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Essays on Cigarette Regulation, Prenatal Smoking, and Cross-Border Shopping

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ABSTRACT

Essays on Cigarette Regulation, Prenatal Smoking, and Cross-Border Shopping

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In my dissertation, I study the impact of cigarette regulation on two main outcomes: prenatal smoking and cross-border shopping. The first chapter of my dissertation concerns how state-level cigarette tax increases affect cross-state border shopping for cigarettes. To estimate this relationship, I use high-resolution census block group-by-month cellphone tracking data from Safegraph. I estimate a Callaway and Sant'Anna (2021) difference-in-differences model that accommodates my unique setting in which the tax increases I consider become effective at different times throughout the full length of the study. The use of this data allows me to avoid measurement error that could be present in self-reported data. I find that the median census block group (CBG) sent 0.53 more cross-border shoppers per month in response to a cigarette tax increase (19% increase from the pre-tax cross-border shopping mean). Further, I show that CBGs with many low educated adults and rural CBGs send substantially more cross-border shoppers than their respective counterparts. Performing a back-of-the-envelope calculation, I estimate that cigarette tax increases before 2019 increased cigarette tax revenue leakage in 2019 by $531,581 in Oklahoma and $9,084,824 in Kentucky. In sum, these results suggest that cross-border shopping remains an ongoing challenge for tobacco control policy efforts and for reducing tobacco-related disparities.

The second chapter of my dissertation, written with my committee chair, studies how cigarette taxes and indoor smoking restrictions impact the propensity and intensity of smoking by a pregnant woman (prenatal smoking). We use data from the Nation Vital Statistics System to get smoking data from almost every pregnant woman in the USA from 1995 to 2018. To reduce bias from dynamic and heterogeneous treatment effects, we estimate a modified stacked difference-in-differences model that accommodates our unique policy environment in which policies are often gradually strengthened over time and dose increases vary. We find some evidence that indoor smoking restrictions modestly decreases prenatal smoking and, contrary to previous literature, that cigarette taxes do not significantly impact prenatal smoking. Indoor smoking restrictions may be better from a health-equity perspective than cigarette taxes.

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Maxwell Christopher Chomas

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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May, 2023

Dedication

I dedicate this dissertation to my loving wife, parents, and brother.

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I could not have completed this dissertation without the help of many people that have encouraged and supported me along the way. I would first like to thank my wife, Seolah Kim, for the advice and love she has given me. Seolah has been a light in my life ever since we met and without her I do not believe I would be the same person I am today. I would also like to thank my parents, Mark and Suzanne Chomas, for their constant support of my academic endeavors ever since I was enrolled in school. The values of persistence and level-headedness they have instilled in me has allowed me to accomplish more than I could have ever imagined. Further, my brother Michael Chomas has been an excellent role model throughout my life. Whether he was leading by example or in conversation with me, Mike has always encouraged me to pursue what I consider important in life. My aunts Alice Fannin and Lynn Knoll have also been immeasurably supportive of me throughout my Ph.D., whether it be in conversation or in the many packages they have sent me filled with food and a note of encouragement.

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Table of Contents

List of Tables

List of Figures

Introduction

In my dissertation, I study the impact of cigarette regulation on two main outcomes: prenatal smoking and cross-border shopping. The first chapter of my dissertation concerns how state-level cigarette tax increases affect cross-state border shopping for cigarettes. This is an important topic as tobacco use leads to over 7 million deaths a year worldwide (World Health Organization 2017). To promote public health, governments then have a strong incentive to reduce cigarette use by implementing cigarette taxes. Besides promoting public health, these taxes also raise substantial revenue and offset what are already substantial public healthcare expenditures (Xu et. al. 2015) on negative health outcomes caused by smoking. The health and fiscal benefits of cigarette taxation, however, are contingent on individuals quitting or reducing cigarette consumption without avoiding taxation.

It has been shown that smokers use a variety of tactics to avoid cigarette tax hikes and so continue their current cigarette consumption patterns. One way that smokers avoid state-level cigarette tax hikes is through cross-state border (from here on cross-border) shopping to lower tax states. Understanding the magnitude of cross-border shopping in response to a cigarette tax hike is important for three main reasons. First, it provides guidance to understand the amount of tax-revenue a state is losing to bordering states when it decides to raise its cigarette tax. Secondly, estimating heterogenous effects by geographic and demographic values can give an insight into which people are spending the time to travel to another state to avoid cigarette taxes. Finally, a significant increase in cross-border shopping would indicate that at least some smokers are continuing their smoking habits even after the state tax hike. This implies that the state they live in may incur the health expenditures cross-border shoppers who smoke have later in life, while not being able offset some of these costs with the tax revenue they would have generated had the cross-border shopper shopped in-state.

Previous papers focusing on cross-border shopping in response to a cigarette tax have been confined to urban areas (Lovenheim 2008, DeCicca et. al. 2013) or have had to use a broad definition of a consumer's home residence (Harding et. al. 2012).[1] Further, Lovenheim (2008), Harding et. al. (2012), and DeCicca et. al. (2013) are all based on survey data where the respondent must self-report cross-border shopping for cigarettes or cigarette consumption. Previous work (Connor et. al. 2009), however, has shown that survey respondents tend to systematically underreport smoking status. As an alternative data source that can be used to identify likely cross-border shopping, I use a cellphone tracking dataset provided by SafeGraph.

The SafeGraph data covers nearly every census block group (CBG)[2] in the USA and does not rely on self-reporting. It further gives the home CBG of most of the devices that it tracks, which is a precise definition of a consumer's home residence. This data is then well-suited for estimating how much cross-border shopping changes in response to a state-level cigarette tax for both urban and rural areas. The Safegraph data also has important drawbacks when it comes to estimating cross-border shopping. The first of these is that I cannot observe what an individual purchased when they enter a retailer. This implies that my measurement of cross-border shopping will include visits that do not involve the purchasing of cigarettes. However, if the visits that do not pertain to cigarette purchases are relatively constant when the cigarette tax becomes effective, my estimates will not be contaminated by this issue. I provide evidence that this is the case through a placebo test and show that the number of visitors to retailers that do

[1] DeCicca et. al. (2012) use urban areas only so they can make use of Metropolitan Statistical Area definitions, which is the lowest geographical identifier they have in the data (TUS-CPS) they use. Harding et. al. (2012), using the Neilson Homescan data, have geographical identifiers down to the census tract level.
[2] Census block groups are the second lowest level of geographical identification provided by the Census Bureau. They generally contain between 600 and 3,000 people.

not sell cigarettes is unchanged when the cigarette taxes in my sample become effective. Secondly, the SafeGraph data does not provide information about the owners of the cellphones being tracked (besides their home CBG), nor is it known how SafeGraph chooses which cellphones to track (besides that the cellphones must be compatible with applications).

To estimate the effect of the tax increases that occur at distinct times on cross-border shopping, I use Callaway and Sant'Anna's (2021) difference-in-differences model. This use of this model is important as it is robust to potential bias introduced when a policy is implemented at distinct times or in a so-called "staggered rollout" fashion (Goodman-Bacon 2021). I consider cigarette tax increases in 3 distinct states (Illinois, Kentucky, and Oklahoma) over the period of January 1st, 2018 through December 31st, 2019. The control states for these treated states are bordering at least one treated state, do not have a tax change over the sample period, and have a lower tax level than the bordered treated state both before and after the tax effective date. I then consider cross-border shopping into the treated states as the control state's outcome.

The second chapter of my dissertation estimates how a pregnant woman's propensity and intensity of smoking (prenatal smoking) is impacted by two cigarette regulations: cigarette taxes and indoor smoking restrictions. This topic is important as prenatal smoking has been linked to infant death within 28 days of birth, low-birth weight, and premature birth (U.S. Department of Health and Human Services, 2014). Further, it has been established that most prenatal smokers have a high school degree or less. If cigarette policies can cause sustained cessation for pregnant women with lower education, this could help reduce education-related health disparities for the pregnant woman and her future child. On the other hand, cigarette taxation could exacerbate education-related disparities for individuals unable to reduce or quit smoking in response to taxes and who therefore spend a larger share of their disposable income on cigarettes.[3]

In this study, we examine the impacts of cigarette taxes and indoor smoking restrictions on prenatal smoking. The data used in the analysis is from the National Vital Statistics System provided by the Centers for Disease Control and Prevention (CDC) for years 1995 to 2018, which contains the near universe of all births in the United States. This study differs from prior work for two main reasons. First, we use a modified stacked difference-in-differences (DD) model (Cengiz et al. 2019) to reduce bias caused by using traditional two-way fixed effect (TWFE) models in cases of staggered policy adoption when policy effects vary over time (Gibbons et al. 2019, Goodman-Bacon 2021). Second, prior work has looked at policies through at most 2010, and we extend the years through 2018. Some recent studies warn that cigarette taxes may be less effective at reducing smoking in recent years than in prior time periods for youth (Hansen et al. 2017) and adults (Callison and Kaestner 2014), possibly because remaining smokers have hardened preferences for smoking. By extending the analysis to 2018 we have an opportunity to re-explore whether tax responsiveness is changing for pregnant women as well. We hypothesize that pregnant women who smoke may also be resistant to contemporaneous tax changes in our sample perhaps due to in-utero cigarette taxes between the years 1965 to 2001. Evidence for this comes from a paper that speculates that higher in-utero taxes may alter the composition of remaining smokers and contribute to reductions in contemporary cigarette tax responsiveness (Hoehn-Velasco et al. 2022).

[3] Decicca et al. (2022) for example find that the lowest income individuals spent over 2.5% of their family income on cigarette taxes in 2014-15, whereas the highest income individuals spent virtually zero percent of their family income on cigarette taxes.

Chapter I: Do Cigarette Tax Hikes Still Increase Cross-Border Shopping? Evidence from Cellphone Tracking Data

Introduction

Tobacco use leads to over 7 million deaths a year worldwide (World Health Organization 2017). Cigarettes are the most used form of tobacco in the United States of America (USA), with 12.5% of the adult population being users in 2020 (Cornelius et. al. 2022). To promote public health, governments then have a strong incentive to reduce cigarette use by implementing cigarette taxes. Besides promoting public health, these taxes also raise significant revenue and offset what are already substantial public healthcare expenditures (Xu et. al. 2015) on negative health outcomes caused by smoking. As Xu et. al. (2015) estimate, the financial burden as a fraction of spending by coverage source is mostly on Medicaid and certain federal coverage programs that do not include Medicare such as Tricare, VA health benefits, and the Indian Health Service. Other work finds that premature mortality limit them from being a large burden on Medicare (Darden and Kaestner 2022). While this would imply that the financial cost is generally on states and some areas of the federal government, it is important to remember that the federal government in the USA provides well over half of the funding for Medicaid. Because of this, both federal and state governments share the financial burden of smoking-related healthcare expenditures.

Previous literature shows that individuals use a variety of strategies to avoid paying high cigarette taxes. For instance, in 2019, it was estimated that 52.2% of cigarettes consumed in New York state did not collect tax revenue for the state.[4] Cigarette tax avoidance strategies include cross-state border shopping for personal use (Lovenheim 2008, Harding et. al. 2012, DeCicca et.

[4] https://taxfoundation.org/state-tobacco-tax-cigarette-smuggling/

al. 2013) and many forms of organized smuggling by criminal organizations (Joossens and Raw 2012). Using the Tobacco Use Supplement of the CPS, Figure C1 shows that about 5% of smokers cross-border shopped for the most recent pack of cigarettes purchased in July 2018.

In this paper, I study how cigarette tax hikes impact cross-border shopping behavior. I do this by estimating how many additional cross-border shoppers a state sends to its lower-tax border states after it increases its cigarette tax. If individuals begin cross-border shopping in response to a cigarette tax, this means they are neither reducing cigarette consumption nor paying the original or higher cigarette tax. This implies that (1) neighboring state governments may wish to consider coordinated adjustments to taxes to promote public health and to avoid leakage in tax revenues in the tax-raising state and/or (2) future cigarette tax increases should be done by the federal government, which abate much or all of the increase in cross-border shopping. However, both approaches may be difficult to implement in practice. For instance, lower-tax border states may be reluctant to give up additional tax revenue from contiguous higher tax states. Federal cigarette tax increases, on the other hand, have been infrequent relative to the number of state-level tax increases, implying that it may be difficult to push cigarette tax legislation through Congress.

Previous papers focusing on cross-border shopping have been confined to urban areas (Lovenheim 2008, DeCicca et. al. 2013) or have had to use a broad definition of a consumer's home residence (Harding et. al. 2012).[5] Further, Lovenheim (2008), Harding et. al. (2012), and DeCicca et. al. (2013) are all based on survey data where the respondent must self-report cross-border shopping for cigarettes or cigarette consumption. Previous work (Connor et. al. 2009),

[5] DeCicca et. al. (2012) use urban areas only so they can make use of Metropolitan Statistical Area definitions, which is the lowest geographical identifier they have in the data (TUS-CPS) they use. Harding et. al. (2012), using the Neilson Homescan data, have geographical identifiers down to the census tract level.

however, has shown that survey respondents tend to systematically underreport smoking status. As an alternative data source that can be used to identify likely cross-border shopping, I use a cellphone tracking dataset provided by SafeGraph. This data covers nearly every census block group (CBG)[6] in the USA and does not rely on self-reporting. It further gives the home CBG of most of the devices that it tracks, which is a precise definition of a consumer's home residence. This data is then well-suited for estimating how much cross-border shopping changes in response to a state-level cigarette tax for both urban and rural areas. Additionally, as the estimation is during a recent period (2018-2019) in the USA, e-cigarettes would have been widely available to all the smokers in my treated states. Previous studies examining cross-border shopping considered a time interval in the USA where e-cigarettes where not widely used by the population.[7] As there is evidence that e-cigarettes and cigarettes are substitutes (Cotti et. al. 2022), changes in cross-border shopping in response to a cigarette tax increase may be lessened by the existence of a widely available substitute.

The Safegraph data also has important drawbacks when it comes to estimating cross-border shopping. The first of these is that I cannot observe what an individual purchased when they enter a retailer. This implies that my measurement of cross-border shopping will include visits that do not involve the purchasing of cigarettes. However, if the visits that do not pertain to cigarette purchases are relatively unchanged in response to cigarette taxes, my estimates will not be contaminated by this issue. I provide evidence that this is the case through a placebo test and show that the number of visitors to retailers that do not sell cigarettes is unchanged when the

[6] Census block groups are the second lowest level of geographical identification provided by the Census Bureau. They generally contain between 600 and 3,000 people.

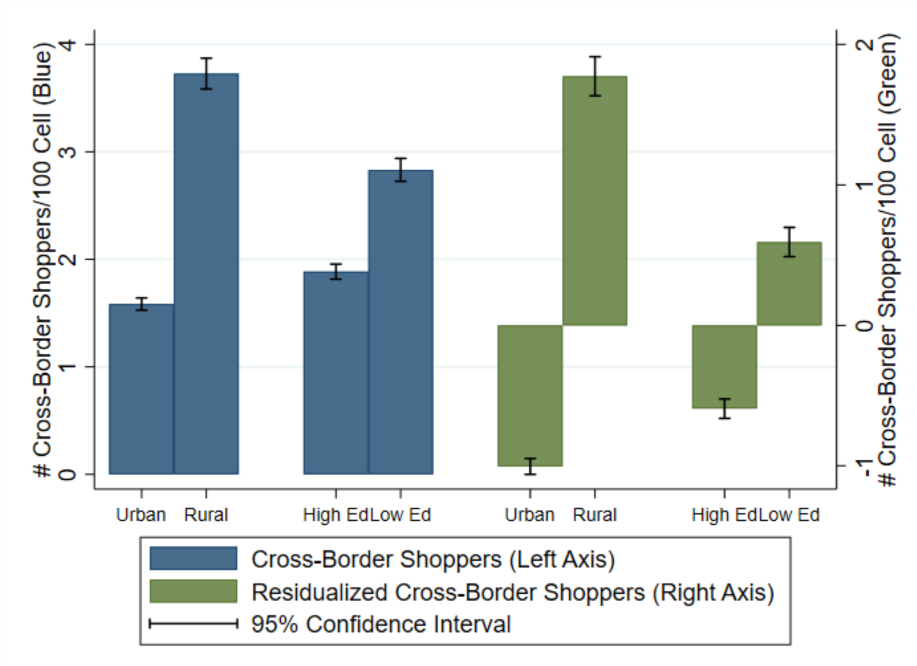
[7] The U.S. Department of Health and Human Services (2016) reports that e-cigarette use in the USA increased greatly starting in 2010. All the papers mentioned until now that estimate cross-border shopping (Lovenheim 2008, Harding et. al. 2012, DeCicca et. al. 2013) contain data at most up through 2007.

cigarette taxes in my sample become effective. Secondly, the SafeGraph data does not provide information about the owners of the cellphones being tracked (besides their home CBG), nor is it known how SafeGraph chooses which cellphones to track (besides that the cellphones must be compatible with applications). These unknowns about who is in the data imply that I do not know if my sample is representative of the USA's population. However, SafeGraph does have at least some coverage of nearly every CBG within the states in my sample.

