The Impact of Medicaid Expansion on Risky Health Behaviors

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Abstract

I utilize variation in Medicaid eligibility requirements at the state level in a differences-in-differences (DiD) framework to estimate the effect of Medicaid expansion on rates of primary care preventative utilization and risky health behaviors. Using modern DiD estimation methods, I significant negative effects on smoking rates in the aggregate population. I find null effects on physical activity both in the aggregate population and potential subpopulations of interest.

1 Introduction

In the debate surrounding health insurance in the US and around the world, an open question is how health insurance affects the extent to which individuals engage in behavior that, due to long-term health impacts, will increase their current or future demand for healthcare. Policy interventions that increase the availability of insurance can lead to increased healthcare utilization and spending through two channels. First, lowered prices should cause individuals to move down their healthcare demand curve, resulting in increased utilization. At the same time, individuals may engage in more risky behavior (e.g. starting to smoke or exercising less) that, in the long term, shifts out the demand curve for healthcare, resulting in increased utilization in the future. This moral hazard effect occurs because the negative results of risky behaviors—long-term health consequences such as chronic disease—are partially insured against. Therefore, individuals expect to face a lower cost of engaging in such behaviors.

However, increased primary care utilization may countermand the effect of insurance on risky behaviors. If individuals utilize more primary care, they may be more likely to reduce their risky health behaviors (or never begin them in the first place) at the advice of their primary care physician or other healthcare professional. In behavioral economic terms, the risks of those behaviors might be made more salient by contact with healthcare professionals.

Therefore, the effect of health insurance on the tendency of individuals to engage in risky behavior is theoretically ambiguous, leaving it up to empirical study. This paper exploits the Medicaid eligibility requirement policy change from the Affordable Care Act and utilizes modern differences-in-differences (DiD) methods to evaluate the policy effect on rates of smoking and physical activity.

Using data from the Center for Disease Control's Behavioral Risk Factor Surveillance Survey (BRFSS), I find a negative effect on smoking rates and no effect on inactivity in the general population. In doing so, I mostly agree with the existing literature. The rest of the paper is organized as follows: Section 2 provides a background of the relevant literature, section 3 explains the data and methodologies used and discusses some assumptions of the models, section 4 showcases results of the primary estimation, section 5 checks robustness against the survey methodology of the source data, and section 6 concludes.

2 Background

Over the last two decades, questions about how health insurance impacts outcomes such as healthcare utilization and high-risk health behaviors have been investigated many times. This literature dates back to the RAND Health Insurance Experiment of the 1970s and 80s. This experiment, reviewed in Aron-Dine et al. (2013), aimed to determine the impacts of health insurance on both healthcare spending and health outcomes utilizing random assignment of healthcare plans. Among other findings, the RAND study found that overall healthcare utilization increased with a decrease in out-of-pocket costs. Generally, this has been interpreted as individuals moving down their demand curve. However, it is also possible that this could be partially explained by a shift out in the demand curve caused by an increase in risky behaviors.

The Oregon Health Insurance Experiment, discussed in Finkelstein et al. (2012), Taubman et al. (2014), and others, allowed certain low income individuals to lottery into the state's Medicaid program. Exploiting this random assignment, Finkelstein et al. (2012) find that after Medicaid enrollment, individuals had significantly higher healthcare utilization, lower out-of-pocket costs, and better health outcomes. Those enrolled have higher probabilities of hospital admission and emergency room utilization. Taubman et al. (2014) also finds an increase in the rate of emergency room utilization among the insured.

Other studies have used identification strategies that involve policy shocks as opposed to random assignment to determine the effects of receiving health insurance. For instance, Dave and Kaestner (2009) uses the exogenous assignment of Medicare to individuals aging into the program to examine the causal effect of health insurance on behaviors and finds that obtaining health insurance reduces preventative behaviors (e.g. a healthy diet and regular exercise) and increases unhealthy behaviors among men over 65, after controlling for contact with medical professionals.

Dave et al. (2019) use vital statistics and employ a DiD strategy based on Medicaid expansion to examine the impacts of health care on prenatal care behaviors. They find that Medicaid expansions led to an increase in prenatal smoking and weight gain for low-educated mothers. Cotti et al. (2019) use data on consumer purchases in a triple differences strategy following Medicaid expansion to find that the introduction of healthcare had large negative effects on the purchase of tobacco products and large positive effects on the purchase of smoking cessation products.

Courtemanche et al. (2018) and Simon et al. (2017) both utilize the BRFSS state-level data and Medicaid expansion to attempt to measure the effects of health insurance. The former uses a triple differences strategy and the latter utilizes DiD. Neither study finds significant effects on health behaviors, despite some evidence of increased care utilization. Two potential drawbacks of each of these studies are the relatively short period of post-treatment data and the use of a two-way (or three-way) fixed effect estimator, which is prone to a negative-weighting problem when interpreted as a weighted average of group-specific treatment effects under heterogeneous effects (Goodman-Bacon, 2021; Sun and Abraham, 2021).

I identify two separate strands in this literature. One strand, in line with the RAND study and the Oregon experiment, attempts to measure the effect of insurance on overall healthcare utilization. As mentioned previously, healthcare utilization can increase for those who receive health insurance through a couple channels. A downward shift in the healthcare supply curve, absent any other general equilibrium effects, would have both income and substitution effects that could increase healthcare utilization in the short term. The decreased out-of-pocket costs may also lead people to engage in behaviors that present higher short term risks, like engaging in activities that increase the risk of injury. The first strand attempts to measure (but not disentangle) these myriad effects on overall utilization.

The other strand, in line with Dave and Kaestner (2009), Dave et al. (2019), and Courtemanche et al. (2018), attempts to directly measure another channel through which insurance can affect utilization. With a decrease in expected future out-of-pocket costs, individuals may engage in behaviors that pose long-run health risks, like smoking or decreasing their physical activity. If these risky health behaviors lead to long-term health consequences (e.g. lung cancer in the case of smoking or heart disease in the case of both smoking and inactivity), that would also increase overall demand for healthcare in the long run.

This paper expands on the research in the second strand. It most directly follows Courtemanche et al. (2018) and Simon et al. (2017) in that it utilizes Medicaid eligibility as a method for constructing a DiD study. This paper contributes to this literature by using modern DiD methods, expanding the post-treatment period available, and examining effects in important subpopulations.

3 Data and Methodology

Ideally, I would have access to a long-term individual-level panel that includes information on Medicaid status, health status, and behavioral outcomes. Such a dataset would allow me to estimate the effect of a quasi-exogenous change in Medicaid eligibility on behavioral outcomes and the associated long-term health outcomes. However, such data does not exist, so instead I use publicly available survey microdata from the CDC's Behavioral Risk Factor Surveillance System (BRFSS).

BRFSS data

This nationally representative survey captures data from across states on individual behavioral outcomes, health outcomes, and demographic covariates, conducted yearly by telephone. The BRFSS survey has been carried out in some form since 1984, and was made nationwide in the 1990s. In most recent years, the sample sizes of BRFSS are over 400,000 individuals across the 50 states and DC.

The primary variables I take from these data are self-reported behavioral outcomes related to physical health. Specifically, I calculate variables that indicate whether an individual smokes daily and whether an individual engaged in exercise (not related to their work) in the 30 days prior to the interview. I also have indicator variables for whether the individual is uninsured and whether they have received a standard check-up from a physician in the 12 months prior to the interview. I also use demographics such as age and education.

The two outcomes of interest, smoking and physical inactivity, were chosen among other behavioral outcomes for a variety of reasons. Smoking daily has very large effects on long-term health outcomes, and the addictive properties of nicotine ensure that it can be difficult for someone who smokes every day to quit (Institute of Medicine, 2015). Therefore, quitting smoking represents a "high-cost, high-reward" behavioral change. On the other hand, exercising at least once a month is considerably easier than quitting smoking. Exercise (as measured by BRFSS) then represents a low-cost behavior. Furthermore, these outcomes are simpler to measure than those related to other behaviors, such as diet.

The BRFSS survey has a couple potential drawbacks compared to an ideal data source. First, all outcomes are self-reported. Therefore, risky behaviors may be systematically misreported.

Second, as a phone survey, the sampling methods of the BRFSS survey have changed over time to adjust for the increased popularity of cell phones. Prior to 2011, the BRFSS used only landlines to conduct the survey, and added cell phones in 2011. Thus, the composition of the sample in the two periods are systematically different, making comparison between the two periods (as a DiD analysis would) somewhat suspect. I address this concern directly in section 5.

Unfortunately, these data also suffer extensively from missing values in certain years. For smoking and inactivity, the most reliable data exists from 1995 and 2000 onward respectively. Before this, many states are missing data for these rates for the vast majority (or all) records. For the rate of yearly check-ups, data is first available starting in the late 1990s, but is almost always missing in 2003 and 2004. In order to have a continuous panel, I begin my analysis of smoking rates, inactivity, and check-ups in 1995, 2000, and 2005 respectively. For all model estimation, I only use data through 2019 to avoid contamination of the sample from differential effects of the COVID-19 pandemic across states.

