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SAVED BY MEDICAID:
NEW EVIDENCE ON HEALTH INSURANCE AND MORTALITY FROM THE UNIVERSE OF LOW-INCOME ADULTS

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ABSTRACT

We examine the causal effect of health insurance on mortality using the universe of low-income adults, a dataset of 37 million individuals identified by linking the 2010 Census to administrative tax data. Our methodology leverages state-level variation in the timing and adoption of Medicaid expansions under the Affordable Care Act (ACA) and earlier waivers and adheres to a preregistered analysis plan, a rarely used approach in observational studies in economics. We find that expansions increased Medicaid enrollment by 12 percentage points and reduced the mortality of the low-income adult population by 2.5 percent, suggesting a 21 percent reduction in the mortality hazard of new enrollees. Mortality reductions accrued not only to older age cohorts, but also to younger adults, who accounted for nearly half of life-years saved due to their longer remaining lifespans and large share of the low-income adult population. These expansions appear to be cost-effective, with direct budgetary costs of $5.4 million per life saved and $179,000 per life-year saved falling well below valuations commonly found in the literature. Our findings suggest that lack of health insurance explains about five to twenty percent of the mortality disparity between high- and low-income Americans. We contribute to a growing body of evidence that health insurance improves health and demonstrate that Medicaid's life-saving effects extend across a broader swath of the low-income population than previously understood.

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A data appendix is available at http://www.nber.org/data-appendix/w33719

1. Introduction

Medicaid provides health insurance to one-quarter of the U.S. population at an annual cost of more than $700 billion, making it by far the largest means-tested transfer program in the United States (Kaiser Family Foundation, 2019). It has also grown rapidly, with enrollment rising by about fifty percent between 2010 and 2021, driven largely by a provision of the Affordable Care Act (ACA) that allowed states to expand eligibility to all low-income adults regardless of parenthood or disability status. Understanding Medicaid's effect on mortality, a fundamental indicator of health and wellbeing, is crucial to evaluating this major policy shift and assessing the implications for states that have not chosen to expand. Such knowledge can also shed light on the relationship between health insurance and health and Medicaid's potential for reducing the considerable mortality disparities associated with socioeconomic disadvantage.

The questions of whether, by how much, and for whom health insurance improves health are actively debated in the economics literature. Until recently, a large body of experimental and quasi-experimental literature offered limited evidence on how health insurance affects health beyond vulnerable sub-populations (Black et al., 2019). Two influential recent studies challenged this view using large individual-level datasets and credible identification strategies, finding that Medicaid and health insurance substantially reduce mortality risk for older adults (Miller, Johnson, and Wherry, 2021; Goldin, Lurie, and McCubbin, 2021). These studies' confidence intervals, however, included both very small and very large effects, and they lacked the statistical power to detect effects in the broader low-income adult population, where younger adults make up the majority of newly eligible Medicaid enrollees but have lower baseline mortality risk (Kaestner 2021). Given Medicaid's outsized role in the safety net and its substantial public expenditures, knowing the magnitude of its causal effect on mortality is arguably as important as knowing the sign and significance. Obtaining precise estimates of Medicaid's effects in the low-income adult population targeted by recent expansions is similarly crucial for assessing the costs and benefits of these policies.

This paper contributes new evidence to this debate by estimating the causal effect of Medicaid on mortality using the universe of U.S. low-income adults. Our main sample is drawn from the 2010 Census and consists of 37 million non-elderly, non-disabled adults with incomes below 138 percent of the poverty level, the threshold for income-based eligibility under ACA expansions. We calculate income by linking the 2010 Census to Internal Revenue Service (IRS) records, an approach that permits more accurate identification of newly eligible adults than in prior work using self-reported income from surveys. We then link these individuals to administrative data on Medicaid enrollment and all-cause mortality and use the adoption and timing of expansions across states to identify Medicaid's causal effect on mortality in the low-income adult population and in sub-groups defined by age, race, ethnicity, gender, family status, nativity status, income, and employment. Finally, we estimate expansions' causal effects on sub-samples that align with the sample definitions and follow-up periods used in key prior studies to facilitate comparisons. We carry out all analyses in adherence with a preregistered analysis plan to limit the possibility of intentional or inadvertent selection of specifications and samples that yield desired results.[1] This practice, which is common for experimental research, is rarely employed in observational studies in economics (Burlig, 2018; Christensen and Miguel, 2018). While preregistration may not be feasible or desirable in many observational settings, we adopt this approach to bolster the credibility of findings on a relatively straightforward question that has long been the subject of vigorous debate in the health economics literature.

We estimate that recent expansions increased Medicaid enrollment by 12 percentage points and reduced the annual mortality hazard by 2.5 percent (95 percent confidence interval: 0.43-4.4 percent) in the low-income adult population. These estimates suggest that people who enrolled in Medicaid experienced a 21 percent reduction in their mortality hazard, on average, assuming no spillover effects on the mortality hazard of untreated individuals. We find mortality reductions of a similar proportional magnitude for Medicaid enrollees across numerous subgroups, although estimates are not statistically significant in some cases. Our estimates fall in the lower end of the confidence intervals from Miller, Johnson, and Wherry (2021) and Goldin, Lurie, and McCubbin (2021) and offer a substantial improvement in precision, with our own confidence intervals excluding these studies' large point estimates for the effect of treatment-on-the-treated.

Our estimates suggest that Medicaid expansions saved the lives of about 27,400 people between the ACA's passage in 2010 and 2022, corresponding to an annual average of 3,200 avoided deaths in post-expansion states and years, which is close to the annual number of non-elderly deaths from leukemia in the United States (Centers for Disease Control and Prevention, 2018). They also suggest that an additional 12,800 lives could have been saved in non-expansion states if they had expanded Medicaid in 2014. While most saved lives were among those who were

[1] Plan can be viewed on the Open Science Foundation website: https://doi.org/10.17605/OSF.IO/4HYJR.

40 and older in 2010, younger individuals accounted for nearly half of all life-years saved due to their longer remaining lifespans and disproportionate share in the low-income adult population.

Using publicly available data on average federal and state Medicaid expenditures for adults made newly eligible by expansions, we estimate a direct budgetary cost of about $5.4 million per life saved and $179,000 per life-year saved. These costs are well below the $10-11 million value of a statistical life used in federal government cost-benefit analyses and Braithwaite et al. (2008)'s inflation-adjusted estimates of societal willingness-to-pay for additional life-years, which range from $217,000 to $313,000 (Office of Management and Budget, 2023). Comparing the cost per life-year saved by Medicaid to hundreds of other life-saving interventions, we find that Medicaid tends to be more cost-effective than injury prevention and toxin regulation measures but less cost-effective than many medical interventions, which can be targeted towards those most likely to benefit (Tengs et al., 1995). The cost per life-year saved by Medicaid expansions appears similar to that of cervical cancer screening.

Beyond its contributions to policy debates, our paper adds to an extensive literature on the relationship between health insurance and health. Basic intuition suggests that insurance should improve health by increasing access to care, but this causal pathway is difficult to establish empirically because the production of health involves many complex and multidirectional relationships between health insurance, utilization, health behaviors, and observed and unobserved individual characteristics (Levy and Meltzer, 2001). Well-established patterns of adverse selection and moral hazard further complicate these efforts (Einav and Finkelstein, 2011; Baicker et al., 2015). An additional obstacle lies in the substantial statistical power required to detect effects on rare outcomes like mortality. These complexities explain why the relationship between health insurance and health remains the subject of debate more than forty years after RAND's seminal health insurance experiment (Brook et al., 1983). While ample evidence has shown that insurance increases health care utilization and improves rates of diagnoses and treatment for chronic diseases (Finkelstein et al., 2012; Baicker et al., 2013; Gruber and Sommers, 2019), establishing a causal link to physical health outcomes has proved more challenging. Until recently, evidence of mortality reductions suggested at most "modest health benefits from general adult health insurance expansions" beyond vulnerable sub-groups (Levy and Meltzer, 2008, p.404). Even the landmark Oregon Health Insurance Experiment (OHIE) found no statistically significant effects of Medicaid on short-term mortality and other physical health outcomes, although they lacked statistical power to rule out meaningfully large effects (Finkelstein et al., 2012; Baicker et al., 2013).

The ACA spurred a new era of research into the effects of health insurance on physical health, including two influential studies that used large-scale, individual-level datasets linked to administrative data to find that health insurance reduces mortality in older adults (Miller, Johnson, and Wherry, 2021; Goldin, Lurie, and McCubbin, 2021). However, the restricted age range of these studies' subjects and their estimates' wide confidence intervals left open the questions of whether, and by how much, health insurance reduces mortality in the general adult population. The present study advances this literature by using the universe of low-income adults to probe the limit of what we can learn about the magnitude of Medicaid's causal effect on mortality from recent expansions, with the use of a pre-registered analysis plan bolstering this contribution. Because we estimate mortality in the entire low-income adult population targeted by recent expansions, our estimates permit the most precise and comprehensive analyses to date of these policies' life-saving effects, including cost-effectiveness.

Our results also contribute to the literature on the socioeconomic gradient in health. Extensive research spanning academic disciplines, countries, and time periods has established a robust socioeconomic gradient in health (Kitagawa and Hauser, 1973; Deaton and Paxson, 1998; Cutler et al., 2012; Chetty et al., 2016). A key question in this literature is whether, and in which direction, health and disadvantage are causally related. Human capital theory emphasizes the lifecycle effects of health endowments and health shocks on investments in human capital and productivity, with causal relationships flowing primarily from health to socioeconomic status (Schultz, 1961; Grossman, 1972; Galama and van Kippersluis, 2013). At the same time, price theory suggests that higher incomes should lead to more consumption of health care (a normal good), which could in turn translate into improved outcomes. To the extent that the cost of obtaining health care is driving health disparities in the United States, we would expect universal public health insurance – effectively a large subsidy – to flatten the income gradient in mortality. We explore this mechanism by predicting the share of the mortality gap between the highest and lowest income quintiles of the U.S. population that would be eliminated in a hypothetical, static scenario where all uninsured people enrolled in Medicaid. Our findings suggest that the lack of health insurance explains about five to twenty percent of the mortality gap between income groups, with these bounds reflecting assumptions about how the average treatment effect across all uninsured individuals differs from the average effect for compliers in the present study. We consider estimates in the middle of this range to be most plausible because they reflect a large degree of selection into Medicaid enrollment among those with the greatest expected benefit.

Put differently, our findings suggest that even if all uninsured people in the United States enrolled in Medicaid, substantial mortality disparities between high- and low-income individuals would persist, a result that is consistent with the existence of such a gradient in many countries that provide universal public health insurance. The lack of health insurance appears to play a modest role in explaining the socioeconomic gradient in mortality in the United States but is not its predominant driver. Other important factors suggested by the literature include the effects of childhood resources on health and productivity over the lifecycle, the effects of income and education on health behaviors like smoking and exercise, and the effects of neighborhood disamenities like crime and environmental hazards on safety and physical health (Lleras-Muney, 2005; Almond and Currie, 2011; Cutler et al., 2012; Brown et al., 2020).

This paper proceeds as follows. Section 2 describes our data and empirical strategy, while Section 3 presents our main results on the effect of expansions on Medicaid enrollment and mortality, along with sub-group analyses, comparisons to prior work, and robustness checks. Section 4 carries out additional analyses to interpret the magnitude of our estimated effects, providing estimates of lives and life-years saved, cost-effectiveness, and Medicaid's potential to reduce mortality disparities by income. Section 5 discusses potential causal mechanisms and caveats related to spillovers and migration. Section 6 concludes.

2. Data and Empirical Strategy

2.1 Data

Identifying the universe of low-income adults

Our main sample consists of non-elderly individuals with incomes below 138 percent of the federal poverty guidelines, the population that became newly eligible for Medicaid under the ACA expansions and prior waivers. We limit our sample to people who were ages 19 to 59 in 2010 in keeping with Medicaid's definition of an adult and to exclude those who aged into Medicare eligibility before the modal expansion year of 2014. We also exclude from our main sample people indicated as Supplemental Security Income (SSI) or Disability Insurance (DI) recipients in 2009

Medicaid and Medicare records, as these individuals were categorically eligible for public health insurance before the ACA.

We base our sample on the 2010 Census, grouping individuals in households into family units according to the definition of a family used by Medicaid to determine eligibility, namely married partners and children under 19.[2] We then use anonymized identification keys to link these individuals to 2009 Internal Revenue Service (IRS) data on taxable income from 1040s, W-2s, and 1099-Rs, adjusting for non-linkage using inverse probability weights.[3] For people who link to an IRS Form 1040, we calculate Modified Adjusted Gross Income (MAGI), an income concept used by Medicaid to determine eligibility which is largely equivalent to AGI as reported on IRS Form 1040 but includes untaxed foreign income, non-taxable social security benefits, and tax-exempt interest. For non-filers, we estimate MAGI using information from IRS forms W-2 and 1099-R. We address various methodological issues in calculating family income from tax records using an approach adapted from Meyer et al. (2020) and described in Appendix B.

Our resulting sample contains 37.5 million non-disabled, low-income adults who were between the ages of 19-59 in 2010, representing the 42.3-million-person universe of low-income adults after applying inverse probability weights to account for non-linkage. A secondary sample of 41.9 million includes disabled individuals as well.

Measuring Medicaid enrollment and mortality

We observe Medicaid enrollment by linking our sample to administrative datasets from the Centers for Medicare and Medicaid Services. These datasets indicate days of enrollment in the year and basis of eligibility, but do not contain information about health care utilization. We obtain death dates by linking our sample to the Census Bureau's Numerical Identification File, or Numident, through April 2022. The Numident, which is derived from Social Security Administration (SSA) records, has been shown to be a "high-quality and timely source of data to study all-cause mortality," but does not indicate cause of death, a key limitation of our study (Finlay and Genadek, 2021).

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[2] Medicaid defines families for the purpose of determining income and eligibility to consist of married partners and dependents under 19. The household relationship variables included in the Census allow us to group about 98 percent of people into families with a high degree of certainty. See Appendix A for a detailed explanation of how we used household relationship indicators in the Census to group people in households into family units.

[3] About ninety percent of adults in the Census are assigned linkage keys by Census software that searches for name, date of birth, gender, and address against reference files from the Social Security Administration (SSA). We adjust for non-linkage using inverse probability weights from a model that regresses linkage status on demographic and household characteristics available in the Census.

