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James Flynn
Miami University and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Tobacco 21 Laws, Prenatal Smoking, and Birth Outcomes

This paper examines the effect of Tobacco 21 laws, which raise the minimum age for tobacco purchases to 21, on prenatal smoking and birth outcomes using restricted access data from the National Vital Statistics System. Using both the synthetic difference-in-differences and stacked event-study designs, I fail to find evidence that these laws reduced prenatal smoking or cause any improvement on birth outcomes. I am able to rule out even modest decreases in prenatal smoking of greater than 6% on the extensive margin and greater than 5% on the intensive margin. Results are unchanged if I focus only on non-high school graduate mothers, who smoke at much higher rates at baseline, or if I focus solely on states which passed the toughest laws. My findings suggest that T21 laws may not be an effective policy tool to prevent prenatal smoking.

JEL Classification: I12, I18
Keywords: Tobacco 21, prenatal smoking, minimum legal sale age

Corresponding author:
James Flynn
Miami University
501 E High St
Oxford, OH 45056
USA
E-mail: flynnj@miamioh.edu

1. Introduction

This paper estimates the effect of Tobacco 21 laws, which raise the minimum league sale age (MSLA) on tobacco products from 18 to 21, on prenatal smoking behavior and birth outcomes among mothers who are restricted from legally purchasing tobacco. The potential harms of smoking while pregnant are severe and well documented, including restricting fetal growth and altering brain structure, low birth weight, placental problems, and compromised lung functioning.[1] At the same time, young mothers between the ages of 18 to 21 are among the most likely to smoke while pregnant of all age groups, so a policy that is effective at reducing prenatal smoking at these ages could lead to substantial health improvements among this vulnerable population.

In general, Tobacco 21 laws (henceforth T21) have been found to reduce smoking among the targeted age groups, particularly among self-reported survey respondents (Hansen, Sabia, McNichols, et al., 2023; Friedman and Wu, 2020; Abouk, De, and Pesko, 2024; Cotti, DeCicca, and Nesson, 2024). It is not clear, however, whether pregnant mothers will respond in the same way as other young smokers. Pregnant mothers already have a strong incentive to quit, both because of the potential harms to their children and because of the stigma associated with prenatal smoking, so the pregnant mothers who otherwise would have smoked may respond differently to such a law than the average survey respondent. They may be more likely to be seriously addicted at baseline and therefore more desperate to find a way around such laws. On the other hand, the added difficulty in acquiring cigarettes due to T21 laws could serve as the final 'nudge' required to get them to quit.

In this paper, I investigate whether T21 laws reduce prenatal smoking using restricted access natality data from the National Vital Statistics System, which includes the near universe of birth records from 2011-2021. Using both the synthetic difference-in-differences method of Arkhangelsky et al., 2021 and the stacked event-study method of Cengiz et al., 2019, I fail to find evidence that T21 laws reduce smoking among 18-21 year old mothers, both before and during pregnancy and along both the extensive and intensive margins. Because of the relatively precise estimates I obtain, I am able to statistically rule out even relatively modest decreases in prenatal smoking in response to the policy. I can rule out decreases of larger than 6% on the extensive margin measure of the likelihood that a mother smokes in any of the three trimesters of pregnancy. On the intensive margin, I can

[1] Bublitz and Stroud (2012), Ekblad, Korkeila, and Lehtonen (2015), Stone, Bailey, and Khraisha (2014), Phelan (2014), Ananth, Savitz, and Luther (1996), Chhabra et al. (2014)

rule out decreases in the average daily number of cigarettes that mothers consume of larger than 5%. I fail to find any evidence of reductions even when concentrating on mothers without a high school degree, who are the most likely to smoke at baseline, or when looking only at the states which have implemented the strongest T21 laws according to the Preventing Tobacco Addiction Foundation. I also estimate the effects of these laws directly on birth outcomes, and once again fail to find any evidence on improvements.

This paper contributes to a growing literature examining the impact of T21 laws on smoking among young people. Several recent studies have found that T21 laws reduce both self-reported smoking among young people using data from the Behavioral Risk Factor Surveillance System Survey (BRFSS) and the Youth Risk Behavioral Surveillance System (YRBS) (Hansen, Sabia, McNichols, et al., 2023; Friedman and Wu, 2020), the Monitoring the Future (MTF) survey (Abouk, De, and Pesko, 2024), the Population Assessment of Tobacco and Health (PATH) survey Friedman and Pesko (2024) and Cotti, DeCicca, and Nesson (2024). Though there is variation in the point estimates, the main results of these studies are encouraging from a policy perspective, but there is concern over bias from using self-reported data. If, for example, T21 raised the salience of underage smoking and made young people more hesitant to admit to smoking, but without changing their true smoking habits, some of these studies may overstate the potential benefits. Two important recent studies have looked into this potential problem, supplementing self-reported data with other evidence.

Abouk, De, and Pesko (2024) ads Nielsen Scanner data to their survey-based findings, demonstrating that counties with T21 laws see sizeable reductions in cigarette purchases. This scanner data does not provide the age of the person making the purchases, however, which means that it is unclear whether these reductions are coming from people constrained by the policy (18-20 year olds). The authors get around this issue by demonstrating that sales reductions are concentrated in counties with large populations under 21 years old. Overall, this study presents convincing evidence that T21 laws reduce tobacco sales to young people in the counties where a law is in effect, but this scanner data is unable to address whether there is cross-border substitution, where impacted 18-20 year olds simply cross a border into an area without T21 laws in order to purchase cigarettes legally.

Cotti, DeCicca, and Nesson (2024) supplement their findings on self-reported smoking by looking for changes in the underlying biomarkers present in respondent's urine samples. They similarly find that T21 laws reduce self-reported smoking, but they fail to find a reduction in biomarker evidence of tobacco use, raising concerns that T21 laws may only be impacting respondents' willingness to admit to smoking. They test this explicitly by looking at whether there is a reduction in self-reported smoking among an analytical sample of respondents who show clear

biomarker evidence of smoking. Among very likely smokers, T21 laws do lead to reductions in the fraction of respondents who admit to smoking, raising serious concerns about some of the survey-based results in this literature.

This paper similarly looks for evidence of reductions in self-reported smoking, along with biological evidence of a reduced impact of smoking. I use natality records from the National Vital Statistics System (NVSS), which includes questions about prenatal smoking behavior, to look at whether T21 laws lead to reductions in self-reported smoking. In addition, I look to see whether these laws also lead to causal changes in birth outcomes, which have been shown repeatedly to be impacted by prenatal smoking. If T21 laws cause reductions in self-reported smoking, but not actual smoking, as in Cotti, DeCicca, and Nesson (2024), we might expect to find reductions in reported smoking, but no changes in birthweight or gestational length. Instead, I find no evidence of improvements in either outcome, suggesting that T21 laws may not be effective at preventing prenatal smoking.

The rest of the paper proceeds as follows. Section two provides background on the various policy mechanisms in place designed to influence smoking in the United States, as well as specifics on the rollout and implementation of the various T21 laws. Section three describes my data and empirical strategies to find a causal effect of T21 laws on prenatal smoking and birth outcomes. Section four presents results and a series of additional specifications and robustness checks, while section five concludes.

2. Background

Several different policy tools have been used to attempt to influence smoking behavior in the United States, with varying degrees of success and many unintended consequences. Pigouvian 'sin taxes' on cigarette purchases are quite common, with all 50 states and the District of Columbia levying some form of excise tax on cigarettes. Excise taxes on cigarettes are generally effective at reducing purchases and consumption of cigarettes (Lien and Evans, 2005; DeCicca and McLeod, 2008; Carpenter and Sansone, 2021; Callison and Kaestner, 2014; Friedson et al., 2020), though with substantial heterogeneity. Most studies find that they are most effective at reducing smoking among younger and lower socioeconomic status (Bader, Boisclair, and Ferrence, 2011), though DeCicca and McLeod (2008) show that they can also reduce smoking among older adults. Hansen, Sabia, and Rees (2017) actually find that young people have become less sensitive to cigarette taxes since 2005, a period that coincides with the introduction of e-cigarettes.

There is an important interplay between the use of cigarettes and e-cigarettes. Several studies have found that the two are generally substitutes Pesko (2022),

Pesko and Currie (2019), Cooper and Pesko (2017), Dave, Feng, and Pesko (2019), and Abouk, Courtemanche, et al. (2023), though Abouk and Adams (2017) finds that banning e-cigarette sales to minors reduces both cigarette and e-cigarette use, suggesting that they can be complements for young people. E-cigarettes have proven to be a useful tool in helping people quit smoking (Auer et al., 2024; Levett et al., 2023), though there are also concerns that e-cigarette consumption among young people could ultimately lead them to start smoking combustible tobacco (Etter, 2018). Abouk, Adams, et al. (2023) find that e-cigarette taxes *increase* prenatal smoking, suggesting that some pregnant mothers were using e-cigarettes to quit smoking.

