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ABSTRACT

Did the ACA Medicaid Expansion Save Lives?*

We estimate the effect of the Affordable Care Act Medicaid expansion on county-level mortality in the first four years following expansion. We find a reduction in all-cause mortality in ages 20 to 64 equaling 11.36 deaths per 100,000 individuals, a 3.6 percent decrease. This estimate is largely driven by reductions in causes of death likely to be influenced by access to health care, and equates to one life saved per 310 newly covered individuals. A cost-benefit analysis shows that the improvement in welfare due to mortality responses may offset the entire net-of-transfers expenditure associated with the expansion.

JEL Classification: H75, I13, I14, I18, I38

Keywords: health insurance, Medicaid, mortality, public health insurance, healthcare, Affordable Care Act

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1 Introduction

Medicaid is the largest means-tested social insurance program in the U.S., providing publicly funded health insurance for low-income families and individuals. Evidence from recent studies has shown that Medicaid reduces financial risk to beneficiaries while also increasing access to healthcare services.[1] Far less evidence links Medicaid access to long-run improvements in health and mortality, particularly among adults.[2] Prior to the Affordable Care Act (ACA) expansion, the best evidence pertaining to the mortality effects of access to the present Medicaid system came from analyses of expansions in Arizona, New York, and Maine in the early 2000s as well as the Massachusetts health reform in 2006 (Sommers, Gawande and Baicker, 2017). Analysis of these earlier reforms is complicated, however, due to the small number of reform states and possibility of coincident but unrelated changes in mortality.

In this paper, we estimate the effects of the ACA Medicaid expansion on adult mortality in the first four years following the 2014 Medicaid expansion. A key part of the ACA, the Medicaid expansion removes categorical exclusions and bases eligibility solely on income at or below 138 percent of the federal poverty level. The ACA Medicaid reform was originally formulated to occur nationwide, but was effectively made a state option by the 2012 Supreme Court ruling *National Federation of Independent Business v. Sebelius*. As a result, just over half of the United States chose to adopt the initial expansion (Center for Medicare and Medicaid Services, 2017). Coverage became effective on January 1, 2014, and over 9 million new individuals between the ages of 19 and 64 enrolled by the end of 2015, accounting for 60 percent of the immediate increase in coverage that resulted from the ACA (Frean, Gruber and Sommers, 2017). While the short follow-up period limits our ability to estimate the long-run effects of health insurance coverage, recent changes in Medicaid, the individual mandate, and other elements of the ACA suggest that the

[1]There is extensive evidence pertaining to these outcomes: Currie and Gruber (1996*a*,*b*); Card and Shore-Sheppard (2004); Long, Coughlin and King (2005); Finkelstein et al. (2012); DeLeire et al. (2013); Sommers, Kenney and Epstein (2014); Taubman et al. (2014).

[2]The effects of Medicaid on adult health and mortality have been examined in Finkelstein et al. (2012); Baicker et al. (2013), Sommers (2017), Goodman-Bacon (2018), Wherry and Miller (2019), and Black et al. (2019). The lack of health insurance is associated with worse health and higher mortality (Wilper et al., 2009), but causality is not clear (Kronick (2009); Black et al. (2017)).

window in which to measure the effects of the ACA Medicaid expansion is closing.

Our primary analysis compares the post-expansion changes in mortality between counties in states which expanded Medicaid in 2014 with changes in those that did not adopt the expansion. To account for pre-existing differences between these counties and improve the efficiency of our estimation, we use propensity-score reweighting based on economic, demographic, and political characteristics. Two key features of the implementation of our research design serve to discipline the analysis and add credibility to the findings. First, we use the double lasso method to specify the propensity score model (Belloni, Chernozhukov and Hansen (2014); Urminsky, Hansen and Chernozhukov (2016)). This model-selection procedure chooses variables which predict the outcome (mortality) and/or the treatment indicator (the decision to expand Medicaid) with a penalization to prevent overfitting. Second, we perform a cross-validation exercise in which we hold out mortality outcomes in the four years before the reform from the propensity score model and check that the reweighted data displays flat pre-trends over this time period.[3] Mortality responses, including age and cause of death, are observed in restricted-access microdata for all deaths in the U.S. from 2000 to 2017. Together, this allows us to estimate the marginal impact of public health insurance coverage for areas that adopted the Medicaid expansion and, further, to examine whether changes occur where Medicaid is likely to have the largest effects.

We find evidence of a reduction in mortality following the ACA Medicaid expansion, with the strongest results for amenable causes of death, those most likely to have been avoided through optimal quality health care. Point estimates on aggregate 4-year mortality suggest an improvement in all-cause mortality among 20-to-64-year-old adults of 11.36 fewer deaths per 100,000 people, a 3.60 percent decrease in mortality. The event study shows flat pre-reform trends and an immediate drop in mortality in the first year following the reform, with evidence of growing effects after that. Changes in aggregate mortality rates are largely explained by a reduction equaling 6.64 fewer deaths per 100,000 people due to amenable causes. We also find a larger reduction in mortality in areas with low rates of health insurance coverage before the expansion. Applying estimates

[3]We discuss the merits of our strategy relative to the triple difference design in Black et al. (2019) in Section 5.

of newly covered individuals in states that expanded Medicaid relative to those that did not, the aggregate effects equate to one additional death averted for each 310 new Medicaid recipients. When interpreting these magnitudes, it is important to note that access to health care may have larger effects over longer time horizons than available in our data, and we find growing effects over time.

An essential aspect of our study is that we examine effects on aggregate mortality. One resulting benefit is that we measure mortality responses along all margins, while a cost is that we can say relatively less about the specific channels, as there may be too many to distinguish their individual roles. The magnitude of the mortality reductions suggest that the program may have affected a broader population than the newly-insured individuals, implying that it is valuable to consider aggregate mortality. Individual-level treatment-on-the-treated (TOT) estimates using our first-stage estimate of a 4.15 percent net increase in health insurance coverage in our sample would imply an 86 percent reduction in mortality among net-insurance compliers (i.e. the group of individuals who switch from no health insurance to having health insurance as a result of the Medicaid expansion). Adjusting this TOT estimate for higher mortality among the Medicaid population would still imply that the expansion prevented 30 percent of deaths in this population, a large reduction in mortality.[4] The TOT extrapolation relies, however, on several strong assumptions, especially, that the beneficial effects of the expansion were confined to net-insurance compliers. This assumption would be violated, for example, if there were responses among those who switched onto Medicaid from other sources of insurance, or if the benefits of health insurance affect households or communities. A growing literature documents responses consistent with diffuse benefits of Medicaid on, for example, financial outcomes (Hu et al. (2016), Miller et al. (2018)), crime (Vogler (2017), He (2018)), and hospital closures (Lindrooth et al. (2018)); there is also little evidence of negative spillovers through congestion effects (Carey, Miller and Wherry (2018)). Large TOT estimates appear to be a feature of public insurance studies, suggesting that assessment of these programs should allow for benefits in the general population, and future research should more closely ex-

[4]Randomized trials for the most effective drugs, such as statins, show short-term mortality reductions around half as large (Baigent et al., 2005; Chou et al., 2016).

amine the sources of gains from health insurance.[5] Findings of large individual TOT estimates in this literature is another reason to value our use of a data-driven model selection procedure and cross-validation of the research design through the examination of held-out pre-period outcomes.

To further explore the robustness of these findings, and to make an apples-to-apples comparison with previous Medicaid reforms analyzed in the literature, we re-estimate our model using the early-2000s Medicaid expansions in Arizona, New York, and Maine. This analysis also serves to replicate, with modifications, the analysis reported in Sommers (2017). We generate short-and long-run estimates by restricting the follow-up period to the first four and eight years following these earlier reforms, and estimate effects on aggregate and amenable mortality. Our findings support the conclusions indicating mortality reductions reported in Sommers, Baicker and Epstein (2012) and Sommers (2017). Taken together, our point estimates are approximately one-half the size of the prevailing estimates of the effects of Medicaid on mortality, however, the ACA expansion had smaller effects on insurance coverage than these earlier reforms. The comparison of effects reveals that per-beneficiary ACA-Medicaid mortality improvements are very similar to the corresponding effects found when applying our model to these earlier reforms.

Despite generating smaller reductions in mortality rates than previously found in the Medicaid literature, the mortality effects of the ACA Medicaid expansion can still have substantial impacts on the cost-effectiveness of the ACA Medicaid expansion. To illustrate this point, we compare program costs to the value of mortality gains using age-specific estimates of the Value of a Statistical Life (VSL) in Aldy and Viscusi (2008). We calculate the benefits of expansion under two sets of assumptions: first, by applying our point estimate for all-cause mortality to the ages 45-64 (taking our results at face value); and second, under a conservative scenario in which our results apply only to amenable cause mortality for the 55 to 64 age group (where previous researchers suggest results are most likely to be found, and our results are most robust). These results imply that mortality benefits offset between one-third and all of the expenditures associated with Medicaid expansion.

[5]Large TOT estimates appear across research designs, such as the RD evidence in Card, Dobkin and Maestas (2009) and the age 64 vs age 65 contrast in Huh and Reif (2017); also see Sommers, Gawande and Baicker (2017) and Dunn and Shapiro (2019).

An economic cost-benefit analysis should weigh the mortality benefits against the efficiency costs of the program, i.e. deadweight loss associated with crowd out from private insurance and raising tax revenue. Evidence from the Oregon Health Insurance Experiment suggests that approximately 60 percent of Medicaid expenditures are transfers (Finkelstein, Hendren and Luttmer, 2016). Applying that figure to the ACA Medicaid expansion would imply that the mortality-related savings may cover the entire net cost of the ACA Medicaid expansion. A full accounting of the social welfare effects of the Medicaid expansion would also include benefits to other outcomes, such as reductions in financial distress among beneficiaries.

2 Data

We use information from the Kaiser Family Foundation (2018) to identify states that expanded Medicaid on January 1, 2014. In addition, we classify Michigan as treated as they adopted the expansion in the first half of 2014. We also classify Wisconsin as treated as they independently chose to expand Medicaid coverage to all adults up to 100 percent of the federal poverty level.

Mortality data come from the National Vital Statistics System (2018) through the the Centers for Disease Control and Prevention (CDC). These restricted access mortality data contain detailed information on age, sex, race, cause of death, and county of residence for every death in the U.S. The CDC assigns cause of death by examining death certificates collected from around the country. We follow Sommers (2017) to classify amenable conditions using the ICD-10 codes included in the mortality data. Amenable conditions are presented in Appendix Table A.1. Despite the care taken by the CDC in constructing the data, it is important to note that the classification of cause of death into amenable and non-amenable may be imperfect due to difficulty in ascertaining cause of death, differences in how death certificates are filled out by coroners, and the inherent challenges in determining which causes would have been prevented by access to optimal-quality health care. Therefore, we use all-cause mortality in our model selection procedure, and report amenable cause mortality as a secondary outcome.