Identifying expansion dates

Medicaid eligibility rules vary across states and over time in terms of income thresholds, eligibility categories, employment requirements, enrollment caps and freezes, the scope of benefits, and requirements for premia or other financial contributions. Although historically low-income, childless adults without disabilities were not eligible under federal Medicaid rules, some states extended coverage to this group prior to 2014 by obtaining a Section 1115 waiver (Sommers et al., 2014). For our analyses, we identify the date that states began to extend income-based eligibility to childless, low-income adults, even if coverage was not as comprehensive as full Medicaid or if smaller early expansions preceded full expansion in 2014.[4] We do not, however, classify states as early expanders if eligibility was tied to employment or if enrollment was frozen prior to 2010.[5]

2.2 Empirical Strategy

Estimating expansions' effects on enrollment and mortality

To estimate the effect of Medicaid expansion on enrollment, we consider a two-way fixed effects linear probability model where Y_{ist} indicates Medicaid enrollment for person i i in state s at any point in year t:

$$Y_{ist} = \tau \cdot I\{t \geq t_s^*\} + \delta_s + \delta_t + \gamma' X_{ist} + \epsilon_{ist} \quad \text{(Eq. 1)}$$

In this model, τ is the average effect of expansion on enrollment, δ_s and δ_t are state and year fixed effects, and X_{ist} is a vector of covariates including age group dummies, race, Hispanic ethnicity, gender. We let t_s^* denote the year state s expanded Medicaid to low-income adults, with the post-period indicator $I\{t \geq t_s^*\}$ being equal to zero in all periods for non-expansion states and one in all

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[4] In assigning expansion dates, we preference the year that states began expanding Medicaid eligibility – even if early expansions were limited geographically or had restrictive income limits – in order to satisfy the difference-in-differences model's no-anticipation identifying assumption (Roth et al., 2023). For example, while California adopted full expansion in 2014, the state obtained a waiver to carry out a limited early expansion beginning in 2010. This policy, which Golberstein et al. (2015) estimate to have increased enrollment by about 7 percentage points through 2013, would violate the no-anticipation assumption if we classified California as a 2014 expander. As emphasized throughout the text, our estimates reflect the combined effect of 2014 expansions and earlier waivers.

[5] Four states expanded full Medicaid to low-income, childless adults before 2010 (DE, NY, VT, and MA) and DC did so in 2011. We classify two states (HI and MD) as having expanded eligibility to this group before 2010, even though their Section 1115 waiver programs offered a more limited set of benefits than full Medicaid. We classify five additional states as early expanders (CT, CA, MN in 2010 and NJ, WA in 2011) because, while these states did not adopt full expansion until 2014, their early expansion programs led to substantial increases in enrollment that would otherwise violate the no-anticipation assumption (Sommers et al., 2014; Kaiser Family Foundation, 2012).

periods for states that expanded prior to 2010.[6] To assess parallel trends in the pre-period, we estimate an event study specification where the post-period indicator is replaced with a sum of event time coefficients and dummies, which are equal to zero in all periods for non-expansion states. We also estimate a version of these models using days of Medicaid enrollment in the year as the outcome variable.

We parametrize the mortality hazard $\lambda_i(t)$ using a proportional hazards form:

$$\lambda_i(t) = \lambda_0(t)\exp\left(z_i(t)'\beta\right) \quad \text{(Eq. 2)}$$

In this model, $\lambda_0(t)$ is the unknown annual baseline hazard in year t, $z_i(t)$ is a vector of time-dependent explanatory variables for individual i, and β is a vector of parameters to be estimated. Letting T_i indicate the time of death and assuming that $z_i(t)$ is constant between times t and $t+1$, the discrete time hazard (i.e., the probability of death in the interval conditional on survival to time t) can be written as follows (Meyer 1990):

$$\Pr(T_i < t+1 \mid T_i \geq t) = 1 - \exp\left(-\exp\left(z_i(t)'\beta + \gamma(t)\right)\right) \quad \text{(Eq. 3)}$$

where $\gamma(t) = \ln(\int_t^{t+1} \lambda_0(u)du)$. We use discrete-time (annual) data to estimate the parameters of the continuous proportional hazard model using Equation (2) with a non-parametric baseline hazard, letting

$$z_i(t)'\beta = \tau \cdot I\{t \geq t_s^*\} + \delta_s + \delta_t + \gamma' X_{ist} \quad \text{(Eq. 4)}$$

where the explanatory variables are defined as in the enrollment model. Exponentiating τ and subtracting one gives the proportional effect of Medicaid expansions on mortality in expansion versus non-expansion states. As with enrollment, we assess the common trends assumption by estimating an event study specification replacing the post-period indicator with a sum of event time dummies and coefficients. In all specifications, we cluster standard errors at the state level to account for potentially serially correlated errors within states over time (Cameron and Miller, 2015; Abadie et al., 2022).

Mortality model and assumptions

Our choice of a proportional hazard model accords with a standard approach to modeling time-to-event data in the economics, public health, and biostatistical literatures, particularly when the outcome of interest is individual mortality risk, which is subject to substantial heterogeneity at baseline (Meyer, 1990; Cameron and Trivedi, 1998; Attanasio and Hoynes, 2000; Meghir et al.,

[6] Year-long periods in our study run from April to March, reflecting the Census reference date of April 1, 2010.

2018). Unlike linear hazard models, which assume a constant additive treatment effect across groups, this model assumes a constant proportional treatment effect, which in turn permits the treatment effect to be larger in absolute terms for demographic groups with greater underlying mortality risk, such as older individuals.

This proportionality assumption is also consistent with many prior studies' findings on the relationship between health shocks or interventions and mortality risk. For example, Finkelstein et al. (2023) find that the Great Recession caused a roughly constant proportional decline in mortality rates across all ages and demographic groups, while Meyer et al. (2023) find that COVID-19 pandemic had a similar proportional effect on the mortality hazard of homeless and housed populations despite substantially different baseline mortality hazard rates. Moreover, the present study's finding of similar proportional treatment-on-the-treated effects across demographic groups (described in Section 3.4), despite our model having made no restrictions on cross-group effects, is also consistent with this assumption.

In Appendix C, we compare the fit of additive and proportional mortality hazard models in a setting that is closely related to the natural experiment in our study but provides greater statistical power to discern between functional forms. Figure C1 displays the annual mortality hazard of insured and uninsured individuals by age based on the public-use National Health Interview Survey (NHIS) Linked Mortality File (2000-2009, with mortality calculated through 2010). The absolute difference between the mortality hazard of uninsured and insured individuals exhibits a clear increasing pattern with age. Figure C2 provides a more formal assessment, with the left panel displaying the ratio of uninsured-to-insured mortality risk by age and the right panel displaying the difference between uninsured and insured mortality risk by age. We fail to reject the null hypothesis of constant proportional differences by age (p-value 0.33) while soundly rejecting the null hypothesis of constant additive differences by age (p-value almost zero). These analyses provide support for the decision, indicated in our study preregistration, to estimate a proportional rather than linear hazard model.

Even though mortality occurs in continuous time, we estimate a discrete-time hazard model because our data are grouped into daily intervals and ties are not infrequent given the size of our sample. The presence of such ties can cause asymptotic bias in the estimation of the regression coefficients and the covariance matrix, and methods commonly used to resolve such ties can be inaccurate if there are many ties in the data set (Breslow, 1974; Kalbfleisch and Prentice, 2002).

The discrete model also has the advantage of facilitating comparisons to other studies, many of which examine the effect of health insurance on the probability of death in a year, as opposed to the instantaneous probability of death conditional on having survived to time t.

Finally, we model the baseline hazard nonparametrically because we do not have strong *a priori* reasons for imposing a particular functional form for the dependence of the hazard rate on duration, and because approaches that assume a parametric form for the baseline hazard provide inconsistent estimates when the assumed baseline hazard is incorrect. The COVID-19 pandemic, which occurred during the last two years of our study period, is one example of an event that could lead to such inconsistency if a standard baseline hazard functional form were assumed. The nonparametric approach is robust to such disruptions in the baseline hazard.

3. Medicaid's Causal Effect on Mortality

3.1 Descriptive statistics

Table 1 displays characteristics of the sample of low-income adults used in our analyses. The main sample excludes people with disabilities that made them eligible for public insurance prior to expansions, while a secondary sample includes these individuals.[7] More than a quarter of the main sample is between the ages of 19 and 24 in 2010 and the average age is about 35.[8] About half are female, 18 percent are Black, and 21 percent are Hispanic. Only about one-quarter are married and about 37 percent are parents. One quarter had no formal income in 2009. About five percent of non-disabled adults died during our study between April 2010 and March 2022.

[7] We emphasize the non-disabled sample in our results because it excludes people who were already eligible for public health insurance through Medicaid or Medicare in 2009 and hence unlikely to have benefited directly from income-related eligibility expansion. This decision accords with Miller, Johnson, and Wherry (2021). We include results for the full (disabled and non-disabled) sample in our results, however, because this sample was indicated in our pre-registration plan.

[8] The ACA allowed dependents to remain on their parents' health insurance plans through age 26 beginning in September 2010. This provision is not a threat to the validity of our causal estimates because it occurred in the first period (and would have been absorbed by year fixed effects even if it had occurred later), but it may in part explain why we see smaller Medicaid enrollment effects for the youngest age cohort in Section 3.4 and it could affect our characterization of the complier group (i.e., disproportionately consisting of young adults whose parents did not have private insurance to extend to them). However, we expect this provision's implications for our study to be minor because the youngest people in our study were age 31 by 2022, well past the dependent mandate cutoff. Moreover, low-income young adults are more likely to have low-income parents, who in turn are less likely to have had private health insurance coverage to extend to their dependents.

3.2 Expansions' effect on Medicaid enrollment and mortality

Medicaid enrollment

Table 2 presents difference-in-differences estimates of expansions' causal effect on enrollment. As seen in the first column, we estimate that expansions increased the share of non-disabled adults ever enrolled in Medicaid in a year by 11.7 percentage points from a baseline of 24 percent enrolled in expansion states in the pre-period. The second column indicates that expansions increased the number of days of enrollment in a year by about 35.9, meaning that on average new enrollees spent about 10 months (300 days) on Medicaid in each post-expansion year. The first stage effect is smaller when we include people who are disabled in the third and fourth columns, consistent with the fact that low-income disabled individuals were eligible for Medicaid even prior to expansions.

The event study specification in Figure 1 provides strong evidence of common trends in Medicaid enrollment in the pre-period, followed by an increase in enrollment in expansion states relative to non-expansion states over the first four years following expansion that begins to fall slightly in the fifth post-expansion year, a pattern that could reflect the aging into Medicare eligibility of people in our study in both expansion and non-expansion states.

Mortality

Table 3 presents difference-in-differences estimates of expansions' causal effect on mortality. Results for our main, non-disabled sample are found in the first two columns, with the first indicating the coefficient from the hazard model and the second indicating the corresponding percentage change in the mortality hazard in expansion. We find that Medicaid expansion reduced the mortality hazard in expansion states by about 2.5 percent relative to non-expansion states, with a 95 percent confidence interval that excludes reductions larger than 4.5 percent and smaller than 0.4 percent. The effect on mortality is attenuated when we include disabled individuals in our sample in the third and fourth columns, with a 1.3 percent reduction in the mortality hazard that is only significant at the 10 percent level.

The event study specification in Figure 2 once again provides evidence of common trends in mortality in expansion relative to non-expansion states the pre-period, followed by a pattern of mortality reductions that increase in magnitude before seeming to level off.

3.3 Medicaid's effect on the mortality risk of new enrollees

The mortality estimates in Table 3 can be interpreted as the effect of Medicaid expansions on aggregate deaths in the low-income adult population. This parameter is of particular interest from a program evaluation standpoint because it reflects the policy's population-level impact, including both direct effects on those newly enrolled in Medicaid and any potential indirect effects on other low-income adults, such as those with private insurance or those already receiving Medicaid due to disability or as very low-income parents.

Another relevant parameter, however, is the effect of Medicaid on the mortality hazard of individuals who enrolled in Medicaid due to the expansions, or the average effect of treatment on the treated. This parameter indicates the magnitude of the individual-level causal relationship between health insurance on the health, a relationship of interest in the health economics literature. Treatment-on-the-treated estimates are also useful for assessing the plausibility of estimated mortality reductions relative to prior expectations and for comparing mortality effects across groups with different enrollment effects.

We obtain estimates of the average effect of treatment on the treated by dividing the proportional mortality effect by the percentage point enrollment effect.[9] For this approach to be valid, we must assume no spillover effects from Medicaid expansions onto the mortality of untreated individuals in our sample. We discuss this assumption in Section 5. Table 4 presents these results. They suggest that Medicaid reduced the mortality hazard of enrollees by about 21 percent with a 95 percent confidence interval that excludes reductions smaller than 3.7 percent and larger than 38 percent. Treatment-on-the-treated estimates are smaller for the sample that includes people who are disabled, but differences between the two samples are not statistically significant.

3.4 Sub-group analyses

Table 5 indicates Medicaid enrollment and mortality estimates for sub-groups defined by age, gender, race, Hispanic ethnicity, income, employment, parental status, marital status and nativity status.[10] Differences in the first stage across groups, although not statistically significant,

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[9] Our treatment-on-the-treated effects could alternatively be scaled by the change in uninsurance, which would have the advantage of accounting for crowd-out of other insurance by Medicaid. This approach would require accurate information on insurance status, however, which is not available in our datasets. Self-reported measures of insurance status, such as those in the ACS, are subject to substantial misreporting (Meyer et al. 2024).

[10] We did not estimate the model for disabled individuals separately, but comparing estimates from the non-disabled and overall samples allows us to infer that the first stage effect would likely be less positive (or negative) and the

are consistent with prior expectations of these groups' likelihood of becoming newly eligible for Medicaid under expansions. For example, our evidence suggests larger first-stage effects for unmarried individuals and non-parents relative to married individuals and parents, respectively, likely reflecting the latter groups' probability of having insurance through a spouse or having qualified for Medicaid as a very low-income parenthood prior to expansions. Similarly, the first stage effect appears to be larger for people who were not employed in 2009 and those with lower incomes, likely reflecting more limited access to employer-sponsored insurance in these groups.

Figures 3 through 6 display estimates of the average effect of treatment on the treated for these sub-groups, assuming no spillovers. Differences between groups are not statistically significant and range from an 11 to 32 percent reduction in mortality for all groups except foreign-born individuals.[11] Differences from zero are not statistically significant for all groups, however, leading us to exercise caution in concluding that Medicaid reduced the mortality hazard across the board.