A few states are missing data in a few years for each of my outcomes. To ensure I have a balanced panel, I drop these states as necessary within each outcome. For my two outcomes of interest, smoking and inactivity, that leaves a panel of 48 states over 25 years and 49 states over 20 years respectively.[1]

Other data

For information on Medicaid eligibility expansion, I use data distributed by the Kaiser Family Foundation, which includes the date of Medicaid expansion in a given state. Because each observation in my data is a state-year pair, I adopt the convention that a state had expanded Medicaid eligibility by year t if such an expansion had occurred prior to July 1st of year t. Most states that have expanded Medicaid eligibility did so on January 1st, 2014, so the impact of this convention should be negligible. Data on national rates of uninsurance and Medicaid utilization are taken from the National Health Interview Survey, accessed via IPUMS.

Differences-in-differences

I utilize the modern differences-in-differences framework developed by Callaway and Sant'Anna (2021) because of the potential for heterogeneous treatment effects across states and heterogeneous treatment timing.

The following restates the setup and goals of Callaway and Sant'Anna (2021). Assume we have data from some set of states $\mathcal{S}$ for years $t \in \{1, \dots, \mathcal{T}\}$, with treatment (Medicaid eligibility expansion) being assigned in groups $g \in \mathcal{G}$. Let $Y_{s,t}$ be an outcome (e.g. share of adults aged 18–64 who smoke) in state s in year t. Let $Y_{s,t}(g)$ be the potential outcome that state s would experience in year t if they first expanded Medicaid eligibility in year g, and let $G_{s,g}$ be an indicator variable that is equal to one when state s initially expanded Medicaid in year g, and zero otherwise. By convention, let $Y_{s,t}(0)$ be the potential outcome for state s in year t if the state did not expand Medicaid eligibility throughout the sample.[2]

[1] The states that are dropped are: DC, HI, and NJ for smoking; HI and NJ for inactivity; NJ for yearly check-up; DC, HI, NJ, RI, and WY for uninsurance.

[2] By assumption, once states have expanded Medicaid eligibility, they do not reverse that policy decision. Fortunately, this is also true in our sample.

Then we can relate the observed and potential outcomes by

$$Y_{s,t} = Y_{s,t}(0) + \sum_{g=2}^{\mathcal{T}} (Y_{s,t}(g) - Y_{s,t}(0)) \cdot G_{s,g} \tag{1}$$

The idea behind the Callaway-Sant'anna estimators is to aggregate group-time specific treatment effect estimands into other estimands of interest, including aggregate average treatment effects and event study treatment effects.

Specifically, the building blocks of our treatment effects of interest are

$$ATT(g,t) = \mathbb{E}\left[Y_t(g) - Y_t(0) | G_g = 1\right] \tag{2}$$

Under a few assumptions, including a conditional parallel trends assumption (conditional on pre-treatment, non-time-varying covariates X_s) and a limited-anticipation assumption, the group-time treatment effects can be identified and estimated through a few methods.[3]

Once $ATT(g,t)$ is estimated for all treatment groups g and years t, different weighted averages can be estimated using different weights. Of interest to us are θ_{ATE}, the simple average treatment effect, and $\theta_{ES}(e)$, the event study treatment effect, which is the average treatment effect at event time $e = t - g$. The associated weighted averages are

$$\theta_{ATE} = \frac{1}{|G| \cdot \mathcal{T}} \sum_{g \in \mathcal{G}} \sum_{t=2}^{\mathcal{T}} ATT(g,t) \tag{3}$$

$$\theta_{ES}(e) = \sum_{g \in \mathcal{G}} \mathbb{1}\{g + e \leq \mathcal{T}\} P(G = g | G + e \leq \mathcal{T}) ATT(g, g+e) \tag{4}$$

Estimating the average treatment effect and event study effects in this manner avoid the negative weight problem found in TWFE specifications of DiD analyses (Goodman-Bacon, 2021; Sun and Abraham, 2021).

4 Empirical results

I restrict my analysis to three different samples: US adults aged 18–64, US adults aged 18–35, and US adults aged 18–64 without a college degree. I take means of the indicator variables mentioned above (for uninsurance status, receiving a yearly check-up, smoking daily, or engaging in monthly exercise) by state, age group, and year, weighted using BRFSS survey weights to be representative of the population. I then age-standardize the rates using the standard 2000 Census age distribution to control for the changing age distribution of the population over time (Klein and Schoenborn, 2001). Sometimes, information on these dummy variables is missing for a respondent, and these respondents are dropped separately for each outcome variable examined.

[3] In this paper, I use the doubly robust method developed in Sant'Anna and Zhao (2020), see Callaway and Sant'Anna (2021) for details.

Trends in insurance usage, primary care utilization, and behaviors

I calculate the share of US adults aged 18–64 who are uninsured and who are on Medicaid over time using survey data from the NHIS. Figure 1 shows that after Medicaid eligibility expansion began in some states in 2014, rates of uninsurance dropped precipitously and the share on Medicaid grew, indicating that the expanded eligibility requirements likely did have the desired effect.

Also on the national scale, Figure 2 shows the trends in the smoking rate and rate of inactivity since 1993. The smoking rate has dropped significantly in recent decades, whereas inactivity has remained more stable.

A break in the downward trend in smoking rates seems likely to have come from composition effects of the updated sampling methodology of the 2011 BRFSS survey. Section 5 attempts to address this break in DiD analyses of the smoking rate, but I will ignore it for the time being.

Figures 3, 5, and 7 compare rates of uninsurance by treatment group for states who expanded eligibility requirements in 2014 (the initial wave) and those that had not expanded by 2019 for all three subsamples.[4] Both groups in all samples show a steep drop off in uninsurance rates after the passage of Medicaid expansion. Those that did not expand Medicaid eligibility requirements likely saw drop offs in the uninsurance rate due to other provisions of the Affordable Care Act. My identification strategy uses variation in the expansion of specific eligibility requirements that differ between these groups of states, ignoring changes that they had in common.

Figures 4, 6, and 8 show analogous plots for smoking and inactivity rates. Again, all three samples share similar characteristics in these trends. Smoking rates have broadly trended down in both groups, whereas inactivity has remained relatively stable after the mid-2000s.

Differences-in-differences estimation

First, I estimate the effects of Medicaid eligibility expansion on the share of uninsured adults and on the share of adults who receive a yearly medical check-up. I can then interpret the effect estimates of risky health behaviors in the context of exposure to the treatment itself. Table 1 shows estimates and standard errors of θ_{ATE} for the three different samples. The top panel shows results for uninsurance rates, and the bottom panel shows results for the share who receive a yearly check-up. Even numbered columns include the pre-treatment covariates of state per capita income and share with a college degree in 2010.[5]

As expected, expansion of Medicaid eligibility requirements significantly reduced uninsurance rates in all sample populations, with the largest effects coming from the population aged 18–35 (3.1% reduction and 3.7%, without and with covariates respectively). Despite this increase in insurance availability, yearly check-up rates did not increase. Although estimates for all samples were positive, none are significantly different from 0. However, there

[4] These are the two largest groups. The never-treated group consists of: AL, FL, GA, KS, MS, MO, NC, OK, SC, SD, TN, TX, WI, and WY. The 2014 group consists of: AZ, AR, CA, CT, DE, DC, HI, IL, IA, KY, MD, MA, MI, MN, NV, NJ, NM, NY, ND, OH, OR, RI, VT, WA, and WV.

[5] The Callaway-Sant'anna framework explicitly allows for pre-treatment, time-constant covariates in the model, but not for time-varying covariates.

are also fewer reliable years of pre-treatment data for check-ups, affecting the precision of the estimates.

Figures 9 and 10 show event study estimates of the dynamic treatment effects $\theta_{ES}(e)$ for uninsurance rates and check-up rates, corresponding to the six columns in the two panels of Table 1. For uninsurance, pretreatment effects hover around zero close to treatment and are significantly negative nearly every year after treatment in all samples.

The event study estimates for check-ups are less clear to interpret. However, they suggest a short run increase in check-up rates through approximately the second year after treatment, before returning to pre-treatment levels. However, it is difficult to rule out problems with these estimates due to pretrends.

With that added context, I now turn to my primary outcomes of interest. Table 2 shows estimates and standard errors of θ_{ATE} for smoking rates and inactivity rates for each of the three samples. Most estimates for effects on smoking rates are negative but fairly small and not significantly different from 0. When conditioning on covariates, the smoking rate in the full population fell by a statistically significant 0.6%. Given that the national smoking rate was between 10 and 20% during the sample period, this is a sizable result.

The estimates for inactivity rates are largely dependent upon whether or covariates are included in the model. When covariates are not included, estimates are positive and, for the under-35 group and the non-college group, are statistically significant. However, when covariates are included, these estimates are negative for the full sample and for the non-college sample, and none are significantly different from 0.

