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 novel approach in observational studies. We find that expansions increased Medicaid enrollment by 12 percentage points and reduced mortality of the low-income adult population by 2.5%, translating to a 21% reduction in the mortality hazard of new enrollees. Medicaid expansions' benefits appear to accrue not only to older age cohorts, 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. Expansions also appear to be a cost-effective means of saving lives, with direct budgetary costs of $5.4 million per life saved and $179,000 per life-year saved, well below valuations commonly found in the literature. Our findings suggest that universal Medicaid enrollment would reduce the mortality gap between high- and low-income groups by about five to twenty percent. We contribute to a growing body of work showing that health insurance improves health and demonstrate that Medicaid's life-saving effects extend across a much broader swath of the low-income population than previously understood.

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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. 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 (Kaiser Family Foundation, 2019). Understanding Medicaid's effect on mortality, a fundamental indicator of health and wellbeing, is crucial to evaluating this monumental 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 reliable evidence on how health insurance affects health" beyond certain vulnerable sub-populations (Levy and Meltzer, 2008; Black et al., 2019). Two influential recent studies challenged this view using large individual-level datasets and compelling identification strategies, finding that Medicaid and health insurance substantially reduce mortality risk for older adults. These studies' confidence intervals, however, included both very small and very large effects and they lacked the statistical power to detect effects in the overall low-income adult population, where younger adults make up the majority of newly eligible Medicaid enrollees (Miller, Johnson, and Wherry, 2021; Goldin, Lurie, and McCubbin, 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. Identifying effects in the overall low-income 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 eligibility under ACA expansions. We calculate income for these individuals by linking the Census to Internal Revenue Service (IRS) records, and approach that permits more accurate identification of newly eligible adults than in prior work using self-reported income. We 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. We examine heterogeneity by age, race, ethnicity, gender, family status, income, and employment and estimate our model on samples that align with those used in 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 on a question that has long been the subject of vigorous debate in the health economics literature. While preregistration may not be feasible or desirable with many observational studies, the present setting is ideal for this practice because its central research question is "important, intensely debated, and well-defined" (Christensen and Miguel, 2018). We are aware of only one other instance of pre-registration on a nonexperimental study in economics (Neumark, 2001).

We estimate that the 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 subgroups defined by age, race, ethnicity, gender, family status, income, and employment, although estimates are not statistically significant for all groups. Our point estimates fall in the lower end of the wide confidence intervals from comparable prior work and offer a substantial improvement in precision, with our own confidence intervals excluding the large mortality reduction point estimates from key prior studies.

We estimate 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). Our estimates 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 are among those who were 40 and older in 2010, younger individuals account for nearly half of all life-years saved due to their longer 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.

We also use our estimates to predict the share of the mortality disparity between the highest and lowest income quintiles that would be eliminated if all uninsured individuals in the U.S. gained health insurance. We predict that universal public health insurance would eliminate about five to twenty percent of the mortality gap, with these bounds reflecting a range of assumptions about how the average treatment effect across all individuals who were uninsured prior to Medicaid expansions differs from the average effect for compliers in our study. We consider estimates in the middle of this range to be more plausible because our estimates come from a setting with likely substantial selection into Medicaid enrollment among those with the greatest expected benefit. In other words, universal health insurance would lead to a meaningful but proportionally modest reduction in the mortality gap between high- and low-income people. This finding suggests that lack of insurance may play some role in explaining the socioeconomic gradient in health in the United States but is not its predominant cause, a finding that is consistent with the existence of such a gradient in many countries that provide universal public health insurance.

Beyond its contributions to policy debates, our paper adds to the 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 multi-directional 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).

These complexities explain why the relationship between health insurance and health remains the subject of heated debate more than forty years after RAND's seminal health insurance experiment (Brook et al., 1983). 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) but establishing the causal link to physical health outcomes has proven more challenging. Until recently, evidence of mortality reductions was limited to vulnerable sub-populations like infants, young children, and people with high-risk health conditions such as HIV/AIDS (Levy and Meltzer, 2008).

The ACA spurred a new era of research into this question, with studies by (Miller et al., 2021) and (Goldin et al., 2021) finding that health insurance reduced mortality in older low-income adults. These studies left open, however, the question of Medicaid's effect on mortality in the broader adult population. The present study advances this literature using the universe of low-income adults to explore the limit of what we can learn about Medicaid's effect on mortality from this natural experiment. Our use of a pre-registered analysis plan further strengthens our contribution to the literature by bolstering this study's credibility.

This paper also relates 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, an association that may be stronger in the U.S. than in many countries that provide universal health insurance (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 shocks on investments in human capital and productivity, suggesting that causality flows from health to socioeconomic status (Schultz, 1961; Becker, 2007). At the same time, price theory suggests that higher incomes should lead to more consumption of health care services, widely understood to be normal goods, and hence potentially improved outcomes (Grossman, 1972).

To the extent that this last channel is driving health disparities, we would expect public health insurance – effectively a large subsidy for low-income individuals' consumption of health care – to narrow substantially the mortality gap between more and less advantaged individuals. Yet our findings suggest that universal public insurance would eliminate at most twenty percent of this disparity, and perhaps as little as five percent. In other words, our findings suggest that factors other than the lack of insurance and cost of health care play a more important role in driving these disparities. Alternative explanations from 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 heterogeneity 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 19-59 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.[1] 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.[2] 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

[1]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.

[2]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.

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).

Identifying expansion dates

Medicaid eligibility rules vary a great deal across states and over time in terms of income eligibility thresholds for parents, employment requirements, enrollment caps and freezes, the scope of benefits, and the presence of premium or other financial contribution requirements. Several states, such as New York, Vermont, and Delaware, offered broad Medicaid eligibility to low-income adults well before the ACA's passage (Kaiser Family Foundation, 2013).

For our analyses, we identify the date when most childless, low-income, non-disabled adults became eligible for Medicaid in each state. We obtain information on states' pre-ACA Medicaid eligibility rules from a dataset compiled by Burns and Dague (2016) and from CMS's Medicaid waiver tracker. We do not classify states as early expanders if pre-ACA eligibility was tied to employment, required premium contributions, if limited spots were available, or if enrollment was frozen prior to the ACA expansions, but we do classify states as early expanders even if coverage was not as comprehensive as full Medicaid. This approach leads us to classify six states as having expanded before 2010 (DE, HI, NY, VT in 1996; MA in 1997; MD in 2006) and six states as having expanded between 2010 and 2014 through the ACA's "early expansion" option (CT, CA, DC, MN in 2010 and NJ, WA in 2011).

2.2 Empirical Strategy

Estimating expansions' effects on enrollment and mortality

To estimate the effect of Medicaid expansion on enrollment, we consider a linear probability model where $Y_i st$ indicates Medicaid enrollment for person i in state s at any point in year t:

$$Y_{ist} = \tau \cdot I\{t \geq t_s^*\} + \delta_s + \delta_t + \gamma^{'} X_{ist} + \epsilon_{ist} \tag{1}$$

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.[3] 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 specify the mortality hazard $\lambda_i(t)$ using a discrete time model with a non-parametric baseline hazard, which we parameterize using a proportional form:

$$\lambda_i(t) = \lambda_0(t) \exp\left(z_i(t)^{'} \beta\right) \tag{2}$$

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.

To estimate the expansions' effect on mortality hazard, we let

$$z_i(t)^{'} \beta = \tau \cdot I\{t \geq t_s^*\} + \delta_s + \delta_t + \gamma^{'} X_{ist} \tag{3}$$

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

[3]Year-long periods in our sample run from April to March, reflecting our study's start date on the Census reference date of April 1, 2010.

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 survival 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., 2018). Unlike linear hazard models, which assume a constant additive effect of Medicaid on mortality risk across demographic groups, this model assumes a constant proportional treatment effect across groups. This assumption is intuitively attractive because it permits the treatment effect to be larger in absolute terms for demographic groups with greater underlying mortality risk, such as the elderly. This proportionality assumption is also consistent with many prior studies' findings on the relationship between 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. Meyer et al. (2023) find the COVID-19 pandemic had a similar proportional effect on the mortality hazard of homeless and housed populations. Moreover, the present study's finding of similar proportional treatment-on-the-treated effects of Medicaid on mortality across demographic groups despite substantial variation in first stage effects and baseline mortality risk is also consistent with our model's proportionality assumption.

