Did the Affordable Care Act's Medicaid Eligibility Expansions Crowd Out Private Health Insurance Coverage?

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Abstract

The Affordable Care Act provided funding to help states expand Medicaid eligibility to those earning up to 138% of the Federal Poverty Level. Such expansions in Medicaid eligibility, however, could "crowd out" private insurance coverage. To estimate the extent of crowd out, I use a difference-in-difference empirical approach, examining changes in health insurance coverage sources among low-income Americans in states that expanded eligibility relative to comparable individuals in states that did not. Using American Community Survey data from 2009 to 2019, I find a 43% crowd out rate. That effect consists of a 10.7 percentage point relative increase in Medicaid coverage among low income adults and a 4.6 percentage point relative decline in private health insurance among respondents in states that expanded Medicaid eligibility. Among working adults, my estimates imply a larger 56% rate of crowding out. Event study analyses provide strong support for a causal interpretation for my findings. My estimates are robust to different sample restrictions and to using other common approaches to identifying crowd out caused by expansions in public insurance coverage. Looking at crowd out across sub-groups, I find the largest effects occur among females and non-white respondents.

Keywords: Private Health Insurance, ESI, Medicaid, Crowd Out

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1 Introduction

To increase health insurance coverage rates in the United States, the Affordable Care Act (ACA) provided funding to help states expand Medicaid eligibility to those earning up to 138% of the Federal Poverty Level (FPL).[1] While a minority of states chose not to expand eligibility, in 2014, adults in a family of four living in a state that expanded eligibility could qualify for Medicaid if their total family income was below $32,913.[2] Further, adults without children could earn up to $16,105 while qualifying.[3] Prior to the ACA, childless adults were almost always ineligible for public insurance coverage.[4]

Looking at the effects of the ACA on health coverage, Courtemanche et al. (2017) find that health insurance coverage rates increased by 5.9 percentage points in states that expanded Medicaid eligibility in the first full year of Medicaid eligibility expansion. Four years post-expansion, Miller and Wherry (2019) estimate "a 17 percentage point increase in Medicaid enrollment among low-income adults in expansion states compared to non-expansion states." Wherry and Miller (2016), Miller and Wherry (2017), Courtemanche et al. (2019), and Duggan et al. (2019) also report that the ACA's Medicaid eligibility expansions had large effects on the proportion of low-income Americans who have Medicaid coverage. Little is known, however, about the extent to which increases in Medicaid coverage relating to the ACA's expanded eligibility criteria could be crowding out other sources of coverage, such as employer-sponsored health insurance (ESI).

To study whether Medicaid eligibility expansions led to crowd out, I use data from the 2009 to 2019 waves of the American Community Survey (ACS) in a staggered roll-out difference-in-difference framework, with identification coming from variation in Medicaid eligibility expansions over time and across states. Focusing on adults with family incomes (or individual incomes if single) up to 138% of the relevant Federal Poverty Level (which varies by year, by family size, and among the 48 continental states, Hawaii, and Alaska) my findings indicate that expansions in Medicaid eligibility are associated with a 43% crowd out rate. That crowding out effect consists of a 10.7 percentage

[1] Healthcare.gov explains that if "your income is below 138% FPL and your state has expanded Medicaid coverage, you qualify for Medicaid based only on your income."

[2] See https://aspe.hhs.gov/2014-poverty-guidelines for 2014 FPL figures.

[3] For someone working part-time (i.e., 1,000 hours per year / 20 hours per week), Medicaid eligibility expansion means they could earn more than $15 per hour and still qualify for Medicaid in states with expanded eligibility.

[4] Certain debilitating conditions could make someone eligible for Medicaid (including blindness and end-stage renal disease) before the ACA, regardless of income.

point increase in the proportion of adults with Medicaid coverage coupled with a 4.6 percentage point decline in the proportion of respondents who have private health coverage in expansion states relative to those in non-expansion states (i.e., the rate of crowd out is $\Delta(\text{Private Coverage})/\Delta(\text{Medicaid Coverage})$ as defined by Cutler and Gruber, 1996).[5] Put differently, my estimates suggest that for every ten adults who are covered by Medicaid because of the ACA's Medicaid eligibility expansions, there were at least four fewer individuals covered by private health insurance.[6]

I use an event-study approach to show there is no evidence of differential pre-trends in Medicaid and private insurance coverage rates across expansion and non-expansion states in my sample, providing support for the parallel trends assumption inherent in any difference-in-difference approach. As appendix items, I also demonstrate that my estimates are not sensitive to the issues with staggered roll-out designs raised by the new difference-in-difference literature (De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021; Borusyak et al., 2021). Formally, my estimates are causal under an identifying assumption that there are no unaccounted-for idiosyncratic shocks that are correlated with individual insurance coverage choices and states' Medicaid eligibility expansion decisions.

When I limit my sample to working adults, I find larger crowd out effects, amounting to a 56% crowd out rate (i.e., for every ten working adults who gain Medicaid coverage in states that expanded eligibility, there are almost six fewer workers covered by private health insurance). Due to employer-sponsored insurance, working adults are naturally more likely to have had private coverage prior to the ACA, increasing the potential for crowd out when extending eligibility for Medicaid to a greater share of adults. One unique contribution of my work is that I provide the first estimates of crowd out among childless adults, a group who were generally not eligible for Medicaid prior to the advent of the ACA. For those childless adults, my estimates suggest a crowd out rate of about 33%. Looking at robustness, sensitivity, and heterogeneity, I find that my estimates are similar when using alternate sample periods and income cut-offs, employing different weighting and clustering choices, and that crowd out effects are largest among females and non-white respondents.

[5] A 10.7 percentage point increase in Medicaid coverage in the post-expansion period is broadly in line with existing work on the effects of ACA-related Medicaid eligibility expansions (Miller and Wherry, 2019; Courtemanche et al., 2019).

[6] I show as an appendix item that the effect on private coverage consists of similar declines in both the proportion of respondents who are covered by ESI and the proportion covered by other private health insurance (i.e., non-group coverage plans). ACS data reports coverage provided via a respondent's or a family member's employer as ESI coverage and provides a separate variable indicating privately-purchased non-group coverage.

My findings add new evidence to a rich literature that studies crowd out relating to earlier expansions in Medicaid eligibility. For instance, Cutler and Gruber (1996) estimate the extent of crowd-out from expansions of Medicaid to pregnant women and children between 1987 and 1992. Defining the rate of crowd out as $^{\Delta(\text{Private Coverage})}/_{\Delta(\text{Medicaid Coverage})}$, Cutler and Gruber find a 49% crowd out rate - for every two new Medicaid enrollees there was about one fewer instance of private coverage among their sample. Kronick and Gilmer (2002), Brown et al. (2007), Gruber and Simon (2008), and Hamersma and Kim (2013), however, also examine whether expansions in public insurance eligibility crowds out private insurance, with significant variation in the level of crowd out across these studies arising partly from differences in setting (i.e., different populations and expansion generosity) and partly from differences in empirical approaches (i.e., defining the parameter of interest differently, accounting for endogenous income and fertility choices, using monthly rather than yearly data, etc.). Most recently, however, Wagner (2015) studies the effect of expansions in Medicaid eligibility for disabled individuals under 65. Wagner exploits a policy change that allowed states to offer Medicaid to their disabled residents who had monthly incomes up to 100% of the FPL. Using a two-stage least-squares approach, instrumenting for eligibility using simulated Medicaid generosity at the state by year level, Wagner's estimates suggest that each new Medicaid enrollee dropped a private plan in favor of the newly-available public coverage (i.e., 100% crowd out).[7] My work contributes to this literature by examining the extent of crowd out relating to an exceptionally broad and generous expansion in Medicaid coverage, including expanding eligibility to low income adults with no children, without disabilities, and who are not pregnant.