To get a better sense of our treatment effects of interest, I once again look at the event study treatment effect estimates. For smoking rates, the event study suggests that treatment effects may be getting more negative over time. Although most pre-treatment estimates are not significantly different from 0, they are still varied enough to cause some concern about the parallel trends assumption, both with and without covariates.

For inactivity rates, parallel trends is clearly violated when covariates are not included in the model, indicating that the significant positive effects found in Table 2 are not valid. The parallel trends assumption is supported by the event study when covariates are included, suggesting a null effect of Medicaid eligibility expansion on inactivity rates.

5 Accounting for BRFSS sampling change

As mentioned previously, Figure 2 shows an apparent discontinuity in the trend of smoking rates in 2011, corresponding to a change in the sampling methodology of the BRFSS survey. If the change in sampling methodology had differential effects across treatment groups, this would impact my results.

In an ideal setting, I would attempt to correct for this my standardizing my rates not just with respect to a standard age distribution, but also with respect to a full set of demographic and socioeconomic variables, including race, sex, income, and education. However, that would require calculating smoking rates (or another outcome) for specific and small cells of the survey sample. Therefore, I take an approach that relies less on the quality of my underlying data.

After the structural break, the downward trend in the national smoking rate continues its decline at a similar pace as before. This motivates the use of "splicing" to correct for the structural break directly instead of the compositional effects that likely caused it.

Specifically, at the state level, I extrapolate the difference between the smoking rate in 2009 and 2010 one more year forward, adding it to the smoking rate in 2010 to predict the value in 2011. I then apply this difference to every year after 2011, creating a smooth series that removes the structural break, separately by state. Formally, let $Y_{s,t}$ be the smoking rate in state s in year t. Then,

$$\hat{Y}_{s,2011} = Y_{s,2011} - ((Y_{s,2011} - Y_{s,2010}) + (Y_{s,2009} - Y_{s,2010})) \tag{5}$$

$$\hat{Y}_{s,t:t>2011} = Y_{s,t} - ((Y_{s,2011} - Y_{s,2010}) + (Y_{s,2009} - Y_{s,2010})) \tag{6}$$

Figure 13 shows the national average smoking rate with this splicing has been applied compared to without.

To see if this adjustment affects my results, I estimate the DiD models for this spliced smoking rate. Table 3 shows the simple ATT results, and Figure 14 show the event study estimates. My estimates are completely unaffected by this adjustment out to more than three decimal places. This has very little affect because the splicing affects all values post-treatment and a few values pre-treatment, which has very little effect on the Callaway-Sant'anna estimator. Furthermore, these splicing changes are relatively small compared to the overall trend in smoking rates over time.

6 Conclusion

In this study, my results mostly agree with those found in Courtemanche et al. (2018) and Simon et al. (2017). Despite an increase in the share of adults with insurance, states that expanded Medicaid eligibility did not see an increase in rates of smoking or inactivity. In contrast to these two studies, I find a decrease in the rate of smoking in these states. From a policy perspective, this is a positive result.

In this context, where a potential positive effect on smoking rates would counteract the purpose of coverage-expanding policies, a *lack* of a large positive effect can be just as important as a large negative effect. The confidence interval for our results in all models rules out effects as large as 0.5 percentage points on smoking rates in most samples, and 2 percentage points on rates of inactivity, when using estimates that include covariates as suggested by the event study. These provide a useful cap on the potential net moral hazard effect of Medicaid expansion.

However, these results should be interpreted with some caution. I did not find an increase in the rate of yearly check-ups, despite the body of evidence that suggests that such a policy should increase healthcare utilization of many kinds.

Furthermore, it is possible that the proportion of individuals receiving yearly check-ups does not sufficiently account for exposure to healthcare professionals. After all, yearly check-ups are relatively routine, and may represent "low-impact" exposure for many individuals. Future work should focus on properly identifying differences in both quantity and intensity of healthcare exposure for different age groups to explain these findings and identify heterogeneity in health behavior by exposure to healthcare.

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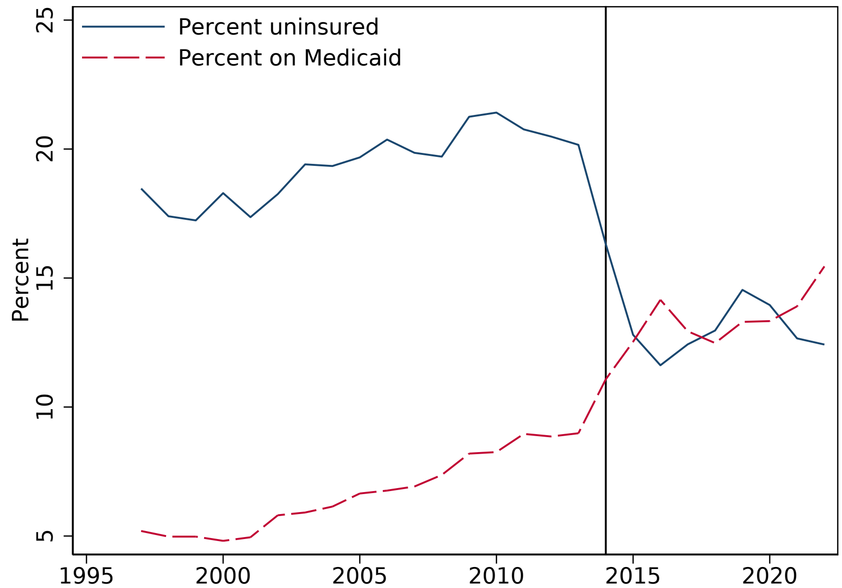
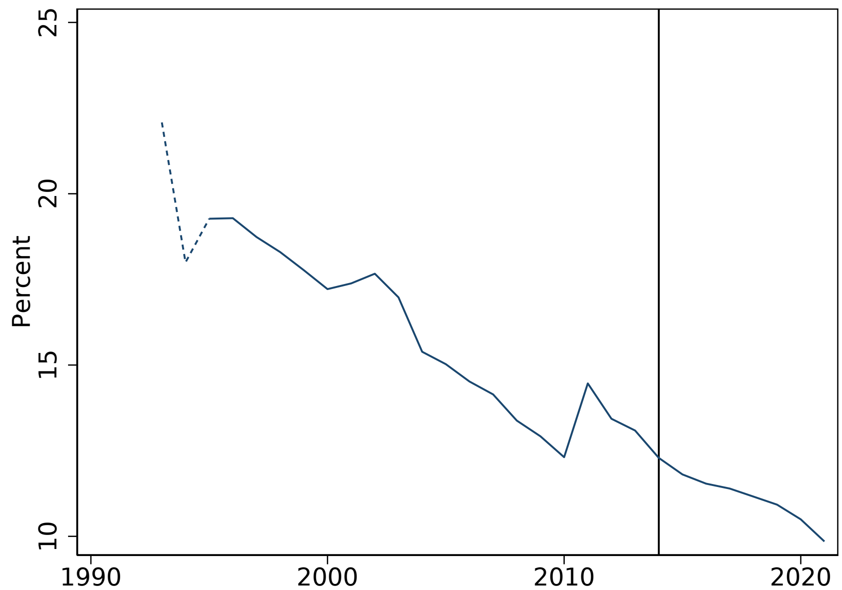
Figure 1: National trends in insurance coverage



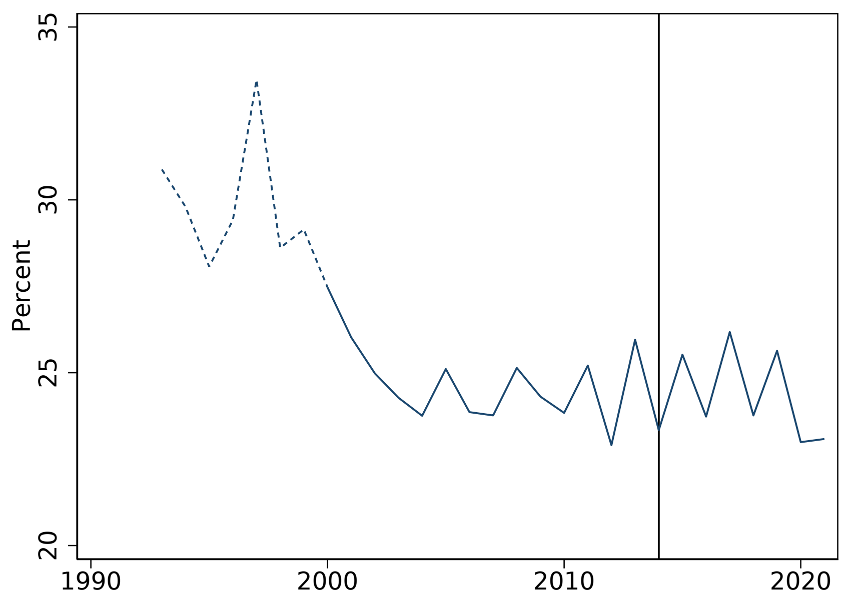
Note: The figure shows the percent of US adults aged 18–64 who do not have health insurance as well as the percent that receive insurance through Medicaid over time. 2014, the year the first wave of Medicaid eligibility expansion took place, is marked by the black vertical line. *Source:* NHIS microdata and author calculations.