Ideally, we would test the proportionality of Medicaid's effect on mortality directly with our Census Medicaid data and compare alternative functional forms, but our study lacks the power to discern between forms given that many of our estimates have t-statistics close to two. Instead, we consider a situation close to the natural experiment in our study where we have sufficient power to examine functional form but where exogeneity is not as clear. Figure 1 displays the annual mortality hazard of insured and uninsured individuals by age estimated using public National Health Interview Survey (NHIS) data (2000-2009, with mortality calculated through 2010). We see that the magnitude of the difference between uninsured and insured individuals' mortality exhibits a clear increasing pattern with age. Figure 2 further illustrates this point by plotting the ratio of uninsured-to-insured mortality risk and difference between uninsured and insured mortality risk by age. We fail to reject the null of constant proportional differences by age (p-value 0.3346) while easily rejecting the null of constant additive differences by age (p-value of approximately zero). These analyses, conducted in a higher-power setting that is similar to the natural experiment in this study, offer strong evidence that proportionality is a reasonable model for Medicaid's effect on the mortality hazard while also suggesting that a linear hazard model would be mis-specified.

Even though mortality occurs in continuous time, we model mortality in discrete time 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 probability of death in continuous time.

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. 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. 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 the course of our study between April 2010 and March 2022.

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 2 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 4 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 an 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 central importance 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 on the magnitude of this individual-level causal relationship 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. 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 Heterogeneity analysis

Table 5 indicates first stage and mortality estimates for sub-groups defined by age, gender, race, Hispanic ethnicity, income, employment, and parental and marital status. Differences in the first stage across groups, although not statistically significant, 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 5 through 8 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 11 percent (among those of races other than Black or white) to 32 percent among Hispanics. Differences from zero are not statistically significant for all groups, however, leading us to exercise caution in concluding that Medicaid reduced the mortality hazard of all groups.

At the same time, the fact that estimates' signs are consistently negative and treatment-on-the-treated effects have similar magnitude across groups offers encouraging evidence that treatment effects are not limited to subsets of the 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 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.[4] [5]

Figure 9 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 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 9 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

[4]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.

[5]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 9 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%, compared to the 122% and 28% reported in Figure 9

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 10 and 11 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 12 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 13 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.

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.

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 used by others, including (Finkelstein et al., 2023), is described in-depth in Appendix C. 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).[6] To

[6] 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.

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.[7] 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 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

[7] 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.

expansions resulted in an additional 28.7 million person-years of Medicaid enrollment (Kaiser Family Foundation 2019b). 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.[8]

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 other life-saving interventions reviewed in Tengs et al. (1995).[9] Figure 14 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

[8] 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.

[9] 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).

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 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.[10] 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:

$$\lambda_j^{full\ insurance} = r_j \lambda_j^{insured} + (1 - r_j)(1 - \tau)\lambda_j^{uninsured} \tag{4}$$

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, τ, is likely smaller due to selection bias in our study 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

[10] As with estimates of lives and life-years saved by Medicaid expansions, a preferred approach to the analyses in this section would draw on estimate of the mortality gradient by income based on restricted administrative data, but such estimates are not publicly available at the present date.

presumptively enroll patients seeking care, likely 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.[11]

Figure 15 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 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. This finding is consistent with the fact that even countries with universal public health insurance exhibit a socioeconomic gradient in health (Cutler et al., 2012). As these findings highlight, the lack of health insurance appears to play some role in explaining the socioeconomic gradient in health but is likely not its predominant cause. Health disparities appear to be driven by complex social and economic factors that go beyond differences in the ability to afford medical care. Other potentially important causal channels identified in economics literature include the effect of health endowments and shocks on productivity over the life cycle, the effect of income and education on health behaviors like substance use, exercise, and nutrition, and neighborhood effects like environmental quality and crime. 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).

[11] Our first-stage estimate suggested that 11.7 percent of low-income individuals enrolled in Medicaid as a result of expansions, which is 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 three-quarters has a treatment effect of 0 percent yields a lower bound of 5.2 percent for the entire uninsured population.

5 Discussion of mechanisms and caveats

In this section, we discuss 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. We are aware of only one other nonexperimental study in economics, Neumark (2001), that has employed this approach. While we leave more general discussion of the pros and cons of preregistration in observational analyses to others (Burlig, 2018; Christensen and Miguel, 2018), we offer the present study as one example of how this approach can serve to mitigate specification searching without imposing excessive constraints. 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
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-the-Treated
		Coefficient	Proportional Change	
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			Population	Avoidable Deaths	Share
	Medicaid	*No Medicaid*	*Difference*			
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			Population	Remaining Life-Years	Share
	Medicaid	*No Medicaid*	*Difference*			
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.

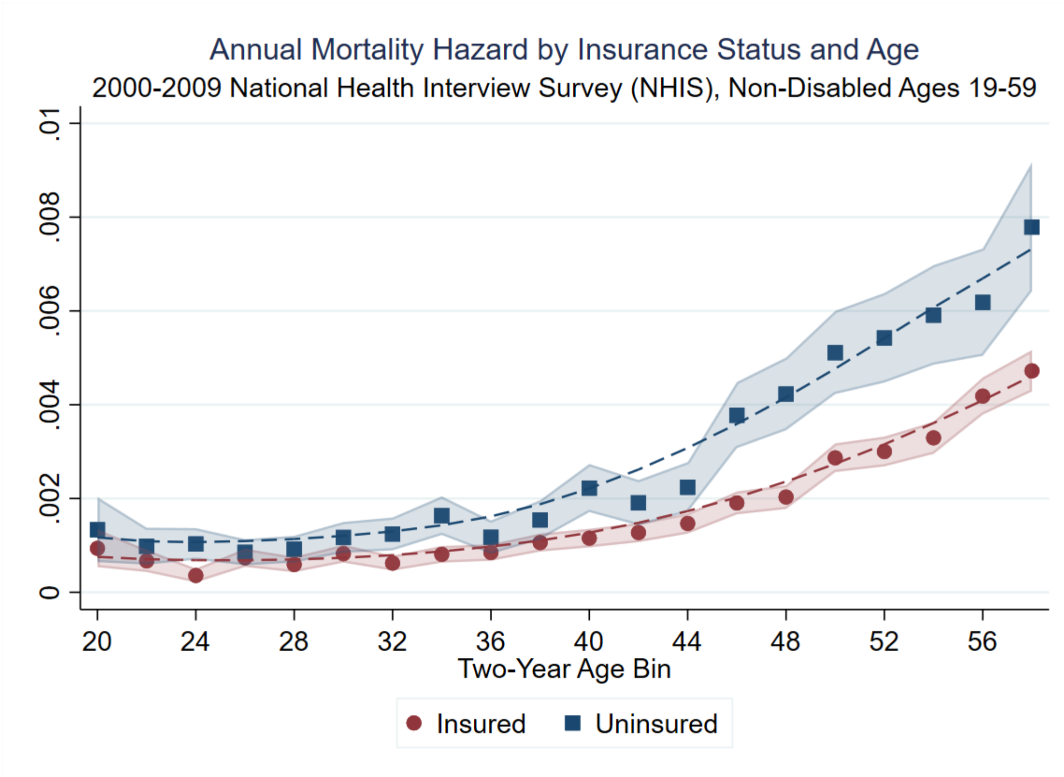
Table 12: Share of those with IRS information return in year in same state as in 2010, conditional on having 2010 IRS information return

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.

