \mathit{Post\text{-}expansion}_t + \mu\ \mathit{Year}_t + \Omega\ \mathit{County}_c + \varepsilon_{ijkct} \qquad (4). \end{aligned}$$

As above, *i* indexed age, *j* race, *k* sex, *c* county, *s* state, and *t* year. The dependent variable is mortality per 100,000 adults in each age-race-sex-county-year observation. $\boldsymbol{X_{ijk}}$ was a vector of demographic traits (age group, race, and sex). ***County-Level Factors*** included county-year-specific poverty rate, median income (in 2007 dollars), unemployment rate, and Latino percentage of the population. β_3 is the coefficient of interest, capturing the effect of being in an expansion state (*Expansion State* = 1) after Medicaid expansion (*Post-expansion* = 1). The direct effects of the post-expansion period and being in an expansion state are respectively captured by μ, a vector of year fixed effects, and Ω, a vector of county fixed effects. Robust standard errors were clustered by state ($n = 50$) (Bertrand, Duflo, and Mullainathan 2004).

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[9] The CDC did not include ethnicity in the mortality data until 1999, and some death certificates after 1999 were still missing this information. I use the year-specific county-wide percentage of the population that is Latino, from the Area Resource File, to allow for the full sample to be included in the analysis. Thus, while race is directly adjusted for at the individual level as part of vector $\boldsymbol{X_{ijk}}$, Latino ethnicity is adjusted for at the county level.

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The primary regressions used linear models, but I also tested the robustness of the results using a generalized linear model (GLM) with a negative binomial distribution and log-link, which reports relative changes in mortality. The GLM regression equation was the same as the linear model, except the dependent variable was the number of deaths, with population size as the exposure variable. The GLM model also replaced county fixed effects with state fixed effects because negative binomial models do not perform well with numerous fixed effects.[10]

D. ANALYSES BY CAUSES OF DEATH

I conducted two additional sets of analyses, analogous to the model in equation 4, but with different disease-specific mortality rates. First, following previous work used in international comparisons of mortality (Nolte and McKee 2003, 2008) and in the analysis of Massachusetts' health reform (Sommers, Long, and Baicker 2014), I identified deaths due to so-called health-care amenable causes, including cardiovascular disease, infections, cancer, diabetes, kidney disease, and other conditions thought to be more responsive to timely medical care. The notion of health-care amenable causes of death has been widely used to assess health system performance and has been embraced by the World Health Organization (2000) as a measure of health-care quality. Major causes excluded from this definition include accidental deaths, suicides, and homicides. There has been suggestive evidence that insurance status can reduce mortality even from some of the latter conditions; for instance, in-hospital mortality may be as much as 40 percent higher for trauma victims without health insurance compared with insured patients (Doyle 2005). However, the majority of trauma deaths occur before hospital admission, as is the case for homicides, suggesting that health insurance has limited ability to impact these conditions at the population level compared with other causes listed above.[11] Meanwhile, the OHIE showed that the acquisition of Medicaid significantly reduces depression rates (Baicker et al. 2013), raising the possibility that coverage might also reduce suicide rates. While some trials of specific cognitive interventions have been shown to reduce recurrent suicide attempts (Brown et al. 2005), the most common medical intervention for depression (antidepressant use) has not been shown to reduce suicides in randomized trials (Fergusson et al. 2005), and the overall evidence base is mixed as to whether medical care reduces the risk of self-harm (Hawton et al. 2000). Thus, it is reasonable to assume that any mortality effects mediated by insurance coverage should be larger for health-care amenable causes than for these other causes of death.

A second set of analyses examined deaths due to HIV and AIDS, and conversely analyzed mortality from all causes *other* than HIV. As discussed earlier, the introduction of highly aggressive antiretroviral therapy (HAART) in the late 1990s and early 2000s led to major reductions in HIV-related mortality (Palella et al. 2006), and this trend was

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[10] See additional discussion of this issue in Greene (2005, 113–218).

[11] According to my analysis of CDC mortality data of nonelderly adults from 1999 to 2014 (available at http://wonder.cdc.gov/ucd-icd10.html), fewer than 20 percent of transportation-related fatalities occur while receiving hospital care; the majority die before arrival to the hospital, and another 19 percent die in the emergency department. Similarly, for homicides, only 17 percent occur during a hospital inpatient stay.

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concurrent with these Medicaid expansions. Separating out this cause of death and looking at the relative declines in mortality between expansion and control states can shed light on the extent to which the all-cause mortality changes are attributable to HIV alone, and also whether Medicaid expansion facilitated the mortality reductions due to HAART among those with HIV.

IV. Results

A. DESCRIPTIVE STATISTICS AND CONTROL GROUP

Table 1 summarizes the baseline characteristics for the three expansion states compared with (1) the propensity-defined control group; (2) the four neighboring states used as the original control group from Sommers, Baicker, and Epstein (2012) (Pennsylvania, Nevada, New Mexico, and New Hampshire); and (3) the full US sample outside of the expansion states. Given the large numbers of counties, many of these differences are statistically significant, but here I focus on the absolute magnitude of those differences and the standardized differences of the mean as more relevant measures of whether the groups are reasonably comparable.[12]

Overall, standardized differences between the expansion states and propensity score group (ranging from 0.01 to 0.28) were smaller than the differences between the expansion states and the neighboring states (ranging between 0.23 and 0.68) for all but one of the 14 estimates. Thus, for nearly all measures, the propensity score control group resembled the expansion states more closely than did the neighboring state control group that had been used in previous research. While these differences were smaller for the propensity score control group, several standardized differences still exceeded the threshold of 10 percent that is one commonly used goal for propensity score analyses (Austin 2011). However, note that while I use propensity scores here to improve the comparability of the control group, the study's causal identification still comes from differences-in-differences analysis, for which there is no precondition of comparability of covariates. Rather, the identifying assumption is still that in the absence of the Medicaid expansion, mortality trends would have been similar in the expansion and control states. The following section presents results of the tests for pre-expansion mortality trends.

B. PRE-EXPANSION TRENDS AND PLACEBO TESTS

Figure 1 depicts mortality changes in the Medicaid expansion states and the propensity score control group (hereafter referred to simply as the control group) for all-cause mortality, deaths due to health-care amenable causes, and deaths from other causes. The pre-expansion mortality curves followed similar trajectories prior to 2001, when the expansions began. In the post-expansion period, mortality in the expansion states fell while it rose slightly in control states. Health-care amenable deaths show a post-2001 decline

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[12] For instance, the percentage of adults in the 35–44 age group was 27.1 percent in the expansion states and 27.5 percent in the propensity score control group, $p = 0.03$, but in the context of a differences-in-differences analysis which also adjusts directly for age, this 0.4 percentage point difference is highly unlikely to bias the results.

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TABLE 1. Baseline features (1997–2000) of Medicaid expansion states, alternative control groups, and all non-expansion states

Variable	Expansion states	PS control	Neighboring control	Rest of US	Standardized difference, expansion vs. PS control	Standardized difference, expansion vs. neighboring control
Age 20–24	11.0%	10.6%	10.5%	11.3%	0.18	0.23
Age 25–34	25.3%	25.3%	23.3%	24.9%	0.01	0.68
Age 35–44	27.1%	27.5%	27.8%	27.5%	−0.22	−0.41
Age 45–54	21.9%	22.0%	23.0%	22.0%	−0.06	−0.60
Age 55–64	14.7%	14.6%	15.4%	14.2%	0.02	−0.43
Male	49.0%	49.2%	49.5%	49.7%	−0.20	−0.30
White	79.2%	81.2%	88.3%	82.7%	−0.14	−0.57
Black	14.0%	10.7%	8.1%	12.2%	0.26	0.46
Other race	6.9%	8.0%	3.6%	5.1%	−0.12	0.49
Latino ethnicity	15.9%	15.7%	8.3%	11.1%	0.01	0.56
Poverty rate	14.2%	12.9%	10.8%	12.0%	0.20	0.58
Median household income	$53,866	$57,880	$53,629	$55,150	−0.24	0.02
Unemployment rate	5.3%	4.8%	4.6%	4.4%	0.20	0.29
Mortality (per 100,000)	318	297	343	330	0.28	−0.35
Number of counties	93	907	127	3,047	1,000	1,000
Number of states	3	47	4	48	50	50

Notes: Medicaid expansion states were New York, Arizona, and Maine. PS control is the propensity score–defined control group. Neighboring control is the original four-state control group (Pennsylvania, Nevada, New Mexico, and New Hampshire) used in Sommers, Baicker, and Epstein (2012), which were demographically similar states neighboring the expansion states. Sample contains adults ages 20–64. "Rest of US" includes 47 states and the District of Columbia.

in expansion states, but not in control states. For non-amenable (or, to be more accurate, "less amenable") causes of death, mortality rose slightly in control states after 2001 and was relatively flat in the expansion states.[13]

Table 2 presents the regression results for tests of parallel trends and the placebo test for the year 2000, comparing expansion states and the control group. In the linear trend analysis, the coefficient on *Expansion State* × *Time Trend* was statistically nonsignificant and close to zero (0.69 per 100,000 per year, $p = 0.56$). In the placebo differences-in-differences

[13] The impact of the terrorist attacks of September 11 are visible in the non-amenable causes of death, with a slight uptick in this outcome for expansion states in 2001, before mortality returns to its 2000 level the following year.

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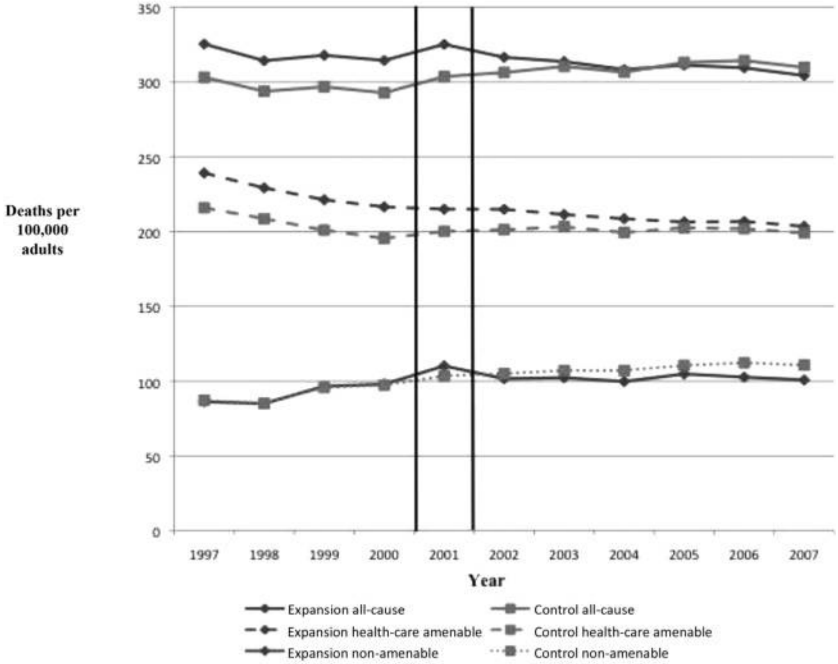
FIGURE 1. Mortality rates (per 100,000) for adults ages 20–64 in Medicaid expansion and control states, 1997–2007



Notes: Vertical black lines indicate the beginning of the Medicaid expansions in New York and Arizona (2001) and Maine (2002). "Non-amenable" causes of death are those not included in the category "health-care amenable." However, this does not mean they cannot be impacted by health insurance, only that these death rates are likely to be *less* responsive to insurance coverage than other causes.

TABLE 2. Tests of pre-expansion trends and placebo tests for all-cause mortality, 1997–2000

Specification	Linear model (deaths per 100,000 per year)	Negative binomial GLM (relative risk of mortality per year)
Linear trend (equation 2):	0.69	−0.0064
Expansion State × Time Trend	(1.18)	(0.0063)
Placebo test (equation 3):	1.80	−0.0096
Expansion State × Year2000	(2.21)	(0.0145)

Notes: $N = 106{,}274$ annual county age-race-sex cells. All coefficients nonsignificant at $p > 0.10$. Sample contains adults ages 20–64.

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model, the coefficient on *Expansion State* × *Year2000* was also small and nonsignificant (1.80 per 100,000, $p = 0.42$). Thus, both the time trend and the placebo analyses show that the propensity score approach yielded a good match in baseline trend, consistent with the visual evidence in Figure 1. The negative binomial GLM model produced similarly reassuring null results. While these results should not be surprising, given that the control group was selected in part based on the pre-expansion mortality rates, it is nonetheless useful to verify this as a necessary condition for the study's identification strategy.

C. CHANGES IN ALL-CAUSE MORTALITY

Table 3 presents the differences-in-differences results for all-cause mortality, in linear and negative binomial GLM models. In both models, Medicaid expansions led to a significant decline in all-cause mortality, equivalent to 19.1 fewer deaths per 100,000 in the linear model and a relative decline of 6.0 percent in the negative binomial model. These two results are nearly identical in comparison to the baseline mortality rate of 318 per 100,000. Other significant predictors of mortality were older age, black race, male sex, and higher poverty/lower median income.

D. ANALYSES BY CASE OF DEATH AND HIV

Table 4 presents differences-in-differences estimates for several types of mortality based on cause of death. Mortality reductions were significant ($p \leq 0.01$) for health-care amenable deaths, with a linear estimate of -12.0 per 100,000 and the negative binomial model showing a 6.7 percent reduction after the Medicaid expansion. Deaths from causes less amenable to health care also declined, though the effects were not significant at $p \leq 0.05$ ($p = 0.068$ in the linear model, $p = 0.063$ in GLM). While generally supportive of the hypothesis that health-care amenable diagnoses should be more highly impacted than other causes of death, the estimates for non-amenable causes are fairly imprecise and contain 95 percent confidence intervals extending well above and below the 6.0 percent relative decline in all-cause mortality.

Figure 2 plots HIV-specific mortality during the study period for the expansion states and the control group. HIV-related mortality in the expansion states was nearly threefold higher than in the control states in the baseline period, and was already declining in both treatment and control states prior to the Medicaid expansions. However, the decline in control states was gradual after 2002, while the decline continued to accelerate in the expansion states, markedly narrowing the gap compared with the control states.

The regression results in Table 4 show that HIV-related deaths were indeed declining more rapidly in the treatment states after Medicaid expansion, by 3.8 deaths per 100,000 in the linear model. However, the linear model is biased by the difference in absolute risk of HIV at baseline, and pre-expansion trends show a significant divergence in HIV mortality (coefficient on *Expansion State* × *Time Trend* $= -0.9$, $p < 0.001$). This bias in the absolute HIV mortality reduction does not require that there was any faster diffusion of antiretrovirals in expansion states than in non-expansion states; the large difference in baseline prevalence of HIV alone creates this bias in the linear differences-in-differences model, even if the relative mortality reduction from these treatments was identical across states. The linear estimate in Table 4 suggests that roughly 20 percent (3.8/19.1) of the all-cause

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TABLE 3. Effect of state Medicaid expansions on all-cause mortality

Model and variable	Linear model (deaths per 100,000)	Negative binomial GLM (relative risk of mortality)
Expansion State × Post-Expansion	-19.1^{a}	-0.060^{a}
	(6.3)	(0.023)
Age 25–34	12.2^{a}	0.08^{a}
	(2.5)	(0.03)
Age 35–44	98.6^{a}	0.79^{a}
	(5.7)	(0.05)
Age 45–54	311.3^{a}	1.60^{a}
	(9.9)	(0.05)
Age 55–64	778.6^{a}	2.42^{a}
	(17.7)	(0.06)
Male	172.6^{a}	0.68^{a}
	(7.0)	(0.01)
White	80.8^{a}	0.53^{a}
	(18.8)	(0.10)
Black	268.9^{a}	1.10^{a}
	(22.5)	(0.10)
% Latino	37.9	-0.56^{a}
	(91.5)	(0.17)
Poverty rate	135.0^{a}	1.41^{a}
	(40.8)	(0.39)
Median household income ($10,000s)	-10.4^{a}	-0.06^{a}
	(3.7)	(0.02)
Unemployment rate	−125.3	0.92
	(93.3)	(0.63)

Notes: $N = 313{,}161$ annual county age-race-sex cells. Robust standard errors, clustered at the state level ($n = 50$), are in parentheses. All models include year fixed effects. Age, sex, and race are binary indicators (0 vs. 1). % Latino, poverty rate, and unemployment rate are expressed as fractions ranging from 0 to 1.0. Linear model also includes county fixed effects, and GLM includes state fixed effects. See equation 4 in text for full details. Sample contains adults ages 20–64. $^{a}p < 0.01$, $^{b}p < 0.05$, $^{c}p < 0.10$.

mortality decline after Medicaid expansions were attributable to HIV. Even after removing HIV deaths from the analysis, mortality from all other causes still experienced a highly significant decline in both models. The most cautious reckoning of this result is to assume that the full HIV effect was solely due to the introduction of antiretroviral medications and had nothing to do with Medicaid expansion.

However, this assumption is likely inaccurate, as insurance coverage may have an interactive effect with new medical technology—especially for expensive treatments such as antiretrovirals. While federal funding through the Ryan White Program provides access to HIV-related treatment to many low-income Americans, it does not provide

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TABLE 4. Effect of state Medicaid expansions on mortality, by cause of death

Cause of death	(1) Pre-expansion mean (deaths per 100,000)	(2) Linear model (deaths per 100,000)	(3) Negative binomial GLM (relative risk of mortality)	(4) Implied linear change from GLM, per 100,000 (column 1 × 3)
All-cause mortality	318	-19.1^{a}	-0.060^{a}	-19.1^{a}
		(6.3)	(0.023)	(7.3)
Health-care amenable mortality	227	-12.0^{a}	-0.067^{a}	-15.2^{a}
		(2.9)	(0.014)	(3.2)
Non-amenable mortality[1]	91	-7.0^{c}	-0.052^{c}	-4.7^{c}
		(3.8)	(0.028)	(2.5)
Non-HIV mortality	301	-15.3^{a}	-0.042^{b}	-12.6^{b}
		(5.5)	(0.019)	(5.7)
HIV-related mortality (standard control group)	17	-3.8^{a}	-0.136^{a}	-2.3^{a}
		(0.8)	(0.032)	(0.5)
HIV-related mortality (HIV-specific control group)[2]	17	-2.6^{b}	-0.154^{a}	-2.6^{a}
		(1.0)	(0.057)	(1.0)

Notes: $N = 313{,}161$ annual county age-race-sex cells. Linear model reports changes in deaths per 100,000. Negative binomial generalized linear model (GLM) reports relative change in mortality. Robust standard errors, clustered at the state level ($n = 50$), are in parentheses. All models include year fixed effects. Linear model also includes county fixed effects, and GLM includes state fixed effects. See equation 4 in text for full details. Sample contains adults ages 20–64. [1] "Non-amenable" causes of death are those not included in the category "health-care amenable." However, this does not mean they cannot be impacted by health insurance, only that these death rates are likely to be *less* responsive to insurance coverage than other causes. [2] "HIV-specific control group" refers to an alternative sample in which the control counties were matched to the expansion counties based on demographic features and pre-expansion HIV-specific mortality rates, rather than all-cause mortality rate (the "standard control group"). $^{a}\ p < 0.01$, $^{b}\ p < 0.05$, $^{c}\ p < 0.10$.

comprehensive insurance and is subject to block grants to states and localities that may limit its reach (Kaiser 2013). This suggests a significant potential role for Medicaid to expand access to HIV treatment, consistent with one prior instrumental variables analysis of insurance coverage in the early years of antiretroviral medications, which detected a 70–85 percent reduction in mortality for HIV-positive individuals obtaining Medicaid (Goldman et al. 2001).

For measuring whether Medicaid expansion facilitated the mortality reductions from antiretroviral therapy, the negative binomial GLM approach is preferable to the linear model, since it measures the differential *relative* change in HIV mortality and is not biased by higher baseline absolute HIV mortality in the expansion states. In the GLM model, there was no significant divergence in pre-expansion trends (-0.013, $p = 0.13$), and the differences-in-differences estimate indicates a 13.6 percent relative reduction in HIV mortality due to the Medicaid expansion. The unadjusted mortality statistics for HIV tell a

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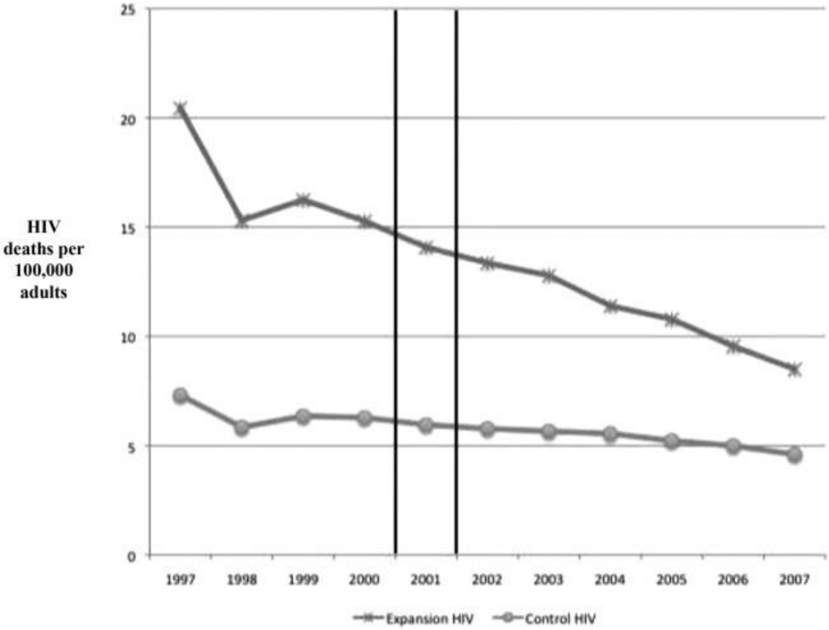
FIGURE 2. HIV mortality rates (per 100,000) for adults ages 20–64 in Medicaid expansion and control states, 1997–2007



Note: Vertical black lines indicate the beginning of the Medicaid expansions in New York and Arizona (2001) and Maine (2002).

similar story: the pre- versus post-expansion HIV death rate went down by 16 percent in the control group, while it decreased by 34 percent in expansion states. This suggests that Medicaid expansion facilitated the impact of antiretrovirals in the treatment of HIV, consistent with the findings in Goldman et al. (2001).

As an additional analysis to probe these results, I created an HIV-specific comparison group, using the same matching approach described earlier, but with a focus on HIV-related mortality in the pre-expansion period. These results are presented in the last row of Table 4, which shows a similar general pattern to the primary analysis—a roughly 15 percent relative reduction in HIV mortality due to the Medicaid expansion (or 2.6 deaths per 100,000). However, even within this HIV-specific control group, there was evidence of divergence in the pre-expansion HIV mortality trends ($-1.0, p < 0.001$). Narrowing the control group to the top decile of propensity-matched counties produced similar divergence. The difficulty in finding a comparable control group for this outcome points to how atypical the expansion states (New York in particular) were in terms of their high HIV rates.

Finally, because of the differential trends in HIV mortality in my expansion and control group, I also tested the effect of repeating the entire process of constructing the

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control group using non-HIV mortality only. These results, presented in Online Appendix Table A2, show very similar results to the primary model—with reductions in all-cause mortality of -17.2 per 100,000 ($p = 0.01$) in the linear model and -5.6 percent ($p = 0.01$) in the GLM model, and for non-HIV mortality of -13.7 per 100,000 in the linear model ($p = 0.02$) and -4.0 percent in the GLM model ($p = 0.02$). Thus, whether or not one includes HIV deaths in the matching process for the control group does not substantially change the overall findings. The use of microdata with information on specific causes of death therefore lets us confidently exclude one important threat to validity—that HIV-related changes were driving the findings for all-cause mortality after expansion.

E. ROBUSTNESS CHECKS AND OMITTING NEW YORK FROM THE SAMPLE

Online Appendix Table A2 presents several additional robustness checks. Pooling the data into a collapsed pre- and post-period to avoid any potential serial autocorrelation (Bertrand, Duflo, and Mullainathan 2004) produces nearly identical results, as does collapsing the county data into 500 state-year cells. Adding the year 2001 back into the sample also has minimal impact on the results. An interrupted time series model, in which the expansion is modeled as a change in slope in the mortality rate instead of an average change in level, shows that mortality gains grew over time: each additional post-expansion year led to a decline in the death rate of 4.3 per 100,000 ($p < 0.001$). Including additional years of data through 2010 also supports the conclusion that mortality gains grew over time.[14] Adding data from 2008–10 increases the estimated change in all-cause mortality from 19.1 per 100,000 to 22.5 per 100,000, and linear coefficients for other causes of death listed in Table 4 show similar changes that are 15–20 percent larger in magnitude than in the primary model. Using traditional "nearest neighbor" propensity score matching (5:1, 3:1, and 2:1) produces slightly smaller but highly significant declines in mortality due to the Medicaid expansions (-13.5 to -14.7 per 100,000, or -4.4 percent to -6.2 percent in GLM), compared with my baseline model that used as the control group the highest quartile of counties based on propensity scores.

State expansion effects may be heterogeneous, given that Medicaid programs vary considerably across states in many domains (Weil 2003), including provider payment rates, provider adequacy, patient cost-sharing, covered benefits, and use of managed care—which has been the exclusive delivery model used in Arizona's Medicaid program since its inception in 1982 (McCall 1997). New York's population also predominates in the full sample. Accordingly, I tested the impact of excluding New York from my sample, using a propensity-matched control group for Maine and Arizona constructed analogously to the overall control group. Online Appendix Table A3 presents these results. The estimated mortality declines in both linear and GLM models were much smaller than for the overall sample. With a more refined control group and richer data on cause of death, I find a

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[14] While data beyond 2010 are available, this crosses into the period of the implementation of the Affordable Care Act, which expanded coverage to young adults on their parents' plans beginning in September 2010, and six states began their ACA Medicaid expansions between 2011 and 2012 under the law's "early expansion" option.

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significant decline of 2.7 percent in health-care amenable deaths in these two states in the negative binomial model. However, this finding is contingent on the functional form and is not significant in the linear model. While suggestive of a potential impact of the Medicaid expansion in these two states, mortality changes in these states nonetheless appear smaller than those in New York.

Numerous policy differences exist between Medicaid programs across states. It is possible that some of these differences help explain the larger and more robust mortality changes detected in New York. For instance, New York's physician capacity, measured per 100,000 residents, far exceeded that of Arizona and Maine (385 per versus 204 and 246, respectively, as of the year 2000), and this may serve as an important facilitator of population health effects in a coverage expansion.[15] New York's Medicaid program during this period was also particularly generous, with one 50-state analysis concluding that New York's Medicaid program was the most comprehensive in the nation in terms of benefits and cost-sharing (Arellano and Wolfe 2007).

While it is possible that the New York Medicaid expansion produced larger mortality changes because of differences in program features, another likely contributor is that the much smaller populations in Maine and Arizona, combined with smaller relative expansions, limited the power to detect a population-level effect on mortality. Enrollment statistics for the Medicaid expansion programs from winter 2005 indicate that New York had enrolled over 460,000 individuals in its expansion (Artiga and Mann 2005)—approximately 4 percent of its nonelderly adult population, based on data from the 2005 Current Population Survey. By contrast, Maine had enrolled just under 24,000 and Arizona 13,000 which combined to represent just 0.9 percent of the nonelderly adult population in those two states. Thus, the expansion in New York was more than 12 times as large in absolute terms and 4 times as large in relative terms than the expansions in the other two states put together. These dramatic differences in population size and expansion impact may explain why results for the two smaller states are much more equivocal.

V. Alternative Specifications

A. DDD MODEL USING ELDERLY ADULTS AS ADDITIONAL CONTROL

One natural test of whether Medicaid expansion was in fact the underlying cause for the observed mortality decline comes from a comparison to elderly adults. Adults 65 and over are generally excluded from Medicaid eligibility expansions, and baseline coverage rates are in the range of 98–99 percent because of Medicare (DeNavas-Walt, Proctor, and Smith 2011). However, many potential state-specific mortality changes related to improvements

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[15] While New York has below-average physician participation rates in Medicaid, this disparity is far outweighed by New York's overall greater number of physicians. Sixty-two percent of New York physicians accept Medicaid, compared with 74 percent in Maine and 78.5 percent in Arizona (Decker 2013). Applying these percentages to physician capacities, New York still has 240 Medicaid-participating physicians per 100,000, compared with 160 in Arizona and 182 in Maine.

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in medical technology, the environment, or epidemiology of disease should be filtered out by the DDD approach.

The model was a standard DDD regression, described in equation 5:

$$\begin{aligned} \mathit{Deaths}_{ijklt} = {} & \beta_0 + \beta \boldsymbol{X}_{\boldsymbol{ijk}} + \beta_1 \mathit{Post\text{-}expansion}_t \times \mathit{Nonelderly}_i + \beta_2 \mathit{Nonelderly}_i \\ & \times \mathit{Expansion\ State}_s + \beta_3 \mathit{Expansion\ State}_s \times \mathit{Post\text{-}expansion}_t \\ & + \beta_4 \mathit{Expansion\ State}_s \times \mathit{Post\text{-}expansion}_t \times \mathit{Nonelderly}_i \\ & + \beta_5 \boldsymbol{\mathit{County\text{-}Level\ Factors}}_{\boldsymbol{ct}} + \mu\ \mathit{Year}_t + \Omega\ \mathit{State}_s + \varepsilon_{ijklst} \qquad (5). \end{aligned}$$

Direct effects for age are captured by the vector $\boldsymbol{X}$ (which includes age group, race, and sex), while direct effects of the post-expansion period and being in an expansion state are captured by year fixed effects (μ) and state fixed effects (Ω). County-level factors are as in equation 4. The full set of interaction terms for age, time period, and expansion state are included. β_4 is the coefficient of interest, capturing the effect of being in an expansion state after expansion for nonelderly adults, relative to adults 65 and older. Given very different baseline death rates for the elderly versus nonelderly (Online Appendix Figure A1), this analysis uses the negative binomial GLM.

Online Appendix Figure A1 shows the mortality trends for elderly adults in expansion and control states, which appear similar before and after the Medicaid expansions. Table 5 presents the regression results. All-cause mortality changed by −1.5 percent among elderly adults after Medicaid expansion in the treatment states, while the DDD estimate for nonelderly adults was a −4.8 percent decline after the expansion, with both coefficients significant at $p < 0.10$. This corresponds to an expansion effect specific to nonelderly adults 4 times larger than for elderly adults (6.3 percent versus 1.5 percent). By cause of death, the DDD estimate for health-care amenable mortality among nonelderly adults was −6.2 percent ($p < 0.001$), with no direct effect on elderly adults, and neither age group experienced a significant decline in non-amenable causes of death after expansion.

B. MORTALITY AND COUNTY-LEVEL CHANGES IN THE UNINSURED RATE

In this section, I test directly whether county-level estimates of health insurance coverage in these expansion states were tied to the observed mortality changes. This approach is similar to several studies of Massachusetts' health reform (Miller 2012; Sommers, Long, and Baicker 2014) and a recent working paper on the Affordable Care Act (Courtemanche et al. 2016), which rely on local variation in insurance coverage rates as an additional means of identifying the effect of coverage expansion. This analysis used county-level data on the percentage of adults ages 19–64 who are uninsured, from the US Census Bureau's Small Area Health Insurance Estimates (SAHIE). SAHIE estimates are based on microdata from the Current Population Survey, combined with data from the 2000 Census and administrative data from the Internal Revenue Service, the Supplemental Nutrition

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TABLE 5. DDD estimates of state Medicaid expansion effect on mortality for nonelderly adults

Variable	All-cause mortality (relative risk)	Health-care amenable mortality (relative risk)	Non-amenable mortality[1] (relative risk)
Expansion State × Post-Expansion × Nonelderly	-0.048^{c} (0.027)	-0.062^{a} (0.017)	−0.048 (0.031)
Expansion State × Post-Expansion	-0.015^{c} (0.008)	−0.009 (0.008)	−0.009 (0.022)
Nonelderly × Expansion State	0.028 (0.073)	0.050 (0.045)	0.158^{c} (0.087)
Post-Expansion × Nonelderly	0.110^{a} (0.009)	0.030^{a} (0.006)	0.086^{a} (0.019)
Age 25–34	0.080^{a} (0.03)	0.74^{a} (0.03)	-0.12^{a} (0.02)
Age 35–44	0.78^{a} (0.05)	1.94^{a} (0.04)	0.22^{a} (0.03)
Age 45–54	1.59^{a} (0.05)	3.05^{a} (0.03)	0.59^{a} (0.03)
Age 55–64	2.41^{a} (0.06)	4.03^{a} (0.03)	0.91^{a} (0.04)
Age 65–74	3.38^{a} (0.07)	4.99^{a} (0.05)	1.75^{a} (0.09)
Age 75–84	4.35^{a} (0.07)	5.91^{a} (0.05)	2.92^{a} (0.08)
Age 85 and older	5.51^{a} (0.06)	6.97^{a} (0.04)	4.35^{a} (0.09)
Male	0.62^{a} (0.01)	0.40^{a} (0.01)	0.80^{a} (0.02)
White	0.52^{a} (0.10)	0.43^{a} (0.08)	0.69^{a} (0.13)
Black	1.06^{a} (0.09)	1.10^{a} (0.06)	1.07^{a} (0.14)
% Latino	-0.52^{a} (0.14)	-0.20^{c} (0.12)	-0.85^{a} (0.19)
Poverty rate	1.25^{a} (0.36)	1.25^{a} (0.23)	1.09 (0.68)
Median household income ($10,000s)	-0.05^{a} (0.01)	-0.05^{a} (0.01)	-0.06^{a} (0.01)

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TABLE 5. *Continued*

Variable	All-cause mortality (relative risk)	Health-care amenable mortality (relative risk)	Non-amenable mortality[1] (relative risk)
Unemployment rate	0.66 (0.58)	−0.12 (0.43)	1.27 (0.89)

Notes: $N = 456{,}660$ annual county age-race-sex cells. Coefficients report relative change in mortality. Robust standard errors, clustered at the state level ($n = 50$), are in parentheses. All estimates are from a negative binomial generalized linear model, which includes year fixed effects and state fixed effects. See equation 5 in text for full details. Sample contains adults ages 20 and older. Age, sex, and race are binary indicators (0 vs. 1). % Latino, poverty rate, and unemployment rate are expressed as fractions ranging from 0 to 1.0. [1] "Non-amenable" causes of death are those not included in the category "health-care amenable." However, this does not mean they cannot be impacted by health insurance, only that these death rates are likely to be *less* responsive to insurance coverage than other causes. $^{a}p < 0.01$, $^{b}p < 0.05$, $^{c}p < 0.10$.

Assistance Program, and Medicaid.[16] SAHIE data are available for 2000–01 (the pilot years of the program), and subsequently from 2005 onwards, which means data are not available for multiple pre- and post-expansion years in the study period. I present several analyses to account for these data limitations.[17]

The first approach simply stratified the original sample into two sets of counties—high-uninsured counties and low-uninsured counties, using the 2000 population-weighted median uninsured rate in the expansion states. The results in Table 6 (model 1) indicate that counties with high-uninsured rates prior to Medicaid expansion experienced much larger mortality reductions (−29.4 per 100,000), compared with counties with low uninsured rates (−7.7 per 100,000). The second approach follows that used by Miller (2012) in her analysis of Massachusetts, in which she used the pre-expansion county uninsured rate interacted with a post-expansion indicator as the independent variable of interest. The results (model 2 in Table 6) show that mortality declines varied significantly with the pre-expansion uninsured rate, with a coefficient of −99.4. This corresponds to a mortality decline of 9.94 per 100,000 for each additional 10 percent increment in the pre-expansion county-level uninsured rate.

The previous two models have the advantage of measuring coverage exogenously, by using only pre-expansion data to identify counties with larger potential gains in coverage. However, for a cost-benefit analysis, it is necessary to measure the mortality change per additional person insured. For this analysis, I estimated the following linear model using those years for which SAHIE data are available (2000, 2005–07), replacing the binary differences-in-differences indicator with a parameterized measure of coverage gains from

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[16] For more detail, see "Small Area Health Insurance Estimates," US Census Bureau, available at https://www.census.gov/did/www/sahie/methods/20052007/index.html.

[17] In addition, the baseline 2000 SAHIE data were for adults ages 19 and older. To make these estimates comparable to the 2005–07 figures (ages 19–64), I subtracted out the elderly population of each county, assuming that the elderly coverage rate was 100 percent.

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TABLE 6. Mortality (deaths per 100,000) and county-level coverage changes due to Medicaid expansions

Model and variable *Years included*	(1) All	(2) All	(3) 2000, 2005–07	(4) 2000, 2007	(5) 2000, 2005–07	(6) 2000, 2007
High-uninsured counties:	-29.4^{b}	-	-	-	-	-
Expansion State × Post	(12.1)					
Low-uninsured counties:	-7.7^{a}	-	-	-	-	-
Expansion State × Post	(2.7)					
Expansion State × Post	-	-99.4^{b}	-	-	-	-
× Uninsured_2000		(46.6)				
Expansion State	-	-	-328.1^{a}	-419.2^{a}	-316.0^{a}	-390.6^{a}
× ΔCoverage			(56.8)	(76.5)	(48.2)	(74.7)
ΔCoverage	-	-	100.0^{b}	94.6^{b}	116.4^{a}	99.7^{c}
			(37.6)	(45.1)	(41.4)	(54.3)
Expansion State × Post	-	-	-	-	-23.7^{a}	-23.8^{a}
	-	-	-	-	(6.7)	(−4.9)

Notes: All coefficients report changes in mortality per 100,000. Model 1 is the baseline linear model with the sample stratified into high-uninsured and low-uninsured counties based on 2000 SAHIE estimates. Model 2 is described by equation 6 in the text and measures the interaction term between *ExpansionStatePost* and the pre-expansion uninsured rate (based on 2000 SAHIE estimates). Model 3 is described by equation 7 in the text and includes all 3 years with SAHIE estimates (2000, 2005–07). Model 4 is analogous to model 2, but uses only 2000 and 2007 data. Models 5 and 6 repeat models 3 and 4, respectively, with the addition of the term *Expansion State × Post. ΔCoverage* is the net change in the county-level uninsured rate for nonelderly adults, from pre-expansion to post-expansion. Robust standard errors, clustered at the state level ($n = 50$), are in parentheses. All models include year fixed effects and county fixed effects. Sample contains adults ages 20–64. $^{a}p < 0.01$, $^{b}p < 0.05$, $^{c}p < 0.10$.

the Medicaid expansion:

$$\begin{aligned} \mathit{MortalityRate}_{ijkct} = {} & \beta_0 + \beta_1 \boldsymbol{X}_{ijk} + \beta_2 \textbf{\textit{County-Level Factors}}_{ct} + \beta_3 \Delta \mathit{Coverage}_{ct} \\ & + \beta_4 \mathit{Expansion\ State}_s \times \Delta \mathit{Coverage}_{ct} + \mu\ \mathit{Year}_t + \Omega\ \mathit{County}_c \\ & + \varepsilon_{ijkct} \qquad (6). \end{aligned}$$

ΔCoverage represents the change in county-level insured rate compared with 2000. β_4 is the coefficient of interest, measuring the impact of coverage gains in Medicaid expansion states. All other variables are defined as in equation 4. I also considered a model limiting the data to a single pre- and post-expansion data point (2000 and 2007) for a balanced panel, as well as models with the addition of the original *Expansion State × Post* interaction term. This latter approach allows for changes in mortality in expansion states not mediated by changes in the county-level coverage rates, given previous findings of spillover effects

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of coverage expansion even on those who already have health insurance (Pauly and Pagán 2007).[18]

Table 6 presents these results. In all models, county-level coverage gains led to significant mortality declines, with point estimates ranging from −316 to −419. *ΔCoverage* is reported as a fraction from −1.0 to 1.0, so these coefficients indicate that each percentage point of coverage led to 3.16–4.19 fewer deaths per 100,000. Alternatively, this suggests that the expansions needed to cover between 239 and 316 people to prevent one death per year, the so-called number needed to treat (NNT).[19] We can use this parameter to translate the observed population-level mortality changes into an individual-level risk reduction in mortality due to gaining coverage, with the following relationship:[20]

$$NNT \times \textit{baseline mortality} \times \textit{individual risk reduction} = \textit{1 death per 100,000}.$$

Using an NNT of roughly 300, this leaves two degrees of freedom: the baseline mortality rate for those gaining insurance, and the individual-level risk reduction of coverage. Neither is observed directly in the mortality data set, but one can make reasonable inferences for the former. The baseline population-wide mortality rate in expansion states was 318/100,000. But the mortality rate for uninsured adults—and in particular for uninsured adults living near or below the federal poverty level—is significantly higher. The relative risk of death among poor adults ages 19–64, compared with the general population, has been estimated to be 1.75 (Galea et al. 2011). Even after controlling for income, sex, and age, the uninsured still have higher mortality rates, with a relative risk estimated by Kronick (2009) to be 1.25. Putting these two hazard ratios together with the baseline mortality rate implies that low-income uninsured adults in these states had a mortality rate of roughly 700 per 100,000. If we assume that the mortality rate for those who signed up for Medicaid was equal to those who did not sign up for Medicaid, this implies an individual-level mortality reduction of 48 percent.

However, previous research has demonstrated that poor health itself is a strong predictor of Medicaid take-up, conditional on eligibility (Kenney et al. 2012). This suggests that baseline mortality among those who actually enrolled in these states' Medicaid

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[18] An alternative approach would be to use Medicaid expansion as an instrument for county-level changes in the uninsured rate. This approach yields a larger reduction in mortality linked to coverage gains, with a coefficient of −1,127 ($p = 0.04$). However, the direct effect of *Expansion State × Post* in Table 6 remains significant in models containing county-level coverage changes, which suggests that the SAHIE county-level estimates of coverage gains do not explain the full mortality effect. This may be because of a combination of measurement error in the SAHIE and spillover effects of expansions that affect mortality through channels other than increasing coverage. In any event, these results suggest that the exclusion restriction does not hold.

[19] A coefficient of −419 means −4.19 fewer deaths per 100,000 for a percentage point of coverage. NNT = 1,000 covered per 100,000/4.19 deaths prevented per 100,000 = 239.

[20] This calculation makes the assumption that all mortality gains accrue to those individuals who gained coverage, without any spillover effects on other people in the state. If there are any spillover effects on population mortality, this calculation will overstate the individual-level risk reduction associated with acquiring health insurance.

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expansions was likely significantly higher. If adverse selection confers an additional 25 percent mortality risk,[21] this would imply an individual-level mortality reduction from gaining insurance of 38 percent, consistent with several prior estimates (Goodman-Bacon 2013; Hadley 2003; Wilper et al. 2009) from the admittedly conflicting literature on the topic. The 95 percent confidence intervals in Table 6 provide a lower bound on the individual-level risk reduction of 25 percent.

To further explore whether these results are solely due to mortality changes in New York, I repeated the regressions using *ΔCoverage* in Table 6 with New York excluded from the sample. The resulting coefficient on *Expansion State* × *ΔCoverage* ranged from −142 to −149 per 100,000 (all $p < 0.05$). These estimates are roughly half as large as those for the full sample including New York. These findings suggest that the coverage expansions in Maine and Arizona were linked to smaller mortality reductions than in New York, though the nonsignificant results in the overall differences-in-differences model appear to be due to lack of power compared with effects that are detectable in models focused on county-level coverage changes.

VI. Cost-Benefit Analysis and Conclusion

The relationship between coverage gains and mortality changes offers a straightforward way to assess the cost-benefit ratio of Medicaid's impact on population mortality. Of course, many if not most of the benefits of coverage are not due to mortality reductions. Health-related quality of life and financial protection are far more common benefits of coverage. One previous analysis suggested a utility-based gain from financial risk protection in Medicaid equivalent to a value ranging from roughly $500 to $3,000 per person, depending on the extent of risk aversion (Sommers and Oellerich 2013), and a recent working paper found substantial improvements in debt related to the ACA's Medicaid expansion equal to a $600–$1,000 per-person reduction in collection balances (Hu et al. 2016). Meanwhile, OHIE found that Medicaid increased the proportion of adults reporting "good, very good, or excellent health" by 13 percentage points (Finkelstein et al. 2012) and reduced depression by 9 percentage points (Baicker et al. 2013), as part of a larger welfare gain among beneficiaries equal to 20–40 percent of total Medicaid spending (Finkelstein, Hendren, and Luttmer 2015). While these financial and non-mortality health benefits are key elements of a comprehensive assessment of Medicaid, here I focus simply on mortality because this allows for a direct comparison to the existing literature on the value of a statistical life for various public policy and health interventions.

In the previous section, I estimated that the Medicaid expansion covered 239–316 adults to prevent one death per year. The Oregon Health Insurance Experiment, studying a similar population of low-income adults eligible for a Medicaid expansion, detected a 25 percent increase in overall medical spending from gaining Medicaid (Finkelstein

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[21] The extent of adverse selection could actually be much larger than this, given that Medicaid enrollment often occurs at the point of care for active medical issues, in doctor's offices, emergency departments, and hospitals.

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et al. 2012).[22] Thus, each death prevented in my analysis leads to an estimated increase in health-care spending of \$186,000 to \$246,000 based on the number needed to treat.

However, this does not account for the fact that Medicaid expansion replaces with tax-supported insurance coverage the other 75 percent of medical spending that was primarily occurring using private dollars—either out-of-pocket spending or uncompensated care. While some of this pre-expansion care was likely publicly financed to begin with via public hospitals and clinics, I make the conservative assumption that it was all privately funded—which means that these results provide an upper bound on the cost per life saved. Factoring in the deadweight loss of this crowd-out of private medical spending, we are left with the following formula:

$$\mathit{Cost\ per\ Life\ Saved} = \mathit{NNT} \times [\$778 \times (1 + \mathit{DWL}) + \$3{,}156 \times \mathit{DWL}],$$

where *NNT* is number needed to treat and *DWL* is the deadweight loss of public financing as an assessment of the distortionary effect of raising taxes to pay for public programs. Typical estimates in the public economics literature for this DWL range from roughly 15 percent to 50 percent; however, it is worth noting that these estimates are primarily driven by labor supply elasticities, which are themselves the subject of considerable imprecision in the literature (Ballard, Shoven, and Whalley 1985; Browning 1987).[23] This range, combined with the set of values for NNT estimated above, produces a cost per life saved ranging from \$327,000 to \$867,000 (Online Appendix Table A4) in 2007 dollars.[24] These values compare favorably with estimates from the literature on the value of a statistical life (Viscusi 1992, 1993) used by the government to evaluate public health and environmental policies, which have been pegged at a mean of \$7.6 million, with a range from \$950,000 to \$21.4 million, in 2007 inflation-adjusted dollars (Robinson 2007).

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[22] LATE results in Table V from Finkelstein et al. (2012) report an estimated \$778 increase from a baseline mean of \$3,156, in 2007 dollars. While New York has the nation's most expensive Medicaid program, this is mostly due to costs for elderly adults and individuals with disabilities. The OHIE estimate for per-person spending among nonelderly adults in Medicaid (\$3,934) is quite similar to a contemporary estimate of per capita costs for nonelderly adults in New York's Medicaid program: \$3,953 in 2003 dollars, equal to \$4,454 in 2007 dollars (http://www.cbcny.org/sites/default/files/reportsummary_medicaid_04202006.pdf, accessed July 1, 2014). Replacing the baseline level of spending from OHIE with the latter estimate simply increases the results proportionately, by 13 percent.

[23] Note that this DWL does *not* include the moral hazard of health insurance (i.e., increased utilization due to the reduced cost of care), which in my calculation is already directly captured by the changes in medical spending derived from the OHIE results.

[24] Several back-of-the-envelope cost-benefit analyses have been advanced following the publication of the Massachusetts mortality study (Cannon 2014; Pollack 2014). However, these typically have treated the government payment towards subsidized coverage as the "cost of insurance." This is incorrect for two reasons. First, we are interested in the marginal societal cost, and in the absence of subsidized coverage uninsured people do not consume zero health care—they appear to consume nearly 80 percent of what insured people do, according to the OHIE. Second, this approach ignores the deadweight loss of public financing. The first omission is much larger than the second, which means that these previous estimates overstate the cost per life compared with the method here.

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In conclusion, in this differences-in-differences analysis of state Medicaid expansions to low-income adults, I find that expansions led to a 6 percent relative decline in mortality over five years of follow-up, compared with a control group of counties with similar pre-expansion mortality trends and demographic features. There was a highly significant decline in health-care amenable causes of death, while effects for other causes of death were generally smaller and only marginally significant. Declines in HIV-related deaths accounted for 20 percent of the overall mortality effect, and the Medicaid expansions appear to have facilitated the impact of antiretroviral therapy during this period. Mortality declines were closely linked to county-level changes in insurance coverage, with one death prevented annually for every 239 to 316 adults gaining health insurance, which implies a cost per life saved that compares favorably with the standards used to evaluate existing public policy interventions.

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Lapses in Medicaid Coverage

Impact on Cost and Utilization Among Individuals With Diabetes Enrolled in Medicaid

Allyson G. Hall, PhD,† Jeffrey S. Harman, PhD,* and Jianyi Zhang, PhD†*

Background: Gaps in Medicaid coverage can result in inadequate access to care. This can be particularly detrimental to those with a chronic disease such as diabetes.

Objective: To assess whether a lapse in Medicaid coverage is associated with an increase in expenditures, and acute care utilization upon reenrollment among beneficiaries with diabetes.

Research Design: Using multivariate regression analyses, we compared pre- versus post-expenditures and utilization among 2102 individuals with diabetes who had experienced at least one 1-month lapse in their Medicaid coverage.

Measures: Dependent variables were the number of inpatient episodes, total length of stay, total number of emergency room visits, total expenditure, and pharmaceutical expenditures. These were aggregated over 3-month spans that either immediately preceded or immediately followed a lapse in coverage. Key predictor variables included a variable that identified the span as occurring pre-lapse or post-lapse in coverage, and a continuous variable identifying the length of the lapse. Predicted expenditure and utilization were calculated.

Results: Overall total program expenditures were higher for post-lapse periods compared with pre-lapse periods. Total expenditures were estimated to increase by $239 per member per month for the 3-month period. The likelihood of having any expenditure was actually lower in the post-lapse period. However inpatient and emergency room use was higher.

Conclusions: The results from this study suggest that interruptions in Medicaid coverage are associated with overall greater program expenditures in the post-lapse periods. However, this increase in expenditures seems to be driven by a subset of individuals whose greater use of inpatient and emergency room services increased overall program costs.

Key Words: Medicaid, diabetes, churning, coverage lapses

(*Med Care* 2008;46: 1219–1225)

Estimates indicate that millions of people experience not just one, but repeated gaps in health insurance coverage. About 38% of the nonelderly population is estimated to have some degree of instability in their health care coverage.[1] Individuals who rely on Medicaid are likely to experience gaps in their coverage.[2] Much has been written about the complex enrollment and reenrollment procedures associated with Medicaid and the State Children's Health Insurance Program (SCHIP) making coverage difficult to maintain over time.[3,4] The difficult nature of the renewal process provides an opportunity for individuals and families to be disenrolled from the program and experience gaps in their coverage. In Florida, most Medicaid beneficiaries must re-enroll after 1 year and if there is a child over 5 years in the household, renewal is conducted every 6 months.[5] National estimates indicate that over a 4-year period only 17% of Medicaid/SCHIP beneficiaries were continuously enrolled for the entire period. And, of those who left Medicaid/SCHIP 65% of them became uninsured.[1]

There has been a significant body of work emanating from survey-based research that documents consequences of lack of continuous health insurance coverage on access and utilization. These survey-based studies have generally found that relative to those with continuous health insurance, those with coverage gaps have lower rates of preventive service use,[6–8] have greater problems paying medical bills,[9,10] lack a usual source of care,[11–13] report no physician visits over a period of time,[11] and are dissatisfied with the health care services they do receive.[10] In addition individuals with coverage lapses are more likely to postpone or delay medical care, to delay prescription drug use, to have no well-child visit,[13,14] to not be up-to-date on vaccination schedules,[15] and are more likely to have an unmet health care need.[16] Health status has also been found to deteriorate after termination from a Medicaid program.[17] A general limitation of this survey-based research is that they tend to be cross-sectional in nature and are unable to judge the immediate impact of a gap in coverage on expenditure and utilization once coverage is restored.

There have been relatively few studies that examined the impact of coverage lapses using claims-based data. Administrative data allows for measuring actual changes or differences in utilization as a result of coverage lapses. One claims-based study that examined the impact of coverage

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lapses on individuals with schizophrenia enrolled in a Medicaid program, found that interruptions in coverage were associated with more psychiatric hospitalizations per beneficiary and higher hospital admission rates.[18] Another study of Medicaid beneficiaries with depression found that expenditures, inpatient episodes, and emergency department visits were higher in the period of time immediately after a beneficiary returned to Medicaid.[19] A study of children with asthma found that although the rate of hospitalizations for respiratory ailments was similar for those with coverage gaps and those without gaps, hospitalization rates for nonrespiratory ailments were higher for children with gaps in their Medicaid coverage.[20]

Access to diabetes related services can be jeopardized by lack of insurance. Nationally, 13 million adults are estimated to have the disease and about 1 million individuals with diabetes receive Medicaid coverage.[21] People with the disease are at risk for a variety of complications that can lead to an increase in the overall cost of their care. Appropriate monitoring and management of the disease can reduce the risk of complications.[22] The Institute of Medicine concluded that uninsured individuals with diabetes are less likely to receive recommended services relative to those with health insurance.[23] Individuals with diabetes who are unable to access the recommended services are more likely to have complications such as uncontrolled hypoglycemia and hypertension, leading to hospitalizations and emergency room use.

The study described here seeks to extend what is known about the consequences of coverage gaps and its impact on beneficiaries served by Medicaid programs, especially those individuals with a chronic disease such as diabetes. We seek to understand whether there is an association between a gap in Medicaid coverage and the length of that gap on inpatient use, emergency room use, and costs of care for a period of time upon reenrollment in the program among individuals with diabetes. We theorize that individuals with diabetes who are on Medicaid and lose that coverage become uninsured.[1] That lack of health insurance is associated with less access to appropriate services. Health status of the individual deteriorates to a point where there is a major event such as an emergency room visit or inpatient stay upon return to Medicaid coverage. We also suspect that the longer the time without coverage, the greater the impact of that coverage loss. We use Florida Medicaid claims data to assess whether an interruption in Medicaid is associated with an increase in post-lapse Medicaid related expenditures, inpatient episodes, and emergency room visits relative to pre-lapse expenditures, and inpatient and emergency room episodes among beneficiaries with diabetes.

METHODS

Data Source

Data used in this analysis comes from the Florida MediPass claims and eligibility database covering the period from January 1999 to December 2002. MediPass is Florida Medicaid's primary care case management program and was operational statewide during the time of this study. Medicaid claims provide reasonably accurate measures of service utilization when providers are paid on a fee-for-services basis as they are in MediPass.

Beneficiaries with diabetes were identified based on diagnoses assigned by providers during health care encounters and recorded on claims in 1999. Medicaid claims provide a primary diagnostic code conforming to the *International Classification of Disease, 9th Edition* (ICD-9-CM). Selected diabetes codes were based on the methodology used in the Medical Expenditure Panel Survey.[24] The selection process identified 31,180 individuals with diabetes. The sample admittedly excludes individuals who did not receive treatment during the study period nor are individuals included who became initially eligible for benefits after December 1999. We also excluded individuals who were dually eligible for both Medicare and Medicaid.

Individuals with diabetes were then identified as having a lapse in Medicaid coverage if they experienced 32 consecutive days or more of not being enrolled in the program. A full month of disenrollment was chosen because in Florida Medicaid, beneficiaries with a gap of 1 month or less can be retroactively enrolled for that 1-month period. Thus, they can be "technically" enrolled in Medicaid but may not appear in the eligibility files for that period. Periods of disenrollment that were open-ended and continued to the end of the study period were not identified as lapses, because claims data would not be available for the subsequent period of reenrollment. The analyses include all individuals with diabetes who experienced at least one lapse in coverage during the study period. Excluded from the analyses are individuals over the age of 64 because they are likely to be enrolled in Medicare and individuals who had a pregnancy/delivery diagnoses in the post-lapse period.

The unit of analysis is an eligibility span. Each individual included in the analysis contributed 2 observations per lapse in coverage—one observation for utilization and expenditures during the 3-month eligible span immediately before the lapse and one observation for the 3-month eligible span immediately after each lapse. Individuals with multiple lapses contributed 2 observations for each lapse experienced. No lapses were counted as both post-lapse and pre-lapse.

Variables

Four sets of dependent variables were created and used in the analysis: the number of inpatient episodes, total length of stay, total number of emergency room visits, and expenditures (total, prescription drug). These measures of expenditure and utilization were aggregated over 3-month spans that either immediately preceded or immediately followed a lapse in coverage.

Hospitalization episodes were determined by using beginning and ending dates of hospitalizations in the claims data. Because a single hospitalization can occur over the course of 1 month, beginning dates from 1 month were compared with ending dates for a previous month to ensure that single episodes were not counted as multiple hospitalizations. A gap of 2 days or more days between an end and a beginning date was used to distinguish between episodes. The number of inpatient episodes was calculated

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by summing all hospitalization episodes for each 3-month pre- or post-span period.

Inpatient length of stay was calculated using the start day of service and ending day of service. The total length of stay for each 3-month period was the sum of the length of stay for each inpatient episode. Emergency room visits were identified using a flag in the claims data. Total emergency room visits are the sum of all visits for each period.

Total and pharmacy expenditure amounts were calculated from the dollar amounts Medicaid paid to providers for services as indicated on the claims data. Total expenditures are the sum of the facility, physician, and pharmacy expenses. We conducted separate analyses on pharmacy expenditures because they are easy to identify within the Florida Medicaid data. All expenditures are based on claims actually paid by Medicaid and not on charges submitted by providers. Expenditures were adjusted to 2002 dollars using the Medical Care Consumer Price Index.

The 2 main predictor variables were a dichotomous variable used to identify whether the 3-month spans occurred pre-lapse or post-lapse and a continuous variable used to describe the number of months of interrupted Medicaid coverage. The purpose of this variable is to determine if the impact of lapses varies depending on the length of time without coverage. For observations relating to the 3-month span before a coverage lapse, this variable is set to zero.

To control for severity of diabetes we examined pharmacy claims to identify medications associated with this disease. Each drug has a unique NDC code, which corresponds to a certain therapeutic class. Diabetes drugs were categorized as either insulin (which are injectable) or hypoglycemics (which are taken orally) based on their therapeutic classification. Insulin is used in more severe cases of diabetes and hypoglycemics are used in less severe cases. Flags were added to the pre-lapse spans that indicated whether during that span an enrollee had a prescription filled for an insulin or hypoglycemic drug. Dummy variables were created for both of these classifications and used in the analyses.

In addition, we controlled for comorbidities in the pre-lapse period by classifying diagnoses into 32 Aggregated Diagnosis Codes (ADGs). These clusters are based on all ICD-9 diagnosis codes in inpatient, outpatient, and physician claims. These codes are used to classify diagnoses based on their duration, severity, diagnostic certainty, etiology of the condition, and specialty care involvement.[25] ADGs and their related categorizations Adjusted Clinical Groups have been used to risk-adjust in previous analyses involving Medicaid claims data.[26–28] Spans were classified into low, middle, or high ADG categories based on whether they had 0–2 (low), 2–6 (middle), or >6 (high) ADGs.

A variable indicating whether the individual lived in an urban or rural area was also included. This variable was constructed by matching enrollee zip codes to rural–urban commuting area codes. Rural–urban commuting area uses urbanization, population densities, and daily commuting to define the settlement patterns throughout the country.[29] Urban areas were defined as locations where the primary flow of individuals was to cities with greater than 50,000 inhabitants.

Other variables used in the analyses included age, gender, and race (white or otherwise), and as a control for health status/severity of illness, whether individuals qualified for Medicaid through a disability. These variables were available from the eligibility files.

Analytic Strategy

The goal of the analysis was to assess whether a lapse in Medicaid coverage was associated with increased Medicaid expenditures, inpatient utilization and emergency room visits after the lapse in coverage. To provide some context for our analysis, we conducted descriptive analyses that compared demographic, expenditure, and utilization between these 2 groups.

The distribution of Medicaid expenditures significantly departed from normality and was characterized by skewness and kurtosis. That is, many of the 3-month spans have no expenditures but a few have very high expenditures. To accommodate this non-normality, 2-part regression modeling was used to estimate Medicaid expenditures. The first part is a logit model, which estimated the probability of having any expenditure. Gamma regression was used in the second part to estimate expenditures on spans that have an expenditure. This technique is thought to be superior to ordinary least squares regression because expenditures and utilization do not have to be transformed. In addition, gamma regressions are robust even in the presence of hetroscedasticity.[30] The results of both regressions were then fitted to obtain estimates of the predicted expenditures for the 3-month periods before and after a lapse in Medicaid coverage. A 3-month eligibility span was the unit of analysis with expenditures during the 3-month period serving as the dependent variable. A dummy variable indicating whether or not the span was from the period immediately after the lapse in coverage was included in the equation and was the variable of interest.

Inpatient episodes and the number of emergency room visits were modeled using negative binomial regression.[31] Negative binomial regression was used because it appropriately models count data that is characterized by overdispersion and skewness. As with the expenditure analyses, a dummy variable was included in the model to indicate whether or not the observation was from the 3-month period immediately after the interruption in coverage.

The analysis of the total number of inpatient days was calculated using exponential regression, a specific form of gamma regression.[32] This form of regression is useful for data that follow a log-normal distribution. All of the regression models adjusted for age, gender, race, urban designation, number of ADGs, diabetes medication type, and disability status. Standard errors were corrected to account for clustering of multiple observations by a single beneficiary. Stata version 7.0 was used to perform the analyses.

Finally, to obtain predicted expenditures and utilization before a lapse in coverage, the variables corresponding to a lapse and to the length of the lapse were set to zero before running the model. Then, to estimate expenditures and utilization post-lapse, the lapse variable was set to one and the length of lapse variable was set to the actual length of the lapse. Differences in expenditures and utilization pre-lapse

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TABLE 1. Demographic Characteristics, Length of Lapse, Expenditures, and Utilization of Medicaid Beneficiaries With Diagnosis of Diabetes Who Did or Did Not Have Gap in Coverage

	Continuous Coverage (N = 26,976)		Interruption in Coverage					
			Single Interruption (N = 1771)		Multiple Interruption (N = 331)		Single and Multiple Interruptions (N = 2102)	
	N	%	N	%	N	%	N	%
Mean length in lapse								
Days (M ± SD)			303.2 ± 275.1		188.5 ± 113.2		285.1 ± 259.8	
Age (M ± SD)	49.1 ± 12.7		41.8 ± 16.4		37.7 ± 15.3		41.2 ± 16.3	
Male	8926	33.1	637	36.0	82	24.8	719	34.2
Caucasian	11,008	40.8	707	39.9	103	31.1	810	38.5
African American	7929	29.4	569	32.1	132	39.9	701	33.4
Latino	1337	5.0	289	16.3	75	22.7	364	17.3
Rural	3681	13.7	264	15.1	45	13.8	309	14.9
Disabled	24,073	89.3	914	51.6	74	22.4	988	47.0
Switch in eligibility category	3401	12.6	330	18.6	45	13.6	375	17.8
Average total expenditures per member per month*	$1049.37		$580.82		$451.17		$560.41	
Average pharmacy expenditures per member per month	$418.86		$215.55		$133.71		$202.66	
Average no. inpatient episodes per 100 member months	5.04		3.22		3.77		3.28	
Average no. emergency room visits per 100 member months	7.93		6.30		8.57		7.17	

Data are from 1999–2002 Florida Medicaid claims and enrollment files. Interruption groups are significantly different ($P < 0.05$) by age, gender, race, Latino ethnicity, disability status, and switch in eligibility category. Among those with coverage interruptions, mean length of lapse is statistically different.

*Member months are the number of calendar months an enrollee is covered by Medicaid.

versus post-lapse were calculated and 95% confidence intervals were constructed by bootstrapping the estimates.[33] Statistically significant differences were assessed using the 95% confidence intervals.

RESULTS

Characteristics of the Sample

Of the individuals identified for inclusion in the study 2102 experienced at least one lapse in Medicaid eligibility during the 4-year period. Table 1 provides a description of the individuals who experienced a single lapse or multiple lapses in coverage and compares them to individuals continuously enrolled for the 4-year period who would otherwise meet the study criteria. Lapsers and nonlapsers looked different across a number of dimensions. Notably, individuals who were continuously enrolled were older and more likely to qualify for Medicaid based on their disability compared with individuals with coverage lapses. Among individuals with a lapse, the mean number of lapses per individual was 1.2 (not shown) and the average length of a lapse was 285 days. About 84% experienced only one lapse. The majority were female and members of a racial and ethnic minority group. Expenditures and the number of inpatient episodes per member month were higher for those continuously enrolled. Individuals with a single interruption had the lowest number of emergency room visits whereas those with multiple interruptions had the highest average number of emergency room visits.

Association Between Lapses in Coverage and Medicaid Utilization and Expenditure

Table 2 presents the coefficients for the post-lapse and length of lapse variables used to predict the probability of any Medicaid expenditures (both parts of the 2-part expenditure regression models). For total expenditures, both the lapse in coverage ($P < 0.001$). and the length of the lapse ($P < 0.001$) were found to be significantly and negatively associated with

TABLE 2. Multivariate Regression Results for Medicaid Expenditures

	Postlapse		Length of Lapse	
	Coefficient	P	Coefficient	P
Any expenditure				
Total	−0.459	<0.001	−0.001	<0.001
Pharmacy	0.264	<0.001	−0.001	<0.001
Expenditures Given Use				
Total	0.583	<0.001	0.0003	0.229
Pharmacy	−0.030	0.488	−0.0001	0.344

Regression analysis controls for age, gender, race, eligibility category, ADG grouping, use of oral hypoglycemics, use of injectible insulin, and metropolitan status.

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TABLE 3. Regression Results for Number of Inpatient Episodes, Length of Stay, and Total Number of Inpatient Visits by Beneficiaries With Diabetes

	Postlapse		Length of Lapse	
	Coefficient	*P*	Coefficient	*P*
No. inpatient episodes*	3.00	<0.001	1.00	0.816
Length of inpatient stay†	1.46	0.005	1.00	0.151
Total no. emergency room visits*	144.87	<0.001	0.99	0.027

Regression analysis controls for age, gender, race, eligibility category, ADG grouping, use of oral hypoglycemics, use of injectible insulin, and metropolitan status.

*Incidence rate ratio.

†Time ratio.

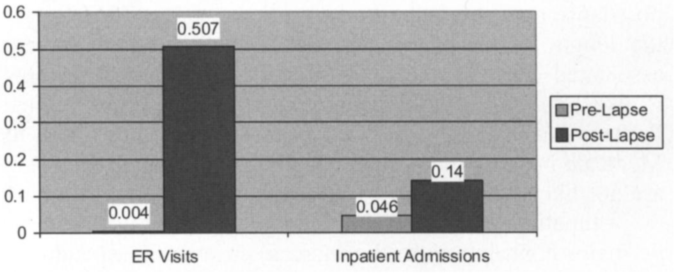

FIGURE 1. Changes in ER visits and inpatient admissions.

the probability of having any expenditure once a lapse occurred (top half of table). However, for pharmacy related expenditures having a lapse was positively associated with expenditures, yet the length of the interruption was negatively associated with those expenditures ($P < 0.001$).

Table 2 also shows post-lapse and length of lapse coefficients for the second part of the model used to estimate Medicaid expenditures for those individuals who had some expenditures. For total expenditures, having a lapse in coverage was positively and significantly associated with the amount of expenditures ($P < 0.001$). However, the length of the lapse was not significantly related to expenditures. For pharmaceuticals, neither the presence of a coverage lapse nor the length of the lapse were related to pharmacy expenditures.

As shown in Table 3, the incidence of inpatient and emergency room utilization is greater in the 3-month period after a coverage lapse compared with the pre-lapse period. The number of inpatient episodes in the post-lapse period was 3 times the number of episodes pre-lapse ($P < 0.001$). Of particular note, is that a lapse in Medicaid coverage among enrollees was associated with an increase in their emergency room utilization by 145-fold or 14,487% ($P < 0.001$). Having a coverage lapse was also associated with longer inpatient stays. In the post-lapse period, inpatient stays were 46% longer than in the pre-lapse period. The length of the coverage lapse was not significantly associated with the number of inpatient episodes or the length of the inpatient stay. However, the total number of emergency visits is positively associated with the length of the lapse ($P < 0.001$). That is, as the length of the lapse increases the number of emergency room visits also increases.

As Table 4 shows, when considered jointly, the presence of a lapse and the length of the lapse were significantly associated with greater expenditure in the post-lapse period. Total Medicaid expenditures increased by $719 per member during the 3-month post-lapse period or $239 per member per month, relative to the 3-month pre-lapse period. There were no statistically significant differences in pharmaceutical related expenditures between the pre-lapse and post-lapse periods.

The 3-month inpatient and emergency room utilization is also higher after a coverage lapse (See Fig. 1). After a lapse in coverage, the adjusted number of hospital admissions per member per 3-month eligibility period rose by 0.094 compared with the 3-month period immediately preceding the lapse in coverage. The average length of an inpatient stay per lapse increased by almost 4 days in the 3-month period of eligibility after a lapse in coverage. The number of emergency room visits per member per 3-month eligibility period increased by 0.503 after a lapse in coverage.

DISCUSSION

The overall results from this study suggest that interruptions in Medicaid coverage may lead to greater program expenditures and higher inpatient and emergency room utilization among beneficiaries with diabetes, at least for the 3 months immediately after the lapse. That is, for the Medicaid program overall, there is an increase in expenditures for post-lapse periods relative to pre-lapse periods.

An interesting finding is that even though overall expenditures are higher in the post-lapse period, the likelihood of incurring any total expenditure is actually lower in the

TABLE 4. Change in Average 3-Month Expenditures/Utilization After Lapse in Medicaid Coverage for Beneficiaries With Diabetes

	Prelapse	Postlapse	$\chi^2(2)$	*P*	Difference in Expenditures/Utilization	95% CI
Total expenditures	961	1680	135.57	<0.001	+719	(552–902)
Pharmacy expenditures	436	408	3.40	0.183	−28	(−72 – 4)
Total hospital admissions	0.046	0.140	84.91	<0.001	+0.094	(0.091–0.097)
Total average LOS for those with at least 1 admission	5.5	9.2	18.99	<0.001	+3.7	(1.61–5.23)
No. ER visits	0.004	0.507	219.88	<0.001	+0.503	(0.452–0.561)

*Expenditures are adjusted to 2002 dollars.

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post-lapse period relative to the pre-lapse period. The longer the length of the lapse was also found to be negatively associated with the likelihood of any total expenditure. This is also a surprising finding and contrary to our expectations. Perhaps individuals with long interruptions do not return to Medicaid because of an urgent health care need and therefore are not likely to incur an expenditure.

Inpatient utilization and emergency room use seem to be major contributors to the increase in overall expenditure post-lapse. Spans that occur after a lapse in coverage have more inpatient episodes, lengths of inpatient stays are longer, and there are more emergency room visits. This is consistent with previous research that found a significant increase in inpatient treatment after a lapse in Medicaid coverage among beneficiaries with schizophrenia.[18] These findings suggest that for a subset enrollees (those who do incur an expenditure), the return to coverage is associated with high rates of inpatient and emergency room utilization. These higher rates of utilization are sufficient to drive up overall Medicaid program costs, despite the fact that the likelihood of having an expenditure is lower in post-lapse periods. For these enrollees, the lack of health insurance could impede an individual's ability to obtain continuous primary/preventive care. As a consequence, complications from their diabetes may have manifested during the period of ineligibility forcing an inpatient or emergency room stay. An alternative explanation for the increase in expenditure and inpatient utilization for this subset of enrollees could be a result of disease progression due to aging. However, this is a somewhat unlikely explanation because the mean length of time between lapses is 285 days. This is a relatively short period of time for significant disease progression to occur among a group of individuals whose mean age is about 41 years. It is also possible that changes in local medical practice, rates of reimbursement, or rates of inflation could account for the increase in expenditures observed. However, we adjusted for inflation in the analysis, and to our knowledge Medicaid reimbursement rates did not change significantly in Florida during this period.

Also worth noting is that the likelihood of a pharmaceutical expenditure is higher in the post-lapse period. In addition, the length of the lapse is positively associated with the likelihood of pharmacy expenditures. Again, assuming that the individuals in this study are without health care coverage during periods of Medicaid disenrollment, and therefore without access to their medications, when they return to coverage they may use more drugs to stabilize their condition. This additional likelihood in pharmacy expenditure does not lead to significant differences in pharmacy cost in the pre-lapse period.

Several limitations of the study should be noted. One limitation is that there is no data available on utilization during the period of lapsed coverage. Therefore, it is not known whether health care services were used during the period of lapsed coverage. There is also no way determining whether persons were compliant with their diet regimens, an essential aspect of diabetes care management. The higher rates of inpatient and emergency room utilization among some of these individuals could be due to behavioral factors and not because of lack of access to health care. Another concern is that administrative data are often subject to inaccuracies in coding. Nevertheless, these kinds of data remain our best source of information on health care utilization. Finally, this analysis demonstrates only that there is an association between a Medicaid coverage lapse and utilization expenditure within a 3-month post-lapse time period. There may be further impacts that would manifest over a longer period of time.

Despite these limitations, findings from this study provide direction for Medicaid and general health system reform. Assuring seamless and continuous health insurance coverage is an obvious recommendation. Understandably, states wanting to be fiscally prudent are forced to implement mechanisms to ensure that only eligible beneficiaries are enrolled in their Medicaid programs. However, this may in fact may increase rather than decrease their costs.

The findings from the study suggest areas for additional research. New research can address the longer-term implications of coverage interruptions. Repeated interruptions over a long period may have a cumulative effect on utilization and health outcomes, especially for those who are chronically ill. It is also important to understand enrollee utilization experiences during periods of disenrollment and how this may impact utilization and expenditures among reinstatement. Research that combines survey data that asks about utilization during disenrollment, with claims and administrative data will be particularly valuable. Finally, our descriptive analyses showed that Medicaid beneficiaries with coverage lapses are different from those who are continuously enrolled. Continuing to explore causes of discontinuous enrollment will provide valuable information to policymakers and others who advocate for improvements in rates of health insurance coverage.

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Annals of Internal Medicine

Interruptions in Medicaid Coverage and Risk for Hospitalization for Ambulatory Care–Sensitive Conditions

Andrew B. Bindman, MD; Arpita Chattopadhyay, PhD; and Glenna M. Auerback, MPH

Background: Many low-income U.S. citizens experience interruptions in health insurance coverage.

Objective: To determine the rate of hospitalization for ambulatory care–sensitive conditions among Medicaid beneficiaries with interruptions in coverage.

Design: Retrospective cohort study.

Setting: California Medicaid population.

Patients: 4 735 797 adults in California age 18 to 64 years who received a minimum of 1 month of Medicaid coverage between 1998 to 2002.

Measurement: Time to hospitalization for an ambulatory care–sensitive condition.

Results: Sixty-two percent of Medicaid beneficiaries experienced at least 1 interruption in coverage during the study period. The 3 most common ambulatory care–sensitive conditions resulting in a hospitalization were heart failure, diabetes, and chronic obstructive pulmonary disease. Interruptions in coverage were associated with a higher risk for hospitalization for an ambulatory care–sensitive condition (adjusted hazard ratio, 3.66 [95% CI, 3.59 to 3.72]; $P <$ 0.001). In subgroup analyses, the association between interrupted coverage and hospitalization was stronger for beneficiaries eligible through the Temporary Aid to Needy Families program (adjusted hazard ratio, 8.56 [CI, 8.06 to 9.08]) than for beneficiaries eligible through the Supplemental Security Income program (adjusted hazard ratio, 1.72 [CI, 1.67 to 1.76]), who typically retain Medicare coverage even when their Medicaid coverage is interrupted.

Limitation: The study lacked information on why interruptions occurred and whether beneficiaries with interruptions transitioned to other insurance coverage.

Conclusion: Interruptions in Medicaid coverage are associated with a higher rate of hospitalization for ambulatory care–sensitive conditions. Policies that reduce the frequency of interruptions in Medicaid coverage might prevent some of the health events that trigger hospitalization and high-cost health care spending.

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Many U.S. citizens experience interruptions in health insurance coverage. A total of 85 million persons, or 38% of the U.S. population, younger than age 65 years were uninsured for at least part of a 3-year period (1). Low-income U.S. citizens are particularly at risk for periodic lack of insurance. Many poor persons move in and out of the Medicaid program with periods of being uninsured in between (2, 3).

Medicaid reenrollment policies affect the number of beneficiaries who experience interruptions in their coverage. A shorter period for eligibility redetermination creates an administrative barrier to continuous coverage (4). Federal law requires that Medicaid eligibility be redetermined at least annually, but many states require this assessment at a shorter interval. In California, for example, adults need to redemonstrate their eligibility for Medicaid every 3 months.

Research suggests that individuals with interrupted insurance coverage are less likely to receive primary care and preventive services (5). One potentially useful but as yet unexplored measure of morbidity and costs associated with interrupted Medicaid coverage is hospitalizations for ambulatory care–sensitive conditions. Ambulatory care–sensitive conditions, such as asthma, diabetes, and hypertension, are conditions that can often be managed with timely and effective treatment in an outpatient setting, thereby preventing hospitalization (6). Hospital admissions for these conditions reflect a decline in health status and, by association, the health consequences of access barriers.

We performed a retrospective cohort study to determine whether interruptions in Medicaid enrollment are associated with an increased risk for hospitalizations in adults with ambulatory care–sensitive conditions.

METHODS

Data

To conduct the analysis, the 1998 to 2002 California hospital patient discharge data available from the California Office of Statewide Health Planning and Development was linked with the Medicaid Monthly Eligibility File for the corresponding period from the California Department of Health Services. A deterministic match was done with social security numbers, sex, and year of birth available in both files, and a probabilistic match by using sex, date of birth, hospital identifier, and hospitalization dates was done on the residual to enhance the linkage. Judging by comparisons with separate Medicaid payment records, the

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California Department of Health Services estimated that more than 98% of Medicaid hospitalization records were successfully linked. Approximately 70% of the records were linked by using the deterministic method, and 27% were linked by using the probabilistic method (7). Furthermore, most records lacking a social security number needed for a deterministic match were for newborns, whom we excluded from this analysis. We were unable to correct for out-of-state hospitalizations of Medicaid beneficiaries.

The California hospital patient discharge database includes a unique patient identifier and information about admission and discharge dates, patient demographic characteristics, diagnosis codes, and insurance status for the hospitalization. By linking the information available in the hospital discharge file with that available from the California Department of Health Services, we could confirm whether a hospitalized individual was in fact a Medicaid beneficiary and capture additional information on a monthly basis regarding Medicaid enrollment status, aid category, and whether the care was delivered through fee-for-service or managed care for all Medicaid beneficiaries, regardless of whether they were hospitalized. Furthermore, this linked file enabled us to capture hospitalizations for individuals who at one time may have had Medicaid coverage but did not have coverage at the time of a hospitalization. We limited our analysis to adults age 18 to 64 years.

Outcome Measure

We created longitudinal records of eligibility status and hospitalizations of any persons who were ever enrolled in Medicaid during the 5-year study period. We measured the duration of time within the study period from enrollment in Medicaid until the first hospitalization for an ambulatory care–sensitive condition and the duration of time to subsequent hospitalizations thereafter.

We classified hospitalizations in the patient discharge file as being for ambulatory care–sensitive conditions on the basis of the definition provided by the Agency of Healthcare Research and Quality (AHRQ). We applied the AHRQ definitions of ambulatory care–sensitive conditions, identifying hospitalizations in which the principal diagnosis International Classification of Disease, Ninth Edition, code was listed in the AHRQ 2001 guidelines (8). We have previously reported that these conditions comprise 26% of non–pregnancy-related hospitalizations for Medicaid beneficiaries in California (9). We compared the pattern of hospitalization rates for specific ambulatory care–sensitive conditions between patients with continuous and those with interrupted Medicaid coverage. Because this pattern was quite similar, and for ease of interpretation and presentation, we followed the conventional practice of aggregating hospital admissions for any of the AHRQ ambulatory care–sensitive conditions.

Context

Many persons in the United States experience interruptions in their health insurance coverage.

Contribution

This study of hospitalized California adults with Medicaid found an association between interruptions in coverage and a higher rate of hospitalization for ambulatory care–sensitive conditions, such as heart failure, diabetes, and chronic obstructive pulmonary disease.

Caution

The study sample was limited to hospitalized patients, and some patients with interrupted coverage may have obtained private insurance.

Implication

Interruptions in insurance coverage were associated with hospitalization for ambulatory care–sensitive conditions. Policies that reduce the interruptions in coverage might prevent some of these hospitalizations.