Cigarette taxes have also had a major impact on prenatal smoking, leading to large and persistent health improvements for developing fetuses. In utero exposure to higher cigarette taxes has been shown to decrease maternal smoking during pregnancy (Lien and Evans, 2005; Levy and Meara, 2006), increase birth weight (Evans and Ringel, 1999), reduce sick days, doctor visits, and hospitalizations as children (Simon, 2016), and improve educational attainment for low socioeconomic status children (Settele and Ewijk, 2018). Indoor smoking bans have also been shown to improve the health of children and infants McGeary et al., 2020, though indoor vaping restrictions do the opposite, leading to increases in prenatal smoking (Cooper and Pesko, 2017)

The town of Needham, Massachusetts became the first municipality to raise the MLSA for tobacco products to 21 in April of 2005. A number of other towns followed suit in the early 2010's before New York City became the first major US city to enact a T21 law on May 18, 2014. Many other cities, including Chicago, Cleveland, Kansas City, St. Louis, and Columbus passed T21 laws in the next few years with Hawaii and California becoming the first states to adopt such a law in 2016, followed by New Jersey in 2017, before a number of other states followed suit between 2018-2021. As of the time of this writing, only seven US states do not currently have some version of a T21 law.[2]

3. Data and Empirical Strategy

3.1. Data on birth records

I use restricted-access records on the near universe of births in the United States from 2011-2021, provided by the National Vital Statistics System (NVSS). Each record includes the age of the mother at the time of the birth, which I use to determine whether the mother could have been influenced by a T21 law if it was in

[2]These are Alaska, Arizona, Missouri, Montana, North Carolina, South Carolina, and Wisconsin.

effect where she lived during the pregnancy. Ideally, I would know the mother's exact age, which would allow me to determine if mothers who were 21 at the time of birth were under 21 during their pregnancy, but this information is unavailable. I therefore consider all mothers who were between the ages of 18 and 21 (inclusive) at the time of birth as potentially treated. These birth records also provide information on the state and county of residence of the mother for each birth, as well as a host of demographic characteristics, including the race and educational attainment of the mother, which I use as controls.

Most importantly for the purposes of this paper, these records also include responses to questions about smoking behavior, both before and during pregnancy. Mother are asked how many cigarettes they smoked per day, on average, prior to the pregnancy, as well as during each of the first, second, and third trimesters. I therefore estimate the effect of T21 laws on the intensive margin measure of how much the mothers smoked during each stage of pregnancy, as well as long the extensive margin of whether or not they reported smoking at all in each stage. The only major data limitation regarding these questions is that a number of states did not reliably ask these questions for all births until after 2014, leading to a substantial portion of records with missing values. Because of this, I drop all states from my analysis where more than 3% of all observations are missing values for the smoking related questions in 2014. Also, in order to line up the periods in which mothers are smoking to the timing of the laws, I calculate for each birth the quarter in which the pregnancy was conceived, using the birth month and the estimated gestational length of the pregnancy. This way, when estimating whether T21 laws impacted prenatal smoking, I will be comparing the enactment of the laws to the timing of conception, instead of the timing of the birth, at which time the prenatal smoking has already occurred (or not occurred).

Appendix Figure A.1 displays a histogram with the percentage of observations that are missing in 2014. The majority of states have less than 2% of all observations missing, but there are five states[3] which are missing more than 10% of all observations. Dropping all observations in these states means excluding 10.46% of all observations. I further exclude five additional states (Arkansas, Maine, Massachusetts, Ohio, and Texas) which included 'grandfather clauses', which carved out an exception for people who were already 18 at the time of the laws implementation, and therefore could already legally purchase tobacco products. Finally, I also exclude states with a large fraction of the population covered by county or city level T21 laws prior to the implementation of the state-level law (New York, Missouri, and Illinois).

Even after making these restrictions, I am left with 1.78 million births to oth-

[3]Connecticut, Hawaii, New Jersey, Rhode Island, Virginia.

ers aged 18-21 over the period 2014-2020, spanning 37 states and the District of Columbia. I include births to both 18 and 21 year old mothers, because both are potentially subject to purchasing restrictions during their pregnancy. The data do not contain the mother's birthday, so it is impossible to tell exactly how much of their pregnancy was 'treated' by a T21 law, but both groups are likely treated for at least some portion of their pregnancy. Importantly, I will also show robustness to estimating my main specifications on 18-20, 19-21, and 19-20 year old mothers in the appendix, as well as showing results for each individual year of age.

I combine these birth records with data on the implementation of T21 laws from both Tobacco21.org, which includes a register of each state-level law that has been enacted, as well as Abouk, De, and Pesko (2024), which includes the city and county-level T21 laws that have been passed, mostly preceding the statewide versions of the laws. Adoption of these laws has been swift and wide-reaching, with Appendix Figure A.2 displaying the evolution of T21 coverage in the US from 2015-2021.

Figure 1 displays a time series which compares the percentage of all births to 18-21 year old mothers in my analytical sample where the mother reports smoking cigarettes at any point during her pregnancy, along with the overall percentage of all pregnancies occur to mothers who live in states which are covered by a T21 law, from 2014-2020. There is clearly a negative correlation between the two time series, with prenatal smoking declining throughout the period as more and more states implement T21 laws. It appears, however, that the decline in smoking predates the rollout of T21 laws, and then continues throughout the period at roughly the same rate. The rate of smoking declines steadily from 11.8% in 2011 down to 6.5% in 2020, a reduction of approximately 45%. The treated population increases steadily from 2015-2017 before slowing down between 2017-2019, and then increasing again as several states pass laws in 2020.

Interestingly, there does not appear to be a 'dose-response', where we might expect to see larger decreases in the smoking rate after larger chunks of the population of births become treated. For example, from 2014 to 2015 the smoking rate decreases from 11.9% to 11.0%, with none of the population treated by state laws in either year. Then as the fraction of births climbs steadily from 0 in 2015 to 7.9% in 2016 and 15.0% in 2017, the rate of smoking continues to decline at roughly the same rate as before, declining 0.8 percentage points to 10.2% in 2016 and only a further 0.5 percentage points in 2017. Then, after the fraction treated levels off somewhat in 2018 and 2019, the rate of smoking declines by 0.8 and 0.9 percentage points, respectively. It is also unclear from this graph whether the reductions in the smoking rate are concentrated in the areas that are getting treated with T21 laws, or if they are simply part of a nationwide trend. The fact that the reductions clearly begin well before the implementation of any of the T21 laws suggests that

it may be the latter.

Figure 2 investigates this further graphically by displaying the time series of each of the smoking variables, comparing states which are treated at any point between 2016-2020 with the states which have no T21 law as of the end of 2020. There are a few things which immediately stand out. First, the areas which implement T21 laws have lower rates of smoking and prenatal smoking to begin with. In 2014, 16.4% of mothers in untreated areas smoked prior to becoming pregnant, compared with 14.6% in the eventually treated areas. The same is true for each trimester, with lower rates of smoking in 2014 for the eventually treated regions compared with the control regions. Second, there is a clear downward trajectory for both treated and control groups which begins well before the implementation of any of the T21 laws. In fact, the reductions appear to be happening at a faster rate in the untreated areas, which could partially be due to the fact that they had higher rates to begin with.

Third, there is little visual evidence of any kind of T21 treatment effect. If there were, we would expect to see a divergence of the two time series begin to take place after 2016, as more and more of the T21 policies switch on. If anything, we see the opposite, where the two lines continue to converge, much like they were already doing before 2016. This figure also raises concerns about the parallel trends assumption in comparing these treated and untreated counties, as they appear to be on converging somewhat even before any of the laws go into effect. This serves to motivate my use of the synthetic difference-in-differences method, which find a control group for each treated period with a more appropriate counterfactual comparison. Appendix Figure A.3 repeats this exercise, replacing the fraction of mothers smoking any cigarettes with the intensive margin measure of how much they are smoking. In general, the results are very similar to Figure 2

3.2. Empirical Strategy

Because of the staggered rollout of Tobacco 21 laws, I implement two complementary difference-in-difference strategies. Because Figure 2 raises concerns about the validity of simply comparing treated versus untreated areas, my main specifications use the synthetic difference-in-differences (SDID) approach of Arkhangelsky et al. (2021). SDID builds on the synthetic control method (SCM) approach of Abadie et al. (2010), which is useful for comparative case studies with a single treated unit. SCM selects the optimal control group as a weighted average of all control observations in the 'donor pool', or the complete list of possible controls. SCM selects non-negative weights for each unit in the donor pool in order to minimize the difference between the actual treated unit and the 'synthetic control' unit on whatever variables the researcher chooses to match on. A common approach

is to match on pretreated values of the dependent variable.

