DOI: 10.1002/pam.22556

RESEARCH ARTICLE

Did the Affordable Care Act's Medicaid eligibility expansions crowd out private health insurance coverage?

Conor Lennon

Rensselaer Polytechnic Institute, Troy, New York, USA

Correspondence
Conor Lennon, Rensselaer Polytechnic Institute, 3402 SAGE, 110 8th Street, Troy, NY, 12180.
Email: lennoc@rpi.edu

Abstract

The Affordable Care Act (ACA) 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, including changes in coverage relating to other ACA provisions. To estimate the extent of such crowd out, I use a difference-in-differences 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, consisting 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 support for a causal interpretation for my findings. I further show that my estimates are robust to different sample restrictions and estimation choices, are not subject to the issues raised by the new difference-in-differences literature, and are similar when I use approaches to identifying crowd out common in the existing literature.

INTRODUCTION

To increase health insurance coverage among low-income Americans, the Affordable Care Act (ACA) provided generous federal funding to help states expand Medicaid eligibility to those earning up to 138% of the Federal Poverty Level (FPL). By mid-2014, 26 states had expanded Medicaid eligibility with that number increasing to 39 as of mid-2022. The expansion in eligibility had a major impact, with the Centers for Medicare and Medicaid Services (CMS) estimating that 21 million Americans were covered in 2022 because of the ACA's Medicaid eligibility expansion.[1] Expansions in public insurance eligibility thresholds, however, can be expected to "crowd out" private health insurance coverage (Cutler & Gruber, 1996).

For that reason, in this paper I examine whether the ACA's Medicaid eligibility expansions led to crowding out of private health insurance. Answering this question is important because if people are being diverted from private coverage, changes in Medicaid coverage rates will not reflect the true impact of expanding Medicaid eligibility on coverage rates among low-income Americans. In turn, this raises concerns about the budgetary and welfare effects of Medicaid expansion. According to CMS, the average cost in 2016 of covering the newly eligible Medicaid population under the ACA was $5,965 (Wolfe et al., 2017). That amounts to more than $120 billion per year if there are 21 million new enrollees, depending upon the extent of crowd out of private insurance, which can be directly or indirectly subsidized. It is important, as a policy matter, to understand how much of that spending is actually contributing to increasing insurance coverage among the target population.

To estimate changes in insurance coverage, I use a difference-in-differences empirical framework, with identification coming from spatial and temporal variation in Medicaid eligibility expansions. My main estimates refer to individuals with incomes 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) who appear in the 2009 to 2019 waves of the American Community Survey (ACS). Throughout, I follow the literature and define crowd out as the decline in private insurance coverage as a share of the increase in Medicaid coverage (Gruber & Simon, 2008).

My findings indicate that expansions in Medicaid eligibility are associated with a 43% crowd-out rate. That crowd-out effect consists of a 10.7 percentage point increase in the proportion of adults with Medicaid coverage in states that expanded eligibility, coupled with a 4.6 percentage point decline in the proportion of respondents who have private health coverage, relative to those in non-expansion states. A 43% crowd-out rate implies that for every 10 adults who are covered by Medicaid due to the ACA's Medicaid eligibility expansions, at least four of them would otherwise have been covered by private health insurance. My estimates also show that the change in private health coverage consists of similar effects on the relative proportion of respondents covered by employer-sponsored insurance (ESI) and the relative proportion covered by other private health insurance such as the non-group plans available on the ACA's healthcare exchanges.[2]

To support a causal interpretation for my findings, I use event studies 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-differences approach. Additionally, I find that crowd-out effects remain large and statistically significant when using instrumental variables approaches to estimating crowd out from the existing literature that are designed to address endogeneity concerns surrounding household size and earnings (Ham & Shore-Sheppard, 2005). I also demonstrate that my estimates are not sensitive to the issues with staggered roll-out designs raised by the new difference-in-differences literature (Borusyak et al., 2021; Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 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.

Notably, 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 10 working adults who gain Medicaid coverage in states that expanded eligibility, almost six of those would otherwise be covered by private health insurance).

[1] See https://www.cms.gov/newsroom/press-releases/new-reports-show-record-35-million-people-enrolled-coverage-related-affordable-car

[2] The healthcare exchanges offer subsidized individual coverage but only to those earning between 100% and 400% of the FPL. This creates a "coverage gap" in non-expansion states where those with incomes below 100% of the FPL are ineligible for either Medicaid or exchange subsidies.

Due to the prevalence of employer-sponsored insurance, working adults are more likely to have had private coverage prior to the ACA, increasing the potential for crowd out when extending Medicaid eligibility thresholds. Among childless adults, a group who generally were not eligible for Medicaid at any income level prior to the advent of the ACA's eligibility expansion, my estimates suggest a crowd-out rate of about 33%.[3] In terms of 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 women and respondents from minority groups.

It is worth emphasizing here how the ACA's Medicaid expansion differs from earlier expansions. In particular, descriptive statistics from my estimation sample show that both public and private insurance coverage rates increase after the ACA's implementation in 2014 in expansion and non-expansion states (see Table 1). Therefore, while my estimates likely include some traditional crowding-out effects (people switching from private coverage to newly available Medicaid coverage), it appears that ACA-related expansions in Medicaid eligibility are working to divert individuals from the private coverage options that people are choosing in non-expansion states. One example would be the healthcare exchange coverage options, available to those with incomes between 100% and 400% of the FPL in non-expansion states but only to those with 138% to 400% of the FPL in Medicaid expansion states. While it remains true that expanded Medicaid eligibility is displacing private coverage, I am careful to position my findings as consisting of *relative* changes in coverage rates across expansion and non-expansion states throughout the paper.

My work is closely related to studies that examine the impact of incremental expansions in Medicaid eligibility over the past 4 decades, including Cutler and Gruber (1996), Yazici and Kaestner (2000), Shore-Sheppard (2000, 2008), Kronick and Gilmer (2002), Lo Sasso and Buchmueller (2004), Card and Shore-Sheppard (2004), Ham and Shore-Sheppard (2005), Gruber and Simon (2008), Hamersma and Kim (2013), and Wagner (2015). In the section "Existing Literature," I explain that literature in detail, highlighting how my work is particularly valuable because it finds clear evidence of crowd out relating to an unprecedented expansion in Medicaid eligibility generosity. In contrast, the target populations for earlier expansions in eligibility tended to be those with very low incomes, and therefore those who are less likely to have alternate coverage options. My work is also naturally related to studies that quantify the ACA's impact on insurance coverage rates, including Wherry and Miller (2016), Frean et al. (2017), Kaestner et al. (2017), Miller and Wherry (2017, 2019), Courtemanche et al. (2017, 2019), Frisvold and Jung (2018), Duggan et al. (2019), and Abraham et al. (2019), among others. Some of these authors examine changes in private coverage but find no evidence of immediate ACA-related effects, suggesting zero crowd out. In "Existing Literature," I explain why these authors were unable to find no immediate evidence of crowd out. In "Main Findings," I also replicate several of their empirical approaches to help explain why we reach different conclusions.

In summary, I make three main contributions to the literature. First, I show that the ACA's expansions in Medicaid eligibility clearly led to significant crowd out of private health insurance. As part of that analysis, I also provide the first comprehensive evidence of crowd out among childless adults, a group who were previously ineligible for Medicaid regardless of income. Second, while some studies of the ACA's effects consider the issue of crowd out, I show that my estimates are more reliable due to a longer and larger sample, including more treatment states, along with using both difference-in-differences and instrumental variables approaches to identification. Third, a significant contribution of my work is that I show that my findings are robust to using estimators that correct for the problems identified in the new difference-in-differences literature (Borusyak et al., 2021; Callaway & Sant'Anna, 2021). These estimates are important in their own right because they provide key support for earlier work, including Wherry and Miller (2016), Miller and Wherry (2017, 2019), Courtemanche et al. (2017, 2019), and Duggan et al. (2019), on the effects of expansions in Medicaid

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

TABLE 1 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
Women	56.9	57.6	55.9	56.1
Children in household	53.3	50.4	52.7	50.7
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 (workers only)	34.9	39.6	36.6	37.5
Non-ESI 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/HIU income	12,671	13,477	12,439	12,986
	(9,314)	(10,278)	(9,403)	(10,251)
Family/HIU 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

Notes: American Community Survey 2009 to 2019 restricted as described in Section 2. HIU = health insurance unit. Note that the summary statistics for private and Medicaid coverage added together do not equal the proportion with "Any" coverage because some individuals report coverage from more than one source. Also, the sum of ESI coverage and non-ESI private coverage does not equal the total proportion with private coverage because the ESI variable is the proportion offered coverage only among those who are working. Even if it were not conditioned on working, there would still be dual coverage issues.

eligibility that could have been subject to the issues with staggered roll-out research designs identified by Goodman-Bacon (2021).