—The Editors

Exposure Variable

We modeled our primary exposure variable as a time-varying covariate indicating whether a beneficiary had or had not experienced an interruption of coverage. We identified an interruption of coverage when a monthly eligibility code after the first enrollment month was no longer present. The California Medicaid Monthly Enrollment file includes a code for Healthy Families, the California State Children's Health Insurance Program (SCHIP). Healthy Families allows persons up to the age of 19 years to qualify for Medicaid-type benefits in California but at somewhat higher income levels. For the purposes of our analysis, we considered enrollment in Healthy Families as a form of Medicaid coverage and did not consider it to be an interruption in coverage if an individual changed between these 2 programs over time. We characterized all periods before the interruption as continuous and those after the interruption as being discontinuous.

Potential Confounders

We measured several characteristics of beneficiaries that could influence their risk for a hospitalization for an ambulatory care–sensitive condition as well as their risk for interrupted Medicaid coverage. These included demographic characteristics, Medicaid aid category, Medicaid health care delivery model, and forms of insurance other than Medicaid. Many of these variables are used to determine payment and were therefore complete in the data set. However, 68 807 beneficiaries (1%) had missing information on race/ethnicity and were classified with those reported as "other."

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Beneficiary demographic characteristics and Medicaid aid category provide an estimate of health status. We categorized aid category as Temporary Assistance to Needy Families (TANF), Supplemental Security Income (SSI), or other by using previously described algorithms (10). Medicaid eligibility through TANF is available to low-income children and their parents regardless of their health status. On the other hand, beneficiaries enrolled in Medicaid through the SSI program are eligible as a result of a chronic disability and therefore tend to be sicker on average than those eligible through TANF (11). From calculations using the Medi-Cal eligibility file data, we determined that most (83%) of the "other" group is composed of low-income persons whose incomes are too high for them to qualify for Medicaid but who subsequently do qualify for the medically needy aid category because of their acute out-of-pocket spending on health care services. The remainder of the "other" group is primarily women who are eligible on the basis of a pregnancy (12%) and persons who are eligible through one of several immigration-related programs (4%).

The AHRQ provides an option for including the Elixhauser comorbid condition measure in the calculation of ambulatory care–sensitive hospitalization rates (12). Incorporating diagnoses from administrative data in risk adjustment could introduce overadjustment if the comorbid conditions are a product of the same access-to-care barriers that result in hospitalizations for ambulatory care–sensitive conditions. Nonetheless, we performed additional analyses incorporating the Elixhauser comorbid condition measure and found that its inclusion did not substantially affect our findings. Therefore, to simplify the presentation, we have chosen not to display these results.

Except for the managed care indicator variable, all potential confounders were measured when beneficiaries enrolled in Medicaid. We classified Medicaid beneficiaries as being in managed care depending on whether they spent most of their enrollment time before a hospitalization for an ambulatory care–sensitive condition in managed care. This was necessary because some beneficiaries changed between fee-for-service and managed care during their enrollment time.

Statistical Analysis

We performed descriptive analysis of the characteristics of Medicaid beneficiaries who did and did not have an interruption in their Medicaid coverage and the frequency distribution of the durations of Medicaid coverage interruptions. We also compared the number and frequency of hospitalizations for ambulatory care–sensitive conditions between beneficiaries with and those without an interruption in coverage.

We used the life-table technique to calculate the probability over time of hospitalizations for ambulatory care–sensitive conditions among beneficiaries with continuous and interrupted Medicaid coverage and a Cox proportional model to compare the average change in relative hazard associated with interrupted Medicaid coverage (13). We adopted the statistical techniques for time to recurrent events (14). The first exposure period starts on the first month of eligibility or the beginning of the study period (1 January 1998) for beneficiaries who were already enrolled in the program. An exposure period ends with a hospitalization for an ambulatory care–sensitive condition, at the end of the study period (31 December 2002), with a person turning 65 years old, or with an interruption in coverage (**Appendix Figure**, available at www.annals.org). A new exposure period begins after an interruption in Medicaid eligibility or a hospitalization for an ambulatory care–sensitive condition. Because the Medicaid eligibility file includes only a month but not a day of enrollment, we assigned the date of enrollment as the 15th of the month. After determining from the Cox model what the average effect of an interruption in Medicaid coverage was on the risk for a hospitalization for an ambulatory care–sensitive condition, we introduced an interaction term for time after an interruption in Medicaid coverage to estimate the duration of the increased risk for such a hospitalization.

Recognizing that other beneficiary characteristics could affect the hospitalization rate and confound our results, we used a Cox proportional hazard model to control for demographic characteristics (age, sex, and race or ethnicity), health care delivery model (fee-for-service or managed care), other insurance coverage (yes or no), temporal trend (year of admission), and Medicaid eligibility categories.

Through readmissions, a patient could contribute more than 1 observation, although this is extremely rare in our data (2%). Nevertheless, we used the fixed-effect partial likelihood method with an aggregate covariance structure to calculate the *P* values. Furthermore, to test for the robustness of our findings, we estimated several variants of our basic model. First, we estimated a model that included a covariate for the number of previous hospitalizations to control for dependence among observations from unmeasured characteristics (15). The parameter estimates and the *P* values generated from this model did not substantially differ from the ones we report in this article and therefore are not displayed. Second, we conducted the analysis on the subset of our data representing only the first hospitalization. We found that the effect of discontinuity was even stronger for this subset than those reported here. Third, because beneficiaries who enrolled before January 1998 (44%) have episode durations with unknown starting times (left-censored), we reestimated our model on the subpopulation of beneficiaries who enrolled after January 1998. Again, our results were very similar and are not displayed. Fourth, we conducted stratified analyses by aid groups to determine whether SSI-eligible Medicaid beneficiaries who tend also to have Medicare coverage were less susceptible to excess hospitalizations for ambulatory care–sensitive conditions in association with an interruption in Medicaid coverage than those who were eligible through

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Table 1. **Characteristics of the California Medicaid Population Age 18 to 64 Years with Continuous and Interrupted Coverage, 1998 to 2002**

Characteristic	Continuous Coverage	Interrupted Coverage	*P* Value*
Beneficiaries, *n* (%)	1 797 479 (38)	2 938 318 (62)	
Mean age, *y*	34	32	<0.001
Female, %	62	66	<0.001
Race/ethnicity, %			
Hispanic	43	47	<0.001
White	27	29	<0.001
Black	12	10	<0.001
Asian	12	8	<0.001
Other†	6	6	1.00
Aid group, %			
TANF	33	28	<0.001
SSI	26	8	<0.001
Other	40	64	<0.001
Managed care, %	20	20	1.00
Other insurance, %	15	9	<0.001
Hospitalized for ambulatory care–sensitive condition, %	2	1	<0.001

SSI = Supplementary Security Income; TANF = Temporary Assistance to Needy Families.

* All *P* values for categorical variables were obtained from differences in the proportion test (*z* test) with Yates correction. The *P* value for age in years was obtained from the *t* test.

† Includes beneficiaries with missing information on race/ethnicity.

TANF or other categories of persons who do not typically have another form of coverage.

To estimate Medicaid's financial responsibility for hospitalizations for ambulatory care–sensitive conditions among beneficiaries who had an interruption in Medicaid coverage, we used health insurance data in the hospital discharge database to create summary counts by source of payment for beneficiaries who were hospitalized after an interruption in coverage.

The protocol for this data analysis was reviewed and approved by the University of California, San Francisco, institutional review board and the California Committee for the Protection of Human Subjects.

Role of the Funding Source

This study was funded by The Commonwealth Fund. The funding source had no role in conceptualization, design, conduct, or analysis of this study or in the decision to submit the manuscript for publication.

RESULTS

There were 4 735 797 persons between the ages of 18 and 64 years who received Medicaid for at least 1 month during the study period. A total of 62% of eligible beneficiaries, including 58% of TANF, 33% of SSI, and 72% of other beneficiaries, experienced some interruption in their Medicaid coverage during the study period; 80% had only 1 interruption. Beneficiaries with an interruption in Medicaid coverage were younger and were more likely to be female and Hispanic. They were also less likely to be eligible for Medicaid through TANF or SSI or to have other insurance (**Table 1**). The mean duration of an interruption was 25 months, and 32% of beneficiaries had an interruption in their Medicaid coverage of less than 1 year during the study period.

More hospitalizations for ambulatory care–sensitive conditions occurred among beneficiaries with continuous rather than interrupted Medicaid coverage (**Table 2**). The distribution of specific ambulatory care–sensitive conditions was similar in the 2 groups.

Beneficiaries who experienced an interruption in Medicaid coverage had a substantially higher rate of hospitalization for an ambulatory care–sensitive condition than did those who did not experience an interruption (hazard ratio, 7.99 [95% CI, 7.88 to 8.11]) (**Figure**). As reflected in the **Figure**, when we incorporated a term into the model for the interaction between an interruption in Medicaid coverage and the time after that interruption, we found that the increased risk for a hospitalization for an ambulatory care–sensitive condition largely occurred in the first 3 months after an interruption in coverage.

After adjustment for demographic, programmatic, and temporal differences, beneficiaries with interrupted coverage had a higher hospitalization rate than beneficiaries with no interruption (adjusted hazard ratio, 3.66 [CI, 3.59 to 3.72]) (**Table 3**). Beneficiaries who were older, black, or Hispanic; were eligible for Medicaid through SSI or aid categories other than TANF; or were receiving services through managed care also had a higher risk for hospitalization.

The impact of an interruption in Medicaid coverage varied somewhat for the different eligibility groups. The adjusted relative risk for hospitalization was 1.72 (CI, 1.67 to 1.76) for SSI-eligible beneficiaries, 8.56 (CI, 8.06 to 9.08) for TANF-eligible beneficiaries, and 4.98 (CI, 4.85 to 5.12) for other beneficiaries.

Table 2. **Hospitalizations for Ambulatory Care–Sensitive Conditions among California Medicaid Beneficiaries Age 18 to 64 Years, 1998 to 2002**

Ambulatory Care–Sensitive Condition	Continuous Coverage, *n* (%)	Interrupted Coverage, *n* (%)
Asthma	17 529 (12)	2784 (14)
Angina	2999 (2)	406 (2)
Congestive heart failure	34 516 (23)	4346 (22)
Chronic obstructive pulmonary disease	24 468 (16)	2697 (14)
Dehydration	6937 (5)	782 (4)
Diabetes	26 477 (18)	4539 (23)
Hypertension	1832 (1)	305 (2)
Lower-extremity amputation	5008 (3)	682 (3)
Pneumonia	19 829 (13)	2142 (11)
Ruptured appendix	37 (<1)	4 (<1)
Urinary tract infection	9255 (6)	1127 (6)
Total	148 887 (100)	19 814 (100)

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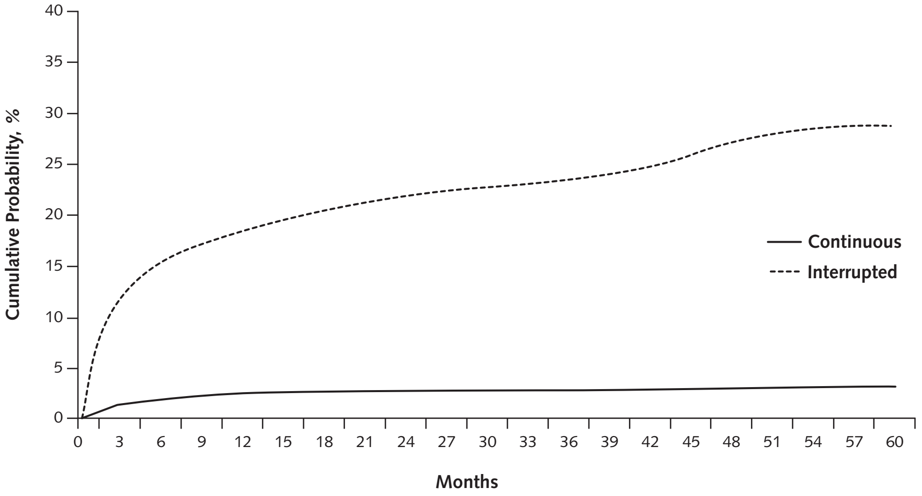
Figure. **Probability of hospitalization for an ambulatory care–sensitive condition over time, by Medicaid coverage status, 1998 to 2002.**



Hazard ratio^2, 7.99 (95% CI, 7.88–8.11).

Using health insurance status information available in the hospital discharge data, we found that, among beneficiaries with interruption in Medicaid coverage, 45% regained Medicaid for the hospitalization, 43% had another form of insurance, and 11% were uninsured.

DISCUSSION

We found substantially higher hospitalization rates for ambulatory care–sensitive conditions associated with an interruption in Medicaid coverage. Hospital admissions for ambulatory care–sensitive conditions are indicative of the quality of health care that people receive outside the hospital (16) and reflect a decline in the health status of persons requiring such hospitalizations (6).

Our findings are consistent with a previous study that found higher rates of psychiatric hospitalization among Medicaid patients with mental illness when they had interruptions in their Medicaid coverage (17). Similarly, studies among Medicaid beneficiaries with breast cancer have found that the diagnostic stage and survival are better among persons who have Medicaid at the time of diagnosis than among those who obtain coverage after diagnosis (18, 19). Although these earlier studies found that serious health consequences were associated with interruptions in Medicaid coverage, the inferences that could be drawn from them are generally limited because of the studies' cross-sectional designs, relatively small sample sizes, and inability to control for differences in the health status of persons with continuous versus interrupted coverage.

Our study builds on earlier ones that explored interruptions in Medicaid coverage by including a broader set of clinical diagnoses and a larger sample size. The link between patient discharge data and Medicaid eligibility files allowed us to look at the contribution of interrupted coverage over time.

The study has several limitations. First, we could not completely capture the insurance status for the group of patients that has interrupted Medicaid coverage. We know on a month-by-month basis whether a person is covered by Medicaid, but if that coverage is disrupted, we do not always know whether the person lost Medicaid coverage because he or she became uninsured or gained private

Table 3. **Adjusted Risk for Hospitalization for an Ambulatory Care–Sensitive Condition among California Medicaid Beneficiaries Age 18 to 64 Years, 1998 to 2002***

Beneficiary Characteristic	Relative Risk (95% CI)
Interrupted coverage	3.66 (3.59–3.72)
Age	1.06 (1.06–1.06)
Female	1.04 (1.03–1.05)
Race/ethnicity	
White	1.00 (reference)
Hispanic	2.46 (2.40–2.52)
Black	4.44 (4.33–4.54)
Asian	0.79 (0.76–0.81)
Other†	2.78 (2.71–2.84)
Aid group	
TANF	1.00 (reference)
SSI	9.21 (9.07–9.35)
Other	1.81 (1.77–1.85)
Managed care	1.48 (1.46–1.50)
Other coverage	1.07 (1.06–1.09)

SSI = Supplementary Security Income; TANF = Temporary Assistance to Needy Families.

* In addition to adjustment for the variables listed, the model includes an indicator variable for year of admission. Confidence intervals are corrected for clustering of observations.

† Includes beneficiaries with missing information on race/ethnicity.

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health insurance or some other type. The linked data set of Medicaid eligibility and statewide hospital discharges provided us with a unique insight into the subsequent insurance status of patients who once had Medicaid coverage. However, because only a small portion of beneficiaries are hospitalized, we cannot say with certainty whether the pattern of insurance coverage we observed in the hospitalized group is the same for all beneficiaries who lost Medicaid coverage.

Second, misclassification of persons with interrupted Medicaid coverage as being continuously covered was possible. Medicaid-eligible patients without insurance coverage who are hospitalized are sometimes given retroactive Medicaid coverage for up to 3 months that precede the hospitalization. The intention is to cover these individuals' health expenses that were presumed to have occurred before the hospitalization. Retroactive eligibility is most common among Medicaid beneficiaries in the medically needy aid category (20). We categorized medically needy beneficiaries in the aid group other than TANF or SSI, and by controlling for aid category in our analysis, we have reduced the effect of misclassification bias. Furthermore, retrospective coverage would result in our misattributing the hospitalizations for patients with interrupted coverage to those covered by Medicaid. Had we been able to identify these errors, we would expect that correcting them would only increase the difference in hospitalizations for ambulatory care–sensitive conditions we observed between patients with continuous Medicaid coverage and those whose coverage was interrupted.

Third, our findings are not from a randomized trial, and the results might be confounded by patients' health status or some other unmeasured difference in the groups. Assuming patients who have interrupted Medicaid coverage are more likely to have better health status (21), this would introduce a conservative bias toward finding higher rates of hospitalization among Medicaid beneficiaries with continuous coverage. Our analytic approach, however, minimizes this bias. Our study includes patients who have not experienced any interruption in coverage as well as those who have had some periods of interruption. We compared the hospitalization rates before and after the interruption in coverage among the subset experiencing an interruption. In this way, patients serve as their own controls. Furthermore, we control for Medicaid eligibility status that, to some degree, serves as a proxy for health status and determined that the addition of a comorbid condition measure to our analysis had no substantial impact on the results.

Fourth, we do not have any direct measures of use of ambulatory care service among patients who have and have not experienced an interruption in Medicaid coverage. Previous research has demonstrated that loss of health insurance is associated with fewer ambulatory care visits and less use of medication (22). The data available for this study do not include information on ambulatory care services, but our analysis of TANF and SSI beneficiaries provides some insight. We found that even though SSI beneficiaries have a higher rate of hospitalizations for ambulatory care–sensitive conditions than that of TANF beneficiaries, the relative risk for a hospitalization for an ambulatory care–sensitive condition after an interruption in Medicaid coverage was substantially higher for TANF beneficiaries than for SSI beneficiaries. This is consistent with the fact that, unlike TANF beneficiaries, those eligible for Medicaid through SSI for the most part retain coverage for physician services through Medicare. The higher rate of hospitalizations for ambulatory care–sensitive conditions in SSI beneficiaries who have had an interruption in Medicaid coverage may largely reflect the loss of prescription drug coverage that was not available through Medicare at the time of this study, whereas the relatively greater increase in these hospitalizations for TANF beneficiaries in association with an interruption in Medicaid coverage represents a loss of both prescription and physician services.

Finally, our study examines retrospective data from only 1 state. However, approximately 1 in 6 Medicaid beneficiaries resided in California during our study period (23). We are not aware of any substantial changes in California's Medicaid policies since 2002 that would make the findings less relevant today. If anything, we suspect that interruptions in Medicaid coverage may have become more frequent in association with federal requirements for enhanced documentation of citizenship in association with pursuing eligibility for Medicaid (24).

There is a widespread recognition that too many U.S. citizens are uninsured. Although public insurance programs can make an important difference in reducing the number of uninsured persons, the success of these programs is also related to their administrative burden. Policies that reduce the frequency of interruptions in Medicaid coverage might prevent some of the health events that require hospitalization and trigger high-cost health care spending.

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Reproducible Research Statement: *Study protocol and statistical code:* Available from Dr. Bindman (e-mail, abindman@medsfgh.ucsf.edu). *Data set:* Available by request from the California Department of Health Care Services.

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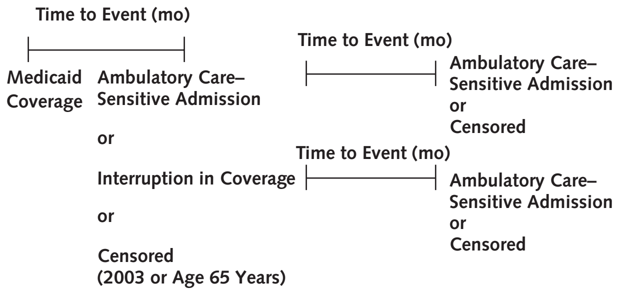
Collection and assembly of data: A. Chattopadhyay, G.M. Auerback.

Appendix Figure. **Diagrammatic representation of repeatable events with time-varying covariates.**



The exposure time starts on the first month of eligibility or at the beginning of the study period (1 January 1998) for beneficiaries who were already enrolled in the program. An exposure period ends with a hospitalization for an ambulatory care–sensitive condition, at the end of the study period (31 December 2002), with a person turning 65 years of age, or with an interruption in coverage. A new exposure time begins after an interruption in Medicaid eligibility or hospitalization for an ambulatory care–sensitive condition.

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Medicaid and Mortality: New Evidence from Linked Survey and Administrative Data
Sarah Miller, Sean Altekruse, Norman Johnson, and Laura R. Wherry
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ABSTRACT

We use large-scale federal survey data linked to administrative death records to investigate the relationship between Medicaid enrollment and mortality. Our analysis compares changes in mortality for near-elderly adults in states with and without Affordable Care Act Medicaid expansions. We identify adults most likely to benefit using survey information on socioeconomic and citizenship status, and public program participation. We find a 0.132 percentage point decline in annual mortality, a 9.4 percent reduction over the sample mean, associated with Medicaid expansion for this population. The effect is driven by a reduction in disease-related deaths and grows over time. We find no evidence of differential pre-treatment trends in outcomes and no effects among placebo groups.

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The Medicaid program is the largest health insurance provider for low income individuals in the United States. Established in 1965, Medicaid currently covers over 72 million enrollees and represents over $500 billion in government spending annually (Centers for Medicare & Medicaid Services, 2019a,b). However, despite the size and scope of this program, we know relatively little about whether Medicaid actually improves the health of its beneficiaries. This is particularly true for low income adults who gained Medicaid eligibility under the Affordable Care Act (ACA), and who are the focus of nearly all of the ongoing policy debate surrounding the program. Studies of the health effects for this group tend to rely on self-reported assessments of health with inconsistent findings across data sources (e.g. Cawley et al., 2018; Courtemanche et al., 2018b; Miller and Wherry, 2017; Sommers et al., 2017). Meanwhile, evidence using objective measures of health, such as mortality, is often inconclusive due to small sample sizes (Baicker et al., 2013; Finkelstein et al., 2012), or the lack of available data linking mortality to individual Medicaid eligibility (Black et al., 2019). The inconclusive nature of these results has led to skepticism among some researchers, policymakers, and members of the media as to whether Medicaid has any positive health impacts for this group.[1]

Understanding what types of public programs, if any, are effective at improving the health of low-income individuals is especially important given that they experience dramatically higher mortality rates and worse health outcomes on a number of dimensions than the general population. For example, the annual mortality rate for individuals ages 55 to 64 in households earning less than 138 percent of the Federal Poverty Level (FPL) is 1.7 percent, more than 4 times higher than the 0.4 percent rate experienced by higher-income individuals of the same age.[2] This low-income group also experiences higher risks of dying from diabetes (by 787%), cardiovascular disease (552%), and respiratory disease (813%) relative to those in higher income households; all of these diseases are at least somewhat amenable to drug therapy. These higher rates of death translate to dramatic differences in life expectancy across income groups. For example, Chetty et al. (2016) find that men at the bottom of the income distribution live on average nearly 15 years less, and women over 10 years less, than those at the top of the income distribution conditional on surviving to age 40. While data from nearly all countries show a positive correlation between income and health, this correlation is stronger in the United States than other high income countries (Semyonov et al., 2013).

Medicaid could play a crucial role in reducing these disparities if it improves access to effective medical care that beneficiaries would not otherwise receive, and recent research suggests this is likely to be the case. For example, Ghosh et al. (2019) find a substantial increase in prescription drug utilization under the ACA Medicaid expansions, including medications for the management of diabetes, treatments for HIV and Hepatitis C, and drug therapies for cardiovascular disease. These particular types of prescription drugs are among those demonstrated to reduce mortality.[3] Changes in access to these

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[1] Flagged as an example of this by Sommers et al. (2017), Congressman Raul Labrador stated that "nobody dies because they don't have access to health care" during a discussion of Medicaid (Phillips, 2017). Also, Goodman-Bacon et al. (2017) provide a review of media discussion and some academic research suggesting that Medicaid may in fact be harmful to health.

[2] Authors' calculations using death rates from 2008 to 2013 derived from the publicly-available National Health Interview Survey Linked Mortality File (National Center for Health Statistics, 2019) for adults with incomes below 138% FPL and those with incomes 400% FPL or greater. We chose these two income cutoffs since adults with incomes below 138% FPL qualify for Medicaid in states that expanded their programs to include low-income adults under the ACA; also, adults with incomes below 400% FPL qualify for subsidies for private insurance coverage.

[3] Systematic reviews and meta-analyses of randomized, controlled trials find significant decreases in all-cause and

medications are likely to be particularly important for this population given their higher prevalence of chronic disease (Karaca-Mandic et al., 2017). Medicaid coverage may also affect health if it leads to earlier detection and treatment of life-threatening health conditions. Existing research has documented increased screening of treatable cancers such as breast and cervical cancer with expanded Medicaid coverage (Finkelstein et al., 2012; Sabik et al., 2018), as well as the detection of cancer both overall and at an early stage (Soni et al., 2018) and improved access to cancer surgery (Eguia et al., 2018). Furthermore, Medicaid coverage increases the number of hospitalizations, procedures performed in the hospital, and the number of emergency department visits for conditions that require immediate care (Duggan et al., 2019; Finkelstein et al., 2012; Taubman et al., 2014), all of which are likely to be associated with serious medical issues that require treatment. In addition to increasing the provision of these types of ostensibly high value services, Medicaid also increases the use of a variety of other types of medical care such as routine screening for chronic illnesses, outpatient physician visits, use of prescription drugs that aid in smoking cessation, and dental care which also have the potential to improve health over the longer term.[4]

In this paper, we provide new evidence of the impact of Medicaid on health by using administrative mortality data linked to large-scale, individual survey records. We use this novel dataset to examine the impact of a sizeable Medicaid eligibility expansion that occurred in some states as the result of the ACA. In 2014, the ACA expanded eligibility for the Medicaid program to include all adults in families with incomes under 138 percent of the Federal Poverty Level (FPL). Previously only pregnant women, adults with disabilities, and very low income parents tended to qualify for Medicaid coverage. Although intended to apply to all states, a 2012 Supreme Court decision made the Medicaid eligibility expansion optional. As a result, only 29 states and the District of Columbia expanded coverage in 2014, with 7 additional states electing to expand over the next several years. Despite non-universal adoption, the ACA Medicaid expansions still represent a historic expansion in insurance coverage. Approximately 13.6 million adults gained Medicaid coverage under the ACA (Medicaid and CHIP Payment and Access Commission, 2018); for comparison, Medicare enrolled about 19 million elderly beneficiaries after its creation in 1965 (Bureau of the Census, 1969). We take advantage of variation in state adoption of this large expansion in coverage to compare changes in mortality among individuals in expansion states and non-expansion states.

In contrast to prior research that relies on death certificate data with limited information on individual characteristics, our data include detailed survey measures collected from the 2008 to 2013 years of the American Community Survey (ACS). This large-scale national survey contains approximately 4 million respondents in each year and allows us to observe information on specific characteristics that determine Medicaid eligibility including income, citizenship status, and the receipt of other social assistance. With this information, we are able to identify individuals who were most likely to benefit from the ACA Medicaid eligibility expansions and, in this way, overcome the inherent limitations present in existing studies that rely only on aggregate death records. We focus on those in this group who were

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cardiovascular mortality for adults who receive statins (Chou et al., 2016) and decreased all-cause mortality for Type 2 diabetics receiving glucose-lowering drugs (Zheng et al., 2018). In addition, systematic reviews of observational studies indicate decreased mortality among HIV-infected adults initiating anti-retroviral therapy (Chou et al., 2005), as well as indirect evidence of decreased mortality linked to cured infection under antiviral treatment for Hepatitis C (Moyer, 2013).

[4] See, for example, Finkelstein et al. (2012); Nasseh and Vujicic (2017); Semyonov et al. (2013). Gruber and Sommers (2019) provide a summary of the evidence to date on the impact of the ACA expansions on health care utilization.

between the ages of 55 and 64 in 2014, who are at greater risk of mortality, although we also present results for all non-elderly adults. We follow individuals in our sample over time to examine changes in mortality associated with Medicaid expansion by linking them to the Census Numident file, which contains administrative records on the date of death for all individuals with Social Security Numbers (SSNs) who die in the United States. This file allows us to observe mortality rates for our sample through 2017, four years after the initial ACA Medicaid eligibility expansions. Despite the high-quality of the death information in the Census Numident file, it does not include cause of death information. In supplemental analyses, we further examine changes in mortality by the underlying cause of death using data from the Mortality Disparities in American Communities (MDAC) project, which links the 2008 year of the ACS to death certificate records using the National Death Index.

Our analysis shows that the ACA Medicaid expansions reduced mortality among this targeted group. Prior to the expansions, individuals in our sample residing in expansion and non-expansion states had very similar trends in both Medicaid coverage and mortality. At the time of the expansion, the trajectories of these two groups diverged significantly, with expansion state residents seeing increases in Medicaid coverage and overall insurance coverage, and decreases in annual mortality rates. In the first year following the coverage expansion, the probability of mortality declined by about 0.089 percentage points, or 6.4 percent relative to the sample mean. The estimated impact of the expansions increases over time, suggesting that prolonged exposure to Medicaid results in increasing health improvements. By year 4, residents of expansion states have an annual mortality rate that is 0.208 percentage points lower than their non-expansion state counterparts. In our supplemental analysis using the MDAC data, we find evidence that healthcare amenable and internal (disease-related) causes of death were reduced by the expansions, but no evidence that deaths due to external causes, such as car accidents, fell. We also conduct several placebo tests to assess the validity of our analysis including examining the impact of the expansions on those age 65 or older in 2014 who did not gain Medicaid eligibility; examining the effect on individuals in higher income households who were less likely to be affected; and, restricting the analysis sample to the pre-ACA period. We find no relative change in coverage or mortality across expansion and non-expansion states among the elderly or in the pre-ACA period, settings in which no Medicaid expansion occurred. Among those in higher income households, we find small but statistically significant increases in Medicaid coverage and similarly small decreases in mortality, consistent with a causal impact of Medicaid on mortality.

Our analysis provides new evidence that Medicaid coverage reduces mortality rates among low-income adults. Our estimates suggest that approximately 15,600 deaths would have been averted had the ACA expansions been adopted nationwide as originally intended by the ACA. This highlights an ongoing cost to non-adoption that should be relevant to both state policymakers and their constituents.

1 Background

Many studies have shown that Medicaid coverage increases access to and use of health care and reduces financial burden for low-income adults,[5] but evidence as to whether it improves their health remains limited. Studies that do examine health tend to rely on self-reported health measures from survey

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[5] See, e.g., Abramowitz, 2018; Allen et al., 2017; Baicker et al., 2013; Brevoort et al., 2019; Buchmueller et al., 2016; Caswell and Waidmann, 2017; Courtemanche et al., 2017; Finkelstein et al., 2012; Gallagher et al., 2019; Ghosh et al., 2019; Hu et al., 2018; Miller and Wherry, 2017, 2019; Simon et al., 2017; Sommers et al., 2015, 2017.

data. The evidence from these studies spans from estimated large or modest improvements in reported health associated with Medicaid expansion (Cawley et al., 2018; Simon et al., 2017; Sommers et al., 2016, 2017), to no effects (Courtemanche et al., 2018a,b; Wherry and Miller, 2016) or even small but marginally significant negative effects (Miller and Wherry, 2017).[6]

One concern with self-reported health data is that it may not accurately measure changes in physical health. In the Oregon Health Insurance Experiment (OHIE), low-income adults selected by a lottery to apply for Medicaid coverage reported near immediate improvements in their health compared to the controls, despite experiencing no significant differences yet in their health care utilization (Finkelstein et al., 2012).[7] The researchers concluded that the change in reported health may at least partly capture a general sense of improved well-being, or "winning" effects resulting from individuals' lottery selection. There is also the risk that changes in self-reported health may reflect increasing awareness of health problems or interactions with the health care system, rather than actual changes in physical health. One example would be increased contact with health providers leading to new information about a previously undiagnosed illness and, as a consequence, a worsened self-perception of health. This could bias downwards estimates of the effect of public health insurance on health.[8] Finally, in general, the reliability of self-reported health measures for U.S. adults and their association with objective health measures are documented to be worse among lower socioeconomic status groups (Dowd and Zajacova, 2007, 2010; Zajacova and Dowd, 2011).

In addition to offering the first experimental evidence on the effects of expanded Medicaid, the Oregon Health Insurance Experiment (OHIE) covered new ground by collecting data on clinical health measures among its participants. The researchers did not observe significant effects on any of the collected measures (blood pressure, cholesterol, and blood sugar levels). Using administrative data, they also found no evidence that Medicaid coverage led to a reduction in mortality during the 16 months following coverage gain. Their estimate suggested a 16 percent reduction in mortality associated with acquiring Medicaid, but with a large confidence interval that could not rule out sizeable changes in either direction.[9]

As the data become available, researchers are beginning to evaluate the mortality effects of the ACA Medicaid expansions.[10] Two current studies use population-level mortality data to estimate changes

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[6] Note that neither the time period of study nor the data sources used seem to explain these inconsistencies. In a series of papers studying expansions in two states (AR and KY), Sommers et al. (2015, 2016, 2017) find evidence of significant health improvements emerging only in the second year of expansion. However, in a national study, Courtemanche et al. (2018a,b) find no evidence of improvements in self-reported health due to expansion during any of the first three years of implementation. Using the same data source as these authors, Simon et al. (2017) and Cawley et al. (2018) find evidence of sizable health improvements over the same period. Finally, Miller and Wherry (2019) trace out the effects of Medicaid expansion during each of the first four years of implementation, using a different data source, and find no evidence of improvements in self-reported health.

[7] The researchers found an improvement in self-reported health for the treatment group during their initial survey, which was conducted, on average, about one month after gaining coverage, that was about two-thirds of the size of their main effect estimated using survey data collected more than a year later.

[8] This bias could also operate in the opposite direction if increased interaction with providers improves one's perception of health. See Currie and Gruber (1995) for more discussion.

[9] Another relevant randomized social experiment provided Medicare to newly entitled Social Security Disability Insurance (SSDI) beneficiaries (as opposed to them being subject to a 2-year waiting period for coverage). The evaluation of this experiment found no reductions in mortality up to 3 years later but the sample sizes were too small to be able to detect effects (Weathers and Stegman, 2012).

[10] A separate but related literature has examined the relationship between public health insurance and child mortality using variation in exposure tied to the introduction of Medicaid and later expansions in public coverage under Medicaid and

in adult mortality in expansion states compared to non-expansion states. In contrast to Oregon, the ACA Medicaid expansions affected a much larger number of people (13.6 million vs. under 11,000) (Finkelstein et al., 2012; Medicaid and CHIP Payment and Access Commission, 2018). However, the authors rely on death certificate data without the information on individual income needed to identify the policy's target population. As a consequence, it can be difficult to detect effects at the population-level, particularly when Medicaid coverage is estimated to have increased by as little as 1 percentage point among all nonelderly adults (Black et al., 2019). The two studies examining the effects of the ACA Medicaid expansions in this manner reach different conclusions, detecting no (Black et al., 2019) and sizeable effects on adult mortality (a 3.6% reduction) (Borgschulte and Vogler, 2019). In addition, research on pre-ACA expansions in Medicaid that also relies on aggregated data finds larger effects on adult mortality. Sommers et al. (2012) and Sommers (2017) find a 6 percent reduction in nonelderly adult mortality in pre-ACA Medicaid expansions in New York, Maine, and Arizona measured over a five-year period.[11] The absence of conclusive evidence on whether Medicaid improves the objective health of adult beneficiaries is a major omission given that Medicaid is a public health program that aims to improve access to and use of efficacious health care.

All of these studies rely on changes in survival for the Medicaid eligible to translate into overall mortality effects observable at the population (or state) level. However, at least two studies suggest that a focus on subgroups most at risk for mortality may increase the likelihood of detecting effects. Swaminathan et al. (2018) examine the impact of the ACA Medicaid expansions on the one-year survival rate of patients with end stage renal disease initiating dialysis. The authors find a significant 8.5 percent reduction in mortality for individuals with this chronic condition, driven primarily by a decrease in deaths due to causes considered health care amenable. More recently, Khatana et al. (2019) find evidence of a decrease in rates of cardiovascular disease among adults ages 45-64 associated with state adoption of the ACA Medicaid expansions.

For this reason, it is likely that the primary impediment to analyzing the impact of Medicaid on mortality has been data availability. Data from death certificate records contain very little socioeconomic information on the decedent; in particular, they contain no information on the decedent's income, whether he or she previously had health insurance coverage, or other characteristics that might affect Medicaid eligibility. Without data that links information on individual Medicaid eligibility and mortality, researchers must rely on eligibility changes over larger population groups – for example, residents of certain states or counties – which contain many individuals who are not directly affected by Medicaid policy. This decreases the power to detect changes in mortality of a plausible magnitude, leading some researchers to conclude that "it will be extremely challenging for a study [on the ACA Medicaid expansions] to reliably detect effects of insurance coverage on mortality unless these data can be linked at the individual level to large-sample panel data" (Black et al., 2019).

This finding of mortality effects for certain subgroups that may not be detectable in larger aggregations of data is consistent with existing work on the effects of Medicare on health. Card et al.

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the Children's Health Insurance Program. For the most part, these studies have found significant declines in mortality associated with expanded coverage for infants and children both in the short-term (e.g. Currie and Gruber, 1996a,b; Goodman-Bacon, 2018b; Howell et al., 2010) and long-term (Brown et al., 2018; Goodman-Bacon, 2016; Wherry and Meyer, 2016).

[11] In addition, an analysis of the mortality effects of insurance expansion under Massachusetts's 2006 health reform by Sommers et al. (2014) finds a significant 2.9 percent reduction in all-cause mortality over four years of follow-up.

(2004) and Finkelstein and McKnight (2008) find little evidence of an effect of Medicare on mortality using death certificate records. However, among those who are hospitalized and severely ill, Card et al. (2009) find a significant 1 percentage point (or 20 percent) reduction in mortality following admission that persists for at least 9 months following discharge. This analysis notably identifies these effects by comparing patients just below and above the Medicare-eligible age of 65 when admitted, which is just above the age range considered in our analyses.

It is also worth noting that, at the time of these studies, Medicare did not provide coverage for prescription drugs. Recent papers studying the introduction of prescription drug coverage under the Medicare Part D program find evidence of mortality declines. Huh and Reif (2017) focus on those age 66 and find that insurance coverage for prescription drugs reduces mortality in this group by about 0.16 percentage points annually (about 9.6 percent). Dunn and Shapiro (2019) find slightly larger effects in an analysis that incorporates individuals with older ages. For both papers, reductions in mortality are driven by a decline in deaths due to cardiovascular disease. Using data for a subset of Medicare beneficiaries, Kaestner et al. (2017) find no significant effect on mortality but do document reductions in hospitalization admissions for heart disease, respiratory disease, and diabetes under the program. Importantly, sizeable increases in the use of prescription drugs that treat these particular diseases have been documented under the ACA Medicaid expansions (see Ghosh et al., 2019).

2 Data and Outcomes

To conduct our analysis, we use data from two sources. First, we select respondents from the 2008 to 2013 waves of the American Community Survey who, based on their pre-ACA characteristics, were likely to benefit from the ACA Medicaid expansions. We include only individuals who either are in households with income at or under 138 percent of the FPL or who have less than a high school degree. Since we only have information on income captured at one point in time, the latter criterion is used to identify individuals who are of low socioeconomic status but might not meet the income cutoff at the time of the ACS interview.[12] We exclude non-citizens, many of whom are not eligible for Medicaid, and those receiving Supplemental Security Income (SSI), who are likely to be Medicaid eligible in the absence of the expansions.[13] We restrict our primary analysis to individuals who were ages 55 to 64 in 2014. This higher age group has relatively high mortality rates, and is also consistent with the sample criteria used in Black et al. (2019). We present results for all non-elderly adults in a supplementary analysis. Finally, we exclude residents of 4 states and DC that expanded Medicaid to low-income adults prior to 2014.[14] There are approximately 566,000 respondents who meet our sample criteria.[15]

Descriptive statistics for the sample by state Medicaid expansion status are reported in Table A1. The average age of the respondents in the two groups is similar. However, individuals in expansion states are slightly better off with higher average income (147% of the FPL vs. 140%) and educational attainment (45.3% with less than high school education vs. 46.8%), as well as lower baseline rates of

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[12] Results are similar if we include only those with less than a high school degree, or only those with incomes under 138 percent of the FPL, rather than defining the sample using the union of these two criteria.

[13] SSI recipients are automatically eligible for Medicaid coverage in most states.

[14] DE, MA, NY, and VT all expanded coverage to individuals with incomes reaching the poverty line or greater prior to the ACA; DC received approval to implement its ACA Medicaid expansion early with enrollment starting in 2011.

[15] Note that Census disclosure rules prohibit the disclosure of exact sample sizes and require rounding. All sample sizes reported in this paper are therefore rounded according to these rules.

uninsurance (32.6% vs. 37.3%), than individuals in non-expansion states. In addition, individuals in expansion states are more likely to be white or Hispanic, while a higher share of those in non-expansion states are black.

These data are linked to the Census Numident file. The Census Numident file is derived from the Social Security Administration (SSA) Numerical Identification file, which includes information on date and county of birth and date of death (if it has occurred) for individuals with a Social Security Number (SSN). These data have been used in, e.g., Brown et al. (2018); Chetty et al. (2011, 2016); Dobbie and Song (2015); Sullivan and von Wachter (2009), and other research relying on death information from tax records. Total deaths reported in the SSA file by age and year closely track the numbers reported by the National Center for Health Statistics (Chetty et al., 2016). In addition to this death information from the SSA, the Census Bureau also has information on date of death from the National Death Index (NDI) for some individuals and years, which it incorporates into its date of death measure when available.[16] The Census Bureau updates its Numident file each year with new information from the SSA Numerical Identification file. It formats the data so that there is a single record per individual, reflecting the most accurate and up-to-date information at that point in time. We use the most recently available version of the Census Numident, which captures date of death through the second quarter of 2018. Because we observe only a partial year in 2018, we limit our analyses to deaths occurring in 2017 and earlier.

The Census Numident and ACS data are linked via the Census Bureau's Personal Identification Validation System (PVS). This system assigns individuals in each dataset a protected identification key (PIK), an anonymized identifier that allows Census to track individuals across datasets. Approximately 94 percent of all ACS respondents are successfully assigned a PIK using available information on name, address, and date of birth, with a slightly higher match rates for citizens (95 percent) (Wagner and Layne, 2014). The assignment of a PIK allows respondents in the ACS to be matched to the Census Numident file. PIKS for the Census Numident file are assigned using social security numbers (SSNs). Since our analysis is restricted to older citizens, and since nearly all American citizens have SSNs assigned by the time they reach adulthood (see Bernstein et al., 2018), we expect to have nearly full coverage of deaths in the Numident file.

Once these data are linked, we observe the vital status of each individual during the year they respond to the ACS and each subsequent year. For example, we observe the vital status of an individual who responds to the 2008 ACS during each year from 2008 through 2017; for an individual who responds to the 2013 ACS, we observe his or her vital status from 2013 through 2017. We construct our outcome measure to represent mortality during each calendar year. If the individual is alive in a given year, the outcome variable takes a value of 0; if that individual died in that year it takes a value of 1. Once an individual has died, he or she is removed from the sample for subsequent years. In this way, we will be able to measure changes in the annual probability of death during a given year among individuals who were alive at the beginning of that year.

The annual mortality rate is about 1.4 percent for our sample on average across all years, and approximately 1.3 percent among respondents in expansion states during the year just prior to expan-

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[16] The NDI collects detailed information on deaths from state vital statistics offices. Respondents to the 2008 ACS were linked to the NDI for the years 2008-2015, as part of the Mortality Disparities in American Communities project.

sion.[17] Note that because we have a fixed sample that ages in each period, mortality rates increase over time (i.e., our sample is oldest in the last year, 2017). In this way, our analysis tracks the mortality trajectory for a fixed cohort defined as adults ages 55 to 64 in 2014 and is representative of the outcomes over time for this group.

While our data uniquely offer the opportunity to link mortality and economic variables at the individual level, there are also several important limitations. First, we observe the economic characteristics of individuals (income and educational attainment, receipt of social services, and citizenship status) at the time they respond to the ACS in the pre-period, between 2008 and 2013. These are time-varying characteristics, however, and may not accurately reflect economic characteristics at the time of the Medicaid expansions for some members of our sample. For example, an individual in a low-income household in 2008 may be in a higher-income household by 2014, at the time the expansions occurred. Similarly, individuals may migrate to different states between the time they responded to the ACS and the time the expansions occurred, resulting in our misclassification of whether that individual was exposed to the eligibility expansion. Note, however, that individual migration decisions do not appear to be correlated with state Medicaid expansion (Goodman, 2017). In general, we expect that this type of misclassification will bias our estimates towards zero.

A second limitation is that our data do not include information on the cause of death. The death information in the Census Numident is derived primarily from the Social Security Administration death records, which contain only date of death. We therefore supplement our main analysis with data from the 2008 year of the ACS, which was linked to death certificate records from 2008 to 2015 as part of the Mortality Disparities in American Communities (MDAC) project. While this drastically reduces both the sample size and follow-up period, it does allow us to conduct exploratory analyses of changes in mortality based on the underlying cause of death as reported on the death certificate.

3 Empirical Strategy

Our empirical strategy looks at changes in annual mortality in the expansion states relative to the non-expansion states before and after the implementation of the ACA Medicaid expansions. We estimate this using an event-study model that allows us to assess the evolution of relative outcomes while controlling for fixed differences across states and national trends over time. We estimate:

$$Died_{isjt} = Expansion_s \times \sum_{\substack{y=-6 \\ y\neq -1}}^{3} \beta_y I(t - t_s^* = y) + \beta_t + \beta_s + \beta_j + \gamma \mathbf{I}(j = t) + \epsilon_{isjt}. \tag{1}$$

As described earlier, our data is constructed at the individual (i) by year (t) level. Each individual responds to the ACS during a survey wave (j) and reports their state of residence (s). The dependent variable $Died_{isjt}$ denotes death during each year t among individuals who were alive at the beginning of year t. We only observe mortality over a partial year during the year of the individual's ACS interview (j), since that individual had to be alive in order to respond to the survey. To account for this, we include an indicator variable that year t is the year that the individual responded to the ACS (i.e.,

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[17] These annual averages are calculated excluding mortality rates for individuals during their year of ACS interview.

that $j = t$).[18] In this equation, β_s denotes state fixed effects and β_j denotes fixed effects associated with each survey wave. β_t denotes calendar year fixed effects, which will account for general trends in mortality for all individuals in our sample, including their gradual aging over time.[19]

The variable $Expansion_s$ equals 1 if individual i was living in a state that opted to expand Medicaid eligibility between 2014 and 2017, and zero otherwise. Indicator variables $I(t - t_s^* = y)$ measure the time relative to the implementation year, t_s^*, of the expansion in each state, and are zero in all periods for non-expansion states.[20] While most states expanded in the beginning of 2014, some states expanded later in the year or in subsequent years. If a state expanded on or after July 1 of a given year, we code it as having expanded in the subsequent year.[21] The omitted category is $y = -1$, the year prior to the expansion. Therefore, each estimate of β_y provides the change in outcomes in expansion states relative to non-expansion states during year y, as measured from the year immediately prior to expansion. If mortality rates for expansion and non-expansion states were trending similarly prior to the ACA, we expect that estimated coefficients associated with event times $y = -6$ to $y = -2$ will be small and not statistically significant. We estimate equation (1) with a linear probability model and report heteroskedasticity-robust standard errors that are clustered at the state level. All analyses use ACS survey weights.

In addition to the event study analyses, we also present difference-in-differences (DD) estimates as a summary of the effect across all post-expansion years. These are estimated using the same equation except that the event study indicators are replaced with a single variable denoting an expansion state during the post period ($Expansion_s \times Post_t$). Recent work by Goodman-Bacon (2018a) shows that the estimates from a two-way fixed effects DD estimator that uses variation in treatment timing, such as this one, can be biased in the presence of time-varying treatment effects. However, we expect that this is unlikely to be a major concern in our context given that we have so few late adopter states and a relatively short post period.[22]

4 Results

4.1 Impact of ACA Expansions on Medicaid Eligibility and Enrollment

We first estimate the impact of the ACA Medicaid expansions on Medicaid eligibility and coverage for individuals similar to those in our sample. We consider changes in eligibility for Medicaid in addition to enrollment changes since eligible individuals are “conditionally covered” by the program, in the sense that they may choose to remain uninsured and enroll only when they become ill. This concept

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[18] Note that we do not have information on the date of the ACS interview. If we drop the observations for which we observe less than a full year of mortality our results are unchanged.

[19] Results are also virtually identical in a model that includes controls for gender, race, and single year of age.

[20] We group together $y \leq -6$ into a single indicator variable interacted with expansion status since we only observe $y < -6$ for late expander states.

[21] In our analyses, states that expanded Medicaid in 2014 are AR, AZ, CA, CO, CT, HI, IL, IA, KY, MD, MI, MN, NJ, NM, NV, ND, OH, OR, RI, WA, and WV. Michigan implemented their expansion in April 2014 with the remainder of states expanding in January 2014. States that we considered to have 2015 expansions are NH (implemented August 15, 2014), PA (January 1, 2015), and IN (February 1, 2015). We consider AK (September 1, 2015) and MT (January 1, 2016) to be 2016 expansion states and LA (July 1, 2016) to be a 2017 expansion state.

[22] In his paper, Goodman-Bacon (2018a) compares the DD estimate to the average of the event study coefficients in the post period to assess whether there this type of bias may be present. When we undertake a similar exercise for our main specification, we arrive at a similarly sized estimate (-0.001320 DD estimate vs. -0.001367 average of ES coefficients), providing some reassurance that this is likely not a concern for our analyses.

of conditional coverage was first discussed by Cutler and Gruber (1996) in their study of historic Medicaid expansions for pregnant women and children; it may be even more relevant in our context, however, given another change under the ACA designed to make it easier for the uninsured to gain immediate access to Medicaid-funded services. For the first time, the federal government required states to implement presumptive eligibility programs under their Medicaid programs. Specifically, the ACA granted hospitals the ability to make presumptive eligibility determinations for Medicaid for certain groups covered in their state, including the non-elderly ACA expansion population (Caucci, 2014).[23] This means that if patients appear to have incomes low enough to qualify for Medicaid, hospitals may grant temporary Medicaid enrollment. Recipients of this temporary enrollment status may immediately receive health services and providers are guaranteed reimbursement for those services. In addition to presumptive eligibility programs, federal law directs states to provide retroactive coverage for new enrollees by covering medical bills incurred up to 3 months prior to their application date if they met the eligibility criteria during that time.[24] By not requiring an individual to first enroll in Medicaid prior to receiving Medicaid-funded care, these policies reinforce the notion that all eligible individuals are effectively covered by the program even if not actually enrolled.

Since we only observe our sample in the ACS during the pre-expansion years, we do not have information on their economic characteristics or coverage decisions during the post-expansion period. However, we are able to estimate model (1) using respondents in the 2008 to 2017 waves of the ACS who were ages 55 to 64 in 2014, and otherwise meet the same sample definition as used in our main analyses. While repeated cross-sectional data for this cohort does not exactly mirror the individual panel data used to study mortality, it allows us to provide an estimate of the changes in eligibility and insurance coverage likely similar to those experienced by our mortality sample.[25] We impute income eligibility for Medicaid using information on family structure and income and state-specific eligibility criteria over this time period.[26] In addition to changes in Medicaid eligibility, we also examine changes in Medicaid coverage and overall insurance status using respondent reports about current health insurance coverage at the time of the ACS survey.

The results are presented in Figure 1 and in the first three columns of Table 1. We find a large increase in Medicaid eligibility associated with the ACA Medicaid expansions with gains of between 41 and 46 percentage points during each post-expansion year, as compared to the year just prior to expansion. Consistent with many other studies of this policy,[27] we also find significant increases in Medicaid coverage and decreases in uninsurance associated with the decision to expand Medicaid eligibility. Reported Medicaid coverage increases by 7.3 percentage points in the first year and by

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[23] Previously presumptive eligibility programs were optional for states and limited to pregnant women and children. States also had discretion over what types of providers could grant presumptive eligibility for these groups.

[24] A handful of states (AR, IA, IN, NH) had federal waivers to waive retroactive coverage for the expansion population, or other existing Medicaid eligibility groups, during our study period (Musumeci and Rudowitz, 2017).

[25] There is one additional difference in the setup of this analysis. To avoid having multiple samples disclosed from the restricted-use data, we use the public-use ACS files for this "first-stage" analysis. The public-use file is a two-thirds random sample of the restricted-use file and will therefore result in nearly identical results, but with slightly larger confidence intervals.

[26] We consider eligibility for low-income parents under Medicaid Section 1931 criteria in each state, as well as expanded eligibility for parents and childless adults under waiver programs that offered comparable coverage to the ACA Medicaid expansions. Please see Appendix Section B for additional details about the eligibility imputation.

[27] E.g., Buchmueller et al. (2016); Cawley et al. (2018); Courtemanche et al. (2017); Miller and Wherry (2017, 2019); Sommers et al. (2015)

9.9 percentage points four years after the expansion relative to the year prior to expansion, while uninsurance decreases by 3.8 percentage points in the first year and 3.9 percentage points four years after the expansion. The estimates for years 2 and 3 are larger than those for year 4, which likely reflects the increasing share of the sample that is aging into Medicare over the study period.

It is important to note that the increases in Medicaid coverage observed in the survey data are most likely smaller than actual enrollment changes for two reasons. First, Medicaid coverage is notoriously underreported in survey data. Boudreaux et al. (2015) link the 2009 ACS to administrative data on Medicaid and Children Health Insurance Program (CHIP) enrollment and find that 23 percent of Medicaid/CHIP enrollees do not report this source of coverage. Rates of underreporting are higher for adults and minority groups; in addition, these groups are more likely to report no insurance coverage than other sources of coverage. This misreporting results in a large downward bias in estimates of the coverage effects of the ACA; Boudreaux et al. (2019) find that the impact of the ACA Medicaid expansions on Medicaid enrollment is 40% smaller when estimated using the ACS rather than administrative records. Second, by asking about coverage only at the time of the survey, the ACS does not capture information on Medicaid coverage for individuals enrolled in Medicaid at other times during the year. Given that there is tremendous churn among adults in the Medicaid program,[28] these estimates, therefore, likely underrepresent the total share of adults gaining any Medicaid coverage during each year, which is the relevant exposure measure.

We conduct our own analysis of underreporting for individuals meeting our sample criteria using data available from the 2008 to 2012 National Health Interview Survey (NHIS) for respondents linked to administrative data on Medicaid enrollment.[29] We find that while 15.7 percent of the sample reported being enrolled in Medicaid at the time they completed the survey, 22.9 percent were actually enrolled at some point during the year according to the CMS administrative records; this suggests an undercount based on survey data of approximately 31.4 percent. Incorporating this underreport into our first stage estimates indicates that the true first stage is likely closer to 14.7 percentage points (i.e., $\frac{0.101}{(1-0.314)}$).

Because this analysis is based on the reporting behavior of Medicaid enrollees prior to the ACA, it may not necessarily reflect the degree of underreporting among those gaining Medicaid coverage under the ACA expansions. In addition, the degree of underreporting in the ACS may be larger than this NHIS-based estimate since the NHIS is considered to have the most valid coverage estimates nationally (Lynch et al., 2011).[30] Therefore, we also estimate by how much we might be undercounting the change in total Medicaid enrollment under the ACA by comparing the "first stage" we obtain from the ACS with a "first stage" obtained from different CMS administrative data reports on total Medicaid enrollment during the study period. The two different administrative sources available for this analysis

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[28] See, for example, analyses in Sommers (2009) and Collins et al. (2018).

[29] Note we do not have access to similarly linked data for the ACS. These data are available from the National Center for Health Statistics for NHIS respondents who consent to the linkage. Due to an unfortunately timed change in the way CMS collects enrollee-level Medicaid administrative records, data are unavailable for most states after 2012.

[30] In contrast to the ACS, the NHIS uses state-specific names for Medicaid/CHIP in its coverage questions; it also includes a verification question for the uninsured. In the Appendix, we estimate the impact of the expansions on Medicaid coverage and uninsurance on our sample using the NHIS. The results are reported in Appendix Figure A1. We find larger impacts of the Medicaid expansions on coverage using the NHIS than we do in the ACS. Medicaid enrollment increases significantly in each post-expansion year by between 7.4 and 17.4 percentage points, and the average effect in the post-expansion period is 13.6 percentage points. If we further adjust this for under-reporting, it suggests there was an increase in Medicaid enrollment of $13.6/(1-0.314) = 19.8$ percentage points.

offer different definitions of enrollment and have different information in terms of the years and states available, as well as the ages for which information on enrollment is collected. Depending on the data source used, we find estimates of undercount ranging from somewhat smaller (19%) to considerably larger (exceeding 100%) than the estimate arrived at with the NHIS-CMS linked data. Since the NHIS-CMS data analysis allowed us to create an analytic sample most similar to that used in this paper, we apply the 31.4 percent undercount estimate when discussing treatment effects in the section that follows. Additional details on the analysis of underreporting in the NHIS-CMS data, as well as the analyses involving the CMS administrative data reports may be found in Appendix Section C.

Taken together, our first stage analysis suggests that there were large and significant impacts of the ACA Medicaid expansions on eligibility, Medicaid enrollment, and coverage in our analysis sample.

4.2 Impact of ACA Expansions on Mortality

Our estimates of equation (1) are presented in Figure 2 and in the fourth column of Table 1. Prior to the ACA expansion, mortality rates trended similar across the two groups: pre-expansion event study coefficients are close to zero and not statistically significant. Starting in the first year of the expansion, we observe mortality rates decrease significantly among respondents in expansion states relative to non-expansion states. The coefficient estimated in the first year following the expansion indicates that the probability of dying in this year declined by about 0.089 percentage points. In years 2 and 3, we find reductions in the probability of a little over 0.1 percentage points and, in year 4, a reduction of 0.208 percentage points. All estimates are statistically significant.

In the difference-in-differences model, we estimate an average reduction in mortality of 0.132 percentage points (top panel of Table 1). We can combine this estimate with the estimates of the first stage to provide information on the treatment effect of Medicaid coverage on the group that actually enrolled.[31] Our analysis of the ACS suggested that Medicaid enrollment increased by about 10.1 percentage points in our sample; when adjusted for survey under-reporting, the estimate is an increase of about 14.7 percentage points (see previous section). Our mortality estimates therefore suggest that the average treatment effect of Medicaid coverage on mortality is a reduction of about 0.898 percentage points ($=\frac{0.132}{0.147}$).[32]

It is important to note that the first stage only considers the immediate, or short-term, effects of Medicaid coverage on mortality. To the extent that there are longer-term effects on health, it is not clear that the average annual change in coverage is the correct first stage. For instance, individuals who gained coverage in 2014 but not later years may still experience health benefits that translate into reduced mortality in subsequent years. This is particularly relevant for the age group we study, as part of the sample ages in to Medicare over our analysis period. These individuals might still experience

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[31] One can further scale up this estimate to arrive at the local average treatment effect of gaining any insurance coverage by incorporating estimated crowd out of other coverage. However, this calculation ignores any potential benefits of Medicaid receipt for the already insured, such as a gain in coverage for certain services or the financial benefit of reductions in out-of-pocket medical costs. For example, individuals who switch to Medicaid from private insurance typically will not pay a premium and have minimal cost sharing, and could thus potentially benefit financially. The average annual premium for private coverage for a single person was $5,886 in 2013(Claxton et al., 2013), representing approximately half of the federal poverty level for this year. It also builds in assumptions regarding the comparability of Medicaid and private insurance coverage. For this reason, we focus on the treatment effect of Medicaid coverage in this discussion, since this is the most direct measure of the condition being varied by the policy.

[32] This effect would be somewhat smaller–about 0.667 percentage points–if we instead relied on the adjusted first stage estimated with the NHIS (i.e. a 19.8 percentage point increase in Medicaid coverage).

reduced mortality after enrollment in Medicare due to long-run health gains from receiving Medicaid at ages 62 to 64. Results in recent work examining the long-term effects of public insurance expansions for children document health improvements that manifest years later.[33] A more appropriate first stage, if the data were available, might be the change in the proportion of the sample with any exposure to Medicaid at the time of each post-expansion year, which will necessarily be larger than the estimates presented here.

4.3 Placebo Tests and Additional Analyses

To assess the validity of our empirical approach, we conduct several "placebo" tests. In these tests, we investigate whether we observe effects of the Medicaid expansions in populations that we expect to be unaffected or less affected by the policy change.

Our first placebo tests uses individuals who were age 65 and older at the time of the ACA expansions. These individuals had near universal coverage through the Medicare program and should not have been directly affected by the coverage expansions.[34] We re-estimate equation (1) for this sample and the results are presented in the first panel of Figure 3. As predicted, we observe no effect of the Medicaid expansions on Medicaid coverage for this group (panel a).[35] We also see no effect of the ACA on mortality rates for this group.

A second placebo test shifts our analysis sample back in time to the pre-ACA period. This test can assess whether any elements of our sample construction, such as drawing the ACS sample only in the pre-expansion period, might lead to spurious results. We construct the data in the same fashion as our main analysis, but use mortality data from 2004 to 2013 for ACS respondents from 2004 to 2009 (rather than mortality data from 2008 to 2017 for 2008-2013 ACS respondents). We construct a variable indicating that a state expanded that corresponds to $Expansion_s$ in equation (1), but estimate our model as if the first expansions occurred in 2010 rather than 2014, with states expanding t years after 2014 treated as if they expanded in 2010+t. The results of this placebo test using the pre-ACA period is presented in the second row of Figure 3.[36] As expected, we find no effect on Medicaid coverage or mortality in expansion states during this pre-ACA period.

Finally, we examine individuals ages 55 to 64 in households earning 400% FPL or greater at the time of the ACS interview. This group should be less affected than our main sample of low income or low education respondents. However, they may still gain Medicaid coverage under the expansions due to changes in income over time, or if their income is reported with error. As seen in the third row of Figure 3, we do find small but statistically significant increases in Medicaid enrollment corresponding with the expansions among this group. We also see small but, for some years, statistically significant reductions in mortality for this group. However, these mortality reductions are quite small, between 15

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[33] Boudreaux et al. (2016) and Goodman-Bacon (2016) document better later life adult health among children who gained exposure to Medicaid under its rollout in the 1960s. Brown et al. (2018); Currie et al. (2008); Miller and Wherry (2018); Thompson (2017); Wherry and Meyer (2016) and Wherry et al. (2017) find evidence of better long-term health for children benefiting from later expansions in Medicaid and CHIP.

[34] Prior work has documented some spillover effects on the health care utilization of this population under pre-ACA state Medicaid expansions, but analyses of the ACA Medicaid expansions have found no evidence of such spillovers and are able to rule out very small effects (Carey et al., 2018).

[35] Results are similar if we also restrict the elderly to be in low income households.

[36] Since the ACS only began collecting data on health insurance in 2008, the analysis for Medicaid coverage is limited to the 2008-2013 years.

and 20% of the size observed in our primary sample. The sample for the higher income group is also nearly three times as large as our main sample, resulting in much tighter confidence intervals. Taken together, all three placebo tests support our empirical design.

In addition to these placebo tests, we also conduct several additional analyses to further understand the impact of the Medicaid expansions. First, we examine changes in death rates by the underlying cause of death using the MDAC. These analyses rely on a much smaller sample and shorter follow-up period, and so we consider this analysis to be exploratory in nature. We examine deaths due to non-disease related (i.e. "external") and disease-related (i.e. "internal") causes separately. A subset of deaths caused by internal factors are considered to be "health care amenable" (Nolte and McKee, 2003), which we also examine separately. These results are presented in Table A2. We observe similar patterns for internal mortality and health care amenable mortality as we do in our main results, with relative decreases beginning in the first year after the expansions occur. The event study coefficients are not statistically significant for health care amenable mortality, and are significant at the $p < 0.10$ level for deaths from internal causes; however, we find a highly significant reduction in deaths related to internal causes under the difference-in-differences model. In contrast, mortality from external causes, which may be less affected by insurance coverage, does not appear to decrease after the expansions. The point estimates on the event study indicators are not statistically significant and the difference-in-differences estimate is only significant at the 10% level. The estimate is positive in sign, although we note that there is a slight upward pre-trend in these deaths in the expansion states relative to non-expansion states.

We further probe cause of death by conducting an analysis using the ICD code groupings by body region. We emphasize that this exercise is meant to be exploratory with the hope that it will provide guidance for future work should better data become available. The results are reported in Table A3. For most diseases, we observe negative coefficients; the largest negative point estimates are observed for deaths related to neoplasms (cancer), endocrine and metabolic diseases (primarily diabetes), cardiovascular and circulatory system diseases, and respiratory diseases. Two of these (cardiovascular/circulatory and endocrine/metabolic) are marginally significant at the 10% level. We also see a small negative but statistically significant impact on diseases related to the skin and subcutaneous tissue.

A second additional analysis uses our main data source but examines changes in mortality for different populations. Our main analysis is limited to individuals ages 55 to 64 at the time of the Medicaid expansions, a group with higher mortality rates that has been the focus of other work on this topic (e.g. Black et al., 2019). In column (1) of Table A4, we also estimate the impact of Medicaid expansion on mortality for individuals who meet our sample inclusion criteria but are ages 19 to 64 in 2014. As with the 55 to 64-year-old group, we find that mortality rates trended very similarly in the two groups of states prior to the expansions, with the event study coefficients for the pre-expansion years very close to zero (except for $y = -6$). Beginning in the first year of expansion, we see relative declines in mortality in the expansion states, although the estimates are much smaller in magnitude than those observed for the 55-64 age group and only statistically significant in the second year following implementation. In that year, we find a reduction in the probability of death of about 0.023 percentage points. Interestingly, when combined with the first stage for this group (a 13.4 percentage point gain in

Medicaid coverage; these results available from the authors), the associated treatment effect is very close to that reported in the Oregon Health Insurance Experiment (although not statistically significant): about a 0.14 percentage point reduction in the probability of mortality, compared to their estimate of 0.13 percentage points (LATE estimate in Table IX in Finkelstein et al., 2012). Our estimate would be even smaller if we applied an adjustment for the underreporting of Medicaid coverage in the ACS survey.

Another additional analysis limits the main sample of 55 to 64 year olds to approximately a 30 percent subset who reported being uninsured at the time of the survey. These results are presented in the second column of Table A4. As this group is somewhat younger, the mean annual mortality rate is slightly lower than in the overall sample, at 1.1% mortality per year. This subsample also has fewer observations – 180,000 individuals (or 1.3 million individual by year observations) – resulting in wider confidence intervals. Nevertheless, we observe the same pattern of no pre-ACA changes and a relative decrease in mortality beginning at the time of expansion. The point estimates indicate somewhat larger decreases in mortality for this group of 0.150 percentage points (or 13.6% of the sample mean) compared to the reduction in the main sample of 0.132 percentage points (or 9.4% of the sample mean).

5 Interpreting the Estimates and Comparisons to Past Work

The above results present consistent evidence of a decrease in all-cause mortality among low socioeconomic status, older adults under the ACA Medicaid expansions. Our point estimate indicates an average decrease in annual mortality of 0.132 percentage points during the four-year post period, or a treatment effect of Medicaid coverage among those who enroll of 0.898 percentage points. To interpret the magnitude of this estimate, we must consider the mortality rate in the absence of Medicaid expansion. The average annual mortality rate in our sample is 1.4 percent.[37] However, baseline mortality among those who actually enrolled in Medicaid (i.e., the "compliers," see Imbens and Angrist, 1994) is potentially much higher. This will be the case if those in worse health are more likely to enroll in Medicaid. The literature indicates that such adverse selection does tend to occur (e.g. Kenney et al., 2012; Marton and Yelowitz, 2015); this may also be exacerbated by policies designed to provide immediate coverage to those in need, as discussed earlier (i.e. presumptive eligibility and retroactive coverage). Data from the 2014 National Health Interview survey linked mortality files indicate that Medicaid enrollees in the 55-64 age range have a 2.3 percent probability of death in the following year.We may therefore expect the mean mortality rate among the compliers to be somewhere in the 1.4 to 2.3 percent range. Combined with our estimated treatment effect of an 0.898 percentage point reduction in mortality, this indicates that Medicaid coverage reduces mortality by between 39% and 64%. Naturally, the uncertainty about both the size of the first stage and the baseline mean among the compliers results in a fairly large range of possible treatment effects. For this reason, we believe the focus should be primarily on the reduced-form estimates of the change in mortality for our overall sample, which was selected based on their likely eligibility for Medicaid, rather than these "back of the

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[37] Our panel is aging over time and the mortality rate rises with age. As a result, comparing our estimates to the mortality rate in only the pre-ACA period as a counterfactual is incorrect. Instead, we opt to use the sample average as a point of comparison. Alternatively, one could use the baseline rate in the expansion states in the year just prior to expansion (1.3 percent) and apply an assumed increase in mortality with age during the post period, perhaps from the observed aging trajectory in the non-expansion states during the post period.

envelope" treatment effect calculations.

Nevertheless, we further assess the plausibility of our estimates by comparing the treatment effect estimate to that in the OHIE. We use the public-use replication kit to examine the effect of Medicaid coverage on participants who were ages 55-64 at the time of the experiment to derive estimates comparable to those presented here. Among this group, receiving Medicaid reduced the probability of mortality over a 16 month period by 1.65 percentage points, or a decline of 71.7% relative to the control mean; this estimate is associated with a p-value of 0.128 (reported in Table A5). We scale this effect by 12/16th to arrive an annual effect of Medicaid on mortality of 1.24 percentage points. This is comparable, but larger, than the 0.898 percentage point treatment effect estimated here. Note that the very high baseline mortality rate among the OHIE control group is consistent with our assertion that the mortality rate among the compliers in our setting is also likely high.[38] Since the OHIE participants are composed only of individuals applying for Medicaid, the high mortality rate is consistent with the idea that Medicaid applicants tend to be in poor health. In addition, we follow (Finkelstein et al., 2012) in our replication analysis and estimate the first stage as the difference in the probability a treatment group participant was *ever* enrolled in Medicaid; if we used a point-in-time enrollment measure, as we do in our analysis of the ACA expansions, the treatment effect in the OHIE would be even larger.[39] Similar to the OHIE, when we examine the implied treatment effects from the existing quasi-experimental literature on Medicaid expansions for low-income adults, our estimates fall well within the range of observed effects (see Appendix Table A9 and Appendix Section D for detailed discussion).

6 Conclusion

There is robust evidence that Medicaid increases the use of health care, including types of care that are well-established as efficacious such as prescription drugs and screening and early detection of cancers that are responsive to treatment.[40] However, due to data constraints, it has been difficult to demonstrate empirically that this increased utilization leads to improvements on objective measures of health, leading to widespread skepticism that Medicaid has any salutary effect on health whatsoever. Our paper overcomes documented data challenges by taking advantage of large-scare federal survey data that has been linked to administrative records on mortality. Using these data, we show that the Medicaid expansions substantially reduced mortality rates among those who stood to benefit the most.

Our estimated change in mortality for our sample translates into sizeable gains in terms of the number of lives saved under Medicaid expansion. Since there are about 3.7 million individuals who meet our sample criteria living in expansion states,[41] our results indicate that approximately 4,800 fewer deaths occurred per year among this population due to Medicaid expansion, or roughly 19,200 fewer deaths over the first four years alone. Or, put differently, as there are approximately 3 million individuals meeting this sample criteria in non-expansion states, failure to expand in these states likely

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[38] The baseline mortality rate among the OHIE control group in the 55-64 age range is 2.3 percent over a 16 month period (A5). This corresponds to an annual mortality rate of 1.7 percent (i.e., $2.3 \times (12/16) \approx 1.7$), more than 20 percent higher than the average mortality rate in our sample.

[39] By the end of the 16-month period, over a third of the individuals who ever enrolled in this age group were no longer enrolled in the program. Authors' calculations based on their analysis using the OHIE replication kit and data.

[40] E.g. Finkelstein et al. (2012); Ghosh et al. (2019); Soni et al. (2018).

[41] Authors' calculation using the public-use ACS.

resulted in 15,600 additional deaths over this four year period that could have been avoided if the states had elected to expand coverage.[42]

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[42] This relies on the assumption that effects of expansion in the non-expansion states would be similar to those observed in the expansion states.

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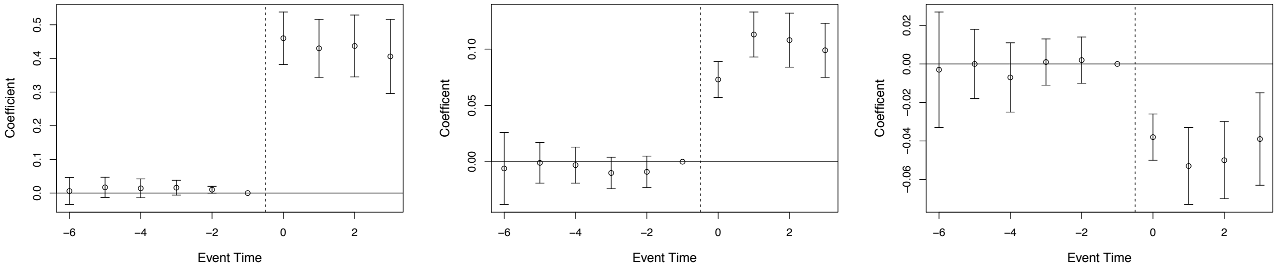
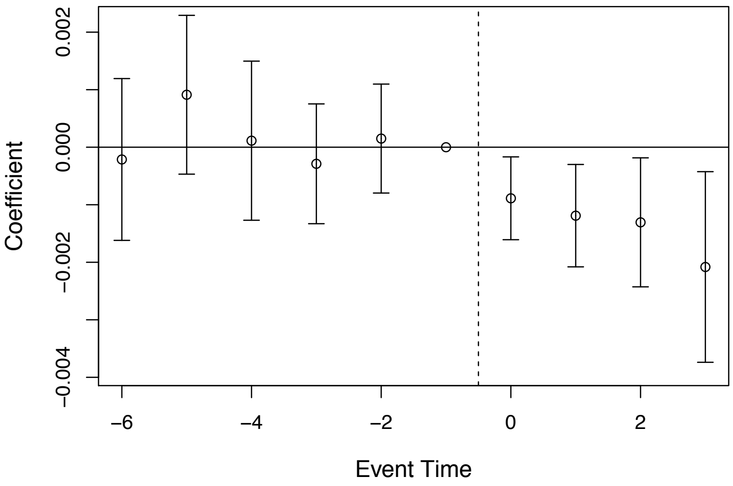
Figure 1: Effect of the ACA Medicaid Expansions on Eligibility and Coverage



(a) Medicaid Eligibility (b) Medicaid Coverage (c) Uninsured

Note: These figures report coefficients from the estimation of equation (1) for the outcomes of Medicaid eligibility, Medicaid coverage, and uninsurance in the 2008-2017 American Community Survey. Note that scales differ across the three figures. The coefficients represent the change in outcomes for expansion states relative to non-expansion states in the six years before and four years after expansion, as compared to the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 55-64 in 2014 who are not SSI recipients and who have either less than a high school degree or household income below 138% FPL. See Appendix Section B for detailed information on Medicaid eligibility determination.

Figure 2: Effect of the ACA Medicaid Expansions on Annual Mortality



Note: This figure reports coefficients from the estimation of Equation 1 for annual mortality. The coefficients represent the change in mortality for expansion states relative to non-expansion states in the six years before and four years after expansion, as compared to the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 55-64 in 2014 observed in the 2008-2013 American Community Survey who are not SSI recipients and who have either less than a high school degree or household income below 138% FPL.

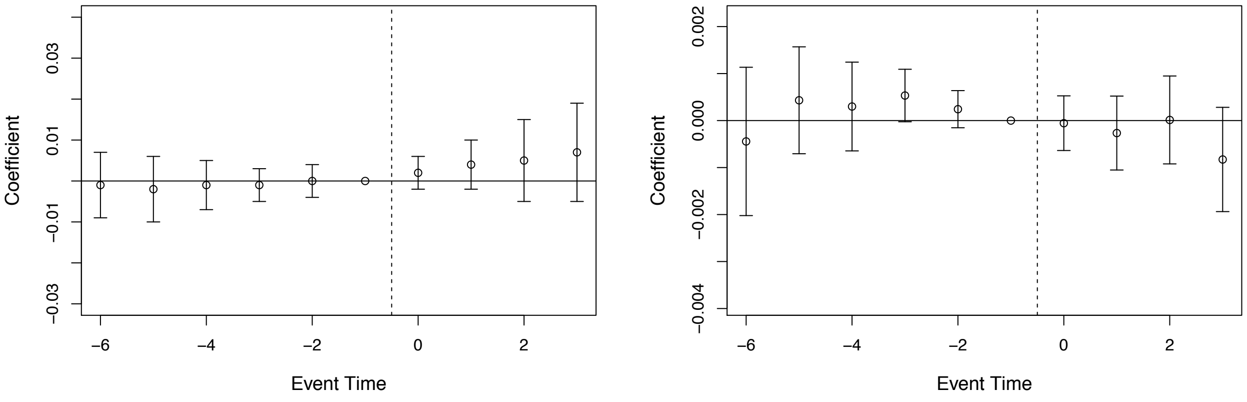
Table 1: Impact of the ACA Expansions on Coverage and Mortality: Difference-in-Differences Estimates

	Medicaid Eligibility	Medicaid Coverage	Any Insurance Coverage	Died in Year Main Sample
Difference-in-Differences Model:				
Expansion × Post	0.428 (0.050)***	0.101 (0.012)***	-0.044 (0.010)***	-0.001320 (0.000497)**
Event Study Model:				
Year 3	0.406 (0.055)***	0.099 (0.120)***	-0.039 (0.012)***	-0.002082 (0.0008284)**
Year 2	0.437 (0.046)***	0.108 (0.012)***	-0.050 (0.010)***	-0.001306 (0.0005606)**
Year 1	0.430 (0.043)***	0.113 (0.010)***	-0.053 (0.011)***	-0.001190 (0.0004449)***
Year 0	0.460 (0.039)***	0.073 (0.008)***	-0.038 (0.006)***	-0.000888 (0.0003600)**
Year -1 (Omitted)	0	0	0	0
Year -2	0.010 (0.005)*	-0.009 (0.007)	0.002 (0.006)	0.0001502 (0.0004735)
Year -3	0.016 (0.011)	-0.010 (0.007)	0.001 (0.006)	-0.0002885 (0.0005306)
Year -4	0.014 (0.014)	-0.003 (0.008)	-0.007 (0.009)	0.0001134 (0.0006915)
Year -5	0.017 (0.015)	-0.001 (0.009)	0.000 (0.009)	0.0009119 (0.0006901)
Year -6	0.006 (0.020)	-0.006 (0.016)	-0.003 (0.015)	-0.0002132 (0.0007031)
N (Individuals x Year)	714673	714673	714673	4030000
N (Individuals)	714673	714673	714673	566000

Note: This table displays the event study coefficient estimates of equation (1). The sample is defined as U.S. citizens ages 55-64 in 2014 who do not receive SSI and who have either less than a high school degree or household income below 138% FPL. For models based on restricted-use data, sample sizes are rounded following Census disclosure rules. See text for more details. Significance levels: *=10%, **=5%, ***=1%.

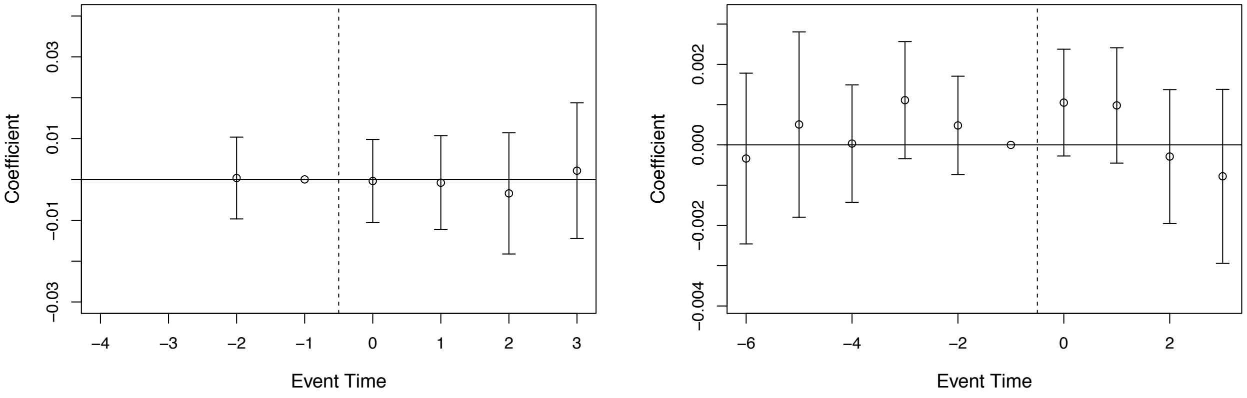
Figure 3: Placebo Tests

Age 65+ in 2014



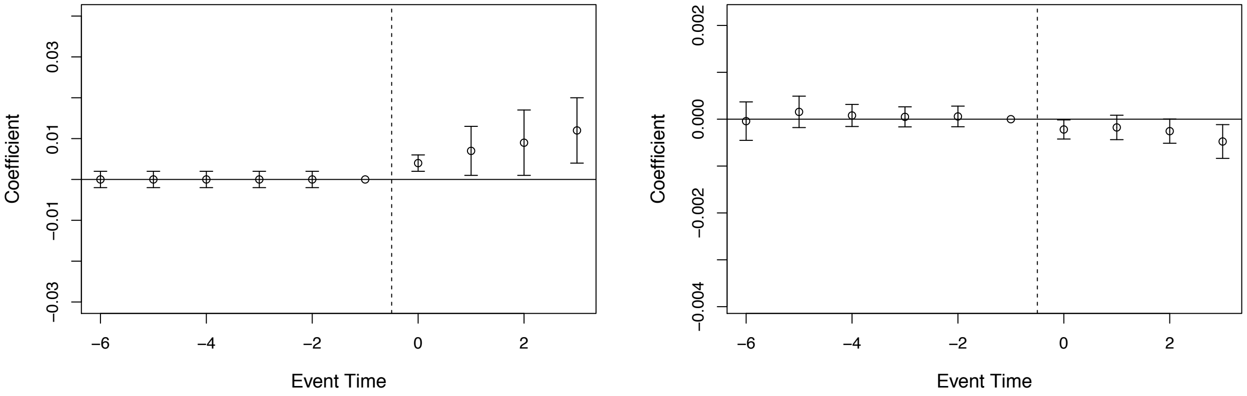
(a) Medicaid Coverage (b) Annual Mortality

Pre-ACA Years



(c) Medicaid Coverage (d) Annual Mortality

Income 400%FPL +



(e) Medicaid Coverage (f) Annual Mortality

Note: These figures plot coefficients from equation (1) for those age 65 and older in 2014 who would not have been affected by the Medicaid expansion due to their eligibility for the Medicare program (Row 1) and for those in higher income households who were likely less affected (Row 3). Row 2 plots the coefficients from (1) but uses pre-ACA years as a placebo test (see text for details).