SDID generalizes this approach, by allowing it to be used in situations where there are multiple treatment groups, as well as when there is variation in treatment over time. In addition to choosing weights for each unit in the donor pool, SDID also selects weights for each period in the pre-treatment period in order to select a control group that optimally matches the trends in the treated group. SDID then calculates the traditional 2x2 DID using these weights, with group and period fixed effects along with any additional controls. When there are multiple periods in which treatment 'turns on' for particular units, SDID iterates through this process for each treatment period and returns a coefficient which is a weighted average of the individual 2x2 DIDs. Crucially, for each treatment timing period, SDID only considers 'never treated' units as potential control units in the donor pool, which means its estimates are results to the recent concerns raised about using earlier treated units as controls for later treated units (Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022).

The main drawback of this approach is that SDID requires panel data, with one observation for each unit in each period. This prevents me from running regressions at the individual level and requires that I instead collapse observations up to state-level rates. I collapse to the state-level instead of the county level because many counties have zero births or very few births per quarter, making their rates unpredictable. At the state level, even Vermont, which has the fewest average births to 18-21 year old mothers in my analytical sample, averages around 400 births per year during my sample. Because I am collapsing up to the state level, I drop the four states which have a substantial portion of their population covered by county-level T21 laws before the state law goes into effect (Illinois, New York, Ohio, and Missouri).

I supplement my SDID findings by estimating the 'stacked DiD' strategy of Cengiz et al. (2019) and Deshpande and Li (2019) in the Appendix. This approach creates a separate dataset for each period where treatment turns on for some units. Within each dataset, all previously treated units and units which will receive treatment before the end of the sample period are dropped. This process is repeated for each period in which treatment turns on, and then these newly created datasets are appended, or 'stacked', onto one another. Then, the following regression equation is estimated:

$$Y_{icqs} = \beta_1 Treat_{cs} XPost_{qs} + \delta_{cs} + \gamma_{qs} \tag{1}$$

where Y_{icqs} represents an outcome for individual i in county c is quarter q, belonging to data stack s. β_1 measures the change in outcomes in treated counties relative to control counties after the T21 laws went into effect. Additionally, there

are county-stack and quarter-stack fixed effects included. This procedure returns a weighted average of the individual stack treatment effects as the overall impact of the T21 policies. This strategy has the advantage of allowing the regressions to be run at the level of the individual observations, turning the specification into a linear probability model. This allows me to include individual-level controls for the mothers age, education, and race, which I supplement with controls for whether there is an e-cigarette minimum legal sale age (MLSA) in the state, the cigarette excise tax in 2020 real dollars, as well as indicators for whether there is an e-cigarette tax and whether smoking is banned in indoor bars.

Another benefit of this approach is that it prevents observations from smaller states like Vermont and Wyoming from exerting more influence on the regression than observations from larger states, as in the state-level SDID where states receive equal weight despite representing vastly different number of births. In each specification of this form, standard errors are clustered at the stack-county level. Each stack will include observations from four full quarters before the T21 law went into effect, as well as six quarters after implementation.

This approach also allows me to run event-study specifications, which give a separate treatment coefficient to each quarter leading up to the implementation of treatment, as well as each quarter after treatment goes into effect. This allows me to trace out the evolution of the treatment effect, and to assess the validity of the parallel trends assumption. Because these regressions are ran at the individual level, this also enables me to include counties which were treated before the state-level laws were passed.

4. Results

4.1. SDID Estimates

Table 1 displays the SDID results for the percentage of births which occur to a mother who smoked cigarettes before pregnancy, as well as in each of the three trimesters. Below each estimate is the baseline (2014) rate for the ever-treated group in the sample, along with the implied percentage change based on the coefficient estimate. In each case, the SDID estimates are *positive* and statistically insignificant. While it is unlikely that T21 laws, which make it more difficult for young people aged 18-20 to access tobacco products, could have actually caused an increase in prenatal smoking for this same age group, the fact that the estimates are positive allows me to rule out even modest decreases in smoking as a result of the policy.

I am able to rule out reductions greater than 0.7 percentage points (4.6% of

the baseline rate of 14.9%) for smoking before pregnancy, .04 percentage points (3.7%) for smoking in the first trimester, 0.5 percentage points (5.7%) in the second trimester, and 0.5 percentage points in the third trimester (6.0%) in the third trimester. Finally, column estimates the effect of T21 laws on the number of trimesters in which 18-21 year old mothers smoke any cigarettes. Once again, the point estimate is positive and insignificant, and allows me to rule out reductions in smoking of .016 trimesters, which is 5.7% of the base rate of .28.

Table 2 replaces the indicator for smoking any cigarettes in each period with the average daily number of cigarettes smoked, including zeroes for non-smokers. It could be that there is no reduction in the share of pregnant mothers that are smoking, but there could be reductions on the intensive margin measure of how much they are smoking. However, all of the estimates in Table 2 are once again positive and insignificant. The estimate on pre-pregnancy smoking implies an increase of 0.0327 cigarettes per day, which is approximately 1.7% of the baseline rate of 1.89. The estimates for each trimester indicate increases of between .03 and .06 cigarettes per day, which are increases of between 4-8% off of the base rates. Once again, this allows me to rule out even relatively modest reductions on the intensive margin measure of tobacco consumption. I can rule out reductions larger than .17 cigarettes per day before pregnancy (8.7% of the base rate of 1.89 cigarettes per day), reductions larger than .053 cigarettes per day in the first trimester (5.0% of the base rate of 1.06), reductions larger than .037 cigarettes per day in the second trimester (5.0% of the base rate of 0.73), and reductions larger than .030 in the third trimester (4.7% of the base rate of .63)

The estimates in Tables 1 and 2 fail to find any evidence that T21 laws were effective in reducing self-reported smoking among 18-21 year old pregnant mothers. Still, it is possible that small reductions did occur, which could still have positive impacts on the birth outcomes among newborns exposed to T21 laws in utero. Table 3 investigates this possibility by reestimating the SDID specifications on birthweight, the percentage of pregnancies deemed 'low birth weight, average gestational length, and the percentage of births which take place after fewer than 27 weeks of gestation. As with the previous specifications, results for all four outcomes are small in magnitude and statistically insignificant, with three of the four estimates suggesting that birth outcomes actually got worse in treated areas. Overall, I am unable to find evidence that T21 laws have led to substantial improvements either prenatal smoking or birth outcomes.

4.2. Robustness Checks

Since non-high school graduates are more likely to be smokers and more likely to smoke more heavily, they may also be more likely to be impacted by T21 laws. Ta-

ble 4 looks into this by repeating the SDID specifications on 18-21 year old mother who have less than a high school degree. Coumns one and two look at whether the mother smoked at all before and during pregnancy, while columns three and four look at the average number of daily cigarettes the mothers consumed. The estimates are once again all insignificant, though the two estimates on the extensive margin question of whether the mother smoked at all are positive.

Still, I can rule out reductions of more than 3 percentage points for smoking before pregnancy (15% of the baseline rate of 20%) and reductions of greater than 2.7 percentage points on smoking during pregnancy (16.1% of the base rate of 16.7%). The intensive margin estimates on average cigarette consumption are both positive, allowing me to rule out reductions of larger than .4 cigarettes per day before pregnancy (15% of the base rate of 2.66) and reductions of larger than .18 cigarettes per day during pregnancy (13.7% of the base rate of 1.31). Even among the group most likely to be smoking, T21 laws do not appear to have generated substantial reductions in smoking before and during pregnancy.

Although 29 states have T21 laws by the end of 2020, each state law is unique and varies in several dimensions, including the type of penalties that are faced by consumers or retailers who violate the law, the strength of enforcement of the law, and the extent of the retail licensing laws in place. The Preventing Tobacco Addiction Foundation has gone through each state law and assigned it a grade of A, B, C, or F, with A signaling the law is likely to be the most effective at preventing teenage tobacco use and F signaling the law is the least likely to be effective. Only five states received grades of A, while four received Bs, 10 received Cs, and 10 received Fs.

Table 5 reestimates my main SDID specification using only states that received strong grades on their laws, and dropping all states which received lower grades, in order to test whether the strength of the T21 law matters. Panel 1 looks at all states which received grades of either A or B, while Panel 2 looks only at states which received grades of A. In both panels, all estimates are small and statistically insignificant, with seven of the eight estimates being positive in magnitude. This suggests that even the strongest T21 laws may not be effective at reducing smoking among young pregnant mothers.