The mortality data are paired with population denominators by county, age group, sex, and race from the U.S. Census and then merged with county-specific economic and demographic variables, such as the unemployment rate, the poverty rate, and real median income. These data were obtained from the the Small Area Unemployment Statistics (SAUS) program through the Bureau Of Labor Statistics (2018) and the Small Area Income and Poverty Estimates Program (SAIPE) through the U.S. Census (SAIPE, 2018). We also gatherd county-level uninsured data from the U.S. Census Small Area Health Insurance (SAHIE) database (SAHIE, 2018).

3 Empirical Strategy

Medicaid expansion states and counties differ in level and trend before the expansion, meaning that any direct comparison of expansion with non-expansion groups will be biased. In Figure A.1 we illustrate this issue. The figure shows estimates from an event-study model using a sample of all counties from 2009-2017, where the outcome variables include the annual, county-level all-cause and amenable mortality rates among adults between 20 and 64 years of age. The figure shows an obvious pre-trend in all-cause and amenable-cause mortality rates in expansion counties relative to non-expansion counties in the years prior 2014.

To address these pre-existing differences, we use event study and difference-in-differences models in conjunction with propensity score reweighting. The combination of these techniques provides important advantages relative to either technique in isolation, both in bias and efficiency (Smith and Todd, 2005; Imbens and Rubin, 2015). The difference-in-differences model provides compelling within-county variation in treatment status, but can be sensitive to the selection of the control group. With this in mind, we use the propensity-score model to select a group of treatment and control counties in a way that balances pre-treatment characteristics and outcomes. We exclude pre-period outcomes to avoid mechanically creating parallel trends in the outcome between treatment and control, meaning that we can interpret the evolution of mortality in held-out period as a test of parallel trends. Propensity-score reweighting has the additional advantage of improving the efficiency of the estimation (Hirano, Imbens and Ridder, 2003). We focus the analysis on the TOT effect to answer the question "Did the ACA Medicaid expansion save lives?"

3.1 Propensity-Score Model

Our first step is to address pre-expansion differences between treatment and control counties by constructing a sample of counties in non-expansion states that is similar to counties in expansion states. To do so, we fit a propensity-score model that predicts whether a county is located in a state that adopted the Medicaid expansion. The model is estimated using a rich set of demographic, economic, and political characteristics of the states in the years before 2014. Given the large number of potential predictors of treatment and resulting possibility of overfitting the propensity score, we use the double lasso procedure described in Belloni, Chernozhukov and Hansen (2014) and Urminsky, Hansen and Chernozhukov (2016) to select the relevant variables to be included in the model. This method identifies variables for inclusion in the propensity score model in two steps: 1) fitting a lasso regression that predicts the outcome of interest, i.e. the mortality rate for adults ages 20-64, and 2) fitting a lasso regression that predicts the focal independent variable, i.e. expanding Medicaid in 2014. The union of the variables estimated to have non-zero coefficients in these steps are then included in the final propensity score model.[6] As discussed above, we hold out mortality outcomes from the four years preceding the 2014 expansion from the set of potential predictors in the propensity score. This allows us to perform a cross-validation exercise by examining pre-trends in this window. In addition to preventing the overfitting of the propensity score model, the procedure allows for imperfect selection of controls in the prediction of treatment through the inclusion of predictors of the outcome in the "first-stage" propensity score.

[6] We estimate each step using the square-root lasso described in Belloni, Chernozhukov and Hansen (2014) and Belloni et al. (2014). We allow the selection procedure to choose from a rich set of pre-expansion county-level variables, including the unemployment rate, the poverty rate, logged real median income, logged population, population density, the Obama vote share in 2008, the Obama vote share in 2012, an indicator of whether the state governor was a Democrat in 2010, logged average state health and welfare expenditures between 2005-2013, the pre-expansion uninsured rate for non-elderly adults, the percentage of population in five age groups distributed between 20 and 64 years of age, the percentage that is male, the percentage that is white, the percentage that is black, and the percentage that is Hispanic. We also include the all-cause mortality rates for adults 20 to 64 for each year between 2005 and 2009 as well as the average all-cause, amenable, and non-amenable mortality rates between 2005 and 2013.

Once we have selected our variables, the propensity score is estimated using the following logistic regression model:

$$Logit(Pr(MedicaidExpansion_c)) = \beta_0 + \beta_1 \mathbf{X}_c, \tag{1}$$

where the dependent variable is an indicator of whether county c expanded Medicaid in 2014. The vector $\mathbf{X}_c$ includes the predictors of expansion selected by the data-driven procedure. Observations with propensity scores that are outside the overlap region are trimmed from the sample. We also exclude counties in six states (PA, IN, NH, AK, MT, LA) that implemented the Medicaid expansion after the first half of 2014. Results that include these states are presented as robustness checks.

There are several reasons for constructing counterfactuals at the county level as opposed to at the state level. First, the chief advantage relative to a state-level analysis is that we can achieve more precise estimates by modeling sub-state variation. Finer geographic data allows us the flexibility to adjust for potentially confounding changes in mortality that would be otherwise be unaccounted for when using a broader geographic unit. For instance, suppose that opioid-related mortality is rising in rural counties at the same time as the Medicaid expansion takes place. Analyzing at the county level gives the model greater flexibility to account explicitly for this confounding change in mortality (e.g. by including county fixed-effects or county-specific linear trends) than a state-level reform, which would bury variation in rural, opioid-related mortality in a much smaller number of state averages. Second, the county level is the smallest geographic unit for which detailed data are available for most variables of interest. Thus counties maximize the size of potential controls, enabling us to trim outliers, select a closely comparable control group, and retain a large sample.[7] The availability of data also enables us to examine heterogeneity by county-level characteristics, such as pre-expansion uninsurance levels. Finally, existing evidence pertaining to the mortality effects of Medicaid has relied largely on county-level analysis. This puts our findings on

[7] In comparison, Sommers (2017) matches on the propensity-score at the county level but then analyzes outcomes for county-race-sex-age cells. This strategy is unusual, in that the regression considers units that are neither at the level at which matching was conducted nor at the level of the treatment assignment. We believe our strategy captures the primary benefits of this earlier work while aligning the analysis more closely with standard econometric practices.

similar ground to those reported in earlier studies, such as Sommers (2017) and Black et al. (2019).

3.2 Event-Study and Difference-in-differences

After constructing the analysis sample and weights, we run event-study models of the form:

$$Y_{cst} = \alpha_c + \gamma_t + \sum_{\tau=2009, \tau \neq 2013}^{2017} \beta_\tau (Expansion_{cs} \times \mathbf{1}[\tau = t]) + \beta \mathbf{X}_{cst} + \varepsilon_{cst}, \tag{2}$$

where Y_{cst} indicates deaths per 100,000 people in each observation cell indexed by county c in state s and year t. Included in the model are a full set of county and year fixed effects, indicated by α_c and γ_t, respectively. Time-varying control variables, represented by $\mathbf{X}_{cst}$, are selected using the double lasso procedure for panel data (Belloni et al., 2016).[8] These controls reduce standard errors and offer additional controls for any time-varying differences between treatment and control unaccounted for by the reweighting procedure. The coefficients of interest are the β_τ's on the interaction between the indicator for Medicaid expansion and indicator function $\mathbf{1}[\tau = t]$. The event-study model enables us to both test for the presence of pre-trends and capture the evolution of the treatment effect over time.

We then report pooled difference-in-differences results, including models which focus on outcomes for specific age groups, pre-expansion insurance coverage, and cause of death. The pooled model effectively averages the year-specific effects estimates in the event study into a single contrast of pre- and post-expansion differences between treatment and control counties. The baseline model is specified as follows:

$$Y_{cst} = \alpha_c + \gamma_t + \beta_1 Expansion_{cs} + \beta_2 Post_t + \beta_3 Expansion_{cs} \times Post_t + \varepsilon_{cst} \tag{3}$$

[8] We allow the selection procedure to choose from time-varying demographic, racial, and economic controls. These include population percentages in five age groups (20-24, 25-34, 35-44, 45-54, and 55-64), three racial groups (white, black, and other), two sex groups (male and female), and the percentage of population that is Hispanic. We also include logged total county population, the logged population among adults between 20 and 64 years of age, and logged population variables for each age, racial, and sex group. Finally, we include county poverty rates, unemployment rates, and median household incomes. Selected controls are indicated in the table notes.

Our preferred specification includes the time-varying controls selected by the double lasso procedure. Throughout the analysis we cluster standard errors by state to account for the fact that our variation comes from state-level adoption decisions. We weight our estimates by the county population 20 to 64 years of age multiplied by $T + (1-T) \times \frac{p}{(1-p)}$ where T is an indicator for treatment and p is the estimated propensity score. Weighting in this way provides a consistent estimator of the TOT.

3.3 Propensity Score Estimates

We first report predictors of expansion selected by the double lasso procedure. The procedure selects county age, race, economic measures, and political variables as predictors of expansion. Counties with large population percentages in the 25-to-34, 35-to-44, and 55-to-64 age groups are more likely to implement the expansion. For males, and Hispanics, higher population shares are positive predictors of expansion, while higher shares of blacks are negative predictors. Expansion counties had higher median incomes, but also higher unemployment rates. The pre-expansion non-elderly uninsured rate negatively predicts expansion. Having Democratic governors at the time of the passage of the ACA and having high 2008 and 2012 Obama county vote shares are both strong predictors. Finally, having higher non-elderly mortality rates in the years prior to the ACA is a modest predictor of expansion, with small and mainly insignificant estimates across these years. The full set of coefficient estimates appear in Appendix Table A.2.

The resulting propensity-score distributions of treatment and control counties display significant overlap for nearly the entire distribution, except that there are almost no non-expansion counties in the very upper tail. We trim the sample at areas in the tails where there is no overlap (propensity scores outside of 3.8 and 97.1 percent). The distribution of the propensity-score and a map of counties appear in Appendix Figure A.2 and Appendix Figure A.3, respectively. Summary statistics are presented in Table 1. Treatment counties generally have slightly lower baseline mortality rates. Treatment and control counties are similar among the independent variables except that treatment counties are more likely to be located in a state with a Democratic governor in 2010.