At the same time, the fact that estimates' signs are consistently negative and treatment-on-the-treated effects have similar magnitude across many different groups offers encouraging evidence that treatment effects extend broadly across the U.S. population. For example, mortality effects are only statistically significant at the 95 percent level among those ages 50-59 in our sample but they are significant at the 90 percent level for those ages 30-39 and 40-49, and our point estimates for the average effect of treatment on the treated are similar across all age groups, ranging from about 16 percent in the 40-49 cohort to 27 percent in the 30-39 cohort. It is worth

mortality effect would be less negative (or positive) for disabled relative to non-disabled individuals. In Appendix Table D1, we approximate values for the disabled group by assuming that estimates in the overall sample are a weighted average of the non-disabled and disabled estimates. This method yields a first-stage effect close to zero (0.5 ppt), which is sensible because disabled individuals were already enrolled in Medicaid or Medicare before the expansions. The estimates suggest about a 10 percent increase in mortality, which (if significant) could reflect negative spillovers onto this group, e.g., through changes in provider behavior or crowding. We reserve additional examination of possible negative spillovers on the mortality hazard of disabled individuals for future work.

[11] We identify nativity status using the Census's Numident file, which is based on Social Security Administration (SSA) records. The foreign-born sample includes both naturalized citizens and non-citizens and, because it uses place of birth from SSA data, excludes unauthorized immigrants, who do not appear in those records. Lawful permanent residents (LPRs) are typically eligible for Medicaid after a five-year waiting period. Our treatment-on-the-treated estimate for this group is positive and close to zero (5.8 percent), although its 95 percent confidence interval (-22.3 to 35.1) does not allow us to reject meaningfully large changes. This apparent null finding may be related to differences in the health care utilization of U.S.- and foreign-born Medicaid enrollees. Kaushal and Muchomba (2023) find using restricted Medicaid Expenditure Panel Survey (MEPS) data that expansions increased the enrollment of both U.S.- and foreign-born people, but expenditures and utilization only increased for the U.S.-born group, which the authors hypothesize to be related to unobserved health, behavior, and cultural differences.

emphasizing that these estimates reflect proportional treatment effects, meaning that those with higher elevated baseline risk would experience much larger reductions in their absolute mortality risk. We consider the distribution of lives and life-years saved across age cohorts, assuming a uniform treatment effect of 21 percent, in Section 4.

3.5 Comparisons to key prior studies

We compare our findings to three key prior studies on the effect of health insurance and Medicaid on mortality. The first is Finkelstein et al. (2012), which estimated the effect of gaining Medicaid through the Oregon Health Insurance Experiment (OHIE) on individuals ages 18-64 with incomes below 100 percent of the poverty level over a two-year follow-up period. The second study, Goldin, Lurie, and McCubbin (2021), used an experiment that randomly assigned uninsured taxpayers to receive a letter informing them of penalties and insurance options to identify the effect of health insurance on two-year mortality among those ages 40-59. The final study, Miller, Johnson, and Wherry (2021), linked low-income adults from the American Community Survey (ACS) to Medicaid and SSA mortality data and used the timing and adoption of ACA expansions across states to identify mortality effects on those adults ages 55-64 over a four-year follow-up period. We translate the absolute mortality reductions reported in these studies into a proportional form and estimate our model on sub-samples that align with these studies' sample definitions and timeframes.[12,13]

Figure 7 displays estimates of the average effect of treatment on the treated in the published studies and in comparable sample from the present study. We estimate effects twice for each study, first using the studies' original follow-up periods and second using the full eight years available in our data. We also present Miller, Johnson, and Wherry (2021)'s main estimate, which assumes a

[12] For Finkelstein et al. (2012), we estimate the model on the full age range available in our study, 19-59, but restrict our sample to those with incomes below 100 percent of the poverty level. For Goldin, Lurie, and McCubbin (2021), we restrict our sample to those ages 40-59 in 2010, recognizing that our sample only includes those with incomes less than 138 percent of the poverty level, unlike that study. Finally, for Miller et al. (2021), we restrict our sample to those ages 50-59 in 2010.

[13] Because the intervention in Goldin, Lurie, and McCubbin (2021) induced a nonuniform distribution of new coverage-months among study participants, the mortality estimates from their instrumental variables model cannot be translated into an average causal effect of a year of coverage without assumptions on the nature of the relationship between months of coverage and the mortality effect. We report in Figure 7 the authors' estimates assuming a linear relationship between months of coverage and mortality. Alternative assumptions about this relationship would change point estimates and confidence interval bounds but have limited qualitative impact on our comparisons. For example, the authors' calculations suggest that the proportional mortality estimate could be as low as 99% and its confidence interval's lower bound as low as 22 percent, compared to the 122 percent and 28 percent reported in Figure 7.

linear relationship between mortality and Medicaid expansions and covariates, as well as an estimate based on a Cox proportional hazards model available in that paper's Appendix, which is more comparable to the present study. We find statistically significant mortality reductions for all three sub-samples. The present study's estimates fall within the wide confidence intervals reported in all three published studies and exclude the large point estimates reported in Goldin, Lurie, and McCubbin (2021) and Miller, Johnson, and Wherry (2021).

Figure 7 also illustrates the improved precision offered by the present study. Potential explanations for this improved precision include our much larger sample size, the use of a proportional hazard model, and better identification of low-income adults due to our use of administrative tax data. Relative to Finkelstein et al. (2012) and Miller, Johnson, and Wherry (2021), the present study likely benefits somewhat from some efficiency gains due to its much larger sample size, but such gains are attenuated because we cluster standard errors at the state level to account for potential serial correlation in errors. The choice of a proportional hazard model, rather than the linear probability models (LPMs) used in these studies, likely supports improved precision due to the inherent heteroskedasticity of linear models with binary outcomes. These efficiency gains are apparent when comparing Miller, Johnson, and Wherry (2021)'s LPM estimate in their main results to the proportional hazard estimate from that paper's Appendix, which is notably more precise. Finally, the present study likely gains efficiency relative to Goldin, Lurie, and McCubbin (2021) and Miller, Johnson, and Wherry (2021) due to its larger first stage. In the former study, the intervention induced a 1.2 percentage point increase in health insurance enrollment relative to a baseline mean of 58.5 percentage points, meaning that their estimates are based on a very small share of treatment compliers in their sample. Miller, Johnson, and Wherry (2021)'s first stage enrollment effect, while substantial at 12.8 percentage points, is smaller than the 13.3 percentage point first stage effect in the present study, a fact that perhaps reflects improved identification of the low-income adult population targeted by expansions due to the use of administrative tax data in the present study rather than survey-reported income.

3.6 Robustness checks

Triple differences with higher-income adults

We test the robustness of our findings to a triple differences specification using higher income adults to provide a third difference. This third difference comes from adults with incomes four to six times the poverty level, a group that is unlikely to have gained insurance either through Medicaid expansions or through ACA marketplace premium subsidies, which were only available to those at four times the poverty level or less. By differencing this higher income group's enrollment and mortality effects from effects in the low-income population, this specification will eliminate any bias in enrollment and mortality due to differing pre-trends in expansion versus non-expansion states that is common to both low- and high-income groups.

Table 6 presents these estimates. This triple differences specification yields a treatment-on-the- treated estimate of about 17.7 percent (95 percent confidence interval: 3.4-32 percent), which is quite similar to the 21 percent (3.7-38 percent) estimate from our main specification. As an extension of this robustness check, Table 7 presents difference-in-differences enrollment and mortality estimates based on this higher-income group alone. We a find small (1.5 percentage point) effect of expansions on enrollment and no statistically significant effect on mortality. In other words, Medicaid expansions appear to have induced a very small increase in enrollment among individuals with 2009 incomes above the 1.38 times the poverty level, likely reflecting changes in these individuals' life circumstances between 2009 and the Medicaid expansion date, but any mortality reduction in this group, if present, was too small to produce a significant mortality effect. The event study specifications in Figures 8 and 9 support common pre-trends in this group and a small increase in Medicaid enrollment coinciding with expansions but no (or very small) effects on mortality risk. These findings provide further support for the assumptions underlying our main difference-in-differences estimates.

Extended pre-period event studies

We also consider whether common trends in Medicaid enrollment and mortality risk predate the study's 2010 start date. The event study in Figure 10 provides evidence of common pre-trends in Medicaid enrollment for our sample over the six years prior to expansion. Because our sample conditions on being alive during the 2010 Census, however, we cannot perform a similar exercise to estimate extended pre-trends in mortality risk. Figure 11 offers instead aggregate mortality rates for those ages 19-59 in 2000-2016 based on CDC data in states that expanded Medicaid in 2014 and states that expanded Medicaid after 2016, or never. This figure shows that despite a higher level of mortality risk in late-expanding and never-expanding states relative to those that expanded in 2014, the two sets of states follow a very similar trajectory, including year-to-year fluctuations in the same direction and with similar magnitude, providing further support to the common trends assumption underlying our estimates.

Discussion of staggered treatment timing and heterogeneous treatment effects

As is well-documented in a growing literature, two-way fixed effects models with staggered treatment timing may produce estimates that lack a sensible interpretation when treatment effects are heterogeneous across units and over time (see de Chaisemartin and D'Haultfoeuille (2023) for a recent review). Various methods have been proposed to isolate so-called "clean" variation between treated and not-yet-treated groups and obtain sensible estimands, but most of these estimators have been developed for linear models and cannot be applied in a straightforward way to the proportional hazard model in this study. We suspect that heterogenous treatment effects will not pose a major problem in our setting, however, for several reasons. First, we have a limited number of treatment cohorts, with most states having expanded either before 2010, in 2014, or not at all. Second, our eight-year follow-up period, while longer than some prior studies of Medicaid expansions, remains modest relative to the follow-up periods in many two-way fixed effects regressions.[14] Third, in a study design closely related to our own, Miller, Johnson, and Wherry (2021) check the robustness of their estimates of Medicaid's effect on mortality to several approaches proposed by this literature with little change to their main findings. Moreover, as discussed in Section 2.2, we expect proportional treatment effects to be less heterogeneous than the additive ones estimated in Miller, Johnson, and Wherry (2021) because they account for substantial variation in baseline mortality risk in the low-income population.

4. Interpreting the magnitude mortality reductions

Because the mortality estimates in this paper are based on the entire universe of the low-income adults targeted by recent expansions, they permit by far the most comprehensive analysis to date of Medicaid's life-saving effects, its cost-effectiveness, and its potential as a policy lever to reduce socioeconomic disparities in mortality risk in the U.S. population. We explore these topics in this section.

[14] For example, Chaisemartin and D'Hautefoueille (2023) apply several heterogeneity-robust estimators to a prior study using state-by-year-level variation to study the effects of unilateral divorce laws over a fifteen-year follow-up period, but the results change little with these alternative approaches.

4.1 Lives and life-years saved

We use our mortality estimates and the sample's age distribution to estimate the number of lives and life-years saved by Medicaid expansions between 2010 and 2022. We also predict the number of lives and life-years that could have been saved in non-expansion states if they had adopted Medicaid expansion in 2014, the modal expansion date. Our methodology, which follows standard life-table methods, is described in-depth in Appendix D. We draw on 2010 SSA life tables to obtain population mortality hazards by age, which we scale by an estimate of the ratio of the mortality hazard in the low-income population to the overall mortality hazard in the U.S. population from the 2010-2013 National Health Interview Survey (NHIS) (Preston et al., 2001).[15] To estimate avoided and avoidable deaths, we deflate these mortality hazards by the proportional treatment effect in post-expansion years and estimate the resulting change in life expectancy and probability of death in the post-expansion period by age cohort.

Lives saved by Medicaid expansions and avoidable deaths

Tables 8 and 9 present the results of this analysis. We estimate that Medicaid expansions reduced the number of deaths in expansion states by about 27,400, corresponding to approximately 3,220 people per year across all expansion states in post-expansion years.[16] We predict that an additional 12,800 lives could have been saved in non-expansion states if they had adopted expansion in 2014, or about 1,600 per year between 2014 and 2022. These are non-trivial numbers of lives saved and potentially avoidable deaths. For comparison, in 2018, 3,200 individuals ages 19-59 died from leukemia and about 4,700 died from pneumonia in the U.S. according to the CDC's WONDER database (Centers for Disease Control and Prevention, 2018).

Life-years saved by Medicaid expansions

Tables 8 and 9 also present estimates of the number of life-years saved by Medicaid expansions by 2010 age cohort. About 70 percent of lives saved by Medicaid expansions accrued to those who were ages 40-59 in 2010 due to these groups' elevated baseline mortality risk. The share of life-years saved by these cohorts, however, was smaller, at about 57 percent. The remaining 43 percent of life-years saved by expansions accrued to those who were ages 19-39, a

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[15] A preferred approach would be to obtain these mortality hazards in the low-income population using restricted Census or ACS data linked to mortality records, but such numbers have not been approved for public disclosure by the Census Bureau and are reserved for future work.

[16] We divide the number of avoided deaths by 8.5, which is the population-weighted average number of post-expansion years for expansion states in our sample.

finding that reflects both the longer life expectancies of individuals in these groups and the fact that these individuals make up about two-thirds of the low-income adult population. These results suggest that earlier analyses emphasizing Medicaid's mortality reductions among older adults may have overlooked substantial benefits among younger adults.

4.2 Cost and cost-effectiveness

Cost per life and life-year saved

We next estimate the cost per life and life-year saved by Medicaid expansions and compare these to valuations of a statistical life and life-year found in the literature. The average direct budgetary cost of a year of Medicaid enrollment among adults made newly eligible under the ACA expansions was \$5,225 in 2019, and our first stage estimates suggest that expansions resulted in an additional 28.7 million person-years of Medicaid enrollment (Kaiser Family Foundation 2019b).[17] We therefore estimate the cost per life saved to be approximately \$5.4 million, well below the \$10-11 million valuation of a statistical life used by the federal government in cost-benefit analyses (Office of Management and Budget 2023). Dividing the policy's cost by the number of life-years saved produces an estimate of \$179,000 per life-year saved, which is well below Braithwaite et al. (2008)'s's inflation-adjusted estimate that societal willingness-to-pay for each additional life-year is \$217,000 to \$313,000.[18]

While our analyses suggest costs per life and life-year saved that compare favorably with willingness-to-pay estimates from the literature, it is worth noting that our estimates of these costs are much higher than those reported in some prior studies, including Sommers et al. (2017). This difference arises from our finding of smaller estimated mortality effects relative to prior work.

