

# MEDICAID AND MORTALITY: NEW EVIDENCE FROM LINKED SURVEY AND ADMINISTRATIVE DATA\*

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We use large-scale federal survey data linked to administrative death records to investigate the relationship between Medicaid enrollment and mortality. Our analysis compares changes in mortality for near-elderly adults in states with and without Affordable Care Act Medicaid expansions. We identify adults most likely to benefit using survey information on socioeconomic status, citizenship status, and public program participation. We find that prior to the ACA expansions, mortality rates across expansion and nonexpansion states trended similarly, but beginning in the first year of the policy, there were significant reductions in mortality in states that opted to expand relative to nonexpansion states. Individuals in expansion states experienced a 0.132 percentage point decline in annual mortality, a 9.4% reduction over the sample mean, as a result of the Medicaid expansions. The effect is driven by a reduction in disease-related deaths and grows over time.

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A variety of alternative specifications, methods of inference, placebo tests, and sample definitions confirm our main result. *JEL Codes*: I13, I18, H75.

## I. INTRODUCTION

Low-income individuals in the United States experience dramatically worse health than those with high incomes. For example, the annual mortality rate for older adults in families earning less than 138% of the Federal Poverty Level (FPL) is 1.7%, more than four times higher than the 0.4% rate experienced by higher-income individuals of the same age.<sup>1</sup> This low-income group also experiences higher risks of dying from diabetes (by 787%), cardiovascular disease (552%), and respiratory disease (813%) in a given year relative to those in higher-income families, despite the existence of effective medical treatment and management for these types of chronic conditions. These higher death rates translate to dramatic differences in life expectancy across income groups. Chetty et al. (2016) find that men at the bottom of the income distribution live on average nearly 15 years less, and women over 10 years less, than those at the top of the income distribution conditional on surviving to age 40. While data from nearly all countries show a positive correlation between income and health, this correlation is stronger in the United States than other high-income countries (Semyonov, Lewin-Epstein, and Maskileyson 2013).

One policy that could play an important role in reducing these disparities is the Medicaid eligibility expansion of the Affordable Care Act (ACA). The ACA brought about the largest expansion of health insurance coverage in the United States since the creation of Medicare and Medicaid in 1965. Over 20 million people have gained coverage since the ACA (Martinez, Zammitti, and Cohen 2018), the majority of whom are low-income adults receiving coverage through the Medicaid program (Frean, Gruber, and Sommers 2017). Other research has shown that this expansion in coverage improved access to effective medical care that beneficiaries otherwise would not receive, including mortality-reducing prescription drugs (Ghosh, Simon, and Sommers 2019), earlier detection and

1. Authors' calculations using death rates from 2008 to 2013 derived from the publicly available National Health Interview Survey Linked Mortality File (National Center for Health Statistics 2019) for adults ages 55 to 64 with family incomes below 138% FPL and those with family incomes 400% FPL or greater.

treatment of treatable cancers (Eguia et al. 2018; Sabik et al. 2018; Soni et al. 2018), and hospital and emergency department visits for conditions that require immediate care (Duggan, Gupta, and Jackson 2019), in addition to a variety of other types of potentially beneficial medical care (see Gruber and Sommers 2019). Despite the size and scope of this policy, its effect on the health of those who benefited remains uncertain.

A major challenge to understanding how the ACA affects health is the limited availability of large-scale data that link individual-level information on eligibility for expanded coverage to objective measures of health. Analyses of survey data can use information on individual characteristics to examine eligible adults, but such data are typically limited to self-reported health measures, which may not correlate with actual changes in health. Studies using these measures have offered inconsistent findings, with some indicating that the ACA expansions improved self-reported health, while others found no effect or even negative impacts of the ACA expansions (Miller and Wherry 2017; Sommers et al. 2017; Cawley, Soni, and Simon 2018; Courtemanche et al. 2018b). Meanwhile, analyses of mortality data rely on death records aggregated to the state or county level, without information on individual-level factors that determine eligibility such as income (Swaminathan et al. 2018; Chen 2019; Khatana et al. 2019; Borgschulte and Vogler 2020; Yan et al. 2020). Without information on these relevant individual-level characteristics, these studies may be underpowered to reliably detect mortality effects of the ACA (Allen and Sommers 2019) and may in fact vastly overstate magnitudes (Black et al. 2019).

In this article, we provide new evidence of the effect of Medicaid on health by using administrative Medicaid enrollment and mortality data linked to large-scale, individual survey records. We use this novel data set to examine the impact of the sizable Medicaid eligibility expansion that occurred in some states under the ACA. In 2014, the ACA expanded eligibility for the Medicaid program to include all adults in families with incomes under 138% of the FPL. Previously only pregnant women, adults with disabilities, and very low-income parents tended to qualify for Medicaid coverage. Although intended to apply to all states, a 2012 Supreme Court decision made the Medicaid eligibility expansion optional. As a result, only 29 states and the District of Columbia expanded coverage in 2014, with 7 additional states electing to expand over the next several years. Despite nonuniversal adoption, the ACA

Medicaid expansions still represent a historic expansion in insurance coverage. Approximately 13.6 million adults gained Medicaid coverage under the ACA ([Medicaid and CHIP Payment and Access Commission 2018](#)); for comparison, Medicare enrolled about 19 million elderly beneficiaries after its creation in 1965 ([Bureau of the Census 1969](#)). We take advantage of variation in state adoption of this large expansion in coverage to compare changes in mortality among individuals in expansion states and nonexpansion states.

In contrast to prior research that relies on death certificate data with limited information on individual characteristics, our data include detailed survey measures collected from the 2008 to 2013 years of the American Community Survey (ACS). This large-scale national survey contains approximately 4 million respondents in each year and allows us to observe information on specific characteristics that determine Medicaid eligibility, such as income, citizenship status, and the receipt of other social assistance. With this information, we are able to identify individuals targeted by the ACA Medicaid expansions and thus overcome the inherent limitations present in existing studies that rely only on aggregate death records. We focus on those in this group who were between the ages of 55 and 64 in 2014, who are at greater risk of mortality, although we also present results for all nonelderly adults and other age groups. We follow individuals in our sample over time to examine changes in coverage by Medicaid, by linking respondents to administrative records on Medicaid enrollment, and we document sizable changes in annual and cumulative Medicaid coverage in the Medicaid expansion states. Next we examine whether there is an associated change in mortality by linking the sample to the Census Numident file, which contains administrative records on the date of death for all individuals with Social Security numbers (SSNs) who die in the United States. The Census Numident file allows us to observe mortality rates for our sample through 2017, four years after the initial ACA Medicaid eligibility expansions. We further examine changes in mortality by the underlying cause of death using data from the Mortality Disparities in American Communities (MDAC) project, which links the 2008 wave of the ACS to death certificate records using the National Death Index.

Our analysis shows that the ACA Medicaid expansions reduced mortality among this targeted group. Prior to the expansions, individuals in our sample residing in expansion and nonexpansion states had very similar trends in both Medicaid coverage

and mortality. At the time of the expansion, the trajectories of these two groups diverged significantly, with expansion state residents seeing increases in Medicaid coverage and decreases in annual mortality rates. In the first year after the coverage expansion, the probability of mortality declined by about 0.089 percentage points, or a 6.4% reduction over the sample mean. The estimated impact of the expansions increases over time, suggesting that exposure to Medicaid results in increasing health improvements. By the fourth year, the expansion reduced annual mortality rates by 0.208 percentage points among expansion state residents. In our supplemental analysis using the MDAC data, we find evidence that health care–amenable and internal (disease-related) causes of death fell as a result of the expansions, but no evidence of a decline in deaths due to external causes, such as car accidents. We show that these estimates are robust to a large number of alternative specifications. We also conduct several placebo tests to assess the validity of our analysis, including examining the effect of the expansions on those age 65 or older in 2014 who did not gain Medicaid eligibility; examining the effect on individuals in higher-income families who were less likely to be affected; and conducting the analysis in the pre-ACA period before we would expect to see relative changes in mortality across state groups. We find no relative change in coverage or mortality across expansion and nonexpansion states among the elderly or in the pre-ACA period, settings in which no Medicaid expansion occurred. Among those in higher-income families, we find small but statistically significant increases in Medicaid coverage as well as small decreases in mortality, consistent with a causal effect of Medicaid on mortality.

Our analysis provides new evidence that expanded Medicaid coverage reduces mortality rates among low-income adults. If we assume that similarly sized mortality reductions would have occurred in the nonexpansion states, our estimates suggest that approximately 15,600 deaths could have been averted if the ACA expansions were adopted nationwide as originally intended by the ACA. This highlights an ongoing cost to nonadoption that is relevant to both state policy makers and their constituents.

## II. BACKGROUND

Many studies have shown that Medicaid coverage, and the ACA expansion in particular, increases access to and use of health

care and reduces financial burden for low-income adults, but evidence on its health effects proves more difficult to document and is less conclusive (Soni et al. 2018; Gruber and Sommers 2019). Studies that do examine health often rely on self-reported health measures from survey data. The evidence from these studies spans from estimated large or modest improvements in reported health associated with Medicaid expansion (Sommers et al. 2016, 2017; Simon, Soni, and Cawley 2017; Cawley, Soni, and Simon 2018; Lee and Porell 2018), to no effects (Sommers et al. 2015; Wherry and Miller 2016; Courtemanche et al. 2018a, 2018b; Miller and Wherry 2019), or even small but marginally significant negative effects (Miller and Wherry 2017). One concern with self-reported health data is that they may not accurately measure changes in physical health. For example, changes in self-reported health may reflect increasing awareness of health problems or interactions with the health care system, rather than actual changes in physical health. Increased contact with health providers could lead to, say, new information about a previously undiagnosed illness and, as a consequence, a worsened self-perception of health. This could bias downward estimates of the effect of public health insurance on health.<sup>2</sup> Furthermore, the reliability of self-reported health measures for U.S. adults and their association with objective health measures are documented to be worse among lower socioeconomic status groups (Dowd and Zajacova 2007, 2010; Zajacova and Dowd 2011).

The Oregon Health Insurance Experiment (OHIE) provided the first experimental evidence on the effects of Medicaid coverage on beneficiaries. The researchers did not observe significant effects of Medicaid coverage on any of the collected health measures (blood pressure, cholesterol, and blood sugar levels; Baicker et al. 2013). Using administrative data, they also found no evidence that Medicaid coverage led to a reduction in mortality during the 16 months after coverage gain. Their estimate suggested a 16% reduction in mortality associated with acquiring Medicaid, but with a large confidence interval that could not rule out sizable changes in either direction (Finkelstein et al. 2012).

