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Jeffrey Clemens
Anwita Mahajan
Joseph J. Sabia

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The Long-Run Effects of the Affordable Care Act: Evidence from a Partially Pre-Committed Research Design Over the COVID-19 Recession and Recovery
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ABSTRACT

Adjustment frictions can cause the long-run effects of social insurance reforms to differ from their short-run effects. Using pre-committed extensions of event study specifications applied previously for short-run analyses, we test the hypothesis that the Affordable Care Act's (ACA) impacts on insurance coverage and employment would increase following the substantial churn generated by the COVID-19 pandemic. Contrary to the hypothesis, the ACA's impacts remained stable through the pandemic. Long-run effects on employment and employer coverage were modest and much smaller than initially projected. Our study illustrates how partially pre-committed designs can yield informative long-run estimates while limiting specification search.

Jeffrey Clemens
University of California, San Diego
Department of Economics
and NBER
jeffclemens@ucsd.edu

Anwita Mahajan
University of California, San Diego
anmahajan@ucsd.edu

Joseph J. Sabia
San Diego State University
Department of Economics,
and Center for Health Economics & Policy Studies
and IZA & ESSPRI
jsabia@sdsu.edu

1 Introduction

The 2010 Patient Protection and Affordable Care Act (ACA) included several provisions that were designed to expand insurance coverage. These included a Dependent Coverage Mandate, expansions of eligibility for states' Medicaid programs, and a blend of regulations, mandates, and subsidies imposed in the markets for individual and small-employer purchases of insurance coverage. The vast majority of studies of the ACA's effects have focused on the first several years that the law's core provisions were in effect and, as a result, were constrained to studying the ACA's effects during the 2010s economic expansion.[1] This is an important limitation, as both the benefits and costs associated with social insurance programs may only emerge in full after labor markets have undergone a period of substantial churn. Whether the ACA's long-run effects differed from its short-run effects is thus a hypothesis in need of testing.

This study presents long-run estimates, extending through the late stages of the COVID-19 pandemic, of the ACA's effects on insurance coverage and employment. A key methodological contribution is that the analyses we execute were pre-committed during the COVID-19 pandemic's early stages. More specifically, at the pandemic's onset we precommitted to extending event study estimates that follow frameworks that are familiar from the literature on the ACA's short- and medium-run effects. By precommitting to a set of event study analyses, inclusive of pandemic-specific robustness checks, we limit the specification search and "file drawer" problems that can otherwise arise in the context of efforts to understand the long-run effects of policy reforms.

Contrary to our hypothesis, we find that the long-run effects of the ACA's Medicaid expansion and other provisions on employer coverage and employment are no more pro-

[1] The ACA has been found to have substantial impacts on insurance coverage (Akosa Antwi, Moriya, and Simon, 2013; Kaestner et al., 2017; Courtemanche et al., 2017; Frean, Gruber, and Sommers, 2017; Soni, Hendryx, and Simon, 2017), on beneficiaries' financial well-being (Brevoort, Grodzicki, and Hackmann, 2017; Hu et al., 2018), and on health care access (Ghosh, Simon, and Sommers, 2017; Courtemanche et al., 2018*a*; Wherry and Miller, 2016). Estimated effects on health outcomes have ranged from modest (Courtemanche et al., 2018*b*; Wherry and Miller, 2016) to substantial (Miller et al., 2019).

nounced than their short- and medium-run effects. Earlier analyses of the ACA's impacts on employment and on employers' insurance offerings have found those impacts to be modest (Frean, Gruber, and Sommers, 2017; Kaestner et al., 2017; Duggan, Goda, and Jackson, 2019; Moriya, Selden, and Simon, 2016; Mathur, Slavov, and Strain, 2016; Duggan, Goda, and Li, 2020). These findings are surprising in that they imply smaller effects than were projected by the Congressional Budget Office (Elmendorf, 2010, 2011; Harris and Mok, 2015; Ellis, 2015). Specifically, CBO projected that several million individuals would, on net, lose employer provided coverage (Elmendorf, 2010) and that the ACA might reduce the number of employed persons by around 800,000 (Elmendorf, 2011). This led us to hypothesize that larger effects might require a period of substantial churn, during which frictions inhibiting firms from adapting their insurance offerings in response to the ACA might be overcome. Put differently, we hypothesized that effects would be larger in the COVID-19 pandemic's wake than they were through the economic expansion of the 2010s.

Our analysis contributes to both the literature on the effects of the ACA and to a broader methodological literature. With respect to the ACA, our core finding is that, contrary to our hypothesis, the ACA's long-run effects on insurance offerings and employment are no larger than its short- to medium-run effects. This may surprise many, as there are a variety of reasons why the elasticity of employment or the generosity of an employer's benefit offerings will tend to be larger over the long-run than over the short-run (Sorkin, 2015; Aaronson et al., 2018). Our findings can be read as providing evidence that the forces that make employer insurance coverage offerings preferable to various alternatives, including those associated with the ACA's Medicaid expansions and individual-market subsidies, are even stronger than the Congressional Budget Office (CBO) and others had forecast based in part on past research on crowd-out (Cutler and Gruber, 1996; Gruber and Simon, 2008; Garthwaite, Gross, and Notowidigdo, 2014). It is also possible that the ACA's employer mandate, and its associated employer shared responsibility payments, were more impactful on employers' behavior than anticipated. The latter possibility appears consistent with our documentation of modest revenue collections from the enforcement of these penalties.

Methodologically, we contribute to a line of research that has grappled with the question of whether pre-analysis plans can be productively employed outside of experiments. We build on the approach of Clemens and Strain (2017, 2018, 2026, Forthcoming), who emphasize the potential value of "partially pre-committed" analyses. The shared premise of the Clemens and Strain project and the present project is that there may be benefits to methodological approaches that sit between the extremes of fully flexible observational studies, on the one hand, and fully pre-committed analyses, on the other hand. This premise is motivated by the observation that the development of credible identification strategies may be implausible for the purpose of developing pre-analysis plans prior to the enactment of most real world policy changes. Put succinctly, fully pre-committed analysis plans may tend to be infeasible in observational studies. At the other extreme, however, fully flexible studies are maximally exposed to p-hacking concerns. This motivates the consideration of project designs that retain some of the benefits of pre-analysis plans while maintaining feasibility.

The innovation from Clemens and Strain is to differentiate between short- and long-run analyses. For short-run analyses, full specification search may be required to develop credible research designs. For long-run analyses, by contrast, p-hacking can be constrained by pre-committing to straightforward extensions to research designs that have been validated for the purpose of estimating short-run effects. In our context, this includes pre-specifying the outcomes to be studied, the empirical specifications to be employed, the covariates to be included, and the population subsamples of interest—domains where p-hacking has been well documented in observational work (Brodeur, Cook, and Heyes, 2020). While by no means a panacea, as identification threats can be time-period specific, the potential reduction in the exposure of long-run analyses to p-hacking may be substantial. Additionally, the present study illustrates how tests for the robustness of results to alternative strategies for controlling for time-period specific, spatially varying shocks, like those associated with the pandemic, can be built into a pre-analysis plan. Given the dilemma posed by the constraints of p-hacking and feasibility, intermediate approaches of this sort merit consideration.[2] The present paper illustrates how pre-committing to analyses that span a period of substantial churn can enhance the reliability of tests for whether the short- and long-run effects of major policy changes differ meaningfully.

To restrain p-hacking and improve reproducibility, pre-analysis plans must be faithfully implemented. The extent to which pre-analysis plans are inspected during the peer review process, however, is difficult to quantify. We contribute to the evolving state of best practice by deploying a large language model to audit the fidelity of our analysis to our pre-commitment plan. In Appendix A, we present the LLM's line-by-line summary of the elements of our plan alongside its assessment of our adherence. We find the LLM's reading of our plan and assessment of our adherence to be very detailed and almost entirely accurate. Our paper thus provides an illustrative proof of concept for the use of LLM's to improve pre-analysis plan adherence (through their use by researchers) and to improve the state of the literature (through their use in the editorial process). Appendix A's discussion of our process and interpretation of the output addresses the continued need for human judgment validating the LLM's assessments of plan adherence as well as assessing the importance of deviations from or modifications to pre-analysis plans.

The remainder of this paper proceeds as follows. Section 2 describes the data we analyze. Section 3 presents the regression frameworks we estimate. Section 4 presents our results and Section 5 concludes.

2 Data

We draw heavily on Clemens, McNichols, and Sabia (2020) for our description of data sources. Clemens, McNichols, and Sabia (2020) analyzed the effects of the ACA through 2018 and pre-

[2] Put differently, it is unlikely that the epistemologically optimal approach of all projects would lie at the corner solutions of full pre-specification or full flexibility.

committed to extend analyses through 2022, so as to capture the labor market's recovery in the COVID-19 pandemic's wake. The present paper carries out the analysis to which Clemens, McNichols, and Sabia (2020) committed. Additionally, we show that conclusions that flow from the pre-committed analysis (which extends through 2022) are reinforced by incorporating data through 2024.

We examine how the ACA's key provisions relate to four measures of insurance coverage, using indicator variables for whether an individual is covered by (i) Medicaid, (ii) employer-provided insurance, (iii) other forms of private insurance, or (iv) no form of insurance (i.e., is uninsured). A question that is central to our analysis is whether the effects documented by Clemens, McNichols, and Sabia (2020) through 2018 changed meaningfully between 2019 and 2024, during and after the substantial employment disruptions of the COVID-19 pandemic. In this context, we also examine whether the ACA's provisions influenced individuals' likelihood of employment.

The primary data source for our analysis consists of public use microdata samples of the American Community Survey (ACS), a comprehensive household survey that covers a 1 percent random sample of the U.S. population. While Clemens, McNichols, and Sabia (2020) obtained ACS data from the Center for Economic and Policy Research (CEPR), our analysis uses ACS extracts from IPUMS USA, as CEPR discontinued its provision of ACS data after 2019. We note that minor differences in the methodologies these providers use to harmonize ACS microdata across years can account for minor differences in the earlier study and present study's short-run results.

We focus primarily on two population subgroups that are particularly likely to experience the effects of the ACA's Medicaid expansions. The first, following prior research (Courtemanche et al., 2017; Kaestner et al., 2017), includes individuals with at most a high school education. Our analysis frequently compares the ACA's effects for this group to those for college-educated individuals, who were less likely to be directly affected by the law's provisions. The second, first emphasized by Clemens, McNichols, and Sabia (2020), comprises individuals associated with industries with low pre-expansion (2011–2013) employer-sponsored insurance coverage; this sample represents a new focus within the ACA literature.[3] Analyses restricted to this second subsample are necessarily limited to individuals for whom industry is defined, which may raise endogenous sample selection concerns. Crucially, the ACS links individuals to industries so long as they were employed at some point over the past five years. Industries are thus coded for individuals whose labor force attachment is highly marginal, including a large number of individuals who are not employed at the time of their interview.

Across our analyses, we use several sets of controls. These include demographic characteristics, such as age, race, gender and education that we obtain directly from the ACS. We also consider controls for the annual state-level unemployment rate (obtained from the Federal Reserve Bank of St. Louis), annual per capita income (from the Federal Reserve Bank of St. Louis), and an annual mean of the state's quarterly house price index (from the Federal Housing Finance Agency) as proxies for the overall condition of a state's economy.[4]

As committed in Clemens, McNichols, and Sabia (2020), some of our specifications include controls for the differential effects on states of the COVID-19 pandemic. A first category of COVID-19 controls includes each state's initial (2020) and cumulative (through 2022) COVID-19 deaths and cases per capita. We obtain data for COVID-19 deaths from the Centers for Disease Control's (CDC) Wonder database. We obtain data for COVID-19 cases from the Hopkins Center for Systems Science and Engineering (CSSE). A second category of COVID-19 control captures the pandemic's short-run economic impacts. Using the Quarterly Census of Employment and Wages (QCEW), we measure the percent chage in each state's employment from the second quarter of 2019 to the second quarter of 2020.

[3] Appendix Table C.11 lists the industries in the lowest quartile of baseline employer-sponsored insurance coverage rates, which comprise the treated group. Appendix Table C.12 lists the industries in the top quartile of baseline coverage rates, representing workers likely to be less affected by the ACA.

[4] We note minor discrepancies between the pre-analysis plan and the current paper in describing state-level covariates. First, while we pre-specified collecting unemployment rates and per-capita income from the Bureau of Labor Statistics and the U.S. Bureau of Economic Analysis respectively, we report both here as sourced from Federal Reserve Economic Data (FRED), which compiles these series from the same agencies. Second, we precommitted to using a state's median housing price index; in the present analysis, we instead use the annual mean across quarters.

In a pre-specified set of secondary analyses, we examine whether the ACA's effects varied with the level of "churn" in the individuals' industry of employment. For these analyses, we define industry-level churn as the change in employment within the individual's industry from 2019 Q2 to 2020 Q2. Industry-level employment data for 2019 and 2020 are obtained from the QCEW. These analyses are restricted to individuals whose industry designation in the ACS maps cleanly into the QCEW.

The primary policy variation in our analysis is driven by the ACA's Medicaid expansions. We obtain data on the timing of states' Medicaid expansion from KFF (KFF, 2026). In a set of triple-difference style specifications, we follow Courtemanche et al. (2017) by using baseline (2013) uninsurance rates to capture a sub-state dimension of the ACA's potential impact. These rates are constructed at the level of each core-based statistical area (CBSA). We construct these rates at the CBSA level using the ACS. Because the ACS identifies geography at the Public Use Microdata Area (PUMA) level, we use the Missouri Census Data Center's Geocorr to map PUMAs into CBSAs before constructing CBSA-level baseline uninsurance rates.[5]

3 Methodology

As with our discussion of data sources, our description of methods closely follows Clemens, McNichols, and Sabia (2020), because we implement the pre-analysis plan outlined therein. First, we present estimates of the ACA's effects in 2021 and 2022, which is the endline of the analyses to which we precommitted, from a standard static two-way fixed effects (TWFE) difference-in-differences model described by equation (1).

[5] The 2011–2024 data span three PUMA definitions. The 2011 data use 2000 Census PUMAs, the 2012–2021 data use 2010 Census PUMAs, and the 2022–2024 data use 2020 Census PUMAs. To maximize CBSA–PUMA matches, we first map all 2020 Census PUMAs to 2010 Census PUMAs using Geocorr, which yields a consistent geography for the 2012–2024 data. We then map PUMAs to CBSAs separately for the 2012–2024 data and for the 2011 data. The algorithm assigns a PUMA to the CBSA containing the largest share of its population. Similarly, it assigns a 2020 Census PUMA to the 2010 Census PUMA containing most of its population.

$$Y_{i,s,t} = \beta \text{Medicaid Expansion}_s \times \text{Post}_t + \mathbf{State_s}\alpha_1 + \mathbf{Time_t}\alpha_2 + \mathbf{X_{i,s,t}}\phi + \varepsilon_{i,s,t}, \quad (1)$$

where i indexes individuals, s indexes states, and t indexes years. Medicaid Expansion$_s$ is set equal 1 for all states which expanded Medicaid in 2014, and equal to 0 otherwise. "Late-expanding" states are excluded from this analysis. In particular, we entirely exclude from the control group any state which implemented a Medicaid expansion between 2015 and 2022. Post$_t$ is set equal 1 for years 2021 and 2022, and equal to 0 for years 2011-2013; the sample excludes all intervening years. Our baseline specification includes state and time fixed effects (the vectors α_1, and α_2), and a vector of covariates $\mathbf{X}$ describing the state's demographic composition and economic conditions, as described in detail in Section 2. The coefficient β is thus a difference-in-differences estimate of the long-run effect of Medicaid expansions on the outcome, Y.

On a sample that now includes all years between 2011 and 2022, we present complementary dynamic estimates from the event study specification described in equation (2).

$$Y_{i,s,t} = \sum_{t \neq 2013} \beta_t \text{ Medicaid Expansion}_s \times \mathbb{1}\{t\} + \mathbf{State_s}\alpha_1 + \mathbf{Time_t}\alpha_2 + \mathbf{X}_{i,s,t}\phi + \varepsilon_{i,s,t}. \quad (2)$$

In this specification, we interact the Medicaid expansion indicator with a full set of calendar-year indicators (t), omitting the interaction $t = 2013$, which serves as the reference period. The coefficients β_t thus trace out the path of insurance coverage in expansion states relative to non-expansion states in each reference year t relative to 2013.

In estimating equation 2, we extend beyond the pre-analysis plan by including the years 2023 and 2024 in the set of post-pandemic periods.[6] While the estimates for 2023 and

[6]This extension creates a complication regarding the inclusion of the small set of states that implemented Medicaid expansions in 2023 and 2024. To preserve the treatment and control status of all states as analyzed through 2022, we drop the post-expansion observations for these specific states. In particular, South Dakota implemented Medicaid expansion in 2023, so we exclude observations from 2023 and 2024 from the control group for South Dakota. North Carolina implemented Medicaid

2024 are outside of our pre-analysis plan, they are useful for confirming that the conclusions we draw from the analyses to which we pre-committed are not overturned by the subsequent impacts of the ongoing, post-pandemic recovery. Our first estimation subsample for equations 1 and 2 is restricted to individuals with a high school diploma or less. This restriction is consistent with a large literature that identifies lower-education populations as among those most affected by the ACA, particularly by Medicaid expansions (Kaestner et al., 2017; Courtemanche et al., 2017). The second subsample comprises individuals associated with industries that, in the pre-treatment period of 2011–2013, fell in the bottom quartile of employer-sponsored insurance coverage. Workers associated with these industries were plausibly among those most poised to gain from the expanded Medicaid coverage.

Two additional specifications we estimate use variation in the ACA's impact across core-based statistical areas (CBSAs). Developed by Courtemanche et al. (2017) and used for additional analysis in Courtemanche et al. (2018*a*,*b*), the basic idea in these models is to use variation in baseline (2013) uninsured rates at the level of the CBSA to estimate the effects of the ACA's nationally implemented provisions.

$$\begin{aligned} Y_{i,a,s,t} = {} & \gamma \text{Medicaid Expansion}_s \times \text{Uninsured}_{a,s} \times \text{Post}_t \\ & + \delta \text{Medicaid Expansion}_s \times \text{Post}_t + \theta \text{Uninsured}_{a,s} \times \text{Post}_t \\ & + \mathbf{Area_{a,s}} \alpha_1 + \mathbf{Time_t} \alpha_2 + \mathbf{X}_{i,s,t} \phi + \varepsilon_{i,a,s,t}. \end{aligned} \tag{3}$$

Equation 3 estimates a static model where i indexes individuals, s indexes states, a indexes CBSAs, and t indexes years. Estimated using data from 2011-2013, and 2021-2022, Medicaid Expansion$_s$ is set equal 1 for states which enacted a Medicaid expansion in 2014, and to 0 otherwise. Post$_t$ is set equal 1 for years 2021 and 2022, and equal to 0 for years 2011-2013. Uninsured$_{a,s}$ equals the CBSA's baseline (2013) uninsurance rate. The specification includes CBSA and time fixed effects.

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expansion in December 2023; because this occurred at the very end of the year, we treat the expansion date as 2024 and exclude only that year.

Following Courtemanche et al. (2017), we interpret the coefficient θ as capturing the impact of the ACA's non-Medicaid-expansion components on outcome Y. The parameter γ reflects the incremental effect associated with Medicaid expansion in states that chose to expand. The combined or "overall" effect of the ACA, incorporating both the expansion and the law's other provisions, is given by $\gamma + \theta$. As in Courtemanche et al. (2017), we report the individual coefficients as well as linear combinations evaluated at the sample mean of $\text{Uninsured}_{a,s}$ (21.2 percent) to summarize the policy's effects at a representative baseline level of uninsurance.

We complement the static estimates of Equation 3 with the event study specification below, which provides year-by-year estimates of the effects of each component of the ACA.

$$\begin{aligned}
Y_{i,a,s,t} = & \sum_{t \neq 2013} \gamma_t \text{Uninsured}_{a,s} \times \mathbb{1}\{\text{Medicaid Expansion}\}_s \times \mathbb{1}\{\text{t}\} \\
& + \sum_{t \neq 2013} \theta_t \text{Uninsured}_{a,s} \times \mathbb{1}\{\text{t}\} \\
& + \sum_{t \neq 2013} \delta_t \mathbb{1}\{\text{Medicaid Expansion}\}_s \times \mathbb{1}\{\text{t}\} \\
& + \mathbf{Area_{a,s}} \alpha_1 + \mathbf{Time_t} \alpha_2 + \mathbf{X}_{i,s,t} \phi + \varepsilon_{i,s,t} \qquad (4)
\end{aligned}$$

As in equation 3, θ_t represents the effects of the ACA's provisions other than the Medicaid expansion, γ_t represents the additional effects of the Medicaid expansions, and $\gamma_t + \theta_t$, represents the full effects of the ACA.

The key assumption we must make for β in equations 1 and 2, and for θ and γ in equations 3 and 4 to identify the ACA's effects has a "parallel trends" flavor. For 1 and 2, we must assume that in the absence of Medicaid expansion, the evolution of all insurance outcomes would have followed the same trajectory in expansion and non-expansion states. For equations 3 and 4, we must assume that baseline rates of uninsurance are not correlated with differential trends in our outcomes of interest. While we cannot test these assumptions directly, we can conduct two sets of checks. First, we investigate robustness to the inclusion of plausibly relevant covariates. Second, we confirm that the pre-implementation coefficients in our event study specifications (equations 2 and 4) are economically and statistically indistinguishable from 0.

For formal inference on the question of whether the ACA's effects were larger over the post-pandemic long-run than over the pre-pandemic short-run, we test for the statistical significance of the difference between the 2019 (pre-pandemic) and 2022 (post-pandemic) estimates from equations 2 and 4. We note that, while our pre-analysis plan outlined separate tests for the equality for coefficients from each calendar years 2020–2022 relative to 2018 and 2019, we focus the formally presented analysis on the equality of the 2019 and 2022 coefficients for the sake of brevity.[7]

In addition to the models described above, we conduct a third pre-specified set of analyses examining whether the long-run effects of the ACA on insurance coverage vary with the degree of pandemic-induced labor-market churn in a worker's industry. We begin by estimating equations 2 and 4 separately for each industry, defined at the three-digit NAICS level. Although we precommitted to conducting analyses at both the three- and four-digit NAICS levels, the NAICS classification system underwent multiple revisions between 2012 and 2022. As a result, aggregation to the three-digit level provides the most consistent harmonization across years. We therefore omit the four-digit analysis in the present study.

For equation 2, analyses are conducted separately for each subsample of interest i.e., individuals with a high school diploma or less, and individuals associated with industries with low baseline employer coverage. For equation 4, we compute only the full effect of the ACA, evaluated at the mean baseline CBSA-level uninsurance rate. For each industry, we then calculate the difference between the 2019 coefficient, representing the pre-pandemic effect, and the 2022 coefficient, representing the post-pandemic, post-recovery effect of the ACA. For each subsample and specification, our main figures present simple scatterplots of

[7] Since 2022 is the last time period included in the pre-analysis plan and since 2019 is the last full year prior to the pandemic, the comparison of 2019 and 2022 point estimates provides the strongest test for the hypothesis that post-pandemic long-run effects differed from pre-pandemic short-run effects.

the 2019-to-2022 change in the ACA's estimated effect against the industry's labor-market churn—measured as the change in the industry's employment from 2019Q2 to 2020Q2. In the appendix, we further test the sensitivity of this relationship to incorporating the industry's average weekly wages to account for baseline differences in worker characteristics, and to weighting each observation by the precision of the original industry-specific coefficient estimates.[8] The inclusion of wages in the estimation was prespecified in our pre-analysis plan, whereas the use of precision weighting is an extension.

A final component of our analyses accounts for the ACA's Medicaid expansions in states that adopted expansion after 2014. Although outside of our pre-committed analysis, this exercise highlights the robustness of our results to the inclusion of late-expanding states. Using the same sub-samples we used to estimate equation 2, we estimate a stacked event study of the form below.

$$\begin{aligned} Y_{i,s,t,c} = \sum_{e \neq -1} \delta_e \, \text{Medicaid Expansion}_{s,c} \times \mathbb{1}\{e = t - E_c\} \\ + \mathbf{State_{s,c}}\alpha_1 + \mathbf{Time_{t,c}}\alpha_2 + \mathbf{X}_{i,s,t}\gamma + \varepsilon_{i,s,t,c}. \end{aligned} \tag{5}$$

Our baseline event study specification, described earlier in equation 2, restricts attention to states that implemented expansion in 2014. Because we restrict attention to a single "treatment cohort," this analysis is not subject to econometric concerns that arise when the event study incorporates states that expanded Medicaid in different years. That is, our restriction to the 2014 treatment states avoids bias linked to "forbidden comparisons" that arise in standard event study specifications when treatment timing is staggered (Goodman-Bacon, 2021). The map in Appendix Figure B.1 presents the timeline of states' Medicaid expansion decisions. While a large share of states—twenty-seven—expanded Medicaid in 2014, seven additional states enacted expansions between 2015 and 2019, five states adopted

[8] Specifically, in some sensitivity checks, we account for the standard error of the linear combination of the 2019 and 2022 coefficients used to construct the outcome. The line-of-best-fit is obtained by regressing their difference on industry-level churn and weighting each observation by the inverse of its squared standard error.

expansions during the pandemic (between 2020 and 2022) and two states expanded in the years that followed. The stacked event study estimator specified in equation 5 allows us to generate unbiased estimates of the effects of states' Medicaid expansions that incorporate the experience of states that expanded Medicaid after 2014.

The stacked event study design is implemented by first constructing separate datasets for each policy cohort c, defined by the year E_c in which states within the cohort adopted Medicaid expansion. Each cohort-specific dataset includes the treated states implementing expansion in year E_c, as well as control states that did not expand by the end of 2024. A distinction in estimating equation 5, relative to all other analyses, is that states expanding Medicaid after 2022 are now included in the treated groups—specifically, South Dakota in the 2023 cohort and North Carolina in the 2024 cohort. Within each dataset, time is measured relative to the expansion year, such that event time e is the difference between the calendar year t and the year of expansion E_c. These cohort-specific datasets are then stacked on top of each other, aligned by time relative to policy implementation, creating a single stacked dataset containing multiple appearances of the control states. The coefficients δ_e trace out the dynamic effects of Medicaid expansion on insurance coverage at each event time e, defined relative to cohort-specific adoption, relative to never-adopting states and normalized to the period immediately preceding expansion.

The key modification in equation 5 relative to equation 2 is the inclusion of cohort-by-state and cohort-by-time fixed effects, which account for the repeated appearances of control states at different event times. By effectively coding all treatment events as occurring simultaneously and restricting the set of counterfactuals to "never adopters," the stacked event study eliminates the negative weighting problem and provides unbiased estimates even in the presence of staggered adoption.