Table 1: Summary Statistics

Variables	*Expansion Counties* *Mean (SD)*	*Matched Counties* *Mean (SD)*
Baseline Mortality Rates		
All Cause Mortality (per 100,000 people)	315.17 (94.69)	359.06 (117.06)
Males	394.31 (120.53)	449.76 (149.54)
Females	237.43 (75.79)	271.23 (94.93)
Ages 20-24	82.03 (46.25)	95.89 (62.41)
Ages 25-34	96.64 (46.99)	114.01 (54.57)
Ages 35-44	160.49 (65.42)	184.84 (80.28)
Ages 45-54	379.59 (119.13)	434.35 (147.28)
Ages 55-64	795.59 (196.38)	885.72 (236.04)
Amenable Cause Mortality (per 100,000 people)	200.78 (61.56)	226.80 (78.03)
Males	239.45 (76.33)	272.11 (98.36)
Females	162.88 (52.74)	183.08 (66.75)
Ages 20-24	11.69 (13.35)	13.18 (19.27)
Ages 25-34	23.83 (15.18)	27.84 (20.07)
Ages 35-44	71.68 (31.64)	83.48 (42.64)
Ages 45-54	238.82 (80.31)	273.46 (100.40)
Ages 55-64	607.86 (158.58)	668.63 (189.14)
Cardiovascular Mortality (per 100,000 people)	63.25 (25.32)	72.03 (32.06)
Respiratory Mortality (per 100,000 people)	17.47 (10.24)	21.17 (13.44)
Suicides (per 100,000 people)	15.28 (7.45)	18.19 (8.77)
Opioid Overdoses (per 100,000 people)	2.66 (3.89)	3.67 (4.30)
Drug & Alcohol Poisonings (per 100,000 people)	13.23 (9.26)	12.95 (9.12)
Independent Variables		
% Population Ages 20-25	0.12 (0.03)	0.12 (0.04)
% Population Ages 25-34	0.23 (0.03)	0.22 (0.03)
% Population Ages 35-44	0.22 (0.02)	0.22 (0.02)
% Population Ages 45-54	0.24 (0.02)	0.24 (0.02)
% Population Ages 55-64	0.20 (0.03)	0.20 (0.04)
% Male	0.50 (0.01)	0.49 (0.02)
% Hispanic	0.19 (0.16)	0.16 (0.19)
% White	0.78 (0.14)	0.79 (0.13)
% Black	0.11 (0.11)	0.15 (0.12)
Unemployed Rate	0.09 (0.02)	0.08 (0.02)
Poverty Rate	0.15 (0.05)	0.16 (0.05)
Real Median Income ($10,000)	5.95 (1.46)	5.33 (1.49)
Uninsured Rate	0.19 (0.06)	0.24 (0.07)
Obama 2008 Vote share	0.46 (0.12)	0.38 (0.13)
Obama 2012 Vote share	0.43 (0.13)	0.35 (0.14)
Democratic Governor	0.76 (0.43)	0.49 (0.50)

Notes: The above table presents population weighted means and standard deviations of baseline variables, measured for 2009-2013, in counties within states that expanded Medicaid in 2014 and counties in non-expansion states.

4 Main Results

4.1 Medicaid and Mortality

We begin by assessing pre-trends in mortality outcomes, as reported in Figure 1. In Figure 1(a), point estimates for all-cause mortality are near zero and flat through 2013. Note that year-by-year mortality outcomes in 2010 to 2013 are not included in the propensity score model, so the flat pre-trends in this time frame serve as a crucial check on the ability of the propensity score model to construct balanced treatment and control groups. When we disaggregate all-cause mortality to examine amenable causes in Figure 1(b), we again find flat pre-trends. We conclude from these figures that the propensity-score model selects a comparison group with similar pre-expansion mortality patterns. Given that the ACA was passed in 2010, and some changes went into effect before 2014, we can also conclude that any effects before 2014 were uncorrelated with the later Medicaid expansion.

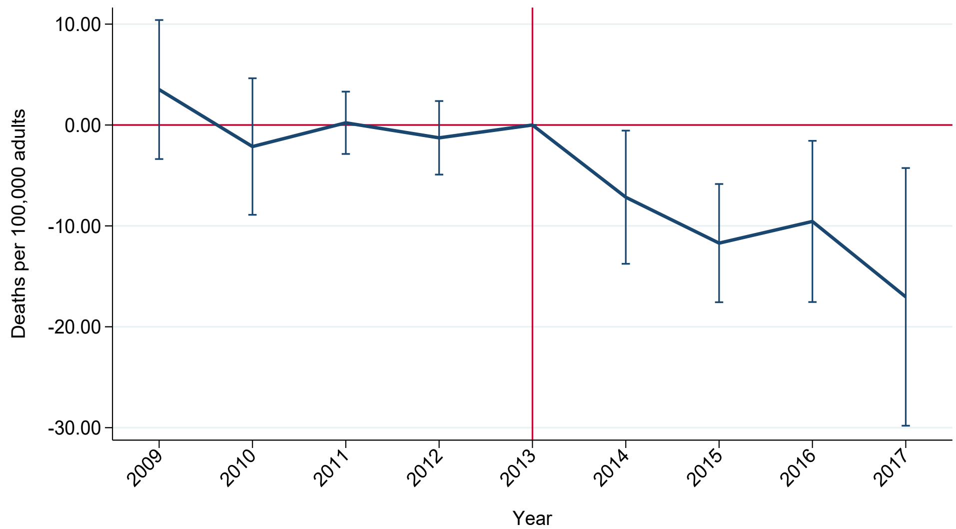
Moving to the post-expansion period, we see a notable reduction in all-cause mortality within the first four years of the expansion. Mortality falls in the first year after expansion, and is lower by approximately 10 deaths per 100,000 inhabitants by 2015 and 2016. In 2017 we find an even larger reduction in mortality of over 15 deaths per 100,000 inhabitants. The timing of the effects align with the expansion and show mortality reductions became larger over time. Disaggregating the post-expansion effects by causes of death that may be amenable to health care reveals that amenable causes account for the majority of the effects in aggregate mortality.[9]

Table 2 reports difference-in-differences estimates for the full sample, by gender and age group, and by pre-expansion uninsured levels. These results effectively pool together the 2014-2017 estimates reported in the event study. In column 1, we find that all-cause mortality drops by 14.83 deaths per 100,000 inhabitants. Adding controls in column 2 only slightly reduces the point estimate to 11.36 deaths per 100,000 averted, a 3.60 percent decrease in mortality. The reduction

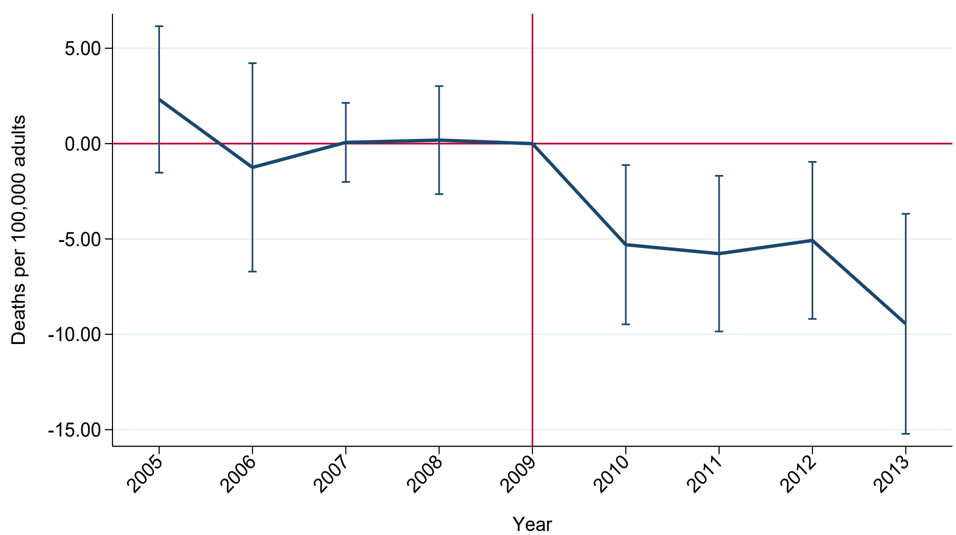
[9] Given that cause of death may be difficult to ascertain or classify as amenable or non-amenable, it is not surprising that we find larger effects on all-cause mortality. Results broken out for non-amenable cause mortality show negative point estimates that are not statistically distinguishable from zero.

Figure 1: Event Study: ACA Medicaid Expansion and Mortality

(a) All-Cause Mortality



(b) Amenable Mortality



Notes: The above figure shows event-study plots of Medicaid expansion on (a) all-cause mortality and (b) amenable causes of mortality for adults aged 20 to 64 year. The vertical line indicates the year prior to the effective date of when expansion. Bands indicate 95% confidence intervals. Standard errors are clustered at the state level. Models include year and county fixed effects. Controls include the county unemployment rate, the percentage of population that is white, the percentage of population that is between 55 and 64 years of age, the logged county population of adults between 20 and 64 years of age, the logged county population of adults between 35 and 44 years of age, and the logged county population of females between 20 and 64 years of age.

in effect size may occur due to time-varying differences between treatment and control captured by the controls; however, we cannot rule out that the difference reflects statistical noise. Both estimates are statistically significant at or below the conventional 5 percent level and we proceed with the model with controls as our preferred specification.

In columns 1 and 2 of Panels B, we see that the majority of the aggregate mortality effect is accounted for by changes in amenable causes of death, i.e., those which may be influenced by access to health care. The estimated reduction in amenable cause mortality is 7.79 and 6.64 deaths per 100,000 people with and without controls, respectively. Confidence intervals around these estimates rule out no response. The effects on amenable cause mortality are the strongest evidence of mortality reduction following the reform. Non-amenable causes of death make slightly smaller and statistically insignificant contributions to the total (coefficient=-7.04 se=4.45 without controls, coefficient=-4.72 se=2.54 with controls). Responses in non-amenable mortality represent an imperfect placebo test. Effects on non-amenable causes may occur due to the difficulty in classifying deaths as amenable cause, or secondary forces such as spillovers from access to care; however, it is reassuring that responses for these causes of death are smaller than those found for amenable causes. In sum, we find the response in all-cause mortality is largely explained by a reduction in causes of death likely to be influenced by access to health care.

In the middle columns of Table 2 we report results by gender and age group. In columns 3 and 4, the reported results imply that mortality effects are nearly equal between men and women in levels, but smaller in percentage terms for men, given their higher baseline mortality rates. As with the combined effect, we find larger and more precisely estimated reductions in amenable causes of death for each gender, which can explain the estimated effects on all-cause mortality for both genders. The analysis of effects by age groups finds the largest effects for individuals approaching Medicare eligibility, in the 55-to-64 age group. We cannot, however, reject a proportionate drop in mortality across other age groups.