Comparisons to other life-saving interventions

To further interpret the cost-effectiveness of Medicaid expansions, we compare the average cost per life-year saved by this policy to the inflation-adjusted cost per life-year saved by numerous

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[17] While the costs of Medicaid vary a great deal across sub-groups (e.g., by age), this variation is not relevant to the cost-effectiveness calculations in this section. The per-person cost estimate we use is an average taken across all newly eligible adults who enrolled in Medicaid as a result of expansions, which is precisely the group for which we estimated the number of new enrollees and reduced deaths. This correspondence between the sample for which average cost estimates are available and the sample used in our enrollment and mortality calculations permits more straightforward cost-effectiveness calculations than in prior work.

[18] Braithwaite et al. (2008) estimate willingness-to-pay using the rise in health expenditures and mortality changes over time. We have updated their estimates to 2019 dollars to be consistent with our Medicaid cost data.

other life-saving interventions reviewed in Tengs et al. (1995).[19] Figure 12 plots the average cost per life-year saved by these interventions and Medicaid expansions. Medicaid's cost-effectiveness is similar to the median intervention in this compilation. It is more cost-effective than many injury prevention and toxin regulation interventions, but tends to be less cost-effective than medical interventions, likely reflecting the ability to target better target medical interventions towards those most likely to benefit. We estimate the cost-effectiveness of Medicaid expansion to be similar to cervical cancer screening.

Caveats on cost analyses

As a caveat, the costs cited here reflect only direct state and federal expenditures, as indicated in Medicaid Budget and Expenditure System (MBES) data. Mortality changes from Medicaid expansions are likely to have many effects on state and federal budgets, including impacts on tax revenue and expenditures on other safety net programs. Moreover, these analyses do not constitute a comprehensive cost-benefit or social welfare analysis of Medicaid expansions. We consider only the direct budgetary costs of expansion and benefits from reduced mortality risk attributable to Medicaid. Prior work indicates that Medicaid confers numerous other benefits not accounted for in this analysis, including health-related quality of life improvements and financial protection, as evidenced by improvements in self-reported health and reduced depression (Finkelstein et al., 2012; Baicker et al., 2013; Finkelstein et al., 2019). The broader cost of Medicaid expansions likely also extend beyond the narrow budgetary costs examined in this paper.

4.3 Medicaid and the socioeconomic gradient in health

As a final analysis, we predict the reduction in mortality disparities by income that would occur if the U.S. implemented universal health insurance by enrolling all uninsured individuals in Medicaid. This analysis serves not only as a modeling exercise for a policy that some have proposed, but also as a thought experiment to gain insight into the degree to which the lack of health insurance and cost of care are driving health disparities in the United States. Here we present a partial equilibrium analysis that does not account for externalities on currently insured individuals or other general equilibrium effects such as health sector reorganizations that might

[19] Tengs et al. (1995) compile cost estimates for five hundred life-saving interventions related to injury prevention, medicine, and toxin regulation. We collapse interventions into seventy-three groups, taking the average cost per life-year saved in each group (e.g. airplane safety, childhood immunization, asbestos control).

occur in response to universal public insurance, changes that could be either favorable or unfavorable reducing mortality disparities by income.

We use public data from the 2010-2013 NHIS to obtain mortality and insurance rates by quintiles of household income. We then consider the insurance rate for individuals in income quintile j, r_j, and obtain estimates of the annual mortality hazard of insured ($\lambda_j^{insured}$) and uninsured ($\lambda_j^{uninsured}$) individuals in the same quintile. Table 10 presents estimates of these quantities. We then predict the annual mortality hazard for all individuals in each income quintile if all uninsured individuals obtained health insurance and experienced proportional change in their mortality hazard of τ:

$$\lambda_j^{full\ insurance} = r_j \lambda_j^{insured} + \left(1 - r_j\right)(1 - \tau)\lambda_j^{uninsured}$$

In our study, we estimate that people who enrolled in Medicaid due to expansions experienced a 21 percent reduction in their mortality hazard. However, the treatment effect across all individuals who were uninsured prior to expansions (indicated as τ in the above model), is likely smaller due to selection bias with respect to the expected benefit from Medicaid enrollment. An extreme case of selection bias might arise in the case of contingent eligibility, where hospitals presumptively enroll patients seeking emergency care, perhaps at a moment when they stand to benefit the most. We therefore take 21 percent to be an upper bound on τ. We estimate a lower bound on τ under the assumption that individuals who enrolled in Medicaid in our study had an average treatment effect of 21 percent and the rest of the uninsured population had a treatment effect of zero, yielding a lower bound estimate of 5.2 percent across all uninsured individuals.[20]

Figure 13 displays the mortality risk of individuals in the bottom four quintiles of income relative to those in the top quintile of income. The solid line indicates the observed disparities by income while the dashed lines indicate predicted mortality disparities with full insurance. The figure also indicates estimates of the share of the mortality gap between the highest and lowest income quintiles that would be eliminated with full insurance. These predictions suggest that universal Medicaid enrollment would eliminate between five to twenty percent of the mortality gap between the highest and lowest income quintiles. We take estimates in the middle of this range

[20] Our first-stage estimate suggested that 11.7 percent of low-income individuals were induced by expansions to enroll in Medicaid, or about one-quarter of the 47 percent of individuals in the lowest income quintile in Table 10 who lacked insurance before 2014. Assuming that one-quarter of the low-income population has a treatment effect of 21 percent and the rest have a treatment effect of 0 percent yields an average treatment effect of 5.2 percent across this group.

to be more plausible given the likelihood of substantial selection into Medicaid enrollment among people most likely to benefit in our study.

These findings suggest that universal health insurance would produce a meaningful but far from complete reduction in mortality disparities between high- and low-income individuals, a result that is consistent with the fact that even countries with universal public health insurance exhibit a pronounced socioeconomic gradient in health (Cutler et al., 2012). Put differently, the lack of health insurance appears to play some role in explaining the socioeconomic gradient in health but does not appear to be its predominant driver, underscoring the likely numerous and complex social and economic channels linking these outcomes. As a caveat, however, we note that our study looks only at the effects of contemporaneous coverage on health and may not capture long-term effects. For example, Medicaid in childhood has been found to improve educational outcomes and productivity in adulthood, and this relationship would not be accounted for in our model (Almond and Currie, 2011).

5. Discussion of mechanisms and caveats

This section discusses potential causal mechanisms underlying our estimates and bias that might arise from spillover effects on the mortality risk of untreated individuals and migration during the study period.

5.1 Possible mechanisms for mortality reductions in younger adults

An important limitation of our analyses is that we do not observe health care utilization or cause of death for individuals in our study, which in turn limits our ability to examine the causal mechanisms linking Medicaid and mortality risk. Our novel finding of similar proportional mortality reductions across age groups, including the youngest cohort, merits particular attention. One reason prior studies have focused on older adults is because they are more likely to die from internal conditions typically understood to be responsive to health care interventions, whereas mortality among younger adults is driven primarily by external causes (Nolte and McKee, 2004). Indeed, Miller, Johnson, and Wherry (2021) find that Medicaid's mortality reductions among near-elderly adults were driven by reduced risk of death from internal but not external causes.

Table 11 indicates the five leading causes of death for the four age cohorts in our study according to public National Center for Health Statistics (NCHS) data. We indicate leading causes of death based on age at the beginning of the study in 2010 and the end of the study in 2022. More than 80 percent of deaths among those ages 19-29 are due to accidents (primarily poisonings related to drug overdose), intentional self-harm, and assault, compared to about 11 percent of deaths from these causes among those ages 50-59. Mortality risk from these external causes remains high even when the 19-29 age cohort has aged to 31-41 years old by the end of our study in 2022, at about 60 percent of deaths.

These findings raise the question of whether health insurance reduces mortality risk from external causes related to substance abuse and mental health in addition to disease-related causes. While there is little direct evidence of this mechanism, prior work has found that Medicaid improves self-reported quality of life, reduces psychological distress, and reduces financial burdens, while also reducing rates of depression and increasing mental health treatment (Finkelstein et al., 2012; Baicker et al., 2013; Allen et al., 2017; McMorrow et al., 2017; Flavin, 2018; Winkelman and Chang, 2018; Gallagher et al., 2019). The ACA also brought about a substantial expansion in Medicaid benefits for the treatment of substance use disorders, benefits which have been further expanded through waiver programs to include coverage in residential treatment programs in many expansion states (Maclean and Saloner, 2019; Medicaid and CHIP Payment and Access Commission, 2023). The potential link between health insurance and mortality related to substance abuse and mental health merits further study in future work.

5.2 Possible spillovers on the mortality risk of untreated individuals

Throughout this paper, we emphasize estimates of the average effect of treatment on the treated, defined in this context to be Medicaid's effect on the mortality risk of people who enrolled because of expansions. We obtain these estimates by dividing the proportional mortality reduction by the first-stage enrollment effect. This approach assumes that expansions did not affect the mortality risk of low-income adults who did not enroll in Medicaid as a result of expansions, a population that includes people who were already enrolled in Medicaid due to disability or as very low-income parents, people with employer-sponsored insurance, and newly eligible adults who did not elect to enroll in Medicaid. Such spillovers, if present, could lead us to either understate or overstate the effect of treatment on the treated. To the extent that spillovers reduced the mortality risk of untreated individuals, our approach would overstate the effect of treatment on the treated by incorrectly attributing these reductions to people who enrolled in Medicaid, and vice versa if spillovers increased the mortality risk of untreated individuals.

Spillovers could arise if expansions affected provider behavior or access to medical care for untreated individuals. There is mixed evidence of expansions' effects on wait times, providers' willingness to accept Medicaid patients, and the intensity of medical treatment. Some studies find that Medicaid expansions increased wait times, while others find that temporary increases in reimbursement rates under the ACA improved access to care even among those eligible for Medicaid prior to expansions (Miller and Wherry, 2017; Tipirneni et al., 2016). Garthwaite (2012) finds that earlier Medicaid expansions to children decreased physician hours with a typical Medicaid patient but increased willingness to accept Medicaid patients. In addition, several studies have found that the ACA's Medicaid expansions reduced uncompensated care and improved hospital profitability, which in turn may have prevented hospital closures in rural areas and improved access to care among untreated individuals (Nikpay et al., 2017; Blavin, 2016; Lindrooth et al., 2018).

While this literature suggests that Medicaid expansions affected health care supply and provider behavior, their ultimate effect on the health of untreated individuals is unclear. Einav et al. (2020) find in a separate context that Medicare payment reforms had substantial spillover effects on the health of untreated individuals in the same direction as their effect on treated individuals, a finding which, applied to the present context, would suggest reductions in mortality risk among untreated individuals and suggest our treatment on the treated estimates are too large. Our own analyses offer some evidence in the opposite direction: the estimated treatment-on-the-treated effect falls from 21 to 12 percent (although not statistically significantly) when we include in our sample disabled individuals receiving public health insurance prior to expansions, a finding that could suggest that expansions increased the mortality risk disabled individuals who were enrolled in Medicaid prior to expansions. At the same time, differencing out enrollment and mortality effects in the higher-income population under our triple differences approach causes the treatment-on-the-treated estimate to decline only slightly, from 21 to 17 percent, suggesting that spillover effects on the higher-income population, if present, are small. We emphasize, however, that these findings are only suggestive and reserve more rigorous analyses for future work.

In summary, existing evidence does not clearly establish the existence or sign of spillover effects on untreated individuals' mortality risk, but this is an evolving literature that may eventually shed light on the magnitude and direction any potential bias in our estimates of the average effect of treatment on the treated.

5.3 Potential bias from migration

We index individuals in our study by the state where they resided during the 2010 Census. Migration between expansion and non-expansion states would tend to bias both enrollment and mortality effects towards zero, assuming monotonicity in expansion's effect on enrollment and mortality. This is because migration from expansion to non-expansion states will decrease the probability of Medicaid enrollment and increase mortality risk, outcomes which we will then incorrectly attribute to expansion states, and conversely for migration from non-expansion to expansion states.

We consider the potential scope for such bias using address information available in IRS information return forms, such as W-2s and 1099-Rs, to examine the extent of migration. Table 12 indicates the annual share of those who received an information return in 2010 who received an information return in the same state each year between 2011 and 2019, conditional on being alive at the end of 2019. About 14 percent of those who received an information return in 2010 did not receive an information return in that same state in 2019, so this share offers an upper bound on the ten-year migration rate. A sizable share of this migration is likely to have occurred between states that shared the same expansion status, a scenario which would not lead to bias in our estimates. This evidence suggests that migration may bias our enrollment and mortality estimates somewhat towards zero but is likely a minor cause for concern.

6. Conclusions

This paper examines Medicaid's causal effect on mortality using the universe of low-income adults. Our dataset includes more than sixty times as many individuals as the second-largest study on this question and allows us to explore the upper limit of what we can learn about the magnitude of Medicaid's effect on mortality from recent expansions. We find that expansions increased Medicaid enrollment by 12 percentage points and reduced mortality by 2.5 percent in the low-income population, suggesting a 21 percent reduction in the mortality hazard of new enrollees. Additionally, our findings suggest similar proportional reductions in mortality across subgroups defined by age, race, ethnicity, gender, family status, income, and employment, although estimates are not statistically significant for all groups.

Our paper contributes to ongoing discussions about the costs and benefits of Medicaid expansions. Because our estimates are based on the universe of low-income adults targeted by recent expansions, they enable the most precise and comprehensive analyses to date regarding the life-saving effects and cost-effectiveness of these policies. We estimate that expansions saved about 27,400 lives between the ACA's passage in 2010 and the end of our study in 2022 and that a further 12,800 deaths could have been prevented in states that did not expand Medicaid. Medicaid's life-saving benefits accrue not only to older age cohorts, who account for about three-quarters of lives saved, but also to younger adults, who account for nearly half of life-years saved due to their longer lifespans and large share of the low-income adult population. Our results further indicate that Medicaid expansions may be a cost-effective way to save lives, with estimates of $5.4 million per life saved and $179,000 per life-year saved falling well below valuations of a statistical life and life-year found in the literature. These analyses highlight the significant health improvements caused by Medicaid expansions and avoidable deaths in states that have not yet expanded, while also bringing attention to potential adverse consequences from administrative barriers to Medicaid enrollment and the unwinding of continuous enrollment policies established during the COVID-19 pandemic.