Figure 2: National trends in selected health behaviors

(a) Smoking

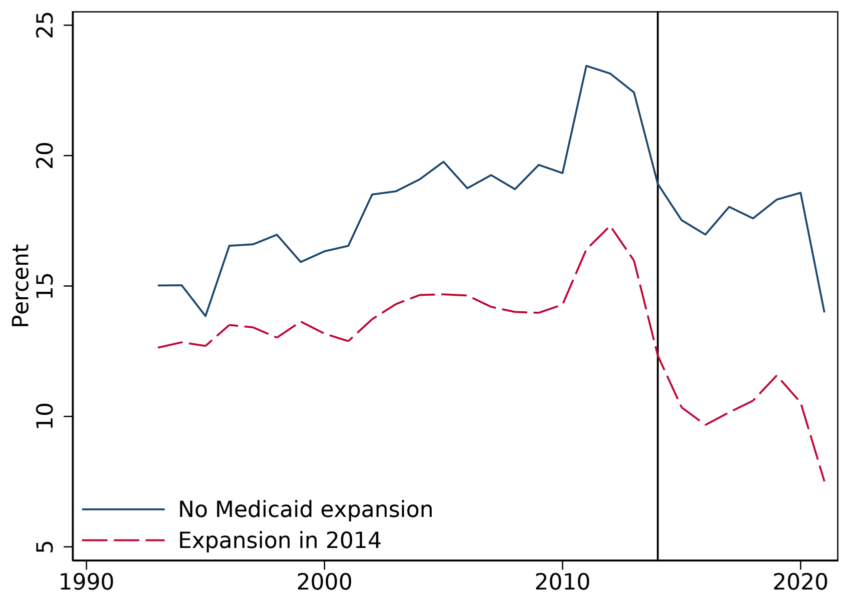


(b) Inactivity



Note: The top panel shows the percent of US adults aged 18–64 who smoke every day. The bottom panel shows the percent of US adults aged 18–64 who had not exercised in the 30 days prior to their interview. Years for which data is missing for many cases in many states is indicated by a dashed line. Rates were age-standardized using the age distribution from the 2000 Census. *Source:* BRFSS microdata and author calculations.

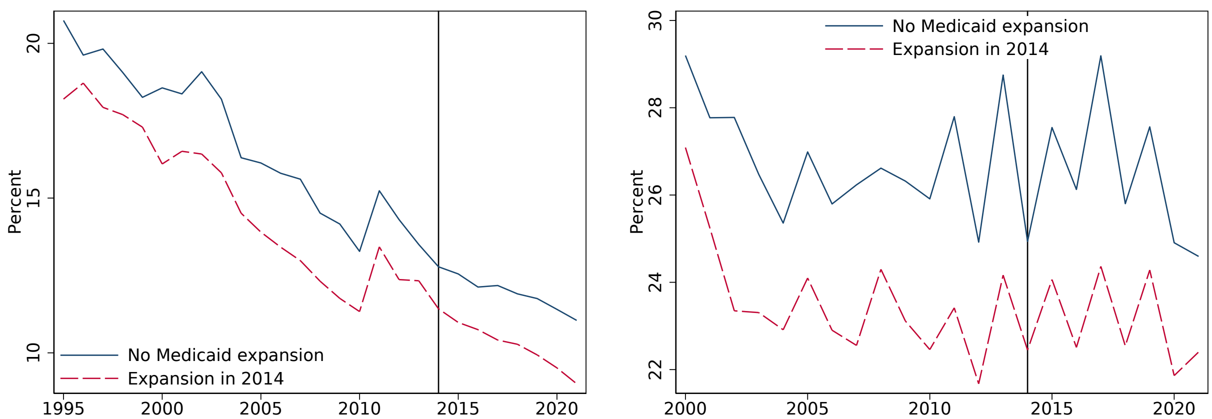
Figure 3: Share uninsured by Medicaid expansion adoption



Note: The figure shows the percent of US adults aged 18–64 who do not have health insurance. The solid blue line shows this for states that had not expanded Medicaid eligibility requirements by 2021, and the red dashed line shows the this for states that expanded Medicaid in 2014. Rates were age-standardized using the age distribution from the 2000 Census. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

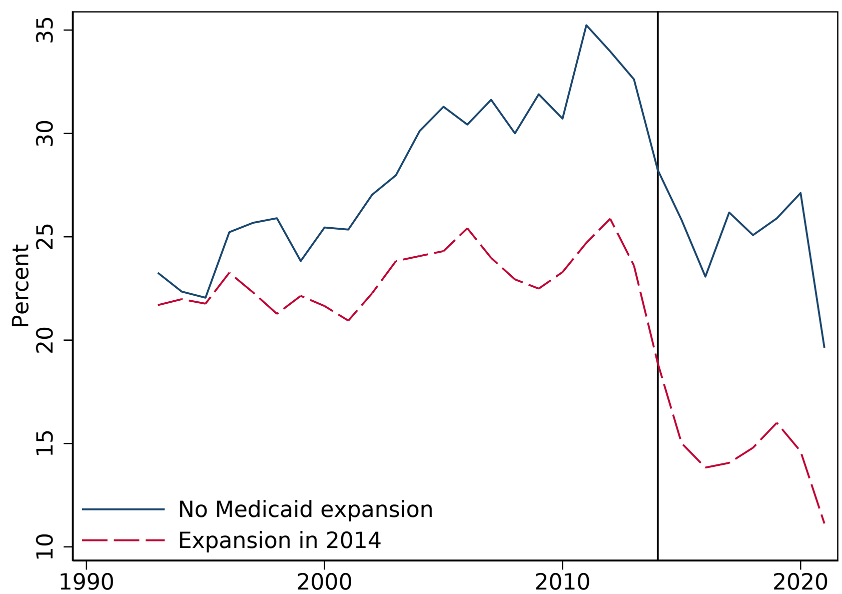
Figure 4: Health behaviors by Medicaid expansion adoption

(a) Smoking (b) Inactivity



Note: The left panel shows the percent of US adults aged 18–64 who smoke every day, with the solid blue line showing this for states that had not expanded Medicaid eligibility requirements by 2021, and the red dashed line shows the this for states that expanded Medicaid in 2014. The left panel shows the share who had not exercised in the 30 days prior to their interview for the same groups. Rates were age-standardized using the age distribution from the 2000 Census. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

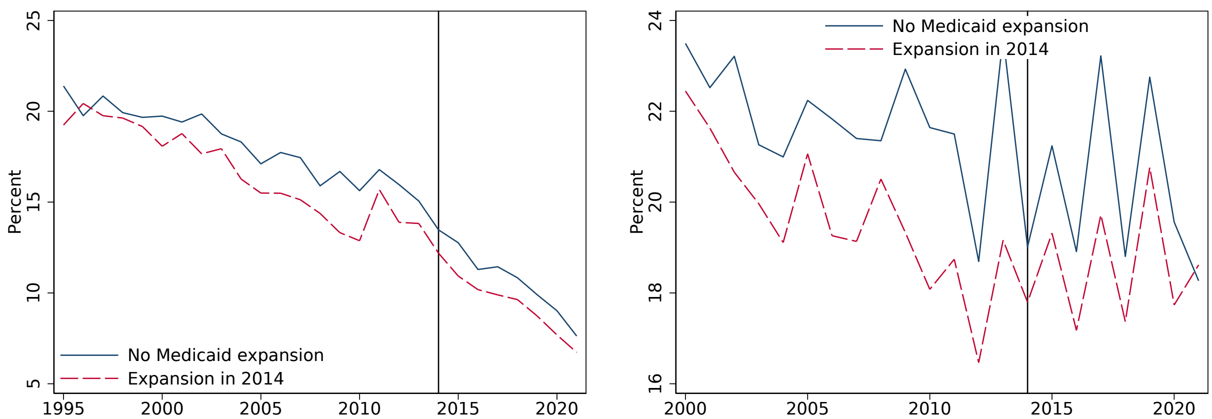
Figure 5: Share uninsured by Medicaid expansion adoption, under 35



Note: The figure shows the percent of US adults aged 18–35 who do not have health insurance. The solid blue line shows this for states that had not expanded Medicaid eligibility requirements by 2021, and the red dashed line shows the this for states that expanded Medicaid in 2014. Rates were age-standardized using the age distribution from the 2000 Census. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