I review the literature on crowd out and the effects of the ACA's Medicaid expansion on insurance coverage in "Existing Literature." I explain my approach to estimation and my data in "Estimation and Data." I present my crowd out estimates in the section "Main Findings," along with a range of robustness, sensitivity, and heterogeneity analyses. I offer concluding remarks in "Conclusion."

EXISTING LITERATURE

My work adds new evidence to a rich literature that studies crowd out relating to prior incremental expansions in public insurance eligibility to children, those who are pregnant, and parents. That work begins with Cutler and Gruber (1996) who estimated the extent of crowd out from expansions of Medicaid to pregnant women and children between 1987 and 1992. They found a 49% crowd-out rate for every two new Medicaid enrollees there was about one fewer instance of private coverage among their sample. Cutler and Gruber's approach used annual CPS data and instruments for changes in public coverage eligibility by constructing average state by year level eligibility measures for a random national sample of 300 children and 3,000 women of child-bearing age. For each state and year, the instrument therefore captured the proportion of a nationally representative random sample who would qualify under the state's rules in that year. It therefore captured increasing generosity in Medicaid eligibility at the state level but was not biased by endogenous individual decisions to attempt to become eligible.

Ham and Shore-Sheppard (2005) built on the instrumental variables approach of Cutler and Gruber (1996) by instrumenting for Medicaid eligibility using the proportion of individuals from *other* states who would be eligible under a given state's rules. That is, for each state, the instrument approximated the proportion of U.S. residents who would qualify under that state's rules, excluding those who live in that state. Replicating Cutler and Gruber's earlier analysis using the improved instrument and more frequent Survey of Income and Program Participation (SIPP) data, Ham and Shore-Sheppard found no evidence of crowd out. Yazici and Kaestner (2000), Blumberg et al. (2000), Shore-Sheppard (2000, 2008), Kronick and Gilmer (2002), Card and Shore-Sheppard (2004), Brown et al. (2007), Hamersma and Kim (2013), and others also found limited evidence to support the idea that expansions in public insurance eligibility significantly crowds out private insurance.

On the other hand, Gruber and Simon (2008) have shown that variation in the level of crowd out across studies arises 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, focusing on the effect of family rather than individual eligibility, using monthly rather than yearly data, etc.). Correcting for these issues, Gruber and Simon found that crowd out was at least 60% in their 1996 to 2002 SIPP data. Wagner (2015) also found large crowding-out effects relating to expansions in Medicaid eligibility for disabled individuals under 65. Wagner exploited a policy change that allowed states to offer Medicaid to their disabled residents who had monthly incomes up to 100% of the FPL. Wagner's instrumental variable estimates (following the method of Ham & Shore-Sheppard, 2005) suggested that each new Medicaid enrollee dropped a private plan in favor of the newly available public coverage (i.e., 100% crowd out).

In contrast to this existing work, the ACA's Medicaid eligibility expansion was significantly broader and more generous. For example, coverage was expanded to adults with no dependent children for the first time. Because the ACA expanded eligibility to individuals with greater incomes, those newly eligible individuals are perhaps more likely to have alternate sources of health insurance coverage relative to earlier expansions in eligibility. In contrast, the target populations for earlier expansions in eligibility tended to be those with very low incomes, and therefore those who are less likely to have alternate coverage options. The ACA's Medicaid expansion therefore provides a unique opportunity to revisit the issue of crowd out.

One key difference between my work and these earlier studies is that the existing work tended to focus on identifying who becomes eligible within a state and then uses those who remain ineligible as a comparison group, using panel data with repeated observations of the same individuals over time if available.[4] This is likely because earlier expansions in coverage created greater variation in eligibility

[4] One notable exception is Hamersma and Kim (2013), who used the actual eligibility threshold as their treatment variable. Hamersma and Kim's estimates can estimate the increase in public insurance coverage among a population for an intensive margin "X%" change in the eligibility

within a state rather than across states. In such a set up, authors have to use instruments to assign eligibility because of endogenous responses to changes in the eligibility thresholds. While I later show that my results are robust to using a similar approach, the state level decisions to expand Medicaid eligibility allow me to assign treatment at the state level and to compare changes in coverage before and after expansions in eligibility, using those in states who do not experience expanded eligibility as a comparison group.

Because my approach compares across expansion and non-expansion states, it is therefore also closely related to several existing studies that quantify the ACA's impact on insurance coverage rates, including Wherry and Miller (2016), Frean et al. (2017), Kaestner et al. (2017), Miller and Wherry (2017, 2019), Courtemanche et al. (2017, 2019), Duggan et al. (2019), and others. Broadly speaking, these authors have used differences in ACA provisions across states (or smaller geographic areas) for identification and found that the ACA's policy changes led to significant increases in health insurance coverage. Just as one example, focusing on the Medicaid expansion components of the ACA, Miller and Wherry (2019) estimated "a 17 percentage point increase in Medicaid enrollment among low-income adults in expansion states compared to non-expansion states" through the end of 2017. However, if increases in Medicaid coverage were accompanied by declines in private coverage, then Miller and Wherry's estimates overstate the effectiveness of Medicaid eligibility expansions in terms of increasing insurance coverage among low-income Americans.

In some of these existing studies, specifically Frean et al. (2017) and Kaestner et al. (2017), the authors considered the potential role of crowd out. In each case, using ACS data like I do, they found little evidence of declines in private coverage, suggesting no crowd out effects. However, these authors were not focused on the issue of crowd out in their work, and it is easy to see why they found no immediate evidence of crowd out. For example, Frean et al. (2017) did not restrict their analysis to those respondents who might be eligible for Medicaid after the eligibility expansion, used data only through 2015, and attempted to examine changes in private coverage due to several ACA provisions at once. They also diluted the main effect of interest by having separate post-expansion indicators for those who would have been eligible for Medicaid before the ACA (i.e., using 2010 rules to assign an indicator for eligibility in 2014 and 2015 to those who would qualify), those who were already eligible under certain early expansion provisions (i.e., using 2011 to 2013 rules in early expansion states to assign an indicator for eligibility in 2014 and 2015), and another separate indicator for those who only gained coverage eligibility in 2014.[5] Moreover, Frean et al. then interacted each of these groups with indicators for 2014 and 2015, and only reported the coefficients on the 2015 interactions when their outcome variable was private insurance coverage. Kaestner et al. (2017), meanwhile, used American Community Survey data from 2010 to 2014 for those with a high school diploma or less for their main analysis, meaning that they had only 1 year of post-expansion data and that relatively few of their estimation sample would be eligible for Medicaid.

Frisvold and Jung (2018) and Abraham et al. (2019) were relatively more focused on changes in private insurance due to the ACA. Frisvold and Jung (2018) looked for evidence of ACA-related crowding out using 2011 to 2015 March CPS data for those age 26 to 64. They found consistent negative effects on ESI prevalence among childless adults in expansion states, ranging from 1.6 to 2.7 percentage points depending on sample restriction. Those estimates would represent a crowd out rate of between 19% and 49%. However, their estimates were statistically no different from zero. The sample size of the March CPS in conjunction with the authors' sample restrictions and limited years of post-expansion data appear to leave them under-powered to detect crowding out. For example, their estimation sample consisted of between 28,701 and 32,066 childless adults, depending on specification.

threshold. However, it is less clear that such an approach would be meaningful in my setting given that the 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.

[5] Note that "early expansion" refers to states using a Section 1115 waiver to expand coverage before the ACA's 2014 "deadline." It is worth noting that, except for DC, no state expanded coverage to all those earning 138% of the FPL statewide before 2014.

Abraham et al. (2019), using the Insurance Component of the Medical Expenditure Panel Survey (MEPS-IC) from 2010 to 2015, found substantial negative effects on workers' take-up of ESI, but the estimates again were not statistically different from zero at conventional levels. In contrast to this earlier work, my findings provide consistent evidence of crowd out relating to the ACA's Medicaid expansion by using a longer sample period and using data that contains sufficient observations to detect relatively small percentage changes.

Sommers et al. (2014) and Ellis and Esson (2021) also examined the issue of crowd out, focusing on early expansion states. Sommers et al. used data through the end of 2011 and found a 30% to 40% rate of crowd out in Connecticut but no crowd out in DC. That being said, their estimates referred to a state level treatment using only a single treatment state in each case. They also only reported estimates for childless adults and only through 2011, before the implementation of the ACA's other provisions. They also did not use any of the established methods (Ham and Shore-Sheppard's instrumental variables approach, for example) as a check on their findings. Ellis and Esson focused on how crowd out might affect California's emergency department utilization, using machine learning techniques to predict who switched from uninsured versus private coverage to public coverage. Ellis and Esson focused on healthcare use because those who are crowded out experience a cost decrease for emergency department visits (because public coverage has lower cost-sharing). In contrast, the previously uninsured would be expected to substitute away from emergency department visits towards primary care visits because public coverage makes emergency departments more expensive for many new public coverage recipients because of existing California legislation regarding charity care for low-income individuals. As their crowd out estimates were used as a step toward a different goal, they also did not give the issue of crowd out a thorough treatment (i.e., using instrumental variable approaches, sensitivity checks, robustness, etc.).