Beyond these policy implications, this paper sheds new light on the complex relationship between health, health insurance, and socioeconomic disadvantage. We add to a growing body of literature showing that health insurance, and Medicaid in particular, improves health. What sets our study apart is the exceptional precision of our estimates and their broad applicability, a finding that challenges the notion that insurance only reduces mortality for older adults and high-risk subgroups while also suggesting that point estimates from key prior studies may have been too large. We also contribute to the literature on the socioeconomic gradient in health by investigating the extent to which incomplete insurance coverage contributes to mortality disparities by income. Our predictions suggest universal coverage would narrow the mortality gap between the highest and lowest income quintiles by five to twenty percent, a substantial reduction in mortality disparities that would likely produce meaningful improvements in well-being. At the same time, this finding illustrates the entrenched nature of such disparities, which appear to be driven primarily by factors other than the inability to afford medical care, with alternative explanations from the literature including the life-cycle effects of health endowments and shocks on human capital, the effect of income and education on health-related behaviors, and neighborhood effects like violence and environmental quality.

In addition to its substantive contributions, this paper shows that the practice of preregistration, which is standard in experimental work, can be applied to observational studies as well, although we leave more general discussion of the pros and cons of preregistration in such settings to others (Burlig, 2018; Christensen and Miguel, 2018). As emphasis on transparency in economic research continues to grow, others may wish to consider whether preregistration is a feasible and desirable strategy for bolstering the credibility of contributions to important and intensely debated questions, as this paper has done with Medicaid's effect on mortality.

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Tables for Saved by Medicaid: New Evidence on Health Insurance and Mortality from the Universe of Low-Income Adults

Table 1: Descriptive Statistics, Ages 19-59 in 2010, Income $<$ 138% Federal Poverty Level

	Main Sample (Non-Disabled)	Full Sample (Including Disabled)
Died in 2010-2022	0.0504	0.0709
Medicaid in 4/2010	0.2581	0.3169
Age in 2010		
Mean	34.61	35.78
19-24	0.2666	0.2440
25-29	0.1616	0.1506
30-34	0.1198	0.1141
35-39	0.1026	0.1001
40-44	0.0986	0.1000
45-49	0.0946	0.1019
50-54	0.0830	0.0962
55-59	0.0651	0.0822
Female	0.5189	0.5186
Black	0.1766	0.1844
Other Race	0.1657	0.1587
Hispanic	0.2082	0.2014
Married	0.2573	
Parent	0.3694	
Income in 2009		
None	0.2583	
0-0.5 x FPL	0.2304	
0.5-1 x FPL	0.2933	
1-1.38 x FPL	0.2180	
Employed in 2009	0.7520	
N (Weighted)	42,270,000	47,320,000
N (Unweighted)	37,460,000	41,930,000

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2009 Medicare and Medicaid enrollment records, 2022 Numident. **Notes:** The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Table 2: Difference-in-Differences Estimates of Effect of Medicaid Expansion on Medicaid Enrollment

	Main Sample (Non-Disabled)		Full Sample (Incl Disabled)	
	Ever in Year	Days in Year	Ever in Year	Days in Year
Post x Expansion	0.117***	35.81***	0.106***	32.57***
	(0.017)	(5.992)	(0.016)	(5.808)
N (People x Years)	441,200,000	441,200,000	489,300,000	489,300,000
N (People)	37,460,000	37,460,000	41,930,000	41,930,000
Mean Medicaid Enrollment				
Expansion States (Pre-Period)	0.24	67.97	0.30	89.82
Non-Expansion States	0.20	51.56	0.25	68.65
Demographic controls	Yes	Yes	Yes	Yes
Fixed effects	State, Year	State, Year	State, Year	State, Year
Std Error Clustering	State	State	State	State

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records, 2022 Numident. **Notes:** Sample includes adults with 2009 income $<$ 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Table 3: Difference-in-Differences Estimates of Effect of Medicaid Expansion on Mortality

	Main Sample (Non-Disabled)		Full Sample (Incl Disabled)	
	Coefficient	Proportional Change	Coefficient	Proportional Change
Post x Expansion	-0.0249**	-2.46%	-0.0128*	-1.27%
SE or 95% CI	(0.011)	(-4.52%, -0.40%)	(0.008)	(-25.96%, 2.15%)
N (People x Years)	441,200,000	441,200,000	489,300,000	489,300,000
N (People)	37,460,000	37,460,000	41,930,000	41,930,000
Demographic controls	Yes	Yes	Yes	Yes
Fixed effects	State, Year	State, Year	State, Year	State, Year
Std Error Clustering	State	State	State	State

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records, 2022 Numident. **Notes:** Sample includes adults with 2009 income $<$ 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Table 4: Effect of Medicaid Expansion on Mortality, Treatment on the Treated Estimates

	Main Sample (Non-Disabled)		Full Sample (Including Disabled)	
	Ever in Year	Full Year	Ever in Year	Full Year
Treatment-on-the-Treated Estimate	-21.02%	-25.07%	-12.00%	-14.25%
95% CI - Upper Bound	-3.68%	-4.39%	2.72%	2.35%
95% CI - Lower Bound	-38.00%	-45.32%	-25.77%	-30.61%

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records, 2022 Numident.
Notes: Treatment on treated estimate assumes no spillovers, i.e. no effect of Medicaid expansion on people not induced to enroll. Full year of enrollment assumes linear relationship between days of enrollment and mortality hazard reduction. Confidence interval takes first-stage estimate to be fixed (non-stochastic). The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Table 5: Medicaid's Causal Effect on Enrollment and Mortality by Subgroup, Non-Disabled Ages 19-59 in 2010

Subgroup	Enrollment			Mortality				Treatment-	Obs
	Coefficient	Signif.	Std Error	Coefficient	Signif.	Std Error	Pct Change	on-Treated	
19-29	0.0993	***	(0.01766)	-0.0260		(0.0223)	-2.57%	-25.85%	198,200,000
30-39	0.1130	***	(0.01618)	-0.0311	*	(0.0164)	-3.06%	-27.10%	96,690,000
40-49	0.1476	***	(0.01551)	-0.0233	*	(0.0131)	-2.30%	-15.60%	84,290,000
50-59	0.1454	***	(0.02197)	-0.0256	**	(0.0111)	-2.53%	-17.38%	62,100,000
Employed	0.1070	***	(0.01692)	-0.0264	***	(0.0103)	-2.61%	-24.35%	356,300,000
Unemployed	0.1317	***	(0.01615)	-0.0206		(0.0140)	-2.04%	-15.48%	84,650,000
Female	0.1088	***	(0.02054)	-0.0230	*	(0.0122)	-2.27%	-20.90%	232,300,000
Male	0.1188	***	(0.01443)	-0.0254	***	(0.0105)	-2.51%	-21.11%	208,900,000
Black	0.1363	***	(0.01797)	-0.0284	**	(0.0128)	-2.80%	-20.54%	76,920,000
Other race	0.1449	***	(0.02456)	-0.0152		(0.0168)	-1.51%	-10.41%	68,150,000
White	0.1006	***	(0.01597)	-0.0220	*	(0.0117)	-2.18%	-21.63%	296,200,000
Hispanic	0.1473	***	(0.02593)	-0.0478	***	(0.0178)	-4.67%	-31.69%	86,200,000
Non-Hispanic	0.1051	***	(0.01474)	-0.0170		(0.0129)	-1.69%	-16.04%	355,000,000
Married	0.1066	***	(0.01671)	-0.0288	**	(0.0139)	-2.84%	-26.63%	122,300,000
Unmarried	0.1135	***	(0.01804)	-0.0229	**	(0.0113)	-2.26%	-19.95%	318,900,000
Parent	0.0937	***	(0.01707)	-0.0226	*	(0.0136)	-2.23%	-23.85%	177,000,000
Non-Parent	0.1245	***	(0.01932)	-0.0260	***	(0.0112)	-2.57%	-20.61%	264,200,000
Foreign-Born	0.1387	***	(0.0275)	0.0081		(0.0201)	0.81%	5.83%	72,080,000
U.S.-Born	0.1087	***	(0.0155)	-0.0247	***	(0.0124)	-2.4%	-22.40%	369,200,000
100-138% FPL	0.1011	***	(0.01656)	-0.0325	**	(0.0146)	-3.20%	-31.63%	105,200,000
50-100% FPL	0.1077	***	(0.01738)	-0.0187	*	(0.0103)	-1.85%	-17.20%	140,900,000
No Income	0.1312	***	(0.01656)	-0.0253	**	(0.0123)	-2.50%	-19.04%	88,150,000
0-50% FPL	0.1099	***	(0.01732)	-0.0210		(0.0149)	-2.08%	-18.91%	107,000,000
GLM (2-yr)	0.1000	***	(0.01534)	-0.0197	***	(0.0071)	-1.95%	-19.51%	207,600,000
GLM (8-yr)	0.1454	***	(0.01727)	-0.0243	***	(0.0104)	-2.40%	-16.51%	146,400,000
MJW (4-yr)	0.1334	***	(0.02046)	-0.0516	***	(0.0092)	-5.03%	-37.70%	176,500,000
MJW (8-yr)	0.1454	***	(0.02197)	-0.0256	**	(0.0111)	-2.53%	-17.38%	62,100,000
OHIE (2-yr)	0.0644	***	(0.01307)	-0.0233	***	(0.0070)	-2.30%	-35.76%	265,500,000
OHIE (8-yr)	0.1145	***	(0.01733)	-0.0233	**	(0.0102)	-2.30%	-20.11%	336,000,000

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.
Notes: Sample includes adults with 2009 income $<$ 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. All specifications include demographic controls, including (where applicable) indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Table 6: Effect of Medicaid Expansion on Mortality, Triple Difference Estimates Comparing Low- and High-Income Groups (Non-Disabled, Ages 19-59 in 2010)

	Enrollment	Mortality		Treatment-on-
		Coefficient	Proportional Change	the-Treated
Post x Expansion	0.09702***	-0.01731***	-1.72%	-17.69%
SE or 95% CI	(0.000116)	(-0.01731)	(-0.33%, -3.09%)	(-31.82%, -3.35%)
N (People x Years)	715,800,000	715,800,000	715,800,000	715,800,000
N (People)	59,650,000	59,650,000	59,650,000	59,650,000
Demographic controls	Yes	Yes	Yes	Yes
Fixed effects	State, year, and interactions	State, year, and interactions	State, year, and interactions	State, year, and interactions
Std errors	Robust	Robust	Robust	Robust

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.
Notes: Sample includes adults with 2009 income $<$ 1.38x the Federal Poverty Level (FPL) and income 4-6x the FPL according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Table 7: Effect of Medicaid Expansion on Mortality in Adults with Income 4-6x the Federal Poverty Level (FPL), Non-Disabled, Ages 19-59 in 2010

	Enrollment	Mortality		Treatment-on-the-Treated
		Coefficient	Proportional Change	
Post x Expansion	0.0147***	-0.00467	-0.47%	-31.69%
Std Error or 95% CI	(0.00252)	(0.00914)	(-2.23%, 1.33%)	(-151.9%, 90.7%)
N (People x Years)	274,600,000	274,600,000	274,600,000	274,600,000
N (People)	22,880,000	22,880,000	22,880,000	22,880,000
Demographic controls	Yes	Yes	Yes	Yes
Fixed effects	State, Year	State, Year	State, Year	State, Year
Std Error Clustering	State	State	State	State

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.
Notes: Sample includes adults with 2009 income 4-6x the FPL according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Table 8: Lives and Life-Years Saved by Expansions in 2010-2022 by 2010 Age Cohort

A: Lives Saved by Expansions

Age in 2010	Share Survived to 2022			Population	Lives Saved	Share
	Medicaid	*No Medicaid*	*Difference*			
19-29	0.9825	0.9822	0.0003	12,300,000	4,052	0.15
30-39	0.9688	0.9682	0.0006	6,369,994	3,959	0.14
40-49	0.9292	0.9278	0.0014	5,534,678	7,740	0.28
50-59	0.8534	0.8506	0.0028	4,241,927	11,673	0.43
Total				28,446,599	27,424	

B: Life-Years Saved by Expansions

Age in 2010	Average Life Expectancy			Population	Life-Years Saved	Share
	Medicaid	*No Medicaid*	*Difference*			
19-29	50.98	50.96	0.0164	12,300,000	201,384	0.24
30-39	41.19	41.16	0.0243	6,369,994	155,104	0.19
40-49	32.08	32.04	0.0412	5,534,678	228,254	0.27
50-59	23.65	23.59	0.0580	4,241,927	246,147	0.30
Total				28,446,599	830,890	

Sources: 2010 Census, 2010 Social Security Administration Life Tables, 2010-2013 National Health Interview Survey (NHIS). **Notes:** Survival and life expectancy rates under ”No Medicaid” are estimated using 2010 SSA life tables, with mortality hazards inflated by 1.424, which is the ratio of the mortality hazard among adults with incomes under 1.38 times the poverty level to the mortality hazard in the general adult population according to the 2010-2013 NHIS. Survival and life expectancy under ”Medicaid” are calculated by deflating the mortality hazard by our estimated treatment effect in post-expansion years.

Table 9: Avoidable Deaths (and Remaining Life-Years) if Non-Expansion States Had Expanded in 2014, by 2010 Age Cohort

A: Avoidable Deaths

Age in 2010	Share Survived to 2022 *Medicaid*	*No Medicaid*	*Difference*	Population	Avoidable Deaths	Share
19-29	0.9825	0.9822	0.0003	5,977,088	1,868	0.15
30-39	0.9688	0.9682	0.0006	3,095,448	1,845	0.14
40-49	0.9291	0.9278	0.0013	2,689,533	3,607	0.28
50-59	0.8533	0.8506	0.0026	2,061,331	5,438	0.43
Total				13,823,400	12,757	

B: Remaining Life-Years Associated with Avoidable Deaths

Age in 2010	Average Life Expectancy *Medicaid*	*No Medicaid*	*Difference*	Population	Remaining Life-Years	Share
19-29	50.98	50.96	0.0163	5,977,088	97,405	0.24
30-39	41.19	41.16	0.0243	3,095,448	75,372	0.19
40-49	32.08	32.04	0.0412	2,689,533	110,918	0.28
50-59	23.65	23.59	0.0580	2,061,331	119,613	0.30
Total				13,823,400	403,308	

Sources: 2010 Census, 2010 Social Security Administration Life Tables, 2010-2013 National Health Interview Survey (NHIS). **Notes:** Survival and life expectancy rates under ”No Medicaid” are estimated using 2010 SSA life tables, with mortality hazards inflated by 1.424, which is the ratio of the mortality hazard among adults with incomes under 1.38 times the poverty level to the mortality hazard in the general adult population according to the 2010-2013 NHIS. Survival and life expectancy under ”Medicaid” are calculated by deflating the mortality hazard by our estimated treatment effect in post-expansion years.