Many different states, across the political divide and with diverse populations, have implemented T21 laws in the US, and each one has the potential to impact prenatal smoking differently. In order to understand how different populations respond to these restrictions, Figure 3 displays SDID estimates for each treated state individually. This is done using the same regression specification as in Tables 1 and 2, only I iteratively drop all treated states except for the one in question on each round. It is possible, for example, that a single state with a large increase in smoking that coincided with a T21 law, could drive the overall SDID estimate up,

even it the average treatment effect were negative.

This does not appear to be the case, however, as the state-level estimates all mainly hover around zero. For smoking before pregnancy, 10 of the 19 estimates are positive, while nine are negative. There is one positive and significant estimate (Vermont), and one negative and significant estimate (New Hampshire), but little in the way of an overall trend. The story is similar in the other three graphs, which display estimates on smoking during pregnancy, as well as average daily cigarettes before and during pregnancy.[4]

Appendix Figure A.7 builds on this with a 'leave one out' analysis, where each treated state is iteratively dropped before running the main specifications. In all cases, the resulting coefficient estimates are statistically insignificant, with positive estimates in 74 of the 76 estimates in total. Because there is ambiguity about treatment status for mothers who are on the threshold of aging into or out of treatment, I estimate my main specifications for 18-20, 19-21, and 18-19 year old mothers separately in Appendix Table A.1. All of the estimates are consistent with my main results, and none of the twelve coefficient estimates is statistically significant, with the smaller p-value at .352. Finally, I also estimate my main specification on mothers from each single age group from 17 to 24, with coefficients displays in Appendix Figure A.8. Once again, I am unable to find evidence that T21 laws reduced prenatal smoking for any single age group, and that general smoking trends in T21 treated states appear to be similar for treated mothers and mothers who are aged out before the policy begins.

5. Conclusion

This paper uses restricted-access data from the National Vital Statistics System to investigate whether Tobacco 21 laws reduce prenatal smoking among mothers who are 18-20 while pregnant. In a similar vein to Cotti, DeCicca, and Nesson (2024), I am able to combine self-reported smoking data with actual biological evidence of smoking. If T21 laws reduce smoking among this group, we could reasonably expect to see both a reduction in self-reported smoking as well as improvements in birth outcomes in treated areas, since smoking has clearly substantiated deleterious effects on developing fetusus. Using both the synthetic difference-in-

[4]For smoking during pregnancy, there are again 10 positive and nine negative coefficients, with South Dakota being positive and significant, while New Hampshire and Indiana are both negative and significant. For average cigarettes before pregnancy, there are nine positive and 10 negative coefficients, with South Dakota and Wyoming both positive and significant, while only Vermont is negative and significant. For average daily cigarettes during pregnancy, there are 10 positive and nine negative coefficients, with only Wyoming, which is positive, being statistically significant.

differences and stacked event-study strategies, I fail to find any evidence that these laws had an impact on either self-reported smoking among pregnant mothers or their eventual birth outcomes.

It is perhaps surprising that I am unable to find reductions in self-reported smoking, as several other papers have found such reductions among the targeted age groups. The major difference between the data used in this study and the rest of the literature is that I am focusing on birthing persons instead of survey respondents which are designed to be representative of the populations they come from. The mothers in my survey are responding to questions just after giving birth to their child. They may be less likely to under-report their true smoking behavior in a medical setting, because they may want to make sure the medical staff is aware and able to diagnose any issues that might be related to their smoking. These mothers are also fresh out of childbirth, which is an intense and life-changing experience to say the very least. New mothers may therefore be less influenced by 'social desirability bias' and the other common biases that can influence traditional survey data.

Pregnant mothers are also a uniquely selected group, and they may be particularly unlikely to be influenced by T21 laws. They smoke at lower rates than the general population at baseline, likely due to both the harms that can accrue to their unborn child and to the social stigma which is attached to prenatal smoking. Those mothers who do still smoke while pregnant in the absence of T21 laws are likely to be more seriously addicted than the average smoker, meaning they may have an extra incentive to find a way to continue to procure tobacco event if they cannot legally purchase it on their own.

Table 1 — Synthetic Difference-in-Differences Estimates of the Effect of Tobacco 21 Laws on Prenatal Smoking During Various Stages of Pregnancy for 18-21 Year Old Mothers, 2011-2019

	(1) Pre-Preg.	(2) 1st Tri.	(3) 2nd Tri.	(4) 3rd Tri.	(5) # of Tris
Treat x Post	0.00325	0.00437	0.00361	0.00328	0.00993
	(0.00626)	(0.00409)	(0.00416)	(0.00439)	(0.0110)
Base Rate (2014)	14.9%	10.9%	8.8%	8.3%	.280
% Change	2.2%	4.0%	4.1%	3.9%	3.5%
Observations	1,064	1,064	1,064	1,064	1,064

Note: This table displays synthetic difference-in-differences estimates of the effect of Tobacco 21 laws on smoking behavior of pregnant women between the ages of 18 and 21, before and during their pregnancy. The first column estimates the effect of these laws on the percent of births which occur to a mother who smoked prior to pregnancy, while the second through fourth columns iteratively estimate the effect on percent of births to a mother who smoked in the first, second, and third trimesters, respectively. The fifth column estimates the effect on the total number of trimesters in which mothers smoke any cigarettes. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 2 — Synthetic Difference-in-Differences Estimates of the Effect of Tobacco 21 Laws on Average Daily Cigarettes Smoked During Various Stages of Pregnancy for 18-21 Year Old Mothers, 2011-2019

	(1) Pre-Preg.	(2) 1st Tri.	(3) 2nd Tri.	(4) 3rd Tri.
Treat x Post	0.0327	0.0478	0.0481	0.0522
	(0.104)	(0.0515)	(0.0432)	(0.0407)
Baseline Rate (2014)	1.89	1.06	0.73	0.63
Percentage Change	1.7%	4.5%	6.5%	8.3%
Observations	1,064	1,064	1,064	1,064

Note: This table displays synthetic difference-in-differences estimates of the effect of Tobacco 21 laws on smoking behavior of pregnant women between the ages of 18 and 21, before and during their pregnancy. The first column estimates the effect of these laws on the average number of daily cigarettes consumed by mothers prior to pregnancy, while the second through fourth columns iteratively estimate the effect on average daily cigarette consumption in the first, second, and third trimesters, respectively. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 3 — Synthetic Difference-in-Differences Estimates of the Effect of Tobacco 21 Laws on Smoking Behavior of 18-21 Year Non-High School Graduate Mothers, 2011-2020

	(1) Birthweight	(2) Low BW	(3) Gestation	(4) Preterm
Treat x Post	4.009	-0.000704	-0.00518	0.00179
	(5.813)	(0.00330)	(0.0225)	(0.00318)
Baseline Rate (2014)	3,211	8.33%	38.62	8.95%
Percentage Change	0.1%	0.8%	0.00%	2.0%
Observations	1,064	1,064	1,064	1,064

Note: This table displays synthetic difference-in-differences estimates of the effect of Tobacco 21 laws on birth outcomes to women between the ages of 18 and 21, before and during their pregnancy. The first columns estimate the effect of these laws on whether the mother smoked at all before and during pregnancy, while the third and fourth columns iteratively estimate the effect on average daily cigarette consumption before and during pregnancy. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 4 — Synthetic Difference-in-Differences Estimates of the Effect of Tobacco 21 Laws on Smoking Behavior of 18-21 Year Non-High School Graduate Mothers, 2011-2020

	(1) Smoked Pre	(2) Smoked During	(3) Cigs Pre	(4) Cigs During
Treat x Post	-0.00639	-0.00260	0.0618	0.0598
	(0.0123)	(0.0125)	(0.239)	(0.123)
Baseline Rate (2014)	20.0%	16.7%	2.66	1.31
Percentage Change	3.2%	1.6%	2.3%	4.6
Observations	1,036	1,036	1,036	1,036

Note: This table displays synthetic difference-in-differences estimates of the effect of Tobacco 21 laws on smoking behavior of pregnant women between the ages of 18 and 21 who have not graduated high school, before and during their pregnancy. The first columns estimate the effect of these laws on whether the mother smoked at all before and during pregnancy, while the third and fourth columns iteratively estimate the effect on average daily cigarette consumption before and during pregnancy. * $p < .05$, ** $p < .01$, *** $p < .001$.