Fundamentally, existing studies that consider changes in private coverage relating to the ACA's Medicaid eligibility expansions are incomplete because they do use a sufficient sample size to detect small changes, do not focus on those likely to qualify, or do not carefully consider the threats to identification that formal approaches to crowd out can handle. As part of my robustness checks, I use my sample to replicate some the existing work on the ACA's effects on private coverage, highlighting why they did not find any evidence of crowd out. For that exercise I focus on the approaches of Frean et al. (2017), Kaestner et al. (2017), and Frisvold and Jung (2018). This is because my approach is already quite similar to Sommers et al. (2014), comparing across states who do and do not expand using ACS data from early expansions, although Sommers et al. only examined through the end of 2011 and only looked at crowd out in DC and Connecticut. In addition, the machine learning approach of Ellis and Esson (2021) is beyond the scope of my work here and my ACS data has no information on employers to replicate the MEPS-IC approach of Abraham et al. (2019).[6] When I limit my sample similarly to Kaestner et al. (2017), Frean et al. (2017), and Frisvold and Jung (2018), I find broadly comparable effects. However, there is clear evidence of crowd out once I expand the sample to include more years of data and when I focus on low-income individuals.

My findings therefore add new high-quality evidence to the existing public insurance crowd out literature by carefully examining 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. As Hamersma and Kim (2013) explained, this work matters because further expansions in eligibility may "draw people away from private coverage, thus failing to improve overall rates of health insurance coverage." In the next section, I explain my ACS data and my approach to estimation.

[6] Because these studies are closely related to my work but reach different conclusions, in Appendix Table B2, I summarize each of their approaches to identification, sample selection choices, and main findings. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

ESTIMATION AND DATA

Data

To study health insurance crowd out, authors have used Current Population Survey (CPS) Data (Cutler & Gruber, 1996; Kronick & Gilmer, 2002) and Survey of Income and Program Participation (SIPP) Data (Hamersma & Kim, 2013; Wagner, 2015). 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) used restricted National Health Interview Survey (NHIS) data while Courtemanche et al. (2018) used Behavioral Risk Factor Surveillance System (BRFSS) data.

I follow Courtemanche et al. (2017, 2019) by using ACS data. I prefer ACS data because of its large sample size (particularly relative to CPS data), and because it has geographic identifiers, income and employment variables, and information on public and private health coverage, including whether that coverage is group/employment-based (ESI) or non-group coverage. It would be ideal to supplement my analysis with SIPP data, but SIPP began a new panel in 2014 ensuring that I would observe no individuals before and after 2014, the year most states expanded Medicaid coverage eligibility. The ACS samples approximately 1% 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 when responding. For each individual, the ACS asks 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 (usually referred to as "non-group" coverage), Medicare, Medicaid, military health care such as TRICARE or VA coverage, and Indian Health Service coverage. Notably, the categories are not mutually exclusive and, as an example, an individual may have coverage via their own job and as a dependent on someone else's plan. Wagner (2015) presented estimates including and excluding "overlap" individuals and shows that they have essentially no effect on her findings. Given it is the modal form of coverage among working age adults, it is worth noting that ACS data reports coverage provided via a respondent's or a family member's employer as ESI coverage (i.e., it makes no distinction between own coverage and coverage as a dependent on someone else's employment-based plan).

For my main estimates, I limit the sample to those aged 18 to 65 and earning no more than 138% of the relevant Federal Poverty Level for their family size, who appear in the 2009 to 2019 ACS data. The restriction to 138% of the FPL is to be as comparable as possible to existing work on the effects of Medicaid eligibility expansions. Moreover, because Medicaid eligibility is based on monthly income, I can only be confident that those whose incomes are below 138% of the FPL on a yearly basis were definitely eligible at least 1 month during the sample year. I begin my sample in 2009 because it gives me 5 years of data for each state prior to 2014, when most states expanded Medicaid eligibility. The final year of my sample is 2019 because of how the COVID-19 pandemic potentially affected both eligibility and health-based motivations to maintain health coverage from 2020 onward. I eliminate those who are covered by military or other sources or individuals who qualify for Medicaid by virtue of being a Supplemental Security Income (SSI) recipient (Burns & Dague, 2017; Hamersma & Kim, 2013).[7] I also eliminate those who report being full-time students. To summarize, my estimation sample therefore consists 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. As part of my sensitivity analyses, I later relax the income cut-off as monthly eligibility determination means there are individuals who would be eligible for Medicaid but who also have annual incomes above 138% of the FPL.

[7] Note that I do not remove the entire household from the sample if one member is eligible due to SSI.

In Table 1, I provide summary statistics regarding age, gender, marital status, race, and education—the demographic controls that I use in my main estimates—stratified first by states that do and do not expand Medicaid during the sample period and then by the period before and after 2014 (or the relevant expansion date for the non-2014 expansion states). Helpfully, the ACS assigns each individual to a unique Health Insurance Unit (HIU). As Hamersma and Kim (2013) explained, it is the total income of an HIU that Medicaid caseworkers can use to judge eligibility for Medicaid. Further, ACS documentation explains 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.[8] Conveniently, ACS data 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 straightforward 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.[9] For that reason, in addition to information on health insurance coverage sources, I also report average individual income, average family income, and the average Federal Poverty Limit (FPL) for the Health Insurance Units in the sample.

I use Kaiser Family Foundation data to determine Medicaid expansions at the state level.[10] I provide a map of Medicaid eligibility expansions by state in 2019, the end of my sample period, as an appendix item. I also provide a table listing the date each state expanded, if ever. Because some counties in California expanded Medicaid eligibility up to the ACA guidelines before the 2014 ACA deadline, I supplement my data using county-level Medicaid eligibility expansion information for California provided by Golberstein et al. (2015). For consistency and to be as conservative as possible, note that I consider expansion of Medicaid to have occurred only in the first full year the area (state, District of Columbia, or county in the case of California) expanded Medicaid eligibility to 138% of the FPL. There are some partial expansions in some counties in California and also in Connecticut, New Jersey, Minnesota, and Washington that do not meet that strict definition and are therefore counted as non-expanders until 2014. Appendix A provides a thorough explanation of Medicaid expansion, including details of limited early expansions. In the next section, I explain how I use my ACS sample to estimate the extent of crowd out relating to Medicaid eligibility expansion.

Estimation

Difference-in-differences

To estimate the extent of ESI crowd out among adults relating to Medicaid eligibility expansion, I first use a difference-in-differences 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, my main estimates use a "two-way fixed effects" specification of the following type:

$$Y_{ist} = \beta_0 + \beta_1 \times Expansion\ State_{ist} + \gamma_s + \lambda_t + X_{ist}\Pi + \varepsilon_{ist}. \quad (1)$$

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

[9] See https://usa.ipums.org/usa_action/variables/HIUFPGBASE.

[10] See https://www.kff.org/health-reform/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/.

In equation (1), Y_{ist} represents indicator variables denoting private insurance or Medicaid coverage for individual i in state s and year t.[11] 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 in states from the first full year t the state expanded Medicaid eligibility to 138% of the FPL, and is zero otherwise. 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 & Wherry, 2019) and public insurance crowd out (Hamersma & Kim, 2013; Wagner, 2015), I estimate equation (1) using a linear probability model (LPM) via OLS, ensuring that the coefficients of interest can be interpreted as percentage point changes in the outcome of interest. In all 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, if 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.

Event studies

My estimation strategy 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 trend 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, after presenting my main estimates (in the following section), I estimate an event-study specification that is a time-disaggregated version of the difference-indifferences approach that I specify in equation (1):

$$Y_{ist} = Expanded\ Medicaid_{ist} \times \sum_{k=-l}^{m} \delta_k 1\left[t - T_{is} = k\right] + \rho_s + \psi_t + X_{ist}\Pi + \varepsilon_{ist} \tag{2}$$

In equation (2), the difference relative to 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 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]

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

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

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

[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 & Abraham, 2021).

