MEDICAID AND MORTALITY: NEW EVIDENCE FROM LINKED SURVEY AND ADMINISTRATIVE DATA

Online Appendix

January 8, 2021

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Contents

1 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 were 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, and most of these switches occurred in 2016.

The switch to T-MSIS generates a move from fiscal year to calendar year reporting and sometimes affects the enrollment data available. 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 cumulative 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, 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.

2 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.

3 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 sample 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.[1] 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.[2] 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

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

[2]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.

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.

4 Sensitivity to Additional Control Variables, Methods of Conducting Inference, and Unobserved Non-linear Differential Trends

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. We also 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. 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.

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 are presented in 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.[3] The results are reported in Table A8. As seen from this table, the standard errors are very similar to those in our main model.

Finally, we also consider the presence of non-linear differential 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 Details on Staggered Treatment Timing with Heterogeneous Treatment Effects

In this section, we provide additional details on how the staggered nature of the ACA Medicaid expansions affects our results. As stated in the main text, Sun and Abraham (2020) show that, if each treatment cohort has a different profile of time-varying treatment effects, the event study estimates can be contaminated by treatment effects from other periods. This is a potential concern 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,

[3] 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.

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 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, Figure 3 in the main text, 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 Figure A2.[4] The results using this method are very 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.

6 Triple Difference Estimates

We formally test for a differential effect in our main sample relative to the two placebo samples we identified: those age 65+ in 2014 and those with incomes at or above 400% FPL. These groups were less affected by the ACA Medicaid expansions (see Figure 4 in the main text). We use these groups as additional control groups and estimate a "triple difference" model. To estimate this model, we include respondents in each placebo group in our sample. We construct 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. Specifically, we estimate:

$$\begin{aligned} Y_{isjt} = &\sum_{\substack{y=-6 \\ y\neq -1}}^{3} \beta_y I(t - t_s^* = y) \times Expansion_s \times Treated_i + \beta_{t\times s} \\ &+ \beta_{s\times Treated} + \beta_{t\times Treated} + \beta_j + \gamma \mathbf{I}(j = t) + \epsilon_{isjt}, \end{aligned}$$

where $Treated_i$ equals 1 for our main sample, and 0 for the comparison sample (those age 65+ or in households earning 400%+ of the FPL in turn). We also estimate an analogous triple differences model that replaces the event study indicators with an indicator that the year is during the post-expansion period. One 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 Figure A6. As illustrated by this figure, we

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

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 the triple difference model that uses the higher income group. The triple difference estimates, reported in the bottom two rows of Figure 3 in the main text, 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 4 in the main text for those in high income households) or modest spillover effects occurring in the placebo groups.[5] 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.

7 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 the NBER. 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 administrative 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 OHIE 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

[5] 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.

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 comparison of quasi-experimental estimates undertaken by Goodman-Bacon (2018), which includes mortality effects of Medicaid observed for infants and children.[6] 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.[7] 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.[8]

As described above, we convert the estimates into proportional mortality effects using the reported

[6] 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).

[7] 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.

[8] 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, while 22.9 percent appeared at some point during the year in the CMS adminstrative records; this suggests an undercount based on survey data of approximately 31.4%.

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 the 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 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 VIII in the text.

We follow the parametric bootstrap procedure outlined in Goodman-Bacon (2018) to estimate confidence intervals for the estimated average treatment 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 to 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.

8 References

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9 Appendix Tables and Figures

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 × 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)	-0.041 (0.093)	-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 V.A and Appendix Section 3 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. All estimates 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 VI.A.

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 VI.A.