Medicaid and Mortality: New Evidence from Linked Survey and Administrative Data

Appendix

Sarah Miller Sean Altekruse Norman Johnson Laura R. Wherry

A Additional Results

We present additional tables discussed in the main text in this section in Tables A1-A4. See the main text for further discussion of these results.

B First Stage Eligibility Estimates

To estimate the change in Medicaid eligibility associated with the ACA Medicaid expansions, we use the 2008-2017 ACS downloaded from IPUMS USA (Ruggles et al., 2019) and impute eligibility for our sample using state eligibility rules for each year. We consider eligibility for low-income parents under Medicaid Section 1931 criteria in each state, as well as expanded eligibility for parents and childless adults under waiver programs that offered comparable coverage to the ACA Medicaid expansions. We do not consider expanded programs that cover a more limited set of services and follow documentation from the Kaiser Family Foundation (KFF) to make this determination.

Information on state eligibility thresholds for coverage for adults were compiled from the sources listed in Table A6. The notes column in the table provides a record of any decisions made in applying the eligibility rules or to reconcile inconsistencies across different sources. KFF documentation on eligibility thresholds over time, which were used as our primary source, take into account state rules on earnings disregards when applicable. We defined the family unit for eligibility determination using the health insurance unit definition prepared by the State Health Access State Assistance Center, see details in State Health Access Data Assistance Center (2012). Following Medicaid rules for countable income (Centers for Medicare & Medicaid Services, 2016), we did not include family income from the Temporary Assistance for Needy Families or SSI programs in the calculation of total family income.

C Evaluating Survey Undercount of Medicaid Enrollment

To explore the extent to which survey measures undercount the number of individuals in our sample who were enrolled in Medicaid at any point during the survey year, we undertake several different analyses. Survey measures may undercount yearly enrollment because of respondents misreporting coverage or because a respondent correctly reports non-enrollment at the time of the survey but enrolls at a different point during the year. To examine this type of undercount, we take advantage of linked survey and administrative data on Medicaid coverage through the National Health Interview Survey via the public use NCHS-CMS Medicaid Feasibility Files. For each eligible respondent in the NHIS, these feasibility files state whether the respondent is present in the CMS MAX Person Summary (PS) file in each year.[43] All Medicaid enrollees are included in the PS file if they were enrolled at any point

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[43] Respondents were eligible for linkage if they were age 18 or older at the time of the survey and if they consented to have their administrative data linked.

during that year, even if they were enrolled for only a partial year. We can therefore compare presence in the PS file to self-reported Medicaid coverage in the NHIS for individuals meeting our sample criteria (i.e., citizens, not receiving SSI, age 55 to 64 in 2014 and either in households earning under 138% FPL or having less than a high school education).

Ideally we would perform this calculation during the 2014 to 2017 years. However, a change in the way CMS collected administrative data from state Medicaid offices occurred in 2013 and resulted in far fewer states providing the necessary administrative data for linkages. Since the public use NHIS file does not contain state identifiers, we limit our analysis to years in our sample period during which all states were available, i.e. the 2008 to 2012 waves of the NHIS.

Our results are presented in Table A7. We see that while that 15.7 percent of NHIS respondents meeting our sample criteria reported being enrolled in Medicaid when asked as part of the survey, 22.9 percent were found to be enrolled at some point during the year in the administrative records. Enrollment would therefore be undercounted by approximately 31.4 percent ($1 - \frac{15.7}{22.9} \approx 0.314$) relying on survey data alone, motivating our re-scaling of the survey-based first stage estimates.

We also supplement this analysis by using administrative enrollment data published in two sets of CMS reports. We calculate the difference-in-differences estimate from each set of reports and compare it to an estimate derived from survey reports for a similar population in the ACS. The first set of reports come from administrative enrollment data published by CMS and compiled by the Kaiser Family Foundation (KFF). Beginning in July 2013, CMS has published monthly total enrollment numbers in their Medicaid and CHIP Application, Eligibility Determination, and Enrollment Data reports. The KFF has compiled these monthly reports and calculated pre-ACA average monthly Medicaid/CHIP enrollment during the period July-September 2013, as well as average Medicaid/CHIP monthly enrollment for each month during the post-expansion period (Kaiser Family Foundation, 2019c). These totals refer to the total number of unduplicated individuals enrolled in Medicaid and CHIP.

We combine these administrative totals with state population estimates from each year of the ACS to create enrollment rates. Using the average monthly enrollment rates for 2013 and the monthly enrollment rates for 2014-2017, we then estimate a difference-in-differences model that includes state, year, and month fixed effects. We follow the same definition and timing of Medicaid expansion, as well as exclude the 5 early expander states, as in our main mortality data analyses. We use population weights and cluster the standard errors at the state level. We then compare these estimates to those acquired using only ACS survey data over the same period.

The results are reported in Table A8. The estimates using CMS data show a larger rate of Medicaid participation at baseline and a larger increase in participation under the ACA Medicaid expansions when compared to the estimates using ACS data. The change associated with Medicaid expansion is 23% larger when estimated with the administrative data.

The second analysis uses the MAX validation reports, which report the total number of Medicaid enrollees by state as well as the percent of enrollees in the 45 to 64 age range. These data have two advantages over the KFF monthly reports: they report the total number of individuals ever enrolled during the year and they are available for a population closer in age to the group examined in the main study. However, there are two major disadvantages to these reports: they are only available through 2014, and only for 16 states.[44] Using these data, and the corresponding sample from the ACS, we conduct a similar comparison. These results are reported in columns 3 and 4 of Table A8. For this age group and set of states, we find a dramatically larger effect of the ACA expansions using the enrollment rates based on the administrative data – about an 8.6 percentage point increase in enrollment – compared to those derived from the ACS – an increase of only 2.6 percentage points.

D Comparisons to Prior Estimates

In the paper, we emphasize that our preference is to focus on the reduced-form intent-to-treat (ITT) estimates of the effects of the ACA Medicaid expansions on mortality rather than re-scaling the estimates into average treatment-on-the-treated (TOT) effects. The former tells us the effect of the policy on overall mortality, while the latter provides an estimate of the mortality change among those newly enrolled under the policy. However, to arrive at an estimate of the TOT effect, accurate information is required on the change in Medicaid enrollment under the policy, as well as the baseline mortality rate specific to the individuals induced to take up coverage. As we discuss in Section 5 of the paper, there is considerable uncertainty about the true size of both of these numbers.

However, when comparing our estimates to prior evidence on the mortality effects of Medicaid expansion, it is helpful to translate the ITT estimates into average TOT effects. There is variation in the magnitude of the policy changes studied in this literature, as well as differences in the baseline mortality of the populations studied. Therefore, converting these existing estimates into proportional TOT effects allows for the effect sizes to be compared more easily across these different policy environments.

In this section, we compare the effect sizes from our study to the existing literature. This exercise is inspired by a similar re-scaling of quasi-experimental estimates undertaken by Goodman-Bacon (2018b), which includes mortality effects of Medicaid observed for infants and children.[45] Here, we focus on studies of changes in all-cause mortality under the ACA Medicaid expansions or similar insurance expansions for low-income adults.

We examine estimates for adults ages 55-64 when available, which is our primary age group of study, but we also examine estimates for all nonelderly adults.[46] We calculate the implied effects for the newly Medicaid insured (i.e. TOT) by dividing the ITT effects reported in each paper by the corresponding change in Medicaid enrollment. In cases where the change in enrollment is derived from survey data, we apply the same adjustment for under-reporting of Medicaid coverage that we use for our own estimates, which assumes an undercount of 31.4% (see discussion in Section 4). In addition, we convert the estimates into proportional mortality effects using the reported baseline mortality rate. For studies other than our own that use aggregate mortality data (rather than data for poor adults), we apply an adjustment that multiplies the general population mortality estimate by 1.75 to account for the higher relative risk of death for poor adults.[47] This adjustment factor is based on the discussion

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[44] These states are CA, GA, ID, IA, LA, MI, MN, MS, MO, NJ, PA, SD, TN, UT, WV, and WY.

[45] Goodman-Bacon (2018b) also includes estimates of adult mortality under the pre-ACA Medicaid expansions in AZ, ME, and NY presented in Sommers et al. (2012). In our analysis here, however, we focus on newer estimates of the mortality effects under these expansions in follow-up work by Sommers (2017).

[46] Note that Borgschulte and Vogler (2019) report reduced-form mortality effects for adults ages 55-64 but we have not included them here since the authors do not report a corresponding first stage.

[47] We do not apply this adjustment in the case of Massachusetts health care reform, given that the expansion included higher income individuals. We also do not apply it for the analysis of patients initiating dialysis for end-stage renal disease.

presented in Sommers (2017). This assumes that the baseline mortality rate for poor adults is similar to the baseline mortality rate for individuals newly gaining coverage under the Medicaid expansions (i.e. the "compliers"). It is likely the to be the case, however, that the mortality rate for these individuals is higher, given the evidence for the presence of adverse selection in insurance coverage decisions. If so, the average TOT estimates presented here may be too large. We discuss this further in Section 5 in the text.

Note that we have chosen to present the average TOT estimates in terms of the effects among individuals with newly gained Medicaid coverage, and not necessarily a change in insurance status. This averages the mortality effects among individuals who gain any insurance coverage, as well as those who would have otherwise had other sources of insurance coverage. In a few instances, when authors reported first stage estimates for insurance coverage only and not Medicaid coverage, we used estimates for the crowd-out of other sources of insurance coverage under the ACA Medicaid expansions to estimate the corresponding changes in Medicaid coverage. We calculated these crowd-out estimates using American Community Survey data and the methods described in Section 3.[48]

The resulting estimates are reported in Table A9. It is clear from this table that our estimates are in the ballpark of those from existing work examining the mortality effects of Medicaid or insurance expansions for adults. As discussed further in the text, our estimate of the proportional mortality change among newly enrolled older adults is similar in magnitude to the IV estimate from the Oregon Health Insurance Experiment (-64.0% vs. -71.7%). We should note, however, that the estimate for this age group from the analysis in Black et al. (2019) is noticeably smaller. In addition, our estimate for all nonelderly adults (-22.6%) is similar in magnitude to estimates from other studies of Medicaid expansions and Massachusetts health care reform, which range from -16.3% to -29.2%. Finally, the estimate for Swaminathan et al. (2018), which focuses on patients initiating dialysis for end-stage renal disease, is -82.8%, indicating that the average treatment effect is high for individuals at increased mortality risk.[49]

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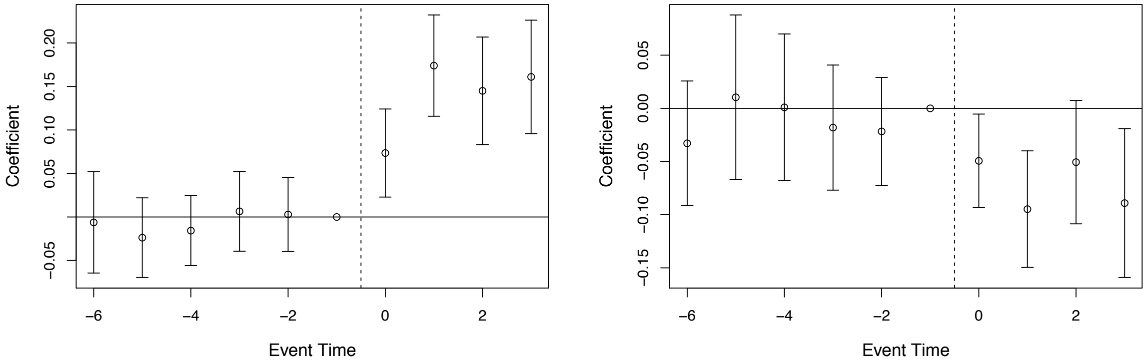
[48] Note that most of this "crowd-out" under the ACA expansions appears to be driven by an increase in private coverage in states that did not expand Medicaid starting in 2014, presumably among those qualifying for subsidized coverage under the exchanges (130-138% FPL) (Miller and Wherry, 2017). Therefore, we hypothesize that crowd-out might have been smaller in magnitude during the pre-ACA Medicaid expansions, a period prior to the existence of subsidized exchange coverage. In this case, the implied TOT effects might be larger than those reported here.

[49] Note, we did not apply an adjustment for higher baseline mortality for poor individuals with ESRD. If the same adjustment is applied as in other analyses (x 1.75), then the proportional average TOT estimate is -47.3%.

Figure A1: Effect of the ACA Medicaid Expansions on Eligibility and Coverage, NHIS Analysis



(a) Medicaid Coverage (b) Uninsured

Note: These figures report coefficients from the estimation of equation (1) for the outcomes of Medicaid coverage and uninsurance in the 2008-2017 National Health Interview Surveys. The coefficients represent the change in outcomes for expansion states relative to non-expansion states in the six years before and four years after expansion, as compared to the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 55-64 in 2014 who are not SSI recipients and who have either less than a high school degree or household income below 138% FPL. In models where all post-expansion years are pooled (i.e., the “difference in differences” model), we find an increase in Medicaid coverage of 13.6 percentage points and a decrease in uninsurance of 5.8 percentage points; both estimates are significant at the 1% level.

Table A1: Descriptive Statistics of Main Sample by State Expansion Status

	Expansion State	Non-expansion State
% White	70.9	68.7
% Black	14.9	24.2
% Hispanic	15.3	12.2
% Uninsured	32.6	37.3
% Medicaid	20.5	16.2
% Less than High School Education	45.3	46.8
Average Age in 2014	55.8	55.9
Average Income relative to FPL	1.47	1.40
N	231,200	190,448

Note: This table displays weighted means for residents in expansion and non-expansion states meeting the sample criteria described in the text. These statistics were calculated using publicly-available 2008-2013 ACS data rather than the restricted version used in the main analysis.

Table A2: Impact of the ACA Expansions on Coverage and Mortality: Cause of Death

	Deaths from Internal Causes	Deaths from Health Care Amenable Causes	Deaths from External Causes
Difference-in-Differences Model:			
Expansion × Post	-0.002351 (0.006754)***	-0.0009907 (0.0005043)*	0.0003831 (0.0001998)*
Event Study Model:			
Year 1	-0.002207 (0.001262)*	-0.0004100 (0.0008170)	0.0000954 (0.0003947)
Year 0	-0.002090 (0.001081)*	-0.001029 (0.0007480)	0.0002501 (0.0003154)
Year -1 (Omitted)	0	0	0
Year -2	-0.0005340 (0.0008272)	0.0006530 (0.0005310)	-0.0000658 (0.0003380)
Year -3	0.0008772 (0.001038)	0.0001387 (0.0007171)	-0.0000658 (0.0004400)
Year -4	-0.0004416 (0.001118)	-0.0000797 (0.0008195)	-0.0003190 (0.0003844)
Year -5	0.0007490 (0.0009490)	0.0004741 (0.0007390)	-0.0002190 (0.0003696)
Year -6	0.0007098 (0.001062)	0.0002333 (0.0006164)	-0.0006014 (0.0003489)
N (Individuals x Year)	683000	683000	683000
N (Individuals)	88500	88500	88500

Note: This table displays the event study coefficient estimates of equation (1) using the MDAC. Sample sizes are rounded following Census disclosure rules. See text for more details. DRB Disclosure Approval #: CBDRB-FY19-310. Significance levels: *=10%, **=5%, ***=1%.

Table A3: Impact of the ACA Expansions on Mortality: Impact by ICD Grouping

	Infectious disease	Neoplasms	Diseases of the blood and blood-forming organs	Endocrine, nutritional and metabolic diseases	Mental/behavioral
Expansion × Post	-0.0000671 (0.0001273)	-0.0005512 (0.0004556)	0.0000337 (0.0000345)	-0.0004314 (0.0002277)*	-0.0000465 (0.0001100)
Mean	0.004121	0.02718	0.0002675	0.005279	0.001676
	Nervous system	Circulatory system and cardiovascular	Respiratory	Digestive	Skin and sub-cutaneous tissue
Expansion × Post	-0.0000131 (0.00011.62)	-0.0008861 (0.0004804)*	-0.0003801 (0.0002758)	-0.0000046 (0.000243)	-0.00002550 (0.0000119)**
Mean	0.002392	0.02504	0.008223	0.006589	0.00008866
	Musculoskeletal system	Genitourinary system	Other		
Expansion × Post	0.0001148 (0.0000706)	-0.0001297 (0.0001101)	0.0003175 (0.0001910)		
Mean	0.0004495	0.002094	0.07006		

Note: This table displays the difference-in-differences coefficient estimates using the MDAC. Each entry is the result from a different regression. Rates are reported under coefficient estimates. Sample sizes are rounded following Census disclosure rules. DRB Approval Number: CBDRB-FY19-400. See text for more details. Significance levels: *=10%, **=5%, ***=1%.

Table A4: Impact of the ACA Expansions on Coverage and Mortality: Additional ACS Analyses

	Died in Year Ages 19-64	Died in Year, Uninsured at Time of ACS
Difference-in-Differences Model:		
Expansion × Post	-0.0001900 (0.0001680)	-0.001500 (0.0006590)**
Event Study Model:		
Year 3	-0.0003183 (0.0002191)	-0.002517 (0.0008802)**
Year 2	-0.0001270 (0.0001702)	-0.001425 (0.0009531)
Year 1	-0.0002309 (0.0001124)**	-0.001437 (0.0006387)**
Year 0	-0.0001541 (0.0001034)	-0.0008643 (0.0008343)
Year -1 (Omitted)	0	0
Year -2	-0.0000201 (0.0001146)	0.0001791 (0.0008579)
Year -3	0.0000318 (0.0001471)	-0.0005425 (0.0007373)
Year -4	-0.0000049 (0.0001663)	0.0001925 (0.0009295)
Year -5	0.0000138 (0.0001890)	0.001409 (0.001057)
Year -6	-0.0002170 (0.0002160)	0.0001959 (0.001126)
N (Individuals x Year)	23630000	1280000
N (Individuals)	3160000	180000

Note: This table displays the event study coefficient estimates of equation (1) using the ACS. Sample sizes are rounded following Census disclosure rules. See text for more details. DRB Disclosure Approval #: CBDRB-FY19-310. Significance levels: *=10%, **=5%, ***=1%.

Table A5: Results from the Oregon Health Insurance Experiment for participants age 55-64 in 2008

	Control Group Mean	Reduced Form	2SLS	p-value
Alive	0.977	0.00422	0.0165	0.128
N	6550 (Control)	4240 (Treatment)		

This table uses the public-use replication kit of the Oregon Health Insurance Experiment to estimate the impact of Medicaid on individuals who were between the ages of 55 and 64 at the time of the experiment. The data and code were downloaded from https://www.nber.org/oregon/4.data.html.

Table A6: Sources for Parent and Adult Medicaid Eligibility Rules by Year

Year	Sources	Notes
2008-2010	Adults: NGA Center for Best Practices (2010), Table 9	We follow the criteria reported in Heberlein et al. (2011), Table 4 to determine whether programs described in NGA Center for Best Practices (2010) meet the full coverage criteria. We turned to additional sources to reconcile other differences with the program details reported in Kaiser Family Foundation (2019b). Specifically, we added a program for AZ following National Conference of State Legislatures (2009), a DC program based on Meyer et al. (2010), and altered HI and VT program details using Agency (2011).
2011-2017	Adults: Kaiser Family Foundation (2019b)	We consider eligibility rules to be in place as of the date of the relevant KFF survey. To be consistent with our definition of implementation of the ACA Medicaid eligibility expansions, we consider the expansion in Indiana to take place in 2015.
2008-2017	Parents: Kaiser Family Foundation (2019a)	We consider eligibility rules to be in place as of the date of the relevant KFF survey with the exception of the December 2009 survey for parents eligibility, which we apply to the 2010 year. To be consistent with our definition of implementation of the ACA Medicaid eligibility expansions, we consider the expansion in Indiana to take place in 2015.

Table A7: Undercount Estimates from the NHIS-CMS Linked Feasibility Files

% Reported Enrolled in Survey	0.157 (0.007)
% Reported Enrolled in Administrative Data	0.229 (0.008)
N	3,437

Note: This table displays the fraction of NHIS respondents meeting sample inclusion criteria who reported being enrolled in Medicaid in the NHIS (first row) versus those who were shown to be enrolled in Medicaid in the CMS administrative data (second row). Standard errors are in parentheses.

Table A8: Comparison of Medicaid Coverage Estimates: CMS vs. ACS

	All Ages and States, 2013-2017		Age 44-64, 17 States, 2012-2014	
	Enrollment Based on CMS Enrollment Reports	Enrollment Based on ACS Data	Enrollment Based on MAX Validation Reports	Enrollment Based on ACS Data
Expansion x Post	0.0382*** (0.0093)	0.0309*** (0.0049)	0.0862*** (0.0248)	0.0258*** (0.0065)
Baseline Mean in Expansion States	0.197	0.172	0.120	0.108
Number of Observations	2,103	14,323,891	48	2,423,253

Note: The first two columns in this table display the difference-in-differences estimates for analyses using monthly enrollment rates constructed from CMS enrollment reports and self-reported enrollment from the ACS for all ages, respectively, for the years 2013-2017. All regressions include state and year fixed effects and the regression with CMS data also includes month dummies. The second two columns display the DID estimates for analyses using total number of adults ages 45-64 ever enrolled in each year during 2012-2014 from the MAX validation reports from 16 states, as well as the estimates derived from a comparison ACS sample for those years. The regressions include state and year fixed effects. For all regressions, robust standard errors are clustered by state. The regressions with administrative data use state population estimates as weights, while the analyses with ACS data use survey weights. See text in Appendix Section C for more details on the data. Significance levels: *=10%, **=5%, ***=1%.

Table A9: Estimates for Annual Mortality Effects of Insurance Expansions for Adults (Deaths per 100,000)

	Population	ITT effect on mortality			First stage effect on Medicaid coverage		Implied mortality effect on new enrollees (i.e. average TOT)	
		Baseline rate	Absolute change	*Sources*	Percentage point change	*Source*	Relative change (%)	*Notes*
Pre-ACA Medicaid expansion								
Finkelstein et al. 2012 (Oregon Health Insurance Experiment)	Adults ages 20-64 signing up for expanded Medicaid	800	-32 (-101.8 to 165.3)	Table 9	25.6 (24.9 to 26.3)	Table 3	-16.3	Uses IV estimate from Table 9
	Adults ages 55-64 signing up for expanded Medicaid	2300	-422 (-968.2 to 124.8)	Authors' estimation, Table A5	25.6 (23.9 to 27.3)	Authors' estimation	-71.7	Uses IV estimate from authors' estimation, Table A5
Sommers 2017 (AZ, ME, NY expansions)	Adults ages 20-64	318	-19.1 (-31.4 to -6.8)	Tables 1, 3	Not reported		-21.4	From model that interacts county level uninsurance rates with Medicaid expansion, Table 6. Applies adjustments (1), (2), (3)
ACA Medicaid expansion								
Miller et al. 2019	Adults ages 19-64, below 138% FPL or low-ed, citizen, no SSI receipt	430	-19.0 (-13.9 to 51.9)	Table A4	13.4 (11.1 to 15.7)	Authors' estimation from ACS	-22.6	Applies adjustment (3)
	Adults ages 55-64, below 138% FPL or low-ed, citizen, no SSI receipt	1400	-132.0 (-229.4 to -34.6)	Table 1	10.1 (7.7 to 12.5)	Table 1	-64.0	Applies adjustment (3)
Black et al. 2019	Adults ages 55-64	854	-2.56 (-14.28 to 9.16)	Estimates provided by authors, backed out by applying relative change to baseline mean	1.7	Text, pg. 16, change in insurance	-6.8	Applies adjustments (1), (2), (3)
Borgschulte and Vogler 2019	Adults ages 20-64	359	-11.36 (-18.4 to -4.3)	Table 2	4.2 (2.0 to 6.4)	Table A.3, change in insurance	-23.5	Applies adjustments (1), (2), (3)
Swaminathan et al. 2018	Nonelderly patients initiating dialysis	6900	-600 (-1000 to -200)	Table 2	10.5 (7.7 to 13.2)	Table 2	-82.8	
Other insurance expansions								
Sommers, Sharon, and Baicker 2014 (MA health care reform)	Adults ages 20-64	283	-8.21 (-13.58 to -2.83)	Table 3, back out by applying relative change to baseline mean	6.8	Table 5	-29.2	Applies adjustment (3)

Notes: OHIE estimates are measured over a 16-month, not 12 month period. Estimates from Black et al. (2019) are from the triple difference specification.

(1) Multiply baseline mortality rate by 1.75 to account for higher relative risk of death for poor adults, based on discussion in Sommers (2017).

(2) Applies estimates for crowdout (53.7% for ages 20-64 and 47.1% for ages 55-64) to estimate change in Medicaid coverage, based on authors' calculation from analysis of the ACA Medicaid expansions.

(3) Divide change in Medicaid coverage estimated from survey data by (1-0.314) to account for underreporting.

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Medicaid Eligibility Expansions May Address Gaps In Access To Diabetes Medications

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ABSTRACT **Diabetes is a top contributor to the avoidable burden of disease. Costly diabetes medications, including insulin and drugs from newer medication classes, can be inaccessible to people who lack insurance coverage. In 2014 and 2015 twenty-nine states and the District of Columbia expanded eligibility for Medicaid among low-income adults. To examine the impacts of Medicaid expansion on access to diabetes medications, we analyzed data on over ninety-six million prescription fills using Medicaid insurance in the period January 2008–December 2015. Medicaid eligibility expansions were associated with thirty additional Medicaid diabetes prescriptions filled per 1,000 population in 2014–15, relative to states that did not expand Medicaid eligibility. Age groups with higher prevalence of diabetes exhibited larger increases. The increase in prescription fills grew significantly over time. Overall, fills for insulin and for newer medications increased by 40 percent and 39 percent, respectively. Our findings suggest that Medicaid eligibility expansions may address gaps in access to diabetes medications, with increasing effects over time.**

Expanding access to prescription medications for diabetes is critical for improving US population health. Diabetes is one of the top ten causes of death and is a risk factor for heart disease, the top cause of death.[1–3] Many of the complications of diabetes can be prevented by the appropriate application of glucose-lowering drugs.[4–8] Yet not all people with diabetes receive the medications they need.[9–11] High out-of-pocket spending contributes to treatment nonadherence among patients with diabetes.[12–15] Uninsured people with diabetes may have difficulty obtaining needed care and often show elevated risk of poor glycemic control.[16–18]

The average per patient cost of diabetes medications has risen in recent years, in part because of the increasing use of newer medications.[19,20] The number of medication categories for blood glucose control has swelled from three to eleven since the early 1990s. In 2013 the mean expenditure per patient for newer insulin analogues was almost double that for older formulations; likewise, the mean expenditure per patient for newer oral antihyperglycemic medications was almost double that for older oral medications.[21]

Despite their higher cost, these newer medications can carry important health benefits. For example, rapid- and long-acting insulin analogues provide equivalent glycemic control to large-dose conventional insulin therapy but with significantly less hypoglycemia in a non–intensive care setting.[22–24] Likewise, extended-release metformin is more effective than conventional formulations in improving glyco-metabolic control and lipid profile with a convenient dosing schedule.[25,26] Finally, glucagon-like peptide-1 (GLP-1) receptor agonists and sodium-glucose cotransporter-2 (SGLT-2) inhibitors have shown favorable effects on rates of hypoglycemia and

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body weight, as well as patients' risk of cardiovascular events and mortality.[27–31] As a result, providing access to these costly newer medications could improve patients' health.

The Affordable Care Act (ACA) originally required that all states expand eligibility for Medicaid to all adults with incomes below 138 percent of the federal poverty level. However, the US Supreme Court ruled that Medicaid expansion would be voluntary for the states.[32,33] Ultimately, twenty-five states expanded Medicaid eligibility in January 2014, and twenty-nine states and the District of Columbia did so in either 2014 or 2015.

These expansions were associated with an increase in the number of Medicaid prescriptions per enrollee and a drop in cost-related prescription nonadherence.[34–36] They also improved access to primary care among newly insured patients, which translated into increased health care use.[37–41]

A prior study showed increases in the numbers of diabetes prescriptions filled using Medicaid insurance after Medicaid eligibility expansions and showed that Medicaid insurance did not crowd out other types of insurance to a great extent during the first fifteen months.[42] Notably, diabetes prescriptions increased more than those in any other clinical category considered in that study. That study did not provide estimates by age and sex, as our study does. Another study measured increases in the prescribing of diabetes prescription drugs among people with one or more chronic conditions who gained Medicaid coverage in the period January 2012–December 2014.[34] This study focused on changes in prescription drug use over time among patients who had already filled prescriptions at baseline.

We are not aware of any studies that measure the additional Medicaid diabetes prescriptions filled during the first twenty-four months of the Medicaid eligibility expansions, or that report how the expansions affected the use of specific classes of diabetes medications. A study of specific drug classes would help define the health benefits associated with the expansions. A substantial increase in the use of newer medications would imply that the expansions helped resolve the slow diffusion of innovation to low-income patients with diabetes, possibly improving their health.[43,44]

Improving access to diabetes medications, including newer ones, has the potential to influence the health of people living with diabetes for decades to come.[8] Therefore, to inform ongoing policy discussions about expanding Medicaid eligibility in additional states or rolling back the expansions in other states, we assessed the impact of the expansions on the use of diabetes prescription medications filled using Medicaid insurance during the first twenty-four months after the expansions. We present estimates of the changes in diabetes prescriptions filled after the expansions by type of medication and patients' sex and age. We used the estimates by age to conduct multiple checks of the data. Finally, we conducted a trend analysis to examine whether the changes associated with the expansions grew over time. Our study contributes to the literature on the implications of the expansions for patients with chronic conditions.

Study Data And Methods

STUDY DESIGN We used a quasi-experimental difference-in-differences design to distinguish changes in diabetes prescription fills related to Medicaid eligibility expansions from temporal trends. Specifically, trends in diabetes prescription fills before versus after the expansions (the first difference) were compared in states with versus those without such expansions (the second difference). The pre-intervention period was January 2008–December 2013, and the post-intervention period was January 2014–December 2015. We defined expansion states as those twenty-nine states (and the District of Columbia) that expanded Medicaid eligibility in 2014 or 2015, and we classified the other twenty-one states as nonexpansion or control states. Online appendix exhibit A1 provides additional details on the classification of states.[45]

DATA We measured fills of diabetes prescriptions using a large and representative administrative data set, the IQVIA Xponent data. The data captured prescription fills in all fifty states and the District of Columbia over eight years, including more than ninety-six million diabetes prescription fills for patients ages 20–64 paid for by Medicaid insurance. We tabulated these data by year, quarter, and state, as well as patients' age group and sex. We combined these data with intercensal population data estimates,[46] and quarterly unemployment rates for each state from the Bureau of Labor Statistics.[47] We also used data for 2013–14 from the National Health and Nutrition Examination Survey to calculate the prevalence of diabetes by age group.[48]

STATISTICAL ANALYSIS We used the difference-in-differences method to model changes in diabetes prescriptions filled using Medicaid insurance after Medicaid eligibility expansions for states with versus those without such expansions. To account for the fact that the number of prescriptions increases along with the population, we used negative binomial models in which current population was the exposure variable.[46]

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We report the effects associated with the expansions as changes in prescription fills per 1,000 population per year (that is, average marginal effects per 1,000 population).[49] Appendix exhibit A2A provides additional details.[45]

Our outcomes of interest were prescription fills for metformin (extended-release and regular), a first-line treatment for non-insulin-dependent type 2 diabetes; insulin (rapid- and long-acting insulin analogues and regular insulin), a treatment for type 1 and insulin-dependent type 2 diabetes; three classes of newer oral medications (dipeptidyl peptidase [DPP]-4 inhibitors, GLP-1 agonists, and SGLT-2 inhibitors); and all other classes of diabetes medications. Prescriptions for all other drug classes were grouped together since they are not first-line agents, are not newer drugs, and were not used as frequently as other classes. We also analyzed the total numbers of diabetes prescriptions filled.

We clustered standard errors at the state level to account for the state-level nature of Medicaid eligibility expansions. We addressed possible residual confounding by adjusting for year by quarter indicator variables, state indicator variables, the age and sex of the person filling the prescription, and quarterly state-level unemployment rates.

We also conducted specification checks such as testing for parallel trends in Medicaid expansion versus nonexpansion states before the expansions, using linear and nonlinear specifications. We conducted a number of robustness checks. These included stratifying the data by age to compare changes in Medicaid diabetes prescription fills after Medicaid eligibility expansions with age-specific diabetes prevalence, estimating changes separately for 2014 and 2015, and examining whether quarterly changes after Medicaid expansions grew over time. We also examined whether the gap in Medicaid diabetes prescription fills between residents of expansion states and those of nonexpansion states shrank as expected once patients became eligible for Medicare at age sixty-five, omitted data from before 2011, and excluded states that expanded Medicaid eligibility before or after January 2014. Appendix exhibit A2A provides additional details on these analyses.[45]

All analyses were performed using Stata/MP, version 14.

LIMITATIONS Our study had several limitations. First, we were not able to track people over time. Instead, we analyzed data at the age-sex-state level across different time periods.

Second, patients' race and ethnicity were not reported in the IQVIA data and therefore these variables were not included in our analyses.

Third, approximately 15 percent of retail pharmacies did not share their prescription fills data with IQVIA. Missing data were imputed by IQVIA using validated methods.[50]

Fourth, nonretail prescriptions and mail-order prescriptions were outside the sampling frame. If Medicaid eligibility expansions also increased prescription fills at federally qualified health centers or mail-order prescription fills, our estimates would be underestimates of the total effect.

Fifth, we evaluated the association between Medicaid eligibility expansions and diabetes prescription fills in states with expanded Medicaid eligibility. Our findings might not be generalizable to a nationwide expansion of Medicaid eligibility.

Finally, ours was an observational study, and we therefore cannot rule out the possibility that other changes also accounted for or contributed to our results.

EXHIBIT 1

Average characteristics in 2010 of states that did and did not expand eligibility for Medicaid under the Affordable Care Act

Characteristic	Nonexpansion states	Expansion states	*p* value of difference
Prevalence of diagnosed diabetes	7.96%	9.60%	0.06
Mortality per 100,000 people	829.57	826.60	0.93
Population	6,161,336	5,887,206	0.89
Male	49.20%	49.46%	0.25
Hispanic	11.53%	9.11%	0.40
Black	10.20%	12.55%	0.46
Older than age 65	13.53%	12.84%	0.15

SOURCE Authors' analysis of data from the Census Bureau, the Centers for Disease Control and Prevention, the National Center for Health Statistics, and the National Vital Statistics System. **NOTE** Twenty-nine states and the District of Columbia had expanded Medicaid eligibility by the end of our sample period.

Study Results

States that did and those that did not expand Medicaid eligibility during 2014–15 appeared similar in several population-level characteristics in 2010, the year of the ACA's passage (exhibit 1).

Prescription fills for diabetes medications showed a slightly increasing trend before the ACA Medicaid eligibility expansions, in both expansion and nonexpansion states (exhibit 2). Appendix exhibit A3 shows the trends by medication class.[45] For each outcome and age group, we could not reject the null hypothesis that trends in our outcomes of interest were similar in these two groups of states before 2014; see Appendix exhibits A4B and A4C.[45] Additionally, an analysis of the annual gap in prescription fills

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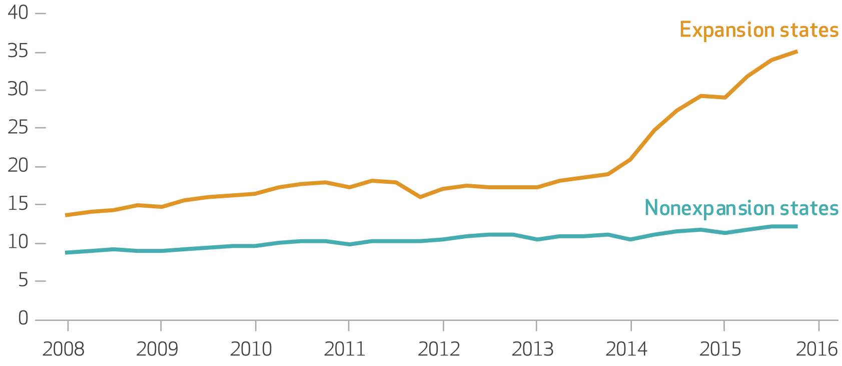
between expansion and nonexpansion states showed a flat trend before 2014 and a break in that trend in 2014 (appendix exhibit A4D).[45]

In 2014–15 Medicaid eligibility expansions were associated with increases of thirty Medicaid prescription fills for diabetes medications per 1,000 population among adults ages 20–64 (exhibit 3). We observed larger estimates for the increases in fills in 2015 than in 2014. When we divided the quarterly increase in fills into an intercept and a slope, we found that the slope was positive and significant—which indicates that the changes after Medicaid eligibility expansions grew over time (appendix exhibit A2D).[45]

Newer medications (rapid- and long-acting insulin analogues, extended-release metformin, DPP-4 inhibitors, GLP-1 agonists, and SGLT-2 inhibitors) accounted for about one-third of the increase in prescriptions (exhibit 3). This likely represented an increase in the uptake of newer medications among patients who previously lacked insurance. In the IQVIA data in 2013, 15 percent of diabetes prescriptions filled using cash were newer medications, compared with 35 percent, 37 percent, and 38 percent filled using Medicare, private insurance, and Medicaid, respectively. The lower uptake of newer medications among uninsured patients is consistent with the fact that these medications required substantially higher out-of-pocket spending at the time of the Medicaid eligibility expansions.[21,51]

EXHIBIT 2

Diabetes prescription fills using Medicaid insurance per 1,000 population ages 20–64, in states that did and did not expand eligibility for Medicaid



SOURCE Authors' analysis of data from IQVIA. **NOTE** Most of the states that expanded Medicaid eligibility among low-income, nondisabled adults in 2014 or 2015 did so in the first quarter of 2014.

All findings remained qualitatively unchanged in additional robustness checks, in which we eliminated states that expanded Medicaid eligibility before 2014, eliminated all states that expanded Medicaid eligibility in months other than January 2014, or omitted data from before 2011. Results of these analyses are in appendix exhibits A2E and A2F.[45]

The relationship between Medicaid eligibility expansions and Medicaid prescription fills for diabetes medications declined dramatically after

EXHIBIT 3

Additional increases in annual Medicaid diabetes prescription fills per 1,000 population ages 20–64 associated with Medicaid eligibility expansions during 2014–15

		Difference-in-differences estimates					
		Average annual change, 2014–15		Change in 2014		Change in 2015	
	Baseline fills	Increase	95% CI	Increase	95% CI	Increase	95% CI
All patients	73.05	29.93	(21.40, 38.45)	24.21	(17.4, 31.39)	35.93	(25.62, 46.25)
Men	58.73	23.98	(17.42, 30.54)	19.19	(13.66, 24.72)	29.40	(21.70, 37.20)
Women	87.22	31.52	(21.77, 41.26)	25.89	(17.66, 34.12)	37.40	(25.63, 49.17)
MOST COMMON TYPES OF DIABETES MEDICATIONS							
Insulin and insulin analogues	23.21	9.35	(6.59, 12.11)	7.73	(5.44, 10.20)	11.40	(7.69, 14.39)
Metformin	29.13	12.17	(8.71, 15.63)	9.88	(6.95, 12.81)	14.58	(10.39, 18.77)
NEWER DIABETES MEDICATIONS							
Rapid-acting insulin analogues	6.13	2.65	(1.87, 3.42)	2.18	(1.55, 2.80)	3.14	(2.18, 4.10)
Long-acting insulin analogues	12.10	4.54	(3.16, 5.92)	3.79	(2.66, 4.63)	5.32	(3.62, 7.10)
Extended-release metformin	3.05	1.59	(1.10, 2.70)	1.24	(0.80, 1.67)	1.95	(1.38. 2.53)
DPP-4 inhibitors, GLP-1 agonists, and SGLT-2 inhibitors	5.16	1.51	(1.60, 1.97)	1.13	(0.71, 1.54)	1.91	(1.37, 2.45)

SOURCE Authors' analysis of data from IQVIA. **NOTES** Twenty-nine states and the District of Columbia had expanded eligibility for Medicaid by the end of our sample period. Baseline fills are those in 2013, measured in states that subsequently expanded eligibility for Medicaid. Difference-in-differences estimates were adjusted for year by quarter indicator variables, state indicator variables, patient's age group and sex, and quarterly state-level unemployment rates. All changes were significant ($p < 0.01$). 95% confidence intervals are in parentheses. DPP is dipeptidyl peptidase. GLP is glucagon-like peptide. SGLT is sodium-glucose cotransporter.

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patients reached age sixty-five, as expected. The increase in prescription fills was 82 percent smaller for people ages 65–69 than for people ages 60–64, despite the fact that the two groups had identical diabetes prevalence (exhibit 4).

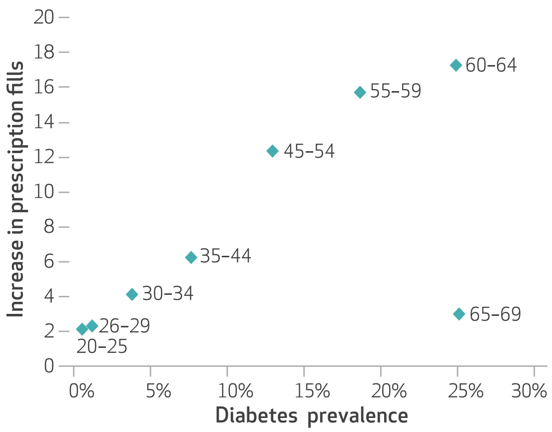
Among people younger than age sixty-five, those in age groups with a higher prevalence of diabetes experienced larger increases in treatment (exhibit 4 and appendix exhibit A5).[45] The correlation between diabetes prevalence and changes in Medicaid prescription fills for diabetes medications among people ages 20–64 was 0.98 ($p < 0.01$).[52] The results (shown in appendix exhibit A6)[45] were similar when, as a robustness check, we included only patients with diagnosed diabetes in the NHANES analysis.

Discussion

This study analyzed the associations between Medicaid eligibility expansions and Medicaid prescription fills for diabetes medications by patients' age and sex and by medication category. We used a large, representative administrative data set that captured over ninety-six million Medicaid prescription fills for diabetes medications in retail outlets in the period January 2008–December 2015. The analysis accounted for changes in population and many possible confounders. Our results imply that an average of thirty additional Medicaid prescriptions for diabetes medications were filled annually per 1,000 population in states that expanded Medicaid eligibility.

EXHIBIT 4

Prevalence of diabetes in 2013–14 and increase in Medicaid diabetes prescription fills per 1,000 population in 2014–15 associated with Medicaid eligibility expansions, by age group (years)



SOURCE Authors' analysis of data from IQVIA and the National Health and Nutrition Examination Survey. **NOTE** 2013–14 are the years just before and during Medicaid eligibility expansions, for most of the twenty-nine states (and the District of Columbia) that had expanded eligibility for Medicaid by the end of our sample period.

Age groups with higher prevalence of diabetes before the ACA, such as ages 55–59, showed larger increases in diabetes prescription fills after Medicaid eligibility expansions. In addition, increases in fills after the expansions were much smaller among people ages sixty-five and older. Because Medicaid is a payer of last resort, eligibility for Medicaid was expected to have a smaller impact among patients who were also eligible for Medicare.

We found that annual prescription fills for insulin and metformin using Medicaid insurance each increased by approximately 40 percent after Medicaid eligibility expansions. In the period 2002–13, insulin's mean price rose 197 percent—growth faster than that of any other drug class used to treat diabetes.[21,53] Estimated insulin spending per patient more than tripled, from $231.48 in 2002 to $736.09 in 2013.[21] Patients without insurance would have been exposed to the full costs of insulin. Gaining Medicaid insurance would have significantly reduced out-of-pocket spending for insulin for previously uninsured patients, thereby facilitating uptake of the medication.

Furthermore, the sizable increase in the use of metformin suggests that many of the newly treated patients may have had recent onset of diabetes. This finding echoes those of previous analyses that linked Medicaid eligibility expansions with increased diabetes diagnoses.[54–57] Indeed, the drug class with the largest relative increase after the expansions (52 percent) was extended-release metformin, a reformulation of the first-line medication for type 2 diabetes.

More broadly, our data suggest that Medicaid eligibility expansions were associated with increased prescription fills for newer diabetes medications. This is important because these medications carry higher costs than the older formulations do but provide benefits such as reduced risk of hypoglycemia and reduced side effects.[22,23,26] Newer medications accounted for about one-third of the increase in Medicaid diabetes prescriptions after the expansions, in line with prior Medicaid prescribing patterns in expansion states.[58] This represents an increase in use of newer medications compared with uninsured patients.

Our findings point to the possible health effects of Medicaid eligibility expansions. In the past, changes in cost sharing for diabetes medications have been associated with changes in health outcomes for patients with diabetes.[59,60] An analysis by the Centers for Disease Control and Prevention found that each additional

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treated patient with diabetes can lead to a reduction of $4,330 (in 1997 US dollars, equivalent to about $6,394 in 2017 US dollars) in inpatient care costs because of prevented hospital admissions.[61] These figures may underestimate the current health effects of treatment, given that improved treatment regimens are now available.[29,62] Indeed, a decline in diabetes-related hospitalizations was observed shortly after Medicaid eligibility expansions in states with high baseline uninsured populations.[63]

We found that the changes in the use of diabetes medications associated with Medicaid eligibility expansions increased over time. Ausmita Ghosh and coauthors reported a 24 percent increase in Medicaid diabetes medications through the first quarter of 2015.[42] We found a 33 percent increase by the end of 2014, within the confidence intervals implied by the standard errors reported in that study, and a 49 percent increase by the end of 2015. The increasing gap over time between states that did and did not expand Medicaid eligibility is apparent from exhibit 2 and is significant (appendix exhibit A2D).[45]

Our study had a number of strengths. First, by using administrative data on prescription fills, we avoided issues of patient self-report bias. Second, these data provided a sufficient sample size to examine the treatment of specific conditions by patients' demographic characteristics and type of medication. Third, because these data were collected as prescriptions were filled, our data were timely and provided eight months of additional follow-up, compared to existing studies.

Fourth, although states with Medicaid eligibility expansions may differ from other states in some respects, population-level factors that differ between the groups do not bias the results in a difference-in-differences analysis as long as trends between the groups would have remained parallel in the absence of an intervention. We presented several analyses indicating parallel trends before the expansions, which provides evidence in support of this assumption.

Finally, we adjusted for state and year by quarter indicator variables, patient age and sex, and quarterly changes in unemployment on the state level to address residual confounding.

Conclusion

This study provides policy makers with new information about the potential benefits of continuing financial support for expansions of Medicaid eligibility. Our findings by drug class suggest that these expansions helped address some of the gaps in access to newer medications for low-income patients. An increase in access to newer medications may have important health effects, because the use of these medications has been linked with improved diabetes control and reduced symptoms in both clinical trials and observational data.[22–28] Furthermore, over a third of the additional diabetes Medicaid prescriptions associated with Medicaid eligibility expansions were for metformin, the first-line oral medication to treat diabetes that is not yet insulin dependent. Improvements in population health that are attributable to improved access to diabetes treatment, including the timely treatment of early-stage disease, could also justify some of the cost of expanding Medicaid. Finally, our study provides new evidence that the increases in treatment associated with Medicaid eligibility expansions can grow over time. ■

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DATAWATCH

Medicaid Expansion Associated With Reductions In Preventable Hospitalizations

Hospitalizations for ambulatory care–sensitive conditions indicate barriers to care outside of inpatient settings. We found that Medicaid expansions under the Affordable Care Act were associated with meaningful reductions in these hospitalizations, which suggests the potential of Medicaid expansions to reduce the need for preventable hospitalizations in vulnerable populations and produce cost savings for the US health care system.

Hospitalizations for ambulatory care–sensitive conditions signal inadequate access to preventive and primary care, and they cost the US health care system over $30 billion in avoidable hospital costs each year.[1–3] Reducing potentially preventable hospitalizations has become a key health policy target for improving population health and bending the health care cost curve.

Medicaid is known to be a crucial mechanism for providing regular sources of ambulatory care for low-income people, a group with high rates of hospitalization for ambulatory care–sensitive conditions.[4] The Affordable Care Act (ACA) has prompted many states to expand eligibility for Medicaid to almost all people with household incomes at or below 138 percent of the federal poverty level. Our study shows that the states that expanded Medicaid under the ACA in 2014 and 2015 saw greater reductions in discharge rates, inpatient days, and hospital costs related to ambulatory care–sensitive conditions than nonexpansion states did (exhibit 1).

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EXHIBIT 1

Unadjusted percent changes from 2009–13 to 2014–15 in discharge rates, inpatient days, and hospital costs related to ambulatory care-sensitive conditions, by whether or not states expanded eligibility for Medicaid

SOURCE Authors' analysis of data for 2009–15 from the State Inpatient Databases of the Agency for Healthcare Research and Quality's Healthcare Cost and Utilization Project. **NOTES** The models were population weighted but not adjusted for other covariates. Significance indicates the difference of the point estimate from zero (that is, no change over time). The error bars indicate 95% confidence intervals, clustered at the state level. $^{***}p < 0.01$ $^{****}p < 0.001$

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In this article we examine all-payer inpatient hospital data to determine the extent to which Medicaid expansion helped alleviate ambulatory care–sensitive conditions and the cost and health burden associated with them.

Study Data And Methods

Our primary data source was the Healthcare Cost and Utilization Project (HCUP) State Inpatient Databases.[5] These all-payer inpatient databases capture information about nearly every hospitalization in US nonfederal, nonrehabilitation hospitals in a uniform format to facilitate multistate comparisons and analyses. Our main analyses used inpatient discharge data from thirty-six states that participated in the HCUP databases in the period 2009–15.

Our primary outcome was the state-level ambulatory care–sensitive condition discharge rate, measured as the number of adult inpatient discharges for these conditions per 1,000 adult residents per year. We used the Prevention Quality Indicators methodology of the Agency for Healthcare Research and Quality to identify ambulatory care–sensitive conditions.[6] (See online appendix A1 for a full list of the *International Classification of Diseases,* Ninth Revision [ICD-9], codes used to identify these diagnoses.)[7]

EXHIBIT 2

Adjusted changes from pre-expansion to post-expansion years in annual discharge rates, inpatient days, and hospital costs related to ambulatory care–sensitive conditions (ACSCs) and associated with Medicaid expansions

	Estimated change	
Variable	Percent	*p* value
DISCHARGE RATES (PER 1,000 ADULT RESIDENTS)		
All ACSCs	−3.47	0.026
All chronic ACSCs	−3.34	0.033
Hypertension and heart failure	−2.12	0.193
COPD and asthma	−4.17	0.043
Diabetes related	−5.07	0.001
All acute ACSCs	−3.71	0.036
Bacterial pneumonia	−4.55	0.029
Urinary tract infection	−2.16	0.076
Dehydration related	−0.74	0.423
INPATIENT DAYS		
Per 1,000 adult residents	−3.09	0.088
Per ACSC discharge	0.69	0.511
HOSPITAL COSTS		
Per adult resident	−2.95	0.004
Per ACSC discharge	−0.72	0.535

SOURCE Authors' analysis of data for 2009–15 from State Inpatient Databases of the Agency for Healthcare Research and Quality's Healthcare Cost and Utilization Project. **NOTES** The models were population weighted and fully adjusted for state-level health care supply, overall economic conditions, the early partial adoption of Affordable Care Act (ACA) Medicaid expansions, and state and year two-way fixed effects. The *p* values were calculated based on state-clustered standard errors. COPD is chronic obstructive pulmonary disease.

We classified the conditions into six categories—three chronic and three acute conditions. We also studied the payer mix of discharge rates for these conditions.

In addition to discharge rates, we studied inpatient days and hospital costs associated with ambulatory care–sensitive conditions. Hospital costs were estimated by multiplying hospital charges from the discharge records by an all-payer inpatient cost-to-charge ratio.[8]

Our key independent variable was an indicator for the implementation of Medicaid expansions under the ACA Medicaid State Plan Amendments provision or a Section 1115 waiver.[9] (See appendix A2 for a detailed policy summary of states' implementation of Medicaid expansion in 2014 and 2015.)[7]

We used a quasi-experimental difference-in-differences design and an approach that included state and year two-way fixed effects, which is commonly used in multistate policy evaluations.[10]

All analyses used a generalized least squares model, weighted by the number of adult residents of a given state in a given year. Standard errors were clustered at the state level. Models were adjusted for state-level health care supply and overall economic conditions, as well as the early partial adoption of the ACA expansions in four states in 2010–11. (See appendix A3 for detailed information on model specification and variable measurement.)[7]

In supplemental analyses we first estimated state-specific policy effects. We then extended the study period to 2001 to include Medicaid expansions under the Health Insurance Flexibility and Accountability initiative in the period 2001–08. These earlier expansions also targeted low-income adults, but the eligibility and benefits tended to be less generous than the ACA expansions and varied more across states (appendix A4).[7]

Our study had several limitations. First, policy decisions to expand Medicaid were not randomly assigned to states. Thus, as is the case with any observational study, we could not definitively establish causality between Medicaid expansions and preventable hospitalizations.

Second, the aggregate nature of the study data did not allow us to examine individual-level changes in insurance coverage and treatment-seeking behavior, thus limiting our ability to make inferences about individual-level mechanisms.

Third, our cost estimates relied on an all-payer cost-to-charge ratio that did not differ across patient populations. Nonetheless, if Medicaid and uninsured patients used hospital services with similar cost-to-charge ratios, estimates of

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cost reallocation between the two groups should have shown minimal bias over time.

Finally, the post-expansion windows in our study were relatively short. Especially for chronic ambulatory care–sensitive conditions, the full potential for Medicaid expansions to reduce preventable hospitalizations remains to be seen.

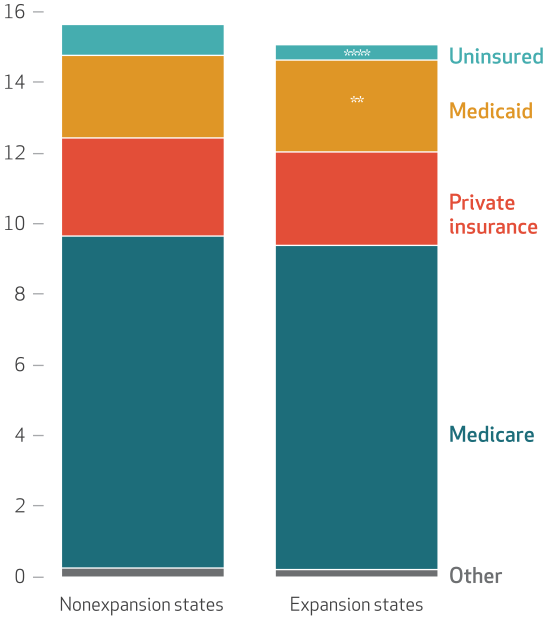
Study Results

When we compared relative changes from the pre-expansion period of 2009–13 to the post-expansion period of 2014–15, our unadjusted estimates showed larger reductions in ambulatory care–sensitive condition discharge rates (–11.96 percent versus –7.80 percent), inpatient days (–11.15 percent versus –7.65 percent), and hospital costs (–8.78 percent versus –5.17 percent) among expansion states than among states that did not expand Medicaid during 2014–15 (exhibit 1).

Our adjusted estimates suggest that Medicaid expansions were associated with a 3.47 percent reduction in annual ambulatory care–sensitive condition discharge rates (exhibit 2), equivalent to 0.54 fewer discharges per 1,000 adult residents (data not shown). Medicaid expansions were also associated with a 3.09 percent reduction in annual ambulatory care–sensitive condition inpatient days, or 2.16 fewer days per 1,000 adult residents per year. Furthermore, we found lower ambulatory care–sensitive condition hospital costs associated with Medicaid expansions (–2.95 percent, or –\$4.23 per adult resident per year). On the other hand, we did not discern any significant change in ambulatory care–sensitive condition inpatient days or hospital costs per discharge, which suggests that the estimated reductions in population-based measures of these inpatient days (per 1,000 adult residents) and hospital costs (per adult resident) were primarily driven by the reductions in ambulatory care–sensitive condition discharge rates. Moreover, the estimated reductions in those discharge rates were largely concentrated among chronic respiratory conditions (chronic obstructive pulmonary disease [COPD] and asthma), diabetes-related complications, and bacterial pneumonia (exhibit 2), which collectively accounted for more than half of the preventable hospitalizations and associated costs during the study period (data not shown).

EXHIBIT 3

Adjusted changes from pre-expansion to post-expansion years in annual discharge rates per 1,000 adult residents for ambulatory care–sensitive conditions, by insurance type and whether or not states expanded eligibility for Medicaid



SOURCE Authors' analysis of data for 2009–15 from State Inpatient Databases of the Agency for Healthcare Research and Quality's Healthcare Cost and Utilization Project. **NOTES** The models were population weighted and fully adjusted as explained in the notes to exhibit 2. The payer mix of discharge rates was calculated (as percentage changes) based on the estimated baseline values and marginal effects of the Affordable Care Act expansions. Significance indicates the difference between expansion and nonexpansion states. *$p < 0.01$ **$p < 0.05$ ****$p < 0.001$

Medicaid expansions were associated with a lower rate of ambulatory care–sensitive conditions discharges not covered by insurance and a higher rate of those discharges covered by Medicaid (exhibit 3). The increase in the latter rate was smaller than the decrease in the former rate, leading to a net reduction of 0.54 discharges per 1,000 adult residents per year associated with Medicaid expansions.

Supplementary analyses revealed heterogeneous effects of Medicaid expansions on preventable hospitalizations across expansion states (appendix A5).[7] Furthermore, similar to the ACA expansions, the earlier Health Insurance Flexibility and Accountability initiative's waiver expansions were associated with reductions in preventable hospitalizations, albeit with a smaller effect size (appendix A6).[7] Finally, the "parallel-trend assumption" tests and falsification tests supported the validity of our methods (appendixes A7–A10).[7]

Discussion

Our study provided comprehensive evidence concerning the implications of state Medicaid expansions for preventable hospitalizations. A recent systematic review of the literature on Medicaid expansions linked them to improve-

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ments in insurance coverage, access to preventive and primary care, and chronic disease management.[4] Building on this literature, our study found that Medicaid expansions were associated with meaningful downstream reductions in hospitalizations for ambulatory care–sensitive conditions.

Reductions in preventable hospitalizations associated with Medicaid expansions were found to be largely concentrated in chronic respiratory conditions (COPD and asthma), diabetes-related complications, and bacterial pneumonia. Our findings were consistent with those of prior research, which has shown that these conditions are particularly amenable to outpatient interventions[11–13] and that Medicaid expansions have been associated with increases in diabetes diagnoses, glucose monitoring, influenza vaccinations, and antibiotic prescriptions.[4]

On the other hand, hospitalizations related to other ambulatory care–sensitive conditions such as circulatory system conditions (hypertension and heart failure) might not have been as responsive to Medicaid expansions, at least in the short run. Possible explanations include that early detection and appropriate management of circulatory system conditions may take longer to reduce the risk for subsequent hospitalizations and that people with these conditions may be older, which increases the relevance of Medicare policies and those targeting the population dually eligible for Medicare and Medicaid.

It is worth noting that despite the overall reductions in preventable hospitalizations associated with Medicaid expansions, the policy effects were shown to vary greatly across states and increase gradually with time. This potential heterogeneity may help reconcile our findings with those of previous studies. For instance, Amy Finkelstein and colleagues focused on the Oregon Health Insurance Experiment, which was not shown to reduce preventable hospitalizations.[14] Our state-specific estimates revealed that the Oregon Medicaid expansion under the ACA was not associated with fewer preventable hospitalizations, either. Furthermore, Gary Pickens and colleagues obtained data for 2011–14 from the HCUP State Inpatient Databases for twenty states and found that the effect of ACA Medicaid expansions on preventable hospitalizations was not significant ($p = 0.068$).[15] However, this estimate may have been influenced by the specific subset of expansion states and limited by the fact that the data covered only one year of the post-expansion period. Future research could explore the driving forces (such as provider capacity, participation, and reimbursement; and cultural and language barriers) behind the different effects of Medicaid expansions on preventable hospitalizations from state to state. Also needed are updated analyses that use the most recent years of data to track the trajectories of preventable hospitalizations in expansion and nonexpansion states. A better understanding of the policy heterogeneity in both state and time dimensions would provide useful evidence for the nonexpansion states that are considering expanding Medicaid and for the expansion states' refinements of their programs.

Conclusion

Our study findings suggest the potential of Medicaid expansions to reduce the need for costly preventable hospitalizations in vulnerable populations and produce cost savings for the US health care system. ■

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Estimating The Effects Of Health Insurance And Other Social Programs On Poverty Under The Affordable Care Act

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ABSTRACT **The effects of health insurance on poverty have been difficult to ascertain because US poverty measures have not taken into account the need for health care and the value of health benefits. We developed the first US poverty measure to include the need for health insurance and to count health insurance benefits as resources available to meet that need—in other words, a health-inclusive poverty measure. We estimated the direct effects of health insurance benefits on health-inclusive poverty for people younger than age sixty-five, comparing the impacts of different health insurance programs and of nonhealth means-tested cash and in-kind benefits, refundable tax credits, and nonhealth social insurance programs. Private health insurance benefits reduced poverty by 3.7 percentage points. Public health insurance benefits (from Medicare, Medicaid, and Affordable Care Act premium subsidies) accounted for nearly one-third of the overall poverty reduction from public benefits. Poor adults with neither children nor a disability experienced little poverty relief from public programs, and what relief they did receive came mostly from premium subsidies and other public health insurance benefits. Medicaid had a larger effect on child poverty than all nonhealth means-tested benefits combined.**

The Affordable Care Act (ACA)—the largest expansion of US social insurance programs since President Lyndon Johnson's War on Poverty initiatives—increased health insurance coverage for millions of lower-income Americans.[1,2] However, it has been difficult to determine the effect of this insurance expansion, or of any health insurance benefits, on poverty because, to date, US poverty measures have not incorporated health needs and benefits.

Poverty is defined as having insufficient resources to meet one's basic needs. The official poverty measure's need standard or "threshold," called the federal poverty level, is defined as three times the cost of a basic diet in 1963, updated annually for price inflation using the Consumer Price Index.[3] The official poverty measure, implemented by the Census Bureau, treats cash income as the only resource available to meet needs. There is also a supplemental poverty measure that includes as resources not only cash income but also noncash benefits, such as housing vouchers.[3,4] The supplemental measure is based on recommendations of a 1995 National Academy of Sciences panel[5] and, though an unofficial measure, is also the responsibility of the Census Bureau. Its threshold for poverty is based on the thirty-third percentile of spending on food, clothing, shelter, and utilities.[3]

Researchers have struggled for decades to find a valid method for including health needs and benefits in poverty measures.[5–7] As a practical matter, it is impossible to determine a health

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care need amount, in dollars, to include in a poverty threshold, because a household's health care needs depend on the detailed health conditions of its members. If a poverty threshold does not include a health care need, the resource measure should not include health insurance benefits, because health insurance cannot be used to purchase nonhealth needs such as food. In turn, if health insurance benefits are not included as available resources, their direct impact on poverty cannot be estimated. In contrast, the direct impacts of other noncash benefits, such as food assistance programs, are routinely estimated.[3,8–13]

Two conditions make it valid to include health needs and benefits in poverty measurement.[14,15] First, health insurance is considered a basic need, like food and shelter. Agreement on the need for health insurance is broad, but not universal.[16–18] Second, guaranteed issue and community rating regulations are in effect (as they are under the Affordable Care Act [ACA]), which means that anyone can purchase insurance, and its price does not depend on health status. This makes it possible to determine the price of health insurance for each household, regardless of the household's specific health care needs.

We examined the impacts of health insurance and other social benefits on poverty using a health-inclusive poverty measure that we recently developed. The measure includes the need for health insurance in determining the poverty threshold and counts health insurance benefits as resources available to meet health needs.[15] We present results for the US population younger than age sixty-five in 2014, the year when the main ACA provisions were implemented. We used the health-inclusive poverty measure in a standard poverty accounting framework[3,8–13] to compare the impacts of a given health insurance program with those of other health insurance programs and of other social programs. We also examined how the impacts of programs varied by disability status, race and ethnicity, family structure, type of health insurance, and age (children and adults).

Integrating all needs and resources into one measure, instead of examining health insurance coverage and material (nonhealth) poverty separately, provides a more complete assessment of the impact of social and health policies. With many provisions of the ACA facing possible elimination,[19] it is important to understand what is at stake for the low-income US population.[20]

Study Data And Methods

CONSTRUCTING A HEALTH-INCLUSIVE POVERTY MEASURE Underlying any poverty measure is a definition of need, which involves social, political, and philosophical judgments. Our health-inclusive poverty measure assumed that health insurance is a basic need. As the specific health insurance need, we chose the cost for nonsmokers of the second-lowest-cost plan on the silver tier available on the ACA Marketplace in the household's ACA geographic rating area. Provisions in the ACA were designed to make this plan affordable. A modification of the supplemental poverty measure, our health-inclusive measure used as its poverty threshold the sum of the health insurance need and the supplemental poverty measure's threshold, which accounts for material (nonhealth) needs.

Measuring poverty also requires valuing household resources. For the health-inclusive poverty measure, we valued health insurance benefits from the government and employers at the cost of the unsubsidized premium of the second-lowest-cost silver plan in 2014. This amount was added to other resources. Even though some people may receive more valuable private or public insurance, we did not allow the value of health insurance resources to exceed the value of the second-lowest-cost silver plan. This was necessary because health insurance is not fungible: Health insurance benefits that exceed health insurance needs cannot be spent on other necessities. For households that received health insurance benefits and had to pay for premiums out of pocket, we deducted from their resources the minimum required out-of-pocket spending on premiums to measure the net value of health insurance resources.

Total household resources were the sum of net health insurance resources and nonhealth resources. Health insurance benefits included both benefits from employer-sponsored insurance and public health insurance (Medicare, Medicaid, and ACA premium subsidies). Nonhealth public benefits included social insurance (unemployment insurance, Social Security, workers' compensation, and veterans' benefits), means-tested cash and in-kind transfers (Temporary Assistance for Needy Families; the Supplemental Nutrition Assistance Program; housing and energy subsidies; the Supplemental Nutrition Program for Women, Infants, and Children); and refundable federal tax credits (the Earned Income Tax Credit and the Child Tax Credit). Nonhealth resources included private cash income and nonhealth public benefits.

We accounted for cost-sharing needs, such as deductibles, by subtracting from resources the out-of-pocket spending for medical care, but we capped those subtractions at the applicable limit. The plans available to individuals determined the applicable cost-sharing limit, from little or

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no cost sharing in Medicaid, to $6,350 for an individual with a silver plan in 2014, to no limit on prescription drug spending in Medicare. In turn, the plans available depended on geographic location, health insurance coverage, health insurance eligibility, employment status, immigration status, and other factors, as described fully in online Appendix Table A.1.[21]

A household is poor if its resources fall below its poverty threshold. As does the Census Bureau, we report the share of people who are poor based on the poverty status of their household.[3,12] We determined poverty status for the same household economic units as the supplemental poverty measure does, and we refer to these units as households.[3]

The Appendix[21] describes our procedures for incorporating cost-sharing needs into the poverty measure; defining health insurance units and household subunits; assigning health insurance types to health insurance units, including for individuals with multiple types of coverage; and aggregating needs and resources to the household level. A conceptual discussion of the health-inclusive poverty measure and its pilot implementation under Massachusetts health reform has been published elsewhere.[15]

DATA We used the same data source as the Census Bureau uses for the official poverty measure and the supplemental poverty measure: the Current Population Survey's Annual Social and Economic Supplement. Our analyses included households with people older and younger than age sixty-five, but we present results only for those younger than age sixty-five. We applied weights to produce estimates representative of the US population.

We included undocumented immigrants in the analyses using a previously developed imputation method.[22] Undocumented immigrants cannot purchase plans on the Marketplaces, but because they can purchase off-Marketplace plans subject to community rating and guaranteed issue, their health insurance needs can be determined.

Information on health insurance premiums and cost sharing was gathered from three sources: Data on ACA Marketplace plans came from the Robert Wood Johnson Foundation's Health Insurance Exchange (HIX) Compare database,[23] data on Medicare Advantage Prescription Drug Plans (the basic plans for Medicare recipients) came from the Centers for Medicare and Medicaid Services via the National Bureau of Economic Research,[24] and information on rules for Medicaid and the Children's Health Insurance Program (CHIP) came primarily from Henry J. Kaiser Family Foundation reports.[25–29] The Appendix provides more detail on our procedures for incorporating premium and cost-sharing information for the silver plans, Medicaid programs, and Medicare Advantage Prescription Drug Plans and for merging plan information with the Current Population Survey public use microdata files.[21]

Medicaid alone has a larger impact on child poverty than all nonhealth means-tested benefits combined.

ACCOUNTING METHODS FOR POVERTY RATES AND GAPS Poverty analysts use a simple accounting method to estimate a social program's impact on the poverty rate.[3,8–13] A counterfactual poverty rate is calculated by subtracting the program's benefit from a household's resources. The impact of the benefit on the poverty rate is calculated as the difference between the actual and the counterfactual poverty rates.

We estimated how much higher the poverty rate would be if a program were eliminated but nothing else changed. Census Bureau poverty reports use this one-at-a-time approach.[3,12] We also estimated how multiple programs reduce poverty in combination. This cumulative approach began by determining a poverty rate in the absence of all programs or transfers—that is, what the poverty rate would be based on private market income alone[30]—or the pretransfer poverty rate. Public benefits were then added sequentially to private income, and the poverty rate was recalculated after each addition. Thus, the cumulative approach shows the incremental contribution of each program to the total poverty-reducing impact of government programs.

Medicare, Social Security Disability Insurance, and Supplemental Security Income transfer large benefits to people with a disability. To focus on the general low-income population whose members do not receive these benefits, we mostly examined poverty rates for people in households with no recipients of disability payments.

Poverty scholars measure the depth of poverty by the *poverty gap*, defined as the amount by which the resources of the poor fall below the poverty threshold.[31] The poverty gap can be used

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The ongoing debate about the ACA demonstrates that there is disagreement about whether health insurance is a fundamental need.

to show how benefits reduce the depth of poverty. This is important because all transfer programs combined are not enough to remove some low-income households from poverty. Yet such transfers provide substantial relief by raising the income of the poor.

Poverty gap accounting starts with the pretransfer poverty gap (the share of needs that is not met by private income) for people who would be poor in the absence of government transfers. This gap measures the depth of poverty without any government benefits. The value of government programs is then added sequentially to resources to show the share of needs that each program meets. Once a household's resources reach the poverty threshold, additional transfers do not reduce the gap further, because the household is no longer poor. Consequently, the order in which benefits are added can influence estimates of their relative contributions to poverty reduction: Resources added later in the accounting process will have smaller effects on the gap than those added earlier. In contrast, order does not matter for the one-at-a-time approach to estimating the effect of a single program on the poverty rate.

Employer-sponsored health insurance is considered private income, although it is subsidized by preferential tax treatment—resulting in $155 billion in federal tax expenditures annually.[32] We use the term *pretransfer* to mean that we did not count employer-sponsored insurance and government benefits as resources.

Including health insurance raises both resources and the poverty threshold. The health insurance need raises the poverty threshold by about $12,000 per household, on average, which represents nearly one-third of the overall health-inclusive poverty threshold. All else being equal, raising the poverty threshold increases the pretransfer poverty rate, while adding health insurance benefits to resources decreases the posttransfer rate.

To obtain needed health care, insured people must pay for cost sharing, such as deductibles. The pretransfer poverty rate should show whether pretransfer income is sufficient to cover all needs: material (such as food and shelter), health insurance, and cost sharing. Similarly, the poverty depth measure should reflect all of these needs. Therefore, for poverty gaps and cumulative rate accounting, we added cost-sharing needs to the threshold, instead of subtracting them from resources. Results for posttransfer poverty rates and one-at-a-time accounting would be mathematically identical whether we added cost-sharing needs to the thresholds or subtracted them from resources. (For an additional discussion of cost sharing, see section 1 of the Appendix.)[21]

LIMITATIONS Our study had several limitations. First, we were unable to account for the direct impact of cost-sharing subsidies or richer Medicaid plans.[33] Consequently, our estimates understate the impact of public health insurance benefits on poverty.

Second, our methods did not incorporate behavioral responses to changes in social programs. For example, if Medicaid benefits or premium subsidies were cut, people might be more motivated to seek a job from an employer offering insurance, or employers might be more likely to offer insurance.

Third, we imputed immigration documentation status. Imputation errors could have led us to overstate or understate the impact of ACA premium subsidies, because people with undocumented status are ineligible for those subsidies.

Fourth, a person's geographical location is sometimes identified at a more aggregated level in the Current Population Survey public use microdata files than in the plans (for example, the rating areas in the ACA Marketplace plans are generally groups of counties). When there was not an exact geographic match, we aggregated plan data to the available CPS geographic unit, as described in Appendix section 3.[21] Therefore, we could have assigned some people higher premiums than they truly faced.

Study Results

HOW MUCH DOES INCLUDING HEALTH INSURANCE CHANGE POVERTY RATES? Overall, the health-inclusive poverty rate was modestly higher than the supplemental and official poverty rates (16.5 percent, 15.4 percent, and 15.7 percent, respectively) (Exhibit 1).[34] Between-group differences in poverty rates were similar across the

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EXHIBIT 1

Health-inclusive poverty rates and impacts of health insurance and public benefits on those rates for people younger than age 65, by selected characteristics, 2014

		Poverty rates						Impacts of health insurance and public benefits from cumulative accounting (percentage points)		
		Cumulative accounting of health-inclusive poverty rate								
	Number	Pre-transfer rate[a]	Adding ESI	Adding public health insurance[b]	Adding other public benefits[c]: HIPM rate	SPM rate[d]	Official poverty rate[d]	ESI	Public health insurance[b]	Other public benefits[c]
All people	173,937	35.2%	31.5%	26.9%	16.5%	15.4%	15.7%	3.7	4.6	10.4
HOUSEHOLDS WITH DISABILITY RECIPIENTS										
People in households with at least one recipient	17,858	70.0%	66.5%	56.6%	21.2%	21.7%	25.9%	3.6	9.9	35.4
PEOPLE IN HOUSEHOLDS WITH NO DISABILITY RECIPIENTS										
All	156,079	31.0%	27.3%	23.3%	16.0%	14.7%	14.5%	3.7	4.0	7.3
Type of insurance										
ESI	100,096	15.6	10.4	9.2	6.0	6.4	4.5	5.1	1.2	3.2
Medicaid or CHIP	25,065	72.7	71.2	60.4	37.4	34.9	44.5	1.5	10.7	23.1
Individual insurance[e]	11,956	39.0	38.5	30.7	23.7	21.8	16.5	0.5	7.8	7.0
Uninsured, subsidies[f]	17,778	52.6	51.5	44.4	35.7	27.4	27.3	1.1	7.1	8.7
Uninsured, no subsidies[f]	17,778	52.6	51.5	48.9	40.6	27.4	27.3	1.1	2.6	8.3
Race/ethnicity										
Hispanic	32,339	50.7	46.0	40.4	29.0	24.5	23.4	4.7	5.6	11.4
Non-Hispanic black	16,571	46.1	41.4	36.5	24.2	22.2	24.8	4.7	4.9	12.3
Non-Hispanic white	91,789	21.2	18.1	15.0	10.0	9.7	9.5	3.1	3.1	5.1
Age group										
Younger than 18	50,398	38.1	34.0	29.5	17.9	16.0	20.3	4.0	4.5	11.7
Household structure[g]										
One parent	11,300	66.1	62.5	57.9	34.7	33.1	42.1	3.6	4.7	23.2
Two parents	66,156	28.2	24.4	20.9	12.8	11.3	14.2	3.7	3.5	8.1
Two adults	20,730	17.3	14.3	12.1	10.2	9.6	8.6	3.0	2.3	1.9
One adult	13,435	32.6	30.1	27.8	25.4	25.2	20.9	2.5	2.3	2.4

SOURCE Authors' analysis of data for 2014 from the Current Population Survey's Annual Social and Economic Supplement (March 2015). **NOTES** Poverty rates are the percentages of people in households with resources below the measure's poverty thresholds. Cumulative accounting examines how the health-inclusive poverty rate changes as categories of programs are added in turn. Impacts are differences between the health-inclusive poverty rate before and after adding each category of benefits to resources. For example, the cumulative impact of employer-sponsored health insurance (ESI) (3.7 percentage points) is the difference between the pretransfer health-inclusive rate with no benefits included in resources (35.2 percent) and the rate after adding ESI to resources (31.5 percent) (see the text for details). Impacts might not sum because of rounding. HIPM is health-inclusive poverty measure. SPM is supplemental poverty measure (defined in the text). CHIP is Children's Health Insurance Program. [a]Income includes private cash income net of taxes and necessary work and child care expenses. Threshold includes cost-sharing needs (expenditures on care, capped at the applicable level). More information is available in the text and the Appendix (see Note 21 in text). [b]Includes Medicare, Medicaid, and Affordable Care Act (ACA) premium subsidies. Except where noted, the uninsured are credited with subsidies for which they are eligible (see the text). [c]Includes social insurance, means-tested cash and in-kind transfers, and refundable federal tax credits (see the text for details). [d]Authors' calculations from the Current Population Survey's Annual Social and Economic Supplement. [e]Any individual health insurance policy, whether purchased directly from a health insurer or on a federal or state ACA Marketplace. [f]Some uninsured people are eligible for premium subsidies. We show the results for the eligible uninsured, both crediting and not crediting them with subsidies. Subsequent exhibits credit the eligible uninsured with subsidies. [g]A one-parent household consists of one parent and at least one child; a two-parent household consists of two adults and at least one child. One- and two-adult households consist of one or two adults, respectively, and no children.

three measures: People younger than eighteen and those in a household with at least one disability recipient had above-average poverty rates, rates for Hispanics and non-Hispanic blacks were more than twice those for non-Hispanic whites, and rates for people in single-parent and lone-adult households were higher than for those in two-parent and two-adult households. Because the official poverty measure does not count in-kind benefits (such as food assistance) as resources, the official poverty rates tended to be higher than the supplemental and health-inclusive poverty rates for groups eligible for benefits, particularly households with children.

The difference between the health-inclusive and supplemental poverty rates was largest for the uninsured. The health-inclusive poverty mea-

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sure raised the poverty threshold for everyone by including the need for health insurance, but it raised resources only for those with health insurance benefits. Some uninsured people would be eligible for ACA premium subsidies if they purchased insurance. Even counting available but unused premium subsidies, the health-inclusive poverty rate for the uninsured was 35.7 percent, which exceeded the corresponding supplemental poverty rate by 8.3 percentage points. Without counting unused premium subsidies, the difference was 13.2 percentage points. Hereafter, results for the uninsured refer only to those credited with subsidies.