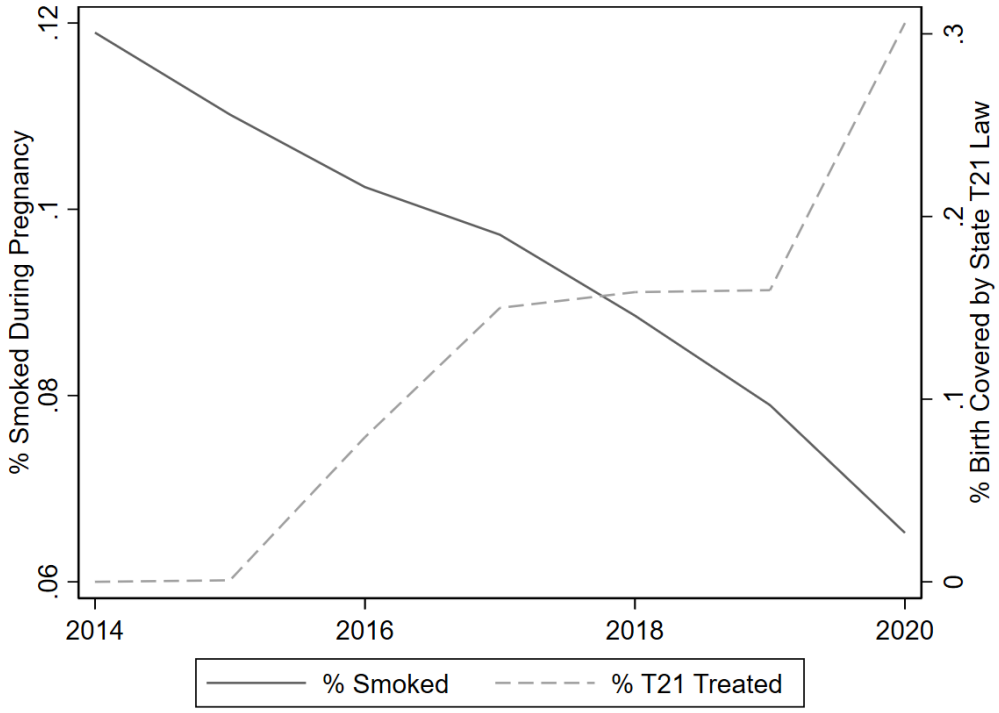
Table 5 — Synthetic Difference-in-Differences Estimates of the Effect of Tobacco 21 Laws with Grades of A or B on Smoking Behavior of 18-21 Year Old Mothers, 2011-2020

Panel 1: Grade A or B Only				
	(1)	(2)	(3)	(4)
	Smoked Pre	Smoked During	Cigs Pre	Cigs During
Treat x Post	0.00929	0.00615	0.171	0.00974
	(0.0119)	(0.0123)	(0.197)	(0.101)
Observations	448	448	448	448

Panel 2: Grade A Only				
	(1)	(2)	(3)	(4)
	Smoked Pre	Smoked During	Cigs Pre	Cigs During
Treat X Post	0.00434	0.00156	0.0210	-0.0190
	(0.00816)	(0.00772)	(0.221)	(0.0637)
Observations	336	336	336	336

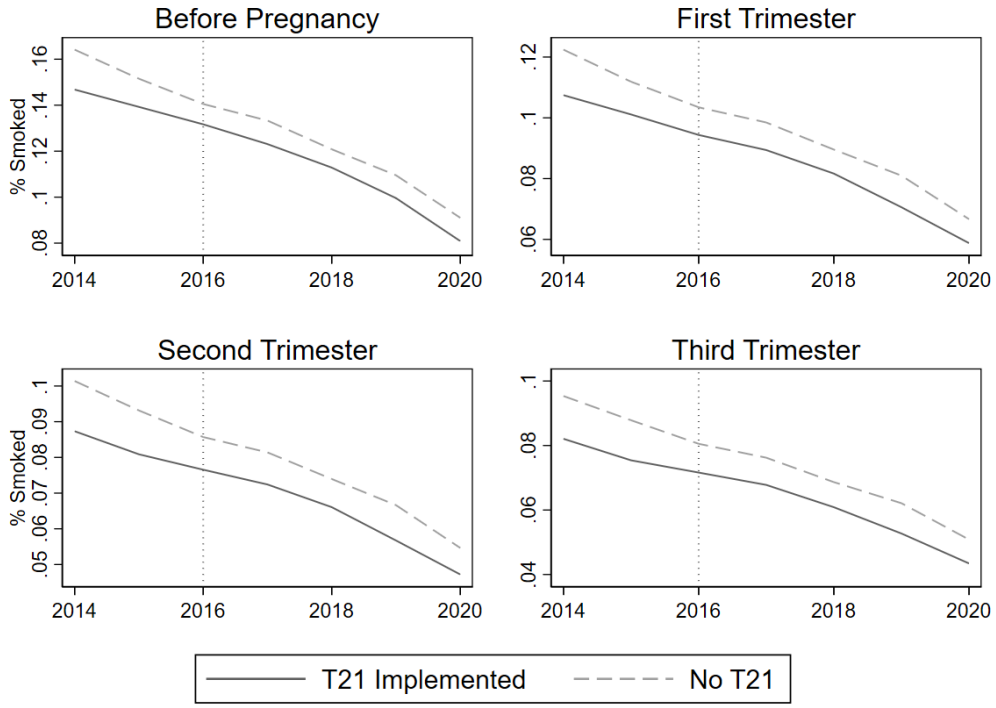
Note: This table displays synthetic difference-in-differences estimates of the effect of Tobacco 21 laws on smoking behavior of pregnant women between the ages of 18 and 21, before and during their pregnancy. The first columns estimate the effect of these laws on whether the mother smoked at all before and during pregnancy, while the third and fourth columns iteratively estimate the effect on average daily cigarette consumption before and during pregnancy. The two panels of the figure show estimates based on the grade each treated state received on their law from the Preventing Tobacco Addiction Foundation. Panel 1 shows the estimates for states which received a grade of A or B, while Panel 2 shows estimates only for states that received a grade of A. * $p < .05$, ** $p < .01$, *** $p < .001$.

Figure 1 — Time Series of the Percentage of 18-21 Year Old Mothers Who Report Smoking During Pregnancy and the Percent of All Births Covered by Tobacco 21 Laws, 2014-2020



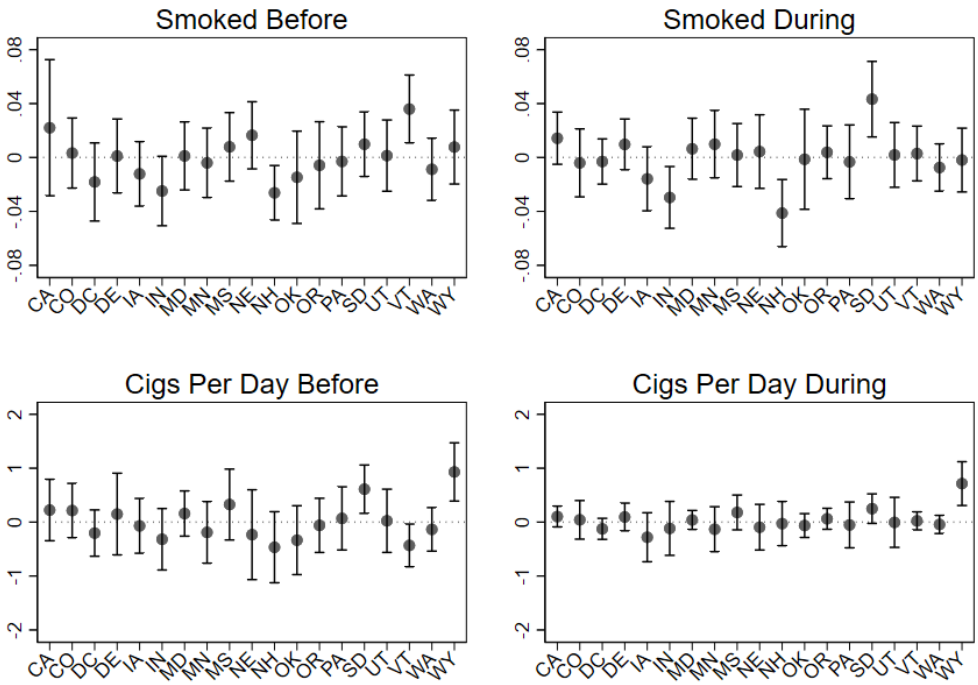
Note: This figure displays the time series of the percentage of births in which the mother reports smoking at any point during the pregnancy, compared with the percentage of all births in which the mother lives in a county that is covered by a Tobacco 21 law in the year in which the birth takes place. This figure uses natality data from the National Vital Statistics System (NVSS) as well as treatment assignment data from Tobacco21.org.