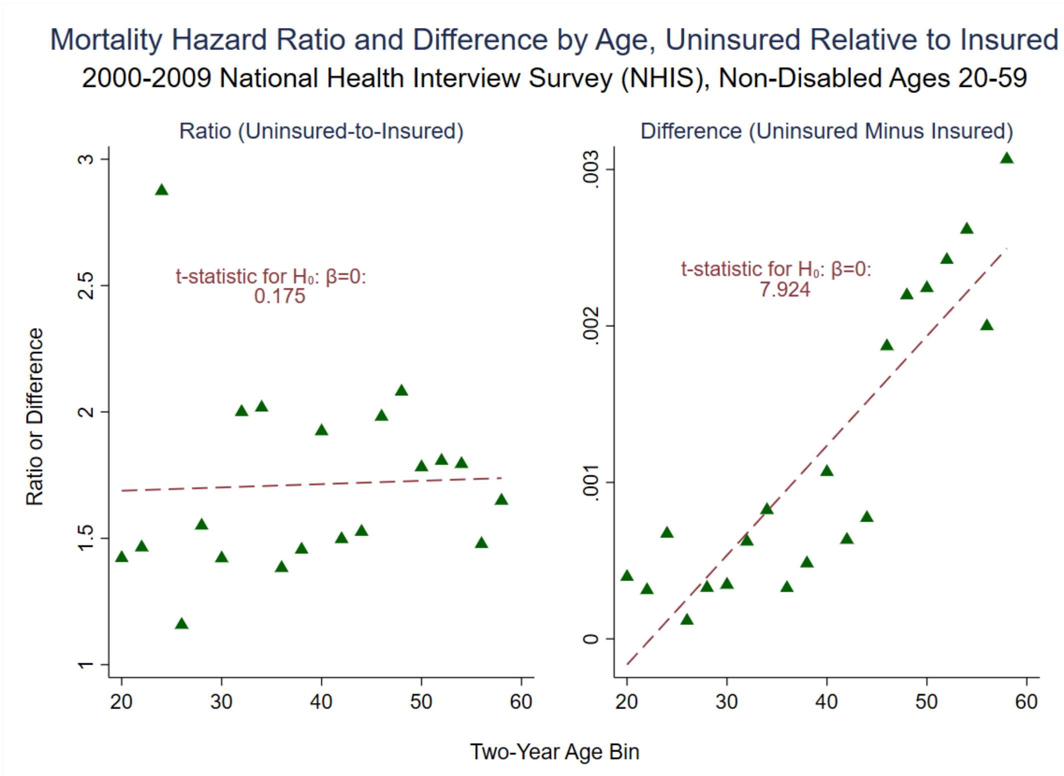
Figures for Saved by Medicaid: New Evidence on Health Insurance and Mortality from the Universe of Low-Income Adults

Figure 1



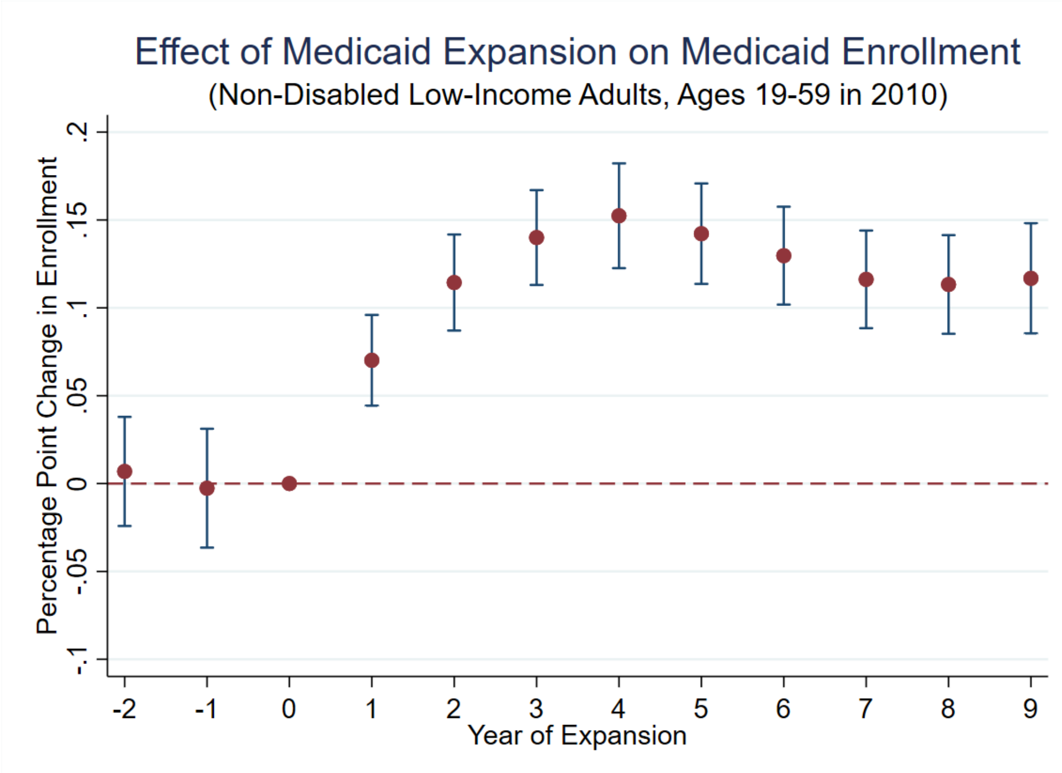
Sources: National Health Interview Survey (2000-2009) *(public-use)*. **Notes:** Figure displays the annual mortality hazard for insured and uninsured individuals in a two-year age bin according to public-use linked NHIS and National Death Index data. Mortality calculated through 2010.

Figure 2



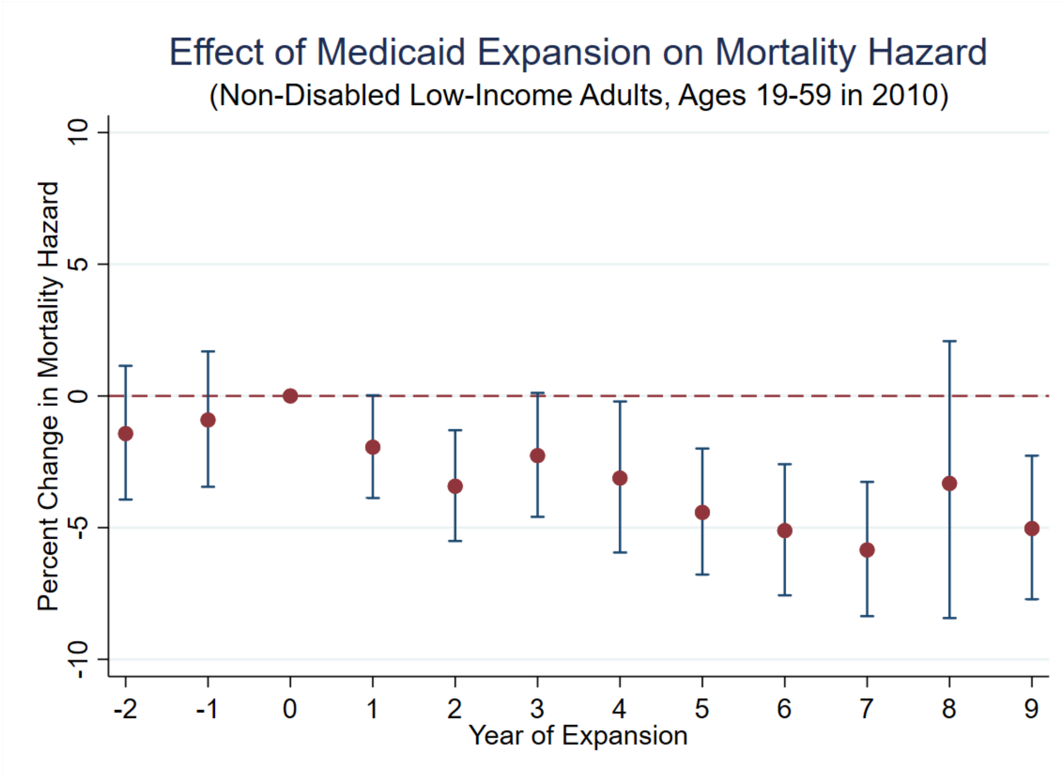
Sources: National Health Interview Survey (2000-2009) *(public-use)*. **Notes:** Figures display absolute difference in annual mortality hazard and ratio of annual mortality hazard for uninsured relative to insured individuals in two-year age bin according to public linked NHIS and National Death Index data. Mortality calculated through 2010.