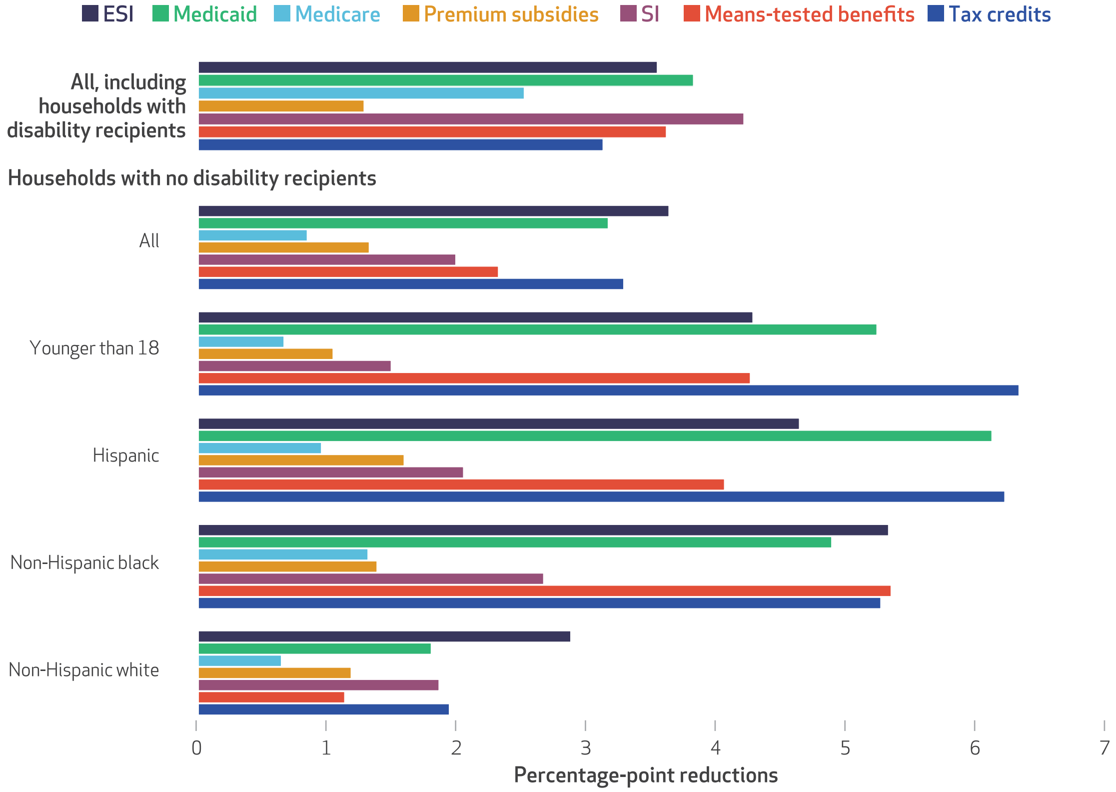
For people with individual plans purchased from an insurer or on a Marketplace, the health-inclusive poverty rate modestly exceeded the supplemental poverty rate (23.7 percent versus 21.8 percent). Hispanics' health-inclusive poverty rate also exceeded their supplemental poverty rate (by 4.5 percentage points), partly as a result of their high uninsurance rate.

IMPACTS OF DIFFERENT TYPES OF HEALTH INSURANCE AND OTHER SOCIAL PROGRAMS Although poverty rates and differentials were broadly similar across the three measures, only the health-inclusive poverty measure can show the direct impact of health insurance benefits on poverty. Before employer-sponsored insurance or government benefits were taken into account, the health-inclusive poverty rate (the pretransfer rate) was 35.2 percent (Exhibit 1). Adding employer-sponsored insurance (ESI) reduced health-inclusive poverty substantially, by 3.7 percentage points. Adding public health insurance reduced it even more, by an additional 4.6 percentage points. Adding all other public benefits reduced it by a further 10.4 percentage points. Thus, public health insurance benefits accounted for nearly one-third of the 15.0 percentage points of poverty reduction from all public (health and nonhealth) benefits.

For people in households with at least one disability recipient, public nonhealth benefits reduced poverty by 35.4 percentage points, and public health insurance benefits reduced it by another 9.9 percentage points (Exhibit 1). In contrast, in households with no disability recip-

EXHIBIT 2

Percentage-point reductions in health-inclusive poverty rates from various programs, by selected characteristics



SOURCE Authors' analysis of data for 2014 from Current Population Survey's Annual Social and Economic Supplement (March 2015). **NOTES** Bars represent one-at-a-time accounting impacts (see the "Study Data And Methods" section). "Disability recipient" means a person who receives disability payments. ESI is employer-sponsored health insurance. SI is social insurance (defined in the text). "Households with no disability recipients" excludes everyone in a household with one or more disability recipients.

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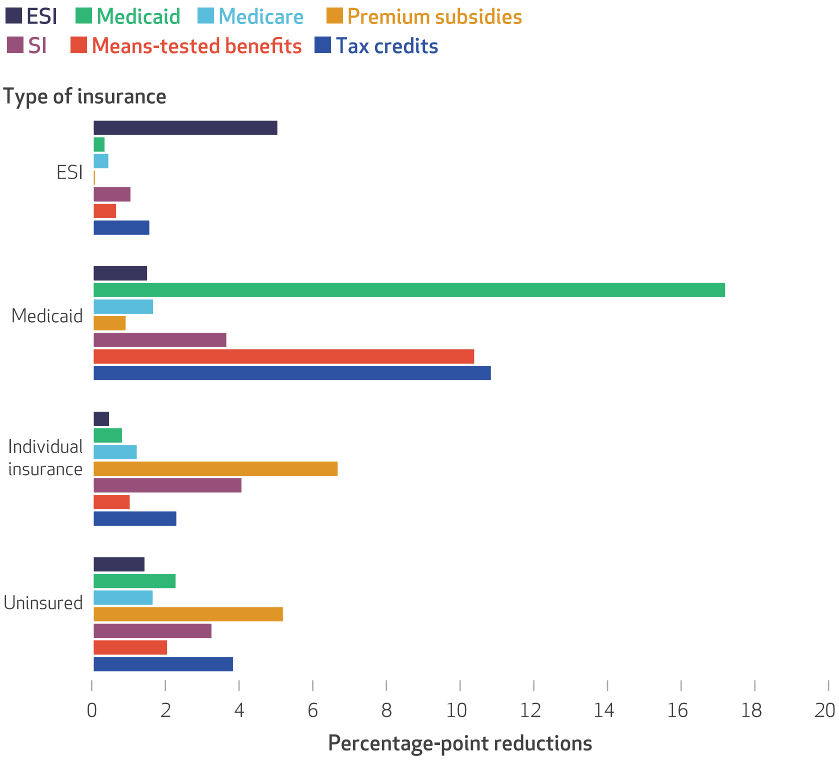
ients, health insurance reduced poverty more than other programs did: by 7.7 percentage points for public and employer-sponsored health insurance combined, compared to 7.3 percentage points for all other public benefits.

We next compared health insurance programs to each other and to three categories of nonhealth programs—social insurance, means-tested cash and in-kind benefits, and federal refundable tax credits—in terms of the impacts on poverty of removing them one at a time. Medicaid is among the most effective antipoverty programs. For all people, Medicaid (including CHIP)[35] reduced health-inclusive poverty by 3.8 percentage points, an impact comparable to the combined effect of all nonhealth social insurance programs and greater than the effects of means-tested benefits and of refundable federal tax credits (Exhibit 2). For people in households with no disability recipients, Medicaid also reduced the poverty rates of Hispanics and non-Hispanic blacks by 6.1 percentage points and 4.9 percentage points, respectively—greater reductions than social insurance produced and comparable to those of means-tested benefits and of tax credits. Medicaid's effect on non-Hispanic whites (whose poverty rates are much lower) was smaller, though notable, at 1.8 percentage points.

EXHIBIT 3

Percentage-point reductions in health-inclusive poverty rates from various programs among people in households with no disability recipients, by health insurance type



SOURCE Authors' analysis of data for 2014 from the Current Population Survey's Annual Social and Economic Supplement (March 2015). **NOTES** Bars represent one-at-a-time accounting impacts (see the "Study Data And Methods" section). Individual insurance is defined in the Notes to Exhibit 1. "Disability recipient" and ESI are defined in the Notes to Exhibit 2. SI is social insurance (defined in the text). Except where noted, the uninsured are credited with subsidies for which they are eligible (see the text).

Medicaid was especially effective for children younger than age eighteen, reducing their poverty rate by 5.3 percentage points (Exhibit 2). That impact is comparable to the effect of means-tested transfers, though not quite as large as that of tax credits. Benjamin Sommers and Donald Oellerich[36] estimated substantially smaller effects of Medicaid on poverty using the supplemental poverty measure: a 1.0-percentage-point reduction among children and a 0.4-percentage-point reduction among nonelderly adults without a disability. As they noted, their estimates showed the impact of reduced out-of-pocket spending but not Medicaid's "presumed primary benefit of improved access to care and health."[36(p829)]

Employer-sponsored insurance also lifted many people from poverty. Premium subsidies had a small effect compared to that of Medicaid, as a result of much smaller expenditures and eligibility restrictions.

Health insurance benefits decreased poverty rates greatly among people who received benefits and their families: Medicaid reduced poverty among its recipients by a remarkable 17.1 percentage points, employer-sponsored insurance reduced the poverty rate of those it covered by 5.0 percentage points, and premium subsidies reduced the poverty of the individually insured by 6.6 percentage points (Exhibit 3). Premium subsidies would reduce the poverty of the uninsured by 5.1 percentage points if those people were to purchase insurance.

DEPTH OF HEALTH-INCLUSIVE POVERTY As noted above, poverty depth is measured by the poverty gap. Before employer-sponsored insurance and public benefits were taken into account, the poverty gap was 52 percent for all people, meaning that private cash income alone met less than half of the needs of the pretransfer poor (Exhibit 4). Employer-sponsored insurance met about 5 percent of their remaining needs, leaving 47 percent of needs still unmet. Public health insurance benefits (that is, Medicaid, Medicare, and ACA premium subsidies) closed roughly one-third of the gap. Together, all other means-tested benefits, social insurance, and refundable tax credits closed another third of the gap. Thus, not counting health insurance benefits would miss half of the impact of government programs on the depth of poverty. After all benefits were accounted for, 15 percent of needs

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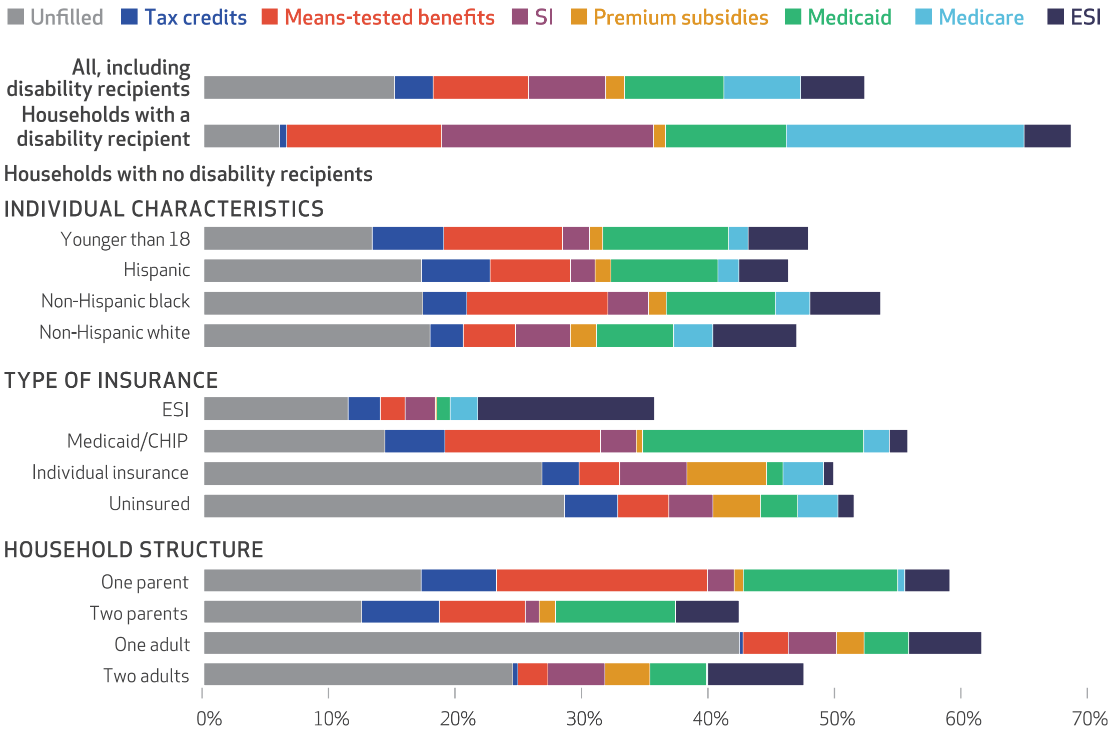
EXHIBIT 4

Percentages of the health-inclusive poverty gap filled, by selected population characteristics



SOURCE Authors' analysis of data for 2014 from the Current Population Survey's Annual Social and Economic Supplement (March 2015). **NOTES** The poverty gap is the average amount by which the household resources of a poor person fall below the health-inclusive threshold. For example, for all people, private cash income alone (net of taxes and necessary work and child care expenses) covered 48 percent of the needs of the average pre-transfer poor household, leaving a gap of 52 percent. Adding to private cash income all of the benefits shown still left a gap of 15 percent. Individual insurance and household structures are defined in the Notes to Exhibit 1. "Disability recipient" and ESI are defined in the Notes to Exhibit 2. CHIP is the Children's Health Insurance Program. SI is social insurance (defined in the text). Except where noted, the uninsured are credited with subsidies for which they are eligible (see the text).

remained unmet.

The pretransfer poverty gap among people in a household with at least one disability recipient was large: 69 percent. However, their poverty was greatly reduced by Medicaid, Medicare, and nonhealth benefits, leaving a gap of 6 percent after accounting for these benefits. Health and nonhealth benefits to low-income families with children also reduced the poverty gap greatly, leaving 12–17 percent of needs unmet.

Other groups got less poverty relief. Despite substantial poverty reduction from premium subsidies to the individually insured—and the uninsured, if they were to buy insurance—after all transfers, more than one-quarter of the needs of these groups remained unfilled. Poor households consisting of one or two adults got little poverty reduction from public benefits, with unfilled poverty gaps of 42 percent and 25 percent, respectively.

Discussion

By including health insurance in poverty measurement, our estimates show the direct impacts of health insurance benefits on poverty rates in the United States. The health-inclusive measure shows that public health insurance benefits have a large impact on poverty among people younger than age sixty-five. Moreover, Medicare, Medicaid and premium subsidies are among the most important antipoverty programs, accounting for over one-third of the poverty reduction from all public benefits for people in households without a disability recipient. For everyone younger than age sixty-five, the impact of Medicaid is nearly 4 percentage points, almost as large as that of nonhealth social insurance and larger than either tax credits or all means-tested programs. And Medicaid alone has a larger impact on child poverty than all nonhealth means-tested benefits combined, though the impact of tax credits is slightly larger.

In contrast, poor adults with neither children nor a disability experienced little poverty relief

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from public programs, and what relief they did receive came mostly from premium subsidies and other public health insurance benefits. By integrating both material and health insurance needs and resources, the health-inclusive poverty measure shows the compounded disadvantage of having a low income, being uninsured, and being ineligible for means-tested transfers. In particular, it makes readily apparent the full scope of unmet needs among the uninsured and individually insured.

While including health insurance needs raises the poverty threshold substantially, valuing the enormous transfers of public and private health insurance benefits raises household resources appropriately. For people who agree that health insurance is a basic need, the health-inclusive poverty measure correctly measures poverty and accounts for the impacts of health and nonhealth transfers. However, the ongoing debate about the ACA and the government's role in health insurance demonstrates that there is disagreement about whether health insurance is a fundamental need.

Conclusion

In his 2014 presidential address to the Population Association of America, Robert Moffitt argued that US social and health spending reflects political choices about who among the poor is deemed to be "deserving."[37] The elderly, people with a disability, and working families with children have been placed in this category. The ACA represented a departure from such a categorical system. Its consequences can be seen, for example, in our results showing the poverty-reducing effects of premium subsidies and the substantial impacts of Medicaid for two-parent families and adults with neither children nor a disability—groups whose eligibility for Medicaid has traditionally been limited. As Congress debates reductions in government support for health insurance,[18] Americans should understand that many of those who would lose health insurance would be thrust into poverty, yet their poverty would not be detected by standard poverty measures. ■

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30 We subtracted taxes and necessary work expenses—those expenses, including child care, deemed necessary for work by the Census Bureau in calculating the supplemental poverty measure—from the private market income to measure the pretransfer private market income. Deducting taxes and necessary work expenses from private income raised the pretransfer poverty rate from 28.3 percent to 35.2 percent.

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34 Confidence intervals for all of the exhibits are available from the authors on request.

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The NEW ENGLAND JOURNAL of MEDICINE

SPECIAL ARTICLE

The Oregon Experiment — Effects of Medicaid on Clinical Outcomes

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ABSTRACT

BACKGROUND

Despite the imminent expansion of Medicaid coverage for low-income adults, the effects of expanding coverage are unclear. The 2008 Medicaid expansion in Oregon based on lottery drawings from a waiting list provided an opportunity to evaluate these effects.

METHODS

Approximately 2 years after the lottery, we obtained data from 6387 adults who were randomly selected to be able to apply for Medicaid coverage and 5842 adults who were not selected. Measures included blood-pressure, cholesterol, and glycated hemoglobin levels; screening for depression; medication inventories; and self-reported diagnoses, health status, health care utilization, and out-of-pocket spending for such services. We used the random assignment in the lottery to calculate the effect of Medicaid coverage.

RESULTS

We found no significant effect of Medicaid coverage on the prevalence or diagnosis of hypertension or high cholesterol levels or on the use of medication for these conditions. Medicaid coverage significantly increased the probability of a diagnosis of diabetes and the use of diabetes medication, but we observed no significant effect on average glycated hemoglobin levels or on the percentage of participants with levels of 6.5% or higher. Medicaid coverage decreased the probability of a positive screening for depression (−9.15 percentage points; 95% confidence interval, −16.70 to −1.60; P=0.02), increased the use of many preventive services, and nearly eliminated catastrophic out-of-pocket medical expenditures.

CONCLUSIONS

This randomized, controlled study showed that Medicaid coverage generated no significant improvements in measured physical health outcomes in the first 2 years, but it did increase use of health care services, raise rates of diabetes detection and management, lower rates of depression, and reduce financial strain.

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*Members of the Oregon Health Study Group are listed in the Supplementary Appendix, available at NEJM.org.

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In 2008, Oregon initiated a limited expansion of its Medicaid program for low-income adults through a lottery drawing of approximately 30,000 names from a waiting list of almost 90,000 persons. Selected adults won the opportunity to apply for Medicaid and to enroll if they met eligibility requirements. This lottery presented an opportunity to study the effects of Medicaid with the use of random assignment. Earlier, nonrandomized studies sought to investigate the effect of Medicaid on health outcomes in adults with the use of quasi-experimental approaches.[1-3] Although these approaches can be an improvement over observational designs and often involve larger samples than are feasible with a randomized design, they cannot eliminate confounding factors as effectively as random assignment. We used the random assignment embedded in the Oregon Medicaid lottery to examine the effects of insurance coverage on health care use and health outcomes after approximately 2 years.

METHODS

RANDOMIZATION AND INTERVENTION

Oregon Health Plan Standard is a Medicaid program for low-income, uninsured, able-bodied adults who are not eligible for other public insurance in Oregon (e.g., Medicare for persons 65 years of age or older and for disabled persons; the Children's Health Insurance Program for poor children; or Medicaid for poor children, pregnant women, or other specific, categorically eligible populations). Oregon Health Plan Standard closed to new enrollment in 2004, but the state opened a new waiting list in early 2008 and then conducted eight random lottery drawings from the list between March and September of that year to allocate a limited number of spots.

Persons who were selected won the opportunity — for themselves and any household member — to apply for Oregon Health Plan Standard. To be eligible, persons had to be 19 to 64 years of age and Oregon residents who were U.S. citizens or legal immigrants; they had to be ineligible for other public insurance and uninsured for the previous 6 months, with an income that was below 100% of the federal poverty level and assets of less than $2,000. Persons who were randomly selected in the lottery were sent an application. Those who completed it and met the eligibility criteria were enrolled in the plan. Oregon Health Plan Standard provides comprehensive medical benefits, including prescription drugs, with no patient cost-sharing and low monthly premiums ($0 to $20, based on income), mostly through managed-care organizations. The lottery process and Oregon Health Plan Standard are described in more detail elsewhere.[4]

DATA COLLECTION

We used an in-person data-collection protocol to assess a wide variety of outcomes. We limited data collection to the Portland, Oregon, metropolitan area because of logistical constraints. Our study population included 20,745 people: 10,405 selected in the lottery (the lottery winners) and 10,340 not selected (the control group). We conducted interviews between September 2009 and December 2010. The interviews took place an average of 25 months after the lottery began.

Our data-collection protocol included detailed questionnaires on health care, health status, and insurance coverage; an inventory of medications; and performance of anthropometric and blood-pressure measurements. Dried blood spots were also obtained.[5] Depression was assessed with the use of the eight-question version of the Patient Health Questionnaire (PHQ-8),[6] and self-reported health-related quality of life was assessed with the use of the Medical Outcomes Study 8-Item Short-Form Survey.[7] More information on recruitment and field-collection protocols are included in the study protocol (available with the full text of this article at NEJM.org); more information on specific outcome measures is provided in the Supplementary Appendix (available at NEJM.org). Multiple institutional review boards approved the study, and written informed consent was obtained from all participants.

STATISTICAL ANALYSIS

Virtually all the analyses reported here were prespecified and publicly archived (see the protocol).[8] Prespecification was designed to minimize issues of data and specification mining and to provide a record of the full set of planned analyses. The results of a few additional post hoc analyses are also presented and are noted as such in Tables 1 through 5. Analyses were performed with the use of Stata software, version 12.[9]

Adults randomly selected in the lottery were given the option to apply for Medicaid, but not all persons selected by the lottery enrolled in

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Medicaid (either because they did not apply or because they were deemed ineligible). Lottery selection increased the probability of Medicaid coverage during our study period by 24.1 percentage points (95% confidence interval [CI], 22.3 to 25.9; P<0.001). The subgroup of lottery winners who ultimately enrolled in Medicaid was not comparable to the overall group of persons who did not win the lottery. We therefore used a standard instrumental-variable approach (in which lottery selection was the instrument for Medicaid coverage) to estimate the causal effect of enrollment in Medicaid. Intuitively, since the lottery increased the chance of being enrolled in Medicaid by about 25 percentage points, and we assumed that the lottery affected outcomes only by changing Medicaid enrollment, the effect of being enrolled in Medicaid was simply about 4 times (i.e., 1 divided by 0.25) as high as the effect of being able to apply for Medicaid. This yielded a causal estimate of the effect of insurance coverage.[10] (See the Supplementary Appendix for additional details.)

All analyses were adjusted for the number of household members on the lottery list because selection was random, conditional on household size. Standard errors were clustered according to household to account for intrahousehold correlation. We fitted linear probability models for binary outcomes. As sensitivity checks, we showed that our results were robust when the average marginal effects from logistic regressions for binary outcomes were estimated and when demographic characteristics were included as covariates (see the Supplementary Appendix). All analyses were weighted for the sampling and field-collection design; construction of the weights is detailed in the Supplementary Appendix.

RESULTS

STUDY POPULATION

Characteristics of the respondents are shown in Table 1. A total of 12,229 persons in the study sample responded to the survey, for an effective response rate of 73%. There were no significant differences between those selected in the lottery and those not selected with respect to the response rates to either the full survey (0.28 percentage points higher in the group selected in the lottery, P=0.86) or specific survey measures, each of which had a response rate of at least 97% among people who completed any part of the survey. Just over half the participants were women, about a quarter were 50 to 64 years of age (the oldest eligible age group), and about 70% were non-Hispanic white. There were no significant differences between those selected in the lottery and those not selected with respect to these characteristics (F statistic, 0.20; P=0.99) or to the wide variety of prerandomization and interview characteristics examined (see the Supplementary Appendix).

Table 1. **Characteristics of the 12,229 Survey Respondents.***

Characteristic	Controls (N=5842)	Lottery Winners (N=6387)†	P Value
	percent		
Female sex	56.9	56.4	0.60
Age group‡			
19–34 yr	36.0	35.1	0.38
35–49 yr	36.4	36.6	0.87
50–64 yr	27.6	28.3	0.43
Race or ethnic group§			
Non-Hispanic			
White	68.8	69.2	0.68
Black	10.5	10.6	0.82
Other	14.8	14.8	0.97
Hispanic	17.2	17.0	0.82
Interview conducted in English	88.2	88.5	0.74

* Values for the control group (persons not selected in the lottery) are weighted means, and values for the lottery-winner group are regression-adjusted weighted means. P values are for two-tailed t-tests of the equality of the two means.

† Lottery winners were adults who were randomly selected in the lottery to be able to apply for Medicaid coverage.

‡ The data on age are for the age of the respondent at the time of the in-person interview. The study sample was restricted to persons who were between 19 and 64 years of age during the study period.

§ Race and ethnic group were self-reported. The categories of non-Hispanic race (white, black, and other) were not mutually exclusive; respondents could report as many races or ethnic groups as they wished.

CLINICAL MEASURES AND HEALTH OUTCOMES

Table 2 shows estimated effects of Medicaid coverage on blood-pressure, total and high-density lipoprotein (HDL) cholesterol, and glycated hemoglobin levels and depression. In the control group, 30% of the survey respondents had positive screening results for depression, and we detected elevated blood pressure in 16%, a high total cholesterol level in 14%, and a glycated hemoglobin level of 6.5% or more (a diagnostic criterion for

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diabetes) in 5%. Medicaid coverage did not have a significant effect on measures of blood pressure, cholesterol, or glycated hemoglobin. Further analyses involving two prespecified subgroups — persons 50 to 64 years of age and those who reported receiving a diagnosis of diabetes, hypertension, a high cholesterol level, a heart attack, or congestive heart failure before the lottery (all of which were balanced across the two study groups) — showed similar results (see the Supplementary Appendix).

The predicted 10-year risk of cardiovascular events was measured with the use of the Framingham risk score, which estimates risk among persons older than 30 years of age according to sex, age, levels of total cholesterol and HDL cholesterol, blood pressure and use or nonuse of blood-pressure medication, status with respect to diabetes, and smoking status, with the predicted risk of a cardiovascular event within 10 years ranging from less than 1% to 30%.[11] The 10-year predicted risk did not change significantly with Medicaid coverage (−0.21 percentage points; 95% CI, −1.56 to 1.15; P=0.76).

We investigated whether Medicaid coverage affected the diagnosis of and use of medication for hypertension, hypercholesterolemia, or diabetes. Table 2 shows diagnoses after the lottery and current medication use. We found no effect of Medicaid coverage on diagnoses after the lottery or on the use of medication for blood-pressure and high cholesterol levels. We did, however, find a greater probability of receiving a diagnosis of diabetes (3.83 percentage points; 95% CI, 1.93 to 5.73; P<0.001) and using medications for diabetes (5.43 percentage points; 95% CI, 1.39 to 9.48; P=0.008). These are substantial increases from the mean rates of diagnosis and medication use in the control group (1.1% and 6.4%, respectively).

A positive result on screening for depression was defined as a score of 10 or more on the PHQ-8 (which ranges from 0 to 24, with higher

Table 2. Mean Values and Absolute Change in Clinical Measures and Health Outcomes with Medicaid Coverage.*

Variable	Mean Value in Control Group	Change with Medicaid Coverage (95% CI)†	P Value
Blood pressure			
Systolic (mm Hg)	119.3±16.9	−0.52 (−2.97 to 1.93)	0.68
Diastolic (mm Hg)	76.0±12.1	−0.81 (−2.65 to 1.04)	0.39
Elevated (%)‡	16.3	−1.33 (−7.16 to 4.49)	0.65
Hypertension			
Diagnosis after lottery (%)§¶	5.6	1.76 (−1.89 to 5.40)	0.34
Current use of medication for hypertension (%)§‖	13.9	0.66 (−4.48 to 5.80)	0.80
Cholesterol**			
Total level (mg/dl)	204.1±34.0	2.20 (−3.44 to 7.84)	0.45
High total level (%)	14.1	−2.43 (−7.75 to 2.89)	0.37
HDL level (mg/dl)	47.6±13.1	0.83 (−1.31 to 2.98)	0.45
Low HDL level (%)	28.0	−2.82 (−10.28 to 4.64)	0.46
Hypercholesterolemia			
Diagnosis after lottery (%)§¶	6.1	2.39 (−1.52 to 6.29)	0.23
Current use of medication for high cholesterol level (%)§‖	8.5	3.80 (−0.75 to 8.35)	0.10
Glycated hemoglobin			
Level (%)	5.3±0.6	0.01 (−0.09 to 0.11)	0.82
Level ≥6.5% (%)††	5.1	−0.93 (−4.44 to 2.59)	0.61
Diabetes			
Diagnosis after lottery (%)§¶	1.1	3.83 (1.93 to 5.73)	<0.001
Current use of medication for diabetes (%)§‖	6.4	5.43 (1.39 to 9.48)	0.008

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Table 2. (Continued.)

Variable	Mean Value in Control Group	Change with Medicaid Coverage (95% CI)†	P Value
Depression			
Positive screening result (%)‡‡	30.0	−9.15 (−16.70 to −1.60)	0.02
Diagnosis after lottery (%)§¶	4.8	3.81 (0.15 to 7.46)	0.04
Current use of medication for depression (%)§‖	16.8	5.49 (−0.46 to 11.45)	0.07
Framingham risk score (%)§§			
Overall	8.2±7.5	−0.21 (−1.56 to 1.15)	0.76
High-risk diagnosis	11.6±8.3	1.63 (−1.11 to 4.37)	0.24
Age of 50–64 yr	13.9±8.2	−0.37 (−2.64 to 1.90)	0.75

* Plus–minus values are weighted means ±SD. Where means are shown without standard deviations, they are weighted means. The effect of Medicaid coverage was estimated with the use of two-stage least-squares instrumental-variable regression. All regressions include indicators for the number of household members on the lottery list, and all standard errors were "clustered," or adjusted to allow for arbitrary correction of error terms within households. For the blood-pressure measures, all regressions also included controls for age (with dummies for age decile) and sex. All analyses were weighted with the use of survey weights. The sample size was all 12,229 survey respondents for all measures except for the Framingham risk score. HDL denotes high-density lipoprotein.

† For variables measured as percentages, the change is expressed as percentage points.

‡ Elevated blood pressure was defined as a systolic pressure of 140 mm Hg or more and a diastolic pressure of 90 mm Hg or more.

§ This analysis was not prespecified.

¶ A participant was considered to have received a diagnosis of a certain condition after the lottery if he or she reported a first diagnosis after March 2008 (the start of the lottery). A participant who received a diagnosis before March 2008 was not considered to have a diagnosis after the lottery.

‖ A participant was considered to have received medication for the condition if one or more of the medications recorded during the interview was classified as relevant for that condition.

** A high total cholesterol level was defined as 240 mg per deciliter (6.2 mmol per liter) or higher. A low HDL cholesterol level was defined as less than 40 mg per deciliter (1.03 mmol per liter). There was no separate measurement of low-density lipoprotein cholesterol.

†† A glycated hemoglobin level of 6.5% or higher is a diagnostic criterion for diabetes.

‡‡ A positive result on screening for depression was defined as a score of 10 or higher on the Patient Health Questionnaire 8 (PHQ-8). Scores on the PHQ-8 range from 0 to 24, with higher scores indicating more symptoms of depression.

§§ The Framingham risk score was used to predict the 10-year cardiovascular risk. Risk scores were calculated separately for men and women on the basis of the following variables: age, total cholesterol and HDL cholesterol levels, measured blood pressure and use or nonuse of medication for high blood pressure, current smoking status, and status with respect to a glycated hemoglobin level ≥6.5%. Framingham risk scores, which are calculated for persons 30 years of age or older, range from 0.99 to 30%. Samples sizes for risk scores were 9525 participants overall, 3099 participants with high-risk diagnoses, and 3372 participants with an age of 50 to 64 years. A high-risk diagnosis was defined as a diagnosis of diabetes, hypertension, hypercholesterolemia, myocardial infarction, or congestive heart failure before the lottery (i.e., before March 2008).

scores indicating more symptoms of depression). Medicaid coverage resulted in an absolute decrease in the rate of depression of 9.15 percentage points (95% CI, −16.7 to −1.60; P=0.02), representing a relative reduction of 30%. Although there was no significant increase in the use of medication for depression, Medicaid coverage led to an absolute increase in the probability of receiving a diagnosis of depression after the lottery of 3.81 percentage points (95% CI, 0.15 to 7.46; P=0.04), representing a relative increase of about 80%.

HEALTH-RELATED QUALITY OF LIFE AND HAPPINESS

Table 3 shows the effects of Medicaid coverage on health-related quality of life and level of happiness. Medicaid coverage led to an increase in the proportion of people who reported that their health was the same or better as compared with their health 1 year previously (7.84 percentage points; 95% CI, 1.45 to 14.23; P=0.02). The physical-component and mental-component scores of the health-related quality of life measure are based on different weighted combinations of the eight-question battery; each ranges from 0 to 100,

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Table 3. Mean Values and Absolute Change in Health-Related Quality of Life and Happiness with Medicaid Coverage.*

Variable	Mean Value in Control Group	Change with Medicaid Coverage (95% CI)†	P Value
Health-related quality of life			
Health same or better vs. 1 yr earlier (%)	80.4	7.84 (1.45 to 14.23)	0.02
SF-8 subscale‡			
Mental-component score	44.4±11.4	1.95 (0.03 to 3.88)	0.05
Physical-component score	45.5±10.5	1.20 (−0.54 to 2.93)	0.18
No pain or very mild pain (%)	56.4	1.16 (−6.94 to 9.26)	0.78
Very happy or pretty happy (%)	74.9	1.18 (−5.85 to 8.21)	0.74

* Plus–minus values are weighted means ±SD. Where means are shown without standard deviations, they are weighted means. The effect of Medicaid coverage was estimated with the use of two-stage least-squares instrumental-variable regression. All regressions included indicators for the number of household members on the lottery list, and all standard errors were clustered on household. All analyses were weighted with the use of survey weights. The sample was all 12,229 survey respondents.

† For variables measured as percentages, the change is expressed as percentage points.

‡ Scores on the Medical Outcomes Study 8-Item Short-Form Health Survey (SF-8) range from 0 to 100, with higher subscale scores indicating better self-reported health-related quality of life. The scale is normalized to yield a mean of 50 and a standard deviation of 10 in the general U.S. population.

with higher scores corresponding to better health-related quality of life. Medicaid coverage led to an increase of 1.95 points (95% CI, 0.03 to 3.88; P=0.05) in the average score on the mental component; the magnitude of improvement was approximately one fifth of the standard deviation of the mental-component score. We did not detect a significant difference in the quality of life related to physical health or in self-reported levels of pain or happiness.

FINANCIAL HARDSHIP

Table 4 shows that Medicaid coverage led to a reduction in financial strain from medical costs, according to a number of self-reported measures. In particular, catastrophic expenditures, defined as out-of-pocket medical expenses exceeding 30% of income, were nearly eliminated. These expenditures decreased by 4.48 percentage points (95% CI, −8.26 to −0.69; P=0.02), a relative reduction of more than 80%.

ADDITIONAL OUTCOMES

Table 5 shows the effects of Medicaid coverage on health care utilization, spending on health care, preventive care, access to and quality of care, smoking status, and obesity. Medicaid coverage resulted in an increase in the number of prescription drugs received and office visits made in the previous year; we did not find significant changes in visits to the emergency department or hospital admissions. We estimated that Medicaid coverage increased annual medical spending (based on measured use of prescription drugs, office visits, visits to the emergency department, and hospital admissions) by $1,172, or about 35% relative to the spending in the control group. Medicaid coverage also led to increases in some preventive care and screening services, including cholesterol screening (an increase of 14.57 percentage points; 95% CI, 7.09 to 22.04; P<0.001) and improved perceived access to care, including a usual place of care (an increase of 23.75 percentage points; 95% CI, 15.44 to 32.06; P<0.001). We found no significant effect of Medicaid coverage on the probability that a person was a smoker or obese.

DISCUSSION

This study was based on more than 12,000 in-person interviews conducted approximately 2 years after a lottery that randomly assigned access to Medicaid for low-income, able-bodied, uninsured adults — a group that comprises the majority of persons who are newly eligible for Medicaid under the 2014 expansion.[12] The results confirm that Medicaid coverage increased overall health care utilization, improved self-reported health, and reduced financial strain; these findings are consistent with previously published results based on mail surveys conducted approximately 1 year af-

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Table 4. **Mean Values and Absolute Change in Financial Hardship with Medicaid Coverage.***

Variable	Mean Value in Control Group	Change with Medicaid Coverage (95% CI)†	P Value
Any out-of-pocket spending (%)	58.8	−15.30 (−23.28 to −7.32)	<0.001
Amount of out-of-pocket spending ($)	552.8±1219.5	−215.35 (−408.75 to −21.95)	0.03
Catastrophic expenditures (%)‡	5.5	−4.48 (−8.26 to −0.69)	0.02
Any medical debt (%)	56.8	−13.28 (−21.59 to −4.96)	0.002
Borrowed money to pay bills or skipped payment (%)	24.4	−14.22 (−21.02 to −7.43)	<0.001

* Plus–minus values are weighted means ±SD. Where means are shown without standard deviations, they are weighted means. The effect of Medicaid coverage was estimated with the use of two-stage least-squares instrumental-variable regression. All regressions include indicators for the number of household members on the lottery list, and all standard errors were clustered on household. All analyses were weighted with the use of survey weights. The sample was all 12,229 survey respondents.

† For variables measured as percentages, the change is expressed as percentage points.

‡ Persons with catastrophic expenditures had out-of-pocket medical expenses that exceeded 30% of their household income.

ter the lottery.[4] With these new data, we found that increased health care utilization observed at 1 year persisted, and we present new results on the effects of Medicaid coverage on objectively measured physical health, depression, condition-specific treatments, and other outcomes of interest.

Medicaid coverage had no significant effect on the prevalence or diagnosis of hypertension or high cholesterol levels or on the use of medication for these conditions. It increased the probability of a diagnosis of diabetes and the use of medication for diabetes, but it had no significant effect on the prevalence of measured glycated hemoglobin levels of 6.5% or higher. Medicaid coverage led to a substantial reduction in the risk of a positive screening result for depression. This pattern of findings with respect to clinically measured health — an improvement in mental health but not in physical health (Table 2) — was mirrored in the self-reported health measures, with improvements concentrated in mental rather than physical health (Table 3). The improvements appear to be specific to depression and mental health measures; Medicaid coverage did not appear to lead to an increase in self-reported happiness, which is arguably a more general measure of overall subjective well-being.

Hypertension, high cholesterol levels, diabetes, and depression are only a subgroup of the set of health outcomes potentially affected by Medicaid coverage. We chose these conditions because they are important contributors to morbidity and mortality, feasible to measure, prevalent in the low-income population in our study, and plausibly modifiable by effective treatment within a 2-year time frame.[13-16] Nonetheless, our power to detect changes in health was limited by the relatively small numbers of patients with these conditions; indeed, the only condition in which we detected improvements was depression, which was by far the most prevalent of the four conditions examined. The 95% confidence intervals for many of the estimates of effects on individual physical health measures were wide enough to include changes that would be considered clinically significant — such as a 7.16-percentage-point reduction in the prevalence of hypertension. Moreover, although we did not find a significant change in glycated hemoglobin levels, the point estimate of the decrease we observed is consistent with that which would be expected on the basis of our estimated increase in the use of medication for diabetes. The clinical-trial literature indicates that the use of oral medication for diabetes reduces the glycated hemoglobin level by an average of 1 percentage point within as short a time as 6 months.[15] This estimate from the clinical literature suggests that the 5.4-percentage-point increase in the use of medication for diabetes in our cohort would decrease the average glycated hemoglobin level in the study population by 0.05 percentage points, which is well within our 95% confidence interval. Beyond issues of power, the effects of Medicaid coverage may be limited by the multiple sources of slippage in the connection between insurance coverage and observable improvements in our health metrics; these potential sources of slippage include access to care, diagnosis of underlying conditions, prescription of appropriate med-

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Table 5. Mean Values and Absolute Change in Health Care Utilization and Spending, Preventive Care, Access to and Quality of Care, and Smoking and Obesity with Medicaid Coverage.*

Variable	Mean Value in Control Group	Change with Medicaid Coverage (95% CI)†	P Value
Utilization (no. of visits or medications)			
Current prescription drugs	1.8±2.8	0.66 (0.21 to 1.11)	0.004
Office visits in past 12 mo	5.5±11.6	2.70 (0.91 to 4.49)	0.003
Outpatient surgery in past 12 mo	0.1±0.4	0.03 (−0.03 to 0.09)	0.28
Emergency department visits in past 12 mo	1.0±2.0	0.09 (−0.23 to 0.42)	0.57
Hospital admissions in past 12 mo	0.2±0.6	0.07 (−0.03 to 0.17)	0.17
Estimate of annual health care spending ($)‡	3,257.3	1,171.63 (199.35 to 2,143.91)	0.018
Preventive care in past 12 mo (%)			
Cholesterol-level screening	27.2	14.57 (7.09 to 22.04)	<0.001
Fecal occult-blood test in persons ≥50 yr	19.1	1.26 (−9.44 to 11.96)	0.82
Colonoscopy in persons ≥50 yr	10.4	4.19 (−4.25 to 12.62)	0.33
Flu shot in persons ≥50 yr	35.5	−5.74 (−19.31 to 7.83)	0.41
Papanicolaou smear in women	44.9	14.44 (2.64 to 26.24)	0.016
Mammography in women ≥50 yr	28.9	29.67 (11.96 to 47.37)	0.001
PSA test in men ≥50 yr	21.4	19.18 (1.14 to 37.21)	0.037
Perceived access to and quality of care (%)			
Had a usual place of care	46.1	23.75 (15.44 to 32.06)	<0.001
Received all needed care in past 12 mo	61.0	11.43 (3.62 to 19.24)	0.004
Care was of high quality, if received, in past 12 mo	78.4	9.85 (2.71 to 17.00)	0.007
Smoking status and obesity (%)			
Current smoker	42.8	5.58 (−2.54 to 13.70)	0.18
Obese	41.5	0.39 (−7.89 to 8.67)	0.93

* Plus–minus values are weighted means ±SD. Where means are shown without standard deviations, they are weighted means. The effect of Medicaid coverage was estimated with the use of two-stage least-squares instrumental-variable regression. All regressions include indicators for the number of household members on the lottery list, and all standard errors were clustered on household. All analyses were weighted with the use of survey weights. The sample size was all 12,229 survey respondents. For some prevention measures, the sample was limited to the 3374 survey respondents who were at least 50 years of age, the 1864 female survey respondents who were at least 50 years of age, or the 1509 male survey respondents who were at least 50 years of age. The sample for quality of care was limited to the 9694 survey respondents who received care in the previous 12 months. PSA denotes prostate-specific antigen.

† For variables measured as percentages, the change is expressed as percentage points.

‡ Annual spending was calculated by multiplying the numbers of prescription drugs, office visits, visits to the emergency department, and hospital admissions by the estimated cost of each. See the Supplementary Appendix for details.

ications, compliance with recommendations, and effectiveness of treatment in improving health.[17]

Anticipating limitations in statistical power, we prespecified analyses of subgroups in which effects might be stronger, including the near-elderly and persons who reported having received a diagnosis of diabetes, hypertension, a high cholesterol level, a heart attack, or congestive heart failure before the lottery. We did not find significant changes in any of these subgroups. To try to improve statistical power, we used the Framingham risk score as a summary measure. This allowed us to reject a decrease of more than 20% in the predicted 10-year cardiovascular risk or a decrease of more than 10% in predicted risk among the participants with high-risk diagnoses before the lottery. Our results were thus consistent with at best limited improvements in these particular dimensions of physical health over this time period, in contrast with the substantial improvement in mental health.

Although changes in health status are of great

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interest, they are not the only important potential benefit of expanded health insurance coverage. Health insurance is a financial product that is aimed at providing financial security by protecting people from catastrophic health care expenses if they become injured or sick (and ensuring that the providers who see them are paid). In our study, Medicaid coverage almost completely eliminated catastrophic out-of-pocket medical expenditures.

Our estimates of the effect of Medicaid coverage on health, health care utilization, and financial strain apply to able-bodied, uninsured adults with incomes below 100% of the federal poverty level who express interest in insurance coverage — a population of considerable interest for health care policy, given the planned expansion of Medicaid. The Patient Protection and Affordable Care Act of 2010 allows states to extend Medicaid eligibility to all adults with incomes of up to 138% of the federal poverty level. However, there are several important limits to the generalizability of our findings. First, the low-income uninsured population in Oregon differs from the overall population in the United States in some respects, such as the proportions of persons who are members of racial and ethnic minority groups. Second, our estimates speak to the effect of Medicaid coverage on the subgroup of people who signed up for the lottery and for whom winning the lottery affected their coverage status; in the Supplementary Appendix we provide some additional details on the characteristics of this group. Medicaid coverage may have different effects for persons who seek insurance through the lottery than for the general population affected by coverage mandates. For example, persons who signed up for the lottery may have expected a greater health benefit from insurance coverage than those who did not sign up. Of course, most estimates suggest imperfect (and selective) Medicaid take-up rates even under mandates.[18] Third, the newly insured participants in our study constituted a small share of all uninsured Oregon residents, limiting the system-level effects that insuring them might generate, such as strains on provider capacity or investment in infrastructure. Fourth, we examined outcomes in people who gained an average of 17 months of coverage (those insured through the lottery were not necessarily covered for the entire study period); the effects of insurance in the longer run may differ.

Despite these limitations, our study provides evidence of the effects of expanding Medicaid to low-income adults on the basis of a randomized design, which is rarely available in the evaluation of social insurance programs. We found that insurance led to increased access to and utilization of health care, substantial improvements in mental health, and reductions in financial strain, but we did not observe reductions in measured blood-pressure, cholesterol, or glycated hemoglobin levels.

The findings and conclusions expressed in this article are solely those of the authors and do not necessarily represent the views of the funders.

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By Naomi Zewde and Christopher Wimer

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Antipoverty Impact Of Medicaid Growing With State Expansions Over Time

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ABSTRACT Out-of-pocket spending on health care pushed over 10.5 million Americans into poverty in 2016. Medicaid helps offset this risk by providing medical coverage to millions of poor and near-poor children and adults and thereby constraining out-of-pocket medical spending. This article examines whether recent state-level expansions to the Medicaid program resulted in reductions in poverty and whether future changes to the program are likely to have similar impacts on poverty. Using a difference-in-differences research design, we found that the recent Medicaid expansion caused a significant reduction in the poverty rate. Moreover, by simulating a counterfactual poverty rate for a hypothetical world without Medicaid coverage, we found that the program's antipoverty impact grew over the past decade independent of expansion, by shielding beneficiaries from growing out-of-pocket spending. Future expansions or retractions of Medicaid are likely to produce associated effects on poverty.

Medicaid continues to be one of the largest components of the safety net for low-income households in the United States. Even before its recent expansion under the Affordable Care Act (ACA), Medicaid funded approximately half of all US births.[1] Since the expansion, the program has covered the medical expenses of millions more poor and near-poor adults than it did previously, helping prevent households from becoming poor because of medical spending. Moreover, the US federal government devotes nearly 10 percent of its annual budget to its majority share of the program's more than \$5.5 billion total cost.[2–4] While Medicaid's reach has increased over the past decade, its future is uncertain. Some states continue to opt in to the Medicaid expansions authorized under the ACA, but others are experimenting with work requirements, and federal policy makers continue to debate major rollbacks.[5,6] To better prepare for the effects of potential changes to the scope of the Medicaid program, state and local policy makers need information about trends in the program's antipoverty impact as a guide for extrapolating to the future.

Medicaid primarily serves to deliver health care, rather than to provide direct material resources. Thus, much of the research on the program centers on its effects on health services and health outcomes, with much less research focusing on its effects on financial outcomes such as poverty. However, many Americans struggle to pay for medical expenses. In 2016 out-of-pocket spending on health care pushed over 10.5 million into poverty.[7] Moreover, a growing body of evidence indicates that Medicaid expansion has an important protective effect on family resources, especially in the context of reducing the incidence of extreme medical spending outcomes—including reductions in the likelihood of incurring medical debt,[8] worrying about medical bills,[9] or resorting to the use of

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risky financial instruments such as payday loans.[10]

Because of the vulnerability of the beneficiary population, a handful of studies over the years have examined the effect of Medicaid on poverty. While these studies suggested that Medicaid affects poverty, they did not examine institutional or demographic trends to extrapolate estimates of the magnitude of future changes.

One study found that in 2010 Medicaid reduced the poverty rate by 0.7 percentage points, relative to a counterfactual world without the program.[11] However, the results might not hold for the expansion population, which differs in important ways from the populations served by Medicaid before the expansion. The study authors found the greatest poverty reduction among these populations—disabled adults, children, and the elderly—whose eligibility was not affected by the ACA expansion. The smallest poverty reduction was found among nondisabled adults, who make up the primary expansion population. Moreover, evidence suggests that the newly eligible are healthier and use less medical care, on average, than their previously eligible counterparts do.[12,13] This suggests that the newly eligible would have a lighter burden of medical spending in the absence of Medicaid coverage, which could preclude any antipoverty impacts at the population level.

A more recent study from the post-ACA era developed a new poverty measure that depends on the availability of insurance regulations implemented in the ACA and thus does not lend itself to historical trend analyses or comparisons with the pre-ACA era. In that study Dahlia Remler and colleagues used publicly available data on the price of a standard insurance policy that provides a comprehensive set of basic benefits to assess whether families had adequate resources to meet this "health-inclusive" poverty threshold.[14(p1828)]

However, the prices and availability of coverage varied widely in the pre-ACA era, which makes it difficult to impose a basic, standard plan. Prices varied with each enrollee's individual characteristics and prior illnesses, based on unique proprietary formulas for rate setting. Moreover, because insurers could deny coverage to applicants with existing illnesses, basic coverage was altogether unavailable for some, and its price cannot be validly estimated.[15]

As policy makers and voters in several states consider the value of funding Medicaid expansions within the context of fixed state budgets, we examine the poverty implications of states' expansion decisions. Did the ACA's Medicaid expansion reduce the nation's burden of poverty, relative to the pre-ACA era? Would future expansions or retractions be likely to have effects of similar or even greater magnitude on poverty? This article addresses both of these questions. First, we harness data on differences in states' timing of ACA expansion implementation to understand the magnitude of the expansion's antipoverty effect. Second, we examine trends in the program's antipoverty effects over time to extrapolate the effects of future changes, to determine whether future expansions or retractions are likely to have similar, smaller, or larger effects than those most recently observed.

Study Data And Methods

DATA We addressed our research questions with data for 2010–16 from the Current Population Survey's Annual Social and Economic Supplement. The data set is ideal for our study primarily because it is the source of data for our outcome of interest, the Supplemental Poverty Measure (SPM). The Census Bureau developed the SPM as a measure of poverty that would improve upon the official poverty measure in four key ways. First, SPM poverty thresholds are based on contemporary spending on a core set of goods, food, clothing, shelter, and utilities (plus a multiplier), rather than on the cost of food in the 1950s and 1960s, as in the official measure.[7] Second, the SPM poverty thresholds (or needs) take into account geographic differences in the level of needs based on metropolitan-level differences in housing costs. Third, the SPM treats cohabiting couples as equivalent to married couples when considering who shares resources within a household. Finally, the SPM considers a broader definition of resources that adds the value of tax credits and in-kind benefits to cash income and deducts out-of-pocket expenditures on medical care, work expenses, and child care expenses. Importantly for this study, by subtracting actual medical out-of-pocket expenses from resources when calculating the poverty rate, the SPM provides a framework for assessing the contribution of policies such as Medicaid to reducing the poverty rate via reductions in out-of-pocket medical spending.

Additionally, we made use of the large sample size in the Current Population Survey to conduct the state-level analyses necessary to isolate the poverty effects of the ACA's Medicaid expansion and to assess how expansions in coverage affect individuals' medical spending. Our study sample reflected the full noninstitutionalized US population, with more than 1.3 million respondent observations over the study period.[16] We analyzed individual- and household-level demographic characteristics including income, race, sex, age, and self-reported health and disability

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status, and we adjusted amounts for inflation to 2015 dollars using the Consumer Price Index for All Urban Consumers.

EVALUATING THE ANTIPOVERTY EFFECT OF MEDICAID EXPANSION We first evaluated the antipoverty effect of the ACA Medicaid expansion using a standard difference-in-differences design, comparing poverty in expansion states after they expanded eligibility relative to poverty in a comparison group that consisted of expansion states before they expanded eligibility and nonexpansion states. While several states expanded Medicaid in 2014 (the year of the implementation of the ACA's expansion), five states expanded earlier, and four states expanded later.[17] We used this variation in the timing of expansion to control for potentially confounding economic and demographic trends with state and year fixed effects and state-specific linear time trends.

Our outcome of interest was whether or not people in the sample were in poverty, defined as living in a family (that is, an SPM poverty unit) whose resources did not meet its needs according to the SPM. We implemented a logistic regression model in which the binary dependent variable indicated SPM poverty status. Additionally, the model controlled for individual-level demographic characteristics with a series of binary indicators for race or ethnicity (black, Hispanic, Asian, or other), full-time employment status, citizenship, age, sex, and category of potential Medicaid eligibility (children, disabled nonelderly adults, parents, other nonelderly adults, and the elderly). Online appendix A provides more details on additional specifications.[18] Finally, we engaged in a falsification test of our results by evaluating the effect of expansion on the poverty rate of nonelderly adults separately from the rate among the elderly, who were not affected by the expansion. In the latter group we created a subset of elderly people in households having no members younger than age sixty-five, to preclude the estimation of any poverty effects driven by nonelderly household members who might be eligible for the expansion.

ANALYZING TIME TRENDS IN MEDICAID'S ANTIPOVERTY EFFECT To assess how the antipoverty effects of Medicaid coverage have evolved over the past seven years, we first compared the actual poverty rate with the rate we would have observed in a counterfactual world without Medicaid coverage. We simulated the counterfactual medical spending of Medicaid beneficiaries, which replaced their reported medical spending in our calculation of remaining household resources available to meet the SPM's threshold of needs. Our simulation model was based on the technique developed by Benjamin Sommers and Donald Oellerich,[11] adapted to apply to the 2010–16 data from the Current Population Survey.

> **If out-of-pocket spending continues to grow, so too will the economic consequences of cutting Medicaid.**

We imputed counterfactual medical out-of-pocket spending for each Medicaid beneficiary from the spending of a similar person who was not a Medicaid beneficiary. We conducted random assignments of the spending within mutually exclusive imputation groups defined by year; category of Medicaid eligibility; and deciles of people's estimated propensity for having Medicaid coverage, which we estimated separately by survey year as a function of demographic characteristics and health status. (See appendix B for information on the comparability of imputation groups and a fuller description of the simulation technique.)[18]

Next, we mechanically replaced reported medical out-of-pocket spending with the imputed value of the spending to assess whether the household would be in poverty in the counterfactual policy environment. As mentioned above, the SPM determines a family's poverty status by first deducting medical expenses from family resources to assess whether the remainder could meet basic living needs of food, clothing, shelter, and utilities.

To make the same calculation for the counterfactual environment, we first added back the reported medical out-of-pocket spending to family resources and then deducted the imputed value of the spending. Then we assessed whether the family's counterfactual remaining resources could meet the family's original SPM threshold for basic living needs. To obtain 95% confidence intervals about these estimates, we conducted 100 bootstrapped repetitions of the full simulation.

Additionally, we tested the sensitivity of our poverty reduction results to characteristics of the potential match recipients by stratifying the matching model on imputed eligibility for Medicaid, fair or poor health status, and above- or below-median rate of community health pro-

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viders per capita. Our results were robust to each variation.

Finally, we assessed the degree to which Medicaid coverage reduced the financial burden of health care for beneficiaries. We evaluated the share of the population that incurred burdensome levels of medical spending under the status quo circumstance with Medicaid coverage, relative to the counterfactual world without Medicaid. We defined *burdensome medical spending* with the thresholds established in the literature of 10 percent and 20 percent of household resources.[19]

LIMITATIONS Our study had several limitations. First, we examined only direct out-of-pocket spending on medical care and did not estimate potential illnesses or earnings losses that beneficiaries might have incurred without access to health care coverage. Nevertheless, out-of-pocket spending is an important indicator of the financial burden of medical care and should be among the primary considerations for health care provision to low-income households.

Second, our study was confined to Medicaid beneficiaries and did not reflect the full scope of publicly provided health care through Medicare, private insurance subsidies, or other public health interventions. Still, understanding the financial effects of Medicaid is an important contribution to the literature on the impacts of health coverage on Americans' well-being.

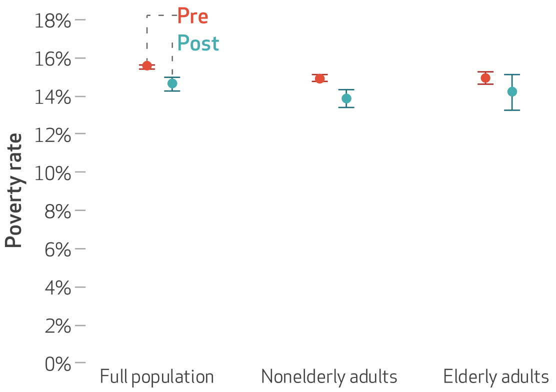
Finally, some Medicaid recipients would have remained above or below the poverty threshold despite becoming better off as a result of their coverage. To address this limitation, we supplemented our primary analysis with an evaluation of the likelihood of incurring financially burdensome medical expenses.

Study Results

THE ANTIPOVERTY EFFECT OF MEDICAID EXPANSION To evaluate the antipoverty effect of the ACA's Medicaid expansion, we used a difference-in-differences design, which relied on variation in the timing of ACA expansion across states. We compared expansion states after they expanded with expansion states before they expanded eligibility and with nonexpansion states. The results show that the expansion of Medicaid reduced the rate of poverty among expansion-state residents by 0.917 percentage points (exhibit 1). In other words, the expansion alone pulled 690,000 Americans out of poverty.[20]

EXHIBIT 1

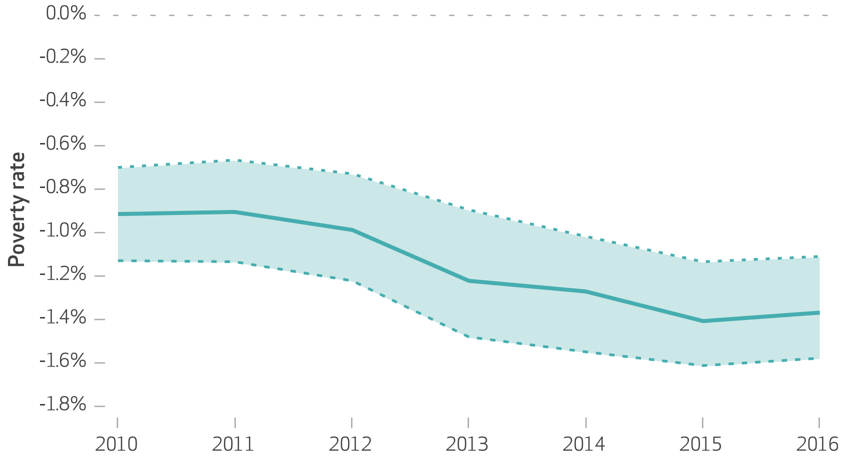
Poverty rates in states that expanded eligibility for Medicaid under the Affordable Care Act and those that did not, 2010–16



SOURCE Authors' analysis of data for 2010–16 from the Current Population Survey. **NOTES** "Poverty rates" use the Supplemental Poverty Measure's poverty thresholds, adjusted for concurrent economic and demographic trends. "Pre" refers to states that had not expanded eligibility as of January 31, 2016. "Post" refers to states that expanded eligibility no later than that date. Estimates are available in appendix exhibits A1–A3 (see note 18 in text). The whiskers indicate 95% confidence intervals. Poverty reduction was significant ($p < 0.001$) for the full population and among nonelderly adults.

EXHIBIT 2

Impact of Medicaid on poverty rates, 2010–16



SOURCE Authors' analysis of data for 2010–16 from the Current Population Survey. **NOTES** The figure shows the difference in poverty rates (explained in the notes to exhibit 1) between the real world and a simulated counterfactual world without Medicaid coverage. For example, in 2010 Medicaid reduced the poverty rate by 0.9 percentage points. The dotted lines indicate 95% confidence intervals.

Poverty reduction was concentrated among the subset of nonelderly adults who were targeted by the expansion (exhibit 1). As expected, we found no significant effect among elderly adults, whose eligibility status was not affected by the ACA—which provides empirical support for our analytical approach. We did find a point estimate that suggested some reduction in the rate of poverty among the elderly. Though not significant, the result could indicate an extension of the "welcome mat" phenomenon[21] to

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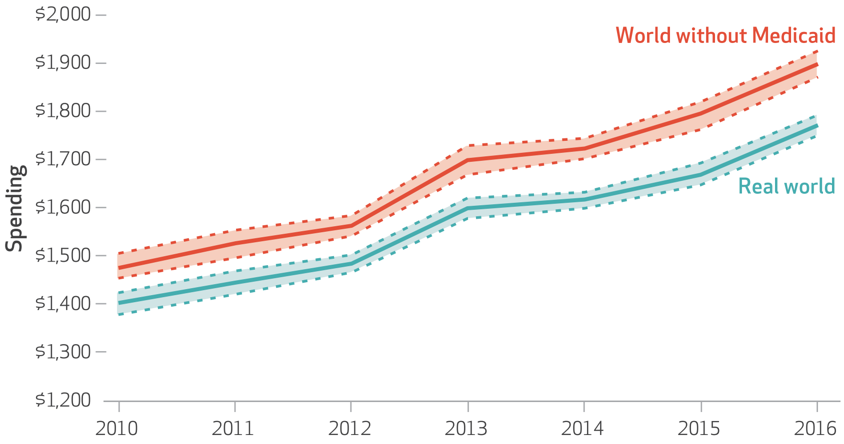
EXHIBIT 3

Impact of Medicaid on per capita medical out-of-pocket spending, 2010–16



SOURCE Authors' analysis of data for 2010–16 from the Current Population Survey. **NOTES** The figure shows the difference in medical out-of-pocket spending between the real world and a simulated counterfactual world without Medicaid coverage. The dotted lines indicate 95% confidence intervals.

the population ages sixty-five and older. Researchers have observed that the expansion of Medicaid eligibility under the ACA increased rates of participation even among people who were previously eligible, in part because of the additional funding provided under the ACA to streamline enrollment applications and administrative requirements. While prior research has documented the existence of the welcome mat phenomenon among previously eligible nonelderly adults, our results suggest that it might also affect previously eligible people ages sixty-five and older.

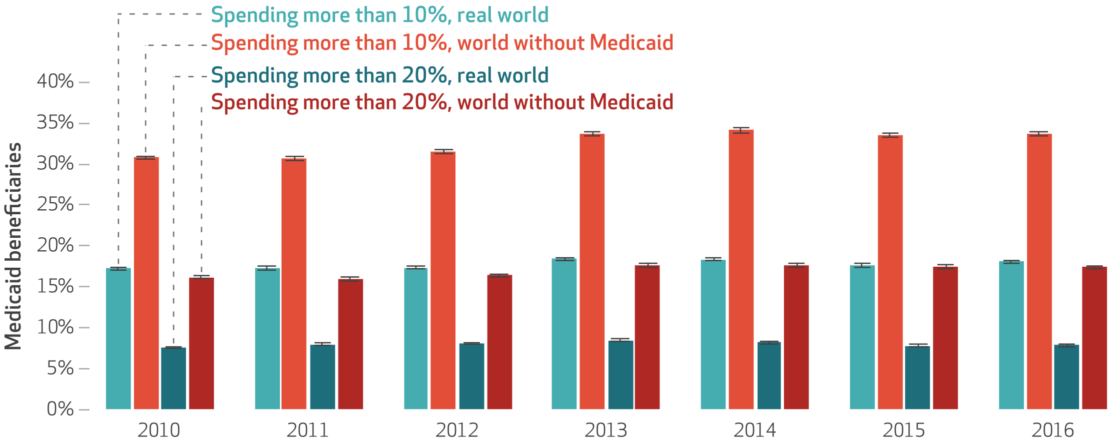
TIME TRENDS If the antipoverty effect of Medicaid is growing over time, that would imply that future expansions or retractions of Medicaid could produce poverty effects that are larger in magnitude than those observed under the ACA. We found consistent evidence that this is likely to be the case. In 2010 Medicaid reduced the poverty rate by 0.9 percentage points, relative to the counterfactual poverty rate in the absence of any Medicaid program (exhibit 2), which is corroborated by results in prior literature.[11] In 2015 and 2016 the antipoverty effect increased by more than 50 percent, to approximately 1.4 percentage points. We found that a portion of this growing impact was due to the ACA's state-level Medicaid expansions, under which more Americans were covered by Medicaid. Additionally, as shown in exhibit 3, Medicaid is increasingly buffering recipients against escalating medical out-of-pocket spending.

In other words, medical spending increased among beneficiaries over the study period. However, in the absence of Medicaid, spending would have grown faster, as shown by the counterfactual trajectory of medical out-of-pocket spending in exhibit 3. The combination of a greater number of recipients and this larger buffering effect on that spending resulted in an increased overall antipoverty impact between 2010 and 2016.

MEDICAL BURDEN Within the context of beneficiaries' limited household budgets, Medicaid coverage halved the likelihood of incurring a burdensome medical expense. In the period 2010–16 nearly one in five Medicaid beneficiaries (17.2–18.4 percent) spent more than 10 per-

EXHIBIT 4

Impact of Medicaid on the financial burden of medical care, 2010–16



SOURCE Authors' analysis of data for 2010–16 from the Current Population Survey. **NOTES** The figure shows the differences in the shares of Medicaid beneficiaries spending more than 10 percent or 20 percent of household resources on medical care between the real world and a simulated counterfactual world without Medicaid coverage. The whiskers indicate 95% confidence intervals.

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cent of household resources on medical care (exhibit 4). Without Medicaid approximately one in three (30.7–34.1 percent) would have exceeded that threshold. Moreover, Medicaid also reduced the share of beneficiaries who would have met a more extreme definition of *health care financial burden*: The share of the population devoting more than 20 percent of household resources to health care fell to under one in ten (7.6–8.5 percent) from approximately one in six (16.0–17.7 percent) without Medicaid.

Discussion

The expansion of Medicaid under the ACA significantly reduced the burden of poverty in the United States. After controlling for broader economic and demographic trends, we found that the expansion reduced the rate of poverty by just under 1 percentage point and was concentrated among the nonelderly adults whom the policy was intended to affect.

Our empirical estimates fell within the expected range relative to the existing literature on the association between Medicaid coverage and SPM poverty rates. The Census Bureau reports that between 2011 and 2016 out-of-pocket medical expenditures raised the SPM poverty rate by 3.3–3.6 percentage points for the population overall.[7] Our estimates imply that entirely eliminating Medicaid would have increased the contribution of medical out-of-pocket spending to the SPM poverty rate by an additional 0.9–1.4 percentage points over that time period (exhibit 2), which is relatively large but reasonable on its face considering the vulnerability of the Medicaid population to both poverty and medical expenses.[22] Our estimate for 2010 replicates the results of Sommers and Oellerich,[11] and our confidence intervals overlap with those authors' confidence interval for that year. Finally, relative to the effect of Medicaid on the "health-inclusive" poverty measure developed by Remler and colleagues,[14] our estimate of a 1.3-percentage-point reduction in SPM poverty rates attributable to Medicaid in 2014 is comparable to the authors' estimate of a 4.6-percentage-point reduction attributable to Medicaid, Medicare, and ACA premium subsidies combined for that year.

Conclusion

By evaluating trends in the antipoverty effects of Medicaid coverage, we found that the magnitude of the effect increased alongside the growing importance of the program in the years since expansion. If out-of-pocket spending continues to grow—through increases in prices of medical care or premiums, deductibles, and out-of-pocket maximums in private insurance—so too will the economic consequences of cutting Medicaid. Thus, in the face of rising medical spending, future expansions or retractions of public health care coverage are likely to produce corresponding effects on poverty. ■

A previous version of this article was presented at the AcademyHealth Annual Research Meeting, Seattle, Washington, June 24, 2018.

NOTES

1 Markus AR, Andres E, West KD, Garro N, Pellegrini C. Medicaid covered births, 2008 through 2010, in the context of the implementation of health reform. Women's Health Issues. 2013;23(5):e273–80.

2 Henry J. Kaiser Family Foundation. Federal and state share of Medicaid spending [Internet]. San Francisco (CA): KFF; c 2018 [cited 2018 Nov 14]. Available from: https://www.kff.org/medicaid/state-indicator/federalstate-share-of-spending/

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12 Hill SC, Abdus S, Hudson JL, Selden TM. Adults in the income range for the Affordable Care Act's Medicaid expansion are healthier than pre-ACA enrollees. Health Aff (Millwood). 2014;33(4):691–9.

13 Jacobs PD, Kenney GM, Selden TM. Newly eligible enrollees in Medicaid spend less and use less care than those previously eligible. Health Aff (Millwood). 2017;36(9):1637–42.

14 Remler DK, Korenman SD, Hyson RT. Estimating the effects of health insurance and other social programs on poverty under the Affordable Care Act. Health Aff (Millwood). 2017; 36(10):1828–37.

15 According to Remler and colleagues (note 14), "Two conditions make it valid to include health needs and benefits in poverty measurement.… First, health insurance is considered a basic need, like food and shelter.… Second, guaranteed issue and community rating regulations are in effect (as they are under the Affordable Care Act [ACA]), which means that anyone can purchase insurance, and its price does not depend on health status. This makes it possible to determine the price of health insurance for each household, regardless of the household's specific health care needs." The second condition does not apply in the pre-ACA era.

16 There were 1,378,863 respondent observations after deletions for missing data.

17 Throughout the study, expansion states are those that implemented the expansion no later January 31, 2016.

18 To access the appendix, click on the Details tab of the article online.

19 Banthin JS, Cunningham P, Bernard DM. Financial burden of health care, 2001–2004. Health Aff (Millwood). 2008;27(1):188–95.

20 Calculated based on a 0.91 percent marginal reduction in the poverty rate among the treated population, which included 24.09 percent of the weighted population count of 313,617,769. Appendix A provides a detailed listing of results (see note 18).

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Medicaid Expansion Increased Coverage, Improved Affordability, And Reduced Psychological Distress For Low-Income Parents

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ABSTRACT Despite receiving less attention than their childless counterparts, low-income parents also experienced significant expansions of Medicaid eligibility under the Affordable Care Act (ACA). We used data for the period 2010–15 from the National Health Interview Survey to examine the impacts of the ACA's Medicaid expansion on coverage, access and use, affordability, and health status for low-income parents. We found that eligibility expansions increased coverage, reduced problems paying medical bills, and reduced severe psychological distress. We found only limited evidence of increased use of care among parents in states with the smallest expansions, and no significant effects of the expansions on general health status or problems affording prescription drugs or mental health care. Together, our results suggest that the improvements in mental health status may be driven by reduced stress associated with improved financial security from insurance coverage. We also found large missed opportunities for low-income parents in states that did not expand Medicaid: If these states had expanded Medicaid, uninsurance rates for low-income parents would have fallen by an additional 28 percent.

When the Affordable Care Act (ACA) was passed in 2010, it included a Medicaid expansion that aimed to reduce uninsurance among adults with incomes at or below 138 percent of the federal poverty level. However, the June 2012 Supreme Court ruling in *National Federation of Independent Business v. Sebelius* made that expansion optional. As of April 2017, thirty-two states (including the District of Columbia) had chosen to expand eligibility for Medicaid under the ACA. Much attention has been focused on potential coverage gains for childless adults,[1] but Medicaid eligibility for parents was also limited in many states before passage of the ACA. Several states, including Alabama, Arkansas, Louisiana, Missouri, and Texas, had income eligibility thresholds below 30 percent of poverty. As a result of this limited Medicaid eligibility in some states as well as other factors, over ten million parents were uninsured in 2010.[2] Studies have found significant effects of previous Medicaid expansions on parents' coverage, access to care, use of services, and mental health status,[3–5] and those findings suggest that uninsured parents are likely to benefit from the ACA Medicaid expansions as well. Furthermore, children may experience positive spillover effects from their parents' gains in coverage and improved access to care, health, and financial well-being.[6,7]

Parents with incomes at or below 138 percent of poverty, but above their state's pre-ACA Medicaid eligibility threshold, became newly eligible for Medicaid coverage in states that opted to participate in the ACA Medicaid expansion. Cov-

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erage options remained limited for low-income parents in nonexpansion states, where Medicaid income eligibility thresholds were often well below the poverty level (with an average threshold of 52 percent of poverty). However, in nonexpansion states, parents with incomes of 100–138 percent of poverty were eligible for federal subsidies to purchase coverage in the health insurance Marketplaces, and in both expansion and nonexpansion states parents who were already eligible for Medicaid may have had an increased probability of enrollment after 2014 as a result of publicity, outreach, and enrollment efforts associated with the ACA expansions.

Strong and consistent evidence has emerged that the ACA Medicaid expansions increased Medicaid coverage and reduced uninsurance rates for low-income adults.[8–10] Several studies have also found increases in access to care and service use and reductions in out-of-pocket spending.[9,11,12] Descriptive evidence has shown coverage and access improvements for all parents under the ACA.[13] However, findings on the impacts of the Medicaid expansion for parents have been mixed. One study found no significant increases in insurance coverage or access to care for low-income parents through 2015,[14] while another study found that the Medicaid expansion reduced uninsurance rates for parents with a high school education or less in 2014.[15] Neither of these studies accounted for the variation in the magnitude of the expansion for parents across states that resulted from states' very different Medicaid eligibility thresholds before implementation of the ACA Medicaid expansion.

In this article we describe changes in insurance coverage, access to care, service use, affordability of care, and health status for low-income parents through 2015. We attempt to isolate the impacts of the ACA Medicaid expansion using variations in the Medicaid eligibility threshold for parents within states over time. We also describe the missed opportunities for parents in states that did not expand Medicaid under the ACA.

Study Data And Methods

DATA AND SAMPLE We used public use data for the period 2010–15 from the Integrated Health Interview Series of the National Health Interview Survey (NHIS).[16] These data provide harmonized versions of NHIS variables across data years. We also obtained access to state and county identifiers through the Research Data Center of the National Center for Health Statistics.

We defined *parents* as US citizen adults ages 19–64 with a biological, step-, or adopted child ages 0–18 years in their health insurance unit—that is, a group of family members who would be considered a family in determining eligibility for Marketplace subsidies, Medicaid, or family/dependent coverage through private insurance (this is a more narrow definition of *family* than that used for other purposes). We also constructed a measure of income relative to poverty for the health insurance unit using NHIS earnings and income information, which includes imputed information for approximately 25 percent of the sample, and poverty guidelines from the Department of Health and Human Services.

Our sample included parents whose health insurance unit had an income at or below 138 percent of poverty. We excluded noncitizens because legal residents face additional restrictions on Medicaid eligibility, and undocumented immigrants are not eligible for Medicaid. We also excluded people who were pregnant or covered by Medicare at the time of the survey and those who had received Supplemental Security Income benefits in the previous calendar year, because those groups are subject to different Medicaid eligibility criteria.

We constructed a health insurance hierarchy that had seven mutually exclusive categories: Medicare, Marketplace insurance, insurance sponsored by the employer (including the military), other private insurance, Medicaid or the Children's Health Insurance Program (CHIP), other public insurance, and no insurance. We report data for four categories: employer-sponsored insurance; Medicaid or CHIP; Marketplace or other private or other public insurance; and no insurance. These categories are based on self-reported information and reflect coverage at the time of survey. We also examined whether parents reported that their health insurance was better, worse, or the same, compared to the previous year.

We constructed several measures of access and use: the percentages of parents who had a usual source of care (other than the emergency department [ED]) and who in the past twelve months had seen a general doctor or any provider (a general doctor, specialist, obstetrician/gynecologist, midlevel provider, or mental health provider). We also identified parents who in the past year had had multiple ED visits or trouble finding a provider who would see them, and those who delayed care for noncost reasons (issues with transportation, wait times for appointments or in the provider's office, inconvenient office hours, or trouble getting through on the phone).

We measured affordability of care by identifying parents who reported being very worried, moderately worried, or not worried at all about

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paying either for the medical costs of a serious illness or injury or for costs of routine health care. We also measured the percentages of parents who reported in the past year having had trouble paying medical bills for themselves or their family members; not receiving needed medical care, prescription drugs, or mental health care because of cost; and having delayed care because of cost.

Finally, we measured self-reported general health status (excellent or very good, good, or fair or poor) at the time of the survey and mental health status in the previous thirty days using the Kessler K6 Psychological Distress Scale.[17] We classified respondents into three categories of psychological distress: none or mild (with a score on the scale of 0–7), moderate (8–12), or severe (13 or more). We also examined whether parents reported that their health was better, worse, or the same, compared to the previous year. Additional details on sample sizes and variable construction are available in the online Appendix.[18]

METHODS We chose the outcomes described above based on the Andersen model of access to care[19] and previous work on Medicaid expansions.[20–22] Expanding Medicaid eligibility is expected to increase health insurance coverage, which has the potential to strengthen patients' access to affordable services, enhance their financial security, and ultimately improve their general and mental health status. Medicaid expansions can also crowd out employer-sponsored coverage, however, and improvements in access, affordability, and health outcomes depend on enrollees' ability to navigate the health care system and that system's capacity to meet increased demand for care.

While we generally hypothesized that there would be increases in coverage and improvements in access, affordability, and health status under the Medicaid expansion, the expected effects on ED use, trouble finding a provider, and delayed care for noncost reasons were less clear. Medicaid expansion could reduce ED use if new enrollees gained access to office-based providers, but it could also increase ED use if the cost of that use declined for the newly insured. Similarly, new enrollees might have less trouble finding a provider after gaining coverage, but if capacity is an issue, they might instead have more trouble finding a provider.

We first estimated changes in insurance coverage, access and use, affordability, and health status for low-income parents before and after the 2014 Medicaid expansions, separately for expansion and nonexpansion states. We classified as expansion states those twenty-six states (including the District of Columbia) that had expanded Medicaid by April 2014. We excluded Indiana, New Hampshire, and Pennsylvania—which expanded Medicaid in late 2014 or early 2015—from our main analyses so that we could focus on the effects of the 2014 expansions over two years.

Children may experience positive spillover effects from their parents' gains in coverage.

We also estimated the simple (unadjusted) difference-in-differences between expansion and nonexpansion states over time to begin to isolate the effects of the Medicaid expansion from other changes occurring in the study period. However, these estimates did not account for the variation in the magnitude of the expansions across states for parents or for other differences between the populations in expansion and nonexpansion states.

To address these issues, we used a multivariate difference-in-differences approach with a continuous policy variable that reflected the Medicaid eligibility threshold for parents in a given state and year. Specifically, we estimated a model with state and year fixed effects to exploit the variation in the Medicaid eligibility threshold within states over time. To increase the precision of our estimates, we pooled NHIS data in two-year intervals (2010–11, 2012–13, and 2014–15) and assigned individuals their state Medicaid eligibility threshold for the earlier year in each pair. This approach allowed us to capture an average effect of the 2014 Medicaid expansions in 2014 and 2015.

We compiled information on state Medicaid eligibility rules for working parents in 2010, 2012, and 2014 from the Henry J. Kaiser Family Foundation (complete citations are in the Appendix).[18] On average, the Medicaid eligibility threshold for parents in expansion states increased from 112 percent of poverty in 2012 to 146 percent in 2014, but this average obscures substantial variation across states (Appendix Table 2).[18] The largest eligibility expansions occurred in Arkansas, West Virginia, and Oregon—which had increases in the eligibility threshold of 122, 107, and 99 percentage points, respectively. Importantly, the expansion states had much

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higher eligibility thresholds in 2012 than the nonexpansion states did (112 percent versus 60 percent), so the potential gains for nonexpansion states were considerably larger, on average, than the actual gains in the participating states. In addition, six expansion states and two nonexpansion states had expanded eligibility to parents beyond the ACA threshold of 138 percent of poverty before 2014. We top-coded the eligibility threshold at 138 percent in all analyses because changes at higher income thresholds were unlikely to affect our sample of low-income parents. Additional details on the eligibility rules, including our use of working rather than jobless parent thresholds and the implications for our analysis, are available in the Appendix.[18]

For ease of interpretation, we estimated linear probability models on binary measures of coverage, access and use, affordability, and health status, and we included parent-level controls for age, sex, race/ethnicity, education, work status, income as a percentage of poverty, marital status, number of children, and presence of an activity limitation. To further account for changing economic conditions, we also controlled for the county employment rate. We clustered standard errors at the state level and adjusted them to account for the multiple imputations of income. Additional details and means for all covariates are available in Appendix Table 3.[18] Our key variable of interest was the state Medicaid income eligibility threshold for parents, measured as a percentage of poverty, and the coefficient of interest reflected the effect of a 100-percentage-point increase in the eligibility threshold on the outcome of interest.

The difference-in-differences approach relies on the assumption that preexisting trends in the outcomes of interest are similar in treatment and comparison groups. In our case, with a continuous policy variable, it required that the preexisting trends not be correlated with changes in the eligibility threshold. To test this assumption, we estimated a model that included state-specific linear trends in addition to state and year fixed effects. We were unable to estimate this model for our measures of worries about medical care costs or psychological distress because we had only one year of preexpansion data for these measures.