To estimate the effect of the tax increases that occur at distinct times on cross-border shopping, I use Callaway and Sant'Anna's (2021) difference-in-differences model. I find strong evidence for an increase in cross-border shoppers in response to a state-level cigarette tax increase. Specifically, I estimate that, for members of the sample, the median CBG sent about 0.53 more monthly cross-border shoppers to a lower-tax border state in response to a cigarette tax increase. This magnitude is approximately 19% of the before tax mean of cross-border shoppers in tax-raising states. I further use my results to estimate that between 0.1% and 2.5% of cigarette tax revenue in 2019 for a subset of my tax-raising states were attributable to cross-border shopping. This back of the envelope calculation suggests that cross-border shopping does constitute a small portion of total tax revenue collected by a state.

This high-resolution data further allows me to explore differences in cross-border shopping by demographic or rural status. As shown in Figure 1, rural CBGs and CBGs with many lower-educated adults in the pre-tax increase period of my sample send substantially more cross-border shoppers to lower-tax states than their counterparts. This result makes sense as the group of adults who pay nearly all cigarette taxes are more likely than the general adult population to have at most a high school degree and correspondingly a low income (Conlon et. al. 2021). Further, this result is an outgrowth of the results presented in Table A1, where I calculate using the Tobacco Supplement of the CPS that rural residents are more likely to smoke and, conditional on smoking, more likely to be heavy smokers and cross-border shop. When dividing my sample by these demographics, I estimate that state-level cigarette tax increases worsen this inequality for both kinds of CBGs.

Figure 1. Before Tax Increase Time Expenditure Inequality in Treated States by CBG-Level Adult Educational Attainment per Capita and Rural Status



Notes: Levels of cross-border shopping are read as per 100 cellphones active in a census block group (CBG). These levels also concern monthly shoppers in the treated state before any treated state in the sample raised its cigarette tax. Each bar is the average number of cross-border shoppers over all CBGs that has the characteristic listed on the horizontal axis. "Residualized" cross-border shoppers is the residual of a regression of cross-border shoppers on the minimum distance a CBG is from a lower-tax border before the treated state raised its cigarette tax.

Finally, earlier estimates of the response of cross-border shopping to a cigarette tax hike have not focused on the differences in the cigarette tax environment of the border state that exports cigarettes. It is important to look at the margin because further tax increases may not substantially change the number of cross-border shoppers the tax-raising state sends to border states that already had a lower tax before the tax hike. My results suggest that this is true for the tax-raising states that surpassed the tax level of multiple border states. However, for the tax-raising state that only surpassed one border state and already had a higher tax level than the remaining border states, this intuition does not hold. Furthermore, the only significant increase in cross-border shoppers for this state were to border states that had lower tax levels than the treated state pre-tax increase. This implies that even if a state can raise its tax to a higher level than most surrounding states, further increases from this level will still incentivize more residents to cross-border shop in surrounding states. This result also confirms the need for border states to coordinate their tax levels to increase similarly with the tax-raising state.

Theoretical Motivation

I use a modified version of the model for cross-border shopping by Nielsen (2002) to inform my analysis. The model states that, given an individual has decided to purchase cigarettes, they choose to purchase cigarettes either in the state they live in (home state) or across their home state's border using the following inequality:

$$V_i(Cig_i) - T * Cig_i - d_i * D_i(t) \geq V_i(Cig_i) - t * Cig_i$$

where Cig_i is the number of packs of cigarettes an individual i purchased, $V_i(Cig_i)$ is the value function of cigarettes i purchased, T is the tax level across the home state's border, t is the tax level for home state, d_i is the cost of travel per mile for i, and $D_i(t)$ is a weakly decreasing function of the home state's tax whose output is the minimum number of miles i needs to travel to shop at a lower tax border.[8] The parameter d_i not only consists of the monetary cost of travel, but also the opportunity cost of time spent to travel. I presume that i has already decided how many cigarettes they would like to purchase (Cig_i is given) and are now considering where to purchase them. This inequality then simplifies to the following:

[8] This function is weakly decreasing in the home state's tax if i does not move from their CBG.

$$D_i(t) \leq \frac{[t - T] * \ Cig_i}{d_i} \tag{1}$$

This relationship tells us the maximum distance an individual is willing to drive to cross-border shop given t, T, d_i, and Cig_i. Notice that an individual will not cross-border shop in a border state if the tax in the border state is larger than their home-state tax as $D_i(t) > 0$. Further, if the individual wants to purchase more cigarettes, they are willing to travel a larger distance given $t > T$. The maximum distance an individual is willing to travel is also lower if the cost per mile traveled (d_i) is high. The probability of cross-border shopping is then one if the inequality in (1) holds and is zero elsewise.

Now consider an increase Δ in the home state tax t, such that $\Delta t = t_1 - t_0$, where t_1 is the new tax level and t_0 is the original tax level. First, note that the upper bound in (1) will increase as:

$$\Delta\left(\frac{[t - T] \ * \ Cig_i}{d_i}\right) = \frac{\Delta t \times Cig_i}{d_i} > 0 \tag{2}$$

This change in the upper bound has three testable predictions. The first is that those living closer to a lower tax border will be more likely to cross-border shop as $D_i(t_1)$ is low and the maximum distance they are willing to drive has increased. I test this prediction by estimating conditional average treatment effects by the first three quartiles of distance from a census block group (CBG) to a lower tax border. This will test whether those closer to the border have a greater response to an increase in t than those further away.

Secondly, those who smoke or smoke heavily will be more likely to cross-border shop as Cig_i would be large or at least positive, implying a larger increase in the maximum distance they are willing to drive. In general, this prediction implies that any group of individuals that are more likely to smoke or smoke heavily will have a high probability of cross-border shopping for cigarettes after an increase in t.

As Conlon et. al. (2021) estimate, nearly all cigarette taxes are paid by a small fraction of individuals, who are much more likely than the general adult population to be low-income and low-educated. Building on this, Darden (2021) argues low-skilled workers either migrate to or remain in rural areas as they cannot compete in urban labor markets. Darden (2021) concludes that these migration patterns have caused rural areas to have more smokers than urban areas. His finding confirms the importance of considering rural areas in any estimate of cross-border shopping as individuals in these areas have a higher probability of being heavy smokers and being a smoker. To incorporate these insights in my estimates, as I do not know who in my sample is a smoker, I divide my sample between CBGs with a high portion of low-educated adults or is a rural CBG. Comparing these groups to their counterparts will then allow me to assess whether more people cross-border shopped from areas with more smokers and heavy smokers conditional on being a smoker.

Finally, the increase in the upper bound depicted in (2) implies that people with distance to a lower tax border $D_i(t)$ such that:

$$D_i(t_1) \leq \frac{[t_1 - T] * \ Cig_i}{d_i} \quad \text{and} \quad \frac{[t_0 - T] * \ Cig_i}{d_i} < D_i(t_0) \tag{3}$$

will now find it conducive to cross-border shop. For those whose $D_i(t_1)$ refers to a lower-tax border state with a T such that $t_0 > T$ (the border state had a lower cigarette tax level before and after the home state), this implies that more individuals in the home state will still cross into these border states. I test this prediction by splitting border states based on their characteristics i.e. whether their tax level was higher than the home state before it raised its tax level or had a lower tax level both before and after. The marginal cross-border shopper whose situation is depicted in (3) could go to both kinds of border states.

Further, note that:

$$D_i(t_0) - D_i(t_1) \geq 0 \tag{4}$$

, or, the change in the minimum distance to a lower tax border is non-negative when the home state increases its tax level. I show this relationship is true in my sample in Figure C2, conditional on the fact that a resident of a CBG did not migrate to a different CBG after their home tax was raised. A change in minimum distance to a lower tax border for a CBG could only be caused by four conditions holding: (i) a border state having a higher tax level than the home state in the pre-period (ii) this border state having its tax level surpassed ($t_1 > T$ and $t_0 < T$) by the home state in the post-period (iii) the CBG is close to this border state and (iv) the CBG was far away from a lower-tax border state in the pre-period. In brief, this implies that a CBG was far away from a lower-tax border, and then became close to a lower-tax border because a nearby border state's tax level was surpassed by the home state.

As (4) implies that $D_i(t_1) \leq D_i(t_0)$, and this change in distance is caused by a border state's tax level being surpassed by the home state's, those that had large changes in their minimum distance to a lower tax border should have been tempted to cross-border shop in these border states whose tax level was surpassed by the home state's. I test this prediction by dividing the treatment group into quartiles of change in minimum distance to a lower-tax border. I predict that quartiles representing a larger change in distance should have large increases in cross-border shoppers to border states described above.

This prediction further underlies the importance of *not* controlling for a time-varying minimum distance to a lower-tax border. Mainly, this distance is a function of the treatment variable (home state cigarette tax) of interest, as shown in Figure C2. Controlling for this time-varying variable will then cause an over-control bias and shut-off the causal path from a change in the home state's cigarette tax to a change in cross-border shopping (Cinelli et. al. 2022).

In the initial set-up of the model, it was presumed that the difference in prices between the home and border state is the differing tax level. From this, the increase in the home tax was defined as $\Delta t = t_1 - t_0$. There is evidence, however, that cigarette firms distribute coupons in states that raise their cigarette tax to offset the price increase for smokers (Ribisl et. al. 2022). The change in price p in this case for smokers that use coupons would be $\Delta p = t_1 - c - t_0$ where c is the nominal coupon amount and $c > 0$. In this case, when considering Δp in (2) as opposed to Δt, the increase in the upper bound of distance willing to travel would be diminished, implying a smaller chance of cross-border shopping. Unfortunately, I cannot observe whether the members of my sample receive coupons and so cannot estimate whether the effect of the tax on cross-border shopping differed by coupon usage.

Literature Review

Many papers have measured either the totality of cigarette tax evasion[9] (Warner 1982, Baltagi and Levin 1986, Baltagi and Gold 1987, Saba et. al. 1995, Thursby and Thursby 2000, Ben Lakhdar et. al. 2016) or criminal-network driven cigarette smuggling (Yurekli and Zhang 2000) using state level cigarette sales data. These papers tend to find a strong presence of cigarette tax evasion. This evidence is usually shown by estimating that a lower cigarette tax or price in a border state negatively impacts cigarette sales in the home state. However, the dependent variable (cigarette sales) in these studies does not indicate where the individual who purchased cigarettes resides. This implies that these studies cannot be sure how many sales are

[9] This includes both criminal-network driven cigarette smuggling and cross-border shopping.

lost to cross-border shopping when border states have a lower tax-level as they do not know the home state of any purchaser.

More recent papers have attempted to address this issue by using individual survey data. Two papers (Stehr 2005, Lovenheim 2008) have used changes in consumption reported in their respective survey to tease out the magnitude of cross-border shopping in response to a cigarette tax. For Stehr (2005), they estimate that that differences between sales and consumption increases by 0.0322 log points in response to a 1 unit increase in the weighted[10] average cigarette tax differential between the home state and all surrounding border states. Lovenheim (2008), on the other hand, estimates the percentage of a state's sales that are due to cross-border shopping using differences in price elasticity of consumption near a low-tax border state versus the same elasticity for those who live far away. Using these estimates, they calculate that the percentage of sales due to cross-border shopping is between 13 to 25 percent.

Another two papers (Harding et. al. 2012, DeCicca et. al. 2013) use more direct methods to tease out the magnitude of change in cross-border shopping in response to a cigarette tax increase. These authors do this by using a survey question that asks about cross-border shopping or only looking at cross-border shopping trips for cigarettes. The work by DeCicca et. al. (2013), who uses the Tobacco Use Supplement of the CPS, measures the increase in the probability of cross-border shopping for smokers in states with higher cigarette taxes. The paper finds that a 1 dollar increase in cigarette tax differential between the home and border state increases cross-border shopping probability by 10 percentage points. They further find that this effect is mitigated by 7 percentage points for each mile from the smoker's residence to a lower-tax border state. Harding et. al. (2012) measures the same outcome, this time using the Nielsen Homescan

[10] This weight depends on home state's population and state-radius.

data. The authors find that for each additional percentage increase in distance from a lower tax border, a 1 cent increase in cigarette taxes increases cross-border shopping probability by 5.36%. This effect is also reduced as distance from a lower-tax border increases.

The more recent literature that relies on surveys to answer questions about cross-border shopping may be influenced by the fact that smoking status or cigarette purchases are self-reported. As Connor et. al. (2009) estimate, when considering 54 studies that estimated whether individuals under-report smoking, the average underreporting of smoking status across these studies was somewhere between 4.8% and 9.4% of participants in the sample.[11] Because smoking was mostly underreported in the 54 studies they considered, it may also be true that smoking status was underreported in the surveys used in the more recent literature on cross-border shopping. Given this would cause a skewed measurement error in the dependent variable (Millimet and Parmeter 2022), estimates of the main parameter on cigarette taxes in these papers may be biased.

Other recent papers have taken the approach of estimating cigarette smuggling by studying state tax stamps on hand-collected cigarette pack litter in different states (Merriman 2010, Chernick and Merriman 2013, Barker et. al. 2016, Wang et. al. 2019). The most comprehensive dataset in this literature is collected in 130 different communities that comprise a nationally representative sample in Barker et. al. (2016) and thoroughly analyzed in Wang et. al. (2019). In Wang et. al. (2019), the authors estimate that a 1 dollar increase in the cigarette tax level will increase proportion of noncompliance cigarette packs by 8 percentage points.[12] This paper and others like it have a similar drawback as papers that use cigarette sales as an outcome.

[11] Connor et. al. (2009) split up their estimates of the average underreporting percentage by method of which smoking status was ascertained besides self-report. The underreporting average was 4.8% for saliva tests; 6.2% for serum, blood, and plasama; and 9.4% for urine.
[12] A "noncompliance cigarette pack" is a pack that does not have the community's state tax stamp on it.

Mainly, they cannot identify the state for those that purchased noncompliance cigarette packs resides in.

The current paper extends this rich literature in many ways. First, like Harding et. al. (2012) and DeCicca et. al. (2013), I give a direct estimate of the change in cross-border shopping in response to a cigarette tax increase. I can make this distinction as I know the home address of most of the cellphones in my sample and I know when the cellphone crossed into a retailer in a state outside of the state where the cellphone resides.

Unlike these papers, my data does not rely on self-reporting and uses a high-resolution definition (census block group) of a respondent's home address. For example, there may be up to nine census block groups in a census tract, which is the level of geocoding used in Harding et. al. (2012). My data is also collected each month, which avoids issues present in datasets like the TUS-CPS, which only asks about the most recent pack of cigarettes purchased. I further use the fine granularity of my data to estimate conditional average treatment effects by terciles of distance from a CBG to a lower-tax border, which allows a non-linear effect on this margin. This additionally avoids issues with including a time-varying distance to a lower-tax border as a control variable, which I showed in Figure C2 is a function of the tax-raising state's tax.

The data used in this paper is also drawn from nearly every census block group (CBG) in the states considered for the analysis. This is an advantage over previous papers that only used urban smokers (Lovenheim 2008, DeCicca et. al. 2013), which I show in Table A1 are less likely to smoke, smoke heavily, and cross-border shop relative to their rural counterparts. As discussed in the theoretical motivation section, this implies that rural areas should send more cross-border shoppers than urban areas, making the inclusion of both urban and rural smokers important. I also provide evidence of a widening inequality of who cross-border shops using the heterogeneity of CBG-level demographics and consider differences in the tax environment of border states.

Data

The main source of data used in this paper is the SafeGraph Patterns dataset, which tracks cellphone movements for about 40 million devices in the USA. I consider relevant records in this dataset between January 1st, 2018 and December 31st, 2019. I chose the end date to avoid the onset of the COVID-19 pandemic in the USA, which caused massive shifts in cross-border movement. The starting date was chosen because this is the earliest date the Patterns dataset is available. The dataset reports how many unique visitors entered a point of interest (POI) and how many visits a POI received in each month. The latter captures a visitor making multiple visits to a POI over the past month, but the former dos not. Most visitor devices are assigned a home census block group (CBG) and the home CBG FIPS code of a device is given (if determined) when a device is recorded as visiting a POI. To be recorded as a "visit" to a POI, the cellphone must be within the POI's geography for five minutes or more.