To summarize my contribution, I show that expansions in Medicaid eligibility brought about by the ACA led to significant crowd out of private health insurance (both ESI plus other non-group coverage). As part of my analysis, I provide the first estimates of crowd out among childless adults, a group who were previously ineligible for Medicaid regardless of income. In addition, I show as an appendix item that my findings are similar when using estimators that correct for the problems discussed in the recent difference-in-difference literature (Callaway and Sant'Anna, 2020; Borusyak et al., 2021), thus providing new evidence to support earlier work by Miller and Wherry (2017, 2019)

[7] Ham and Shore-Sheppard (2005) were the first to propose this instrument. Many subsequent studies, including Hamersma and Kim (2013) and Wagner (2015) use the instrument. I present estimates using the imputed eligibility approach of Cutler and Gruber (1996) and the two-stage least squares approach suggested by Ham and Shore-Sheppard (2005) as appendix items.

and others on the effects of Medicaid eligibility expansions on coverage. Finally, illustrating that Medicaid coverage is likely to be replacing private coverage (rather than changes in Medicaid and private coverage occurring among distinct groups) my estimates show that crowd out effects are largest among working adults, where I find the rate of crowding out to be greater than 50% (i.e., for every two individuals who gain Medicaid coverage at least one person drops a private health plan). Of course, those who can choose Medicaid coverage rather than costly private coverage because of the ACA's Medicaid eligibility expansions are almost surely better-off, even if Medicaid coverage is not accepted by all healthcare providers. However, as a policy matter, it is important to understand the net (rather than only the gross) effect of increasingly-generous expansions in Medicaid eligibility on coverage rates. As Hamersma and Kim (2013) explain, further expansions in eligibility may "draw people away from private coverage, thus failing to improve overall rates of health insurance coverage."

I explain my approach to estimation and my data in Section 2. There, I highlight some important differences in the empirical strategies used to study the effect of the ACA's Medicaid eligibility expansions on coverage rates and health outcomes compared to the literature that studies crowd out relating to less expansive changes in public insurance eligibility in the decades prior to the ACA. I present my main findings in Section 3, along with a range of robustness, sensitivity, and heterogeneity analyses. I offer concluding remarks in Section 4.

2 Estimation and Data

2.1 Data

Authors have used Current Population Survey (CPS) Data (Cutler and Gruber, 1996; Kronick and Gilmer, 2002) and Survey of Income and Program Participation (SIPP) Data (Hamersma and Kim, 2013; Wagner, 2015) to study public insurance crowd out effects. In contrast, when looking at the effects of the ACA's Medicaid eligibility expansion on coverage and subsequent health outcomes, Wherry and Miller (2016) and Miller and Wherry (2017, 2019) use restricted National Health Interview Survey (NHIS) data while Courtemanche et al. (2018) use Behavioral Risk Factor Surveillance System (BRFSS) data. However, due in part to its sample size, but also because SIPP began a new panel in 2014 and because I do not have access to restricted NHIS data, I follow

Courtemanche et al. (2017, 2019) by using American Community Survey (ACS) data. The ACS samples approximately 1 percent of Americans each year and participation is mandatory. The ACS identifies all 50 states and the District of Columbia, allowing me to know whether individuals reside in Medicaid expansion or non-expansion states over the sample period. For my main estimates I limit the sample to those age 18 to 65 and earning no more than 138% of the relevant Federal Poverty Level for their family size. I begin my sample in 2009 because it gives me five years of data for each state prior to 2014, when most states expanded Medicaid eligibility. The final year of my sample is 2019 because, as of the time of writing, ACS data for 2020 was not yet available.[8]

For each individual, the ACS surveys whether the person is "currently covered by any of the following types of health insurance or health coverage plans?" where possible answers include employment-based coverage (i.e., ESI), insurance purchased directly from an insurance company, Medicare, Medicaid, military health care such as TRICARE or VA coverage, and Indian Health Service coverage. I eliminate those who are covered by military or other sources or who qualify for Medicaid by virtue of being a Supplemental Security Income (SSI) recipient (Hamersma and Kim, 2013; Burns and Dague, 2017). I also eliminate those who report being full-time students. My estimation sample therefore consists only of 2009 to 2019 ACS respondents, aged 18 to 65, with incomes below 138% of the FPL, who are not SSI recipients or students, and are either uninsured or covered by Medicaid, ESI, or insurance purchased directly from an insurance company.

I present summary statistics in Appendix Table A1. In the table, the estimates are stratified by states that expanded Medicaid and those that did not during the sample period and then by the period before and after 2014 (or the relevant expansion date for the small number of non-2014 expansion states). Summary statistics include age, gender, marital status, race, and education information. I also report average individual income, average family income, and the imputed average Federal Poverty Level for individuals and family units. In the appendix, I explain how ACS variables that identify "Health Insurance Units" aid my analysis. I also provide a map of Medicaid eligibility expansions by state in 2019, the end of my sample period.[9] In the next section, I explain how I use my ACS sample to estimate the extent of crowd out relating to Medicaid eligibility expansion.

[8] Even if it were available, it is questionable whether including it would add value, given how the Covid-19 pandemic potentially affected health and incentives to have health coverage in 2020.

[9] I use Kaiser Family Foundation data to determine Medicaid expansions at the state level. See https://www.kff.org/health-reform/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/. I supplement that state-level data with county-level expansion data from California from Golberstein et al. (2015).

2.2 Estimation

To estimate the extent of ESI crowd out among adults relating to Medicaid eligibility expansion, I first use a difference-in-difference approach that compares outcomes for low-income adults in expansion and non-expansion states before and after the ACA's Medicaid eligibility expansion provisions come into effect. In particular, I examine specifications of the following type:

$$Y_{ist} = \beta_0 + \beta_1 \times \textit{Expansion State}_{ist} + \gamma_s + \lambda_t + X_{ist}\Pi + \epsilon_{ist}. \tag{1}$$

In equation (1), Y_{ist} represents indicator variables denoting private insurance or Medicaid coverage for individual i in state s and year t.[10] To account for differences across locations or changes that affect all respondents over time, I include state (γ_s) and year (λ_t) fixed effects in each specification. The *Expansion State*$_{ist}$ term equals one only for individuals who live in states that expanded Medicaid eligibility in the first full year t where eligibility is expanded, and is zero otherwise.[11] In my preferred specifications, I include demographic controls for each respondent along with occupation and industry fixed effects, represented by X_{it}, while the ϵ_{it} term refers to an idiosyncratic error.[12]

Because my outcomes of interest are indicator variables, and like other work on the effect of the ACA's Medicaid eligibility expansions (Courtemanche et al., 2017; Miller and Wherry, 2019) and public insurance crowd out (Hamersma and Kim, 2013; Wagner, 2015), I estimate equation (1) using a linear probability model via OLS, ensuring that the coefficients of interest can be interpreted as percentage point changes in the outcome of interest. In all of my analyses, unless otherwise noted, I report standard errors that are robust to clustering at the state level and I use ACS-provided sample weights. Within such a setup, as long as there are not omitted idiosyncratic shocks that are correlated with insurance coverage choices and states' Medicaid eligibility expansion decisions then β_1 (i.e., the coefficient on the *Expansion State* indicator term) represents the causal effect of Medicaid eligibility expansion on each outcome of interest for ACS respondents in expansion states relative to comparable ACS respondents in non-expansion states.

[10] The description here borrows liberally from Lennon (2021), who examines whether the ACA's ESI-related provisions (e.g., the employer mandate) led to greater ESI availability among workers at small firms.

[11] Note that California allowed counties to expand coverage prior to 2014. I use information on those expansions provided by Golberstein et al. (2015) to assign expansion status to residents in counties that expanded coverage early.

[12] The occupation and industry fixed effects include a "not applicable" category to account for those adults who are not working.