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 (0.019)***	46.75 (8.41)***	0.409 (0.061)***	-0.059 (0.010)***	-0.001012** (0.00050)
Event Study Model:						
Year 3	0.466 (0.033)***	NA	NA	0.732 (0.076)***	-0.061 (0.013)***	-0.00199 (0.00081)**
Year 2	0.482 (0.027)***	0.148 (0.0271)***	49.87 (9.273)***	0.516 (0.061)***	-0.070 (0.011)***	-0.00098 (0.00054)*
Year 1	0.475 (0.026)***	0.142 (0.020) ***	55.59 (11.97)***	0.327 (0.044)***	-0.065 (0.010)***	-0.00133 (0.00048)**
Year 0	0.489 (0.023)***	0.121 (0.019)***	34.80 (7.00)***	0.134 (0.020)***	-0.042 (0.007)***	-0.00084 (0.00045)*
Year -1 (Omitted)	0	0	0	0	0	0
Year -2	0.012 (0.008)	-0.010 (0.007)	-2.48 (2.18)	-0.021 (0.012)*	0.002 (0.008)	-0.00040 (0.00051)
Year -3	0.010 (0.012)	-0.007 (0.012)	-3.03 (4.08)	-0.035 (0.023)	0.009 (0.008)	-0.00020 (0.00059)
Year -4	0.008 (0.010)	-0.0007 (0.012)	-0.60 (3.17)	-0.067 (0.034)*	-0.005 (0.010)	0.00055 (0.00077)
Year -5	0.009 (0.012)	-0.006 (0.016)	-0.014 (3.98)	-0.067 (0.039)*	0.001 (0.011)	0.00055 (0.00077)
Year -6	0.007 (0.012)	-0.021 (0.023)	-4.20 (5.99)	-0.095 (0.048)	-0.001 (0.016)	-0.00069 (0.00083)
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 VI.C 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***
	(0.00050)	[0.051]	(0.00042)
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 Appendix Section 4 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	Mortality 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 VII 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 VII 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 (ACS-Numident) Counterfactual rate	Mortality (ACS-Numident) Change
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 VII 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 7 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 A9	11.3 (8.2 to 27.3) Table A9	7.8 (5.5 to 10.0) Table A9	-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 eligibility; 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 7.

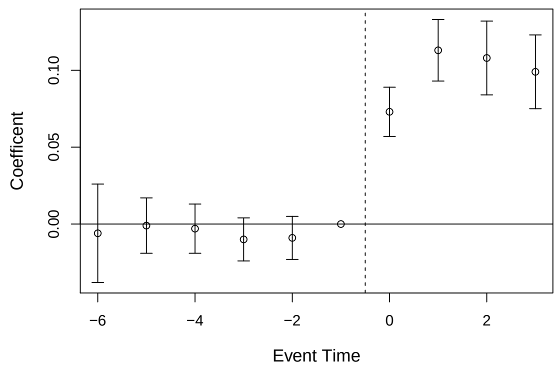
Adjustment (1): Multiply baseline mortality rate by 1.60 to account for higher relative risk of death for poor adults, calculated using linked NHIS-mortality data (see Appendix Section 7).

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

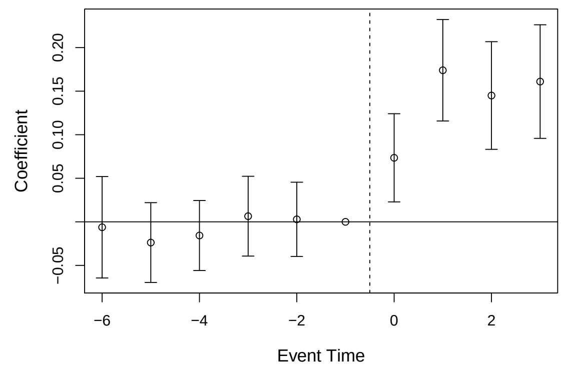
Table A14: Sources for Parent and Adult Medicaid Eligibility Rules by Year

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.