In columns 10 and 11 we report results in counties with high and low rates of uninsured individuals. Here we reweight counties after classifying them as being above or below the median

Table 2: Effect of ACA Medicaid Expansion on Mortality

	Full Sample		*Gender*		*Age Groups*					*Uninsured*	
	Base	*Controls*	*Males*	*Females*	*20-24*	*25-34*	*35-44*	*45-54*	*55-64*	*High*	*Low*
Model and Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: All Cause Mortality											
Expansion x Post	-14.83**	-11.36***	-11.36***	-11.60***	-0.95	-5.19	-7.93**	-15.35*	-20.65*	-12.32***	-9.42**
	(6.12)	(3.59)	(4.27)	(3.31)	(2.61)	(3.99)	(3.42)	(8.51)	(11.09)	(4.47)	(3.96)
% Effect Relative to Baseline	-4.71	-3.60	-2.88	-4.89	-1.16	-5.37	-4.94	-4.04	-2.60	-3.89	-2.97
Panel B: Amenable Cause Mortality											
Expansion x Post	-7.79***	-6.64***	-5.94***	-7.41***	0.29	1.07	-2.44*	-8.12*	-20.22***	-7.02**	-6.63***
	(2.15)	(1.93)	(2.13)	(2.15)	(0.39)	(1.21)	(1.37)	(4.86)	(6.55)	(2.72)	(2.17)
% Effect Relative to Baseline	-3.88	-3.31	-2.48	-4.55	2.48	4.49	-3.40	-3.40	-3.33	-3.54	-3.25
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	45	45	45	45	45	45	45	45	45	34	41
Observations	20340	20340	20340	20340	20340	20340	20340	20340	20340	11142	11016

Notes: Observations include annual county-level mortality rates for adults aged 20 to 64 years. Columns (1) and (2) include results for the full sample with and without controls. In columns (3) and (4) we report results for males and females, respectively. In columns (5) through (9) we report results for five separate age groups. In columns (10) and (11) we report heterogeneous effects by county baseline uninsured levels, where *High*/*Low* uninsured counties are defined as being above/below the median baseline uninsured rate for individuals aged 19 to 64 years. Coefficients indicate deaths per 100,000 people. Standard errors are clustered at the state level. All models include year and county fixed effects. Controls include the county unemployment rate, the percentage of population that is white, the percentage of population that is between 55 and 64 years of age, the logged county population of adults between 20 and 64 years of age, the logged county population of adults between 35 and 44 years of age, and the logged county population of females between 20 and 64 years of age.

baseline uninsured rate for non-elderly adults, 20.2 percent. Not surprisingly, we find larger mortality reductions between *high* and *low* uninsured counties. Our estimates bracket the main effects in column 2 and imply a reduction of 12.32 and 9.42 deaths per 100,000 residents in counties above and below the median baseline uninsured rate, respectively. These estimates are again largely accounted for by a drop in amenable cause mortality. These results suggest a larger reduction in mortality in counties with high and low uninsured rates.

We report results for the detailed cause-of-death analysis in Appendix Table A.4. We find reductions in cardiovascular and respiratory-related mortality (e.g. due to heart attacks, strokes, chronic obstructive pulmonary disease, asthma). These results echo previous research that has shown fewer uninsured cardiac surgery patients and improved predicted risk scores and morbidity rates among cardiovascular related illnesses in states that expanded Medicaid (Charles et al., 2017) as well as substantial increases in the use of cardiovascular and respiratory related prescription medications (Ghosh, Simon and Sommers, 2017). Most people with cardiovascular or respiratory diseases experience very large benefits from access to the correct medication, such as statins for cardiovascular disease; however, cost can be a barrier to accessing these treatments. According to the CDC, 80 percent of people with uncontrolled high blood pressure or cholesterol were uninsured in 2011. There is persuasive evidence that connects higher Medicaid prescription drug spending reduces mortality, particularly among cardiovascular, circulatory, and respiratory diseases (Clayton, 2019). Thus, responses on these disease margins are consistent with the potential for large gains from effective treatments that could be accessed through Medicaid (Centers for Disease Control and Prevention, 2011).

We also examine the effect of Medicaid expansion on diseases of despair, including suicide, opioid overdoses, and drug and alcohol poisonings. These causes of death, particularly opioid overdoses, have risen over the previous two decades. While the connection between these causes of death and Medicaid is more complex than with cardiovascular diseases and respiratory disease, access to health care may also influence them. For suicides, the results suggest reductions in the 25-to-34 and 55-to-64 age groups. Opioid overdoses and drug and alcohol poisonings show little response, although we conclude that there is insufficient statistical power to evaluate the effect of the Medicaid expansion on these causes of death. We caution against drawing strong conclusions from individual sub-analyses as our research design is intended to estimate effects at a higher level of aggregation and there is potential for multiple hypothesis testing issues as we disaggregate the analysis.

By most measures, these point estimates imply large reductions in mortality. Individual-level TOT effects can be calculated based on a 4.15 percent gain in health insurance among all non-elderly adults, which we estimate using county-level health insurance as the outcome for the same sample and specification as above (results appear in Appendix Table A.3). Using this "first stage" implies an implausibly large 86 percent reduction in mortality relative to the gain in percent insured. When we adjust the estimate by the ratio of affected to average mortality, the percentage effect becomes more credible. Specifically, we combine the first-stage estimate with national mortality data that includes mean individual incomes, deaths, and population counts from Chetty et al. (2016). Adjusting for the higher mortality rate among the Medicaid population relative to average, the estimated TOT effect falls to between 29 and 31 percent. Taken together, our calculations imply that one death was averted for every 310 newly covered individuals.[10] As discussed in the introduction, this scaling makes several assumptions that may not hold and we interpret the evidence as consistent with effects accruing in a broader population than those who receive insurance as a result of Medicaid expansion.

To summarize, we find consistent and meaningful reductions in all-cause mortality among adults 20-to-64 years of age in counties in states that chose to expand Medicaid in 2014 relative to the control group. The aggregate mortality reductions are largely explained by reductions in causes of mortality where Medicaid should have the largest effects and in counties where gains in insurance are predicted to be the largest.

5 Robustness

In this section we first discuss results from a wide range of alternative specifications. We then present and discuss results from a permutation test designed to gauge the likelihood that our model is indeed capturing causal morality effects. We conclude this section by discussing mortality effects among elderly adults and the role of spillovers.

[10] The Chetty et al. (2016) data is limited to ages 40 and older, so we included the ages 40 to 64 in 2013. The mean mortality rate in this age range is approximately 344 deaths per 100,000 people. We then calculate the average mortality rate for individuals with incomes at or below 138 percent of the federal poverty level. This corresponded to about 1037 deaths per 100,000 using the income threshold for individuals and 967 deaths per 100,000 using the threshold for a household of two. The adjusted mortality effect is then between $0.86 \times \frac{344}{1037} = 0.29$ and $0.86 \times \frac{344}{967} = 0.31$). For the per beneficiary effects, a 4.15 percent first stage implies 4,436,350 newly covered individuals based off a population in expansion states of approximately 106,900,000. Using the death rates for the total (344) and expansion (1037) population, a 3.60 percent reduction in mortality corresponds to 14,345 reduced deaths, and $\frac{4{,}436{,}350}{14{,}345} = 310$.

Table 3: Alternative Specifications: Effect of Medicaid Expansion on Mortality

	All Cause		*Amenable*	
	No Controls	*Controls*	*No Controls*	*Controls*
Model	(1)	(2)	(3)	(4)
No Pre-treatment Outcomes	-12.97**	-10.12***	-7.10***	-6.35***
	(6.05)	(3.58)	(1.99)	(1.95)
Imbens Selection Procedure	-14.52**	-11.73***	-7.22***	-6.27***
	(6.27)	(3.75)	(2.08)	(2.03)
Ridge Regression	-10.76*	-10.55***	-6.42***	-6.61***
	(6.04)	(3.22)	(2.07)	(1.81)
1:1 Nearest Neighbor Matching	-14.98***	-9.37**	-7.22***	-5.48**
	(5.17)	(3.71)	(1.83)	(2.27)
2:1 Nearest Neighbor Matching	-15.98***	-11.54***	-8.05***	-6.36***
	(5.59)	(3.56)	(2.32)	(2.04)
Adding Late Expanding States	-11.22**	-10.10***	-6.77***	-6.60***
	(5.57)	(3.16)	(1.95)	(1.55)
Exclude Early Expanding States	-12.74**	-9.13**	-6.70***	-5.48***
	(6.43)	(3.82)	(2.23)	(1.80)
Average Treatment Effect	-17.15***	-14.36***	-8.80***	-7.60***
	(5.91)	(3.15)	(2.27)	(1.93)
County-Specific Trends	-14.75**	-12.06***	-7.74***	-6.96***
	(6.46)	(3.91)	(2.27)	(2.06)
Logged Deaths	-0.031***	-0.030***	-0.026***	-0.028***
	(0.009)	(0.009)	(0.009)	(0.009)
Trim propensity score .1/.9	-6.66	-7.42*	-5.96***	-5.52**
	(5.68)	(4.16)	(2.17)	(2.15)
Controls	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes

Notes: Observations include annual county-level mortality rates for adults ages 20 to 64 for the years 2009-2017. Columns (1) and (2) include results for the full sample with and without controls. In columns (3) and (4) we report results for amenable causes of mortality. Coefficients indicate deaths per 100,000 people. Standard errors are clustered at the state level. All models include year and county fixed effects. Controls selected using double lasso for each model. In the first row we show estimated mortality effects without including pre-treatment outcome variables in the matching procedure. In the second row, we present estimated mortality effects using the Imbens variable selection procedure. In the third row, we present estimates where we use ridge regression to select variables for the propensity score. In rows four and five we present estimate using 1:1 and 2:1 nearest-neighbor matching, respectively. In row six we include states that expanded Medicaid after 2014 (AK, IN, NH, PA and MT). In row seven we excluded states that expanded Medicaid prior to 2014 (DC, DE, MA, NY and VT). In row eight we present estimates of the average treatment effect. In row nine we include county-specific linear time trends to the regressions. In row ten we define the outcome variables as logged deaths. In the final row we trim the sample of counties with propensity scores below 0.1 or above 0.9.

5.1 Alternative Specifications

We conduct a substantial range of robustness analyses, reported in Table 3. In the first five rows we use a number of alternative methods for the propensity score adjustment. First, we report results where we exclude pre-treatment outcomes in the double lasso procedure. We then report results from implementing the data-driven method outlined in Imbens and Rubin (2015) to select variables for the propensity-score model. Next, we show estimates where we replace the double lasso method with ridge regression for model selection. Finally, we present estimates using 1-to-1 and 2-to-1 nearest-neighbor matching (with replacement) in place of reweighing. We implement double lasso to select variables for these matching models. In all alternative specifications for model-selection, we find results that are very similar to our preferred estimates in both magnitude and precision.

We next consider robustness to the definition of treatment that focuses on initial adopters of the 2014 expansion. To do so, we add back the few states (PA, IN, NH, AK, MT, LA) that implemented the Medicaid expansion after the first half of 2014. We then exclude five states (DC, DE, MA, NY, and VT) that chose to expand provided Medicaid or similar coverage to low-income adults from 2009 through 2013 (Miller and Wherry, 2017). These states expanded Medicaid to low-income adults earlier than 2014 to help prepare for the main expansion in 2014. The results from these specifications again do not differ from our preferred estimates. Finally, we move from estimating the TOT to estimating average treatment effects. Here we weight our regression by population multiplied by $\frac{p}{1-p}$, where p is the estimated propensity score. The average treatment effects show moderately larger effect sizes, suggesting that non-expansion states may have benefited more from Medicaid expansion than those that adopted.

We also include county-specific linear trends to address concerns pertaining to county-level unobserved characteristics that may be evolving over time and correlated with mortality (e.g. changes in population compositions between counties with different ex ante insurance rates), potentially biasing our results. Including the time trends do little to change the estimates, reinforcing evidence that our model is not confounded by unobserved trends between expansion and non-expansion counties. We then estimate our model using logged deaths as opposed to death rates. We first add one death to each observation to avoid excluding (the very small number of) counties that experienced no deaths. The results using logged deaths closely mirror our estimates using mortality rates as the outcome, with estimates suggesting a 3 percent reduction in all-cause and amenable mortality. Finally, we explore how estimates change when we use alternative trimming our sample. We trim counties with propensity scores that are less than 0.1 or greater than 0.9, the extreme end of values suggested by Imbens and Rubin (2015). The results for all cause mortality are about half the size of our preferred estimates (and less precise). The difference in the all-cause estimates appears to be driven by non-amenable causes of death. Even with a more conservative trimming, we again find decreases in the causes of mortality most likely to be influenced by the ACA Medicaid expansion. The results for amenable causes are slightly smaller than our preferred estimates but remain statistically significant with and without controls.