Note that my empirical approach is intended to be comparable with existing work on the ACA's Medicaid eligibility expansions (e.g., Miller and Wherry, 2019). For that reason, I limit my sample to single individuals and those in families with income below 138% of the relevant FPL and then use variation in expansion decisions across states to examine how the ACA's Medicaid eligibility expansion affected Medicaid coverage and private health coverage to determine the extent of crowd out. In contrast, perhaps because expansions in eligibility were not so clearly delineated by state of residence, earlier work on public insurance crowd out focused on identifying individuals who become eligible, including using instruments that capture spatial and temporal variation in Medicaid generosity to help avoid endogeneity concerns. I present estimates using versions of those approaches adapted for my setting in Appendix C. There, the estimates also provide significant evidence of crowd out, with effects ranging from 32% to 75% depending on specification.

2.3 Event Study Specification

My empirical approach relies on the assumption that, if expansion states had not expanded Medicaid eligibility due to the ACA, then individual outcomes would evolve similarly to the outcomes in states that did not expand eligibility (i.e., a parallel trends assumption). While I cannot test this assumption directly, an event-study framework can help us study whether outcomes in states that expanded eligibility evolved similarly to those in states that didn't expand in the years immediately prior to Medicaid eligibility expansion. Specifically, to study whether there are differential trends that would undermine my approach, I estimate an event-study specification that is a time-disaggregated version of the difference-in-difference approach that I specify in equation (1):

$$Y_{ist} = \textit{Expanded Medicaid}_{ist} \times \sum_{k=-l}^{m} \delta_k 1[t - T_{is} = k] + \rho_s + \psi_t + X_{ist}\Pi + \epsilon_{ist}. \tag{2}$$

In equation (2), the key difference from equation (1) is that I include a set of indicators $1(t - T_{is} = k)$ interacted with an *Expanded Medicaid*$_{ist}$ indicator term that equals 1 for respondent i (and for all t) in states that ever expand Medicaid and is zero otherwise.[13] The indicator term equals 1 only for respondents in year t when it is k years away from the time of Medicaid eligibility expansion T_{is} in state s. The δ_k coefficients on each time period indicator represent the difference in

[13] Note that the description of my event study analysis borrows from Miller and Wherry (2019) and Teltser et al. (2021).

outcome Y_{ist} between respondents in states that do and do not expand Medicaid between 2009 and 2019. The "omitted" year is $k = -1$, the year prior to implementation, where the difference between states that expanded and those that did not is essentially normalized to zero.[14]

3 Main Findings

In Panel A of Table 1, my estimates rely on 2009 to 2019 ACS respondents aged 18 to 65 with (family) income below 138% of the relevant Federal Poverty Level. In the first three columns of estimates, the outcome of interest is an indicator for Medicaid coverage with the first column presenting estimates from a parsimonious specification that does not include demographic controls or industry and occupation fixed effects. In the second and third columns, I add demographic controls and then industry and occupation fixed effects. Across specifications, my estimates suggest that there are large and consistent increases in Medicaid coverage among respondents in expansion states relative to non-expansion states, in line with the existing literature on the effects of the ACA's Medicaid eligibility expansion. Because my outcomes of interest are indicator variables, the coefficients in Table 1 should be interpreted as percentage point changes in the proportion of respondents who have Medicaid or private coverage as indicated. Therefore, focusing on the specification that includes demographic controls and industry and occupation fixed effects, the .107 coefficient represents a 10.7 percentage point relative increase in Medicaid coverage among respondents in expansion states.

In the second three columns, I present a corresponding set of estimates from specifications that use an indicator for private health insurance coverage instead of Medicaid coverage. There, the estimates indicate consistent relative declines in private health coverage among respondents in expansion states. Again focusing on estimates from the specification that includes demographic controls and industry and occupation fixed effects, the -.046 coefficient estimate represents a 4.6 percentage point relative decline in private health coverage among respondents in expansion states compared to similar respondents in non-expansion states. A decline in private coverage of 4.6 percentage points, compared to an increase in Medicaid coverage of 10.7 percentage points represents a 43% crowd out rate, meaning that for every ten individuals covered by Medicaid due to the ACA's eligibility expansions, there are around four fewer individuals covered by private insurance.

[14] Note that the key parameters of interest, δ_k, remain identified when collapsing observations where $t > m$ into period $k = m$ and those where $t < -l$ into period $k = -l$ (Sun and Abraham, 2020).

In Panel B of Table 1, I restrict the estimation sample to working adults. The estimates in Panel B suggest larger levels of crowd out, with the coefficients from specifications that include demographic controls and industry and occupation fixed effects implying a crowd out rate of 56% (i.e., 5.9 divided by 10.5). In Panel C, I focus on childless adults, a group who were previously ineligible for Medicaid. Unsurprisingly, given that they were previously ineligible, I find larger increases in Medicaid coverage among childless adults. In addition, childless adults are historically less likely to have any insurance, including private coverage, thus limiting the extent of crowd out. That said, my estimates still imply a 33% crowd out effect (i.e., 4.4 divided by 13.4).

Overall, my estimates imply significant crowd out effects relating to the ACA's Medicaid eligibility expansions. Notably, these effects are changes among respondents in expansion states *relative* to similar individuals in non-expansion states. Summary statistics in Table A1 indicate that these relative changes consist of increases in both Medicaid and private health coverage (in line with the ACA's goals and its other policy levers including the individual and employer mandates and the subsidized coverage provided via the act's healthcare exchanges). Put differently, the crowd out I am observing is consistent with low-income individuals gaining coverage mostly via Medicaid in expansion states and, to a lesser extent, via private health coverage options (ESI or privately-purchased coverage) in non-expansion states. My findings therefore imply that if the ACA had not included funding to expand Medicaid eligibility anywhere, the act's other components would have led to larger increases in private health coverage in those states that ultimately did expand Medicaid (i.e., Medicaid eligibility expansions crowd out private health insurance coverage).

Table 1: Effects of Medicaid Expansion on Medicaid and Private Health Coverage

	(1)	(2)	(3)	(4)	(5)	(6)
	Has Medicaid Coverage			Has Private Insurance Coverage		
	Panel A: All ACS Respondents Aged 18 to 65 with Family Income below 138% FPL					
Effect of Medicaid Expansion	0.106***	0.107***	0.107***	-0.047***	-0.046***	-0.046***
	(0.022)	(0.021)	(0.021)	(0.011)	(0.011)	(0.011)
Observations	1,987,814	1,987,814	1,987,814	1,987,814	1,987,814	1,987,814
R-squared	0.087	0.124	0.137	0.011	0.077	0.127
	Panel B: Working Adults Aged 18 to 65 with Family Income below 138% FPL					
Effect of Medicaid Expansion	0.105***	0.106***	0.105***	-0.061***	-0.059***	-0.059***
	(0.019)	(0.019)	(0.019)	(0.011)	(0.011)	(0.011)
Observations	1,039,787	1,039,787	1,039,782	1,039,787	1,039,787	1,039,782
R-squared	0.089	0.128	0.142	0.015	0.074	0.148
	Panel C: Childless Adults Aged 18 to 65 with Income below 138% FPL					
Effect of Medicaid Expansion	0.133***	0.133***	0.134***	-0.043***	-0.042***	-0.044***
	(0.024)	(0.023)	(0.023)	(0.012)	(0.011)	(0.011)
Observations	952,833	952,833	952,832	952,833	952,833	952,832
R-squared	0.093	0.124	0.152	0.013	0.094	0.138
State and Year Fixed Effects	Y	Y	Y	Y	Y	Y
Demographic Controls		Y	Y		Y	Y
Industry and Occupation Fixed Effects			Y			Y

Data: American Community Survey Data from 2009 and 2019 restricted as described in Section 2 (in Panels B and C that sample is further restricted to working adults and then childless adults as indicated). Standard errors, clustered at the state level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include age, gender, education, marital status, and race.