We conclude by briefly recapitulating which elements of our analysis implement our pre-analysis plan and which elements extend beyond it. We pre-committed to the empirical specifications in equations 1, 2, 3, and 4, including the set of covariates and their functional forms.[9] We also pre-committed to the sample of states included in the implementation of these specifications, defining the treated group as states that expanded Medicaid in 2014 and the comparison group as states that had not expanded by 2022. For equations 1 and 2, we further pre-specified the population subgroups of interest—individuals with a high school degree or less and those associated with industries in the lowest quartile of pre-ACA employer-sponsored coverage. Any deviations from the pre-analysis plan are disclosed throughout and are limited to necessary adjustments driven by unforeseen changes in the data sources, or modifications aimed at presenting, in a more concise form, analyses that were originally specified in greater detail.

We extend beyond our pre-analysis plan along two primary dimensions. First, we incorporate data from 2023 and 2024 into the event study estimates to shed light on whether conclusions drawn based on data through 2022, as pre-specified, might need to be updated. Second, we augment the analysis with a stacked event study specification, as in equation 5, to align with emerging best practices for handling settings in which policy changes are implemented by various states at different points in time. Any remaining extensions involve straightforward robustness checks and are disclosed where described. We reference readers to Appendix Appendix A for a presentation and discussion of an LLM audit of the present paper's adherence to the analyses precommitted in (Clemens, McNichols, and Sabia, 2020)

4 Results

We turn in this section to our empirical findings. We begin by presenting time trends in insurance coverage (Section 4.1). We then examine the ACA's effects on each insurance outcome, as well as employment, using our full set of empirical specifications (Section 4.2). Next, we contextualize the magnitude of the effect on employer-sponsored coverage (Section 4.3), and conclude with an exploration of heterogeneity across industries (Section 4.4).

[9]Specifically, state-level macroeconomic controls enter as state-by-year varying covariates, while COVID-19 controls are measured at two distinct time points—2020 and 2022—and interacted with post-COVID period indicators.

4.1 Trends in Insurance Coverage

A key provision of the Patient Protection and Affordable Care Act (ACA) was the expansion of eligibility for states' Medicaid programs. Several states added new population groups to their Medicaid programs, including adults without dependent children and households with higher incomes, typically up to 138% of the federal poverty level. Assessing the impact of this provision on insurance outcomes is a primary focus of our analysis.

In Figure 1, we present the annual evolution of mean insurance coverage rates separately for (i) 2014 expansion states, and (ii) control states that had not expanded Medicaid by 2022. Panel I shows insurance coverage among individuals with at most a high school diploma. Panel II presents outcomes for individuals associated with industries that fell in the lowest quartile of employer-sponsored coverage in 2011–2013. A first key pattern that emerges in both Panels I and II is the sharp divergence in Medicaid coverage (panel (a)) and uninsurance (panel (d)) rates between expansion and control states after 2014. Expansion states experienced increases in Medicaid coverage and declines in uninsurance relative to non-expansion states after 2014. The divergence in employer-sponsored and individual coverage is less apparent in these descriptive graphs. This is unsurprising, as other provisions of the ACA more directly targeted these outcomes. That said, the time series suggests that employer-sponsored coverage in expansion states experienced smaller increases after 2014 relative to control states, while individual coverage appears to have risen more sharply in the control states than in the expansion states over the same period.

We contrast the evolution of insurance coverage among individuals most likely to be affected by the Medicaid expansions, and the ACA more broadly, with that among individuals less likely to be affected. Series for the latter groups are shown in Appendix Figure B.2. Panel I includes individuals with at least a college degree, while panel II includes individuals associated with industries in the highest quartile of baseline employer-sponsored coverage. While the data indicate modestly larger increases in Medicaid coverage rates (panel (a)) in expansion states relative to control states, the relative increase is considerably smaller in magnitude than in the main sample.

4.2 The ACA's Long-Run Effects on Coverage and Employment

We now present our estimates of the ACA's long-run effects on insurance coverage and employment. Estimates from equation 1, capturing the *post-pandemic* effects of the 2014 Medicaid expansions, are reported in Tables 1 and 2. Estimates from equation 2, which trace the dynamic effects of the 2014 Medicaid expansions, are shown in Figures 2, 3, and 4. Estimates from the stacked event study, incorporating all Medicaid expansion states, are presented in Figures 5 and 6. Table 3 reports estimates from equation 3, capturing the *post-pandemic* effects of the ACA's full set of provisions. The corresponding dynamic estimates, based on equation 4, are presented in Figures 7, 8, 9 and 10.

4.2.1 Medicaid Coverage

Our estimated effects of the ACA's key provisions on Medicaid coverage are highly consistent across specifications. Table 1, which presents post-pandemic effects of the ACA's Medicaid expansions using our difference-in-differences specification, shows that Medicaid expansion increased Medicaid coverage relative to the pre-expansion period by roughly 9 percentage points within the sample of individuals with at most a high school diploma. The various columns of Table 1 show that this estimate is impacted little as we move from the sparse specification of column 1 to the most controlled specification of column 5. Table 2 shows that we obtain very similar point estimates when analyzing the sample of individuals associated with industries with low pre-treatment employer coverage. The dynamic event study estimates in Figures 2 and 3 reveal further that this roughly 9 percentage point increase in Medicaid coverage has been stable following a 2-year phase in after implementation.[10] Stepping outside of

[10]We compare the effects of the Medicaid expansion in our subsamples of interest in Figures 2 and 3 to those in plausibly less affected demographic subgroups shown in Appendix Figures B.3 (individuals with at least a college degree) and B.4 (individuals associated with industries in the highest quartile of pre-treatment insurance coverage). Within these less affected

our pre-analysis plan, we see that using equation 5's stacked event study estimator to enable the inclusion of states that expanded Medicaid later than 2014 has modest impacts on the results. Additionally, the stability of the results holds in the 2023 and 2024 ACS data, which extend beyond the scope of our pre-analysis plan. Finally, we see similar implications of the ACA's full set of provisions for Medicaid coverage in the triple-difference style estimates of equations 3 and 4, as presented in Table 3 and Figure 7. The dynamic triple-difference estimates exhibit the same stability as the dynamic difference-in-differences estimates.

For formal inference on the question of whether the ACA's effects were larger over post-pandemic long-run than over the pre-pandemic short-run, we test for the statistical significance of the difference between the 2019 (pre-pandemic) and 2022 (post-pandemic) estimates of equations 2 and 4. These differences are presented in Appendix Tables C.1-C.5. Panel I in Tables C.1, C.2 and C.3 confirms that, across the specifications from our pre-analysis plan, the Medicaid expansion's estinated impacts on Medicaid coverage through 2022 do not differ significantly its estimated impact through 2019.[11]

4.2.2 Employment and Employer Coverage

We now turn to employment and employer coverage, which are the outcomes that connect most directly to our hypothesis regarding the ACA's short- and long-run effects. We note at the outset that our estimates for these outcomes exhibit greater variability across specifications than did our estimates for Medicaid coverage. That said, as laid out in the discussion below, the estimates speak consistently to the question of whether ACA-driven declines in either employment or employer coverage became larger over the course of the pandemic.

Our estimates of equation 1, as presented in panel II of Tables 1 and 2, show that the ACA's Medicaid expansion had a modest long-run effect on employer coverage. In both

samples, the ACA increased Medicaid coverage, but the magnitudes are substantially smaller than in the more affected groups.

[11] We document small increases in Medicaid coverage from 2019 to 2022 attributable to non-expansion components of the ACA, as illustrated in panel I of Figure C.4.

tables, the estimates range from modestly negative (e.g., -1.8 percentage points in column 1 of Table 1 and -2.0 percentage points in column 1 of Table 2) to economically negligible (e.g., -0.4 percentage points in column 5 of Table 1 and 0.4 percentage points in column 5 of Table 2). The more negative estimates are associated with sparser specifications.

Our dynamic difference-in-differences estimates in panel (b) of Figures 2 and 3 indicate that declines in employer-sponsored coverage initially emerged over the relatively short-run. While the relationship between estimated short- and long-run effects varies across specifications, the incremental declines are thus uniformly smaller than the long-run effects themselves. The more saturated specifications suggest a recovery, rather than further decline, in employer provided coverage while the sparser specifications suggest modest additional declines in employer coverage. This is formally analyzed in Appendix Tables C.1 and C.2, which presents point estimates and standard errors on the changes in our estimated coefficients from 2019 to 2022. In our analysis of the low-education sample (Table C.1), the estimated changes from 2019 to 2022 range from -1.1 to 0.4 percentage points, while in our analysis of the low-baseline coverage sample (Table C.2) the estimated changes range from -0.7 to 1.4 percentage points.

We next turn to the analyses that extend beyond our pre-analysis plan by incorporating data from 2023 and 2024, and by implementing the stacked event study estimator of equation 5 to enable the inclusion of states the implemented Medicaid expansions later than 2014. As could be seen previously in Figures 2 and 3, estimates for 2023 and 2024 are similar to the reported estimates for 2022. Panel (b) of Figures 5 and 6 shows that incorporating the post-2014 Medicaid expansion states into the analysis by employing a stacked event study estimator also yields similar conclusions. It is again the case that the more saturated specifications are suggestive of modest recoveries (as opposed to additional declines) of employer coverage in states that implemented Medicaid expansions.[12]

[12] Additionally, the estimates show that this is true whether we restrict treated states to pre-pandemic Medicaid expanders or not.

Employer coverage is an outcome for which the inclusion of state-by-time varying economic controls may be especially relevant, since employer coverage is linked to employment. The inclusion of such controls is fraught, however, because they risk being endogenous to the policy change; they may also risk approaching equivalence to controlling for the outcome itself. Here it is relevant to reiterate that the most saturated specifications from our pre-analysis plan include a statewide housing price index, unemployment rate, and per capita income as covariates. To explore whether the estimates from these specifications are driven by potentially problematic dimensions of these particular controls, we move outside of our pre-analysis plan to test the robustness of these sets of estimates to alternatives. As shown in Appendix Figures B.5 (low-education sample) and B.6 (low-baseline-coverage sample), we obtain very similar results when comparing the most saturated event study specifications to estimates in which the economic controls consist solely of the housing price index—arguably less directly tied to employment outcomes—or to the housing price index and the state unemployment rate specifically for individuals with at least some college education.[13] With even the most parsimonious inclusion of a covariate related to state macroeconomic conditions, the evidence becomes suggestive that employer coverage may have rebounded in Medicaid expansion states over the course of the pandemic.

Our estimates of the ACA's effects on employment follow a similar pattern as our estimates of the ACA's effects on employer coverage. Overall, the estimates yield evidence that the effects of the ACA Medicaid expansion on employment changed little over the course of the pandemic, with relatively sparse specifications suggesting additional declines while relatively saturated specifications imply modest recoveries. This can be seen in Figure 4 as well as in Appendix Figures B.5 and B.6. Panel V of Appendix Tables C.1 and C.2 present the ranges of estimates for the changes in coefficients between 2019 and 2022.

We now move to the triple-difference estimates of equations 3 and 4. For these estimates, the implied long-run, post-pandemic effect of the Medicaid expansion on employment is a

[13] Across our main estimates in Figures 5 and 6, results appear relatively insensitive to the inclusion of COVID-19 controls.

null (column 5 of Table 3) and the estimated effect on employer coverage is positive (column 2 of Table 3). Panel II of Table C.3 confirms that the dynamic estimates of the implied effects of the expansion, as shown in Figure 7's panel (b), rule out long-run reductions in employer coverage. These triple-difference estimates are robust to the sets of covariates we include, which bolsters the strength of our overall conclusions regarding the relative magnitudes of the ACA's short- and long-run effects on employment and employer coverage.

The ACA's non-expansion components, which could be expected to impact employer coverage through the employer shared responsibility provisions, have no significant post-pandemic effects on either employment or employer coverage (Table 3). As with the difference-in-difference style estimates, dynamics reveal some nuance across specifications—in Figure 8's panel (b) and Table C.4's panel II, the evidence is suggestive of long-run reductions in employer coverage. Overall, however, the full set of the ACA's provisions yields long-run estimates of effects on employment (panel (c) of Figure 10) and employer coverage (panel (b) of Figure 9) that, if anything, point to modest long-run increases, with no statistically significant differences between short- and long-run estimates (panels II and V of Table C.5).

To summarize, our estimates of the Medicaid expansion and the overall ACA's effects on employment and employer coverage exhibit some sensitivity to specification choices. However, the evidence points consistently against the hypothesis that the ACA's long-run effects on either employment or employer coverage were substantially more negative than its short-run effects. We return to this issue with additional emphasis on the magnitudes of both our point estimates and the bounds they can rule out in Section 4.3.

4.2.3 Effects on Individual Coverage and Uninsurance

We round out the discussion of our results by presenting the estimated effects on individual coverage and on an individual's status as uninsured. As can be seen in Tables 1 and 2, as well as in Figures 2, 3, 5 and 6, our estimates for both of these outcomes exhibit robustness to the sets of covariates included. Interestingly, these sets of difference-in-differences estimates reveal that the effect of Medicaid expansions on individual coverage became systematically more negative over time, which, contrary to the hypothesis specified in Clemens, McNichols, and Sabia (2020), translates into an erosion of the effects of Medicaid expansions on the fraction of individuals who lack insurance. The estimates in Tables C.1 and C.2 indicate that the effect of Medicaid expansions on individual coverage became 1 to 3 percentage points more negative between 2019 and 2022, with a corresponding though slightly more modest increase in the fraction uninsured.

As reported in Figure 7, our triple difference estimates of the incremental effects of Medicaid expansion tell a similar story. In contrast, the results in Figure 8 estimate that the ACA's other provision are associated an ongoing rise individual coverage. Taken together, the estimated effects of the ACA's full set of provisions, as shown in Figure 9, imply relatively stable medium-to-long-run gains in individual coverage and declines in the fraction uninsured. Tables C.3, C.4, and C.5, which present our formal statistical comparisons of estimated effects through 2022 relative to 2019, reveal that on net the full effects of the ACA on uninsurance are estimated to have become larger over the long run. This reflects, as discussed above, that the estimated effects of provisions other than Medicaid expansion were sufficiently large to offset an estimated erosion of the effect of the Medicaid expansions per se. The underlying effects on individual coverage appear central to this story.

4.3 Discussion of the Magnitude of Employer-Coverage Effects

Section 4.2 presented findings to the effect that the ACA's effects on employment and on employer-provided insurance coverage were similar in magnitude when comparing the short-run to the post-pandemic long-run. Our evidence that ACA-induced crowd-out of employer coverage remained modest over the long-run is surprising when set against expectations based on the early projections of the Congressional Budget Office (CBO). Specifically, the CBO estimated that the ACA would reduce employer coverage by roughly 5 million individuals by 2019 (Harvey, Hearne, and Anders, 2012).[14] Short-run empirical studies tended to find little erosion in employer offerings or coverage attributable to either the Medicaid expansion or other ACA provisions, such as individual-market premium subsidies (Frean, Gruber, and Sommers, 2017; Kaestner et al., 2017; Duggan, Goda, and Jackson, 2019; Moriya, Selden, and Simon, 2016; Mathur, Slavov, and Strain, 2016; Duggan, Goda, and Li, 2020). We add to this literature by both replicating existing short-run estimates and providing novel evidence that the ACA's effects on employer coverage did not intensify over time. In the remainder of this section, we further contextualize these results and elaborate further on the magnitudes of our point estimates as well as the bounds our confidence intervals are able to rule out. While not pre-specified, we include this additional discussion to aid in interpreting our findings and to subject our results to closer scrutiny.

4.3.1 Contextualizing Magnitudes Relative to Early Forecasts

To contextualize our results, we turn to estimates of employer-mandate penalty collections that we obtain from annual reports of the Office of Management and Budget of the U.S Government (Office of Management and Budget, N.d.). We restrict our attention to line items in each fiscal year's budget that report receipts to the Department of the Treasury under "Penalties on Employers Who Do Not Offer Health Coverage or Delay Eligibility for New Employees." We look to these line items beginning in FY 2017, when key features of employer penalty enforcement were fully implemented after earlier delays (Council of Economic Advisers, 2021), and ending in FY 2022, which closely corresponds to the end of our sample period. Communications with Treasury staff confirm that the appropriate interpretation of the provided numbers is somewhat opaque. That said, the penalty collections reported for a given fiscal year in the subsequent fiscal year's budget appears to approximate an initial estimate of the revenues collected in that year. By contrast, the figures reported for a given year in the preceding fiscal year's budget appear to reflect forecasts. Dividing these annual

[14] See Table 3 in Harvey, Hearne, and Anders (2012).

collection amounts by $3,000—a rough estimate of the annual penalty per employee (Internal Revenue Service, N.d.)[15]—yields a back-of-the-envelope estimate of the number of employees for whom required coverage may not have been offered, in the former case, or was forecast not to be offered, in the latter case. We present these estimates in Appendix Figure B.7.

Using the backward-looking collection figures, our estimates of the number of affected workers (blue line in Appendix Figure B.7—0.48 million in 2017, 0.87 million in 2018, 1.28 million in 2019, 0.25 million in 2020, 0.05 million in 2021, 0.06 million in 2022) are lower than the CBO's (Harvey, Hearne, and Anders, 2012) estimates for all years (purple line in Appendix Figure B.7—5 million in 2017, 5 million in 2018, 5 million in 2019, 4 million in 2020, 3 million in 2021, 3 million in 2022). Notably, however, the forecast figures from the prior year's budget produces estimates that are similar in magnitude to CBO's forecasts for the pre-pandemic years (green line in Appendix Figure B.7—4.33 million in 2017, 5.03 million in 2018, 2.67 million in 2019, 1.86 million in 2020, 1.57 million in 2021, 0.57 million in 2022). The numbers suggest that through 2018 the Treasury's forecasts were aligned with the earlier forecast by CBO, but that the realized collections (and associated reductions in employer coverage) were smaller than both the Treasury and CBO forecasts.

Population estimates of ACA-induced losses in employer coverage, based on our difference-in-differences analysis, lead to the same conclusion. To arrive at this assessment, we extrapolate the annual estimates from our fully saturated specification in panel (b) of Figure 2 to the population of individuals who live in Medicaid expansion states and who have at most a high school degree. The resulting estimate suggests that the count of employees losing employer coverage was an order of magnitude lower than the CBO's estimates (gray line in Appendix Figure B.7—0.65 million in 2017, 0.59 million in 2018, 0.31 million in 2019, 0.25 million in 2020, 0.15 million in 2021, 0.16 million in 2022). Using the lower bound of the confidence interval from our least saturated specification, which implies the largest possible reductions

[15] The penalty amounts for each calendar year between 2017 and 2022 can be found in the answer to Question 55 in Internal Revenue Service (N.d.).

in employer coverage, we can rule out that the number of affected employees (1.07 million in 2017, 1.04 million in 2018, 0.86 million in 2019, 0.99 million in 2020, 1.26 million in 2021, and 1.34 million in 2022) exceeded a moderate fraction of the CBO's estimates. While our estimates pertain to a narrower population than that affected by all ACA provisions, they are nonetheless consistent with the conclusion that losses in employer coverage were smaller than initially anticipated. Recall that our triple-difference estimates imply even smaller reductions, and possibly increases, in employer coverage, and thus reinforce this conclusion.

4.3.2 Further Assessment and Interpretation of Magnitudes

Next, we translate our employer coverage estimates into implied rates at which Medicaid coverage crowded out employer-provided coverage, as presented in Table 4. Panel I reports estimates that enter as inputs into our crowd-out calculations. Columns 1-4 present the information required to infer long-run crowd-out estimates, while columns 5-8 present the information required to infer changes in crowd-out from the short- to the long-run. Moving across columns, we present results from the difference-in-differences event study (DD ES) within the sample of individuals with less than a high school education in columns 1 and 5, the DD ES within the sample of individuals associated with industries with low baseline employer coverage in columns 2 and 6, the difference-in-difference-in-differences event study (DDD ES) of the implied (evaluated at mean pre-treatment uninsured rate) effects of the Medicaid expansion in columns 3 and 7, and the DDD ES of the implied full effects of the ACA in columns 4 and 8.

We now turn to a description of row entries. In row (a), we report the most negative—across specifications with varying controls—2022 employer-coverage coefficients. We note that the coefficients in Table 4's row (a) are the most negative 2022 employer-coverage coefficients from panel (b) of Figures 2, 3, 7 and 9. In Table 4's row (b), we report the 2022 Medicaid coverage coefficient from the same specification used for the employer coverage coefficient in row (a). These can be found in panel (a) of Figures 2, 3, 7 and 9. In row (c), we report the lower bound of the confidence interval for the employer coverage coefficient in row (a). In rows (d)–(f), we report the averages across specifications of the 2022 employer-coverage coefficients, the 2022 Medicaid-coverage coefficients, and the lower bounds of the confidence intervals for the 2022 employer-coverage coefficients, respectively. The implied crowd-out rates derived from rows (a)–(f) and columns (1)–(4) capture the long-run (2022) ACA-induced crowd-out of employer-sponsored insurance by Medicaid. We compute crowd-out rates by dividing each long-run employer-coverage estimate by its corresponding Medicaid-coverage estimate, and report them in columns (1)–(4) of Panel II.

Pooling across DD estimates, the average implied crowd-out rate based across both subsamples and specifications is 13.9%, with an upper bound of 17.7%.[16] In contrast, the maximum implied crowd-out rate based on specification-level extremes is 22.6%, with an upper bound of 40.4%.[17] Pooling across DDD specifications, the long-run estimates uniformly imply crowding-in rather than crowding-out of employer coverage. Pooling across both the DD and DDD style long-run estimates, our analysis suggests that the ACA induced at most 40.4% crowd-out and potentially a modest to moderate degree of crowd-in. The crowd-in effects we estimate are consistent with prior analyses showing that Medicaid expansions can raise private coverage by reducing adverse selection pressures in community-rated markets, as when high-cost individuals are shifted out of the private risk pool (Clemens, 2015).

We next turn to our estimates of the change in ACA-induced crowd-out rates from the short-run to the long-run. The structure of columns (5)–(8) follows directly from that in columns (1)–(4), with the only difference being that the employer-coverage inputs now correspond to the 2019–2022 change rather than the 2022 level. The corresponding Medicaid-

[16] Here, we report the arithmetic average of our crowd-out estimates in rows (d)/(e) and (f)/(e) for the DD specifications (columns 1 and 2) in Table 4.

[17] Here, we report the largest-in-magnitude crowd-out estimate across in rows (a)/(b) and (c)/(d) for the DD specifications (columns 1 and 2) in Table 4.

coverage inputs continue to represent the 2022 estimate. Together, these inputs are combined to produce implied changes in crowd-out rates.[18]

Among DD specifications, the average implied increase in the crowd-out rate between 2019 and 2022 is 1.8%, and we rule out a long-run increase above 12.7%. Taking instead the most negative individual estimates, the implied increase in the crowd-out rate reaches a maximum point estimate of 11.9%, with a confidence-interval upper bound of 19.9%. Averaging across DDD specifications, the point estimates systematically imply a shift towards crowding-in rather than additional crowding-out of employer coverage, and allow us to reject a crowd-out increase larger than 1.3%. Even the lower-bound of the confidence interval on the smallest individual estimate allows us to rule out a change of more than 13.9%. Pooling across both the DD and DDD style estimates, our analysis suggests that between 2019 and 2022, the ACA induced at most a crowd-out rate increase of 19.9%.

Appendix Table C.6 presents a similar summary of estimated long-run employment effects and differences between short- and long-run employment effects. The summary reveals that averages across specifications tend towards 0, and are sometimes modestly economically positive and sometimes modestly economically negative. Long-run employment effects of the ACA's key provisions thus do not appear larger than short-run effects. That said, for a number of specifications the lower bound of the estimated effects on employment extends below 1 (and in rarer cases 2) percentage point. The bounds are thus insufficiently precise to rule out magnitudes that would be consistent with CBO's early forecasts.

4.4 Heterogeneity Across Industries

Finally, we conclude the analyses from our pre-analysis plan by considering the possibility that the ACA's long-run effects on insurance coverage may have been heterogeneous across industries, given that some sectors, such as tourism and food service, experienced far greater employment churn than others. Figure 11 presents the association between employment

[18] Employer-coverage inputs in rows (a)-(c) of columns 5-8 can be found in Appendix Tables C.1, C.2, C.3 and C.5.

"churn" in industries in which individuals with at most a high school education worked, measured as the percentage change in total employment between 2019Q2 and 2020Q2 in the industry, and the change in the effect of the Medicaid expansion on insurance coverage from 2019 to 2022, obtained using estimates from equation 2. We find little relationship between industry churn and effects of the ACA on coverage, across insurance types.[19] These patterns also hold in our analysis within the sample of individuals who are associated with industries in the bottom quartile of baseline employer coverage—Figure 12 suggests no evidence of a link between churn and and the change in insurance coverage.[20] Finally, estimates shown in Appendix Figure B.8 show no correlation between employment churn and changes in the full effect of the ACA on insurance coverage from 2019 to 2022. This conclusion is robust to whether we omit covariates for states' COVID-19 exposure from the equations used to estimate the 2019 and 2022 coefficients (Appendix Figures B.9, B.10 and B.11).

5 Conclusion

This paper investigates whether there were empirically appreciable differences between the short- and long-run impacts of the Affordable Care Act's (ACA) key provisions. In particular, we hypothesized that the ACA's effects on the provision of employer provided health insurance would grow in magnitude over time. Similarly, we hypothesized that the ACA's effects on employment would grow larger over time. Our core finding is that the data do not support these hypotheses. Across a broad range of specifications, the ACA's short- and long-run effects on insurance coverage and employment look remarkably similar. In some

[19] Appendix Table C.7 shows no apparent correlation between industry churn and insurance coverage, except in the case of the change in Medicaid coverage which negative and significant in a specification that controls for the industry's average weekly wage in June 2020.

[20] Appendix Table C.8 shows that accounting for the industry's average weekly wage in June 2019 strengthens the relationship between churn and both the change in employer coverage as well as the change in uninsurance, suggesting that workers in high-churn industries may have been better insulated against the loss of insurance as well as employer coverage during the pandemic in Medicaid expansion states.

specifications, employer coverage and employment appear, if anything, to begin to rebound in expansion relative to non-expansion states between the short- and the long-run.