As the data become available, researchers are beginning to evaluate the mortality effects of the ACA Medicaid

2. This bias could also operate in the opposite direction if increased interaction with providers improves one's perception of health. See Currie and Gruber (1995) for more discussion.

expansions.<sup>3</sup> A few recent studies use population-level mortality data to estimate changes in adult mortality in expansion states compared with nonexpansion states. These studies reach very different conclusions, either unable to detect mortality effects (Black et al. 2019) or estimating varying-sized reductions in adult mortality: 3.6% among adults ages 20–64 (Borgschulte and Vogler 2020) and 1.2% among adults ages 55–64 (Chen 2019). Yan et al. (2020) find no reduction in all-cause mortality among adults ages 20–64, but a 2.7% decrease in health care–amenable mortality. Khatana et al. (2019) focus on changes in cardiovascular disease-related mortality among adults ages 45–64 and document a 2.9% reduction over the baseline mortality rate. Finally, Swaminathan et al. (2018) find an 8.5% decrease in one-year mortality for patients with end-stage renal disease initiating dialysis associated with the ACA Medicaid expansions.

There is also recent evidence that health insurance coverage (inclusive of Medicaid) can affect mortality. Goldin, Lurie, and McCubbin (2021) provide new experimental evidence documenting such mortality effects by studying an IRS program that sent informational letters about health insurance enrollment to a randomly selected sample of taxpayers who were subject to the ACA mandate penalty for being uninsured. The letters both significantly increased enrollment in insurance (both private exchange coverage and Medicaid) and reduced mortality among those between the ages of 45 and 64 and uninsured at the time of the intervention. The results from this experiment suggest that one additional month of insurance coverage lowered the two-year mortality rate of recipients by about 0.18 percentage points, or over 10%. The authors found no effects of the letter on mortality for those under age 45.

One of the primary impediments to fully understanding the effect of Medicaid on mortality has been data availability. Previous studies using aggregated data rely on changes in survival for the Medicaid eligible to translate into overall mortality effects

3. A separate but related literature has examined the relationship between public health insurance and child mortality using variation in exposure tied to the introduction of Medicaid and later expansions in public coverage under Medicaid and the Children's Health Insurance Program. For the most part, these studies have found significant declines in mortality associated with expanded coverage for infants and children in the short term (e.g. Currie and Gruber 1996a, 1996b; Howell et al. 2010; Goodman-Bacon 2018) and the long term (Goodman-Bacon 2016; Wherry and Meyer 2016; Brown, Kowalski, and Lurie 2018).



observable at the population level. Data from death certificate records contain very little socioeconomic information on the decedent; in particular, they contain no information on the decedent's income, use of public programs, whether he or she previously had health insurance coverage, or other characteristics that might affect Medicaid eligibility. Without data linking information on individual Medicaid eligibility to mortality, researchers must rely on differences in exposure over larger population groups—for example, residents of certain states or counties—which contain many individuals who are not directly affected by Medicaid policy.<sup>4</sup> This decreases the power to detect changes in mortality of a plausible magnitude, leading some researchers to conclude that it will be challenging to reliably detect effects of insurance coverage on mortality unless these data can be linked at the individual level to large-sample panel data (Black et al. 2019).

In this article, we aim to contribute to this knowledge base by providing new evidence on the mortality effects of Medicaid. We build on the accumulation of evidence from prior studies but offer three important advantages. First, we use individual characteristics to study mortality among the Medicaid-eligible population, rather than using analyses that rely on population-level data, which may be underpowered to detect effects. This may be particularly salient in the case of the ACA Medicaid expansions, when insurance coverage is estimated to have increased by as little as 1 percentage point among all nonelderly adults (Black et al. 2019). Second, using administrative Medicaid enrollment information, we confirm a large change in coverage for our study population. The longitudinal enrollment data improve on the more commonly used survey measures of Medicaid coverage, which can be subject to substantial misreporting (Boudreaux et al. 2015), and allow us to examine the total amount of coverage accumulated by residents of expansion states over the entire sample period. Documenting changes in accumulated coverage is important because studies suggest that health insurance coverage can have

4. Variation in exposure to the policy based on state of residence, as well as local geographic characteristics (such as baseline uninsurance or poverty rates at the county level) are sources that have been used. In addition, some researchers have used information on the decedent's educational attainment as a proxy for individual income, although this information is reported by the next of kin and is subject to substantial measurement error (see Sorlie and Johnson 1996; Rostron, Boies, and Arias 2010).



longer-term effects that extend beyond just the coverage period (Currie, Decker, and Lin 2008; Boudreaux, Golberstein, and McAlpine 2016; Goodman-Bacon 2016; Wherry and Meyer 2016; Thompson 2017; Wherry et al. 2017; Brown, Kowalski, and Lurie 2018). Third, we estimate effects for this Medicaid-eligible population using much larger sample sizes than were used in the only experimental evaluation of Medicaid coverage, the OHIE. This larger sample is particularly important when examining mortality as an outcome because it is an extreme and infrequent health event. These advantages overcome the existing limitations in both experimental and nonexperimental studies of Medicaid, allowing us to further advance the evidence on the program's mortality effects.

### III. DATA AND OUTCOMES

To conduct our analysis, we link individuals across three different data sources. First, we use restricted data from the 2008 to 2013 waves of the ACS, which has a sample size approximately 50% larger than the public-use version, to identify our population of interest. We select respondents who, based on their pre-ACA characteristics, were likely to benefit from the ACA Medicaid expansions. We include only individuals who either are in families with income at or under 138% of the FPL or who have less than a high school degree.<sup>5</sup> Because we only have information on income captured at one point in time, the latter criterion is used to identify individuals who are of low socioeconomic status but might not meet the income cutoff at the time of the ACS interview. Results are similar if we include only those with less than a high school degree or only those with incomes under 138% of the FPL, rather than defining the sample using the union of these two criteria; see the later discussion in [Section VII](#). We exclude noncitizens, many of whom are not eligible for Medicaid, and those receiving Supplemental Security Income (SSI), who are automatically eligible for Medicaid in most states. We restrict our primary analysis to individuals who were ages 55 to 64 in 2014. This higher age group has relatively high mortality rates and is consistent

5. We define family income using the census family definition of all related individuals living in the same household. Results are very similar if we follow the more restrictive definition of a family unit proposed by the [State Health Access Data Assistance Center \(2012\)](#) for studies of health insurance coverage.

with the sample criteria used in [Black et al. \(2019\)](#). We present results for all nonelderly adults, and for a variety of different age subgroups, in additional analyses in [Section VII](#). Finally, we exclude residents of four states and DC that expanded Medicaid to low-income adults prior to 2014.<sup>6</sup> There are approximately 566,000 respondents who meet our sample criteria.<sup>7</sup>

Descriptive statistics for the sample by state Medicaid expansion status are reported in [Online Appendix Table A1](#) (all appendix information is in the [Online Appendix](#)). The average age of the respondents in the two groups is similar. However, individuals in expansion states are slightly better off with higher average income (147% of the FPL versus 140%) and educational attainment (45.3% with less than high school education versus 46.8%), as well as lower baseline rates of uninsurance (32.6% versus 37.3%), than individuals in nonexpansion states. In addition, individuals in expansion states are more likely to be white or Hispanic, whereas a higher share of those in nonexpansion states are Black.

Second, we link these data to the Census Numident file. The Census Numident file is derived from the SSA Numerical Identification file, which includes information on date and county of birth and date of death (if it has occurred) for individuals with an SSN.<sup>8</sup> These data have been used in, for example, [Brown, Kowalski, and Lurie \(2018\)](#); [Chetty et al. \(2011, 2016\)](#); [Dobbie and Song \(2015\)](#); [Sullivan and von Wachter \(2009\)](#), and other research relying on death information from tax records. Total deaths reported in the SSA file by age and year closely track the numbers reported by the National Center for Health Statistics ([Chetty et al. 2016](#)). The Census Bureau updates its Numident file each year with new information from the SSA Numerical Identification file. It formats the data so that there is a single record per individual,

6. DE, MA, NY, and VT all expanded coverage to individuals with incomes reaching the poverty line or greater prior to the ACA; DC received approval to implement its ACA Medicaid expansion early with enrollment starting in 2011.

7. Note that census disclosure rules prohibit the disclosure of exact sample sizes and require rounding. All sample sizes for these data are therefore rounded according to disclosure rules.

8. In addition to this death information from the SSA, the Census Bureau has information on date of death from the National Death Index (NDI) for some individuals and years, which it incorporates into its date of death measure when available. The NDI collects detailed information on deaths from state vital statistics offices. Respondents to the 2008 ACS were linked to the NDI for 2008–2015, as part of the Mortality Disparities in American Communities project.

reflecting the most accurate and up-to-date information at that point in time. We use data from the Census Numident for deaths occurring in 2017 and earlier.

The Census Numident and ACS data are linked via the Census Bureau's Personal Identification Validation System (PVS). This system assigns individuals in each data set a protected identification key (PIK), an anonymized identifier that allows the census to track individuals across data sets. Approximately 90% of all ACS respondents are successfully assigned a PIK using available information on name, address, and date of birth, with a slightly higher match rates for citizens (92%) (Bond et al. 2014). The assignment of a PIK allows respondents in the ACS to be matched to the Census Numident file. PIKS for the Census Numident file are assigned using SSNs and date of birth (Mulrow et al. 2011). Because our analysis is restricted to older citizens, and because nearly all U.S. citizens have SSNs assigned by the time they reach adulthood (see Bernstein et al. 2018), we expect to have nearly full coverage of deaths in the Numident file.

Once these data are linked, we observe the vital status of each individual during the year they respond to the ACS and each subsequent year. For example, we observe the vital status of an individual who responds to the 2008 ACS during each year from 2008 through 2017; for an individual who responds to the 2013 ACS, we observe his or her vital status from 2013 through 2017. We construct our outcome measure to represent mortality during each calendar year. If the individual is alive in a given year, the outcome variable takes a value of 0; if that individual died in that year it takes a value of 1. Once an individual has died, he or she is removed from the sample for subsequent years. In this way, we measure changes in the annual probability of death during a given year among individuals who were alive at the beginning of that year. The annual mortality rate is about 1.4% for our sample on average across all years, and approximately 1.3% among respondents in expansion states during the year just prior to expansion.<sup>9</sup>

Third, we link our sample of ACS respondents to administrative records on Medicaid enrollment from the Centers for Medicare & Medicaid Services (CMS). We use the same PVS methods as were applied to the mortality data to link these newly available

9. These annual averages are calculated excluding mortality rates for individuals during their year of ACS interview.

records to our sample of ACS respondents. About 93% of Medicaid enrollment records successfully receive a PIK assignment using SSN and date of birth information (Fernandez et al. 2015). CMS data on enrollment allow us to observe Medicaid enrollment longitudinally for our sample and document how the probability of enrollment, and total cumulative exposure to Medicaid, changed in the expansion states relative to the nonexpansion states. We use data from 2008 through 2016, the most recent data available. In our analyses of cumulative enrollment, we impute the number of days enrolled in 2017 using enrollment information from 2016, state of residence, age, gender, and race. To construct this imputation, we use information on transitions into and out of Medicaid observed in the previous year. See Online Appendix 1 for more information.<sup>10</sup> When examining the probability of being enrolled any time during a calendar year, we simply omit 2017 and use data on enrollment through 2016.