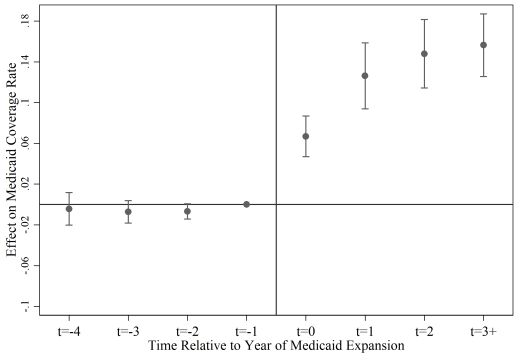
3.1 Event Studies

As I mention in Section 2, an event study framework can help determine whether outcomes in states that expanded eligibility evolved similarly to those in states that didn't expand in the years prior to Medicaid eligibility expansion. To study pre-trend patterns, I present event study plots in Figure 1. To line up with the estimates in Table 1, sub-figures (a) and (b) present event studies looking at changes in Medicaid and private health coverage using the main estimation sample (i.e., age 18 to 65, income below 138% FPL). Then, in (c) and (d), I limit the sample to just those who report being employed (as in Panel B of Table 1), while (e) and (f) present event studies where the sample is limited to childless adults. In each case, there is no evidence of a problematic pre-trend.[15]

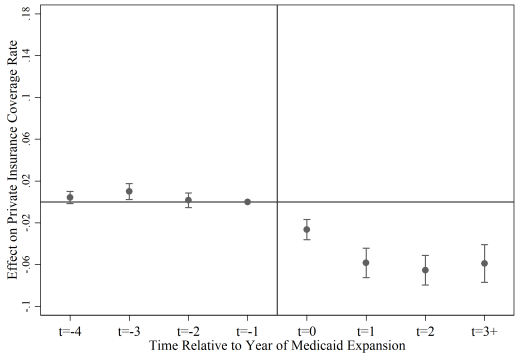
Note that, because I am using a staggered roll-out approach, my estimates could be biased by the issues regarding heterogeneous treatment timing and dynamic treatment effects raised in the "new" difference-in-difference literature (see, for instance, Goodman-Bacon, 2021 or Callaway and Sant'Anna, 2020). That said, I later show (in Table 2) that my estimates are similar when removing respondents living in states that expanded Medicaid eligibility prior to 2014 from the estimation sample. Further, as an appendix item I present difference-in-difference estimates where I collapse my sample to a two period, two group setting that eliminates the potential for heterogeneous timing and dynamic treatment effects to create a bias when estimating treatment effects. Finally, while most of the available solutions to the staggered roll-out design problems are limited to settings where the researcher has a true panel (i.e., repeated measures on units, such as states, over time), the Borusyak et al. (2021) difference-in-difference imputation approach can be used with repeated cross-sectional data in an event study framework. I present event studies that rely on the Borusyak et al. imputation approach to estimation in Figure A2, where I again find no evidence of pre-trends of concern and clear evidence of declines in private health insurance coverage in states that expanded Medicaid eligibility relative to states that did not.

[15] Notably, Courtemanche et al. (2019), who also use ACS data, start their sample period in 2011 to "avoid the effects of the 2010 dependent coverage mandate." That mandate required employers to continue to offer coverage to employees' children up to age 26 (see Antwi et al., 2013 and Barbaresco et al., 2015). However, given the mandate applied in both Medicaid expansion and non-expansion states, and given there is no pretrend in private insurance coverage in the event studies in Figure 1, I include 2009 and 2010 data to be able to study more pre-expansion time periods.

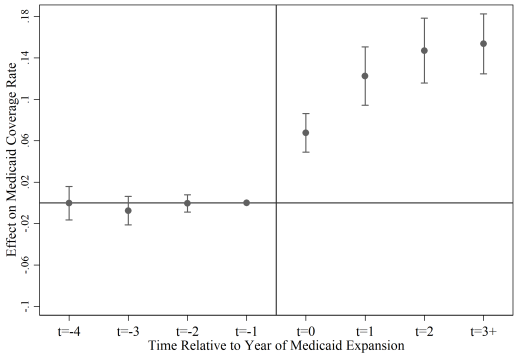
Figure 1: Event Studies



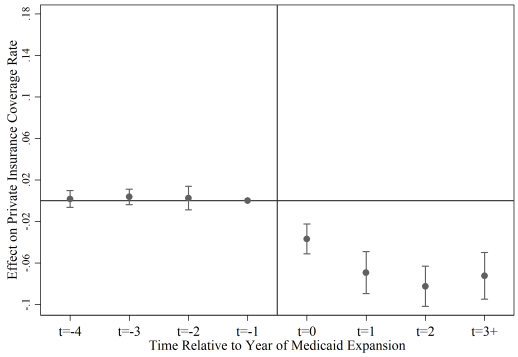
(a) Medicaid Coverage - Main Sample



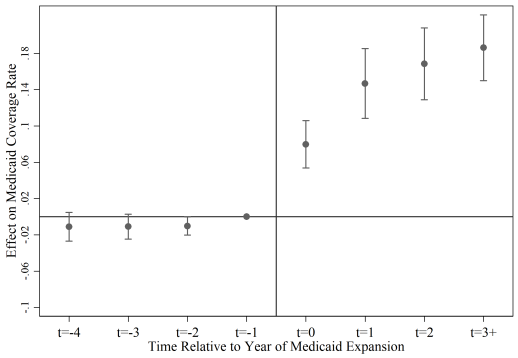
(b) Private Coverage - Main Sample



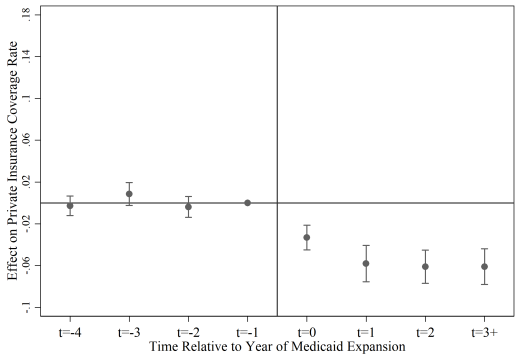
(c) Medicaid Coverage - Working Adults



(d) Private Coverage - Working Adults



(e) Medicaid Coverage - Childless Adults



(f) Private Coverage - Childless Adults

Notes: Data is ACS 2009 to 2019 restricted as described in Section 2. Sub-figures (a) and (b) present event studies for the main estimation sample, (c) and (d) limit the sample to working adults, while (e) and (f) limit the sample to childless adults. State fixed effects, year fixed effects, and demographic controls (age, marital status, education, race, and gender) are included in all regressions. Standard errors are clustered at the state level. Bars around point estimates represent 95% confidence intervals. Note that t=-4 refers to periods 4 or more years prior to expansion and t=3+ refers to 3 or more years post expansion.

3.2 Sensitivity and Heterogeneity Analyses

To examine whether my estimates are robust to different weighting, clustering, and sample selection decisions, I present a range of sensitivity checks in Table 2. In the first column I extend my sample to include respondents with family incomes up to 200% of the Federal Poverty Level. While ACS data constructs "Health Insurance Units" and provides the relevant Federal Poverty Level for that size family unit in each year and each particular state, it is possible that the ACS's family units and family income are not what a Medicaid caseworker observes for an individual in a given family. Therefore, I relax the cut off for eligibility by including ACS respondents who report family incomes above the official Medicaid income eligibility threshold. The estimates show effects that are similar to my main estimates.