The economic implications of our findings merit additional discussion. Our hypotheses were motivated by a blend of economic theory and initial forecasts of the ACA's effects. Forecasts foresaw larger declines in both employment and employer provided health insurance than had emerged in the data used to study the ACA's short-run effects. We entered this study with the hypothesis that the short-run divergence between forecasts and data could plausibly be explained by canonical factors that can lead long-run impacts to exceed short-run impacts (Hurst et al., Forthcoming). Existing firms are constrained in their short-run adjustments along many margins, for example, while new firms make all choices afresh. The minimum wage literature has emphasized this point in the context of firms' choices regarding their mix of capital, high-skilled labor and low-skilled labor (Sorkin, 2015; Aaronson et al., 2018). In our context, the notion that benefit packages may be difficult for continuing firms to overhaul led us to hypothesize that the dramatic churn associated with the COVID-19 pandemic might lead longer-run adjustments of firms' health insurance benefits (in response to key ACA provisions) to manifest themselves. The fact that such effects did not grow over the long-run relative to the short-run provides evidence that frictions to the adjustment of firms' benefit packages were not a key factor in shaping the labor market's responses to the ACA's major provisions.

Methodologically, our analysis contributes to an ongoing debate over the prospects for using pre-analysis plans outside of experimental settings, and provides an illustration of their implementation in such contexts. We note that "partially pre-committed" analysis plans of the sort we implement here have the potential to conserve some of the benefits of pre-analysis plans (e.g., reductions in specification search) while relieving the impracticability of developing a fully pre-committed analysis plan prior to a major piece of legislation's implementation. Some of the key methodological trade-offs are discussed more fully by Clemens and Strain (Forthcoming), who take a similar approach in a study that contrasts the shortand long-run effects of relatively large vs. small minimum wage increases. A novel point we emphasize here, given the null finding with respect to our motivating hypothesis, is that the public release of a pre-analysis plan through a high profile working paper series creates a reputational incentive to follow through on the release of all analyses. This mechanism can be valuable for overcoming the classic "file drawer" source of publication bias, whereby null findings might not otherwise see the light of day.

Finally, we illustrate the utility of Large Language Models (LLMs) for auditing adherence to pre-analysis plans. While human judgment remains central to the editorial and peer review processes, we note that tasks like the inspection of authors' adherence to pre-analysis plans can be very labor intensive. The time intensity of such tasks may regularly exceed what can reasonably be expected of peer reviewers, in particular when that work is unpaid.[21] We illustrate that LLMs are already capable of generating detailed and, in our reading, generally accurate audits of this sort. We view the adoption of LLM adherence audits as having the potential to significantly improve the scientific value of pre-analysis plans going forward, in particular if utilized by both authors and editorial systems.

[21] The appendix to Nyhan et al. (2023) provides a useful illustration of this point. The paper's "Supplementary information" appendix spans 384 pages, including a detailed 8 page description of deviations from the paper's pre-analysis plan, accompanied by justifications. It is difficult to imagine unpaid peer reviewers exerting the effort required to assess whether an author teams' self-assessment of this sort is indeed exhaustive.

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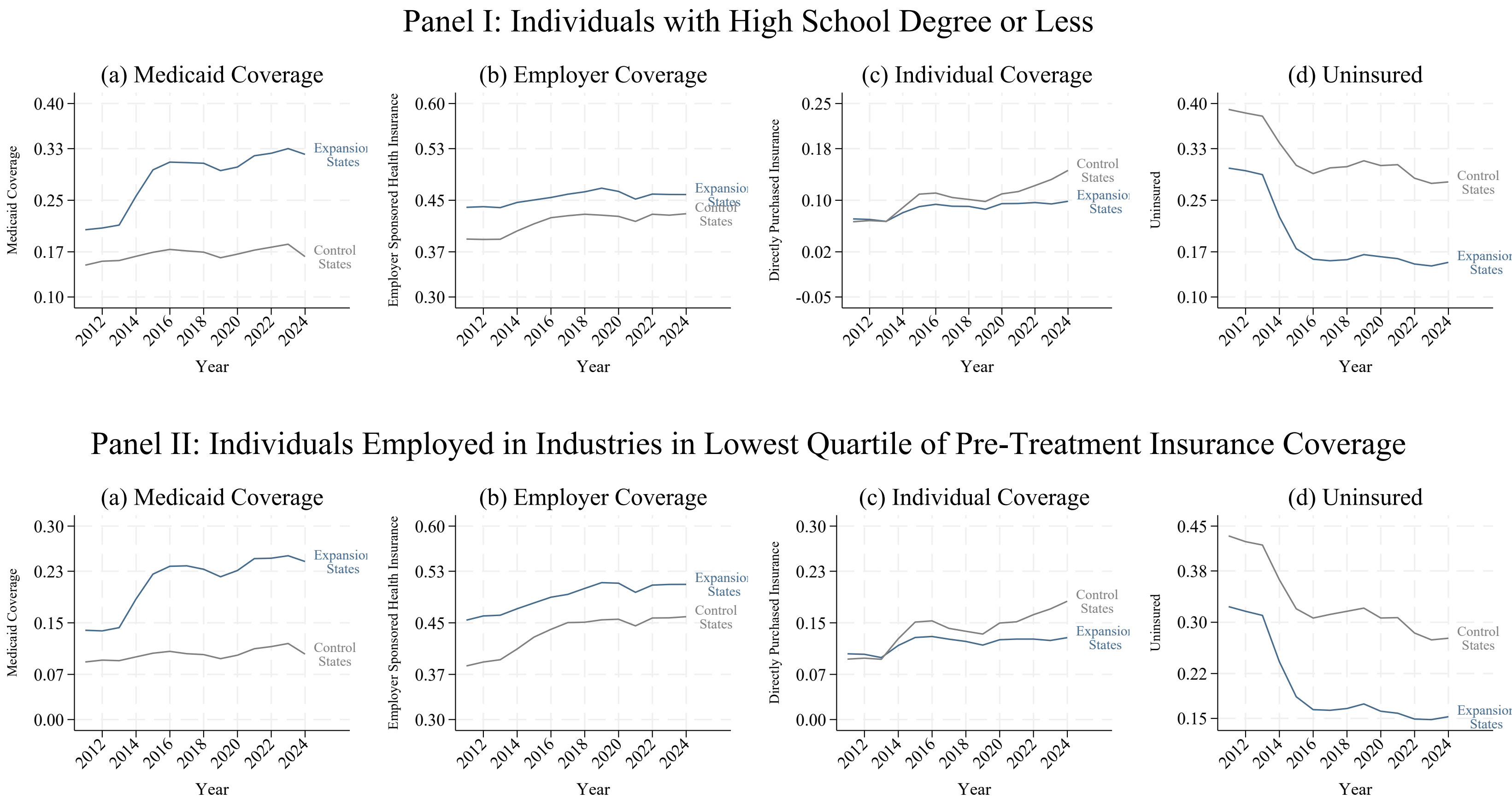
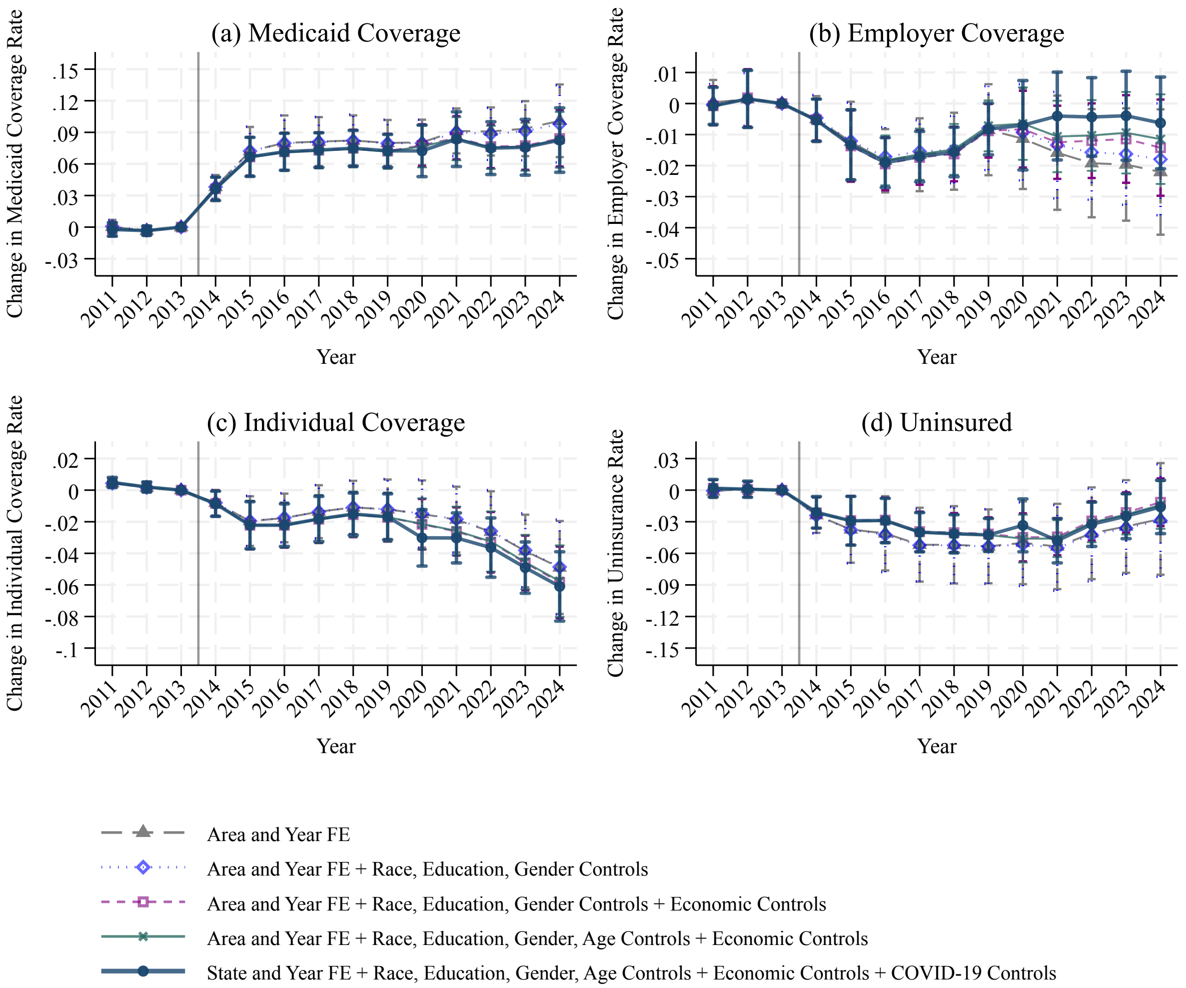
Figures

Figure 1: Expansion vs. Non-Expansion States, 2011-2024



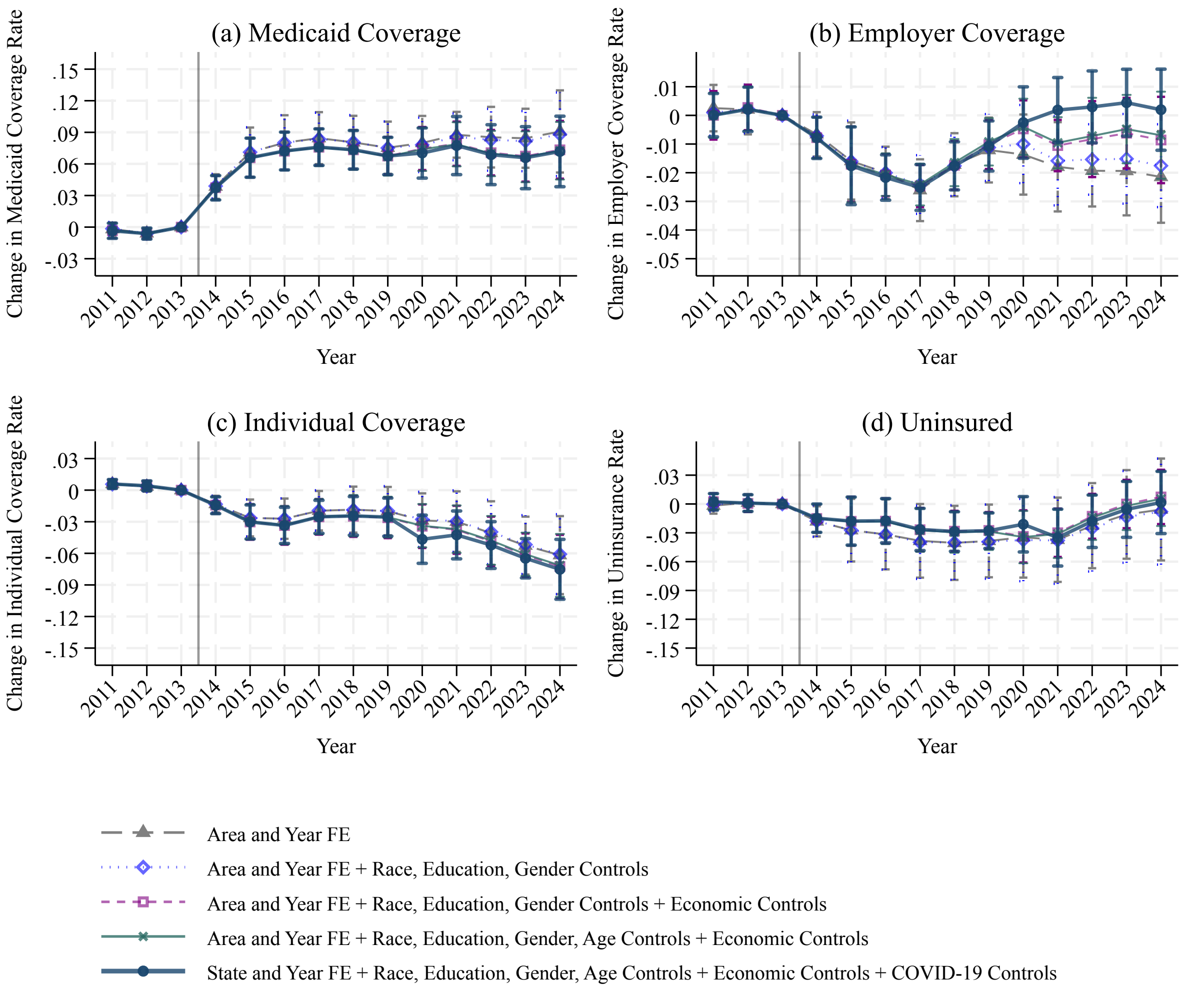
Notes: This figure illustrates the coverage rates for different types of insurance among individuals with a high school degree or less (panel I), and among individuals associated with industries in the lowest quartile of employer coverage rates between 2011 and 2013 (panel II). The blue line is the coverage rate among expansion states, while the gray line is the coverage rate among control states. Expansion states are those which expanded Medicaid in 2014. Control states are those that did not expand Medicaid by the end of 2022. The main data source is the American Community Survey.

Figure 2: Pre-Committed Event Study Design, 2014 Medicaid Expansion States and Health Insurance Coverage, Individuals with No More than a Completed High School Education



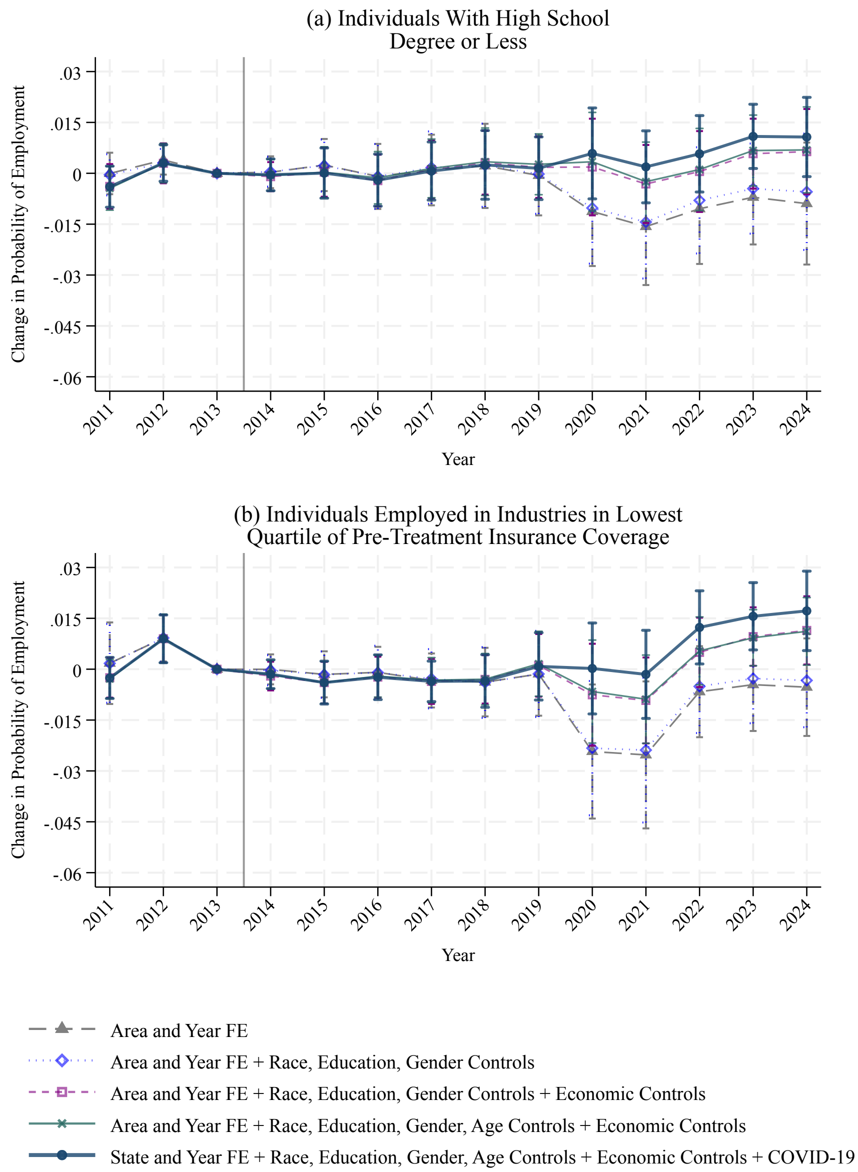
Notes: This figure presents weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The sample includes individuals with no more than a high school degree. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 3: Pre-Committed Event Study Design, 2014 Medicaid Expansion States and Health Insurance Coverage, Individuals in Low Pre-Treatment Insurance Coverage Industries



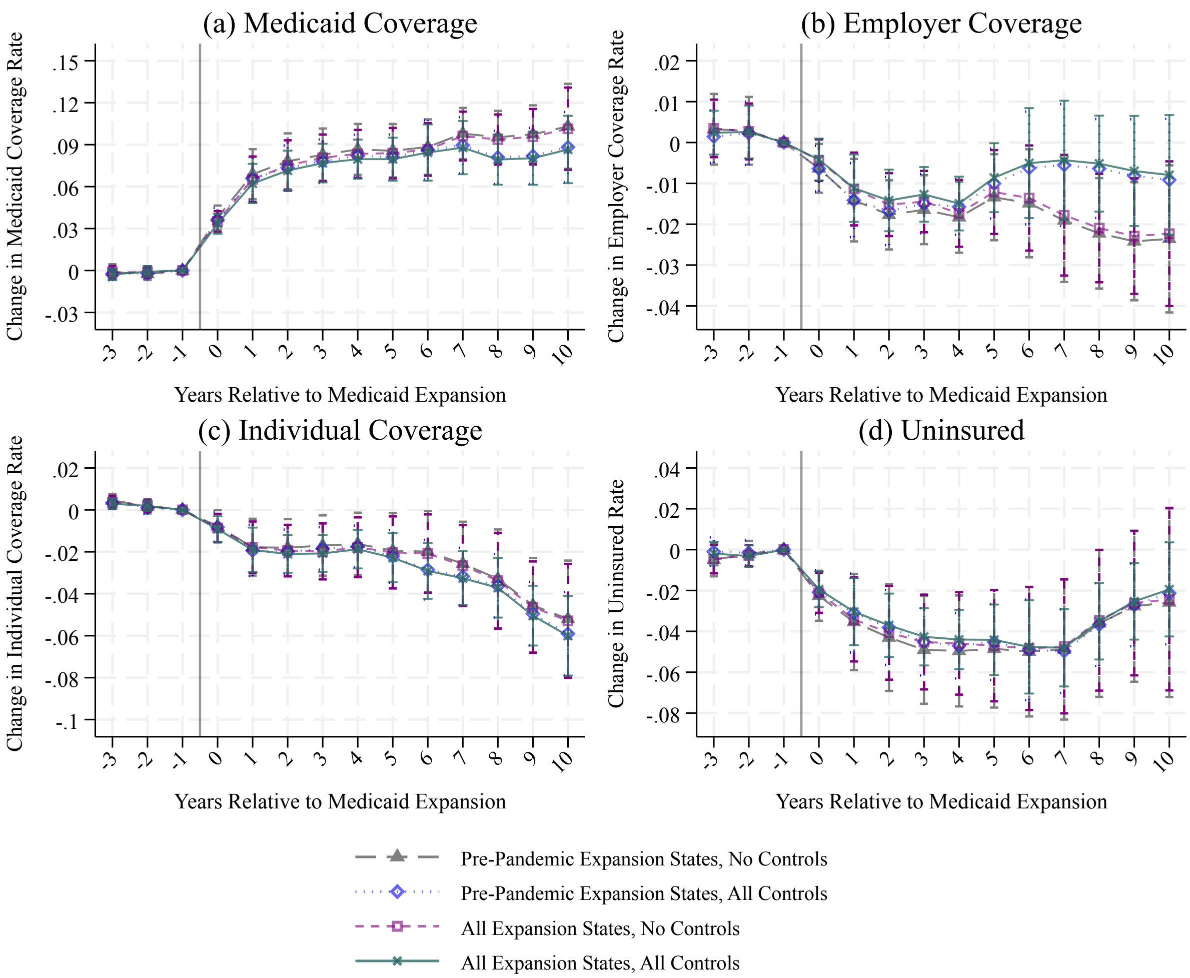
Notes: This figure presents weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The sample includes individuals associated with industries in the lowest quartile of mean employer-sponsored health insurance between 2011 and 2013. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 4: Pre-Committed Event Study Design, 2014 Medicaid Expansion States and Probability of Employment



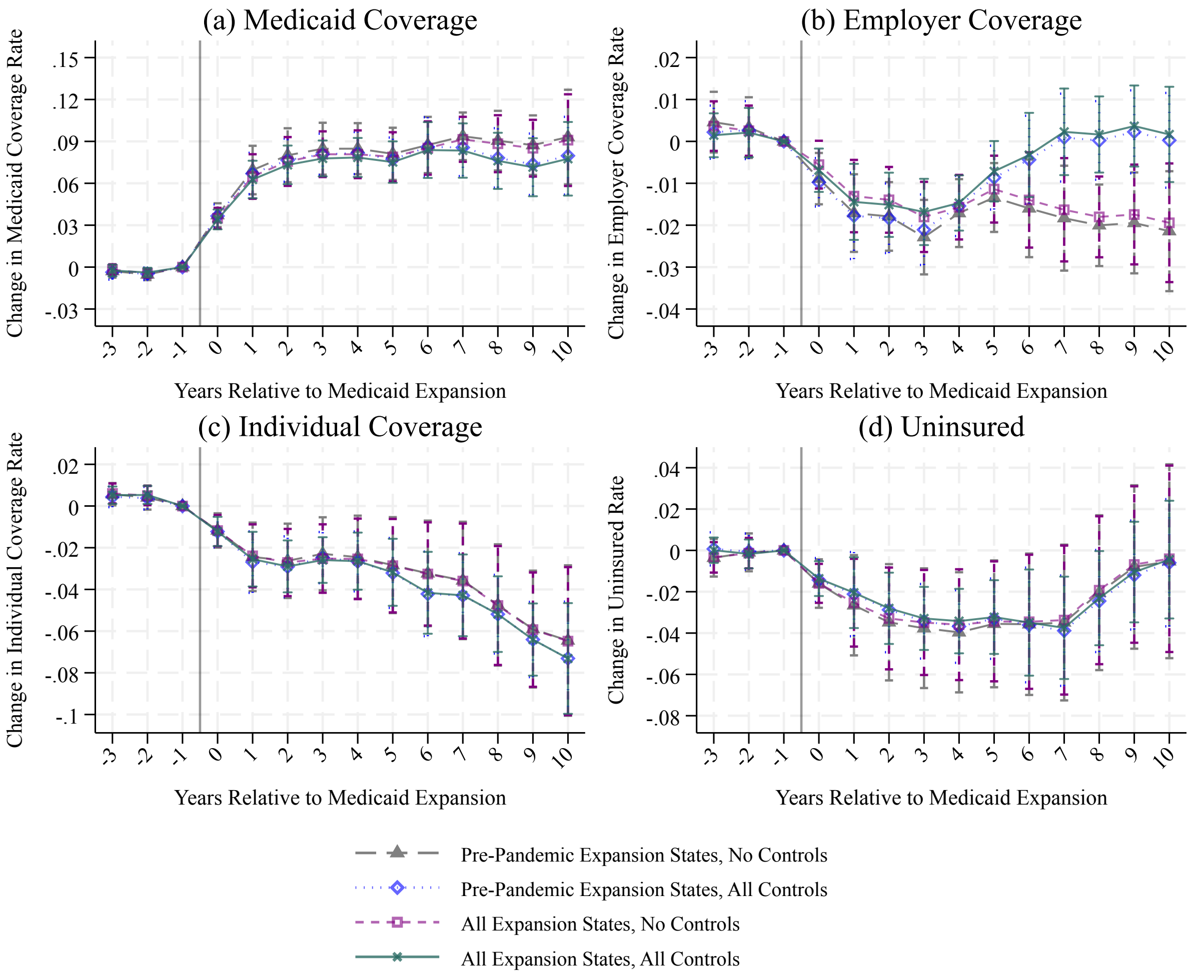
Notes: This figure presents weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey Standard errors are clustered at the state level. Vertical lines represent 95 percent confidence intervals. The sample includes individuals with no more than a high school degree in panel (a), and individuals associated with industries in the lowest quartile of mean employer-sponsored health insurance between 2011 and 2013 in panel (b). Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 5: Stacked Event Study Design, All Medicaid Expansion States and Health Insurance Coverage, Individuals with No More than a Completed High School Education