Although our data uniquely offer the opportunity to link mortality, Medicaid enrollment, and economic variables at the individual level, there are also several important limitations. First, we observe the economic characteristics of individuals (income and educational attainment, receipt of social services, and citizenship status) at the time they respond to the ACS in the preperiod, between 2008 and 2013. These are time-varying characteristics, however, and may not accurately reflect economic characteristics at the time of the Medicaid expansions for some members of our sample. For example, an individual in a low-income family in 2008 may be in a higher-income family by 2014, at the time the expansions occurred. Similarly, individuals may migrate to different states between the time they responded to the ACS and the time the expansions occurred, resulting in our misclassification of whether that individual was exposed to the eligibility expansion.<sup>11</sup> In general, we expect that this type of misclassification will bias our estimates toward zero. In addition, the CMS administrative enrollment information allows us to accurately examine and characterize the amount of exposure to the policy for our analytic sample during the post period for the years the data are available.

10. There are a number of other minor data issues surrounding the CMS enrollment data, which are outlined in Online Appendix 1.

11. Note, however, that individual migration decisions do not appear to be correlated with state Medicaid expansion (Goodman 2017).

A second limitation is that our data do not include information on the cause of death. The death information in the Census Numident is derived primarily from the SSA death records, which contain only date of death. We supplement our main analysis with data from the 2008 year of the ACS, which was linked to death certificate records from 2008 to 2015 as part of the Mortality Disparities in American Communities (MDAC) project. Although this drastically reduces both the sample size and follow-up period, it does allow us to conduct exploratory analyses of changes in mortality based on the underlying cause of death as reported on the death certificate.

#### IV. EMPIRICAL STRATEGY

Our empirical strategy looks at changes in annual mortality in the expansion states relative to the nonexpansion states before and after the implementation of the ACA Medicaid expansions. We estimate this using an event study model that allows us to assess the evolution of relative outcomes while controlling for fixed differences across states and national trends over time. We estimate:

$$(1) \quad Y_{isjt} = \text{Expansion}_s \times \sum_{\substack{y=-6 \\ y \neq -1}}^3 \beta_y \mathbf{I}(t - t_s^* = y) + \beta_t + \beta_s + \beta_j \\ + \gamma \mathbf{I}(j = t) + \epsilon_{isjt}.$$

As described earlier, our data are constructed at the individual ( $i$ ) by year ( $t$ ) level. Each individual responds to the ACS during a survey wave ( $j$ ) and reports their state of residence ( $s$ ). In our first-stage analysis,  $Y_{isjt}$  measures eligibility or coverage in each year. For our analysis of mortality, the dependent variable is  $Died_{isjt}$ , which denotes death during each year  $t$  among individuals who were alive at the beginning of year  $t$ . We only observe mortality over a partial year during the year of the individual's ACS interview ( $j$ ), because that person had to be alive to respond to the survey. To account for this, we include an indicator variable that year  $t$  is the year that the individual responded to the ACS (i.e., that  $j = t$ ).<sup>12</sup> In this equation,  $\beta_s$  denotes state fixed effects

12. We do not have information on the date of the ACS interview. If we drop the observations for which we observe less than a full year of mortality, our results are unchanged.

and  $\beta_j$  denotes fixed effects associated with each survey wave.  $\beta_t$  denotes calendar year fixed effects, which account for general trends in mortality for all individuals in our sample, including their gradual aging over time.

The variable  $Expansion_s$  equals 1 if individual  $i$  was living in a state that opted to expand Medicaid eligibility between 2014 and 2017, and 0 otherwise. Indicator variables  $\mathbf{I}(t - t_s^* = y)$  measure the time relative to the implementation year,  $t_s^*$ , of the expansion in each state, and are 0 in all periods for nonexpansion states.<sup>13</sup> While most states expanded in the beginning of 2014, some states expanded later in the year or in subsequent years. If a state expanded on or after July 1 of a given year, we code it as having expanded in the subsequent year.<sup>14</sup> The omitted category is  $y = -1$ , the year prior to the expansion. Therefore, each estimate of  $\beta_y$  provides the change in outcomes in expansion states relative to nonexpansion states during year  $y$ , as measured from the year immediately prior to expansion. If mortality rates for expansion and nonexpansion states were trending similarly prior to the ACA, we expect that estimated coefficients associated with event times  $y = -6$  to  $y = -2$  will be small and not statistically significant. We estimate equation (1) with a linear probability model and report heteroskedasticity-robust standard errors clustered at the state level.<sup>15</sup> All analyses use ACS survey weights.

In addition to the event study analyses, we present difference-in-differences (DD) estimates as a summary of the effect across all postexpansion years. These are estimated using the same

13. We group together  $y \leq -6$  into a single indicator variable interacted with expansion status because we only observe  $y < -6$  for late expander states.

14. In our analyses, states that expanded Medicaid in 2014 are AR, AZ, CA, CO, CT, HI, IL, IA, KY, MD, MI, MN, NJ, NM, NV, ND, OH, OR, RI, WA, and WV. Michigan implemented their expansion in April 2014 with the remainder of states expanding in January 2014. States that we considered to have 2015 expansions are NH (implemented August 15, 2014), PA (January 1, 2015), and IN (February 1, 2015). We consider AK (September 1, 2015) and MT (January 1, 2016) to be 2016 expansion states and LA (July 1, 2016) to be a 2017 expansion state.

15. In Online Appendix Table A2, we examine the sensitivity of our results to using both a standard logistic regression and a Cox proportional-hazards model. The difference-in-differences estimates from these nonlinear models are modestly smaller in magnitude than the linear probability model, but continue to show large and statistically significant reductions in mortality as a result of the ACA Medicaid expansions. In addition, our inference is similar if we cluster the errors at the census division level or allow for spatial correlation of the errors using the methods proposed by Conley (1999). See the discussion in Online Appendix 4 and results in Online Appendix Table A8.

equation except that the event study indicators are replaced with a single variable denoting an expansion state during the post period ( $Expansion_s \times Post_t$ ). This indicator turns on starting in the year of expansion for each state.

Because we have a fixed sample that ages in each period, mortality rates increase over time (i.e., our sample is oldest in the last year, 2017). In this way, our analysis tracks the mortality trajectory for a fixed cohort defined as adults ages 55 to 64 in 2014 and is representative of the outcomes over time for this group. Because mortality rates are higher during the post period for our sample due to aging, regardless of exposure to Medicaid, comparing our estimates of the effects of the ACA Medicaid expansions to the mortality rate in the pre-ACA period as a counterfactual is incorrect. Instead, we estimate a “counterfactual” rate for those living in expansion states that depends on average mortality rates observed during the post period. We calculate this rate as the average annual mortality rate in the expansion states in the postexpansion period minus the reduced-form mortality difference-in-differences estimate. This rate tells us what the mortality rate would have been in the treated states in our sample if their mortality rates had followed the same trajectory we observe in the control states. By constructing the counterfactual rate in this manner, we are able to take into account both the aging of the sample and the mortality reduction of expanded Medicaid coverage during the post period, see discussion in [Goodman-Bacon \(2016\)](#).

## V. RESULTS

### V.A. *Medicaid Eligibility, Medicaid Enrollment, and Insurance Coverage*

We first estimate the effect of the ACA Medicaid expansions on Medicaid eligibility and coverage for individuals in our sample. Using a variety of data sources and approaches, we show that respondents residing in the expansion states experienced significant increases in Medicaid eligibility, Medicaid enrollment, and health insurance coverage relative to those in the nonexpansion states. The effects we document among our disadvantaged sample are substantially larger than those estimated in the population overall (e.g., [Black et al. 2019](#)), indicating that the individual-level characteristics we used to define our sample are successful



in identifying those most likely to be affected by the eligibility expansions.

First, we examine how eligibility for Medicaid changed in expansion relative to nonexpansion states. Individuals eligible for Medicaid are “conditionally covered” by the program, in the sense that they may choose to remain uninsured and enroll only when they become ill. This concept of conditional coverage was first discussed by [Cutler and Gruber \(1996\)](#) in their study of historic Medicaid expansions for pregnant women and children; it may be even more relevant in our context given another change under the ACA designed to make it easier for the uninsured to gain immediate access to Medicaid-funded services. For the first time, the federal government required states to allow presumptive eligibility under their Medicaid programs. Specifically, the ACA granted hospitals the ability to make presumptive eligibility determinations for Medicaid for certain groups covered in their state, including the nonelderly ACA expansion population ([Caucci 2014](#)).<sup>16</sup> This means that if patients appear to have incomes low enough to qualify for Medicaid, hospitals may grant temporary Medicaid enrollment. Recipients of this temporary enrollment status may immediately receive health services and providers are guaranteed reimbursement for those services. In addition to presumptive eligibility, federal law directs states to provide retroactive coverage for new enrollees by covering medical bills incurred up to three months before their application date if they met the eligibility criteria during that time.<sup>17</sup> By not requiring a person to first enroll in Medicaid prior to receiving Medicaid-funded care, these policies reinforce the notion that all eligible individuals are effectively covered by the program even if not actually enrolled.

We estimate Medicaid eligibility for our sample using information on state eligibility rules (see [Online Appendix 2](#) for additional information) and characteristics of respondents in the 2008 to 2017 waves of the ACS who were ages 55 to 64 in 2014, and otherwise meet the same sample definition as used in our main analyses. Note that we are unable to use individual panel data for this analysis because we only observe respondent

16. Previously presumptive eligibility programs were optional for states and limited to pregnant women and children. States also had discretion over what types of providers could grant presumptive eligibility for these groups.

17. A handful of states (AR, IA, IN, and NH) had federal waivers to waive retroactive coverage for the expansion population, or other existing Medicaid eligibility groups, during our study period ([Musumeci and Rudowitz 2017](#)).

characteristics for our linked ACS-mortality sample during the year they completed the ACS survey. While repeated cross-sectional data for this cohort does not exactly mirror the individual panel data used to study mortality, it allows us to provide an estimate of the changes in eligibility likely similar to those experienced by our main sample. We rely on the same data to study changes in overall insurance coverage over time, since this information is also not captured in our panel data.

Next we use the longitudinal administrative data on Medicaid enrollment to examine changes in Medicaid coverage in each year, and in the total number of years of Medicaid coverage experienced.<sup>18</sup> We use three different dependent variables to measure coverage changes. The first is equal to 1 if the individual is enrolled in Medicaid during year  $t$  and 0 otherwise, capturing changes in coverage in each year. The second variable equals the number of days enrolled by the individual in each year, including zero days for those who do not enroll. Third, we examine the total days during which the individual was enrolled up to and including year  $t$ , divided by 365, that is, the total accumulated number of years of Medicaid enrollment. This third version acknowledges that health insurance coverage may have beneficial health effects that extend even after the period of coverage is over and that health benefits may accumulate over time.<sup>19</sup>

Finally, we also document how overall insurance coverage evolved following the expansion. In contrast to Medicaid enrollment, we do not have administrative data on insurance coverage. Instead, we use a repeated cross-section of respondents from the 2008 to 2017 waves of the ACS to measure point-in-time insurance coverage for individuals who meet our sample criteria, as we did to determine changes in eligibility.<sup>20</sup> We also explore the effect of the expansion on insurance coverage using two

18. In this analysis, we use Medicaid enrollment data for all individuals starting with the year of their ACS interview. However, because we have complete enrollment information for each year, we exclude the  $\gamma \mathbf{I}(j = t)$  term from equation (1). This term is also excluded from the other first-stage analyses described above that rely on repeated cross-sections of the ACS.