In the second column, I restrict the sample to those with family incomes below 100% of the Federal Poverty Level. I limit the sample in such a way because the ACA's healthcare exchanges provided subsidies for those earning between 100% and 138% of the FPL in states that did not expand Medicaid eligibility. With such subsidies there might be more low income adults covered by private health insurance in non-expansion states after 2014, relative to those in expansion states. Indeed, Miller and Wherry (2019) report a relative decline in private insurance among their sample respondents, while suggesting that states without expanded Medicaid eligibility would see more individuals turning to the subsidized coverage on the act's exchange marketplaces. However, while the point estimate on the effect of Medicaid eligibility expansion on private coverage in column 2 is a little smaller at 3.7 percentage points (supporting Miller and Wherry's argument), the effect still amounts to more than a 35% crowd out rate even excepting those who could potentially qualify for subsidized coverage on the ACA's healthcare exchanges. In any case, greater enrollment in private coverage via the healthcare exchanges in non-expansion states is itself evidence that public insurance crowds out private coverage.

In subsequent columns in Table 2, I narrow the sample period by removing 2009 and 2019 data and then eliminate respondents in states that either had coverage eligibility limits similar to the ACA's Medicaid eligibility expansions before the ACA or who expanded Medicaid early.[16] In the final two columns, I present estimates that do not use ACS-provided weights and that do not cluster

[16]This restriction eliminates NJ, NY, CT, MA, CA, DE, MN, WA, DC, and VT who each either used a Section 1115 waiver to expand Medicaid between 2010 and 2013, or already had eligibility limits that were similar to the ACA's expanded limits prior to 2010, see Miller and Wherry (2019) for more on this.

standard errors. In each set of estimates, I again find similar positive effects on Medicaid coverage and negative effects on private health insurance. Together, the estimates suggest that my findings are robust to choices regarding sample selection and approaches to estimation.

In Table 3, I present estimates that look at changes in Medicaid coverage and private health coverage by sub-groups consisting of men only, women only, white respondents, non-white respondents, younger respondents, and older respondents. The estimates, regardless of sample restriction, reflect large positive effects on Medicaid coverage with corresponding negative effects on private health insurance. The estimates for females and for younger respondents (defined as those aged under 40) are notable. For instance, when looking at only female respondents, the estimates suggest that the rate of crowd out is 50% (based upon a 10.2 percentage point increase in Medicaid coverage along with a 5.1 percentage point relative decline in private coverage). Among those under 40 years old, the estimates indicate relatively larger changes in coverage including a 13 percentage point increase in Medicaid coverage among this group and a 6.1 percentage point decline in private coverage, relative to similar respondents in non-expansion states.

In Appendix C, I provide estimates showing that changes in private coverage consist of statistically significant relative declines in both ESI (employer-sponsored insurance) and in non-group coverage. As I mention earlier, as further appendix items, I present estimates from a specification where I "collapse" my sample to a two period, two group difference-in-difference setting and event studies where I implement the new difference-in-difference imputation estimator developed by Borusyak et al. (2021) to deal with the issues raised by the recent difference-in-difference literature. As a final appendix item, I present estimates that use the imputed (Cutler and Gruber, 1996) and simulated eligibility (i.e., instrumenting for public insurance eligibility using a measure of Medicaid generosity that is unrelated to a state's actual population, as recommended by Ham and Shore-Sheppard, 2005) approaches.[17]

[17]Note that I do not employ the "threshold" approach of Hamersma and Kim (2013) - which is ideal for examining the net change in public insurance coverage rates relating to intensive margin changes in coverage generosity - because so many respondents in my sample experience an extensive margin change in coverage eligibility.

Table 2: Sensitivity Analyses

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Dependent Variable - Has Medicaid Coverage					
Effect of Medicaid Expansion	0.098***	0.099***	0.099***	0.142***	0.110***	0.107***
	(0.018)	(0.022)	(0.025)	(0.017)	(0.023)	(0.001)
	Panel B: Dependent Variable - Has Private Health Coverage					
Effect of Medicaid Expansion	-0.047***	-0.037***	-0.044***	-0.052***	-0.051***	-0.046***
	(0.010)	(0.010)	(0.012)	(0.008)	(0.011)	(0.001)
Observations	3,175,422	1,330,253	1,660,821	1,460,019	1,987,814	1,987,814
R-squared	0.126	0.143	0.137	0.127	0.130	0.137
State, Year, Ind., & Occ. FEs	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y
Sample/Estimation Restriction	**18 to 64, up to 200% of FPL**	**18 to 64, up to 100% of FPL**	**2010 to 2018 ACS Data Only**	**Eliminate Early Expanders**	**No Weights**	**No Clustering**

Data: American Community Survey Data from 2009 and 2019 restricted as described in Section 2 with further restrictions as noted. Specifically, in column 1, the sample is further restricted to those with a family income below 200% of the relevant Federal Poverty Level (FPL). In column 2, the sample consists only of those with incomes below 100% of relevant FPL. In column 3, I remove data from 2009 and 2019 from the sample. In column 4, I restrict the sample only to those states that expanded Medicaid coverage in 2014 or later (this restriction eliminates NJ, NY, CT, MA, CA, DE, MN, WA, DC, and VT who each either used a Section 1115 waiver to expand Medicaid between 2010 and 2013, or already had eligibility limits that were similar to the ACA's expanded limits prior to 2010, see Miller and Wherry, 2019 for more on such states). In the final two columns, I present estimates where I do not use ACS-provided weights and where I do not cluster standard errors. Standard errors, in parentheses, are clustered at the state level in each of the first five specifications in the table. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include age, gender, education, marital status, and race.

Table 3: Heterogeneity Analyses

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Dependent Variable - Has Medicaid Coverage					
Effect of Medicaid Expansion	0.102***	0.112***	0.121***	0.081***	0.130***	0.090***
	(0.022)	(0.021)	(0.024)	(0.017)	(0.026)	(0.019)
	Panel B: Dependent Variable - Has Private Health Coverage					
Effect of Medicaid Expansion	-0.051***	-0.040***	-0.050***	-0.041***	-0.061***	-0.036***
	(0.011)	(0.011)	(0.012)	(0.009)	(0.016)	(0.008)
Observations	1,122,575	865,235	1,369,995	617,812	929,053	1,058,759
R-squared	0.142	0.129	0.128	0.149	0.148	0.144
State, Year, Ind., & Occ. FEs	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y
Sample Restrictions	**Females Only**	**Males Only**	**Caucasian**	**Non-Caucasian**	**Aged 40 or Younger**	**Aged Over 40**

Data: American Community Survey Data from 2009 and 2019 restricted as described in Section 2. The sample is further restricted by gender, race, and age as indicated in the "Sample Restrictions" row of the table. Standard errors, clustered at the state level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include age, gender, education, marital status, and race (except when the sample is restricted by that particular characteristic).

4 Conclusion

I document significant private health coverage crowd out relating to the ACA's Medicaid eligibility expansions. My main estimates suggest that for every ten adults who are covered by Medicaid because of the ACA's eligibility expansions, there were at least four fewer individuals covered by private health insurance (including roughly equal effects on employment-based and other non-group coverage). Illustrating that Medicaid coverage is likely replacing private coverage for at least some individuals (rather than changes in Medicaid and private coverage occurring among distinct groups) my estimates show that crowd out effects are largest among working adults, where I find that for every ten workers who gain Medicaid coverage due to the ACA's expansions almost six workers drop a private health plan.[18]

Those who can choose Medicaid coverage rather than costly private coverage because of the ACA's Medicaid eligibility expansions likely experience significant welfare gains due to the availability of Medicaid (which has virtually no cost sharing) rather than having to purchase coverage privately or share in the cost of ESI with their employer. On the other hand, to the extent that the ultimate goal of expansions in Medicaid eligibility is to improve public health via improved health insurance options, it remains important to understand the net effect of relatively generous expansions in Medicaid eligibility on health coverage. My findings, given adults with income above 138% of the FPL are increasingly likely to already be covered by some kind of private coverage, suggest that further expansions in Medicaid eligibility might fail to meaningfully improve overall rates of health insurance coverage.