Instrumental variables estimates

My approach to studying crowd out relating to Medicaid eligibility expansion is mainly intended to produce estimates comparable to existing work on the ACA's Medicaid eligibility expansions (e.g., Miller & Wherry, 2019). For that reason, I follow that literature and limit my sample to those 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 for coverage, including using instruments that capture spatial and temporal variation in Medicaid generosity to predict eligibility and help avoid endogenous selection into Medicaid eligibility. This type of approach focuses on the coefficient on an eligibility indicator at the individual level, using non-eligible individuals (in the same state) as a comparison group. To examine whether my estimates are robust to selection into eligibility, I provide estimates of crowd out using the two-stage least squares instrumental variables approach of Ham and Shore-Sheppard (2005), which builds on the work of Cutler and Gruber (1996) and Currie and Gruber (1996a, 1996b). The approach uses the fraction of a nationally representative sample from their data that would be eligible under a given state's rules as an instrument. Ham and Shore-Sheppard instead excluded those who live in the state in question, determining the fraction of the entire sample who reside in other states who would be eligible under that state's rules. As Wagner (2015) explained, the instrument therefore provides a state by year measure of Medicaid generosity that is unrelated to the characteristics of the state's population. This IV approach is common in much of the recent Medicaid crowd out literature (Gruber & Simon, 2008; Hamersma & Kim, 2013; Wagner, 2015).

For the IV estimates, my approach to estimation employs the following linear probability model specification in the first stage:

$$ELIG_{ist} = \alpha_0 + \alpha_1 \times FRACELIG_{st} + \gamma_s + \lambda_t + X_{ist}\Pi + \varepsilon_{ist} \tag{3}$$

In this estimating equation, i, s, and t index individuals, states, and years. The dependent variable $ELIG_{ist}$ is an indicator for actual coverage (Medicaid or private depending on specification). The variable $FRACELIG_{st}$ indexes the proportion of my ACS respondents who would be eligible under the rules of state s in time t. The equation also includes 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 procedure then generates predicted values for eligibility ($\widehat{ELIG}_{ist}$) and uses those in the second stage instead of using actual eligibility:

$$Y_{ist} = \beta_0 + \beta_1 + \widehat{ELIG}_{ist} + \rho_s + \kappa_t + X_{ist}\Theta + \mu_{ist} . \tag{4}$$

The coefficient of interest is β_1 as it represents the percentage point change in the probability of individuals having coverage Y when moving from being ineligible to being eligible. The equation includes the same demographic controls and fixed effects indexed by X_{ist} while ρ_s, κ_t, and μ_{ist} now represent the state fixed effects, year fixed effects, and an idiosyncratic error term.

MAIN FINDINGS

I present my main findings in Panel A of Table 2. In the first three columns of estimates, the outcome of interest is an indicator for Medicaid coverage with the first column presenting the estimates from a parsimonious specification that does not include any demographic controls or industry and

TABLE 2 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 <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 <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 <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
State and year fixed effects	Y	Y	Y	Y	Y	Y
Demographic controls		Y	Y		Y	Y
Industry and occupation fixed effects			Y			Y

Notes: 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.

occupation fixed effects. In columns (2) and (3), I add demographic controls and then industry and occupation fixed effects. Because my outcomes of interest are indicator variables, the coefficients in Table 2 should be interpreted as percentage point changes in the proportion of respondents who have Medicaid or private coverage as indicated. 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. Across specifications, however, the 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.

In columns (4), (5), and (6), 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.

By using a Wald Chi-squared test on the coefficients from columns (3) and (6) of Panel A in a seemingly unrelated regression setup, I can rule out both zero crowd out and complete crowd out. Specifically, when I test whether the difference between the coefficients on the treatment indicator in columns (3) and (6) is zero (i.e., complete crowd out), I find $\chi^2 = 31.01$ ($p < 0.000$). When testing whether there is no crowd out, I find $\chi^2 = 1611.44$ ($p < 0.000$).

In Panel B of Table 2, 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. Note that I define childless adults as those who have no child as part of their household. 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. As I mention in the introductory section, these effects are changes among respondents in expansion states *relative* to similar individuals in non-expansion states. Summary statistics in Table 1 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), but with expansion states seeing more Medicaid coverage and non-expansion states seeing greater increases in private coverage. Put differently, the crowd out I observe 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 non-group coverage, such as the plans available via the ACA's healthcare exchanges) 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).

In the following subsections, I first present event studies that demonstrate that there are no pre-trends of concern in my data. Then, I examine the robustness of my estimates to alternate sample selection and estimation choices, study how the effects of interest vary across key subgroups, and contrast my main findings to estimates that use approaches common in the existing public insurance crowd out literature. In each case, I present estimates only from my preferred specification, one that includes demographic controls along with state, year, industry, and occupation fixed effects. I also demonstrate that my estimates are not sensitive to the issues raised by the new difference-in-differences literature (Goodman-Bacon, 2021), providing evidence to support earlier work by Miller and Wherry (2017, 2019) and Courtemanche et al. (2017) that used staggered roll-out designs to study the effects of Medicaid expansion on health coverage rates. Finally, I revisit some of the earlier work on the ACA's impact on private coverage, including work by Frean et al. (2017), Kaestner et al. (2017), and Frisvold and Jung (2018), to explain why they did not find statistically significant evidence of crowd out in their studies.

Event studies

As I mention in "Estimation and Data," 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 2, Figures 1(a) and 1(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 Figures 1(c) and 1(d), I limit the sample to just those who report being employed (as in Panel B of Table 2), while Figures 1(e) and 1(f) present event studies where the sample is limited to childless adults. In each case, there is no evidence of a problematic

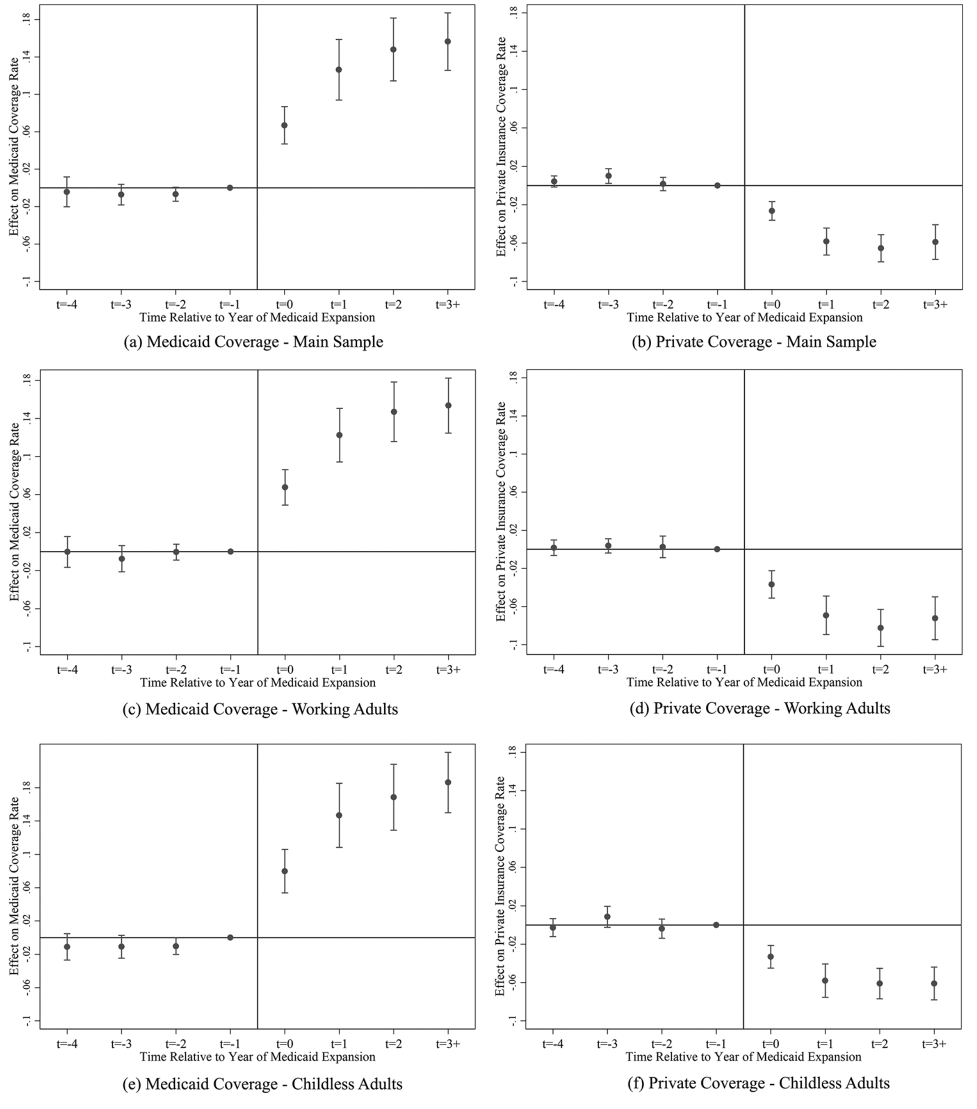


FIGURE 1 Event studies.

Notes: Data is ACS 2009 to 2019 restricted as described in Section 2. Panels (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.

pre-trend and clear evidence to suggest that any increase in Medicaid coverage is accompanied by a relative decline in private health coverage.[15]

[15] Notably, Courtemanche et al. (2019), who also used ACS data, started 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.