19. See Currie, Decker, and Lin (2008); Boudreaux, Golberstein, and McAlpine (2016); Goodman-Bacon (2016); Wherry and Meyer (2016); Wherry et al. (2017); Brown, Kowalski, and Lurie (2018) for evidence of such long-term effects.

20. To assess comparability with our estimates based on administrative data, we provide estimates of how self-reported Medicaid enrollment changed using the same data, although it is important to note that there is a well-known

alternative data sources. First, we examine changes in contemporaneous insurance coverage using the National Health Interview Survey (NHIS), which is considered to have the most valid coverage estimates nationally (Lynch et al. 2011). In contrast to the ACS, the NHIS uses state-specific names for Medicaid/CHIP in its coverage questions; it also includes a verification question for the uninsured. Second, we use the panel Health and Retirement Study (HRS) to examine cumulative changes in insurance coverage. We use restricted versions of both data sets that include state identifiers and construct analytic samples using the same sample criteria as our main sample.

The results are presented in Figure I and in the first five columns of Table I. We find a large increase in Medicaid eligibility associated with the ACA Medicaid expansions with gains of between 49 and 51 percentage points in each postexpansion year, as compared with the year just before expansion. Consistent with many other studies of this policy, we find significant increases in Medicaid coverage and decreases in uninsurance associated with the decision to expand Medicaid eligibility. We find the probability an individual is enrolled in Medicaid during the year increases by 12.8 percentage points, and on average, individuals in expansion states experience 43 additional Medicaid-enrolled days a year relative to those in nonexpansion states (with those who do not enroll coded as having 0 enrolled days). We aggregate the number of days enrolled in each year to examine the cumulative number of Medicaid-enrolled years. Cumulative Medicaid enrollment experienced by our sample also increases significantly, with respondents in Medicaid expansion states experiencing 0.38 additional years of Medicaid relative to those in nonexpansion states on average, or about 0.67 additional years by the end of our sample period.

Using data from the ACS, we find that self-reported uninsurance decreases by 4.4 percentage points, on average, following the expansions. The estimates for years 1 and 2 are larger than those for year 3, which likely reflects the increasing share of the sample that is aging into Medicare over the study period. Note that we find somewhat larger effects of the Medicaid expansions on coverage using the NHIS, with decreases ranging from 4.9 to 9.5 percentage points across the postexpansion years and an average decline in the postexpansion period of 5.8 percentage points (see

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underreport of Medicaid enrollment in survey data (Boudreaux et al. 2015). See Online Appendix Figure A1 and Online Appendix Table A3.

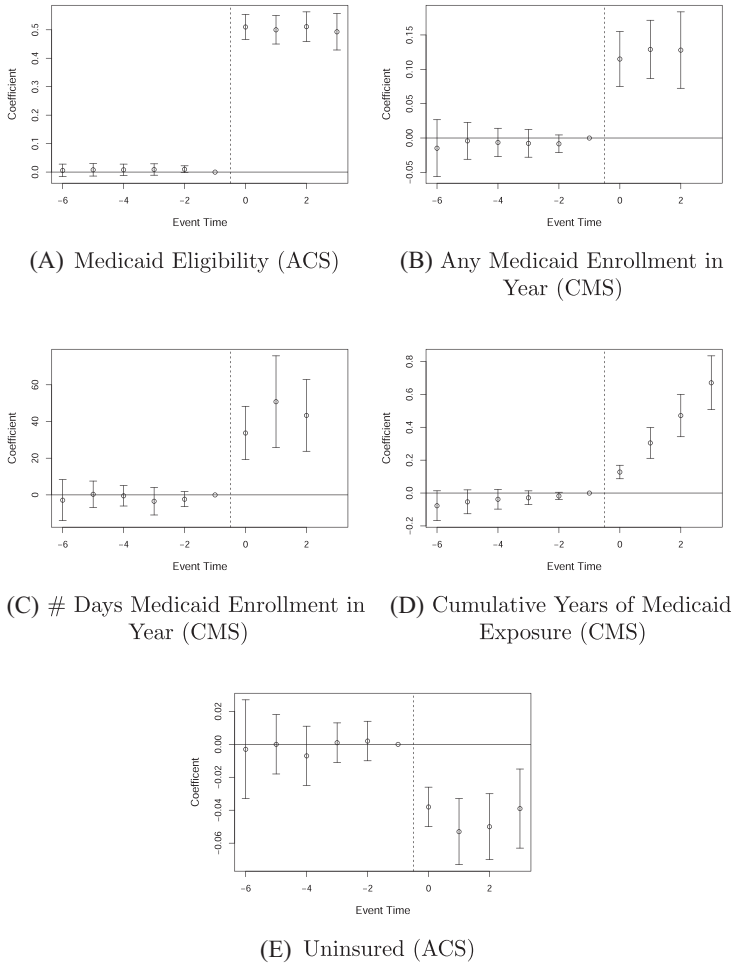


FIGURE I

### Effect of the ACA Medicaid Expansions on Eligibility and Coverage

These figures report coefficients from the estimation of [equation \(1\)](#) for the outcomes of Medicaid eligibility, Medicaid coverage, and uninsurance from the 2008–2017 American Community Survey (ACS) and 2008–2016 Centers for Medicare & Medicaid Service (CMS) administrative enrollment data. Note that scales differ across the five panels. The coefficients represent the change in outcomes for expansion states relative to nonexpansion states in the six years before and four years after expansion, as compared with the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 55–64 in 2014 who are not SSI recipients and who have either less than a high school degree or family income below 138% FPL. See [Section V.A](#) for additional details on the analysis and [Online Appendix 2](#) for information on the Medicaid eligibility determination.

TABLE I  
EFFECT OF THE ACA EXPANSIONS ON COVERAGE AND MORTALITY: DIFFERENCE-IN-DIFFERENCES ESTIMATES

	Medicaid eligibility (1)	Any Medicaid coverage in year (2)	Days of Medicaid in year (3)	Cumulative Medicaid years experienced (4)	Uninsured (5)	Died in year (6)
<i>Difference-in-differences model</i>						
Expansion × post	0.498 (0.026)***	0.128 (0.020)***	42.99 (8.89)***	0.375 (0.061)***	−0.044 (0.010)***	−0.00132 (0.00050)**
<i>Event study model</i>						
Year 3	0.493 (0.032)***	NA	NA	0.671 (0.082)***	−0.039 (0.012)***	−0.00208 (0.00083)**
Year 2	0.511 (0.026)***	0.128 (0.028)***	43.33 (9.78)***	0.472 (0.065)***	−0.050 (0.010)***	−0.00131 (0.00056)**
Year 1	0.500 (0.025)***	0.129 (0.021)***	50.79 (12.49)***	0.305 (0.047)***	−0.053 (0.011)***	−0.00119 (0.00044)***
Year 0	0.510 (0.022)***	0.115 (0.020)***	33.74 (7.27)***	0.128 (0.021)***	−0.038 (0.006)***	−0.00089 (0.00036)**
Year −1 (omitted)	0	0	0	0	0	0
Year −2	0.010 (0.006)*	−0.008 (0.006)	−2.33 (2.05)	−0.018 (0.011)	0.002 (0.006)	0.00015 (0.00047)
Year −3	0.009 (0.010)	−0.008 (0.010)	−3.45 (3.75)	−0.029 (0.021)	0.001 (0.006)	−0.00029 (0.00053)
Year −4	0.008 (0.010)	−0.006 (0.010)	−0.49 (2.78)	−0.038 (0.030)	−0.007 (0.009)	0.00011 (0.00069)
Year −5	0.008 (0.011)	−0.004 (0.013)	0.35 (3.59)	0.053 (0.036)	0.000 (0.009)	0.00091 (0.00069)
Year −6	0.006 (0.011)	−0.015 (0.021)	−2.86 (5.58)	0.077 (0.045)*	−0.003 (0.015)	−0.00021 (0.00070)
N (Individuals × year)	714,673	3,493,000	3,493,000	4,000,000	714,673	4,030,000
N (Individuals)	714,673	566,000	566,000	566,000	714,673	566,000

Notes. This table displays the event study coefficient estimates of equation (1) and the coefficient from the difference-in-differences model. The sample is defined as U.S. citizens ages 55–64 in 2014 who do not receive SSI and who have either less than a high school degree or family income below 138% FPL. For models based on restricted-use data, sample sizes are rounded following census disclosure rules. See the text for more details. Significance levels: \* = 10%, \*\* = 5%, \*\*\* = 1%.

[Online Appendix](#) Figure A1 and [Online Appendix](#) Table A3). Estimates from the NHIS and ACS indicate that a significant share of new Medicaid enrollees would have some form of insurance coverage in the absence of Medicaid expansion. Some of this “crowd-out” may reflect the availability of subsidized exchange coverage for individuals with family incomes above the FPL starting in 2014 in nonexpansion states.

Finally, it is important to note that the insurance coverage changes documented in the ACS and NHIS only provide information on coverage changes in any given year. However, just as the ACA Medicaid expansions affected the accumulation of Medicaid coverage over time, they also affected the accumulation of insurance coverage. We do not have longitudinal administrative data on the total number of years of insurance coverage as we do with Medicaid enrollment. However, we are able to shed light on cumulative insurance coverage using data from the HRS.<sup>21</sup> Our sample in the HRS is much smaller than the ACS-mortality panel ( $N = 1,359$  versus 566,000) but allows us to provide at least some suggestive evidence regarding cumulative insurance coverage. We find that respondents living in expansion states report having accumulated 0.39 additional years of continuous insurance coverage in the postexpansion period relative to respondents in the nonexpansion states. This estimate is imprecise and only significant at the 10% level, albeit of a similar magnitude to the cumulative change in Medicaid coverage that we document in both the HRS ([Online Appendix](#) Table A3) and the administrative CMS data ([Table I](#)).

Taken together, our first-stage analysis indicates that there were large and significant effects of the ACA Medicaid expansions on eligibility, Medicaid enrollment, and insurance coverage for our target sample. In addition, individuals accumulated more years of exposure to Medicaid and insurance coverage as a result of these expansions.