Given the variety of methodological approaches available to estimate the impacts of Medicaid eligibility expansions, we also tested the sensitivity of our results by using the simulated eligibility approach pioneered by Janet Currie and Jonathan Gruber.[23] We imputed individual eligibility to our sample of low-income parents based on state, year, and income. We then drew a national sample of 3,000 parents and applied the eligibility rules for each state and year to the sample to generate the simulated eligibility instrument, or the share of the national sample that would be eligible under each state's rules. We estimated the model using two-stage least squares, with the endogenous eligibility indicator as our main variable of interest. We provide additional details on the advantages and disadvantages of each approach and discuss other robustness checks in the Appendix.[18]

To investigate nonlinearities in the relationship between the eligibility threshold and our outcomes of interest, we replaced the continuous threshold with four categorical variables: income eligibility thresholds of less than 50 percent of poverty, of 50–99 percent of poverty, of 100–137 percent of poverty, and of 138 percent of poverty and above. We then estimated the effect of being in a state with an eligibility threshold of 138 percent of poverty and above compared to each of the other categories, to capture the separate effects of small, medium, and large eligibility expansions under the ACA.

Finally, we used the results of our threshold model to predict the insurance coverage status of low-income parents in nonexpansion states if the eligibility threshold in their state had increased to 138 percent of poverty. This approach assumed that people in nonexpansion states would respond to a Medicaid expansion as similar people in expansion states did.

LIMITATIONS This analysis had several limitations. First, there is measurement error in the eligibility thresholds, incomes, and types of health insurance coverage. Specifically, we could not reliably determine the appropriate threshold for an individual parent based on the NHIS data on work status, so we used the threshold for working parents in our main specification and tested the sensitivity of our results to using the nonworking threshold. In addition, we allocated income across the health insurance units that made up a family, and NHIS income measures refer to annual income in the previous calendar year (for example, income reported in the 2014 survey refers to 2013 annual income). As a result, using our income measure to approximate the Medicaid target population was subject to error. Furthermore, reports of the presence or absence of coverage are generally valid, but measurement error is more likely in reports of the type of coverage and is likely to be increasing with the changes introduced under the ACA.[24]

Second, we analyzed two measures that captured perceptions of coverage and health status compared to the previous year. With respect to coverage, we would expect any reported improvement to occur immediately after a respon-

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dent gained coverage in either 2014 or 2015. With respect to health status, the likely timing and persistence of any improvements are ambiguous. Thus, pooling 2014 and 2015 data for health insurance compared to the previous year might understate reported coverage improvements if coverage gains were concentrated in 2014. Any likely bias in pooling data on health status compared to the previous year would be less obvious, but our estimates reflect an average of reported changes in 2014 and 2015. Furthermore, all of our outcome measures were self-reported and could be subject to recall or social desirability bias.

Third, there could be unobserved factors at the individual or state level that were correlated with the magnitude of the eligibility expansions and the outcomes of interest. For example, if states with larger expansions invested more resources in outreach and education, compared to states with smaller expansions, or if parents in states with larger expansions differed from those in states with smaller expansions on characteristics not captured in the regression analysis (such as severity of health care need), the estimates of differences in outcomes by the size of the expansion would also reflect the effects of these other factors.

Fourth, relatively small sample sizes for some analyses reduced our ability to detect small changes, and the design of the NHIS makes it likely that we underestimated the full effects of the expansion at two years—given the continuous fielding of the NHIS over a given year and the need to rely on many survey questions that are based on experiences during the previous twelve months.

Finally, we designed this analysis to detect the overall effect of the eligibility expansion on the outcomes of interest, not the effects of gaining Medicaid coverage on access or affordability or on health status. Thus, our ability to detect these second-order effects was more limited than our ability to detect effects on insurance coverage.

Study Results

All results reported in the text are significant at the 5 percent level ($p < 0.05$) unless otherwise noted.

CHANGES IN EXPANSION AND NONEXPANSION STATES Based on simple comparisons over time in both expansion and nonexpansion states, we found that insurance coverage for low-income parents changed significantly after the ACA's 2014 Medicaid expansions. The uninsurance rate for parents in expansion states fell 13.0 percentage points from 2012–13 to 2014–15, and there was a nearly corresponding increase in Medicaid or CHIP coverage (Exhibit 1). The share of parents in expansion states who reported that their coverage was better than in the previous year also increased ($p < 0.10$). In nonexpansion states, the uninsurance rate fell by 10.6 percentage points, a change driven by increases in Medicaid or CHIP (4.0 percentage points, $p > 0.10$), employer-sponsored coverage (3.0 percentage points, $p < 0.10$), and other coverage (3.6 percentage points). When we compared the unadjusted changes in coverage in expansion and nonexpansion states over time, however, only the unadjusted difference-in-differences for employer-sponsored coverage was marginally significant ($p < 0.10$).

We also found significant increases in access and use among low-income parents in expansion states. The share of parents in those states who had a usual source of care and who had had a general doctor visit or any provider visit increased (Exhibit 1). There were also strong improvements in almost every affordability measure examined for parents in expansion states. Changes in health status were mixed, with a decline in the shares of parents who reported that their health was better than in the previous year and who reported severe psychological distress following the expansions.

There were no significant changes in access and use or health status in nonexpansion states, but there were strong improvements in several affordability measures. When we compared the unadjusted changes in access and use, affordability, and health status in expansion and nonexpansion states, only the unadjusted difference-in-differences on delayed care because of noncost reasons was marginally significant ($p < 0.10$). This finding suggests that there was an increase in non-cost-related delays among parents in expansion states relative to nonexpansion states.

IMPACT ESTIMATES ACCOUNTING FOR THE SIZE OF MEDICAID ELIGIBILITY EXPANSIONS FOR LOW-INCOME PARENTS To better isolate the impacts of the Medicaid expansion on low-income parents, we estimated multivariate models that accounted for the characteristics of the parents and the variation in the magnitude of the expansions to parents across states. We found that a 100-percentage-point increase in the Medicaid income eligibility threshold would result in an 11.0-percentage-point decrease in uninsurance and a 14.6-percentage-point increase in Medicaid or CHIP coverage, if all else were equal (Exhibit 2). The estimated effect on employer-sponsored coverage was a decline of 5.2 percentage points ($p < 0.10$), which suggests some evidence of crowd-out of employer-sponsored coverage. These estimates suggest that the average

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EXHIBIT 1

Coverage, access and use, affordability, and health status for low-income parents, by state Medicaid expansion status, 2012–13 and 2014–15

	Expansion states			Nonexpansion states			
	2012–13 (%)	2014–15 (%)	Change[a]	2012–13 (%)	2014–15 (%)	Change[a]	Unadjusted DD
COVERAGE							
No coverage	24.4	11.4	−13.0**	44.3	33.7	−10.6**	−2.3
Medicaid/CHIP	49.2	61.0	11.8**	28.6	32.7	4.0	7.7
Employer sponsored	19.1	18.3	−0.8	22.1	25.0	3.0*	−3.8*
Other coverage[b]	7.3	9.3	2.0	5.0	8.6	3.6**	−1.6
Coverage compared to previous year							
Better	11.9	15.9	4.0*	17.6	17.3	−0.3	4.2
Same	78.2	76.2	−2.0	74.0	73.3	−0.8	−1.2
Worse	9.9	7.9	−2.0	8.4	9.5	1.0	−3.0
ACCESS AND USE							
At least one usual source of care[c]	76.7	83.3	6.6**	66.1	70.9	4.8	1.8
In past twelve months:							
Had trouble finding a provider	5.2	5.9	0.8	6.6	5.2	−1.4	2.1
Delayed care for noncost reasons[d]	12.6	14.2	1.6	12.6	9.7	−2.9	4.6*
Had a general doctor visit	59.5	68.8	9.3**	52.4	55.0	2.6	6.7
Had any provider[e] visit	73.9	80.2	6.3**	68.6	71.4	2.8	3.5
Had more than one ED visit	16.9	16.5	−0.4	16.9	14.9	−2.1	1.7
AFFORDABILITY							
Worried about medical costs of serious illness or accident							
Very worried	41.0	31.2	−9.8**	45.7	37.7	−8.1**	−1.8
Moderately worried	35.9	39.8	3.9*	33.9	38.1	4.3**	−0.4
Not worried	23.0	29.0	5.9	20.4	24.2	3.8	2.1
Worried about costs of routine health care							
Very worried	30.6	22.3	−8.3**	37.2	26.3	−11.0**	2.7
Moderately worried	40.7	41.4	0.7	40.5	42.7	2.2	−1.5
Not worried	28.8	36.4	7.6	22.2	31.0	8.8**	−1.2
In past twelve months:							
Had problems paying family medical bills	28.6	20.2	−8.3**	36.6	32.3	−4.3	−4.1
Delayed care because of cost	13.8	9.2	−4.6**	19.8	14.6	−5.2**	0.6
Because of cost, had unmet need for:							
Medical care	12.9	7.5	−5.4**	18.0	14.0	−4.0**	−1.3
Rx drugs	15.5	9.8	−5.7**	22.0	16.2	−5.8**	0.1
Mental health care	4.1	2.5	−1.6*	4.9	4.6	−0.3	−1.2
Any of the three	23.6	15.4	−8.2**	32.1	24.4	−7.8**	−0.5
HEALTH STATUS							
Self-reported general health status							
Excellent or very good	50.0	50.6	0.7	52.8	53.8	0.9	−0.3
Good	33.2	32.9	−0.3	31.3	31.7	0.4	−0.7
Fair or poor	16.8	16.4	−0.4	15.9	14.5	−1.4	1.0
Health status compared to previous year							
Better	21.8	17.9	−3.9**	18.2	15.3	−2.9	−1.0
Same	67.7	71.8	4.1**	70.6	73.6	3.0	1.1
Worse	10.6	10.3	−0.2	11.3	11.2	−0.1	−0.1
Psychological distress[f,g]							
None or mild (0–7)	74.6	81.0	6.4**	78.3	81.9	3.6	2.8
Moderate (8–12)	13.2	11.4	−1.8	13.7	10.6	−3.1	1.3
Severe (13 or more)	12.2	7.6	−4.6**	8.0	7.5	−0.5	−4.1

SOURCE Authors' analysis of data for 2012–15 from the National Health Interview Survey. **NOTES** Low-income parents are US citizen adults ages 19–64 whose health insurance unit (defined in the text) income is no more than 138 percent of the federal poverty level and who were the biological, step-, or adoptive parent of a child ages 0–18 years in that unit. The sample excluded people who were pregnant or covered by Medicare at the time of the survey; those who had received Supplemental Security Income benefits in the previous calendar year; and those living in Indiana, New Hampshire, or Pennsylvania (states excluded from our main analyses, as explained in the text). Nonexpansion states are those that had not expanded eligibility for Medicaid by April 2014. Change and unadjusted difference in differences (DD) may not equal difference in point estimates because of rounding. CHIP is Children's Health Insurance Program. [a]Percentage points. [b]Coverage through the health insurance Marketplaces and other public and other private coverage. [c]Not including the emergency department (ED). [d]Transportation, wait times for appointment or in office, inconvenient office hours, or trouble getting through on phone. [e]General doctor, specialist, mid-level provider, mental health provider, or obstetrician/gynecologist. [f]In the previous thirty days. [g]Score on the Kessler K6 Psychological Distress Scale (see Note 17 in text). *$p < 0.10$ **$p < 0.05$

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EXHIBIT 2

Effects of expanding Medicaid on coverage, access and use, affordability, and health status for low-income parents

	Threshold model	Threshold model with state linear trends	Simulated eligibility
COVERAGE			
No coverage	−0.110***	−0.105**	−0.137***
Medicaid/CHIP	0.146***	0.112*	0.188***
Employer sponsored	−0.052*	−0.032	−0.070*
Other coverage[a]	0.016	0.026	0.019
Coverage compared to previous year			
Better	0.113***	0.156**	0.151***
Same	−0.107***	−0.148*	−0.147***
Worse	−0.007	−0.008	−0.004
ACCESS AND USE			
At least one usual source of care[b]	0.011	0.023	0.013
In past twelve months:			
Had trouble finding a provider	0.010	0.027	0.011
Delayed care for noncost reasons[c]	0.031	0.026	0.038
Had a general doctor visit	0.049	0.066	0.064
Had any provider[d] visit	−0.010	0.019	−0.011
Had more than one ED visit	0.028	0.074	0.038
AFFORDABILITY			
Worried about medical costs of serious illness or accident			
Very worried	−0.050	—[e]	−0.073
Moderately worried	−0.014	—[e]	−0.016
Not worried	0.065	—[e]	0.089
Worried about costs of routine health care			
Very worried	0.003	—[e]	0.001
Moderately worried	−0.049	—[e]	−0.069
Not worried	0.046	—[e]	0.067
In past twelve months:			
Had problems paying family medical bills	−0.099***	−0.122*	−0.136***
Delayed care because of cost	−0.028	−0.063*	−0.036
Because of cost, had unmet need for:			
Medical care	−0.031	−0.065	−0.039
Rx drugs	0.000	−0.038	−0.003
Mental health care	−0.008	−0.030	−0.012
Any of the three	−0.018	−0.064	−0.027
HEALTH STATUS			
Self-reported general health status			
Excellent or very good	−0.007	0.050	−0.014
Good	0.003	−0.012	0.011
Fair or poor	0.004	−0.038	0.004
Health status compared to previous year			
Better	−0.018	−0.032	−0.024
Same	0.021	0.071	0.032
Worse	−0.003	−0.039	−0.008
Psychological distress[f,g]			
None or mild (0–7)	0.062**	—[e]	0.084**
Moderate (8–12)	0.010	—[e]	0.026
Severe (13 or more)	−0.073*	—[e]	−0.109**

SOURCE Authors' analysis of data for 2010–15 from the National Health Interview Survey. **NOTES** Low-income parents and the sample are explained in the Notes to Exhibit 1. In both threshold models, the coefficient reflects the effect of a 100-percentage-point change in the state Medicaid eligibility threshold on the outcome of interest. For the simulated eligibility model, the coefficient reflects the effect of a change in individual eligibility on the outcome of interest. CHIP is the Children's Health Insurance Program. [a]Coverage through the health insurance Marketplaces and other public and other private coverage. [b]Not including the emergency department (ED). [c]Transportation, wait times for appointment or in office, inconvenient office hours, or trouble getting through on phone. [d]General doctor, specialist, midlevel provider, mental health provider, or obstetrician/gynecologist. [e]Not available because we had only one year of pre-expansion data for these measures. [f]In the previous thirty days. [g]Score on the Kessler K6 Psychological Distress Scale (see Note 17 in text). *$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

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We found a meaningful impact of the Medicaid expansion on mental health for low-income parents.

change in income eligibility thresholds in expansion states of 34 percentage points (Appendix Table 2)[18] decreased uninsurance rates by about 3.7 percentage points. Consistent with these coverage gains, an increase in the eligibility threshold also increased the share of parents reporting better coverage than in the previous year.

We found no significant overall improvements in access and use for parents in response to an increase in the Medicaid eligibility threshold. Increasing the threshold reduced problems in paying family medical bills, but we found no other significant effects on affordability. Finally, increasing the threshold reduced the share of low-income parents who reported severe psychological distress ($p < 0.10$) and increased the share who reported no or mild psychological distress.

When we added controls for state linear trends, we generally found similar results with reduced precision. However, we also found a marginally significant decline in delayed care because of cost. Using the simulated eligibility approach also resulted in findings that were very similar to those in our main specification.

Our investigation of nonlinearities in the relationship between the Medicaid income eligibility threshold and our outcomes revealed some interesting patterns. The estimates can be interpreted as the effect of moving from a state with one of the lower thresholds to a state with eligibility of at least 138 percent of poverty. These estimates thereby capture the separate effects of small (from 100–137 percent of poverty), medium (from 50–99 percent of poverty) and large (from less than 50 percent of poverty) eligibility expansions to 138 percent of poverty.

We found that expansions of all sizes had significant effects on rates of uninsurance and Medicaid/CHIP coverage and that the magnitude of the effects increased as the size of the expansion did (Exhibit 3). These findings support the assumption of linearity in our main specification. Similarly, the patterns for quality of insurance coverage compared to the previous year and having problems paying medical bills support the findings from our main model. On measures of access and use, however, we found that small expansions were associated with an increased probability of having a usual source of care ($p < 0.10$), and having had a general doctor visit and any provider visit, compared to the larger expansions. Small expansions were also associated with an increase in having trouble finding a provider.

We also found evidence of reductions in unmet needs and delayed care because of cost that resulted from medium-size expansions. Finally, we found similar reductions in severe psychological distress associated with large and medium-size expansions, but large expansions were associated with a shift toward no or mild psychological distress, while medium expansions were associated with a shift toward moderate distress. Altogether, this analysis suggests that our main specification generally captured the effects of the Medicaid expansion on coverage, affordability, and psychological distress but did not capture the effects of small expansions on access and use.

We explored a variety of additional subgroup analyses and robustness checks on our main specification. For example, we found that men and women experienced similar coverage changes in response to the Medicaid expansion, but women had an increase in doctor visits and a reduction in worries about costs, while reductions in psychological distress were concentrated among men (Appendix Table 4).[18] We also found results that were generally consistent, but smaller in magnitude, when we included noncitizens in the sample (Appendix Table 5).[18] And we found additional evidence of reduced affordability problems when we used the Medicaid income eligibility threshold for nonworking parents (Appendix Table 6).[18] These and other sensitivity analyses are discussed in more detail in the Appendix.[18]

As indicated above, the states that expanded Medicaid under the ACA already had much higher eligibility thresholds for parents, compared to the states that did not expand. Nonexpansion states would have experienced, on average, a 78-percentage-point increase in their Medicaid eligibility threshold for parents if they had opted to expand eligibility (Appendix Table 2).[18] If the nonexpansion states had expanded Medicaid in 2014, our model suggests that the uninsurance rate among low-income parents would have fallen to an average rate of 24.3 percent in 2014–15, compared to the actual 2014–15 uninsurance rate of 33.7 percent (Exhibit 4). We estimate that the Medicaid/CHIP coverage rate

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EXHIBIT 3

Effects of small, medium, and large Medicaid expansions on coverage, access and use, affordability, and health status for low-income parents

	Small	Medium	Large
COVERAGE			
No coverage	−0.045**	−0.071**	−0.098**
Medicaid/CHIP	0.090***	0.120***	0.125***
Employer sponsored	−0.032	−0.064***	−0.030
Other coverage[a]	−0.012	0.016	0.002
Coverage compared to previous year			
Better	0.009	0.043	0.116***
Same	0.028	−0.075***	−0.121***
Worse	−0.037***	0.033	0.004
ACCESS AND USE			
At least one usual source of care[b]	0.036*	−0.012	0.023
In past twelve months:			
Had trouble finding a provider	0.042**	−0.002	0.019
Delayed care for noncost reasons[c]	0.001	0.022	0.063*
Had a general doctor visit	0.093***	0.032	0.024
Had any provider[d] visit	0.084***	−0.010	−0.025
More than one ED visit	0.027	0.016	0.024
AFFORDABILITY			
Worried about medical costs of serious illness or accident			
Very worried	−0.003	−0.061	−0.015
Moderately worried	0.011	0.010	−0.030
Not worried	−0.008	0.051	0.045
Worried about costs of routine health care			
Very worried	−0.022	−0.037	0.052
Moderately worried	0.037	−0.021	−0.068
Not worried	−0.015	0.058	0.015
In past twelve months:			
Had problems paying family medical bills	−0.012	−0.075***	−0.092***
Delayed care because of cost	0.005	−0.037**	−0.013
Because of cost, had unmet need for:			
Medical care	−0.002	−0.032**	−0.021
Rx drugs	0.030	−0.022	0.010
Mental health care	−0.007	−0.012	0.006
Any of the three	0.013	−0.033	−0.007
HEALTH STATUS			
Self-reported general health status			
Excellent or very good	0.001	−0.022	0.006
Good	−0.011	0.030	−0.017
Fair or poor	0.010	−0.008	0.011
Health status compared to previous year			
Better	−0.001	−0.003	−0.013
Same	0.004	0.027	−0.012
Worse	−0.002	−0.024	0.025
Psychological distress[e,f]			
None or mild (0–7)	0.028	0.014	0.067**
Moderate (8–12)	−0.011	0.061**	−0.007
Severe (13 or more)	−0.017	−0.075***	−0.060**

SOURCE Authors' analysis of data for 2010–15 from the National Health Interview Survey. **NOTES** Low-income parents and the sample are explained in the Notes to Exhibit 1. The coefficients can be interpreted as the effect of moving from a state with one of the lower eligibility thresholds to a state with a threshold of at least 138 percent of poverty. Thus, the estimates capture the separate effects of small, medium, and large eligibility expansions (from 100–137 percent of poverty, from 50–99 percent of poverty, and from less than 50 percent of poverty, respectively, to at least 138 percent of poverty). CHIP is Children's Health Insurance Program. ED is emergency department. [a]Coverage through the health insurance Marketplaces and other public and other private coverage. [b]Not including the ED. [c]Transportation, wait times for appointment or in office, inconvenient office hours, or trouble getting through on phone. [d]General doctor, specialist, mid-level provider, mental health provider, or obstetrician/gynecologist. [e]In the previous thirty days. [f]Score on the Kessler K6 Psychological Distress Scale (see Note 17 in text). *$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

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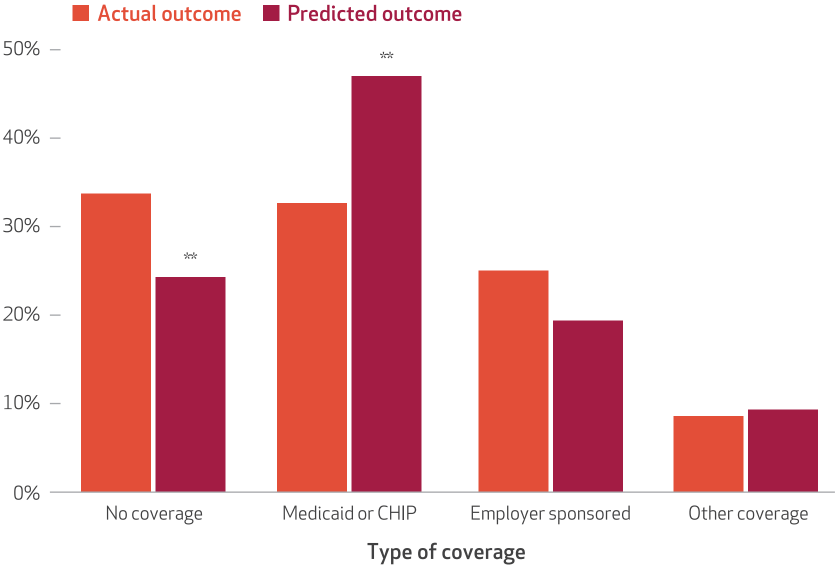
in 2014–15 would have increased to 47.0 percent in nonexpansion states, compared to the actual 2014–15 rate of 32.7 percent. This would have been offset by an estimated decline in employer-sponsored coverage that was not significant.

EXHIBIT 4

Percentages of low-income parents in nonexpansion states in 2014–15, by type of insurance coverage, both actual and predicted if states had expanded eligibility for Medicaid



SOURCE Authors' analysis of data for 2010–15 from the National Health Interview Survey. **NOTES** Low-income parents, the sample, nonexpansion states, and "other coverage" are explained in the Notes to Exhibit 1. Significance refers to the difference from the actual percentage. CHIP is Children's Health Insurance Program. $^{**}p < 0.05$

Discussion

We estimated the effects of the ACA Medicaid expansion on insurance coverage, access to care, service use, affordability of care, and health status for low-income parents. In contrast to previous studies of the ACA expansion,[9–12] we accounted for the wide variation in the size of the expansion for parents across states to better capture the average impact on parents, and we specifically estimated the effects of expansions of different sizes.

We found strong and consistent evidence that the Medicaid expansion increased Medicaid coverage and reduced uninsurance rates among low-income parents in 2014–15. We also found some evidence of a reduction in rates of employer-sponsored coverage, but this result was more sensitive to the model specification and disappeared when we focused on parents with incomes below poverty (Appendix Table 6).[18] Our results suggest that low-income parents in nonexpansion states would have experienced an additional 9.4-percentage-point drop in their uninsurance rate—a decline of 28 percent—if those states had opted to participate in the ACA Medicaid expansion.

We found that only smaller Medicaid expansions were associated with an increased probability of having a visit with a general doctor or other provider in the previous year, compared to larger expansions. However, smaller expansions had no effects on affordability of care. In contrast, we found that both medium-size and large expansions reduced problems paying medical bills and that medium-size expansions also reduced delayed care and unmet need because of cost.

It is important to remember that the size of the expansion is explicitly tied to the income of the target population. Thus, small expansions target parents with somewhat higher incomes, while large expansions target a broader group—including those with very low incomes. This suggests that expansions in different states reached parents with different characteristics (for example, varying degrees of financial resources and health needs). While we included some controls for these characteristics, there might still be unobserved factors that could have contributed to our results.

Finally, we found a meaningful impact of the Medicaid expansion on mental health for low-income parents, with significant reductions in severe psychological distress concentrated among states with medium and large expansions. Given the lack of impacts on service use and the significant improvements in affordability of care in these states, the findings on psychological distress could suggest that the security of having health insurance provides mental health benefits beyond those obtained through medical care. This is consistent with evidence on the "warm glow" of health insurance from the Oregon Health Insurance Experiment.[22]

Conclusion

We found strong and consistent evidence that the ACA Medicaid expansion increased coverage, reduced problems paying medical bills, and reduced psychological distress among low-income parents. We also found important missed opportunities for coverage gains among nonexpansion states. Importantly, this analysis might underestimate the potential gains for nonexpansion states because we had limited power to detect the effects of large expansions. Only three expansion states had eligibility levels below 50 percent of poverty before implementation of the ACA Medicaid expansion, but thirteen nonexpansion states had thresholds that low (Appendix Table 2).[18]

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The benefits of the Medicaid expansion to low-income parents also have the potential to produce spillover effects for low-income children. Evidence suggests that children benefit when their parents are insured, and the mental health improvements for parents gaining coverage under the ACA could have particularly strong effects on the health and well-being of their children. As policy makers continue to debate the future of the ACA, this study provides important evidence on the benefits of expanding Medicaid eligibility for low-income parents and the missed opportunities for states not participating in the ACA Medicaid expansion. ■

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Medicaid and Financial Health
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ABSTRACT

This paper investigates the effects of the Medicaid expansion provision of the Affordable Care Act (ACA) on households' financial health. Our findings indicate that, in addition to reducing the incidence of unpaid medical bills, the reform provided substantial indirect financial benefits to households. Using a nationally representative panel of 5 million credit records, we find that the expansion reduced unpaid medical bills sent to collection by $3.4 billion in its first two years, prevented new delinquencies, and improved credit scores. Using data on credit offers and pricing, we document that improvements in households' financial health led to better terms for available credit valued at $520 million per year. We calculate that the financial benefits of Medicaid double when considering these indirect benefits in addition to the direct reduction in out-of-pocket expenditures.

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1 Introduction

Health insurance protects households against the financial hardships that result from adverse health shocks and helps them smooth their consumption in times of poor health. According to survey evidence from Hamel et al. (2016), over half of non-elderly adults without insurance have difficulty paying their medical bills, a rate more than double that of consumers with health insurance. These figures suggest that expanding health care coverage may significantly mitigate financial distress faced by consumers, particularly those with lower incomes who may have limited ability to bear the financial burdens that accompany adverse health shocks.

In this paper, we quantify the effect of health insurance on financial health. The existing literature highlights that consumer welfare gains from financial risk protection arise from reductions in the mean and variance of out-of-pocket medical expenses (Zeckhauser, 1970). We argue that, although low-income uninsured individuals pay only a small portion of the cost of their care, the overall benefit of insurance to them may be large. Specifically, we show that indirect effects of unpaid medical bills, through access to credit markets, may be an important factor to consider in establishing the overall value of insurance. Our empirical framework complements previous landmark studies estimating the benefits of insurance (Finkelstein and McKnight, 2008; Finkelstein, Hendren and Luttmer, 2015) by highlighting the impact of unpaid medical bills on the access to and price of credit. Our analysis also suggests that the incidence of unpaid medical bills (uncompensated care) at least partially falls on the low-income uninsured patients themselves, through this indirect *credit channel*.

We evaluate the financial benefits to consumers in the context of the Patient Protection and Affordable Care Act (ACA), which was passed into law in 2010. One of the ACA's marquee provisions sought to expand Medicaid eligibility to all individuals earning less than 138% of the federal poverty level (FPL).[1] While this expansion was intended to apply nationwide, the Supreme Court ruled that the states had to be allowed to decide for themselves whether they would adopt the expanded Medicaid eligibility rules. As a result, only about half the states had signed on when the expansion went into effect in 2014, providing us with quasi-experimental variation in the Medicaid expansion.

Our analysis combines state-level variation from the Medicaid expansion with administrative data from the Consumer Financial Protection Bureau's Consumer Credit Panel (CCP), a nationally representative panel of over 5 million de-identified credit records. An important advantage of this credit panel, when compared to other panels, e.g. Hu et al. (2016), is that it contains information on individual credit obligations (trade lines). In particular, this

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[1] Prior to passage of the ACA, Medicaid eligibility was largely determined by the states, subject to federal mandatory minimum coverage levels. Most eligible individuals were minor children or single parents.

includes whether or not the debt was reported by a medical provider and the date it was credited. As a result, we are able to separately identify unpaid medical bills that are in collection and the dates in which they were credited.

We find that the Medicaid expansion reduced the incidence of newly-accrued medical debt by 30% to 40%, with a disproportionately greater effect for larger medical debts. On average, the reform led to a large annual decline in accrued medical debt of \$37 per person, or \$900 per treated person, which translates into an overall reduction of \$3.4 billion in the two years following the reform. When compared to overall health care utilization and out-of-pocket spending, our estimates indicate that about 50% of unpaid medical bills (uncompensated care) of the uninsured go into collection. Our findings also suggest that collection agencies are able to recover between 10% of the face value over the first two years, providing a financial incentive for health care providers to sell uncompensated medical claims to collection agencies.

The CCP also makes it possible to identify movements into an out-of-repayment delinquency for various debts. We use this to calculate the effects of the policy on delinquency and insolvency. We find that the likelihood of becoming newly delinquent on a debt obligation dropped by 2.1%. For consumers with subprime credit scores, who may be the most susceptible to financial distress, this effect was twice as large. Consequently, we measure substantial improvements in credit scores for individuals in treatment states, relative to control, following the reform. Credit score gains were also disproportionately larger for subprime borrowers, who enjoyed gains over 3 times larger than the average. We further find that the expansion led to about 50,000 fewer bankruptcies among subprime borrowers in the two years following the reform.

Next we look at how improved financial health translates into better credit outcomes. For this purpose we use novel data on direct-mail credit offers from Mintel Comperemedia (Mintel) in conjunction with aggregated lender rate sheets collected by the Fair Isaac Corporation (Fico) to assess potential effects of the policy on the availability and pricing of credit to consumers. This analysis suggests that, following the reform, individuals in adopting states received more offers of credit and at substantially better terms relative to individuals in non-adopting states. To calculate a dollar value of implied interest savings, we simulate a refinancing of debt by individuals in adopting states given improved credit terms estimated using these data. Our estimates suggest large annual interest rate savings, predominantly on credit card debt and personal loans, of about \$12 per person, or \$280 per treated person. This translates into \$520 million in annual savings overall.

Finally, we turn to the effects on consumer welfare. To this end, we model uninsured individuals who derive utility from consumption and face a disutility from leaving medical bills unpaid. Disutility from unpaid medical bills captures costs like worsening credit options, the hassle of dealing with debt collectors, and the risk of legal action taken by creditors. Individuals choose what portion of their medical expenses to leave unpaid, trading off greater consumption with the disutility of not paying their bills. This simple framework helps formalize the notion of an indirect credit channel of insurance by decomposing uninsured's compensating variation (CV) of forgoing medical bills (e.g. insurance) into two distinct components: (1) the direct gains from reduced out of pocket spending and (2) the reduction in disutility from fewer unpaid bills. We quantify these components separately using two alternative approaches that rely on different assumptions.

In the first approach, which we call the direct approach, we simply add our calculated interest savings to the direct benefits of reduced out-of-pocket spending. Using this method, we find that the financial benefits of a mean reduction in medical bills increases by 60% when considering the indirect benefits in addition to reduced out-of-pocket spending. We view this as a conservative estimate of the financial benefits of health insurance since it ignores several other benefits, including a reduction in hassle costs of collections and legal actions.

In the second and more comprehensive revealed preference approach, we calibrate individuals' consumption utility and recover their disutility over medical debt by combining the first order condition with observed optimal repayment decisions of outstanding medical bills. In addition to obtaining closed form expressions of the CV for a mean reduction in medical bills, we implicitly quantify the risk premium and assess the value of risk protection from a reduction in the variance of medical expenditures. The revealed preference approach suggests that the financial benefits of a mean and variance reduction in medical bills more than double when considering the indirect financial benefits of insurance.

Our paper contributes to three main literatures. First, our findings add to a growing body of work studying the link between Medicaid and financial health (Finkelstein et al., 2012; Mazumder and Miller, 2016; Gross and Notowidigdo, 2011; Hu et al., 2016; Sojourner and Golberstein, 2017). In addition to providing new evidence on the effects of insurance on medical debt and financial distress at the national level, and in a policy-relevant context, we view this paper as a systematic assessment of the financial consequences of unpaid medical bills. Combining novel data on consumer debt obligations, credit worthiness, and access to and pricing of credit, we make explicit the connection between unpaid medical bills and financial consequences. We then quantify the significance of this credit channel of insurance by contrasting interest rate savings with changes in repayments to isolate the net consumer gains in dollars.

Second, our analysis complements a number recent studies on the value of Medicaid (Finkelstein, Hendren and Luttmer, 2015) and the value of public insurance more generally (Kowalski, 2015; Cabral and Cullen, 2016). These studies investigate the overall consumer benefit of public insurance, taking financial and health related benefits into account. In the context of Medicaid, Finkelstein, Hendren and Luttmer (2015) find that beneficiaries value the program by only \$0.2 to \$0.4 per dollar of government spending, mostly stemming from reduced out-of-pocket spending. Our approach is less ambitious as we only focus on the financial benefits of Medicaid insurance. Specifically, as our data is not informative on these, we do not consider changes in health care utilization as uninsured individuals gain Medicaid insurance. Instead, we extend the analysis of financial benefits by adding the indirect benefits from a reduction in unpaid medical bills.

Third, our results shed new light on the incidence of uncompensated care. Several recent studies document the important role of uncompensated care for health care delivery (e.g., (Coughlin, 2014) and (Dranove, Garthwaite and Ody, 2016)). Notably, Garthwaite, Gross and Notowidigdo (2015) document that hospitals act as ”insurers of last resort”, as the uninsured pay only a small fraction of their medical bills out-of-pocket. However, very little is known about the incidence of uncompensated care. We use trade-line level variation in credits and subsequent repayment of medical debt in collection to study the incidence of uncompensated care. Specifically, we examine the likelihood with which providers seek repayment through third party collections, the rate at which new medical collections are repaid, and how these debts affect low-income uninsured patients through their subsequent interaction with broader credit markets.

The remainder of this paper is organized as follows. We start with a discussion of institutional details surrounding the Medicaid expansion and unpaid medical bills in Section 2. We describe the data in Section 3 and lay out our difference-in-difference approach in Section 4. In Sections 5 and 6 we present our main findings on medical debt and financial distress, respectively. We then examine the impact of improved financial health on credit market outcomes and quantify the dollar value of this benefit in Section 7. Turning to the effects on consumer welfare, we formalize the effects of paid and unpaid medical bills on consumer welfare in Section 8 and present our overall financial benefit estimates in Section 9. Section 10 concludes.

2 Institutional Details

2.1 The Medicaid Expansion

Signed into law in 2010, the Patient Protection and Affordable Care Act (ACA) was one of the most sweeping health care reforms in U.S. history. Among its most important and controversial provisions was its expansion of the Medicaid program to include all individuals earning less than 138% of the federal poverty level (FPL). Prior to the reform, Medicaid's principal beneficiaries were low-income children, their parents, as well as people with disabilities. Childless adults between the ages of 18 and 65 were for the most part ineligible to receive insurance in nearly all states. Under the ACA, states either had to agree to this expansion or lose their federal Medicaid funding. Twenty-six states challenged the constitutionality of this provision (and other portions of the ACA) an in its famous decision *NFIB vs. Sebelious* the Supreme Court declared the law to be unconstitutional. Instead, it required that states be allowed to maintain their existing Medicaid programs and retain the option to adopt expanded coverage.[2]

By January 1, 2014, on the eve of the expansion's intended rollout, only 24 states plus the District of Columbia had adopted the measure. Of these, 19 states expanded their Medicaid programs on January 1, 2014. The other 5 states and the District of Columbia expanded their programs prior to this date. Another 7 states would adopt expanded eligibility, but after January 1, 2014. This left 19 non-adopting states as of the date this analysis was conducted. Figure 1 illustrates the states' adoption decisions since passage of the ACA. In our analysis, we exclude consumers in the early- and late-adopting states and focus on trends in the 19 states that expanded Medicaid on January 1, 2014 (which we refer to throughout as the adopting or treatment states) and the 19 non-adopting states (control states).

Health care coverage increased substantially in adopting states. According to the *Medicaid and Children Health Insurance Program (CHIP) Enrollment Report* from January 2016, there were 6.1 million more people enrolled in Medicaid in the 19 adopting states in December 2015 than the average enrollment in these same states from July-September 2013, an increase of 31.8%. In control states, enrollment was up by 2.2 million people or 11.7%.[3] Hence, we attribute a Medicaid enrollment increase of 3.3 million, about 4.1% of the non-

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[2] *National Federation of Independent Business v. Sebelius*, 567 U.S. Also see Kaiser Family Foundation (2012) for more detail.

[3] See https://www.medicaid.gov/medicaid/program-information/medicaid-and-chip-enrollment-data/monthly-reports/index.html, last accessed on June 26, 2017. Enrollment figure for the control states exclude Maine, for which data are unavailable. The increase in enrollment is concentrated among adults. We find only small changes in CHIP enrollment over this period.

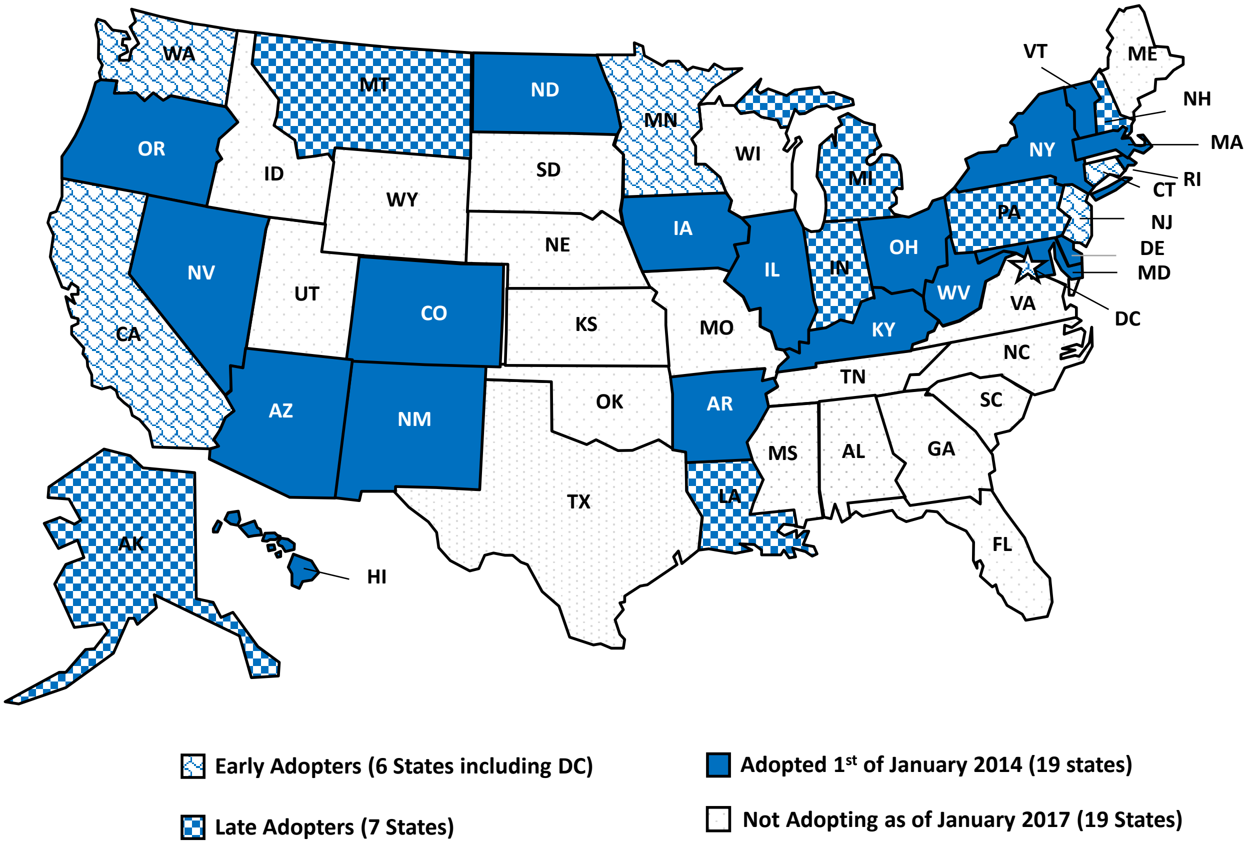

Figure 1: Medicaid Adoption Across States

Source: *Kaiser Family Foundation, see http://kff.org/health-reform/slide/current-status-of-the-medicaid-expansion-decision/ for further details.*

elderly population, to the Medicaid expansion, which is roughly consistent with estimates from the literature.[4]

2.2 Unpaid Medical Bills in Uninsured's Balance Sheet

Recent survey evidence from the Kaiser Family Foundation (KFF) (Hamel et al., 2016) notes that about a quarter of non-elderly adults in the U.S. have difficulties paying their medical bills, with that figure rising to more than half among the uninsured. Not surprisingly, previous studies have found that the uninsured pay only up to 20% of medical bills out-

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[4] Most closely related to our context, Courtemanche et al. (2016) find a coverage increase of 5.9 percentage points among the non-elderly adults in Medicaid expansion states by the end of 2014. In contrast, coverage increased by only 3 percentage points in non-expansion states suggesting an additional 2.9 percentage point increase due to the Medicaid expansion. Frean, Gruber and Sommers (2016) find that the ACA Medicaid expansion increased insurance coverage by 9 percentage points among individuals who were newly eligible for Medicaid with no evidence that the expansion crowded out private insurance.

of-pocket (Finkelstein, 2007), or $480 out of about $2,400 in overall annual health care spending according to recent estimates based on data from the Medical Expenditure Panel Survey (MEPS). The remaining cost is left as uncompensated care (Coughlin, 2014).

Uncompensated care comprises both charity care and uninsured care or bad debt. According to the American Hospital Association (AHA), charity care comprises services for which the hospital never received but also never expected payment, possibly because of the patient's inability to pay. Bad debt consists of services for which the hospital anticipated but did not receive payment. While charity care is not charged to consumers, 'bad debt' is billed to consumers through third party collection agencies. Collection accounts placed on individuals' records severely impact their credit worthiness, reducing the quality of credit options available to them.

In practice, the distinction between charity care and bad debt is blurry and hospitals often struggle to draw the distinction. Not surprisingly, there is little empirical evidence on the relative magnitudes of charity care and bad debt. Instead, studies have focused on quantifying the prevalence of uncompensated care in general and how it is affected by the Medicaid expansion. For example, (Bachrach, Boozang and Lipson, 2015) find that the Medicaid expansion led to a net reduction in uncompensated care in hospitals of about $2.6 billion per year in expansion states. This translates into a reduction in total uncompensated care of about $4.3 billion considering that hospitals provide about 60% of uncompensated care to the uninsured, see (Coughlin, 2014). An important advantage of our data is that we can document changes in medical debt in collection directly, allowing us to provide new evidence on the relative importance of bad debt when compared to charity care. We discuss these estimates in detail below.

3 Data

3.1 Consumer Credit Panel

The main data used in this study come from the Consumer Financial Protection Bureau's Consumer Credit Panel (CCP), a nationally representative 1-in-48 random sample of de-identified credit records drawn quarterly from a nationwide credit reporting company (NCRC). The CCP contains de-identified account-level information about sampled consumers' individual debt obligations (trade lines), including each account's opening date, current balance, and past payment history. Although de-identified, credit records in the CCP are linked over time, allowing us to study the evolution of debts for consumers in our sample.

Information in the CCP on individual trade lines makes it possible to determine the source of a debt obligation and the debt-origination date on which reported debts originated. Specifically, we can identify medical debts as those that were either directly reported by a medical provider or were reported by third-party debt collectors as unpaid medical bills.[5] We focus on the flow of new medical debts incurred each quarter because this measure better reflects the effects of Medicaid expansion than the stock of outstanding medical debt. This definition of medical debt is somewhat narrow by necessity. For example, credit card balances that are acquired by paying for medical services could be considered a type of medical debt. However, while credit records contain information about outstanding credit card balances, the information is insufficient to determine the portion of those balances derived from medical services versus other types of expenditures. Consequently, we exclude debts from paid medical bills in our definition of medical debt.[6]

Like medical debt, we base our measures of financial distress on flows, which better depict the timing of delinquency and bankruptcy decisions and allow us to more cleanly identify changes in the distribution of distress following reform. For each credit account, the CCP includes up to 84 months of payment history. Using this information, we can determine whether each account transitioned into a higher state of delinquency during each quarter. Such transitions could include accounts that were current in the previous quarter but are now (at least) 30 days past due. This also includes accounts that had been 30 days past due but became 90 days past due during this quarter.[7]

We restrict our analysis to a balanced sample of adults aged 18-64 in the 19 adopting (treatment) states and the 19 non-adopting (control) states (Figure 1).[8] We aggregate the data to the year-quarter level and focus on outcomes in the 10 quarters before and 8 quarters following the expansion.[9] This covers the period 2011Q3 to 2015Q4.[10] Oftentimes there are significant lags between when debts are acquired and when they are reported to the NCRCs,

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[5] The data, however, do not include any information that reveals the name of the medical provider or the type of medical service provided.

[6] In Appendix B.2 we evaluate the effects of Medicaid expansion on the credit card debt position of households and find significant effects.

[7] We consider any account that starts a quarter as 90 days past due or worse to be in default and do not include further transitions, such as charge-offs or repossessions, which often reflect lender-initiated actions, as instances of financial distress.

[8] Attrition in administrative credit record data is exceedingly rare. We balance the sample to exclude (1) accounts that were flagged as fraudulent and (2) accounts created during the sample period, or account for individuals just entering the formal credit sector.

[9] Our analysis is limited to the 10-quarters before the expansion of Medicaid because the variable necessary to determine which third-party collection accounts were medical is not available in the data for quarters prior to September 2011.

[10] Quarterly intervals allow us to smooth out monthly variation in the accrual of medical debt and in measures of financial distress (like bankruptcy) that can be rare and highly volatile.

though the delay does not affect the reported trade line's opening date. To account for this lag, we use a one quarter forward archive to identify new medical debts in our analysis. For example, we measure new medical debts acquired in quarter q using the CCP archive for quarter $q+1$. Our analysis suggests that this lag provides the most complete coverage of the amount of medical debt reported. Finally, our baseline sample covers approximately 2.13 million credit records and 38 million quarterly observations.

Table 1 provides summary information on the measures of medical debt and financial distress used in the analysis. Column 1 in the table shows overall means in the data. Columns 2 and 3 summarize the data separately for the pre- and post-reform quarters, respectively, and for adopting (treatment) and non-adopting (control) states. As shown in the table, about 5% of consumers acquired a new medical debt each quarter during the analysis period, and the propensity was somewhat lower in adopting states than in non-adopting states. This difference can at least partially be attributed to differences in the fraction of uninsured individuals across treatment and control states. In the post reform period, new collections remained largely stable in non-adopting states, while the prevalence was falling by about 12% in adopting states. Moreover, an average consumer with new medical debt accrues 1.7 new obligations with an average value of about $1,200. The number and value of new

Table 1: CCP Summary Statistics

	All (1)		Pre-Expansion (2)	Post-Expansion (3)
New Medical Collections				
Receiving (p.p.)	4.94	*Not Adopting*	6.14	6.08
		Adopting	3.63	3.19
Average Number	1.67	*Not Adopting*	1.68	1.71
		Adopting	1.62	1.60
Average Value ($)	1,186	*Not Adopting*	1,227	1,325
		Adopting	1,032	944
Delinquency Rate (p.p.)				
Any New Delinquency	6.58	*Not Adopting*	6.81	6.70
		Adopting	6.46	6.21
Consumer Risk				
Credit Score (Fico)	675	*Not Adopting*	664	672
		Adopting	681	689
Observations	38,270,034	*Not Adopting*	11,897,972	9,554,005
		Adopting	9,363,158	7,454,899

Notes: *This table shows summary statistics of medical debt and financial distress from the CFPB's Consumer Credit Panel (CCP). The data are quarterly for 19 adopting and 19 non-adopting states (see Figure 1 for list of states) from 2011Q3 to 2015Q4. Medical debts and Delinquencies are counted as flows in that quarter.*

medical debts, among those who acquire them, is also greater in non-adopting states and decreases following the implementation of the reform for adopting states.

About 6.6% of individuals in our sample become newly delinquent (e.g. new 30-day or new 90-day delinquency) on an existing debt. This rate is slightly higher in non-adopting states and declines more following reform in adopting states. On average, their credit score, measured by their Fico score as of the end of each quarter, is 675. This score is considered as Prime for purposes of credit.[11] Note that, although credit scores went up on average following the reform, they increased slightly more in adopting states.

Medicaid is a means-tested program. As a result, a large portion of American households remained unaffected by the expansion. Average effects, although large, may mask substantial heterogeneity in the impact of the policy across wealthier and more modest communities. Because the CCP provides geographic information on accounts at the Census tract level, we can explore this heterogeneity by merging demographic data on Census Tract poverty rates from the American Community Survey (ACS). For this match we use the 2009-2013 ACS 5-year averages. Using pre-reform eligibility criteria by state for childless adults as of January 1, 2013, and the policy's new eligibility benchmark of 138% of the federal poverty line (FPL), we calculate the proportion of non-elderly adults in each Census tract that would be newly eligible for Medicaid following the expansion. We calculate this fraction in treatment states (which expanded Medicaid) as well as control states, which we refer to as the fraction of newly eligible non-elderly adults.

3.2 Loan Offers and Pricing (Mintel and MyFico)

To study the effect of improved financial health on consumer's credit option, we use data on loan offers and pricing from Mintel Comperemedia (Mintel) and MyFico. We focus on the four most common sources of debt for the Medicaid population: (1) credit cards (2) personal loans (3) auto loans (4) mortgages.

We measure changes in credit card and personal loan rates using data on direct mail offers from Mintel Comperemedia (Mintel) from January 2012 to December 2016. The Mintel data are generated via a nationally representative survey of approximately 2,000 households, or 4,000 individuals. Each month participating households are asked to provide Mintel with all mail solicitations they received during the month, which include offers of new credit

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[11] Prime consumers are often defined as having a credit score higher than 620. If the consumer has a credit record that the credit scoring model deemed unscorable, we treat the consumer as subprime. For a detailed discussion of what makes credit records unscorable and the characteristics of 11% of adults with such records, see Brevoort, Grimm and Kambara (2016).

from any lender in the marketplace.[12] Direct mail remains one of the most popular and effective channels by which lenders advertise both credit cards and personal loans to potential customers. Furthermore, we observe the county of residence of each resident. As a result, these data are uniquely suited for exploring changes in credit terms offered to consumers following the Medicaid expansion.

In our analysis we focus on new acquisitions of credit card and personal loan offers that have been pre-screened.[13] Pre-screened offers are made to potential customers whose credit quality has been previously checked and as a result are targeted toward specific risk types.[14]

Table 2 shows summary information on credit card and personal loan offers and pricing by the fraction of newly eligible non-elderly adults. For each respective loan product, and by quartile of indigent adults, the table shows the proportion receiving offers and the average rate on those offers. Slightly less than half of surveyed individuals receive credit card offers, and this proportion remains stable across poorer and richer communities. On average, recipients are offered a 16.5% interest rate on purchases, with rates increasing in the share of poor adults. Personal loans, often advertised as 'credit consolidation' loans, are part of a much newer and smaller market that frequently targets subprime consumers. As shown in the table, the incidence of personal loan offer rates is about 12.5%, or one quarter that of

Table 2: Mintel Credit Card and Personal Loan Offers

	All (1)	1^{st} *Quartile* (2)	2^{nd} *Quartile* (3)	3^{rd} *Quartile* (4)	4^{th} *Quartile* (5)
Credit Card Offers					
Percent Receiving	49.82	49.71	50.27	48.55	51.04
Average Rate	16.53	16.34	16.54	16.90	17.82
Personal Loans					
Percent Receiving	12.46	11.68	12.44	13.77	20.51
Average Rate	8.24	7.86	8.24	9.08	9.80
Observations	105,973	46,349	43,678	13,499	2,447

Notes: The table shows summary information on credit card and personal loan offers from Mintel comperemedia by the fraction of newly-eligible non-elderly adults. The proportion of individuals receiving is the *un-weighted* proportion of individuals in the sample receiving at least one offer. The average rate is conditional on receiving and is *weighted* by a *mail-volume* variable calculated by Mintel.

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[12] These include nearly all marketing solicitations and are not restricted to direct credit offers.

[13] Pre-screened offers are identified via a flag for the presence of a pre-screen opt out disclosure. Opt-out disclosures are required by law for pre-screened mail out offers.

[14] Often mail out offers are made without screening consumers. These often occur with the roll out of new products in an effort to learn their profitability.

credit cards. Moreover, this proportion is increasing with the rate of poor adults. Individuals living in the poorest communities are more than 60% more likely to be offered these products. The average rate on a personal loan offer is lower than for a credit card, at just more than 8%. This is in part because, unlike credit cards, personal loans are installment loan products which do not provide an open ended line of credit. However, like credit card offers, rates on offered personal loan products are higher in lower income communities.

Mortgages and auto loans are less commonly offered through direct mail. However, in pricing mortgage and auto loans, lenders often set rates uniformly within credit score ranges. These *rate sheets*, which are often statewide or nationally determined, make translating credit score ranges into lower interest rates less complicated. We use publicly available rate sheet information on published by Fair Isaac Corporation, the creator of the widely-used FICO score, in their *MyFico* web tool. This information, which is aggregated from lender rate sheets, provides credit score ranges that are widely used for lenders for both products and the prevailing market interest rates for each of those ranges.

Table 3 shows the *MyFico* aggregated rate sheets for 5-year auto loans and 30-year fixed rate mortgages as of March 19, 2017. In the analysis we estimate potential interest rate effects of the policy by assigning each consumer the interest rate they would have qualified for in that quarter based on their credit score (see Table 1). Consumers with credit scores below the bottom price tiers are excluded from calculations, as they are not eligible for a loan. This imputation implies that any changes in average rates arise directly from the changes in credit scores.

Table 3: Rate Sheets for Auto Loans and Mortgages

Auto Loan Pricing Tiers						
Credit Score Bin	500-589	590-619	620-659	660-689	690-720	>720
Auto Loan APR	15.117	13.970	9.653	6.948	4.863	3.514
Mortgages Pricing Tiers						
Credit Score Bin	620-639	640-659	660-679	680-699	700-759	>760
Mortgage APR	5.484	4.938	4.508	4.294	4.117	3.895

Notes: This table shows rate sheets for Mortgages and Auto Loans from the Fair Isaac Corporation's (FICO) *MyFico* web page (`http://www.myfico.com/credit-education/calculators/loan-savings-calculator`).

4 Difference-in-Difference Design

We now turn to our main empirical strategy, which exploits the quasi-experimental variation provided by states' option to expand Medicaid. We apply a difference-in-differences (DD) approach to identify the effects of the reform on medical debt accruals, the rate of flows into delinquency, and lenders' pricing and offers of credit to consumers. Specifically, we provide graphical and regression based evidence on two different levels of aggregation. First, we graphically compare outcomes in expansion states relative to non-expansion states before versus after the reform. Under the standard parallel trend assumption, we interpret differential changes in expansion states in the post-reform years as the intent-to-treat (ITT) effect of the Medicaid expansion.

These effects may be relatively small given that only about 4.1% of the non-elderly adults in the expansion states gained Medicaid coverage because of the expansion. To corroborate the graphical evidence, we exploit more granular variation in Medicaid eligibility at the census-tract level in our primary regression specification:

$$y_{ct}^{k} = \alpha_{c}^{k} + \eta_{t}^{k} + \delta^{k} \cdot (ER_{c} \cdot Post_{t}) + \beta^{k} \cdot (ER_{c} * \cdot Post_{t} \cdot Exp_{s(c)}) + \epsilon_{ct}^{k}. \tag{1}$$

Here, y_{ct}^{k} denotes the respective average outcome k in census tract c in year-quarter t. The specification includes census tract fixed effects α_{c}^{k} and quarter-year fixed effects η_{t}^{k}. $Post_{k}$ is an indicator for the post expansion period, and ER_{c} denotes the fraction of newly-eligible non-elderly adults in the census tract. Finally, $Exp_{s(c)}$ is an indicator variable that turns on if the census tract is located in an expansion state.

Our primary parameter of interest is β^{k} which now captures differential effects of the Medicaid expansion across census tracts. Specifically, we expect larger effects in those census tracts that have a larger fraction of Medicaid eligible non-elderly adults if the state expands its coverage criteria. Again, we interpret the β^{k} coefficients as intent-to-treat effects since Medicaid eligibility does not imply Medicaid take-up (treatment). Therefore, we construct the average treatment effect on the treated (ATT) by dividing the reform effect in an average census tract, $\beta^{k} * \overline{ER_{c}}$, by 0.041.

In the following sections, we turn to the results, considering two main effects. The first, the *Direct Effect* on medical debt, measures the effects of the reform on medical debt obligations. The second, the *Indirect Effect* on distress, measures the effects on of the reform financial distress, as measured by the flow of delinquencies and subsequent improvements in consumers' credit risk and offered rates. In each we use the following basic specification to measure these effects and finally to calculate potential interest rate savings to consumers resulting from the reform.

5 Direct Effects of Medicaid on Medical Debt

In this Section, we present graphical and regression-based evidence on the direct effects of the Medicaid expansion on medical debt.

5.1 Average Effects

We begin by presenting graphical evidence in Figure 2, which plots raw data trends in newly-accrued medical collections for treatment and control states, respectively. Plotted trends are normalized by the pre-expansion mean for each respective group. In the Figure, the left panel shows trends in the overall propensity to receive a collection, the middle panel shows the total number of collections credited to the record in a given quarter, and the right panel shows the total value of new collections reported. As illustrated in the figure, two-years after the reform, the propensity to accrue new medical debt fell by 20% in treatment states relative to control states. These effects are of similar relative magnitudes when looking at the instances and total value of collections received, the middle and right most panels,

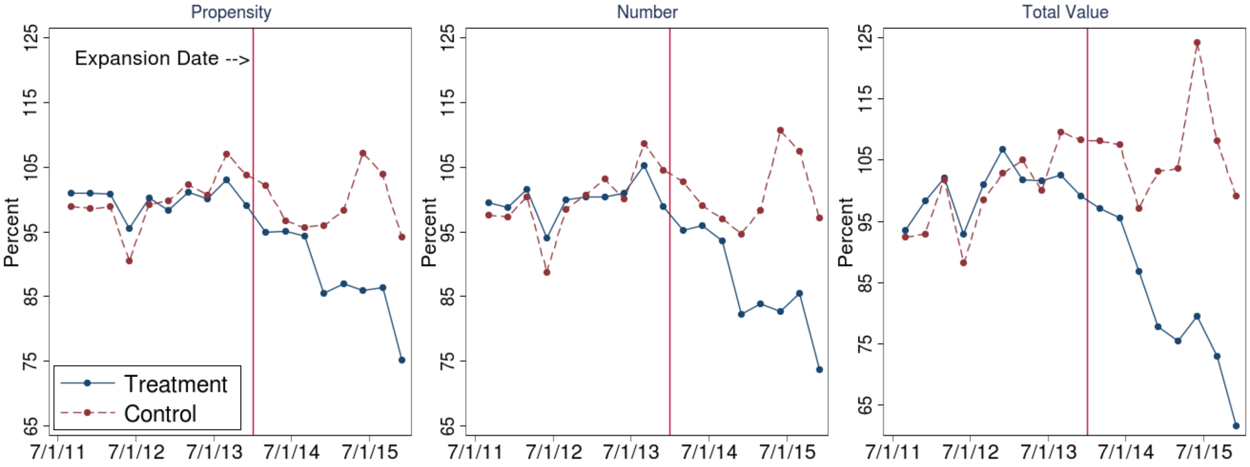

Figure 2: Trends in Newly Accrued Medical Debt

Notes: *The figure shows trends in the incidence, frequency, and value of newly-accrued medical collections. Data are from the CFPB's Consumer Credit Panel described in section 3. Trends are quarterly means of newly-accrued collections for treament and control states, respectively, and are normalized by the pre reform mean for each group. Vertical lines highlight the implementation date of the expansion - January* 1^{st}*, 2014.*

respectively. Within 24 months following the reform, the average number of collections and the average total value of newly accrued medical debt were approximately 20% and 30% lower, respectively, in treatment states relative to control states.

We corroborate these findings in two robustness checks. First, the findings are not driven by systematic changes in collection activities in expansion states. We find no evidence for changes in non-medical collections. Second, the findings are not driven by differential openings of private market insurance exchanges in treatment states. Repeating the analysis among states which use the federal platform leaves the findings largely unchanged. For details on these robustness checks see the Appendix Section B.1.

Turning next to the regression-based evidence, Figure 3 shows the census tract specific treatment effects by the fraction of newly-eligible adults: $\hat{\beta} * ER_c$. The left figure presents the quarterly percentage reduction in new medical debt along with the 95% confidence interval.[15] The vertical lines denote the 25th, the 50th, and the 75th percentile of census tracts when ordered by the fraction of newly eligible adults. For instance, about 20% of the adults in the median census tract are newly-eligible for Medicaid and see a 20% reduction in the amount of new medical debt. As expected, the decline in newly-accrued debt is greater in tracts with a larger proportion of eligible individuals. In tracts with 12% of newly eligible adults (25^{th} percentile), accrued medical debt per person-quarter decreased by approximately 10%, while that reduction was closer to 35% for tracts with 30% of newly-eligible adults (75^{th} percentile).

In the right panel, we simply scale the estimates by the the average pre-reform amount of new medical debt in collection in the given census tract, to measure the quarterly reduction in dollars. At the 25^{th} percentile of tract eligibility, medical collections per person decreased by about \$5 per quarter. The reduction for those living in tracts at the 75^{th} percentile of eligibility was on average 5 times larger, or \$25 dollars per person-quarter.

5.2 Distributional Effects

We next divide the analysis by the dollar amount of underlying medical collection, to assess whether the Medicaid expansion differentially affected larger collections. To this end, we build on our regression model, equation (1), and separately investigate the effects of the Medicaid expansion for large ($\geq \$1,000$) and small ($< \$1,000$) collections. The top panel of Figure 4 shows larger reductions for large collections when compared to small collections. The panel shows regressions results using equation 1 where the dependent variable is

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[15] Standard errors are clustered at the census tract level and we use the STATA package "predictnl" to construct the confidence intervals.

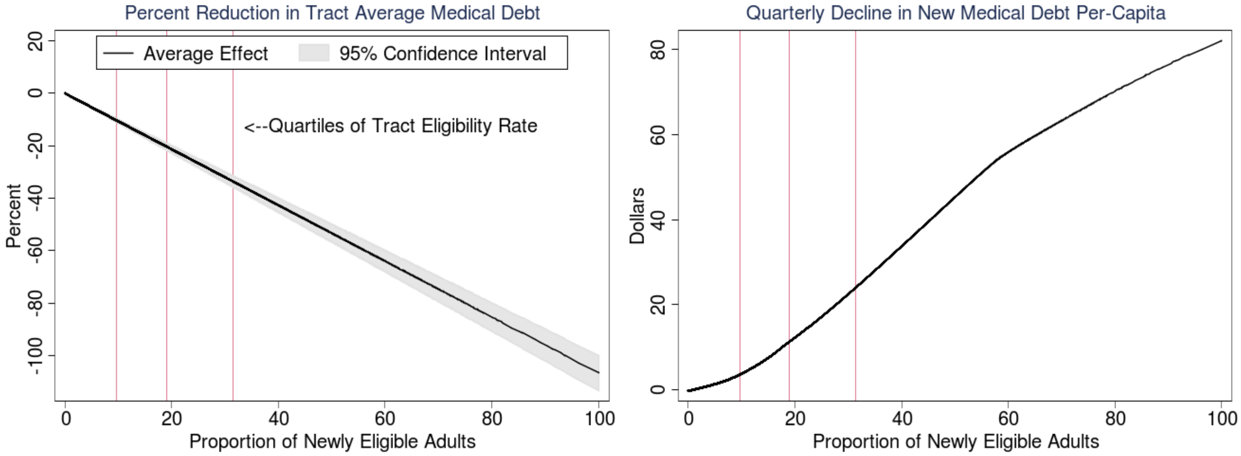

Figure 3: Medicaid Expansion and Declines in Medical Debt

Notes: *The figure shows percent changes in and level changes in newly-accrued medical debt by Census tract eligibility rate. The left panel of the figure shows estimates from equation 1 with related point-wise 95% confidence intervals. The effect for a given eligibility rate is defined as* $\hat{\beta} \times ER_c$*. Regressions are weighted using the number of adults in the Tract. All standard errors are clustered at the Census tract level. The right panel of the figure plots the corresponding level effects,* $\hat{\beta} \times ER_c \times MD_c^{pre}$*, where* MD_c^{pre} *abbreviates the average pre-reform amount of new medical debt in collection. The panel shows a smoothed trend using weighted local linear regression. In each panel, the vertical lines represent Census tract eligibility rate quartiles. From left to right, these denote the* 25^{th}*,* 50^{th}*,* 75^{th} *percentiles of Tract level eligibility rates, respectively. Data are from the CFPB's CCP and quarterly from July 2012 to July 2015 for 19 adopting (treatment) and 19 non-adopting (control) states.*

$\mathbb{1}[\text{New Medical Debt} \in j]$, $j = \{\text{small, large}\}$. While the propensity to accrue large unpaid medical collections is less than a third of that for small medical collections, the decline in accrual due to the reform is substantially greater. For example, in a community with a 12% eligibility rate, the 25^{th} percentile, the propensity to receive large medical collections declines by approximately 0.4 percentage points, or 52%. In that same community, the expected decline in the incidence of small unpaid medical collections is closer to 0.2 percentage points, or 7%. Often small-value medical collections result from clerical errors in doctors' bills or disputes about insurance coverage, whereby insured individuals may incur collections without any knowledge of a missed payment (Brevoort and Kambara, 2015). In contrast, large value medical collections are significantly more likely to arise from emergency room visits or hospital admissions of uninsured individuals. Consequently, a relatively greater impact on large value medical debts supports the idea that newly insured individuals are no longer incurring large medical bills after treatment.

The bottom panel of Figure 4 presents further evidence on changes in the distribution of newly-accrued medical debt. In the bottom left panel, we present regression outcomes for

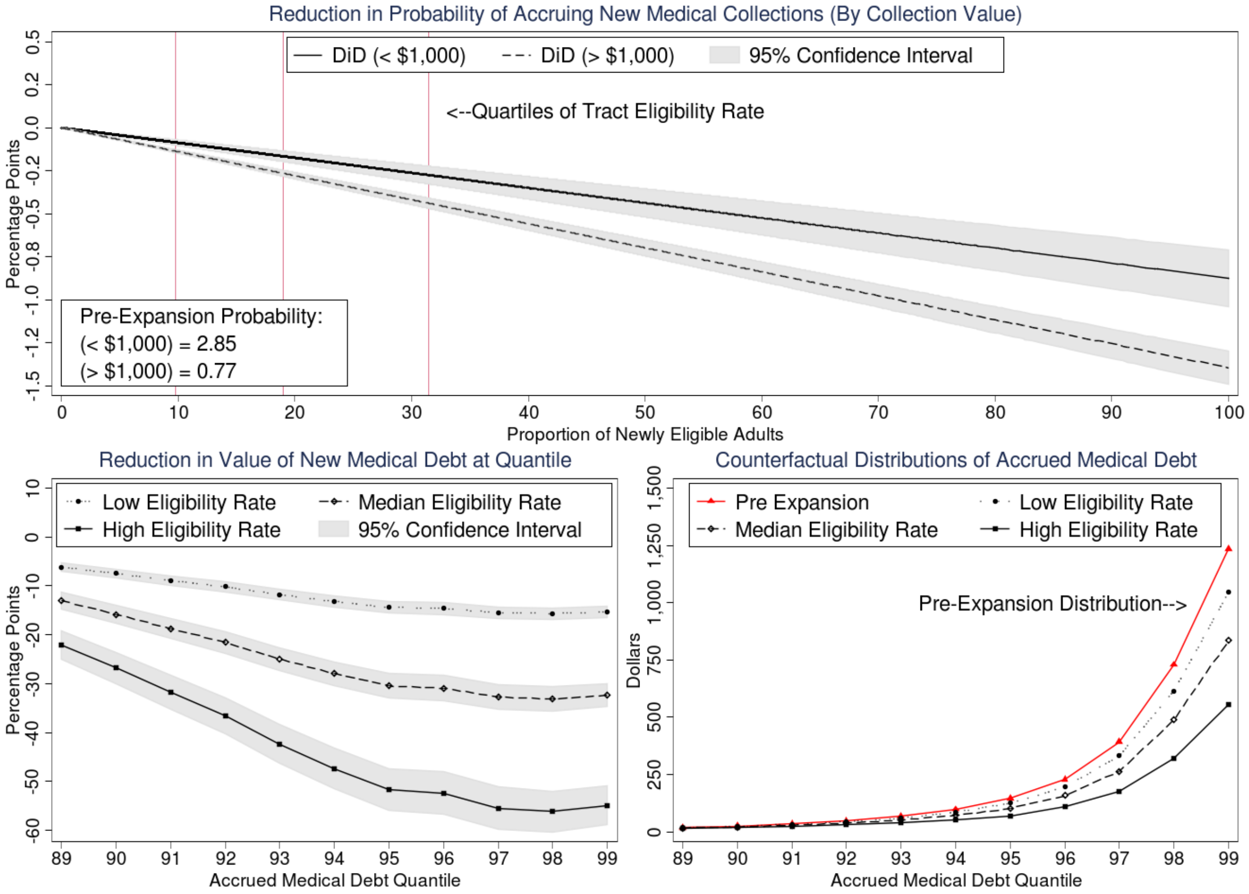

Figure 4: Distributional Effects of Expansion on Medical Debt

Notes: *The figure shows distributional effects of the reform on the accrual of medical debt. Data are from CFPB's CCP. The top panel show treatment effects and 95% confidence intervals for large* $(\geq \$1,000)$ *vs. small* $(< \$1,000)$ *collections using Equation 1. The bottom left panel plots treatment effects and confidence intervals at each quantile of medical debt in tract* c *and quarter* t*. Regressions are weighted using the proportion of adults in a Census tract. In all regressions, standard errors are clustered at the Census tract level. From left to right, these denote the* 25^{th}*,* 50^{th}*,* 75^{th} *percentiles of Tract level eligibility rates, respectively. Data are from the CFPB's CCP and quarterly from July 2012 to July 2015 for 19 adopting (treatment) and 19 non-adopting (control) states.*

each of the highest percentiles in the medical debt distribution. The point estimates for each percentile summarize the results of a separate regression, where the dependent variable is simply the corresponding percentile in the distribution of newly-accrued medical debt at the census tract quarter level. Instead of presenting the full linear extrapolation by eligibility, we only present the effects for (1) low eligibility tracts (25^{th} percentile of eligibility), (2) median eligibility tracts (50^{th} percentile of eligibility), and (3) high eligibility tracts (75^{th} percentile of eligibility). The bottom right panel then plots the corresponding level effects, where we simply scaled the percentage reduction with the pre-reform levels.

Our findings suggest that the effect of the expansion increases for higher quantiles and again more so in high eligibility tracts. Among high eligibility tracts, for example, the policy induced reduction in new medical debt rises from approximately 20% at the 89^{th} percentile to nearly 60% at the 99^{th} percentile.[16] The dollar reductions (bottom right) further confirm the assertion. Among high eligibility tracts, an average 20% reduction at the 89^{th} percentile, on a base of about \$20 in average debt at the quantile, translates to a modest savings of only \$4. However, the savings become quite substantial past the 95^{th} percentile. For the highest quantile, a nearly 60% reduction in the accrual of new medical debt translates into roughly \$700 of savings or about 60% the average size of a newly accrued medical bill in collections (Table 1).

5.3 Medical Debt and Consumer Payments

In this section, we use our parameter estimates to calculate the amount of new medical debt that is not accrued annually due to the reform. We then combine these with estimated repayment patterns of medical collections to calculate how much of this decline in accrual translated directly into reductions in out-of-pocket payments for treated tracts. As shown in Table 5, the policy led to an average annual reduction in new medical debt of about ($\sim$ \$37.71) per person. Scaled by the population of non-elderly adults in treatment states, this amounts to a \$1.7 billion annual reduction overall. More than half this decline ($\sim$ \$860m) came from individuals living in the poorest communities, where per-capita reductions were nearly about 4 times the average. Overall, our results show that the program was progressive, investing heavily in low income neighborhoods and less so in wealthy communities.

Although the accrual of medical debt fell sharply, the majority of unpaid medical bills sent to collections are not repaid. As a result, fewer accrued medical debts do not necessarily translate directly into a reduction in consumer payments. The middle panel in Table 5 shows repayment and removal rates of medical collections up to two years after a medical collection appears on an individual's credit report for those living in treatment states prior to the expansions. One difficulty with ascertaining repayment rates is that a sizable proportion of collections are removed from records within one or two years of their appearance. Collections often are removed from a credit record in cases where individuals were wrongly billed and a complaint was placed with the provider, although removal could occur for any number of other reasons. Since the data provide no information regarding the repayment status of removed collections we form bounds on repayment rates. The lower bound of repayment

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[16] Although fewer than 5% of consumer receive a medical collection in each quarter on average, this may mask some variation across census tracts. This is why we can identify effects at the 89^{th} quantile.

Table 4: Reduction and Repayment of Medical Debt

	All	1st *Quartile*	2nd *Quartile*	3rd *Quartile*	4th *Quartile*
	(1)	(2)	(3)	(4)	(5)
Annual Decrease in Accrued Medical Collections					
Average Per Person ($)	37.71	4.57	20.80	58.21	145.96
Total ($Billions)	1.69	0.08	0.26	0.49	0.86
Proportion of New Medical Collections Repaid (p.p)					
After One Year					
Repaid	7.85	9.49	8.83	8.78	6.00
Repaid or Removed	36.83	33.94	35.44	37.48	38.11
After Two Years					
Repaid	9.05	10.53	10.19	10.26	6.94
Repaid or Removed	51.68	48.08	49.78	52.67	53.22
Annual Decrease in Per Person Expected Medical Debt Payment ($)					
After One Year					
Lower Bound	2.96	0.43	1.84	5.11	8.76
Upper Bound	13.89	1.55	7.37	21.82	55.62
After Two Years					
Lower Bound	3.41	0.48	2.12	5.97	10.13
Upper Bound	19.49	2.20	10.36	30.66	77.68
Population 18-64 in Expansion States (Millions)	44.86	18.22	12.32	8.38	5.93

Notes: *This table presents estimates of annual per-capita average reduction in medical debt, repayment rates, and total accrued savings using estimates from equation 1 in Figure 3. Repayment rates are within eligibility rate quartile. Percent repaid is the proportion of new medical collections in quarter t that were repaid one and two years later, respectively. Percent removed is the proportion of new medical collections in quarter t that were removed one and two year later, respectively. The lower and upper bound correspond to repaid and repaid or removed medical collections, respectively. The CCP Population is calculated by multiplying the number of records in 2013Q4 by 48, the sampling rate of the data (Section 3)*

assumes none of the removed collections were repaid, and the upper bound of repayment assumes all of the removed collections were repaid.

On average, 8% of newly accrued debt is repaid within one year of appearing on an individual's credit report, and 9% within two years. About half of newly-accrued debt is removed entirely from the credit record within two years, the majority of that within one year. Although the repayment rate is lower in high eligibility, often low-income, communities, the proportion of debt repaid or removed is higher. For example, in the lowest-eligibility communities, the bottom quartile, about 11% of debt is repaid and 48% is repaid or removed. In high-eligibility communities, the top quartile, that proportion changes to 7 and 53%, respectively.

The bottom panel of Table 4 combines effects on collections and repayments to calculate upper and lower bounds on reductions in medical debt repayments. As aforementioned, the lower bound assumes that bills removed were not repaid by consumers while the upper bound assumes that all collections removed were repaid. Given this, we calculate that annual repayments per person declined by between $3.40 and $19.49. Despite lower repayment rates, the largest reductions came from the poorest communities, for which the decline was up to 20 times greater than for the richest communities.

Table 5 also allows us to benchmark our results to previous work on Medicaid provision. Note from the top row of column 1 in the table that the Medicaid expansion led to a \$37.71 annual per person reduction in medical debt accrual. Dividing this point estimate by an estimated coverage gain of 4.1 percentage points from Medicaid expansion we calculate a debt reduction of $-\frac{\$37.71}{0.041} = -\$920$ per newly-insured person per year. As a point of comparison, estimates from the landmark Oregon Health Insurance Experiment imply a treatment effect of Medicaid insurance on medical debt of -\$390 (standard error 177) per treated person per year (Finkelstein et al., 2012). When accounting for differences in the measurement of medical collections resulting from attrition (e.g. $\sim$ 50% of collections disappear after two years) we find a debt reduction per treated person per year of approximately \$460. Although the Oregon experiment focused on a small and geographically concentrated sample of consumers, we find its estimated savings to be remarkably close to our national averages. We interpret this congruence in two ways. First, we see it as further evidence in favor of the validity of our approach in identifying the exogenous effects of the reform. Second, we see it as verifying a natural generalization of the experimental result to the context of a large national reform.

Our estimates also provide evidence on the relative significance of uninsured care or bad debt in uncompensated care, an estimate that is not readily available from the literature, to the best of our knowledge. As outlined in Section 2.2, we assume that the uninsured pay about 20% of overall health care utilization, worth \$2,400 per year, out-of-pocket. This suggests that uncompensated care equals about \$1,920 per uninsured person and year.[17] We find a reduction in medical debt in collation of about \$920 per treated person, which is about 38% of overall health care utilization or about 48% of uncompensated care. Hence, we conclude that about half of uncompensated care is sent to collection.

6 Indirect Effect of Medicaid on Financial Health

Newly reported medical collections often indicate a broader financial hardship, raising the likelihood of future delinquencies in non-medical debt repayment and even bankruptcy. As a result, unpaid medical bills sent to third party collections and reported to credit bureaus

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[17] This is roughly consistent with the evidence from Garthwaite, Gross and Notowidigdo (2015), who find that each additional uninsured person costs a local hospital about \$900 annually in uncompensated care, given that hospitals only provide about 60% of the overall uncompensated care to the uninsured, see (Coughlin, 2014).

can directly impact consumers' credit scores, potentially making credit both less available and more expensive. Using an event study framework, we provide evidence of these effects by documenting a steep rise in delinquency and a sharp decline in credit scores following a newly credited medical collection (See Appendix A for details). Motivated by this evidence, we turn to the indirect effects of the Medicaid expansion on consumer delinquency and credit scores.

6.1 Repayment Delinquencies

Consumers in financial distress are more likely to miss payments on their outstanding loans. This is why credit delinquency rates are commonly-used indicators of financial distress and prospective borrower risk. Using the payment history for each account in the CCP, we determine the extent to which the expansions affected consumer transitions into a delinquency on outstanding loans. Our measure includes mild delinquency, a transition from current to 30 days or more past due, moderate delinquency, a transition from 30 to 60 days past due, or serious delinquency, a transition from 60 into 90 or more days past due. We consider any of these transitions on any loan a new delinquency. Isolating flows into missed repayments, rather than looking at contemporaneous payment status of all outstanding accounts, allows us to focus on episodes of worsening distress. We use the resulting worsening delinquency rate to explore whether the Medicaid expansion reduced the likelihood of financial distress.

We start with the graphical evidence in the left two panels of Figure 5, which plot raw (normalized) data trends in worsening delinquency for treatment and control states. The left panel shows trends for the whole CCP population while the middle panel shows trends for individuals with baseline credit scores below 620, the ex-ante *subprime* group .[18] While the trends for both groups are similar during the pre-expansion period, delinquency rates trend notably lower after the expansion in states that expanded Medicaid (e.g. treatment states). As is shown in the figure, this is especially true for our ex-ante subprime group, for whom the proportional decline is nearly twice as large. Subprime borrowers are more likely to be positively affected by the Medicaid expansion for several reasons. First, their low scores suggest past financial distress(past payment history is generally the most important factor used to generate scores) or have characteristics, such as a high utilization rate on their revolving accounts, that indicate that they are more likely to become delinquent in the future. Second, lower income consumers, who are more likely to be eligible for Medicaid, are

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[18] Industry standards consider individuals with credit scores below 620 as subprime. We separate prime and subprime consumers by their score as of the first of quarter in the sample, their baseline.