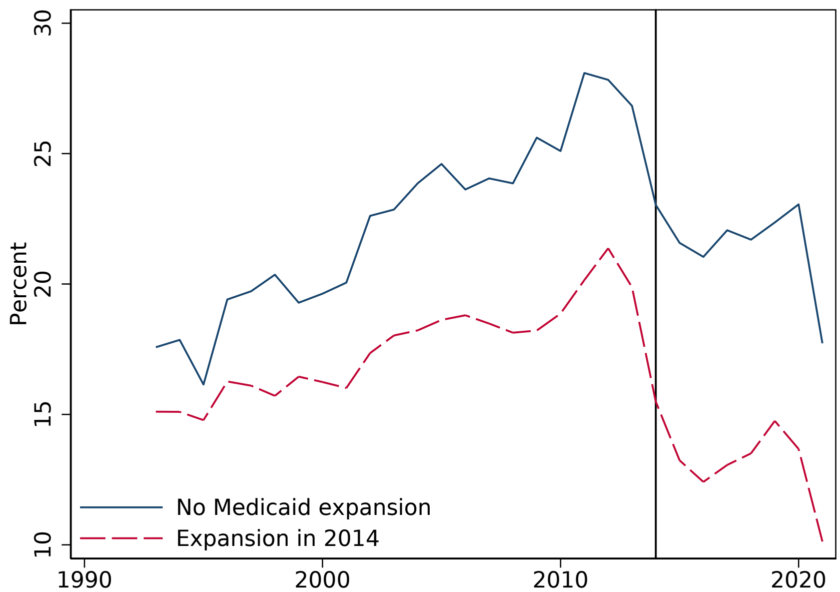
Figure 6: Health behaviors by Medicaid expansion adoption, under 35

(a) Smoking (b) Inactivity



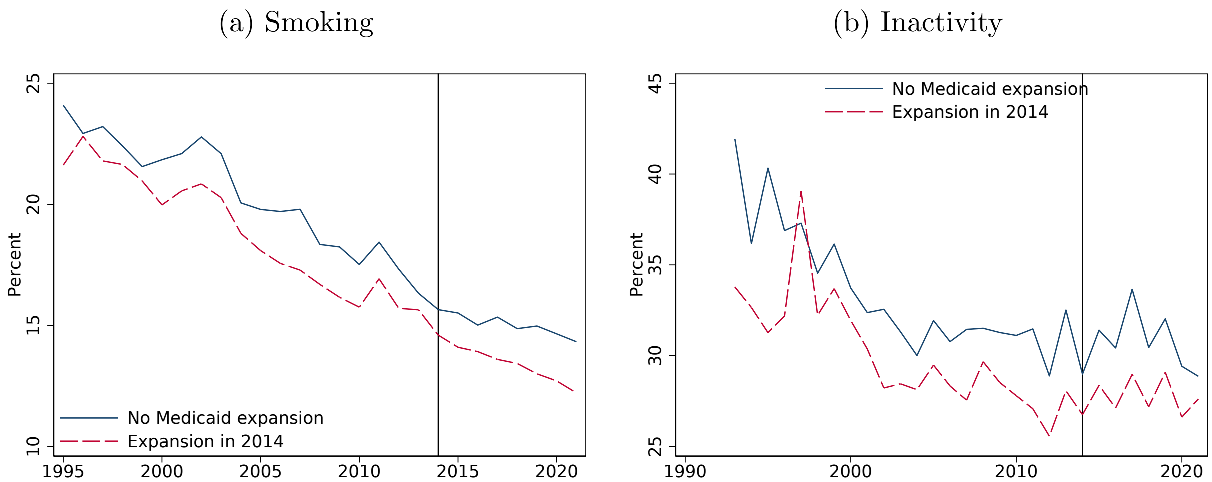
Note: The left panel shows the percent of US adults aged 18–35 who smoke every day, with the solid blue line showing this for states that had not expanded Medicaid eligibility requirements by 2021, and the red dashed line shows the this for states that expanded Medicaid in 2014. The left panel shows the share who had not exercised in the 30 days prior to their interview for the same groups. Rates were age-standardized using the age distribution from the 2000 Census. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 7: Share uninsured by Medicaid expansion adoption, non-college



Note: The figure shows the percent of US adults aged 18–64 without a college degree who do not have health insurance. The solid blue line shows this for states that had not expanded Medicaid eligibility requirements by 2021, and the red dashed line shows the this for states that expanded Medicaid in 2014. Rates were age-standardized using the age distribution from the 2000 Census. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 8: Health behaviors by Medicaid expansion adoption, non-college



Note: The left panel shows the percent of US adults aged 18–64 without a college degree who smoke every day, with the solid blue line showing this for states that had not expanded Medicaid eligibility requirements by 2021, and the red dashed line shows the this for states that expanded Medicaid in 2014. The left panel shows the share who had not exercised in the 30 days prior to their interview for the same groups. Rates were age-standardized using the age distribution from the 2000 Census. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Table 1: Callaway-Sant'anna ATTs on healthcare access

(a) Uninsurance rates

	(1) Full sample	(2) Full sample	(3) Under 35	(4) Under 35	(5) Non-college	(6) Non-college
ATT	–0.020**	–0.026***	–0.031**	–0.037***	–0.027***	–0.033***
	(0.006)	(0.006)	(0.010)	(0.010)	(0.007)	(0.008)
Covariates	N	Y	N	Y	N	Y
N	1150	1150	1150	1150	1150	1150

(b) Yearly check-up rates

	(1) Full sample	(2) Full sample	(3) Under 35	(4) Under 35	(5) Non-college	(6) Non-college
ATT	0.009	0.008	0.013	0.010	0.009	0.006
	(0.006)	(0.010)	(0.009)	(0.011)	(0.007)	(0.010)
Covariates	N	Y	N	Y	N	Y
N	750	750	750	750	750	750

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The table shows average treatment effects of the expansion of Medicaid eligibility estimated in a differences-in-differences framework in the style of Callaway-Sant'anna. The top panel shows the treatment effects on the share of adults who don't have health insurance, and the bottom panel shows the effects on the share of adults who had a check-up with a physician in the previous year. The first two columns use the full sample of adults aged 18–64, columns 3 and 4 use the sample of adults aged 18–35, and columns 5 and 6 only include adults aged 18–64 without a college degree. A doubly robust DiD estimator based on stabilized inverse probability weighting and OLS is used. See Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020) for more details. Models that include covariates include share of state population with a college degree and state per capita income in 2010 (only pre-treatment, static covariates are used). Standard errors in parentheses. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Table 2: Callaway-Sant'anna ATTs on health behaviors

(a) Smoking rates

	(1) Full sample	(2) Full sample	(3) Under 35	(4) Under 35	(5) Non-college	(6) Non-college
ATT	–0.003	–0.006*	–0.003	–0.005	–0.005	–0.008
	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
Covariates	N	Y	N	Y	N	Y
N	1200	1200	1200	1200	1200	1200

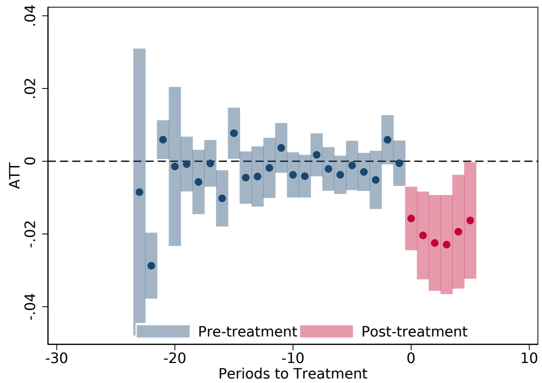
(b) Inactivity rates

	(1) Full sample	(2) Full sample	(3) Under 35	(4) Under 35	(5) Non-college	(6) Non-college
ATT	0.008	–0.003	0.017*	0.004	0.010*	–0.002
	(0.005)	(0.005)	(0.007)	(0.008)	(0.005)	(0.006)
Covariates	N	Y	N	Y	N	Y
N	980	980	980	980	980	980

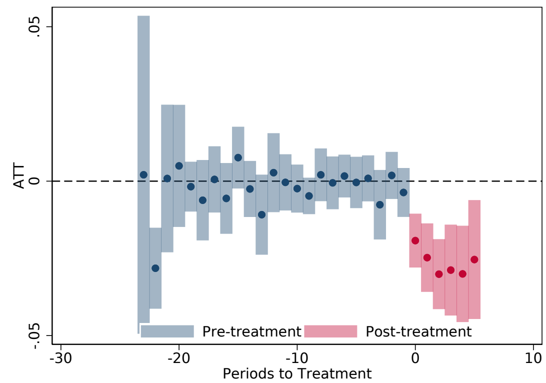
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The table shows average treatment effects of the expansion of Medicaid eligibility estimated in a differences-in-differences framework in the style of Callaway-Sant'anna. The top panel shows the treatment effects on the share of adults who smoke daily, and the bottom panel shows the effects on the share of adults who did not exercise in the previous 30 days. The first two columns use the full sample of adults aged 18–64, columns 3 and 4 use the sample of adults aged 18–35, and columns 5 and 6 only include adults aged 18–64 without a college degree. All rates have been age-standardized using the 2000 Census age distribution. A doubly robust DiD estimator based on stabilizied inverse probability weighting and OLS is used. See Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020) for more details. Models that include covariates include share of state population with a college degree and state per capita income in 2010 (only pre-treatment, static covariates are used). Standard errors in parentheses. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 9: Event study plots of uninsurance rates