### *V.B. Mortality*

The previous section established that the Medicaid expansions had a meaningful effect on eligibility, enrollment, and coverage for our sample. We now examine the effect of this expansion on mortality. Our estimates of [equation \(1\)](#) are presented in

21. See [Online Appendix](#) 3 for details on the HRS sample construction and analysis.

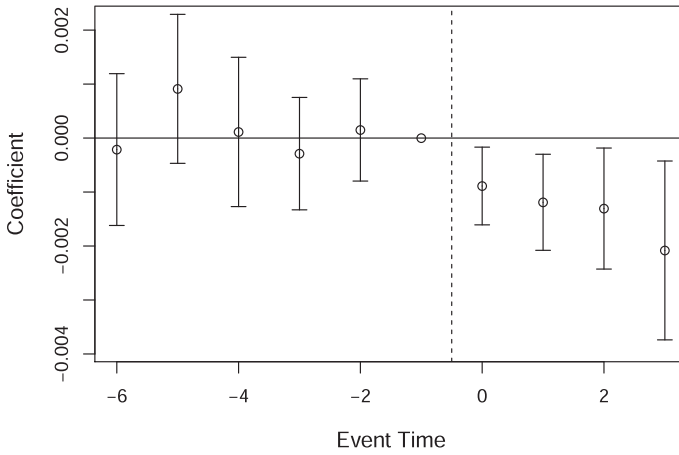


FIGURE II

Effect of the ACA Medicaid Expansions on Annual Mortality

This figure reports coefficients from the estimation of [equation \(1\)](#) for annual mortality. The coefficients represent the change in mortality for expansion states relative to nonexpansion states in the six years before and four years after expansion, as compared to the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 55–64 in 2014 observed in the 2008–2013 American Community Survey who are not SSI recipients and who have either less than a high school degree or family income below 138% FPL.

[Figure II](#) and the sixth column of [Table I](#). Prior to the ACA expansion, mortality rates trended similar across the two groups: preexpansion event study coefficients are close to zero and not statistically significant. Starting in the first year of the expansion (year 0), we observe mortality rates decrease significantly among respondents in expansion states relative to nonexpansion states. The coefficient estimated in the first year of expansion indicates that the probability of annual mortality declined by 0.089 percentage points. In years 1 and 2, we find reductions in the probability of a little over 0.1 percentage points and, in year 3, a reduction of 0.208 percentage points. All estimates are statistically significant, and our confidence intervals include meaningfully sized effects.<sup>22</sup>

The immediate effect of the Medicaid expansion on mortality mirrors several analyses that find sizable changes in health care

22. For example, in year 0, we are able to rule out reductions in mortality smaller in magnitude than 0.018 percentage points; in year 1, 0.033 percentage points; in year 2, 0.021 percentage points; and in year 3, 0.045 percentage points.



utilization during the first year of the expansions. For example, [Garthwaite et al. \(2017\)](#) find large changes in hospital ED usage; [Wherry and Miller \(2016\)](#) find increases in hospitalizations, physician visits, and diagnoses of chronic illnesses; [Ghosh, Simon, and Sommers \(2019\)](#) find increases in the use of prescription drugs; and [Sommers et al. \(2015\)](#) find improvements in access to medication and personal physicians all emerging in the first year of the Medicaid expansions.

In addition, we find evidence that the mortality effects of the expansions are growing over time. As discussed earlier, mortality is increasing over time for our panel as they reach older ages. However, when we examine the coefficient estimates as compared with a counterfactual mortality rate for each event time, we find that the proportionate change in mortality is increasing in each year, from an estimated 7.0% reduction in year 0 to an 11.9% reduction in year 3.<sup>23</sup>

The top panel of [Table I](#) shows the difference-in-differences estimate that pools all postexpansion years together. Using this model, we estimate an average reduction in annual mortality of 0.132 percentage points. This represents a reduction in mortality of about 9.4% relative to the sample mean, or 8.1% relative to our estimated counterfactual mortality rate during the post period of 1.63%.<sup>24</sup>

We conduct additional analyses to identify the changes in death rates by the underlying cause of death using the MDAC. These analyses rely on a much smaller sample and shorter follow-up period, so we consider this analysis to be exploratory in nature. We examine deaths due to non-disease-related (i.e., “external”) and disease-related (i.e., “internal”) causes separately. We also

23. As described earlier, we estimate a counterfactual mortality rate that takes into account that our panel is aging over time and the mortality reduction of expanded Medicaid coverage during the post period. For each event time, it is equal to the sum of the postexpansion mortality rate in expansion states in that period and the absolute value of the corresponding event study coefficient for that period. The counterfactual rates are 1.28% in year 0, 1.51% in year 1, 1.60% in year 2, and 1.75% in year 3. We are then able to express our reduced-form effects as a 7.0% reduction in year 0, 7.9% reduction in year 1, 8.2% reduction in year 2, and 11.9% reduction in year 3.

24. The counterfactual mortality rate is calculated as the average annual mortality rate observed in the expansion states in the postexpansion period (1.494%) minus the reduced-form mortality difference-in-differences estimate (−0.1320 percentage points).

TABLE II  
EFFECT OF THE ACA EXPANSIONS ON COVERAGE AND MORTALITY: CAUSE OF DEATH

	Deaths from internal causes (1)	Deaths from health care–amenable causes (2)	Deaths from external causes (3)
<i>Difference-in-differences model</i>			
Expansion × post	−0.00235 (0.00675)***	−0.00099 (0.00050)*	0.00038 (0.00020)*
<i>Event study model</i>			
Year 1	−0.00221 (0.00126)*	−0.00041 (0.00082)	0.00010 (0.00039)
Year 0	−0.00209 (0.00108)*	−0.00103 (0.00075)	0.00025 (0.00032)
Year −1 (omitted)	0	0	0
Year −2	−0.00053 (0.00083)	0.00065 (0.00053)	−0.00007 (0.00034)
Year −3	0.00088 (0.00104)	0.00014 (0.00072)	−0.00007 (0.00044)
Year −4	−0.00044 (0.00112)	−0.00008 (0.00082)	−0.00032 (0.00038)
Year −5	0.00075 (0.00095)	0.00047 (0.00074)	−0.00022 (0.00037)
Year −6	0.00071 (0.00106)	0.00023 (0.00062)	−0.00060 (0.00035)
N (Individuals × year)	683,000	683,000	683,000
N (Individuals)	88,500	88,500	88,500

Notes. This table displays the event study coefficient estimates of [equation \(1\)](#) using the MDAC. Sample sizes are rounded following census disclosure rules. See text for more details. DRB Disclosure Approval no. CBDRB-FY19-310. Significance levels: \* = 10%, \*\* = 5%, \*\*\* = 1%.

examine a subset of deaths caused by internal factors that are considered to be “health care–amenable” ([Nolte and McKee 2003](#)), which have been studied in the existing literature (e.g., [Sommers, Long, and Baicker 2014](#); [Sommers 2017](#)).

These results are presented in [Table II](#). We observe similar patterns for internal mortality and health care–amenable mortality as we do in our main results, with relative decreases beginning in the first year after the expansions occur. The event study coefficients are not statistically significant for health care–amenable mortality and are significant at the  $p < .10$  level for deaths from internal causes; however, we find a highly significant reduction in deaths related to internal causes under the difference-in-differences model. In contrast, mortality from external causes, which may be less affected by insurance coverage, does not appear to decrease after the expansions. The point estimates

on the event study indicators are not statistically significant and the difference-in-differences estimate is only significant at the 10% level. The estimate is positive in sign, although we note that there is a slight upward pretrend in these deaths in expansion states relative to nonexpansion states.

We further probe cause of death by conducting an analysis using the ICD code groupings by body system. This exercise is meant to be exploratory with the hope that it will provide guidance for future work should better data become available. The results are reported in [Online Appendix Table A4](#). For most diseases, we observe negative coefficients; the largest negative point estimates are observed for deaths related to neoplasms (cancer), endocrine and metabolic diseases (primarily diabetes), cardiovascular and circulatory system diseases, and respiratory diseases. Two of these (cardiovascular/circulatory and endocrine/metabolic) are marginally significant at the 10% level. The point estimates suggest that cardiovascular disease might account for approximately 38% of the overall reduction in internal mortality, with endocrine and metabolic diseases accounting for another 18%. Changes in mortality for these two body groupings are consistent with recent evidence documenting a decrease in metabolic syndrome-related conditions (obesity, high blood pressure, diabetes) and complications arising from these conditions among near-elderly adults under the Medicaid expansions ([McInerney et al. 2020](#)). We also see a small negative but statistically significant effect on diseases related to the skin and subcutaneous tissue, for which diabetes is an important risk factor ([Ki and Rotstein 2008](#)).

## VI. ALTERNATIVE SPECIFICATIONS AND ROBUSTNESS CHECKS

### VI.A. *Role of Staggered Treatment Timing*

Recent research demonstrates that variation in treatment effects across different treatment cohorts, defined by their treatment timing, can make event study estimates difficult to interpret. In our context, we have four different treatment cohorts with 21 states expanding Medicaid in 2014, 3 states expanding in 2015, 2 states in 2016, and 1 state in 2017. [Sun and Abraham \(2020\)](#) show that, if each of these cohorts have different profiles of time-varying treatment effects, the event study estimates can be contaminated by treatment effects from other periods. We demonstrate that this variation in treatment timing is not a concern in

our context by conducting additional analyses that (1) limit the expansion states to the 2014 expanders, and (2) use the alternative estimation method proposed in [Sun and Abraham \(2020\)](#). The event study results from these two approaches are reported in [Online Appendix Figure A2](#) and are very similar to our main results. In addition, the difference-in-differences estimate when dropping the late expanding states is reported in [Figure III](#), with the estimate from our main specification provided for comparison purposes. The estimate is very similar to our main result. Further details on these analyses may be found in [Online Appendix 5](#).

In related work, [Goodman-Bacon \(2019\)](#) demonstrates that staggered treatment timing combined with the presence of time-varying treatment effects can lead to biased DD estimates. This can occur when earlier treatment cohorts function as controls for later treatment cohorts while on a differential path due to their earlier treatment. We do not expect this to be a major concern in our context given that we have so few late adopter states and a relatively short post period. However, to determine the potential influence of comparisons of states with different treatment times, we implement the [Goodman-Bacon \(2019\)](#) decomposition that examines the role of each  $2 \times 2$  DD comparison in the two-way fixed effects DD estimate.<sup>25</sup> We find that only 11% of the DD estimate is derived from comparisons of states with different treatment times. The DD estimate that excludes this source of variation, relying solely on comparisons of treated and untreated units, is very similar, although slightly larger in magnitude than our main DD estimate (see [Online Appendix Table A5](#)).

### *VI.B. Further Investigating Differential Pretrends*

Next we consider the potential effect of differential linear pretrends in expansion and nonexpansion states. Although we do not find any evidence of differential pretrends in the event study, we explore how well powered this test is in our context. To do this, we first determine the size of a linear pretrend we are powered to detect following the procedure described in [Roth \(2019\)](#). Our analysis suggests that we could detect with 80% power a fairly small negative linear trend of a magnitude of 0.03235 percentage points or greater (in absolute terms) in our event study model (i.e.,

25. We implement this decomposition following the methods outlined in [Goodman-Bacon \(2019\)](#) and based on the code in [Goodman-Bacon, Goldring, and Nichols \(2019\)](#).