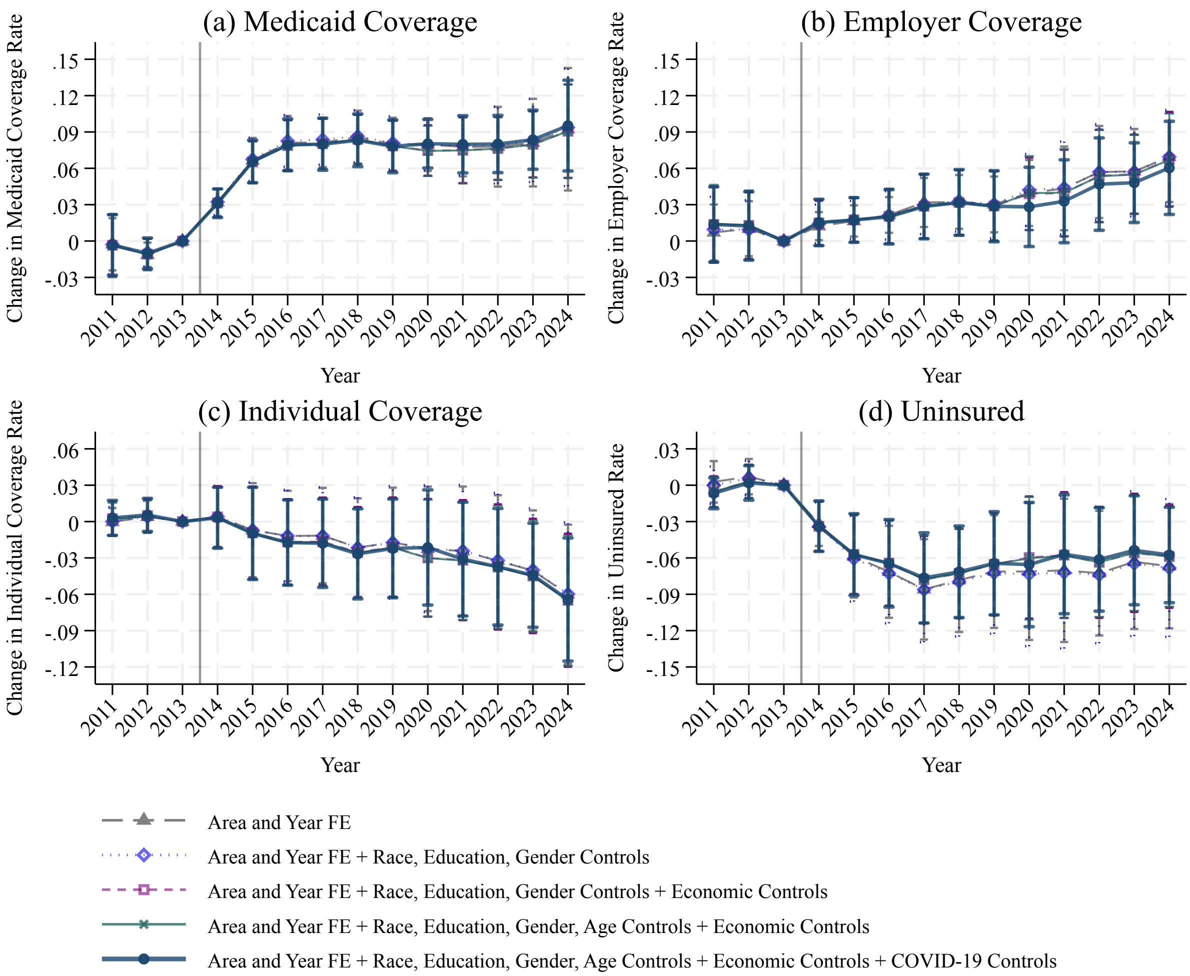
Notes: This figure presents estimates of equation 5 generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The sample includes individuals with no more than a high school degree. Standard errors are clustered at the state level. All specifications include state-cohort and event-time-cohort fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 6: Stacked Event Study Design, All Medicaid Expansion States and Health Insurance Coverage, Individuals in Low Pre-Treatment Insurance Coverage Industries



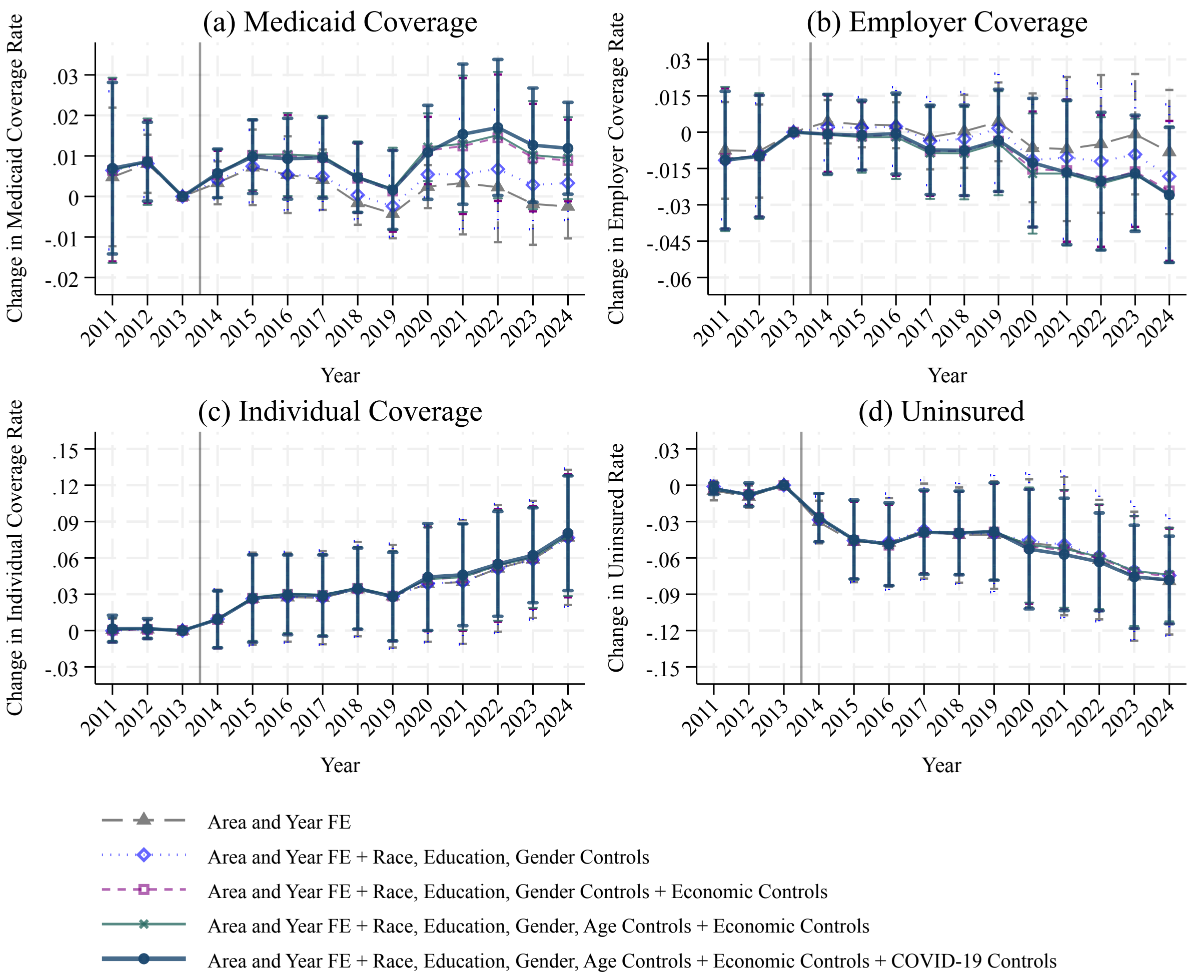
Notes: This figure presents estimates of equation 5 generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The sample includes individuals associated with industries in the lowest quartile of mean employer-sponsored health insurance between 2011 and 2013. Standard errors are clustered at the state level. All specifications include state-cohort and event-time-cohort fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 7: DDD Event Study Analysis of State Medicaid Expansion



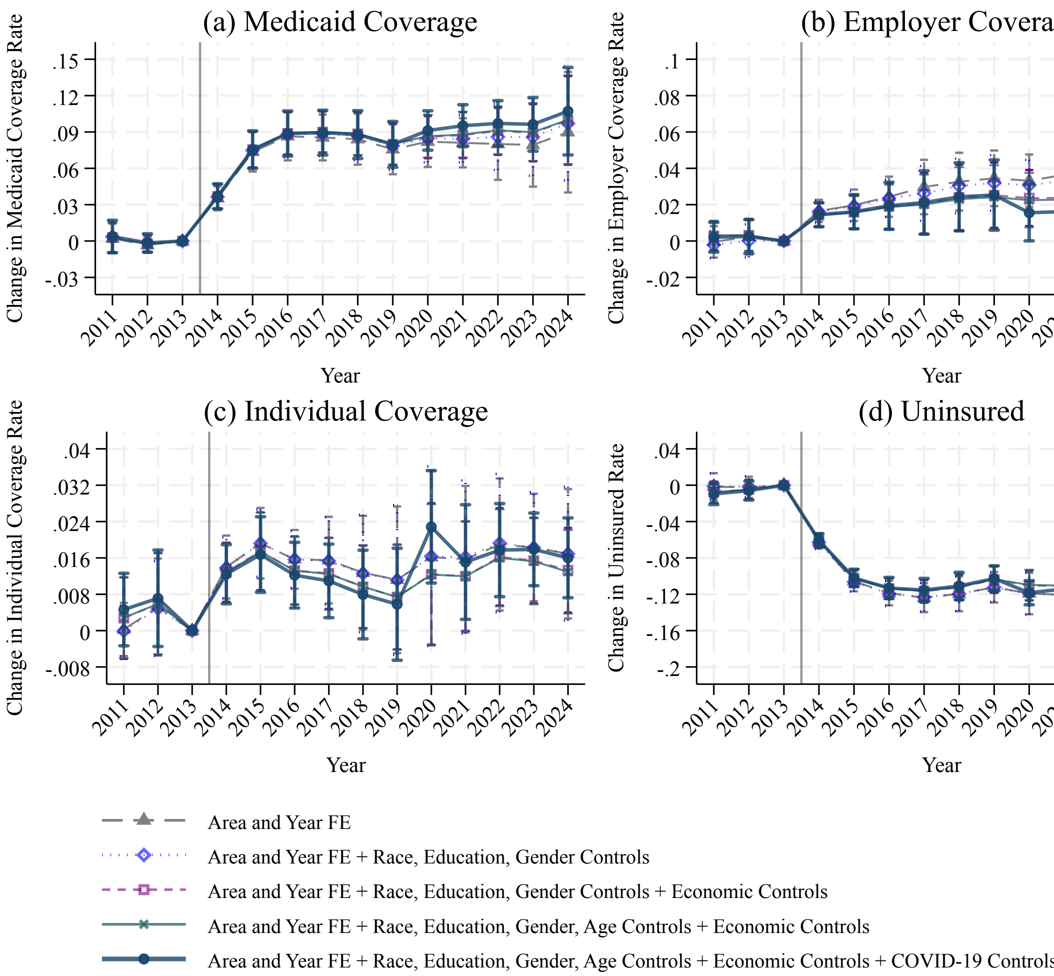
Notes: This figure presents weighted least squares estimates generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The estimates in the figure represent the implied effects of the Medicaid Expansion provision of the ACA at the mean pre-treatment uninsured rate of 21.2 percent. They are linear combinations of coefficients derived from equation (3), as described in detail in the main text. Standard errors are clustered at the state level. All specifications include CBSA and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 8: Effect of the ACA Without Medicaid Expansion from DDD Event Study Analysis



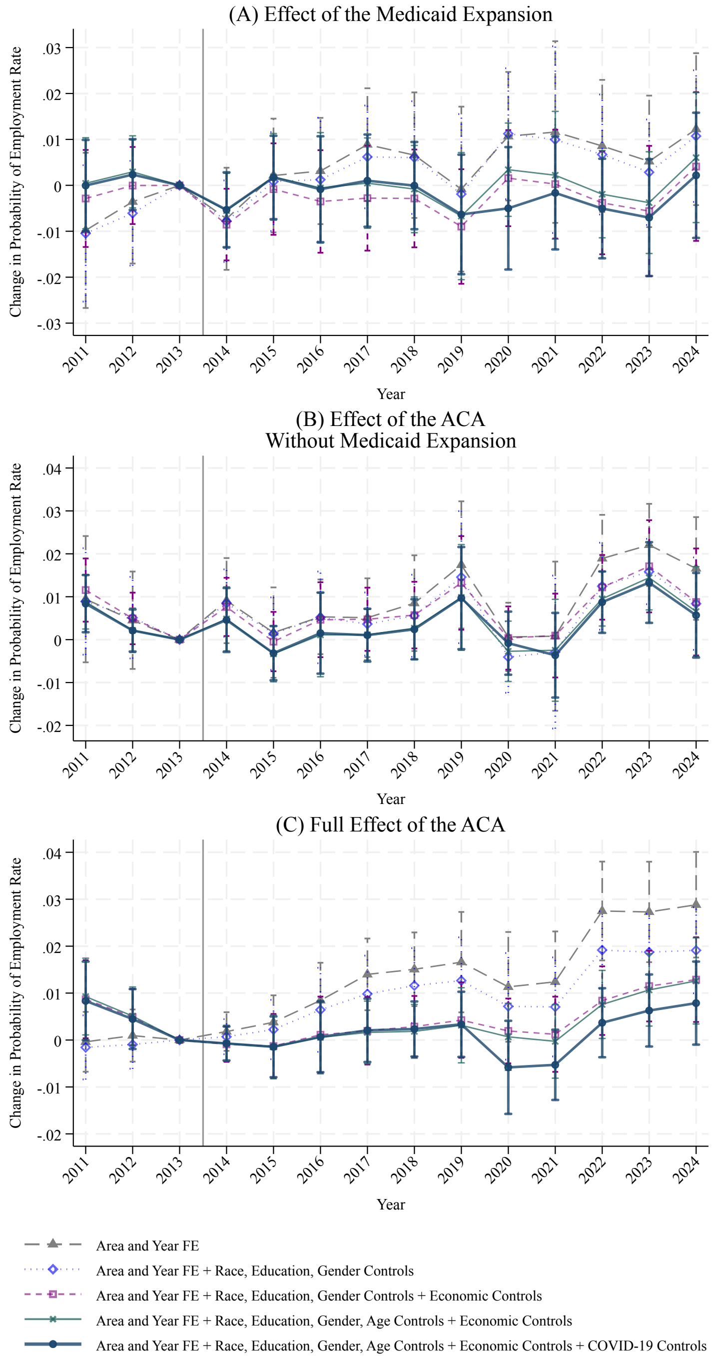
Notes: This figure presents weighted least squares estimates generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The estimates in the figure represent the implied effects of provisions of the ACA other than the Medicaid expansion at the mean pre-treatment uninsured rate of 21.2 percent. They are linear combinations of coefficients derived from equation (3), as described in detail in the main text. Standard errors are clustered at the state level. All specifications include CBSA and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 9: Full effect of ACA from DDD Event Study Analysis



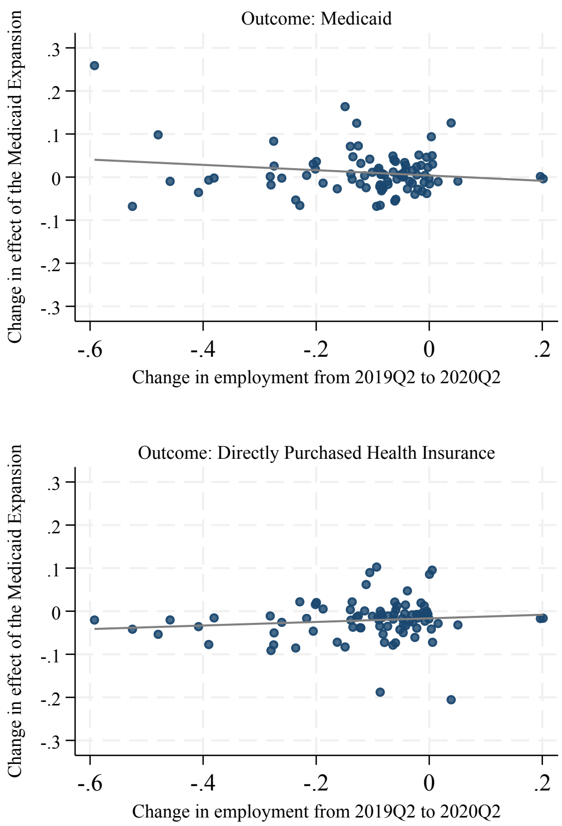
Notes: This figure presents weighted least squares estimates generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The estimates in the figure represent the implied effects of all provisions of the ACA at the mean pre-treatment uninsured rate of 21.2 percent. They are linear combinations of coefficients derived from equation (3), as described in detail in the main text. Standard errors are clustered at the state level. All specifications include CBSA and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 10: DDD Event Study Analysis for Probability of Employment



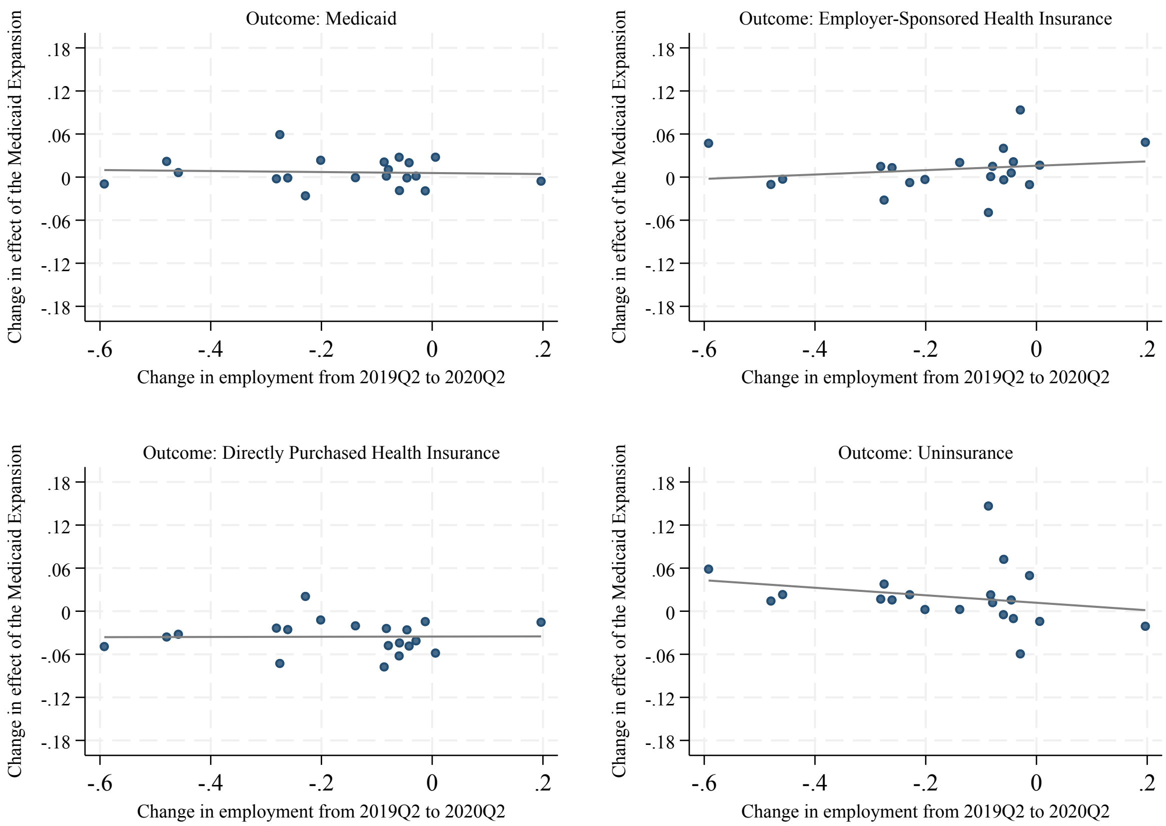
Notes: This figure presents weighted least squares estimates generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The estimates in the figure represent the implied effects of various provisions of the ACA on employment at the mean pre-treatment uninsured rate of 21.2 percent. Panel (A) represents the implied effects of the Medicaid Expansion, panel (B) represents the implied effects of other provisions of the ACA and panel (C) represents the implied full effects of the ACA i.e. of all of its provisions. Estimates presented are linear combinations of coefficients derived from equation (3), as described in detail in the main text. Standard errors are clustered at the state level. All specifications include CBSA and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure 11: Relationship Between Industry Churn and the Change in the Effect of the Medicaid Expansion on Insurance from 2019 to 2022: Individuals with a High School Degree or Less



Notes: This figure presents the relationship between the change in the effect of the Medicaid expansion from 2019 to 2022 within an industry among individuals with low education and churn in the industry in which they are employed. Industries are categorized at the 3-digit level of the NAICS codes. Churn, represented on the x-axis, is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The coefficients plotted on the y-axis are derived as follows. Equation (2) is estimated by industry for the subpopulation of employed individuals with a high-school degree or less and a difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. Each dot thus represents the change from 2019 to 2022 of the effect of the Medicaid expansion for an industry. The specification used to derive the coefficients includes state and year fixed effects and controls for race, gender, education, age, state-level unemployment rate, per-capita income and housing price index, and controls that capture a state's exposure to the COVID-19 pandemic, with standard errors clustered at the state level.

Figure 12: Relationship Between Industry Churn and the Change in the Effect of the Medicaid Expansion on Insurance from 2019 to 2022: Individuals in Industries with Low Baseline Employer Coverage



Notes: This figure presents the relationship between the change in the effect of the Medicaid expansion from 2019 to 2022 within an industry among employed individuals associated with industries in the lowest quartile of pre-treatment insurance coverage and churn in the industry. Industries are categorized at the 3-digit level of the NAICS codes. Churn, represented on the x-axis, is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The coefficients plotted on the y-axis are derived as follows. Equation (2) is estimated by industry for the subpopulation of employed individuals associated with industries in the lowest quartile of pre-treatment insurance coverage and a difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. Each dot thus represents the change from 2019 to 2022 of the effect of the Medicaid expansion for an industry. The specification used to derive the coefficients includes CBSA and year fixed effects, controls for race, gender, education, age, state-level unemployment rate, per-capita income and housing price index, and controls that capture a state's exposure to the COVID-19 pandemic, with standard errors clustered at the state level.

Tables

Table 1: Difference-in-Differences Estimates of the Relationship Between State Medicaid Expansion and Health Insurance Coverage in 2021 and 2022, Individuals with No More than a Completed High School Education

	(1)	(2)	(3)	(4)	(5)
	Panel I: Medicaid Coverage				
Medicaid Expansion$_s$×Post$_t$	0.092***	0.090***	0.088***	0.087***	0.096***
	(0.011)	(0.012)	(0.013)	(0.013)	(0.017)
	Panel II: Employer Coverage				
Medicaid Expansion$_s$×Post$_t$	-0.018*	-0.015*	-0.013*	-0.011*	-0.004
	(0.009)	(0.008)	(0.006)	(0.006)	(0.007)
	Panel III: Individual Coverage				
Medicaid Expansion$_s$×Post$_t$	-0.025**	-0.024**	-0.033***	-0.033***	-0.038***
	(0.011)	(0.012)	(0.008)	(0.008)	(0.008)
	Panel IV: Uninsured				
Medicaid Expansion$_s$×Post$_t$	-0.048**	-0.049**	-0.041***	-0.042***	-0.053***
	(0.021)	(0.022)	(0.010)	(0.010)	(0.013)
	Panel V: Employed				
Medicaid Expansion$_s$×Post$_t$	-0.014*	-0.012	<0.001	0.002	0.006
	(0.008)	(0.008)	(0.006)	(0.006)	(0.005)
Observations	2,885,706	2,885,706	2,885,706	2,885,706	2,885,706
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race		Y	Y	Y	Y
Economic Controls			Y	Y	Y
Age				Y	Y
COVID-19 Controls					Y

Notes: Weighted least squares estimates generated using data from the 2011-2013 and 2021-2022 American Community Survey. Standard errors are clustered at the state level. Race controls include indicators for non-Hispanic white, black and Hispanic. Education controls include indicators for less than high school, high school degree, some college, and college degree. Economic controls include state-level per-capita income, unemployment rate, and housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a dummy for "year after 2020". The estimates in this table are the primary coefficients of interest from in equation (1). Low education is defined as individuals with a high school degree or less.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2: Difference-in-Differences Estimates of the Relationship Between State Medicaid Expansion and Health Insurance Coverage in 2021 and 2022, Individuals in Low Pre-Treatment Insurance Coverage Industries

	(1)	(2)	(3)	(4)	(5)
	Panel I: Medicaid Coverage				
Medicaid Expansion$_s \times$Post$_t$	0.089***	0.087***	0.083***	0.082***	0.091***
	(0.013)	(0.013)	(0.014)	(0.014)	(0.020)
	Panel II: Employer Coverage				
Medicaid Expansion$_s \times$Post$_t$	-0.020**	-0.017**	-0.010	-0.009	0.004
	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)
	Panel III: Individual Coverage				
Medicaid Expansion$_s \times$Post$_t$	-0.039**	-0.038**	-0.047***	-0.046***	-0.052***
	(0.015)	(0.016)	(0.011)	(0.011)	(0.010)
	Panel IV: Uninsured				
Medicaid Expansion$_s \times$Post$_t$	-0.030	-0.032	-0.026*	-0.028**	-0.040**
	(0.023)	(0.023)	(0.014)	(0.014)	(0.019)
	Panel V: Employed				
Medicaid Expansion$_s \times$Post$_t$	-0.020*	-0.018*	0.001	0.001	0.010*
	(0.010)	(0.010)	(0.006)	(0.006)	(0.006)
Observations	1,956,399	1,956,399	1,956,399	1,956,399	1,956,399
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race		Y	Y	Y	Y
Economic Controls			Y	Y	Y
Age				Y	Y
COVID-19 Controls					Y

Notes: Weighted least squares estimates generated using data from the 2011-2013 and 2021-2022 American Community Survey. Standard errors are clustered at the state level. Race controls include indicators for non-Hispanic white, black and Hispanic. Education controls include indicators for less than high school, high school degree, some college, and college degree. Economic controls include state-level per-capita income, unemployment rate, and housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a dummy for "year after 2020". The estimates in this table are the primary coefficients of interest from in equation (1). Low pre-treatment coverage industries are defined as industries in the bottom quartile of mean employer-sponsored health insurance by industry between 2011 and 2013.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3: Difference-in-Difference-in-Differences Estimates of the Relationship Between Affordable Care Act Implementation and Health Insurance Coverage

	(1) Medicaid Coverage	(2) Employer Coverage	(3) Individual Coverage	(4) Uninsured	(5) Employed
Medicaid Expansion$_s$ $\times$Post$_t$	-0.017	-0.025*	0.015	0.013	0.013**
	(0.015)	(0.014)	(0.021)	(0.023)	(0.006)
Post$_t$$\times$Uninsured$_{as}$	0.045	-0.037	0.252***	-0.277***	-0.005
	(0.038)	(0.033)	(0.087)	(0.096)	(0.020)
Medicaid Expansion$_s$ $\times$Post$_t$$\times$Uninsured$_{as}$	0.390***	0.119*	-0.184*	-0.254**	-0.033
	(0.055)	(0.059)	(0.101)	(0.105)	(0.027)
Implied effects of ACA at pre-treatment uninsured rates					
ACA without Medicaid Expansion	0.010	-0.008	0.054***	-0.059***	-0.001
	(0.008)	(0.007)	(0.019)	(0.020)	(0.004)
Medicaid Expansion	0.083***	0.025**	-0.039*	-0.054**	-0.007
	(0.012)	(0.013)	(0.022)	(0.022)	(0.006)
Full ACA (with Medicaid Expansion)	0.093***	0.017	0.014*	-0.113***	-0.008**
	(0.012)	(0.011)	(0.007)	(0.009)	(0.003)
Observations	8,027,724	8,027,724	8,027,724	8,027,724	8,027,724
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race	Y	Y	Y	Y	Y
Economic Controls	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y
COVID-19 Controls	Y	Y	Y	Y	Y