Table 10: Insurance and Mortality by Income Quintile and Predicted Disparity Reduction with 100% Insured (Non-Disabled, Ages 19-59 in Survey Year)

			Observed				Predicted				
			Mortality Hazard				Mortality Hazard				
Income Quintile	Lower Bound Relative to FPL	Share Insured	Insured	Uninsured	All	Relative to Highest Quintile	Insured	Uninsured	All	Relative to Highest Quintile	Share of Disparity Eliminated
$\tau = -21\%$											
1	0%	0.53	0.0026	0.0034	0.0030	1.9041	0.0026	0.0027	0.0026	1.7122	21.2%
2	120%	0.60	0.0027	0.0025	0.0026	1.6751	0.0027	0.0020	0.0024	1.5607	16.9%
3	220%	0.79	0.0019	0.0031	0.0021	1.3760	0.0019	0.0024	0.0020	1.3088	17.9%
4	360%	0.91	0.0015	0.0033	0.0017	1.0588	0.0015	0.0026	0.0016	1.0344	41.4%
5	550%	0.96	0.0015	0.0033	0.0016	1.0000	0.0015	0.0026	0.0015	1.0000	-
$\tau = -5.3\%$											
1	0%	0.53	0.0026	0.0034	0.0030	1.9041	0.0026	0.0032	0.0029	1.8562	5.3%
2	120%	0.60	0.0027	0.0025	0.0026	1.6751	0.0027	0.0024	0.0026	1.6465	4.2%
3	220%	0.79	0.0019	0.0031	0.0021	1.3760	0.0019	0.0029	0.0021	1.3592	4.5%
4	360%	0.91	0.0015	0.0033	0.0017	1.0588	0.0015	0.0031	0.0016	1.0527	10.3%
5	550%	0.96	0.0015	0.0033	0.0016	1.0000	0.0015	0.0031	0.0016	1.0000	-
N	516,201										

Sources: National Health Interview Survey (2010-2013). **Notes:** Table indicates insurance and mortality by quintile of income-to-poverty ratio among non-disabled adults surveyed in the NHIS in 2010-2013. Share insured is calculated in survey year. Mortality hazard is taken as average of 2011-2014 annual mortality hazards. Predicted mortality hazard is a weighted average of observed mortality hazard and the mortality hazard assuming the reduction indicated by tau, where the weights are equal to the share insured and uninsured, respectively.

Table 11: Leading Causes of Death by 2010 Age Cohort, By Age at Beginning (2010) and End (2022) of Study

Rank	Cause of Death Based on 2010 Age		Cause of Death Based on 2022 Age	
	Cause	Share of Deaths	Cause	Share of Deaths
	Ages 19-29 in 2010		Ages 31-41 in 2022	
1	Accidents (unintentional injuries)	49.2%	Accidents (unintentional injuries)	39.4%
2	Intentional self-harm (suicide)	19.3%	Intentional self-harm (suicide)	12.5%
3	Assault (homicide)	14.4%	Malignant neoplasms	12.3%
4	Malignant neoplasms	4.9%	Diseases of heart	12.1%
5	Diseases of heart	4.5%	Assault (homicide)	6.4%
	Ages 30-39 in 2010		Ages 42-51 in 2022	
1	Accidents (unintentional injuries)	42.0%	Malignant neoplasms	23.3%
2	Intentional self-harm (suicide)	13.3%	Diseases of heart	21.3%
3	Malignant neoplasms	11.1%	Accidents (unintentional injuries)	20.3%
4	Diseases of heart	10.7%	Intentional self-harm (suicide)	7.3%
5	Assault (homicide)	7.1%	Chronic liver disease and cirrhosis	5.6%
	Ages 40-49 in 2010		Ages 52-61 in 2022	
1	Accidents (unintentional injuries)	23.5%	Malignant neoplasms	33.0%
2	Malignant neoplasms	21.2%	Diseases of heart	24.4%
3	Diseases of heart	20.0%	Accidents (unintentional injuries)	9.4%
4	Intentional self-harm (suicide)	8.3%	Chronic liver disease and cirrhosis	5.0%
5	Chronic liver disease and cirrhosis	5.4%	Chronic lower respiratory diseases	4.9%
	Ages 50-59 in 2010		Ages 62-71 in 2022	
1	Malignant neoplasms	31.4%	Malignant neoplasms	36.6%
2	Diseases of heart	24.1%	Diseases of heart	25.3%
3	Accidents (unintentional injuries)	11.0%	Chronic lower respiratory diseases	7.6%
4	Chronic liver disease and cirrhosis	5.4%	Diabetes mellitus	4.8%
5	Diabetes mellitus	4.5%	Cerebrovascular diseases	4.6%

Source: 2018 Centers for Disease Control and Prevention (CDC) WONDER Database.

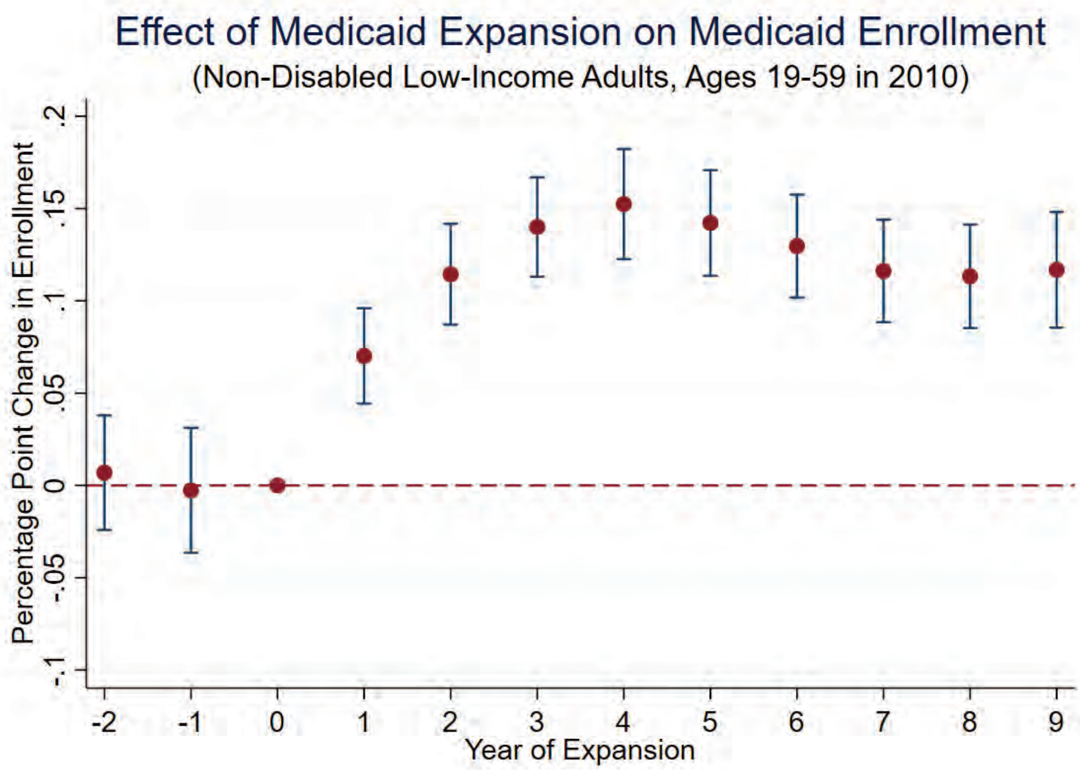
Table 12: Share of those with IRS information return in year in same state as in 2010, conditional on having 2010 IRS information return

Year	Share	N
2010		2,979,000
2011	0.961	2,706,000
2012	0.937	2,640,000
2013	0.919	2,582,000
2014	0.905	2,583,000
2015	0.893	2,583,000
2016	0.883	2,587,000
2017	0.874	2,583,000
2018	0.866	2,577,000
2019	0.859	2,574,000

Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2009 Medicare and Medicaid enrollment records, 2022 Numident. **Notes:** Sample includes non-disabled adults with 2009 income $<$ 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Sample in all years is restricted to those alive at the end of 2019. Migration is calculated on a 10% random sample of the low-income adult universe. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

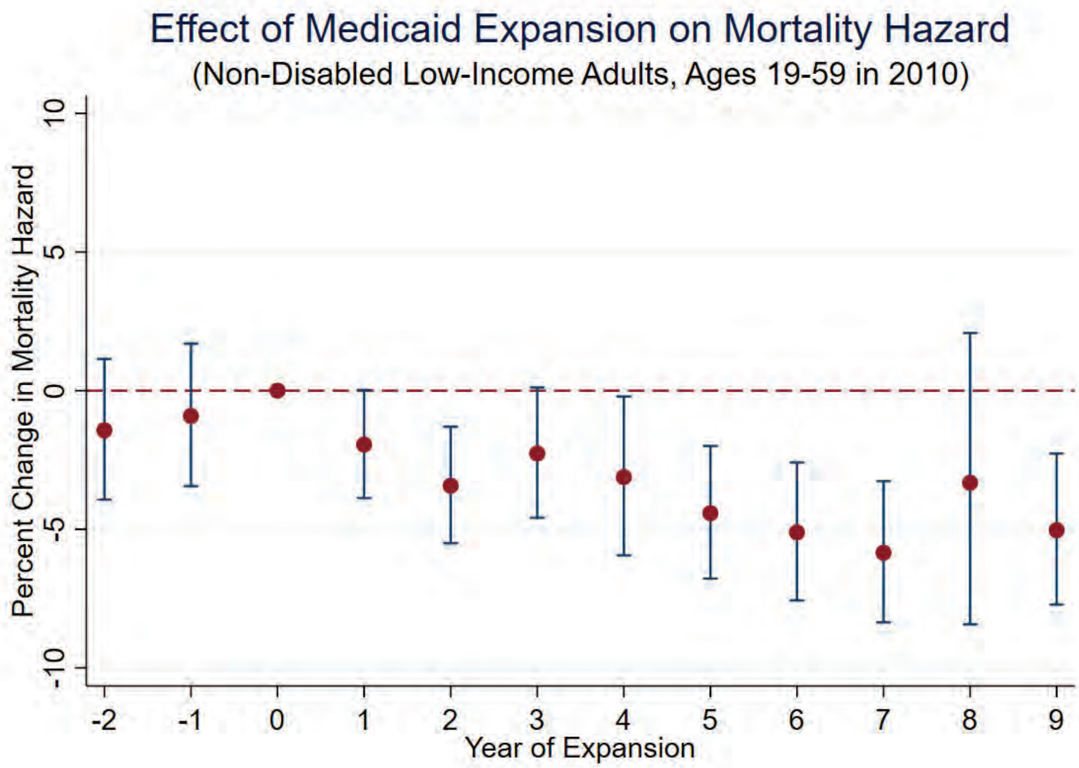
Figures for Saved by Medicaid: New Evidence on Health Insurance and Mortality from the Universe of Low-Income Adults

Figure 1



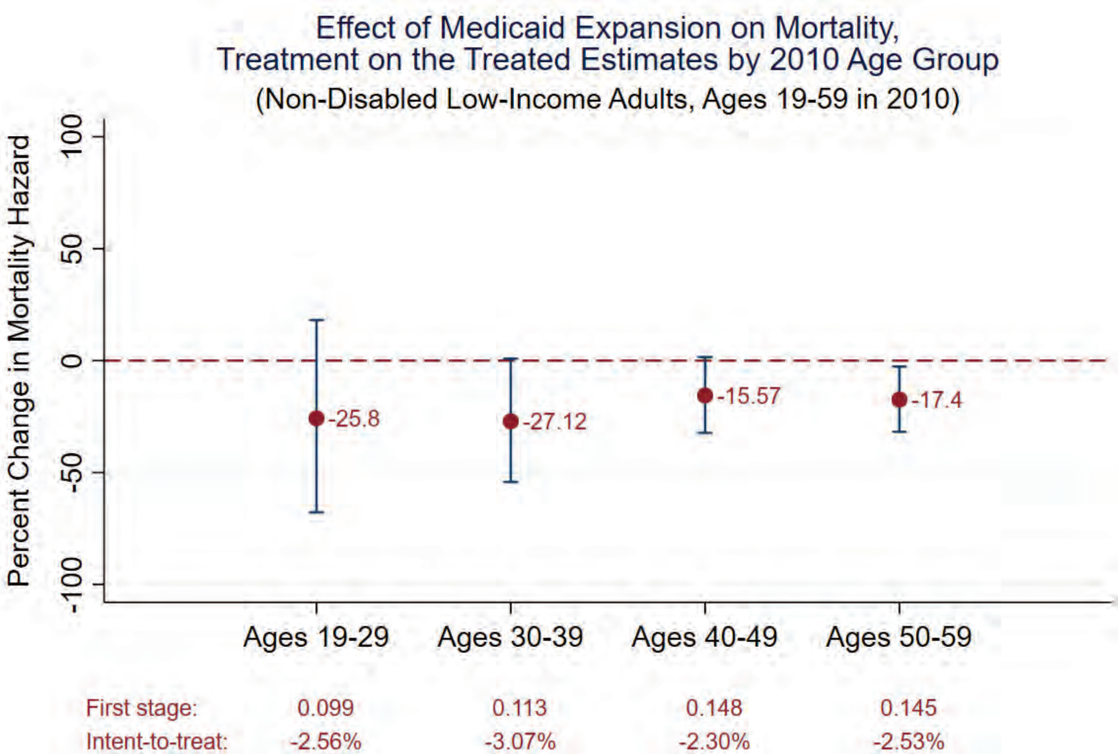
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.
Notes: Figure displays coefficients on event time dummies from event study model described in text, with Medicaid enrollment in the year as the outcome variable. Sample includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 2