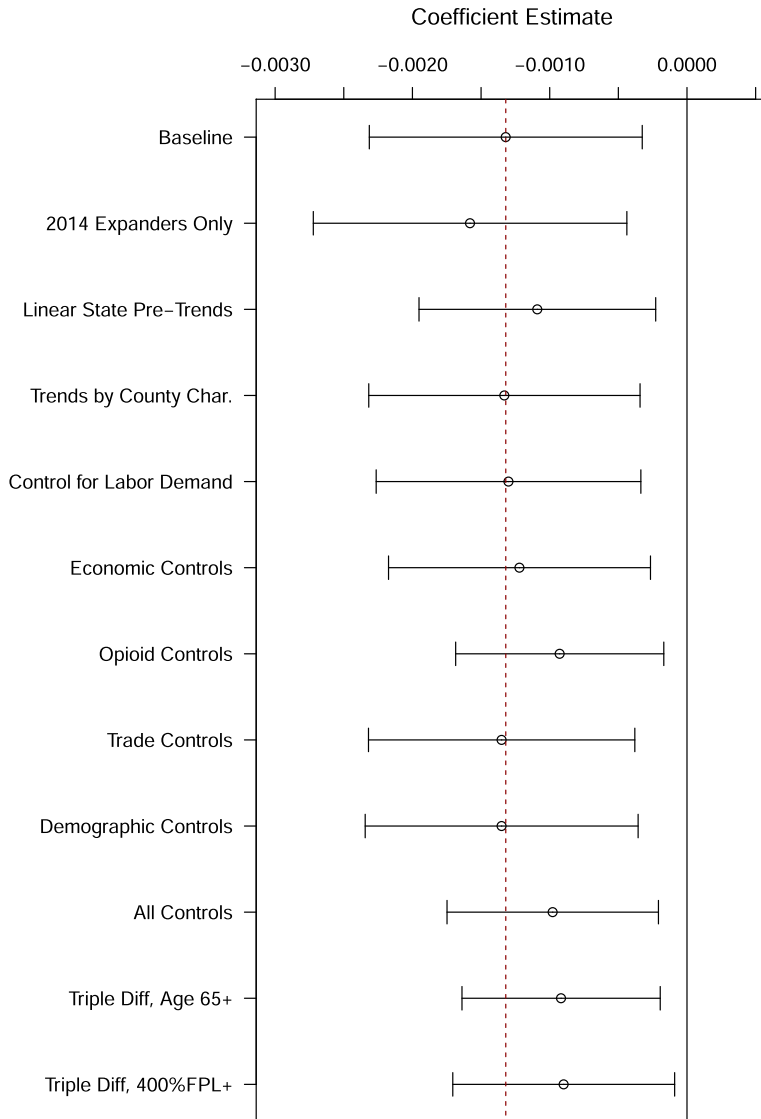


FIGURE III

Effect of Medicaid Expansions on Mortality: Alternative Specifications

This figure plots difference-in-differences estimates for models that include only 2014 expanders, include linear state pretrends, include different sets of covariates, or use an additional comparison group (triple-difference model). The dotted line denotes our main estimate, which is also plotted with confidence intervals at the top of the figure for comparison purposes.

a pretrend of such size is likely to generate at least one statistically significant preperiod event study coefficient). If a trend of a size of up to  $-0.03235$  percentage points is indeed present (although not detectable to us), we calculate that it would generate, by year 3 following the expansion, a bias of at most  $-0.08873$  percentage points.<sup>26</sup> Our actual estimate in year 3 is  $-0.2082$  percentage points, and substantially larger ( $2.3\times$ ) than this potential bias.

We explicitly allow for differential linear trends in the model. First, we estimate an alternative version of the model that allows for differential pretrends in expansion and nonexpansion states by replacing the preexpansion event study coefficients with a linear trend in event time for expansion states, as in [Dobkin et al. \(2018\)](#) and [Gross, Notowidigdo, and Wang \(2020\)](#). The estimates of this specification are reported in [Online Appendix Table A6](#) and are very similar to our main estimates. Second, we estimate a version of the model that includes state-specific linear pretrends, following the two-step procedure proposed by [Goodman-Bacon \(2019\)](#). The result from this approach is presented in the third row of [Figure III](#) and is similar to our main model. Third, we estimate a model that controls for local trends in mortality among counties with different demographic and economic characteristics by interacting the 2013 county-level unemployment rate, median income, poverty rate, share Black, share Hispanic, and share female population with linear year trends. Including these trend terms does not appreciably affect our estimate, as seen in the fourth row of [Figure III](#).<sup>27</sup>

### VI.C. Confounding Factors and Threats to Validity

Even if outcomes were evolving similarly for the two groups of states prior to expansion, the assumptions of our model would be violated if they experienced differential economic or policy shocks around the time of the expansion that drove changes in health outcomes. We explore this by introducing different sets of covariates and examining the effect of their inclusion on our estimates. The difference-in-differences estimates are presented graphically

26. We calculate the bias following the formula presented in [Roth \(2019\)](#), which takes into account the additional bias introduced by passing a pretest.

27. We also consider the presence of nonlinear differential pretrends using methods outlined in [Rambachan and Roth \(2019\)](#). Our analysis suggests our results are reasonably robust to unrelated deviations in the two groups. See [Online Appendix 4](#) for more details.

in [Figure III](#). We also include event study versions for these alternative specifications, with our original estimates depicted in gray for comparison, in [Online Appendix Figure A3](#).

We first examine sensitivity to controls for local economic conditions. In the fifth line of [Figure III](#), we control for predicted changes in labor demand at the county level (see [Online Appendix 4](#) for further details). Line 6 includes time-varying controls for county-level economic characteristics (unemployment rate, poverty rate, median household income). Our estimate remains unchanged with the inclusion of these variables.

We examine whether differential exposure to the opioid crisis could be an important confounding factor. Analysis of the MDAC data shows that the change in mortality we observe is driven by a decrease in deaths due to internal, disease-related causes, which do not include drug overdoses. However, we further explore this possibility by directly controlling for state policies that have been tied to opioid overdose rates in the seventh line of [Figure III](#). This model includes the following controls: indicators for prescription drug monitoring programs (PDMP), mandatory PDMPs that require physicians to access patients' prescription histories, state regulations for pain clinics, legalization of medical marijuana, open legal marijuana dispensaries, and interactions between an indicator that the state had a triplicate prescription program and year fixed effects.<sup>28</sup> We continue to find a large and statistically significant decrease in mortality with the inclusion of these controls.

Next we examine the sensitivity of our results to a control for different exposure to trade from China (i.e., the "China shock" described in [Autor, Dorn, and Hanson 2013, 2019](#)). This previous research has connected increased trade competition with declining economic and social conditions, which may in turn affect health. We allow counties to have different time trends by interacting the commuting zone level of exposure to Chinese imports per worker from 2000 to 2014 with year fixed effects.<sup>29</sup> Including this control variable does not affect our mortality estimate.

Finally, in the ninth line, we control for the demographic composition of our sample by adding individual covariates for race,

28. We draw heavily on [Alpert et al. \(2019\)](#) to identify these opioid-related policies, following the sources and coding outlined in their paper. See [Online Appendix 4](#) for additional details.

29. Note that these measures are not available for Alaska and Hawaii, so we drop these two states when estimating this specification.



age, and gender to our model. Once again the results are largely unchanged by the inclusion of these covariates. We also estimate a model (tenth line) that includes all of the controls described above. We continue to find a negative and statistically significant effect of the ACA Medicaid expansions on mortality rates in our sample and cannot reject that the effect estimated in this model is significantly different than our main result.

One separate concern is that a small subset of our sample ages into Medicare during the 2015–2017 years in the post period. If the Medicare program differs systematically across states in a manner that is correlated with Medicaid expansion, we might inadvertently be picking up differential mortality effects under the Medicare program. To rule out this concern, we reestimate our model for the subgroup ages 55–61 in 2014, who do not qualify for Medicare during the post period. The results from this analysis may be found in [Online Appendix Table A7](#) and are similar to those for our main sample.

#### VI.D. Placebo Tests

To further assess the validity of our empirical approach, we conduct several placebo tests. In these tests, we investigate whether we observe effects of the Medicaid expansions in populations that were unaffected or less affected by the policy change.

Our first placebo test shifts the analysis sample back in time to the pre-ACA period. This test can assess whether any elements of our sample construction, such as drawing the ACS sample only in the preexpansion period, might lead to spurious results. We construct the data in the same fashion as our main analysis, but use mortality data from 2004 to 2013 for ACS respondents in the 2004 to 2009 survey years (rather than mortality data from 2008 to 2017 for the 2008 to 2013 survey years). We construct a variable indicating that a state expanded that corresponds to  $Expansion_s$  in [equation \(1\)](#), but we estimate our model as if the first expansions occurred in 2010 rather than 2014, with states expanding  $t$  years after 2014 treated as if they expanded in 2010 +  $t$ . The results of this placebo test using the pre-ACA period is presented in the first row of [Figure IV](#). As expected, we find no effects on Medicaid coverage or mortality in expansion states during this placebo pre-ACA period.

Next we expand on this analysis by randomly assigning Medicaid expansion status to the same number of states and years

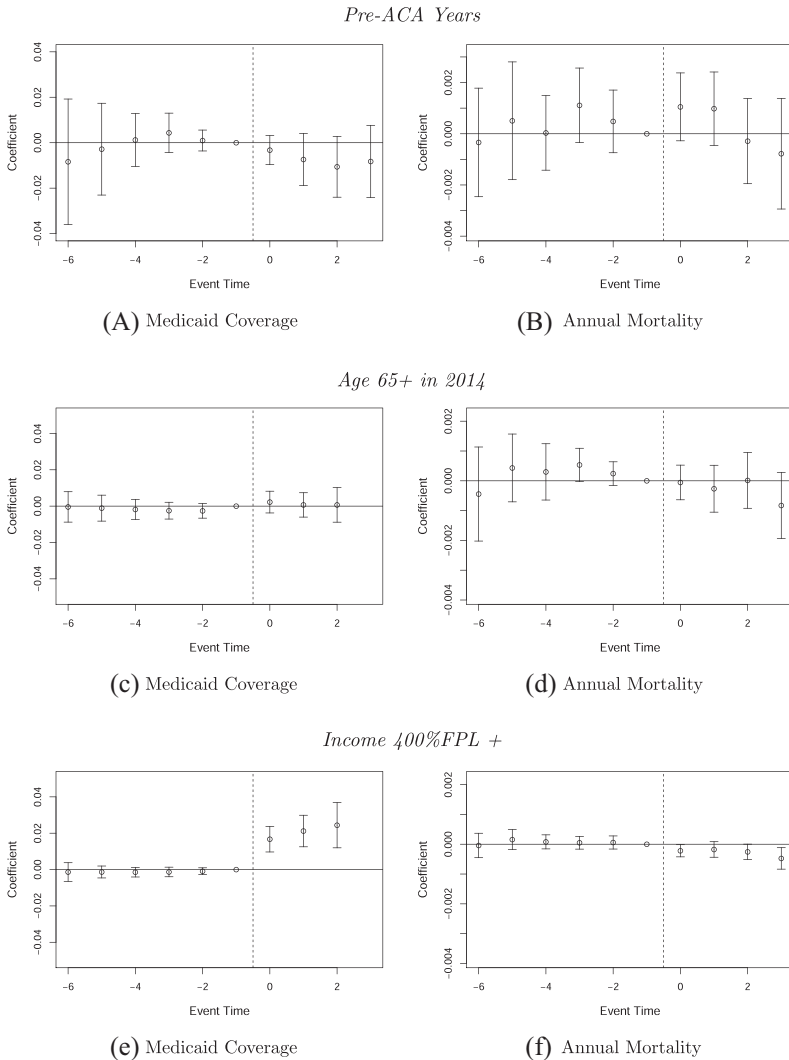


FIGURE IV

### Placebo Tests

Top row figures plot coefficients from [equation \(1\)](#) using pre-ACA years of data. Middle row presents estimates from the ACA study period for those age 65 and older in 2014 who would not have been affected by the Medicaid expansion due to their eligibility for the Medicare program. Bottom row presents estimates for individuals in higher-income families who were less likely to gain Medicaid coverage. See [Section VI.D](#) for additional information.

as occurred under the ACA Medicaid expansions, rather than estimating placebo effects for the actual expansion states. As with the previous exercise, we conduct this placebo test using pre-ACA data, prior to when the actual expansions occurred. We repeat this exercise 10,000 times and compile the coefficient estimates and *t*-statistics from the difference-in-differences model. The results are presented in [Online Appendix Figure A4](#), with the vertical lines indicating the 5th and 95th percentiles of this “placebo” distribution; our model estimate is depicted with a dashed line. This exercise allows us to further investigate the likelihood that we might encounter similarly sized mortality effects by chance. Our estimates for the ACA Medicaid expansions fall well below the 5th percentiles of the distributions of placebo coefficient estimates and *t*-statistics, further increasing our confidence that we are estimating a true policy effect.