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 3. In column (1) of the table, the sample includes 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 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. Also, as I mention earlier, Medicaid eligibility is determined monthly, but I only have an annual measure of income in my data. Therefore, many of those who have incomes over 138% of the FPL on an annual basis could qualify for Medicaid in some months. For that reason, 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 column (2), 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. Those with incomes below 100% of the FPL, however, are not eligible for subsidies to afford coverage via a healthcare exchange. 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) reported 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 column (3) of Table 3, I present estimates where I limit my estimation sample to only those with a high-school education or less (following Kaestner et al., 2017, and Frisvold & Jung, 2018).

This sensitivity analysis allows me to identify those who are perhaps most likely to be eligible for Medicaid (given education is strongly related to income) but helps to avoid concerns about endogenous labor market decisions. Broadly speaking, the estimates suggest that limiting the sample by education is a relatively ineffective way to target the Medicaid eligible population, with the sample size increasing notably and the effect of Medicaid expansion on Medicaid coverage decreasing to just 3.6 percentage points. That said, the corresponding estimate for the change in private coverage indicates a 1.6 percentage point decline, implying a crowd-out rate of 44.4% even when limiting the sample by education to try to avoid endogenous labor market choices. Note that I revisit the approaches of Kaestner et al. (2017) and Frisvold and Jung (2018) later when I consider why earlier work on the ACA's Medicaid expansions found no evidence of crowd out.

In columns (4) and (5), 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 fully or partially expanded Medicaid early.[16] In columns (6) and (7), I present estimates that do not use ACS-provided weights and that do not cluster 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.

[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.

TABLE 3 Sensitivity analyses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Dependent variable - has Medicaid coverage							
Effect of Medicaid expansion	0.098***	0.099***	0.034***	0.099***	0.142***	0.110***	0.107***
	(0.018)	(0.022)	(0.007)	(0.025)	(0.017)	(0.023)	(0.001)
Panel B: Dependent variable - has private health coverage							
Effect of Medicaid expansion	−0.047***	−0.037***	−0.016***	−0.044***	−0.052***	−0.051***	−0.046***
	(0.010)	(0.010)	(0.006)	(0.012)	(0.008)	(0.011)	(0.001)
Observations	3,175,422	1,330,253	3,182,129	1,660,821	1,460,019	1,987,814	1,987,814
State, year, ind., & occ. FEs	Y	Y	Y	Y	Y	Y	Y
Demographic eontrols	Y	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	Limit to high-school or less	2010 to 2018 ACS data Only	Eliminate early expanders	No weights	No clustering

Notes: 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 provide estimates where I limit the sample to those with a high-school diploma or less education. In Column (4), I remove 2009 and 2019 data from the sample. In Column (5), 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, 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 4 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
State, year, ind., & occ. FEs	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y
Sample restrictions	Women only	Men only	White	All other races	Aged 40 or younger	Aged over 40

Notes: 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 characteristic).

TABLE 5 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	1,039,782	1,987,814
State, year, ind., & occ. fixed effects	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y

Notes: American Community Survey 2009 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.

In Table 4, 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 women and for younger respondents (defined as those aged under 40) are notable. For instance, when looking at only women, 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.

Changes in ESI vs. changes in non-group coverage

Table 5 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 then to

TABLE 6 Estimates using other approaches to measuring crowd out.

	(1) Medicaid coverage	(2) Private coverage	(3) Medicaid coverage	(4) Private coverage
	OLS		2SLS/IV	
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
F-stat from first stage			115.9	115.9
State, year, ind., & occ. fixed effects	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y

Notes: American Community Survey data restricted as described in Section 2 except that I cannot include 2009 and 2010 data here because the Kaiser Family Foundation only tracks eligibility for childless adults from 2011. I also expand the sample to 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 actual eligibility based on observable individual characteristics. Estimates in Columns (3) and (4) use the Ham and Shore-Sheppard (2005) Medicaid eligibility instrument to impute eligibility. 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.

separately estimate changes in private coverage by employment-based coverage (i.e., ESI) versus other private coverage (i.e., non-group coverage). I also again report the main effect of Medicaid eligibility expansion from Table 2 for the sake of comparison. In Table 5, 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 (note, the sample is restricted to working adults for these estimates) and other private non-group health coverage. These estimates suggest that the decline in private coverage that I report in Table 2 consists of similarly sized declines in both ESI and private non-group health coverage.

Estimates comparable to existing crowd out literature

In Table 6, 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 consider actual and imputed eligibility at the individual level. The first set of estimates uses actual eligibility (i.e., ignoring any 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 instrumental variable approach, as described in "Estimation and Data." 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 incomes up to 200% of the relevant Federal Poverty Level in the sample. Previous empirical work on crowd out 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 & Kim, 2013, for example).

Using this alternative approach, I again find significant evidence of crowd out. For example, the estimates in column (1) of Table 6 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, implying a crowd-out rate of over 75%. However, as Ham and Shore-Sheppard (2005) explained, 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 2. These estimates also help to ease concerns that my main findings are driven by selection into Medicaid eligibility income groups.

Issues with staggered roll-out research designs

My main estimates attempt to identify crowd out relating to Medicaid eligibility expansions in a staggered roll-out setting. My approach is similar to existing work on the effects of Medicaid expansion on rates of health coverage (Courtemanche et al., 2017; Miller & Wherry, 2017). However, such approaches are potentially subject to the issues raised by the "new" difference-in-differences literature (Borusyak et al., 2021; Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). For example, 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 "two-way fixed effects" difference-in-differences approach will estimate a treatment effect that is a weighted average of the treatment effect estimates for all potential comparison groups in the data (e.g., "early treated units versus never treated units"). Problematically, one potential comparison group is later treated versus earlier treated units. The new difference-in-differences literature highlights that 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-differences estimator. Further, 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 plurality of states expanded Medicaid eligibility in 2014 and there are a large number of states who do not expand eligibility during my sample period, immediately limiting the extent to which my estimates could be biased by the issues raised by Goodman-Bacon (2021) and others.

That said, in this section I 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 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-differences approach.

I present the estimates from such an exercise in Table 7, 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 can be compared to the estimates in column (5) of Table 3, 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 & Wherry, 2019). In column (5) of Table 3, the treatment effects amounted to 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 data from 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 early and later treated groups in a two-way fixed effects difference-in-differences approach.

The new difference-in-differences literature also provides a range of alternative estimator options that attempt to directly address any biases that could arise when using a two-way fixed effects difference-in-differences approach. However, to date, many of these approaches (see Callaway & Sant'Anna, 2021) can be implemented only when using true panel data with multiple observations

TABLE 7 Effects using a two group, two period approach.

	(1) Has Medicaid coverage	(2) Has private health coverage
Expansion state	0.059**	−0.007
	(0.023)	(0.020)
After 2014	0.032***	0.097***
	(0.004)	(0.005)
Expansion state × after 2014	0.148***	−0.056***
	(0.020)	(0.007)
Observations	451	451
N of states	41	41

Notes: American Community Survey 2009 to 2019 first restricted as described in Section 2 and then collapsed to state level observations with those states that expanded Medicaid in years other than 2014 excluded from the analysis.

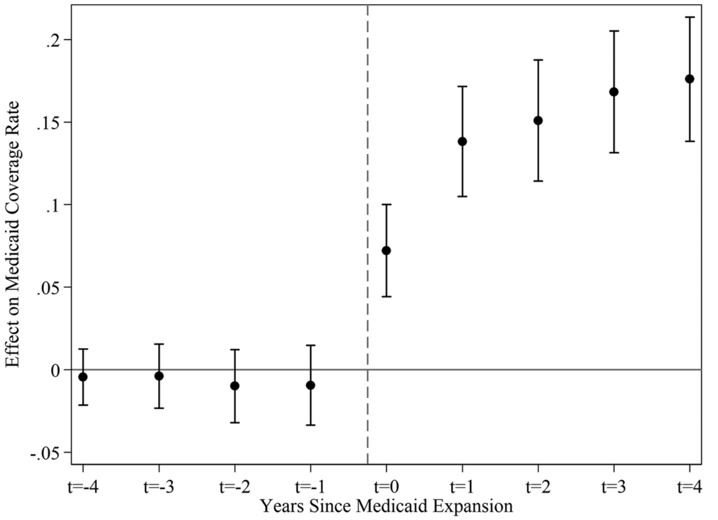
TABLE 8 Estimates using a difference-in-differences imputation approach.