Figure 2 — Time Series of Smoking Behavior During Pre-Pregnancy and Each Trimester, by Treatment Status: 2014-2020



Note: This figure displays the time series of the percentage of all 18-21 year old mothers who report having smoked any cigarettes during each stage of pregnancy, in areas that had Tobacco 21 laws by 2020 compared with those that did not. The top left graph displays the share which smoked prior to pregnancy, while the top right through bottom right display the share who smoked during each of the first through third trimesters, respectively.

Figure 3 — Individual SDID Regression Coefficients for Each Tobacco 21 Treated State: 2011-2020



Note: This figure displays individual state-level estimates of the effect of Tobacco 21 laws on self-reported smoking behavior of pregnant mothers, using restricted-access data from the National Vital Statistics System. The top left graph displays the effect on whether mothers in each treated state smoked at all prior to pregnancy, the top right graph displays the effect on whether the mothers smoked at all during pregnancy, while the bottom two graphs displays intensive-margin estimates on the average number of cigarettes smoked each day before and during pregnancy, respectively.

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A. Online Appendix Figures (Not for Publication)

Table A.1 — Synthetic Difference-in-Differences Specifications of the Effect of Tobacco 21 Laws on Prenatal Smoking for Various Age Groupings - 2011-2021

Panel A: 18-20 Year Olds

	(1) Smoked Before	(2) Smoked During	(3) Cigs Before	(4) Cigs During
Treat X Post	0.00330	0.00298	-0.00401	0.00754
	(0.00656)	(0.00582)	(0.108)	(0.0494)
Observations	1,064	1,064	1,064	1,064

Panel B: 19-21 Year Olds

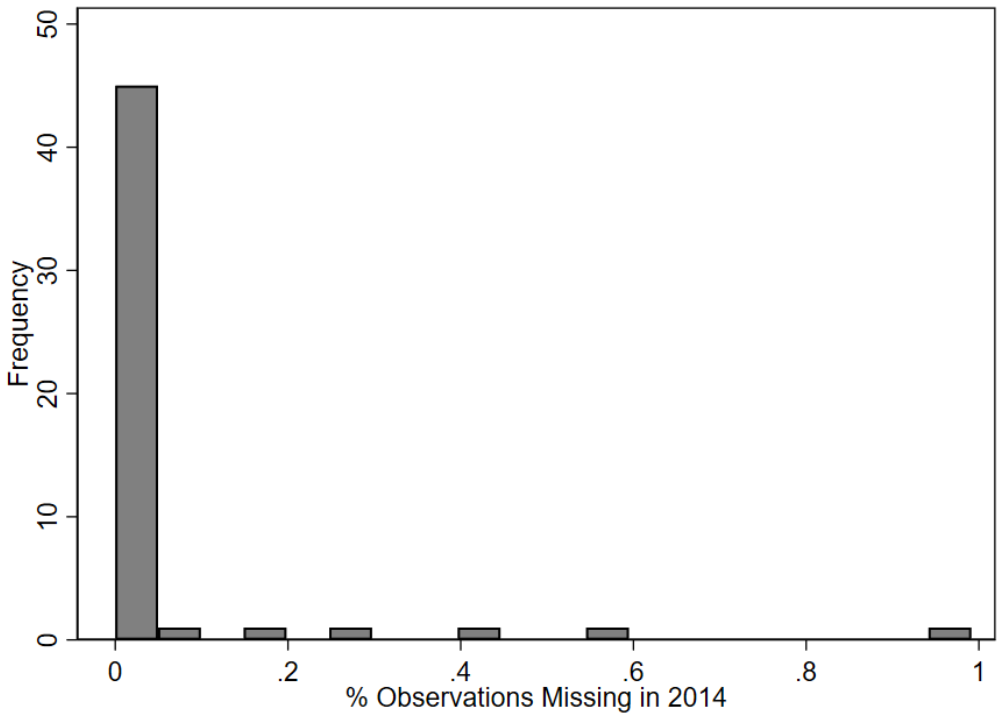
	(1) Smoked Before	(2) Smoked During	(3) Cigs Before	(4) Cigs During
Treat X Post	0.00227	0.00421	-0.0209	0.0270
	(0.00698)	(0.00452)	(0.115)	(0.0424)
Observations	1,064	1,064	1,064	1,064

Panel C: 18-19 Year Olds

	(1) Smoked Before	(2) Smoked During	(3) Cigs Before	(4) Cigs During
Treat X Post	-0.000355	0.00467	-0.0439	-0.000229
	(0.00852)	(0.00873)	(0.173)	(0.0542)
Observations	1,064	1,064	1,064	1,064

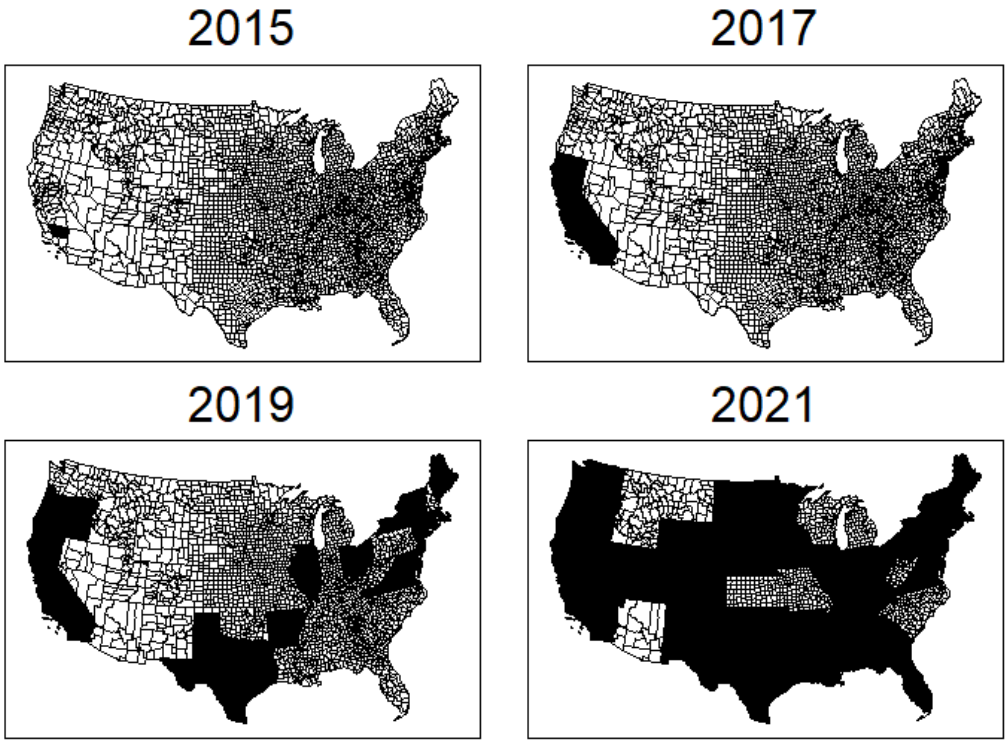
Note: This table displays stacked difference-in-differences estimates for the effect of Tobacco 21 laws on prenatal smoking behavior among various age groupings of mothers, using restricted access data from the National Vital Statistical System (NVSS). Panel A displays estimates on mothers who are 18-20 when the child is born, Panel B displays estimates for 19-21 year old mothers, while Panel C displays estimates for 18-19 year old mothers. The first columns looks at whether the mother smoked at all prior to pregnancy. The second columns looks at whether the mother smoked during pregnancy. The third column looks at the average daily number of cigarettes the mother smoked before pregnancy, while the fourth column looks at the average daily number of cigarettes consumed during pregnancy. * $p < .05$, ** $p < .01$, *** $p < .001$.