We also explored how results change when we vary the penalty parameter during the double lasso selection procedure. Appendix Figure A.4 shows results for all-cause mortality across several values of the penalty. As we increase the penalty, fewer variables are selected for inclusion into the propensity score model. A penalty of zero corresponds to all variables selected for inclusion in the model while the largest value, "limit," corresponds to zero variables selected for inclusion. The optimal penalty is labeled "205*" and corresponds to our main result for all-cause mortality. As we increase the penalty level, the estimated mortality effects becoming small and statistically insignificant. This pattern suggests that as fewer variables are selected for inclusion, the corresponding estimated propensity scores become more noisy. As a result, the re-weighting adjustment does less to correct for pre-existing differences among treatment and control counties and reduces our power to detect mortality effects.

Finally, we performed a simple falsification exercise where we estimated our model using fatalities due to automobile crashes. These deaths are unlikely to be related to the Medicaid expansion. We analyzed automobile deaths among adults 20 to 64 years of age using data from the Fatality Analysis Reporting System (FARS) that is provided by the National Highway Traffic Safety Adminstration. FARS is an annual, nationwide census that provides counts of fatal injuries suffered in motor vehicle traffic crashes. As expected we find no response for motor vehicle related deaths, as shown in Appendix figure A.5. The pooled difference-in-differences estimates are very close to zero and do not approach statistical significance (coefficient=0.11, s.e.=0.43 without controls); (coefficient=0.06, s.e.=0.41 with controls).

5.2 Permutation Tests

In addition to investigating how our mortality estimates change across various alternative specifications, we also conducted a permutation test to investigate the likelihood that our model is identifying the causal effects of the ACA Medicaid expansion on mortality or whether the effects we find are simply due to chance as a result of potentially confounding issues such as model misspecification or lack of power (Black et al., 2019). The permutation test essentially compares our preferred estimates to a distribution of pseudo-treatment effects.

To conduct the permutation test, we first shift the sample period back four years and include the time-period from 2005-2013. We then randomly assigned treatment to 27 states, the same number of states that expanded Medicaid in 2014. States that expanded after the first half of 2014 are again excluded from the sample. The remainder of the states make up the control group. After the random assignment to treatment, we follow the same estimation procedure described in section 3. We conducted this procedure 1000 times and compared the psuedo-treatment effects to our preferred estimates.

The distribution of results from the permutation test are illustrated in Figure A.6. The figures show binned histograms with our preferred estimates represented by the dashed vertical line. For all-cause and amenable mortality, our preferred estimates are located in the extreme left tail of the distribution of pseudo-treatment effects. Table A.5 presents summary statistics from the test. For each cause of death, the mean treatment effects are small and generally positive in sign. There are also few estimates that are statistically significant at the conventional 5 percent level, the majority of which are *greater* than zero. The results of this test provide strong evidence that the mortality effects we estimate for the ACA Medicaid expansion are very unlikely to be due to chance.

5.3 Elderly Mortality, Triple Difference, and Spillovers

Estimating unbiased causal effects using a difference-in-differences design requires that treatment and control units would have followed parallel trends in the absence of treatment. In our setting, we have shown expansion and control counties did not follow parallel trends in mortality prior to the expansions becoming effective in 2014, which suggests that the unadjusted difference-in-difference would be biased. We have employed the commonly-used method of propensity score re-weighting in combination with model selection techniques from machine learning to address this issue. Our strategy has been effective at constructing a balanced treatment and control group, as measured by parallel pre-trends reported in Figure 1. An alternative strategy would have been to employ a triple-difference strategy using a separate control group that is unlikely to be affected by the Medicaid expansion. Black et al. (2019) follow Finkelstein and McKnight (2008) in using ages 65 to 74 as an additional control group.[11]

To assess this alternative design, we follow Black et al. (2019) and estimate a triple-difference model using mortality among adults ages 55 to 74 years of age with our re-weighted sample of counties. The results are presented in Appendix Table A.6; to make comparisons across groups with different underlying risks we define mortality as $log(\frac{\text{deaths}}{\text{100,000 people}} + 1)$. We find a marginally significant decrease of 3.4 percent for amenable causes of death. The effect sizes are quite consistent with our main results for the 55 to 64 age group, though less precisely estimated. Given the weight of evidence presented in our main analysis, these results strengthen our confidence in the findings of a reduction in amenable cause mortality for the Medicaid-eligible population.

There are several possible explanations for reductions in mortality among individuals above 65. First, individuals who turn 65 between 2014 and 2017 may have been newly insured through

[11] It is worth noting that the literature has largely moved past the use of 10-year age bins in the construction of counterfactuals, preferring tighter age ranges, as in Card, Dobkin and Maestas (2009), Huh and Reif (2017), and Duggan, Gupta and Jackson (2019)). One reason may be that causes of death change very significantly over the ages 55 to 74.

Medicaid prior to turning 65 and received health benefits that carryover to older ages, including longer life. We find evidence of growing effects in our main analysis, consistent with just such an accumulating effect of health insurance. A second explanation is spillovers. When we estimate mortality reductions among adults 70 to 74 years of age, a group that would have already been eligible for Medicare in 2014, we find statistically significant reductions in all-cause (coefficient=-33.65, s.e.=13.12) and amenable causes of death (coefficient=-34.08, s.e.=12.54). These effects correspond to 1.5 and 1.9 percent reduction, respectively. There is evidence that previous expansions in Medicaid eligibility have spillover effects among seniors. For instance, Sommers, Baicker and Epstein (2012) finds that among persons 65 years of age or older, Medicaid expansions in the early 2000s were associated with a small but significant reductions in the uninsured rate, cost-related delays in care, and absolute mortality.[12] Recent work has shown that the ACA Medicaid expansions significantly reduced the number of unpaid bills and increased credit scores for previously uninsured populations while also improving self-assessed health (Hu et al. (2016); Brevoort, Grodzicki and Hackmann (2017); Courtemanche et al. (2017)). We speculate that these findings, though focused on the newly insured, may very well translate into improved health outcomes for the elderly.

6 Welfare Gains

We consider two scenarios that provide us with total welfare gains associated with the estimated mortality reductions following the ACA Medicaid expansions. In our primary scenario, we focus on amenable causes of mortality among people 55 to 64 years of age, the group most likely to respond to the Medicaid expansion. Since amenable-cause mortality reductions in this age range do not account for the total estimated effect, we provide a second estimate that assumes that the individuals 45 to 54 years of age also experienced the same proportional reduction as our all-cause

[12] There is also evidence that spillover effects are not limited to older adults. For instance Bhatt and Beck-Sagué (2018) found the ACA Medicaid expansions to be associated with reductions in infant mortality rates, particularly among African American infants.

estimate of 3.60 percent. It is important to note that calculations consider only welfare gains from the estimated mortality reductions and do not represent the total welfare gains of the Medicaid expansions. In particular, we do not consider potential welfare gains associated with increases in individual or household financial stability, labor supply, and reductions in costly social activity such as crime.

To obtain the primary welfare estimate, we first calculate a "per-county" mortality reduction by multiplying the estimated amenable-cause percentage response in Table 2 for the 54-to-65 age-group by the relevant baseline mortality rate and population (in 100,000s) for the same age-group in expansion counties. We then multiply this value by the total number of expansion counties, 1,252, and by age-specific VSL estimates gathered from Aldy and Viscusi (2008). The procedure for the secondary estimate is similar except we calculate the "per-county" mortality reduction using the estimated all-cause percentage response of 3.60 percent and population of individuals 45-to-64 years of age.

Results from the welfare calculations are presented in Table 4. We estimate welfare gains (in 2017 dollars) at between \$25.1 and \$69.7 billion. These estimates equate to between 35 and 100 percent of the total expenditures associated with the Medicaid expansion. Given that an estimated 60 percent of Medicaid expenditures may be transfers, our estimates imply the mortality-related savings following the ACA expansions may cover the entire net cost of the program. This calculation sets aside several known issues in valuing mortality reductions. For example, the welfare benefits of mortality reduction could be adjusted for social welfare weights and VSL specific to the target population (see Finkelstein, Hendren and Luttmer (2016) for longer discussion), as well as harvesting effects (see Deryugina et al. (2016)). Nevertheless, even if we were to discount heavily the welfare gains from mortality, our point estimates would still imply that mortality reduction can significantly impact the program's cost effectiveness.

Table 4: Estimated Welfare Gains

Projections	Mortality Reduction	Counties	VSLY	Total Gains
Ages 55-64 Amenable	4.13	1252	\$4,867,840	**\$25,166,253,430**
Ages 45-64 All-Cause	6.64	1252	\$16,354,233	**\$69,764,303,186**

Notes: VSL values calculated as using estimates from Aldy and Viscusi (2008). All monetary values are reported in 2017 dollars. *Ages 55-64 Amenable* captures only amenable-cause mortality reductions among the population aged 55-64 years. *Ages 45-64 All-Cause* calculates welfare-gains using the all-cause mortality estimate of 3.60 percent for the population 45 to 64 years of age.

7 Comparison with Pre-ACA Expansions

The estimated per-beneficiary mortality reductions reported above are smaller than those found in previous studies. Several differences may explain the divergence. Most importantly, we have only four years of data following the reform. Effect sizes appear to grow over time, particularly for amenable-cause mortality. Additionally, our empirical model differs from specifications estimated in the previous literature.

To facilitate the comparison of our results with previous findings, we re-estimate our model using three early-2000s Medicaid expansions in New York, Arizona, and Maine as the treatments. We estimate both short- and long-run impacts on mortality using post-treatment windows of 4 and 8 years. We limit the follow-up to 8 years (end of year 2009) to avoid overlapping with the implementation of the ACA. The results, along with our estimates from the ACA Medicaid expansion, appear in Table 5. The estimated 4-year impact of the earlier reforms are strikingly similar to our estimated effects of the ACA expansion. The point estimates for all-cause and amenable mortality are nearly identical between columns 1 and 2. We also find that mortality reductions from the earlier reforms appear to grow over time, with 8-year estimates that approximately double in magnitude relative to the corresponding impact after 4 years.