	(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 <138% FPL					
Effect of Medicaid expansion	0.136***	0.137***	0.137***	−0.059***	−0.058***	−0.058***
	(0.014)	(0.014)	(0.014)	(0.007)	(0.007)	(0.007)
Observations	1,987,814	1,987,814	1,987,814	1,987,814	1,987,814	1,987,814
	Panel B: Working adults aged 18 to 65 with family income <138% FPL					
Effect of Medicaid expansion	0.132***	0.133***	0.133***	−0.071***	−0.069***	−0.069***
	(0.013)	(0.013)	(0.013)	(0.008)	(0.008)	(0.008)
Observations	1,039,787	1,039,787	1,039,766	1,039,787	1,039,787	1,039,766
	Panel C: Childless Adults Aged 18 to 65 with Income <138% FPL					
Effect of Medicaid expansion	0.163***	0.163***	0.165***	−0.056***	−0.054***	−0.056***
	(0.016)	(0.016)	(0.016)	(0.007)	(0.007)	(0.007)
Observations	952,833	952,833	952,823	952,833	952,833	952,823
State and year fixed effects	Y	Y	Y	Y	Y	Y
Demographic controls		Y	Y		Y	Y
Industry and occupation fixed effects			Y			Y

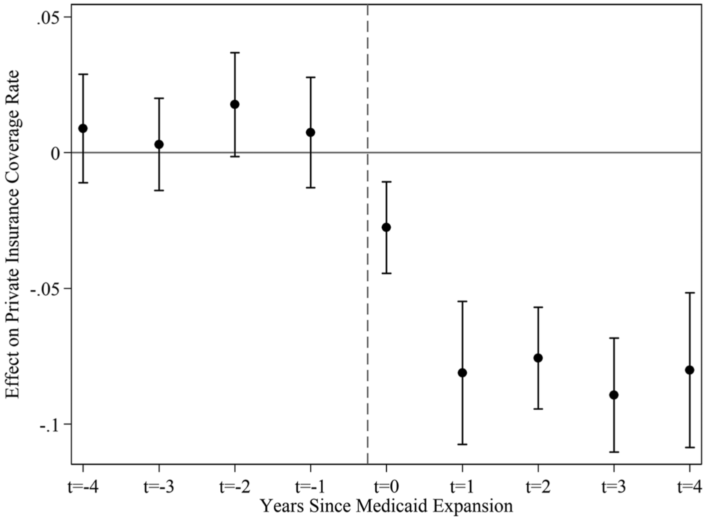
Notes: American Community Survey Data from 2009 and 2019 restricted as described in Section 2 (in Panels B and C that sample is further restricted to working adults and then childless adults as indicated). Estimates generated using Borusyak et al.'s difference-in-differences imputation approach to account for any heterogeneous treatment timing bias. 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.

per unit over time. In contrast, my ACS data consists of repeated cross-sections. For that reason, I use the Borusyak et al.'s (2021) "imputation" estimator as it can handle repeated cross-section data.[17] In Table 8, I provide estimates that repeat those from Table 2 with the same sample but using the Borusyak et al. imputation approach. The estimates are consistently larger in magnitude for both changes in Medicaid and private insurance coverage. On the other hand, the estimates for the rate

[17] In Stata, this is implemented via the "did_imputation" package with the associated "event_plot" package to then plot the estimates.



(a) Medicaid Coverage



(b) Private Coverage

FIGURE 2 Event studies using Borusyak et al. difference-in-differences imputation approach.

Notes: Data is ACS 2009 to 2019 restricted as described in Section 2. Panels (a) and (b) present event studies that are comparable to sub-figures (a) and (b) in Figure 1. Standard errors are clustered at the state level. Bars around point estimates represent 95% confidence intervals. Notice that the Borusyak et al. (2021) 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.

of crowd out are quite consistent with my earlier estimates. For example, in the preferred specifications in columns (3) and (6) of Panel A, there is a 13.7 percentage point increase in Medicaid coverage and a 5.8 percentage point decline in private coverage after eligibility expansion. Those estimates imply a crowd out rate of 42% (= 5.8/13.7). In Panel B, the crowd out rate among working adults is 52% (= 6.9/13.3). In Panel C, for childless adults, the crowd out rate is 34% (= 5.6/16.5). In my OLS estimates, my estimates implied crowd out of 43%, 56%, and 33% across the same three groups.

Figure 2 presents the event studies corresponding to my preferred specification with demographic controls and fixed effects. In particular, Figures 2(a) and 2(b) present event studies that are comparable to Figures 1(a) and 1(b). The pattern of estimates is quite similar even when using an approach that corrects for any potential heterogeneous treatment timing bias.

The estimates in this sub-section show that my findings are not driven by the issues surrounding staggered roll-out difference-in-differences approaches (Goodman-Bacon, 2021). My estimates in Table 8 also provide clear support for earlier work that examines the effects of Medicaid expansion on health coverage, which usually employed a staggered roll-out difference-in-differences approach but was published before the potential issues with such approaches became apparent.

Replicating existing work on ACA-related crowd out

As I mention in the section "Existing Literature," Sommers et al. (2014), Kaestner et al. (2017), Frean et al. (2017), Frisvold and Jung (2018), Abraham et al. (2019), and Ellis and Esson (2021) considered the issue of crowd out relating to the ACA's Medicaid eligibility expansions. Among these, Sommers et al. (2014) and Ellis and Esson (2021) reported evidence of crowd out. Sommers et al. focused only on two early expansion states through 2011 and Ellis and Esson reported crowd out based on a machine learning approach that predicts prior coverage status among emergency department patients in California. In contrast, Kaestner et al. (2017), Frean et al. (2017), Frisvold and Jung (2018), and Abraham et al. (2019) found little evidence of statistically significant crowding-out effects relating to the ACA's changes. However, I show in this section, by revisiting the approaches of Kaestner et al. (2017), Frean et al. (2017), and Frisvold and Jung (2018) in Table 9, that this is likely to be due to sample choices and limitations. I explain those choices and limitations in the "Existing Literature" section and summarize them in Appendix Table B2.

In particular, in Panel A of Table 9, I focus on the Kaestner et al. (2017) approach. Therefore, my ACS sample is restricted to the years 2010 to 2014 and those aged 22 to 64. In columns (1) and (2), my sample includes all those with a high school diploma or less. In columns (3) and (4), I restrict to 138% of the FPL for comparison purposes. Regardless of sample restriction, and mirroring the findings of Kaestner et al., I find a statistically significant positive effect on Medicaid coverage and a negative effect on private coverage that is not statistically significant.

In Panel B, I replicate the approach of Frisvold and Jung (2018), who followed a similar approach to Kaestner et al. (2017), limiting to those with a high school education or less, but used March CPS data from 2011 to 2015, which represents 1 additional post-expansion year. Each March CPS is 2% to 3% of the sample size of the ACS for the same year. For that reason, I take a random 2.5% sample of my ACS data for 2011 to 2015 to create those estimates. I find no evidence of a statistically significant effect on Medicaid or private coverage. However, when I instead limit the sample to low-income individuals, rather than only high school graduates or less, I see a statistically significant increase in Medicaid and a statistically significant decline in private coverage. The estimates imply 65% crowd out (= 3.5/5.4). These estimates highlight the importance of using more than 1 expansion year and of focusing on those likely to be eligible under the new rules to study crowd out. Specifically, in both Panels A and B, those with income below 138% of the FPL are a small fraction (less than 30%) of the total with a high school diploma or less.

In Panel C, I closely follow Frean et al. (2017) by using ACS data from 2012 to 2015, with no education or income restrictions on the sample, and by using three indicators to divide the sample into those who, after the Medicaid eligibility expansion in 2014, would also have been eligible before the expansion ("previously eligible"), those made eligible at some point before 2014 due to partial expansions in CA, CT, DC, NJ, MN, and WA ("early eligible"), and those who only gained eligibility in 2014 ("newly eligible"). I use Kaiser Family Foundation data on state level eligibility over time to assign eligibility across states and years. As they do, I report estimates for each group interacted with an indicator for 2015 and include location, income group, and year fixed effects, each interacted with household type (married or single, with kids and without kids). My estimates also include interactions with 2014 that are not reported in the table, but that show a similar but slightly attenuated pattern.

TABLE 9 Replicating estimates from earlier work on ACA Medicaid expansion.