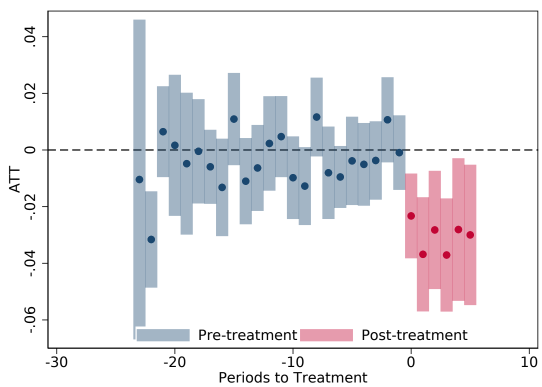
(a) Full sample, no covariates



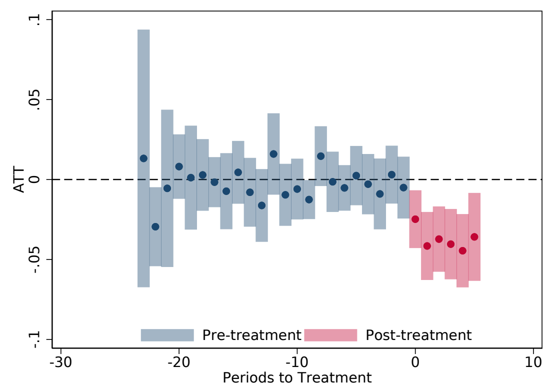
(b) Full sample, with covariates



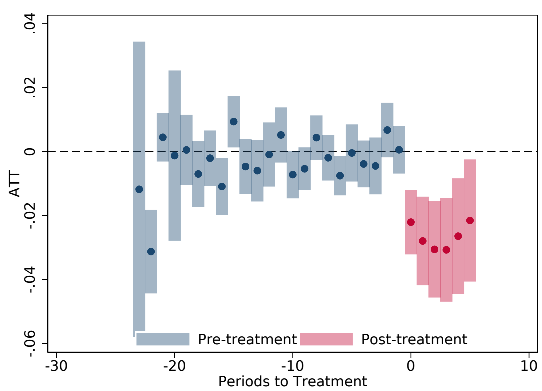
(c) Under 35, no covariates



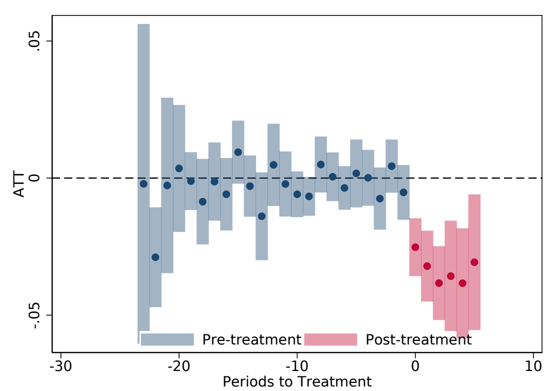
(d) Under 35, with covariates



(e) Non-college, no covariates



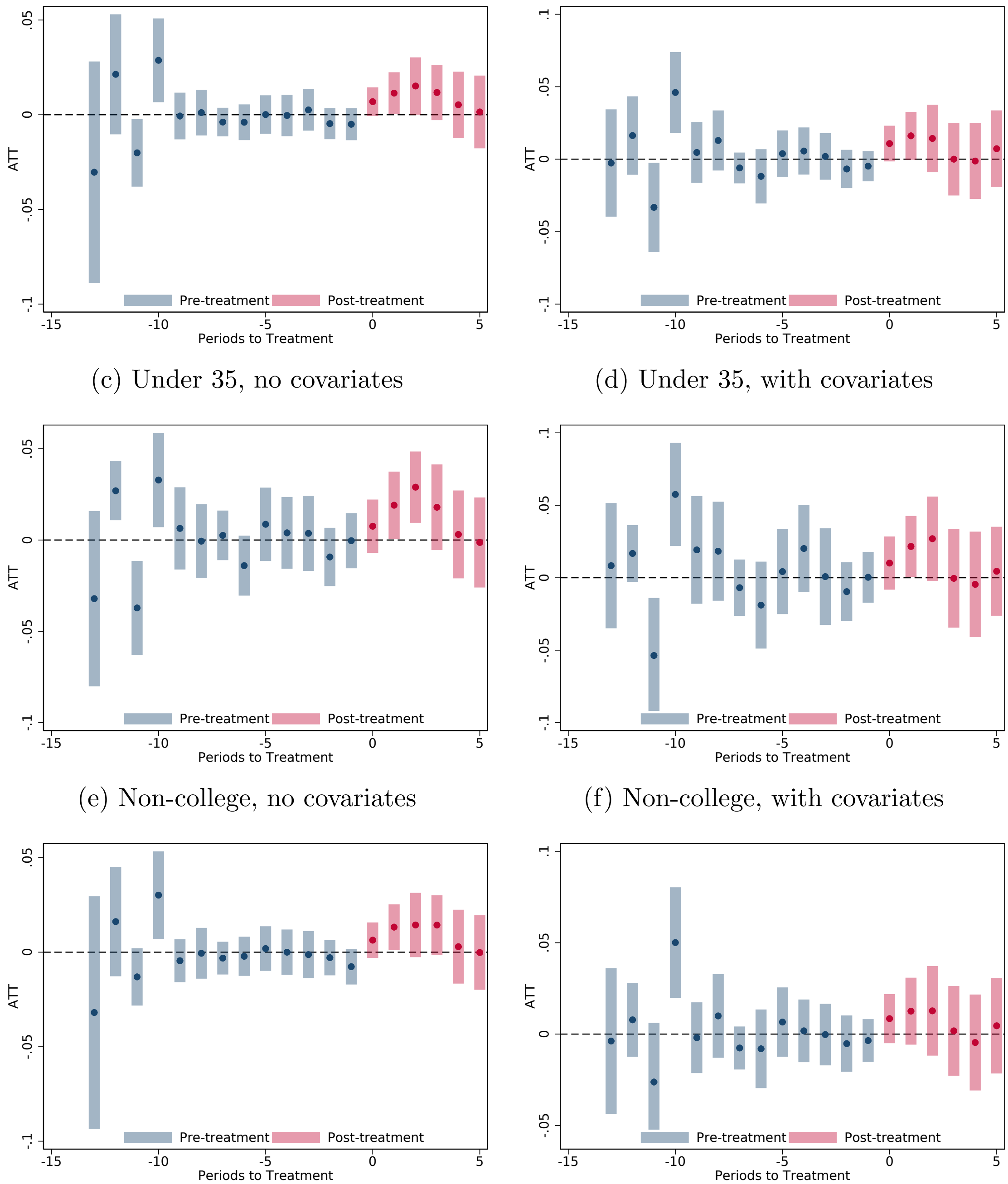
(f) Non-college, with covariates



Note: Each panel shows estimated treatment effects of Medicaid eligibility expansion on rates of uninsurance and associated 95% confidence intervals by period relative to the start of treatment, as estimated in the Callaway-Sant'anna DiD framework. The estimates in blue are pre-treatment, and those in red are post-treatment. The left column shows estimates from models that don't include pre-treatment covariates, and the right column includes state per capita income and share of adults with a college degree in 2010 as pre-treatment covariates. The top row uses the full sample of US adults aged 18–64, the middle row limits the sample to adults aged 18–35, and the bottom row limits the sample to adults 18–64 without a college degree. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 10: Event study plots of check-up rates

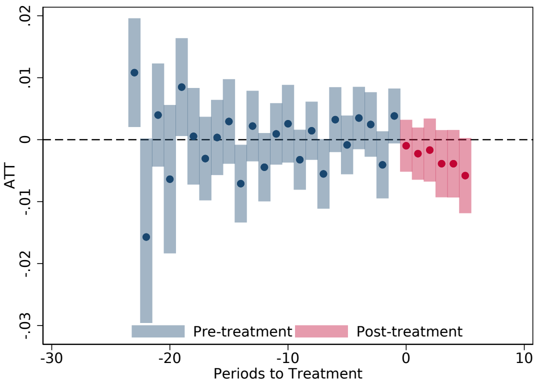
(a) Full sample, no covariates (b) Full sample, with covariates



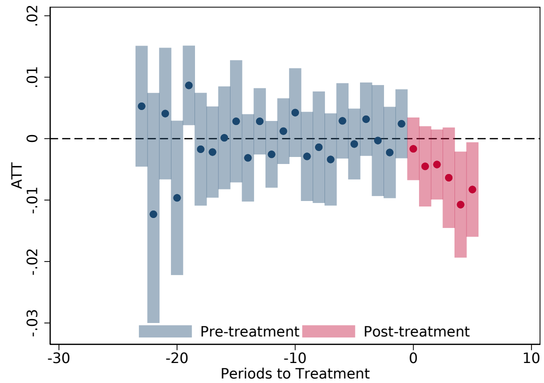
Note: Each panel shows estimated treatment effects of Medicaid eligibility expansion on the share of adults who received a check-up in the prior 12 months and associated 95% confidence intervals by period relative to the start of treatment, as estimated in the Callaway-Sant'anna DiD framework. The estimates in blue are pre-treatment, and those in red are post-treatment. The left column shows estimates from models that don't include pre-treatment covariates, and the right column includes state per capita income and share of adults with a college degree in 2010 as pre-treatment covariates. The top row uses the full sample of US adults aged 18–64, the middle row limits the sample to adults aged 18–35, and the bottom row limits the sample to adults 18–64 without a college degree. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 11: Event study plots of smoking rates