Figure 3



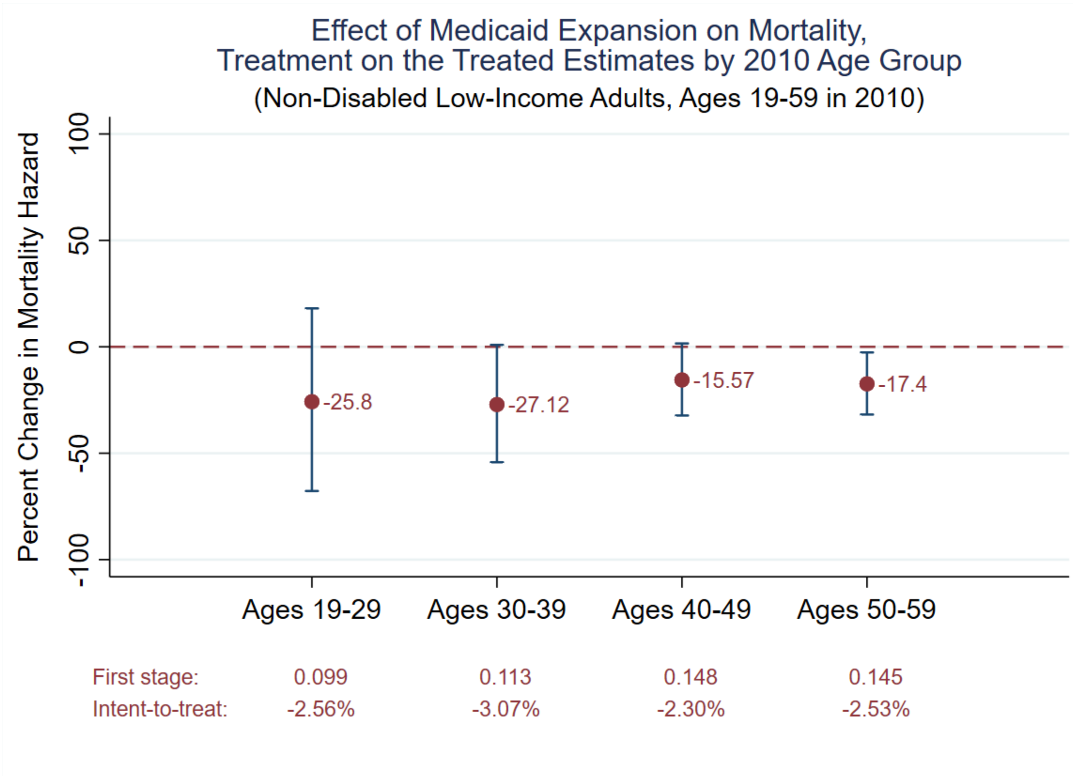
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident. **Notes:** Figure displays coefficients on event time dummies from event study model described in text, with Medicaid enrollment in the year as the outcome variable. Sample includes non-disabled 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 4



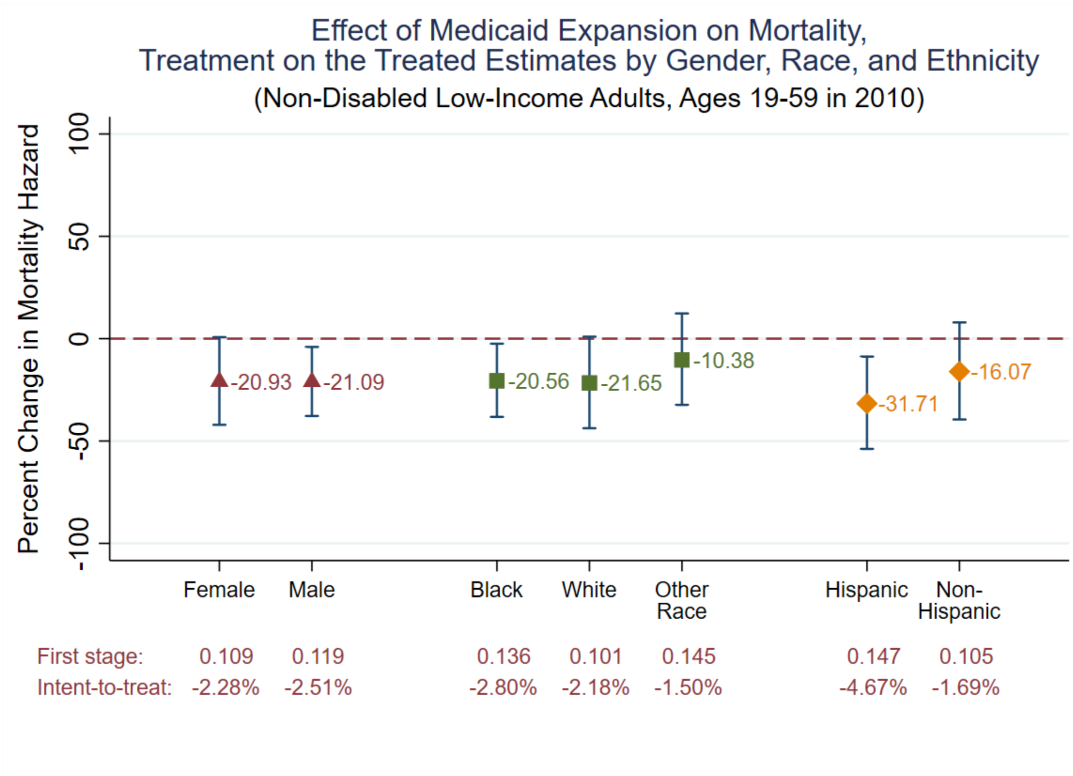
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2022 Numident. **Notes:** Figure displays proportional change in mortality implied by coefficients on event time dummies from event study mortality hazard model described in text. Sample includes non-disabled adults with 2009 income < 1.38x the 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 5



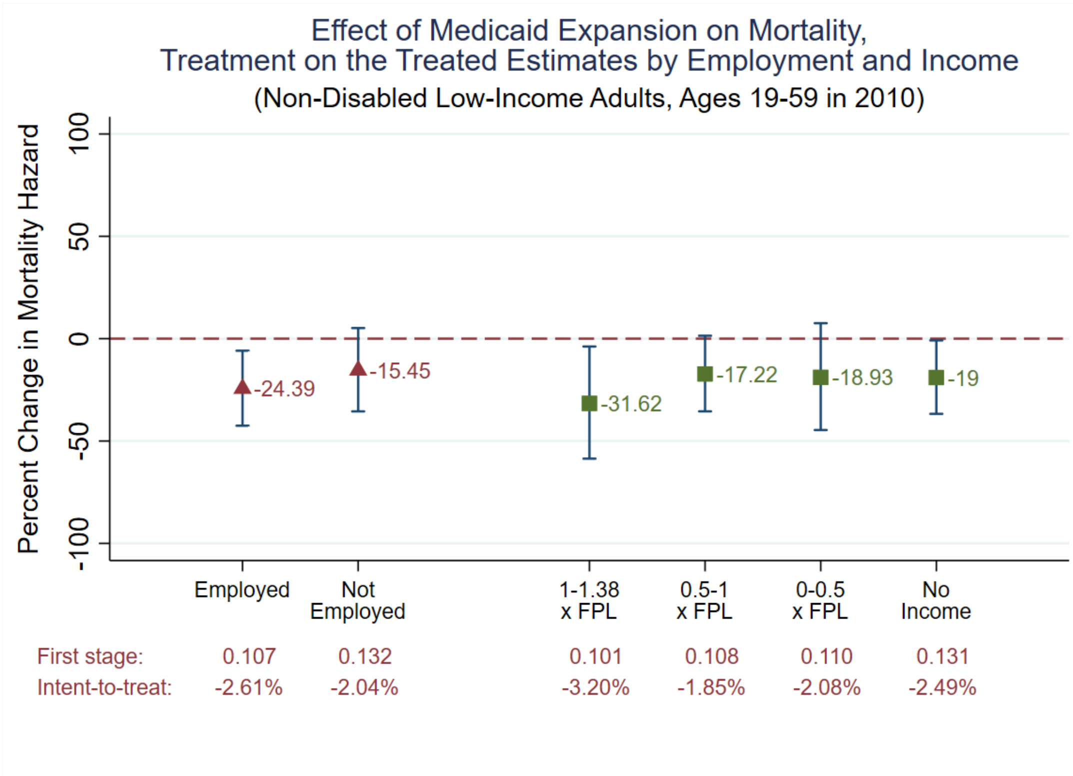
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident. **Notes:** Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the age 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 6



