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NEW EVIDENCE FROM LINKED SURVEY AND ADMINISTRATIVE DATA

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Medicaid and Mortality: New Evidence from Linked Survey and Administrative Data
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ABSTRACT

We use large-scale federal survey data linked to administrative death records to investigate the relationship between Medicaid enrollment and mortality. Our analysis compares changes in mortality for near-elderly adults in states with and without Affordable Care Act Medicaid expansions. We identify adults most likely to benefit using survey information on socioeconomic and citizenship status, and public program participation. We find that prior to the ACA expansions, mortality rates across expansion and non-expansion states trended similarly, but beginning in the first year of the policy, states that opted to expand experienced significant reductions in mortality relative to non- expanders. Individuals in expansion states experienced a 0.132 percentage point decline in annual mortality, a 9.4 percent 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. A variety of alternative specifications, methods of inference, placebo tests, and sample definitions confirm our main result.

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Low-income individuals in the U.S. experience dramatically worse health than those with high incomes. For example, the annual mortality rate for nearly-elderly adults in families earning less than 138 percent of the Federal Poverty Level (FPL) is 1.7 percent, more than 4 times higher than the 0.4 percent rate experienced by higher-income individuals of the same age.[1] 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 rates of death 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 U.S. than other high income countries (Semyonov et al., 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 is the largest expansion of health insurance coverage in the U.S. since the creation of Medicare and Medicaid in 1965. Over 20 million people have gained coverage since the ACA (Martinez et al., 2018), the majority of whom are low-income adults receiving coverage through the Medicaid program (Frean et al., 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 et al., 2019),[2] earlier detection and treatment of treatable cancers (e.g. 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 et al., 2019).[3] However, despite the size and scope of this policy, its impact 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 links 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

[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. We chose these two income cutoffs since adults with family income below 138% FPL qualify for Medicaid in states that expanded their programs to include low-income adults under the Affordable Care Act, the policy of study in this paper; also, adults with incomes below 400% FPL qualify for subsidies for private insurance coverage.

[2]Ghosh et al. (2019) find a substantial increase in prescription drug utilization under the ACA Medicaid expansions, including medications for the management of diabetes, treatments for HIV and Hepatitis C, and drug therapies for cardiovascular disease. These particular types of prescription drugs are among those demonstrated to reduce mortality. Systematic reviews and meta-analyses of randomized, controlled trials find significant decreases in all-cause and cardiovascular mortality for adults who receive statins (Chou et al., 2016) and decreased all-cause mortality for Type 2 diabetics receiving glucose-lowering drugs (Zheng et al., 2018). In addition, systematic reviews of observational studies indicate decreased mortality among HIV-infected adults initiating anti-retroviral therapy (Chou et al., 2005), as well as indirect evidence of decreased mortality linked to cured infection under antiviral treatment for Hepatitis C (Moyer, 2013).

[3]In addition to increasing the provision of these types of ostensibly high value services, Medicaid also increases the use of a variety of other types of medical care such as routine screening for chronic illnesses, outpatient physician visits, use of prescription drugs that aid in smoking cessation, and dental care, which also have the potential to improve health over the longer term. See, for example, Finkelstein et al. (2012); Nasseh and Vujicic (2017); Semyonov et al. (2013). Gruber and Sommers (2019) provide a summary of the evidence to date on the impact of the ACA expansions on health care utilization.

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 on self-reported health (e.g. Cawley et al., 2018; Courtemanche et al., 2018b; Miller and Wherry, 2017; Sommers et al., 2017). Meanwhile, analyses with objective measures of health, such as mortality, measure changes in aggregated, population-level statistics compiled from administrative data (e.g. Borgschulte and Vogler, 2020; Chen, 2019; Khatana et al., 2019; Swaminathan et al., 2018; Yan et al., 2020). These data sources do not have the information needed to determine individual eligibility and directly examine the targeted beneficiaries of the coverage expansions. A concern is that, without information on these relevant individual-level characteristics, these studies are 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 paper, we provide new evidence of the impact of Medicaid on health by using administrative Medicaid enrollment and mortality data linked to large-scale, individual survey records. We use this novel dataset to examine the impact of the sizeable 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 percent 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 non-universal adoption, the ACA Medicaid expansions still represent a historic expansion in insurance coverage. Approximately 13.6 million adults gained Medicaid coverage under the ACA (Medicaid and CHIP Payment and Access Commission, 2018); for comparison, Medicare enrolled about 19 million elderly beneficiaries after its creation in 1965 (Bureau of the Census, 1969). We take advantage of variation in state adoption of this large expansion in coverage to compare changes in mortality among individuals in expansion states and non-expansion states.

In contrast to prior research that relies on death certificate data with limited information on individual characteristics, our data include detailed survey measures collected from the 2008 to 2013 years of the American Community Survey (ACS). This large-scale national survey contains approximately 4 million respondents in each year and allows us to observe information on specific characteristics that determine Medicaid eligibility including income, citizenship status, and the receipt of other social assistance. With this information, we are able to identify individuals targeted by the ACA Medicaid eligibility expansions and, in this way, overcome the inherent limitations present in existing studies that rely only on aggregate death records. We focus on those in this group who were between the ages of 55 and 64 in 2014, who are at greater risk of mortality, although we also present results for all non-elderly adults 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 document sizeable 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 year of the ACS to death certificate records using the National Death Index.

Our analysis shows that the ACA Medicaid expansions reduced mortality among this targeted group. Prior to the expansions, individuals in our sample residing in expansion and non-expansion states had very similar trends in both Medicaid coverage and mortality. At the time of the expansion, the trajectories of these two groups diverged significantly, with expansion state residents seeing increases in Medicaid coverage and decreases in annual mortality rates. In the first year following the coverage expansion, the probability of mortality declined by about 0.089 percentage points, or a 6.4 percent 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 healthcare amenable and internal (disease-related) causes of death were reduced by the expansions, but no evidence that deaths due to external causes, such as car accidents, fell. We show 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 impact of the expansions on those age 65 or older in 2014 who did not gain Medicaid eligibility; examining the effect on individuals in higher income 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 non-expansion 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 impact 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 non-expansion 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 non-adoption that is relevant to both state policymakers and their constituents.

1 Background

Many studies have shown that Medicaid coverage increases access to and use of health care and reduces financial burden for low-income adults,[4] but evidence on its health effects proves more difficult to document and is less conclusive. 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

[4] See, e.g., Abramowitz, 2018; Allen et al., 2017; Baicker et al., 2013; Brevoort et al., 2019; Buchmueller et al., 2016; Caswell and Waidmann, 2017; Courtemanche et al., 2017; Finkelstein et al., 2012; Gallagher et al., 2019; Ghosh et al., 2019; Hu et al., 2018; McMorrow et al., 2017; Miller and Wherry, 2017, 2019; Simon et al., 2017; Sommers et al., 2015, 2017.

improvements in reported health associated with Medicaid expansion (Cawley et al., 2018; Lee and Porell, 2018; Simon et al., 2017; Sommers et al., 2016, 2017), to no effects (Courtemanche et al., 2018a,b; Miller and Wherry, 2019; Sommers et al., 2015; Wherry and Miller, 2016) or even small but marginally significant negative effects (Miller and Wherry, 2017).[5]

One concern with self-reported health data is that it may not accurately measure changes in physical health. In the Oregon Health Insurance Experiment (OHIE), low-income adults selected by a lottery to apply for Medicaid coverage reported near immediate improvements in their health compared to the control group, despite experiencing no significant differences yet in their health care utilization (Finkelstein et al., 2012).[6] The researchers concluded that the change in reported health may at least partly capture a general sense of improved well-being, or "winning" effects resulting from individuals' lottery selection. There is also the risk that changes in self-reported health may reflect increasing awareness of health problems or interactions with the health care system, rather than actual changes in physical health. One example would be increased contact with health providers leading to new information about a previously undiagnosed illness and, as a consequence, a worsened self-perception of health. This could bias downwards estimates of the effect of public health insurance on health.[7] Finally, in general, the reliability of self-reported health measures for U.S. adults and their association with objective health measures are documented to be worse among lower socioeconomic status groups (Dowd and Zajacova, 2007, 2010; Zajacova and Dowd, 2011).

In addition to offering the first experimental evidence on the effects of expanded Medicaid, the Oregon Health Insurance Experiment (OHIE) covered new ground by collecting data on clinical health measures among its participants. The researchers did not observe significant effects on any of the collected measures (blood pressure, cholesterol, and blood sugar levels). Using administrative data, they also found no evidence that Medicaid coverage led to a reduction in mortality during the 16 months following coverage gain. Their estimate suggested a 16 percent reduction in mortality associated with acquiring Medicaid, but with a large confidence interval that could not rule out sizeable changes in either direction.[8]

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

[5]Note that neither the time period of study nor the data sources used seem to explain these inconsistencies. In a series of papers studying Medicaid expansions in two states (AR and KY), Sommers et al. (2015, 2016, 2017) find evidence of significant health improvements emerging only in the second year of expansion. However, in a national study, Courtemanche et al. (2018a,b) find no evidence of improvements in self-reported health due to Medicaid expansion during any of the first three years of implementation. Using the same data source as these authors, Simon et al. (2017) and Cawley et al. (2018) find evidence of sizable health improvements over the same period. Finally, Miller and Wherry (2019) trace out the effects of Medicaid expansion during each of the first four years of implementation, using a different data source, and find no evidence of improvements in self-reported health. The discussion here focuses on studies of overall changes in self-reported health but a more comprehensive review of the health effects of the ACA Medicaid expansions may be found in Soni et al. (2020), including evidence for low-income parents (McMorrow et al., 2017), adults with chronic health conditions (Winkelman and Chang, 2018), women of reproductive age (Margerison et al., 2019), and the safety net population (Graves et al., 2020).

[6]The researchers found an improvement in self-reported health for the treatment group during their initial survey, which was conducted, on average, about one month after gaining coverage, that was about two-thirds of the size of their main effect estimated using survey data collected more than a year later.

[7]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.

[8]Another relevant randomized social experiment provided Medicare to newly entitled Social Security Disability Insurance (SSDI) beneficiaries (as opposed to them being subject to a 2-year waiting period for coverage). The evaluation of this experiment found no reductions in mortality up to 3 years later but the sample sizes were too small to be able to detect effects (Weathers and Stegman, 2012).

ACA Medicaid expansions.[9] A small number of recent studies use population-level mortality data to estimate changes in adult mortality in expansion states compared to non-expansion states. The studies examining the effects of the ACA Medicaid expansions using this approach reach very different conclusions, either unable to detect mortality effects (Black et al., 2019) or estimating varying sized reductions in adult mortality: 3.6 percent among adults ages 20-64 (Borgschulte and Vogler, 2020) and 1.2 percent 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 percent decrease in health care amenable mortality.[10] Khatana et al. (2019) focus on changes in cardiovascular disease-related mortality among adults ages 45-64 and document a 2.9 percent reduction over the baseline mortality rate. Finally, Swaminathan et al. (2018) find an 8.5 percent decrease in one-year mortality for patients with end stage renal disease (ESRD) initiating dialysis associated with the ACA Medicaid expansions.[11]

There is also recent evidence that health insurance coverage (inclusive of Medicaid) can affect mortality. Goldin et al. (2020) 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 percent. The authors found no effects of the letter on mortality for those under age 45.

While the evidence to date suggests that it is likely that there are mortality effects of the ACA Medicaid expansion, it remains unclear for whom and of what magnitude. It is likely that the primary impediment to fully understanding the impact of Medicaid on mortality has been data availability. All of the studies using aggregated data rely on changes in survival for the Medicaid eligible to translate into overall mortality effects 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, other characteristics that might affect Medicaid eligibility, or whether he or she previously had health insurance coverage. Without data that links 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.[12] This decreases the power to detect changes in mortality of a

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

[10]Research on pre-ACA expansions in Medicaid that also relies on aggregated data finds larger effects on adult mortality. Sommers et al. (2012) and Sommers (2017) find a 6 percent reduction in nonelderly adult mortality in pre-ACA Medicaid expansions in New York, Maine, and Arizona measured over a five-year period. Another analysis of expanded coverage under Massachusetts's 2006 health reform finds a significant 2.9 percent reduction in all-cause mortality over four years of follow-up Sommers et al. (2014).

[11]While it is the case that ESRD patients are eligible for Medicare, coverage only begins in the fourth month after initiating dialysis. The authors provide evidence that the expansions increased insurance coverage among individuals initiating dialysis, and increased the receipt of pre-dialysis nephrologhy care.

[12]Variation in exposure to the policy based on state of residence, as well as local geographic characteristics (such as

plausible magnitude, leading some researchers to conclude that "it will be extremely challenging for a study [on the ACA Medicaid expansions] to reliably detect effects of insurance coverage on mortality unless these data can be linked at the individual level to large-sample panel data" (Black et al., 2019).

The absence of conclusive evidence on the magnitude of the health effects of Medicaid is a major omission given the aim of this public program to improve access to and use of efficacious health care. In this paper, 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 use analyses that rely on population-level data and 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 improves on the more commonly used survey measures of Medicaid coverage, which can be subject to substantial misreporting (Boudreaux et al., 2013), and also 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 longer term effects that extend beyond just the coverage period (e.g. Boudreaux et al., 2016; Brown et al., 2018; Currie et al., 2008; Goodman-Bacon, 2016; Thompson, 2017; Wherry and Meyer, 2016; Wherry et al., 2017). 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 Oregon Health Insurance Experiment. This larger sample is particularly important when examining mortality as an outcome since it is rare. These advantages overcome the existing limitations in both experimental and non-experimental studies of Medicaid, allowing us to further advance the evidence on the program's mortality effects.

2 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 American Community Survey, which has a sample size approximately 50 percent 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 percent of the FPL or who have less than a high school degree.[13] Since we only have information on income captured at one point in time, the latter criterion is used to identify individuals who are of low socioeconomic status but might not meet the income cutoff at the time of the ACS interview.[14] We

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 subject to substantial measurement error (see e.g., Rostron et al., 2010; Sorlie and Johnson, 1996).

[13]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 State Health Access Data Assistance Center (2012) for studies of health insurance coverage.

[14]Results are similar if we include only those with less than a high school degree, or only those with incomes under 138 percent of the FPL, rather than defining the sample using the union of these two criteria; see later discussion in Section

exclude non-citizens, many of whom are not eligible for Medicaid, and those receiving Supplemental Security Income (SSI), who are likely to be Medicaid eligible in the absence of the expansions.[15] We restrict our primary analysis to individuals who were ages 55 to 64 in 2014. This higher age group has relatively high mortality rates, and is also consistent with the sample criteria used in Black et al. (2019). We present results for all non-elderly adults, and for a variety of different age subgroups, in a supplementary analysis. Finally, we exclude residents of 4 states and DC that expanded Medicaid to low-income adults prior to 2014.[16] There are approximately 566,000 respondents who meet our sample criteria.[17]

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

Second, we link these data to the Census Numident file. The Census Numident file is derived from the Social Security Administration (SSA) Numerical Identification file, which includes information on date and county of birth and date of death (if it has occurred) for individuals with a Social Security Number (SSN).[18] These data have been used in, e.g., Brown et al. (2018); Chetty et al. (2011, 2016); Dobbie and Song (2015); Sullivan and von Wachter (2009), and other research relying on death information from tax records. Total deaths reported in the SSA file by age and year closely track the numbers reported by the National Center for Health Statistics (Chetty et al., 2016). The Census Bureau updates its Numident file each year with new information from the SSA Numerical Identification file. It formats the data so that there is a single record per individual, reflecting the most accurate and up-to-date information at that point in time. We use 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 dataset a protected identification key (PIK), an anonymized identifier that allows Census to track individuals across datasets. Approximately 90 percent of all ACS respondents are successfully assigned a PIK using available information on name, address, and date of birth, with a slightly higher match rates for citizens (92 percent) (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 social security numbers (SSNs) and date

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[15]SSI recipients are automatically eligible for Medicaid coverage in most states.

[16]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.

[17]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.