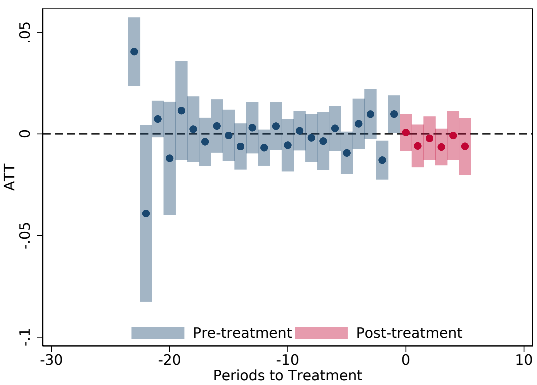
(a) Full sample, no covariates



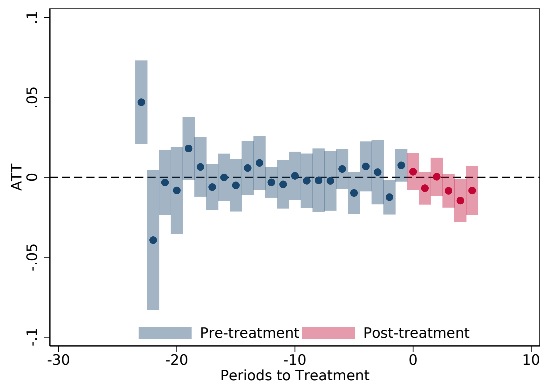
(b) Full sample, with covariates



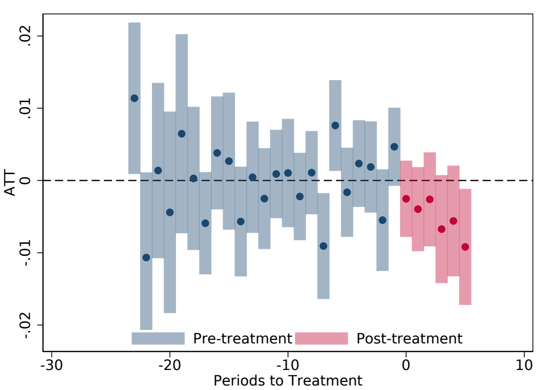
(c) Under 35, no covariates



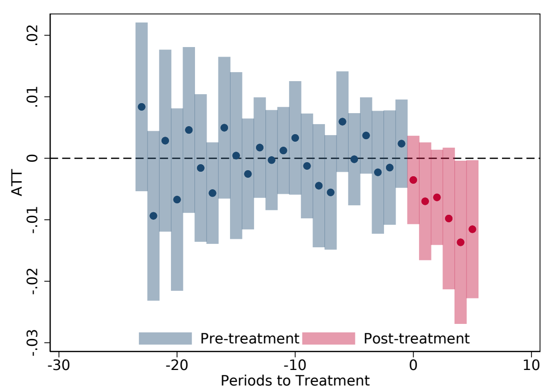
(d) Under 35, with covariates



(e) Non-college, no covariates



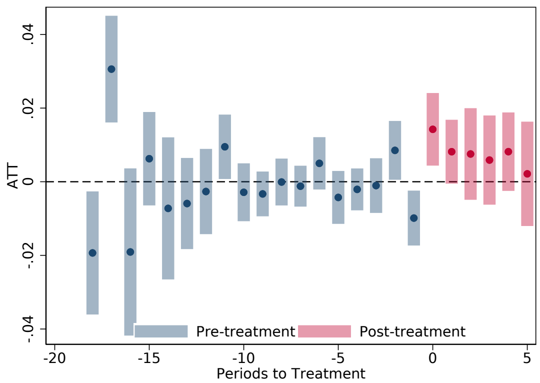
(f) Non-college, with covariates



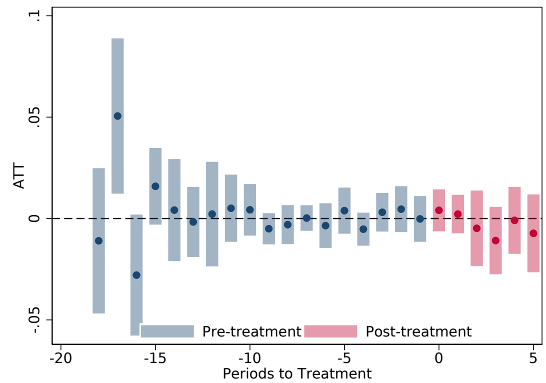
Note: Each panel shows estimated treatment effects of Medicaid eligibility expansion on the share of adults who smoke daily and associated 95% confidence intervals by period relative to the start of treatment, as estimated in the Callaway-Sant'anna DiD framework. The estimates in blue are pre-treatment, and those in red are post-treatment. The left column shows estimates from models that don't include pre-treatment covariates, and the right column includes state per capita income and share of adults with a college degree in 2010 as pre-treatment covariates. The top row uses the full sample of US adults aged 18–64, the middle row limits the sample to adults aged 18–35, and the bottom row limits the sample to adults 18–64 without a college degree. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 12: Event study plots of inactivity rates