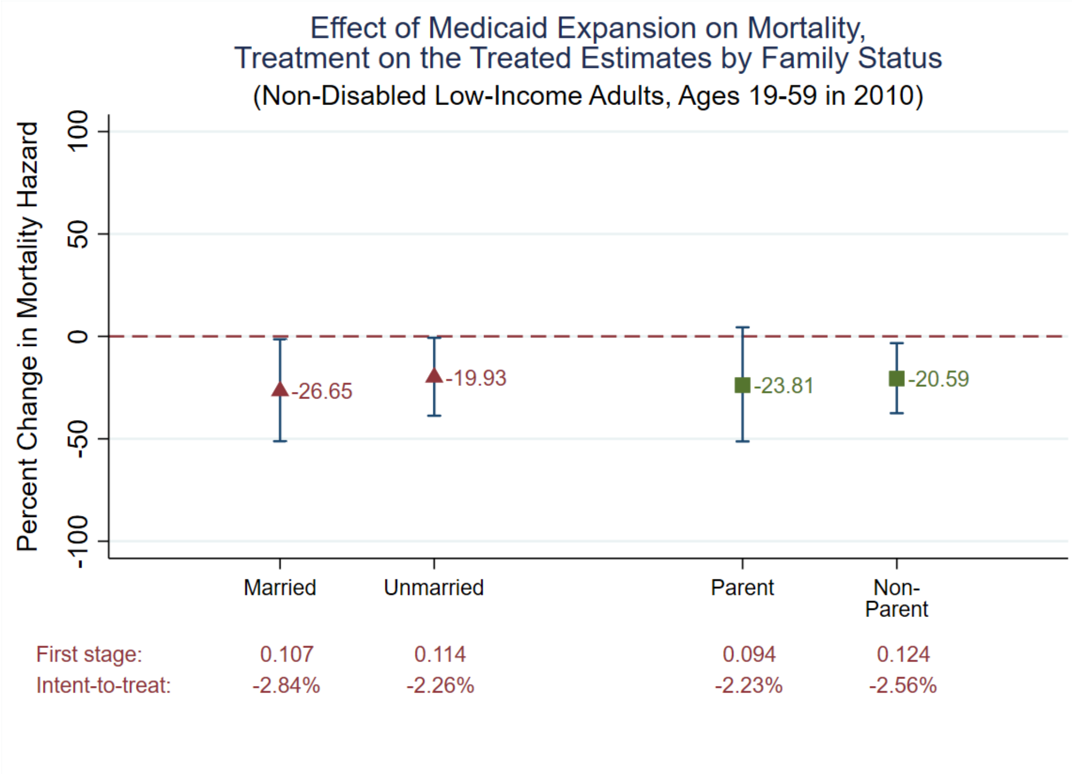
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident.**Notes:** Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the demographic 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 7



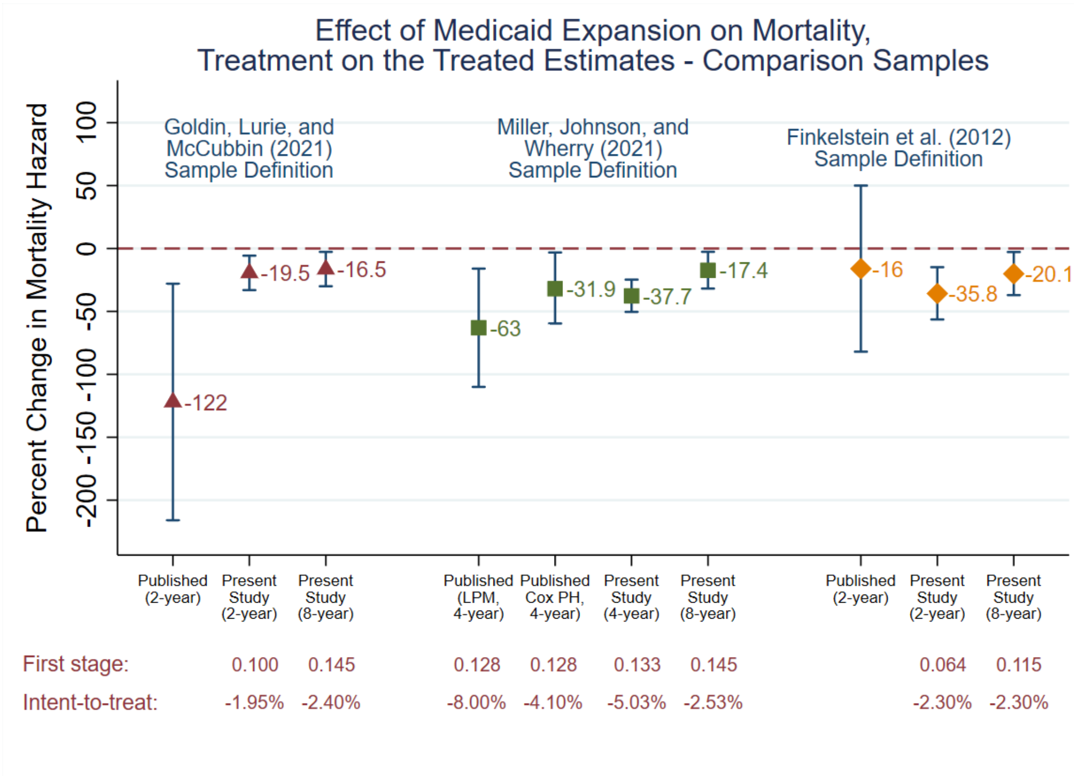
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident. **Notes:** Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the employment 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 8



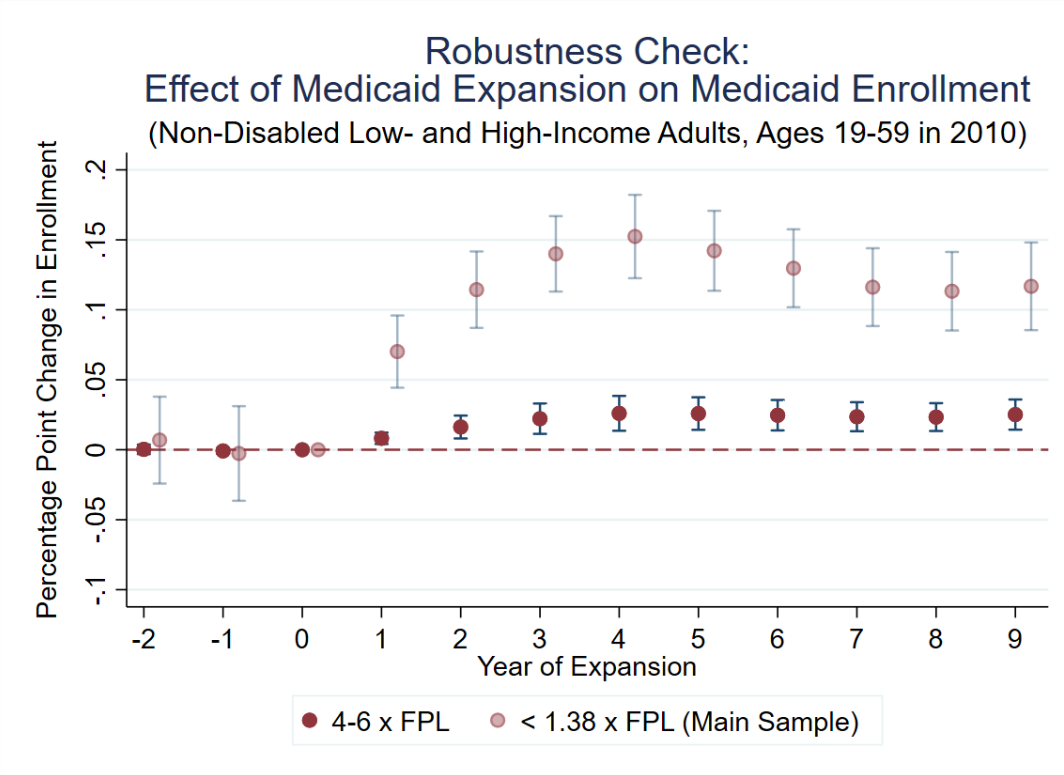
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident. **Notes:** Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients obtained by estimating the models described in the text on the family 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 9