This dataset lends itself naturally to estimate cross-border shopping behavior. First, as the data allows me to observe home CBG FIPs code of most visitors, I can use this information to define which visitors are from out of state, or cross-border shoppers, and which visitors are from in-state. Secondly, the fine geography of the data enables me to construct precise measurements of distance to the state border for each CBG. This feature of the data gives me the opportunity to be specific when estimating how cross-border shopping behavior differs by distance to the border. Third, each point of interest is given a detailed, six-digit NAICS industry code, store name, and geographic coordinates. Using this information, I construct a set[13] of potential

[13] These include tobacco stores; gas stations with convenience stores; convenience stores; beer, wine, and liquor stores; pharmacies and drug stores; supermarkets and other grocery (except convenience) stores; and discount

cigarette retailers and exclude stores that are in a cigarette retailer industry but do not sell cigarettes. One drawback of identifying cigarette retailers in this way would be that not all retailers in some industries sell cigarettes. For example, the “tobacco store” industry includes stores that only sell e-cigarettes. Unfortunately, I cannot distinguish which retailer in this industry only sells e-cigarettes, a mixture of e-cigarettes and other tobacco products, or tobacco products excluding e-cigarettes. Because of this, there may be measurement error in my outcome variable where trips to e-cigarette only stores are counted as trips to purchase cigarettes. and exclude stores that are in a cigarette retailer industry but do not sell cigarettes. I also use the latitude and longitude provided for each point of interest to exclude cigarette retailers that more than 35 miles away from a treated state’s border.[14] Finally, nearly every CBG in the states considered in my analysis are covered by the data, with an average of 8% of the CBG population having a traced cellphone.[15]

From the initial dataset, I construct a panel on the CBG-level, which records the monthly sum of visitors from a given CBG to cigarette retailers. This data structure allows me to observe the change in this monthly sum within a CBG over my sample period. The count of visitors is split in each month between the number of visitors who entered a potential cigarette retailer which was located within the state that the CBG is a member of (in-state shoppers), and visitors who entered a potential cigarette retailer outside of their state but within a border state (cross-border shoppers). I further assign each CBG a linear distance from its centroid to the closest

department stores (only Wal-Mart and Family Dollar). This list follows Golden et. al.’s (2020) list of tobacco retailers. Unfortunately, Safegraph does not offer the NAICS code for “Warehouse clubs and supercenters”, which is included in Golden et. al.’s list.

[14] 35 miles is the maximum distance in the first quartile of the distribution of these distances.

[15] This assumes that a member of a population would only have one cellphone to trace. Less of the population would be covered in the dataset if members of the CBG population had multiple devices.

lower-tax border state. I use this information to conduct analyses for CBGs by quartiles of distance to a lower-tax border state.

My dataset has important limitations. First, while I can observe visits to cigarette retailers I do not know if visitors purchased cigarettes or not. However, my identification strategy acknowledges that a certain share of visitors will purchase commodities that are not cigarettes. If there is no changes in cross-border visits to these retailers for reasons besides cigarette purchasing after the cigarette tax is increased, my difference-in-differences estimation will be independent of the constant flow of cross-border visiting. I provide evidence that the number of visitors to retailers in the industries I consider that do not sell cigarettes does not respond to cigarette tax changes. Secondly, I do not know information about the owners of the devices being tracked in this sample. This may cause issues if certain CBGs have more representation in the dataset than others, leading my sample to be unrepresentative of the USA's population.

Methods

Main Analysis

I use a Callaway and Sant'Anna's (2021) (CS21) difference-in-differences model to assess the impact of cigarette taxes on cross-border shopping. The final dataset used for the regression is constructed in the following way. I identify an isolated, state-level cigarette tax increase with no change in this tax 6 months before and 5 months after the effective month of the tax. This policy change occurs in what I call the "treated state". I then found states that were not treated over my period and designated a subset of these as the control states. All control states are bordering at least one treated state, do not have a tax change over the sample period, and have a lower tax level than the bordered treated state both before and after the tax effective date. I then consider cross-border shopping into the treated states as the control state's outcome. As their cigarette tax level is lower than the treated state's both before and after the treated state increases its tax, their cross-border shopping behavior should be unaffected. My final dataset considers three state-level cigarette taxes in Illinois, Kentucky, and Oklahoma. Details on the effective date and control states chosen can be found in Table A6. The number of treated states I have is lower relative to other similar papers (Harden et. al. 2012, DeCicca et. al. 2013), implying my results may not be as generalizable to the USA. However, relative to these same papers, I cover a similar span of time in my sample (2 years).[16]

CS21 deals with estimation bias in traditional two-way fixed effect (TWFE) models with staggered policy roll-out (Goodman-Bacon 2021) by only considering the "good" 2-by-2 difference-in-differences that comprise any TWFE estimate. Their 2-by-2 difference-in-differences estimates take the following form for any treated state g in period $t > g^*$:

$$ATT_{g,t} = \mathrm{E}\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{E\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)\left(Y_t - Y_{g^*-1} - E\left(Y_t - Y_{g^*-1}\middle|X, C = 1\right)\right)\right]$$

, where g^* is the time period when g becomes treated, $G_g = 1$ indicates observations in g are being considered, $C = 1$ indicates never-treated observations are being considered, $p_g(X)$ is the propensity score, and $\mathrm{Y_t}$ is either the number of cross-border or in-state shoppers per 100 cellphones in the visitors CBG. The 2-by-2 difference-in-difference for any pre-period $t < g^*$ for treated state g is similar:

$$ATT_{g,t} = \mathrm{E}\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{E\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)\left(Y_t - Y_{t-1} - E(Y_t - Y_{t-1}|X, C = 1)\right)\right]$$

[16] Harden et. al. (2012) covers January 1st, 2006-December 31st 2007 and DeCicca et. al. (2013) covers TUS-CPS waves 2003, 2006, and 2007.

These equations represent a doubly-robust estimator proposed by Sant'Anna and Zhao (2020). The estimator works by first estimating $p_g(X)$by a logit equation and $E(Y_t - Y_{t-1}|X, C = 1)$ or $E\left(Y_t - Y_{g^*-1}\middle|X, C = 1\right)$ using OLS. These estimates are then plugged into the equations above and the remaining parameters are filled with information from the sample. The advantage of the doubly robust estimator is that the practitioner need only estimate the propensity scores or imputed counterfactual correctly, but not both.

CS21 also proposes their own cluster bootstrap to yield asymptotically valid standard errors. Per CS21's recommendation, I will also use this to conduct inference. For the bootstrap, I cluster based on state of residence by quartile of minimum distance to a lower-tax border.[17] This clustering decision was made as treatment not only varies by state, but also by distance to a lower-tax border in the state that raised its cigarette tax. This occurs as a cigarette retailer's passthrough of the tax to the final price is diminishing the closer they are to a lower-tax border (Harding et. al. 2012).

Concerning covariates, I use the quartiles of the proportion of a CBG that is white, has a high school education or less, and that drives to work. I also use whether the CBG is rural or urban and the first three quartiles of minimum distance to a lower-tax border over the whole sample period as covariates.[18] The doubly-robust estimator only uses initial values of covariates and so are not time-varying.

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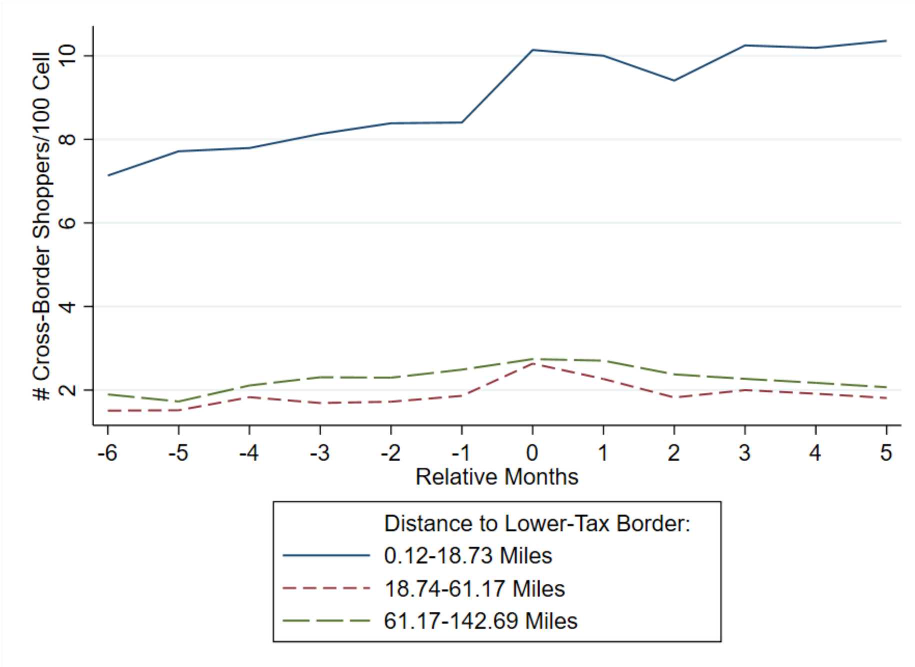
[17] By the fourth quartile, there is no longer any treated units. This is caused by the fact that "distance to a lower-tax border" for control states is the linear distance from the centroid of a CBG to the nearest treated state's border, given the control state is contiguous to this treated state. I then only use the first 3 quartiles of distance to a lower tax border as a control and as a variable to cluster.

[18] When estimating conditional average treatment effects by distance to the border, I do not control for quartiles of distance to a lower tax border. Similarly, I do not control for CBG rural status when running regressions conditional on being in a rural or urban area. For control states, "distance to a lower tax border" is the closest distance to their border treated state. I did this to facilitate the fact that the control states outcome is cross-border shopping into the treated state.

I further run an event study within a balanced window to assess parallel trends and dynamic treatment effects. The window I choose, six months before the effective month and five months after, mirrors my selection criterion for treatment states discussed above. Importantly, it assures that all treated states have observations for each pre and post period. CS21 then estimates each coefficient in the event study by weighting together the ATTs for each treated state. The weight in this case is chosen to be the proportion of all the treated state observations that one treated group comprises. The raw data for treated states only using the event study window is presented in Figure 2. This Figure displays that CBG closest to a lower-tax border (within 18 miles) send substantially more cross-border shoppers after the tax becomes effective in relative month zero. CBGs farther from a lower-tax border either see no or a small increase in cross-border shoppers after the cigarette tax becomes effective.

My main results concern how the cigarette tax impacted cross-border shopping from the state adopting a higher cigarette tax to a *lower-tax* border state. The border states that I examine may either have had a lower tax both before and after the treated state raised its cigarette tax or just after. This is an important distinction as lower-tax border states are the areas where cross-border shoppers from the treated state should travel in response to a cigarette tax increase. Further, all my results only consider cross-border shopping to cigarette retailers in the border state but within 35 miles of the treated state's border. As Table A2 shows, the largest CS21-based difference-in-difference coefficient is for retailers within 35 miles of a treated state's border. This result is intuitive as cross-border shoppers are unlikely to travel far over their state's border to purchase cigarettes unless necessary.

Figure 2. Relative Months to Cigarette Tax Increase using Raw Data for Treated Groups



Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Each point on the graph is calculated by taking the average of cross-border shoppers per 100 devices in all the census block groups (CBGs) for each treated state. The relative month of "0" is when the cigarette tax became effective in each state. Each division of distance from a lower-tax border is made by considering quartiles of distance from a lower-tax border as described in the methods section. As the last quartile does not contain any treated units, I dropped it from consideration.

Extensions

I also divide my main results to estimate conditional average treatment effect by quartiles of distance from the centroid of treated state CBGs to a lower-tax border state.[19] As mentioned above, I expect those in the treated state that live closer to a lower-tax border state are more likely to cross-border shop in response to a cigarette tax than those that live further away. This partition further allows me to view if changes in cross-border shopping vary non-linearly by

[19] The division is made in the following way: I first take the quartiles of the entire distribution of distance to a lower-tax border. This means considering these distances in both treatment and control states, where the distance for the control state is the minimum distance to a treated state's border. When I do this, the fourth quartile contains almost no treated units, and so I drop it from the analysis.

distance to a lower-tax border. These estimates are conditional on CBGs in both the treated and control states to be within the distance interval defined for each estimate.

Further, I split the sample by adult educational attainment in 2018 or rural CBG status in 2018. The division of the sample by educational attainment is into CBGs with many (top 50% of the distribution) adults per capita with a high school degree or less and few (bottom 50% of the distribution). These additional analyses are important as adults with low educational attainment or live in rural areas are 1.86 times and 1.28 times, respectively, to be smokers than their counterparts. Further, conditional on being a smoker, adults with low educational attainment are 1.18 times more likely to be a heavy smoker and adults in rural areas are 1.17 times more likely.[20] Together, this implies that individuals in areas with many low educated adults or are rural have a stronger incentive to cross-border shop than their counterparts on account of being more likely to be smokers. This division of the data also allows me to comment on which group of people are cross-border shopping more in response to a cigarette tax.

I additionally estimate four separate average treated on the treated effects by dividing my treatment group into quartiles of change in minimum distance to a lower tax border. The outcome for each of these regressions is cross-border shopping into a border state whose cigarette tax level was surpassed by the treated state's as a result of the recent tax change. As discussed in the theoretical motivation section, a large change in the minimum distance to a lower-tax border should imply that an individual lived far away from a lower-tax border before their treated state raised its tax, and then became very close to lower-tax border state in the post-period. As this change in distance could only have occurred if the treated state's tax level

[20] These calculations come from the TUS-CPS from the years 2003-2019.

surpassed a nearby border state's tax level, I will focus on cross-border shopping only into these "surpassed border states" for this specification, as opposed to all lower-tax border states.

Finally, I divide the CBGs within 18 miles (the first quartile of distance) of a lower-tax border state (treated state's border for control states) by how "connected" they are to a bordering state. By connected, I refer to the portion of a CBG that commutes to a border state for work. I approximate this measurement in the following way. Using the LEHD (Longitudinal Employer-Household Dynamics) Origin-Destination Employment Statistics (LODES)[21], I can calculate the proportion of workers in each CBG that commutes from their CBG to a CBG in a border state. This is possible as the LODES data is administrative and uses unemployment insurance covered wage and salary jobs to determine where workers reside and where they work. As Graham et. al. (2014) states, the LODES data should cover approximately 95% of wage and salary jobs. Then, for each CBG within 18 miles of a lower-tax border[22], I divide the number of commuters by the adult population in each CBG. The highest 50% of these proportions are then deemed as "more connected" CBGs while the bottom 50% are deemed as "less connected" CBGs. Considering the theoretical section above, residents of a "more connected" CBG will likely have either a lower cost of travel (potentially because of public transportation or ride sharing) or a shorter distance traveled (perhaps due to a road or highway) to a border state. Either of these advantages likely increased the probability of commuting, making the CBG "more connected" to a border state and so easier to cross-border shop.

[21] For more information on how this data is collected, Graham et. al. (2014) compares the LODES data with the American Community Survey data on commuting.

[22] Considering only CBGs within 18 miles of a treated state's border for this extension is important as using all CBGs would potentially make this analysis redundant. For example, if I were to use all CBGs, it is likely that CBGs close to the state's border would have more cross-border commuters than CBGs further away because of distance. In this case, I would be reproducing the results from the above analysis that examines the effect by quartiles of distance from a lower-tax border state.

Robustness Checks

As mentioned earlier, I removed retailers from my main analysis who do not sell cigarettes but are classified in a cigarette retailer industry.[23] I then use most[24] of these retailers I removed as a placebo test by only considering cross-border shopping to these non-cigarette selling retailers. When using these retailers as an outcome, a null result would provide evidence that the increase in cross-border shopping to cigarette retailers was caused by the treated states increasing their cigarette tax, as opposed to another concurrent intervention that increased visits to all retailers in the industries I consider.