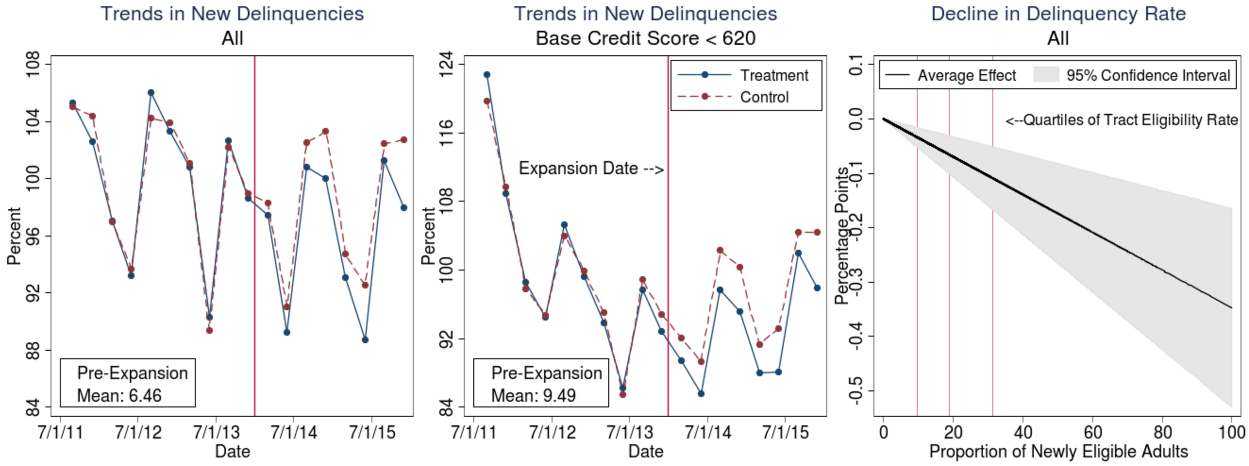

Figure 5: Medicaid Expansion and Growing Delinquency

Notes: *The figure shows quarterly flows into new delinquency for consumers in treament and control states. Trends are normalized by the pre-reform mean for each group. Delinquency is defined as consumers having one or more credit accounts that became 30, 60, 90, or more days past due during the quarter. The left panel shows normalized trends for all consumers in the data (see notes in Figure 2) and the middle panel shows normalized trends among individuals with a credit score less than 620 as of the first quarter of the sample period (the subprime group). The right panel shows percentage point declines in new delinquencies from Equation 1, with respective point-wise 95% confidence intervals. All standard errors are clustered at the Census tract level.*

more likely to have subprime credit scores. Third, the declines in the incidence of medical debt observed in section 5 were concentrated in this group.

We present the regression-based evidence for all borrowers in the right panel, based on equation (1), which plots the estimated percentage point reduction in worsening delinquency by the fraction of newly eligible adults in the the census tract. Consistent with the graphical evidence, we find that the Medicaid expansion reduced delinquency rates and more so in census tracts with a larger fraction of newly-eligible adults. At the 50^{th} percentile, new delinquencies decreased by approximately 0.08 percentage points, or about $0.08/0.041 = 1.95$ percentage points per treated person. This translates into a 30% reduction relative to the pre-expansion mean, suggesting that the the reform's effect on financial distress was substantial.

6.2 Credit Scores

On-time repayment of existing debt is among the most important determinants of future credit worthiness, which is often summarized by a consumer credit score. In turn, credit scores are used pervasively by lenders for credit underwriting and pricing. The fall in medical debt accrual and new delinquencies resulting from the Medicaid expansion likely benefit consumers in the form of higher credit scores.

The top left and top middle panel of Figure 6 present normalized credit scores in treatment and control states for all and subprime consumers, respectively. As shown in the figure,

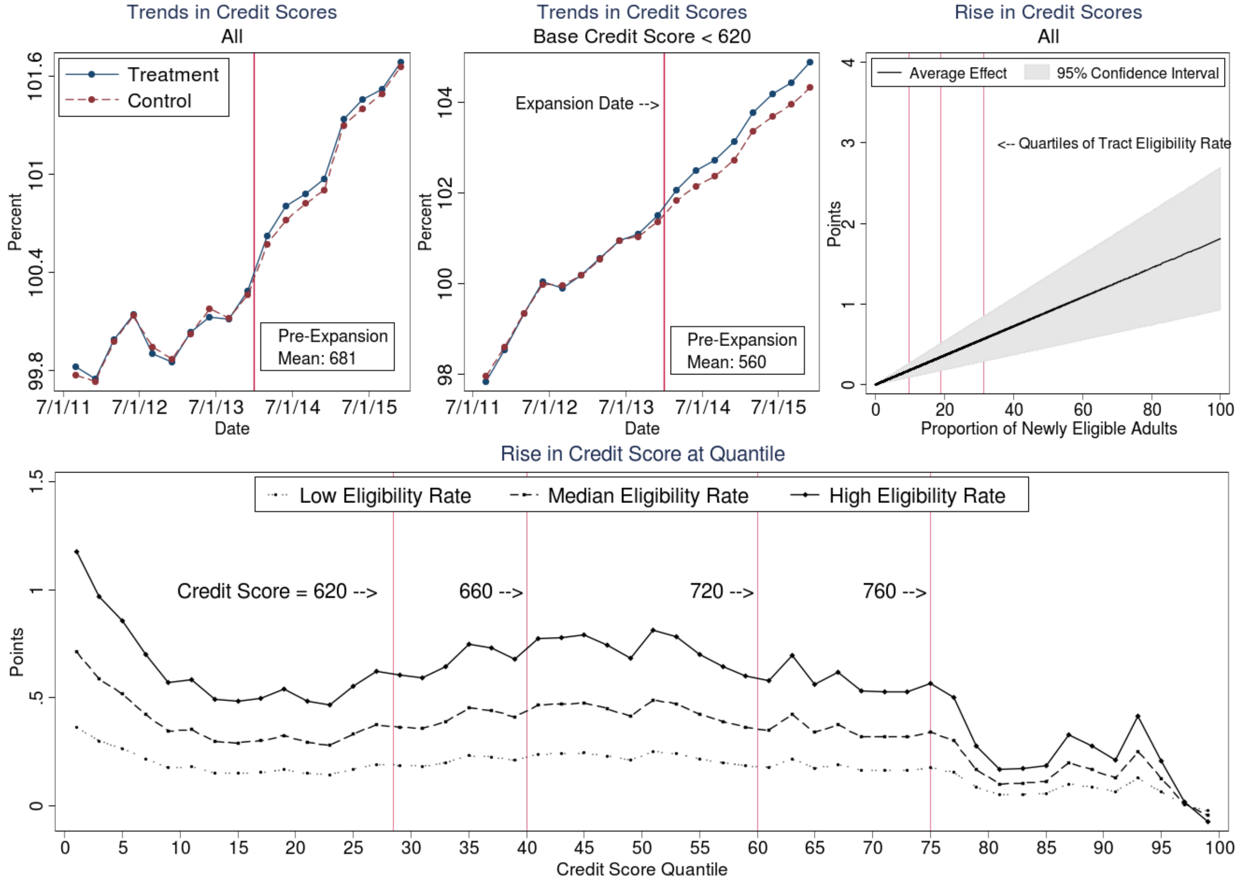

Figure 6: Medicaid Expansion and Credit Scores

Notes: *The top left and top middle panels of the figure show normalized trends in the credit scores of consumers in treament and control states, respectively (see notes in Figure 2). The top left panel shows trends for all the entire CCP sample with a credit score. The top middle panel shows normalized trends among individuals with a credit score less than 620 as of the first quarter of the sample period (the subprime group). The top right panel shows percentage point declines in end of quarter credit scores (the dependent variable) from Equation 1, with respective point-wise 95% confidence intervals. The bottom panel shows regression from Equation 1 with the credit score quantile* q_{ct} *as the dependent variable (See notes in Figure 4 for details). All standard errors are clustered at the Census tract level.*

the overall effect on credit scores is noticeable yet small. Nevertheless, the effect of the expansion on the ex-ante sub-prime group (base credit score < 620) is more than 4 times larger. This can occur for several reasons. First, a given 'level' decrease in risk will mechanically imply larger point gains for individuals who have low scores to begin with, a mechanical effect. Second, and more substantively, individuals who are ex-ante more financially vulnerable are also those most likely to gain most from receiving insurance, as shown above (Figure 5). Third, as credit worthiness is often associated with income, subprime individuals likely reside in Census tracts with high eligibility. As a result, they are more likely to be treated.

Turning again to the regression based evidence for all borrowers, the top right panel shows percentage point declines in end of quarter credit scores, by the fraction of newly eligible adults. At the 25^{th} percentile of eligibility (low eligibility) the effect is somewhat small at roughly 0.2 points increase. At the 75^{th} percentile of eligibility (high eligibility), this effect is much larger, about 0.5 points. To put these estimates into perspective, we again divide the estimate for the 50th percentile by the fraction of newly-insured Medicaid beneficiaries. This suggests a $0.35/0.041 = 8.8$ point increase in the credit score per treated person. We will return to the interest rate implications in the next section.

Finally, the bottom panel of Figure 6 shows the distributional effects similar to those in Figure 4. As shown in the figure, the impact of the policy is greatest at the bottom of the credit score distribution, with an average effect of more than 1 point per person in high eligibility tracts. While this effect declines for higher quantiles, it remains somewhat strong in the middle portion of the distribution. Importantly, it has a very small effect at the very top of the credit score distribution, where likely few individuals are treated and those who are treated are financially less fragile.

7 Pricing and Availability of Credit

To facilitate the interpretation of the indirect effects on financial health and to assess their economic significance, we now turn to the effects on access and price of credit. Specifically we study the four most common types of debt obligations held by consumers: (1) Credit Cards (2) Personal Loans (Unsecured installment credit) (3) Auto loans (4) Mortgages. We estimate the effects of the reform on credit cards and personal loans using offer data from Mintel and the effects on auto loans and mortgages using rate sheet data from MyFico (See Section 3 for details). Specifically, we impute automobile and mortgage interest rates based on observed credit scores and the credit score interest rate crosswalk provided by the rate sheets. Finally, we calculate how changes in credit terms might translate into lower monthly payments (savings) by simulating a debt refinance under these new credit terms.

7.1 Changes in Availability and Terms of Credit

We begin with an analysis of the reform's effects on offered credit card interest rates. The left panel of Figure 7 shows (normalized) trends in the credit card rates in treatment and control states. Consistent with our findings on delinquency rates and credit scores, we see a relative decline in the interest rate in treatment states following the expansion. The right panel shows the regression based evidence, building on equation (1). We leverage the county of residence information on survey participants in the Mintel data, and aggregate the individual data to the county-year-quarter level. Therefore, we present the credit interest rate changes by the fraction of newly eligible non-elderly adults in the respective county. Again we see a significant decline in the offered credit card rate of about 0.5 percentage points at the the 50^{th} percentile of eligibility. This effect is significant and increasing in county eligibility rates, reaching more than 1 percentage point in counties with a high fraction of eligible adults.

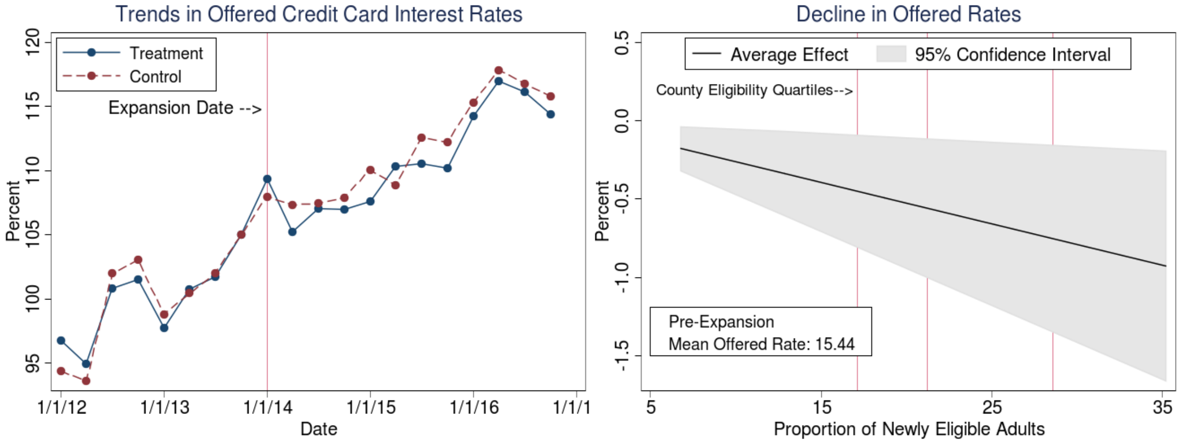

Figure 7: Medicaid Expansion and the Pricing of Credit Cards

Notes: *The left panel of the figure shows normalized trends in offered credit card rates for adopting (treatment) and non-adopting (control) states. (See notes in Figure 2 for details.) The right panel shows regression results and related 95% confidence intervals for a regression using Equation 1 where the dependent variable is the mean rate of offers sent to a respondent. Regressions and trends are weighted using Mintel's mail-volume weight. (See section 3 for details). Standard errors are clustered at the county level.*

Figure 8 presents analogous effects for interest rates on personal loans. Unlike credit cards, personal loans form part of a smaller and nascent market which largely focuses on highly indebted subprime customers (Section 3). As a result, the incidence of personal loan offers in the data is much lower than for credit cards (Table 2). This smaller sample size on offers leads to noisier trends. Nevertheless, as shown in the left panel of Figure 8, offered rates on personal loans seem to decline for recipients in expanding states relative to non-

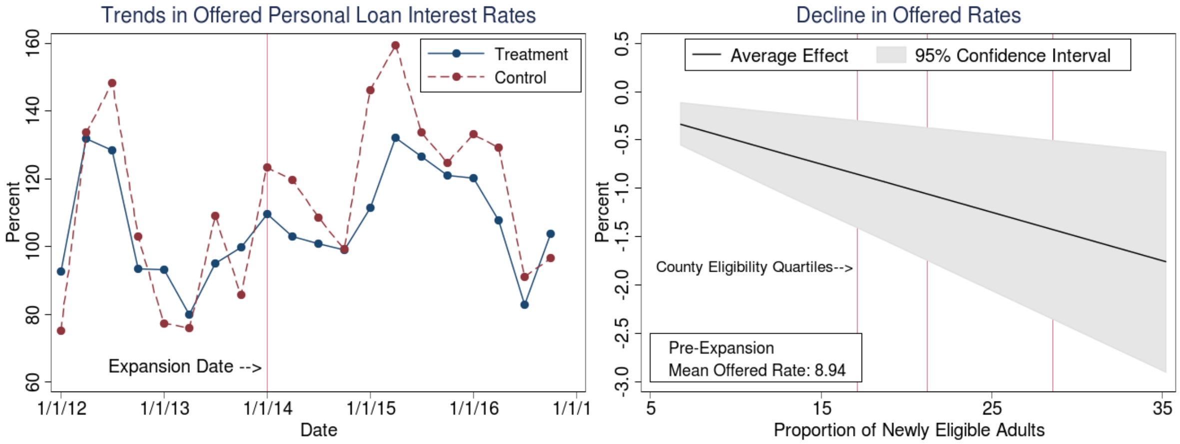

Figure 8: Medicaid Expansion and the Pricing of Personal Loans

Notes: *The figure shows trends (left panel) and treatment effects (right panel) for offered rates on personal loans using the Mintel data. See notes in Figure 7 for details.*

expanding states following the reform. In the right panel of the figure, we confirm that this effect is nonetheless statistically significant and larger in counties with more newly eligible adults. Moreover, the absolute decrease in rates is larger for personal loans relative to credit cards. This result is consistent with the fact that this market focuses on indebted subprime borrowers. For this segment of consumers, a modest improvement in credit worthiness can considerably increase outside borrowing options, prompting substantial responses from lenders making personal loan offers.

With respect to access to credit, we find evidence for a positive effect of the Medicaid expansion on credit card offer rates not only when comparing treatment and control states, but also when leveraging the more granular variation in eligibility at the county level. The evidence for personal loans is mixed. Overall, this points to increased access to credit following the expansion, providing an additional indirect financial benefit of health insurance. In what follows, we abstract away from this potential benefit suggesting that our primary estimates may provide a conservative estimate of the indirect benefits. For details on the access to credit, see the Appendix Section B.3.

Returning to changes in interest rates, Figure 9 shows the regression based evidence for imputed auto (left panel) and mortgage (right panel) rates based on equation 1.[19] Auto loans and mortgages are for the most part priced using lender rate sheets. Consequently, the effects documented in the figure reflect almost mechanically from the policy's impact on individual's credit scores (Figure 6). As shown in the figure, the expected reduction in these loan types,

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[19] See Section 3 and Appendix C.

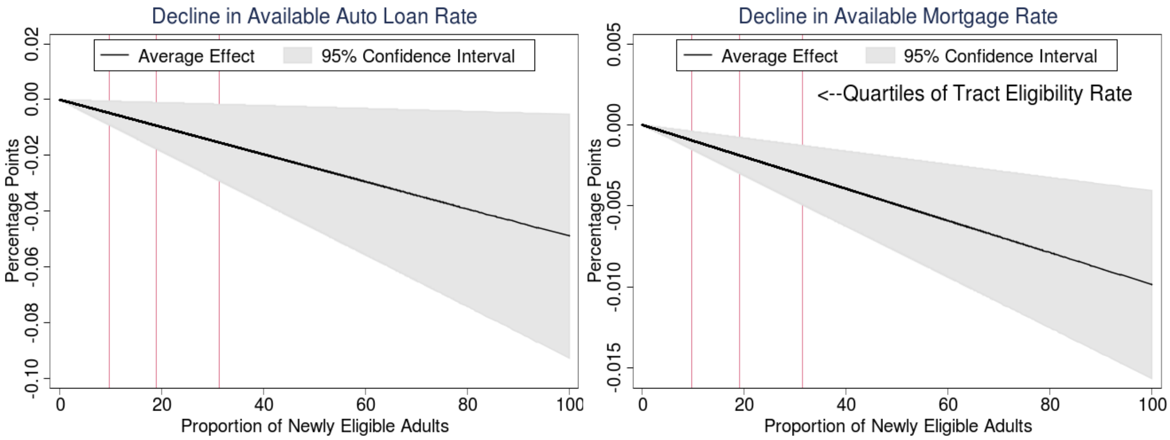

Figure 9: Medicaid Expansion and Available Rates for Auto Loans and Mortgages

Notes: *The figure shows regression results from Equation 1 with dependent variables (1) imputed auto loan rate (left panel) and (2) imputed mortgage rates (right panel). Auto loan and mortgage rates are imputed using MyFico rate sheets (Table 3). For imputation details see Section 3.*

although modest, is statistically significant and increasing in tract eligibility. Also, while mechanical, we believe these effects provide further meaningful information regarding the improved terms of credit potentially available to consumers, which we use in the simulation below.

7.2 Dollar Value of Improved Financial Health

We use our results on the pricing of credit to calculate the potential dollar value of improved financial health by simulating a refinancing of debt held by consumers in treatment states under the expectation of new credit terms.We restrict our population to individuals living in treatment states and consider a refinancing of their debt just prior to the expansion, e.g. December 2013. We further assume that the credit cards and personal mortgages are amortized over 36 months, that auto loans are refinanced as 5-year loans, and that mortgages are refinanced at 30-year, fixed-rate loans. This is consistent with the interest rates published by FICO. Moreover, for credit cards and personal loans, which, unlike mortgages and auto loans, are not backed by valuable assets, we net out any effects due to increased repayments. We express savings in annual terms. The details of our simulation are set out in the Appendix C.

Table 5 shows the results from our simulation exercise. The table shows per-person and aggregate annual savings, which we interpret as the intent-to-treat effects. As in Table 4, simulation results are shown separately by eligibility quartile (Columns 2-5) as well as overall

(Column 1). As shown in the table, the overall savings to consumers are substantial. We

Table 5: Simulated Interest Rate Savings

	All (1)	1^{st} *Quartile* (2)	2^{nd} *Quartile* (3)	3^{rd} *Quartile* (4)	4^{th} *Quartile* (5)
Credit Cards					
Average Per Person ($)	8.19	4.30	8.76	12.12	13.43
Total ($Millions)	367.34	78.41	107.92	101.57	79.43
Personal Loans					
Average Per Person ($)	2.81	1.01	2.55	4.77	6.15
Total ($Millions)	126.05	18.35	31.36	39.97	36.36
Auto Loans					
Average Per Person ($)	0.22	0.08	0.21	0.37	0.47
Total ($Millions)	10.01	1.52	2.62	3.09	2.78
Mortgages					
Average Per Person ($)	0.40	0.26	0.46	0.55	0.48
Total ($Millions)	17.89	4.78	5.69	4.59	2.83
All					
Average Per Person ($)	11.63	5.66	11.98	17.81	20.52
Total ($Millions)	521.29	103.07	147.59	149.23	121.40
Population 18-64 in Expansion States (Millions)	44.86	18.22	12.32	8.38	5.93

Notes:*The table shows results from simulations of consumer savings using Intent-to-Treat estimates in Figures 7-9. The table shows per person effects and total effects calculated using the CCP Population. See Appendix C for further details.*

find interest savings worth $11.63 per person and year, which is about 30% of the per-person reduction in medical debt (Table 4). To put this estimate into perspective, we again divide by the fraction of non-elderly adults that gained Medicaid insurance because of the reform and find annual interest savings of about $11.63/0.041=$284 per treated person. Savings on unsecured loans, and in particular credit cards dominate the total effect. Simulated savings for credit cards and personal loans add up to about $11, or ∼95% of the total. This is consistent with other studies showing that the most at risk individuals carry a disproportionate amount of unsecured debt, which can be discharged at bankruptcy (Domowitz and Sartain, 1999; White, 2006). Lenders react accordingly by increasing prices more on these types of loans relative to loans backed by an asset. The dollar value of improved financial health then might largely flow through reduced prices on this type of credit.

Also shown in Table 5, per-capita and aggregate savings varied by rate of newly eligible adults in a tract. As might be expected, savings in tracts at the top quartile of eligibility (Column 5) were nearly four times larger than those in tracts at the bottom quartile of eligibility (Column 2). Nevertheless, a smaller population in tracts with a high proportion of new Medicaid-eligible adults implies that aggregate savings were greatest in the third quartile of eligible tracts (Column 4). Interestingly, although still important, the share of savings from lower credit card rates is lower in tracts with a high rate of eligible adults. Whereas in the bottom quartile (Column 2) the share of savings due to refinancing credit card debt is approximately 75%, the share at the top quartile (Column 5) is closer to 65%. The difference is explained by added savings from personal loan refinancing among individuals in these tracts. As higher Medicaid eligibility occurs in more financially modest communities, this change in the mix of savings is further consistent with personal loans being used primarily by individuals in greater financial distress.

An important detail to note is that the CCP provides end-of-quarter snapshots of loan balances for respective individuals. Although this is not a concern for installment loans, whose balances reflect true debt, it is possible that a portion of credit card balances may not constitute credit card borrowing. This is because a portion of reported credit card balances may still be held within the 'grace' period, and as a result not incur any finance charges.[20] However, we note that aggregate credit card borrowing rates measured in the CCP accord quite well with more direct measures of credit card borrowing taken from the CFPB's Credit Card Database (CCDB) (Bureau, 2015).[21] Moreover, individuals' credit card utilization rate (e.g. the ratio of balances to credit limit) is surprisingly stable over time, helping to quell potential concerns of large fluctuations over time in borrowing (Fulford and Schuh, 2017).

8 Medical Bills and Consumer Welfare

In this section, we illustrate how paid and unpaid medical bills affect consumer welfare. Within a simple framework, we show how restricting attention to changes in out-of-pocket spending (paid bills) can vastly understate the full financial benefit of insurance against paid and unpaid medical bills. The outlined model leverages the observation that the share of the total medical bill that is paid out-of-pocket provides information on the disutility of higher debt levels. Finally, we turn to a quantitative analysis of the effects on consumer welfare in the next section.

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[20] Note that individuals who pay off their balance at the end of the billing cycle, e.g. while still in their grace period, are commonly called *transactors*. Individuals who carry, or revolve, balances across billing cycles are called *revolvers*. The latter type often accrue finance, or interest, charges on those carried balances. Once a balance has been carried across a billing cycle, there is no longer a grace period on any balances until the account is repaid in full.

[21] The CFPB's CCDB is a large de-identified panel of credit card accounts that provides direct evidence of revolving behavior.

8.1 Paid vs. Unpaid Medical Bills

We consider a static environment in which consumers derive positive utility from consumption, $g(c)$, and face a utility loss from medical debt in collection, $-h(D)$. Utility losses from unpaid bills capture costs such as future reductions in consumption due to worse credit options, through pricing and availability, disutility from dealing with debt collectors, as well as legal costs related to unpaid bills and bankruptcy. Consider then consumer preferences of the form

$$U = g(c) - h(D) \tag{2}$$

with $g'(\cdot) > 0, g''(\cdot) < 0$ and $h'(\cdot) > 0, h''(\cdot) \geq 0$. Consumers' marginal utility of consumption is decreasing while their marginal disutility of medical debt is weakly increasing. Consumers earn income Y and are exposed to random medical bills $\epsilon_{MB} \sim G$, where G denotes the underlying distribution function. We assume that a fixed fraction of medical bills, $0 \leq \alpha_{charity} \leq 1$, goes as charity care, and is not held financially against the patient. The remainder, $1 - \alpha_{charity}$, is either paid out-of-pocket or goes into collection and becomes medical debt. To simplify the theoretical analysis, we assume $\alpha_{charity} = 0$ and revisit the role of charity care in the numerical analysis in Section 9.

We assume that consumers have existing medical debt $\bar{D}$ and decide on the optimal amount of new medical bills $0 \leq b \leq \epsilon_{MB}$ that goes unpaid, trading off utility from consumption and disutility from medical debt. Conditional on a realized medical bill, ϵ_{MB}, consumers maximize:

$$\max_{0 \leq b \leq \epsilon_{MB}} g(Y - (\epsilon_{MB} - b)) - h(\bar{D} + b) \tag{3}$$

where in optimality

$$g'(Y - (\epsilon_{MB} - b^*)) - h'(\bar{D} + b^*) = 0 \ . \tag{4}$$

Introducing a trade-off between consumption utility and disutility from medical debt changes the consumer welfare implications from reductions in the mean and the variance in medical bills. We discuss these implications in detail below.

8.2 Mean Reduction and Consumer Welfare

We start with an analysis of the effect of mean reductions in medical bills on consumer welfare. To this end, we ignore uncertainty in medical bills and evaluate the financial harm of a fixed medical bill (ϵ_{MB}). The key implications of the model are discussed graphically in Figure 10. The Figure depicts consumption on the horizontal axis and marginal (dis) utility on the vertical axis. For simplicity, we assume linear marginal utility functions. The

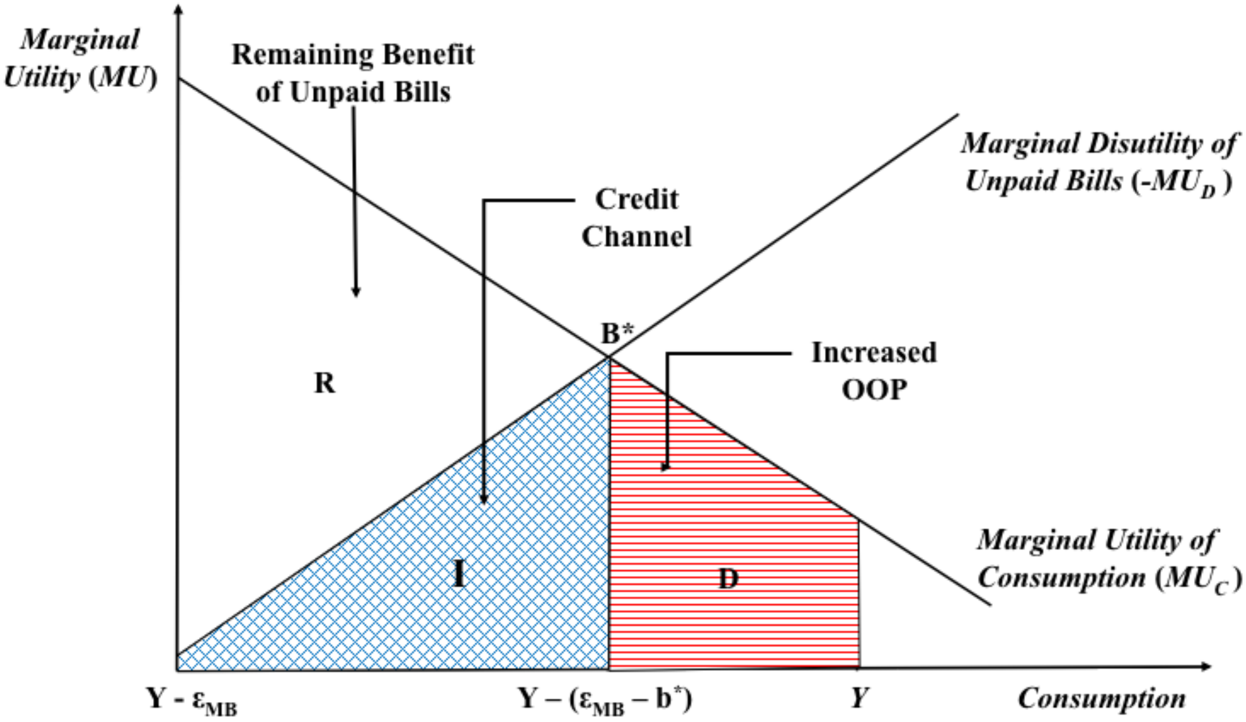

Figure 10: Welfare Benefits of Mean Reduction: Example

downward sloping line is the marginal utility of consumption (MU_C), and the upward sloping line is the marginal disutility of medical debt ($-MU_D$).

Absent any medical expenses, an individual consumes her income Y. When facing a medical bill of size ϵ_{MB}, she decides on the amount that she is willing to pay out-of-pocket, $\epsilon_{MB} - b^*$. In an optimum, the marginal utility of an additional dollar of consumption must equal the marginal disutility of an additional dollar in medical debt. This is depicted in point B^*. We can then define the welfare loss resulting from a medical bill as the sum two effects: (1) the direct effect on out-out of pocket spending and (2) the indirect effect, or the *credit channel*.

In the figure, the red area D bounded by the marginal utility of consumption, the individual's baseline income Y, and her final consumption, $Y-(\epsilon_{MB}-b^*)$, captures the direct effect, or the utility loss from reduced consumption due to increased out-of-pocket payments. The indirect, or *credit channel* effect is then the blue area, bounded by the marginal disutility of medical debt, final consumption, $Y-(\epsilon_{MB}-b^*)$, and final consumption minus the borrowed amount $Y-\epsilon_{MB}$. As described above, this *credit channel* highlights the potentially adverse consequences of unpaid bills on access to and the price of credit as well as other costs associated with not paying bills. The sum of the two areas captures the overall utility loss from the medical bill shock ϵ_{MB}. Finally, the white area (R) captures any remaining net benefit from unpaid medical bills. To see this, note that were the individual to pay the entire amount out-of-pocket, the utility loss would be the entire area underneath the marginal utility of consumption between: $(= R + I + D)$.

8.2.1 Transfer Gain from Insurance: Compensating Variation

To gauge the transfer gain from insurance, in dollars, we analyze the compensating variation (CV). In this context, the CV describes the amount of income a person is willing to forgo if the medical bill of the amount ϵ_{MB} is removed:

$$CV = e(p_0, u_0) - e(p_1, u_0) = e(\epsilon_{MB}, u_0) - e(0, u_0) \ .$$

Here, $e(\cdot)$ denotes the underlying expenditure function. Naturally, we have $CV = \epsilon_{MB}$ if the person pays the entire bill out-of-pocket. Conversely, if only a portion of the medical bill is paid out-of-pocket, then we have $\Delta OOP \leq CV \leq \epsilon_{MB}$, where ΔOOP denotes the counterfactual savings in out-of-pocket payments. Building on the graphical intuition from Figure 10, $Y - CV$ corresponds to the point on the horizontal axis, where the area underneath the marginal utility of consumption curve bounded by $Y - CV$ from the left and Y from the right equals the sum of the blue and the red area (D+I).

It is evident from this graphical characterization that the CV depends on the shape of the marginal utility curves and, of course, the underlying medical bill amount. To quantify the CV, we adopt two alternative approaches that rely on different assumptions. The first approach builds on the financial benefit estimates discussed above. We refer to this approach as the *direct approach*. Specifically, we add the implied annual interest savings to the reductions in out-of-pocket payments to find:

$$CV = \Delta OOP + \Delta Interest \ .$$

We view this approach as a conservative lower bound for the CV as it ignores the benefits from increased access to credit as well as reduced hassle costs from dealing with debt collectors.

8.2.2 Revealed Preference Approach

Our second approach builds directly on the outlined utility model and provides a more comprehensive evaluation of the financial benefits from paid and unpaid medical bills. In this approach, we calibrate the utility over consumption and reveal the disutility over medical debt from realized out-of-pocket payments. We refer to this approach as the *revealed preference approach.*

To infer preferences over medical debt, we need to impose two additional simplifying assumptions. First, and building on the graphical analysis, we use a linear approximation to the marginal utility function around b^*. This implies that we only need to characterize the first and the second derivative of the disutility of medical debt. Second, and supported by our data, we assume that the fraction of unpaid bills, τ is "locally" constant: $\frac{b^*}{\epsilon_{MB}} = \bar{\tau}$.[22]. Using the first order condition and the implicit function theorem, we can then express the disutility of medical debt, and importantly the CV, in terms of the utility over consumption and the fraction of unpaid bills, see the appendix for details. In what follows we focus on the CV under increasing marginal disutilities in medical debt, $h''(\cdot) > 0$, which provides a lower bound for the case, $h''(\cdot) = 0$, see the Online Appendix for details.

An advantage of this approach is that we can also consider the comparative statics of the CV with respect to the underlying bill amount, the repayment rate, and the curvature in utility as stated in the following proposition, see the Online Appendix for proofs:

Proposition 1 *If $g'(\cdot) > 0, g''(\cdot) < 0$ and $h(\cdot) > 0, h''(\cdot) > 0$ and $b^* = \bar{\tau}\epsilon_{MB}$, then the linear approximation to the marginal utility function around b^* can be characterized as follows*

1. *The CV is given by:*

$$CV = -\phi(\cdot) + (1-\bar{\tau})\epsilon_{MB} + \sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2},$$

   where $\phi(\cdot) = -\frac{g'(\cdot)}{g''(\cdot)}$ and $\cdot = Y - (1-\bar{\tau})\epsilon_{MB}$ if $\epsilon_{MB} \leq \frac{\phi(\cdot)}{1-\bar{\tau}}$.

2. *The CV is increasing in $\phi(\cdot)$*

3. *The CV is decreasing in $\bar{\tau}$ if $\frac{g'''(\cdot)g'(\cdot)}{g''(\cdot)^2} \leq 2$ and $\epsilon_{MB} < min\{\frac{\phi(\cdot)}{\bar{\tau}+\frac{1}{8}}, 4\phi(\cdot)\}$*

4. *CV over ϵ_{MB} is decreasing in the medical bill amount if $\frac{g'''(\cdot)g'(\cdot)}{g''(\cdot)^2} \leq 1 + \frac{\phi(\cdot)}{1-\bar{\tau}}$.*

The proposition shows that the CV can be expressed in terms of three objects: the inverse curvature of individuals' consumption utility, $\phi(\cdot)$ the share of unpaid medical bills, $\bar{\tau}$, and the size of the medical bill, ϵ_{MB}.[23] More specifically, the CV is decreasing in the curvature

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[22] In contrast, $h''(\cdot) = 0$ implies quasilinear preferences. In this case, individuals repay medical bills up to given amount and borrow the rest in the form of medical debt. In the data, we observe that consumers choose to repay a positive (relatively constant) portion of their medical bills, which is inconsistent with quasilinear preferences.

[23] The condition $\epsilon_{MB} \leq \frac{\phi(\cdot)}{1-\bar{\tau}}$ requires that the extrapolated marginal utility of consumption at $c = Y$ is weakly greater than zero.

of consumption utility. Holding the repayment rate fixed, the implicit function theorem reconciles less curvature in consumption with less curvature in the disutility of medical debt. Graphically speaking, a decrease in curvature flattens out both marginal utility curves in Figure 10. This reduces the value of borrowing and hence raises the CV. For example, as $g''(\cdot)$ converges to zero, both marginal utility curves become horizontal and the CV converges to ϵ_{MB}.

Furthermore, the CV decreases in the share of unpaid medical bills $\bar{\tau}$, provided minimal curvature and sufficiently small medical bills as outlined in the proposition. An extreme case is $\bar{\tau} = 0$, in which case medical bills are fully repaid, the CV equals ϵ_{MB}. Intuitively, there are two reasons for this finding. First, a decrease in $\bar{\tau}$ raises out-of-pocket spending and hence the CV. Second, a decrease in $\bar{\tau}$ signals that additional medical debt is costly from the point of view of the patient (otherwise a higher fraction of medical bills would go unpaid). This also raises the CV. Finally, the ratio of CV over the medical bill, ϵ_{MB}, decreases in ϵ_{MB}, provided minimal curvature as outlined in the proposition. This suggests that the credit channel is relatively more important for smaller medical bills.

Overall, the analysis suggests that considering the reduction of unpaid medical bills can increase the CV from $(1-\bar{\tau})\epsilon_{MB}$ to ϵ_{MB}, a factor of $\frac{1}{1-\bar{\tau}}$. This can be quite large given that uninsured patients pay only about $1-\bar{\tau} = 20\%$ of health care services out-of-pocket. We revisit the CV in a numerical example in Section 9.

8.3 Variance Reduction and Consumer Welfare

Next we turn to the effects of the reduction in the variance of medical bills on consumer welfare, which corresponds to the value of risk protection. To this end, we reintroduce uncertainty in medical bills and quantify the risk premium RP, which isolates the the financial benefits from a variance reduction in medical bills. The risk premium is implicitly defined by the following equation:

$$EU = g(Y - (1-\bar{\tau}) \cdot \bar{\epsilon}_{MB} - RP) - h(\bar{D} + \bar{\tau} \cdot \bar{\epsilon}_{MB}) \ ,$$

where EU denotes expected utility, $\bar{\epsilon}_{MB}$ denotes the average medical bill, and $\bar{\tau} \cdot \bar{\epsilon}_{MB}$ is the average increase in medical debt.

To quantify the risk premium, we consider a second order Taylor approximation to consumer utility, evaluated at average medical bills $\bar{\epsilon}_{MB}$ holding the repayment ratio $(1-\bar{\tau})$ fixed. We again calibrate utility over consumption, and leverage the first order condition and the implicit function theorem to express $h'(\cdot)$ and $h''(\cdot)$ in terms of $g'(\cdot)$, $g''(\cdot)$, and $\bar{\tau}$.

Finally, we can implicitly express RP as follows, see the Online Appendix for details:

$$\begin{aligned} & g(Y-(1-\bar{\tau})\cdot\bar{\epsilon}_{MB})-g(Y-(1-\bar{\tau})\cdot\bar{\epsilon}_{MB}-RP) \\ = \; & -\frac{1}{2}\cdot(1-\bar{\tau})\cdot g''(Y-(1-\bar{\tau})\cdot\bar{\epsilon}_{MB})\cdot var(\epsilon_{MB})\ . \end{aligned} \tag{5}$$

We then benchmark the derived risk premium to its counterpart in a more simplistic model, which ignores the role of unpaid medical bills: $h(\cdot)=0$. We refer to the risk premium from this out-of-pocket benchmark model as RP^{oop}. Based on a second order Taylor approximation, first order condition, and implicit function theorem, we derive an analogues implicit condition for the risk premium RP^{oop}:

$$\begin{aligned} & g(Y-(1-\bar{\tau})\cdot\bar{\epsilon}_{MB})-g(Y-(1-\bar{\tau})\cdot\bar{\epsilon}_{MB}-RP^{oop}) \\ = \; & -\frac{1}{2}\cdot(1-\bar{\tau})^2\cdot g''(Y-(1-\bar{\tau})\cdot\bar{\epsilon}_{MB})\cdot var(\epsilon_{MB})\ . \end{aligned} \tag{6}$$

Comparing equations (5) and (6), we find that:

$$RP^{oop} < RP < \frac{1}{1-\bar{\tau}}\cdot RP^{oop}\ ,$$

suggesting that considering unpaid medical bills can increase the risk premium by factor of $\frac{1}{1-\bar{\tau}}$. We quantify the risk premium in a numerical example in Section 9.

9 Overall Effects of Medicaid on Financial Health

In this last section, we quantify the consumer welfare gains from reductions in paid and unpaid medical bills. We start with the revealed preference approach before discussing the findings from the direct approach.

9.1 Revealed Preference Approach

We begin with a numerical analysis of the mean reduction of unpaid medical bills. To this end, we consider CRRA utilities with parameters of relative risk aversion ranging between 2 and 4. Following (Finkelstein, Hendren and Luttmer, 2015) we normalize income to 3,800. We assume that patients pay 20% of the original medical bill out-of-pocket. Motivated, by the direct evidence on reductions in medical debt, we also assume that 40% of medical bills go as charity care, such that individuals are only held responsible $1-\alpha_{charity} = 0.6$ of medical bills.[24]

In Figure 11, we plot the ratio of the implied compensating variation (CV) over the corresponding medical bill ($\frac{CV}{Medical\ Bill}$) (vertical axis) against the underlying medical bill (horizontal axis). As implied by the model, this ratio decreases from a maximum of 60% for small bills to $1-\bar{\tau} = 0.2$ for large bills. Moreover, $\frac{CV}{Medical\ Bill}$ is convex in the underlying medical bill amount suggesting that evaluating the ratio at the average medical bill amount wound understate the expected $\frac{CV}{Medical\ Bill}$ when considering the full distribution in medical bills. Evaluated at $\theta = 3$, this ratio exceeds 50% (30%) for medical bills worth less than $1,000 ($5,000). Our previous estimates suggest a medical debt reduction of about $920 per treated person, which corresponds to a raw bill of $\frac{\$920}{0.40} \approx \$2,300$. At $2,300 this ratios exceeds 44%. The calibration thus implies that restricting consideration to reductions in out-of-pocket payments may understate the effects on consumer welfare by a factor of $\frac{44\%}{20\%} = 2.2$.

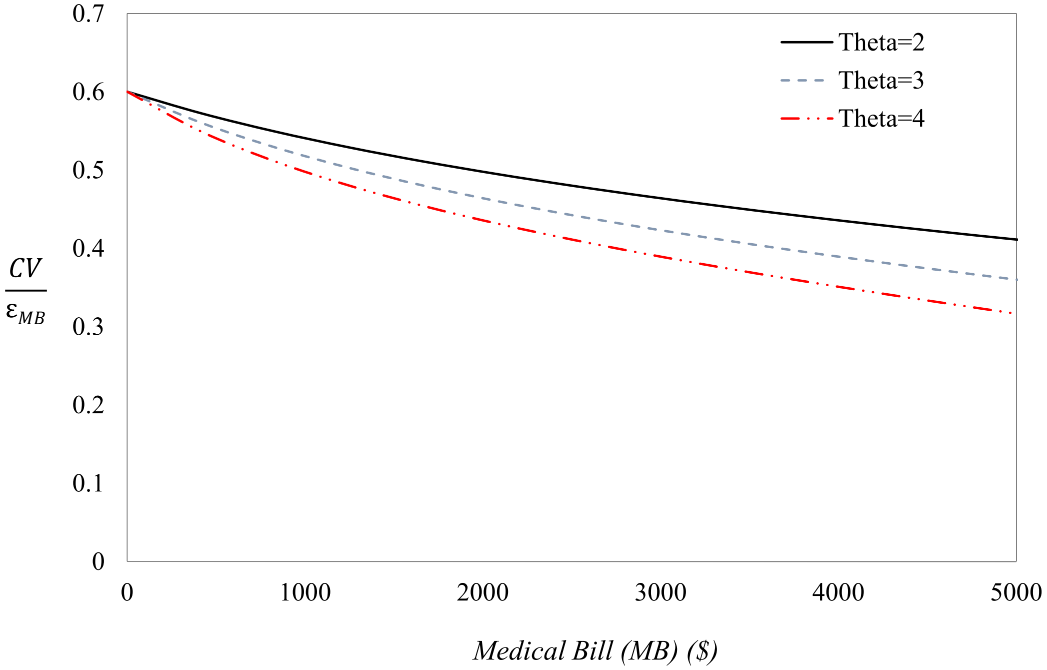

Figure 11: Compensating Variation/Medical Bill by Medical Bill

Using the above calibrated factor of 2.2, an associated parameter of risk aversion of 3, and considering overall annual health care spending of $2,400 per uninsured non-elderly person

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[24] We find that medical debt is reduced by about $920 per treated person, which corresponds to roughly 40% of overall health care utilization. Adding 20% of out-of-pocket spending suggests that the remaining 40% is treated as charity care.

(see Section 2.2), we calculate out-of-pocket spending and implied compensating variation of \$480 and $\$480 \times 2.2 = \$1,056$, respectively. This suggests an indirect benefit through the credit channel of \$1,056-\$480=\$576. These results are detailed in column 1 of Table 6.

Table 6: Overall Annual Financial Benefits

	Revealed Preference (1)	Direct Approach (2)
Mean Effects		
Credit Channel (Indirect)	576	283
Out-of-Pocket (OOP) Spending (Direct)	480	480
Compensating Variation (CV)	1,056	763
Ratio: $\frac{CV}{OOP}$	2.2	1.6
Variance Effects		
Risk Premium (RP)	600	(600)
Risk Premium OOP Benchmark (RP OOP)	240	(240)
Ratio: $\frac{RP}{RP\ OOP}$	2.5	(2.5)
Total Benefit	1,656	1,363
Total Spending (Coughlin, 2014)	2,400	2,400
Ratio: Benefit/Spending	0.69	0.57

Risk averse consumers are also willing to pay a premium for a reduction in risk. We evaluate this risk premium based on equation (5) around average annual consumption of \$3,300 and consider a standard deviation in consumption of \$768 as in (Finkelstein, Hendren and Luttmer, 2015).[25] We need to make two adjustments to equation (5) to take the role of charity care into account. First, we replace the variance in the medical bill, $var(\epsilon_{MB})$, by the variance in non-charity care, $var(\epsilon_{MB}^{non-charity})$. Second, we need to adjust the out-of-pocket spending ratio $(1-\bar{\tau})$ to express out-of-pocket spending relative to the bill amount that is not covered by charity care. Since only 60% of the medical bill is held against the patient (40% is charity care), we replace $(1-\bar{\tau})$ by $(1-\bar{\tau}_{non-charity}) = \frac{0.2}{0.6} = \frac{1}{3}$. To quantify the variance in non-charity care, we build on the observation that only one third of the non-charity care amount is paid out-of-pocket. Specifically, the variance in consumption then equals $(1-\bar{\tau}_{non-charity})^2 \times var(\epsilon_{MB}^{non-charity})$. Leveraging the observed variance in consumption allows us to pin down the variance in non-charity care. Solving for RP in the revised equation

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[25] The consumption level corresponds to income net of average out-of-pocket spending: $\$3,800 - \$480 \approx \$3,300$.

(5), we find a risk premium of \$600, which exceeds the pure OOP benchmark, building on a revised equation 6, by a factor of 2.5 (column 2 of Table 6). Combining the estimates, we find an overall annual financial benefit of \$1,656, about 69% of overall medical spending, which exceeds the out-of-pocket benchmark by a factor of 2.3.

9.2 Direct Approach

We benchmark our calibration to our direct estimates presented above. The estimated indirect benefits from reduced costs of credit equal $\frac{\$8.19+\$2.81+\$0.22+\$0.40}{0.041} = \frac{11.62}{0.041} = \$283$ per year (column 2 of Table 6). Combined with the reduction in out-of-pocket spending, we calculate a compensating variation of \$763, which exceeds the out-of-pocket reduction by 60%. These findings are a bit smaller than the results from the revealed preference approach, which is sensible because the direct approach ignores other benefits from a reduction in unpaid bills such as reduced hassle costs with collection agencies, reduction in costs related to bankruptcy filing, and improved credit offer rates.

When compared to the overall reduction in medical debt, the estimated credit channel (indirect) benefit is valued at $\frac{\$11.62}{\$37} = \$0.31$ per dollar of reduced medical debt. Taking repayments of medical debt on the order of 8% into account (Table 5), we find a total financial benefit of a reduction in unpaid medical bills of about $\$0.31 + \$0.08 \approx \$0.39$ per dollar of reduced medical debt. Unfortunately, our direct approach does not yield an estimate for the risk premium. Therefore, we borrow the corresponding estimates from the revealed preference approach to calculate an overall annual financial benefit of $\frac{\$1{,}363}{\$2{,}400} \approx 57\%$ of overall medical spending. This exceeds the out-of-pocket benchmark by a factor of 1.9.

9.3 Other Insurance Value

The above suggests that, absent changes in health care utilization, individuals may not be willing to buy Medicaid insurance even when offered at a fair premium.[26] This may be because of charity care and the option to not pay the medical bill, including the option to file bankruptcy, provide implicit insurance over of health care spending. Dividing the CV by overall medical spending, ignoring the risk premium, we find an effective price of only 40 cents per dollar, suggesting that perhaps charity care and default options insure about 60% of health care spending.

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[26] This finding is consistent with the results in (Finkelstein, Hendren and Luttmer, 2015) and (Finkelstein, Hendren and Shepard, 2017).

We revisit the role of charity care and medical debt in two thought experiments. In the first, we use the revealed preference approach to calculate the benefit-spending ratio in the absence of charity care, holding constant utilization and the proportion of the bill paid out of pocket (%20). Our model implies that $\frac{CV}{Medical\ Bill}$ now increases to 89%, or that the out-of-pocket spending would understate the CV by a factor of 89%/20%=4.45. The implied CV and risk premium equal \$2,136 and \$849, respectively. This leads to a total benefit of $\$2,136 + \$849 = \$2,985$, which now exceeds overall health care spending by 24%.

In the second, we consider one possible mechanism for the net value of unpaid medical bills: the insurance value of bankruptcy protection. Medical debt can be discharged in bankruptcy proceedings (Mahoney, 2015) which may explain why patients value a one dollar reduction in medical debt at only 51 cents. However, we find that that subprime borrowers discharge on average only \$860 per bankruptcy filing, see Table 7. Considering an annual reduction of about 25,000 bankruptcies, see Section B.4 for details, this can account for only about $\$860 \times 25,000 = \$21.5m$ in medical debt or 1% of the overall reduction in medical debt. However, we note that the marginal filers, who were affected by the Medicaid expansion, may hold considerably more medical debt. If so, the \$21.5m estimate provides a very conservative estimate of the potential insurance value of bankruptcy protection.

Overall, this suggests that charity care is more important in explaining low valuations of health insurance than the option to default.

10 Conclusion

More than half of the uninsured in the U.S. report difficulties paying their medical bills and pay on average only about 20% of overall health care utilization out-of-pocket. If the residual 80% of utilization are provided as charity care, then the out-of-pocket payments provide a good estimate of the financial cost of health care utilization for the uninsured population. In practice, however, a large fraction of unpaid medical bills goes into collection, which may have profound negative effects on these individuals' financial health, through access to and terms of credit. This suggests that the incidence of unpaid medical bills (uncompensated care) at least partially falls on the low-income uninsured patients themselves, through an indirect credit channel.

In this paper, we quantify the financial benefits of health insurance, including the indirect benefits through the credit channel, in the context of the Medicaid expansion provision under the Affordable Care Act (ACA). Combining state-level variation between adopting and non-adopting Medicaid expansion states with a nationally representative panel of 5 million credit reports, we find that the expansion reduced households' medical debt in collection by $3.4 billion in its first two years. This corresponds to an annual reduction of about $920 per treated person or about 40% of overall health care spending. We further find that the Medicaid expansion significantly reduced debt delinquencies and led to higher credit scores for consumers. Using data on loan pricing, we document that improved financial health led to better terms of credit for individuals in treated states. We then simulate a debt refinance given improved credit conditions and calculate annual interest rate savings of about $520 million, which is about 60% of the reduction in out-of-pocket spending. Overall, we find that the financial benefits of health insurance double when considering the indirect benefits of improved terms of credit in addition to reductions in out-of-pocket payments. Our estimates also suggest that beneficiaries value reductions in medical debt by about 40 cents per dollar in face value.

Finally, we find that uninsured patients pay effectively 32 to 44 cents per dollar of health care utilization, divided about equally between changes in direct out-of-pocket and indirect interest rate payments. This suggests that charity care and the ability to not pay medical bills (or borrow) effectively insures over 60% of health care spending. As a result, beneficiaries value Medicaid insurance only at about 65% of health care spending.

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A Collections, Debts, & Distress: An Event Study

In this section we discuss the relationship between medical collections and financial distress. In doing so we provide further detail on the analysis in Section 6. Our approach closely follows the event study methodology in Dobkin et al. (2016) which tracks how individuals' financial outcomes fare following a hospital admission. As we do not observe hospitalization, we replace the event of hospital admission with reporting of a large new medical collection ($\$1,000$). Large new collections are likely associated with hospitalization for uninsured individuals.

There several differences between a hospital admission and a medical collection. For example, new collections are generally not reported for up to 180 days following services rendered. Moreover, not all hospital admissions result in patients having their unpaid medical bills sent to collections. However there are also similarities, especially when considering uninsured individuals. As such, in addition to illustrating the relationship between collections and distress, we benchmark our event study results to those in Dobkin et al. (2016).

We subset our sample to include only large collections, which likely result from hospital admissions. Each individual in our sample received at least one collection valued at more $\$1,000$ prior to January 1, 2014. We then follow each of these individuals from six quarters prior to receiving the collection and for eight quarters, or two years, following the event. We use a non-parametric methodology similar to Dobkin et al. (2016) as follows:

$$y_{ict}^k = \alpha_c^k + \eta_t^k + \sum_{r=S}^{r=-2} \beta_r^k + \sum_{r=0}^{r=F} \beta_r^k + \epsilon_{ict}^k \tag{7}$$

where y_{ict}^k denotes the respective outcome k for record i in census tract c during year-quarter t, such as delinquency. As in equation 1, the specification includes tract fixed effects α_c^k and quarter-year fixed effects η_t^k. The key parameters of interest are the β^r, which are indicators for time relative to having a collection placed on the record. Outcomes are normalized to the end of the quarter just prior to a collection being placed on the account. All analyses allow for an arbitrary variance-covariance matrix at the Census Tract level.

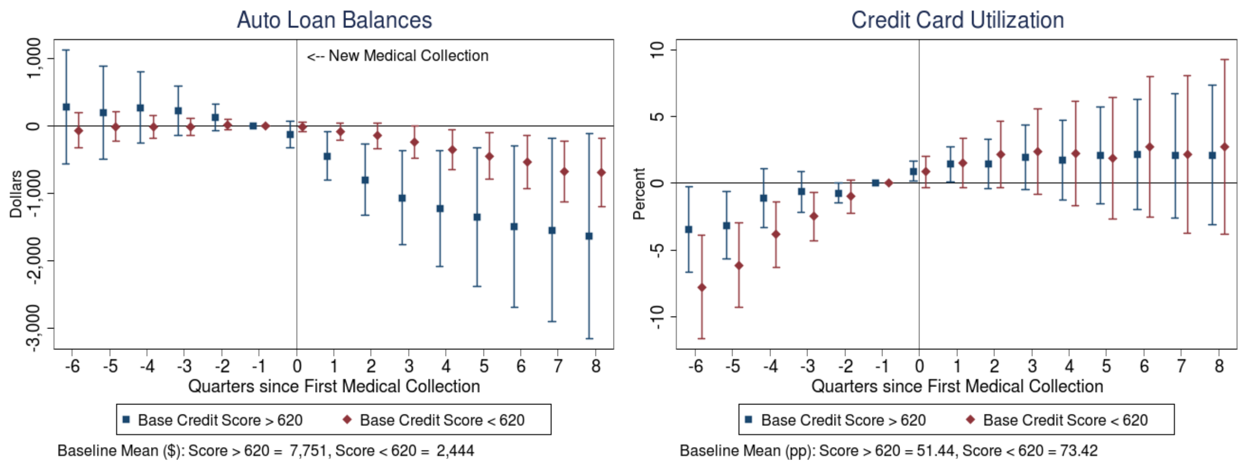
Figure 12 plots the raw β_r^ks and their respective confidence intervals. The figure plots these for medical collection balances (left panel), serious delinquencies (middle panel), and credit scores (left panel) separately for individuals with base credit score < 620 and > 620, or subprime and prime borrowers, respectively. As shown in the figure, following a new collections, and by construction, individuals' collections balances increase substantially. Nevertheless, this increase in medical debt is long lasting, as the high level of medical collections

Figure 12: Event Study: Credit Worthiness (By Risk)

Notes: *The figure shows how ’healthy’ individuals who receive a medical collection fair in the eight quarters (2 years) following the event. It does so along three dimensions: (1) Overall medical collections balances (left panel) (2) serious (90 day or more) delinquency (middle panel) (3) credit score (right panel). Serious delinquency is defined as the individual ever having become delinquent on a non-medical credit line, or debt, by that quarter. Data are from the CFPB’s Consumer Credit Panel, which is described in detail in section 3. The figure includes only individuals who received large collections prior to January 1, 2014. Effects are as of the end of each quarter and are normalized to the quarter just prior to the first collection an individual receives on their record (the event). All regressions (Equation 7) include Census tract and year-quarter fixed effects. Confidence intervals in the figure are calculated using standard errors clustered at the Census tract level. For estimation details see appendix A.*

balances remains on individuals’ accounts for at least 2 years after the first one is reported. This is true for both prime and subprime consumers. As might be expected, following a new medical collection, loan delinquency rates increase dramatically. However, in contrast to medical debt balances, there is a stronger surge in delinquency for prime borrowers. This is likely because prime borrowers’ base levels of delinquency are low to begin with, whereas subprime borrowers are likely troubled by delinquencies prior to receiving a new medical collection. It follows that a new collection also dramatically reduces borrowers’ credit scores, and that this effect is much greater among prime borrowers. As is shown in the figure, credit scores begin to fall prior to the collection, likely because the actual health event, and distress resulting from it, begin some time before a medical collection is placed on individuals’ records. However, there is a substantial drop just after the first collection is reported which persists for several years following. This is likely a direct result of the new collections account, which is used by credit scoring companies to help predict future delinquencies.

Figure 13 plots coefficients β_r^k for auto loan balances (left panel) and credit card utilization (right panel) for prime and subprime borrowers, respectively. From the figure we find that, as



Baseline Mean ($): Score > 620 = 7,751, Score < 620 = 2,444

Baseline Mean (pp): Score > 620 = 51.44, Score < 620 = 73.42

Figure 13: Event Study: Non-Medical Debt (By Risk)

Notes: *See notes in Figure 12.*

in Dobkin et al. (2016), auto loan balances decline following a new collection being reported. This is consistent with individuals having lower income and fewer borrowing options, being unable to either refinance their car loans or make new purchases. Two years following a collection, their balances are nearly $1,500, or about 20%, lower than just prior to the event. Consistent with this story, we find that credit card utilization increases in the quarters up to and for almost two years following the event. As large medical collections are spurred on by adverse health events, it is likely that individuals use unsecured credit lines to smooth out consumption during these bad times. Moreover, signaled financial distress likely restricts the availability of credit to these individuals, leading them to draw further into their already available credit.

In all, these figures suggest that individuals who have a large medical collection placed on their account become financial distressed in the two years following this event. This is signaled by their increased delinquency and significantly reduced credit scores. Moreover, this greater distress leads to poorer borrowing options, as indicated by their lower auto loan balances and increased credit card utilization rate.

B Robustness and Empirical Appendix

B.1 Robustness: Other Collections & Federal Exchanges

Figure 14 plots trends in non-medical collections. To the extent that reduction in medical debt is driven by increased insurance rates reducing unpaid medical bills, trends in non-

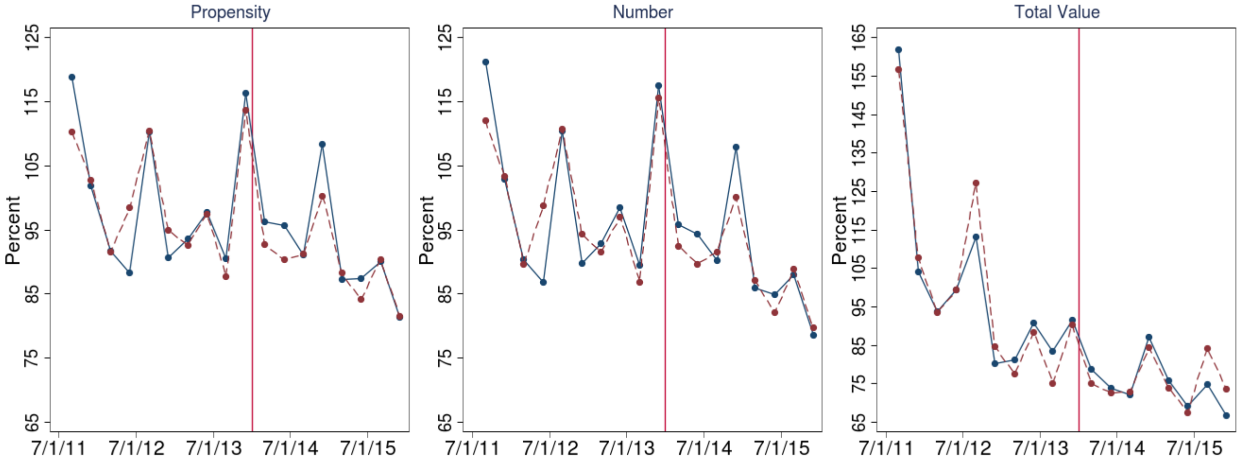


Figure 14: Trends in Newly Accrued Non-Medical Collections

Notes: *The figure shows trends in the incidence, frequency, and value of newly accrued non-medical collections. Data are from the CFPB's Consumer Credit Panel described in section 3. Trends are quarterly means of newly accrued non-medical collections for treament and control states, respectively, and are normalized by the pre reform mean for each group. Vertical lines highlight the implementation date of the expansion - January* 1^{st}*, 2014.*

medical collections should not differ in treatment states relative to control following the reform. Indeed, we note no evidence of differences in trends of non-medical collections for treatment states relative to control following the reform. We conclude that there was no systematic change in overall collections activity driving the reduction in medical debt accruals. Rather, reductions in unpaid medical bills sent to collections are a result of newly-insured households not generating newly-unpaid medical bills following unexpected adverse health events.

Figure 15 plots trends in medical collections for states opening insurance exchanges using the federal platform. Other factors governing medical debt may be associated with the opening of the exchanges and, specifically, platform choice among states. To account for these factors, we subset our sample to include only states that adopted the federal platform. In other words, for these states, all individuals using the exchanges did so on the same platform.

We find that this pruning does not materially alter our results. For the most part, we see that medical collection declines dramatically in propensity, number, and volume across treatment and control states all of which opted to use the federal platform for the exchanges. Moreover, the magnitudes are quite similar when considered alongside the full sample.

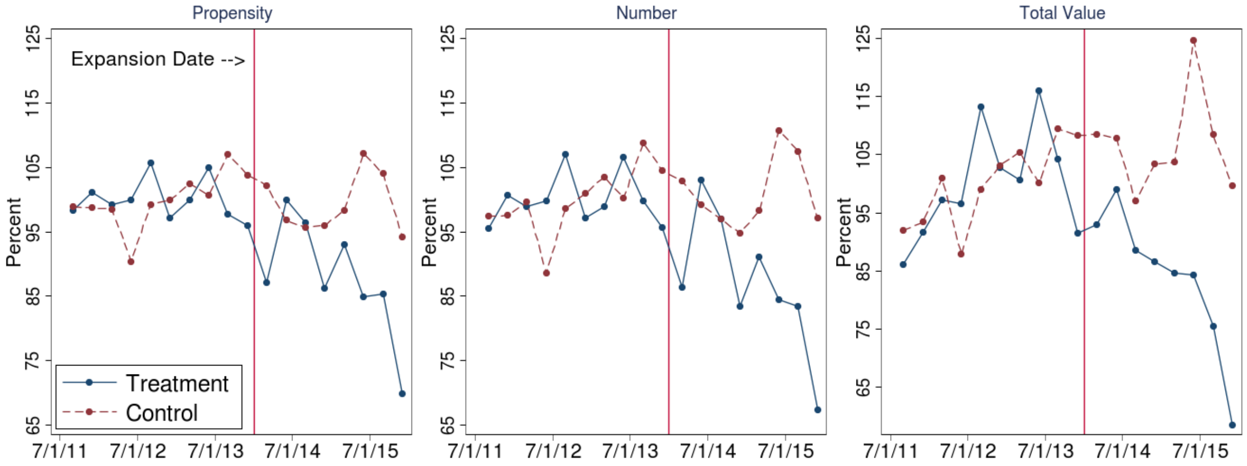


Figure 15: Newly Accrued Medical Debt for States Running Federal Exchanges

Notes: *The figure shows trends in the incidence, frequency, and value of accrued medical debt. Data are from the CFPB's Consumer Credit Panel described in section 3. Trends are quarterly means of newly accrued medical loans for treament and control states, respectively, and are normalized by the pre reform mean for each group. Vertical lines highlight the implementation date of the expansion - January* 1^{st}*, 2014.*

B.2 Reductions in Credit Card Debt

Often individuals pay medical bills using their credit cards. This is true at a private doctor's office as well as in a hospital. Although we do not observe the source of debt on credit cards in the CCP, we may expect that the Medicaid expansion's effect on credit card debt may have flowed through a reduction in the payment of medical expenditures for newly insured individuals. Figure 16 plots trends in credit card balances for consumers in adopting (treatment) and non-adopting states (control). As shown in the top panel of the table, credit card balances on average declined by about 1.9% for individuals in treatment vs. control states in the two years following the reform. We interpret this decline as the overall per-person reduction after 4 quarters, the mid-point of the post-reform period, given that the negative effect on non-medical debt is gradually growing in magnitude over time. The Moreover, the bottom right panel of the table shows that this decrease was proportionally greater in poorer communities with higher Medicaid eligibility rates. The level reduction, however, was greater in richer communities, where it is likely that individuals had more generous credit lines from which to borrow to pay for medical services.

Under the assumption that the observed reduction in credit card debt resulting from to the expansion is entirely due to reduced out-of-pocket payment of medical bills, we calculate a reduction in out of pocket payments from reduced credit card debt to be $0.0186 \cdot \$4,026 = \$74.88$ per person, or approximately \$3.8 billion.

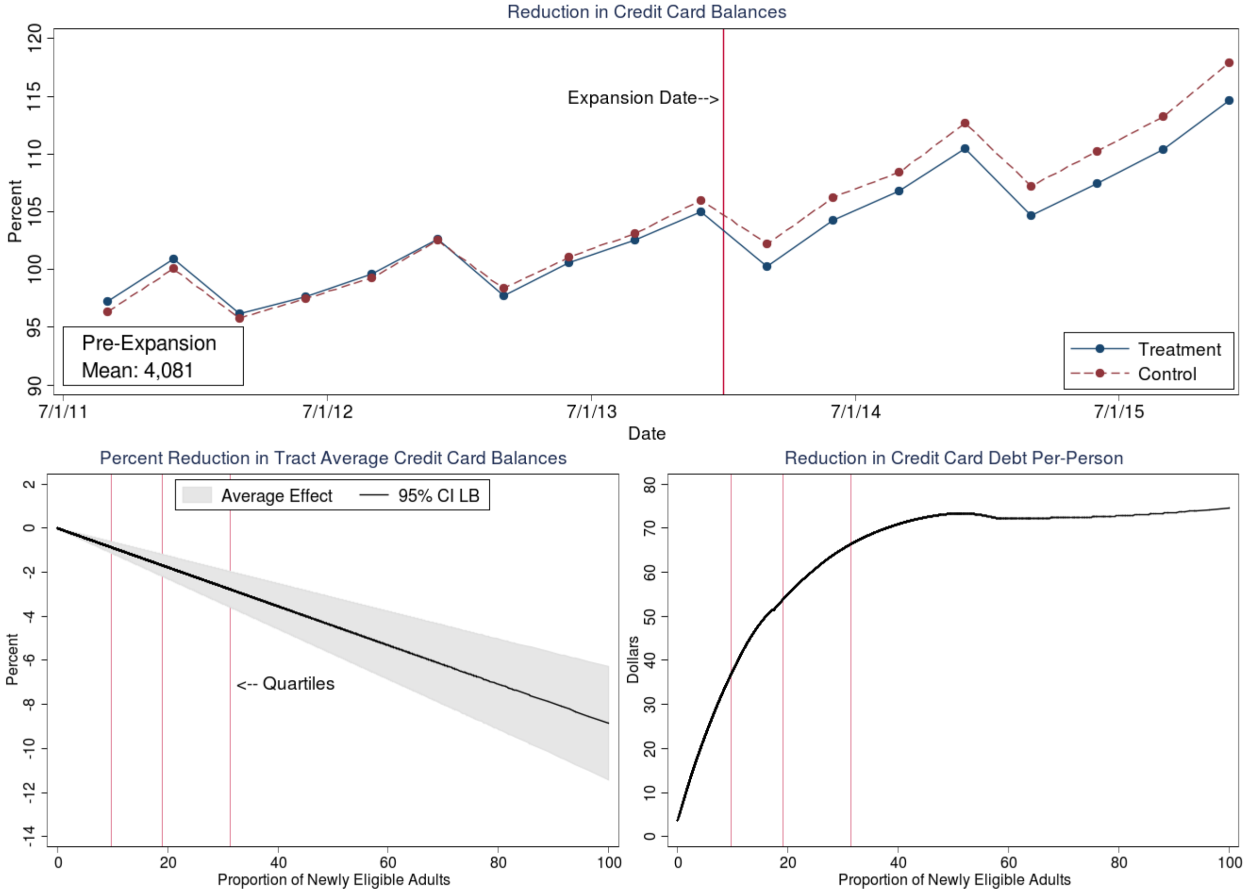


Figure 16: Effects of the Medicaid Expansion on Credit Card Balances

Notes: *The figure shows trends in the credit card balances. Data are from the CFPB's Consumer Credit Panel described in section 3. Trends are quarterly means in the level of credit card balances for treatment and control states, respectively, and are normalized by the pre-reform mean for each group. The vertical line in the top panel highlights the implementation date of the expansion - January* 1^{st}*, 2014. Trends exclude extreme outliers (*$\sim 95^{th}$*Pctl.)in credit card balances, which are likely not affected by the reform. The DiD estimate is the from a regression of the log average balance in Census tract c in quarter t and includes Census tract and quarter year fixed effects. Standard errors are clustered at the tract level.*

B.3 Access to Credit

In this Section, we present evidence on the reform's effect on access to credit card debt and personal loans using the Mintel data. Figure 17 provides evidence on the share of adults that receives any new credit card offer in the given quarter. The left panel provides suggestive evidence for an increase in the offer rate in treatment states following the expansion. This is supported by the right panel, which provides analogous regression based evidence based on our primary empirical difference-in-difference specification.

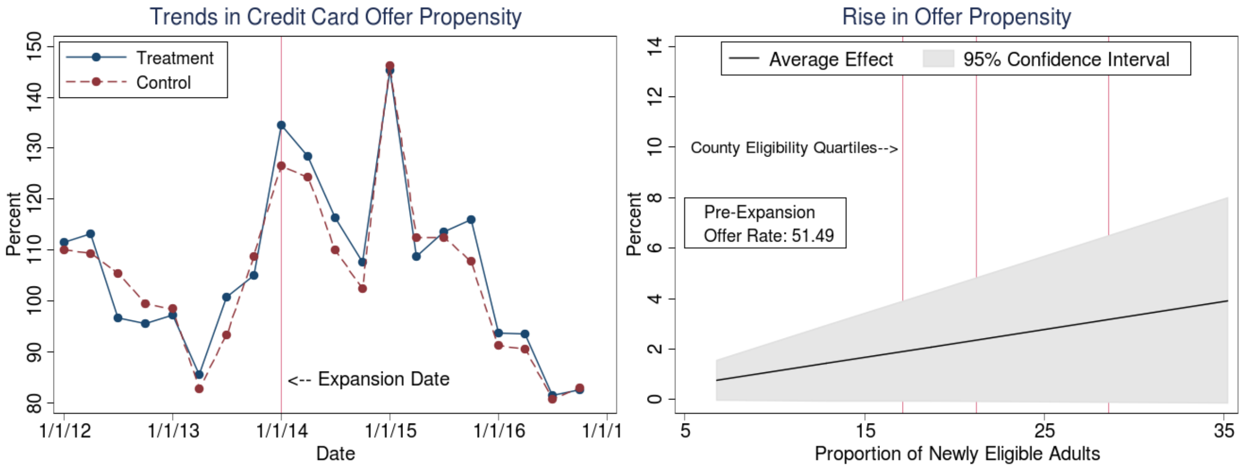


Figure 17: Effects of the Medicaid Expansion on Access to Credit Cards

Figure 18 presents the analogous results for personal loans. Here the evidence is mixed. While the left panel suggests an increase, the right panel suggests a decrease in offer rates.

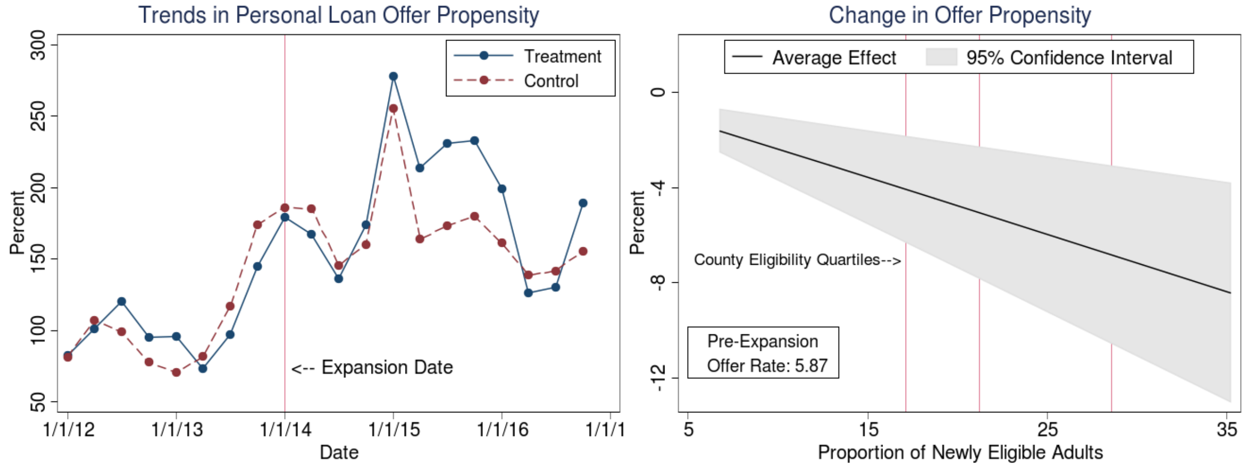


Figure 18: Effects of the Medicaid Expansion on Access to Personal Loans

Overall, we interpret these results as supportive evidence for an increase in access to credit because credit card debt is a common form of debt among poor households that benefit from the Medicaid expansions.

B.4 Bankruptcy

Another measure of financial distress, often discussed in the context of medical expenditures, is bankruptcy, or insolvency. In the U.S., individuals most commonly file for bankruptcy under Chapter 7 or Chapter 13, the former being about twice as common. Under Chapter 7, a filer can discharge nearly all debts. However, the filer is required to relinquish any of their non-exempt assets.[27] Once the debts have been discharged, the consumer is given a fresh start and not required to make any additional payments out of her future income. In contrast, Chapter 13 is geared towards consumers with wage incomes who are permitted to retain their assets but must enter into a repayment plan. Under repayment only a portion of debts are discharged. Chapter 13 bankruptcy has the additional requirement that creditors must receive at least as much from the repayment plan as they would have by liquidating the debtor's assets in a Chapter 7 bankruptcy.

In Table 7, we provide summary statistics on the debt distribution of bankruptcy filers. About of third of bankruptcy filers hold medical debt, worth, on average, \$2,000. The average, however, masks substantial heterogeneity. The top 1% of filers with medical debt aim to discharge nearly twelve times that amount, or \$24,000, suggesting that medical debt may be an important contributor to bankruptcy filing. More generally, bankruptcy filers hold about twice as much unsecured non-medical debt as the average consumer (Table 1), with prime filers holding slightly more. This is expected, given that filers benefit from discharging unsecured debt. Conversely, we do not find clear evidence for differences in secured debt, such as mortgage loans or other non-mortgage debt, which is plausible, given that filers would also lose some underlying assets.

The previous comparison indicates a positive correlation between unsecured debt and bankruptcy filing. We now revisit this mechanism using the Medicaid expansion, which shields beneficiaries from accruing new unsecured medical debt. Figure 19 shows normalized trends in bankruptcy rates for consumers in treatment and control states around the time of the expansion. Each panel also shows results from a DD regression of the form

$$\mathbb{1}[AnyFiling]_{ict} = \alpha_c + \eta_t + \beta \cdot (Adopt \times Post) + \epsilon_{ict}. \tag{8}$$

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[27] Some debts may be ineligible to be discharged under Chapter 7. Most notably, student loans and taxes cannot be discharged without the debtor showing undue hardship. The size of the asset exemption varies across the states, the only part of bankruptcy law that is not uniform nationwide (White, 2006). Many states also have different exemptions for a debtor's principle residence and for other types of personal property. Secured debts may also be discharged if the debtor gives up the collateral securing the loan.

Table 7: Debt at Bankruptcy

	All (1)	(Base Credit Score < 620) (2)	(Base Credit Score > 620) (3)
Percent Filing with Medical Debt	33.32	40.93	13.63
Medical Debt at Filing			
Mean if Medical Debt >0	1,976	2,025	1,585
Median	550	569	392
75^{th} Pctl.	1,553	1,614	1,023
90^{th} Pctl.	3,919	4,142	2,785
99^{th} Pctl.	23,413	23,385	24,844
Other Debt at Filing			
Credit Cards	8,171	7,149	10,905
Personal Loans	1,138	986	1,531
Auto Loans	4,874	4,197	6,628
Mortgages	48,194	41,832	64,651

Notes: *This table shows debt portfolios of individuals declaring bankruptcy. The data are from the CFPBs CCP and include only pre-expansion filings (before January 1, 2014) among those living in Medicaid expansion states (Figure 1). Debt figures include also debt that has been charged off by the lenders. Column 1 shows debt portfolios among all filers. Columns 2 and 3 show debt portfolios among subprime and prime filers, respectively.*

where α_c are Census tract fixed effects and η_t are quarter-year fixed effects.[28] Like our analysis of financial distress, we distinguish the effects of the policy for consumers with credit scores of 620 or above (left panel) or below 620 (right panel). As illustrated in the figure, the Medicaid expansion had little effect on the likelihood of filing for bankruptcy among consumers with baseline credit scores of 620 or higher. For this more resilient group, overall filing rates are low and do not seem influenced by the expansion. In contrast, among financially vulnerable consumers, with baseline credit score of less than 620, the Medicaid expansion reduced the quarterly rate of bankruptcy filings by a substantial 0.03 percentage points, or 8% of the pre-expansion mean. Given our sample frame, this translates into approximately 50,000 fewer bankruptcies over the first two post-reform years.[29]

To put our estimates into perspective, Mazumder and Miller (2016) find that the Massachusetts health reform reduced bankruptcy filing by 0.08 percentage points over two years

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[28] For the bankruptcy analysis we depart from the functional form in the main paper, Equation 1. This is because bankruptcies are somewhat rare and much lagged.