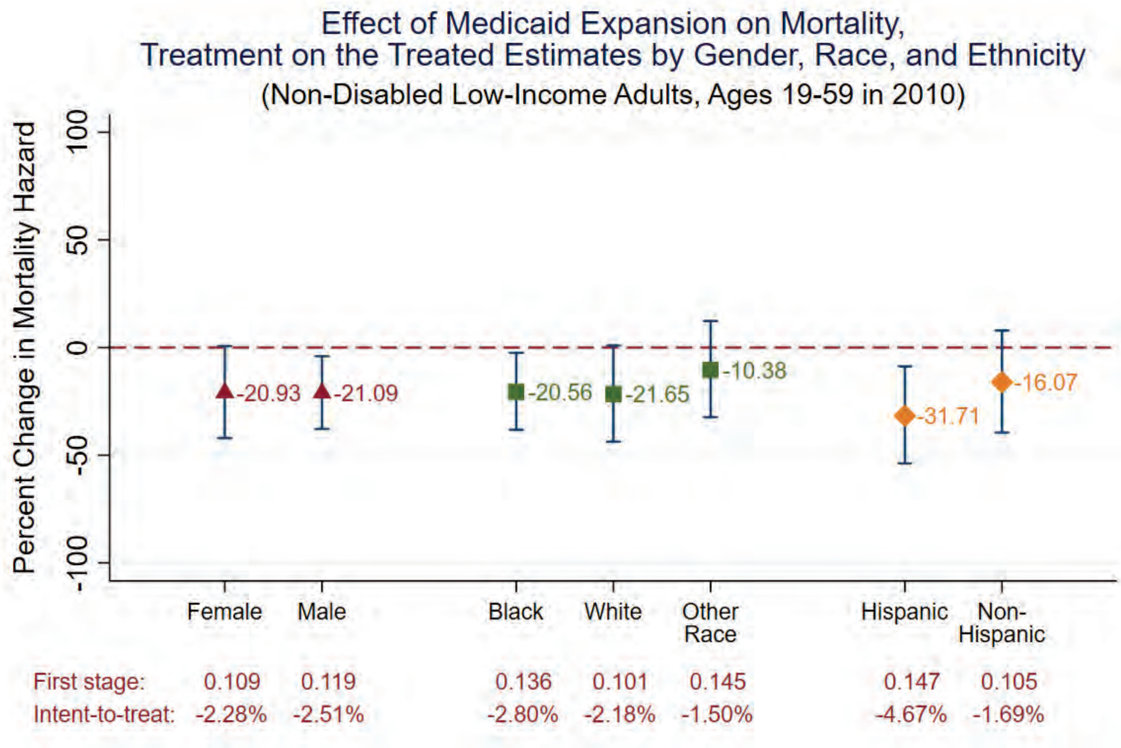
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2022 Numident. **Notes:** Figure displays proportional change in mortality implied by coefficients on event time dummies from event study mortality hazard model described in text. Sample includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 3



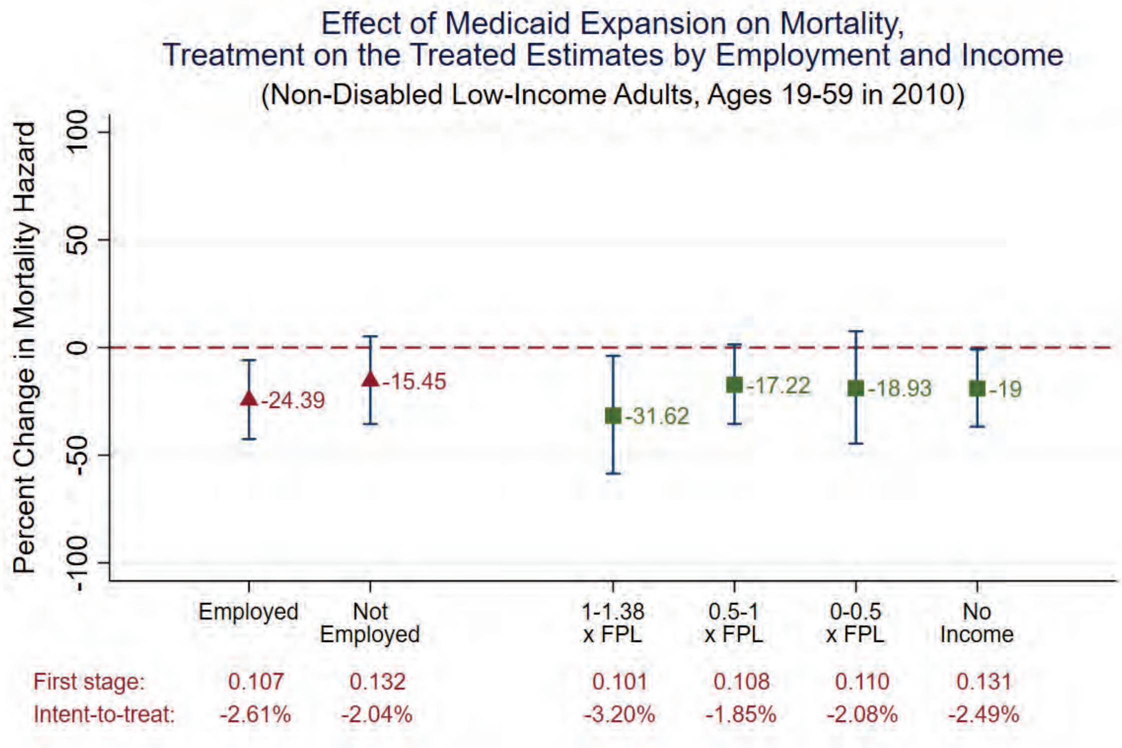
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.
Notes: Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the age groups indicated. Sample includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 4



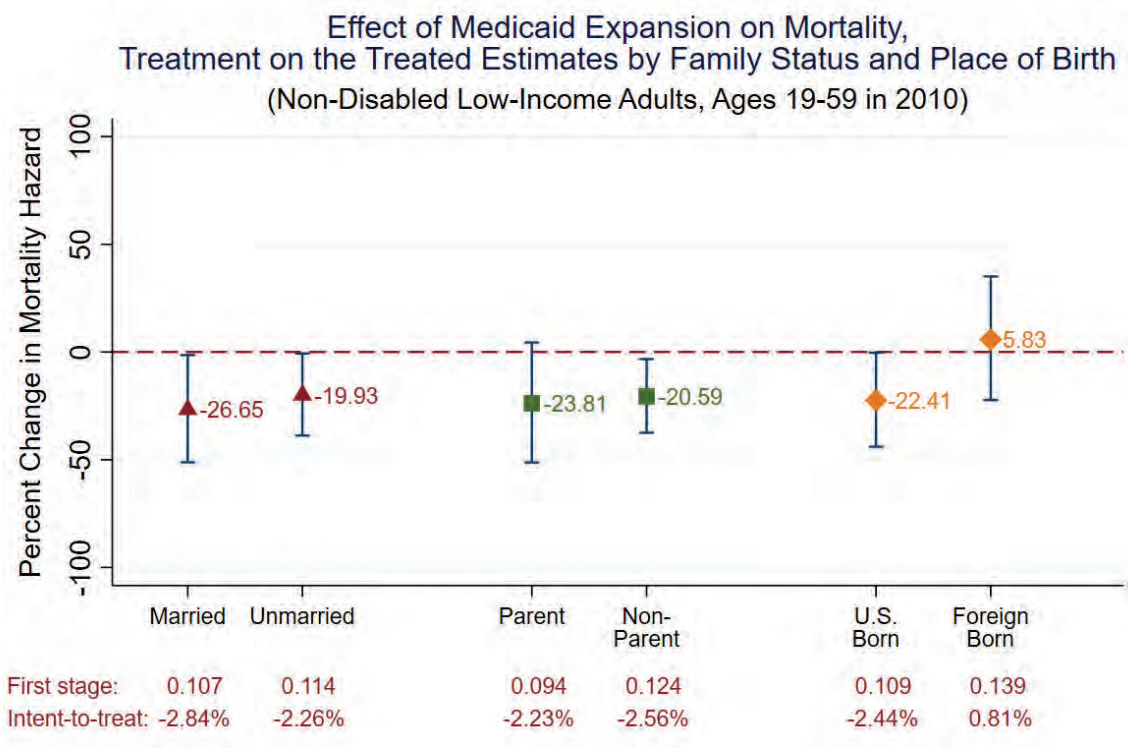
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.**Notes:** Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the demographic groups indicated. Sample includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for age groups, female, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 5



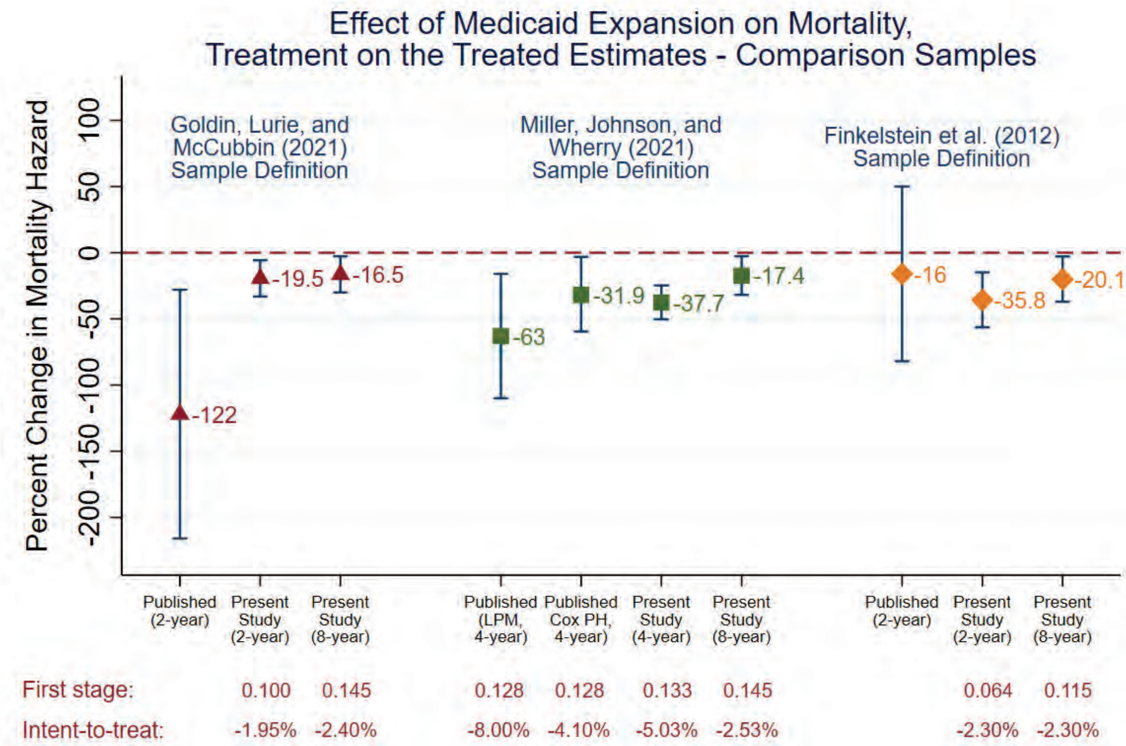
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.
Notes: Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the employment and income groups indicated. Sample includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 6



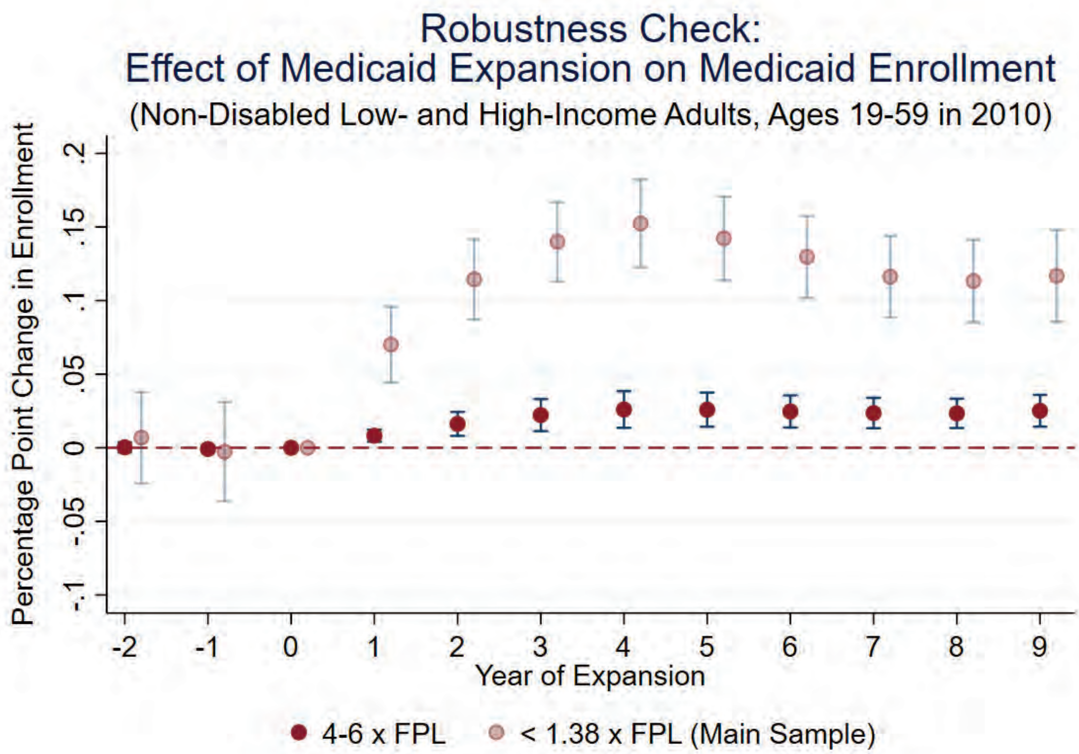
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.
Notes: Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the family status groups indicated. Sample includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 7



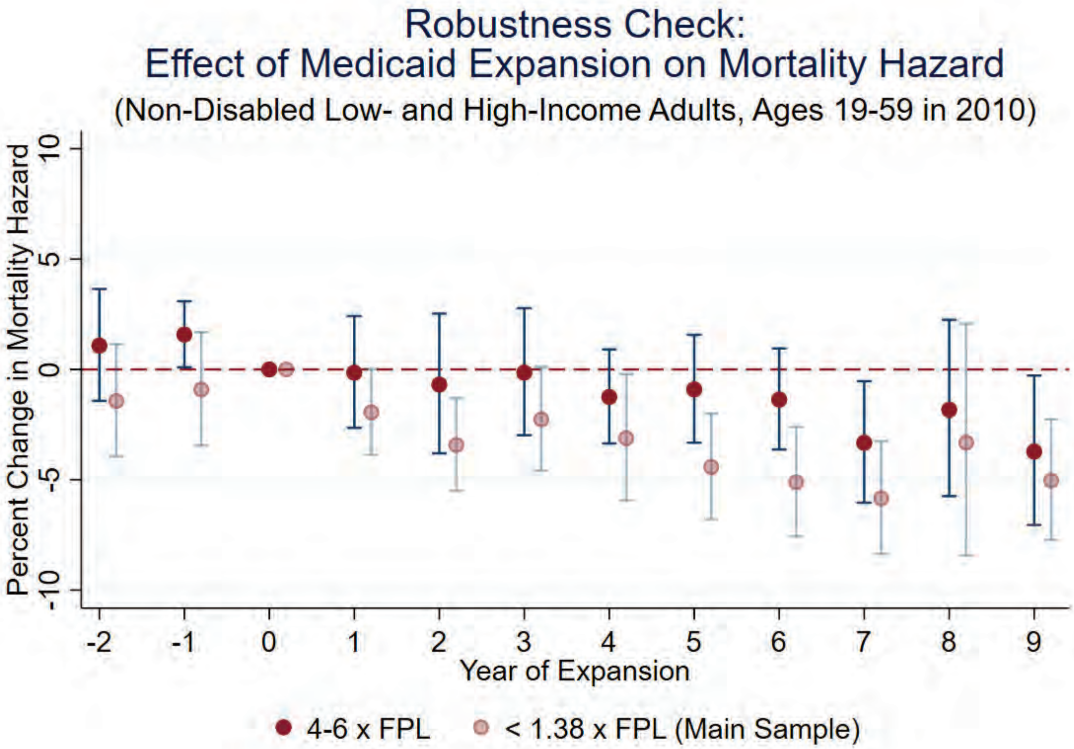
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident. Published estimates' source is the paper indicated on the figure. **Notes:** Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the sub-sampes described in the text. Sample includes non-disabled adults with 2009 income $<$ 1.38x the Federal Poverty Level (FPL) according to administrative tax data. Demographic controls include indicators for age groups, female, Black, other race, and Hispanic. We report the estimates from Goldin, Lurie, and McCubbin (2021) assuming a linear relationship between months of coverage and mortality. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 8