This placebo test of using retailers in the industries I consider whom I do not believe sell cigarettes could also provide evidence that the number of cross-border shoppers to the retailers in my sample who purchase other goods is constant throughout my sample period. Specifically, a null result in this case will suggest there was no significant change in cross-border shoppers to retailers in the industries I consider but whom I do not believe sell cigarettes. This will be important as my outcome variable will overreport the number of visitors to the retailers I consider as not all trips to these retailers will be to purchase cigarettes. However, if this overreporting is constant, this error from the true value will subtract out in both equations given for $ATT_{g,t}$ above.[25]

[23] These include ALDI, Natural Grocers, Whole Foods Market, Binny's Beverage Depot, Virginia ABC, CVS, Walmart Pharmacy, and Walgreen's Pharmacy.
[24] All the stores in footnote 20, except for Walmart Pharmacy and Walgreen's Pharmacy. This decision was made as trips to Walmart Pharmacy and Walgreen's Pharmacy may be part of a trip to buy cigarettes in the same store.
[25] To see how this is true, consider the true number of cross-border shoppers $Y_t^* = Y_t + \epsilon_t$ where epsilon is the error term, which in my case will be the overreporting of cross-border trips. If it is true that $\epsilon_t = \epsilon_{t-i} = \epsilon, \forall t$ and $\forall i$ with $i < t$, then the CS21 will give:

$$ATT_{g,t} = \mathrm{E}\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{E\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)\left(Y_t^* - \epsilon - \left(Y_{g^*-1}^* - \epsilon\right) - E\left(Y_t^* - \epsilon - \left(Y_{g^*-1}^* - \epsilon\right)\middle| X, C = 1\right)\right)\right]$$

A key identification assumption for my difference-in-difference model is that there is no other policy change around the same time as the state cigarette tax increase that also impacts cross-border shopping. The changes in policies like this are shown in Figure C3. For many of the other related policies, there appears to be no change in their level that coincides with the state cigarette tax increases.[26] However, Figure C3 does show that two related taxes (gas and e-cigarette) increased around the same time as the state cigarette tax. For gas taxes, Illinois increased this tax at the same time they increased their cigarette tax. This may be a concern as gasoline stations are one of the industries I use to identify cigarette retailers. To check if this impacted my estimates, I run a specification that does not include the number of visitors to gas stations in my outcome variable. For e-cigarette taxes, Cook County, Illinois raised their e-cigarette tax around the same time as Illinois increased their cigarette tax. As Cook County is a large county in Illinois that contains Chicago, this may have impacted e-cigarette prices for a large portion of the Illinois population. To account for this, I estimate a separate model excluding Cook County.

Another concern may be my choice of control states. Recall that the outcome for my controls are cross-border trips into the treated state. This was chosen as the control states have a lower tax level than the treated state for the entirety of the sample period. Given the theoretical discussion above, this implies that control state residents would have no reason to change their cross-border shopping behavior as the cigarette price should be lower in the state they reside in both before and after the treated state increased its cigarette tax. However, this may not always

$$ATT_{g,t} = \mathrm{E}\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{E\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)\left(Y_t^* - Y_{g^*-1}^* - E\left(Y_t^* - Y_{g^*-1}^* \middle| X, C=1\right)\right)\right]$$

[26] These include sales tax, county and place cigarette taxes, liquor tax, and beer tax.

be the case. For example, a control state resident's closest cigarette retailer may be in the treated state. When the treated state raises its cigarette tax, this control state resident may find it more conducive to shop in their own state. This would imply that the control CBGs may be impacted by the treatment.

I address these concerns about my control group in two separate ways. First, I show a time-series of raw data in the event study window for the control states in Figure C4.[27] The above discussion would suggest that a drop in cross-border shopping for control states due to the treated states raising their cigarette tax is an identification threat. An examination of this Figure would suggest no large change in cross-border shopping for the control CBGs around this time. Secondly, I run an alternative difference-in-differences specification where now the control group is the inner portion of my initial set of treated states, while the new treated groups are the outer portion of the initial set of treated states.[28]

Finally, I run three different estimators to test the robustness of my findings with the CS21 estimator. Two of these estimators include the stacked difference-in-differences estimator popularized by Cengiz et. al. (2019) and the Two-Way Mundlak Regression popularized by Woolridge (2021). Both estimators have been shown to estimate an unbiased average treatment on the treated effect in the presence of staggered policy implementation. The final estimator I use is the traditional TWFE model. While it is likely this estimator will provide a biased estimate of the average treated on the treated effect, I present it to show how biased my estimates would have been if I ran this specification. Covariates are the same in the stacked and TWFE models as

[27] Earlier in the paper, I only showed this figure for treated states.

[28] More specifically, the inner portion of the initial set of treated states is defined as the 3rd quartile of distance from a treated states border. The outer portion of the state is the 1st and 2nd quartile. As discussed above, the 4th quartile of distance from a treated state's border does not contain any observation from the initial set of treated states and so these observations were dropped.

they are in CS21, except they are interacted with a dummy variable for each month in the sample. For the Two-Way Mundlak Regression, continuous versions of covariates are used to accommodate the STATA command.

Results/Discussion

Main Analysis

My main results are given in Table 1. The first two columns of this Table concern difference-in-differences models for cross-border shoppers and in-state shoppers. For the first column, I estimate that that CBGs in my sample send an additional 0.69 monthly cross-border shoppers per 100 devices active in response to a cigarette tax increase. This coefficient represents an increase of 19% from the treated state's dependent variable mean in the pre-tax period. Using the fact that the median CBG has 77 devices active over the sample period, this implies that the median CBG sends an additional $0.69 \times (77/100) \approx 0.53$ monthly cross-border shoppers. The second column estimates that a CBG sends 4.98 fewer monthly in-state shoppers per 100 devices active to cigarette retailers in response to a cigarette tax increase. Notice the size of the coefficient is not symmetric with the number of cross-border shoppers a CBG sends. This result may have occurred if cross-border shoppers condensed in-state trips to multiple cigarette retailers to an out-of-state trip to one or few cigarette retailers. It may also reflect a drop in consumption for treated state residents.[29]

[29] Using the Behavioral Risk Factor Surveillance System (BRFSS), I do not find evidence of a drop in the probability of smoking using the CS21 estimator. This analysis uses the same states as my main analysis, except that the control variables are an individual's race, indicator for high school or less educational attainment, whether they live in an urban or rural area, and their age. Fixed effects and clusters in this analysis are on the state level, as this is the finest geography available in the BRFSS. Finally, I used the truncated adult weights to assure that no adult made up most or too few of the adult population in each state. Unfortunately, the BRFSS does not have a question about intensity of smoking for all the states over the sample period in my analysis. Further, the Tobacco Use Supplement of the CPS, which does have a question about the intensity of smoking, does not have all the states in my analysis over my sample period.

Table 1. Cross-Border Shoppers to Lower-Tax State and In-State Shoppers

	Cross-Border	In-state	Cross-Border (High Edu)	Cross-Border (Low Edu)	Cross-Border (Urban)	Cross-Border (Rural)	Cross-Border (Less Connect)	Cross-Border (More Connect)
DD	0.690**	-4.982^{+}	0.465*	0.801*	0.306*	1.140*	0.021	2.123^{+}
	(0.254)	(2.984)	(0.181)	(0.354)	(0.142)	(0.498)	(0.224)	(1.196)
Obs	658,210	658,210	309,487	348,699	382,959	275,251	108,840	108,813
Dep Mean	3.61	210.65	2.48	4.67	2.01	3.42	1.97	13.35
# Clusters	23	23	23	23	22	23	7	7

Notes: Bootstrapped standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as the number of monthly cross-border shoppers per 100 cellphones active in a CBG. "Dep Mean" is the mean of the dependent variable (noted at the top of the column with sample restrictions in parentheses) before the policy became effective. The number of clusters is smaller for the columns concerning connectiveness because they only consider the first quartile of distance to a lower tax border.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Using DeCicca et. al. (2013) as reference for my effect size, the authors in this paper found that a 10% increase in the cigarette tax increases the probability of cross-border shoppers among smokers by 30% of the sample average. As the tax increases in my sample have an average 77% increase, my result of only a 19% increase from the pre-tax cross-border shopping mean is small in comparison. These may have occurred for multiple reasons. First, smokers are about 2 percentage points (t-statistic of -5.46) less likely to own a cellphone compared to non-smokers according to the Behavioral Risk Factor Surveillance System (BRFSS) in 2018-2019. This statistic from the BRFSS implies that I may not be picking up all the cross-border shopping occurring, specifically for smokers that do not have a cellphone, therefore attenuating my estimate. Secondly, as mentioned in the introduction, e-cigarette sales did not begin to rapidly increase in the USA until 2010. As DeCicca et. al.'s (2013) sample ends before 2010, many smokers in their sample were likely not considering the purchase of a readily available substitute for cigarettes. Given this, smokers in my sample may have opted to purchase e-cigarettes as opposed to cross-border shopping for cigarettes. Finally, many members of my sample should not be cigarette users. This implies that I am considering an intent-to-treat analysis which will bias my estimate downwards. However, my estimate could be similar to DeCicca et. al.'s (2013) estimate in terms of number of cigarettes purchased in a lower-tax state if more cigarettes were purchased per trip in my sample than in DeCicca et. al.'s (2013) sample. Unfortunately, neither DeCicca et. al.'s (2013) sample nor my sample have information about the amount of cigarettes purchased per trip.

Extensions and Event Studies

The next six columns of Table 1 estimate on a sample that has been divided by the adult proportion of a CBG that has at most a high school degree, rural status of the CBG, and CBG connectiveness. Here I find, as predicted above, that rural CBGs send substantially more cross-border shoppers than urban CBGs. As a percentage of the pre-tax mean, rural CBGs send 33% more cross border shoppers, while the coefficient when conditioning the sample only on urban CBGs is 15% of its pre-tax mean. This result also suggests that previous papers that only used urban residents may have undercounted the extent that the probability of cross-border shopping would change in the face of a cigarette tax. It also implies that the time expenditure inequality presented in Figure 1 between rural and urban CBGs was made worse by these cigarette tax increases. I also find that CBGs with many lower educated adults send more cross-border shoppers than those with fewer lower educated adults. This result highlights that lower-educated adults may not only suffer a fiscal expenditure inequality on cigarettes as estimated by Conlon et. al. (2021), but also an exacerbated time expenditure inequality to avoid increases in cigarette taxes. Finally, I find that CBGs with a relatively high connectivity to border states send substantially more cross-border shoppers than CBGs with a relatively low connectivity.

Table 2 presents heterogenous effects by treated state and distance to the border. The first three columns of Table 2 displays group treatment effects for each treated state. The difference-in-difference estimate given uses the entire pre and post-period for each state indicated at the top of the column. For the first row that reports cross-border shopping, I find a difference in effect size by treatment dosage which suggests a dose-response relationship. Specifically, Kentucky, which increased its tax level by $0.50 has a smaller treatment effect that Oklahoma and Illinois, both of which raised their cigarette tax by $1.00. However, the estimate for Oklahoma is much smaller than that for Illinois. Moreover, only Kentucky and Illinois has a significant increase in cross-border shopping. For in-state shoppers, I find that none of the treated states show a significant decrease on this margin.

In the remaining three columns of Table 2, I present conditional average treatment effects by the minimum distance to a lower-tax border state. For cross-border shoppers, those whose distance is in the first quartile (within 18 miles) have the largest change in cross-border shoppers. As the quartiles of distance from the border increases, the coefficients significantly decline in magnitude compared to the first quartile and eventually become insignificant. For example, the effect size for the second quartile is about 14% of the first quartile. These results conform to the prediction above that there is a stronger increase in cross-border shopping for those closer to the border when faced with a cigarette tax. Further, these estimates in Table 2 suggest a non-linear response in cross-border behavior, which may not have been picked up in previous parametric estimates of this relationship (Harding et. al. 2012, DeCicca et. al. 2013). For the second row, in-state shoppers follow a similar pattern to cross-border, where there is no evidence of a decrease or increase for the second and third quartile of distance.

Table A3 presents results that pertain to the cigarette tax environment of bordering states. Here I find that states that surpassed several border states' tax level (Kentucky and Oklahoma) only sent a significant increase in cross border shoppers to states that had a lower tax level only after the tax hike. Further, I find that Illinois, who surpassed one border state's tax level, only sent a significant increase in cross-border shoppers to the remaining border states whose tax level was already lower than Illinois pre-tax. This result suggests that states that already have a higher tax level than most of its border states, as Illinois did, will still send a significant number cross-border shoppers after a tax increase. This further suggests that each state did have a significant increase in cross-border shoppers, but this increase depends on the destination of cross-border shopping being considered.

Table 2. Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by State and Minimum Distance of CBG to Lower-Tax Border Quartile

	Kentucky	Oklahoma	Illinois	0.12-18.73 Miles	18.74-61.17 Miles	61.17-142.69 Miles
DD (Cross-Border)	0.478^{+}	0.616	0.968^{+}	1.557^{***}	0.222^{***}	-0.008
	(0.247)	(0.454)	(0.524)	(0.397)	(0.037)	(0.096)
Obs	240,713	192,196	445,971	219,212	219,471	219,527
DD (In-State)	-2.541	-3.227	-8.941^{*}	-8.538^{+}	-6.120	2.014
	(1.888)	(6.993)	(4.182)	(5.145)	(5.363)	(5.254)
Obs	240,713	192,196	445,971	219,311	219,422	219,410

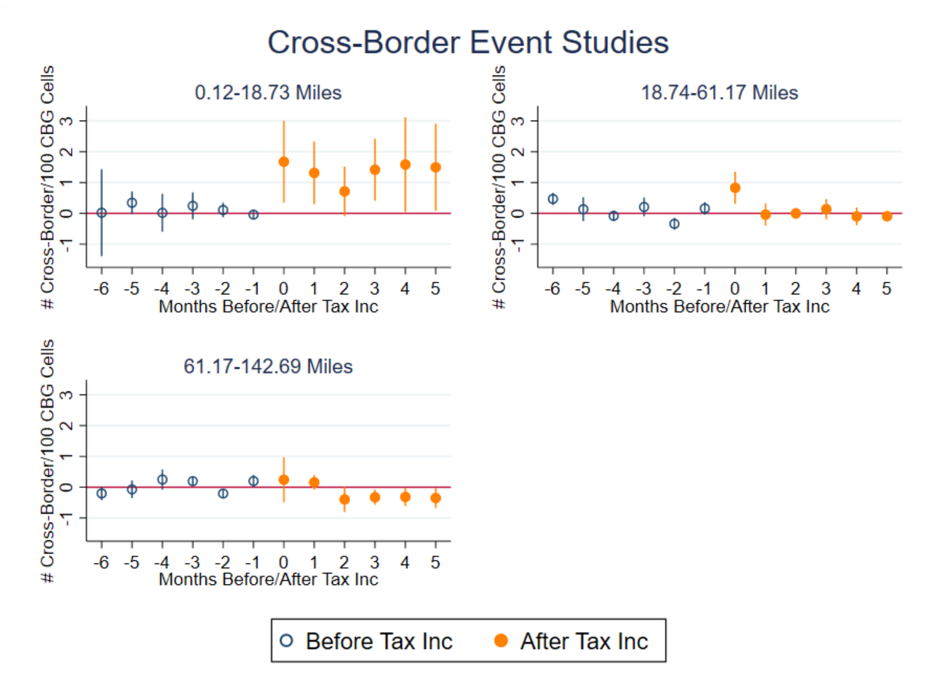
Notes: Bootstrapped standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as monthly cross-border shoppers per 100 cellphones active in a CBG. "Cross-Border" or "In-State" outcomes are indicated in row titles.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4 presents results when splitting the treatment group into quartiles of change in the minimum distance to a lower-tax border. Here I find that for those CBGs with the lowest change in distance to a lower tax border did not send a conventionally significant number of cross-border shoppers to lower-tax border states whose tax level was surpassed by the treated state. However, as this this change in minimum distance grows for larger quartiles, the number of additional cross-border shoppers a CBG sends to the set of lower-tax border states mentioned earlier tends to rise. Further, these estimates at higher quartiles tend to be conventionally significant, suggesting that lowering the minimum distance to a lower-tax border does spur more residents of the treated state to cross-border shop. The results in Table A4 then suggest that the decline in minimum distance to a lower-tax border is an important mechanism that can explain why cross-border shopping may increase when the treated state's cigarette tax increases. These results also suggest that controlling for a time-varying version of the minimum distance to a lower tax border could shut-off a potentially important component of how a raise in the treated state's cigarette tax can increase cross-border shopping.

The event studies for my results in Tables 1 and 2 are presented in Figures 3, C5, and C6. In Figure C5, I present the event studies for the full sample results presented in Table 1. The Figure presents a good case for parallel trends when the outcome is cross-border shoppers. Further, there appears to be evidence for a increase in cross-border shoppers for the first month only. For in-state shoppers, there appears to be some violations of parallel trends in the pre-period, however, no upward or downward trend emerges. The post-period also displays a short-lived drop in in-state shoppers that almost immediately returns to pre-period levels.

Figures 3 and C6 presents event studies for the estimates concerning cross-border and in-state shopping respectively by quartiles of distance to the border. Figure 3 shows there is a strong, sustained increase in cross-border shopping for CBGs within 18 miles of a lower tax border (first quartile). As the quartiles get larger, however, the initial increase becomes lower and generally does not last the full five months. Further, the event studies for these results show good evidence of parallel trends. For in-state shoppers presented in Figure C6, much like displayed in Figure C5, there appears to be no permanent drop in in-state shoppers after the tax becomes effective.