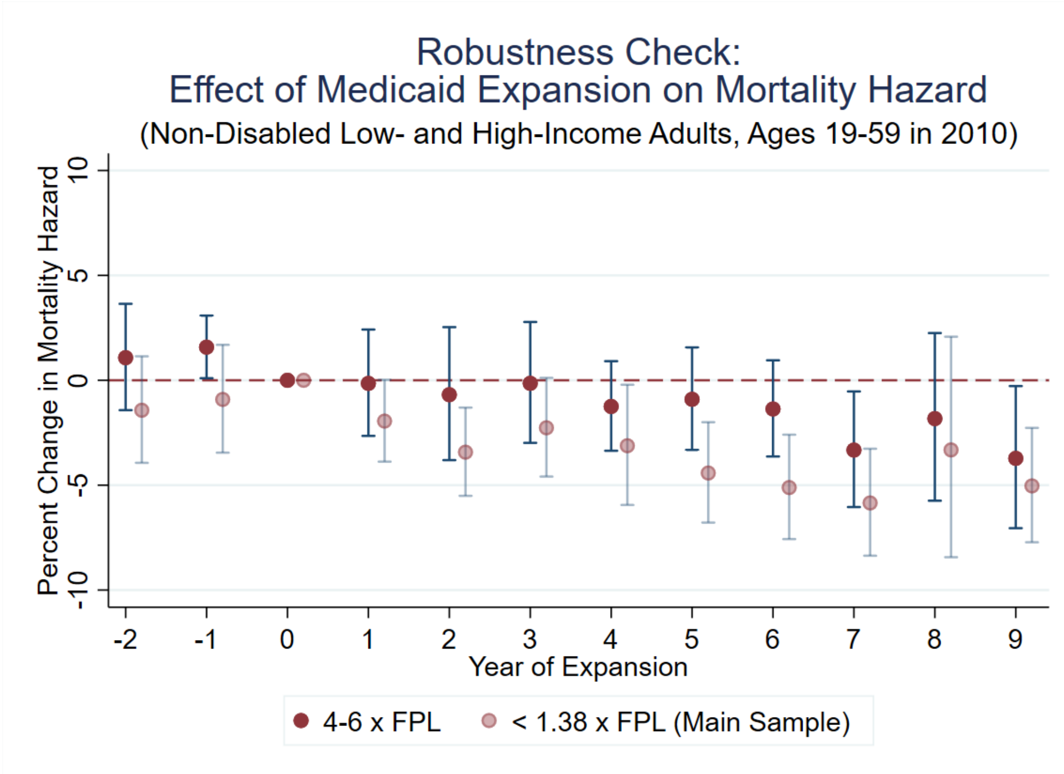
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2010-2019 Medicaid enrollment records; 2022 Numident. Published estimates' source is the paper indicated on the figure. **Notes:** Figure displays treatment-on-the-treated effects suggested by difference-in-differences and mortality coefficients 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 10



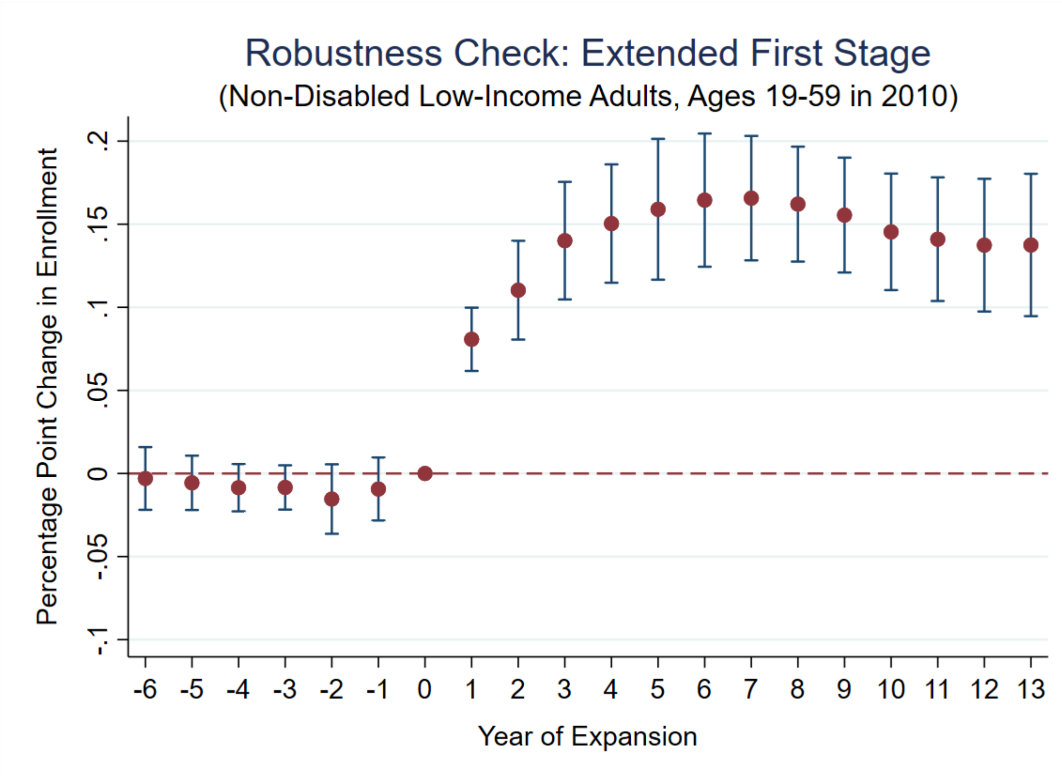
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2005-2019 Medicaid enrollment records; 2022 Numident. **Notes:** Figure displays coefficients on event time dummies from event study model described in text, with Medicaid enrollment in the year as the outcome variable. Samples includes non-disabled 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 11



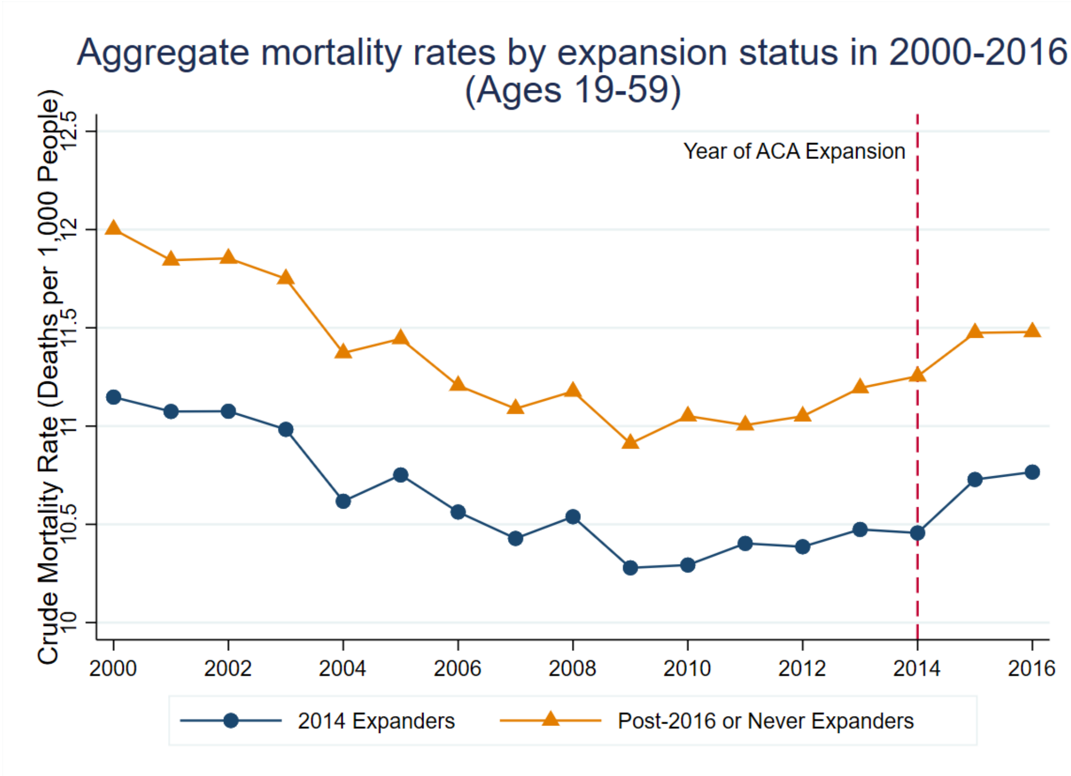
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2009 Medicare and Medicaid enrollment records, 2022 Numident. **Notes:** Figure displays proportional change in mortality implied by coefficients on event time dummies from event study mortality hazard model described in text. Sample includes non-disabled 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 12