Notes: Weighted least squares estimates generated using data from the 2011-2013 and 2021-2022 American Community Survey. Standard errors are clustered at the state level. Race controls include indicators for non-Hispanic white, black and Hispanic. Education controls include indicators for less than high school, high school degree, some college, and college degree. Economic controls include state-level per-capita income, unemployment rate, and housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a dummy for "year after 2020". The estimates in this table are the primary coefficients of interest from equation (3) estimated on the full sample of all non-elderly adults, as described in detail in the main text. Entries presented below the label "Implied effects of ACA at pre-treatment uninsured rates" are linear combinations of the coefficients presented earlier in the table. The mean pre-treatment uninsured rate is 21.2 percent and is derived using the 2013 uninsured rate at the CBSA level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4: Implied Crowd-Out Rates: Employer Coverage Changes Relative to Medicaid Effects

		(1) DD ES: Low Educ.	(2) DD ES: Low Cov.	(3) DDD ES: Med. Exp.	(4) DDD ES: Full ACA		(5) DD ES: Low Educ.	(6) DD ES: Low Cov.	(7) DDD ES: Med. Exp.	(8) DDD ES: Full ACA
Panel I: Event Study Estimates										
	Most negative 2022 coefficient					Most negative change (2019-2022) coefficient				
(a)	2022 coefficient for employer coverage	-0.019	-0.019	0.047	0.027	Coefficient for change (2019-2022) in employer coverage	-0.011	-0.007	0.018	0.001
(b)	Associated 2022 Medicaid coefficient	0.091	0.085	0.080	0.097	Associated 2022 Medicaid coefficient	0.091	0.085	0.080	0.097
(c)	Associated 2022 CI LB for employer coverage	-0.037	-0.032	0.009	0.003	Associated CI LB for change (2019-2022) in employer coverage	-0.018	-0.015	-0.004	-0.013
	Average 2022 coefficient					Average change (2019-2022) coefficient				
(d)	2022 coefficient for employer coverage	-0.012	-0.009	0.054	0.038	Coefficient for change (2019-2022) in employer coverage	-0.004	0.001	0.024	0.010
(e)	2022 Medicaid coefficient	0.081	0.075	0.078	0.089	2022 Medicaid coefficient	0.081	0.075	0.078	0.089
(f)	2022 CI LB for employer coverage	-0.016	-0.012	0.015	0.015	CI LB for change (2019-2022) in employer coverage	-0.012	-0.008	0.002	-0.004
Panel II: Crowd-Out Estimates										
(a)/(b)	Most negative 2022 coefficient	-0.212	-0.226	0.587	0.275	Most negative change (2019-2022) coefficient	-0.119	-0.085	0.228	0.015
(c)/(b)	CI LB of most negative 2022 coefficient	-0.404	-0.372	0.110	0.034	CI LB for most negative change (2019-2022) coefficient	-0.199	-0.171	-0.055	-0.139
(d)/(e)	Average 2022 coefficient	-0.151	-0.126	0.688	0.425	Average change (2019-2022) coefficient	-0.052	0.016	0.311	0.109
(f)/(e)	Average CI LB of 2022 coefficient	-0.194	-0.159	0.196	0.166	Average CI LB of change (2019-2022) coefficients	-0.149	-0.105	0.021	-0.047

Notes: Panel I reports event-study estimates of effects on employer-sponsored and Medicaid coverage, focusing on both point estimates and confidence interval lower bounds (CI LB) for the 2022 coefficient and the change from 2019–2022, including the most negative 2022 estimate and averages across specifications. The estimates in Panel I are drawn from other exhibits reporting our DD and DDD event-study results, or from linear combinations thereof, as described in Section 4.3. Panel II reports implied crowd-out rates, calculated as the ratio of employer coverage estimates to the corresponding Medicaid coefficients.

Appendix A Comparison of the Reported Results and Pre-Committed Analysis Plan

We conducted an audit of the extent of our fidelity to our precommitted analysis using a large language model (LLM)—Claude Sonnet 4.6. To do this, we provided the LLM with both the Clemens, McNichols, and Sabia (2020) working paper and the current paper in PDF format, including only the text and excluding all exhibits. We then supplied the following prompt, which represents the final version arrived at after iteratively refining both its language and content with the assistance of an LLM.

```
Instructions:
   You are a rigorous methods auditor.  You will be given two documents:
a pre-registration plan (labeled ‘‘pre-analysis-plan’’) and a published
paper (labeled ‘‘analysis’’).  Your job is to systematically compare
them and identify any correspondences, deviations, or omissions.
```

1. Carefully read both documents in full before producing any output.
2. Extract every substantive methodological commitment from the pre-registration plan, including but not limited to:
   - Hypotheses (primary and secondary)
   - Outcome measures (primary and secondary)
   - Sample size and power calculations
   - Inclusion/exclusion criteria
   - Randomization or assignment procedures
   - Analysis plan (statistical tests, covariates, model specifications)
   - Correction for multiple comparisons
   - Planned subgroup or moderation analyses
   - Any other explicitly pre-committed decisions
3. For each element, locate what the paper actually reports and compare it against the pre-registration. Critical distinction you must enforce:
   - A supplemental analysis is one the paper adds beyond what was pre-registered. This is acceptable and should be flagged only as ‘‘additional,’’ not as a deviation.
   - A deviation is when the paper executes a pre-registered analysis differently from how it was promised - e.g., using a different statistical test, different covariates, a different operationalization of the outcome, or dropping a promised analysis entirely without disclosure.
   - Do not conflate these two. Adding new analyses is not a deviation. Changing or omitting promised analyses is.
4. Output a structured markdown table with the following columns:

   | Element | Preregistered | Reported in Paper | Deviation? | Disclosed? | Severity | Notes |

   Column definitions:

   - Element - the specific methodological commitment (e.g., ‘‘Primary hypothesis,’’ ‘‘Exclusion criterion: outliers,’’ ‘‘Covariate adjustment’’)

- Preregistered - verbatim or close paraphrase of what the pre-registration committed to
- Reported in Paper - what the paper actually did; if absent, write ‘‘Not reported’’
- Deviation? - Yes / No / Partial (use Partial if the paper follows the spirit but not the letter)
- Disclosed? - Yes if the paper explicitly acknowledges the deviation; No if not; N/A if there is no deviation
- Severity - None (no deviation), Minor (unlikely to affect conclusions), Major (could affect conclusions or constitutes selective reporting)
- Notes - any important context, ambiguities, or flags (e.g., "paper adds this as exploratory," "pre-reg wording was vague," "deviation favors significant result")

After the table, write a brief Summary Assessment (3-5 sentences) characterizing the overall fidelity between the pre-registration and the paper.

5. After the Summary Assessment, compute and report the following metrics:
   - Pre-registered Element Execution Rate
     - Formula: (# elements executed as pre-committed) / (# total pre-registered elements)
     - Provide two versions, once where an element is considered "executed as pre-committed" only if Deviation? = No, and once where an element is considered "executed as pre-committed" only if Deviation? = No or Partial
     - Report as a fraction and percentage, e.g. 8/11 (73%)
     - This analysis will be restricted to the pre-registered elements of the paper only, not to any supplementary analyses.
   - Transparency Rate in Precommitted Analyses
     - Formula: (# differences acknowledged in paper) / (# total differences detected)
     - A ‘‘difference’’ is an element where Deviation? = Yes or Partial
     - Count as ‘‘acknowledged’’ if Disclosed? = Yes
     - If a pre-registered element is entirely absent from the paper with no acknowledgment, count it as an unacknowledged execution deviation
     - Report as a fraction and percentage
   - Transparency Rate in Supplemental Analyses
     - Formula: (# supplemental analyses acknowledged as exploratory/ additional) / (# total supplemental analyses identified)
     - A supplemental analysis is acknowledged if the paper labels it as exploratory, post-hoc, additional, or otherwise flags it as outside the pre-registration
     - Report as a fraction and percentage

The LLM performed a text-based comparison between the working paper that contains our pre-analysis plan (Clemens, McNichols, and Sabia, 2020) and the present paper, generating an exhaustive list of precommitted elements, and assessing the extent of deviations and omissions in the current study. The output, reproduced verbatim, is presented in Appendix

Table A.5. The LLM identified 28 elements that we committed to, spanning hypotheses, outcome measures, data sources, sample horizon, population subgroups, treated and control group definitions, empirical specifications and analyses, covariates, as well as 5 supplementary analyses we included in the present paper but did not pre-specify. A review of our pre-analysis plan confirmed that a subset of the 28 elements consisted of explicit commitments (i.e., statements such as "we commit to"), while others were embedded more diffusely throughout the document in less explicit language. Further, it provided the following summary.

```
Overall, the published paper adheres closely to its pre-registered analysis
plan.  The core empirical specifications (Equations 1-4), outcome variables,
population subsamples, COVID-19 control strategy, and industry heterogeneity
analyses are all implemented substantively as committed.  The most consequential
disclosed deviation is the narrowing of the formal coefficient equality
tests:  rather than separately testing each of the 2020, 2021, and 2022
coefficients against both 2018 and 2019 baselines, the paper focuses
formal inference on the 2019-vs.-2022 comparison alone.  While the authors
provide a reasonable justification (this is the strongest test of the
long-run hypothesis), the pre-committed tests for interim years (2020
and 2021) and the 2018 baseline comparison are not formally presented,
though the underlying estimates are visible in event study figures.  A
second notable deviation is the omission of 4-digit NAICS industry analyses,
which is plausibly justified by NAICS reclassification inconsistencies
but goes unacknowledged in the main text (disclosed in Section 3).  The
source data switches (CEPR → IPUMS, NYT → CDC/Hopkins, BLS/BEA → FRED,
median → mean HPI) are all minor and most are explicitly disclosed.  The
paper is transparent about extensions beyond the pre-analysis plan (2023-2024
data, stacked event study, precision weighting, crowd-out analysis, LLM
audit), correctly labeling them as additions rather than pre-committed
analyses.
```

Finally, in response to our instruction to compute metrics of fidelity and transparency, the LLM reported that our pre-registered element execution rate—defined more stringently as the share of elements executed exactly as originally specified, without any deviations—was 75% (21 out of 28 elements), and the execution rate—defined less stringently as the share of elements executed with no or partial deviations—was 93% (26 out of 28 elements). The elements that the LLM classified as partial deviations (6, 18, 19, 20, 21 in Appendix Table A.5) were all cases in which the analyses remained substantively equivalent to those originally proposed but differed in data sources or in the level of detail. Further, we obtained a 100% transparency rate for all pre-committed analyses, explicitly disclosing both partial and full deviations in-text, as well as a 100% transparency rate for all supplemental analyses, with all five additional analyses clearly disclosed.

Here we note an important aspect of our fidelity audit. The procedure was iterative—while the prompt itself remained fixed, we repeatedly reviewed and refined the analysis to address any deviations from the pre-committed specifications and to ensure appropriate disclosure of supplemental analyses. This process continued until the LLM no longer flagged avoidable non-disclosures or discrepancies.

In addition to adherence, the completeness of the pre-analysis plan constitutes a separate and important consideration. We again leveraged an LLM to review the pre-analysis plan in Clemens, McNichols, and Sabia (2020). Before the LLM could conduct an audit of our plan's completeness, we were tasked with defining "completeness". For this, we drew from Ofosu and Posner (2023) who outline the following four criteria—(1) Specifying a clear hypothesis; (2) Specifying the primary dependent variable(s) sufficiently clearly so as to prevent post-hoc adjustments; (3) Specifying the treatment or main explanatory variable sufficiently clearly so as to prevent post-hoc adjustments; (4) Spelling out the precise statistical model to be tested including functional forms and estimator.[22] Our first approach was to have the LLM evaluate our pre-analysis plan against these criteria, using the prompt below, allowing for interpretive discretion in defining their precise scope.

```
You are an expert methodologist specializing in pre-analysis plans (PAPs)
for empirical research.  Your task is to evaluate whether a submitted
PAP meets the four criteria for completeness.  Be rigorous but fair -
look for what IS specified, not just what is missing.  Evaluate the pre-analysis
plan (labeled ‘‘pre-analysis-plan’’) provided below against the following
four criteria for completeness:

  1. Specifying a clear hypothesis

  2. Specifying the primary dependent variable(s) sufficiently clearly
     so as to prevent post-hoc adjustments

  3. Specifying the treatment or main explanatory variable sufficiently
     clearly so as to prevent post-hoc adjustments

  4. Spelling out the precise statistical model to be tested including
     functional forms and estimator

   For each criterion, provide:

  • Met:  Yes / No

  • Confidence:  High / Medium / Low - reflecting how much ambiguity
    you encountered in making that call

  • Evidence:  Quote or paraphrase the relevant passage from the PAP
    to justify your answer

  • Gap (if any):  What specific information is absent or ambiguous.
    End with an Overall Completeness Summary (1-2 sentences) and a Priority
    Recommendation identifying the most critical gap to address first.
```

The exercise produced the following output.

```
Criterion 1:  Clear Hypothesis
Met:  Partially --- Yes with caveats   Confidence:  Medium
Evidence:  The authors specify directional hypotheses for both benefits
and costs.  On benefits, they predict that low-education individuals
and those in low-coverage industries will experience smaller increases
in uninsurance during the pandemic if they live in Medicaid expansion
states.  On costs, they predict those same groups will become less likely
to have employer-provided insurance in expansion states post-COVID, with
effects largest in industries and areas disproportionately hit by both
COVID-19 and the initial ACA rollout.
Gap:  The hypotheses are directional but not magnitude-specific.  More
critically, the paper does not pre-specify the precise threshold or benchmark
against which "confirmation" or "rejection" of the hypothesis would be
```

[22] See the notes for Figure 3 of Ofosu and Posner (2023).

declared --- e.g., no minimum effect size is stated, and no correction for multiple hypothesis testing is committed to despite testing across multiple subgroups, outcomes, and time periods simultaneously.

Criterion 2: Primary Dependent Variable(s) Specified
Met: Yes Confidence: High
Evidence: The authors are explicit that four insurance coverage indicators are the outcomes: (i) Medicaid coverage, (ii) employer-provided insurance, (iii) other private insurance, and (iv) uninsured status, all drawn from the American Community Survey. They also state that employment will be added as an outcome in the pre-committed long-run analysis, which is a transparent ex ante extension.
Gap: Minor ambiguity exists around the employment outcome --- it is mentioned as an extension but its precise construction (e.g., employment-to-population ratio, hours worked, full- vs. part-time status) is not defined. Given prior debate in the ACA literature about part-time employment effects specifically, this leaves some room for post-hoc variable selection.

Criterion 3: Treatment / Main Explanatory Variable Specified
Met: Yes Confidence: High
Evidence: The primary treatment variable is a binary indicator for whether a state's ACA Medicaid expansion was in effect in a given state-year cell (Medicaid Expansion_s,t = 1). Variation in timing across states is explicitly acknowledged and mapped in Figure 1. The authors also pre-specify a secondary explanatory dimension --- baseline (2013) CBSA-level uninsured rates --- for the sub-state identification strategy following Courtemanche et al. (2017).
Gap: The handling of late-expanding states (those that expanded after 2014) in the pre-committed event-study is not fully resolved. The authors note that the illustrative figures restrict to states that expanded in 2014 and never-expanders for clarity, but do not formally commit to how staggered adoption will be handled econometrically in the long-run analysis --- an issue that became significantly more salient in the DiD literature after 2020 (Callaway & Sant'Anna, Sun & Abraham, etc.).

Criterion 4: Precise Statistical Model, Functional Form, and Estimator
Met: Partially --- No with caveats Confidence: Medium
Evidence: The authors specify three estimating equations in full mathematical notation: a basic DiD (Equation 1), an event-study extension (Equation 2), and a sub-state CBSA-level triple-difference (Equation 3). Control variables are enumerated --- state unemployment, log per capita income, a house price index, and individual demographics. The omitted event-time category is defined (the year prior to expansion). They also commit to COVID-19 robustness controls, specifying that they will interact cumulative case and death counts and peak employment declines with two time dummies (2020 and a pooled 2021--2022 indicator).
Gap: Three meaningful gaps remain. First, standard error clustering is not pre-specified (state-level clustering is conventional but not stated). Second, the industry-level heterogeneity analysis --- estimating Equations 2 and 3 separately by 3- and 4-digit NAICS code and then regressing estimated ACA effects on industry-level churn --- is described only in prose, without a formal equation, and the authors acknowledge this portion requires "more discretion." Third, the coarsened exact matching (CEM) procedure is described for the baseline analysis but not formally committed

```
to for the long-run updates, leaving open whether and how reweighting
will be applied to post-pandemic data.

Overall Completeness Summary
The PAP is stronger than most observational pre-analysis plans: the
core estimating equations are fully written out, dependent variables
are explicitly enumerated, and the treatment variable is clearly defined
with known timing variation. However, the plan has meaningful gaps in
hypothesis precision (no effect size benchmarks, no multiple-testing
corrections), incomplete specification of the employment outcome variable,
unresolved treatment of staggered adoption in the long-run DiD, and a
notably underspecified industry-heterogeneity analysis that the authors
themselves flag as requiring discretion.

Priority Recommendation: The most critical gap to address first is the
handling of staggered treatment adoption in the event-study design. The
pre-committed long-run analysis will necessarily include states that
expanded Medicaid in 2015, 2016, and beyond --- and without a pre-specified
estimator (e.g., a heterogeneity-robust DiD method or explicit justification
for the standard TWFE approach), the core event-study results remain
vulnerable to post-hoc methodological choices that can substantially
alter estimates and their interpretation.
```

As seen in the output, the LLM characterized our satisfaction of Criteria 2 and 3—relating to the clarity of the dependent and explanatory variables—as high. It assessed Criterion 1—the clarity of the hypothesis—as partially satisfied, leading to an overall "yes" with caveats, since while we stated the expected direction of the hypotheses in our pre-analysis plan, we did not pre-specify effect sizes. the LLM assessed Criterion 4—relating to the detail of empirical strategies—as only partially satisfied, resulting in an overall 'no' with caveats, due to three identified areas of non-specificity. Notably, the LLM flagged that substantial vagueness in our description of how post-2014 Medicaid expansion states are incorporated leaves our core estimation strategies vulnerable to post hoc modification.

In a second approach to assess the completeness of the pre-analysis plan, we provided the LLM a prompt that laid out the assessment criteria in more detail. To do this, we followed Ofosu and Posner (2023)'s publicly available questionnaire. In particular, we asked the LLM to apply to our pre-analysis plan the same questions that Ofosu and Posner (2023) posed to human research assistants when grading their sample of plans for completeness.[23] Depending on the share of questions pertaining to each criterion answered satisfactorily, the LLM assigned a completeness score for each category.

```
You are an expert methodologist specializing in pre-analysis plans (PAPs)
for empirical research. Your task is to evaluate whether a submitted
PAP meets the four criteria for completeness. Be rigorous but fair ---
look for what IS specified, not just what is missing.
   Evaluate the pre-analysis plan provided (labeled ‘‘pre-analysis-plan’’
below against each of the four criteria. For each criterion, provide:
```

[23] We restricted attention to the sections *Hypotheses*, *Clarity of variable definitions*, and *Statistical model specification* that pertain solely to the pre-analysis plan and do not reference the executed paper, and which permitted categorical responses—Yes, No, or Not Applicable.

- Rating: Complete (all questions answered Yes or N/A) / Partially Complete (some No) / Missing (all No). Include a % score = questions answered Yes or N/A ÷ total questions in that criterion.
- Evidence: Quote or paraphrase the relevant passage from the PAP to justify each answer.
- Gap (if any): What specific information is absent or ambiguous.

End with an Overall Completeness Summary (1--2 sentences) and a Priority Recommendation identifying the most critical gap to address first.

Criterion 1 --- Clear Hypothesis

- Q1. Does the PAP specify a clear hypothesis (or hypotheses) to be tested? A ‘‘clear hypothesis’’ describes a relationship between a clearly identified independent and dependent variable in which the direction of the effect is specified. (Example of a Yes: ‘‘providing citizens with information about service delivery will increase political participation.’’) [Yes / No]
- Q2. Does the PAP specify more than one hypothesis? [Yes / No]
- Q3. If yes to Q2: Are some hypotheses designated as primary and some as secondary/exploratory? [Yes / No / N/A --- only one hypothesis]

Criterion 2 --- Primary Dependent Variable(s)

- Q4. Are the primary dependent variable(s) operationalized sufficiently clearly to prevent post-hoc adjustments? (Note: Following Olken, ‘‘a good rule of thumb is that if you gave the PAP to two different programmers, and asked each to prepare the data for the primary dependent variable(s), they should be both able to do so without asking any questions, and they should both be able to get the same answer.’’ If multiple primary DVs are specified, code based on whether, in general, they are operationalized clearly. Where some dependent variables are designated as primary, focus only on those.) [Yes / No]
- Q5. If secondary/exploratory DVs are specified: Are they operationalized sufficiently clearly to prevent post-hoc adjustments? (Note: Following Olken, ‘‘a good rule of thumb is that if you gave the PAP to two different programmers, and asked each to prepare the data for the primary dependent variable(s), they should be both able to do so without asking any questions, and they should both be able to get the same answer.’’ If multiple primary DVs are specified, code based on whether, in general, they are operationalized clearly. Where some dependent variables are designated as primary, focus only on those.) [Yes / No / N/A --- no secondary/exploratory DVs specified]

Criterion 3 --- Treatment or Main Explanatory Variable

- Q6. Is the treatment or main explanatory variable specified sufficiently clearly to prevent post-hoc adjustments? (Note: Following Olken, ‘‘a good rule of thumb is that if you gave the PAP to two different programmers, and asked each to prepare the data for the primary dependent variable(s), they should be both able to do so without asking any questions, and they should both be able to get the same answer.'' If multiple primary DVs are specified, code based on whether, in general, they are operationalized clearly. Where some dependent variables are designated as primary, focus only on those.) [Yes / No]

Criterion 4 --- Statistical Model

- Q7. Does the PAP spell out the precise statistical model to be tested, including functional form and estimator (e.g., OLS, probit, logit, Poisson, instrumental variables)? [Yes / No]
- Q8. Does the PAP state how standard errors will be treated (e.g., robust, clustered, bootstrapped)? [Yes / No]
- Q9. Does the PAP specify a multiple testing adjustment? [Yes / No / N/A --- too few DVs for a multiple testing adjustment to be necessary]
- Q10. Does the PAP commit the researchers to presenting a simple difference-in-means test? [Yes / No]
- Q11. If yes to Q10: Do the authors specify whether the test will be one-tailed or two-tailed? [Yes / No / N/A --- no difference-in-means test specified]
- Q12. Does the PAP specify whether/how covariates (including fixed effects) will be included in the regression model? [Yes / No]

The second, more detailed prompt generated the following response.

Criterion 1 | Clear Hypothesis
Rating: Partially Complete | 67% (2/3 questions answered Yes)

Q	Answer	Evidence & Notes
Q1	Yes	Section 6 states: "we hypothesize that low-education individuals and those last employed in low-coverage industries will experience smaller increases in their probability of being uninsured during the pandemic if they live in states that enacted ACA Medicaid expansions." Direction (smaller increase = benefit of Medicaid expansion) is clearly specified. A cost-side hypothesis is also stated with direction: Medicaid expansion states will see greater declines in employer insurance offerings post-COVID.
Q2	Yes	Multiple hypotheses are stated: (1) uninsurance rates during the pandemic (benefits side); (2) employer coverage declines post-pandemic (costs side); (3) heterogeneous effects by industry/geography; (4) employment outcomes.

Q	Answer	Evidence & Notes
Q3	No	The PAP does not designate any hypothesis as primary vs. secondary or exploratory. The benefits hypothesis (uninsurance) and costs hypothesis (employer coverage) appear coordinate. Employment is introduced as an extension ("we extend our set of outcomes to include employment") but is not formally labelled secondary or exploratory.

Gap: No hierarchy among hypotheses is established. Readers cannot determine which result would be the headline finding and which would be subsidiary, creating room for post-hoc prioritization.

Criterion 2 | Primary Dependent Variable(s)
Rating: Partially Complete | 50% (1/2 questions answered Yes)

Q	Answer	Evidence & Notes
Q4	Yes	Insurance coverage outcomes are well-operationalized. Section 3 defines four binary indicator variables: covered by (i) Medicaid, (ii) employer-provided insurance, (iii) other private insurance, or (iv) uninsured. These map directly to ACS survey items. Two programmers given this PAP and the ACS codebook could independently construct identical variables.
Q5	No	Employment is introduced as a DV in Section 6 ("we extend our set of outcomes to include employment") with no operationalization. It is unclear whether this means: employed vs. not, hours worked, full-time vs. part-time, wage earnings, or some other measure. No data source for the employment outcome is named for the future analysis (the ACS is used for current analysis, but it is not confirmed as the source for the pre-committed employment outcome).

Gap: The employment outcome - introduced specifically as a new addition to the pre-committed analysis - is entirely without operationalization. This is the single largest DV-level vulnerability in the PAP.

Criterion 3 | Treatment or Main Explanatory Variable
Rating: Complete | 100% (1/1 questions answered Yes)

Q	Answer	Evidence & Notes
Q6	Yes	The treatment variable is precisely defined. Medicaid Expansion_{s,t} is set to 1 for all state-year combinations during which an ACA Medicaid expansion was in effect, and 0 otherwise (equation 1, Section 4). The source of expansion timing data is specified (Kaiser Family Foundation). For the sub-state specification (equation 3), the continuous treatment intensity variable - baseline (2013) CBSA-level uninsured rate - is also clearly defined. Two programmers could independently construct identical treatment variables.

Gap: None substantive. The PAP could marginally improve by specifying how late-adopting expansion states (those expanding after 2014) will be handled in calendar-time analyses, but this is addressed implicitly through the event-study design.