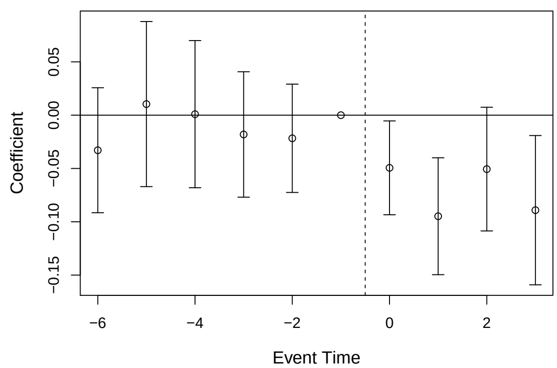
Figure A1: Effects of the ACA Medicaid Expansions on Medicaid and Insurance Coverage: Alternative Measures



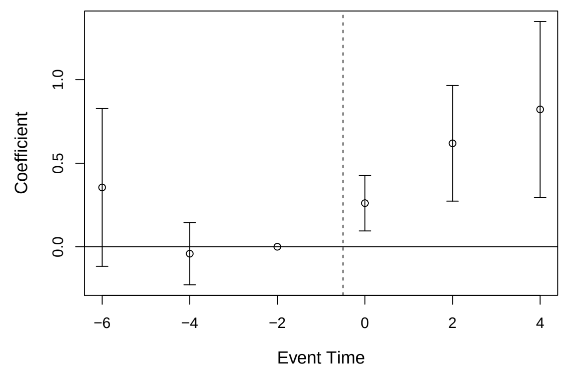
(a) Medicaid Coverage (ACS)



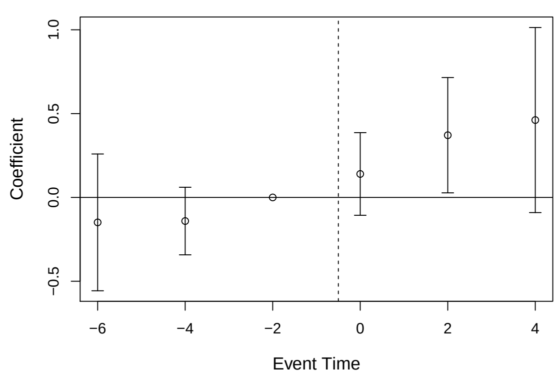
(b) Medicaid Coverage (NHIS)



(c) Uninsured (NHIS)



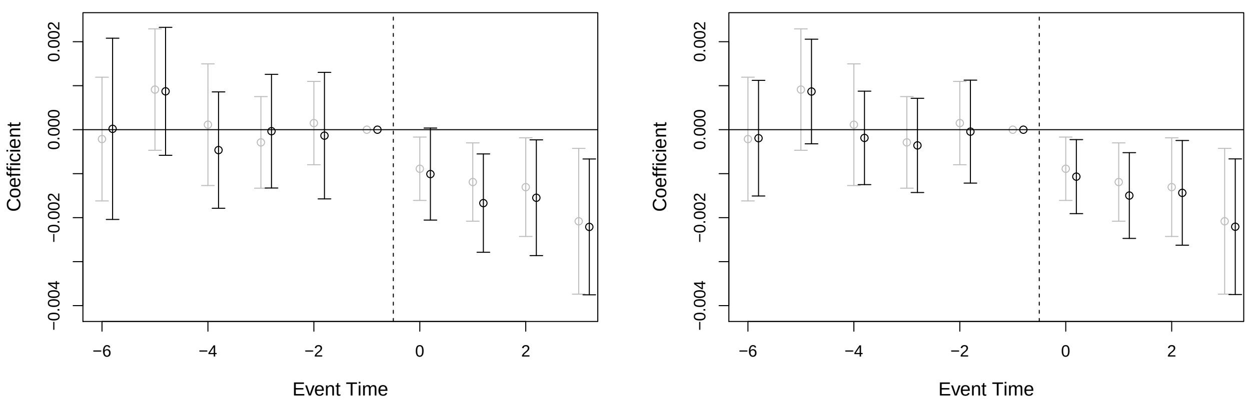
(d) Cumulative Medicaid (HRS)



(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 V.A and Appendix Section 3 for additional information.