	(1) Medicaid	(2) Private coverage	(3) Medicaid	(4) Private coverage
Panel A: Replicates Kaestner et al. (2017)				
Sample restriction: 2010 to 2014 and	HS or Less	HS or Less	<138% of FPL	<138% of FPL
effect of Medicaid expansion	0.021*	−0.006	0.042*	−0.017
	(0.011)	(0.006)	(0.024)	(0.012)
Observations	3,190,533	3,190,532	895,108	895,107
Panel B: Replicates Frisvold and Jung (2018)				
Sample Restriction: 2011 to 2015 and	HS or Less	HS or Less	<138% of FPL	<138% of FPL
effect of Medicaid expansion	0.025	−0.019	0.054**	−0.035*
	(0.019)	(0.014)	(0.024)	(0.018)
Observations (Random 2.5% of ACS data)	73,077	73,049	23,522	23,470
Panel C: Replicates Frean et al. (2017)				
Sample restriction: 2012 to 2015 and	None	None	<138% of FPL	<138% of FPL
Previously eligible × 2015	0.154***	−0.057***	0.108***	−0.086***
	(0.013)	(0.006)	(0.015)	(0.008)
Early eligible × 2015	0.050**	0.005	0.067***	−0.011
	(0.024)	(0.017)	(0.014)	(0.012)
Newly eligible × 2015	0.196***	−0.045***	0.147***	−0.057***
	(0.014)	(0.007)	(0.014)	(0.006)
Observations	5,940,971	5,940,971	765,113	765,113

Notes: American Community Survey Data from 2009 and 2019. In Panel A, the sample is restricted to the years 2010 to 2014 only, age 22 to 64, to replicate Kaestner et al. (2017). In Panel B, I replicate Frisvold and Jung (2018) who follow a similar approach to Kaestner et al. but use March CPS data from 2011 to 2015. Each March CPS is about 2 to 3% of the sample size of the ACS for the same year. For that reason, I take a random 2.5% sample of my ACS data for 2011 to 2015 to create the estimates. In Panels A and B, I provide estimates for those with a high school (HS) diploma or less and those below 138% of the FPL as indicated. In Panel C, I closely follow Frean et al. (2017) by using data from 2012 through 2015 and provide estimates for the full sample and those below 138% of the FPL as indicated. Standard errors, clustered at the state level, in parentheses in all panels.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Demographic controls include age, gender, education, marital status, and race.

In contrast to Frean et al.'s (2017) findings, I find relatively strong evidence of crowd out using this type of approach. For those who were previously eligible, I find a 15.4% increase in Medicaid coverage in 2015, relative to the years 2012 and 2013. That is combined with a 5.7% decline in private coverage, indicating a 37% crowd out rate. For the newly eligible group, I find a 23% crowd out rate. On the other hand, there is no evidence of crowd out among the early eligible group. When I limit the sample to those under 138% of the FPL, the estimates of crowd out are 80% and 39% for the previously and newly eligible groups with again no evidence of crowd out for the early eligible group.

What is unusual is that I find consistent evidence of crowd out even though my estimates on the effect on Medicaid coverage is generally similar to Frean et al.'s (2017) findings (see their Table 6, p. 82). I will caution, however, that my approach is not an exact replica of Frean et al. because I do not include measures of the penalty for non-coverage and the percent subsidy available to those who qualify for subsidies. While these should not apply to any of those with below 138% of the FPL in expansion states (i.e., they should have little impact on the group I am primarily interested in here), it is therefore hard to draw strong conclusions from this quasi-replication exercise. It is beyond the scope of my work to identify exactly why we find different effects when taking similar approaches, especially because it is clear from my main estimates in Table 2 that there is significant evidence of

TABLE 10 Summary of key estimates.

Sample/restriction	Main sample	Working adults	Childless adults	<100% of the FPL	High school education or less	Women	Other races	Under 40	Instrumental variables
Change in Medicaid coverage	10.7%	10.5%	13.4%	9.9%	3.4%	10.2%	8.1%	13.0%	51.2%
Change in private insurance coverage	−4.6%	−5.9%	−4.4%	−3.7%	−1.6%	−5.1%	−4.1%	−6.1%	−16.6%
Implied crowd out	43%	56%	33%	37%	47%	50%	51%	47%	32%

Notes: Main sample consists 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. Subsequent columns gather and summarize the change in coverage for various policy-relevant subgroups and alternate approaches to estimation.

crowd out when using a longer and larger sample, and that the effects are robust to alternate empirical strategies and a variety of sensitivity analyses and robustness checks.

Gathering estimates of crowd out

In Table 10, I summarize the change in Medicaid and private coverage from earlier tables of estimates. With those estimates, I also report the implied crowd-out effects for various policy-relevant subgroups and alternate approaches to estimation. In the table, my estimates reflect crowd out effects of between 32% to 56%, which is in line with the work of Cutler and Gruber (1996), Gruber and Simon (2008), and Sommers et al. (2014), but still shy of the 100% crowd out estimates reported by Wagner (2015).

The estimates also highlight that crowd out effects are greatest among policy-relevant subgroups such as women and respondents who are not white. Ignoring the source or cost of coverage, the estimates therefore suggest that expansions in eligibility are having a bigger impact on health insurance coverage rates among men and white Americans. On the other hand, the estimates also suggest that those in vulnerable groups are experiencing greater relative declines in private coverage, and likely experiencing a significant decline in expenditure on monthly premiums and cost sharing for healthcare.

CONCLUSION

I document significant crowd out relating to the ACA's Medicaid eligibility expansions. My estimates suggest that for every 10 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 (consisting of roughly equal relative effects on employment-based and other non-group coverage). Illustrating that Medicaid coverage is likely displacing 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 fewer workers are covered by a private health plan, relative to comparable respondents in non-expansion states.[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) when compared to the pre-ACA options of purchasing coverage privately or sharing in the cost of ESI with an employer. This suggests that crowd-out of private insurance from Medicaid eligibility expansions is neither inherently good or bad in terms of overall welfare effects. On the other hand, to the extent that the ultimate goal of expansions in Medicaid eligibility is to improve public health via increases in the proportion of Americans who have health insurance, it remains important to understand the net effect of relatively generous expansions in Medicaid eligibility on overall rates of coverage.

It is worth noting that my findings, given adults with income above 138% of the FPL are increasingly likely to already have some kind of private health insurance, suggest that further expansions in Medicaid eligibility might fail to meaningfully improve overall rates of health insurance coverage. Indeed, because the ACA created health insurance exchanges for those ineligible for Medicaid, further increases in public insurance eligibility would require people switching people from the subsidized private coverage on the exchanges to Medicaid coverage, essentially mandating significant crowding out. Previous expansions in Medicaid eligibility did not have to consider the extent to which

[18] Notably, these effects mean that a large fraction of adults gained coverage. It is therefore worth noting that Carey et al. (2020) showed 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) showed 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.

the expansion would crowd out participation in other programs that are also designed to increase insurance coverage. For those interested in the effect of expansions in public coverage eligibility, it appears that analyses of future expansions in eligibility will be significantly more complex.

ACKNOLWEDGEMENTS

Thanks to Alice Chen, Laura Wherry, Monica Deza, Keith Teltser, Kathryn Wagner, David Slusky, Lucie Schmidt, Kosali Simon, Joshua Gottlieb, Xioaxue Li, Michael Sacks, several anonymous reviewers, and session participants at the 2021 Southern Economic Association, 2022 American Society of Health Economists, and 2023 Eastern Economic Association annual meetings for helpful comments.

REFERENCES

Abraham, J. M., Royalty, A. B., & Drake, C. (2019). The impact of Medicaid expansion on employer provision of health insurance. *International Journal of Health Economics and Management*, *19*(3), 317–340. https://doi.org/10.1007/s10754-018-9256-x

Antwi, Y. A., Moriya, A. S., & Simon, K. (2013). Effects of federal policy to insure young adults: Evidence from the 2010 Affordable Care Act's dependent-coverage mandate. *American Economic Journal: Economic Policy*, *5*(4), 1–28. https://doi.org/10.1257/pol.5.4.1

Barbaresco, S., Courtemanche, C. J., & Qi, Y. (2015). Impacts of the Affordable Care Act dependent coverage provision on health-related outcomes of young adults. *Journal of Health Economics*, *40*, 54–68. https://doi.org/10.1016/j.jhealeco.2014.12.004

Blumberg, L. J., Dubay, L., & Norton, S. A. (2000). Did the Medicaid expansions for children displace private insurance? An analysis using the SIPP. *Journal of Health Economics*, *19*(1), 33–60. https://doi.org/10.1016/s0167-6296(99)00020-x

Borusyak, K., Jaravel, X., & Spiess, J. (2021). *Revisiting event study designs: Robust and efficient estimation* [Working paper]. arXiv. https://doi.org/10.48550/arXiv.2108.12419

Brown, J. R., Coe, N. B., & Finkelstein, A. (2007). Medicaid crowd-out of private long-term care insurance demand: Evidence from the Health and Retirement Survey. *Tax Policy and the Economy*, *21*, 1–34. https://doi.org/10.1086/tpe.21.20061913

Burns, M., & Dague, L. (2017). The effect of expanding Medicaid eligibility on Supplemental Security Income program participation. *Journal of Public Economics*, *149*, 20–34. https://doi.org/10.1016/j.jpubeco.2017.03.004

Callaway, B., & Sant'Anna, P. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200–230. https://doi.org/10.1016/j.jeconom.2020.12.001