Criterion 4 | Statistical Model
Rating: Partially Complete | 58% (3.5/6 questions answered Yes)

Q	Answer	Evidence & Notes
Q7	Yes	Three estimating equations are fully written out in mathematical notation (equations 1-3). Equation 1 is a standard DiD (linear probability model via OLS implied, though not named). Equation 2 is an event-study extension. Equation 3 is a triple-difference specification interacting baseline uninsured rates with Medicaid expansion status and event-year dummies, following Courtemanche et al. (2017). Functional form (linear) is clear throughout.
Q8	No	Standard error treatment is never mentioned. The PAP does not specify whether standard errors will be robust, clustered (e.g., at the state level, which is standard in DiD with state-level policy variation), or bootstrapped. This is a meaningful gap given the well-known sensitivity of inference in DiD designs to clustering choices.
Q9	N/A	Four DVs are specified but the PAP does not address multiple testing. Given that four outcomes are examined simultaneously across two sub-samples and three specifications, a multiple testing correction (e.g., Bonferroni, Benjamini-Hochberg, or a family-wise error rate adjustment) would ordinarily be warranted. The absence of any mention is a gap, though the PAP's primary focus is on coefficient comparisons across time periods rather than on simultaneous hypothesis rejection. Scored N/A given the event-study framing, but flagged as a meaningful omission.
Q10	No	No simple difference-in-means test is committed to. All pre-committed analyses use regression-based event-study frameworks.
Q11	N/A	Not applicable (no difference-in-means test specified).

Q	Answer	Evidence & Notes
Q12	Yes (partial)	The baseline specification includes state and year fixed effects and a set of covariates (Xi,s,t): state unemployment rates, log per capita income, and median house price index (Section 4). For the pre-committed analyses, additional COVID-19 controls are pre-specified in detail: cumulative cases and deaths per capita interacted with indicators for 2020 and for 2021-2022, plus peak state-level employment declines interacted with the same time dummies. The covariate strategy for the industry-level heterogeneity analysis is less fully specified ("control for or match on each industry's average earnings"). Scored partial credit.

```
Gap: The absence of any statement on standard error clustering is the
most consequential statistical omission. In DiD designs with state-level
treatment, clustering at the state level can substantially widen confidence
intervals relative to robust or heteroskedasticity-only SEs. Pre-committing
to a clustering level is standard practice and its omission leaves an
important analytic degree of freedom open.
```