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

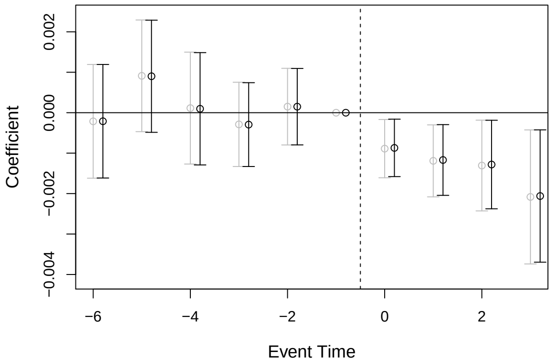


(a) 2014 expanders only

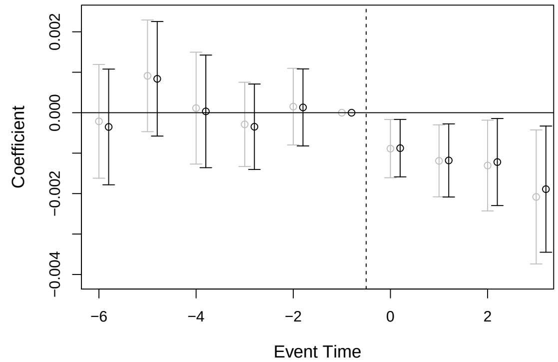
(b) Sun and Abraham (2020) method

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 Appendix Section 5 for more details.

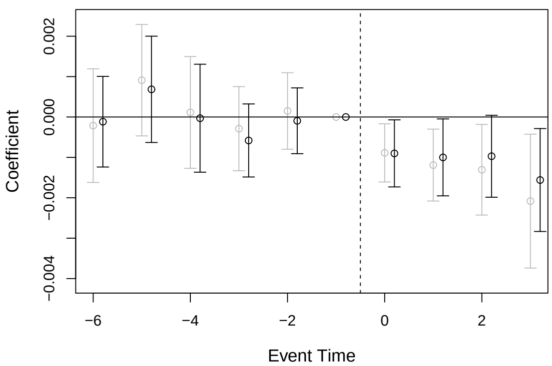
Figure A3: Effect of the ACA Medicaid Expansions on Annual Mortality: Alternative Specifications (Original Estimates in Grey)



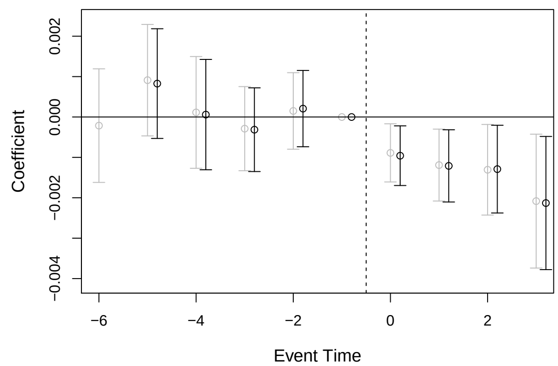
(a) Control for Labor Demand



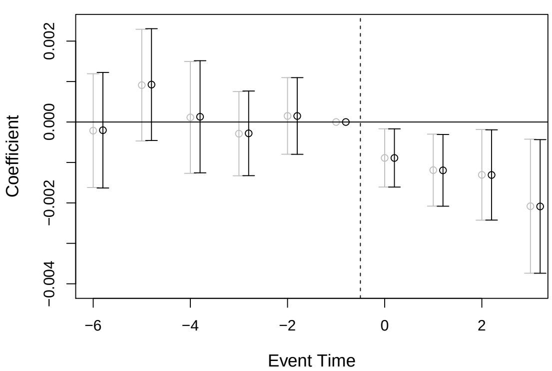
(b) Control for County Economic Variables