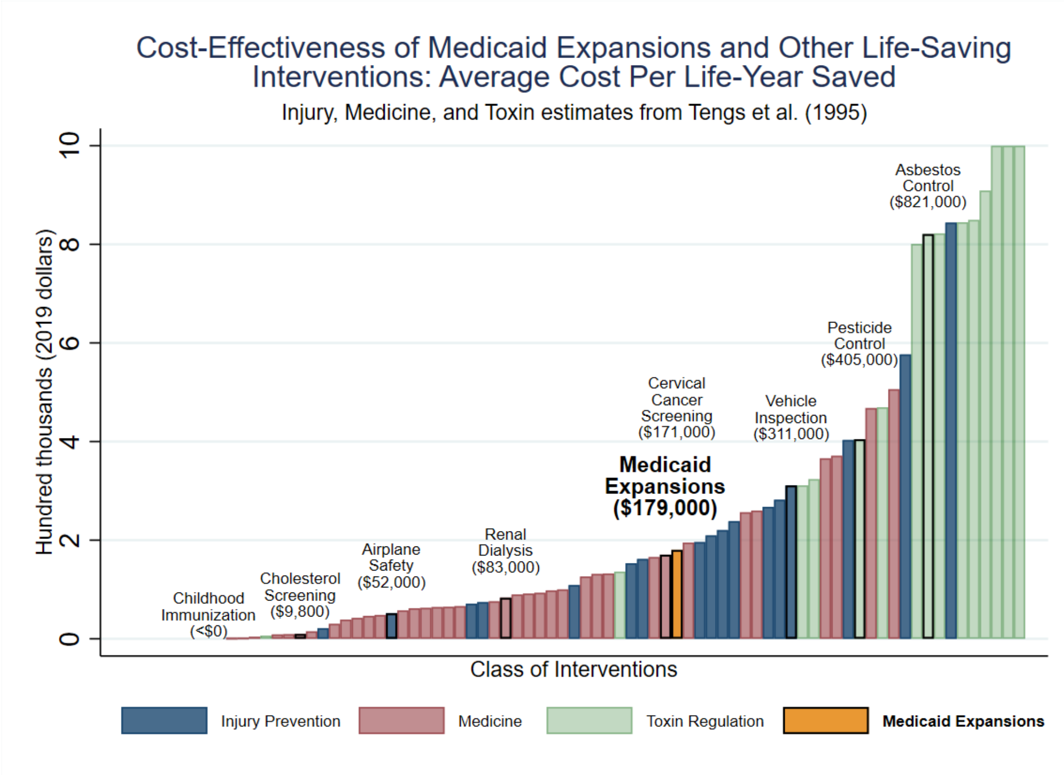
Sources: 2010 Census; 2009 IRS Forms 1040, W-2, 1099-R; 2009 Medicare and Medicaid enrollment records, 2022 Numident. **Notes:** Figure displays coefficients on event time dummies from event study model described in text, with Medicaid enrollment in the year as the outcome variable, including Medicaid 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 13



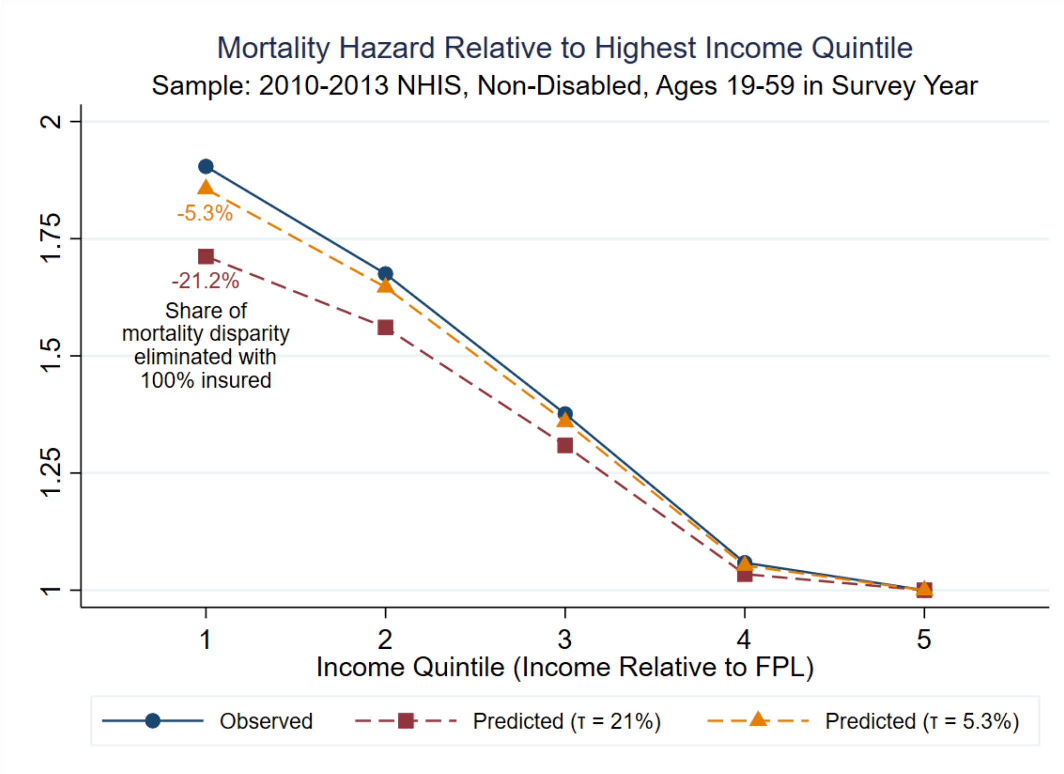
Source: Compressed Mortality File 2000-2016 on CDC WONDER Online Database, released June 2017. Centers for Disease" "Control and Prevention, National Center for Health Statistics *(public-use)*. **Notes:** Figure displays aggregate mortality rates in 200-2016 among those ages 19-59 in states that expanded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

Figure 14



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 15



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.

Appendix

A: Grouping Individuals in Census Households into Families

Summary of methodology

We define a family unit to consist of an adult, their spouse, and their children under 19 residing in the same Census household. Most people (about 98.0%) can be grouped into family units with a reasonable degree of certainty using the Census's indicator for relationship to householder. The remaining 2.0% are adults and children who have ambiguous relationships with one another based on the household relationship variables in the Census. In these cases, we assign each adult to his or her own family unit (i.e., we do not assume any spousal relationships between them). Of the 2.0% of remaining individuals with ambiguous family relationships, 1.8% are adults whom we assign to their own family unit. The remaining 0.2% are children belonging to indeterminate family units (i.e., there is more than one family in the household that the child could plausibly belong to), and in these cases we randomly assign children to a family unit in the household.