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

of birth (Mulrow et al., 2011). Since our analysis is restricted to older citizens, and since nearly all American citizens have SSNs assigned by the time they reach adulthood (see Bernstein et al., 2018), we expect to have nearly full coverage of deaths in the Numident file.

Once these data are linked, we observe the vital status of each individual during the year they respond to the ACS and each subsequent year. For example, we observe the vital status of an individual who responds to the 2008 ACS during each year from 2008 through 2017; for an individual who responds to the 2013 ACS, we observe his or her vital status from 2013 through 2017. We construct our outcome measure to represent mortality during each calendar year. If the individual is alive in a given year, the outcome variable takes a value of 0; if that individual died in that year it takes a value of 1. Once an individual has died, he or she is removed from the sample for subsequent years. In this way, we measure changes in the annual probability of death during a given year among individuals who were alive at the beginning of that year. The annual mortality rate is about 1.4 percent for our sample on average across all years, and approximately 1.3 percent among respondents in expansion states during the year just prior to expansion.[19]

Third, we further 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 records to our sample of ACS respondents. About 93 percent of Medicaid enrollment records successfully receive a PIK assignment using SSN and date of birth information (Fernandez et al., 2015). CMS data on enrollment allows us to observe Medicaid enrollment longitudinally for our sample and to document how the probability of enrollment, and total cumulative exposure to Medicaid, changed in the expansion states relative to the non-expansion 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 Appendix Section C for more information.[20] When examining the probability of being enrolled any time during a calendar year, we simply omit 2017 and use data on enrollment through 2016.

While our data uniquely offer the opportunity to link mortality and economic variables at the individual level, there are also several important limitations. First, we observe the economic characteristics of individuals (income and educational attainment, receipt of social services, and citizenship status) at the time they respond to the ACS in the pre-period, between 2008 and 2013. These are time-varying characteristics, however, and may not accurately reflect economic characteristics at the time of the Medicaid expansions for some members of our sample. For example, an individual in a low-income 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

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

[20] There are a number of other minor data issues surrounding the CMS enrollment data. First, data on Louisiana's Medicaid enrollment is missing in 2015, so Louisiana residents have missing values for that year in our analyses. Results are similar if we exclude Louisiana entirely. Second, we do not observe the number of days enrolled for the fourth quarter of 2015 for a number of states, and impute the number of days enrolled using enrollment information from Q3 2015 and Q1 2016. Third, we do not observe Q4 2016 enrollment for Arkansas' Medicaid program, and assume it is the same as Q3 2016 enrollment. Our first stage results are not sensitive to variations on these assumptions. See Appendix Section C for further details.

ACS and the time the expansions occurred, resulting in our misclassification of whether that individual was exposed to the eligibility expansion.[21] In general, we expect that this type of misclassification will bias our estimates towards 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.

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

3 Empirical Strategy

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

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

As described earlier, our data is constructed at the individual (i) by year (t) level. Each individual responds to the ACS during a survey wave (j) and reports their state of residence (s). The dependent variable $Died_{isjt}$ denotes death during each year t among individuals who were alive at the beginning of year t. We only observe mortality over a partial year during the year of the individual's ACS interview (j), since that individual had to be alive in order to respond to the survey. To account for this, we include an indicator variable that year t is the year that the individual responded to the ACS (i.e., that $j = t$).[22] In this equation, β_s denotes state fixed effects and β_j denotes fixed effects associated with each survey wave. β_t denotes calendar year fixed effects, which will account for general trends in mortality for all individuals in our sample, including their gradual aging over time.[23]

The variable $Expansion_s$ equals 1 if individual i was living in a state that opted to expand Medicaid eligibility between 2014 and 2017, and zero otherwise. Indicator variables $I(t - t_s^* = y)$ measure the time relative to the implementation year, t_s^*, of the expansion in each state, and are zero in all periods for non-expansion states.[24] While most states expanded in the beginning of 2014, some states expanded

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

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

[23] Results are also virtually identical in a model that includes controls for gender, race, and single year of age. We show this later in Section 5.3.

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

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.[25] The omitted category is $y = -1$, the year prior to the expansion. Therefore, each estimate of β_y provides the change in outcomes in expansion states relative to non-expansion states during year y, as measured from the year immediately prior to expansion. If mortality rates for expansion and non-expansion states were trending similarly prior to the ACA, we expect that estimated coefficients associated with event times $y = -6$ to $y = -2$ will be small and not statistically significant. We estimate equation (1) with a linear probability model and report heteroskedasticity-robust standard errors that are clustered at the state level.[26] All analyses use ACS survey weights.

In addition to the event study analyses, we also present difference-in-differences (DD) estimates as a summary of the effect across all post-expansion years. These are estimated using the same equation except that the event study indicators are replaced with a single variable denoting an expansion state during the post period ($Expansion_s \times Post_t$). 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. Since 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 sum of the average annual mortality rate in the expansion states in the post-expansion period and 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).

4 Results

4.1 Medicaid Eligibility, Medicaid Enrollment, and Insurance Coverage

We first estimate the impact of the ACA Medicaid expansions on Medicaid eligibility and coverage for individuals in our sample. We show that, using a variety of data sources and approaches, respondents residing in the expansion states experienced significant increases in Medicaid eligibility, Medicaid

[25] 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.

[26] In 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.

enrollment, and health insurance coverage relative to those in the non-expansion 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 non-expansion 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, however, given another change under the ACA designed to make it easier for the uninsured to gain immediate access to Medicaid-funded services. For the first time, the federal government required states to 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 non-elderly ACA expansion population (Caucci, 2014).[27] 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 3 months prior to their application date if they met the eligibility criteria during that time.[28] By not requiring an individual to first enroll in Medicaid prior to receiving Medicaid-funded care, these policies reinforce the notion that all eligible individuals are effectively covered by the program even if not actually enrolled.

We estimate Medicaid eligibility for our sample using information on state eligibility rules (see Appendix Section B 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 since we only observe respondent characteristics for our linked ACS-mortality sample during their year of ACS interview. 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 mortality sample. In addition, we rely on the same data to study changes in overall insurance coverage over time, which is not captured in the longitudinal Medicaid enrollment data from CMS.

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.[29] 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

[27]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.

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

[29]In this analysis, we use Medicaid enrollment data for all individuals starting with the year of their ACS interview. However, since we have complete enrollment information for each year, we exclude the $\gamma \mathbf{I}(j = t)$ term from equation (1).

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; i.e., 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.[30]

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.[31] We also explore the impact of the expansion on insurance coverage using two 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 datasets that include state identifiers and construct analytic samples using the same sample criteria as our main sample.

The results are presented in Figure 1 and in the first five columns of Table 1. We find a large increase in Medicaid eligibility associated with the ACA Medicaid expansions with gains of between 49 and 51 percentage points during each post-expansion year, as compared to the year just prior to expansion. Consistent with many other studies of this policy,[32] 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 that on average, individuals in expansion states experience 43 additional Medicaid enrolled days per year relative to those in non-expansion 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 non-expansion 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 impacts of the Medicaid expansions on coverage using the

[30] See, e.g., Boudreaux et al. (2016); Brown et al. (2018); Currie et al. (2008); Goodman-Bacon (2016); Thompson (2017); Wherry and Meyer (2016); Wherry et al. (2017) for evidence of such long-term effects.

[31] To assess comparability with our estimates based on administrative data, we also 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 under-report of Medicaid enrollment in survey data (Boudreaux et al., 2015). See Appendix Figure A1 and Appendix Table A3.

[32] E.g., Buchmueller et al. (2016); Cawley et al. (2018); Courtemanche et al. (2017); Miller and Wherry (2017, 2019); Sommers et al. (2015).

NHIS, with decreases ranging from 4.9 to 9.5 percentage points across the post-expansion years, and an average decline in the post-expansion period of 5.8 percentage points (see Appendix Figure A1 and Appendix Table A3).

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.[33] Our sample in the HRS is much smaller than the ACS-mortality panel (N=1,359 vs. 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 post-expansion period relative to respondents in the non-expansion states. This estimate is imprecise and only significant at the 10 percent level, albeit of a similar magnitude to the cumulative change in Medicaid coverage that we document in both the HRS (see Appendix Table A3 and Appendix Figure A1) and the administrative CMS data.

Taken together, our first stage analysis indicates that there were large and significant impacts 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.

4.2 Mortality

The previous section established that the Medicaid expansions had a meaningful impact on eligibility, enrollment, and coverage for our sample. We now examine the impact of this expansion on mortality. Our estimates of equation (1) are presented in Figure 2 and in the sixth column of Table 1. Prior to the ACA expansion, mortality rates trended similar across the two groups: pre-expansion event study coefficients are close to zero and not statistically significant. Starting in the first year of the expansion (year 0), we observe mortality rates decrease significantly among respondents in expansion states relative to non-expansion states. The coefficient estimated in the first year of expansion (year 0) 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.[34]

The immediate impact of the Medicaid expansion on mortality mirrors several analyses that find

[33] Waves of the HRS occur every other year, and the survey asks respondents about their current insurance status and their insurance status since the last wave. We use respondents to the 2008 through 2018 HRS waves to determine the total number of years of continuous insurance coverage. For respondents that join the HRS after 2008, we measure the cumulative amount of coverage they report starting with the first year of data available. We include fixed effects for the year the respondent joins the HRS in our analyses. In addition, since the HRS does not ask about the exact dates of coverage, we use questions about whether there were spells of uninsurance since the last wave to determine whether coverage was continuous across waves. Because the post-period in the HRS is one year longer than in our other analyses, we scale the estimates to represent a similar length of time. See Appendix Section D for further details on the HRS analysis.

[34] 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, 0.032 percentage points; in year 2, 0.021 percentage points and in year 3, 0.046 percentage points.

sizable changes in health care 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 et al. (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 within 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 to 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 percent reduction in year 0 to a 11.9 percent reduction in year 3.[35]

The top panel of Table 1 shows the difference-in-differences estimate that pools all post-expansion 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 percent relative to the sample mean, or 8.1 percent relative to our estimated counterfactual mortality rate during the post period of 1.63 percent.[36]

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, and so we consider this analysis to be exploratory in nature. We examine deaths due to non-disease related (i.e. "external") and disease-related (i.e. "internal") causes separately. We also 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 et al., 2014; Sommers, 2017).

These results are presented in Table 2. We observe similar patterns for internal mortality and health care amenable mortality as we do in our main results, with relative decreases beginning in the first year after the expansions occur. The event study coefficients are not statistically significant for health care amenable mortality, and are significant at the $p < 0.10$ level for deaths from internal causes; however, we find a highly significant reduction in deaths related to internal causes under the difference-in-differences model. In contrast, mortality from external causes, which may be less affected by insurance coverage, does not appear to decrease after the expansions. The point estimates on the event study indicators are not statistically significant and the difference-in-differences estimate is only significant at the 10 percent level. The estimate is positive in sign, although we note that there is a slight upward pre-trend in these deaths in expansion states relative to non-expansion 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 Appendix Table A4. For most

[35] As described earlier, we estimate a counterfactual mortality rate that takes into account both 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 post-expansion mortality rate in expansion states in that period and the corresponding event study coefficient estimating the mortality reduction effect of Medicaid for that period. The counterfactual rates are 1.28 percent in year 0, 1.51 percent in year 1, 1.60 percent in year 2, and 1.75 percent in year 3. We are then able to express our reduced form effects as a 7.0 percent reduction in year 0, 7.9 percent reduction in year 1, 8.2 percent reduction in year 2, and 11.9 percent reduction in year 3.

[36] The counterfactual mortality rate is calculated as the average annual mortality rate observed in the in expansion states in the post-expansion period (1.494 percent) plus the reduced form mortality difference-in-differences estimate (0.1320pp).

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 percent level. The point estimates suggest that cardiovascular disease might account for approximately 38 percent of the overall reduction in internal mortality, with endocrine and metabolic diseases accounting for another 18 percent. 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 impact on diseases related to the skin and subcutaneous tissue, for which diabetes is an important risk factor (Ki and Rotstein, 2008).

5 Alternative Specifications and Robustness Checks

5.1 Staggered Treatment Timing with Heterogeneous Treatment and Dynamic Treatment Effects

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. This is a potential concern both for the interpretation of the event study estimates and the validity of any test for differential pre-trends between expansion and non-expansion states.

To further explore the sensitivity of our event study estimates to heterogeneous treatment effects, we undertake two additional analyses. First, we re-run the event study analysis and limit the sample to states that expanded in 2014 (which comprise 82 percent of expansion state residents in our study sample) and states that did not expand during the study period. These results are reported in panel (a) of Appendix Figure A2, with our main results plotted in grey for comparison purposes. Limiting our analysis to this set of states, and thereby focusing on the primary treatment cohort, leads to very similar results. We continue to find no evidence of differential pre-trends and diverging mortality trends after Medicaid expansion, although the estimate in the first year of expansion is only significant at the 10 percent level. Our difference-in-differences estimate is also similar when limiting the sample to 2014 expanders (row 2, Appendix Figure A3, with our baseline difference-in-differences estimate in the top row and the size of the point estimate indicated by the dotted line).

Second, we implement the alternative estimation method for estimating dynamic treatment effects proposed in Sun and Abraham (2020), which is robust to variation in treatment effects across cohorts. We estimate event study coefficients separately for each expansion timing cohort and aggregate these coefficients using the fraction of the treated sample in each group for the relevant period as weights. We report these results in panel (b) of Appendix Figure A2.[37] The results using this method are very

[37] We constructed the estimates and standard errors following the methods outlined in Sun and Abraham (2020) with the aid of their replication code.

similar to our main results and we continue to find no evidence of differential pre-trends. We conclude from this analysis that our results are not overly sensitive to any differences in treatment profiles across cohorts.

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 2x2 DD comparison in the two-way fixed effects DD estimate.[38] We find that only 11 percent 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 than our main DD estimate (see Appendix Table A5).

5.2 Further Investigating Differential Pre-Trends

Next, we consider the potential impact of differential linear pre-trends in expansion and non-expansions states. While we do not find any evidence of differential pre-trends 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 pre-trend we are powered to detect following the procedure described in Roth (2019). Our analysis suggests that we could detect 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. a pre-trend of such size is likely to generate at least one statistically significant pre-period event study coefficient).[39] 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.[40] We estimate an effect in year 3 that is more than 2.3x larger, of -0.2082 percentage points. Under this "worse case scenario" that the largest non-detectable pre-trend is present, the true effect in year 3 might be only -0.11933 percentage points, which still represents a substantial (8.5 percent) reduction in mortality relative to the sample mean.

Next, we explicitly allow for differential linear trends in the model. We do this in three different ways. In the first, we estimate an alternative version of the model that allows for differential pre-trends in expansion and non-expansion states by replacing the pre-expansion event study coefficients with a linear trend in event time for expansion states, as in, e.g. Dobkin et al. (2018) and Gross et al. (2020).

[38]We implement this decomposition following the methods outlined in Goodman-Bacon (2019) and based on the code in Goodman-Bacon et al. (2019).

[39]We estimate that we can detect a negative linear trend of this size or greater with 80 percent power. With 50 percent power, we can detect a negative linear trend of 0.01774 percentage points or greater (in absolute terms). These rejection probabilities are calculated under the scenario that the research design is rejected if any of the pre-period event study coefficients is statistically significant at the 5 percent level.

[40]We calculate the bias following the formula presented in Roth (2019), which takes into account the additional bias introduced by passing a pre-test.

Specifically, we estimate

$$Died_{isjt} = \theta Expansion_s \times (t - t_s^*) + Expansion_s \times \sum_{y=0}^{3} \beta_y I(t - t_s^* = y) \tag{2}$$
$$+ \beta_t + \beta_s + \beta_j + \gamma \mathbf{I}(j = t) + \epsilon_{isjt}$$

where $Expansion_s \times (t - t_s^*)$ is a linear trend in event time for expansion states. Including this term allows us to measure how mortality evolved in expansion states after policy implementation while taking into account any pre-existing differential trend across expansion and non-expansion states.