In this version of the PAP-completeness audit, we received completeness scores of 67% for hypothesis clarity, 50% for dependent variable specification, 100% for independent variables, and 58% for statistical model detail. We were also provided the following summary assessment.

```
Overall Completeness Summary:
The PAP is moderately complete: it specifies the treatment variable
with precision and lays out the regression equations clearly, but it
leaves critical gaps in hypothesis prioritization (Q3), operationalization
of the new employment outcome (Q5), standard error treatment (Q8), and---with
less severity---multiple testing adjustment (Q9). The plan is strongest
in its identification strategy and weakest in its statistical specification
details.

Priority Recommendation:
Address standard error clustering (Q8) first. In a DiD design where
the policy variable (Medicaid expansion) varies at the state level, the
choice of clustering unit directly determines the width of confidence
intervals and the plausibility of statistical significance claims for
the pre-committed results. This is a one-sentence fix that eliminates
a meaningful degree of freedom; closing it costs nothing and materially
strengthens the pre-commitment. A close second priority is operationalizing
the employment dependent variable (Q5), which is currently an entirely
open question.
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Our LLM-assisted audit of both the pre-commitment plan in Clemens, McNichols, and Sabia (2020) and the present execution of that plan reveals several important nuances in the use of pre-analysis plans for observational research. First, we echo the sentiment in Ofosu and Posner (2023) that norms governing what constitutes a complete and well-specified pre-analysis plan remain underdeveloped.[24] While the four broad criteria appear reasonable, the standards used to evaluate them are subject to considerable discretion. This ambiguity extends further to the assessment of adherence, where norms are even less well established. Together, these observations point to substantial scope for introducing greater structure in both the definition of completeness and the evaluation of compliance.

At the same time, there is an important role for flexibility and judgment in setting such standards. Appropriate thresholds may differ across research designs with distinct constraints and considerations. In observational settings, for instance, unforeseen changes in data construction or release may need to be accommodated within acceptable bounds of specificity or completeness. In contrast, randomized controlled trials may require tolerance for uncertainty in implementation arising from logistical, infrastructural, or temporal constraints.

Although our exercise does not attempt to offer formal recommendations to resolve these broader issues, we view LLMs as a potentially useful tool for both researchers and the editorial process. That said, a systematic evaluation of the strengths and limitations of LLMs for this class of tasks remains an important area for future work. We highlight, for example, several limitations of LLM use within our own auditing procedure. First, we conducted the adherence audit—described at the start of this appendix—using alternative LLM tools, including Google Gemini 3.1 Pro and ChatGPT 5.4 Pro, and found that models varied moderately in both the number of pre-specified elements (as shown in Table A.5) they identified and in how they classified partial versus full deviations. Additionally, we note particular instances in which the audits we have conducted strike us as being off base. In the completeness audit, for example, the LLM asserted that "standard error treatment is never mentioned." While our plan may indeed lack a direct "we commit to" statement with reference to standard errors, we note that our exercise centers on extending event study estimators for which we had already implemented a standard error estimation method. A deviation from that method would clearly have constituted a deviation in our design from the pre-analysis plan, implying in turn that the pre-analysis plan was complete along this dimension. Similarly, the LLM questioned whether our pre-specification of our employment outcome was sufficiently complete. Here again, because the pre-committed analyses extend our earlier analyses, any change to our definition of employment would have constituted an obvious deviation from the plan. This implies, once again, that the plan was, in fact, complete along this particular dimension. Designing prompts that are sufficiently detailed to minimize such errors and other instances of "hallucination"—without enabling researcher-driven prompt engineering that biases outputs toward favorable conclusions—remains an open challenge.

While best practices for ensuring transparency in research that implements pre-analysis plans are still evolving, our approach illustrates how emerging AI tools can be used to subject papers to systematic scrutiny and enhance transparency. For researchers, this may include using such tools to refine both the writing and execution of papers, and to improve the clarity and completeness of reporting. For readers and peer review processes, these tools may similarly serve as an additional layer of scrutiny, complementing traditional review

[24] Ofosu and Posner (2023) provide a detailed review of the literature outlining recommended inclusions and exclusions in pre-analysis plans.

practices at a fraction of the time cost of conducting such audits manually. Finally, we emphasize that the use of LLM audits by authors and peer review processes together are complementary, as an author teams' incentive to game such audits can be kept in check by the expectation of an editorial audits.

Table A.5: LLM Audit of Preregistration Compliance

No.	Element	Preregistered	Reported in Paper	Deviation?	Disclosed?	Severity	Notes
1	Primary hypothesis — insurance benefits	Low-education individuals and those in low-coverage industries will experience smaller increases in uninsurance during the pandemic if they live in Medicaid expansion states	Tested and reported; ACA's Medicaid expansions maintained roughly 9 pp Medicaid coverage gains through 2022, with uninsurance effects partly eroding over time	No	N/A	None	Paper finds results consistent with pre-specified direction but with nuance: individual coverage gains eroded, partially offsetting Medicaid gains
2	Primary hypothesis — employer coverage costs	Bust and recovery will lead to greater declines in employer insurance offerings in Medicaid expansion states; effects will be particularly large in COVID-impacted industries and geographic areas	Tested; paper finds no evidence of larger long-run declines in employer coverage; some saturated specs suggest modest recovery	No	N/A	None	Null finding on the primary hypothesis, faithfully reported
3	Primary hypothesis — employment	ACA's effects on employment will grow larger over the long run (i.e., post-pandemic) relative to short-run	Tested; paper finds no growth in employment effects over the long run	No	N/A	None	Added as explicit outcome per pre-reg's final sentence in Section 6
4	Outcome variables — insurance	Four indicator variables: (i) Medicaid coverage, (ii) employer-provided insurance, (iii) other forms of private insurance, (iv) uninsured	All four outcomes reported across all main specifications	No	N/A	None	Exact match
5	Outcome variable — employment	Pre-registration extends outcomes to include employment	Employment analyzed in event study figures (Figure 4) and appendix tables (C.1, C.2, C.6)	No	N/A	None	Exact match
6	Primary data source	American Community Survey (ACS); pre-reg used data from CEPR	Paper uses IPUMS USA extracts instead of CEPR, because CEPR discontinued ACS provision after 2019	Partial	Yes	Minor	Paper explicitly discloses the switch and notes it may cause minor discrepancies in short-run results. Forced by data availability change.
7	Sample period — main analysis	2011–2018 for short-run; pre-committed extension through 2022 for long-run	Main pre-committed analyses extend through 2022 as specified	No	N/A	None	Exact match
8	Sample period — event study extension	Not pre-specified beyond 2022	Paper additionally incorporates 2023 and 2024 ACS data into event study estimates	No	Yes	None	Clearly flagged as an extension beyond the pre-analysis plan ("extend beyond the pre-analysis plan")

Notes: This table reports the output of an LLM that was provided the instructions described in Appendix A to audit the compatibility between elements of our analysis pre-specified in Clemens, McNichols, and Sabia (2020) and those executed in the present paper.

Table A.5 (Cont.)

No.	Element	Preregistered	Reported in Paper	Deviation?	Disclosed?	Severity	Notes
9	Primary subsample — education	Individuals with no more than a high school education	Implemented as specified	No	N/A	None	Exact match
10	Primary subsample — low-coverage industry	Individuals in industries in the lowest quartile of pre-ACA (2011–2013) employer-sponsored insurance coverage	Implemented as specified	No	N/A	None	Exact match
11	Treatment group definition	States that expanded Medicaid in 2014	2014 expansion states used as treated group in Equations 1–4; late-expanders excluded from control	No	N/A	None	Exact match
12	Control group definition	States that had not expanded Medicaid	Pre-reg specifies non-expanding states as control; paper excludes any state that expanded between 2015 and 2022 from the control group	No	N/A	None	Consistent with pre-reg intent; refinement appropriate given new late-expanders through 2022
13	Empirical specification — static DiD (Eq. 1)	Basic difference-in-differences model with state and time fixed effects and covariates; Medicaid Expansion × Post indicator	Estimated as specified; Post defined as 2021–2022 vs 2011–2013; intervening years excluded	No	N/A	None	Exact match in structure; post-period correctly set to long-run years
14	Empirical specification — event study DiD (Eq. 2)	Event-study specification tracking year-by-year effects of Medicaid expansions; calendar-year interactions with 2013 as reference period	Implemented as specified with calendar-year indicators interacted with Medicaid expansion indicator; 2013 as reference	No	N/A	None	Exact match
15	Empirical specification — triple-difference static (Eq. 3)	Triple-difference model using CBSA-level baseline uninsurance rates; Courtemanche et al. (2017) framework; static version	Implemented as specified (Equation 3 in paper)	No	N/A	None	Exact match
16	Empirical specification — triple-difference event study (Eq. 4)	Event-study version of the triple-difference specification (Equation 3 in pre-reg)	Implemented as specified (Equation 4 in paper)	No	N/A	None	Exact match
17	Covariates — demographic	Age, education, and other household demographic characteristics from ACS	Implemented; paper lists age, race, gender, and education	No	N/A	None	Slight expansion (race, gender explicitly mentioned) but not a material deviation
18	Covariates — macroeconomic: unemployment rate	State-level unemployment rate from Bureau of Labor Statistics (BLS)	Sourced from Federal Reserve Economic Data (FRED), which compiles the same BLS series	Partial	Yes	Minor	Paper explicitly notes the discrepancy in footnote 4: "sourced from Federal Reserve Economic Data (FRED), which compiles these series from the same agencies." Functionally identical data.

Table A.5 (Cont.)

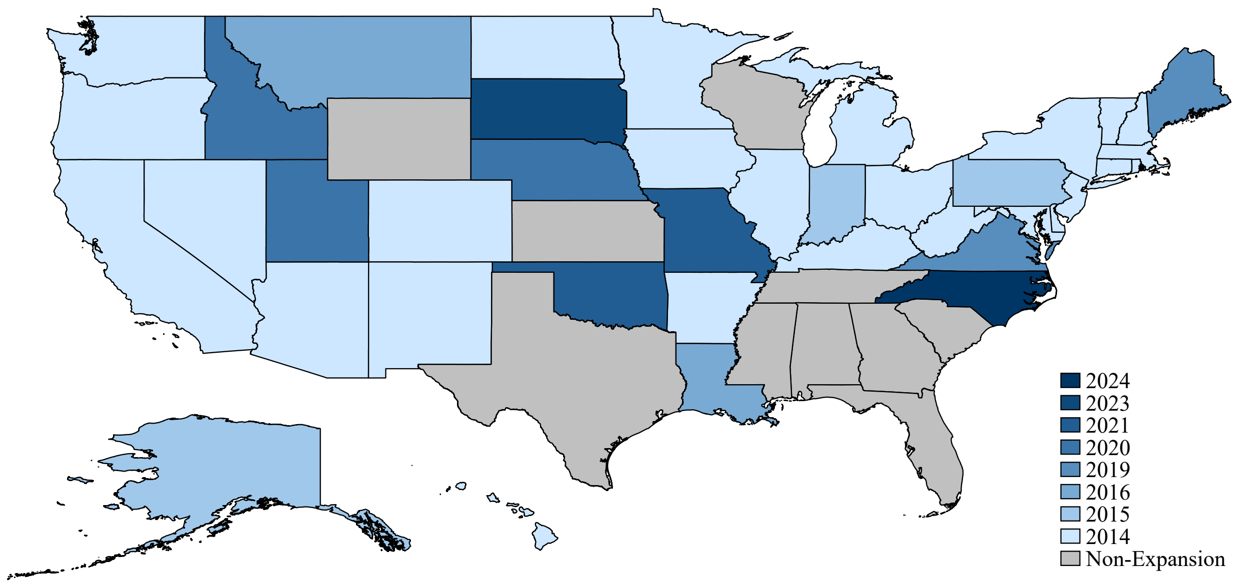
No.	Element	Preregistered	Reported in Paper	Deviation?	Disclosed?	Severity	Notes
19	Covariates — macroeconomic: per capita income	Log per capita income from Bureau of Economic Analysis (BEA)	Sourced from FRED (same underlying BEA series)	Partial	Yes	Minor	Same disclosure as above; data identical in substance
20	Covariates — macroeconomic: house price index	Median house price index from Federal Housing Finance Agency (FHFA)	Annual mean of state's quarterly HPI used instead of median	Partial	Yes	Minor	Paper explicitly discloses: "we precommitted to using a state's median housing price index; in the present analysis, we instead use the annual mean across quarters." Mean vs. median is a small but real methodological change.
21	COVID-19 controls — health shocks	Cumulative COVID-19 cases and deaths per capita interacted with time dummies for 2020 and for 2021–2022 combined; New York Times as data source	Implemented; deaths from CDC Wonder, cases from Hopkins CSSE; interactions with post-COVID indicators	Partial	Yes	Minor	Data source changed from NYT to CDC/Hopkins (likely due to NYT discontinuing COVID data); structure of controls matches precommitment. Paper does not call explicit attention to the source switch but it is visible in Section 2.
22	COVID-19 controls — employment shocks	Each state's peak employment decline (percent terms) interacted with time dummies; called "peak employment decline"	Implemented using percent change in state employment from 2019 Q2 to 2020 Q2 from QCEW	No	N/A	None	Functionally consistent; "2019 Q2 to 2020 Q2" captures the peak decline period referenced in prereg
23	Formal inference — coefficient equality tests	Test equality of coefficients for each of calendar years 2020, 2021, and 2022 relative to 2018 and 2019	Paper focuses formal tests on equality of 2019 vs. 2022 coefficients only, rather than testing each of 2020–2022 against both 2018 and 2019	Yes	Yes	Minor	Paper explicitly discloses in footnote 7: "our pre-analysis plan outlined separate tests for the equality for coefficients from each calendar year 2020–2022 relative to 2018 and 2019, we focus the formally presented analysis on the equality of the 2019 and 2022 coefficients for the sake of brevity." Rationale is sound (2019 vs. 2022 is the strongest test), and underlying estimates for all years appear in event study figures.
24	Industry heterogeneity analysis — NAICS level	Estimate event-study equations separately on each 3-digit and 4-digit NAICS industry code	Only 3-digit NAICS analysis conducted; 4-digit analysis omitted	Yes	Yes	Minor	Paper explicitly discloses: "we precommitted to conducting analyses at both the three- and four-digit NAICS levels" but omits 4-digit due to NAICS reclassification inconsistencies across years 2012–2022. Methodological justification is sound.

Table A.5 (Cont.)

	Element	Preregistered	Reported in Paper	Deviation?	Disclosed?	Severity	Notes
25	Industry heterogeneity analysis — churn measure	Use QCEW employment data to measure industry-specific churn; compare change in ACA effects between 2019 and later years against industry churn	Implemented; churn measured as % change in industry employment 2019 Q2 to 2020 Q2 from QCEW	No	N/A	None	Exact match
26	Industry heterogeneity — control for average wages	Control for or match on each industry's average earnings in churn analysis	Implemented; industry average weekly wages included as control in sensitivity checks (Appendix Tables C.7, C.8)	No	N/A	None	Exact match; paper confirms "The inclusion of wages in the estimation was prespecified in our pre-analysis plan"
27	Industry heterogeneity — precision weighting	Not pre-specified	Paper adds precision-weighted regressions (weighting by inverse squared standard error of 2019–2022 difference) in appendix	No	Yes	None	Correctly identified by the paper as an extension beyond the pre-analysis plan
28	CBSA construction — baseline uninsurance rates	Construct CBSA-level baseline (2013) uninsurance rates using ACS; reference to 20.5% as sample mean	Implemented; mean reported as 21.2% in paper (minor shift due to data/sample refinements)	No	N/A	None	Negligible numerical difference attributable to IPUMS vs. CEPR source; approach identical
29	Stacked event study for late-expanding states	Not pre-specified	Paper adds stacked event study (Equation 5) incorporating late-expanding states; explicitly flagged as outside pre-committed analysis	No	Yes	None	Clearly identified as an extension throughout
30	Pre-implementation parallel trends check	Confirm pre-implementation event-study coefficients are economically and statistically indistinguishable from 0	Conducted; pre-trends verified in all event study specifications (Figures 2–10)	No	N/A	None	Exact match
31	Robustness to covariates	Investigate robustness to inclusion of plausibly relevant covariates	Implemented across specifications of varying saturation (Columns 1–5 in Tables 1–3; Appendix Figures B.5–B.6)	No	N/A	None	Exact match
32	Crowd-out rate calculations and employer mandate penalty analysis	Not pre-specified	Paper adds Section 4.3 contextualizing employer coverage magnitudes via OMB employer mandate penalty data and implied crowd-out rate table (Table 4)	No	Yes	None	Clearly flagged as "not pre-specified" in Section 4.3
33	LLM audit of adherence	Not pre-specified	Paper adds Appendix A presenting an LLM audit of pre-analysis plan adherence	No	Yes	None	Novel methodological contribution; not a deviation

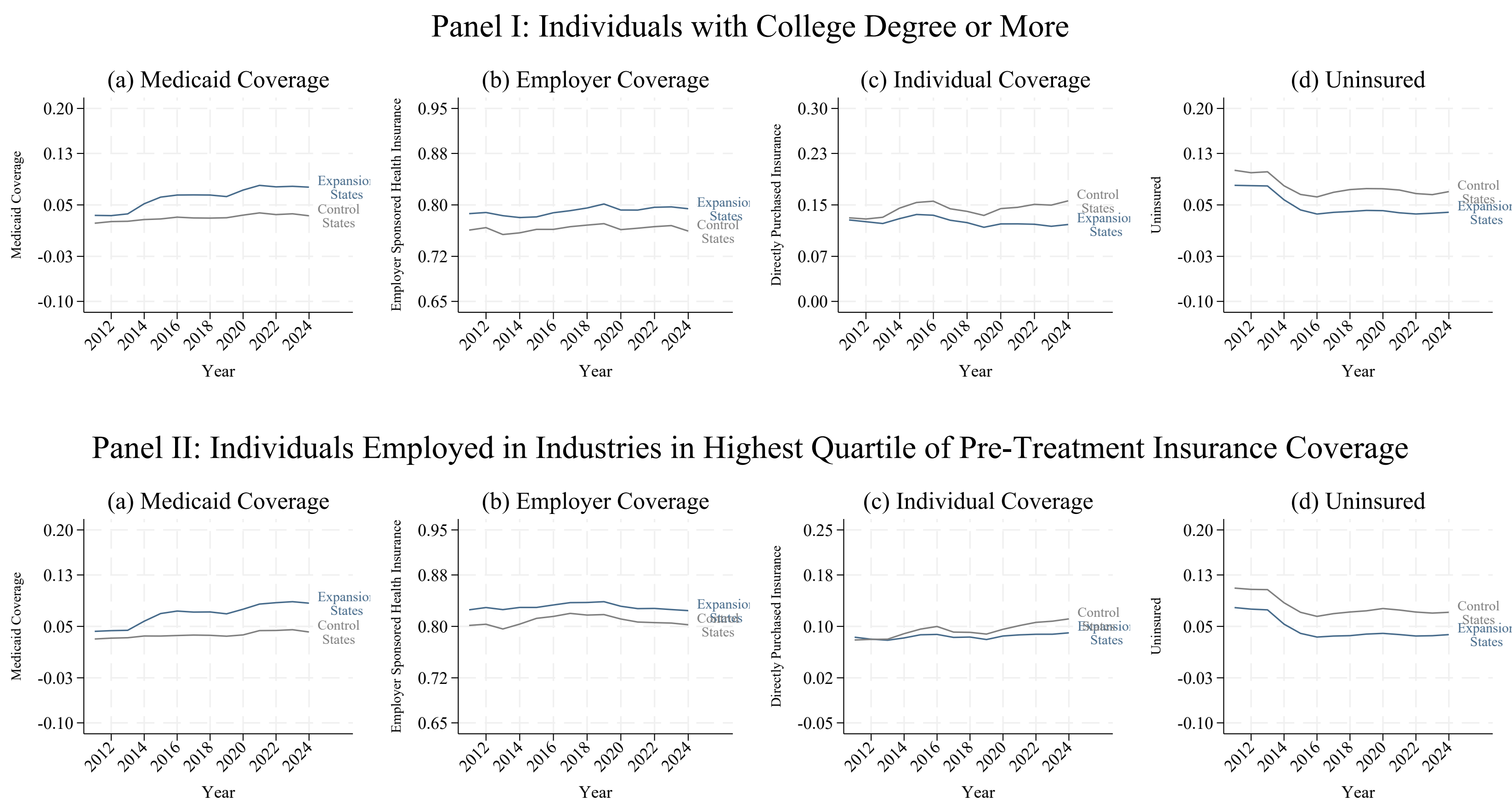
Appendix B Figures

Figure B.1: Affordable Care Act State Medicaid Expansions (2011-2024)



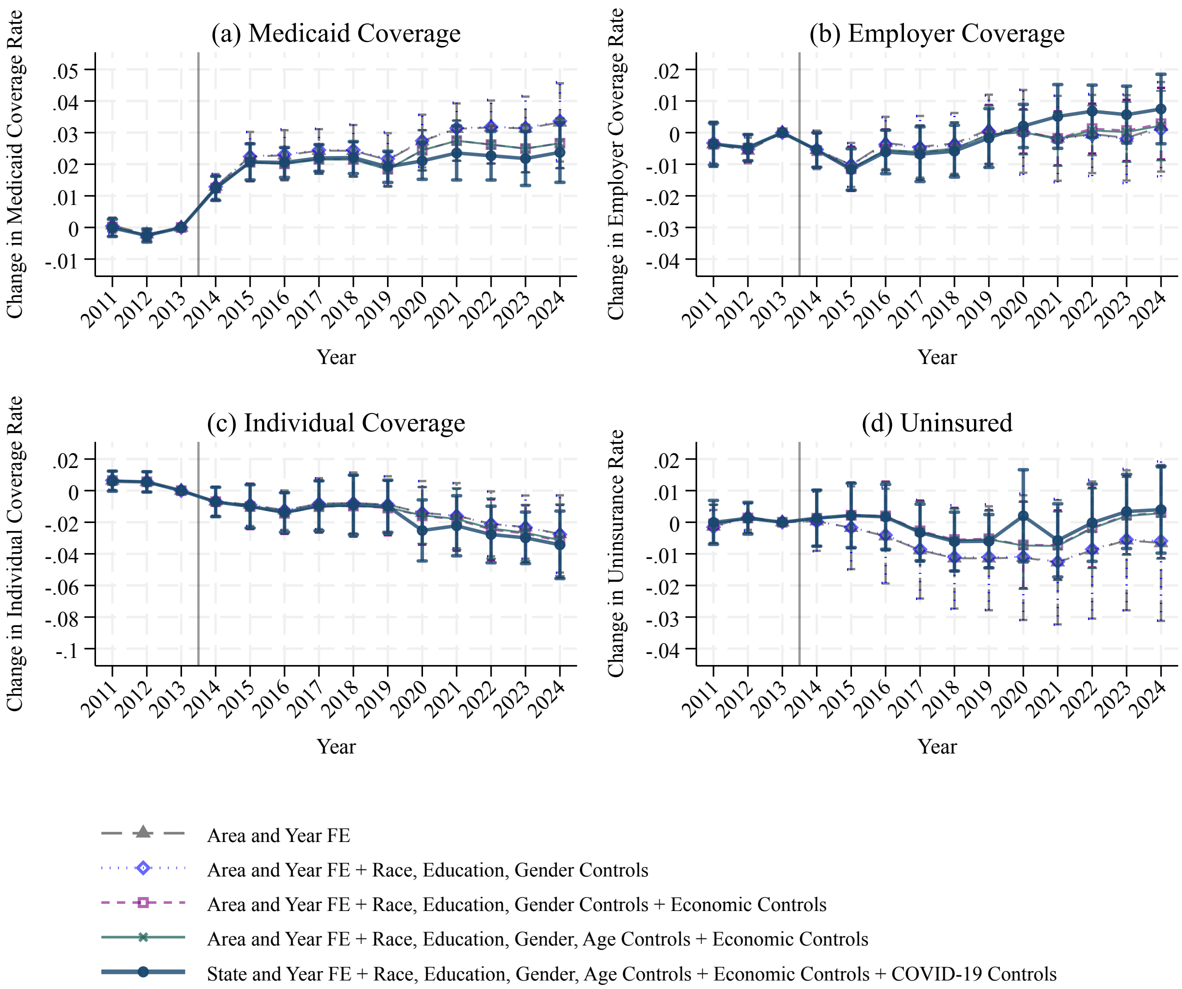
Notes: This figure illustrates the roll-out of ACA state Medicaid expansions from 2011 to 2024. Data for Medicaid expansion dates by state are from the Kaiser Family Foundation. North Carolina implemented Medicaid expansion in December 2023; because this occurred at the very end of the year, we treat the expansion date as 2024 in our analyses.

Figure B.2: Expansion vs. Non-Expansion States, 2011-2024



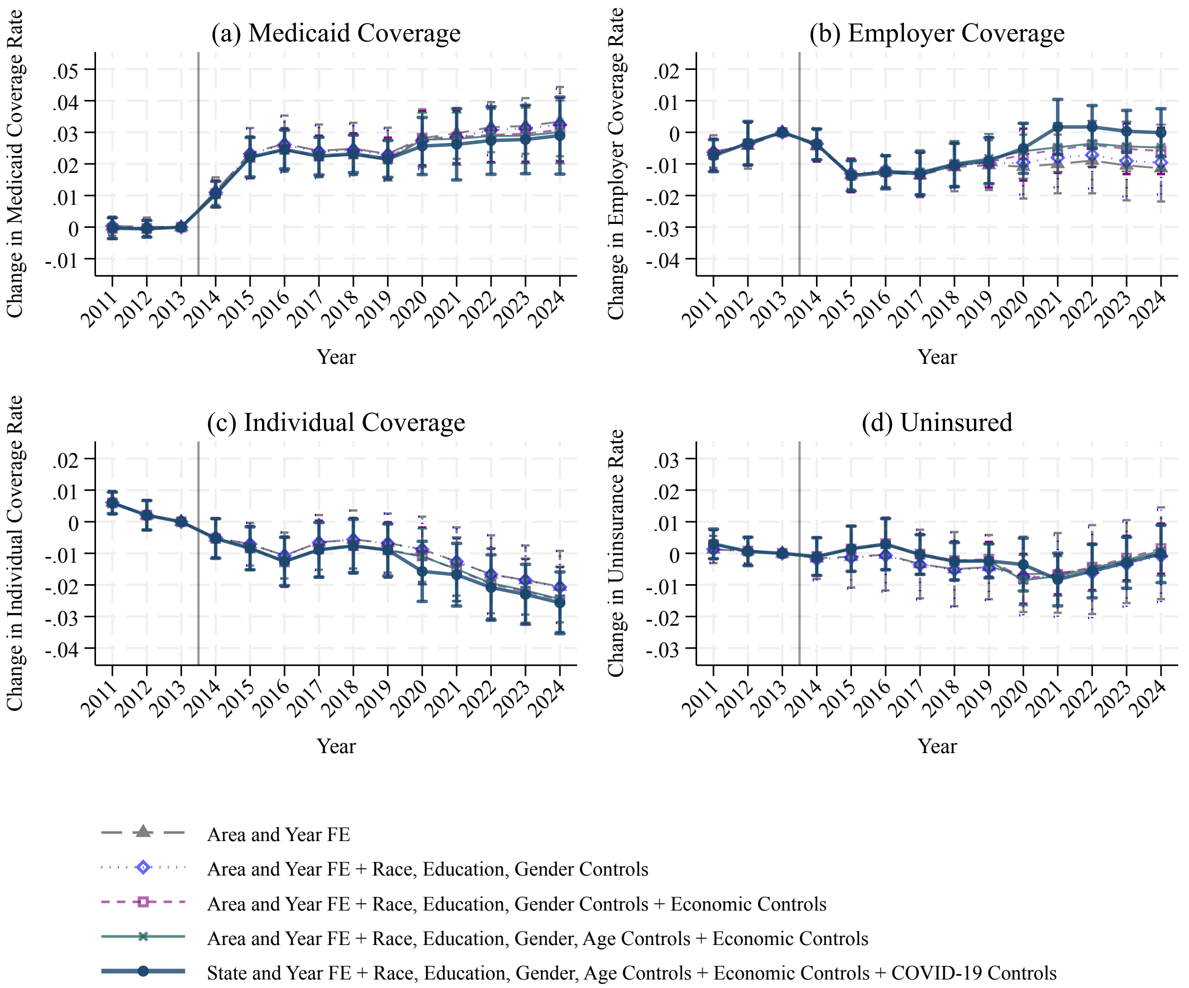
Notes: This figure illustrates the coverage rates for different types of insurance among individuals with a college degree or more (panel I), and among individuals associated with industries in the highest quartile of employer coverage rates between 2011 and 2013 (panel II). The blue line is the coverage rate among expansion states, while the gray line is the coverage rate among control states. Expansion states are those which expanded Medicaid in 2014. Control states are those that did not expand Medicaid by the end of 2022. The main data source is the American Community Survey.

Figure B.3: Pre-Committed Event Study Design, 2014 Medicaid Expansion States and Health Insurance Coverage, Individuals with a College Degree or Higher



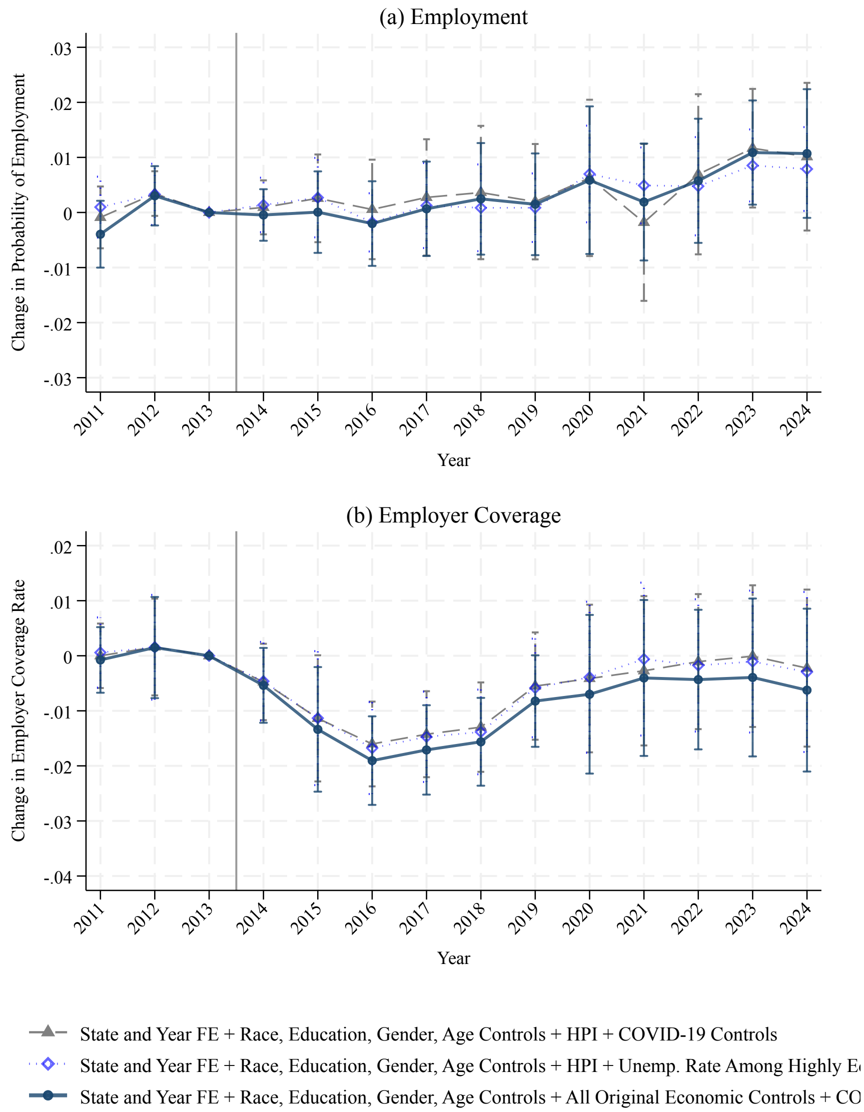
Notes: This figure presents weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. The sample includes individuals with a college degree or higher. Vertical lines represent 95 percent confidence intervals. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure B.4: Pre-Committed Event Study Design, 2014 Medicaid Expansion States and Health Insurance Coverage, Individuals in High Pre-Treatment Insurance Coverage Industries



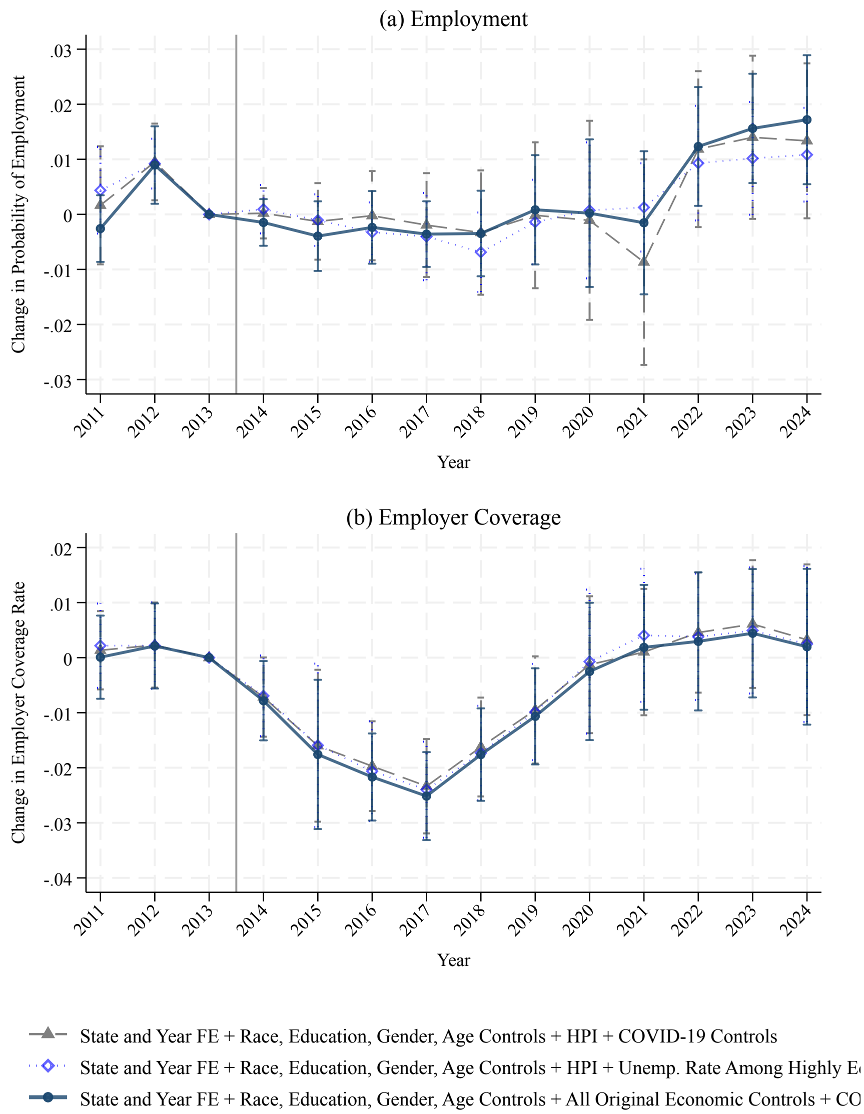
Notes: This figure presents weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. Vertical lines represent 95 percent confidence intervals. The sample includes individuals associated with industries in the highest quartile of mean employer-sponsored health insurance between 2011 and 2013. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.

Figure B.5: Sensitivity of DD Event Study Estimates of the ACA's Impacts on Employment and Employer Coverage to the Inclusion of State Macroeconomic Controls, Individuals With a High School Degree or Less



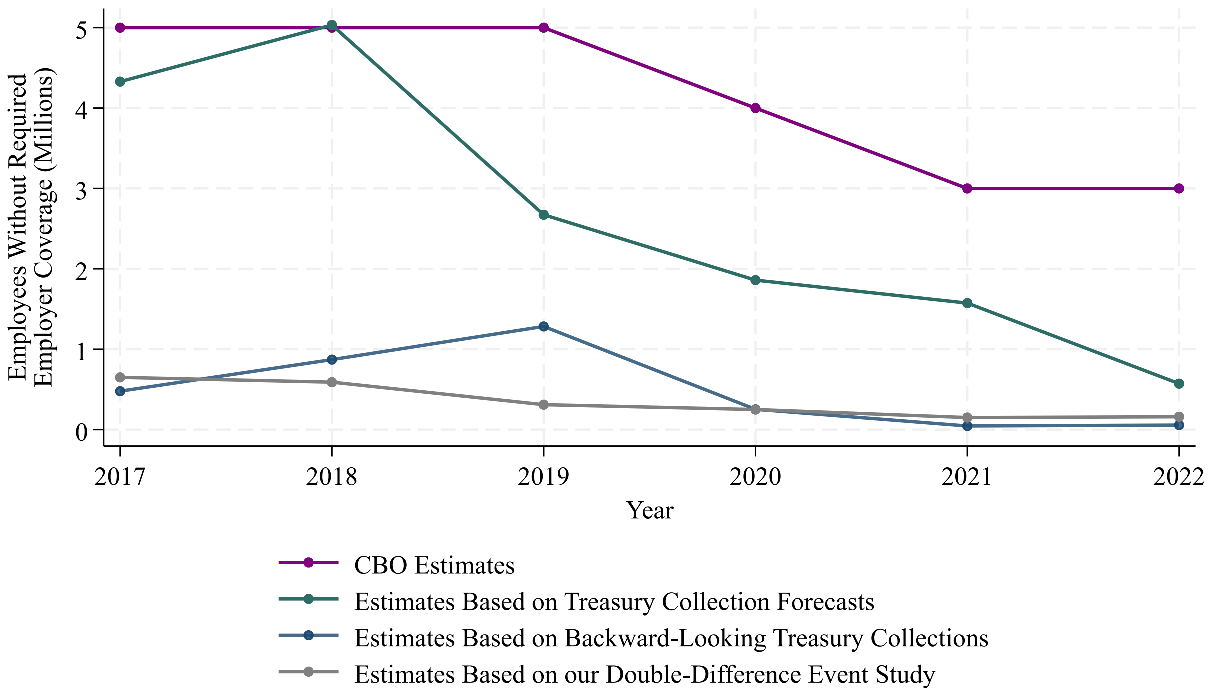
Notes: This figure presents weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. The outcome in panel (a) is the employment status, and the outcome in panel (b) is employer-sponsored insurance coverage. Vertical lines represent 95 percent confidence intervals. The sample includes individuals with no more than a high school degree. Standard errors are clustered at the state level. All specifications include state and year fixed effects, demographic controls including race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree), gender and age, and COVID-19 controls. The specifications differ in the set of economic controls included. The first specification includes only the state-level housing price index. The second adds the housing price index and the state-level unemployment rate among individuals with some college education or higher. The third incorporates the full set of baseline economic controls, including state-level per capita income, the overall state-level unemployment rate, and the housing price index.

Figure B.6: Sensitivity of DD Event Study Estimates of the ACA's Impacts on Employment and Employer Coverage to the Inclusion of State Macroeconomic Controls, Individuals in Low Pre-Treatment Insurance Coverage Industries



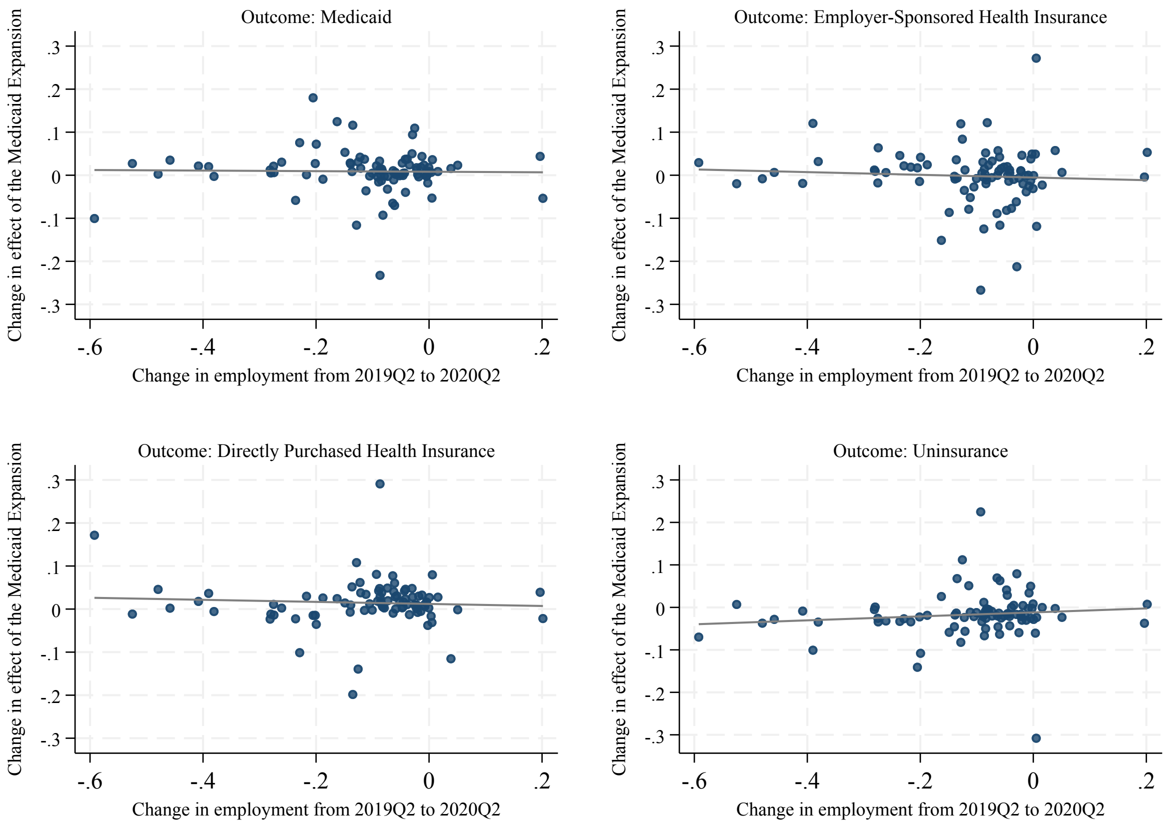
Notes: This figure presents weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. The outcome in panel (a) is the employment status, and the outcome in panel (b) is employer-sponsored insurance coverage. Vertical lines represent 95 percent confidence intervals. The sample includes individuals associated with industries in the lowest quartile of mean employer-sponsored health insurance between 2011 and 2013. Standard errors are clustered at the state level. All specifications include state and year fixed effects, demographic controls including race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree), gender and age, and COVID-19 controls. The specifications differ in the set of economic controls included. The first specification includes only the state-level housing price index. The second adds the housing price index and the state-level unemployment rate among individuals with some college education or higher. The third incorporates the full set of baseline economic controls, including state-level per capita income, the overall state-level unemployment rate, and the housing price index.

Figure B.7: Estimates of the ACA's Impacts on Employer Coverage



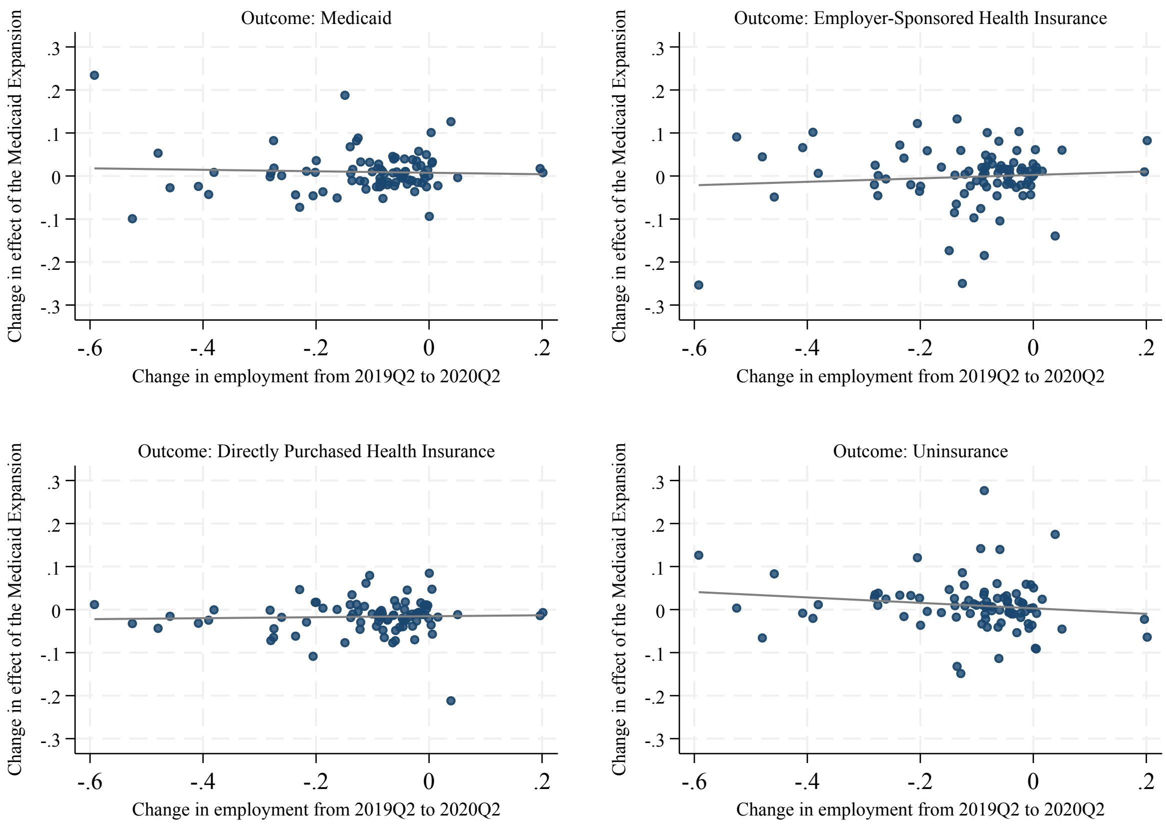
Notes: This figure presents estimates of the number of employees without required employer coverage following the implementation of the ACA. A first set of estimates (purple) comes directly from the CBO's forecasts (Harvey, Hearne, and Anders, 2012). Two additional series (green and blue) are derived from our calculations using Treasury data obtained from Office of Management and Budget (N.d.). For each year, we use the Public Budget Database tables reporting receipts from "Penalties on Employers Who Do Not Offer Health Coverage or Delay Eligibility for New Employees." We first use a forecast of Treasury collections (green) from the year prior to the reference year and divide it by the average penalty per employee ($3,000) to obtain a projected estimate of the number of affected employees. Next, we use Treasury collection figures reported in the year after the reference year (blue), again dividing by $3,000 to obtain a plausible estimate of the realized number of affected employees. Finally, we use our own difference-in-differences estimates from Equation 2, applied to the population of individuals with a high school degree or less, to estimate the number of employees losing employer coverage (gray). Treasury collections likely reflect employees without employer coverage more broadly, rather than employees who lost employer coverage specifically due to the ACA, as captured by the CBO forecasts and our difference-in-differences estimates. A detailed discussion of these estimates and our methodology is provided in Section 4.3.

Figure B.8: Relationship Between Industry Churn and the Change in the Full Effect of the ACA on Insurance from 2019 to 2022



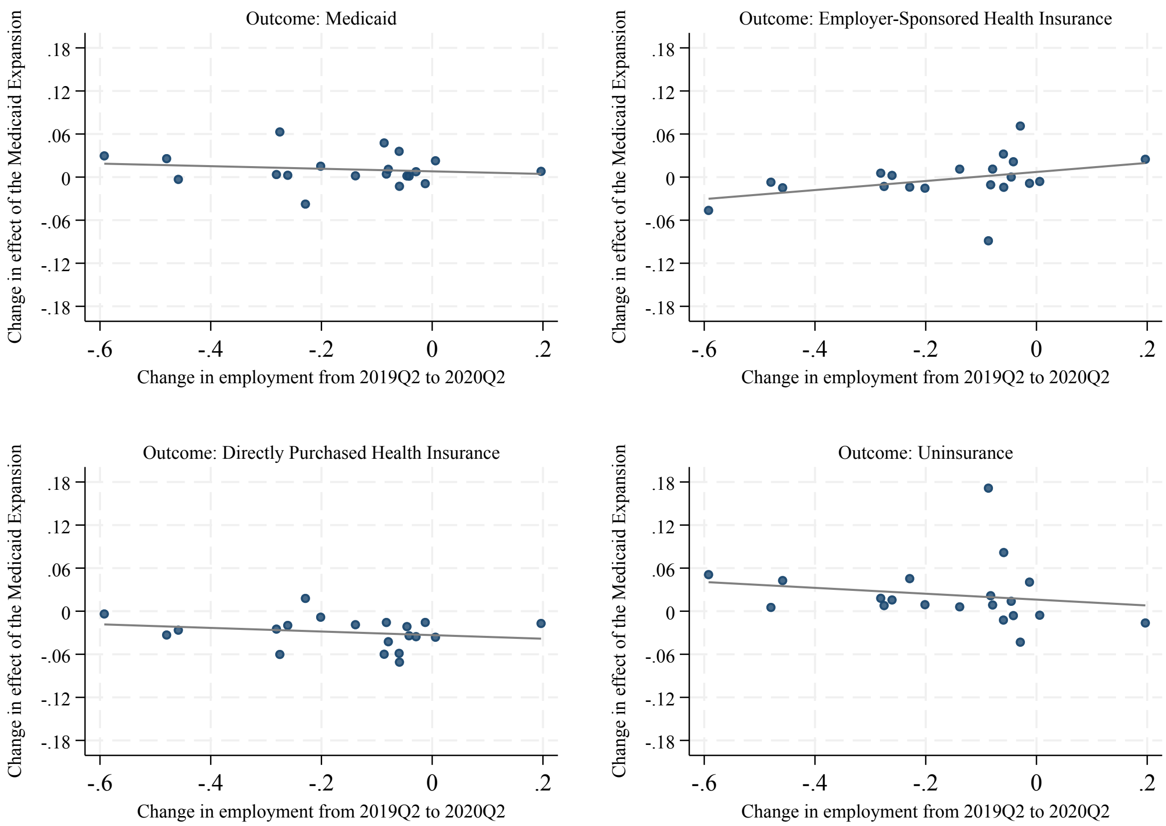
Notes: This figure presents the relationship between the change in the implied full effect of the ACA from 2019 to 2022 at the mean pre-treatment uninsured rate of 21.2 percent within an industry and churn in the industry. Industries are categorized at the 3-digit level of the NAICS codes. Churn, represented on the x-axis, is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The coefficients plotted on the y-axis are derived as follows. Equation (3) is estimated by industry for the subpopulation of employed individuals and linear combinations of coefficients are calculated to derive the effects of all of the ACA's provisions as described in the main text. A difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. Each dot thus represents the change from 2019 to 2022 of the full implied effect of the ACA for an industry. The specification used to derive the coefficients includes CBSA and year fixed effects, controls for race, gender, education, age, state-level unemployment rate, per-capita income and housing price index, and controls that capture a state's exposure to the COVID-19 pandemic, with standard errors clustered at the state level.

Figure B.9: Relationship Between Industry Churn and the Change in the Effect of the Medicaid Expansion on Insurance from 2019 to 2022 (Excluding Covariates for COVID-19 Exposure): Individuals with a High School Degree or Less



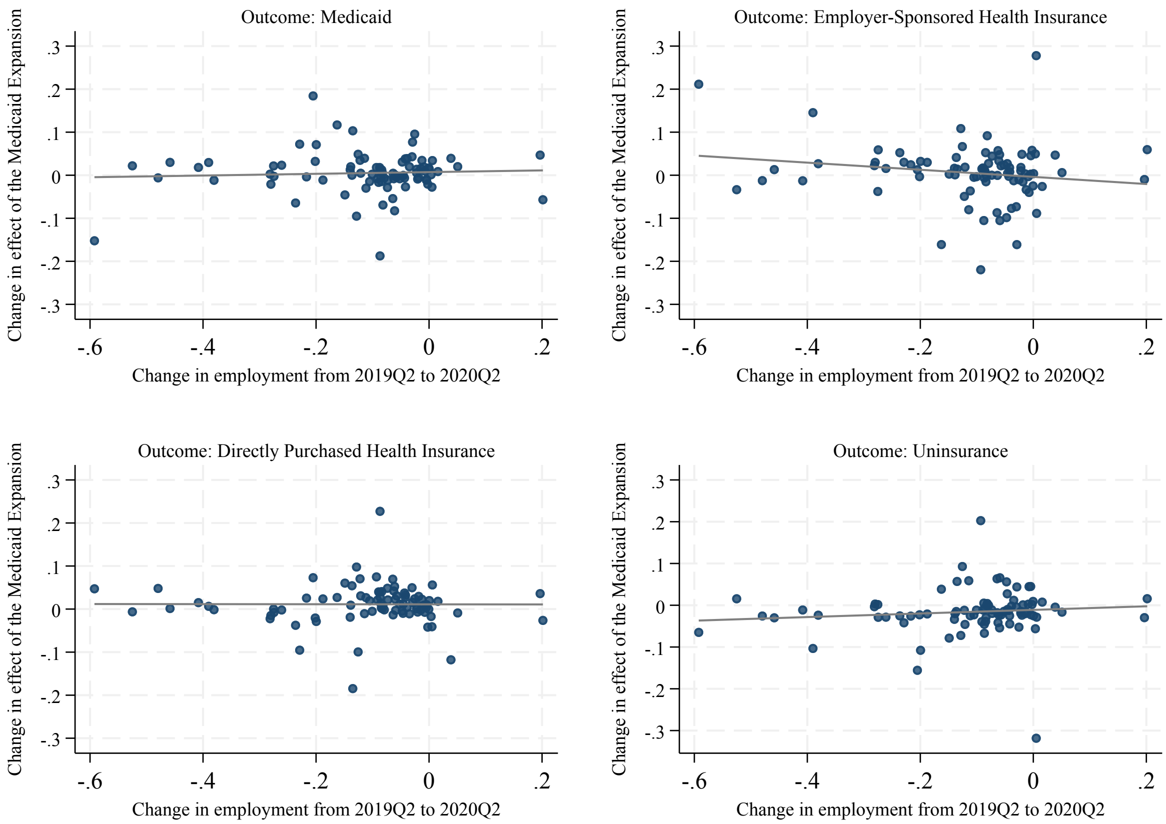
Notes: This figure presents the relationship between the change in the effect of the Medicaid expansion from 2019 to 2022 within an industry among individuals with low education and churn in the industry in which they are employed. Industries are categorized at the 3-digit level of the NAICS codes. Churn, represented on the x-axis, is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The coefficients plotted on the y-axis are derived as follows. Equation (2) is estimated by industry for the subpopulation of employed individuals with a high-school degree or less and a difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. Each dot thus represents the change from 2019 to 2022 of the effect of the Medicaid expansion for an industry. The specification used to derive the coefficients includes state and year fixed effects and controls for race, gender, education, age and state-level unemployment rate, per-capita income and housing price index, with standard errors clustered at the state level.