[18]Notably, these effects mean that a large fraction of adults gained coverage. It is therefore worth noting that Carey et al. (2020) show that Medicaid eligibility expansions did not result in negative spillovers in terms of healthcare utilization and access among those who already had other public insurance coverage. Indeed, Miller et al. (2021) show reductions in mortality among the near-elderly in states that opted to expand Medicaid relative to non-expanders. See Soni et al. (2020) for an overview of 43 studies that use quasi-experimental methods to study the ACA's effects on health outcomes.

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A Additional Estimates

A.1 ACS Data, Sample Selection Details, and Summary Statistics

In Table A1, I present summary statistics for my main estimation sample, American Community Survey respondents age 18 to 65 with family incomes below 138% of the relevant Federal Poverty Level. Helpfully, the ACS assigns each individual to a unique Health Insurance Unit (HIU). ACS documentation reports that HIUs consist of individuals who would be considered a "family unit" in determining eligibility for either private or public coverage. Each HIU is constructed based on living arrangements and familial relationships.[A1] Conveniently, ACS data also reports the number of individuals in a HIU and attaches state by year base and increment Federal Poverty Levels for the lower 48 states, Alaska, and Hawaii. Therefore, it is easy to construct a variable that reports 138% of the FPL for each HIU and compare it to the total HIU income. For example, for a family consisting of one adult and two children, the Medicaid income limit for the family in an expansion state is the FPL base value for that state and year plus two FPL increments multiplied by 1.38.[A2]

My main estimates eliminate those above 138% of the FPL from the estimation sample. Following Hamersma and Kim (2013), I also eliminate all SSI recipients from the sample "to avoid the issue of their adjunctive eligibility by way of SSI receipt." My sample does, however, include a small number of respondents who report having both private and Medicaid coverage at the time of their ACS response. Wagner (2015) presents estimates including and excluding these "overlap" individuals and shows that they have essentially no effect on her findings.[A3]

In Table A1, I present summary statistics for age, gender, marital status, race, and education - the demographic controls that I use in my main estimates - stratified first by states that expanded Medicaid those that did not during the sample period and then by the period before and after 2014 or the relevant expansion date for the small number of early expansion states. In addition to information on health insurance coverage sources, I also report average individual income, average family income, and the average FPL for HIUs/families in the sample (where a family can be as small as one person).

[A1] See https://usa.ipums.org/usa-action/variables/HIURULE.

[A2] See https://usa.ipums.org/usa-action/variables/HIUFPGBASE.

[A3] Perhaps because of the ACS sample size, excluding such respondents from the estimation sample has essentially no noticeable effect on my findings. For brevity, I therefore omit estimates that exclude these "overlap" individuals.

Table A1: Summary Statistics

		Non-Expansion States		Expansion States	
		Pre-2014	Post-2014	Pre-Expansion	Post-Expansion
		Proportion of Sample		**Proportion of Sample**	
Education	Less than HS	28.4	24	25.9	23.7
	High School	62	63.8	62.4	62.1
	College	7.4	9.1	8.6	10.3
	Grad Level	2.3	3.1	3	3.9
Race	White	68.7	69.8	70.1	67.6
	Black	21.1	19.8	13.1	12.4
	Other	10.3	10.4	16.8	20.1
Married		41.3	38.8	38.9	37.1
Female		56.9	57.6	55.9	56.1
Insurance	Any	44.8	57.4	57.3	75.8
	Medicaid	18.9	22	30.5	46.3
	Private Health Coverage	27.4	37.6	29	32.4
	ESI	34.9	39.6	36.6	37.5
	Other Private Coverage	7.6	13.6	8.3	10.2
		Mean (Std. Dev.)		**Mean (Std. Dev.)**	
Age		39.8	40.5	39.9	40.6
		(12.7)	(13.0)	(12.7)	(12.9)
Individual Income		7,513	8,037	7,635	8,039
		(11,209)	(11,976)	(12,510)	(12,341)
Family Income		12,671	13,477	12,439	12,986
		(9,314)	(10,278)	(9,403)	(10,251)
Family Federal Poverty Level		16,882	18,045	16,689	17,762
		(6,451)	(7,039)	(6,563)	(7,092)
Observations		360,448	384,690	565,933	676,743

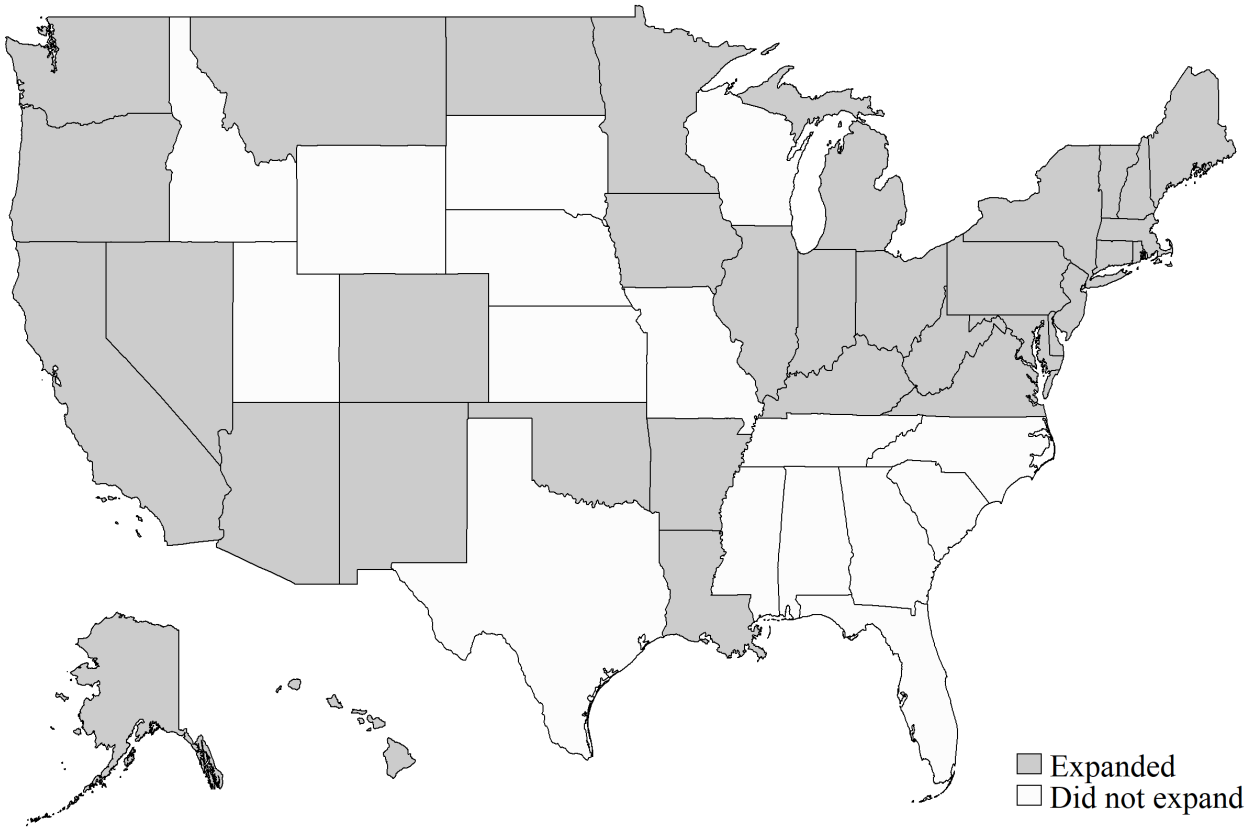
Data: American Community Survey 2009 to 2019 restricted as described in Section 2.