The estimates of this specification are reported in Appendix Table A6. We find very similar post-expansion coefficients after allowing for and modeling a pre-existing trend. In addition, the estimated trend is not statistically significant and extremely small (only -0.004 percentage points), indicating that expansion and non-expansion states were trending very similarly prior to the ACA and diverged only after the implementation of the Medicaid expansion. Note that this estimated trend is less than 1/8th the size of the "worst case scenario" trend that was potentially undetectable in the event study framework, as discussed above.

In our second approach, we estimate a version of the model that includes state-specific linear pre-trends. We estimate these trends using observations from the pre-period only (2008 to 2013) and generate the predicted values for all observations. We then subtract the predicted value from our outcome variable and estimate our regression equation using this transformed outcome variable, such that our model measures changes from the pre-policy trend. This approach follows the two-step procedure proposed by Goodman-Bacon (2019) for estimating pre-trends. The result from this approach is presented in the third row of Appendix Figure A3 and is similar to our main model (estimated reported in row 1).

In the third approach, we estimate a model that controls for local trends in mortality among counties with different demographic and economic characteristics. We do this 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 trends does not appreciably affect our estimate, as seen in the fourth row of Appendix Figure A3.

Finally, we also consider the presence of non-linear differential pre-trends. We use the methods outlined in Rambachan and Roth (2019) and the R package `HonestDiD` (Rambachan and Roth, 2020) to examine the sensitivity of our estimates to non-linear differences in trends. For year 3 following the expansion, we estimate that the "breakdown" value of the degree of non-linearity (i.e. change in the differential slope from period to period) at which we can no longer reject the null hypothesis is 0.0035 percentage points. This corresponds with allowing for a change in the differential slope from period to period of nearly the same magnitude as the linear pre-trend we estimate in our data; i.e., in a worst case, allowing a cumulative differential slope between period -1 and 0 to be 2x the size of the estimated linear pre-trend, 3x between periods 0 and 1, 4x between periods 1 and 2, etc. While we are unable to rule this out, it seems unlikely based on the observed changes in the differential slope during the pre-period event times, which did not accumulate in one direction or the other. This suggests to us that our results are reasonably robust to unrelated deviations in the two groups relative to what we might expect based on the pre-expansion data.

5.3 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 impact of their inclusion on our estimates. The difference-in-differences estimates are presented graphically in Appendix Figure A3. We also include event study versions for these alternative specifications with our original estimates depicted in grey for comparison (see Appendix Figure A4).

We first assess whether including controls for changes in economic factors meaningfully affects our results. In the fifth row of Appendix Figure A3, we control for predicted changes in labor demand at the county level. We predict county-level labor demand for each industry using the 2008 industry employment share at the county level and applying the national growth in employment in that industry in each year (as in Bartik, 1991). We then aggregate this predicted labor demand in each industry up to the county-level to produce predicted total labor demand for each county by year relative to the 2008 base year. Our estimate is essentially unchanged with the inclusion of this variable. Row 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.

One might also be concerned that 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 sixth row of Appendix Figure A3. 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.[41] The inclusion of these opioid policy controls does not have a large effect on our estimated impact of the Medicaid expansions on mortality. Since we know from existing research these policies can have important effects on opioid misuse (see, e.g. Alpert et al., 2019; Bachhuber et al., 2014; Buchmueller and Carey, 2018; Powell et al., 2018), the fact that the inclusion of these controls does not drastically change our estimate of the impact of the Medicaid expansion on mortality is reassuring.

In row 7 of Appendix Figure A3, we include a control for trade shocks that have been linked to mortality (Autor et al., 2019; Pierce and Schott, 2020), and specifically to the rise in "deaths of despair" (e.g. drug and alcohol poisoning and suicide) over this time period (Case and Deaton, 2015, 2017). Specifically, we allow for counties with different exposure to trade from China (i.e., the "China Shock" described in Autor et al., 2013, 2019) to have different time trends by interacting the commuting-zone level exposure to Chinese imports per worker from 2000 to 2014 from Autor et al. (2013) with year fixed effects. Note that these measures are not available for Alaska and Hawaii, so we drop these two

[41] We draw heavily on Alpert et al. (2019) to identify these opioid related policies, following the sources and coding outlined in their paper. See Appendix Section E for additional details.

states when estimating this specification. The inclusion of this control variable does not impact our mortality estimate.

Finally, in row 8, we control for the demographic composition of our sample by adding individual covariates for race, age, and gender to our model. Once again the results are largely unchanged by the inclusion of these covariates. We then estimate a model (in row 9) that includes all of the controls described above. We continue to find a negative and statistically significant impact 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 baseline 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 re-estimate our model for the subgroup who are the ages of 55-61 in 2014, who do not qualify for Medicare during the post period. The results from this analysis may be found in Appendix Table A7; the estimates are very similar to those for our main sample.[42]

5.4 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 pre-expansion period, might lead to spurious results. We construct the data in the same fashion as our main analysis, but use mortality data from 2004 to 2013 for ACS respondents 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 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 3.[43] 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 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 Appendix Figure A5, with the vertical lines indicating the 5th and 95th percentiles of this "placebo" distribution. This exercise allows us to further investigate the

[42]We find a significant reduction in mortality in this group that is only slightly smaller in levels than the overall result documented; the reduction in mortality when compared to the counterfactual mortality rate in this sample is very similar. For those ages 55-61, mortality rates fell by 7.7 percent relative to their counterfactual mortality rate, similar to the 8.1 percent reduction we estimate in our main sample.

[43]Since the ACS only began collecting data on health insurance in 2008, the analysis for Medicaid coverage is limited to the 2008-2013 years.

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 next 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.[44] We re-estimate equation (1) for this sample and the results are presented in the second panel of Figure 3. As predicted, we observe no effect of the Medicaid expansions on Medicaid coverage for this group.[45] We also see no effect of the expansions on mortality rates for this group.

We also examine individuals ages 55 to 64 in families earning 400% FPL or greater at the time of the ACS interview. This group should be less affected than our main sample of low income or low education respondents. However, they may still gain Medicaid coverage under the expansions due to changes in income over time, or if their income is reported with error. As seen in the third row of Figure 3, we do find small but statistically significant increases in Medicaid enrollment 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 coverage on mortality. One interesting note is those reporting higher incomes in the survey, but who later enroll in Medicaid, exhibit very high average mortality compared to those who do not enroll in Medicaid. This indicates that those 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).[46]

Finally, we 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. To estimate this model, we include respondents in each placebo group in our sample as well as an indicator that the respondent is in the main sample, rather than the placebo group. We fully interact this "main sample" indicator with the state and year fixed effects, and include state by year fixed effects in the model. The coefficients on the three way interaction between $MainSample_i$, $Expansion_s$, and the event year indicators captures the difference in the change in mortality in the main sample in expansion states relative to the placebo sample, as compared to that same difference in the non-expansion states. The advantage of this specification is that it controls for all state-year changes during the time of expansion that impacted both the treatment and placebo groups similarly.

We plot these triple difference event study coefficients in Appendix Figure A6. As illustrated by this figure, we see little evidence of differential pre-expansion trends in either mortality or Medicaid coverage within the year across both models. After the expansion, we observe significant increases in Medicaid coverage and reductions in mortality rates, although the increase in coverage is somewhat smaller in

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

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

[46] 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 in size than for our main sample. We believe this reflects a larger role of adverse selection for the higher-income group.

the triple difference model that uses the higher income group. The triple difference estimates, reported in the bottom two rows of Figure A3, are slightly smaller than our main results but remain statistically significant. The smaller size of the estimates suggests that there may be some treatment effect (as suggested in Figure 3 for those in high income households) or modest spillover effects occurring in the placebo groups.[47] It could also be the case that this analysis better controls for state-year changes that are unrelated to the Medicaid expansions. However, it is important to note that the confidence interval for these triple difference estimates includes our main estimate, and that the triple difference estimates are still consistent with a large causal impact of the ACA expansions on mortality.

5.5 Alternative Methods for Conducting Inference

Next we examine the sensitivity of our results to alternative approaches for conducting inference. In our main analyses, we cluster our standard errors at the intervention (state) level. However, there is some noticeable spatial clustering in the states expanding and not expanding Medicaid. Many of the states expanding Medicaid are located in the west or northeast areas of the country, while non-expansion states are mainly located in the midwest and southern parts of the U.S. This spatial clustering may be a concern for the analysis if any of these groups of states experienced common shocks that are not accounted for with standard errors clustered by state.

To assess the sensitivity of our results to the assumption that all states have independent shocks, we performed a couple of additional analyses designed to allow for spatial correlation in error terms across states. First, we clustered our standard errors by Census division. The nine Census divisions are groupings of states in different geographic areas, which are correlated with Medicaid expansion status. Due to the small number of Census divisions, we also present the results when we implement this using a Wild cluster bootstrap procedure. The results from this analysis may be found in Appendix Table A8. We continue to find a significant reduction in mortality associated with the Medicaid expansions, with conventional clustered errors implying a p-value of 0.026 and the wild cluster bootstrap procedure producing a p-value of 0.051.

The second approach was to implement a model that accounts for spatial correlation between errors using the method proposed by Conley (1999). Specifically, we assume that the error for individuals in each county is correlated with those for all individuals in counties that are located within a radius of 500 kilometers. We assume a distance linear decay in the correlation structure, as well as a temporal decay. We compute heteroskedastic and autocorrelation consistent standard errors.[48] The results are reported in Appendix Table A8. As seen from this table, the standard errors are very similar to those in our main model.

6 Additional Analyses

We also conduct several additional analyses on different samples and subgroups to further understand the impact 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

[47] Although there is little evidence the expansions affected use of care among the elderly who were already insured (Carey et al., 2018), it did appear to reduce hospital closures (Lindrooth et al., 2018) and decrease the fraction of the elderly living with uninsured relatives (Borgschulte and Vogler, 2020), suggesting that such spillover effects may be plausible.

[48] We use the `acreg` Stata package developed by Colella et al. (2019) to estimate this model. Due to its computational intensity, we first aggregate the data to the county level prior to running the estimation.

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 Appendix Table A9, we also estimate the impact of Medicaid expansion on mortality for individuals who meet our sample inclusion criteria but are ages 19 to 64 in 2014. 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 percent decrease relative to the counterfactual rate. We next 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, however, to rule out meaningful declines in mortality for any age group.

Next, we further probe heterogeneity by age within our main age group age interest, adults age 55 to 64 years old in 2014, and at older ages. In Figure A7 we report the percent reduction in mortality associated with the Medicaid expansions in 3-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 both 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 would have been Medicare eligible.

We next 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 Appendix Table A10. We find evidence of larger mortality effects for white, non-Hispanic adults when compared to other racial and ethnic groups. We also find larger effects for the males 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. And, when we narrow the socioeconomic criteria to either less than a high school degree or less than 138 percent of the FPL, we find overall very similar first stage and mortality estimates for the two groups.

Finally, an additional analysis limits the main sample to approximately a 30 percent subset who reported being uninsured at the time of the survey. As this group is younger on average,[49] the counterfactual mortality rate is slightly lower than in the overall sample, at 1.46 percent per year (vs. 1.63 percent in the sample overall). The point estimates indicate somewhat larger decreases in mortality for this group of 0.150 percentage points (or 10.3 percent of the counterfactual rate) compared to the reduction in the main sample of 0.132 percentage points (or 8.1 percent 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

[49] We estimate that the average age of uninsured respondents in our sample is 59.0 in 2014, compared to an average of 59.4 years for insured respondents. This difference is statistically significant with a p-value of 0.0000.

experienced larger gains in Medicaid enrollment;[50] however, this may be due to the lower mortality rate in this group.

7 Interpreting the Estimates and Comparisons to Past Work

7.1 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 1). 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 example by reducing hospital closures, or improved the health of non-enrollees through other means such as increasing available resources to families that had some uninsured and some insured 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 4.1). 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 know 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 (e.g. Kenney et al., 2012; Marton and Yelowitz, 2015); this may also be exacerbated by policies designed to provide immediate coverage to those in need, as discussed earlier (i.e. presumptive eligibility and retroactive coverage). 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 estimate annual mortality for enrollees in the expansion states during the post-period at 2.0 percent. We consider this an upper bound because the CMS enrollment

[50] 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 Appendix Table A11). Due to 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.

data does not identify the eligibility pathway for coverage and some of these individuals likely qualified for coverage even if there were no eligibility expansions (e.g. if they became disabled);[51] we expect that this group likely have 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. 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 experience 2.7 additional years of coverage due to the expansions, their counterfactual mortality rate would therefore be 2.95 percent ($2.0 + 2.7 \times 0.35$).

Given these calculations, we may therefore expect the average annual mortality rate among the compliers to fall somewhere in the 1.63 to 2.95 percent range. Combined with our estimated treatment effect of a 0.35 percentage point reduction in mortality associated with 1 year of accumulated Medicaid enrollment, this indicates that one year of Medicaid coverage reduces individual mortality by between 11.9 and 21.5 percent. Meanwhile, our estimate of the treatment effect for contemporaneous Medicaid enrollment (1.03pp) indicates a resulting reduction in individual mortality of between 34.9 and 63 percent.

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 impact 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 1) implies much larger treatment effects, ranging from 127 to 184 percent 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 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.[52] 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 et al., 2019), which may be important for individuals with complex health needs and high mortality risk. Alternatively, this calculation may miss other spillover effects among Medicaid non-recipients, or the presence of cumulative effects of insurance coverage that are not captured in contemporaneous coverage measures.[53] Finally, it is important to note that the confidence interval of this estimated effect includes much smaller reductions in mortality.

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

[52] 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).

[53] 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 14 and 21 percent. However, these estimates are fairly imprecise.

7.2 Further Interpretation

In addition to comparing the observed change in mortality to the changes in insurance status in our population, it is also useful to assess the plausibility of our results by comparing them to the documented increases in medical care utilization under the ACA expansions. Beyond providing a benchmark for the expected magnitude of any change in mortality, such a synthesis may also provide insights into the most important mechanisms driving the mortality reduction, particularly when considered in light of the analysis of mortality by cause (Table A4).

First, a plausible mechanism underlying our mortality effects is the increased use and access to prescription drugs. Ghosh et al. (2019) find that Medicaid expansions increase the number of prescription drugs taken by about 9 additional prescriptions per year per new enrollee. Some of the types of drugs that see large increases in usage, such as medications for the management of diabetes, treatments for HIV and Hepatitis C, and drug therapies for cardiovascular disease, are associated with sizeable decreases in mortality in the medical literature.[54] The increase in prescriptions to treat these diseases often mirrors the reductions in mortality we find by disease type, in particular the large (albeit imprecise) reductions in cardiovascular disease and diabetes documented in Table A4. Therefore, if even a small fraction of the new prescriptions associated with the Medicaid expansion are effective at reducing the probability of mortality, they may account for a sizeable portion of the overall decline in mortality we observe.[55]

Similarly, Soni et al. (2018) find early cancer detection increased by 15.4 diagnoses per 100,000 patients in 2014 as a result of the ACA Medicaid expansions using a reduced form difference-in-differences model. They also find an increase of 23.3 diagnoses per 100,000 for adults ages 55-64, although this subgroup analysis is imprecisely estimated. This translates into approximately 150 new diagnoses per 100,000 adults who are income eligible for Medicaid.[56] Importantly, the authors find that the increase in early-stage diagnoses was concentrated among cancers amenable to screening. Because earlier cancer diagnosis is highly predictive of better survival (McPhail et al., 2015), some portion of these early diagnoses may have also averted deaths during our study period, contributing to the 132 fewer deaths per 100,000 we observe annually for our sample.

Researchers have also documented changes in hospital and emergency department care associated with the ACA. Wen et al. (2019) finds that hospitalizations for ambulatory-care sensitive conditions such as diabetes decreased by about 54 visits per 100,000 residents in expansion states relative to non-expansion states. Meanwhile, Duggan et al. (2019) find that the expansions were associated with increased rates of hospitalization and ED visits overall (not specifically for ambulatory care sensitive conditions). Among those age 64, they find that hospitalizations increased by 8 stays per 1,000 indi-

[54]For example, a recent review of clinical trial evidence finds that patients randomized to receive statins have all-cause mortality rates that are 14 percent lower than the untreated group (Chou et al., 2016).