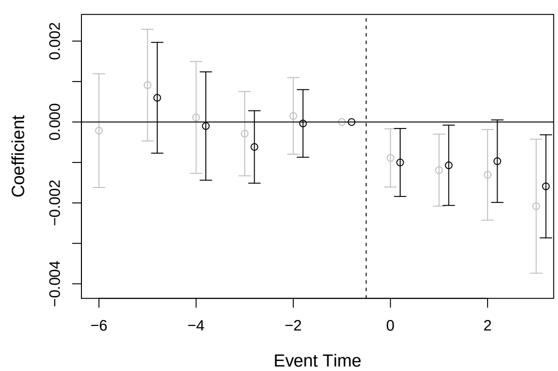
(c) Control for Drug Policy



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



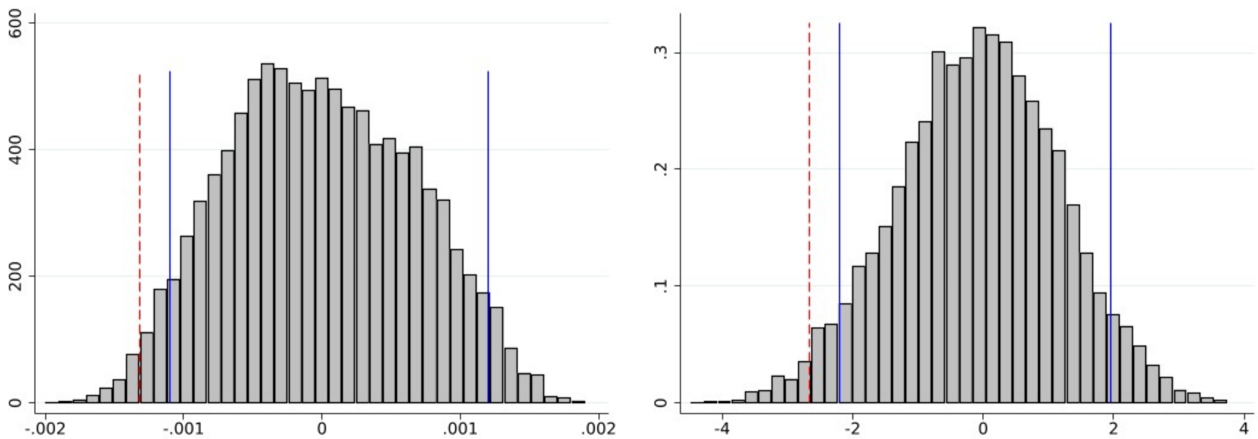
(e) Control for Sample Demographics



(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 A4: Distributions of Coefficient Estimates and T-Statistics from 10,000 Placebo Simulations

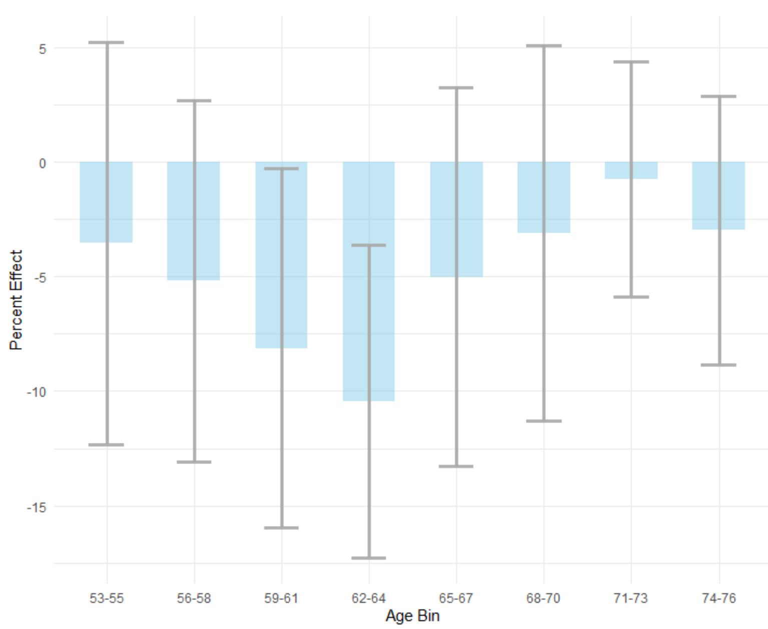


(a) Coefficient Estimates

(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 VI.D. 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.