Figure 3. Event studies For Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by Minimum Distance to Lower-Tax Border Quartile



Notes: Bootstrapped confidence intervals are given as spikes on the point estimates presented as dots. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as per 100 cellphones active in a CBG. The outcome concerns monthly cross-border shoppers.

As stated above, the only lasting increase in cross-border shopping appears to occur within 18 miles of a lower tax border. Using this fact and the results from Table 2, I calculate the additional total number of monthly cross-border shoppers from CBGs in treated states within 18 miles of a lower tax border. This calculation is done by taking the coefficient of 1.55 from Table 2, dividing this by 100, multiplying this division by each CBG's population, and then summing the final product of each CBG by the state it resides. When this is done, I calculate that Illinois sent an additional 57,547.86 monthly cross-border shoppers, while Kentucky sent 44,547.69 and Oklahoma sent 12,158.58 additional monthly cross-border shoppers.

Alternative Estimation Strategies

Table A5 presents the results of the placebo analysis described above, the use of an alternative control group, the estimate where I leave out the number of visitors to gas stations when calculating the dependent variable, removing Cook County CBGs, and alternative difference-in-differences estimators. For the placebo analysis, I find that there is no significant increase in cross-border shopping to non-cigarette selling retailers who are classified in a cigarette retailer industry. This is evidence that there was no concurrent change around the time of each state's cigarette tax which increased cross-border shoppers to all retailers in the industries I consider. The null result in this analysis also provides support for my identification assumption that the overcounting of cross-border visitors to my set of retailers is constant over time.

When using an alternative control group where now the inner-core of the initial set of treated states is the control group and the outer CBGs of this set are the treatment group, I find an increase of 1.30 monthly cross-border shoppers per 100 cellphones active. While this estimate is larger than my preferred estimate of 0.69, it is less precise, most likely due to the decline in sample size. This drop in precision appears to lead to a large overlap in the 95% confidence intervals for both estimates, suggesting they are similar.

I further run my original analysis making two modifications. The first is that I do not include the number of visitors to gas stations in the dependent variable. This robustness check is performed as Illinois had a gas tax increase at the same time as its cigarette tax increase. When I perform this analysis, my difference-in-differences estimate is 0.41 ($p = 0.070$) additional monthly cross-border shoppers per 100 cellphones active. The fact that this estimate is smaller than my preferred specification estimate of 0.69 is expected as about 70% of smokers typically purchase cigarettes from a gas station/convenience store (Kruger et. al. 2017). The second modification I make is that I drop CBGs from Cook County, Illinois. This modification was done because Cook County had an e-cigarette tax increase around the same as Illinois's cigarette tax increase. For this specification, my estimate (0.771) is significant and similar to my preferred estimate of 0.69, implying the e-cigarette tax increase did not have sizeable impact on cross-border shopping.

The remaining columns in the Table deal with various difference-in-differences estimators. The first column repeats the Callaway and Sant'Anna (2021) (CS21) estimate I provided in Table 1. The two-way fixed effects (TWFE) model provides a similar estimate to CS21 and is significant but is about 42% higher. This result suggests that using the TWFE model would have provided a substantial overcount of the increase in cross-border shopping. The remaining difference-in-differences estimators presented in the Table agree that the cigarette tax increases significantly raised the amount of cross-border shoppers into lower-tax border states.

Percentage of 2019 Cigarette Tax Revenue

Using The Tax Burden on Tobacco data, I further compute how much cigarette tax revenue in Oklahoma and Kentucky in 2019 was lost due to cross-border shopping. To determine this, I first calculate how many sales were lost to cross border shopping in 2019 for both states. To do this, I first get the average treated on the treated coefficient for the 3 quartiles of distance to a lower-tax border for both Oklahoma and Kentucky. I then use the following relationship to get the change in cross-border shopping for each CBG[30]:

[30] The relationship is calculated in the following way:

$$\partial\text{Cross-Border}/_{\partial\text{Tax}} \approx \frac{\widehat{\beta^{J}_{ATT}}}{100} \times \text{CBG Population}$$

, where $J \in \{1,2,3\}$ and indexes the quartile of distance from a lower-tax border. As I know all of the values on the right hand side of the final equation, I can then calculate how many more monthly cross-border shoppers every CBG sent after its state raised its cigarette tax.

Next, I assign each CBG in each month of 2019 a proportion of sales that cross-border shoppers purchase in a border state. I do this by assigning each CBG-month a random number from a uniform distribution with a lower bound of 0.5 and an upper bound of 1. I then multiply this proportion by 30, which assumes that each cross-border shopper consumes one pack of cigarettes per day. If I add up 12 of these values multiplied by the relationship above for each CBG-month in either state considered, this will give me the total amount of sales lost to cross-border shopping in 2019. To calculate how much revenue leaked out of these states, I multiply this sum by the prevailing tax level in these states in 2019. This tax level for Oklahoma is \$2.03 and for Kentucky it is \$1.10.

Putting all these parts together, I compute the amount of leaked revenue attributable to cross-border shopping as a proportion of the given state's 2019 cigarette tax revenue as follows for a state $M \in$ {Kentucky, Oklahoma}:

$$\frac{\partial\widehat{\text{Cross-Border}}/_{\partial\text{Tax}}}{\text{\# of CBG Cell Phones}} \approx \frac{\partial\text{Cross-Border}/_{\partial\text{Tax}}}{\text{CBG Population}}$$
$$\frac{\partial\widehat{\text{Cross-Border}}/_{\partial\text{Tax}}}{\text{\# of CBG Cell Phones}} \times \frac{100}{100} \approx \frac{\partial\text{Cross-Border}/_{\partial\text{Tax}}}{\text{CBG Population}}$$
$$\frac{\widehat{\beta^{J}_{ATT}}}{100} \approx \frac{\partial\text{Cross-Border}/_{\partial\text{Tax}}}{\text{CBG Population}}$$
$$\partial\text{Cross-Border}/_{\partial\text{Tax}} \approx \frac{\widehat{\beta^{J}_{ATT}}}{100} \times \text{CBG Population}$$

The approximation on the first line should hold if the CBG is sampled randomly, implying the sample estimate is an unbiased estimator of the population parameter displayed on the right hand side.

$$\% \text{ of Revenue}_M = \frac{\left(\sum_{k=1}^{N} \sum_{b=1}^{12} z_{b,k,M} \times 30 \times \frac{\widehat{\beta_{M,ATT}^{J}}}{100} \times \text{CBG Population}_{k,M}\right) \times \text{Tax/Sale}_M}{\text{Cigarette Tax Revenue}_M}$$

, where N is the total number of CBGs in a state M and $z_{b,k,M}$ is the proportion of cigarette sales that cross-border shoppers in CBG k purchase in a border state in month b. Note that J is determined by which CBG is being considered. After performing this calculation, I find that cross-border shopping comprised 2.5% of Kentucky's 2019 cigarette tax revenue and about 0.1% of this revenue for Oklahoma. Without dividing by a state's cigarette tax revenue, I estimate that cigarette tax increases before 2019 increased cigarette tax revenue leakage in 2019 by $531,581 in Oklahoma and $9,084,824 in Kentucky. These results underly the impact cross-border shopping can have on tax revenue if a state decides to raise its cigarette tax.

Conclusion

In this paper, I estimated the change in cross-border shopping in response to a cigarette tax increase for three states in the USA over 2018-2019. I found that the median census block group (CBG) sent about 0.53 more monthly cross-border shoppers in response to a cigarette tax increase. This magnitude is approximately a 19% increase from the before tax mean. I confirmed this result by showing larger increases in cross-border shopping for CBGs closer to a lower-tax border. Event studies revealed that the only lasting change in cross-border shopping happened for CBGs whose minimum distance to a lower-tax border is less than 18 miles. Using my main estimates, I also found that cross-border shopping comprised 2.5% of Kentucky's 2019 cigarette tax revenue and about 0.1% of this revenue for Oklahoma. This back of the envelope calculation showed that increases in cross-border shopping can impact revenue in states that raise their cigarette taxes.

I further divided my sample by adult educational attainment in a CBG, rural CBG status, and CBG connectiveness to border states. First, I estimated that CBGs with higher connectivity (as measured by cross-border commuting) to border states sent substantially more cross-border shoppers than those CBGs with relatively lower connectiveness. I also found that CBGs with more adults with a high school or less educational attainment send substantially more cross-border shoppers in response to a cigarette tax than CBGs with a low amount of these adults. I further estimated that rural CBGs send many more cross-border shoppers when faced with a cigarette tax increase than CBGs in urban areas. The latter result suggests that previous papers that relied solely on urban smokers likely undercounted the extent of the increase in cross-border shopping when a treated state's cigarette tax increases. Both results detailing CBG demographics highlights that cigarette tax increases can exacerbate spatial and education-based time expenditure inequality for who cross-border shop.

In sum, my paper suggests that the increase in cross-border shopping in response to a cigarette tax increase is remains substantial in recent years (2018-19). Policy makers should keep cross-state evasion opportunities in mind when raising state cigarette tax. To avoid this issue, states may wish to coordinate tax increases. For example, states have been successful in coordinating simultaneous cigarette tax increases with Native American reservations in Oklahoma and New Mexico. Given the theoretical discussion earlier in this paper, a simultaneous tax increase by border states of the same magnitude as the tax-raising state would cause no change in cross-border shopping. However, this may cause another issue where states surrounding this initial group of border states would have to increase their cigarette tax to avoid receiving additional cross-border shoppers and so on. This kind of coordination may be politically infeasible. Another option would be to enact cigarette tax increases at the federal level, which could eliminate any increase in cross-border shopping across states.

Beyond coordination, states may need to resort to other methods to decrease cigarette consumption in their state besides using increases in cigarette prices. As multiple recent studies suggest (Callison and Kaestner 2014, Hansen et. al. 2017), cigarette taxes have recently[31] had a smaller impact on cigarette consumption than in the past. This may be due to smokers in the current era having relatively price-inelastic demand, implying further price increases will not impact the total quantity demanded for cigarettes substantially. Moreover, I confirmed that cigarette taxes in my sample do not seem to immediately impact smoking probability using data from the Behavioral Risk Factor Surveillance System.[32] Together with my finding that cigarette tax hikes continue to incentivize additional cross-border shopping behavior, states may want to consider alternative policy levers to decrease cigarette consumption. These could include making sure less harmful substitutes such as e-cigarettes or General Snus are not too expensive, lessening the price of smoking cessation devices such as nicotine patches, and making readily available medication to abate smoking such as Chantix.

[31] Callison and Kaestner's (2014) estimates pertain to adult smoking from 1995-2007. Hansen et. al. (2017) estimates pertain to youth smoking from 2007-2013.
[32] See footnote 27.

Chapter II: Revisiting the Effect of Cigarette Taxes and Indoor Smoking Restrictions on Prenatal Smoking

Introduction

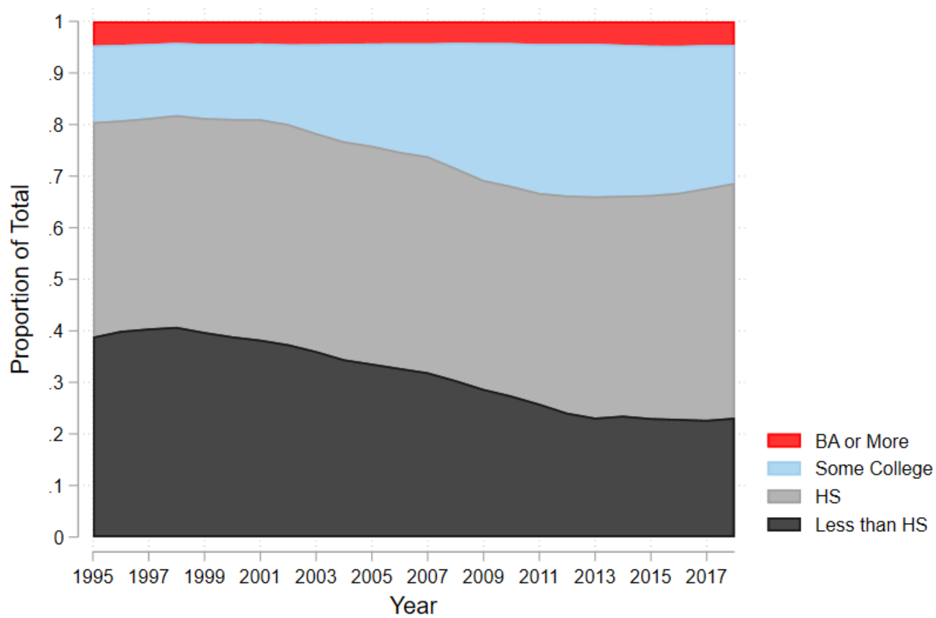
In 1969, the Surgeon General warned that prenatal smoking increases the probability of neonatal mortality, defined as infant death within 28 days of birth. Since then, research has also linked prenatal smoking to low birth weight and premature birth (U.S. Department of Health and Human Services, 2014). Descriptively, national vital statistics data in 2018 indicate that pregnant women smoking in the third trimester, compared to pregnant women not smoking at all prenatally, were 1.8 times more likely to have a low-birth-weight birth, 1.4 times more likely to have a premature birth, and these infants were 2.2 times less likely to survive their first year of life.[33] Given the estimated danger of prenatal smoking to a pregnant women's child, reducing prenatal smoking prevalence has been a major public health goal in the USA. For example, a major objective of Healthy People 2030 is to decrease cigarette smoking by pregnant women from 6.5% in 2018 to 4.3% by 2030.[34]

Further, we show in Figure 4 that in 2018, 68.7% of first-time pregnant women that smoke during pregnancy have at most a high school degree. If cigarette policies can cause sustained cessation for pregnant women with lower education, this could help reduce education-related health disparities for the pregnant woman and her future child. On the other hand, cigarette taxation could exacerbate education-related disparities for individuals unable to reduce

[33] While these statistics are descriptive in nature, they support conclusions made in numerous Surgeon General reports that prenatal smoking is harmful. While not specific to pregnant women, recent economics research supports the negative relationship between smoking and health (Darden et al. 2018, Friedson et al. 2021).
[34] https://health.gov/healthypeople/objectives-and-data/browse-objectives/pregnancy-and-childbirth

or quit smoking in response to taxes and who therefore spend a larger share of their disposable income on cigarettes.[35]

Figure 4. Prenatal Smoking by Education Attainment



Notes: The outcome in this figure shows the proportion of pregnant women giving their first birth who prenatally smoked by educational attainment between 1995-2018.

In this study, we examine the impacts of cigarette taxes and indoor smoking restrictions (ISR) on prenatal smoking. We make three distinct contributions to the prior literature on the effect of cigarette taxes and ISRs on prenatal smoking. First, we use a modified stacked difference-in-differences (DD) model (Cengiz et al. 2019) to reduce bias caused by using traditional two-way fixed effect (TWFE) models in cases of staggered policy adoption when policy effects vary over time (Gibbons et al. 2019, Goodman-Bacon 2021). The stacked DD

[35] Decicca et al. (2022) for example find that the lowest income individuals spent over 2.5% of their family income on cigarette taxes in 2014-15, whereas the highest income individuals spent virtually zero percent of their family income on cigarette taxes.

model reconstructs the data so that states with a "policy event" during the sample period do not return as controls for later treated groups to study the same policy. Thus, we can mitigate this source of bias in calculating the effect of cigarette taxes and ISRs. Second, prior work has looked at policies through at most 2010, and we extend the years through 2018. Some recent studies warn that cigarette taxes may be less effective at reducing smoking in recent years than in prior time periods for youth (Hansen et al. 2017) and adults (Callison and Kaestner 2014), possibly because remaining smokers have hardened preferences for smoking. In our analysis, we consider six policy changes whose post tax/ISR increase period extend beyond 2010. By extending the analysis to 2018 we have an opportunity to re-explore whether tax responsiveness is changing for pregnant women as well. Third, we evaluate the parallel trends assumption which has been missing from prior work on this topic, which provides evidence towards whether our study is appropriately identified.