[55]It is also worth noting that recent papers studying the introduction of prescription drug coverage under the Medicare Part D program also find evidence of mortality declines. Huh and Reif (2017) focus on those age 66 and find that insurance coverage for prescription drugs reduces mortality in this group by about 0.16 percentage points annually (about 9.6 percent). Dunn and Shapiro (2019) find slightly larger effects in an analysis that incorporates individuals with older ages. For both papers, reductions in mortality are driven by a decline in deaths due to cardiovascular disease. Using data for a subset of Medicare beneficiaries, Kaestner et al. (2017) find no significant effect on mortality but do document reductions in hospitalization admissions for heart disease, respiratory disease, and diabetes under the program.

[56]Using the ACS, we estimate that 15.5 percent of adults ages 55-64 in expansion states during the post-expansion period had incomes less than or equal to 138 percent of the federal poverty line.

viduals and ED visits increased by 14 visits per 1,000 individuals. This implies about 5,161 additional hospitalizations and 9,032 ED visits for every 100,000 income eligible individuals.[57] If even a small fraction of these hospital or ED visits averted mortality, they could also explain a sizeable fraction of the 132 fewer deaths per 100,000 we estimate in our study.[58]

Finally, Medicaid also increases the use of a variety of other types of medical care such as routine screening for chronic illnesses, outpatient physician visits, use of prescription drugs that aid in smoking cessation, and dental care which also have the potential to improve health over the longer term.[59] While the mortality impacts of such utilization are uncertain, it is plausible that such care could have beneficial health effects.

To summarize, the ACA Medicaid expansions increased access to and use of care along several dimensions. While it is difficult to pinpoint exactly which element of the medical care received by Medicaid beneficiaries is the most important in improving health, it is clear that the magnitude of the increase in utilization resulting from the Medicaid expansion is consistent with the mortality effect we document.

7.3 Comparisons to Previous Estimates

In this section, we compare the treatment effects that we estimate to those published in the literature. In order 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 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 percent relative to the control mean; this estimate is not statistically significant (associated with a p-value of 0.128).[60] This estimated proportional treatment effect is very similar to the estimate in our analysis (i.e. 63 percent) described in the last section, which uses the counterfactual mortality rate for the sample without any adjustment for adverse selection. We also use the administrative data from the OHIE replication kit to estimate the impact of each year of Medicaid enrollment on mortality. We find that one year of Medicaid enrollment is associated with a 61.2% reduction in mortality over the sample period. This is substantially larger than our estimated 22% reduction in individual mortality associated with one year of Medicaid coverage, although this difference may in part reflect the fact that we observe cumulative Medicaid enrollment over a longer period (4 years in our study vs 16 months in OHIE).

We can also use OHIE data to estimate the effect of insurance coverage, rather than Medicaid

[57]As in the previous paragraph, we scale by the fraction of near elderly adults who are in households under 138% of the FPL, as measured in the ACS, to arrive at these rates.

[58]There is also evidence of increases in other specific types of hospital care that may play an important role in reducing mortality, including cancer-related surgeries (Eguia et al., 2018), cardiac surgeries (Charles et al., 2017), and surgeries considered minimally invasive (Eguia et al., 2020).

[59]See, for example, Finkelstein et al. (2012); Nasseh and Vujicic (2017); Semyonov et al. (2013).

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

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 a survey approximately one year later (an average of 13 months after coverage approval, see Finkelstein et al., 2012).[61] Mirroring our own results, this exercise results in considerably larger estimates of the effect of insurance coverage. Using the gain in coverage measured in the initial survey, we find a decrease in mortality of 326 percent (p=0.071) when compared to the control group mean. Using the 12-month measure, we find a 335 percent mortality reduction (p=0.153).[62] 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.

The large size of these estimates suggests three things. First, there is likely additional adverse selection in who actually enrolled in Medicaid and experienced insurance status changes. Only 30 percent of individuals selected by the lottery successfully enrolled in the program, either because they did not submit the appropriate paperwork or no longer met the income eligibility criteria. In addition, compliers tended to be older, in worse health, and in lower socioeconomic status than the overall study population (Finkelstein et al., 2012). This indicates that the control mean is likely not the right comparison. Second, the large size of the treatment effect might also indicate that the benefits of Medicaid do not operate only through insurance status changes (i.e. Medicaid may reduce mortality even if the counterfactual is enrolling in private insurance). Third, scaling by self-reported insurance coverage at a given point in time may understate the impact of gaining Medicaid eligibility through the lottery, either because it fails to capture previous exposure to the program or because the self-reported measure is subject to measurement error.

Recent work by Goldin et al. (2020) also provides experimental evidence on the impact 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 0.166 percentage points over a two-year period, or 11.9 percent compared to their estimated complier baseline mortality rate. This implies 12 months of coverage results in a reduction in mortality over the two-year period of over 140 percent, which is much larger than our implied effects of cumulative insurance coverage using estimates from the HRS. However, the authors note that the effect may be non-linear in length of coverage, in which case such scaling could be an overestimate of the effect of a full year of coverage.

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

[61] It may be preferable to scale mortality changes by the total number of months of insurance coverage experienced by the treated group, rather than insurance at the time of the survey. Information on cumulative insurance coverage over the 16-month period is not available, however, so we report both of the available measures for point-in-time coverage. Also, it is important to note that while the gains in insurance coverage are measured at these two points in time (either immediately after the lottery or 12 months later), we are scaling mortality over the entire 16-month period covered by the OHIE administrative data.

[62] Note that baseline mortality for the control group is lower among survey respondents since participants had to be alive in order to respond to the survey.

counterfactual mortality rate; we therefore apply a consistent set of adjustments across these studies. We also present 95 percent confidence intervals for these estimates. Further details on the approach used to construct these re-scaled estimates and their confidence intervals may be found in Appendix Section F.

The resulting estimates are reported in 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 percent among individuals gaining insurance coverage. Similarly, taking advantage of county-level variation in the impact of the Massachusetts health reform of 2006, Sommers et al. (2014) find that insurance coverage reduces mortality by 27.3 percent 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 percent reduction in mortality under the ACA Medicaid expansions. In our analysis, we find that new insurance coverage reduces mortality by 50 percent 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 percent for adults ages 25-64 under his analysis of the ACA Medicaid expansions.[63]

Fewer papers include estimates for the age group that we study.[64] Black et al. (2019) and Chen (2019) find smaller implied mortality effects of individual insurance coverage, when compared to our study. Although, the estimate in Black et al. (2019) 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 very large effects, suggesting that reductions in mortality associated with insurance are double or even triple the average mortality rate of the control group. Such large effects may be due to adverse selection, i.e. 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 percent), reductions in mortality associated with coverage. Our estimates are smaller than those documented in the experimental literature but somewhat larger than those reported in quasi-experimental studies.

8 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 first 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.

[63] Swaminathan et al. (2018) find that the Medicaid expansions generated large reductions in mortality among patients with end-stage renal disease; however, given the high health risk of that population, it is unclear whether those estimates are directly comparable to those discussed here. We include the estimates in Appendix Table A13, however.

[64] While some studies present subgroup analyses by age, they do not include the corresponding first stage estimates needed to estimate the implied individual treatment effect.

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 paper, 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 mis-reporting 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 sizeable gains in terms of the number of lives saved under Medicaid expansion. Since there are about 3.7 million individuals who meet our sample criteria living in expansion states,[65] our results indicate that approximately 4,800 fewer deaths occurred per year among this population due to Medicaid expansion, or roughly 19,200 fewer deaths over the first four years alone. This calculation relies on the assumption used throughout the paper; namely, that in the absence of the ACA expansions, mortality in expansion and non-expansion 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 non-expansion states would be similar to those observed in the expansion states. The change in mortality in non-expansion states if they were to adopt the expansion could be larger (e.g., if the beneficiaries in the non-expansion 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 non-expansion states). Given these caveats, we estimate that, as there are approximately 3 million individuals meeting this sample criteria in non-expansion 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.

[65] Authors' calculation using the public-use ACS.

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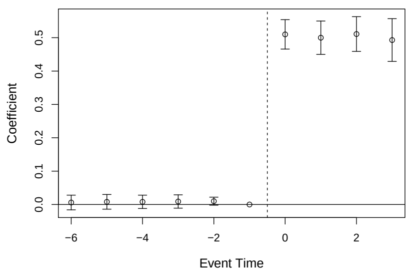
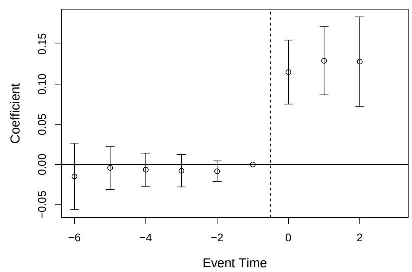
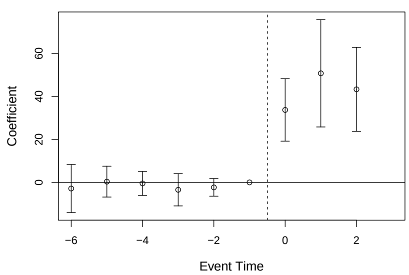
Figure 1: Effect of the ACA Medicaid Expansions on Eligibility and Coverage



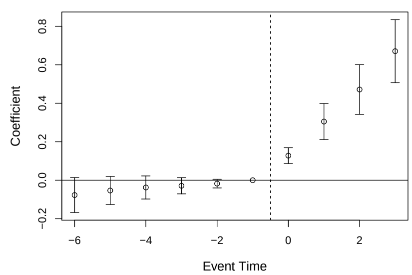
(a) Medicaid Eligibility (ACS)



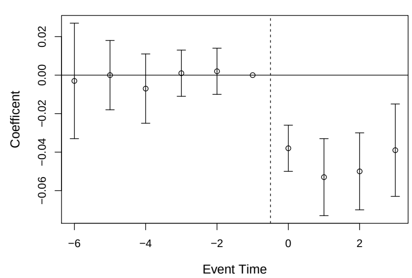
(b) Any Medicaid Enrollment in Year (CMS)



(c) # Days Medicaid Enrollment in Year (CMS)



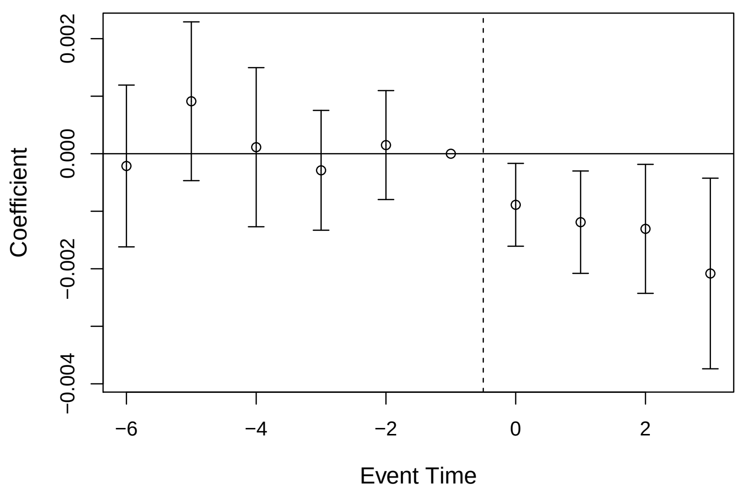
(d) Cumulative Years of Medicaid Exposure (CMS)



(e) Uninsured (ACS)

Note: 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 figures. The coefficients represent the change in outcomes for expansion states relative to non-expansion states in the six years before and four years after expansion, as compared to the year immediately prior to the expansion. The sample is defined as U.S. citizens ages 55-64 in 2014 who are not SSI recipients and who have either less than a high school degree or family income below 138% FPL. See Section 4.1 for additional details on the analysis and Appendix Section B for information on the Medicaid eligibility determination.

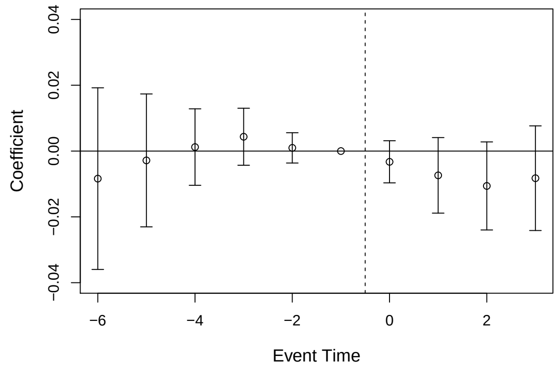
Figure 2: Effect of the ACA Medicaid Expansions on Annual Mortality



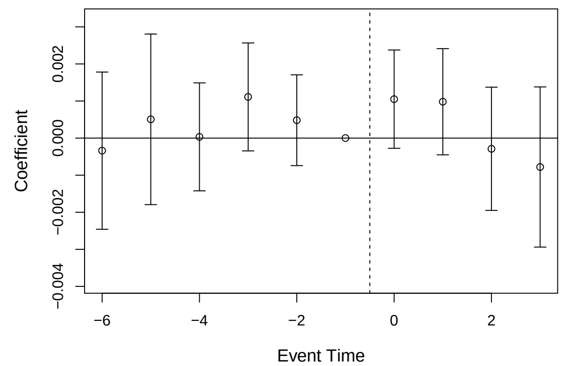
Note: This figure reports coefficients from the estimation of Equation 1 for annual mortality. The coefficients represent the change in mortality for expansion states relative to non-expansion states in the six years before and four years after expansion, as compared to the year immediately prior to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

Figure 3: Placebo Tests

Pre-ACA Years

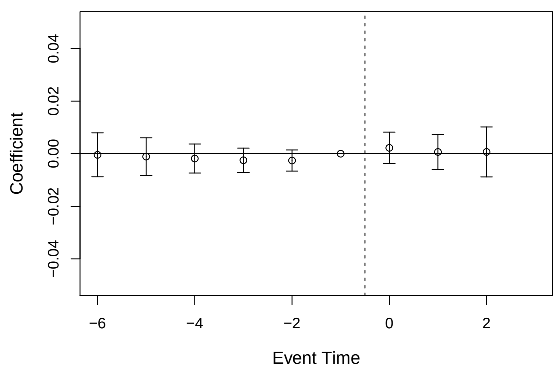


(a) Medicaid Coverage

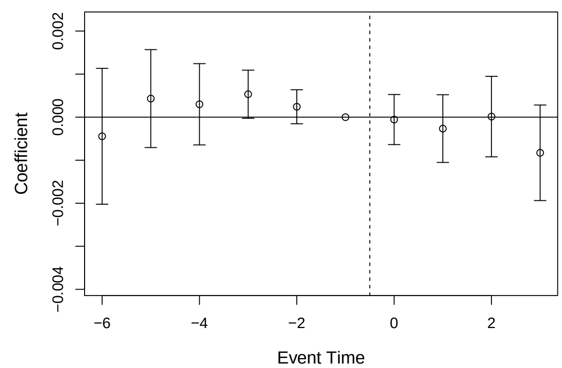


(b) Annual Mortality

Age 65+ in 2014

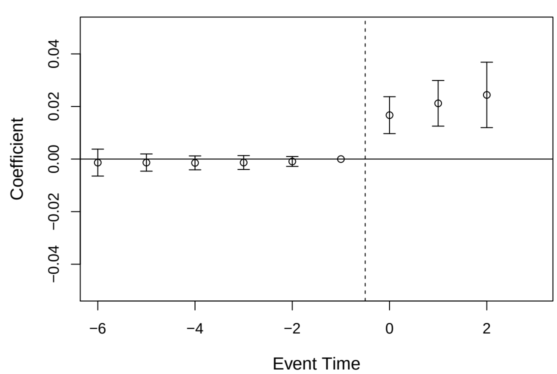


(c) Medicaid Coverage

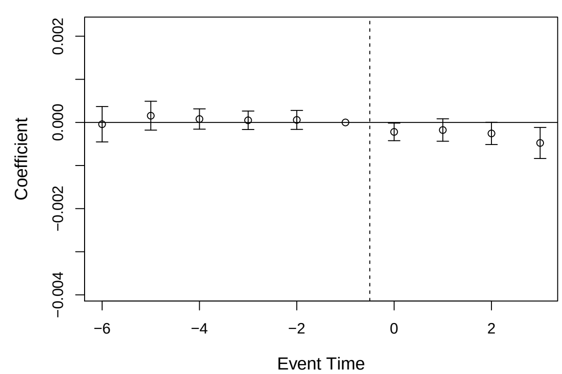


(d) Annual Mortality

Income 400%FPL +



(e) Medicaid Coverage



(f) Annual Mortality

Note: Row 1 figures plot coefficients from equation (1) using pre-ACA years of data. Row 2 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. Row 3 presents estimates for individuals in higher income families who were less likely to gain Medicaid coverage. See text in Section 5.4 for additional information.