[29] The above are calculated from our sample and estimated coefficients as follows:

$$\Delta Bankruptcy = \underbrace{468,144}_{\#\ of\ subprime\ Records\ in\ Treament\ States} \times \underbrace{48}_{pop.\ wgt.} \times \underbrace{-0.000271}_{\Delta\ percentage\ points} \times \underbrace{8}_{post\ quarters} \approx -48,717$$

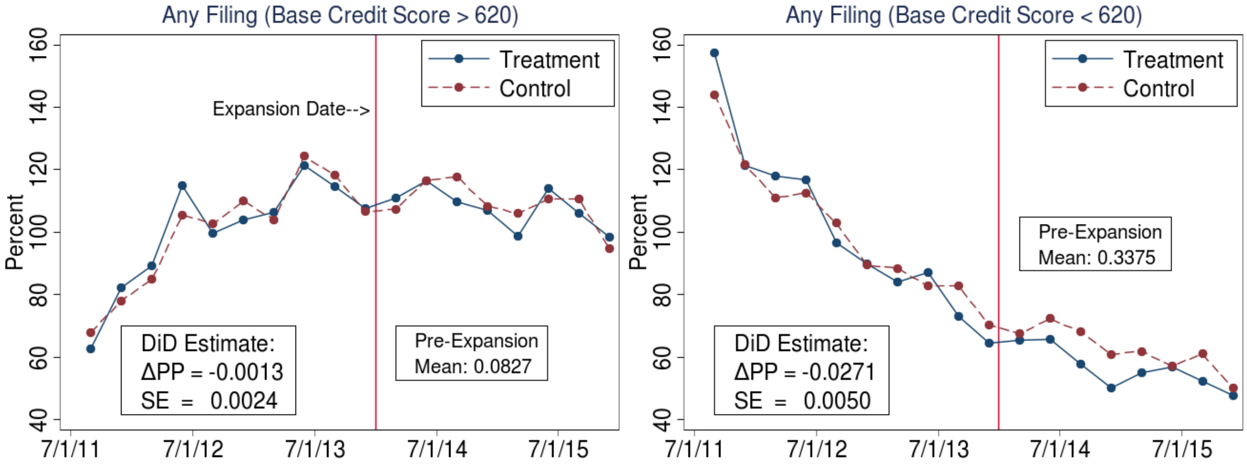

Figure 19: Effects of Medicaid Expansion on New Bankruptcy Filings

Notes: *The figure shows trends of bankruptcy rates among consumers for treament and control states, respectively. Trends are are normalized by the pre reform mean for each group. Bankruptcy is defined as a consumer having filed for Chapter 7 or Chapter 13 bankruptcy protection during a particular quarter. The left panel shows trends for consumers with a baseline credit score* ≥ 620*. The right panel shows respective filings for consumers with a baseline credit score* < 620*. Each panel also shows estimates from a DD regression as described in equation 1 in which* $\mathbb{1}[Bankruptcy\,Filing]$ *is the dependent variable. All standard errors are clustered at the Census tract level.*

per 1 percentage point increase in coverage among subprime borrowers. Our estimates are very similar in magnitude, suggesting a $8 \times 0.0255 = 0.2$ percentage point increase over two years, per 3-4 percentage point increase in coverage among subprime borrowers. This suggests a reduction of 0.05 to 0.067 percentage points over two years per 1 percentage point increase in coverage. Gross and Notowidigdo (2011) find that a 10 percentage point increase in insurance, resulting from Medicaid expansions, reduced bankruptcy filings by 8% overall. We find an 8% reduction for a 4 percentage point increase among subprime borrowers.

Overall, however, we find that medical debt plays an important role in individuals' bankruptcy decisions and that the expansion led to substantial reduction in bankruptcy. Moreover, this effect was more important for financially vulnerable consumers.

C Calculations of Simulated Decline in Monthly Bills

As described in Section 7.2, we use offer data for credit cards and personal loans from Mintel Compremedia and rate sheet pricing data for auto loans and mortgages from MyFico to estimate how the interest rates available to consumers were affected by the Medicaid expansion. In this section, we detail how we convert those interest rate changes into the savings in interest rate expenses that were available to consumers via a simulated refinancing.

First, note that a borrower i residing in Census tract c paying a monthly interest rate r_c (e.g. $\frac{APR}{12}$) with current balance $B_{i,0}$ and amortization period m (e.g. 12, 24 or 36 months) faces a monthly payment of

$$P_{i,c}(m, r_c, B_{i,0}) = B_{i,0} \cdot \frac{r_c \cdot (1+r_c)^m}{(1+r_c)^m - 1}. \tag{9}$$

As aforementioned (Section 7.2), our exercise simulates a debt refinancing as of the end of 2013Q4, just prior to the expansion. It follows that for each borrower we take B_0 to be their outstanding debt of that loan type as of that date. Moreover, in our calculations we assume fixed-payment loans with fixed interest and loan terms of 5-years for auto loans, 30-years for mortgages, and 3-years for credit cards and personal loans.[30] Because credit cards are revolving debt, they generally do not have fixed repayment terms or fixed payments. We use 3 years as an admittedly arbitrary estimate of how long it would take consumers to pay off their existing balances. Our results do not vary much if we reduce the payoff period to 1 year.

For unsecured loans, the scheduled monthly payments for a loan can overstate the expected cost to borrowers since some borrowers will fail to repay. A borrower who fails to repay an auto loan or mortgage loses the car or house backing the loan and is deprived of the flow of transportation and housing services those products provide. As a result, any money saved by not making payments will be at least partially offset by the loss of collateral. In contrast, unsecured borrowers do not surrender collateral when they default and are unlikely to face any directly offsetting expenses (though they do incur the costs of dealing with debt collectors and may have to pay higher costs for credit in the future).[31] For these borrowers, the stream of scheduled monthly payments likely overstates the cost of the loan. We therefore calculate an expected repayment amount for these loans as

$$\overline{P}_{i,c}(m, r_c, B_{i,0}, d_c) = (1-d_c) \cdot P_c(m, r_c, B_{i,0}) + d_c \cdot 0 = (1-d_c) \cdot P_c(m, r_c, B_{i,0}) \tag{10}$$

where d_c is the monthly default rate in tract c. We measure default d_c as the likelihood of having a new 90-day delinquency or worse during a month for a respective debt type

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[30] Specifically, mortgage rates are for a 30-year, fixed rate mortgage of \$150,000 on a single-family owner-occupied property with a loan-to-value ratio of 80% and 1 point in origination fees. Auto rates are for a 60-month loan of between \$10,000 and \$20,000 for a new automobile.

[31] While lenders can seek wage garnishments or other ways of compelling payment from unsecured borrowers, these options are not commonly pursued.

(e.g. credit card or personal loan). Following 90 day delinquencies, the probability of ever repaying a loan is nearly zero. Borrowers who become 30 days or more delinquent are much more likely to return to repayment. We then estimate the effects of the policy on default rates for consumers in each debt category separately using our baseline specification (Equation 1) in which the dependent variable is $y^k_{ict} = \mathbb{1}[\text{New Delinquency}]^k_{ict}$ with $k \in \{\text{Credit Card, Personal Loan}\}$. These estimates are shown in Table 8. Since our specification provides estimates for quarterly flows into delinquency (q_c), we approximate the monthly default rate as $d_c \approx \frac{q_c}{3}$.[32]

Table 8: 90 Day Delinquency For Credit Cards and Personal Loans

	Coefficient (1)	Std. Error (2)	Delinquency Rate (Quarterly) (3)	Delinquency Rate (Monthly) (4)
Credit Cards	-0.00182515	0.00086205	0.01135771	0.00378590
Personal Loan	-0.00028142	0.00114327	0.00068417	0.00002281

Notes: *This table shows effects of the Expansion on new 90 day or more delinquencies for credit cards and personal loans. Each regression is estimated using Equation 1. Standard errors are clustered by Tract. Columns 3 and 4 display the pre-reform means at the quarterly and monthly level, respectively.*

For the completion of the exercise, we must define *baseline* and *refinanced* rates and delinquencies for each of the four loan categories. Because we do not observe borrowers' individual interest rates, we assume borrowers residing in Census tract c face as their baseline the prevalent, or average, rate in their respective tract. For auto loans and mortages, expected interest rates are imputed directly into the CCP. As a result, we set borrowers' baseline rate for these products as the average imputed (monthly) rate for the respective product in their respective tract c prior to the expansion. Credit card and personal loan rates entail a further complication as they are not directly imputed in the CCP. For these products, we must take the extra step of using the Mintel data to predict a tract level interest rate for treated counties prior to the expansion as a function of county eligibility. Our estimating equations and subsequent estimates are

$$\bar{r}^{\text{CC}}_{baseline,c} = \frac{1}{12}(r_0^{CC} + r_1^{CC} \times ER_c) = \frac{1}{12}(14.41 + 6.18 \times ER_c) \tag{11}$$

$$\bar{r}^{\text{PL}}_{baseline,c} = \frac{1}{12}(r_0^{PL} + r_1^{PL} \times ER_c) = \frac{1}{12}(6.27 + 15.88 \times ER_c) \tag{12}$$

---

[32] Assuming independent in delinquency over months we have $\frac{q_c}{3} = \hat{m}(1-\hat{m})^2$ whereby $\hat{m} < m$ so our simplification in fact modestly understates net savings.

where ER_c is the proportion of newly Medicaid eligible adults (Section 3).[33] We then impute these predicted rates by tract into the CCP and define the baseline interest rate for these product as this newly imputed rate. Note that we divide by 12 to transform APRs into monthly rates, under the assumption of monthly compounding. Delinquencies are directly observed in the CCP. Consequently, we set as the baseline the delinquency rate in each tract

$$\overline{d}^{k}_{baseline,c} = \frac{1}{3} \cdot \overline{q}^{k}_{baseline,c}$$

for $k \in \{CC, PL\}$.

To determine *refinanced* rates and delinquencies, we predict counterfactuals of each using the difference in difference estimates (Figures 7, 8, and 9) as follows

$$\overline{r}^{\ell}_{refinanced,c} = \overline{r}^{\ell}_{baseline,c} + \frac{1}{12} \times \beta^{\ell} \times ER_c \tag{13}$$

for $\ell \in \{CC, PL, AUT, MTG\}$. Again, β^{ℓ} is the key difference-in-difference coefficient from equation (1). Note that we divide the DiD estimate by 12 to transform our estimated APR reduction into a monthly interest rate decline. Similarly for delinquencies, we calculate

$$\overline{d}^{k}_{refinanced,c} = \overline{d}^{k}_{baseline,c} + \frac{1}{3} \times \beta^{k} \times ER_c \tag{14}$$

for $k \in \{CC, PL\}$. Finally we define expected annual savings (ASV) to be the sum of expected monthly savings as follows

$$ASV_{i,c} = 12 \times [\overline{P}_{i,c}(m^{\ell}, \overline{r}^{\ell}_{baseline,c}, B^{\ell}_{i,0}, \overline{d}^{k}_{baseline,c}) - \overline{P}_{i,c}(m^{\ell}, \overline{r}^{\ell}_{refinanced,c}, B^{\ell}_{i,0}, \overline{d}^{k}_{refinanced,c})] \tag{15}$$

for ℓ and k as shown above.

In our simulations we calculate an average per-person annual savings. As aforementioned, these Intent-to-Treat effects on rate savings are generated using slightly different methods for the secured and unsecured loans. For our estimates on secured products, we use the entire sample. Our estimates for the unsecured products, however, were estimated conditional on receiving a credit offer. We have no information on the correlation between receiving an offer and Medicaid eligibility. Absent this information, we assume independence between these receiving an offer and Medicaid enrollment and treat our estimates as Intent-to-Treat similar to the case for secured loans. There is another interpretation of this approach.

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[33] Specifically, we regress the pre-reform interest rates on the proportion of newly eligible adults at the county level. The two numbers in brackets denote the intercept and slope estimate of the underlying regression model.

Suppose there is non-zero correlation between Medicaid enrollment and the propensity to receive credit offers. Nevertheless, all individuals with improved credit scores still qualify for new loans at an equally lower rate, were they to seek them out. This interpretation assumes zero correlation between Medicaid enrollment and eligibility for lower rates, which is a weaker and quite plausible condition. Finally, we simulate aggregate potential savings by multiplying our per person effects with the CCP Population in 2013Q4 similar to Table 5.

D Details on the Consumer Welfare Analysis

We assume that consumers have existing medical debt $\bar{D}$ and decide on the optimal amount of new medical bills $0 \leq b \leq \epsilon_{MB}$ that go unpaid, trading off utility from consumption and disutility from medical debt. Conditional on a realized medical bill, ϵ_{MB}, consumers maximize:

$$\max_{0 \leq b \leq \epsilon_{MB}} g(Y - (\epsilon_{MB} - b)) - h(\bar{D} + b)$$

where in optimality

$$F(\epsilon_{MB}, b) = g'(Y - (\epsilon_{MB} - b^*)) - h'(\bar{D} + b^*) = 0 \ . \tag{16}$$

Applying the implicit function theorem, it follows that

$$\begin{aligned} \frac{\partial F(\epsilon_{MB}, b)}{\partial \epsilon_{MB}} \Delta \epsilon_{MB} + \frac{\partial F(\epsilon_{MB}, b)}{\partial b} \Delta b &= -g'' \Delta \epsilon_{MB} + \Big[g'' - h'' \Big] \Delta b = 0 \\ \iff \frac{\Delta b}{\Delta \epsilon_{MB}} &= \frac{g''(Y - \epsilon_{MB} + b^*)}{g''(Y - \epsilon_{MB} + b^*) - h''(\bar{D} + b^*)} \in [0, 1] \end{aligned} \tag{17}$$

where we normalize $b^*(\epsilon_{MB} = 0) = 0$. It follows that a fraction $\tau(\epsilon_{MB}) \in [0, 1]$ of new medical bills remains unpaid and becomes medical debt with

$$b^* = \tau(\epsilon_{MB}) \cdot \epsilon_{MB} \ \Rightarrow \ \frac{\Delta b}{\Delta \epsilon_{MB}} = \tau'(\epsilon_{MB}) \epsilon_{MB} + \tau(\epsilon_{MB}). \tag{18}$$

Equations 16, 17, and 18 allow us to express (locally) the first and second derivative of $h(D)$ in terms of $g'(c)$, $g''(c)$, and $\tau(\epsilon_{MB})$. We return to this observation below.

D.1 Details on Compensating Variation

To gauge the transfer gain from insurance, in dollars, we quantify the compensating variation (CV) As outlined above, we assume that the demand for medical care is price inelastic. Then, if consumers do not have the option to leave bills unpaid (e.g. borrow), we trivially have

$$CV = e(p_0, u_0) - e(p_1, u_0) = e(\epsilon_{MB}, u_0) - e(0, u_0) = Y - (Y - \epsilon_{MB}) = \epsilon_{MB}$$

where $e(\cdot)$ denotes the expenditure function. If consumers can leave bills unpaid, then we have to take the substitution patterns between consumption and unpaid bills into account. The compensating variation is implicitly defined by

$$\begin{aligned} u_0 &= g(Y - (1 - \tau(\epsilon_{MB}))\epsilon_{MB}) - h(\bar{D} + \tau(\epsilon_{MB})\epsilon_{MB}) \\ u_0 &= g(Y - dc) - h(\bar{D} - dd) \end{aligned} \tag{19}$$

with

$$CV = dc - dd \geq [1 - \tau(\epsilon_{MB})] \cdot \epsilon_{MB}.$$

It follows that dc and dd correspond to the optimal reductions in consumption and unpaid bills (medical debt) if the income is reduced by CV. Under the assumption that consumers cannot take out medical debt to finance consumption, absent a new medical bill, we also have that $dd \geq 0$. The first order condition combined with, $g''(\cdot) < 0$, and $h''(\cdot) > 0$ imply that $g'(Y - dc) - h'(\bar{D}) > 0$ if $dc \geq (1 - \tau(\epsilon_{MB}))\epsilon_{MB}$. Therefore, individuals will not be willing reduce consumption in exchange for fewer unpaid bills. Hence, they optimally choose $dd = 0,\ dc = CV$. Consequently, we can rewrite the equation 19 as

$$\int_{Y-CV}^{Y-(1-\tau(\epsilon_{MB}))\epsilon_{MB}} g'(x)dx = \int_{\bar{D}}^{\bar{D}+\tau(\epsilon_{MB})\epsilon_{MB}} h'(x)dx \ . \tag{20}$$

In the context of Figure 10, $Y - CV$ corresponds to the point on the horizontal axis such that the corresponding area underneath MU_C bounded by $Y - CV$ from the left and $Y - (\epsilon_{MB} - b^*)$ from the right equals the blue area (I). It is evident from here that the CV is bounded from below by $(1 - \tau(\epsilon_{MB}))\epsilon_{MB}$ and by the entire bill ϵ_{MB} from above.[34]

---

[34] The lower bound is achieved if the right had side of equation (20) equals zero. The upper bound is achieved if $-\int_{\bar{D}}^{\bar{D}+\tau(\epsilon_{MB})\epsilon_{MB}} h'(x)dx \geq \int_{Y-\epsilon_{MB}}^{Y-(1-\tau(\epsilon_{MB}))\epsilon_{MB}} g'(x)dx$.

D.2 Proposition 1

The specific value of CV depends on the shape of both marginal utility functions. Unfortunately, it is difficult to calibrate $h'(\cdot)$ directly. However, we can combine the first order condition and the result from the implicit function theorem with observed out-of-pocket payments to approximate the marginal disutility of medical debt in terms of the marginal utility of consumption. We start with the case $h''(\cdot) > 0$ and turn to the case $h''(\cdot) = 0$ below. Specifically, we propose a local linear approximation of the marginal disutility of debt around the optimal borrowing decision:

$$\begin{aligned} h'(\bar{D} + x) &= h'(\bar{D} + \tau(\epsilon_{MB})\epsilon_{MB}) + h''(\bar{D} + \tau(\epsilon_{MB})\epsilon_{MB}) * (x - \tau(\epsilon_{MB})\epsilon_{MB}) \\ &= g'(Y - (1 - \tau(\epsilon_{MB}))\epsilon_{MB}) - \frac{1 - \tau'(\epsilon_{MB})\epsilon_{MB} - \tau(\epsilon_{MB})}{\tau'(\epsilon_{MB})\epsilon_{MB} + \tau(\epsilon_{MB})} \\ &* \; g''(Y - (1 - \tau(\epsilon_{MB}))\epsilon_{MB}) * (x - \tau(\epsilon_{MB})\epsilon_{MB}) \ , \end{aligned}$$

where the second equality uses the first order condition and the implicit function theorem. Similarly, using a local linear approximation around $g'(\cdot)$ and assuming that locally a constant fraction of medical bills is unpaid $\tau(\epsilon_{MB}) = \bar{\tau}$, we can rewrite equation (20) as:

$$\begin{aligned} & g'(Y - (1 - \bar{\tau})\epsilon_{MB}) \Big[CV - (1 - \bar{\tau})\epsilon_{MB} \Big] \\ + \;& g''(Y - (1 - \bar{\tau})\epsilon_{MB}) \int_{Y-CV}^{Y-(1-\bar{\tau})\epsilon_{MB}} (x - (Y - (1 - \bar{\tau})\epsilon_{MB}))dx \\ = \;& g'(Y - (1 - \bar{\tau})\epsilon_{MB}) * \bar{\tau}\epsilon_{MB} - \frac{1 - \bar{\tau}}{\bar{\tau}} * g''(Y - (1 - \bar{\tau})\epsilon_{MB}) \int_{\bar{D}}^{\bar{D}+\bar{\tau}\epsilon_{MB}} (x - (\bar{D} + \bar{\tau}\epsilon_{MB}))dx \ . \end{aligned}$$

Simplifying terms, we have

$$\begin{aligned} & g'(Y - (1 - \bar{\tau})\epsilon_{MB}) \Big[CV - (1 - \bar{\tau})\epsilon_{MB} \Big] - g''(Y - (1 - \bar{\tau})\epsilon_{MB}) \int_{0}^{CV-(1-\bar{\tau})\epsilon_{MB}} xdx \\ = \;& g'(Y - (1 - \bar{\tau})\epsilon_{MB}) * \bar{\tau}\epsilon_{MB} + \frac{1 - \bar{\tau}}{\bar{\tau}} * g''(Y - (1 - \bar{\tau})\epsilon_{MB}) \int_{0}^{\bar{\tau}\epsilon_{MB}} xdx \ . \end{aligned}$$

and

$$\begin{aligned} & g'(Y - (1 - \bar{\tau})\epsilon_{MB}) \Big[CV - \epsilon_{MB} \Big] - \frac{1}{2} g''(Y - (1 - \bar{\tau})\epsilon_{MB}) \Big[CV - (1 - \bar{\tau})\epsilon_{MB} \Big]^2 \\ = \;& \frac{1 - \bar{\tau}}{2 * \bar{\tau}} * g''(Y - (1 - \bar{\tau})\epsilon_{MB}) \Big[\bar{\tau}\epsilon_{MB} \Big]^2 . \end{aligned}$$

Finally, we have

$$CV = \frac{\Big[-g'(\cdot) - (1-\bar{\tau})\epsilon_{MB} g''(\cdot)\Big] + \sqrt{g'(\cdot)^2 - 2\bar{\tau} g'(\cdot) g''(\cdot)\epsilon_{MB} - \bar{\tau} g''(\cdot)^2 \epsilon_{MB}^2 (1-\bar{\tau})}}{-g''(\cdot)}.$$

Let $\phi(\cdot) = -\frac{g'(\cdot)}{g''(\cdot)}$, then we have

$$CV = -\phi(\cdot) + (1-\bar{\tau})\epsilon_{MB} + \sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}\ ,$$

which establishes the first part of the proposition.

Case $h''(\cdot) = 0$: Before we turn to the comparative statics, we establish that the CV discussed above provides a lower bound for the case $h''(\cdot) = 0$. Simplifying the former derivation we now have,

$$g'(Y - (1-\bar{\tau})\epsilon_{MB})\Big[CV - \epsilon_{MB}\Big] - \frac{1}{2} g''(Y - (1-\bar{\tau})\epsilon_{MB})\Big[CV - (1-\bar{\tau})\epsilon_{MB}\Big]^2 = 0.$$

This implies the following compensating variation:

$$\begin{aligned} CV^* &= -\phi(\cdot) + (1-\bar{\tau})\epsilon_{MB} + \sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB}} \\ &\geq -\phi(\cdot) + (1-\bar{\tau})\epsilon_{MB} + \sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}, \end{aligned}$$

where the second row replicates the CV derived above.

Comparative statics: We now turn to the comparative statics. We first show that $\frac{dCV}{d\phi(\cdot)} > 0$. Taking the first derivative, we have

$$\frac{dCV}{d\phi(\cdot)} = -1 + \frac{\phi + \bar{\tau}\epsilon_{MB}}{\sqrt{\cdot}}.$$

Now we show that $\Big[\phi + \bar{\tau}\epsilon_{MB}\Big]^2 > \Big(\sqrt{\cdot}\Big)^2$. So we have

$$\begin{aligned} \Big[\phi + \bar{\tau}\epsilon_{MB}\Big]^2 &> \Big(\sqrt{\cdot}\Big)^2 \\ \phi(\cdot)^2 + 2\bar{\tau}\epsilon_{MB}\phi(\cdot) + \bar{\tau}^2\epsilon_{MB}^2 &> \phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2 \\ \leftrightarrow 0 &> -\bar{\tau}\epsilon_{MB}^2\ , \end{aligned}$$

which establishes the second part of the proposition.

Next we show that $\frac{dCV}{d\bar{\tau}} < 0$. Taking the first derivative, we have

$$\begin{aligned}
\frac{dCV}{d\bar{\tau}} &= -\epsilon_{MB} + \frac{1}{2 * \sqrt{\cdot}}\Big[2\phi(\cdot)\epsilon_{MB} - \epsilon_{MB}^2 + 2\bar{\tau}\epsilon_{MB}^2\Big] - \frac{d\phi(\cdot)}{d\bar{\tau}} + \frac{1}{2 * \sqrt{\cdot}}\Big[2\phi(\cdot)\frac{d\phi(\cdot)}{d\bar{\tau}} + 2\epsilon_{MB}\bar{\tau}\frac{d\phi(\cdot)}{d\bar{\tau}}\Big] \\
&= -\underbrace{\epsilon_{MB}\Big[1 - \frac{\sqrt{\Big[\phi + (\bar{\tau} - \frac{1}{2})\epsilon_{MB}\Big]^2}}{\sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}}\Big]}_{\text{A}} \\
&- \underbrace{\frac{d\phi(\cdot)}{d\bar{\tau}}\Big[1 - \frac{\sqrt{(\phi(\cdot) + \bar{\tau} * \epsilon_{MB})^2}}{\sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}}\Big]}_{\text{B}}
\end{aligned}$$

First, we note that $\sqrt{\Big[\phi + (\bar{\tau} - \frac{1}{2})\epsilon_{MB}\Big]^2} < \sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}$, which implies that term A is greater than 0. Hence, we have

$$\begin{aligned}
\Big[\phi + (\bar{\tau} - \frac{1}{2})\epsilon_{MB}\Big]^2 &< \Big(\sqrt{\cdot}\Big)^2 \\
\phi(\cdot)^2 + 2(\bar{\tau} - \frac{1}{2})\epsilon_{MB}\phi(\cdot) + (\bar{\tau} - \frac{1}{2})^2\epsilon_{MB}^2 &< \phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2 \\
\leftrightarrow -\phi(\cdot)\epsilon_{MB} + [\bar{\tau}^2 - \bar{\tau} + \frac{1}{4}]\epsilon_{MB}^2 &< [\bar{\tau}^2 - \bar{\tau}]\epsilon_{MB}^2 \\
\leftrightarrow \frac{\epsilon_{MB}^2}{4} < \phi(\cdot)\epsilon_{MB} & \\
\leftrightarrow \epsilon_{MB} < 4\phi(\cdot)\ . &
\end{aligned}$$

which is true if $\epsilon_{MB} < min\{\frac{\phi(\cdot)}{\bar{\tau}+\frac{1}{8}}, 4\phi(\cdot)\}$ as required in the proposition.

Second, we have that $\sqrt{(\phi(\cdot) + \bar{\tau} * \epsilon_{MB})^2} \geq \sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}$, which implies that $sign(B) = sign(-\frac{d\phi(\cdot)}{d\bar{\tau}})$. Here, we have

$$\frac{d\phi(\cdot)}{d\bar{\tau}} = -\frac{d\frac{g'(\cdot)}{g''(\cdot)}}{d\bar{\tau}} = -\frac{g''(\cdot)^2\epsilon_{MB} - \epsilon_{MB}g'''(\cdot)g'(\cdot)}{g''(\cdot)^2}.$$

If $\frac{g'''(\cdot)g'(\cdot)}{g''(\cdot)^2} \leq 2$ then $\frac{d\phi(\cdot)}{d\bar{\tau}} \leq \epsilon_{MB}$. Then we have

$$
\begin{aligned}
\frac{dCV}{d\bar{\tau}} &\geq -\epsilon_{MB} * \Big[2 - \frac{\sqrt{\Big[\phi + (\bar{\tau} - \frac{1}{2})\epsilon_{MB}\Big]^2} + \sqrt{\Big[\phi(\cdot) + \bar{\tau} * \epsilon_{MB}\Big]^2}}{\sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}}\Big] \\
&= -\epsilon_{MB} * \Big[2 - \frac{\phi + (\bar{\tau} - \frac{1}{2})\epsilon_{MB} + \phi(\cdot) + \bar{\tau} * \epsilon_{MB}}{\sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}}\Big] \\
&= -\epsilon_{MB} * \Big[2 - 2\frac{\sqrt{(\phi(\cdot) + (\bar{\tau} - \frac{1}{4})\epsilon_{MB})^2}}{\sqrt{\phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2}}\Big].
\end{aligned}
$$

Finally, we show that

$$
\begin{aligned}
(\phi(\cdot) + (\bar{\tau} - \frac{1}{4})\epsilon_{MB})^2 &< \phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2 \\
\leftrightarrow \phi(\cdot)^2 + 2\phi(\cdot)(\bar{\tau} - \frac{1}{4})\epsilon_{MB} + (\bar{\tau} - \frac{1}{4})^2\epsilon_{MB}^2 &< \phi(\cdot)^2 + 2\bar{\tau}\phi(\cdot)\epsilon_{MB} - \bar{\tau}(1-\bar{\tau})\epsilon_{MB}^2 \\
\leftrightarrow -\frac{1}{2}\phi(\cdot)\epsilon_{MB} + \frac{1}{2}\bar{\tau}\epsilon_{MB}^2 + \frac{1}{16}\epsilon_{MB}^2 &< 0 \\
\leftrightarrow \phi(\cdot) > (\bar{\tau} + \frac{1}{8})\epsilon_{MB} & \\
\leftrightarrow \epsilon_{MB} < \frac{\phi(\cdot)}{\bar{\tau} + \frac{1}{8}} &
\end{aligned}
$$

which is true if $\epsilon_{MB} < min\{\frac{\phi(\cdot)}{\bar{\tau}+\frac{1}{8}}, 4\phi(\cdot)\}$ as required in the proposition. This establishes the third part of the proposition.

Finally, we turn to

$$
\frac{CV}{\epsilon} = -\frac{\phi(\cdot)}{\epsilon_{MB}} + (1-\bar{\tau}) + \sqrt{\frac{\phi(\cdot)^2}{\epsilon_{MB}^2} + 2\frac{\bar{\tau}\phi(\cdot)}{\epsilon_{MB}} - \bar{\tau}(1-\bar{\tau})}
$$

Here we have

$$
\begin{aligned}
d\frac{CV}{\epsilon_{MB}}/d\epsilon_{MB} &= -\Big[\frac{\frac{d\phi(\cdot)}{d\epsilon_{MB}} - \phi(\cdot)}{\epsilon_{MB}^2}\Big] + 2 * \frac{\Big[\frac{\phi(\cdot)}{\epsilon_{MB}} + \bar{\tau}\Big]}{2 * \sqrt{\cdot}}\Big[\frac{\frac{d\phi(\cdot)}{d\epsilon_{MB}} - \phi(\cdot)}{\epsilon_{MB}^2}\Big] \\
&= -\Big[\frac{\frac{d\phi(\cdot)}{d\epsilon_{MB}} - \phi(\cdot)}{\epsilon_{MB}^2}\Big] * \Big[1 - \frac{\sqrt{\Big[\frac{\phi(\cdot)}{\epsilon_{MB}} + \bar{\tau}\Big]^2}}{\sqrt{\cdot}}\Big].
\end{aligned}
$$

Since $\sqrt{\left[\frac{\phi(\cdot)}{\epsilon_{MB}}+\bar{\tau}\right]^2} \geq \sqrt{\cdot}$, the second factor is smaller than zero. Hence the sign of the effect equals the sign of $\left[\frac{\frac{d\phi(\cdot)}{d\epsilon}-\phi(\cdot)}{\epsilon_{MB}^2}\right]$.

We have

$$\begin{aligned} \frac{d\phi(\cdot)}{d\epsilon_{MB}} - \phi(\cdot) &= -(1-\bar{\tau})\Big[\frac{g''(\cdot)^2 - g'''(\cdot)g'(\cdot)}{g''(\cdot)^2}\Big] - \phi(\cdot) \\ &< -(1-\bar{\tau}) + \phi + (1-\bar{\tau}) - \phi(\cdot) = 0, \end{aligned}$$

where the second line uses $\frac{g'''(\cdot)g'(\cdot)}{g''(\cdot)^2} \leq 1 + \frac{\phi(\cdot)}{1-\bar{\tau}}$. This establishes the last part of the proposition.

D.3 Details on Effects of Variance Reduction

The second order Taylor expansion yields:

$$\begin{aligned} U(\epsilon_{MB}, \epsilon_{\bar{MB}}) &= g(Y-(1-\bar{\tau})*\epsilon_{\bar{MB}}) - h(\bar{D}+\bar{\tau}*\epsilon_{\bar{MB}}) \\ &- \Big[(1-\bar{\tau})*g'(Y-(1-\bar{\tau})*\epsilon_{\bar{MB}}) + \bar{\tau}*h'(\bar{D}+\bar{\tau}*\epsilon_{\bar{MB}})\Big](\epsilon_{MB}-\epsilon_{\bar{MB}}) \\ &+ \frac{1}{2}\Big[(1-\bar{\tau})^2 g''(Y-(1-\bar{\tau})*\epsilon_{\bar{MB}}) - \bar{\tau}^2 * h''(\bar{D}+\bar{\tau}*\epsilon_{\bar{MB}})\Big](\epsilon_{MB}-\epsilon_{\bar{MB}})^2 \ . \end{aligned}$$

The first order condition and the condition from the implicit function theorem allow us to replace the derivatives of $h(\cdot)$ with derivatives of $g(\cdot)$ as follows:

$$\begin{aligned} U(\epsilon_{MB}, \bar{\epsilon}) &= g(Y-(1-\bar{\tau})*\epsilon_{\bar{MB}}) - h(\bar{D}+\bar{\tau}*\epsilon_{\bar{MB}}) \\ &- g'(Y-(1-\bar{\tau})*\epsilon_{\bar{MB}})(\epsilon_{MB}-\epsilon_{\bar{MB}}) \\ &+ \frac{1}{2}*(1-\bar{\tau})*g''(Y-(1-\bar{\tau})*\epsilon_{\bar{MB}})(\epsilon_{MB}-\epsilon_{\bar{MB}})^2 \ . \end{aligned}$$

Finally, expected utility is given by:

$$EU = \int U(\epsilon, \epsilon_{\bar{MB}})dG$$

and the risk premium, RP, is implicitly given by:

$$EU = g(Y-(1-\tau)*\epsilon_{\bar{MB}} - RP) - h(\bar{D}+\tau*\epsilon_{\bar{MB}}) \ .$$

Hence we have

$$\begin{aligned} & g(Y-(1-\bar{\tau}) * \bar{\epsilon_{MB}}) - g(Y-(1-\bar{\tau}) * \bar{\epsilon_{MB}} - RP) \\ = & -\frac{1}{2} * (1-\tau) * g''(Y-(1-\bar{\tau}) * \bar{\epsilon_{MB}}) \int (\epsilon_{MB} - \bar{\epsilon_{MB}})^2 dG \\ = & -\frac{1}{2} * (1-\bar{\tau}) * g''(Y-(1-\bar{\tau}) * \bar{\epsilon_{MB}}) * var(\epsilon_{MB}) \ . \end{aligned}$$

D.4 Pure Out-Of-Pocket Benchmark

Conversely, had we ignored the impact of unpaid medical bills, we could have applied a second order Taylor approximation around $U^{oop} = g(Y - (1-\bar{\tau}) \cdot \bar{\epsilon}_{MB})$. This would deliver:

$$\begin{aligned} U^{oop}(\epsilon_{MB}, \bar{\epsilon}_{MB}) &= g(Y-(1-\bar{\tau}) \cdot \bar{\epsilon}_{MB}) \\ &- (1-\bar{\tau}) \cdot g'(Y-(1-\bar{\tau}) \cdot \bar{\epsilon}_{MB})(\epsilon_{MB} - \bar{\epsilon}_{MB}) \\ &+ \frac{1}{2}(1-\bar{\tau})^2 g''(Y-(1-\bar{\tau}) \cdot \bar{\epsilon}_{MB})(\epsilon_{MB} - \bar{\epsilon}_{MB})^2. \end{aligned}$$

Compared to the case also considering unpaid medical bills, the first and the second order term are now each smaller by a factor of $\frac{1}{1-\bar{\tau}}$. The implied risk premium ignoring the impact of unpaid medical bills RP^{oop} is then

$$\begin{aligned} & g(Y-(1-\bar{\tau}) \cdot \bar{\epsilon}_{MB}) - g(Y-(1-\bar{\tau}) \cdot \bar{\epsilon}_{MB} - RP) \\ = & \frac{1}{1-\bar{\tau}} \cdot \Big[g(Y-(1-\bar{\tau}) \cdot \bar{\epsilon}_{MB}) - g(Y-(1-\bar{\tau}) \cdot \bar{\epsilon}_{MB} - RP^{oop}) \Big]. \end{aligned}$$

It follows that

$$RP^{oop} < RP < \frac{1}{1-\bar{\tau}} \cdot RP^{oop} \ .$$

As with the mean reduction, this suggests that considering unpaid medical bills can increase the risk premium by factor of $\frac{1}{1-\bar{\tau}}$. We quantify the risk premium in a numerical example in Section 9.

CULTURE OF HEALTH

By Heidi L. Allen, Erica Eliason, Naomi Zewde, and Tal Gross

Can Medicaid Expansion Prevent Housing Evictions?

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ABSTRACT **Evictions are increasingly recognized as a serious concern facing low-income households. This study evaluated whether expansions of Medicaid can prevent evictions from occurring. We examined data from a privately licensed database of eviction records in fourteen states (286 counties) and used a difference-in-differences research design to compare rates of eviction before and after California's early Medicaid expansion (51 counties). Early Medicaid expansion in California was associated with a reduction in the number of evictions, with 24.5 fewer evictions per month in each county from a pre-expansion average of 224.7. These results imply that for every thousand new Medicaid enrollees in California, Medicaid expansion was associated with roughly twenty-two fewer evictions per year. Additionally, we found a 2.9-percentage-point reduction in evictions per capita associated with early expansion. The effects were concentrated among counties with the highest pre-expansion rates of uninsurance. We conclude that health insurance coverage is associated with improved housing stability.**

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Medicaid expansion has been shown to reduce medical debt and poverty by alleviating the financial burden of medical care on low-income beneficiaries and their families. Another of the challenges facing this population is home eviction. The growing rate of home eviction in the United States is drawing the attention of researchers and policy makers. We investigated whether Medicaid coverage plays a role in mitigating the devastating phenomenon of home eviction.

Eviction can have long-term damaging consequences for the families affected. Prior research suggests that experiencing a housing eviction may lead to poorer physical and mental health outcomes and a considerable worsening of health-related behaviors.[1] There are several potential mechanisms for this decline. First, with a legal record of eviction, renters may be forced into a higher-risk rental market in higher-crime, higher-poverty neighborhoods with substandard housing conditions.[2] Second, eviction precipitates acute risk of homelessness, which imparts a well-documented toll on health.[3] Third, for those receiving health care, forced displacement can interrupt treatment continuity and disrupt patient-provider relationships,[4] thus increasing the likelihood of worsened health outcomes. The causes of eviction are understudied and poorly understood, and these factors are of growing importance as evictions become increasingly common.

In the most immediate sense, eviction is initiated by a court judgment in favor of a landlord, usually in a dispute over unpaid rent. The tenant then either voluntarily vacates the premises in two weeks or is forcibly removed with their belongings by local police. The rising incidence of urban eviction may result from narrowing margins between rising rents and stagnant incomes.[5] For example, in Los Angeles County, California,

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which has seen a sharp increase in homelessness,[6] working full time at minimum wage and renting a one-bedroom apartment leaves $148 a month for all other expenses. Nationwide, most low-income families spend over half of their income on housing.[2] Poor health can be a contributor to eviction when people with limited resources cannot afford both housing and medical care. Poor health can cause absenteeism or job loss, further constraining resources through income reduction, becoming uninsured, or both. As a result, poor health can increase one's exposure to health care costs, which can contribute to financial decline and increase one's risk for housing instability.

Medicaid may mitigate the risk of eviction directly by reducing the cost of medical care and indirectly by protecting earning potential through better health. The strength of the latter effect is moderated in this population by concurrently weak ties to the labor force and a high burden of work-limiting disease and disability.[7] Within this context, medical debt or urgent medical needs, acute or chronic (for example, filling prescriptions), may compete with housing obligations when finances are tight. If so, providing otherwise uninsured low-income adults with affordable health care coverage may help prevent eviction.

In this study we used a quasi-experimental difference-in-differences design to examine whether the expansion of eligibility for Medicaid, which provided medical care at low or no cost for low-income adults, could prevent eviction. Our hypothesis that Medicaid expansion might reduce evictions was supported by prior evidence of the program's effects on social and economic aspects of the lives of low-income patients. Evidence from a randomized trial of Medicaid expansion in Oregon has shown that the coverage nearly eliminated catastrophic medical debt (defined as out-of-pocket medical spending totaling more than 30 percent of income),[8] decreased the probability of having debt in collections, lowered the amount owed in existing collections, and lowered financial strain in general (including borrowing money from friends and family members and skipping some bills to pay others).[9] In California, Medicaid expansion was also associated with a reduction in the use of payday loans.[10] Thus, it is possible that improved finances related to Medicaid could help families avoid being sent to court for an eviction or being evicted by allowing tenants to come to a financial agreement with their landlords.

We analyzed data from California because it is a large state that initiated early Medicaid expansions through the Affordable Care Act (ACA) county by county over a period of months in late 2011 and throughout 2012. The California Low Income Health Program (LIHP), also called the Bridge to Reform, was established through a Medicaid Section 1115 waiver and built upon a demonstration program in ten counties, the Health Care Coverage Initiative, in 2007–10. Most counties opted to participate in the LIHP expansions. Eligibility for expanded coverage was established by counties and varied considerably. For example, Sacramento County expanded eligibility to people with incomes that were 67 percent of the federal poverty level, while Alameda County used 200 percent of poverty. An estimated 680,000 people gained coverage through the LIHP (based on unduplicated cumulative enrollment in December 2012), and most of them were transferred to Medi-Cal or the state Marketplace (Covered California) in January 2014.

Our data set of fourteen states allowed us to compare 51 California counties that expanded Medicaid early to 235 counties in states that did not expand Medicaid during the same time period, as well as to counties in California that opted out of the LIHP. As noted above, we used a difference-in-differences research design. We were unable to evaluate the 2014 ACA Medicaid expansions using this data source and methodological approach, as most states in our database adopted the 2014 expansions. As a result, we included data only through 2013 in our analysis.

Study Data And Methods

DATA This study used a commercial evictions database that was originally designed to help landlords screen tenants, from American Information Research Services (AIRS). The database was licensed to us for the purpose of this research. It has been used and validated in previous eviction research by the Eviction Lab at Princeton University.[11] AIRS retrieves public eviction records from counties in periodic batches, through automated retrieval, or both, depending on the contractual arrangement with the county.

For our main analyses we used evictions in 286 counties in fourteen states (including California) for each month in the period January 2008–December 2013—roughly including thirty-six months before and twenty-four months after the early California Medicaid expansions in 2011 and 2012. For counties in several states (Arizona, California, Nevada, New Jersey, Utah, Washington, and Wisconsin) we analyzed the universe of evictions in the time period above. For counties in several other states (Delaware, Florida, Hawaii, Massachusetts, Ohio, and Pennsylvania) some time periods were not covered in the data.

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Health care coverage may be keeping households from "falling over the brink."

We conducted a number of analyses that excluded particular states from the analysis to ensure that the results did not depend on the inclusion or exclusion of particular states, but our findings were not sensitive to the choices. In our final analyses, two states (Massachusetts and Hawaii) that had achieved near-universal coverage before the start of the California Medicaid expansions were labeled "always treated" states. A description of our sample, showing the share of counties with Medicaid expansion over our study period, is in the online appendix (also see appendix exhibit A.1. and table A.2. for more details on the data set).[12]

ANALYSES We evaluated the effect of California's early Medicaid expansion on evictions by running a difference-in-differences regression, comparing 51 early-expanding "treated" counties to 235 "control" counties without early expansion. The regression compared the average monthly change in evictions for expansion counties before and after they expanded Medicaid to the average monthly change in nonexpansion counties over the same time period observed in our study.

We aggregated information from the evictions database to the county-month level. We included county-specific and year-month fixed effects in all main models, in addition to adjusting for county-specific linear time trends. We controlled for yearly county unemployment using data from the Local Area Unemployment Statistics of the Bureau of Labor Statistics. We controlled for annual county-specific poverty, using American Community Survey data published by the Census Bureau and aggregated by the Princeton Eviction Lab.[11]

Our outcomes of interest were the number of evictions in each county-month, the number of evictions per capita, the natural logarithm of the number of evictions, and the number of evictions per rental unit. Per capita estimates adjust for total population, but evictions are at the household level, which vary in size. Estimates per rental unit are better in this regard, but we had only one measure of rental units per year, compared to precise monthly estimates of evictions.

To calculate evictions per capita, we used estimates of county population size for each county from the 2000 census. To calculate evictions per rental unit, we used the number of rental-occupied housing units in each county for each study year from the American Community Survey.

To examine whether our results were driven by populations most likely to obtain coverage under the Medicaid expansion, we stratified our estimation model of monthly numbers of evictions between counties, using a higher versus a lower share of uninsured residents before the expansion. We stratified counties by the share of residents with incomes below the threshold for Medicaid eligibility (138 percent of the federal poverty level) who were uninsured in 2010. We obtained estimates for the share uninsured by income from the Small Area Health Insurance Estimates published by the Census Bureau. We present results from the difference-in-differences regression of monthly number of evictions, estimated separately for counties with above- and below-median shares of uninsured residents.

SUPPLEMENTAL ANALYSES In our appendix[12] we present the results of several exercises that we conducted to visually inspect our results and assess the soundness of our empirical approach: We show the results of our main specification without adjusting for county-specific linear trends. We present a "raw count" graph of the percentage changes in evictions from January 2008 levels, comparing California counties that expanded Medicaid in January 2012 to all other counties until 2014. We display "event studies" for all of our outcome measures to probe the parallel trends assumption of the difference-in-differences design. In each exercise, but particularly in the last, we were trying to evaluate the counterfactual assumption that in the absence of Medicaid expansion, trends in evictions would not have differed between treated and control counties during the study period.

Our visual inspection of the event studies suggested that treated California counties had higher eviction rates than control counties did before Medicaid expansion. Pre-expansion differences in trend could partially account for the magnitude of change observed in difference-in-differences models, which is why we adjusted for county-specific linear trends in our main specifications. Additionally, we explored the robustness of our main results using a synthetic control approach as an alternative to difference-in-differences regression.[13,14] Synthetic control methodology is not sensitive to an assumption of parallel trends. For this approach, we created

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a synthetic control group of counties for each California county using an algorithm created by Alberto Abadie and colleagues, the economists who developed this specific methodology for comparative case studies.[13] The algorithm assigned a weight of 0 to 1 to each synthetic control county so as to closely match the pre-expansion trend in evictions in the treated county across the pre-expansion period. We then calculated the average difference between the actual evictions each month in the treated county and the evictions in the synthetic control group after expansion. Details on the synthetic control approach are in the appendix.[12]

LIMITATIONS The primary limitation of our study was the aggregate nature of the data. We were unable to directly link individual-level data on insurance status to people's rental histories or eviction records. The data we had on evictions were administrative in nature and included very few details about people's demographic characteristics such as insurance status, income level, or age. To examine the link between insurance status and eviction, we performed stratified analyses by uninsurance rates, expecting that the main effect of the expansion would be most prominent in counties with a greater share of uninsured residents before expansion if the result was tied to the change in insurance status.

There were also issues related to using a commercial evictions database instead of undergoing primary data collection (which would have been prohibitively costly and time-consuming). One issue was that we could not verify that every eviction was included in our data set. However, the Eviction Lab at Princeton University examined undercoverage in the AIRS data by comparing internally aggregated counts of evictions in AIRS with publicly reported aggregated counts of evictions by states. Fewer than 5 percent of counties in AIRS states were suspected to have counts below the externally verified ones.[11]

An additional limitation involved the assumptions underlying the empirical approach. The difference-in-differences estimation strategy assumed that evictions in California and other states were evolving along parallel trends. That assumption might be violated, and in fact exhibit 1 suggests a slight divergence in trend before California expanded Medicaid. For that reason, we explored other empirical approaches, such as the synthetic control methodology, and adjusted for county-specific linear trends in our main specification.

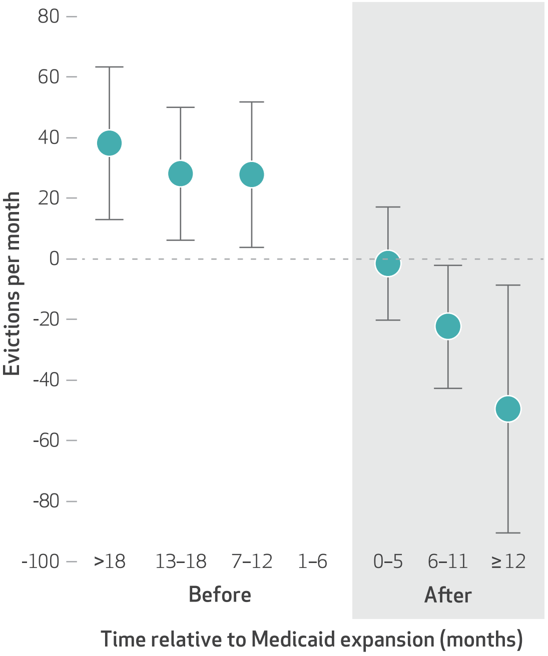
We also could not completely rule out changes in the housing market that may have affected rates of eviction during our study period, although we had no reason to believe that such changes would have coincided specifically with Medicaid expansion. The nation faced a housing crisis during the Great Recession (2007–09), which led to increased regulation and oversight of the mortgage industry. New federal regulations were not specific to California, although California experienced a disproportionately high increase in evictions during the Great Recession. California did implement a law in 2017 that allowed some evictions to be sealed from disclosure, but this did not affect our study.

Study Results

We found that the Medicaid expansion in California led to a significant reduction in the number of evictions occurring in each county. Exhibit 1 illustrates this relationship as an event study of the monthly number of evictions. Before the expansion, California expansion counties had slightly higher rates of evictions (particularly

EXHIBIT 1

Estimated monthly number of evictions in California counties that expanded eligibility for Medicaid relative to counties that did not, by time period relative to expansion, 2008–13



SOURCE Authors' analysis of data from American Information Research Services (AIRS). **NOTES** The exhibit shows point estimates from one regression of evictions in each county on a series of indicator variables for time relative to the expansion of eligibility for Medicaid. Estimated numbers of evictions during the period 1–6 months before expansion are omitted. The error bars indicate 95 percent confidence intervals. All coefficients with the exception of that for the indicator on 1–6 months after expansion are statistically significant at the 5 percent level. The associated p-values for each coefficient from left to right are 0.003, 0.011, 0.023, 0.872, 0.030, and 0.017, respectively.

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following the Great Recession) than comparison counties did. Following the expansion, we saw a change in sign, with California expansion counties having significantly fewer total numbers of evictions. Point estimates of the reduction in evictions following expansion grew as more time elapsed following expansion, reaching significance more than twelve months post-expansion. The event studies of the estimated effect of Medicaid on eviction rates per capita and per rental unit are presented as appendix exhibits D.3.–D.5.[12]

The difference-in-differences regression estimate, adjusted for county-specific linear trends, suggests that expansion was associated with 24.517 fewer evictions per month in each county, compared to a pre-expansion mean of 224.718 evictions per month (exhibit 2). Based on publicly available estimates of the number of Californians who gained coverage during Medicaid expansions, these estimates imply that for every thousand new enrollees, there were approximately twenty-two fewer evictions per year[15] (see appendix B.2. for an explanation of calculating eviction reductions per new enrollee).[12] Relative to the number of residents in each county, we found that the expansion reduced evictions per capita by 0.029 from a pre-expansion mean of 0.261 (exhibit 2). When we examined evictions per rental unit, we saw a nonsignificant reduction of 0.172 evictions from a pre-expansion mean of 4.037.

As expected, we found a more pronounced effect of Medicaid expansion on evictions in counties with above-median shares of uninsured residents before expansion, as measured in 2010, compared to those with below-median shares: 51.505 versus 3.270 fewer evictions after expansion (exhibit 3).

When we examined our main results without the adjustment for county-specific linear trends, we observed both much greater reductions in evictions and an increase in statistical significance (see appendix table D.1. for more detail).[12] A visual inspection of the event studies suggested to us that there were differences in trends following the Great Recession that would support an additional examination of the data using synthetic control methods. Reassuringly, we found that the results were robust to using a synthetic control approach of matching expansion counties to nonexpansion counties by their pre-expansion eviction trends. Using this approach, we also saw a significant divergence following Medicaid expansion, with expansion counties experiencing fewer evictions than their nonexpansion counterparts did (see appendix table E.1. for differences between actual evictions and evictions in a synthetic control group).[12] These estimates suggest an effect of roughly seventy fewer evictions, on average, which is quite similar to the results in the difference-in-differences models that were unadjusted for county-specific linear trends. All of the associated *p* values in the post-expansion period were significant below conventional levels.

We explored the effect of Medicaid expansion on evictions in four ways, using difference-in-differences regressions, event study regressions, a synthetic control approach, and a visual comparison of the "raw count" percentage change from January 2008 to January 2014 (see appendix exhibit D.2.).[12] Some patterns in the results suggested that we did not have a perfect comparison group. That said, all four approaches pointed in the same direction and implied that Medicaid expansion was associated with a decrease in evictions.

EXHIBIT 2

Estimated effects of California counties' expansion of eligibility for Medicaid and county-specific covariates on eviction measures

	Dependent variables			
Independent variables	**Evictions per month**	**Evictions per capita**	**Log of evictions**	**Evictions per rental unit**
California, after expansion	−24.517**	−0.029**	−0.117**	−0.172*
Unemployment rate	0.617	0.005	0.019	0.045
Poverty rate	−3.853	−0.006	−0.018	−0.069**
Pre-expansion mean	224.718	0.261	4.037	4.037
R^2	0.949	0.921	0.946	0.918
Number of county-month observations	17,925	17,925	16,974	17,925

SOURCE Authors' analysis of data from American Information Research Services (AIRS). **NOTES** The exhibit shows the results of difference-in-differences regressions that were adjusted for county-specific fixed effects, year-month-specific fixed effects, and county-specific linear time trends (not shown). The sample consists of county-month observations. More information on the composition of the sample and the dates of Medicaid expansion is in appendix exhibit A.1., table A.2., and table B.1., and appendix table C.1. is a version of this exhibit with standard errors and detailed *p* values (see note 12 in text). *$p < 0.10$ **$p < 0.05$

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EXHIBIT 3

Estimated effects of California counties' expansion of eligibility for Medicaid and county-specific covariates on the number of evictions by the share of the population uninsured

	Dependent variable: number of evictions	
Independent variable	**Share uninsured above median**	**Share uninsured below median**
California, after expansion	−51.505**	3.270
Unemployment rate	1.341	2.150**
Poverty rate	−6.384	−1.427
Pre-expansion mean	318.858	117.568
R^2	0.946	0.954
Number of county-month observations	8,419	9,506

SOURCE Authors' analysis of data from American Information Research Services (AIRS). **NOTES** The exhibit shows the results of difference-in-differences regressions in which counties were stratified by the share of the population that was uninsured in 2010. The median share uninsured across counties in the sample was 0.28. Regressions were adjusted for county-specific fixed effects, year-month-specific fixed effects, and county-specific linear time trends (not shown). The sample consists of county-month observations. More information on the composition of the sample and the dates of the Medicaid expansion is available, as explained in the notes to exhibit 2. Appendix table C.2. is a version of this exhibit with standard errors and detailed *p* values (see note 12 in text). $^{**}p < 0.05$

Discussion

This study found that Medicaid expansion reduced the rate and number of home evictions, using data from the counties in California that expanded Medicaid early. The estimated magnitude of this relationship falls within the range expected from related results in the existing literature on the economic impacts of Medicaid expansion. Examining public records that covered credit reports, evictions, bankruptcies, and wage garnishments, Sarah Miller and colleagues found a reduction of 11–16 percent in adverse financial outcomes in the first year following Medicaid expansion in Michigan.[16] Similar to our findings, that study showed that the estimated effect increased over time, with the emergence of significant effects six to ten months following expansion.

Furthermore, Naomi Zewde and Christopher Wimer found that Medicaid reduced annual out-of-pocket medical spending by approximately $100 per beneficiary, on average.[17] People who became eligible for Medicaid under the expansion necessarily live on small margins between income and expenses. Thus, the share of the population with Medicaid-related spending reductions near or greater than the $100 mean could plausibly better meet housing obligations and avoid eviction.

This study contributes to an existing body of literature suggesting that one of the principal benefits of Medicaid expansion is related to protection from encumbering medical debt, leading to improved financial well-being. Research has shown that California's early Medicaid expansion was also associated with a reduction in payday borrowing, which provides further evidence of the financial protection that the expansion provided.[10] In addition, the Oregon Health Insurance Experiment's randomized trial findings demonstrated an improvement in financial stability and a reduction in catastrophic medical debt among Medicaid recipients.[8,9] Health care coverage may be keeping households from "falling over the brink," helping them meet their living expenses as the growing cost of medical care constrains household budgets.

Conclusion

Our findings suggest that Medicaid expansion may play an important role in preventing eviction and its devastating long-term consequences for the physical and mental health, housing quality, and financial well-being of already burdened low-income families. The findings add to a body of evidence suggesting that the economic consequences for recipient households will be felt beyond the immediate coverage implications. While voters in Idaho, Nebraska, and Utah recently approved Medicaid expansion through ballot referendums,[18] other states are experimenting with programmatic retractions that include instituting work requirements.[19] This body of literature helps inform policy makers of the ramifications of expanding or retracting public coverage on the economic well-being of the population. Our findings indicate that Medicaid not only is an important part of the health care safety net but also may be considered a key strategy for addressing poverty-related housing instability. ■

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By Heidi Allen, Ashley Swanson, Jialan Wang, and Tal Gross

Early Medicaid Expansion Associated With Reduced Payday Borrowing In California

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ABSTRACT We examined the impact of California's early Medicaid expansion under the Affordable Care Act on the use of payday loans, a form of high-interest borrowing used by low- and middle-income Americans. Using a data set for the period 2009–13 (roughly twenty-four months before and twenty-four months after the 2011–12 Medicaid expansion) that covered the universe of payday loans from five large payday lenders with locations around the United States, we used a difference-in-differences research design to assess the effect of the expansion on payday borrowing, comparing trends in early-expansion counties in California to those in counties nationwide that did not expand early. The early Medicaid expansion was associated with an 11 percent reduction in the number of loans taken out each month. It also reduced the number of unique borrowers each month and the amount of payday loan debt. We were unable to determine precisely how and for whom the expansion reduced payday borrowing, since to our knowledge, no data exist that directly link payday lending to insurance status. Nonetheless, our results suggest that Medicaid reduced the demand for high-interest loans and improved the financial health of American families.

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Various studies have demonstrated that health insurance coverage lowers the medical debt that consumers must take on when they receive health care services.[1,2] Lower medical debts, in turn, can improve consumers' credit scores and other credit-related outcomes.[3–5] Only a few studies have focused on individual financial outcomes related to the recent expansion of health insurance coverage under the Affordable Care Act (ACA),[6,7] and no studies to date have focused on how health insurance coverage affects the use of alternative financial products.

Research on this topic is especially important given the documented relationship between poverty, medical debt, and bad credit outcomes. People in poverty are more likely to be uninsured and have fewer financial resources to pay for out-of-pocket health care spending.[8] Poor financial health can permeate all aspects of life, making it difficult to secure affordable housing, purchase homes or automobiles, and manage day-to-day expenses. Importantly, people with bad credit ratings have less access to traditional methods of borrowing, such as credit cards or personal loans.[9,10]

One form of borrowing that has been increasingly scrutinized by policy makers is payday loans.[11] Payday loans are short-term, unsecured loans that are primarily used by low- and middle-income Americans in states where the loans are legal. In 2012 it was estimated that twelve million Americans take out at least one payday loan annually, with an average of eight loans of $375 each per year and a total of $520 spent on fees.[12] The loans are advertised as a two-week credit

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product meant to address temporary cash-flow issues. Critics argue that when the loans' fees are converted into an annualized interest rate, the rates are much higher than those of other financial products—typically over 300 percent—and many borrowers end up in long-term cycles of debt.[12,13] Low-income adults, defined in the 2012 study as those with annual incomes of less than $40,000, were 62 percent more likely than their higher-income counterparts to use payday loans.[12]

Evidence suggests that payday borrowing occurs when access to traditional credit is most limited,[10] which supports the hypothesis that payday loans are perceived as a last resort by consumers.[14] About 16 percent of payday loan consumers report using the loans for emergency or unexpected expenses, while 69 percent report borrowing to pay for recurring expenses.[12] Medical debts could fall into either category, such as when consumers are faced with unexpected financial shocks (for example, an emergency department visit) or when they are balancing recurring medical expenses (for example, for prescriptions) with competing demands like housing and food.

There is early evidence that expansions of eligibility for Medicaid might be an important policy lever for improving the financial stability of low-income Americans.[1,3] The Oregon Health Insurance Experiment found that Medicaid reduced financial strain and improved the credit outcomes of low-income adults, who experienced fewer delinquencies in medical bills and lower amounts of medical debt. Catastrophic medical liability, defined as exceeding 30 percent of annual income, was almost entirely eliminated.[15] Other studies have confirmed that Medicaid expansion improves credit scores and may reduce rates of bankruptcy.[6] In particular, the Massachusetts health care reform, which expanded coverage in a way similar to the ACA, led to a decrease in bankruptcies and an improvement in credit scores.[4] Going back further, the Medicaid expansions of the 1990s have been shown to decrease the risk of bankruptcy.[3]

The fate of existing and future Medicaid expansions is currently unclear, as Congress and President Donald Trump continue to consider repealing and replacing the ACA. As national and state health policy enter a new era of flux, it is critical to have a broad empirical understanding of the costs and benefits of providing Medicaid to low-income adults—especially populations that historically have not been eligible for Medicaid.

We examined the relationship between Medicaid coverage and risky borrowing in the state of California, which was an early adopter of Medicaid expansion through the ACA. Specifically, we compared payday lending in California counties that expanded Medicaid in advance of the ACA's 2014 expansion to lending in counties throughout the United States (including four in California) that had not yet expanded Medicaid.

Study Data And Methods

DATA This study used a novel source of national data on payday loans provided to the authors by an industry trade group, the Community Financial Services Association of America (CFSA). The data set consisted of the universe of payday loans originated by five national storefront payday lending chains with locations around the country. It contained information on over ninety-three million loans, which had been deidentified for research purposes. For each loan, we observed the dates that the loan was made and was due, the outcome of the loan (for example, a default or late payment), and the ZIP code of the payday lender's storefront. The data set covered all loans from 2009 through the early months of 2014. Appendix Exhibit A1 maps the states included in the data set.[16]

METHODS We focused on the state of California, which entered into an agreement with the administration of President Barack Obama for early county-by-county implementation of the ACA's Medicaid expansion in 2011 and 2012. We studied the early expansions in California, because our data did not offer a sufficiently long time series to study the 2014 expansions and provided relatively little information about loans in other early expansion states. We compared California counties that expanded Medicaid early to counties nationwide that did not do so, including four California counties that delayed expansion.

We aggregated the CFSA data to the county-month level, creating aggregate loan counts, default rates, and other measures of loan volumes and outcomes in each county and month combination. The aggregated data set contained 58,020 county-month observations for the period 2009–13, which covered roughly twenty-four months before and twenty-four months after the California Medicaid expansions. California rolled out Medicaid expansion over 2011 and 2012, and we used the dates of expansion by county provided by Benjamin Sommers and coauthors.[17] These dates are listed in Appendix Exhibit A2, along with county-specific average monthly payday borrowing before to expansion.[16] Appendix Exhibit A3 shows the aggregate study sample statistics.[16] We examined outcomes in the 43 expansion counties in California, using as a comparison group 920 counties in non-

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Regardless of the merits of payday lending, a decline in loan volume attributable to Medicaid is a positive policy outcome.

expanding states and 4 California counties that delayed expansion.

Our primary outcomes were three measures of loan volume: the number of loans, the amount of money borrowed, and the number of unique borrowers. We measured unique borrowers in the data each month using the data set's anonymized borrower identifiers. Medicaid expansions provide health insurance for uninsured adults younger than age 65, so we stratified our outcomes by age and focused on people younger than age 65. Given previous research findings that Medicaid expansions disproportionately benefited those younger than age 50, we further examined the distribution of the number of loans among nonelderly adults by borrower's age (18–34, 35–49, and 50–64).

Additionally, we thought that we might see higher reductions in payday lending within counties with higher preexpansion shares of low-income uninsured adults. We investigated this possibility by comparing counties with a high share of uninsured to those with a low share. Counties categorized as having a high share were those in the top tercile of the share uninsured with incomes of less than 138 percent of the federal poverty level, according to the 2010 Census Bureau's Small Area Health Insurance Estimates; counties categorized as having a low share were in the bottom tercile.

Our secondary outcomes were the shares of loans that ended in default, were repaid late, and were rollovers. Rollovers are loans that are taken out at the same time a previous loan is due, which allows the borrower to extend the loan's duration without repaying the principal—in exchange for paying a finance charge. We identified likely rollovers in the data as loans that began within two days of a previous due date for the same borrower and same lender.[18]

For both our primary and secondary outcomes, we used a standard difference-in-differences analysis of county-month outcomes that covered roughly twenty-four months before and twenty-four months after the 2011–2012 California Medicaid expansions. As noted above, we compared 43 California early expansion counties to 924 nonexpansion counties (including the 4 previously mentioned nonexpansion California counties) in the national data set, with standard errors clustered at the county level. We stratified our findings by the age of the borrower—focusing on people younger than age sixty-five, who would have been most likely to be affected by Medicaid expansion. As a sensitivity test (see Appendix Exhibit A7),[16] we examined borrowers older than age sixty-five and used a triple-differences approach at the county-month-age level.

To rule out systemic preexisting time trends that could have undermined our difference-in-differences approach, we estimated an "event study" regression of the effect of Medicaid expansion on the number of loans. This tested the validity of our assumption that payday borrowing would have had similar trends in expansion and nonexpansion counties if none of the counties had expanded Medicaid. The regression included a fixed effect for every county, a fixed effect for every month, and indicators for four six-month periods before Medicaid expansion and three six-month periods after expansion (see Appendix Exhibit A8).[16]

LIMITATIONS Our study was not able to directly link individual insurance status to payday borrowing; to our knowledge, the data to do so do not exist.

Additionally, although we found no evidence of this, we could not rule out the possibility that state- or county-level changes in the regulation (or enforcement of regulations) of payday loans or other industry changes might have occurred in California in the period 2010–14. However, we tested the appropriateness of our approach in several ways. First, we stratified our models by age group (people younger or older than age sixty-five): Those in the younger group would be beneficiaries of the Medicaid expansion, while those in the older group would not, since they would be eligible for Medicare. Second, we examined how changes in payday lending varied with the share of uninsured people in the county before expansion: We would expect to find a greater reduction in payday lending in areas with higher shares than in areas with lower shares. Last, we conducted an "event study" regression, described above, to assess any preexisting time trends in payday lending. Our additional methodology provided reassuring evidence that our findings were attributable to the Medicaid expansion.

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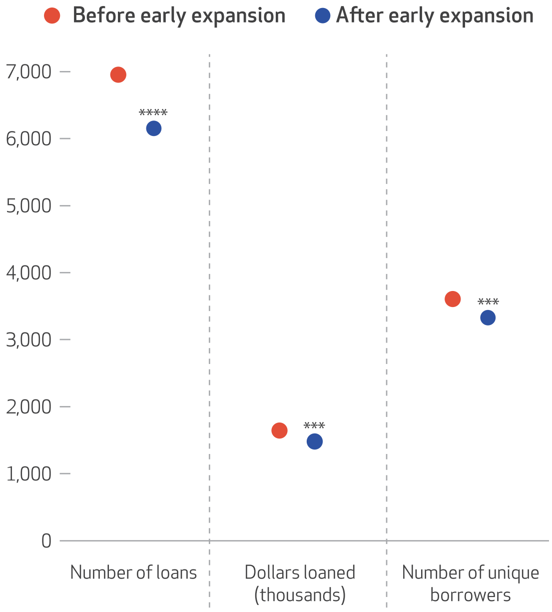
Study Results

The difference-in-differences methodology we relied on compared payday lending before and after California's early Medicaid expansion in the state's expansion counties versus nonexpansion counties nationwide. To control for confounding, time-varying factors that affect all counties at particular times (such as recessions, holidays, and seasonality), this approach used nonexpansion counties, in California and other states, as a control group.

Exhibit 1 presents estimates of the impact of Medicaid expansion on the overall volume of payday lending, our primary outcomes; the accompanying table is in Appendix Exhibit A4.[16] We found large relative reductions in borrowing after the Medicaid expansion among people younger than age sixty-five. The number of loans taken out per month declined by 790 for expansion counties, compared with nonexpansion counties. Given a preexpansion mean of 6,948 loans per month, that amounts to an 11 percent drop in the number of loans. This reduction in loan volume translates to a $172,000 decline in borrowing per month per county, from a mean of $1,644,000—a drop of 10 percent. And 277 fewer unique borrowers per county-month took out loans, which represents an 8 percent decrease from the preexpansion mean of 3,603.

EXHIBIT 1

Effect of early expansion of eligibility for Medicaid on monthly payday loans for borrowers younger than age 65, 2009–13



SOURCE Authors' analysis of data for 2009–13 from the Community Financial Services Association of America. **NOTES** The exhibit shows the results of difference-in-differences regressions of the outcomes, comparing differences over time in county-month data between 43 counties in California that expanded Medicaid early (in 2011–12) under the Affordable Care Act and 924 counties nationally (including 4 in California) that did not expand Medicaid before 2014. There were 58,020 county-month observations. Dollars loaned is measured in thousands. Appendix Exhibit A4 shows the regression estimates (see Note 16 in text). For number of loans, $R^2 = 0.971$, and p (associated with a test that the Medicaid expansion had no effect) < 0.001. For dollars loaned, $R^2 = 0.969$, $p = 0.001$. For number of unique borrowers, $R^2 = 0.981$, $p = 0.002$. $^{***}p < 0.01$ $^{****}p < 0.001$

Exhibit 2 presents the effect of Medicaid expansion on the number of loans in three age categories: 18–34, 35–49, and 50–64; the accompanying table is in Appendix Exhibit A5.[16] The reduction in the number of loans per month was entirely driven by borrowers younger than age fifty (the slight increase among older borrowers was not significant). For expansion counties in California, relative to the nonexpansion counties in California and other states, postexpansion borrowers ages 18–34 took out 486 loans per county-month, compared to a preexpansion mean of 2,268—a reduction of 21 percent. For borrowers ages 35–49, the decline was 345 from a preexpansion mean of 2,715, a reduction of 13 percent. This observed relationship across age categories remained when we examined the number of unique borrowers and total dollars loaned (data not shown).

Exhibit 3 examines the impact of Medicaid expansion on the volume of payday lending as it varies by the share of low-income uninsured people in 2010. Counties with the highest tercile of low-income uninsured people in 2010 (that is, in the top tercile in terms of the share of uninsured people with incomes below 138 percent of poverty) showed greater declines in payday loan volume in terms of both numbers and percentages, when compared to counties in the lowest tercile of low-income uninsured people. For example, the number of monthly loans per county declined by 1,571 (12 percent) in counties with a high share of uninsured borrowers, versus 362 (10 percent) in counties with a low share. There were comparable differences in the amounts loaned and the numbers of unique borrowers.

Exhibit 4 shows the effect of Medicaid on the payment outcomes of payday loans, our secondary outcomes; the accompanying table is in Appendix Exhibit A6.[16] We found a proportionally large and significant postexpansion increase of 0.5 percentage points in the share of defaults, from a preexpansion mean of 3 percent. There was a marginally significant change in the share of late payments and a significant increase in rollovers, which had a high preexpansion mean (50 percent of the loans) and a postexpansion increase of almost 3 percentage points.

It is important to recognize that the interpretation of the effect of expanding Medicaid is

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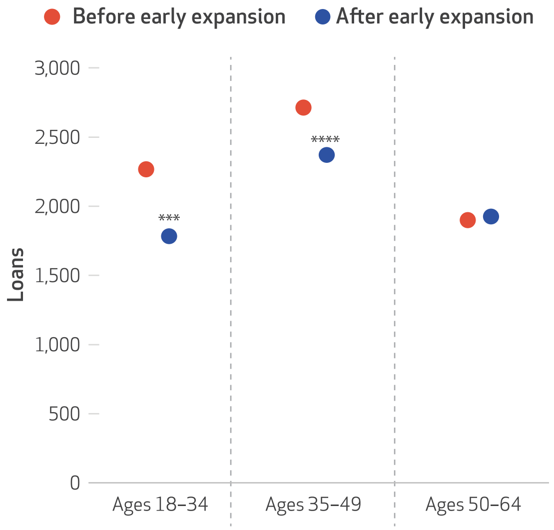
EXHIBIT 2

Effect of early expansion of eligibility for Medicaid on the number of payday loans for borrowers younger than age 65, by age group, 2009–13



SOURCE Authors' analysis of data for 2009–13 from the Community Financial Services Association of America. **NOTES** The exhibit shows the results of difference-in-differences regressions of the outcomes as explained in the Notes to Exhibit 1, which also give the sample size. Appendix Exhibit A5 shows the regression estimates (see Note 16 in text). For ages 18–34, $R^2 = 0.951$, p (associated with a test that the Medicaid expansion had no effect) = 0.001. For ages 35–49, $R^2 = 0.969$, $p < 0.001$. For ages 50–64, $R^2 = 0.976$, $p = 0.442$. $^{***}p < 0.01$ $^{****}p < 0.001$

less straightforward for the secondary outcomes than for the primary outcomes. Since we observed a decline in overall loan volume, Medicaid expansion could have changed the types of people who took out payday loans. We could not distinguish between the effect on the types of borrowers and a direct effect of on reducing default, late payment, or rollover rates across all borrower types.

Appendix Exhibit A7 presents the results of our sensitivity analyses for borrowers older than age sixty-five.[16] As noted above, we examined payday loan volume stratified for people in that age group as well as conducting a triple-difference analysis of county-month-age (younger or older than age sixty-five). We found small but significant increases in payday volume among the older borrowers. When we used those borrowers as an additional within-state control group, we had triple-difference estimates that were roughly similar, though slightly larger in magnitude, than the difference-in-differences estimates in Exhibit 1. To the extent that the effects on the older population captured unobserved, latent trends in expansion counties, this suggests that our main estimates might be slight underestimates of the effects of Medicaid expansion on payday loan volume.

As mentioned above, the key assumption in the difference-in-differences framework on which we relied is that California's expansion counties and all of the nonexpansion counties would have shown similar trends in the absence of the expansion. That assumption would be violated, for instance, if California had experienced a uniquely robust job-market recovery during the study period. That said, we are aware of no evidence that the job-market recovery in California was different from the recovery in other states in a way that would affect payday borrowing. But, more important, Appendix Exhibit A8 shows the time trends in numbers of loans both before and after the expansion.[16] Reassuringly, the exhibit suggests that there were no observable differences between future expanding and nonexpanding counties in preex-

EXHIBIT 3

Effects of early expansion of eligibility for Medicaid, by county share of uninsured residents younger than age 65, 2009–13

	Number of loans		Dollars loaned (thousands)		Number of unique borrowers	
	High share of uninsured	Low share of uninsured	High share of uninsured	Low share of uninsured	High share of uninsured	Low share of uninsured
Mean change in Medicaid-expansion counties, after expansion	−1,571.39	−361.91	−343.60	−76.14	−610.13	−125.31
Standard error[a]	(624.484)	(122.526)	(149.714)	(28.03)	(264.786)	(40.294)
p value	0.012	0.003	0.022	0.007	0.022	0.002
Mean before expansion	13,066.70	3,720.60	3,098.80	875.30	6,896.80	1,949.30
Implied change	−12.00%	−9.70%	−11.10%	−8.70%	−8.80%	−6.40%
R^2	0.971	0.976	0.966	0.977	0.982	0.98

SOURCE Authors' analysis of data for 2009–13 from the Community Financial Services Association of America. **NOTES** The exhibit shows the results of difference-in-differences regressions of the outcomes as explained in the Notes to Exhibit 1, which also give the sample size. There were 19,740 counties with a high share of borrowers—that is, counties in the top tercile for share of uninsured people with incomes below 138 percent of the federal poverty level. There were 19,140 counties with a low share of borrowers—that is, counties in the bottom tercile. County and year-month fixed effects not shown. [a]Clustered at the county level.

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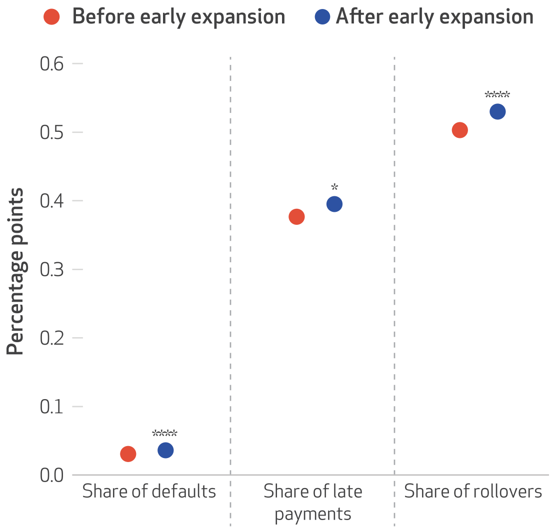
EXHIBIT 4

Effect of early expansion of eligibility for Medicaid on the payment outcomes of payday loans for borrowers under age 65, 2009–13



SOURCE Authors' analysis of data for 2009–13 from the Community Financial Services Association of America. **NOTES** The exhibit shows the results of difference-in-differences regressions of the outcomes as explained in the Notes to Exhibit 1, which also give the sample size. Appendix Exhibit A6 shows the regression estimates (see Note 16 in text). For share of defaults, $R^2 = 0.451$, p (associated with a test that the Medicaid expansion had no effect) <0.001. For share of late payments, $R^2 = 0.686$, $p = 0.060$. For share of rollovers, $R^2 = 0.853$, $p < 0.001$.

isting time trends, which validates the parallel-trends assumption that underlies our difference-in-differences approach. Specifically, in the twenty-four months before Medicaid expansion, we observed no preexisting differences in the number of payday loans that could confound the estimated effect of Medicaid expansion when we later compared groups. We therefore found no evidence that the parallel trends assumption was violated. In addition, the Appendix exhibit suggests that a negative effect of the Medicaid expansions on the numbers of loans began approximately six months after expansion, which seems credible given that medical needs and medical bills accumulate slowly.

Discussion

Medicaid expansion has improved access to high-quality health care, increased the use of outpatient and inpatient medical services,[15,19] and improved the personal finances of low-income adults by reducing the number of medical bills subject to debt collection and by improving credit scores.[1] This study adds to the existing evidence of the benefits of Medicaid expansion by demonstrating that it decreased the use of payday loans in California.

Previous research showing that Medicaid expansions led to substantive reductions in medical debt suggested that we might find a reduction in the need for payday borrowing following California's early expansion. Indeed, our primary results suggest a large decrease (11 percent) in the number of loans taken out by borrowers younger than age 65, and an even larger decline (21 percent) among those ages 18–34. We observed a slight increase in borrowing for those older than age 65, which we found surprising. We also found the reduction in payday borrowing to be concentrated among those younger than age 50, which is plausible given that half of new Medicaid enrollees in California in 2012–14 as a result of the expansion of eligibility for adults were younger than age 40, and almost 80 percent were younger than age 55.[20] Previous research has also suggested that younger adults are the primary beneficiaries of Medicaid expansions.[21]

We were unable to identify precisely how and for whom Medicaid reduces payday borrowing. To our knowledge, there are no data that directly link payday lending to insurance status. One possibility is that although a relatively small share of California residents (roughly 8 percent of the low-income population)[22] gained coverage, the coverage gain may have been disproportionately bigger in the subset of low-income California residents likely to frequent payday lenders. Thus, the observed magnitude of declines in loan volume could simply be driven by a large change in borrowing for county residents who gained coverage. There is previous evidence that California's early Medicaid expansions decreased out-of-pocket medical spending by 10 percentage points among low-income adults.[22] Another possibility is that the Medicaid expansion affected many more people beyond those who gained coverage directly. Household members of people who gained Medicaid coverage may have also decreased their payday borrowing.

Payday loans are of particular policy import because they are a controversial financial product, outlawed in many states and tightly regulated in several others. These loans would be severely restricted under new rules proposed by the Consumer Financial Protection Bureau.[23] Proponents of payday loans have argued that they are an important resource for people with bad credit ratings who would otherwise not have access to cash in dire circumstances or who would accrue even higher fees through bank overdrafts or informal loans. However, evidence has shown that at least some payday borrowing results from behavioral biases, and some consumers would be

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better off avoiding these loans. Such behavioral biases may lead consumers to make mistakes when budgeting, be overly optimistic about their ability to repay loans in the future, or focus on short-term financial needs rather than the long-term consequences of high-interest borrowing.[24,25] Regardless of the merits of payday lending, a decline in loan volume attributable to Medicaid is a positive policy outcome and supports previous research on the spillover effects of Medicaid on financial health.[1,3,6–7]

Reductions in medical expenditures should also theoretically make it easier to pay back payday loans. Yet the outlook for postexpansion borrowers in our study was more mixed. While there was a slight reduction in the number of loans per borrower, we observed marginal increases in late payments and significant increases in the shares of defaults and rollovers. There are several potential mechanisms for these increases that warrant further study. First, the substantial reduction in payday volume we observed could have a corresponding influence on the composition of the remaining borrowers or on the characteristics of their debts. Specifically, people who borrow because of medical expenses may be socio-demographically different than people who borrow for other reasons (for example, job loss). Second, a trending loss of revenue in the industry could exert influence on the business model, leading to riskier lending practices if payday lenders detect a significant decline in loan volume.

Conclusion

For people younger than age sixty-five, Medicaid expansion in California was associated with significant declines in the average number of payday loans per month, the amount borrowed, and the number of unique borrowers. This decline in payday borrowing did not appear to be due to a preexisting trend. It was concentrated in young adults, was not observed among people ages sixty-five and older, and was more pronounced in areas that had a higher share of uninsured people before the expansion—which is consistent with the view that the Medicaid expansion caused the reductions in payday borrowing. These findings add to the previous literature on the benefits of Medicaid in improving the financial health of low-income Americans. ■

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Effect of limited transportation on medication adherence in patients with epilepsy

Timothy E. Welty, Stacy L. Willis, and Elisabeth A. Welty

Abstract

Objective: To determine whether limited transportation affects medication adherence in patients with epilepsy.