We examine whether we observed similar mortality effects for two population groups that were less likely to be affected by the policy. The first test uses individuals who were age 65 and older at the time of the expansions. These individuals have near-universal coverage through the Medicare program and should not be directly affected by the coverage expansions.<sup>30</sup> We reestimate [equation \(1\)](#) for this sample, and the results are presented in the middle row of [Figure IV](#). As predicted, we observe no effect of the Medicaid expansions on Medicaid coverage for this group. We also see no effect of the expansions on mortality rates for this group.<sup>31</sup>

We examine individuals ages 55 to 64 in families earning 400% FPL or greater at the time of the ACS survey. This group should be less affected than our main sample of low-income or low-education respondents. However, they may still gain Medicaid coverage under the expansions due to changes in income over time, or if their income is reported with error. As seen in the third row of [Figure IV](#), we find small but statistically significant increases in Medicaid enrollment associated with the expansions among this group. We also see correspondingly small but, for some years, statistically significant reductions in mortality for this group. These changes are consistent with a causal effect of expanded Medicaid

30. Prior work has documented some spillover effects on the health care utilization of this population under pre-ACA state Medicaid expansions, but analyses of the ACA Medicaid expansions find no evidence of such spillovers and are able to rule out very small effects ([Carey, Miller, and Wherry 2018](#)).

31. Results are similar if we also restrict the elderly to be in low-income families.

coverage on mortality. One interesting note is that those reporting higher incomes in the survey who later enroll in Medicaid exhibit very high average mortality compared with those who do not enroll in Medicaid. This indicates that individuals in the high-income group who do enroll in Medicaid are highly selected (e.g., having experienced a serious health event that resulted in income loss).<sup>32</sup>

We also formally test for a differential effect in our main sample relative to the age 65+ and 400% FPL+ placebo groups by estimating a triple-difference model. Details on this approach are found in [Online Appendix 6](#). The triple-difference estimates reported in the bottom two lines of [Figure III](#) are slightly smaller than our main DD results but remain statistically and economically significant.

## VII. ADDITIONAL ANALYSES

We conduct several additional analyses on different samples and subgroups to further characterize the effect of the Medicaid expansions. First, we examine changes in mortality for all nonelderly adults and other age subgroups. Our main analysis is limited to individuals ages 55 to 64 at the time of the Medicaid expansions, a group with higher mortality rates that has been the focus of other work on this topic (e.g., [Black et al. 2019](#)). In the first row of [Online Appendix Table A9](#), we also estimate the effect of Medicaid expansion on mortality for individuals who meet our sample inclusion criteria but are ages 19 to 64 in 2014. We find a significant increase in enrollment in any Medicaid coverage of 12.7 percentage points. The mortality estimate is not statistically significant but suggests a 3.9% decrease relative to the counterfactual rate. We estimate the effects for other subgroups in this age range (ages 19–29, 30–39, 40–49, and 50–54). We find significant increases in Medicaid enrollment for all groups, but less evidence of corresponding mortality changes. The largest decrease in mortality is observed at ages 50–54 but is not statistically significant. We are unable to rule out meaningful declines in mortality for any age group.

32. If we scale the average mortality effect by the corresponding estimate for the change in any Medicaid coverage, the decrease for the high-income group is larger than for our main sample. We believe this reflects a larger role of adverse selection for the higher-income group.

We further probe heterogeneity by age in our main age group of interest, adults age 55–64 years old in 2014, and at older ages. In [Online Appendix Figure A5](#) we report the percent reduction in mortality associated with the Medicaid expansions in three-year age bins. We construct this by dividing the reduced-form difference-in-differences estimate by the counterfactual mortality rate for each age group. We see that the largest mortality reductions occur at the oldest age groups, with those age 59–61 and 62–64 experiencing statistically significant reductions in mortality. Reductions in mortality are smaller and not statistically significant for those at younger ages (53–55 and 56–58) and for those age 65 and older in 2014, who were Medicare eligible.

We examine the effects of the expansions on different subgroups of our main analysis sample defined by race and ethnicity, gender, marital status, or other characteristics. The results of these analyses are reported in [Online Appendix Table A10](#). We find evidence of larger mortality effects for white, non-Hispanic adults when compared to other racial and ethnic groups. Surprisingly, we detect no effect of the expansions on non-Hispanic Black respondents. We also find larger effects for the men in the sample. These differences are present despite sometimes smaller first-stage estimates for these two groups, although their estimated mortality risk is higher (as seen by a comparison of their counterfactual mortality rates). In addition, the confidence intervals on the mortality estimates do not rule out meaningful declines in mortality for any subgroup. We did not find evidence of mortality differences by marital status. When we narrow the socioeconomic criteria to either less than a high school degree or less than 138% of the FPL, we find overall very similar first-stage and mortality estimates for the two groups. In this set of heterogeneity analyses, there does not appear to be a strong correlation between the size of the first stage and the effect of the expansion on mortality. However, we note that different groups tend to experience mortality from different causes, some of which may be more or less amenable to health interventions. Across subgroups there may also be differential barriers to seeking and receiving care. We leave further exploration of this heterogeneity for future work.

Finally, an additional analysis limits the main sample to approximately a 30% subset who reported being uninsured at the time of the survey. As this group is slightly younger on

average,<sup>33</sup> the counterfactual mortality rate is a bit lower than in the overall sample, at 1.46% a year (versus 1.63% in the sample overall). The point estimates indicate somewhat larger decreases in mortality for this group of 0.150 percentage points (or 10.3% of the counterfactual rate) compared with the reduction in the main sample of 0.132 percentage points (or 8.1% of the counterfactual rate). The fact that those who were uninsured prior to the ACA experienced larger mortality improvements as a result of the policy is in line with our expectations. Indeed, it is somewhat surprising that the differential in the reduction in mortality between those who were uninsured at the time of the ACS and the overall population of low-income older adults is not even larger, particularly because they experienced larger gains in Medicaid enrollment.<sup>34</sup> However, this may be due to the lower mortality rate in this group.

## VIII. INTERPRETING THE ESTIMATES AND COMPARISONS WITH PAST WORK

### VIII.A. *Scaling the Mortality Effects by Coverage Changes*

Our results show consistent evidence of a decrease in all-cause mortality among low socioeconomic status, older adults under the ACA Medicaid expansions. In the difference-in-differences model, we estimate an average decrease in annual mortality of 0.132 percentage points during the four-year post period (top panel of [Table I](#)). We can combine this estimate with the first-stage estimates to provide information on the treatment effect of Medicaid coverage on the group that actually enrolled. For such a scaling to be interpretable as the treatment effect, we must assume that the Medicaid expansions only affected individuals who enrolled in Medicaid. This assumption may be violated if the Medicaid expansions improved access to care more broadly, for

33. We estimate that the average age of uninsured respondents in our sample is 59.0 in 2014, compared with an average of 59.4 years for insured respondents. This difference is statistically significant with a  $p$ -value  $< .0001$ .

34. Note that we are unable to estimate the change in Medicaid eligibility or contemporaneous insurance coverage for this subgroup as we did for the other subgroups, due to the lack of information on historical insurance coverage in the ACS. However, we have explored changes in contemporaneous and cumulative insurance coverage for a similar panel in the HRS (see [Online Appendix Table A11](#)). Because of the small sample size, the estimates are imprecisely estimated but do suggest changes in both contemporaneous and cumulative insurance coverage that are larger in size than those for our main sample.

example, by reducing hospital closures, or improved the health of nonenrollees through other means, such as increasing available resources to families that had some uninsured members prior to the expansion.

Our analysis of administrative Medicaid enrollment records indicates that our sample accumulated 0.375 additional years of Medicaid enrollment, on average, as a result of the ACA expansions (see [Section V.A](#)). Combining this estimate with the 0.132 percentage point reduction in mortality implies that one year of Medicaid enrollment decreases mortality by about 0.35 percentage points ( $= \frac{0.132}{0.375}$ ). We also find that the probability of enrollment in Medicaid in any given year increased by 12.8 percentage points as a result of the ACA expansions. Assuming that mortality responds only to coverage in a given year, rather than the total amount of coverage experienced, this estimate suggests that contemporaneous enrollment in Medicaid reduces mortality by 1.03 percentage points ( $= \frac{0.132}{0.128}$ ).

To better understand the magnitude of these changes, we would ideally compare them to the mortality rate for sample members who enrolled in Medicaid as a result of the expansions (i.e., the “compliers,” see [Imbens and Angrist 1994](#)) in the absence of the policy. As discussed earlier, we estimate a counterfactual mortality rate of approximately 1.63% for all sample members in the expansion states. However, we expect that the counterfactual mortality rate among the compliers is likely much higher if, for example, those in worse health are more likely to enroll in Medicaid. The literature indicates that such adverse selection does occur ([Kenney et al 2012](#); [Marton and Yelowitz 2015](#)); this may also be exacerbated by policies designed to provide immediate coverage to those in need, as discussed earlier (presumptive eligibility and retroactive coverage). As further evidence of this, a recent study by [Garthwaite et al. \(2019\)](#) finds that individuals moving from uninsured to Medicaid status in ACA Medicaid expansion states had higher than average hospital and ED visits during the pre-ACA period.

We estimate an upper bound for this counterfactual rate for the compliers using observed mortality for actual enrollees in our sample. We first estimate average annual mortality for enrollees in the expansion states during the post period at 2.0%. We consider this an upper bound because the CMS enrollment data does not identify the eligibility pathway for coverage and some of these people likely would have qualified for coverage even if there were no



eligibility expansions (e.g., if they became disabled).<sup>35</sup> We therefore expect that this group likely exhibits worse health than those who are induced to enroll by the policy change.