A.2 Medicaid Expansion States

Figure A1 shows the status of Medicaid eligibility expansion by state as of 2019, which is the end of my sample period. Several states have chosen to expand Medicaid since 2019, including Idaho, Nebraska, and Oklahoma.[A4]

[A4] See https://www.kff.org/medicaid/issue-brief/status-of-state-medicaid-expansion-decisions-interactive-map/ for the latest updates.

Figure A1: Medicaid Expansion Status in 2019



Source: Kaiser Family Foundation.

B New Difference-in-Difference Literature

As I mention in the text, I am trying to identify the effect of Medicaid eligibility expansions in a staggered roll-out setting. This means I am potentially subject to the issues raised by the "new" difference-in-difference literature (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; Borusyak et al., 2021). Specifically, while most states adopted the new Medicaid eligibility criteria in January of 2014, there are respondents in my ACS data that experience Medicaid eligibility expansion in both earlier (CA, DC, etc.) and later (NH, ME, etc.) periods. Using a standard difference-in-difference approach will estimate a treatment effect that is a weighted average of the treatment effect estimates for all potential comparison groups in the data (one example being early treated units versus never treated units). Problematically, one potential comparison group is later treated versus earlier treated units. However, if the treatment effect of interest affects all groups equally upon treatment (i.e., there is no heterogeneity in treatment effect), and the treatment effects are not dynamic (i.e., changing with the number of time periods since treatment), then the comparison between later treated versus earlier treated units will have a negligible effect on the familiar two-way fixed effects difference-in-difference estimator. Even in cases where we suspect there may be a bias, the new literature shows that the bias introduced by heterogeneous treatment timing is minimal in cases where most units are treated at a single time and/or by having a large group of never-treated units. In my setting, a majority of states expanded Medicaid eligibility in 2014 and there are a large number of states who do not expand, immediately limiting the extent to which my estimates could be biased by the issues raised in the new literature regarding difference-in-difference estimation.

That said, in this section I additionally demonstrate that my estimates are not driven by any bias introduced by my staggered roll-out setting. To do so, I first eliminate ACS respondents in states that did not expand Medicaid in 2014 and then collapse my data to the state level. This allows me to estimate the treatment effects of interest using a canonical two period (i.e., before 2014 and from 2014 onward), two group (i.e., states that expanded versus those that did not) difference-in-difference approach.

I present the estimates from such an exercise in Table B1, where I find a 14.8 percentage point increase in Medicaid coverage and a 5.9 percentage point decline in private health coverage. These

estimates should be compared to the estimates in column 4 of Table 2 in the main text, which uses individual-level data but also eliminates states that expanded Medicaid early or that already covered working adults prior to the advent of the ACA (see Miller and Wherry, 2019). In column 4 of Table 2, the treatment effects were a 14.2 percentage point increase in Medicaid coverage and a 5.2 percentage point decline in private health coverage. The similarity in the effects of interest when using the simple two period, two group approach versus when using individual level data suggests that dynamic treatment effects are not driving my findings. Moreover, using only those states that expanded Medicaid eligibility at the same time (in 2014) eliminates the possibility that my estimates are driven by the bias created by problematic comparisons among treated groups in a standard two-way fixed effects difference-in-difference approach.

Finally, the new difference-in-difference literature provides a range of alternative estimator options that corrects for the biases that could arise when using a two-way fixed effects difference-in-difference approach. However, to date, most of these approaches (Callaway and Sant'Anna, 2020) can be implemented only when using true panel data with multiple observations per unit over time. In contrast, my ACS data is a repeated cross-section. The only new estimator, at the time of writing, that can handle repeated cross-section data (like the ACS) is the Borusyak et al. (2021) estimator. In Figure A2, therefore, I present event studies that implement Borusyak et al.'s imputation approach.[B1] In particular, sub-figures (a) and (b) of Figure A2 present event studies that are comparable to the sub-figures (a) and (b) in Figure 1 in the main text. The pattern of estimates is quite similar, with perhaps suggestive evidence that there are larger effects on the reduction in private health coverage using an approach that corrects for potential bias, suggesting that my OLS approach maybe overly conservative.

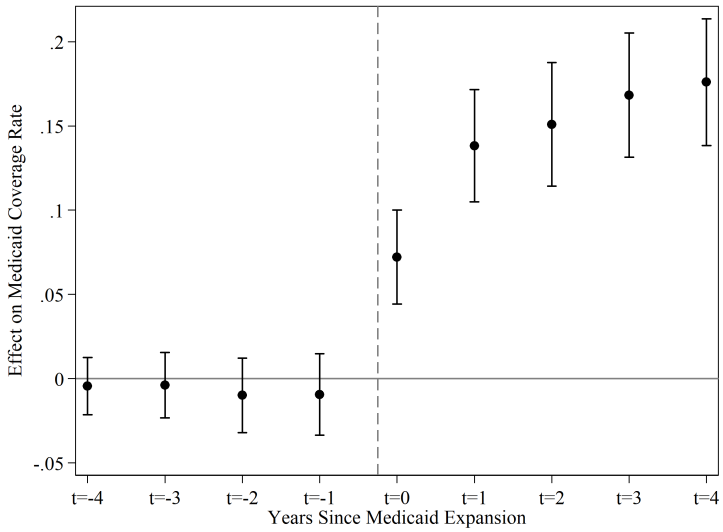
[B1] In Stata, this is implemented via the "did_imputation" package with the associated "event_plot" package to then plot the estimates. These packages are provided by Borusyak et al. and can be installed using the familiar "ssc install [package name]" approach.

Table B1: Effects using a Two Group, Two Period Approach

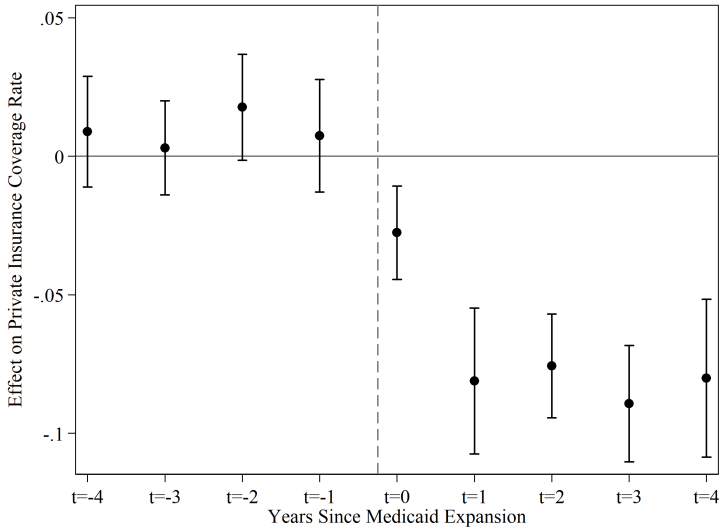
	(1) Has Medicaid Coverage	(2) Has Private Health Coverage
Treated (= Expansion State)	0.059**	-0.007
	(0.023)	(0.020)
Post (= After 2014)	0.032***	0.097***
	(0.004)	(0.005)
Treated * Post	0.148***	-0.056***
	(0.020)	(0.007)
Observations	451	451
R-squared	0.544	0.269
N of States	41	41

Data: American Community Survey 2009 to 2019 restricted as described in Section 2 and collapsed to the state level.

Figure A2: Event Studies using Borusyak et al. Difference-in-Difference Imputation Approach



(a) Medicaid Coverage



(b) Private Coverage

Notes: Data is ACS 2009 to 2019 restricted as described in Section 2. Sub-figures (a) and (b) present event studies that are comparable to the same sub-figures in Figure 1 in the main text. Standard errors are clustered at the state level. Bars around point estimates represent 95% confidence intervals. Notice that the Borusyak et al. approach does not require "dropping" the t=-1 period (i.e. normalizing the difference to zero in that period) as is common when using an Ordinary Least Squares approach.