Using the estimated 3.60 percent reduction in mortality and an estimated first stage equaling a 4.15 percent increase in insurance coverage in expansion counties relative to non-expansion counties, our findings for all-cause mortality imply one death prevented per 310 newly covered

Table 5: Effect of ACA Medicaid Expansions on Mortality: Comparison to Early 2000s Reforms

	ACA Medicaid Expansion	*Early 2000s Reforms (4-Year Estimates)*	*Early 2000s Reforms (8-Year Estimates)*
Model and Variable	(1)	(2)	(3)
Panel A: All Cause Mortality			
Expansion x Post	-11.36***	-10.65***	-22.60***
	(3.59)	(3.88)	(5.77)
% Effect Relative to Baseline	-3.60	-3.35	-7.11
Panel B: Amenable Cause Mortality			
Expansion x Post	-6.64***	-6.52**	-12.31***
	(1.93)	(2.66)	(3.77)
% Effect Relative to Baseline	-3.88	-2.89	-5.45
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
Clusters	45	248	372
Observations	20340	12040	18060

Notes: Observations include annual county-level mortality rates for adults aged 20 to 64 years. Column (1) reports the estimated effects of the ACA Medicaid expansion on mortality. In columns (2) and (3) we report estimates of Medicaid expansions in AZ, NY, and ME during the early 2000s on mortality 4 and 8 years after the policies began. Coefficients indicate deaths per 100,000 people. In column (1) the standard errors are clustered at the state level. In columns (2) and (3) the standard errors are two-way clustered at the county and state-by-year levels. All models include year and county fixed effects. Controls for each model selected using double lasso method.

individuals. For the early 2000s expansion, we estimate approximately one death saved per 295 newly covered using coverage data reported in Sommers (2017).[13] Taken together, this suggests the gains in coverage needed to reduce mortality are slightly larger following the ACA Medicaid expansion compared to earlier expansions after 4 years.

8 Conclusion

The results we report in this paper add to a growing body of literature on the effects of public health insurance, Medicaid, and the ACA on health outcomes. Findings from our study suggest that the

[13]County-level insurance data from SAHIE is not available prior to 2005. Therefore the per-beneficary calculation is based off estimates of 497,000 covered in Arizona, Maine, and New York four years following the Medicaid expansions and population of adults aged 20 to 64 years of 15.8 million in these states. An estimate of -10.65 per 100,000 people implies newly covered people per life saved = 1/((10.65/100000) x (15.8/.497)) = 295.

ACA Medicaid expansion reduced mortality in the portion of the population that is between 20 and 64 years of age by 11.36 deaths per 100,000, a 3.60 percent reduction, within the first four years of the reform. The estimated effects are concentrated in reductions in causes of death that are likely to be influenced by access to health care. Specifically, our most robust finding is a reduction of 6.64 deaths per 100,000 in amenable-cause mortality. Considering that the Medicaid expansion covered over 10 percent of the population in these states, and induced a 4.15 percent net increase in insurance coverage, these estimates imply that the program likely affected mortality through channels beyond the access to health care among newly-insured individuals. Improvement in welfare due to mortality responses in 2017 may cover the entire additional expenditure associated with Medicaid expansion.

We believe the estimates derived from the ACA Medicaid expansion provide us with several important advantages relative to the quasi-experiments available to previous researchers. First and most fundamentally, the ACA expansion affected a much larger share of the population than earlier policy reforms. Previous studies of Medicaid expansions have examined at most three state-level reforms. The preeminent study on the effects of Medicaid is the Oregon Health Insurance Experiment (OHIE), the only large-scale randomization of access to Medicaid, lacks sufficient statistical power to detect mortality effects of the magnitude we document here (Baicker and Finkelstein (2011); Finkelstein et al. (2012)). Second, the nationwide treatment and other details of the ACA reduce the likelihood that concurrent changes in state policy and mortality trends bias the results. Previous reforms were initiated by individual states and may have been accompanied by other changes in policy, or were motivated by specific mortality trends (Kaestner, 2012). In contrast, the federal government covered 100 percent of the initial costs of Medicaid expansion (and promised to cover 90 percent of the costs starting in 2020), greatly reducing the role of state health and budgetary conditions in the decision to expand. Third, we can conduct several important secondary analyses and robustness checks that do not appear in the previous work. Most notably, we assess pre-reform trends and outcomes in an event study framework, and examine the statistical performance of the model using permutation analysis. Finally, our study serves to update analyses of earlier Medicaid reforms to account for the changing mortality environment. A substantial portion, 20 percent, of the effects of early-2000s Medicaid expansions on mortality came through reductions in HIV-related mortality (Sommers, 2017). Since then, medical innovations have greatly reduced mortality due to HIV, while other causes of death have risen, in both relative and absolute terms (Okie (2010), Phillips (2014), and Case and Deaton (2015)). Our findings suggest that the ACA Medicaid expansion resulted in meaningful reductions in mortality, despite the changing mortality environment.

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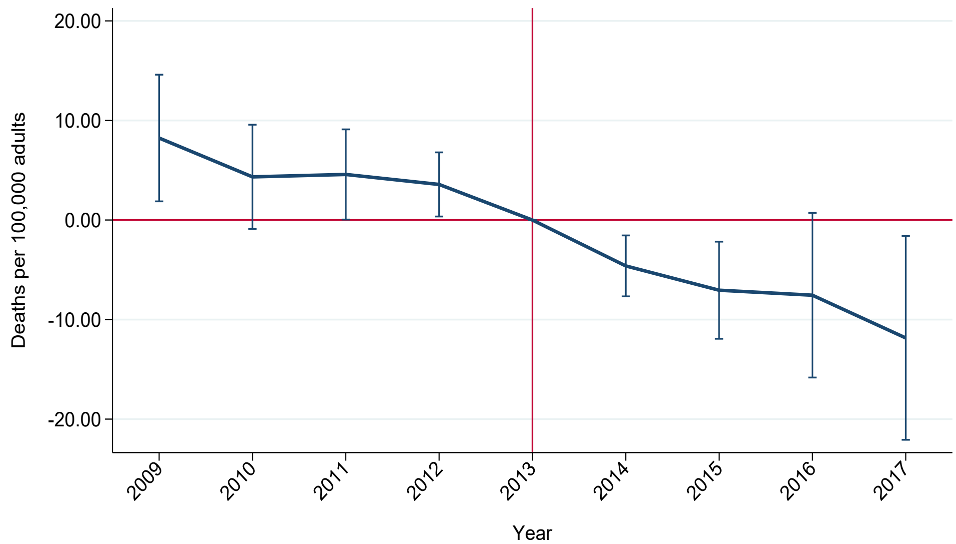
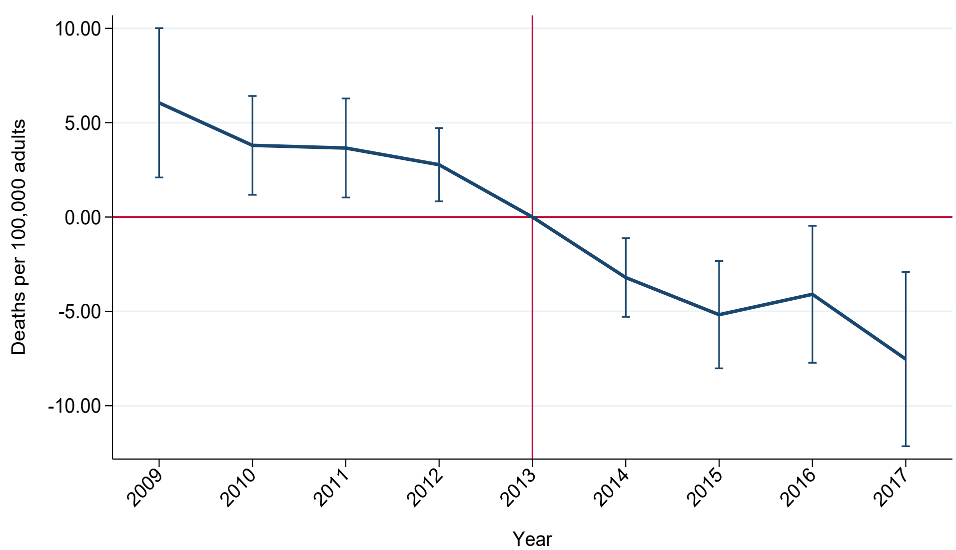
A Appendix Figures and Tables

Figure A.1: ACA Medicaid Expansion and Mortality Without Propensity Score Adjustment

(a) All-Cause Mortality



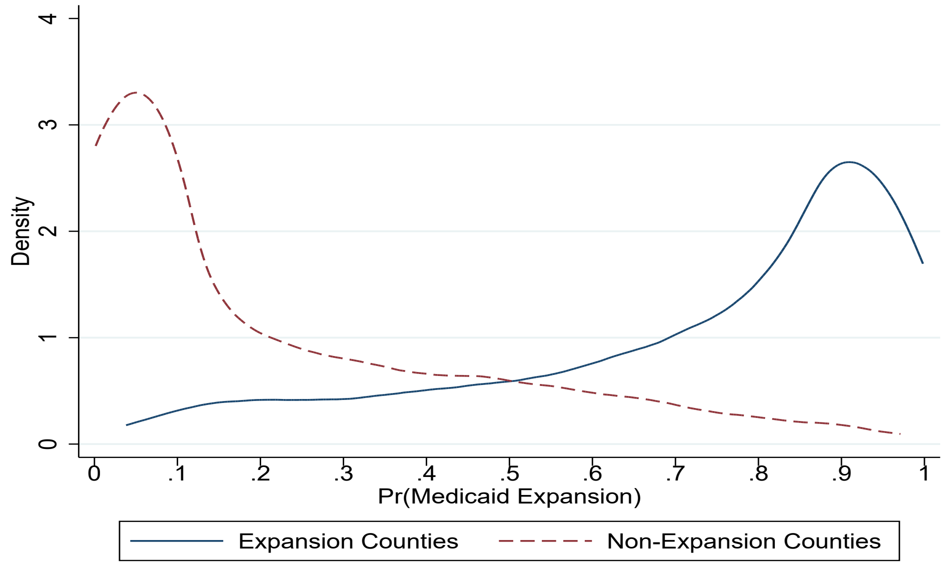
(b) Amenable Mortality



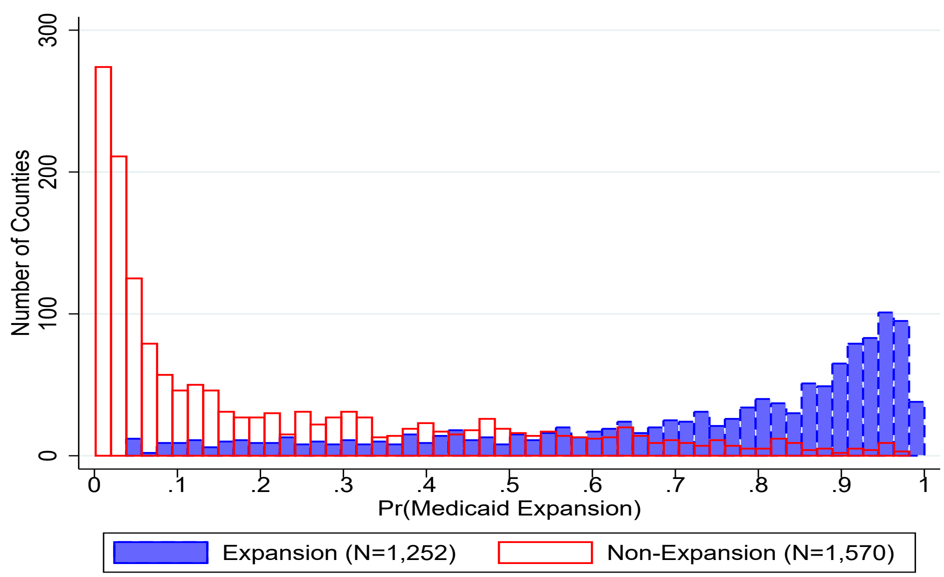
Notes: The above figure shows event-study plots of Medicaid expansion on (a) all-cause mortality and (b) amenable causes of mortality for adults aged 20 to 64 year. The vertical line indicates the year prior to the effective date of when expansion. Bands indicate 95% confidence intervals. Standard errors are clustered at the state level. Models include year and county fixed effects and are weighted by the 20-64 year old county population. Controls include the county unemployment rate, the percentage of population that is between 55 and 64 years of age, the logged county population of adults between 45 and 54 years of age, and the logged county population of females between 20 and 64 years of age.