Table 1: Impact of the ACA Expansions on Coverage and Mortality: Difference-in-Differences Estimates

	Medicaid Eligibility (1)	Medicaid Coverage in Year (2)	Days of Medicaid in Year (3)	Cumulative Medicaid Years Experienced (4)	Uninsured (5)	Died in Year (6)
Difference-in-differences Model:						
Expansion × Post	0.498 (0.026)***	0.128 (0.0201)***	42.99 (8.892)***	0.375 (0.061)***	-0.044 (0.010)***	-0.00132 (0.00050)**
Event Study Model:						
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 x 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). 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 text for more details. Significance levels: *=10%, **=5%, ***=1%.

Table 2: Impact 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)
Difference-in-Differences Model:			
Expansion × Post	-0.00235 (0.00675)***	-0.00099 (0.00050)*	0.00038 (0.00020)*
Event Study Model:			
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 x 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 #: CBDRB-FY19-310. Significance levels: *=10%, **=5%, ***=1%.

Medicaid and Mortality: New Evidence from Linked Survey and Administrative Data

Appendix

Sarah Miller   Norman Johnson   Laura R. Wherry

A Additional Results

We present additional figures and tables discussed in the main text in this section in Figures A1-A7 and Tables A1-A13. See the main text for further discussion of these results.

B First Stage Eligibility Estimates

To estimate the change in Medicaid eligibility associated with the ACA Medicaid expansions, we use the 2008-2017 ACS downloaded from IPUMS USA (Ruggles et al., 2019) and impute eligibility for our sample using state eligibility rules for each year. We consider eligibility for low-income parents under Medicaid Section 1931 criteria in each state, as well as expanded eligibility for parents and childless adults under waiver programs that offered comparable coverage to the ACA Medicaid expansions. We do not consider expanded programs that cover a more limited set of services and follow documentation from the Kaiser Family Foundation (KFF) to make this determination.

Information on state eligibility thresholds for coverage for adults were compiled from the sources listed in Table A14. The notes column in the table provides a record of any decisions made in applying the eligibility rules or to reconcile inconsistencies across different sources. KFF documentation on eligibility thresholds over time, which were used as our primary source, take into account state rules on earnings disregards when applicable. We defined the family unit for eligibility determination following the health insurance unit definition prepared by the State Health Access State Assistance Center, see details in State Health Access Data Assistance Center (2012). Following Medicaid rules for countable income (Centers for Medicare & Medicaid Services, 2016), we did not include family income from the Temporary Assistance for Needy Families or SSI programs in the calculation of total family income.

C Administrative Data on Medicaid Enrollment

We use longitudinal administrative records on Medicaid enrollment to document the impact of the ACA expansions on Medicaid coverage from 2008 to 2016. These data were provided to the Census Bureau by CMS and assigned a PIK at the individual level by Census using the PVS.

The data CMS collects from states changed over time with the move from the Medicaid Statistical Information System (MSIS) to the Transformed Medicaid Statistical Information System (T-MSIS). States provided data to CMS over our period of study in one of three formats. The first format, MSIS, was the original format used by CMS to collect individual level data from states. Data provided in this format was based on federal fiscal year (FY) of enrollment. The second format, T-MSIS Analytic File (TAF), follows the same fiscal year format as the MSIS files. The newest format, T-MSIS, is based on calendar year (CY).

All states except AR switched to T-MSIS by 2016. This switch generates a move from fiscal year to calendar year reporting. Most of these switches occurred in 2016. Twenty-five states provided 2016 enrollment data in the TAF format, in addition to the T-MSIS format, allowing us to observe enrollment in the fourth quarter of CY 2015, or had switched to T-MSIS prior to 2016, also allowing us to observe Q4 CY 2015 enrollment information. The states for which we have this information are AK, FL, KS, ME, MD, MT, NE, NM, ND, WI, AL, AR, CO, CT, DE, DC, GA, NV, NH, NC, RI, SC, TN, VA, and WA. For all other states, we are unable to see enrollment information for the fourth quarter of CY 2015, which would be part of FY 2016 but not CY 2016. For these states, we impute the number of days enrolled in the fourth quarter of CY 2015 as the number of days enrolled in the third quarter of CY 2015 *if* the individual is also enrolled at least one day in the first quarter of CY 2016. Our results are similar if we do not condition on Q1 CY 2016 enrollment for the imputation.

In addition to missing Q4 CY 2015 for several states, we are also missing a small number of additional state-quarters. Wisconsin switched from MSIS to T-MSIS in CY 2014 and did not provide equivalent FY 2014 enrollment information; as a result, we do not observe Q4 CY 2014 data for Wisconsin. We impute enrollment for Wisconsin in Q4 CY 2014 in the way described above. We are also missing data from Louisiana for all of CY 2015 and Q4 of CY 2014. We impute Q4 of CY 2014 for Louisiana as being the same as Q3 CY 2014 enrollment and code enrollment in Medicaid as missing for CY 2015; for measures of cumulative enrollment, Louisiana residents are given missing values for CY 2015 forward. Our results are similar if we instead drop Louisiana entirely from our first stage analysis. Finally, Arkansas did not provide any T-MSIS data during our sample period. For this state, we only have data by FY, which ends in Q3 of CY 2016. We impute Q4 CY 2016 data for AR by assuming individuals had their same enrollment as in Q3 of this same calendar year, similar to our Q4 CY 2014 imputation for Louisiana.

All versions of the CMS enrollment data provide information on whether the respondent had any enrollment in that quarter and year, and the number of days he or she was enrolled. We aggregate this data to the individual by calendar year level, summing the number of days enrolled across different states for an individual if necessary (e.g., if a respondent is enrolled in Medicaid in Florida for 30 days and in Arkansas for 30 days, we count the total number of days enrolled in that year as 60). To determine the cumulative years of Medicaid exposure for each individual in year t, we sum the total number of days of enrollment observed up to and including year t.

Finally, in order to make the time period over which we observe Medicaid enrollment comparable to that over which we observe mortality, we impute respondents' enrollment in 2017 based on their 2016 enrollment, their state of residence, age, gender, race, and whether or not they are in our main targeted sample (i.e. low income or less than high school education, citizens, not receiving SSI and between 55 and 64 in 2014). We implement this imputation by first estimating the following regression using observed information on 2015 and 2016 enrollment:

$$
\begin{aligned}
\mathit{DaysEnrolled2016}_i = {} & \beta_s + \beta_a + \beta_r + \beta_1 \mathit{DaysEnrolled2015Bin}_i + \beta_2 \mathit{DaysEnrolled2015Bin}_i \times \mathit{MainSample}_i + \\
& \beta_3 \mathit{DaysEnrolled2015Bin}_i \times \mathit{ExpState}_s + \beta_4 \mathit{ExpState}_s \times \mathit{MainSample}_i + \\
& \beta_5 \mathit{ExpState}_s \times \mathit{MainSample}_i \times \mathit{DaysEnrolled2015Bin}_i + \beta_6 Female_i + \epsilon_i
\end{aligned}
$$

where β_s are state fixed effects, β_a are age fixed effects, β_r are race and ethnicity fixed effects (non-Hispanic Black, non-Hispanic white, Hispanic, non-Hispanic other race), *DaysEnrolledBin* are indicator variables for having 0 days enrolled, 1 day to 3 months enrolled, 3 months to 6 months enrolled, 6 months to 9 months enrolled, and 9 months to 12 months enrolled, *MainSample* equals 1 if the respondent is in our primary sample composed of those with low income or less than high school education, citizens, not receiving SSI and between 55 and 64 in 2014, and *Female* equals one if the respondent is female and 0 otherwise. Note that the age fixed effects account for any age effects that may drive Medicaid enrollment (e.g. aging into eligibility for Medicare). We apply the estimated coefficients from this regression to 2016 values for each of these variables to estimate the predicted number of days enrolled in 2017 for each individual.

D Measuring Cumulative Coverage in the Health and Retirement Study

Repeated cross-sectional surveys such as the ACS and NHIS only document the fraction of respondents enrolled in a given year. However, previous research has shown that Medicaid coverage may have beneficial health effects observed even after the period of enrollment (e.g. Boudreaux et al., 2016; Brown et al., 2018; Currie et al., 2008; Goodman-Bacon, 2016; Wherry and Meyer, 2016; Wherry et al., 2017). To examine how cumulative exposure to Medicaid changed following the ACA expansions, we take advantage of panel data from the Health and Retirement Study (HRS). This study surveys respondents every two years, with new respondents added every year as older respondents leave the sample through attrition or death. We use restricted-use data from the 2008, 2010, 2012, 2014, and 2016 HRS, and the early release version of the 2018 HRS. We apply the same sample criteria as used throughout the paper to identify US citizens between the ages of 55 and 64 in 2014 who do not receive SSI and who either are in households earning under 138% of the FPL or who have less than a high school degree. We define the income and SSI receipt criteria using the first response we observe for the participant in our sample period. For example, if we first observe a respondent in 2008 and he/she does not receive SSI and meets all other sample inclusion criteria, we include him/her in the same even if in 2010 he/she reports receiving SSI. This results in 1,359 unique individuals meeting the sample eligibility criteria, or 5,573 individual by year observations.

Our main outcome variable for the analysis is the number of years of insurance coverage we observe the respondent having in our sample period until year t. At the time of interview, the survey asks the respondent about their current insurance status and their status since the date of last interview. If the respondent is currently enrolled in health insurance and experienced no uninsurance spell since the last survey, we assume the respondent experienced 2 years of coverage over the 2 year period. If the respondent is currently enrolled in health insurance but did experience a period of uninsurance since the last survey, we assume the respondent experienced 1 year of continuous coverage over the 2 year period. Finally, if the respondent is currently uninsured, we assume he experienced 0 years of continuous coverage over this 2 year period. We use these assumptions to arrive at the outcome variable, number of years with insurance coverage up until, and including, the survey year. In certain analyses, we also examine the total number of years of Medicaid enrollment, which we define using similar rules.

With these outcome variables, we estimate both an "event study" and difference-in-differences version of the model. These models include individual, state, and time fixed effects.[66] We use only 2014 expanders for the event study model and include indicators for 6 years prior, 4 years prior, the year of, two years after, and four years after the expansion. We omit the observation two years prior to the expansion, which corresponds to the 2012 wave of the HRS.[67] For the difference in differences model, we include these late expansion states.

We apply HRS survey weights to all regression models. The 2018 data is the early release version and does not yet have survey weights available; we instead use the respondent's 2016 weights for this year. Similarly, we apply the respondent's geographic information from 2016 to the 2018 data, as the geographic data has not yet been released for the 2018 survey. Finally, in order to make the cumulative coverage measures cover a comparable time period (i.e. 4 years post-expansion rather than 5 years), we scale our difference-in-differences estimates by 4/5ths.

E Sensitivity to Additional Control Variables

In sensitivity analyses, we examine whether our estimates substantially change when we include control variables related to local economic conditions, employment growth, and factors related to the severity of the opioid epidemic over our study period.

To control for local economic conditions, we use the annual average unemployment rate for each county and year from the Bureau of Labor Statistics. In addition, we control for the China shock using the measure of exposure to Chinese imports per worker defined at the commuting zone level over the 2000 to 2014 period from Autor et al. (2013) interacted with year fixed effects.

We also control for pharmaceutical policies that have been tied to opioid-related outcomes. First, we allow for differential trends in states with and without triplicate programs, pulling information on which states had programs in place from Alpert et al. (2019). Second, following Alpert et al. (2019), we include controls for other opioid related policies. We control for the enactment of state prescription drug monitoring programs (PDMP) using information collected by Horwitz et al. (2018). We also control for state adoption of mandatory access PDMPs that require physicians to access the patient's prescription history, as well as adoption of pain clinic regulations. The dates of adoption for both are taken from the Prescription Drug Abuse Policy System. Finally, we control for state legalization of medical marijuana and the legalization and operation of dispensaries. Dates of enactment for the 2008-2014 years are from Powell et al. (2018). We found detail on more recent medical marijuana legalization, including dispensary legalization, from the National Conference of State Legislatures. We also followed a similar method as that described in Powell et al. (2018) to identify the date of the first legally operating dispensary for each state. For all policies, we consider them to be in effect in a given state-year provided that the policy was in place during the first half of that year.

[66] State fixed effects are identified only off of individuals who move during the sample period since individual fixed effects are included.

[67] We exclude late expansion states since there are relatively few observations in these states and the odd event study indicators would be identified exclusively off of this small sample.

F Comparisons to Prior Estimates

In this section, we compare the effect sizes from our study to the existing literature. First, we provide new analysis of the Oregon Health Insurance Experiment (OHIE) data with a focus on the age group relevant for our study, those age 55 to 64. Second, we compile quasi-experimental estimates of the impact of Medicaid and insurance coverage on mortality to compare to those documented in the main text.

To undertake the OHIE analysis, we downloaded the replication kit from https://www.nber.org/programs-projects/projects-and-centers/oregon-health-insurance-experiment/oregon-health-insurance-experiment-data. To match the age of our sample, we define age in 2008 (the year of lottery assignment) as 2008 minus the participant's birth year as recorded in the lottery list data. We then restrict the sample to those at least age 55 in 2008 (the maximum observed age in 2008 is 63). We estimate both the "reduced form" effect of being selected by the lottery on mortality, as well as an IV estimate that instruments for whether the participant was ever enrolled in Oregon Health Plan (OHP) standard using the indicator that the participant was selected by the lottery. Following Finkelstein et al. (2012), we include fixed effects for the number of household members entered in the lottery and the lottery draw associated with the participant's entry. We also conduct a similar rescaling that uses the months enrolled in OHP standard from the match notification date until September 30, 2009, as derived from the OHP admin data, divided by 12, to produce a mortality effect per year of enrollment. The first stage for this analysis indicates that lottery winners in this age group were 25.6 percentage points more likely to ever enroll in OHP Standard, and that they experienced 3.53 additional months of enrollment on average, relative to those who were not selected by the lottery. The reduced form and IV estimates are reported in rows 1 and 2 of Table A12.

In addition to analyzing the OHP administrative data, we also conduct an analysis of the survey data collected by the Oregon Health Insurance Experiment research team. We examine both the initial and 12-month surveys. Results from the 6-month survey were similar, but we did not include those results due to the small sample sizes. In this analysis, we examine 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 a survey approximately one year later (an average of 13 months after coverage approval, see Finkelstein et al., 2012). We use the change in insurance status documented at these two time periods to scale the reduced form change in mortality we observe among survey respondents who were or were not selected by the lottery to enroll in OHP. Note that while insurance coverage is measured at these two points in time (either one month after the lottery or 12 months later), we continue to measure mortality over the entire 16-month period covered by the OHIE administrative data. We conduct this analysis applying the relevant weights and including survey wave and survey wave by household size fixed effects, following the original analysis. The first stage indicates that at the initial survey, lottery winners are 11.3 percentage points more likely to have any insurance coverage and, at the 12-month survey, are 17.9 percentage points more likely to have any insurance coverage, relative to survey respondents not selected by the lottery. The reduced form and IV estimates of these analyses are reported in the bottom two rows of Table A12.