C Additional Estimates

C.1 Any Insurance, ESI, and Privately-Purchased Coverage

Table C1 uses my main estimation sample - those aged 18 to 65 with income below 138% of the FPL - to study changes in insurance coverage overall (i.e., any private or public coverage) and to split private coverage into employment-based coverage (i.e., ESI) or other non-group private coverage (i.e., privately-purchased coverage). I also report the effect of Medicaid eligibility expansion from Table 1 on Medicaid coverage for reference purposes. In the table, we can see a 5.9 percentage point increase in any coverage, consisting of a 10.7 percentage point increase in Medicaid coverage plus a 2.7 and a 2.8 percentage point relative decline in ESI and other private health coverage. These estimates suggest that the decline in private coverage we see in Table 1 in the text consists of similarly-sized declines in both ESI and private health coverage.

Table C1: Effects on Any Coverage, Employer-Sponsored Health Insurance, and Other Private Coverage

	(1) Any Health Coverage	(2) Medicaid Coverage	(3) Has ESI	(4) Has Other Private Coverage
Effect of Medicaid Expansion	0.059***	0.107***	-0.027***	-0.028***
	(0.015)	(0.021)	(0.007)	(0.008)
Observations	1,987,814	1,987,814	1,158,379	1,987,814
R-squared	0.142	0.137	0.211	0.057
State and Year Fixed Effects	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y
Industry and Occupation Fixed Effects	Y	Y	Y	Y

Data: American Community Survey 2011 to 2019 restricted as described in Section 2. In column 3, I further limit the sample to working adults. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include age, gender, education, marital status, and race.

C.2 Estimates Comparable to Existing Crowd Out Literature

In Table C2, I use data on Medicaid eligibility at the state by year level for families and single individuals from the Kaiser Family Foundation to develop estimates that are more comparable to the existing Medicaid crowd out literature (Cutler and Gruber, 1996; Hamersma and Kim, 2013; Wagner, 2015).[C1] In the existing crowd out literature, authors typically try to impute Medicaid eligibility based on the observable characteristics of respondents, with identification coming from changes in eligibility rules across states and over time. When using imputed eligibility, their approach to estimation employs the following Linear Probability Model specification:

$$Y_{ist} = \beta_0 + \beta_1 \times ELIG_{ist} + \gamma_s + \lambda_t + X_{ist}\Pi + \epsilon_{ist}. \tag{C.1}$$

In this estimating equation, i, s, and t index individuals, states, and years. The dependent variable Y_{ist} is an indicator for coverage (either private or public) and the specification also accommodates a rich set of demographic controls and fixed effects indexed by X_{ist} while γ_s, λ_t, and ϵ_{ist} represent state fixed effects, year fixed effects, and an idiosyncratic error term. The coefficient of interest is β_1, which, because $ELIG_{ist}$ is equal to one only when a respondent's observable characteristics suggest they would be eligible for Medicaid in state s at time t and is zero otherwise, represents the percentage point change in the probability of individuals having coverage Y when moving from being ineligible ($ELIG_{ist} = 0$) to being eligible ($ELIG_{ist} = 1$).

Ham and Shore-Sheppard (2005), however, explain that such an approach to estimation is biased because eligibility is endogenously determined. To resolve the endogeneity, Ham and Shore-Sheppard propose instrumenting for eligibility using an instrument consisting of the proportion of sample respondents in all states that would be eligible for Medicaid coverage under a given state's eligibility rules for each year, excluding respondents from the state whose rules are being used to simulate eligibility. As Wagner (2015) explains, the instrument therefore provides a state by year measure of Medicaid generosity that is unrelated to the characteristics of the state's population. Using such an instrument Ham and Shore-Sheppard find little evidence of crowd-out resulting from the expansions of Medicaid to pregnant women and children that Cutler and Gruber (1996) study.

[C1] The Kaiser Family Foundation data is available at https://www.kff.org/state-category/medicaid-chip/trends-in-medicaid-income-eligibility-limits/. I cannot include 2009 and 2010 ACS data because the Kaiser Family Foundation only tracks eligibility for childless adults from 2011.

Similarly, Hamersma and Kim (2013) use Ham and Shore-Sheppard's instrument and find little evidence of crowd out when studying state-level parental Medicaid eligibility expansions using the 1996, 2001, and 2004 panels of the Survey of Income and Program Participation (SIPP).[C2] However, Wagner (2015), using the Ham and Shore-Sheppard instrument to study crowd out among disabled individuals, finds a 100% crowd out rate (i.e., every disabled person covered by Medicaid dropped a private insurance plan).

In Table C2, I first present estimates that follow Cutler and Gruber (1996) using imputed eligibility (i.e., ignoring the potential endogeneity issues) to examine changes in Medicaid and private insurance coverage. Then, I present two-stage least squares estimates that rely on the Ham and Shore-Sheppard instrument. I include the first stage coefficient and F-statistic in the table. Note that data on childless adults' Medicaid eligibility is only available from the Kaiser Family Foundation from 2011 onward, meaning that my estimates limit the sample to the years 2011 to 2019. Also, to be more comparable to the existing literature, I include those with up to 200% of the relevant Federal Poverty Level in the sample.[C3]

As I mention when describing the estimating equation, the coefficient on the eligibility indicator represents the percentage point change in the probability of individuals having coverage Y (i.e., Medicaid or Private Coverage depending on specification) when moving from being ineligible ($ELIG_{ist} = 0$) to being eligible ($ELIG_{ist} = 1$). Using this alternative approach, I again find significant evidence of crowd out. For example, the estimates in column 1 of Table C2 suggest a 19.6 percentage point increase in the probability of having Medicaid when becoming eligible. In column 2, there is a corresponding 14.9 percentage point decline in the probability of having ESI, suggesting a crowd out rate of over 75%. However, as Ham and Shore-Sheppard explain, we cannot rely on OLS estimates due to the endogeneity of income, state of residence, marital status, and fertility decisions. Using Ham and Shore-Sheppard's instrument, I find a crowd out rate of around 32% (i.e., 16.6/51.2) which is similar to my main estimates in Table 1.

[C2] Notably, Hamersma and Kim also study crowd out by modeling participation using the eligibility threshold itself. Such estimates can estimate the increase in public insurance coverage among a population for an intensive margin "X%" change in the eligibility threshold. However, it is less clear that such an approach works in my setting given that the Medicaid eligibility expansion represented an extensive margin change for so many. For example, childless adults went from being ineligible, regardless of income, to being eligible up to 138% of the FPL in expansion states.

[C3] The literature typically includes many respondents who would never be eligible to gain coverage at any point in the sample period even in states with the most generous eligibility rules, see Hamersma and Kim (2013) for example.

Table C2: Estimates Using Traditional Approaches to Crowd Out

	(1) Medicaid Coverage	(2) Private Coverage	(3) Medicaid Coverage	(4) Private Coverage
	OLS		2SLS	
Eligibility	0.196***	-0.149***	0.512***	-0.166***
	(0.008)	(0.006)	(0.022)	(0.028)
First-stage Effect			2.746***	2.746***
			(0.152)	(0.152)
Observations	2,580,864	2,580,864	2,580,864	2,580,864
R-squared	0.157	0.165	0.035	0.023
F-Stat from First Stage			115.9	115.9
State and Year Fixed Effects	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y
Industry and Occupation Fixed Effects	Y	Y	Y	Y

Data: American Community Survey 2011 to 2019 restricted as described in Section 2 except that I include those with income up to 200% (rather than 138%) of the relevant Federal Poverty Level, which varies by state, year, and across family sizes. Estimates in columns 1 and 2 use OLS with imputed eligibility based on observable individual characteristics. Estimates in columns 3 and 4 use the Ham and Shore-Sheppard (2005) Medicaid eligibility instrument. Note, I cannot include 2009 and 2010 data here because the Kaiser Family Foundation only tracks eligibility for childless adults from 2011. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include age, gender, education, marital status, and race.