In Table B3, we summarize studies that use a clear treatment and control group to estimate the effect of contemporaneous cigarette taxes or ISRs on prenatal smoking. Most of these studies find statistically significant evidence that cigarette taxes can be effective in reducing prenatal smoking (Evans and Ringel 1999, Colman et al. 2003, Lien and Evans 2005, Adams et al. 2012, Simon 2016, Gao and Baughman 2017). Markowitz et al. (2013), however, reports large standard errors for their insignificant point estimates and Gao and Baughman (2017) do not find effects of cigarette taxes on older pregnant women. Collectively these studies find evidence that cigarette taxes decrease prenatal smoking, suggesting an average decline of 3.0 ppts in prenatal smoking probability per one dollar increase in cigarette taxes.[36]

[36] We use all tax studies listed in Appendix Table 1 for this average except Adams et al. (2012) since this study did not have a prenatal smoking outcome. Each study received equal weight in our average. When multiple estimates of cigarette tax effects on prenatal smoking existed within a single study (such as for different ages or states) we first averaged within the study to obtain a study-specific estimate, which we then used as part of the overall average

In contrast, we find little effect of cigarette taxes on prenatal smoking, extending the conclusion that cigarette tax responsiveness has been lower in recent years for youth (Hansen et al. 2017) and adults (Callison and Kaestner 2014). One recent study finds large effects of in-utero cigarette taxes between the years 1965 to 2001 on the female fetus' later-life prenatal smoking when she herself gives birth. The paper speculates that higher in-utero taxes may alter the composition of remaining smokers and contribute to reductions in contemporary cigarette tax responsiveness (Hoehn-Velasco et al. 2022).[37] Another possible explanation for our result that could be viewed as complimentary with Hoehn-Velasco et al. (2022) is that cigarette firms distribute coupons in states that raise their cigarette tax to offset the price increase for smokers (Ribisl et. al. 2022). These coupons could allow pregnant women who prenatally smoke to decrease the effective price they pay for cigarettes and so continue their pre-tax increase cigarette consumption habits.

Fewer studies examine the impact of ISRs on prenatal smoking. Previous studies using clear treatment and control groups have examined the impact of ISRs on prenatal smoking (Adams et al. 2012, Markowitz et al. 2013, Bharadwaj et al. 2014, Barholomew and Abouk 2016, Gao and Baughman 2017), and only one (Bharadwaj et al. 2014) finds a statistically significant impact. The Bharadwaj et al. (2014) study was for the effect of a comprehensive ISR implemented in hospitals in Norway, which has unclear generalizability for the effect of more general ISRs affecting restaurants, bars, and private workplaces. Overall, there is limited evidence that ISRs affect prenatal smoking. In our study, we do not find that large ISRs reduce

across studies. Our math is as follows (following studies chronically as presented in Appendix Table 1): 0.036 = {-0.045 + -0.11 + ([-0.0092 + -0.0011 + -0.0379 + -0.0114] / 4) + ([0.0003 + 0.001 + 0.004 + -0.003] / 4) + -0.0092 + ([-0.0004 + -0.0002 + 0.0006 +0.0040] / 4)} / 6.

[37] While in-utero cigarette taxes have large effects on prenatal smoking, controlling for these or not had no impact on contemporary cigarette tax coefficients. We do not include this study in Appendix Table 1 since the paper's primary focus is on in-utero taxation and the authors make no attempt to study contemporary cigarette tax effects causally.

the probability of prenatal smoking but we do find that ISRs reduce the intensity of prenatal smoking by about 1.5 packs over a full, nine-month pregnancy (unconditional on smoking status).

Data

The main source of data used in this paper is the National Vital Statistics System provided by the Centers for Disease Control and Prevention (CDC) for years 1995 to 2018, which contains the near universe of all births in the United States. Each year there are approximately 4 million live births. The certificate of live birth form provides information on pregnant woman's education, race, age, and marital status at the time of birth along with information on prenatal smoking habits. [38] Unfortunately, the data only provides information on cigarette consumption and not other nicotine dispensing devices such as e-cigarettes. Therefore, our focus in this study will be on cigarette consumption as opposed to both e-cigarette and cigarette consumption. In our preferred specification, we restrict our sample to first births who may be most responsive to tobacco control policies; however, we explore the effect of including subsequent births in a sensitivity analysis.

Our total cigarette tax measure is created by summing federal, state, and county-level taxes.[39] We obtain state-level taxes from the CDC State Tobacco Activities Tracking and Evaluation (STATE) System and local taxes from proprietary American Non-Smokers Rights Foundation (ANRF) data. We construct the ISR policy variable following methods used by Cooper and Pesko (2017) and Pesko et al. (2020). Specifically, we use ANRF data on smoking bans in restaurants, bars, and private workplaces at the state, county, and smaller geographic

[38] www.cdc.gov/nchs/data/dvs/birth11-03final-ACC.pdf
[39] For taxes at a geography lower than the county level, we weight them up to the county level by using the proportion of the county population that the geography comprises.

levels to calculate the percent of the population covered under these laws in each county. Laws applied to bars, restaurants, and private workplaces are weighted equally, with partial bans (e.g., separate smoking areas) given half the weight of a full ban. This county-level measure is then weighted up to the state-level using the proportion of the county's population out of the total state's population.

We control for a number of concurrent policies, including: Tobacco 21 laws (Abouk 2021), indoor vaping restrictions from the ANRF data, e-cigarette taxes (Cotti et al. 2021), marijuana legalization laws (Anderson and Rees 2021), state-level beer taxes,[40] state-level poverty rate determined by the Census, state-level unemployment rate from the Bureau of Labor Statistics, and state-level effective minimum wage levels.[41] All treatment and control variables are merged on date of conception and state of residence. We show trends for our main smoking outcomes and policy variables in Figure D1.

Methods

We use a stacked DD model to assess the impact of our tobacco control policies of interest, cigarette taxes and ISRs, on prenatal smoking. To implement this, we isolate policy changes with no change in the policy four years before and three years after the effective date. The state that implements such a policy change is counted as an "event". As an example, Florida passed a $1.00 cigarette tax increase in quarter 3 of 2009. For the period between quarter 3 in 2005 and quarter 3 in 2012, neither the state of Florida nor any jurisdictions within passed another cigarette tax change; therefore, this time interval in Florida is identified as an event. Of all available events in our study, only events in the top 50% of changes in magnitude are kept

[40] https://www.taxpolicycenter.org/statistics/state-alcohol-excise-taxes
[41] http://ukcpr.org/resources/national-welfare-data

since these are likely to have the largest impact on prenatal smoking.[42] Tables B4 and B5 detail the events chosen for cigarette taxes and ISRs, respectively. Cigarette tax increases ranged from $0.54 to $1.00 and ISR expansions ranged from 39 to 99 ppt increase in population coverage of bans on smoking in bars, private workplaces, and restaurants.[43]

We then choose control states for each event, which cannot have been previously used as an "event" to avoid comparisons of earlier treatment states being used as a control group for later treatment states (Goodman-Bacon 2021). Using Florida again as an example, the control states for Florida (i) did not have a cigarette tax change from quarter 3 in 2005 to quarter 3 in 2012 and (ii) were not events at a date earlier than quarter 3 of 2009, which is the effective date for Florida's cigarette tax. All states have some level of cigarette taxation at baseline, and so allowing states without tax increases within the event window to be used as controls (provided these states were not used previously as events in our study) is necessary so that there are available control states.[44] While not ideal to allow states to be used as controls that have an existing policy, there is little literature to guide how to correct bias from TWFE estimation of staggered adoption in the presence of heterogeneous treatment effects when all units are treated on some level for the full time period, the treatment is continuous, and it changes regularly; therefore, our use of the stacked DD model in this case is novel.

Each event and its control states then constitute a "stack", which has its own effective date for the corresponding event. As shown by Tables B4 and B5, there are 13 cigarette tax

[42] This is consistent in spirit with Cengiz et al. (2019). Specifically, they exclude minimum wage increases less than 25 cents or where less than 2 percent of the workforce earned between the new and the old minimum wage.
[43] These amounts are lower than 100% due to the presence of local ISRs passed at least four years before the "large" ISR used as the ISR event.
[44] Further, control states also could have had a different policy change over the period being considered for each treated state (e.g., an ISR change would not preclude a state from being used as a treatment or control for the cigarette tax stacks).

stacks and there are 9 ISR stacks. We then append the stacks together for each policy, resulting in separate analytic datasets for cigarette taxes and ISRs.

Descriptive statistics for the cigarette tax stacks are shown in Table B1 and for the ISR stacks are shown in Table B2. For the cigarette tax stack, prenatal smoking rates are lower and tobacco control policies higher for the treatment groups rather than control groups. For the ISR stack, prenatal smoking and tobacco control policies are similar.

Using cigarette taxes and ISRs as treatment variables, we run the following model for each policy separately:

$$y_{i,k,s,t} = \beta_0 + \beta_1(Post_{k,t} \times Treat_{k,s}) + \delta X_{i,s,t} + \lambda_{k,t} + \eta_{k,s} + \epsilon_{i,k,s,t}$$

, which indexes individual *i* residing in state *s* and conceiving at year-quarter *t*. Each event, along with its selected control states, are grouped in stack k. $X_{i,s,t}$ is a set of demographic and policy controls,[45] $\lambda_{k,t}$ is stack-year-quarter fixed effects, and $\eta_{k,s}$ is stack-state fixed effects. The variable $Post_{k,t} \times Treat_{k,s}$ is used to estimate the average treated on the treated estimate β_1, where $Post_{k,t}$ is a binary variable denoting observations before (=0) or after (=1) the policy became effective in stack k and $Treat_{k,s}$ is a binary variable that takes a value of 1 if the state experienced an event at any point in stack k or 0 if it did not. The variable $y_{i,k,s,t}$ is either a binary variable with a value of 1 if the pregnant woman reported smoking at all during pregnancy and 0 if not, or is an intensive measure of the average number of cigarettes smoked per day during pregnancy, which includes zeroes if the pregnant woman reported not smoking throughout their whole pregnancy.

[45] In the full specification, this includes pregnant women demographics (age, race/ethnicity), e-cigarette policies (indoor air laws, purchasing laws, and taxes), marijuana laws (medical and recreational), tobacco 21 laws, beer taxes, unemployment rate, poverty rate, and minimum wage.

For all models, we drop the year before the effective date of the policy, which drops all pregnant women who had the policy event occur during their pregnancy (versus before or after it). This sample choice eliminates ambiguity regarding whether this period of time should be counted as part of the control or treatment group.[46]

We run an event study of the following form:

$$y_{i,k,s,t} = \alpha_0 + \sum_{j=-16, j\neq -5}^{11} \alpha_j b_{k,s,t}^{j} + \delta X_{i,s,t} + \lambda_{k,t} + \eta_{k,s} + \epsilon_{i,k,s,t}$$

, where $b_{k,s,t}^{j}$ are indicators for treatment state s when the event occurs 5 to 16 year-quarters before conception and 0 to 11 year-quarters after conception (year-quarters 1 to 4 before conception are dropped for reasons described previously). Our event study specification provides a test for parallel trends in the outcome variable $y_{i,k,s,t}$ before an event, and also allows observation of heterogenous effects of the policy in the post period. An indication that the parallel trends assumption holds is if each α_j is statistically indistinguishable from each other for $j < 0$. Standard errors for all regressions are clustered at the state level.

We also consider a variety of extensions and robustness checks. First, we stratify our sample by education, race, and age to see if there are heterogenous treatment effects across demographic groups that are more likely to prenatally smoke (and hence potentially more responsive to policy changes) (Drake et al. 2018). Second, we run sensitivity checks that vary different aspects of our stacked DD regression model, such as considering only second or later births (rather than first births), adding controls for distances to locations with lower cigarette

[46] Average gestational length is roughly 9 months, but we exclude a full year prior to the policy to avoid censoring based on gestational length.

taxes and ISRs (to control for ease in which the policy event can be avoided),[47] looking at differences in laws that became effective before and after 2005 (roughly the midpoint of our sample), dropping all time-varying policy and economic indicator controls, using a continuous treatment variable rather than indicator variable for the event,[48] and expanding the number of years before and after the effective date of the policy to 5 years. These analyses help to solidify and extend the main results described above.

Finally, we also consider whether either policy shifted the pregnancy decisions of women in the USA. We consider this margin as a shift in fertility rates could give misleading results for the impact of either policy on prenatal smoking. For example, a tax increase could crowd out women who smoke from having children. If this occurred, our model may estimate that cigarette taxes decreased prenatal smoking, when in fact these tax increases discouraged women who smoke from having children. As these women wouldn't appear in our data after the tax increase, the proportion of mothers who smoke would appear to decline in this period. However, this decline would not be attributable to the tax decreasing smoking during pregnancy, but instead because women who smoke did not have children.

Results

Main Analysis

The results from our stacked DD model described above are presented in Table 3. In Panel A, we find little evidence that cigarette tax changes affect the probability of smoking

[47] For each county we calculate 1) closest county with a lower cigarette tax, 2) closest county with 50 cents or more lower cigarette tax, 3) closest county with a one dollar or more lower cigarette tax, and 4) closest county that does not have a comprehensive ISR (equals zero for counties that themselves do not have a comprehensive ISR). These variables represent travel opportunities to avoid the regulation, with lower values making it less likely the regulation is binding. We then population weight each of the four variables to the state level. We include the three tax variables as controls in the cigarette tax model and the ISR distance variable as controls in the ISR model.

[48] One benefit of using a continuous treatment variable is that it makes our methods more compatible with previous papers.

during pregnancy, estimating that a cigarette tax increase "event" (which averages to a $0.76 increase in cigarette taxes) is associated with a 0.2 percentage point (ppt) decrease in prenatal smoking. This coefficient is a 1.7% change from the pre-period dependent variable mean for treated states and is not statistically significant. We also do not find evidence that cigarette tax increases impact the intensity of prenatal smoking.

Table 3. Main Results for Maternal Prenatal Smoking by Cigarette Policy

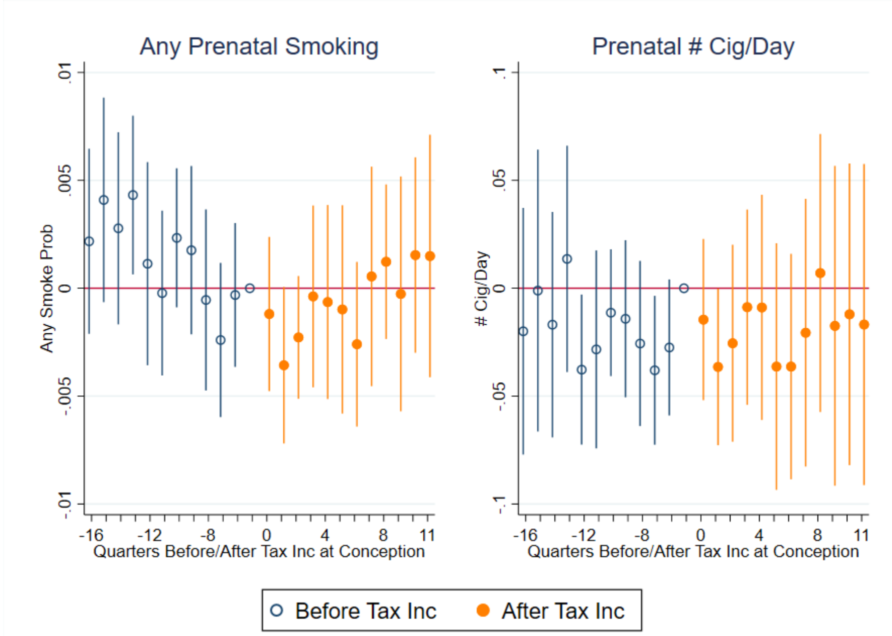
	Any Cig	Cig/Day
Panel A: Cigarette Taxes		
DD	-0.0019	-0.0018
	(0.0021)	(0.0242)
	[0.3815]	[0.9403]
Dep. Var. Mean for Treated States	0.086	0.703
Obs	290,892	290,853
Panel B: ISRs		
DD	-0.0069	-0.1103+
	(0.0062)	(0.0575)
	[0.2773]	[0.0687]
Dep. Var. Mean for Treated States	0.07	0.58
Obs	194,557	194,528

Notes: The data in this table shows results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in table B4 or B5. Standard errors in parentheses and p-values in brackets. ISR stands for "Indoor Smoking Restriction". The outcome for the first column is whether a pregnant woman smoked or not during pregnancy and for the second column it is the unconditional average number of cigarettes per day during pregnancy. All models use stack-state FEs and time is controlled by stack-year-quarter FEs. All columns control for the variables mentioned in the summary statistics table (either table B1 or B2) at the time of conception.
+ p<0.1,* p < 0.05, ** p < 0.01, *** p < 0.001

In Panel B, we do find that that ISRs significantly decrease the intensity of prenatal smoking ($p < 0.10$). An ISR "event" (which on average increases the population coverage of ISRs by 62.3 ppts) is estimated to reduce cigarettes smoked per day by 0.11 (20% of the mean for treatment states in the pre-period), or roughly 1.4 fewer cigarettes daily per smoker ($-1.4 =$

−0.11 / 0.079, using a prenatal smoking rate of 7.9%). Also in Panel B, an ISR event reduces the probability of prenatal smoking by 0.7 ppt, which is economically significant but not statistically significant.