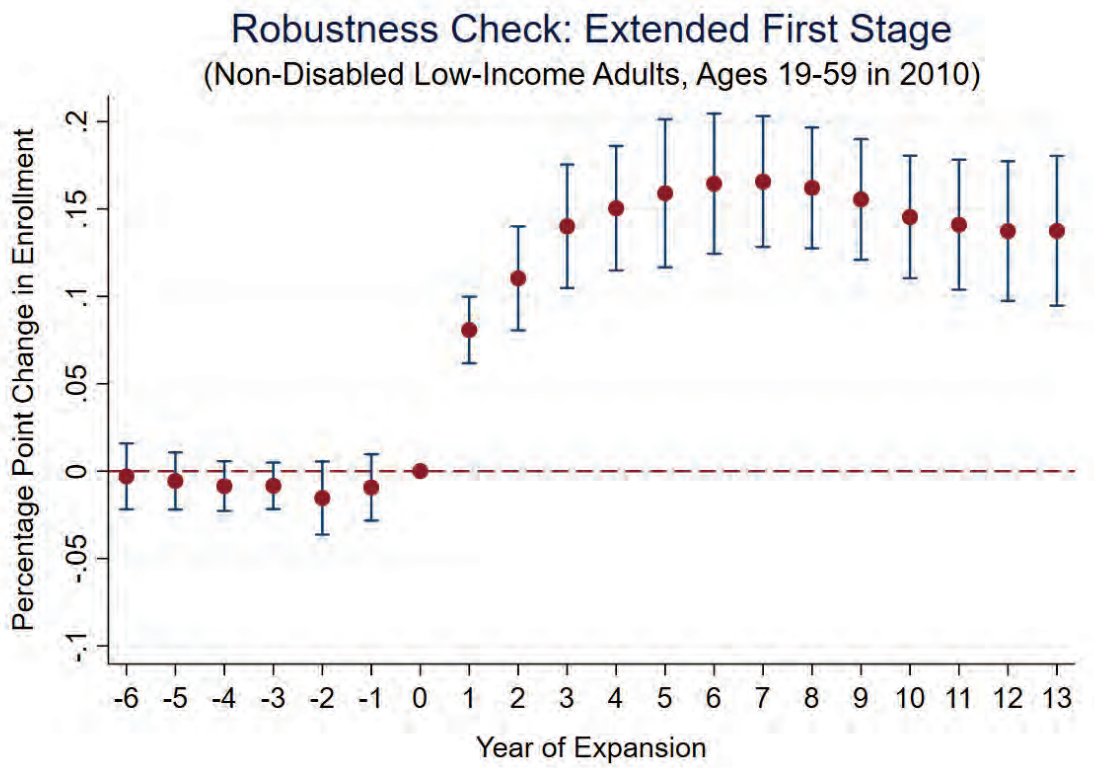
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2005-2019 Medicaid enrollment records; 2022 Numident.
Notes: Figure displays coefficients on event time dummies from event study model described in text, with Medicaid enrollment in the year as the outcome variable. Samples includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) and 4-6x the FPL according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 9



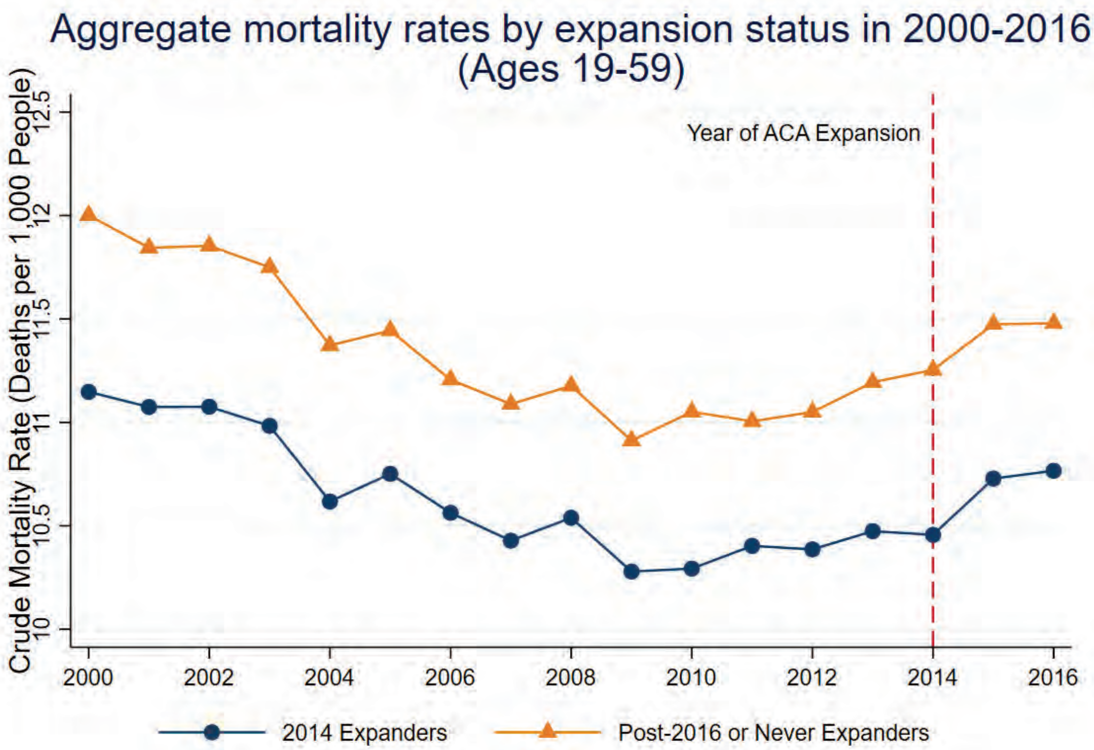
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2009 Medicare and Medicaid enrollment records, 2022 Numident. **Notes:** Figure displays proportional change in mortality implied by coefficients on event time dummies from event study mortality hazard model described in text. Sample includes non-disabled adults with 2009 income < 1.38x the Federal Poverty Level (FPL) and 4-6x the FPL according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 10



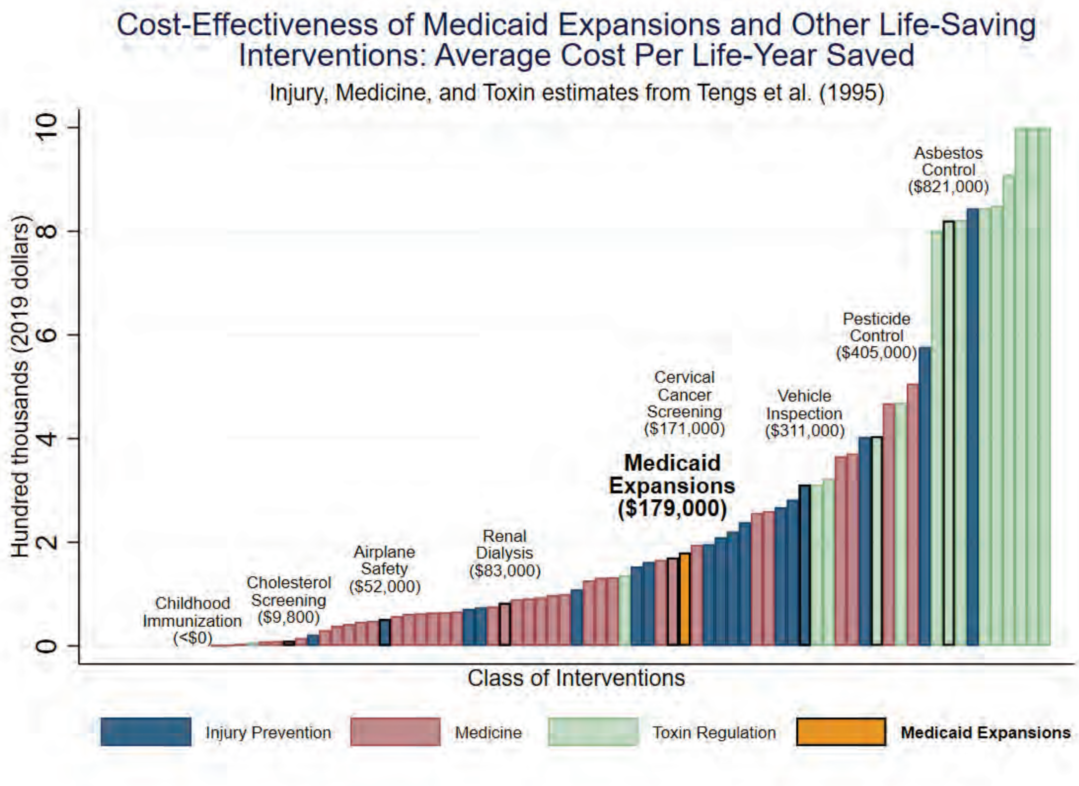
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2009 Medicare and Medicaid enrollment records, 2022 Numident. **Notes:** Figure displays coefficients on event time dummies from event study model described in text, with Medicaid enrollment in the year as the outcome variable, including Medicaid enrollment data from 2005 and later. Sample includes non-disabled adults with 2009 income $<$ 1.38x the Federal Poverty Level (FPL) according to administrative tax data who were ages 19-59 in 2010. Demographic controls include indicators for 5-year age groups, female, Black, other race, and Hispanic. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number CBDRB-FY2023-CES005-010.

Figure 11



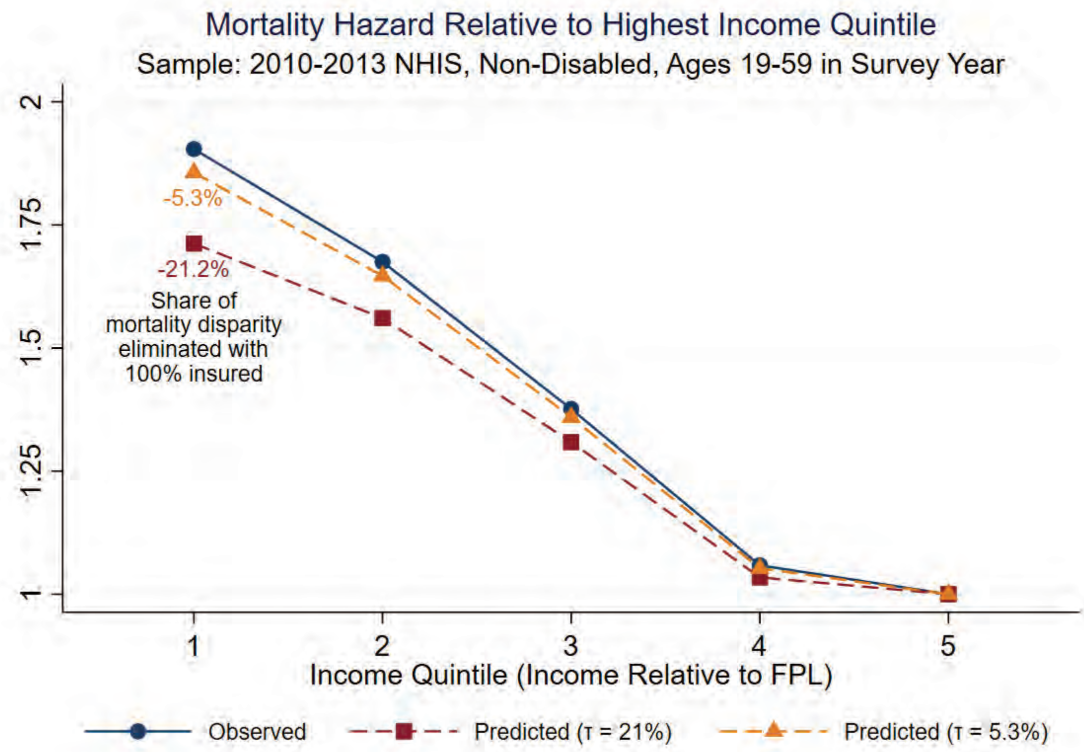
Source: Compressed Mortality File 2000-2016 on CDC WONDER Online Database, released June 2017. Centers for Disease" "Control and Prevention, National Center for Health Statistics *(public-use)*. **Notes:** Figure displays aggregate mortality rates in 200-2016 among those ages 19-59 in states that expanded Medicaid in 2014 and states that expanded after 2016 or never.

Figure 12



Sources: Tengs et al. (1995) *(public-use)*. **Notes:** Figure displays inflation-adjusted estimates of the cost per life saved by the 500 interventions reviewed in Tengs et al. (1995). We collapse interventions into seventy-three groups, taking the average cost per life-year saved in each group (e.g. airplane safety, childhood immunization, asbestos control). Costs are top-coded at $1 million and bottom coded at $0. Bars indicate average cost per life-year saved across interventions within a given class, e.g. "Airplane safety" includes several types of fire-prevention interventions and floor lighting.

Figure 13



Sources: National Health Interview Survey (2010-2013) *(public-use)*. **Notes:** Figure displays mortality hazard ratio relative to the highest income quintile, where income is defined as a share of the FPL. Predicted mortality ratio assumes all uninsured individuals in quintile obtain insurance and experience a mortality reduction of τ. Mortality hazard is taken as average of annual mortality hazards through the end of 2014.