Figure A.1 — Histogram of the Percentage of Smoking Observations Missing in Each State in 2014



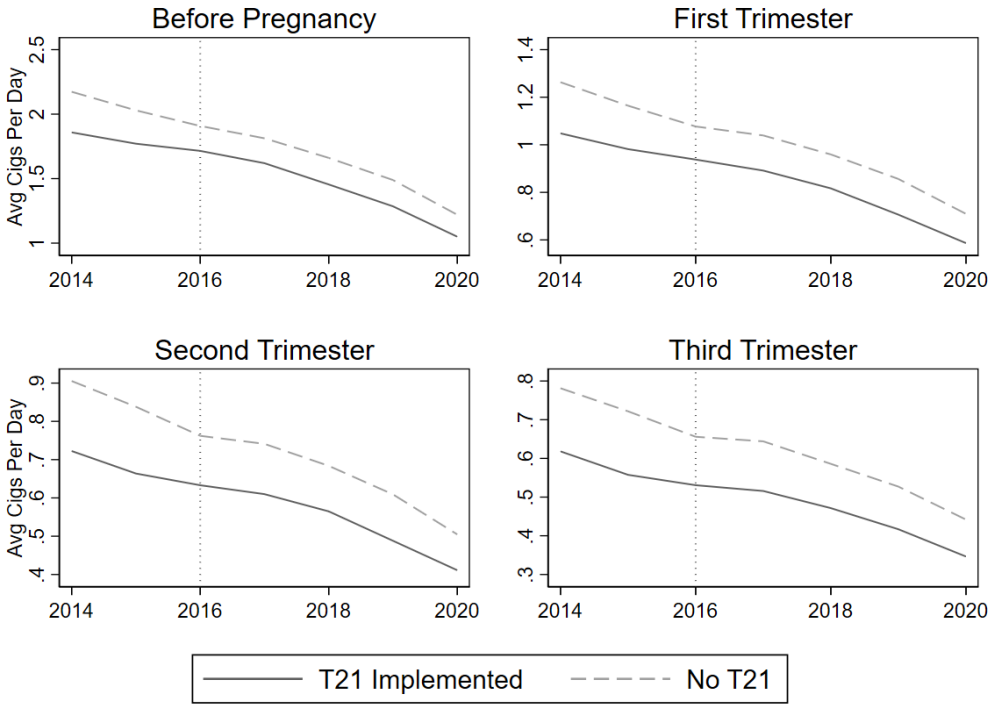
Note: This figure displays a histogram of the percentage of observations which are missing for the smoking variables, for each state in 2014, using natality data from the National Vital Statistics System.

Figure A.2 — Maps of the Evolution of Tobacco 21 Law Implementation in the United States: 2015-2021



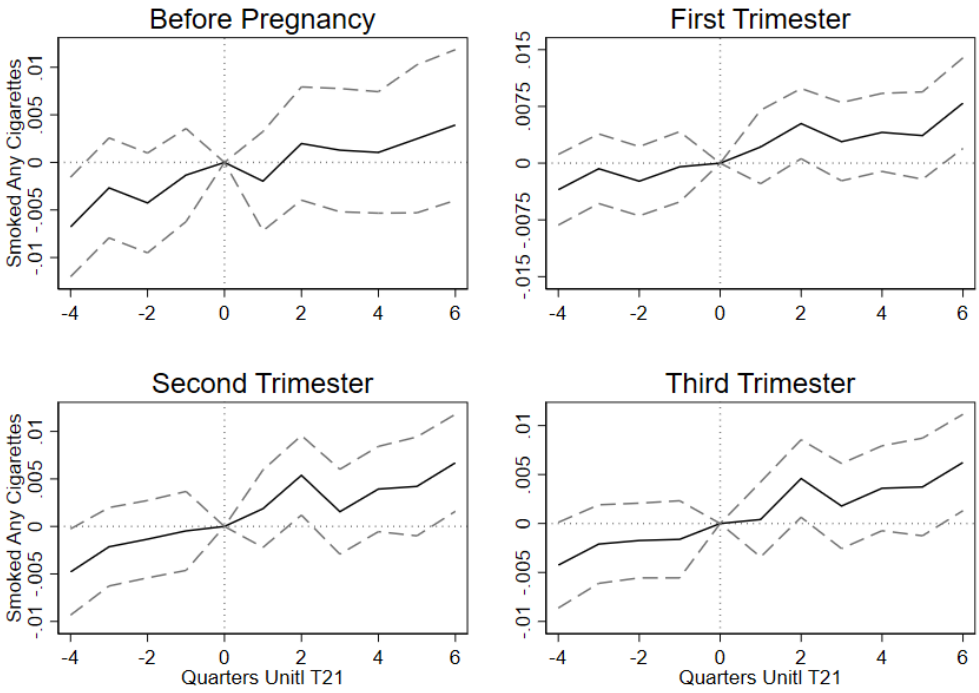
Note: This figure includes maps which display the evolution of the rollout of Tobacco 21 laws to various counties and states across the United States. Counties in black are areas that have a Tobacco 21 law in place by December 31 of that year.

Figure A.3 — Time Series of Average Cigarettes per Day During Pre-Pregnancy and Each Trimester, by Treatment Status: 2014-2020



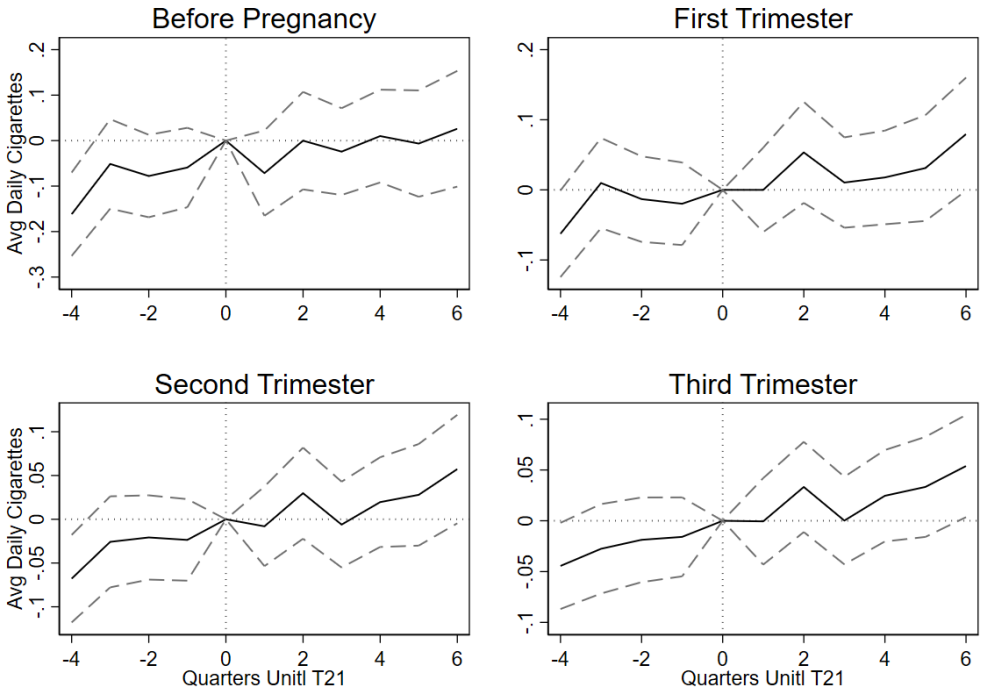
Note: This figure displays the time series of the average number of cigarettes per day of all 18-21 year old mothers, in areas that had Tobacco 21 laws by the end of 2020 compared with those that did not. The top left graph displays the share which smoked prior to pregnancy, while the top right through bottom right display the share who smoked during each of the first through third trimesters, respectively.

Figure A.4 — Stacked Event-Study Estimates of the Effect of Tobacco 21 Laws on Smoking Before and During Pregnancy: 2014-2021



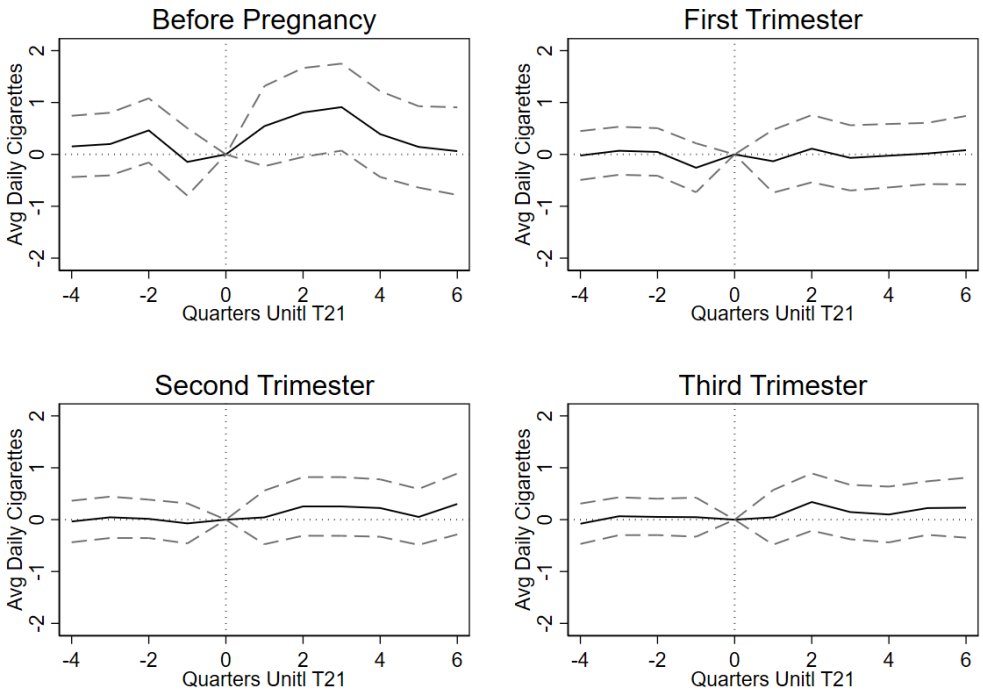
Note: This figure displays event-study estimates of the effect of Tobacco 21 laws on self-reported smoking behavior of pregnant mothers, using restricted-access data from the National Vital Statistics System.