Card, D., & Shore-Sheppard, L. D. (2004). Using discontinuous eligibility rules to identify the effects of the federal Medicaid expansions on low-income children. *Review of Economics and Statistics*, *86*(3), 752–766. https://doi.org/10.1162/0034653041811798

Carey, C. M., Miller, S., & Wherry, L. R. (2020). The impact of insurance expansions on the already insured: The Affordable Care Act and Medicare. *American Economic Journal: Applied Economics*, *12*(4), 288–318. https://doi.org/10.1257/app.20190176

Courtemanche, C., Marton, J., Ukert, B., Yelowitz, A., & Zapata, D. (2017). Early impacts of the Affordable Care Act on health insurance coverage in Medicaid expansion and non-expansion states. *Journal of Policy Analysis and Management*, *36*(1), 178–210. https://doi.org/10.1002/pam.21961

Courtemanche, C., Marton, J., Ukert, B., Yelowitz, A., & Zapata, D. (2018). Early effects of the Affordable Care Act on health care access, risky health behaviors, and self-assessed health. *Southern Economic Journal*, *84*(3), 660–691. https://doi.org/10.1002/soej.12245

Courtemanche, C., Marton, J., Ukert, B., Yelowitz, A., Zapata, D., & Fazlul, I. (2019). The three-year impact of the Affordable Care Act on disparities in insurance coverage. *Health Services Research*, *54*, 307–316. https://doi.org/10.1111/1475-6773.13077

Currie, J., & Gruber, J. (1996a). Health insurance eligibility, utilization of medical care, and child health. *The Quarterly Journal of Economics*, *111*(2), 431–466. https://doi.org/10.2307/2946684

Currie, J., & Gruber, J. (1996b). Saving babies: The efficacy and cost of recent changes in the Medicaid eligibility of pregnant women. *Journal of Political Economy*, *104*(6), 1263–1296. https://doi.org/10.1086/262059

Cutler, D. M., & Gruber, J. (1996). Does public insurance crowd out private insurance? *The Quarterly Journal of Economics*, *111*(2), 391–430. https://doi.org/10.2307/2946683

De Chaisemartin, C., & d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, *110*(9), 2964–2996. https://doi.org/10.1257/aer.20181169

Duggan, M., Goda, G. S., & Jackson, E. (2019). The effects of the Affordable Care Act on health insurance coverage and labor market outcomes. *National Tax Journal*, *72*(2), 261–322. https://doi.org/10.17310/ntj.2019.2.01

Ellis, C. M., & Esson, M. I. (2021). Crowd-out and emergency department utilization. *Journal of Health Economics*, *80*, 102542. https://doi.org/10.1016/j.jhealeco.2021.102542

Frean, M., Gruber, J., & Sommers, B. D. (2017). Premium subsidies, the mandate, and medicaid expansion: Coverage effects of the Affordable Care Act. *Journal of Health Economics*, *53*, 72–86. https://doi.org/10.1016/j.jhealeco.2017.02.004

Frisvold, D. E., & Jung, Y. (2018). The impact of expanding Medicaid on health insurance coverage and labor market outcomes. *International Journal of Health Economics and Management*, *18*(2), 99–121. https://doi.org/10.1007/s10754-017-9226-8

Golberstein, E., Gonzales, G., & Sommers, B. D. (2015). California's early ACA expansion increased coverage and reduced out-of-pocket spending for the state's low-income population. *Health Affairs*, *34*(10), 1688–1694. https://doi.org/10.1377/hlthaff.2015.0290

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254–277. https://doi.org/10.1016/j.jeconom.2021.03.014

Gruber, J., & Simon, K. (2008). Crowd-out 10 years later: Have recent public insurance expansions crowded out private health insurance? *Journal of Health Economics*, *27*(2), 201–217. https://doi.org/10.1016/j.jhealeco.2007.11.004

Ham, J. C., & Shore-Sheppard, L. (2005). The effect of Medicaid expansions for low-income children on Medicaid participation and private insurance coverage: Evidence from the SIPP. *Journal of Public Economics*, *89*(1), 57–83. https://doi.org/10.1016/j.jpubeco.2003.07.011

Hamersma, S., & Kim, M. (2013). Participation and crowd out: Assessing the effects of parental Medicaid expansions. *Journal of Health Economics*, *32*(1), 160–171. https://doi.org/10.1016/j.jhealeco.2012.09.003

Kaestner, R., Garrett, B., Chen, J., Gangopadhyaya, A., & Fleming, C. (2017). Effects of ACA Medicaid expansions on health insurance coverage and labor supply. *Journal of Policy Analysis and Management*, *36*(3), 608–642. https://doi.org/10.1002/pam.21993

Kronick, R., & Gilmer, T. (2002). Insuring low-income adults: Does public coverage crowd out private? *Health Affairs*, *21*(1), 225–239. https://doi.org/10.1377/hlthaff.21.1.225

Lennon, C. (2021). *Did the Affordable Care Act increase the availability of employer-sponsored health insurance?* [Symposium article]. Southern Economic Journal Association. https://doi.org/10.1002/soej.12543

Lo Sasso, A. T., & Buchmueller, T. C. (2004). The effect of the state children's health insurance program on health insurance coverage. *Journal of Health Economics*, *23*(5), 1059–1082. https://doi.org/10.1111/2Fj.1475-6773.2007.00707.x

Miller, S., Johnson, N., & Wherry, L. R. (2021). Medicaid and mortality: New evidence from linked survey and administrative data. *The Quarterly Journal of Economics*, *136*(3), 1783–1829. https://doi.org/10.1093/qje/qjab004

Miller, S., & Wherry, L. R. (2017). Health and access to care during the first 2 years of the ACA Medicaid expansions. *New England Journal of Medicine*, *376*(10), 947–956. https://doi.org/10.1056/NEJMsa1612890

Miller, S., & Wherry, L. R. (2019). Four years later: Insurance coverage and access to care continue to diverge between ACA Medicaid expansion and non-expansion states. *AEA Papers and Proceedings*, *109*, 327–333. https://doi.org/10.1257/pandp.20191046

Shore-Sheppard, L. D. (2000). The effect of expanding Medicaid eligibility on the distribution of children's health insurance coverage. *ILR Review*, *54*(1), 59–77. https://doi.org/10.2307/2696032

Shore-Sheppard, L. D. (2008). Stemming the tide? The effect of expanding Medicaid eligibility on health insurance coverage. *The BE Journal of Economic Analysis & Policy*, *8*(2). https://doi.org/10.2202/1935-1682.2022

Sommers, B. D., Kenney, G. M., & Epstein, A. M. (2014). New evidence on the Affordable Care Act: coverage impacts of early Medicaid expansions. *Health Affairs*, *33*(1), 78–87. https://doi.org/10.1377/hlthaff.2013.1087

Soni, A., Wherry, L. R., & Simon, K. I. (2020). How have ACA insurance expansions affected health outcomes? Findings from the literature: A literature review of the Affordable Care Act's effects on health outcomes for non-elderly adults. *Health Affairs*, *39*(3), 371–378. https://doi.org/10.1377/hlthaff.2019.01436

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199. https://doi.org/10.1016/j.jeconom.2020.09.006

Teltser, K., Lennon, C., & Burgdorf, J. (2021). Do ridesharing services increase alcohol consumption? *Journal of Health Economics*, *77*, 102451. https://doi.org/10.1016/j.jhealeco.2021.102451

Wagner, K. L. (2015). Medicaid expansions for the working age disabled: Revisiting the crowd-out of private health insurance. *Journal of Health Economics*, *40*, 69–82. https://doi.org/10.1016/j.jhealeco.2014.12.007

Wherry, L. R., & Miller, S. (2016). Early coverage, access, utilization, and health effects associated with the Affordable Care Act Medicaid expansions: A quasi-experimental study. *Annals of Internal Medicine*, *164*(12), 795–803. https://doi.org/10.7326/M15-2234

Wolfe, C. J., Rennie, K. E., & Truffer, C. J. (2017). *2017 actuarial report on the financial outlook for Medicaid*. U.S. Department of Health and Human Services, Centers for Medicare & Medicaid Services, Office of the Actuary. https://www.cms.gov/Research-Statistics-Data-and-Systems/Research/ActuarialStudies/Downloads/MedicaidReport2017.pdf

Yazici, E. Y., & Kaestner, R. (2000). Medicaid expansions and the crowding out of private health insurance among children. *Inquiry*, *37*(1), 23–32. https://www.jstor.org/stable/29772870

SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Lennon, C. (2023). Did the Affordable Care Act's Medicaid eligibility expansions crowd out private health insurance coverage? *Journal of Policy Analysis and Management*, 1–28. https://doi.org/10.1002/pam.22556

AUTHOR BIOGRAPHY

Conor Lennon is an Associate Professor of Economics at Rensselaer Polytechnic Institute, 3402 SAGE, 110 8th Street, Troy, NY 12180 (email: lennoc@rpi.edu).