We then take into account the mortality reduction resulting from the gain in Medicaid coverage. Our analysis suggests that a year of Medicaid coverage is associated with a reduction in annual mortality of 0.35 percentage points (as discussed above). On average, those who were not enrolled in Medicaid prior to the ACA but did enroll in expansion states after the ACA remain enrolled for about 2.7 years over our sample period. If compliers experienced 2.7 additional years of coverage due to the expansions, their counterfactual mortality rate would be 2.95% ( $= 2.0 + 2.7 \times 0.35$ ).

Given these calculations, we may expect the counterfactual mortality rate for the compliers to fall somewhere above the estimate for all sample members of 1.63%, but below this upper bound of 2.95%. Combined with our estimated treatment effect of a 0.35 percentage point reduction in mortality associated with one year of accumulated Medicaid enrollment, these counterfactual rates suggest that one year of Medicaid coverage reduces individual mortality by between 11.9% and 21.5%. Meanwhile, our estimate of the treatment effect for contemporaneous Medicaid enrollment (1.03 percentage points) indicates a resulting reduction in individual mortality of between 34.9% and 63.2%.

If we expect that the mortality effects of Medicaid are solely driven by changes in access to care for individuals who would otherwise be uninsured, we may prefer to scale our reduced-form estimates by the change in net insurance coverage, rather than the change in Medicaid coverage. This calculation implicitly assumes that there is no difference between private insurance coverage and Medicaid that would meaningfully affect mortality risk. If coverage only affects mortality during the year in which the respondent is enrolled (i.e., no longer-term effects), the change in contemporaneous insurance coverage (a 4.4 percentage point increase in overall insurance, see [Table I](#)) implies much larger treatment effects, ranging from 102% to 184% reductions in individual mortality.

These large estimate sizes suggest that this scaling may miss important effects for those who enrolled in Medicaid as a result of the expansions but would have otherwise enrolled in exchange- or

35. Note that those who were disabled prior to the ACA should be excluded as our sample criteria excludes those reporting SSI receipt.

employer-based coverage. For example, there may be significant financial differences in the cost of coverage or medical care that affect health care utilization decisions and household finances, both of which may affect mortality risk.<sup>36</sup> There may also be differences in the types of services covered. Unlike most private insurers, Medicaid covers many long-term home- and community-based services and supports (Rudowitz, Garfield, and Hinton 2019), which may be important for people with complex health needs and high mortality risk. Alternatively, this calculation may miss other spillover effects among Medicaid nonrecipients, or the presence of cumulative effects of insurance coverage that are not captured in contemporaneous coverage measures.<sup>37</sup> Finally, it is important to note that the confidence interval on the estimated mortality effect includes much smaller reductions in mortality.

### *VIII.B. Comparisons to Previous Estimates*

In this section, we compare the treatment effects that we estimate to those published in the literature. To take into account differences in coverage changes and population mortality rates across the different study settings, we compare the proportional average treatment effects for new Medicaid enrollees or for the newly insured.

First, we use the public-use replication kit for the OHIE to examine the effect of Medicaid coverage on participants who were ages 55–64 at the time of the experiment to derive estimates comparable to those presented here (reported in [Online Appendix Table A12](#)). Among this age group, ever receiving Medicaid reduced the probability of mortality over a 16-month period by 1.65 percentage points, or a decline of 71.7% relative to the control mean, although this estimate is not statistically significant (associated

36. Medicaid is typically free for beneficiaries with no or minimal cost-sharing for service receipt. In contrast, private insurance coverage typically requires an annual premium, as well as additional cost-sharing. A recent study estimates that under the ACA Medicaid expansions, new Medicaid enrollees ages 50 to 64 saved approximately \$3,100 in out-of-pocket expenditures each year (McInerney et al. 2020).

37. When we consider cumulative exposure to insurance coverage using estimates from our HRS sample, the implied treatment effects indicate that each year enrolled in any coverage reduces mortality by between 12% and 21%. However, these estimates are fairly imprecise.

with a  $p$ -value of .128).<sup>38</sup> This estimated proportional treatment effect is very similar to the estimate in our analysis (i.e., 63%) described in the last section.

We also use OHIE data to estimate the effect of insurance coverage, rather than Medicaid coverage, on mortality. We do this by examining the subsample of participants who responded to the OHIE survey and provided information on their insurance status at an initial survey (completed an average of 1 month after coverage approval), or at a survey approximately one year later (an average of 13 months after coverage approval, see [Finkelstein et al. 2012](#)). Using the gain in coverage measured in the initial survey, we find a decrease in mortality of 326% ( $p = .071$ ) compared with the control group mean. Using the 12-month measure, we find a 335% mortality reduction ( $p = .153$ ).<sup>39</sup> These are both larger than our estimate of the impact of new insurance coverage under the ACA Medicaid expansions, although the confidence intervals overlap with our estimate. See [Online Appendix 7](#) for further discussion.

Recent work by [Goldin, Lurie, and McCubbin \(2021\)](#) also provides experimental evidence on the effect of health insurance on mortality. Taking advantage of changes in insurance status generated by randomly assigned informational letters, the authors find that each additional month of health insurance coverage reduced mortality in their sample of 45- to 64-year-olds by 0.178 percentage points over a two-year period, or 10.1% compared to their estimated complier baseline mortality rate. Assuming the effect of coverage decays geometrically, the authors estimate that 12 months of coverage results in a reduction in mortality over the two-year period of 106% relative to their estimated complier mean, which is in the range of our estimates for the effects of contemporaneous insurance coverage.

In addition to these two experiments, there are also several quasi-experimental analyses that examine the effects of insurance coverage expansions on nonelderly adult mortality. To facilitate comparisons across these studies, we estimate the implied mortality effects for individuals either gaining Medicaid or insurance

38. We can also scale this effect by 12/16th to arrive at an annual effect of Medicaid on mortality of 1.24 percentage points. This is comparable, but larger, than the 1.03 percentage point treatment effect of contemporaneous Medicaid enrollment estimated in our analysis.

39. Note that baseline mortality for the control group is lower among survey respondents since participants had to be alive to respond to the survey.

coverage as proportionate changes over a counterfactual mortality rate. Studies often vary in how they treat measurement error in survey reports of Medicaid enrollment and in the assumed counterfactual mortality rate; we therefore apply a consistent set of adjustments across these studies. We also present 95% confidence intervals for these estimates. Further details on the approach used to construct these scaled estimates and their confidence intervals may be found in [Online Appendix 7](#).

The resulting estimates are reported in [Online Appendix Table A13](#). It is clear from this table that our estimates are in the ballpark of those from existing work examining the mortality effects of Medicaid or insurance expansions for low-income adults. [Sommers \(2017\)](#) examines changes in mortality among 20- to 64-year-olds following pre-ACA Medicaid expansions in Arizona, Maine, and New York. Using a difference-in-differences design, he finds a mortality reduction of 64.5% among individuals gaining insurance coverage. Similarly, taking advantage of county-level variation in the effect of the Massachusetts health reform of 2006, [Sommers, Long, and Baicker \(2014\)](#) find that insurance coverage reduces mortality by 27.3% among 20- to 64-year-olds. Using mortality records aggregated to the county level, [Borgschulte and Vogler \(2020\)](#) find adults age 20 to 64 experienced a 47.3% reduction in mortality under the ACA Medicaid expansions. In our analysis, we find that new insurance coverage reduces mortality by 50% among those in our sample age 19 to 64 (although this effect was not statistically significant), in line with the estimates from these other studies. [Chen \(2019\)](#), however, finds a much smaller and statistically insignificant effect on the order of 9% for adults ages 25–64 under his analysis of the ACA Medicaid expansions.

Fewer papers include estimates for the age group that we study.<sup>40</sup> [Black et al. \(2019\)](#) and [Chen \(2019\)](#) find smaller implied mortality effects of individual insurance coverage compared with our study. The estimate in [Black et al. \(2019\)](#), however, is imprecisely estimated with a large confidence interval inclusive of our estimate.

To summarize, the existing experimental literature on the effect of health insurance on mortality shows large effects,

40. Although some studies present subgroup analyses by age, they do not include the corresponding first-stage estimates needed to estimate the implied individual treatment effect.

suggesting that reductions in mortality associated with insurance well exceed the average mortality rate of the control group. Such large effects may be due to adverse selection, that is, that those who sign up for health insurance are particularly vulnerable and would have experienced high mortality rates in the absence of insurance. Evidence from the quasi-experimental literature tends to find smaller, but still substantial (9%–65%), reductions in mortality associated with coverage. Our estimates tend to be smaller than those documented in the experimental literature but somewhat larger than those reported in quasi-experimental studies.

## IX. CONCLUSION

Low-income adults in the United States experience dramatically worse health and reduced longevity as compared to their higher-income counterparts. The ACA expansion of Medicaid eligibility to poor and near-poor adults represents a historic effort to improve access to high-quality medical care for this population. Robust evidence that Medicaid increases the use of effective medical care indicates that the ACA expansion could play a crucial role in reducing income disparities in health among U.S. adults. However, the magnitude of any health improvements experienced by beneficiaries remains largely uncertain, in part due to the limited amount of data available that contains information on both socioeconomic status and objective measures of health.

In this article, we evaluate these expansions by leveraging data linkages across large-scale federal survey and administrative data. These data linkages allow us to overcome several empirical challenges that previous research in this area encountered, including the lack of information about individual-level characteristics that determine Medicaid eligibility and the systematic misreporting of Medicaid enrollment that occurs in survey data. Using these data, we show that the ACA Medicaid expansions substantially reduced mortality rates among those who stood to benefit the most.

Our estimated change in mortality for our sample translates into sizable gains in terms of the number of lives saved under Medicaid expansion. Using the ACS, we calculate that there are about 3.7 million people who meet our sample criteria living in expansion states. Our results therefore indicate that approximately 4,800 fewer deaths occurred per year among this population due to Medicaid expansion, or roughly 19,200 fewer deaths over the

first four years alone. This calculation relies on the assumption used throughout the article; namely, that in the absence of the ACA expansions, mortality in expansion and nonexpansion states would have trended similarly.

Our estimates also reveal some information about the potential cost of state decisions not to expand Medicaid. These calculations necessarily rely on the assumption that the effects of Medicaid expansion in the nonexpansion states would be similar to those observed in the expansion states. The change in mortality in nonexpansion states if they were to adopt the expansion could be larger (e.g., if the beneficiaries in the nonexpansion states have worse baseline access to effective health care) or smaller (e.g., if the health system has less capacity to treat the newly insured in nonexpansion states). Given these caveats, we estimate that, as there are approximately 3 million people meeting this sample criteria in nonexpansion states, failure to expand in these states resulted in 15,600 additional deaths over this four-year period that could have been avoided if the states had elected to expand coverage.

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#### SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

#### DATA AVAILABILITY

Code replicating the figures and tables in this article can be found in [Miller, Johnson, and Wherry \(2021\)](#) in the Harvard Dataverse, <https://doi.org/10.7910/DVN/UEJRKH>.

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