Next, we compare the effect sizes from our study to those documented in previously published quasi-experimental analyses. This exercise is inspired by a similar re-scaling of quasi-experimental estimates undertaken by Goodman-Bacon (2018), which includes mortality effects of Medicaid observed for infants and children.[68] Here, we focus on studies of changes in all-cause mortality under the ACA Medicaid expansions or similar insurance expansions for low-income adults.

When comparing our estimates to prior evidence on the mortality effects of Medicaid expansion, it is helpful to translate the ITT estimates into average treatment effects since there is variation in the magnitude of the policy changes studied in this literature. Scaling ITT estimates into their implied individual treatment effects allows us to compare the magnitude of the mortality reduction for the newly enrolled or insured. Since there are also differences in the baseline mortality of the populations studied, we also convert existing estimates into proportional effects. This presentation allows for the effect sizes to be compared more easily across these different policy environments.

We examine estimates for adults ages 55-64 when available, which is our primary age group of study, but we also examine estimates for all nonelderly adults. We calculate the implied effects for both new Medicaid enrollees and the newly insured whenever possible by dividing the ITT effects reported in each paper by the corresponding changes in Medicaid and insurance status.[69] In cases where the change in Medicaid enrollment is derived from survey data, we apply an adjustment for under-reporting of Medicaid coverage that we estimate from linked survey and administrative data on Medicaid coverage through the National Health Interview Survey via the public-use NCHS-CMS Medicaid Feasibility Files. We calculate a survey undercount of 31.4% and apply this in the calculations described in this section.[70]

As described above, we convert the estimates into proportional mortality effects using the reported baseline mortality rate. For studies that use aggregate mortality data (rather than data for poor adults), we apply an adjustment that multiplies the general population mortality estimate by 1.6 to account for the higher relative risk of death for poor adults. We calculate this adjustment using the 2007 to 2012 NHIS linked mortality files and its corresponding survey weights. It equals the ratio of the fraction respondents with incomes less than or equal to 138% FPL and of ages 19-64 who die in the year following the survey and the fraction of respondents ages 19-64 of all income levels who die in year following the survey.

These calculations inherently assume that the baseline mortality rate for poor adults is similar to the baseline mortality rate for individuals newly gaining coverage under the Medicaid expansions (i.e. the "compliers"). It is likely the to be the case, however, that the mortality rate for these individuals is higher, given the evidence for the presence of adverse selection in insurance coverage decisions. If so, the average treatment effect estimates presented here may be too large. We discuss this further in Section 7 in the text.

[68] Goodman-Bacon (2018) also includes estimates of adult mortality under the pre-ACA Medicaid expansions in AZ, ME, and NY presented in Sommers et al. (2012). In our analysis here, however, we focus on newer estimates of the mortality effects under these expansions in follow-up work by Sommers (2017).

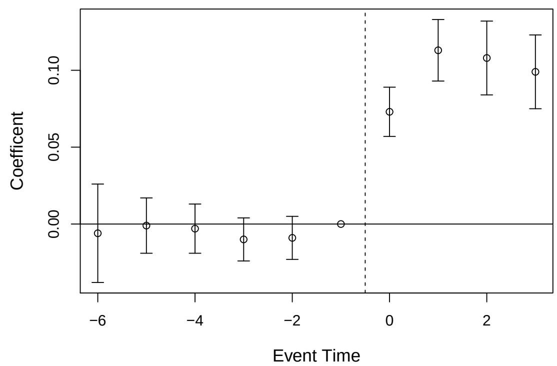
[69] For this reason, we only include studies that present estimates for first stage effects on Medicaid or insurance coverage, in addition to reduced form mortality effects.

[70] This estimate is from an analysis included in an earlier version of this paper for individuals meeting our sample criteria using data available from the 2008 to 2012 NHIS for respondents linked to administrative data on Medicaid enrollment. These data are available from the National Center for Health Statistics for NHIS respondents who consent to the linkage. We found that 15.7 percent of the sample reported being enrolled in Medicaid at the time they completed the survey, 22.9 percent were actually enrolled at some point during the year according to the CMS adminstrative records; this suggests an undercount based on survey data of approximately 31.4%.

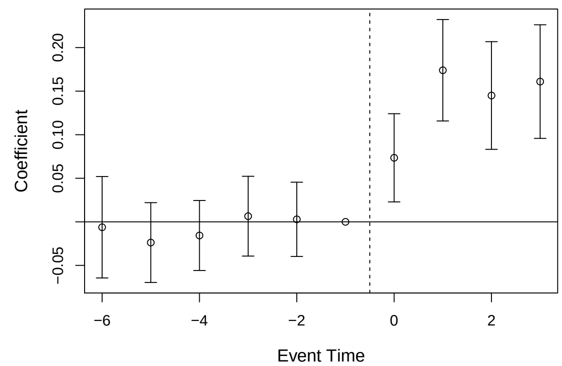
We follow the parametric bootstrap procedure outlined in Goodman-Bacon (2018) to estimate confidence intervals for the estimated average TOT effects. For both the ITT mortality and first stage estimates, we store 10,000 random draws from a normal distribution with the mean equal to the regression coefficients and the standard deviation equal to its standard error. We then estimate the implied individual treatment effect for each of these 10,000 replications. To apply the scaling factor for survey underreporting of Medicaid estimated using the NHIS, we create 10,000 samples with the number of observations used in its estimation and calculate the share of the draws that are less than or equal our estimated Medicaid reporting rate. We perform a similar calculation for each of the components used to construct the ratio of poor to all income mortality rates from the NHIS linked mortality data. We do not bootstrap for the baseline mortality rate. We follow the modified percentile method also outlined in Goodman-Bacon (2018) to construct the confidence intervals. We use the 5th percentile of draws that are below the mean and the 95th percentile of draws that are above the mean as the lower and upper bounds.

The resulting estimates are reported in Table A13.

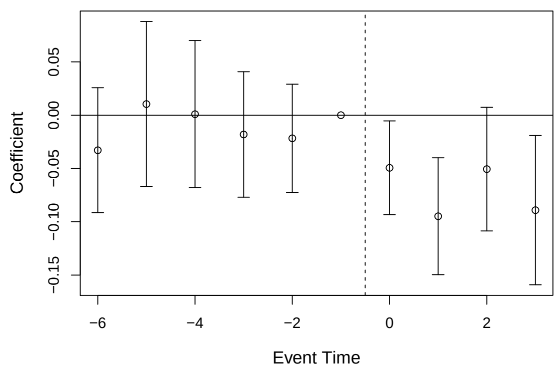
Figure A1: Effects of the ACA Medicaid Expansions on Medicaid and Insurance Coverage: Alternative Measures



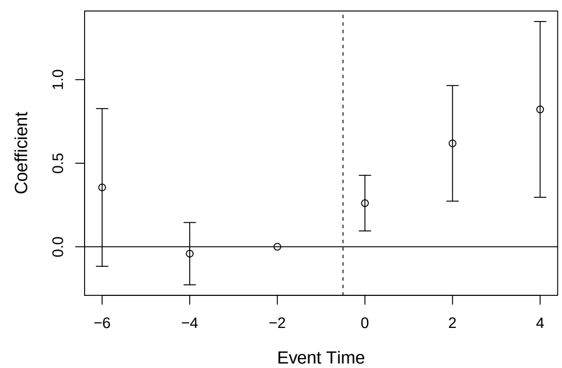
(a) Medicaid Coverage (ACS)



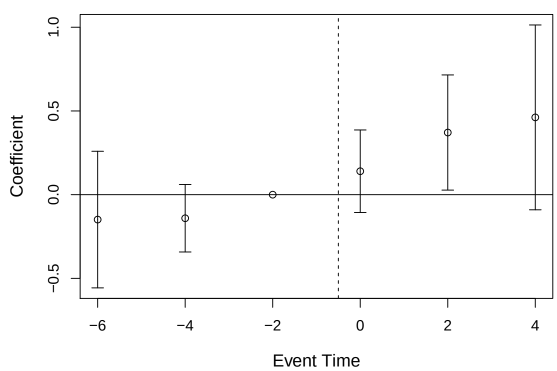
(b) Medicaid Coverage (NHIS)



(c) Uninsured (NHIS)



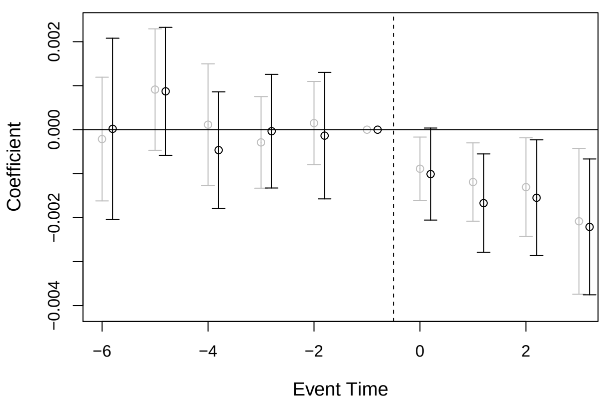
(d) Cumulative Medicaid (HRS)



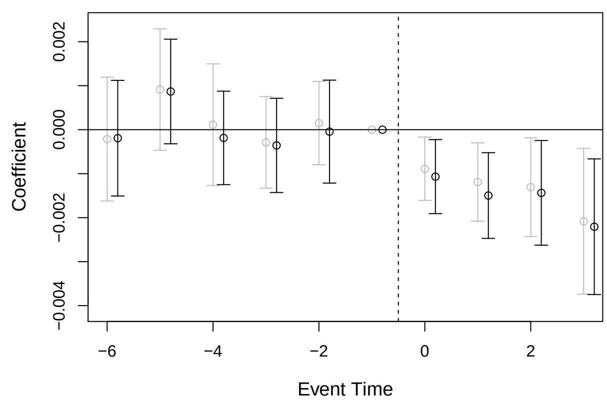
(e) Cumulative insurance (HRS)

Notes: These figures report coefficients from the estimation of equation (1) for additional first stage outcomes in the ACS, NHIS, and HRS. The sample is defined as U.S. citizens ages 55-64 in 2014 who do not receive SSI and who have either less than a high school degree or household income below 138% FPL. See text in Section 4.1 and Appendix Section D for additional information.

Figure A2: Effect of the ACA Medicaid Expansions on Annual Mortality: Accounting for Variation in Treatment Timing



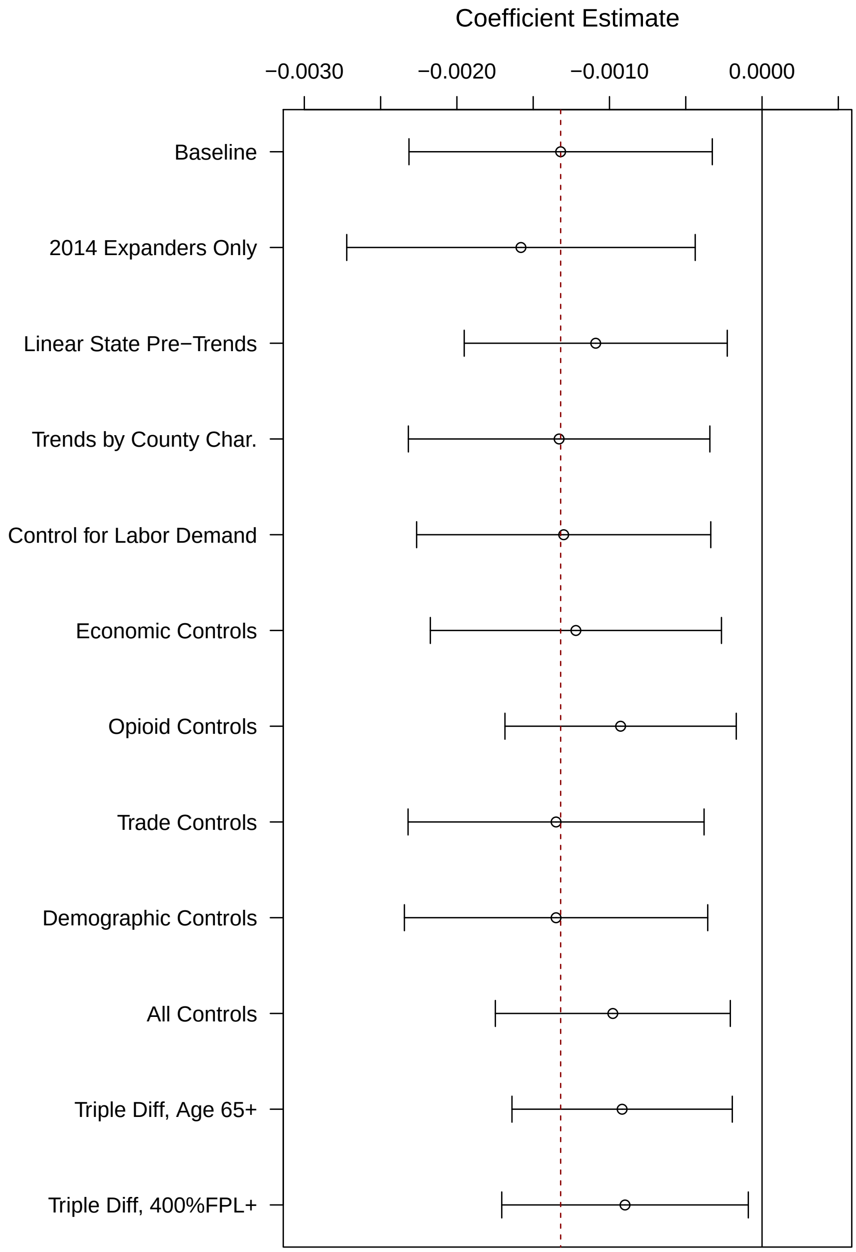
(a) 2014 expanders only



(b) Sun and Abraham (2020) method

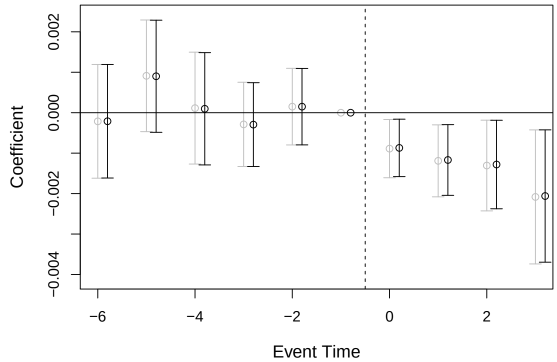
Notes: Panel (a) plots coefficients from equation (1) for a sample that excludes states that expanded after 2014. Panel (b) reports the results from using an alternative "interaction-weighted" estimator from Sun and Abraham (2020). For comparison, event study estimates from our main model are plotted in grey in both figures. See the text in Section 5.1 for more details.