Figure B.10: Relationship Between Industry Churn and the Change in the Effect of the Medicaid Expansion on Insurance from 2019 to 2022 (Excluding Covariates for COVID-19 Exposure): Individuals in Industries with Low Baseline Employer Coverage



Notes: This figure presents the relationship between the change in the effect of the Medicaid expansion from 2019 to 2022 within an industry among individuals associated with industries in the lowest quartile of pre-treatment insurance coverage and churn in the industry. Industries are categorized at the 3-digit level of the NAICS codes. Churn, represented on the x-axis, is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The coefficients plotted on the y-axis are derived as follows. Equation (2) is estimated by industry for the subpopulation of employed individuals associated with industries in the lowest quartile of pre-treatment insurance coverage and a difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. Each dot thus represents the change from 2019 to 2022 of the effect of the Medicaid expansion for an industry. The specification used to derive the coefficients includes CBSA and year fixed effects, controls for race, gender, education, age, state-level unemployment rate, per-capita income and housing price index, with standard errors clustered at the state level.

Figure B.11: Relationship Between Industry Churn and the Change in the Full Effect of the ACA on Insurance from 2019 to 2022 (Excluding Covariates for COVID-19 Exposure)



Notes: This figure presents the relationship between the change in the implied full effect of the ACA from 2019 to 2022 at the mean pre-treatment uninsured rate of 21.2 percent within an industry and churn in the industry. Industries are categorized at the 3-digit level of the NAICS codes. Churn, represented on the x-axis, is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The coefficients plotted on the y-axis are derived as follows. Equation (3) is estimated by industry for the subpopulation of employed individuals and linear combinations of coefficients are calculated to derive the effects of all of the ACA's provisions as described in the main text. A difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. Each dot thus represents the change from 2019 to 2022 of the full implied effect of the ACA for an industry. The specification used to derive the coefficients includes CBSA and year fixed effects, controls for race, gender, education, age, state-level unemployment rate, per-capita income and housing price index, with standard errors clustered at the state level.

Appendix C : Tables

Table C.1: Difference between the 2019 and 2022 Event Study Estimates of the Effect of the Medicaid Expansion on Health Insurance Coverage, Individuals With No More than a Completed High School Education

	(1)	(2)	(3)	(4)	(5)
	Panel I: Medicaid Coverage				
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$ $(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	0.011***	0.009***	0.004	0.004	0.003
	(0.003)	(0.003)	(0.006)	(0.006)	(0.008)
	Panel II: Employer Coverage				
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$ $(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	-0.011***	-0.008***	-0.003	-0.003	0.004
	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)
	Panel III: Individual Coverage				
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$ $(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	-0.014***	-0.014***	-0.016***	-0.015***	-0.020***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
	Panel IV: Uninsured				
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$ $(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	0.012**	0.011**	0.012**	0.012**	0.010
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
	Panel V: Employed				
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$ $(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	-0.010**	-0.008**	-0.001	-0.002	0.004
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	7,788,266	7,788,266	7,788,266	7,788,266	7,788,266
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race		Y	Y	Y	Y
Economic Controls			Y	Y	Y
Age				Y	Y
COVID-19 Controls					Y

Notes: This table presents the difference between the 2019 and 2022 weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. The observation count reflects the full sample used in the baseline event study regression. The sample includes individuals with no more than a high school degree. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.2: Difference between the 2019 and 2022 Event Study Estimates of the Effect of the Medicaid Expansion on Health Insurance Coverage, Individuals in Low Pre-Treatment Insurance Industries

	(1)	(2)	(3)	(4)	(5)
			Panel I: Medicaid Coverage		
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$	0.010***	0.008**	0.003	0.003	0.001
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	(0.004)	(0.004)	(0.006)	(0.006)	(0.008)
			Panel II: Employer Coverage		
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$	-0.007*	-0.004	0.002	0.002	0.014**
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
			Panel III: Individual Coverage		
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$	-0.021***	-0.020***	-0.022***	-0.022***	-0.026***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
			Panel IV: Uninsured		
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$	0.016***	0.014***	0.015**	0.014**	0.010
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	(0.004)	(0.004)	(0.006)	(0.006)	(0.0079)
			Panel V: Employed		
$\mathbb{1}\{\text{Medicaid Expansion}\}_s \times$	-0.005	-0.004	0.004	0.004	0.011***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t)$	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	5,380,176	5,380,176	5,380,176	5,380,176	5,380,176
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race		Y	Y	Y	Y
Economic Controls			Y	Y	Y
Age				Y	Y
COVID-19 Controls					Y

Notes: This table presents the difference between the 2019 and 2022 weighted least squares estimates of equation 2 generated using data from the 2011-2024 American Community Survey. The observation count reflects the full sample used in the baseline event study regression. The sample includes individuals employed in industries in the lowest quartile of mean employer-sponsored health insurance between 2011 and 2013. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.3: Difference between the 2019 and 2022 DDD Event Study Estimates of the Implied Effect of the Medicaid Expansion

	(1)	(2)	(3)	(4)	(5)
	Panel I: Medicaid Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	-0.002	-0.002	-0.002	-0.002	0.002
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.010)	(0.009)	(0.008)	(0.008)	(0.007)
	Panel II: Employer Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.027**	0.027**	0.025**	0.025**	0.018
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.013)	(0.011)	(0.010)	(0.010)	(0.011)
	Panel III: Individual Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	-0.015**	-0.015**	-0.016**	-0.016**	-0.015**
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
	Panel IV: Uninsured				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	-0.001	-0.002	0.002	0.002	0.003
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
	Panel V: Employed				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.009	0.009	0.005	0.005	0.001
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	22,189,045	22,189,045	22,189,045	22,189,045	22,189,045
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race		Y	Y	Y	Y
Economic Controls			Y	Y	Y
Age				Y	Y
COVID-19 Controls					Y

Notes: This table presents the difference between the 2019 and 2022 weighted least squares estimates of the effect of the Medicaid expansion from equation 4 generated using data from the 2011-2024 American Community Survey. The observation count reflects the full sample used in the baseline event study regression. The sample includes individuals with no more than a high school degree. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.4: Difference between the 2019 and 2022 DDD Event Study Estimates of the Implied Effects of the ACA Without Medicaid Expansion

	(1)	(2)	(3)	(4)	(5)
	Panel I: Medicaid Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.007	0.009	0.013**	0.013**	0.015***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
	Panel II: Employer Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	-0.009	-0.014	-0.016**	-0.017**	-0.017**
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	0.010	(0.008)	(0.008)	(0.008)	(0.008)
	Panel III: Individual Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.023***	0.023***	0.025***	0.025***	0.027***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
	Panel IV: Uninsured				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	-0.020***	-0.018***	-0.021***	-0.021***	-0.025***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
	Panel V: Employed				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.001	-0.002	-0.001	<0.001	-0.001
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.004)	(0.004)	(0.006)	(0.005)	(0.006)
Observations	22,189,045	22,189,045	22,189,045	22,189,045	22,189,045
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race		Y	Y	Y	Y
Economic Controls			Y	Y	Y
Age				Y	Y
COVID-19 Controls					Y

Notes: This table presents the difference between the 2019 and 2022 weighted least squares estimates of the full effect of the ACA from equation 4 generated using data from the 2011-2024 American Community Survey. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.5: Difference between the 2019 and 2022 DDD Event Study Estimates of the Implied Full Effect of the ACA

	(1)	(2)	(3)	(4)	(5)
	Panel I: Medicaid Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.004	0.007	0.011**	0.011**	0.017***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)
	Panel II: Employer Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.018**	0.013**	0.008	0.008	0.001
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.009)	(0.007)	(0.006)	(0.006)	(0.007)
	Panel III: Individual Coverage				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.008***	0.008***	0.009***	0.009***	0.012***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
	Panel IV: Uninsured				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	-0.022***	-0.020***	-0.020***	-0.019***	-0.022***
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
	Panel V: Employed				
$\mathbb{1}\{\text{Medicaid Exp.}\}_s \times \{\text{Unins.}\}_{a,s} \times$	0.011	0.007	0.004	0.004	<0.001
$(\mathbb{1}\{2022\}_t - \mathbb{1}\{2019\}_t) \times \overline{Unins.}_{a,s}$	(0.007)	(0.005)	(0.004)	(0.004)	(0.004)
Observations	22,189,045	22,189,045	22,189,045	22,189,045	22,189,045
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race		Y	Y	Y	Y
Economic Controls			Y	Y	Y
Age				Y	Y
COVID-19 Controls					Y

Notes: This table presents the difference between the 2019 and 2022 weighted least squares estimates of the full effect of the ACA from equation 4 generated using data from the 2011-2024 American Community Survey. Standard errors are clustered at the state level. All specifications include state and year fixed effects. Demographic controls include race (non-Hispanic white, black and Hispanic), education (less than high school, high school degree, some college, and college degree), gender and age. Economic controls are state-level per-capita income, state-level unemployment rate, and state-level housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2020, each interacted with a 2020 year dummy, the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a 2020 year dummy and a dummy for "year after 2020" each.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.6: Summary of the Estimates of the Effect of the ACA on Employment

		(1) DD ES: Low Educ.	(2) DD ES: Low Cov.	(3) DDD ES: Med. Exp.	(4) DDD ES: Full ACA		(5) DD ES: Low Educ.	(6) DD ES: Low Cov.	(7) DDD ES: Med. Exp.	(8) DDD ES: Full ACA
	Most negative 2022 coefficient					Most negative change (2019-2022) coefficient				
(a)	2022 coefficient for employment	-0.010	-0.007	-0.005	0.004	Coefficient for change (2019-2022) in employment	-0.010	-0.005	0.001	0.0003
(b)	Associated 2022 CI LB for employment	-0.027	-0.020	-0.016	-0.004	Associated CI LB for change (2019-2022) in employment	-0.018	-0.013	-0.013	-0.007
	Average 2022 coefficient					Average change (2019-2022) coefficient				
(c)	2022 coefficient for employment	-0.002	0.002	0.001	0.013	Coefficient for change (2019-2022) in employment	-0.003	0.002	0.006	0.005
(d)	2022 CI LB for employment	-0.016	-0.009	-0.011	0.005	CI LB for change (2019-2022) in employment	-0.011	-0.006	-0.009	-0.004

Notes: This table reports event-study estimates of effects on the probability of employment, focusing on both point estimates and confidence interval lower bounds (CI LB) for the 2022 coefficient and the change from 2019–2022, including the most negative 2022 estimate and averages across specifications. The estimates are drawn from panel (a) of Figure 4 (column 1), panel (b) of Figure 4 (column 2), panel (a) of Figure 10 (column 3), panel (c) of Figure 10 (column 4), as well as panel V of Table C.1 (column 5), Table C.2 (column 6), Table C.3 (column 7), and Table C.5 (column 8).

Table C.7: Relationship Between Industry Churn and the Effect of the Medicaid Expansion on Insurance from 2019 to 2022: Individuals with a High School Degree or Less

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medicaid				Employer-Sponsored Health Insurance			
Percent change in employment from 2019Q2 to 2020 Q2	-0.062	0.006	-0.089**	-0.012	0.025	0.039	0.038	0.037
	(0.039)	(0.026)	(0.039)	(0.027)	(0.053)	(0.031)	(0.055)	(0.034)
Industry average weekly wage in 2019 Q2	N	N	Y	Y	N	N	Y	Y
Precision weights	N	Y	N	Y	N	Y	N	Y
Obs	87	87	87	87	87	87	87	87
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Directly Purchased Health Insurance				Uninsurance			
Percent change in employment from 2019Q2 to 2020 Q2	0.042	0.031	0.026	0.020	-0.059	-0.043	-0.030	-0.013
	(0.037)	(0.022)	(0.038)	(0.022)	(0.047)	(0.029)	(0.048)	(0.030)
Industry average weekly wage in 2019 Q2	N	N	Y	Y	N	N	Y	Y
Precision weights	N	Y	N	Y	N	Y	N	Y
Obs	87	87	87	87	87	87	87	87

Notes: This table present the coefficients from regressing the change in the effect of the Medicaid expansion from 2019 to 2022 within an industry among individuals with low education on churn in the industry in which they are employed. Industries are categorized at the 3-digit level of the NAICS codes. Churn is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The dependent variable is derived as follows. Equation (2) is estimated by industry for the subpopulation of employed individuals with a high-school degree or less and a difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. The specifications used to derive the coefficients include state and year fixed effects and controls for race, gender, education, age and state-level unemployment rate, per-capita income and housing price index, with standard errors clustered at the state level. Columns (1), (5), (9) and (13) mirror the relationship visible in Figure 11. Specifications in the even-numbered columns employ precision weights where the weight is the inverse of the square of the standard error associated with the calculation of the industry-level estimate derived as described above. Specifications in the columns (3),(4),(7),(8), (11),(12), (15) and (16) control for the average weekly wage in the industry in Q2 of 2019, derived from the QCEW.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.8: Relationship Between Industry Churn and the Effect of the Medicaid Expansion on Insurance from 2019 to 2022: Individuals in Low Pre-Treatment Insurance Coverage Industries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medicaid				Employer-Sponsored Health Insurance			
Percent change in employment from 2019Q2 to 2020 Q2	-0.007	-0.025	-0.007	-0.026	0.031	0.062**	0.027	0.064**
	(0.025)	(0.023)	(0.026)	(0.024)	(0.038)	(0.026)	(0.037)	(0.028)
Industry average weekly wage in 2019 Q2	N	N	Y	Y	N	N	Y	Y
Precision weights	N	Y	N	Y	N	Y	N	Y
Obs	20	20	20	20	20	20	20	20

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Directly Purchased Health Insurance				Uninsurance			
Percent change in employment from 2019Q2 to 2020 Q2	0.001	-0.001	0.002	<0.001	-0.052	-0.044*	-0.048	-0.044*
	(0.029)	(0.026)	(0.030)	(0.029)	(0.050)	(0.024)	(0.050)	(0.025)
Industry average weekly wage in 2019 Q2	N	N	Y	Y	N	N	Y	Y
Precision weights	N	Y	N	Y	N	Y	N	Y
Obs	20	20	20	20	20	20	20	20

Notes: This table present the coefficients from regressing the change in the effect of the Medicaid expansion from 2019 to 2022 within an industry among individuals in low pre-treatment insurance coverage industries on churn in the industry in which they are employed. Industries are categorized at the 3-digit level of the NAICS codes. Churn is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The dependent variable is derived as follows. Equation (2) is estimated by industry for the subpopulation of employed individuals low pre-treatment insurance coverage industries and a difference between the 2022 coefficient and the 2019 coefficient for each industry is calculated. Columns (1), (5), (9) and (13) mirror the relationship visible in Figure 12. The specifications used to derive the coefficients include state and year fixed effects and controls for race, gender, education, age and state-level unemployment rate, per-capita income and housing price index, with standard errors clustered at the state level. Specifications in the even-numbered columns employ precision weights where the weight is the inverse of the square of the standard error associated with the calculation of the industry-level estimate derived as described above. Specifications in the columns (3),(4),(7),(8), (11),(12), (15) and (16) control for the average weekly wage in the industry in Q2 of 2019, derived from the QCEW.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.9: Relationship Between Industry Churn and the Change in the Full Effect of the ACA on Insurance from 2019 to 2022

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medicaid				Employer-Sponsored Health Insurance			
Percent change in employment from 2019Q2 to 2020 Q2	-0.007 (0.040)	-0.014 (0.023)	0.004 (0.042)	-0.015 (0.025)	-0.031 (0.053)	-0.018 (0.027)	-0.061 (0.054)	-0.017 (0.030)
Industry average weekly wage in 2019 Q2	N	N	Y	Y	N	N	Y	Y
Precision weights	N	Y	N	Y	N	Y	N	Y
Obs	87	87	87	87	87	87	87	87
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Directly Purchased Health Insurance				Uninsurance			
Percent change in employment from 2019Q2 to 2020 Q2	-0.024 (0.043)	0.030 (0.020)	-0.014 (0.045)	0.048** (0.022)	0.047 (0.044)	0.043* (0.023)	0.063 (0.045)	0.038 (0.026)
Industry average weekly wage in 2019 Q2	N	N	Y	Y	N	N	Y	Y
Precision weights	N	Y	N	Y	N	Y	N	Y
Obs	87	87	87	87	87	87	87	87

Notes: This table present the coefficients from regressing the change in the full effect of the ACA implied at the mean pre-treatment uninsurance level from 2019 to 2022 within an industry among individuals in low pre-treatment insurance coverage industries on churn in the industry in which they are employed. Industries are categorized at the 3-digit level of the NAICS codes. Churn is calculated to be the percentage change in employment from Q2 of 2019 to Q2 of 2020 by industry and is calculated using employment count data from the QCEW. The dependent variable is derived as follows. Equation (3) is estimated by industry for the subpopulation of employed individuals. Linear combinations to estimate the effect of all of the ACA's provisions are calculated and a difference between the 2022 effect and the 2019 effect for each industry is evaluated at the mean pre-treatment uninsurance rate of 21.2 percent. Columns (1), (5), (9) and (13) mirror the relationship visible in Figure B.8. The specifications used to derive the coefficients include CBSA and year fixed effects and controls for race, gender, education, age and state-level unemployment rate, per-capita income and housing price index, with standard errors clustered at the state level. Specifications in the even-numbered columns employ precision weights where the weight is the inverse of the square of the standard error associated with the calculation of the industry-level estimate derived as described above. Specifications in the columns (3),(4),(7),(8), (11),(12), (15) and (16) control for the average weekly wage in the industry in Q2 of 2019, derived from the QCEW.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.10: Difference-in-Difference-in-Differences Estimates of the Relationship Between Affordable Care Act Implementation and Health Insurance Coverage: Individuals with a High School Degree or Less

	(1) Medicaid Coverage	(2) Employer Coverage	(3) Individual Coverage	(4) Uninsured	(5) Employed
Medicaid Expansion$_s$	-0.009	-0.029*	0.032	-0.005	0.039***
$\times$Post$_t$	(0.021)	(0.017)	(0.024)	(0.027)	(0.011)
Post$_t \times$Uninsured$_{as}$	0.020	-0.043	0.258**	-0.250**	0.029
	(0.047)	(0.036)	(0.099)	(0.105)	(0.028)
Medicaid Expansion$_s$	0.048***	0.113*	-0.259**	-0.280**	-0.138***
$\times$Post$_t \times$Uninsured$_{as}$	(0.066)	(0.064)	(0.113)	(0.122)	(0.039)
Implied effects of ACA at pre-treatment uninsured rates					
ACA without	0.004	-0.010	0.057***	-0.055**	0.006
Medicaid Expansion	(0.010)	(0.008)	(0.022)	(0.023)	(0.006)
Medicaid Expansion	0.107***	0.025*	-0.057**	-0.062**	-0.030***
	(0.014)	(0.014)	(0.025)	(0.027)	(0.009)
Full ACA (with	0.111***	0.015	-0.0003	-0.117***	-0.024***
Medicaid Expansion)	(0.013)	(0.013)	(0.007)	(0.013)	(0.006)
Observations	2,991,945	2,991,945	2,991,945	2,991,945	2,991,945
State and Year FE	Y	Y	Y	Y	Y
Gender, Education, Race	Y	Y	Y	Y	Y
Economic Controls	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y
COVID-19 Controls	Y	Y	Y	Y	Y

Notes: Weighted least squares estimates generated using data from the 2011-2013 and 2021-2022 American Community Survey. Standard errors are clustered at the state level. Race controls include indicators for non-Hispanic white, black and Hispanic. Education controls include indicators for less than high school, high school degree, some college, and college degree. Economic controls include state-level per-capita income, unemployment rate, and housing price index. COVID-19 controls include the state's cumulative COVID-19 deaths per capita and cases per capita in 2022, each interacted with a dummy for "year after 2020" and the state's percentage change in employment from 2019 Q2 to 2020 Q2 interacted with a dummy for "year after 2020". The estimates in this table are the primary coefficients of interest from equation (3) estimated on the sample of individuals with a high school degree or less. Entries presented below the label "Implied effects of ACA at pre-treatment uninsured rates" are linear combinations of the coefficients presented earlier in the table. The mean pre-treatment uninsured rate is 22 percent and is derived using the 2013 uninsured rate within the low education sample at the CBSA level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.11: First (Lowest) Quartile of Industries By Employer Coverage Rate 2011-2013

Description	Baseline Employer Coverate Rate
Fishing, hunting, and trapping	0.316
Private households	0.324
Crop production	0.347
Gasoline stations	0.377
Animal production and aquaculture	0.379
Food services and drinking places	0.407
Support activities for agriculture and forestry	0.408
Administrative and support services	0.418
Personal and laundry services	0.430
Cut and sew, apparel accessories and other apparel manufacturing	0.434
Repair and maintenance	0.443
Forestry and logging	0.479
Construction	0.482
Florists, office supplies and stationary stores, used merchandise stores, gift, novelty and souvenir ships, miscellaneous retail stores	0.498
Scenic and sightseeing transportation	0.511
Truck transportation	0.512
Performing arts, spectator sports, and related industries	0.514
Knitting fabric mills, and apparel knitting mills	0.517
Not specified retail trade	0.522
Accommodation	0.528
Warehousing and storage	0.534
Social assistance	0.537

Notes: This table lists industries in the bottom quartile of employer sponsored health insurance coverage for from 2011 to 2013. Industries are aggregated up to 3 digits of the ACS industry codes which are in turn constructed from the NAICS industry codes. Coverage rate is calculated by taking an unweighted mean of the value of employer coverage among all individuals in the ACS sample.

Table C.12: Fourth (Highest) Quartile of Industries By Employer Coverage Rate 2011-2013

Description	Baseline Employer Coverate Rate
Broadcasting (except internet)	0.757
Depository and nondepository credit and related activities	0.760
Primary metal manufacturing	0.765
Electrical equipment, appliance, and component manufacturing	0.766
Pulp, paper, and paperboard mills, paperboard container manufacturing, miscellaneous paper and pulp products	0.766
Machinery manufacturing	0.768
Mining (except oil and gas)	0.776
Pipeline transportation	0.778
Insurance carriers and related activities	0.780
Transportation equipment manufacturing	0.783
Educational services	0.792
Oil and gas extraction	0.793
Postal service	0.801
Chemical manufacturing	0.805
Computer and electronic product manufacturing	0.806
Management of companies and enterprises	0.811
Petroleum refining, miscellaneous petroleum and coal products	0.811
Telecommunications	0.813
Other banking (monetary authorities, securities, commodities, funds, trusts and other financial investments)	0.820
Air transportation	0.827
Hospitals	0.831
Utilities	0.856
Not specified utilities	0.869
Rail transportation	0.875

Notes: This table lists industries in the top quartile of employer sponsored health insurance coverage for from 2011 to 2013. Industries are aggregated up to 3 digits of the ACS industry codes which are in turn constructed from the NAICS industry codes. Coverage rate is calculated by taking an unweighted mean of the value of employer coverage among all individuals in the ACS sample.