Detailed description of methodology

Immediate family of the householder. We group the householder's spouse and children under 19 (if present) into his or her family unit. We also include the householder's grandchildren under 19 if no adult child or son-/daughter-in-law of the householder is present, as well as the householder's siblings under 19 if the householder's parents are not present.

Adult siblings, children, or grandchildren of the householder and their families. Siblings, children, or grandchildren of the householder who are over 19 become their own family units. When the householder has only one adult child, we assign his or her son- or daughter-in-law and grandchildren (if present) to the adult child's family unit. When there is a son- or daughter-in-law but no adult child, they become their own family unit along with any grandchildren. Finally, we assign any "other non-relative children" to the family of the adult sibling or adult grandchild.

Unmarried partners and their children. Many people also reside in households where the householder has an unmarried partner. This unmarried partner forms their own family unit and we assign any children that are unrelated to the householder to this family unit.

Parents of the householder and their families. Some households include one or two parents or parents-in-law of the householder. These parents/parents-in-law become their own family unit(s). In some cases, the household also includes siblings of the householder who are under 19. We assign these siblings to the family unit of the householder's parent(s), if present.

Adults and children with ambiguous relationships to one another. The above-described scenarios allow us to group 98% of people into family units. The remaining 2% are in households where family units cannot be identified in a straightforward fashion using relationship to household head variables available in the Census. These remaining cases largely involve the presence of multiple roomers/boarders or housemates/roommates and other non-relatives of the householder, where these individuals could be married to one another or not. We assign each person over 19 in these categories becomes to own family unit, along with any children of the same category (e.g. roomer/boarder, housemate/roommate, non-relative). There are a very small number of people (0.2% of the Census population) who are children who could plausibly belong to multiple family units in the household. In these cases, we randomly assign them to one such family unit within the household.

B: Calculating Family Income in Tax Records

Defining Income and Poverty

Because we wish to identify adults newly eligible for Medicaid under expansions, we follow the definitions of income and poverty status used to determine Medicaid eligibility. To calculate income, Medicaid employs an income concept called Modified Adjusted Gross Income (MAGI), which is equal to adjusted gross income (AGI) plus any untaxed foreign income, non-taxable Social Security benefits, and tax-exempt interest.

Calculating Family Income and Poverty Status

We confront various issues in attaching income sources to family units and address these according to an approach adapted from Meyer et al. (2020).

Attaching Income Sources to Families. We link all adults assigned a linkage key in the 2010 Census to 2009 IRS 1040s, W-2s, and 1099-Rs. We attach at most one 1040 return to each family, keeping only 1040s where the adult filer, co-filer, or both links to the family unit (i.e. we do not keep returns where only a dependent on the return links to the family).

When one adult in a family links to a joint return, but another does not we either bring in all of the income (when the other adult is not PIKed in the Census) or half if the other adult is PIKed and not on the return. In some cases, the two adults in a family link to separate 1040s; in such cases, we do not bring in the 1040 return information for either, but rather proceed to bring in income information from W2s associated with each of these individuals.

For any adult in the family who does not link to a 1040, we bring in W-2s and 1099-Rs, keeping all forms that link (since each form represents a separate income stream).

Calculating Income to Poverty Ratio. When 1040s are available, we calculate MAGI as the sum of AGI, tax-exempt interest and estimated non-taxable SS benefits. When no 1040 is available, we sum taxable wages and retirement distributions from W-2s and 1099-Rs. For families with no W-2s or 1099-Rs, we set MAGI equal to zero. We then compare 2009 MAGI to the Federal Poverty Guidelines to identify adults in families with income less than 138% of the FPL who could have benefited expansions.

C: Estimating Lives and Life-Years Saved by Medicaid Expansions, Avoidable Deaths, and Cost-Effectiveness

This Appendix presents a framework for estimating the number and age cohort distribution of lives and life-years saved by Medicaid expansions and estimating the cost per life and life-year saved by expansions.

Estimating Lives and Life-Years Saved and Avoidable Deaths

Life-Table Framework. We let $\lambda_j(t)$ be the mortality hazard for age cohort $j \in (19, ..., 59)$ in period $t \geq 2010$, where cohorts are indexed by age in 2010. The cumulative survival rate $S_j(\lambda(t)) = \prod_{z=2010}^{t}(1 - \lambda_j(z))$ is calculated using the vector of mortality hazards up to time t. The share of individuals in age cohort j who survive to the end of our study is equal to $S_j(\lambda_j(2022)) = \prod_{z=2010}^{2022}(1 - \lambda_j(z))$. The average life expectancy for individuals in age cohort j, T_j, is equal to the sum of the cumulative survival rates through the end of the human life span, i.e. $T_j = \sum_{t=2010}^{\infty} S_j(t)$.

Estimating Lives and Life-Years Saved. We estimate the number of lives and life-years saved in each age cohort by Medicaid expansions. Allowing $D \in (0, 1)$ to index Medicaid expansion status, the number of lives saved is equal to the weighted sum of the change in survival probabilities:

$$\Delta_j^{lives} = \left(S_j^{D=1}(\lambda_j(2022)) - S_j^{D=0}(\lambda_j(2022))\right) \cdot N_j$$

and the number of life-years saved is the weighted sum of the change in life expectancies:

$$\Delta_j^{lifeyears} = (T_j^{D=1} - T_j^{D=0})N_j$$

where N_j indicates the count of people in each age cohort residing in expansion states in our sample.

We obtain estimates of the mortality hazard $\lambda_j^{D=0}(t)$ for $t \in (2010, \ldots, 2129)$ from SSA life tables, allowing individuals to age until the maximum of the human life span indicated in these tables (119 years old).[12] We use these mortality hazards to estimate $S_j^{D=0}(\lambda_j^{D=0}(2022))$ and $T_j^{D=0}$.

To obtain estimates of the mortality hazard under expansion, $\lambda_j^{D=1}(t)$, we deflate mortality hazards in post-expansion years from the SSA life tables by 2.5 percent, our estimated treatment effect. Because the population-weighted average number of post-expansion years in states that expanded is 8.5 and the total number of years in our study is 12, we employ the following stepwise function to

[12] Because the data in SSA life tables reflects the mortality hazard in the overall population, which we expect to be lower than the mortality hazard in the low-income population, we use the 2010-2013 National Health Interview Survey (NHIS) to obtain an estimate of the ratio of the annual mortality hazard among those ages 19-59 with incomes below 1.38 times the poverty level to the annual mortality hazard in the overall population of these ages.

estimate the mortality hazard:

$$\begin{aligned}
\lambda_j^{D=1}(t) &= \lambda_j^{D=0}(t) \text{ for } t \in (2010, 2011, 2012) \\
&= \lambda_j^{D=0}(t)(1 - \frac{\tau}{2}) \text{ for } t = 2013 \\
&= \lambda_j^{D=0}(t)(1 - \tau) \text{ for } t \in (2014, ..., 2022) \\
&= \lambda_j^{D=0}(t) \text{ for } t \in (2023, ..., 2129)
\end{aligned}$$

We then use these mortality hazards to estimate $\lambda_j^{D=1}(2022)$ and $T_j^{D=1}$, which in turn allows us to estimate Δ_j^{lives} and $\Delta_j^{lifeyears}$.

Estimating Avoidable Deaths. We adopt analogous methods to estimate the number of lives that could have been saved in non-expansion states if they had expanded in 2014.

Estimating Cost Per Life and Per Life-Year Saved.

According to Kaiser Family Foundation (KFF) analysis of Medicaid data, the average cost for adults newly eligible under the ACA expansions was $5,225 in 2019. This cost reflects enrollment at any point during the year. Using our first-stage estimates, we estimate that expansions resulted in an additional 28.7 million person-years of Medicaid enrollment, or about $149.9 billion total. Dividing this by the number of lives and life-years saved by expansions yields cost estimates of about $5.4 million and $179,000, respectively. Braithwaite et al. (2008) use the rise in health expenditures and mortality changes over time to estimate that societal willingness-to-pay for each additional life-year is $217,000 to $314,000 (updated to 2019 dollars).