Figure A3: Alternative Specifications

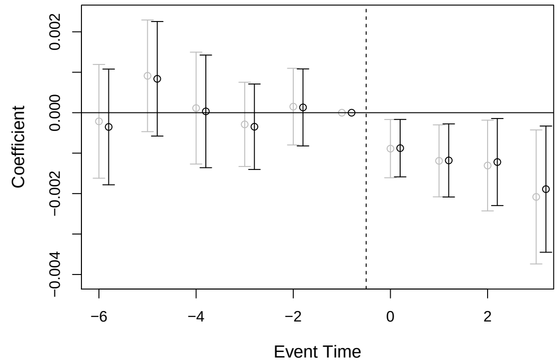


Notes: This figure plots difference-in-differences estimates for models that include only 2014 expanders, include linear state pre-trends, include different sets of covariates, or use an additional comparison group (triple difference model). The dotted line denotes our main estimate, which is also plotted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

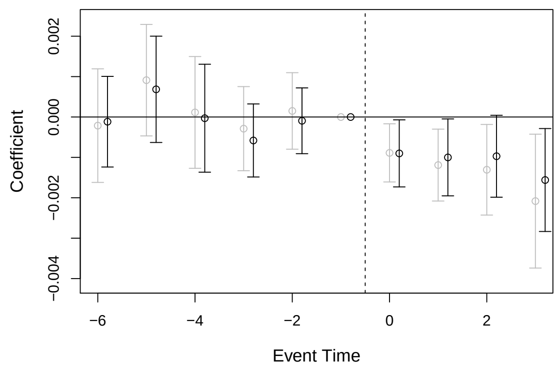
Figure A4: Effect of the ACA Medicaid Expansions on Annual Mortality: Alternative Specifications (Original Estimates in Grey)



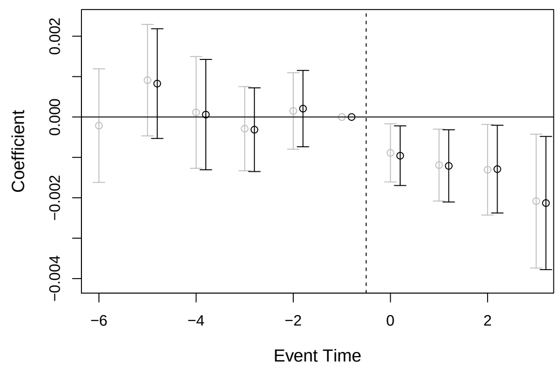
(a) Control for Labor Demand



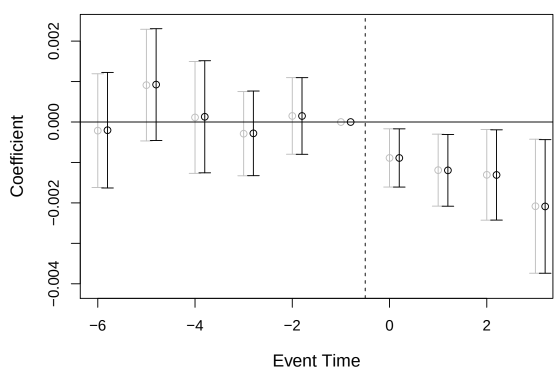
(b) Control for County Economic Variables



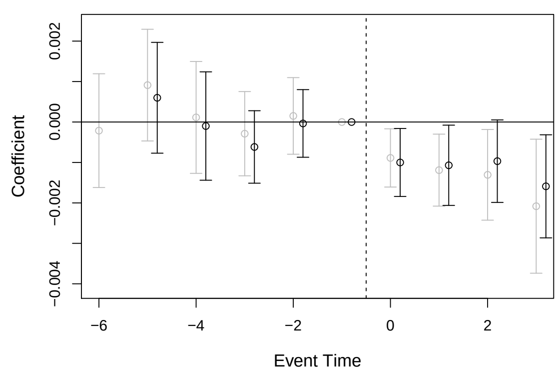
(c) Control for Drug Policy



(d) Control for Exposure to Trade with China (excl. AK and HI)



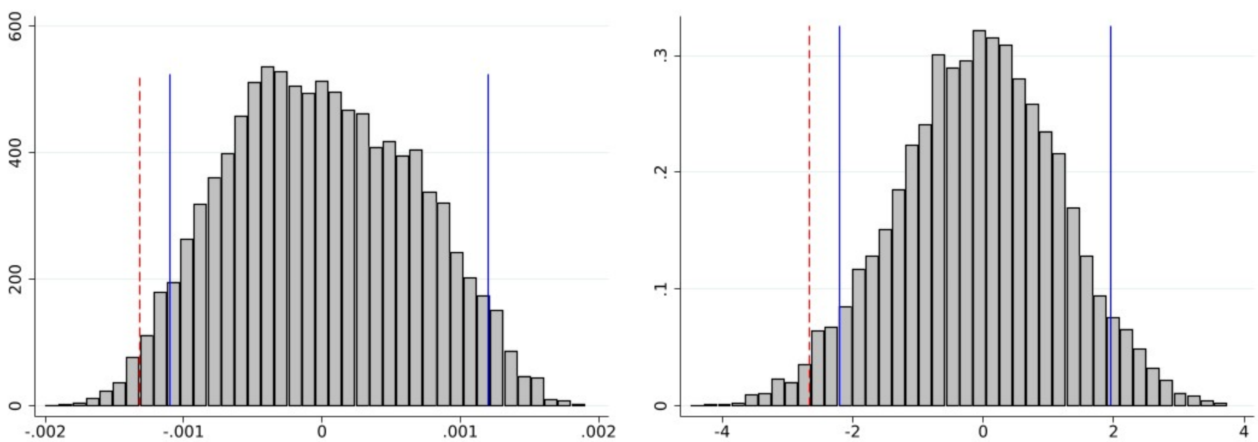
(e) Control for Sample Demographics



(f) All Controls (excl. AK and HI)

Notes: These figures plot event study coefficients that add additional county- and state-level covariates. Estimates from the original model reported in Figure 2 in the main text are plotted in grey. Panel (a) includes a “Bartik instrument” for predicted change in labor demand, panel (b) adds county-level unemployment rate, poverty rate, and median income. Panel (c) adds state-level drug policy controls. Panel (d) allows year fixed effects to vary by a measure of exposure to trade from China. Panel (e) controls for individual characteristics (age, race, gender). Panel (f) includes all of the controls. Note that AK and HI are excluded from panels (d) and (f) because trade exposure measures are not readily available for these states.

Figure A5: Distributions of Coefficient Estimates and T-Statistics from 10,000 Placebo Simulations



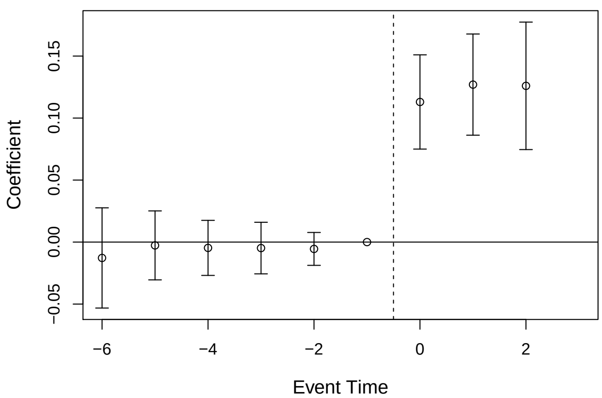
(a) Coefficient Estimates

(b) T-Statistics

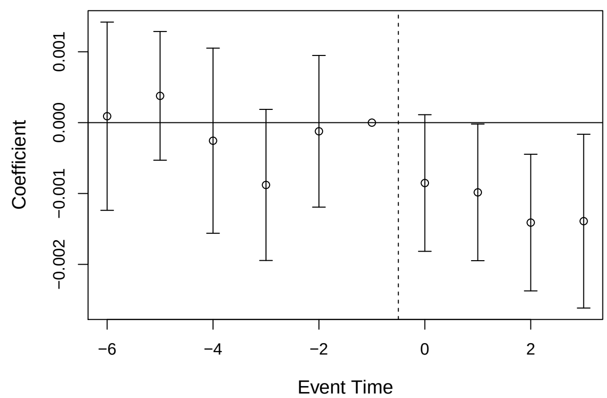
Notes: These figures present the distributions of coefficient estimates and t-statistics generated from the 10,000 placebo simulations using pre-ACA years of linked ACS-mortality data as described in Section 5.4. The 5th and 95th percentiles are marked with a blue vertical line, while the magnitude of our true estimate is depicted with a red dashed line.

Figure A6: Estimates of Mortality Impact of ACA Medicaid Expansions using Triple Difference Models

Additional Comparison Group: Ages 65+ in 2014

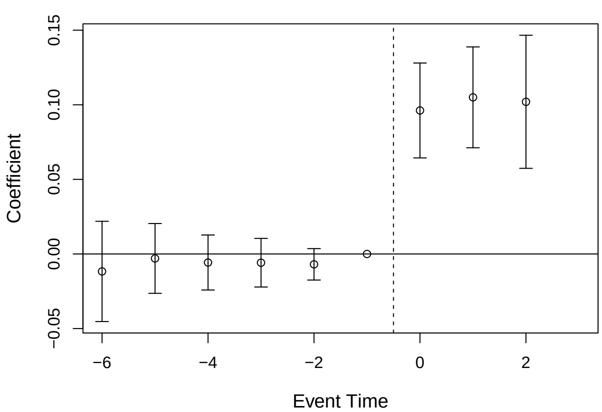


(a) Any Medicaid in Year

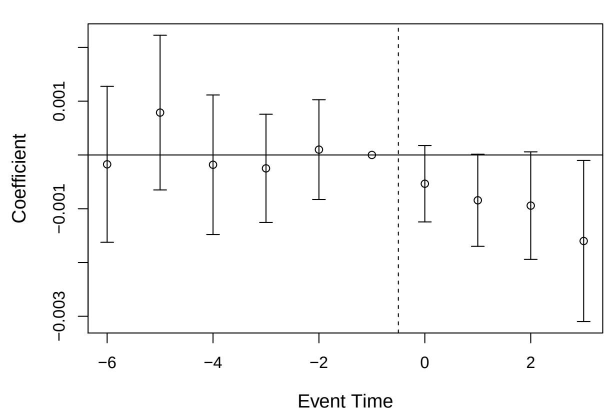


(b) Mortality

Additional Comparison Group: Ages 55-64 in 2014 with family income 400% FPL +



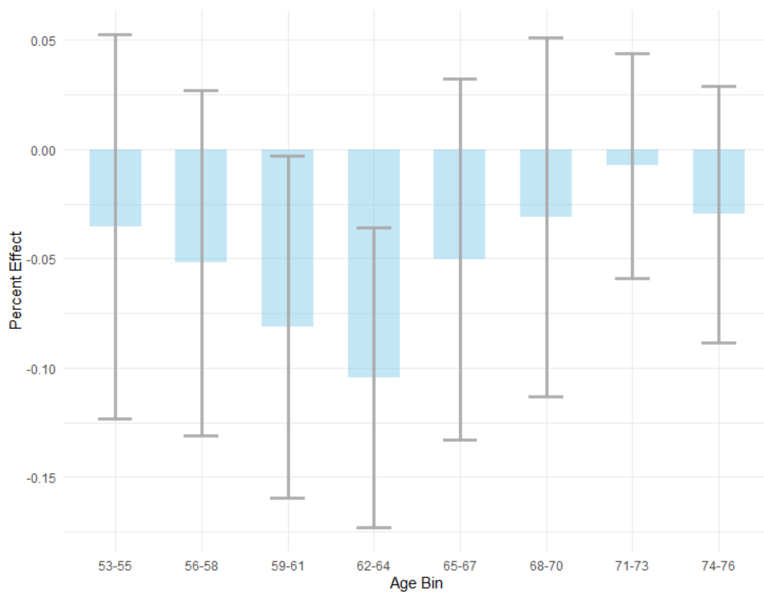
(c) Any Medicaid in Year



(d) Mortality

Notes: These figures plot event study coefficients from the triple difference models described in Section 5.4. The comparison group is labeled in italics for each set of graphs.

Figure A7: Estimates of Mortality Impact of ACA by 3-Year Age Bin



Mortality

Notes: These figures plot difference-in-differences coefficients scaled by the "counterfactual" mortality rate from a series of models that include respondents meeting our sample criteria but of different ages.

Table A1: Descriptive Statistics of Main Sample by State Expansion Status

	Expansion State	Non-expansion State
% White	70.9	68.7
% Black	14.9	24.2
% Hispanic	15.3	12.2
% Uninsured	32.6	37.3
% Medicaid	20.5	16.2
% Less than High School Education	45.3	46.8
Average Age in 2014	59.3	59.3
Average Income relative to FPL	1.47	1.40
N	231,200	190,448

Notes: This table displays weighted means for residents in expansion and non-expansion states meeting the sample criteria described in the text. These statistics were calculated using publicly-available 2008-2013 ACS data rather than the restricted version used in the main analysis.

Table A2: Difference-in-Differences Estimate: Coefficients from Nonlinear Models

	Logistic Regression	Cox Proportional Hazard Model
Expansion $\times$ Post	-0.0440**	-0.0417**
	(0.0194)	(0.0192)
N	4,034,000	3,461,000

Notes: Table displays estimates for coefficients for difference-in-differences model describe in text. The cox proportional hazard model drops observations with a death during the year of the ACS interview.

Table A3: Impact of the ACA Expansions on Medicaid and Insurance Coverage: Alternative Measures

	Medicaid Coverage (ACS) (1)	Medicaid Coverage (NHIS) (2)	Uninsurance (NHIS) (3)	Cumulative years Medicaid (HRS) (4)	Cumulative years insurance (HRS) (5)
Difference-in-Differences Model:					
Expansion × Post	0.101 (0.012)***	0.136 (0.020)***	-0.058 (0.019)***	0.370 (0.146)***	0.388 (0.207)*
Event Study Model:					
Year 4	—	—	—	0.822 (0.263)***	0.577 (0.345)
Year 3	0.099 (0.120)***	0.161 (0.033)***	-0.089 (0.035)**	—	—
Year 2	0.108 (0.012)***	0.145 (0.031)***	-0.051 (0.029)*	0.619 (0.173)***	0.464 (0.215)***
Year 1	0.113 (0.010)***	0.174 (0.029)***	-0.095 (0.027)***	—	—
Year 0	0.073 (0.008)***	0.074 (0.025)***	-0.049 (0.022)**	0.261 (0.083)***	0.175 (0.154)
Year -1 (Omitted)	0	0	0	—	—
Year -2	-0.009 (0.007)	0.003 (0.021)	-0.022 (0.025)	0	0
Year -3	-0.010 (0.007)	0.006 (0.023)	-0.018 (0.029)	—	—
Year -4	-0.003 (0.008)	-0.016 (0.020)	0.001 (0.035)	-0.0414 (0.0932)	-0.176 (0.126)
Year -5	-0.001 (0.009)	-0.024 (0.023)	0.010 (0.039)	—	—
Year -6	-0.006 (0.016)	-0.006 (0.029)	-0.033 (0.029)	0.355 (0.236)	-0.186 (0.255)
N (Individuals x Year)	714,673	18,033	18,033	5,573	5,573
N (Individuals)	714,673	18,033	18,033	1,359	1,359

Notes: This table displays the event study coefficient estimates of equation (1) and difference-in-differences estimates for additional first stage outcomes in the ACS, NHIS, and HRS. The sample is defined as U.S. citizens ages 55-64 in 2014 who do not receive SSI and who have either less than a high school degree or household income below 138% FPL. The difference-in-differences estimates for the HRS have been scaled to represent the 2014-2017 post period. See text in Section 4.1 and Appendix Section D for additional information. Significance levels: *=10%, **=5%, ***=1%.

Table A4: Impact of the ACA Expansions on Mortality: Impact by ICD Grouping

	Infectious disease	Neoplasms	Diseases of the blood and blood-forming organs	Endocrine, nutritional and metabolic diseases	Mental/behavioral
Expansion × Post	-0.0000671 (0.0001273)	-0.0005512 (0.0004556)	0.0000337 (0.0000345)	-0.0004314 (0.0002277)*	-0.0000465 (0.0001100)
Mean	0.004121	0.02718	0.0002675	0.005279	0.001676
	Nervous system	Circulatory system and cardiovascular	Respiratory	Digestive	Skin and sub-cutaneous tissue
Expansion × Post	-0.0000131 (0.0001162)	-0.0008861 (0.0004804)*	-0.0003801 (0.0002758)	-0.0000046 (0.000243)	-0.00002550 (0.0000119)**
Mean	0.002392	0.02504	0.008223	0.006589	0.00008866
	Musculoskeletal system	Genitourinary system	Other		
Expansion × Post	0.0001148 (0.0000706)	-0.0001297 (0.0001101)	0.0003175 (0.0001910)		
Mean	0.0004495	0.002094	0.07006		

Notes: This table displays the difference-in-differences coefficient estimates using the MDAC. Each entry is the result from a different regression. Rates are reported under coefficient estimates. Sample sizes are rounded following Census disclosure rules. DRB Approval Number: CBDRB-FY19-400. See text for more details. Significance levels: *=10%, **=5%, ***=1%.