Figure A.5 — Stacked Event-Study Estimates of the Effect of Tobacco 21 Laws on Average Daily Cigarette Consumption Before and During Pregnancy: 2014-2021



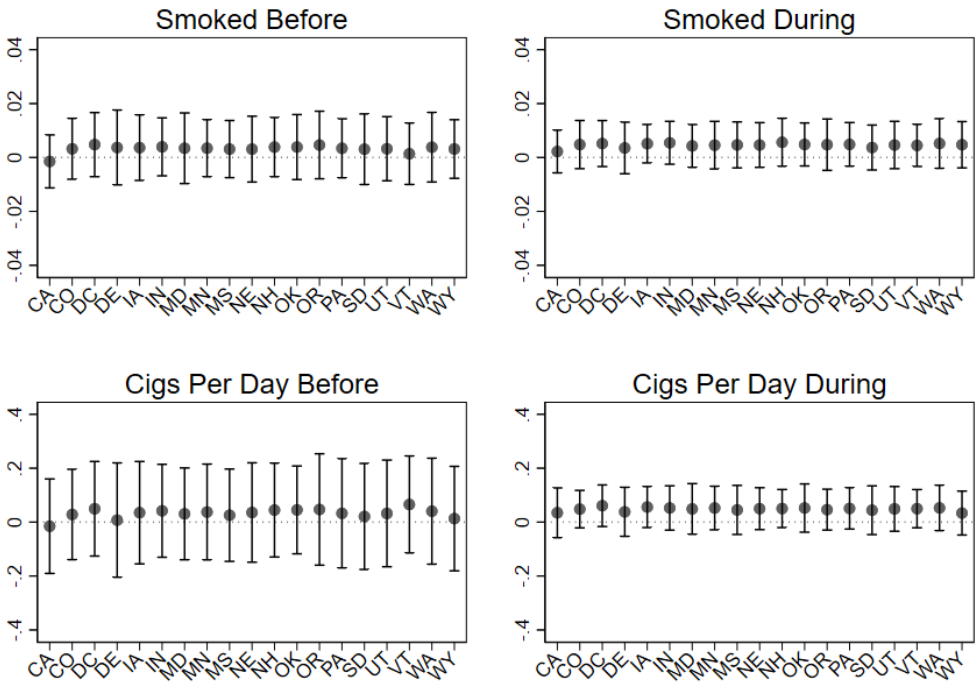
Note: This figure displays event-study estimates of the effect of Tobacco 21 laws on self-reported smoking behavior of pregnant mothers, using restricted-access data from the National Vital Statistics System.

Figure A.6 — Stacked Event-Study Estimates of the Effect of Tobacco 21 Laws on Average Daily Cigarette Consumption Before and During Pregnancy, Conditional on Some Smoking: 2011-2020



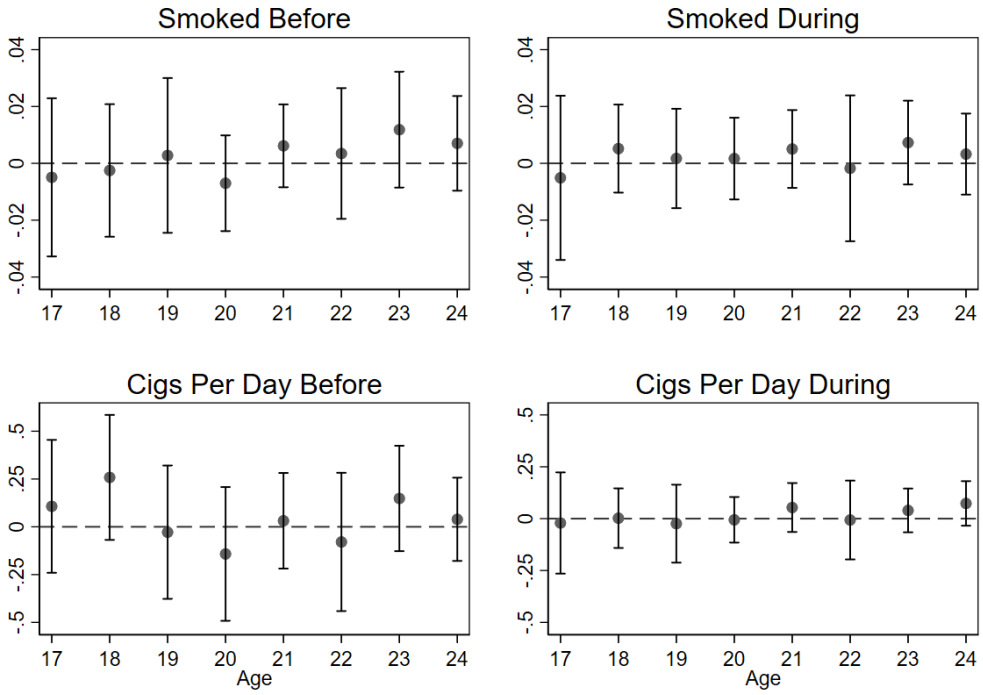
Note: This figure displays event-study estimates of the effect of Tobacco 21 laws on self-reported smoking behavior of pregnant mothers, using restricted-access data from the National Vital Statistics System.

Figure A.7 — Leave One Out SDID Regression Coefficients for Each Tobacco 21 Treated State: 2011-2020



Note: This figure displays estimates of a 'leave one out' analysis, where each individual state is dropped before estimating the effect of Tobacco 21 laws on self-reported smoking behavior of pregnant mothers, using restricted-access data from the National Vital Statistics System. The top left graph displays the effect on whether mothers in each treated state smoked at all prior to pregnancy, the top right graph displays the effect on whether the mothers smoked at all during pregnancy, while the bottom two graphs displays intensive-margin estimates on the average number of cigarettes smoked each day before and during pregnancy, respectively.

Figure A.8 — Single Age Group SDID Regression Coefficients for Mothers Aged 17-24: 2011-2020



Note: This figure displays estimates of the effect of Tobacco 21 laws on self-reported smoking behavior of pregnant mothers from each specific year of age from 17 to 24, using restricted-access data from the National Vital Statistics System. The top left graph displays the effect on whether mothers in each treated state smoked at all prior to pregnancy, the top right graph displays the effect on whether the mothers smoked at all during pregnancy, while the bottom two graphs displays intensive-margin estimates on the average number of cigarettes smoked each day before and during pregnancy, respectively.

B. Stacked DiD Estimates

Results from estimating event-studies on the likelihood that a mother smoked during each specific phase of her pregnancy are displayed in Appendix Figure A.4. Similar to the findings to SDID, there is no evidence that T21 laws led to reductions in smoking at any point before or during pregnancy. In all four cases, almost all of the posttreatment lags are positive, though most are statistically insignificant. There does appear to be some differential trends prior to the implementation of the laws, with treated counties slightly trending upward in the quarters leading up to implementation. Part of the reason the treatment lags are positive could simply be due to a continuation of the trends which were already taking place. Visually, this interpretation lines up with the evidence from Appendix Figure 2, which showed that T21 treated counties had much lower rates of smoking throughout the sample period, but that the untreated areas appeared to be slowly converging, even before any T21 laws were in place. The lack of support for the parallel trends evidence suggests that the SDID estimates are likely more reliable than the stacked DiD, though both fail to find evidence of reductions in smoking.

Figure Appendix A.5 replaces the binary smoking variable with the average daily number of cigarettes consumed, and the story is roughly the same. Once again, the pretreatment leads are mostly small in magnitude and statistically insignificant, though there does appear to be a suggestive upward trend. Following treatment, there appears to be no movement in either direction for smoking prior to pregnancy, with modest but insignificant increases occurring during each of the three trimesters, though these again look like a continuation of trends which were occurring before the T21 laws. Appendix Figure A.6 displays similar estimates of the average daily number of cigarettes consumed among those for whom some positive amount of smoking took place. Here, there is no evidence of divergence, either before or after the T21 implementation.