Design: Descriptive, nonexperimental, cross-sectional study.

Setting: United States and worldwide, February to April 2007.

Patients: 143 patients with epilepsy.

Intervention: A 22-item survey was developed to ask patients with epilepsy or their caregivers about the impact of limited transportation on adherence with medications. The survey was placed on Zoomerang.com. An invitation to participate in the survey was sent via e-mail to members of the Epilepsy.com website, and an invitation with a link to the survey was placed on Epilepsy.com.

Main outcome measures: Whether patients with epilepsy have difficulty picking up prescriptions on time because of transportation problems and whether they felt they would miss fewer doses if transportation was not an issue.

Results: 143 individuals with epilepsy completed part or all of the survey. Of patients who were unable to drive, 45% reported that fewer doses would be missed if transportation was not a problem. Patients who were unable to drive had an odds ratio of 4.2 ($P < 0.0001$) of being unable to get medications on time. No differences were observed in the number of patients missing prescription medications associated with availability of insurance, use of mail service pharmacies, or population size of patients' area of residence. Ability to drive and distance to the pharmacy were the only factors associated with nonadherence.

Conclusion: Limited transportation may be a factor in poor medication adherence in patients with epilepsy.

Keywords: Epilepsy, seizures, adherence (medication), transportation, prescription refills, antiepileptic agents.

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Adherence to antiepileptic drug therapy is essential to optimal control of seizures. Multiple studies have demonstrated a definite correlation between the level of adherence with antiepileptic drugs and control of seizures in patients with epilepsy.[1,2] From these studies, numerous factors have been associated with poor adherence. However, many of these factors have focused on cognitive, social, psychological, and economic issues, including an inability to pay for medications. Little attention has been given to other factors, including transportation.

All states in the United States place restrictions on driving for patients with epilepsy.[3] These restrictions generally prevent or limit patients with epilepsy and ongoing seizures from driving a motor vehicle. As a result, patients with ongoing seizures must rely on alternate forms of transportation to perform routine activities of daily living, including shopping and visiting a pharmacy to receive prescription medications. Typically, these methods of transportation include public transportation or reliance on friends and family. Often, these options are limited, unreliable, or inconsistent. As a result, patients with epilepsy may have difficulty getting to a pharmacy to pick up prescriptions and ensure consistent access to medications. This problem could affect medication adherence.

Studies of other chronic diseases have demonstrated that problems with transportation affect adherence with care considerably. In spinal cord injury patients, problems with transportation were the primary factors associated with nonadherence in keeping clinic visits.[4] Limited transportation was the only factor associated with poor medication adherence in human immunodeficiency virus/acquired immunodeficiency syndrome patients in Costa Rica.[5] Another study demonstrated that longer distances from home to the site of primary care resulted in poorer control of glucose in patients with diabetes.[6] A survey of patients with schizophrenia also demonstrated that transportation problems were strongly associated with nonadherence in receiving depot doses of antipsychotics.[7] Additional studies indicated that limited transportation is an important contributor to nonadherence among patients with tuberculosis, glaucoma, asthma, and cardiovascular disease.[8–12] More global analyses of factors contributing to nonadherence with medications have identified transportation problems as important among both rural and urban populations.[13,14]

Clearly, transportation plays an important role in adherence for patients with other chronic disorders. Patients with epilepsy may be similar, given the fact that the disease may result in the loss of driving privileges. However, the connection between medication adherence and limited transportation has not been studied in patients with epilepsy.

At a Glance

Synopsis: Transportation difficulties may adversely affect medication adherence among patients with epilepsy, according to this study of 143 individuals with epilepsy who completed an online survey. Of patients who reported not being able to drive, 51% stated having trouble getting medications on time because of transportation problems, with 45% reporting that they would miss fewer doses of antiepileptic drugs if transportation was not a problem. More than one-quarter (28%) of patients who did not drive reported having seizures because of not being able to obtain medications on time.

Analysis: *Refill reminders, delivery services, special accommodations for delivery of prescriptions to patients, subsidizing transportation, providing information on transportation availability, and telepharmacy are potential options for pharmacists or third-party payers to consider in helping to alleviate transportation problems for patients with epilepsy. Limited transportation should be considered in the clinical management of patients and included as a factor in future studies of medication adherence in patients with epilepsy.*

Objective

The objective of the current study was to determine the extent to which patients with epilepsy identified limited transportation as an issue in adherence with medications.

Methods

To assess the possible impact of limited transportation on medication adherence, we developed a 22-item survey for patients with epilepsy or their caregivers (survey questions shown in Table 1). For this report, only data from patients with epilepsy were included because of concerns about how accurately caregiver responses reflected patient experience. The survey was designed to account for various factors that might influence transportation and medication adherence. These included insurance coverage for prescription medications, availability and use of mail service pharmacies, mode of transportation, distance from home to the pharmacy, and population size of patients' area of residence.

An electronic format of the survey was placed on Zoomerang.com, and an e-mail invitation to participate in the survey was sent to the membership of Epilepsy.com. This invitation included a direct link to the survey. Additionally, an announcement of the survey with a direct link was placed on the Epilepsy.com homepage. Epilepsy.com is a website for patients and health care providers. It has approximately 175,000 unique viewers monthly to the public section and approximately 25,000 unique viewers to the professional section. Although visitors to the website are primarily from the United States, the site also attracts viewers from outside the United States. Additional demographic data on the makeup of members and viewers are not available as a result of issues related to confidentiality. Epilepsy.com was chosen as a venue for this survey because of the number of visits each month and its popularity as a recommended website for patients with epilepsy. Other than websites dedicated to professional organizations (e.g.,

Table 1. Responses to survey questions by ability to drive of patients with epilepsy

Question/responses	Able to drive No. (%)	Unable to drive No. (%)
Do you have insurance that covers at least a portion of the cost of medications?		
Yes	42 (81)	58 (72)
No	9 (17)	21 (26)
Uncertain	1 (2)	2 (2)
If there is coverage for prescription medications, does it allow for prescriptions to be filled in mail service pharmacies?		
Yes	28 (67)	29 (50)
No	7 (17)	16 (28)
Uncertain	7 (17)	12 (22)
If so, do you use this option?		
Yes	11 (39)	14 (48)
No	17 (61)	15 (52)
How many medications for seizures do you currently take?		
1	24 (46)	33 (41)
2	17 (33)	24 (30)
3	9 (17)	8 (20)
≥4	2 (4)	8 (10)
What best describes the area in which you live (i.e., population size)?		
<10,000	6 (12)	12 (15)
10,000–49,000	12 (23)	24 (30)
50,000–99,000	5 (10)	6 (8)
100,000–199,000	11 (21)	12 (15)
200,000–499,000	4 (8)	2 (6)
500,000–999,000	5 (10)	5 (6)
≥1 million	9 (17)	15 (19)
What best describes the pharmacy at which you get your prescriptions filled?		
Local community independent	8 (15)	18 (22)
Community chain (e.g., Walgreens, Rite Aid, CVS)	23 (44)	28 (35)
Grocery store chain (e.g., Brunos, Kroger, Fred's, Winn Dixie, Walmart)	9 (17)	14 (17)
Clinic/VA	2 (4)	2 (2)
Mail service	9 (17)	9 (11)
Other	1 (2)	9 (11)
Approximately how far from your home is your pharmacy located (miles)?		
<1	10 (20)	27 (34)
1–4	22 (44)	27 (34)
5–9	7 (14)	8 (10)
10–14	6 (12)	5 (6)
≥15	5 (10)	13 (16)
Does your pharmacy provide any of the following services (select all that apply)?		
Consultation on drugs and diseases	24 (48)	42 (55)
Delivery services	5 (10)	15 (19)
Mail medications	5 (10)	9 (12)
Patient education services	11 (22)	18 (23)
Don't know	22 (44)	28 (36)
If you do not drive, are other forms of transportation available?		
Yes	NA	56 (69)
No	NA	18 (22)
Uncertain	NA	7 (9)
What best describes your primary mode of transportation?		
Self	23 (83)	7 (9)
Friend/family member	6 (12)	43 (53)

Public transportation (e.g., subway, taxi, bus)	2 (4)	26 (32)
I do not have a primary mode of transportation	1 (2)	5 (6)
Do you ever have trouble picking up prescription medications on time because of transportation problems?		
Yes	10 (20)	41 (51)
No	41 (80)	40 (49)
How often do transportation problems keep you from getting your prescription medications on time?		
Every month	1 (10)	10 (25)
Every other month	5 (50)	7 (18)
A couple times per year (every 2–4 months)	0	14 (35)
One or fewer times per year	3 (30)	8 (20)
Other	1 (10)	1 (2)
How often do you miss doses of antiepileptic medication(s)?		
Daily	0	0
Weekly	5 (10)	6 (8)
1–3 times/month	13 (27)	27 (34)
Never	23 (47)	27 (34)
Other	8 (16)	20 (25)
How often do you ever miss doses of medication because you cannot get to the pharmacy to pick up your medications?		
Daily	0	0
Weekly	3 (6)	0
1–3 times/month	7 (14)	9 (11)
Never	33 (67)	51 (65)
Other	6 (12)	19 (24)
Do you believe you have seizures because you are not able to pick up your medications on time?		
Yes	9 (18)	23 (28)
No	41 (82)	58 (72)
How often does this happen?		
Every month	1 (12)	3 (16)
3–4 times/year	3 (38)	6 (32)
1–2 times/year	4 (50)	10 (53)
Do you feel you would miss fewer doses of medication if transportation was not an issue?		
Yes	11 (22)	35 (45)
No	24 (49)	27 (35)
Uncertain	14 (29)	16 (21)

Abbreviations used: NA, not applicable; VA, Department of Veterans Affairs.

American Epilepsy Society, Epilepsy Foundation, American Academy of Neurology), Epilepsy.com is the only website dedicated to providing patients and caregivers with the most up-to-date information on a variety of topics related to epilepsy.

All individuals who accessed the survey were eligible to take it. However, individuals indicating that they were younger than 19 years were not able to proceed beyond the question regarding age.

The survey was set up to prohibit more than one completion of the survey from a given computer. Individual identifiers were not tracked by the investigators. All surveys were completed anonymously. The study protocol was approved by the Samford University Institutional Review Board (IRB). Informed consent was not required by the IRB. The survey was available from February 5, 2007, through April 13, 2007.

The primary responses analyzed were to questions 16 (Do you ever have trouble picking up prescription medications on time because of transportation problems?) and 22 (Do you feel you would miss fewer doses of medication if transportation was not an issue?). These questions directly asked about the association between problems with transportation and the ability to get medications on time. A logistic regression analysis with each variable treated as categorical data was performed using SPSS software (SPSS, Chicago) to determine the effect of each factor on adherence with medications. To determine the factors significantly associated with outcome variables, a nominal logistic regression analysis was performed. All factors were included in the original model, and nonsignificant factors were subsequently eliminated until only the factors significantly associated with outcome variables were included in the final regression model. Chi-square analysis was used to compare responses with the primary questions of interest and responses to the questions about insurance coverage, use of mail service pharmacies, and residence in the United States.

Results

A total of 193 individuals (of 350 visits) either partially or completely responded to the survey. Of these, 16 patients indicated that they were younger than 19 years and were excluded from completing the remainder of the survey and from data analysis. Of respondents, 81% were patients and 19% were caregivers. Only the responses of patients with epilepsy (n = 143) are reported here. A total of 24 patients lived outside the United States. Three-quarters of respondents were women, and 74% were between 19 and 50 years of age.

In response to the question about driving a car, 52 respondents reported that they were able to drive a car and 81 indicated that they did not drive a car. Of patients who reported not being able to drive, 51% stated that they had trouble getting medications on time because of transportation problems, with 45% reporting that they would miss fewer doses of antiepileptic drugs if transportation was not a problem. More than one-quarter (28%) of patients who did not drive believed that they had seizures because of not being able to get medications on time. For patients who were able to drive, 20% reported trouble getting medications because of transportation problems, 22% indicated that they would miss fewer doses if transportation was not a problem, and 18% believed that they had seizures because of an inability to pick up medications on time (Table 1).

A multifactorial logistic regression analysis of the ability to pick up medications from the pharmacy included demographic features, availability of transportation, population size of area of primary residence, availability of insurance, number of medications, and type of pharmacy where prescriptions were filled. The analysis showed that ability to drive and distance to the pharmacy were the only statistically significant factors in the model (Table 2). Individuals who reported being unable to drive had an odds ratio of 4.2 (95% CI 2.0–12.3, $P < 0.0001$) of not getting medications on time compared with patients who reported being able to drive. Interestingly, patients who lived fewer than 4 miles from the pharmacy were more likely to have difficulty obtaining medications on time.

Table 2. Relationship between explanatory variables and difficulty picking up medications

Explanatory variable	Odds ratio (95% CI)	*P*
Not driving a car	4.2 (2.0–12.3)	<0.0001
Distance to pharmacy (miles)		
<1	4.3 (1.2–15.2)	0.02
1–4	4.6 (1.4–15.7)	0.01
5–9	1.2 (0.3–5.5)	0.79
10–14	1.6 (0.3–8.5)	0.25
>15	Reference	Reference

Discussion

Medication adherence is a major concern for patients with epilepsy. Multiple studies have identified economic, social, psychological, and environmental factors that have a clear impact on medication nonadherence. However, the effect of a disease such as epilepsy on personal and societal issues possibly influencing adherence has rarely been considered. For patients with epilepsy, the ability to drive is the largest concern as it relates to maintaining a somewhat normal lifestyle.[3] Obviously, access to driving and transportation can profoundly alter an individual's ability to obtain basic services, including pharmacy services and prescription medications. With limited, inconsistent, or no transportation to a pharmacy for prescription refills, obtaining a consistent supply of antiepileptic agents is a problem that can result in poor seizure control. Studies of patients with other chronic diseases and more global studies of issues related to nonadherence have demonstrated that limited transportation affects adherence considerably and therefore results in poor response to treatment.[4–14] Results from our survey are consistent with these findings, demonstrating that the ability to drive may affect patients' ability to adhere with antiepileptic drug regimens. This nonadherence most likely results from not being able to obtain prescription refills on time and may cause increased seizures for these patients. Our study did not determine the reasons for patients not being able to drive.

An interesting finding from our survey is that use of a mail service pharmacy did not appear to improve receipt of on-time prescription refills. Reasons for mail service pharmacies not providing benefit in terms of on-time delivery of prescription refills were not explored. Possible explanations may include delays in the processing of prescription refills by the mail service pharmacy, mail or postage delays, or forgetfulness on the part of the patient to order prescription refills from the mail service pharmacy. Although often identified as a factor in poor adherence, health insurance coverage was not identified as a factor in the current study. Our findings are consistent with results of a study of medication adherence among a rural population.[13] In this study, transportation and patient dissatisfaction and lack of confidence in physicians were the only factors associated with medication nonadherence; health insurance coverage was not correlated with adherence.

Another intriguing result from our study is that patients who lived closer to their pharmacy claimed to have more difficulty obtaining medication refills on time. The reasons for this finding are unclear. One potential explanation is that alternate transportation for extremely short distances is more difficult to obtain compared with longer distances. Another possible explanation is that trips of a longer distance are more difficult to obtain, resulting in more careful planning of what should be accomplished during that trip. Whatever the reason(s) for this observation, it is consistent with results from a study of medication adherence in an inner-city population.[14] For inner-city patients, distance to the pharmacy should not have been as great a problem as for a rural population. However, difficulties with transportation to the pharmacy were cited among the top three barriers to medication adherence.

Nearly 30% of patients in the current report stated that limited transportation may have contributed to ongoing seizures, which has important clinical effects beyond simply missing doses of medication. Based on these results, physicians, nurses, and pharmacists need to include limited transportation as a possible explanation for ongoing seizures. Transportation issues should be explored with patients and attempts should be made to alleviate this problem.

Limitations

This study involved several limitations. We relied on individual responses and patient recollection without more objective confirmation of their responses. Problems with memory and cognition may have interfered with the ability to respond to questions.

Because the survey required individuals to have access to a computer and the Internet, economic or social bias may have been introduced. The large constituency of Epilepsy.com appears to be consistent with the general population of adults with epilepsy in the United States. Specific demographic information to confirm the makeup of the membership was not available as a result of confidentiality concerns. Important groups of patients (e.g., children, developmentally delayed, elderly) may have been omitted or have had limited representation on the website. These groups of patients tend to rely more heavily on forms of transportation that do not require the patient to drive, possibly leading to different results. However, transportation is noted as a factor in adherence for elderly patients.[15] Additionally, the decision to participate was voluntary, possibly attracting individuals who recognized transportation as an issue. Because more than 75% of respondents were women, the respondents were not necessarily representative of the general population of patients with epilepsy. However, gender did not have a significant effect when evaluated as a potential factor. Of important note, in a large study of patients in a cardiovascular rehabilitation program, transportation difficulties were a significant factor related to nonadherence for women but not for men.[12] Although we blocked repeated entries of surveys from a single computer, we were unable to prevent a person from completing the survey using multiple, different computers. Finally, this was a cross-sectional study. While considering these concerns, it is important to note that our results are highly consistent with other prospective studies on the impact of limited transportation on medication adherence.[4–14]

Conclusion

The results from this survey demonstrate that limited transportation may be a factor in medication adherence for patients with epilepsy. Interventions such as refill reminders, delivery services, special accommodations for delivery of prescription medications to patients, subsidizing transportation, providing information on transportation availability, and telepharmacy are possible options for pharmacists or third-party payers to consider in overcoming this barrier to medication adherence.[13-15] Limited transportation needs to be a consideration in the clinical management of patients and included as a factor in future studies of medication adherence in patients with epilepsy.

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Factors Associated with Adherence to Chemotherapy Guidelines in Patients with Non-Small Cell Lung Cancer

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Abstract

Background—Evidence-based guidelines recommend chemotherapy for medically fit patients with stage II–IV non-small cell lung cancer (NSCLC). Adherence to chemotherapy guidelines has rarely been studied among large populations, mainly because performance status (PS), a key component in assessing chemotherapy appropriateness, is missing from claims-based datasets. Among a large cohort of patients with known PS, we describe first line chemotherapy use relative to guideline recommendations and identify patient factors associated with guideline concordant use.

Patients and Methods—Insured patients, ages 50+, with stage II–IV NSCLC between 2000–2007 were identified via tumor registry (n=406). Chart abstracted PS, automated medical claims, Census tract information, and travel distance were linked to tumor registry data. Chemotherapy was considered appropriate for patients with PS 0–2. Multivariate logit models were fit to evaluate patient characteristics associated with chemotherapy over- and under-use per guideline recommendations. Tests of statistical significance were two sided.

Results—Overall compliance with first line chemotherapy guidelines was 71%. Significant ($p<0.05$) predictors of chemotherapy underuse (19%) included increasing age (odds ratio [OR], 1.09), higher income (OR, 1.02), diagnosed before 2003 (OR, 2.05), and vehicle access (OR, 6.96) in the patient's neighborhood. Significant predictors of chemotherapy overuse (10%) included decreasing age (OR, 0.92), diagnosed after 2003 (OR, 3.24), and higher income (OR, 1.05) in the patient's neighborhood.

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Among NSCLC patients 29% do not receive guideline recommended chemotherapy treatment missing opportunities for cure or beneficial palliation, or receiving chemotherapy with more risk of harm than benefit. Care concordant with guidelines is influenced by age, economic considerations such as income and transportation barriers.

Keywords

guideline adherence; non-small cell lung cancer; chemotherapy; performance status; underuse; overuse

Introduction

Evidence-based treatment guidelines recommend the use of chemotherapy for medically fit patients with unresectable or stage IV non-small cell lung cancer (NSCLC) to improve survival, symptoms, and quality of life [1–4]. Although previous studies have documented variability in the receipt of chemotherapy among patients with NSCLC [5–8] none of these studies have included performance status (PS) [9], a key clinical component in assessing chemotherapy appropriateness [3], because it is typically missing from claims-based databases. The inability to study PS-appropriate chemotherapy use among large populations of NSCLC patients has further precluded an understanding of the patient and other factors that place patients with NSCLC at risk of chemotherapy use that is inconsistent with existing guideline recommendations.

Determining whether care meets professional standards is important in lung cancer care. Adherence to evidence-based guidelines has been used to assess the quality of health care for a wide range of conditions [10], so it is natural to ask how often lung cancer care agrees with guideline recommended care. Previous studies suggest that chemotherapy is sometimes overused at the end of life, with 20% [11] to 43% [12] or more of lung cancer patients receiving chemotherapy within just a few weeks of a patient’s death. Yet, lung cancer patients who use hospice for at least one day – thus avoiding chemotherapy during their stay in hospice, and getting appropriate symptom management – appear to live significantly longer than lung cancer patients who never use hospice [13]. At the same time, an underuse of curative surgery, combined chemotherapy and radiation [5, 6] and palliative chemotherapy may unnecessarily increase the symptom burden and the death rate from this disease.

The issue of guideline adherence has been more comprehensively examined in breast cancer research where a number of studies have demonstrated improvement in survival when patients were treated according to clinical practice guidelines [14–17]. Several breast cancer studies have found that increased age, comorbidity, black race, lower educational attainment, and advanced disease stage are associated with receipt of nonstandard treatment regimens (including underuse), which in turn contributes to less favorable outcomes [18–22]. Furthermore, a conceptual model that explains the underuse of effective therapy in breast cancer has been proposed, where therapy underuse is explained by the interaction of patient, physician, and system factors, each of which exists within a health care system and an individual’s community [23] and is potentially fixable.

The purpose of this research is to use medical-record documented PS to quantify the extent of adherence to evidence-based guidelines for use of chemotherapy among an insured population diagnosed with NSCLC between 2000 and 2007, and to evaluate the patient and other factors associated with both the under- and over-use of chemotherapy within this cohort. By combining data from medical records with those available via an automated tumor registry, medical claims, and Census data, we are able to consider the patients’

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clinical and socio-demographic characteristics, as well as characteristics of the neighborhood in which they reside.

Methods

Study Population and Setting

Study patients were those receiving care from a 900-physician member, multispecialty, salaried medical group practice in southeast Michigan. Data available from the medical group's tumor registry were used to identify all patients aged ≥ 50 years who were diagnosed with NSCLC between January 1, 2000 and December 31, 2007. The medical group, which provides care under both fee-for-service and capitated arrangements, staffs 27 primary care clinics throughout Detroit and the surrounding metropolitan area. Patients eligible for study inclusion were those continuously enrolled in an affiliated health plan (i.e., health maintenance organization) for the 1-year period preceding their date of lung cancer diagnosis. Patients for whom no stage of disease was available or for whom the stage at diagnosis was 0 or I were excluded. The latter were excluded because chemotherapy was not indicated for patients with stage 0 or I disease during this time period [24]. We also excluded patients who died within one month of their diagnosis. None of the patients included in the study sample received targeted therapies in lieu of chemotherapy as Gefitinib and Erlotinib were neither approved for used as first line chemotherapy. The medical group's Institutional Review Board approved all aspects of the study protocol.

Primary Outcome of Interest

Instances in which patients with good PS did not receive first line chemotherapy were classified as "underuse," while instances in which patients with poor PS received chemotherapy were classified as "overuse." We have listed the relevant ASCO lung cancer guidelines in Table 1, which shows the relative consistency. Per the American Society of Clinical Oncology (ASCO) clinical practice guidelines issued in 2009 [25], chemotherapy was recommended for patients with good PS (i.e., PS= 0–2) and not recommended for patients with poor PS (i.e., PS=3–4). Earlier 1997 ASCO guidelines had recommended chemotherapy for patients with PS=0–1 only [1]. The 2003 ASCO guideline recommended combination chemotherapy for patients with PS=0–1, and single agent chemotherapy for patients with PS=2, but no chemotherapy for patients with PS=3–4 [4]. Our baseline models included patients with PS=0–2 in the good PS group. Alternative models that considered PS=2 patients with the poor PS group were also evaluated. Two trained chart abstractors reviewed inpatient and outpatient nursing and physician notes available within the patient's electronic medical record from 2 months before diagnosis until the first notation of death, disenrollment, initiation of chemotherapy, or 6 months after diagnosis to obtain PS. Abstractors recorded the PS documented closest to the diagnosis date, since most NSCLC patients start treatment at the time of symptomatic diagnosis and watchful waiting is not recommended by any guideline. If no specific PS was documented, they estimated PS based on medical notes. In the latter case, notes regarding the patient's functionality (e.g., references to shortness of breath, use of a wheelchair or other personal mobility devices, labor force participation, exercising habits, activities of daily living, or other references to mobility) were recorded and used to estimate PS. Inter-rater reliability between the 2 abstractors was assessed on a random subset of 40 observations and the resulting Cohen κ was 0.88. Further details regarding abstraction of PS are provided elsewhere [26].

Automated Tumor Registry and Claims Data

Automated tumor registry and claims data were accessed to obtain patient demographic characteristics, date of cancer diagnosis, stage at diagnosis, and comorbidities in the 12-month period preceding diagnosis for each patient. Patient demographics included age,

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gender, and race. The age of the patient (in years) was recorded as of the date of lung cancer diagnosis. Clinical measures for each patient included stage of disease at the time of diagnosis and the Charlson comorbidity index [27]. Cancer stage was reported using the American Joint Committee on Cancer (AJCC) stages II through IV. The Deyo adaptation of the Charlson comorbidity index and each of its component diagnostic subgroups were constructed using inpatient and outpatient diagnostic information available in the 12-month period preceding diagnosis [28].

Socioeconomic Data

Socioeconomic information, including education level, median household income, and vehicles per household were obtained from the 2000 US Census. Using patients' residential street address, Census tract level data were used to characterize the socioeconomic profile of each patient's neighborhood of residence. We also used MapPoint (2010; Microsoft Corporation, Redmond, WA) to calculate the travel distance between each patient's home and the nearest chemotherapy facility that was affiliated with the group practice.

Statistical Analysis

We assigned patients into 4 distinct groups: patients with good PS (0–2, as baseline) who received chemotherapy; patients with good PS who did not receive chemotherapy; patients with poor PS who received chemotherapy; and patients with poor PS who did not receive chemotherapy. Systematic unadjusted differences between patients receiving first line chemotherapy and those who did not receive chemotherapy, within the good PS and poor PS groups (patients in the first two groups and patients in the latter two groups) were examined, using 2-sample Student *t* tests (or Wilcoxon rank sum tests) and chi-square tests, depending on the nature of the characteristic. Two multivariate logistic regression models were fit to evaluate the factors associated with receipt of first line chemotherapy, given the patient's PS. The first model estimated chemotherapy receipt among patients with good PS (i.e., evaluated factors associated with chemotherapy under use) while the second model estimated chemotherapy receipt among patients with poor PS (i.e. evaluated factors associated with chemotherapy overuse). In both models, we controlled for patient age at diagnosis, gender, race, and comorbidities as well as the college graduation rate, median household income, and vehicle access in their neighborhood, distance to nearest chemotherapy facility, and year of diagnosis.

We used SAS statistical software (version 9.1.3; SAS Institute Inc, Cary, NC) for all analyses, and considered $p < .05$ to be statistically significant. All tests for statistical significance were two sided.

Results

Cohort Characteristics

A total of 1,099 NSCLC patients were originally considered for the study, but only 406 met the criteria for study eligibility; we excluded 385 patients whose AJCC stage was either 0, 1, or unknown, 162 patients who did not meet the minimum enrollment criteria, 110 patients whose PS was unknown because it was not documented in the chart, and 36 patients who died within one month of diagnosis. Overall sample characteristics are reported in Table 2. The mean age of the cohort was 67.4 years (standard deviation [SD], 8.9 years). Just under half (41%) were female, whereas the racial distribution was 69% white, 29% black, and 2% of other races. The AJCC staging distribution was as follows: 11% of patients were diagnosed with stage II disease, 41% were diagnosed with stage IIII disease, and 48% were diagnosed with stage IV disease. The average Charlson comorbidity index across the sample was 1.3 (SD, 1.6).

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At the Census tract level, the mean college graduation rate for the cohort was 6.9% (SD, 5.6), the median household income (in 2000) was $49,200 (SD, 21,900), and 12.2% (SD, 19.0) of residents lived in households that had no vehicles. The average travel distance of patients to the nearest chemotherapy facility was 10.8 miles (SD, 11.7).

Across the sample, 13% of patients received no anti-cancer treatment for their lung cancer, 1% received surgery only, 16% received radiation therapy only, 13% received chemotherapy only, 5% received a combination of surgery and chemotherapy but no radiation, 44% received radiation and chemotherapy but no surgery, and 8% received all three modes of treatment.

Chemotherapy Receipt by Performance Status

Table 3 reports receipt of first line chemotherapy by stage and PS. Overall, 77 patients (19%) with good PS (0–2) did not receive chemotherapy, while 39 patients (10%) with poor PS received chemotherapy. Among patients diagnosed with stage II disease, 9 (20%) with good PS did not receive chemotherapy and 6 (14%) with poor PS received it. Among those diagnosed with stage III disease, 31 (19%) with good PS did not receive chemotherapy and 9 (5%) with poor PS received it. Among those diagnosed with stage IV disease, 37 (19%) with good PS did not receive chemotherapy and 24 (12%) with poor PS received it.

Table 4 reports the unadjusted differences in cohort characteristics between patients receiving first line chemotherapy and those not receiving it, across 2 groups: patients with good PS and poor PS. Among patients with good PS, there were significant differences between those receiving chemotherapy and those not receiving it by age at diagnosis, the patient's Charlson comorbidity index, and the vehicle ownership rate in the patient's neighborhood. Among patients with poor PS, there were significant differences between those receiving chemotherapy and those not receiving it by age and median household income in the patient's neighborhood.

Factors Associated with the Non-Receipt of Chemotherapy among Patients with Good Performance Status

Results from the multivariable logistic regression model for factors associated with the non-receipt of chemotherapy among patients with good PS are presented in Table 5. As indicated in the model, patients who are significantly less likely to receive chemotherapy when the PS is good include older patients, patients residing in neighborhoods with higher median household income, and those living in neighborhoods with a higher percentage of households without any vehicle. At the same time, patients who are more likely to receive chemotherapy when their PS is good include patients residing in neighborhoods with a higher percentage of college graduates and patients diagnosed in 2003 or later. Factors that were not significant in this model included gender, race, comorbidities, and distance to nearest chemotherapy facility. Results from models in which patients with PS=2 (n=44) were aligned with the poor PS group (as opposed to the good PS group) were neither statistically nor substantively different (data not shown).

Factors Associated with the Receipt of Chemotherapy among Patients with Poor Performance Status

Table 6 reports the results of the logistic regression model for chemotherapy receipt among patients with poor PS. Factors that were associated with significantly higher odds of chemotherapy receipt when PS is poor include median household income and being diagnosed in 2003 or later. Older patients and those who lived in neighborhoods with a higher percentage of college graduates are less likely to receive chemotherapy when they have poor PS. Factors that were not significant in this model included gender, race,

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comorbidities, vehicle access, and distance to nearest chemotherapy facility. Results from models in which patients with PS=2 (n=44) were aligned with the poor PS group were neither statistically nor substantively different (data not shown).

Discussion

Performance status is widely recognized as a predictor for treatment appropriateness and reducing chemotherapy to patients with poor PS has been recommended as one way to increase health care quality and reduce costs [29]. Using the first large cohort of patients with lung cancer for whom PS is known, we found the overall adherence to evidence-based guidelines for chemotherapy treatment to be 71%. Among those whose care was non-concordant with guideline recommendations, 19% did not receive chemotherapy when it was indicated and 10% received chemotherapy when it was not recommended. We recorded a higher adherence rate than previous lung cancer studies that used population-based Medicare data and did not control for PS [5–8]. We find that older patients are less likely to use chemotherapy, regardless of their PS. That is, among patients with good PS, older patients are less likely to receive recommended chemotherapy, and among patients with poor PS, they are also less likely to receive chemotherapy. While the latter likely implies high quality care, the former does not since elderly patients derive benefit too [25]. Variations in the receipt of chemotherapy by age are consistent with findings from previous studies [5–8]. Whether this is a result of patient preferences or barriers, physician bias, or a combination of these is not known.

The higher the median household income in the patient's neighborhood of residence the more likely they are to be out of compliance in both directions, both "overusing" and "underusing" chemotherapy. As with our findings of differences in chemotherapy use by patient age, we are not able to determine the extent to which observed utilization is a result of patient preferences or barriers, physician bias or a combination of these.

Unlike other studies that analyzed Medicare claims data, we considered a rarely studied managed care cohort that included younger patients as well as older ones, although our median age of 67 was close to the US median age at diagnosis of 71 [30]. Another notable difference of this study is that we found no racial differences in the receipt of chemotherapy (either underuse or overuse). This difference may be attributable to two factors. First, our study population consisted of patients who received their care through a managed care plan, whereas previous studies have focused mainly on seniors with traditional Medicare (i.e., Parts A and B), not enrolled in a Medicare managed care plan (i.e., Part C). Research suggests that managed care plans reduce health care disparities, at least for some broadly defined measures of access to care [31]. Second, unlike prior studies [5, 6, 8], we were able to control for PS as well as several socioeconomic characteristics at the census tract level, i.e., education, income, and car ownership. It may not be race, per se, that leads to previously documented treatment disparities, but rather PS and socioeconomic characteristics (both of which are highly correlated with race), that underlies observed chemotherapy treatment patterns.

Finally, among the urban/suburban population studied here, we did not find travel distance to be associated with recommended chemotherapy treatment. Instead, we found that if fewer households in a patient's neighborhood had access to a car, this travel barrier was associated with underuse of chemotherapy relative to guideline recommendations. This finding implies that even among a non-rural population, the presence of transportation barriers is an important predictor of the underuse of chemotherapy among patients with good PS. Thus, despite the health system in which this study was conducted having multiple and geographically dispersed clinics that offer chemotherapy treatment, our findings suggest that

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patients without access to a car may have difficulty reaching a clinic, even when there is a clinic a relatively short distance from their home. This suggests a helpful question to ask on intake screening: “Will you have difficulty getting to your next appointment?”

The results of the current study should be interpreted in light of the following limitations. First, these findings are based on a cohort of insured cancer patients, and adherence rates as well as the factors associated with them may differ among an uninsured population. Similarly, models were developed on a sample of patients receiving their care from one delivery system located in a large urban area. Therefore, care should be taken when generalizing findings to other delivery settings and locales. Likewise, models may exclude important factors associated with chemotherapy receipt including provider characteristics and variations across health systems and geographical regions. However, the average age and other characteristics of our cohort are similar to the whole U.S. Finally, this study was not intended to assess appropriateness of specific chemotherapy regimens and further it is not known whether chemotherapy was given with good intent for palliative reasons in lieu of hospice to those with poor PS, and if there was any subsequent impact on symptom burden or hospitalizations for side effects.

In summary, about 71% of patients in an insured population received chemotherapy concordant with guideline recommendations based on performance status, but 29% did not. There will be over 222,000 people diagnosed in the U.S. with lung cancer in 2011 [32]. Given the effectiveness of modern chemotherapy for palliation and prolonged survival, 19% of patients almost certainly did not live as long or as well as they might have with chemotherapy, and about 10% of patients received chemotherapy that had little chance of benefit and excess risk of toxicity including hospitalizations, excess cost, and delay of entry into hospice.

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Table 1

ASCO Recommendations for Chemotherapy by Eastern Cooperative Oncology Group (ECOG) Performance Status (PS) and Year

	ECOG PS		
Year	**0–1**	**2**	**3–4**
1997[1]	Chemotherapy appropriate	Chemotherapy possibly appropriate	Not appropriate due to toxicity and lack of response
2003[2]	Combination chemotherapy recommended	No change; chemotherapy possibly appropriate. If chemotherapy is given, single agent chemotherapy recommended	Not recommended due to toxicity and lack of response
2009[3]	No change	No change	

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Table 2

Overall Sample Characteristics, Lung Cancer Patients, Stages II–IV (n = 406)

Demographic Characteristics	
Average age at diagnosis (SD)	67.4 (8.9)
Gender (%)	
Female	41
Male	59
Race (%)	
Black	29
White	69
Other	2
Clinical Characteristics	
AJCC stage (%)	
II	11
III	41
IV	48
Average Charlson comorbidity index (SD)	1.3 (1.6)
Socioeconomic Characteristics	
Pct with college degree (SD)	6.9 (5.6)
Median household income in $1000s (SD)	49.2 (21.9)
Access to Treatment	
Pct without vehicle (SD)	12.2 (19.0)
Distance (miles) to chemotherapy facility (SD)	10.8 (11.7)
Treatment(s) Received	
No treatment (%)	13
Surgery only (%)	1
Radiation therapy only (%)	16
Chemotherapy only (%)	13
Surgery + radiation therapy (%)	0
Surgery + chemotherapy (%)	5
Radiation + chemotherapy (%)	44
Surgery + radiation + chemotherapy (%)	8

SD: Standard deviation

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Table 3

Chemotherapy Receipt by Performance Status (PS), (N = 406)

		Chemotherapy Received?	
		Yes	**No**
All Cases, (N = 406)			
	0–1	213 (52%)	63 (16%)
ECOG PS	2	30 (7%)	14 (3%)
	3–4	39 (10%)	47 (12%)
Stage II, (N = 44)			
	0–1	25 (57%)	9 (20%)
ECOG PS	2	1 (2%)	0 (0%)
	3–4	6 (14%)	3 (7%)
Stage III, (N = 165)			
	0–1	99 (60%)	25 (15%)
ECOG PS	2	8 (5%)	6 (4%)
	3–4	9 (5%)	18 (11%)
Stage IV, (N = 197)			
	0–1	89 (45%)	29 (15%)
ECOG PS	2	21 (11%)	8 (4%)
	3–4	24 (12%)	26 (13%)

ECOG: Eastern Cooperative Oncology Group

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Table 4

Sample Characteristics, by Performance Status (PS) and Choice of Chemotherapy Receipt or Non-Receipt, for Lung Cancer Stages II–IV (n = 406)

	Good PS (n = 320)		**Poor PS (n = 86)**	
	Chemo (n = 243)	**No Chemo (n = 77)**	**Chemo (n = 39)**	**No Chemo (n = 47)**
Demographic Characteristics				
Average age at diagnosis (SD)	65.5 (8.5)[a]	71.4 (8.9)[a]	66.9 (9.0)[b]	71.1 (7.5)[b]
Gender (%)				
Female	41	47	36	36
Male	59	53	64	64
Race (%)				
Black	26	30	31	36
White	72	66	69	58
Other	2	4	0	6
Clinical Characteristics				
AJCC stage (%)				
II	11	12	15	6
III	44	42	23	39
IV	45	48	62	55
Average Charlson comorbidity index (SD)	1.0 (1.4)[a]	1.4 (1.5)[a]	1.9 (2.0)	2.3 (2.3)
Socioeconomic Characteristics				
Pct with college degree (SD)	7.1 (6.0)	6.1 (4.9)	6.7 (5.9)	6.9 (4.4)
Median household income in $1000s (SD)	50.4 (22.2)	47.6 (19.8)	52.4 (27.8)[b]	42.7 (16.6)[b]
Access to Treatment				
Pct without vehicle (SD)	10.0 (15.1)[a]	15.9 (21.8)[a]	12.8 (17.7)	16.3 (29.3)
Distance (miles) to chemotherapy facility (SD)	11.3 (13.6)	10.6 (8.7)	9.5 (8.0)	9.6 (6.4)
Treatment(s) Received				
No treatment (%)	-	42	-	45
Surgery only (%)	-	5	-	2
Radiation therapy only (%)	-	52	-	53
Chemotherapy only (%)	17	-	31	-
Surgery + radiation therapy (%)	-	1	-	-
Surgery + chemotherapy (%)	8	-	-	-
Radiation + chemotherapy (%)	62	-	64	-
Surgery + radiation + chemotherapy (%)	13	-	5	-

Good PS: ECOG 0–2, Poor PS: ECOG>2

SD: Standard deviation

[a] Among patients with good PS, significant difference by chemotherapy receipt/non-receipt, at 5% level

[b] Among patients with poor PS, significant difference by chemotherapy receipt/non-receipt, at 5% level

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Table 5

Factors Associated with Non-Receipt of Chemotherapy among Patients with Good Performance Status (PS), for Lung Cancer Stages II–IV (N = 320)

Performance Status = Good	**Odds of Under Use (95% CI)**	***P* Value**
Patient Demographics		
Age at diagnosis (years)	1.09 (1.05–1.13)	<0.01
Gender = female	1.35 (0.77–2.37)	0.29
Race = white	0.75 (0.36–1.58)	0.46
Clinical Characteristics		
Charlson comorbidity index	1.16 (0.96–1.39)	0.13
Socioeconomic Characteristics		
College degree	0.93 (0.86–1.00)	0.07
Median income ($1000s)	1.02 (1.00–1.04)	0.05
Access to Treatment		
Pct without vehicle	6.96 (1.00–49.34)	0.05
Distance to chemo facility	1.00 (0.99–1.03)	0.50
Guidelines		
Year of diagnosis < 2003	2.05 (1.17–3.62)	0.01
Model Performance		
Pseudo-R^2		0.13
C-statistic		0.74

Good PS: ECOG 0–2

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Table 6

Factors Associated with Receipt of Chemotherapy among Patients with Poor Performance Status (PS), for Lung Cancer Stages II–IV (N = 86)

Performance Status = Poor	**Odds of Over Use (95% CI)**	***P* Value**
Patient Demographics		
Age at diagnosis (years)	0.92 (0.86–0.98)	0.01
Gender = female	0.87 (0.32–2.38)	0.79
Race = white	0.83 (0.24–2.85)	0.77
Clinical Characteristics		
Charlson comorbidity index	1.01 (0.78–1.32)	0.92
Socioeconomic Characteristics		
College degree	0.89 (0.78–1.02)	0.08
Median income ($1000s)	1.05 (1.01–1.10)	0.02
Access to Treatment		
Pct without vehicle	3.41 (0.25–46.81)	0.36
Distance to chemo facility	0.97 (0.90–1.04)	0.38
Guidelines		
Year of diagnosis ≥ 2003	3.24 (1.07–9.85)	0.04
Model Performance		
Pseudo-R^2		0.19
C-statistic		0.75

Poor PS: ECOG>2

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Nonemergency Medical Transportation and Health Care Visits among Chronically Ill Urban and Rural Medicaid Beneficiaries

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Nonemergency Medical Transportation and Health Care Visits among Chronically Ill Urban and Rural Medicaid Beneficiaries

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Inaccessibility to health care services due to lack of transportation affects the most vulnerable segments of the society. The effect of Medicaid-provided nonemergency medical transportation (NEMT) in Oklahoma on health care visits for the management of chronic illnesses is examined. Analyses of claims data show that African Americans are the highest users of NEMT. Medicaid beneficiaries who use NEMT services are significantly more likely to make the recommended number of annual visits for the management of chronic conditions than those who do not use NEMT. Increased use of NEMT by making the services more accommodating and convenient for beneficiaries is proposed.

Keywords: Medicaid, nonemergency medical transportation, NEMT, health care visit

The Patient Protection and Affordable Care Act (PPACA; 2010), signed into law by President Obama on March 23, 2010, will extend coverage to those without access to health care (Marsico, n.d.; PPACA, 2010). The thrust of the legislation is to reduce health care costs and improve access. Buried in the avalanche of studies about health insurance and access to care (Brook et al., 1984; Freeman & Corey, 1993; Herman, Walsh, & Rissi, 2011; Hoffman & Paradise, 2008) is a small but significant group of individuals who have health insurance coverage but go without health care because they do not have the means to travel to health care facilities.

Medicaid, Title XIX of the Social Security Act, is an entitlement program that was signed into law on July 30, 1965. It ensures insurance coverage for medical services to indigent children, adults, elderly, and individuals with disabilities (Koch, 1993). Although almost all public and commercial health insurance plans provide coverage for transportation to emergency medical care, entrenched in the Medicaid law is a rare provision that ensures transportation coverage for nonemergency, but medically necessary, care. Recognizing the futility of providing financial coverage for medical services that Medicaid beneficiaries cannot reach physically, and thus, in essence, depriving a beneficiary access to legally entitled care, the law requires states to ensure

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beneficiaries round trip transportation for medical services. The law also mandates that the states give details on how this would be accomplished (Rosenbaum, Lopez, Morris, & Simon, 2009).

The mandate for achieving administrative efficiency by ensuring necessary transportation was not stated explicitly in the original Medicaid law when it was enacted in 1965. However, stipulations in the law and several successful litigations brought against states by beneficiaries, not the least of which was the first lawsuit, *Smith v. Vowell* (1974), brought by a beneficiary with cerebral palsy against the state of Texas for denying him transportation for medical services, have led to clearer expressions of regulations regarding states' transportation obligations (Rosenbaum et al., 2009; *Smith v. Vowell*, 1974).

As the states have considerable latitude in defining the program within federal guidelines, there are substantial variations among states in how the ensurance of necessary transportation is achieved. Nevertheless, the legislation requires that states provide the least expensive and appropriate mode of travel to and from providers. States have the flexibility of obtaining federal matching funds for transportation either as an administrative service, at 50% match, or as a medical service, at 50% to 83% match. In 2006, states spent more than $3 billion of the Medicaid budget on nonemergency medical transportation (NEMT) that, though only a fraction of the Medicaid budget, accounted for a substantial segment of the federal transportation spending. The 2006 Deficit Reduction Act gave states the option of using intermediaries, such as transportation brokers, to manage and execute all aspects of the transportation mandate (Rosenbaum et al., 2009).

PPACA (2010) is expected to add millions of new low-income beneficiaries to the Medicaid program in the next 4 years. Despite initial concerns, the ensurance of NEMT has been retained in the new legislation. It is expected to lead to a surge in the number of Medicaid beneficiaries eligible for transportation services that, in turn, is anticipated to raise the demand for community and public transit systems (Marsico, n.d.). The association between NEMT and access to care for the management of chronic conditions is assessed in this study.

LITERATURE REVIEW

An examination of the literature reveals the size and vulnerability of those who do not have transportation to necessary medical care. It is estimated that at least 3.6 million people a year in the United States forgo needed medical care solely because they either do not own or operate a personal vehicle or because they cannot afford the cost of public or private transportation. This population, which consists largely of the economically disadvantaged, less educated, women, elderly, individuals with disabilities, and members of racial and ethnic minority groups, has to rely on the convenience of relatives, friends, or neighbors for rides. The dependence on informal sources can adversely affect access to medical care in the form of missed appointments, late arrivals, delayed care, and inadequate follow-up visits (Hughes-Cromwick, Wallace, Mull, & Bologna, 2005). At an in-center for hemodialysis, social workers noted that due to difficulties with transportation patients typically skipped 4.5 visits per month. Difficulties reported by patients included problems arranging trips, length of waiting time for the rides, and discord with drivers (Iacono, 2004). In another study of dialysis patients, African Americans were more likely to miss or shorten dialysis treatment due to difficulties with transportation than members of other races (Gordon, Leon, & Sehgal, 2003).

The health status of those who do not receive medical care due to transportation barriers is considerably worse than the rest of the U.S. population. Analysis of national data revealed that as many as 66% of those who were unable to find transportation for needed care endured asthma, hypertension, heart disease, chronic obstructive pulmonary disease, diabetes, end-stage renal disease, and mental health conditions. Not only was the occurrence of these chronic conditions greater in this population than in the general population, but the percentage of individuals experiencing more than one chronic illness at a time was considerably higher, and so was the seriousness of those conditions. The analysts add that this population without transportation is also likely to reside in localities with high concentrations of poverty that can lead to further deterioration of health and the need for more health care visits. The investigators conclude that, for such an unhealthy population, skipping needed medical care due to inadequate transportation could result in unnecessary emergency room use, expensive hospitalizations, avoidable surgeries, and reduced quality of life (Hughes-Cromwick et al., 2005).

Even when public transit services are available, its cost and inconvenience can deter its use. In a focus group interview of financially disadvantaged women from inner-city neighborhoods of Detroit, Michigan, 90% of whom were unemployed but had insurance coverage, transportation was the most frequently cited obstacle to obtaining prenatal and well-baby care. Participants described the difficulties of using the public transit system. Besides such barriers as high bus fares, unpredictable bus schedules, and long bus rides, participants noted that health care facilities were not on direct bus lines. Consequently, women had to ride more than one bus, which required them to change buses, sometimes with children in strollers, and wait with their children at bus stops located in dangerous locations, at times in bad weather, before reaching the medical facilities. Hispanic women reported that they did not use the public transit system because they lacked knowledge of the English language needed to communicate with bus drivers (McCray, 2011).

A project that examined barriers to health care experienced by members of minority races in some of the most impoverished neighborhoods in the Bay Area also revealed several transit deprivations of residents that could put, among others, those with chronic conditions especially at peril. Many racial and ethnic minority households in the neighborhoods lacked personal vehicles; poorly planned public transit systems made routes to community clinics, hospitals, grocery stores, and recreational parks nonexistent; reduction of medical services and mergers or shut down of medical facilities located in economically disadvantaged areas exacerbated inaccessibility; and African American pedestrians were several times more likely to be victims of traffic fatalities and injuries than Whites, making neighborhoods dangerous for walking and bicycling. The authors note that given the high rate of illnesses among economically disadvantaged racial minorities, such transportation deprivations are likely to widen the health care chasm among the races. Yet, the authors noted further, the focus of government spending on transportation had been on reducing traffic congestion for white-collar commuters who travelled from afar. In an extraordinary display of community organization, members of these impoverished neighborhoods, committed to racial justice, joined forces with supporters of the public transit system, and succeeded in shifting the Bay Area's Metropolitan Transportation Commission's focus toward investing funds that would enhance the public transit system. The primary goal of the project was to improve the health of low-income racial and ethnic minority communities through improved transportation (Hobson, Quiroz-Martinez, & Yee, 2002).

Although rural areas have a higher proportion of residents with chronic conditions (Braden & Beauregard, 1994), they have fewer visits to specialists and more hospital admissions than urban areas (Schur & Franco, 1999). Self-reported measures of fair or poor health are also higher in rural areas (Braden & Beauregard, 1994). Yet almost 25% of those living in rural areas had not been to a physician in 12 months, and 10% to 13% of rural dwellers in self-described fair or poor health had not visited a physician in a year (Schur & Franco, 1999). Although rural and urban residents on Medicaid have the assurance of insurance coverage, the shortage of physicians in rural areas poses considerable difficulty for Medicaid recipients. Such difficulties arise because when the few providers there are do not accept Medicaid recipients, rural residents must travel far to receive care from providers who do accept Medicaid patients.

Rural residents encounter more barriers to health care access than urban residents, and their difficulties increase with the degree of geographic isolation. Much of the isolation from health care services can be overcome by transportation. However, poor roads, absence of interstate

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highways, inhospitable terrain, lack of automobiles, inadequate public transportation, distance to providers, and longer travel time deter many rural residents from visiting medical facilities (Ricketts, Johnson-Webb, & Randolph, 1999).

To estimate the net benefit of providing NEMT, Hughes-Cromwick et al. (2005) selected, among others, seven chronic conditions (asthma, congestive heart failure [CHF], chronic obstructive pulmonary disease, hypertension, diabetes, end-stage renal disease, and depression) that are most responsive to disease management, that are pervasive in populations without transportation, and that account for a large segment of the medical budget. To determine the number of visits required to manage each chronic condition, the researchers reviewed the disease management literature and consulted the Disease Management Association of America, experts from various disease groups, federal agencies committed to quality, and medically established guidelines. Factoring in this data on the recommended number of visits for each chronic condition, as well as the difference in per-capita medical cost of treating a well-managed and poorly managed patient, the percentage of patient compliance with the treatment regiment suggested by the literature, the average cost of transportation by the area and type of vehicle, and adjusting for the quality of life resulting from the medical intervention, the investigators estimated the net-profit of providing NEMT to patients without transportation for each chronic condition. Results showed an estimated cost saving of $333 per person per year for asthma, $2,743 per person per year for CHF, and for patients with hypertension NEMT could add one quality adjusted life year for a mere $6.00 (Hughes-Cromwick et al., 2005).

Given the estimated savings in health-care costs and improved quality of life by providing NEMT to the chronically ill who cannot travel to health care facilities, this study analyzes the differences in health care visits between those who use NEMT services and those who do not among Medicaid beneficiaries in Oklahoma with asthma, heart disease, and hypertension. The hypothesis is that there will be a difference in the number of visits made to providers by those who use NEMT services and those who do not. Individuals who use the NEMT services are more likely to complete the recommended number of visits needed to manage their chronic conditions than those who do not use NEMT services.

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METHOD

Study Setting

Oklahoma Health Care Authority (OHCA), established in 1993, administers the Medicaid program in Oklahoma. The state switched from a traditional fee-for-service Medicaid program to a Health Maintenance Organization (HMO) following the creation of the OHCA. From 1996 to 2004, the state operated two distinct managed care delivery systems: SoonerCare Plus and SoonerCare Choice. Under the SoonerCare Plus option, the OHCA contracted directly with HMOs to provide medical services only in urban counties whereas SoonerCare Choice operated in the rest of the state. In 2004 OHCA ended its HMO contracts and expanded SoonerChoice Care to cover the urban areas as well.

Nonemergency medical transportation (NEMT) has been a part of the Medicaid program since 1969 (fee-for-service). In 1999 the Metropolitan Tulsa Transit Authority (MTTA) began operating SoonerRide in seven counties. In 2000 SoonerRide was expanded to cover the entire state under a capitated agreement with MTTA. The MTTA contracted with rural and community transit agencies throughout the state. In addition to contracting with providers, the MTTA's responsibilities included screening trip requests for eligibility, selecting the most appropriate provider, monitoring service, and reimbursing providers. In 2002, the OHCA applied for a Medicaid 1915 (b) (4) waiver to contract out the SoonerRide program to a broker and receive the state's medical service federal

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match rate (>70.5%) for the services. In June 2003, in an attempt to improve cost-savings, NEMT services were contracted out to LogistiCare Solutions, the nation's leading provider of specialized transportation network management (Oklahoma Health Care Authority, 2005–2006). Non-Emergency Medical Transportation through LogistiCare is available to all Medicaid recipients in Oklahoma. Information for contacting LogistiCare Solutions to schedule a ride is available to all recipients at the back of the Medicaid card.

Sample

The data set consisted of Medicaid beneficiaries with chronic conditions who resided in Oklahoma and who received services from a health care provider between January 1, 2005, and December 31, 2005. The sample was restricted to beneficiaries who were over 20 years of age on January 1, 2004, and, thus, children were excluded from this study. The sample was limited to individuals with asthma, heart disease (CHF), and hypertension. The process resulted in a sample of 10,824 adults.

Data Source

Data were provided by the Oklahoma Health Care Authority. Using a unique member identification number, the OHCA isolated demographic information, residential zip code, and primary diagnosis of medical condition for each individual at the time of each visit. These were then combined with claims data to obtain information on the date of visit to the health care facility and the date when transportation was provided for each identification number. Finally, the identifying numbers and claims numbers were de-identified, and the data were provided to the investigators in Excel format.

Selected Variables

Rural-urban classification. Several classification mechanisms have been used to designate localities and populations as either rural or urban (Ricketts et al., 1999). In this study, Rural-Urban Commuting Area code (RUCA) developed by investigators from the University of Washington and Economics Research Service was used. In developing RUCA, the investigators determined the level of urbanization of each area by using its population size, commuting pattern of its residents, and the size and urbanization of the area to which the commuters travelled for work (Danaher, Hart, McKay, & Severson, 2007). A version of RUCA taxonomy now offers values based on zip-codes. Hence, the RUCA values for zip codes in Oklahoma were merged with the zip codes of Medicaid beneficiaries provided in the OHCA data set to generate RUCA values for the zip codes of each of the Medicaid beneficiaries in the OHCA data set. The RUCA values were then aggregated to define the area of residence of each of the Medicaid beneficiaries as either urban or rural. The rural areas included large rural towns, small rural towns, and isolated small rural towns.

Chronic condition and number of visits. Chronic condition was the primary diagnosis determined by the provider at the time of the patient visit and noted in the claims data. Number of visits was calculated from the dates when the beneficiary visited a provider. Using information from an earlier study (Hughes-Cromwick et al., 2005) on the number of visits recommended for disease management, the management of each chronic condition was divided into two categories. Poorly managed were those who made fewer visits than the recommended number per year, and well-managed were those who made the recommended number of visits or more per year to the

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health care provider. The characteristics of each of the three chronic conditions in this study and the recommended number of visits are as follows.

Asthma is a chronic inflammation of the airways which limits the flow of air and leads to repeated incidences of breathlessness, coughing, wheezing, and chest tightness that can be controlled, among other methods, by medication and medical supervision through periodic visits to the physician (U.S. Department of Health and Human Services National Asthma Education and Prevention Program, 2007). Neglect can lead to being rushed to the emergency room, hospitalization, and mortality. Depending on the severity of the condition, clinical guidelines recommend two to 12 visits to medical personnel per year for well-managed care (Hughes-Cromwick et al., 2005).

Heart disease or CHF is the primary cause of death in the United States. It affects 2% of the population though the mortality rate increases with age and is higher among men than women (Schocken, Arrieta, Leaverton, & Ross, 1992). When compared by race and ethnicity, African Americans are at the greatest risk, followed by Hispanics, and trailed by Whites, and Chinese Americans (Bahrami et al., 2008). Recommended management of the condition involves 10 visits to the physician per year two of which would be to cardiology specialists (Hughes-Cromwick et al., 2005).

Hypertension affects approximately 33% of the United States population who are age 18 years and older, and more than one half of these conditions are not managed. The incidence is more prevalent and occurs at a younger age among African Americans than among individuals from other races. It is treated through life-style changes and medication. Uncontrolled hypertension can have serious consequences, such as stroke, cardiovascular disease, kidney disease, and early death (Centers for Disease Control and Prevention [CDC], 2011, n.d.). Recommended standard for the proper management of the condition is a minimum of four visits per year to a primary care physician (Hughes-Cromwick et al., 2005).

Transportation use. Transportation use was a dichotomous variable. Those who used LogistiCare NEMT at least once were classified as transportation users (used NEMT), whereas those who did not use LogistiCare NEMT-provided at all were classified as nontransportation users (not used NEMT).

Statistical Analysis

The association between NEMT use and management of chronic conditions, determined by the number of visits, was assessed using frequencies and bivariate analysis. A chi-squared (χ^2) test was used to determine the statistical significance of the associations. As the recommended number of visits for well-managed care differs by chronic condition, the relationship between NEMT and number of visits was analyzed separately for each chronic condition. Similarly, as the number of visits made to a health care provider can be affected by a number of environmental factors, such as, the availability of public transportation, the length of travel, the local culture that influences the value of health and health maintenance behavior (Strickland & Strickland, 1996), the effects of these variables were controlled by analyzing the relationship between transportation use and number of visits separately for urban and rural areas.

RESULTS

The total sample of 10,824 Medicaid beneficiaries with asthma, CHF, and hypertension consisted of 65.21% Whites, 22. 87% African Americans, 8.92% American Indians, 2.45% Hispanics, and 0.56% Asians (Figure 1). A total of 6.44% ($n = 697$) beneficiaries used NEMT services.

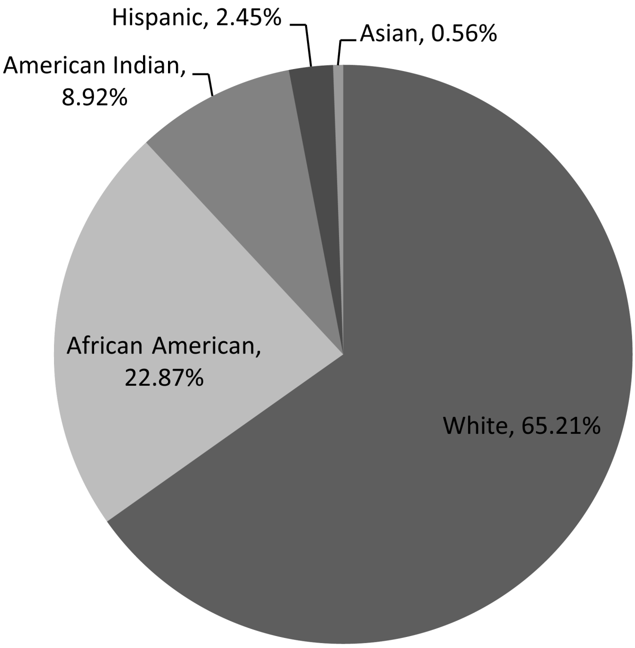

FIGURE 1 Racial distribution of Medicaid beneficiaries in Oklahoma with visits in 2005 for asthma, heart disease, and hypertension.

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Significant racial and ethnic differences were found in the use of NEMT services ($p < 0.001$). African Americans were the highest users of NEMT services (8.81%), followed by American Indians (6.11%), and Whites (5.79%). Hispanics (3.77%), and Asians (1.64%) were the least likely to use NEMT services (Figure 2).

Table 1 presents the results of the association between use of NEMT and number of visits for each chronic condition controlling for area of residence. In the case of asthma, two to 12 visits per year are recommended for appropriate management. A total of 48 (4.15%) Medicaid patients with asthma in the urban area used NEMT services of which 81.25% made the recommended number of visits to a provider, whereas of those who did not use NEMT services, only 54.82% made the requisite number of visits ($\chi^2 = 13.03$, $df = 1$, $p < 0.01$). Of the Medicaid beneficiaries with asthma in the rural areas ($n = 896$), there was no statistically significant difference in the recommended number of visits completed between those who used and did not use NEMT. However, the distribution was in the expected direction with those who used NEMT being more likely to make the proposed number of visits for well-managed care (60%) than those who did not use NEMT (52.70%).

Patients with CHF are advised to make at least 10 visits per year to a physician for appropriate maintenance of the condition. A sum of 43 (8.19%) Medicaid beneficiaries with CHF living in the urban areas used NEMT services. Of these, 55.81% made the prescribed number of 10 visits or more while only 28.22% of those who did not use the NEMT services made the recommended number of visits. This implies that though 44.19% of those who used NEMT services did not meet the standard of 10 visits per year, as many as 71.78% of those did not use NEMT services did not meet the standard. These differences were statistically significant ($\chi^2 = 14.19$, $df = 1$, $p < 0.01$). Among the rural Medicaid residents with CHF, 70.77% of those who employed NEMT services completed the 10 requisite visits a year or more for well-managed care whereas only

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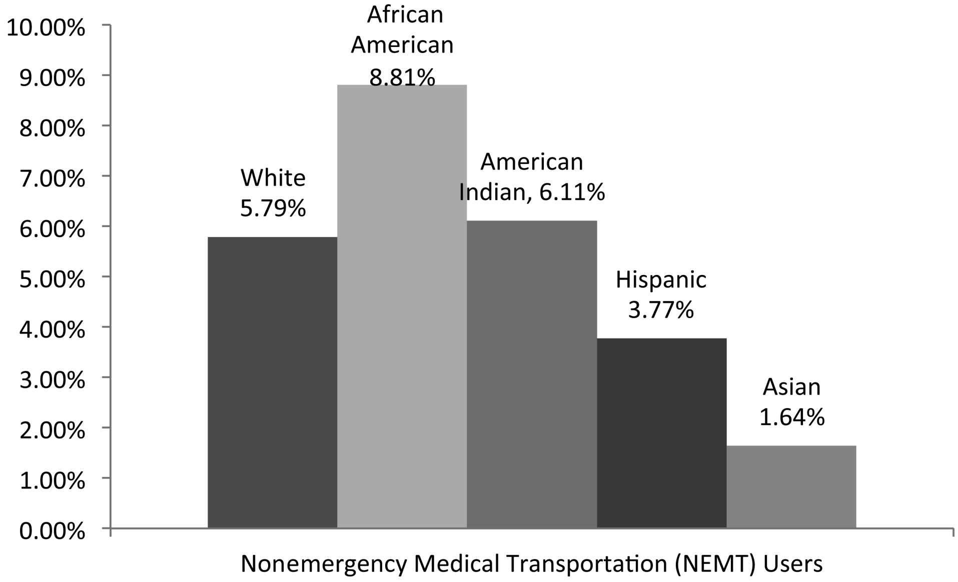

FIGURE 2 Distribution by race of Medicaid nonemergency medical transportation users with asthma, heart disease, and kidney disease.

TABLE 1
NEMT Use and Visits to Providers for Each Chronic Condition and Area of Residence

Chronic Condition/Area of Residence/Visits	*Used NEMT % (n)*	*Not Used NEMT % (n)*	χ^2	*df*	*p Value*
Asthma (Recommended 2–12 visits per year)					
Urban					
Made recommended number of visits or more	81.25 (39)	54.82 (608)			
Did not make recommended number of visits	18.75 (9)	45.18 (501)	13.03	1	<0.001
Rural					
Made recommended number of visits or more	60.00 (15)	52.70 (459)			
Did not make recommended number of visits	40.00 (10)	47.30 (412)	0.52	1	0.47
CHF (Recommended 10 visits per year)					
Urban					
Made recommended number of visits or more	55.81 (24)	28.22 (136)			
Did not make recommended number of visits	44.19 (19)	71.78 (346)	14.19	1	<0.001
Rural					
Made recommended number of visits or more	70.77 (46)	27.03 (143)			
Did not make recommended number of visits	29.23 (19)	72.97 (386)	51.04	1	<0.001
Hypertension (Recommended 4 visits per year)					
Urban					
Made recommended number of visits or more	50.17 (146)	27.50 (1,008)			
Did not make recommended number of visits	49.83 (145)	72.50 (2,658)	67.10	1	<0.001
Rural					
Made recommended number of visits or more	52.00 (117)	26.89 (933)			
Did not make recommended number of visits	48.00 (108)	73.11 (2,537)	65.51	1	<0.001

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27.03% of those who did not use NEMT services saw a health care provider 10 or more times a year. The difference between the two groups was statistically significant ($\chi^2 = 51.04$, $df = 1$, $p < 0.01$).

Clinical guidelines for the management of hypertension call for at least four outpatient visits per year (Hughes-Cromwick et al., 2005). Results show a significant difference between users and nonusers of NEMT services in the minimum number of trips made for the management of the condition in urban and rural areas. In the urban areas, 50.17% of the Medicaid beneficiaries with hypertension who used NEMT services made the recommended number of visits or more per year for the management of the condition whereas only 27.50% of those who used alternative modes of travel met the standard of at least four visits a year. This difference between NEMT users and nonusers in the number of visits completed was statistically significant ($\chi^2 = 67.10$, $df = 1$, $p < 0.01$). In the rural areas also more than one half (52.00%) of the Medicaid residents with hypertension who used NEMT visited the health care provider 4 times or more whereas only 26.89% of non-NEMT users did so, thus showing a statistically significant difference in the number of visits realized between users and nonusers of NEMT ($\chi^2 = 65.51$, $df = 1$, $p < 0.01$).

DISCUSSION

With data from the Oklahoma Health Care Authority (2005-2006), the effect of Medicaid-provided NEMT services on routine visits to health care providers' care for the management of chronic conditions was tested. This study focused on three conditions that are highly prevalent in the population without access to medically necessary but nonemergency care. Results showed that the most likely users of NEMT services are African Americans, which supports national and local estimates of minority races being the most vulnerable to medical inaccessibility from lack of transportation (Gordon et al., 2003). However, the least-used NEMT services by Hispanics and Asians are noteworthy. A possible explanation is language barrier as suggested by Hispanic residents of Detroit in focus group interviews (McCray, 2011). In discussions of transportation barriers to health care services (Hughes-Cromwick et al., 2005; Ricketts, 1999) investigators and policy makers have focused on the financial barriers to using public transit systems but rarely on the inconvenience of using the services. The fear of getting on the wrong bus, getting off at the wrong bus stop, and not being able to find the way to the medical facility or back home can be compounded for those who lack the language skills to seek help from bus drivers or fellow commuters. The complication of changing buses, due to lack of direct bus routes to health care facilities, is another impediment that can impede use of public transit systems for those with limited language skills. Transportation is a component of the culture of the health care system in the United States and, thus, there is a need for individuals to have the skills to advocate for transportation services within the bureaucracy that accompanies many Medicaid systems. Issues of language and literacy are important to any discussion in which policies are developed to address health determinants, such as transportation.

The findings support the proposed hypothesis of this study that there is a difference in the number of visits made to providers by those who use NEMT services and those who do not. However, this study has several limitations. The data lacked information on whether the visit was to a specialist or a general practitioner or to other provider. However given the chronic nature of the condition, any visit is assumed to be better than no visit at all. Also, a cautionary note is that when a provider treats a patient for a primary chronic condition, a secondary chronic condition may also get treated which may not get recorded. Nevertheless, available data show that for all three chronic conditions users of NEMT services in rural and urban areas had a higher probability of meeting the required number of maintenance visits to a provider. Even when the criterion for statistical significance was not met, the direction of the distribution suggested that users of NEMT

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services had a better chance of not missing appointments than those who did not use the NEMT services. It highlights the possible difficulties experienced by Medicaid beneficiaries who depend on the unreliability of their own vehicle or convenience of others.

Although, the percentage of NEMT users in Oklahoma (6.44%) is larger than the national average of 1.21% (Hughes-Cromwick et al., 2005), the proportion of NEMT users is small relative to the nonusers. Although the data accessible to this study lack information on the reasons Medicaid recipients do not use NEMT, a possible explanation could be that some Medicaid recipients had their own mode of transportation and did not need the service. However, given the low economic status of Medicaid recipients and the relatively high cost of operating personal vehicles, another explanation for the low use of NEMT despite its availability at no cost, could be the inconvenience of using the services. Medicaid's policy of providing the least expensive mode of transportation, which results in transportation brokers accommodating several passengers in one vehicle, can exacerbate for patients the of problems of arranging trips, waiting for rides, and arguing with drivers as reported by Iacono (2004). Moreover, the long trips and the constant stop and start associated with picking up and dropping off of several passengers can cause physical discomfort, such as nausea, for patients with chronic conditions.

Given the escalation in national health care expenditures, the policy decision to require the least expensive mode of transportation in an effort to curb Medicaid costs is understandable. Nevertheless, if NEMT services are not used and chronically ill Medicaid beneficiaries go without needed care, the purpose of providing NEMT and Medicaid coverage to increase access is defeated. Moreover, as suggested by the estimates of Hughes-Cromwick et al. (2005), investing in NEMT for the chronically ill actually reduces the total cost of health care. Hence, to increase the use of NEMT, along with the stipulation to find the cheapest mode of travel, policy makers must consider the medical condition of the patient and the commuting distances involved.

CONCLUSION

The study findings support the belief that it is important to make transportation services available to the poor and underserved among the chronically ill if they are expected to access available medical care and services. Furthermore, evidence is presented to indicate that users of NEMT are more likely than nontransportation users to meet the recommended number of visits to providers—a measure that suggests more successful outcomes. As political entities deliberate on challenging the implementation of health care reform, it will be important for health care reform advocates (especially at the state level) to monitor any proposed efforts to reduce NEMT services and to propose expansion where need can be demonstrated to exist.

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New Medicaid barrier: Waivers ending retrospective eligibility shift costs to providers, patients

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ABSTRACT (ENGLISH)

A history lesson and the impact on beneficiaries Retrospective eligibility was built into federal Medicaid law early on as a safety net protection for very low-income people and their medical providers. In 2017, Iowa received a CMS waiver of the 90-day retrospective eligibility requirement, including for nursing home residents, despite warnings that nursing homes would refuse to admit people who were awaiting Medicaid eligibility. [...]Iowa, which expanded Medicaid in 2014, has not conducted any review of the cost savings to the government or of the financial impact on providers and beneficiaries, according to a spokesman for the Iowa Department of Human Services. There's already an effort to roll back the new retrospective eligibility waiver in Florida, which didn't expand Medicaid, so that it applies to nursing home residents and all other Medicaid eligibles except pregnant women and children.

FULL TEXT

Last year, Jackson Memorial Hospital in Miami admitted an uninsured, low-income patient who stayed in the hospital for 86 days and ran up total charges of more than $1 million.

It took the public hospital's staff 65 days to complete a Medicaid application for the patient. Once it was approved, the Florida Medicaid agency covered bills for the previous 90 days, as per federal Medicaid policy in effect across the country since 1972. Jackson received a payment of $82,000, based on the state's limit of 45 covered hospital days per year.

But on Feb. 1, Florida ended retrospective Medicaid eligibility under a waiver granted by the CMS in November and effective through June, which likely will be extended. Now it will only cover claims back to the first day of the month in which an application is filed. The state projects this will save it and the federal government $100 million a year. The Trump administration so far has granted similar waivers to five other states.

If the waiver had been in effect last year, Jackson would have eaten that patient's entire bill. It estimates the new policy will cost the hospital at least $4 million a year in uncompensated care, and likely far more.

"We get trauma cases where we can't identify the patient or get documentation for weeks," said Myriam Torres, Jackson's vice president of revenue cycle. "This will save Medicaid dollars at providers' and patients' expense."

A costly incentive

Over the past two years, despite strong objections from hospitals and other provider groups, the CMS has granted waivers of 90-day retrospective eligibility to Arizona, Arkansas, Florida, Iowa and Kentucky. Some were part of broader Medicaid Section 1115 demonstrations of work requirements. Maine also received a waiver but its new Democratic governor announced she won't implement it. The CMS is considering similar waiver requests from Ohio and other states.

In its approval letters, the CMS argued that demonstrations ending 90-day retrospective eligibility will test whether that gives beneficiaries an incentive to enroll in Medicaid before they need healthcare services, so they can receive preventive services and stay healthier. It also says the change will facilitate a smoother transition of beneficiaries into commercial health plans, which don't offer retroactive coverage.

The CMS is requiring states to develop outreach and education strategies to encourage providers and beneficiaries to submit Medicaid applications as early as possible, though providers say they haven't seen any significant new state activity there.

A CMS spokesman said that as in all Section 1115 demonstration waivers, the agency is requiring states to monitor and regularly report the outcomes and financial impact.

But experts say there's no evidence that eliminating retrospective eligibility encourages Medicaid-eligible people to sign up earlier, and there are plenty of reasons why that hypothesis is implausible.

"Many people who aren't enrolled are not aware they are eligible or they have difficulty with the enrollment process," said Dr. Benjamin Sommers, an associate professor of health policy and economics at Harvard University. "The notion that most people will sign up by getting rid of retrospective eligibility is unlikely. They typically do not even understand it."

Critics say eliminating retrospective eligibility is one more administrative barrier the Trump administration has erected to make Medicaid and other public benefits harder to access. These include work and reporting requirements, premium payments, healthy behavior incentives, benefit lockouts, and proposed penalties for legal immigrants who use public programs. States like Arkansas that have added new hurdles have seen sharp drops in Medicaid enrollment.

"Shortening the (retrospective eligibility) window gives people less time to figure out they'd be eligible," said Pamela Herd, a public policy professor at Georgetown University, who calls that form of administrative burden a learning cost. "Republicans have employed these types of changes to reduce use of social welfare programs."

Changing nature of waivers

Under previous administrations, Delaware, Indiana, Maryland, Massachusetts, New Hampshire and Tennessee received waivers of the federal requirement for retrospective eligibility, typically as part of coverage expansions. In contrast, the Trump administration's waivers have been part of programs to restrict coverage.

Most of these waivers retain retroactive coverage for pregnant women, infants, disabled people and those in nursing homes. Florida's waiver, however, excludes such coverage for the nursing home population.

Herd and other experts say that if the goal is to get people to enroll as soon as they are eligible, there are proven ways to achieve that, such as streamlining the enrollment process and doing more aggressive outreach. The Trump administration has sharply cut funding for enrollment education and assistance.

On the other hand, if the goal is to reduce federal and state spending on Medicaid and shift costs to providers and patients, eliminating retroactive eligibility likely is effective.

Actuarial analyses of Medicaid payments have shown that about 5% of Medicaid payments occur during the retrospective eligibility period. Ending retrospective coverage would reduce Medicaid outlays by an estimated $13.3 billion from 2017 to 2026, according to the Commonwealth Fund.

In 2016, Indiana reported that 14% of beneficiaries to whom the waiver applied ran up significant out-of-pocket medical expenses as a result, averaging more than $1,500 per person. Sixteen percent of providers said they saw charity cases and bad debt increase as a result of the policy.

"If this is really an experiment, what is the policy goal other than to reduce program costs?" asked Joseph Antos, a conservative health policy analyst at the American Enterprise Institute. "Presumably this should have something to do with patient outcomes or efficiencies. I don't see the word efficiency in any of this. I see cut."

A history lesson and the impact on beneficiaries

Retrospective eligibility was built into federal Medicaid law early on as a safety net protection for very low-income people and their medical providers. It encourages providers to treat patients knowing they'll get paid and to help them sign up quickly for the program.

Another key rationale is that unlike in private insurance, many Medicaid beneficiaries "churn" on and off coverage due to changes in income and because states impose a demanding annual eligibility redetermination process. It's estimated that 25% or more of beneficiaries are at least temporarily disenrolled as a result of the redetermination process and other factors.

Many other people aren't even aware they are eligible. The Kaiser Family Foundation recently reported that 6.8

million uninsured adults and children were eligible for Medicaid but were uninsured in 2017.

All these factors leading to loss of coverage for eligible people makes retrospective eligibility an important backstop, patient advocates say.

But some state and federal officials long have complained about the cost of retroactive coverage, which generally can't be passed on to the private Medicaid plans that administer most state programs.

Tennessee received a waiver in 1994 as part of its major Medicaid coverage expansion program known as TennCare. Even though that program largely has been rolled back and the state has not expanded Medicaid under the Affordable Care Act, the elimination of 90-day retroactive coverage remains in place for nearly all beneficiaries.

That has led to many Medicaid-eligible people incurring large medical bills before their Medicaid applications are approved, with some facing lifetime debt, said Michele Johnson, executive director of the Tennessee Justice Center, which tries to help people clear up these bills.

The problem was exacerbated by a recent major computer glitch in the state's Medicaid enrollment system, which left thousands unable to file their annual enrollment redetermination applications online.

Before her Medicaid application was approved, one Memphis woman racked up $250,000 in bills resulting from her baby being born with severe health problems. "She said that was the hardest thing in her life--going home with a disabled child and being consigned to poverty for the rest of her life," said Johnson, whose group helped with her case.

After a nine-month court fight, the woman finally got Tennessee's Medicaid program to pick up the entire bill.

Yet there has never been a study of the policy's impact in Tennessee. "It hasn't led people to sign up ahead of time," Johnson said. "All these other policies make it almost impossible to sign up. If the state were interested in that, they would make the whole process less bureaucratic."

In 2017, Iowa received a CMS waiver of the 90-day retrospective eligibility requirement, including for nursing home residents, despite warnings that nursing homes would refuse to admit people who were awaiting Medicaid eligibility. Last year, under pressure from nursing homes, the state Legislature restored retroactive coverage for that population.

Brent Willett, CEO of the Iowa Health Care Association, said it takes an average of 71 days to assemble complicated income and assets information, file the application, and receive approval for Medicaid nursing home coverage. Under the policy the state reversed, facilities only received payment back to the first day of the month when the application was filed, even though they may have admitted the resident many weeks earlier.

The association projected that policy would cost Iowa nursing homes $7 million in the first year. "It sounds nice that people should start the application process early and we agree, but it's not practical in practice," Willett said. "If we are maintaining a system to ensure coverage for people who don't have assets for care, it makes no sense to penalize providers for providing that care. That policy wasn't cost containment, it was a cost shift to providers."

Iowa hospitals looking for a reversal

As to the broader group of beneficiaries affected by Iowa's waiver, the Iowa Hospital Association is pushing to have 90-day retroactive eligibility reinstated this year. The policy hurts urban trauma centers that provide intensive care to people before an application can be completed, as well as rural hospitals that lack a profit cushion to absorb those unexpected costs, said Scott McIntyre, the association's vice president of communications.

The Legislature ordered it as a cost-containment measure, with the state projecting it would affect nearly 40,000 Iowans and save it and the feds $36.7 million a year. The CMS waiver required the state to provide outreach and education to the public to ensure that eligible people apply for Medicaid as soon as possible.

But McIntyre said the state has not ramped up enrollment outreach to mitigate the end of retrospective eligibility.

In addition, Iowa, which expanded Medicaid in 2014, has not conducted any review of the cost savings to the government or of the financial impact on providers and beneficiaries, according to a spokesman for the Iowa Department of Human Services. The CMS, he said, did not require the state to conduct such a report on the impact of eliminating retrospective eligibility. "We've made so much progress with Medicaid expansion to reduce uncompensated care, and this really undermines that progress," McIntyre lamented.

There's already an effort to roll back the new retrospective eligibility waiver in Florida, which didn't expand Medicaid, so that it applies to nursing home residents and all other Medicaid eligibles except pregnant women and children.

It's basically impossible for many people who may need a nursing home placement to apply for Medicaid ahead of time because they're living in the community and don't qualify until they enter institutional care, explained Tom Parker, director of reimbursement for the Florida Health Care Association.

"I would think that undercuts the main argument for this policy," he said.

THE TAKEAWAY

Critics say eliminating retrospective eligibility is one more administrative barrier the Trump administration has erected to make Medicaid and other public benefits harder to access.

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