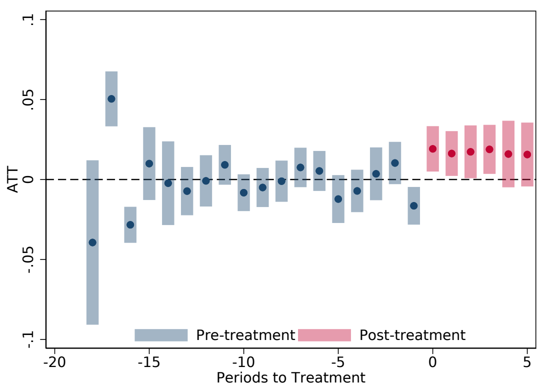
(a) Full sample, no covariates



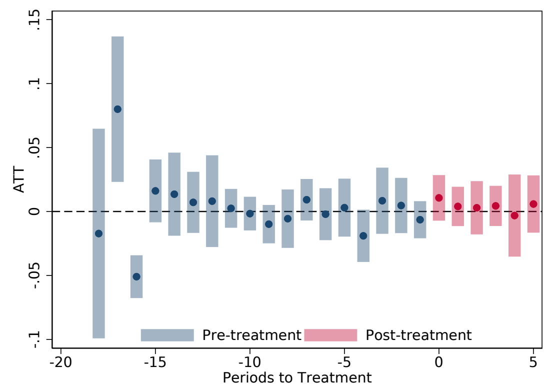
(b) Full sample, with covariates



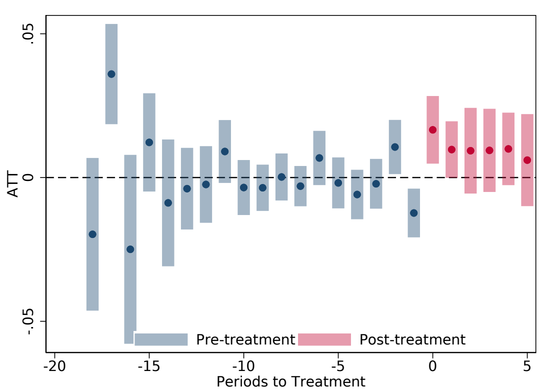
(c) Under 35, no covariates



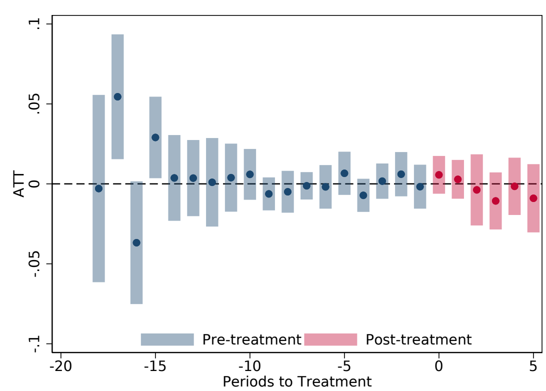
(d) Under 35, with covariates



(e) Non-college, no covariates



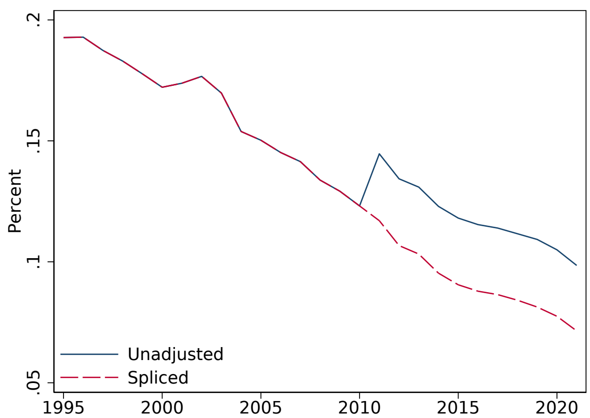
(f) Non-college, with covariates



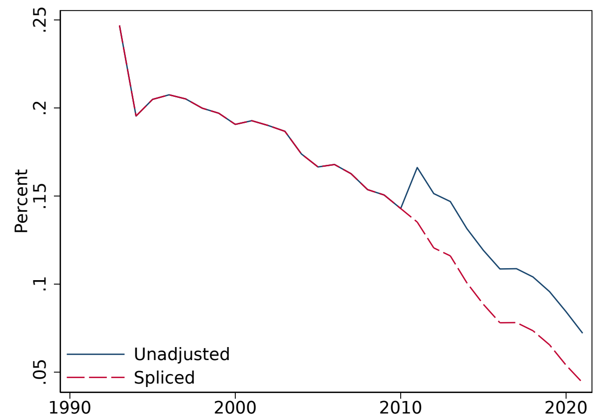
Note: Each panel shows estimated treatment effects of Medicaid eligibility expansion on the share of adults who exercised at least once in the prior 30 days and associated 95% confidence intervals by period relative to the start of treatment, as estimated in the Callaway-Sant'anna DiD framework. The estimates in blue are pre-treatment, and those in red are post-treatment. The left column shows estimates from models that don't include pre-treatment covariates, and the right column includes state per capita income and share of adults with a college degree in 2010 as pre-treatment covariates. The top row uses the full sample of US adults aged 18–64, the middle row limits the sample to adults aged 18–35, and the bottom row limits the sample to adults 18–64 without a college degree. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 13: Comparison of raw and spliced smoking rates

(a) Full sample



(b) Under 35



(c) Non-college

Note: Each panel shows the effect of splicing smoking rates from post-2011 data to pre-2011 data to adjust for composition changes in the post-2011 BRFSS. For each state, the difference between the smoking rate in 2009 and the smoking rate in 2010 was extrapolated to 2011 to generate a predicted value for 2011. The values for 2012 and onwards were then spliced onto this series (see text for details). These rates were then averaged by year, using state populations as weights. The blue line shows the raw smoking rates averaged in this way, and the red dashed line shows the spliced rates. The top left panel shows the rates for the full sample (adults aged 18–64), the top right shows the rates for adults aged 18–35, and the bottom panel shows the rates for adults aged 18–64 without a college degree. *Source:* BRFSS microdata and author calculations.

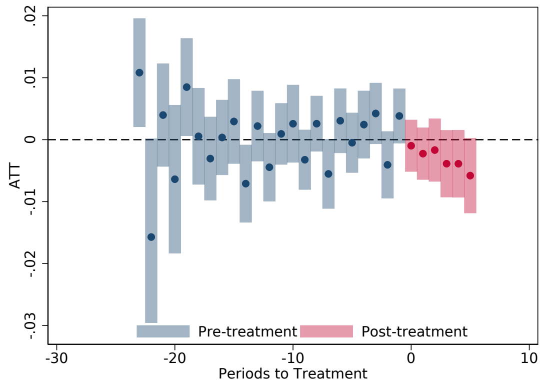
Table 3: Callaway-Sant'anna ATTs: spliced smoking rates

	(1) Full sample	(2) Full sample	(3) Under 35	(4) Under 35	(5) Non-college	(6) Non-college
ATT	–0.003	–0.006*	–0.003	–0.005	–0.005	–0.008
	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
Covariates	N	Y	N	Y	N	Y
N	1200	1200	1200	1200	1200	1200

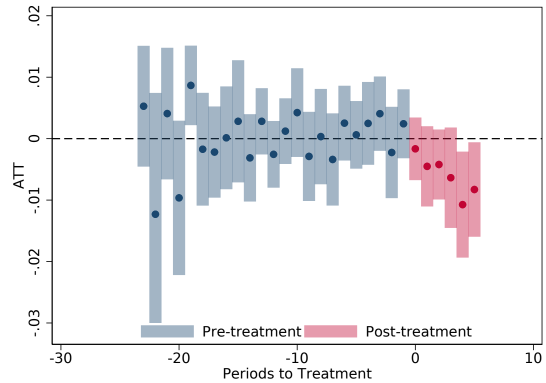
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The table shows average treatment effects of the expansion of Medicaid eligibility estimated in a differences-in-differences framework in the style of Callaway-Sant'anna. This table shows the treatment effects on the share of US adults in different samples who smoke daily, adjusting for the composition of the BRFSS survey via splicing (see text for details). The first two columns use the full sample of adults aged 18–64, columns 3 and 4 use the sample of adults aged 18–35, and columns 5 and 6 only include adults aged 18–64 without a college degree. All rates have been age-standardized using the 2000 Census age distribution. A doubly robust DiD estimator based on stabilizied inverse probability weighting and OLS is used. See Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020) for more details. Models that include covariates include share of state population with a college degree and state per capita income in 2010 (only pre-treatment, static covariates are used). Standard errors in parentheses. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.

Figure 14: Event study plots of spliced smoking rates

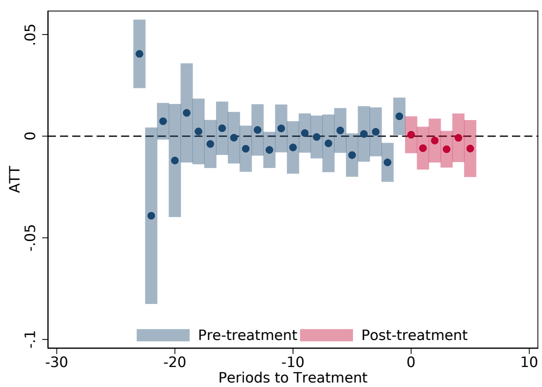
(a) Full sample, no covariates



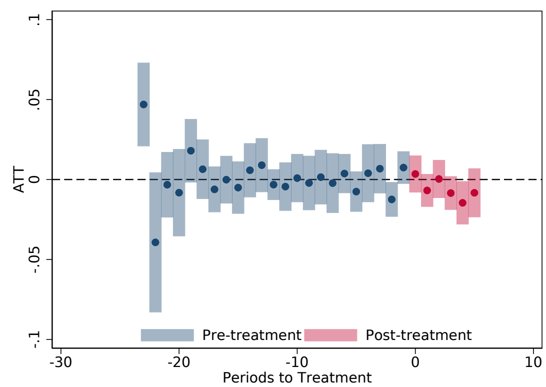
(b) Full sample, with covariates



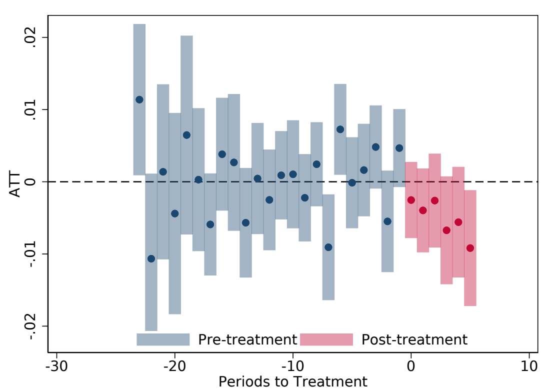
(c) Under 35, no covariates



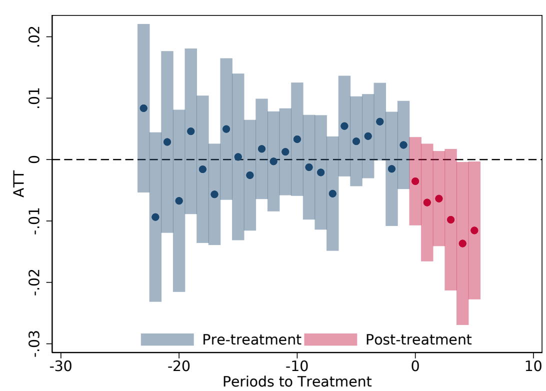
(d) Under 35, with covariates



(e) Non-college, no covariates



(f) Non-college, with covariates



Note: Each panel shows estimated treatment effects of Medicaid eligibility expansion on spliced rates of adults who smoke daily and associated 95% confidence intervals by period relative to the start of treatment, as estimated in the Callaway-Sant'anna DiD framework. The estimates in blue are pre-treatment, and those in red are post-treatment. The left column shows estimates from models that don't include pre-treatment covariates, and the right column includes state per capita income and share of adults with a college degree in 2010 as pre-treatment covariates. The top row uses the full sample of US adults aged 18–64, the middle row limits the sample to adults aged 18–35, and the bottom row limits the sample to adults 18–64 without a college degree. *Source:* BRFSS microdata, KFF data on Medicaid expansion, and author calculations.