Figure A.2: Propensity-Score Distribution

(a) Kernel Density



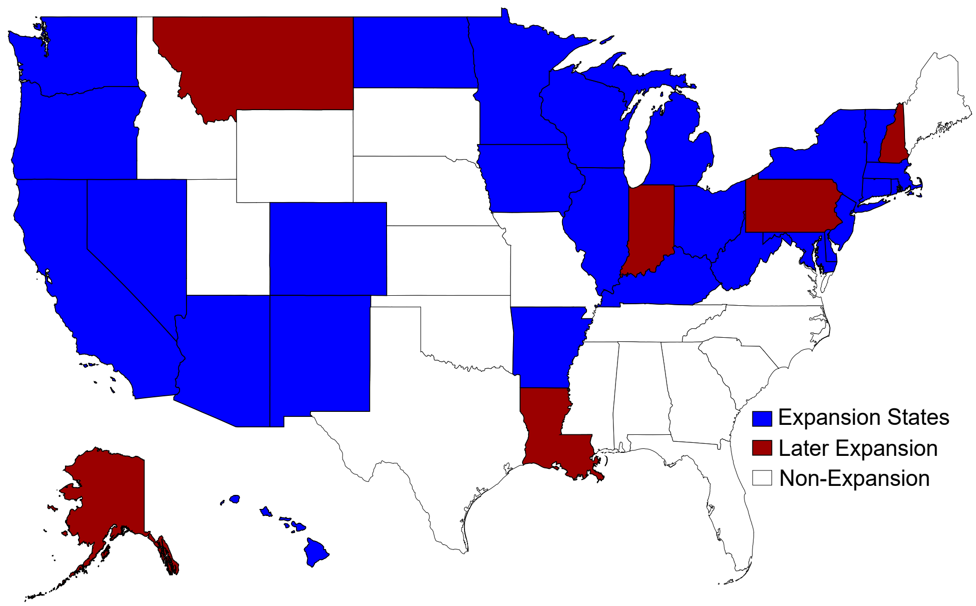
(b) Histogram



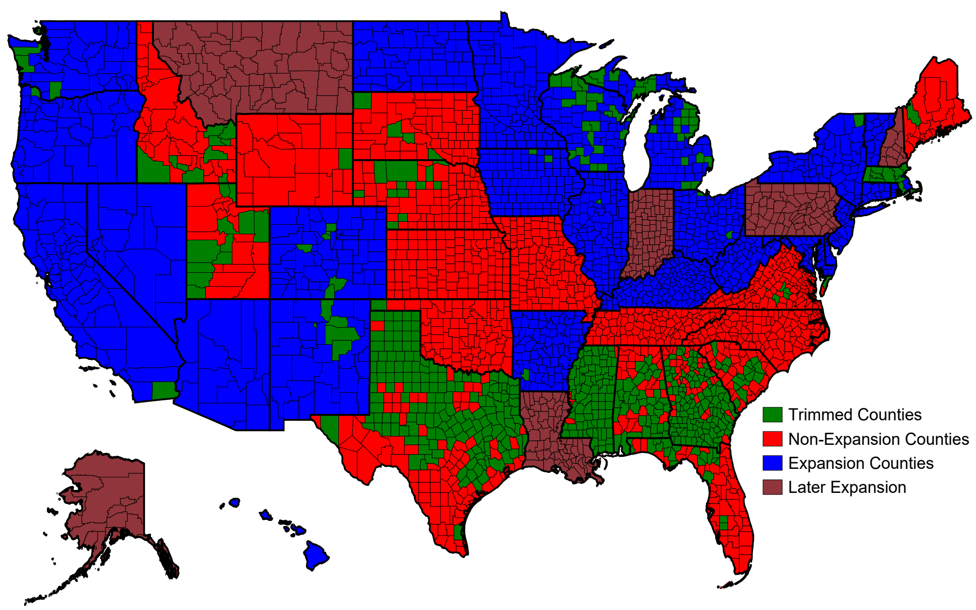
Notes: The above figures illustrate the kernel densities and histograms of the propensity score for both expansion and non-expansion counties.

Figure A.3: State and County Expansion Maps

(a) State Expansion Decisions

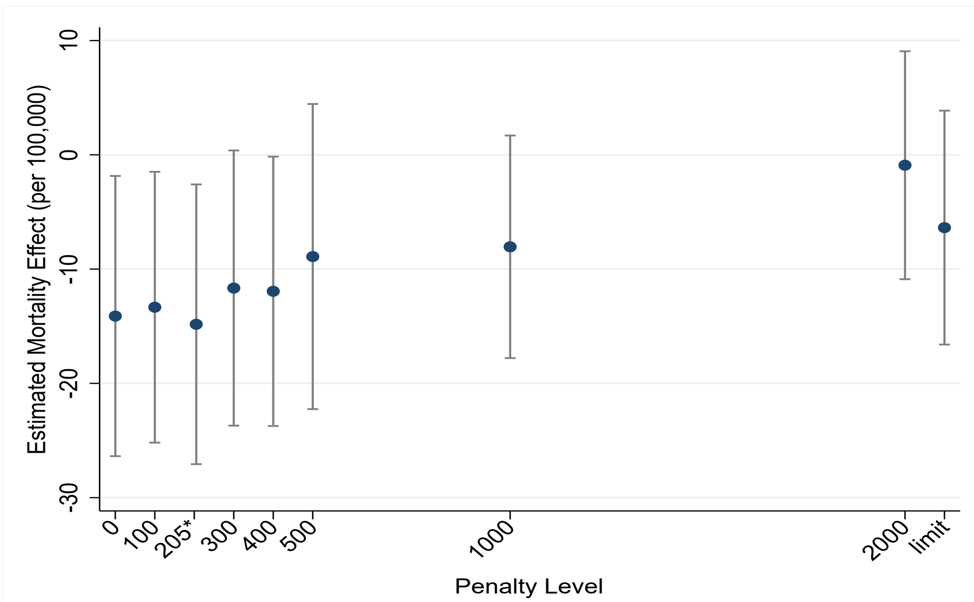


(b) County Classification



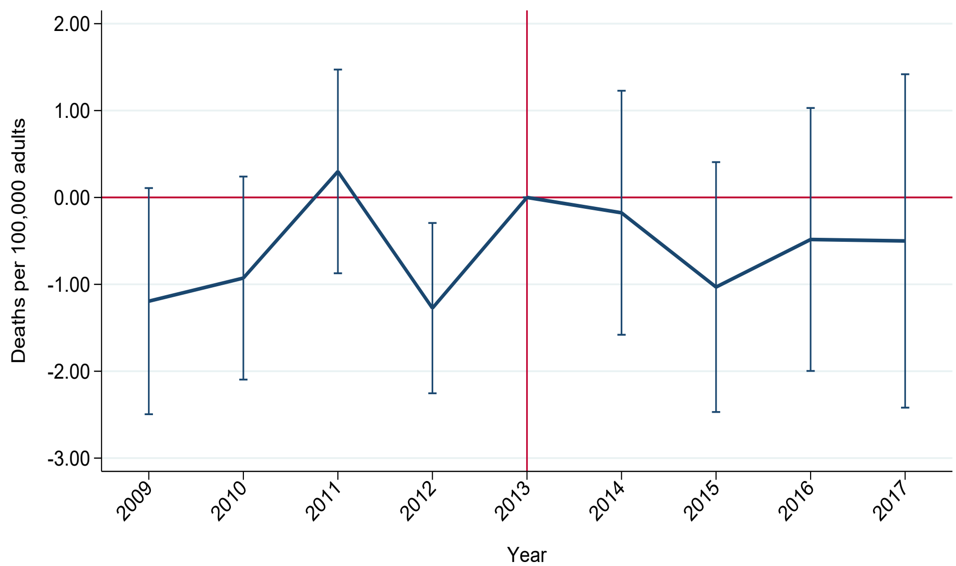
Notes: Map (a) shows state Medicaid expansion decisions. *Expansion States* are states that expanded Medicaid in the first half of 2014. States classified as *Later Expansion* are those that expanded Medicaid after the first half of 2014. *Non-Expansion* states have not expanded Medicaid. Map (b) depicts county classifications after the propensity-score estimation procedure.

Figure A.4: Effects Varying Lasso Penalty Parameter



Notes: The above figure depicts estimated all-cause mortality effects as a function of the penalty level employed in the lasso variable selection procedure. The level “205*” corresponds to our preferred estimates. The level ("limit") corresponds to no variables selected for inclusion in the propensity score model. Bands indicate 95% confidence intervals. Standard errors are clustered at the state level.

Figure A.5: Effects for Automobile Deaths

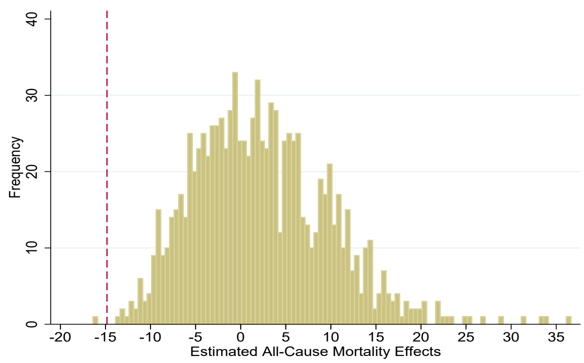


Notes: The above figure shows event-study plots of Medicaid expansion on automobile-related mortality for adults aged 20 to 64 year. The vertical line indicates the year prior to the effective date of when expansion. Bands indicate 95% confidence intervals. Standard errors are clustered at the state level. The model include year and county fixed effects. Controls the percentage of population that is white and the percentage of population that is between 55 and 64 years of age.