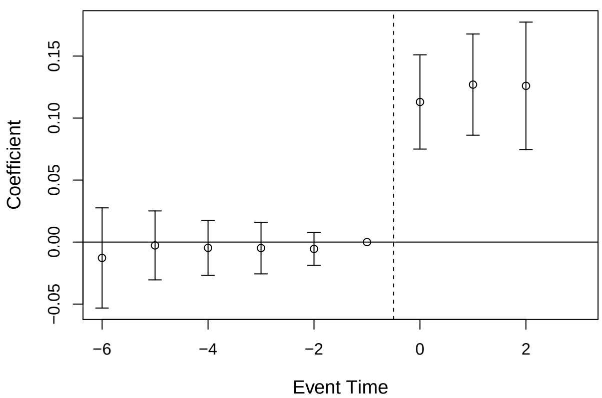
Figure A5: Estimates of Mortality Impact of ACA by 3-Year Age Bin



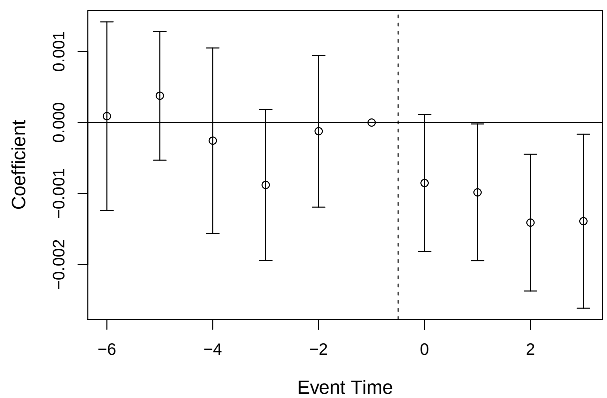
Notes: These figures plot difference-in-differences coefficients scaled by the "counterfactual" mortality rate from a series of models that include respondents meeting our sample criteria but of different ages.

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

Additional Comparison Group: Ages 65+ in 2014

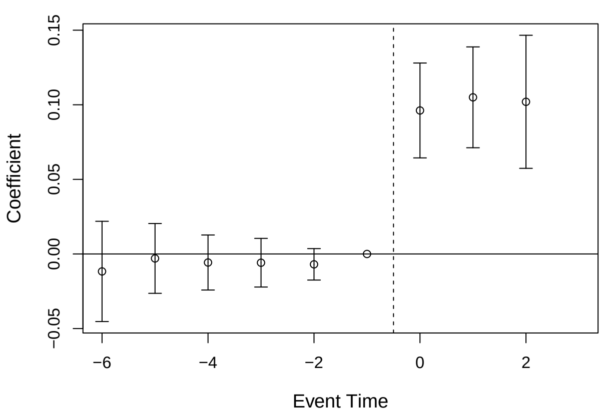


(a) Any Medicaid in Year

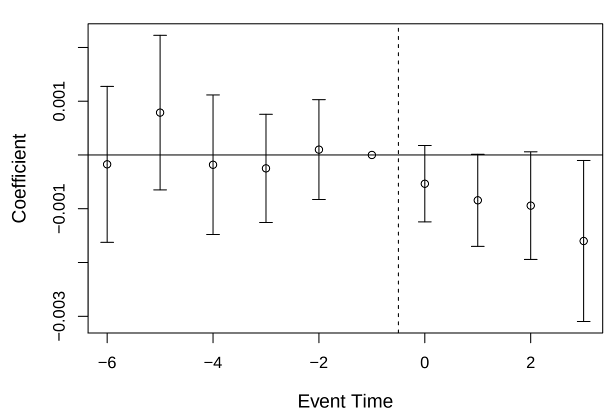


(b) Mortality

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 Appendix Section 6. The comparison group is labeled in italics for each set of graphs.