Table A5: Difference-in-Differences Estimate: Goodman-Bacon (2019) Decomposition

	Coefficient	Total Weight
Timing group comparisons	-0.0003136	0.1062
Never treated vs. timing group comparisons	-0.001454	0.8938
Within-group variation from covariates	1.473	6.82E-006

Notes: Table displays estimates results from the DD decomposition described in Goodman-Bacon (2019) implemented with the aid of code in Goodman-Bacon et al. (2019). The decomposition shows comparisons amongst timing groups, comparisons of timing groups to units never receiving treatment, and the component due to within-group variation in controls. See additional discussion in Section 5.1.

Table A6: Expansion State "Time to Expansion" Linear Trend Model, Mortality Outcome

	Main Model (1)	Control for "Time to Expansion" (2)
Year 3	-0.00208**	-0.00193**
	(0.00083)	(0.00079)
Year 2	-0.00131**	-0.00119**
	(0.00056)	(0.00051)
Year 1	-0.00119***	-0.00112***
	(0.00044)	(0.00037)
Year 0	-0.00089**	-0.00084**
	(0.00036)	(0.00041)
Linear trend		-0.00004
		(0.00013)
N	4,034,000	4,034,000

Notes: Table displays estimates for post-expansion event times from equation (1) in column (1). Column (2) reports the coefficients from a model that replaces the pre-period event study indicators with a linear trend in event time for the expansion states. See additional discussion in Section 5.2.

Table A7: Impact of the ACA Expansions on Coverage and Mortality: Ages 55 to 61 in 2014

	Medicaid Eligibility	Medicaid Coverage in Year	Days of Medicaid in Year	Cumulative Medicaid Years Experienced	Uninsured	Died in Year
Difference-in-differences Model:						
Expansion × Post	0.472 (0.027)***	0.140 (0.0188)***	46.75 (8.409)***	0.409 (0.061)***	-0.059 (0.010)***	-0.001015** (0.00050)
Event Study Model:						
Year 3	0.466 (0.033)***	NA	NA	0.732 (0.076)***	-0.061 (0.013)***	-0.00199 (0.000806)**
Year 2	0.482 (0.027)***	0.148 (0.02709)***	49.87 (9.273)***	0.516 (0.061)***	-0.070 (0.011)***	-0.000975 (0.000536)*
Year 1	0.475 (0.026)***	0.142 (0.0204) ***	55.59 (11.97)***	0.327 (0.044)***	-0.065 (0.010)***	-0.00133 (0.000476)**
Year 0	0.489 (0.023)***	0.121 (0.0192)***	34.80 (7.003)***	0.134 (0.020)***	-0.042 (0.007)***	-0.000836 (0.000453)*
Year -1 (Omitted)	0	0	0	0	0	0
Year -2	0.012 (0.008)	-0.00963 (0.00741)	-2.479 (2.176)	-0.021 (0.012)*	0.002 (0.008)	-0.000402 (0.000506)
Year -3	0.010 (0.012)	-0.00749 (0.0118)	-303 (4.080)	-0.035 (0.023)	0.009 (0.008)	-0.000203 (0.000593)
Year -4	0.008 (0.010)	-0.000696 (0.0124)	-0.602 (3.167)	-0.067 (0.034)*	-0.005 (0.010)	0.000546 (0.000767)
Year -5	0.009 (0.012)	-0.00573 (0.0155)	-0.0137 (3.975)	-0.067 (0.039)*	0.001 (0.011)	0.000546 (0.000767)
Year -6	0.007 (0.012)	-0.0205 (0.0232)	-4.195 (5.986)	-0.095 (0.048)	-0.001 (0.016)	-0.000692 (0.000833)
N (Individuals x Year)	513,702	2,509,000	2,509,000	2,875,000	513,702	2,899,000
N (Individuals)	513,702	346,000	346,000	346,000	513,702	346,000

Notes: This table displays the event study coefficient estimates of equation (1). The sample is defined as U.S. citizens ages 55-61 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 text in Section 5.3 for more details. Significance levels: *=10%, **=5%, ***=1%.

Table A8: Difference-in-Differences Estimate: Alternative Methods of Inference

	Cluster by Census Division (Analytic) (1)	Cluster by Census Division (Bootstrap) (2)	Spatial Correlation of Errors (Conley, 1999) (3)
Expansion × Post	-0.00132**	-0.00132*	-0.00132***
	(0.000497)	[0.051]	(0.0004206)
N	4,034,000	4,0340,00	4,034,000

Notes: Table displays estimates for coefficients for difference-in-differences model describe in text. The standard error is reported in parentheses under the coefficient estimate for Columns (1) and (3); the p-value is reported in square brackets in Column (2). See Section 5.5 for additional discussion. Significance levels: *=10%, **=5%, ***=1%.

Table A9: Impact of the ACA Expansions on Other Age Groups

	Medicaid eligibility	Medicaid coverage	Uninsurance	Mortality Counterfactual rate	Mortality Change	N
Age 19-64	0.461***	0.127***	-0.078***	0.00487	-0.00019	23,630,000
	(0.028)	(0.017)	(0.011)		(0.00017)	
Age 19-29	0.524***	0.121***	-0.095***	0.00109	0.00007	10,210,000
	(0.027)	(0.020)	(0.013)		(0.00005)	
Age 30-39	0.423***	0.133***	-0.084***	0.00262	-0.00005	3,734,000
	(0.030)	(0.017)	(0.011)		(0.00017)	
Age 40-49	0.360***	0.135***	-0.082***	0.00462	0.00023	3,038,000
	(0.036)	(0.017)	(0.012)		(0.00022)	
Age 50-54	0.394***	0.142***	-0.079***	0.00967	-0.00032	2,125,000
	(0.030)	(0.016)	(0.011)		(0.00048)	

Notes: Age group defined using respondent's age in 2014. Table displays estimates for coefficients for the difference-in-differences model described in text. Counterfactual mortality rate calculated as sum of post-period mean in expansion states and the absolute value of the DD estimate. N refers to sample size in mortality analyses. See Section 6 for additional discussion. Significance levels: *=10%, **=5%, ***=1%.

Table A10: Difference-in-Differences Estimates: Heterogeneity Analysis

	Medicaid eligibility	Medicaid coverage	Uninsurance	Mortality Counterfactual rate	Mortality Change
Race/ethnicity					
White, non-Hispanic	0.543***	0.116***	-0.044***	0.01849	-0.00169***
N=2,672,000	(0.023)	(0.014)	(0.010)		(0.00041)
Black, non-Hispanic	0.537***	0.111***	-0.050***	0.01805	0.00045
N=629,000	(0.018)	(0.020)	(0.015)		(0.00097)
Other, non-Hispanic	0.412***	0.185***	-0.045***	0.00953	-0.00047
N=238,000	(0.028)	(0.029)	(0.013)		(0.00149)
Hispanic	0.333***	0.174***	-0.035**	0.00892	-0.00072
N=513,000	(0.022)	(0.020)	(0.014)		(0.00044)
Gender					
Female	0.526***	0.136***	-0.048***	0.01265	-0.00085
N=2,085,000	(0.027)	(0.022)	(0.010)		(0.00058)
Male	0.469***	0.119***	-0.040***	0.02004	-0.00184***
N=1,948,000	(0.024)	(0.018)	(0.011)		(0.00063)
Marital status					
Married, spouse present	0.373***	0.114***	-0.026**	0.01203	-0.00133*
N=1,846,000	(0.023)	(0.021)	(0.012)		(0.00075)
Unmarried, spouse not present	0.576***	0.138***	-0.055***	0.01942	-0.00132**
N=2,188,000	(0.026)	(0.021)	(0.011)		(0.00052)
Other					
Less than high school	0.276***	0.111***	-0.032**	0.01523	-0.00163**
N=1,897,000	(0.012)	(0.024)	(0.013)		(0.00080)
Less than 138% FPL	0.664***	0.142***	-0.055***	0.01801	-0.00131***
N=2,670,000	(0.032)	(0.020)	(0.011)		(0.00047)
Uninsured at time of ACS	–	0.246***	–	0.01460	-0.00150**
N=1,280,000		(0.026)			(0.00066)

Notes: Table displays estimates for coefficients for the difference-in-differences model described in text. Counterfactual mortality rate calculated as sum of post-period mean in expansion states and the absolute value of the DD estimate. N refers to sample size in mortality analyses. See Section 6 for additional discussion. Significance levels: *=10%, **=5%, ***=1%.

Table A11: Difference-in-Differences Estimates: Previously Uninsured

	Medicaid coverage in year	Days of Medicaid in year (ACS-CMS)	Cumulative years Medicaid experienced	Uninsured in survey year	Uninsured in survey year or in inter-survey year (HRS)	Number years continuous insurance coverage	Mortality Counterfactual rate (ACS-Numident)	Mortality Change (ACS-Numident)
Expansion x Post	0.246***	82.43***	0.598***	-0.087	-0.109*	0.453	0.01460	-0.00150**
	(0.026)	(11.38)	(0.080)	(0.059)	(0.064)	(0.243)		(0.00066)
N (Individuals x Year)	1,110,000	1,110,000	1,271,000	2,952	2,952	2,952		1,280,000

Notes: Table displays estimates for coefficients for the difference-in-differences model described in text. Counterfactual mortality rate calculated as sum of post-period mean in expansion states and the absolute value of the DD estimate. 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, as well as report no insurance coverage at the time of the ACS interview or during the pre-ACA period in the HRS. See Section 6 for additional discussion. Significance levels: *=10%, **=5%, ***=1%.

Table A12: Results from the Oregon Health Insurance Experiment for Participants Age 55-64 in 2008

Full Sample (Admin Data):

	Control Group Mean	Reduced Form	2SLS Effect of "Ever Medicaid"	P-value	As % of Control Mean
Died	0.023	-0.00422	-0.0165	0.128	-71.7%
N	10,790				

Full Sample (Admin Data):

	Control Group Mean	Reduced Form	2SLS Effect of "# Medicaid Years"	P-value	As % of Control Mean
Died	0.023	-0.00422	-0.0143	0.128	-61.2%
N	10,790				

Initial Survey Respondents:

	Control Group Mean	Reduced Form	2SLS Effect of "Insured at Initial Survey"	P-value	As % of Control Mean
Died	0.018	-0.00660*	-0.0587*	0.071	-326.1%
N	4,835				

12-month Survey Respondents:

	Control Group Mean	Reduced Form	2SLS Effect of "Insured at 12-Mo Survey"	P-value	As % of Control Mean
Died	0.004	-0.00206	-0.0134	0.153	-335.0%
N	4,458				

Notes: This table uses the public-use replication kit of the Oregon Health Insurance Experiment to estimate the impact of Medicaid on individuals who were between the ages of 55 and 64 at the time of the experiment. See Appendix Section F for additional details.

Table A13: Quasi-Experimental Estimates for Annual Mortality Effects of Insurance Expansions for Non-Elderly Adults (Deaths per 100,000)

	Population	**ITT effect on mortality (per 100,000 adults)**		**First stage effects on coverage (percentage point change)**		**Implied mortality effects for newly covered (proportionate change, %)**	
		Baseline	Absolute change	Medicaid	Any insurance	Medicaid	Any insurance
ACA Medicaid expansion							
Miller, Johnson, and Wherry 2020	Adults ages 19-64, below 138% FPL or low-ed, citizen, no SSI receipt	487	-19.0 (-13.9 to 51.9) Table A6	11.3 (8.2 to 27.3) Table A6	7.8 (5.5 to 10.0) Table A6	-34.5 (-101.4 to 24.9)^ Authors' estimation	-50.0 (-144.1 to 36.0)^ Authors' estimation
	Adults ages 55-64, below 138% FPL or low-ed, citizen, no SSI receipt	1630	-132.0 (-229.4 to -34.6) Table 1	12.8 (8.9 to 30.2) Table 1	4.4 (2.4 to 6.4) Table 1	-63.3 (-124.9 to -16.8)^ Authors' estimation	-184.1 (-424.0 to -49.3)^ Authors' estimation
Black et al. 2019	Adults ages 55-64	854	-2.6 (-14.3 to 9.2) Estimates provided by authors, converted into level change from baseline	NR	1.1 (0.2 to 1.6) Table A-3, estimated for ages 50-64	NR	-17.3 (-782.9 to 66.6)^ Authors' estimation + Adjustment (1)
Borgschulte and Vogler 2020	Adults ages 20-64	359	-11.4 (-18.4 to -4.3) Table 2	NR	4.2 (2.0 to 6.4) Table A.3	NR	-47.3 (-118.7 to -17.4)^ Authors' estimation + Adjustment (1)
Chen 2019	Adults ages 25-64	386	-2.2 Table 5	5.3 Table 2	3.9 Table 2	-4.6 (-10.7 to 0.6)^ Authors' estimation + Adjustments (1)-(2)	-9.1 (-21.2 to 1.2)^ Authors' estimation + Adjustment (1)
	Adults ages 55-64	859	-10.6 Table 3	4.0 Table 2	2.4 Table 2	-13.2 (-23.4 to -4.2)^ Authors' estimation + Adjustments (1)-(2)	-32.1 (-57.3 to -10.0)^ Authors' estimation + Adjustment (1)
Swaminathan et al. 2018	Nonelderly patients initiating dialysis	6900	-600 (-1000 to -200) Table 2	10.5 (7.7 to 13.2) Table 2	4.2 (2.3 to 6.0) Table 2	-35.5 (-96.5 to 17.8)^ Authors' estimation + Adjustments (1)-(2)	-129.4 (-282.9 to -43.9)^ Authors' estimation + Adjustment (1)
Other insurance expansions							
Sommers 2017 (AZ, ME, NY expansions)	Adults ages 20-64	318	-19.1 (-31.4 to -6.8) Tables 1, 3	NR	NR	NR	-64.5 (-89.1 to -42.3)^ Table 6 + Adjustment (1)
Sommers, Sharon, and Baicker 2014 (MA health care reform)	Adults ages 20-64	283	-8.2 (-13.6 to -2.8) Table 3, converted into level change from baseline	N/A	6.8 Table 5	N/A	-27.3 Authors' estimation + Adjustment (1)

Notes: NR=Not Reported. Baseline (counterfactual) mortality rate for Miller, Johnson, and Wherry is calculated as described in the text. Sommers (2017) average treatment effect estimates based on results from a model that interacts county level uninsurance rates with Medicaid expansion. Estimates from Black et al. (2019) are from the triple difference specification and received through e-mail correspondence with the authors. Models in Chen are estimated in terms of changes in Medicaid eligilibility; we follow the author in interpreting the change associated with an increase from the 5th to the 95th percentile of changes in Medicaid eligibility between 2016 and 2012 as the effect of the ACA expansions. ^Confidence interval was estimated using the parametric bootstrapping procedure described in Appendix Section F.

Adjustment (1): Multiply baseline mortality rate by 1.60 to account for higher relative risk of death for poor adults, calculated using linked NHIS-mortality data (see Appendix Section F).

Adjusment (2): Divide change in Medicaid coverage estimated from survey data by (1-0.314=0.686) to account for underreporting.

Table A14: Sources for Parent and Adult Medicaid Eligibility Rules by Year

Year	Sources	Notes
2008-2010	Adults: NGA Center for Best Practices (2010), Table 9	We follow the criteria reported in Heberlein et al. (2011), Table 4 to determine whether programs described in NGA Center for Best Practices (2010) meet the full coverage criteria. We turned to additional sources to reconcile other differences with the program details reported in Kaiser Family Foundation (2019a). Specifically, we added a program for AZ following National Conference of State Legislatures (2009), a DC program based on Meyer et al. (2010), and altered HI and VT program details using Indiana Legislative Services Agency (2011).
2011-2017	Adults: Kaiser Family Foundation (2019a)	We consider eligibility rules to be in place as of the date of the relevant KFF survey. To be consistent with our definition of implementation of the ACA Medicaid eligibility expansions, we consider the expansion in Indiana to take place in 2015.
2008-2017	Parents: Kaiser Family Foundation (2019b)	We consider eligibility rules to be in place as of the date of the relevant KFF survey with the exception of the December 2009 survey for parents eligibility, which we apply to the 2010 year. To be consistent with our definition of implementation of the ACA Medicaid eligibility expansions, we consider the expansion in Indiana to take place in 2015.