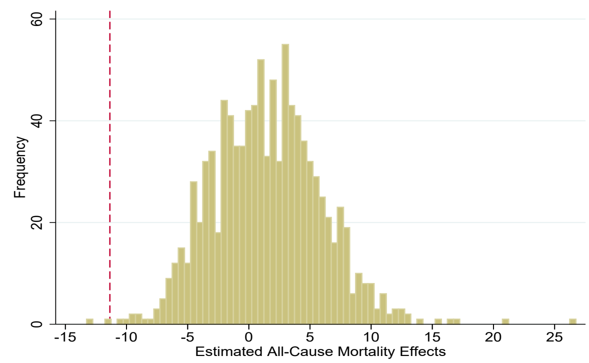
Figure A.6: Distribution of Effects from Permutation Tests

All-Cause Mortality

(a) No Controls

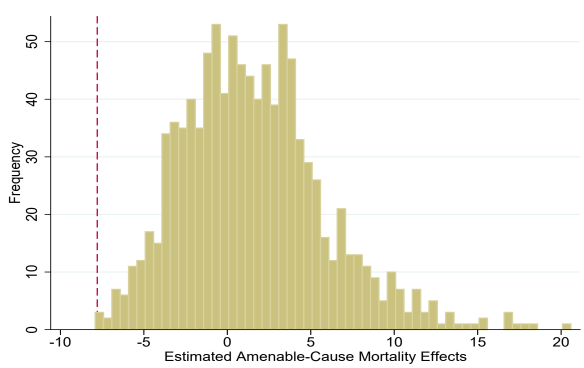


(b) Controls

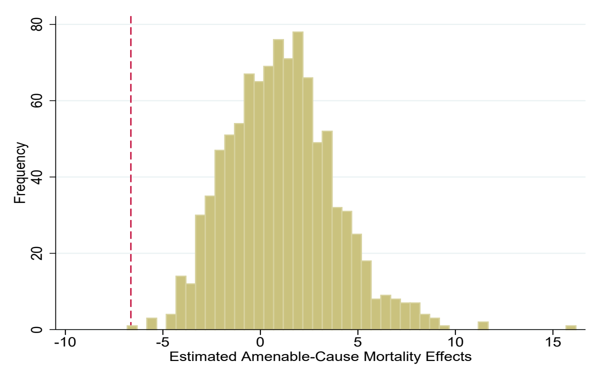


Amenable-Cause Mortality

(c) No Controls



(d) Controls



Notes: Figures shows histograms of the estimated mortality effects from 1000 simulations where treatment is randomly assigned to 27 states. The dashed vertical lines indicate all-cause and amenable mortality estimates from columns (1) and (2) in Table 2.

Table A.1: List of Amenable Health Conditions

Conditions	*ICD-10 Codes*
Infectious & Parasitic Diseases	A00-B99
Neoplasms (ALL)	C00-D48
Disorders of thyroid gland	E00-E07
Diabetes Mellitus	E10-E14
Epilepsy	G40-G41
Chronic rheumatic heart diseases	I05-I09
Hypertensive diseases	I10-I13, I15
Ischemic heart diseases	I20-I25
Cardiomyopathy	I42
Atrial fibrillation and flutter	I48
Other cardiac arrhythmias	I49
Heart failure	I50
Cerebrovascular diseases	I60-I69
All respiratory diseases	J00-J98
Gastric and duodenal ulcers	K25-K27
Gastrojejunal ulcers	K28
Diseases of appendix	K35-K38
Hernia	K40-K46
Diseases of gallbladder and biliary tract	K80-K83
Acute pancreatitis	K85
Infections of the skin and subcutaneous tissue	L00-L08
Infectious arthropathies	M00-M02
Glomerular diseases	N00-N07
Renal tubulo-interstitial diseases	N10-N15
Renal failure	N17-N19
Unspecified contracted kidney, small kidney unknown cause	N26-N27
Hyperplasia of prostate	N40
Pregnancy, childbirth and the puerperium	O00-O99
Congenital malformations originating in the perinatal period	P00-P96
Misadventures to patients during surgical and medical care	Y60-Y69, Y83-Y84

Notes: In the above table we report amenable health conditions with their associated ICD-10 codes.

Table A.2: Propensity-Score Regression: Estimated Probability of Expanding Medicaid

Variable	*Estimated Coefficient*
% Age 20-24	3.46
	(2.34)
% Age 45-54	10.35**
	(4.38)
% Age 55-64	6.98***
	(2.34)
% Male	12.58***
	(2.04)
% White	0.17
	(0.83)
% Black	-7.94***
	(0.85)
% Hispanic	1.66**
	(0.66)
Log Median Household Income	1.27***
	(0.41)
Unemployment Rate	31.77***
	(3.18)
Pre-2014 Uninsured Rate	-12.20***
	(1.73)
2008 Obama County Election Share	3.74***
	(1.42)
2012 Obama County Election Share	3.94***
	(1.42)
Democratic Governor	1.83***
	(0.13)
2005 All Cause Mortality Rate	-0.0013**
	(0.0006)
2006 All Cause Mortality Rate	-0.0010
	(0.0007)
2007 All Cause Mortality Rate	-0.0000
	(0.0007)
2008 All Cause Mortality Rate	0.0000
	(0.0007)
2009 All Cause Mortality Rate	0.0011*
	(0.0006)
Observations	2823

Notes: In the above table we report estimates from a propensity-score Logit model. The outcome is an indicator for Medicaid expansion in 2014 for each county in the sample. Standard errors are reported in parentheses.

Table A.3: First Stage: Medicaid Expansion and Percent Uninsured

	All Income Levels	All Income Levels	$\leq 138\%$ FPL	$\leq 138\%$ FPL
Expansion x Post	-4.20***	-4.15***	-8.33***	-8.18***
	(1.13)	(0.95)	(2.28)	(1.89)
Controls	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Clusters	45	45	45	45
Observations	20,337	20,337	20,337	20,337

Notes: Notes: Observations include annual county-level uninsured rates for adults aged 19 to 64 years. Data is from the Small Area Health Insurance Program through the U.S. Census. Columns (1) and (2) include results for uninsured rates for all income levels. In columns (3) and (4) we report results for adults with incomes at or below 138 percent of the federal poverty level. Standard errors are clustered at the state level. All models include year and county fixed effects. Controls selected using double lasso procedure include the the percentage of population that is white, the percentage of population between 25 and 34 years of age, the percentage of population between 55 and 64 years of age, and the logged county population of adults between 45 and 54 years of age.

Table A.4: Effects of Medicaid Expansion on Detailed Causes of Mortality

Model and Variable	*Full Sample* *Base* (1)	*Controls* (2)	*Gender* *Males* (3)	*Females* (4)	*Age Groups* *20-24* (5)	*25-34* (6)	*35-44* (7)	*45-54* (8)	*55-64* (9)
Panel A: Cardiovascular Mortality									
Expansion x Post	-2.46***	-2.18**	-1.60	-2.78***	0.35**	1.10	-1.40*	-4.93**	-5.29*
	(0.88)	(0.87)	(1.29)	(0.94)	(0.18)	(0.79)	(0.77)	(2.35)	(2.76)
% Effect Relative to Baseline	-3.89	-3.45	-1.80	-7.34	15.28	17.30	-5.87	-6.22	-2.81
Panel B: Respiratory Mortality									
Expansion x Post	-2.86***	-2.51***	-2.33***	-2.70***	0.42	-0.09	-0.26	-1.40*	-8.16***
	(1.11)	(0.67)	(0.66)	(0.71)	(0.31)	(0.16)	(0.34)	(0.80)	(2.04)
% Effect Relative to Baseline	-16.37	-14.37	-12.33	-16.78	26.75	-3.63	-5.09	-7.89	-14.55
Panel C: Suicides									
Expansion x Post	-0.85	-0.49	-0.65	-0.32	-0.70	-1.70***	0.54	0.24	-1.24**
	(0.69)	(0.62)	(0.89)	(0.38)	(0.85)	(0.56)	(1.29)	(0.88)	(0.59)
% Effect Relative to Baseline	-5.56	-3.21	-2.74	-4.64	-5.34	-12.82	3.58	1.32	-7.62
Panel D: Opioid Overdoses									
Expansion x Post	-0.19	-0.03	-0.14	0.07	0.57**	0.12	-0.28	0.01	-0.32
	(0.36)	(0.27)	(0.46)	(0.15)	(0.26)	(0.46)	(0.36)	(0.34)	(0.27)
% Effect Relative to Baseline	-7.14	-1.13	-4.42	3.26	35.63	5.17	-9.96	0.27	-12.55
Panel E: Drug and Alcohol Poisonings									
Expansion x Post	-3.37	-1.90	-3.01	-0.91	-1.39	-5.25	-3.61	-1.78	2.10
	(4.58)	(3.01)	(4.79)	(1.36)	(1.87)	(5.07)	(3.53)	(3.72)	(1.37)
% Effect Relative to Baseline	-25.47	-14.36	-16.78	-10.54	-15.36	-41.27	-25.51	-10.02	18.36
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	45	45	45	45	45	45	45	45	45
Observations	20340	20340	20340	20340	20340	20340	20340	20340	20340

Notes: Observations include annual county-level mortality rates for adults aged 20 to 64 years. Columns (1) and (2) include results for the full sample with and without controls. In columns (3) and (4) we report results for males and females, respectively. In columns (5) through (9) we report results for five separate age groups. Coefficients indicate deaths per 100,000 people. Standard errors are clustered at the state level. All models include year and county fixed effects. Controls include the county unemployment rate, the percentage of population that is white, the percentage of population between 55 and 64 years of age, the logged county population of adults between 20 and 64 years of age, the logged county population of adults between 35 and 44 years of age, and the logged county population of females between 20 and 64 years of age.

Table A.5: Summary Statistics Of Permutation Test

	Mean Effect (1)	*Std. Dev.* (2)	Min/Max (3)	*Signif. Effects <0* (4)	*Signif. Effects >0* (5)
All Cause Mortality (No Controls)	2.26	7.47	-16.37/36.47	35	125
All Cause Mortality (Controls)	1.51	4.45	-13.26/26.71	37	131
Amenable Mortality (No Controls)	1.58	4.32	-7.92/20.43	29	115
Amenable Mortality (Controls)	1.09	2.74	-6.82/16.01	29	119

Notes: The above table reports summary statistics from 1000 permutation tests. Columns (1)-(3) report means, standard deviations, and min/max effects. Columns (4) and (5) present the number of results (out of 1000) that are statistically significant at the 5% level.

Table A.6: Triple Difference: Ages 55-64 vs. 65-74

	All Cause		*Amenable*	
	No Controls	*Controls*	*No Controls*	*Controls*
Model	(1)	(2)	(3)	(4)
Expansion x Post	-0.010	-0.008	-0.010	-0.009
	(0.011)	(0.009)	(0.011)	(0.009)
Under 65	-0.316***	-0.316***	-0.381***	-0.381***
	(0.021)	(0.021)	(0.021)	(0.021)
Expansion x Post x Under 65	-0.023	-0.023	-0.034*	-0.034*
	(0.019)	(0.019)	(0.019)	(0.019)
Controls	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Clusters	45	45	45	45
Observations	40602	40602	40602	40602

Notes: Observations include annual county-level mortality rates for adults ages 55 to 74 years of age for the years 2009-2017. Columns (1) and (2) include results for the full sample with and without controls. In columns (3) and (4) we report results for amenable causes of mortality. Coefficients indicate logged deaths per 100,000 people (plus 1) . Standard errors are clustered at the state level. All models include year and county fixed effects. Controls selected using double lasso for each model.