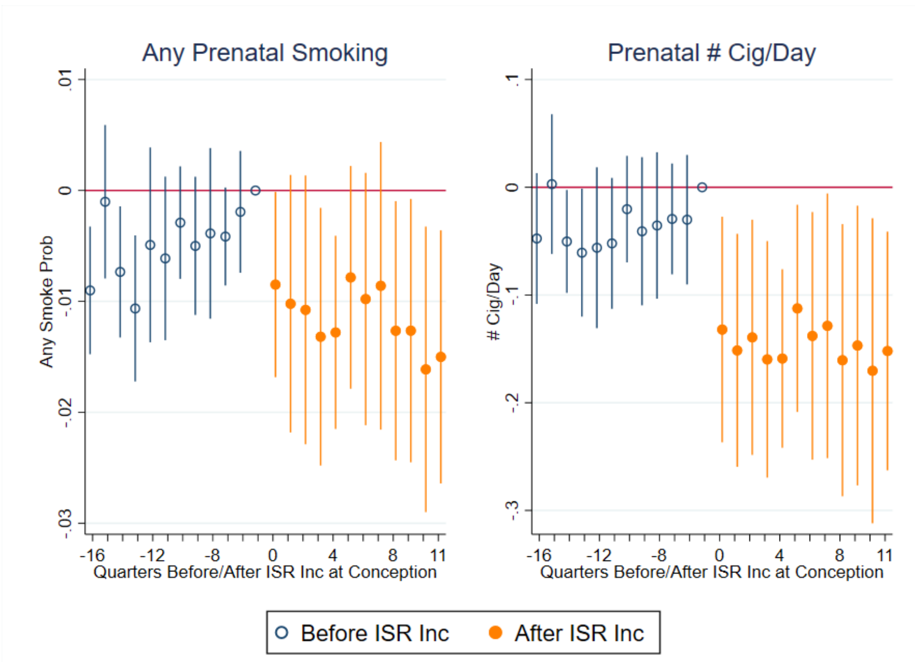
Figure 5. Event studies for Cigarette Tax Increase



Notes: The outcomes in these figures show results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in Table B4 or B5. "ISR" refers to an Indoor Smoking Restriction. "Any Prenatal Smoke" refers to whether a pregnant woman smoked or not during pregnancy and "# Cig/Day" is the unconditional average number of cigarettes per day during pregnancy. All models use state FEs and time is controlled by national event-year-by-quarter FEs. All models also control for the variables mentioned in the summary statistics table (either B1 or B2).

Figures 5 and 6 shows event study results. These plots provide evidence of the parallel trends assumption being satisfied for ISRs, but there is some evidence based on the gradual monotonic decrease in cigarette tax pre-period coefficients that cigarette tax events may be more likely to occur in states with pre-existing declines in prenatal smoking prevalence. However, this pre-period monotonic decline is not noticeable in prenatal smoking intensity event study graphs. In looking at the post-period, there is little noticeable change from the pre-period due to cigarette taxes, but an immediate reduction in prenatal smoking outcomes due to ISRs. Therefore, the event study results align very closely with the Table 3 results, suggesting prenatal smoking reductions due to ISRs and no effect of cigarette taxes (regardless of any potential bias that may be caused by cigarette tax pre-trends). The post-period event study coefficients for ISRs suggest approximately a 1.0 ppt reduction in prenatal smoking and 1.5 fewer cigarettes per day, which closely match stacked DD estimates from Table 3.

Figure 6. Event studies for Indoor Smoking Restrictions



Notes: The outcomes in these figures show results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in Table B4 or B5. "ISR" refers to an Indoor Smoking Restriction. "Any Prenatal Smoke" refers to whether a pregnant woman smoked or not during pregnancy and "# Cig/Day" is the unconditional average number of cigarettes per day during pregnancy. All models use state FEs and time is controlled by national event-year-by-quarter FEs. All models also control for the variables mentioned in the summary statistics table (either B1 or B2).

Heterogeneity, Extensions, and TWFE

In Figures D2 and D3, we stratify our sample by education, race, and age to see if there are heterogenous treatment effects across demographic groups. There is some evidence that Black pregnant women may experience disproportionate reductions in prenatal smoking due to cigarette taxes, which is particularly noteworthy given that Black women have lower prenatal smoking rates than whites (5.6% vs. 9.6% in our estimation sample). Meanwhile, ISRs may reduce prenatal smoking intensity by more among lower educated and younger pregnant women.

Sensitivity checks are presented in Figures D4 and D5. For ease of comparison, the first coefficient in each graph shows our Table 3 preferred model result, which can then be compared to each sensitivity analysis result. In order of sensitivity checks, we do the following: (1) use second and later births instead of only first birth, (2) control for distances to locations with lower cigarette taxes and ISRs, (3 and 4) use data from 2005 and before and 2006 and after (roughly the midpoint of our sample, and helping to test whether responsiveness changes over time), (5) drop time-varying control variables, (6) use a continuous treatment variable for the level of the "event" rather than an indicator variable (thus compensating for the relative sizes of events), and (7) use a five-year window on both sides of the event (versus just a three year window on both sides of the event like we use otherwise).[49]

Overall, sensitivity check results are very similar to baseline results. There is some evidence that more recent cigarette taxes and ISRs (2006 and beyond) may have had a larger effect on smoking intensity than earlier policies (2005 and before), but these differences are not statistically significant, nor do they materialize in terms of smoking prevalence.

In Table B6, we further compare our stacked DD estimates to the traditional TWFE model. The TWFE model includes continuous versions of both treatment variables simultaneously. Coefficients between TWFE and stacked models are virtually identical for smoking prevalence, but vary somewhat for smoking intensity (with the stacked models producing larger reductions).

[49] To perform this analysis, we use fewer stacks and control states as many states have an additional policy change in the 5-year window before/after the initial effective date, making them ineligible to be an event when using this event window length. In turn, this lowers the size of our sample for this analysis and will make our estimates noisier.

We further ran specifications which estimated whether the policies we considered shifted the fertility rate of women in the sample. The main table for these estimates is Table B7. We find little evidence that either policy impacted the fertility rate of women in our sample. One exception is that ISRs may have decreased fertility for women who are neither white nor Black (labeled as "Other Race"). However, this estimate is small, representing a 15% decline from the pre-ISR increase mean. Further, the event study for this result, which is given in Figure D8, reveals a downward trend in the fertility rate for other race women before the ISR became effective. As such, it is unlikely that ISRs causally decreased the fertility rate for this subset of women. The other event studies given in Figures D6, D7, and D8 confirm the results presented in Table B7 that neither policy appears to have impacted the fertility rate of women in our sample.

Chapter II Conclusion

We find some evidence that indoor smoking restrictions (ISRs) modestly decrease prenatal smoking and that cigarette taxes do not significantly impact prenatal smoking. Markowtiz et al. 2013 also finds that cigarette taxation has no effects on prenatal smoking, but our findings differ from several other studies showing effects (Evans and Ringel 1999, Colman et al. 2003, Lien and Evans 2005, Adams et al. 2012, Harris et al. 2015, Simon 2016, Gao and Baughman 2017). In our sensitivity checks (Figures D4 and D5), we report that a $1 change in cigarette tax events is associated with a 0.25 ppt reduction in prenatal smoking, which is considerably lower than the average effect of 3.0 ppts from across these studies (see footnote 4). Additionally, we find evidence that ISRs cause small reductions in prenatal smoking, which differs from prior work that has not found effects (Adams et al. 2012, Markowitz et al. 2013, Gao and Baughman 2017). Since our TWFE and stacked DD results are similar for smoking participation, the differences between our results and the prior literature for the effect of cigarette taxes on any use appears to stem from adding extra years to the analysis rather than differences in estimation method.

One explanation as to how pregnant women in the 21st century who smoke have more unanimously acquired these hardened preferences comes from Hoehn-Velasco et al. (2022). In this paper, the authors argue that higher cigarette taxes in-utero decreases the propensity to prenatally smoke later in adult life. However, this effect is only found for women who gave birth after 2006, which would correspond to these women being in-utero in the 1980's and 1990's. As the authors point out, one implication of this finding is that higher in-utero cigarette taxes may have nudged women giving birth after 2006 who would have otherwise prenatally smoked to not do so. This would further imply that the remaining women who did prenatally smoke after 2006 were not on the margin of participating in prenatal smoking, and so many may not respond significantly to contemporaneous cigarette taxes. This could help explain not only why our paper did not find a significant effect of cigarette taxes on prenatal smoking, but also way previous papers with less tax variation after 2006 and more before tended to find significant effects. However, as presented in Figure D4, we cannot tell whether the effects of the contemporaneous taxes differ before and after 2006 as our results are noisy for both estimates.

Another possible explanation for our findings is that that cigarette firms distribute coupons in states that raise their cigarette tax to offset the price increase for smokers (Ribisl et. al. 2022). These coupons could then allow pregnant women who prenatally smoke to decrease the effective price they pay for cigarettes and so continue their pre-tax increase cigarette consumption habits.

One implication of our findings is that policymakers interested in reducing prenatal smoking-related health disparities may be better off using ISRs rather than cigarette taxes for several reasons. First, we find ISRs to be more effective in reducing prenatal smoking, including for lower-educated individuals that are more likely to prenatally smoke. ISRs, compared to cigarette taxes, may also reduce education-related health disparities by less directly reducing disposable income among low-income individuals that already pay a large share of their income in cigarette taxes (DeCicca et al. 2022). ISRs nudge pregnant women towards smoking cessation without removing money from their hands like cigarette taxes do; therefore, ISRs do not crowd-out other potential health investments pregnant women may otherwise make (e.g., prenatal vitamins). Further, ISRs may also have spillover benefits disproportionately accrued to low-educated pregnant women that are more likely to work in restaurants and bars and therefore experience larger reductions in secondhand smoke exposure.[50]

A second implication of our findings is that policymakers may wish to consider alternative strategies to reduce prenatal smoking. We find that cigarette taxation has little immediate effect on prenatal smoking. While ISRs could reduce prenatal smoking in places that do not have comprehensive bans, in 2018 the ISR index for the average pregnant women was 80.4%, meaning that there is little upward expansion opportunity. One option for policymakers to consider for the goal of continuing to reduce prenatal smoking is to avoid over-regulating reduced-risk tobacco products like e-cigarettes. Prior research finds that e-cigarette regulation increases prenatal smoking, suggesting that low regulation (or even subsidizing these products) could lead to sizable reductions in prenatal smoking (Cooper and Pesko 2017, Pesko and Currie 2019, Abouk et al. 2021) and possibly improvements in infant health outcomes (Cooper and

[50] Using the National Health Interview Series, we calculate that about 60% of pregnant women in a Food Preparation and Serving Related Occupation have at most a high school degree, compared to the approximately 40% of pregnant women in this occupational class that have more than a high school degree. This calculation was made over the years 2004-2018 to keep the definition of "Food Preparation and Serving Related Occupations" constant. Further, the calculation used the "final basic annual weight" for each observation.

Pesko 2022). However, with this policy option, careful attention should be paid to the tradeoffs of reducing prenatal smoking and increasing use of reduced-risk tobacco products.

Dissertation Conclusion

In this dissertation, I estimated how cigarette regulation impacts two distinct outcomes: cross-border shopping and prenatal smoking. In the first chapter, I estimated the change in cross-border shopping in response to a cigarette tax increase for three states in the USA over 2018-2019. I found that the median census block group (CBG) sent about 0.53 more monthly cross-border shoppers in response to a cigarette tax increase. This magnitude is approximately a 19% increase from the before tax mean. Event studies revealed that the only lasting change in cross-border shopping happened for CBGs whose minimum distance to a lower-tax border is less than 18 miles. Using my main estimates, I also found that cross-border shopping comprised 2.5% of Kentucky's 2019 cigarette tax revenue and about 0.1% of this revenue for Oklahoma. This back of the envelope calculation showed that increases in cross-border shopping can impact revenue in states that raise their cigarette taxes.

In the second chapter, we estimated how indoor smoking restrictions and cigarette taxes impact prenatal smoking. We found some evidence that indoor smoking restrictions (ISRs) modestly decrease prenatal smoking and that cigarette taxes do not significantly impact prenatal smoking. Markowtiz et al. (2013) also finds that cigarette taxation has no effects on prenatal smoking, but our findings differ from several other studies showing effects (Evans and Ringel 1999, Colman et al. 2003, Lien and Evans 2005, Adams et al. 2012, Harris et al. 2015, Simon 2016, Gao and Baughman 2017). In our sensitivity checks (Figures D4 and D5), we reported that a $1 change in cigarette tax events is associated with a 0.25 ppt reduction in prenatal smoking, which is considerably lower than the average effect of 3.0 ppts from across these studies.

Additionally, we find evidence that ISRs cause small reductions in prenatal smoking, which differs from prior work that has not found effects (Adams et al. 2012, Markowitz et al. 2013, Gao and Baughman 2017). These finds suggest that neither cigarette regulation has a strong impact on prenatal smoking, which may have resulted because pregnant women who smoke in the 21st century have a strong, inelastic demand for cigarettes as has been reported for both adults (Callison and Kaestner 2014) and youth (Hansen et al. 2017).

Appendix A. Chapter 1 Supplemental Tables

Table A1. Differences in Smoking and Cross-Border Shopping Habits by Urban/Rural Status

Binary Outcome	Urban Mean	Rural Mean	Difference
Everyday Smoker?	0.120	0.163	-0.042***
>20 Cigs/Day | Smoker	0.440	0.516	-0.076***
Cross-Border Shop | Smoker	0.046	0.054	-0.008***

Notes: Calculations from Tobacco Use Supplement of the CPS from 2003-2019. Individual weights were used when computing the conditional averages by geographic status. All outcomes take the value of 1 if the statement in the row-header is true and 0 elsewise. "… | Smoker" indicates that the outcome before the "|" is conditioned on the respondent being a some day or every day smoker. A t-test was performed to determine the significance of the difference in means between Urban and Rural residents.

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2.Cross-Border Shoppers to Lower-Tax State by Quartiles of Distance of POI to Tax-Raising State's Border

	0.00-34.92 Miles	34.92-86.42 Miles	86.42-203.82 Miles
DD	0.690^{**}	0.244^{***}	0.091^{*}
	(0.254)	(0.054)	(0.037)
Obs	658,210	658,210	563,962
# Clusters	23	23	20

Notes: Bootstrapped standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as monthly cross-border shoppers per 100 cellphones active in a CBG. Each column splits the point of interest (POI) in a border state that received cross-border shoppers from the treated state into quartiles of distance from the tax-raising state's border. Quartile 1 then refers to the POIs that are closest to the tax-raising state's border and Quartile 3 refers to the POIs that are furthest away.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.Cross-Border Shoppers to Lower-Tax State by Border State Tax Environment

	Lower Before and After	Lower Before and After Group	Lower After	Lower After Group
DD	0.590		0.812^{+}	
	(0.680)		(0.470)	
Obs	658,210		658,210	
Kentucky DD		-0.335^{+}		1.058^{***}
		(0.191)		(0.191)
Obs		240,713		240,713
Oklahoma DD		-0.205		3.166^{***}
		(0.216)		(0.216)
Obs		192,196		192,196
Illinois DD		1.786^{***}		-0.046
		(0.151)		(0.151)
Obs		445,971		445,971

Notes: Bootstrapped standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as monthly cross-border shoppers per 100 cellphones active in a CBG. The first two columns only observe cross-border shopping to lower-tax border states that had a lower tax level than the tax-raising state before and after the latter increased its tax. The final two columns only observe cross-border shopping to lower-tax border states that had a lower tax only after the tax-raising state increased its tax. The tax-raising state being considered is listed on the title for each row.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4.Cross-Border Shoppers to Lower-Tax State by Change in Minimum Distance to Lower-Tax Border Quartile

	0.00-6.47 Miles	6.48-67.32 Miles	67.34-86.48 Miles	86.49-263.87 Miles
DD	-0.015	0.518*	-0.055	0.568*
	(0.102)	(0.208)	(0.074)	(0.250)
Obs	372,663	372,531	372,439	372,444
# Clusters	23	23	21	21

Notes: Bootstrapped standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as monthly cross-border shoppers per 100 cellphones active in a CBG. Each column splits the treatment group into a quartile of the change in minimum distance from the centroid of the CBG to a lower-tax border state. Further, this Table only considers cross-border shoppers to border states whose tax level was surpassed by the home state's cigarette tax level.

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5.Alternative Difference-in-Differences Estimators and Robustness Estimates

	CS21	TWFE	Stacked	TWMR	Placebo	Alt. Control	No Gas Stations	Cook County Excluded
DD	0.690**	0.983*	0.655*	1.098***	-0.048	1.303*	0.410^{+}	0.771*
	(0.254)	(0.430)	(0.251)	(0.197)	(0.047)	(0.543)	(0.226)	(0.322)
Obs	658,210	658,214	410,028	658,215	658,162	285,594	658,210	563,612
# Clusters	23	23	24	27,430	23	9	23	23

Notes: Standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level for all columns except for the "TWMR" column, where they are clustered by CBG. Coefficients are read as monthly cross-border shoppers per 100 cellphones active in a CBG.The column titled "CS21" refers to the main Callaway and Sant'Anna (2021) model I present in previous tables. The "TWFE" column estimates a traditional two-way fixed effects model. The "Stacked" column estimates a stacked difference-in-differences model popularized by Cengiz et. al. (2019). The "TWMR" column estimates a Two-Way Mundlak Regression popularized by Woolridge (2021).
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6.Summary of Policies for Treated and Control States

Treated State	Border States (Controls)*	$ Increase	Effective Date
Kentucky	Missouri, Virginia	0.50	July 1st, 2018
Oklahoma	Colorado, Missouri	1.00	August 23rd, 2018
Illinois	Indiana, Iowa, Missouri	1.00	July 1st, 2019

* Controls may repeat in the table above, but they are only considered once in all analyses. Controls are chosen if they are bordering at least one treated state, do not have a tax change over the sample period, and have a lower tax level than the bordered treated state both before and after the tax effective date.

Appendix B. Chapter II Supplemental Tables

Table B1.Treatment/Control Summary Statistics for Cigarette Tax Events

Variable	Control Group for Cigarette Taxes				Treatment Group for Cigarette Taxes			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Outcomes:								
Any Prenatal Smoking	0.089	0.092	0	1	0.066	0.077	0	1
Average Prenatal Cigarettes/Day	0.742	0.839	0	85.333	0.499	0.638	0	98
Time-Varying Controls:								
Real Cigarette Tax (1984$)	0.496	0.258	0.208	1.408	0.899	0.258	0.308	2.074
Index of ISRs	0.423	0.231	0.011	1	0.581	0.239	0.195	1
% Population Tobacco 21	0.000	0.001	0	0.022	0.000	0.001	0	0.007
Index of Indoor Vaping Restrictions	0.002	0.039	0	1	0.004	0.019	0	0.126
Any E-Cigarette MLSA Law	0	0	0	0	0	0	0	0
E-cigarette Tax Level (1984$)	0	0	0	0	0.003	0.036	0	0.43
Medical MJ Law	0.052	0.230	0	2	0.165	0.372	0	1
Recreational MJ Law	0	0	0	0	0	0	0	0
Beer Tax (1984$)	0.163	0.112	0.025	0.494	0.109	0.060	0.036	0.227
Unemployment Rate	6.047	2.031	2.863	13.747	6.380	1.844	3.505	11.243
Real Minimum Wage (1984$)	2.895	0.274	2.447	3.484	3.137	0.376	2.447	3.881
Demographic Controls:								
Pregnant Woman's Age	24.077	5.713	14	50	24.772	5.959	14	50
White Non-Hispanic	0.568	0.495	0	1	0.530	0.499	0	1
Black Non-Hispanic	0.178	0.383	0	1	0.114	0.318	0	1
Hispanic	0.212	0.409	0	1	0.284	0.451	0	1
Other Non-Hispanic or Unknown	0.042	0.200	0	1	0.072	0.258	0	1
Observations	18,551,185				3,021,287			

Notes: The data in this table shows summary statistics for pregnant women giving birth between 1995-2018 and belonging to one of the stacks identified in Appendix Table 2. Maternal prenatal smoking, race/ethnicity, and age are taken from the NVSS.

Table B2. Treatment/Control Summary Statistics for ISR Events

Variable	Control Group for ISRs				Treatment Group for ISRs			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Outcomes:								
Any Prenatal Smoking	0.071	0.088	0	1	0.079	0.088	0	1
Average Prenatal Cigarettes/Day	0.597	0.814	0	40	0.621	0.757	0	30
Time-Varying Controls:								
Real Cigarette Tax (1984$)	0.739	0.450	0.21	2.748	0.622	0.319	0.327	2.074
Index of ISRs	0.690	0.287	0.062	1	0.685	0.250	0.011	1
% Population Tobacco 21	0.004	0.038	0	0.506	0	0	0	0
Index of Indoor Vaping Restrictions	0.009	0.037	0	0.408	0	0	0	0
Any E-Cigarette MLSA Law	0	0	0	0	0	0	0	0
E-cigarette Tax Level (1984$)	0	0	0	0	0.000	0.003	0	0.021
Medical MJ Law	0.600	0.490	0	1	0.047	0.212	0	1
Recreational MJ Law	0	0	0	0	0	0	0	0
Beer Tax (1984$)	0.123	0.077	0.036	0.584	0.207	0.074	0.039	0.284
Unemployment Rate	6.962	2.505	2.585	12.346	6.089	2.378	3.249	11.243
Real Minimum Wage (1984$)	3.302	0.410	2.447	4.015	3.139	0.291	2.447	3.803
Demographic Controls:								
Pregnant Woman's Age	25.248	6.153	14	50	25.119	5.992	14	50
White Non-Hispanic	0.446	0.497	0	1	0.579	0.494	0	1
Black Non-Hispanic	0.099	0.298	0	1	0.176	0.381	0	1
Hispanic	0.323	0.468	0	1	0.194	0.396	0	1
Other Non-Hispanic or Unknown	0.132	0.339	0	1	0.051	0.220	0	1
Observations	20,371,950				1,156,972			

Notes: The data in this table shows summary statistics for pregnant women giving birth between 1995-2018 and belonging to one of the stacks identified in Appendix Table 3. Maternal prenatal smoking, race/ethnicity, and age are taken from the NVS

Table B3. Review of Previous Literature

Study	Data/Sample	Treatment	Years	Outcome	Effect in Primary Specification
Evans and Ringel (1999)	National Vital Statistics System (NVSS)	$1 cigarette tax increase	1989-1992	1) Any prenatal smoking 2) Cigs/day	Any Prenatal Smoking: -0.045 (t-stat = -8.95) Cigs/Day: -0.28 (t-stat = -3.64)
Colman et al. (2003)	PRAMS	$1 cigarette tax increase	1993-1999	1) Any prenatal smoking 2) Quit smoking before pregnancy 3) Quit smoking during pregnancy	Any prenatal smoking: -0.11 (z-stat = 3.08) Before Pregnancy: 0.16 (z-stat = 2.42) During Pregnancy: 0.35 (z-stat = 3.13)
Lien and Evans (2005)	NVSS for Arizona, Illinois, Massachusetts, and Michigan	Any cigarette tax increase	1991-1994	Any prenatal smoking	Arizona: -0.0092 (SE = 0.0013) Illinois: -0.0011 (SE = 0.0009) Massachusetts -0.0379 (SE = 0.0017) Michigan: -0.0114 (SE = 0.0014)
Adams et al. (2012)	PRAMS	$1 cigarette tax increase and comprehensive workplace ISR	2000-2005	1) Any pre-pregnancy smoking 2) Quit by third trimester (conditional on smoking before third trimester)	Any pre-pregnancy smoking, Tax:-0.0048 (p>0.10) Any pre-pregnancy smoking, ISR: -0.0033 (p>0.10) Quit by third trimester, Tax: 0.0484 (p<0.05) Quit by third trimester, ISR: 0.0512 (p<0.05)

Table B3. Review of Previous Literature (continued)

Markowitz et al. (2013)	Pregnancy Risk Assessment Monitoring System (PRAMS)	$1 cigarette tax increase and any restaurant ISR becoming effective	1996-2008	Any prenatal smoking during third trimester	Any prenatal smoking, Tax (Age <20): 0.0003 (z-stat = 0.02) Any prenatal smoking, ISR (Age <20): 0.034 (z-stat = 1.54) Any prenatal smoking, Tax (Age 20-24): 0.001 z-stat = (0.10) Any prenatal smoking, ISR (Age 20-24): -0.004 (z-stat = -0.37) Any prenatal smoking, Tax (Age 25-34): 0.004 (z-stat = 0.86) Any prenatal smoking, ISR (Age 25-34): -0.004 (z-stat = -0.93) Any prenatal smoking, Tax (Age >34): -0.003 (z-stat = -0.59) Any prenatal smoking, ISR (Age >34): 0.033 (z-stat = 1.74)
Bharadwaj et al. (2014)	Medical Birth Registry of Norway Data	Comprehensive ISR in hospitality industry	2003-2005	Quit smoking during pregnancy?	During Pregnancy: 0.150 (SE = 0.072)
Bartholomew and Abouk (2016)+	West Virginia Vital Statistics	Comprehensive ISR in either workplace, restaurants, or bars.	1995-2010	Any prenatal smoking	Any prenatal smoking: -0.015 (SE=0.013)
Simon (2016)	NVSS	$1 cigarette tax increase	1989-2009	Any prenatal smoking	Any prenatal smoking: -0.0092 (SE = 0.0025)

Table B3. Review of Previous Literature (continued)

Gao and Baughman (2017)	NVSS	$1 cigarette tax increase and any ISR.	1995-2009	1) Any prenatal smoking 2) Cigarettes/Day	Any prenatal smoking, ISR (Age 14-19): -0.0005 (z-stat= -0.16) Any prenatal smoking, Tax (Age 14-19): -0.0004 (z-stat=-2.30) Cigs/Day, ISR (Age 14-19): -0.1057 (z-stat=-1.24) Cigs/Day, Tax (Age 14-19): -0.0589 (z-stat=-2.84) Any prenatal smoking, ISR (Age 20-24): -0.0002 (z-stat=-0.25) Any prenatal smoking, Tax (Age 20-24): -0.0002 (z-stat=-2.15) Cigs/Day, ISR (Age 20-24): -0.1078 (z-stat=-1.95) Cigs/Day, Tax (Age 20-24): -0.0864 (z-stat=-1.11) Any prenatal smoking, ISR (Age 25-34): 0.0001 (z-stat=0.03) Any prenatal smoking, Tax (Age 25-34): 0.0006 (z-stat=0.26) Cigs/Day, ISR (Age 25-34): 0.0539 (z-stat=1.38) Cigs/Day, Tax (Age 25-34): 0.0344 (z-stat=0.83) Any prenatal smoking, ISR (Age 35-45): 0.0021 (z-stat=1.08) Any prenatal smoking, Tax (Age 35-45): 0.0040 (z-stat=1.57) Cigs/Day, ISR (Age 35-45): 0.0705 (z-stat=1.52) Cigs/Day, Tax (Age 35-45): 0.0190 (z-stat=0.54)

[+] Standard error calculation was done by hand using reported confidence interval.

Table B4. Treated States for Cigarette Tax

State	Year	Quarter	Nominal Tax Change ($)	# of Control States
Arizona	2002	4	0.60	8
Arkansas	2009	1	0.56	6
Colorado	2005	1	0.64	5
Florida	2009	3	1.00	7
Iowa	2007	2	1.00	5
Maryland	2008	1	1.00	4
Massachusetts	2002	3	0.54	9
Minnesota	2005	4	0.75	5
New Mexico	2003	3	0.70	7
Oregon	2002	4	0.60	8
Rhode Island	2009	2	0.89	7
Texas	2007	1	1.00	4
Washington	2002	1	0.60	12

Table B5. Treated States for Indoor Smoking Restriction (ISR)

State	Year	Quarter	Change in ISR Index	# of Control States
Connecticut	2003	4	0.67	10
Delaware	2002	4	0.81	14
DC	2006	2	0.42	3
Florida	2003	3	0.66	11
Iowa	2008	3	0.67	3
Maine	2004	1	0.50	9
New Hampshire	2007	4	0.50	3
North Carolina	2010	1	0.39	5
Rhode Island	2005	1	0.99	4

Table B6. TWFE and Stacked DD Model Comparison

	Any Cig (TWFE)	Any Cig (Stacked)	Cig/Day (TWFE)	Cig/Day (Stacked)
Panel A: Cigarette Taxes				
DD	-0.0030	-0.0015	0.1021*	-0.0027
	(0.0023)	(0.0020)	(0.0459)	(0.0243)
	[0.2011]	[0.4357]	[0.0307]	[0.9124]
Dep. Var. Mean^	0.100	0.086	0.940	0.703
Obs	521,718	262,427	521,565	262,413
Panel B: ISRs				
DD	-0.0060+	-0.0072	-0.0424	-0.1117+
	(0.0036)	(0.0063)	(0.0684)	(0.0548)
	[0.0997]	[0.2596]	[0.5379]	[0.0541]
Dep. Var. Mean^	0.111	0.070	0.994	0.580
Obs	521,718	183,622	521,565	183,600

Notes: The data in this table shows results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in table 1 or 2. Standard errors in parentheses and p-values are in brackets. ISR stands for "Indoor Smoking Restriction". The outcome for the first column is whether a mother smoked or not during pregnancy and for the second column it is the average number of cigarettes per day during this time. Stacked models use event-state Fes and time is controlled by national event-year-by-quarter Fes. Two-way fixed effect (TWFE) models use only state and national year-by-quarter FEs. All columns control for the variables mentioned in the summary statistics table at the time of conception.
^ For stacked models, dependent variable means are for treated states prior to the treatment for stacked models; for TWFE models, dependent variable means are for the prior to the first statewide $0.05 tax change or any ISR change.
+ $p<0.1$,* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

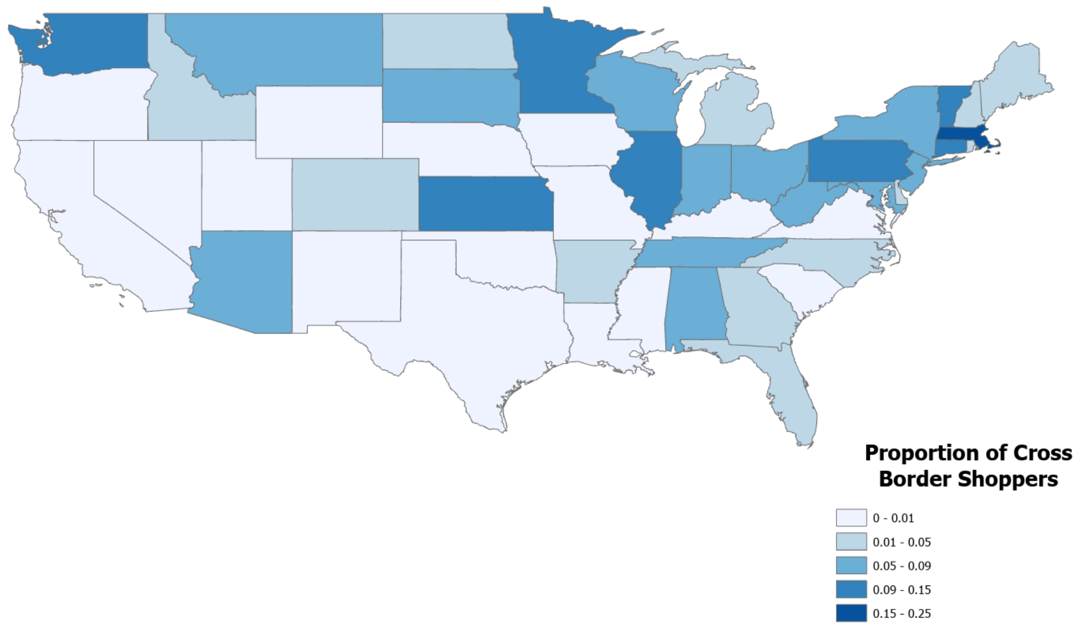
Table B7. Stacked DD Estimated for Fertility Rate

	All	>25	<=25	White	Black	Other Race
Panel A: Cigarette Taxes						
DD	-0.0001	-0.0000	-0.0003	-0.0001	-0.0001	0.0007
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0012)
	[0.1514]	[0.8511]	[0.1256]	[0.3815]	[0.2389]	[0.5484]
Dep. Var. Mean	0.005	0.003	0.010	0.004	0.005	0.042
Obs	2,274	2,274	2,274	2,274	2,269	2,269
Panel B: ISRs						
DD	-0.0001	-0.0001	-0.0001	-0.0001	-0.0002	-0.0027**
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0009)
	[0.6320]	[0.2209]	[0.7084]	[0.2597]	[0.2307]	[0.0067]
Dep. Var. Mean	0.005	0.003	0.008	0.003	0.004	0.017
Obs	1,375	1,375	1,373	1,375	1,369	1,371

Notes: The outcomes in these figures show results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in Table 1 or 2. “ISR” refers to an Indoor Smoking Restriction. The outcome in this table is the number of conceptions per fertile aged woman, which refers to the number of conceptions in a state-quarter divided by the number of women residing in that state between the age of 15 and 49 in the corresponding year. All models use state FEs and time is controlled by national event-year-by-quarter FEs. All models also control for the variables mentioned in the summary statistics table.

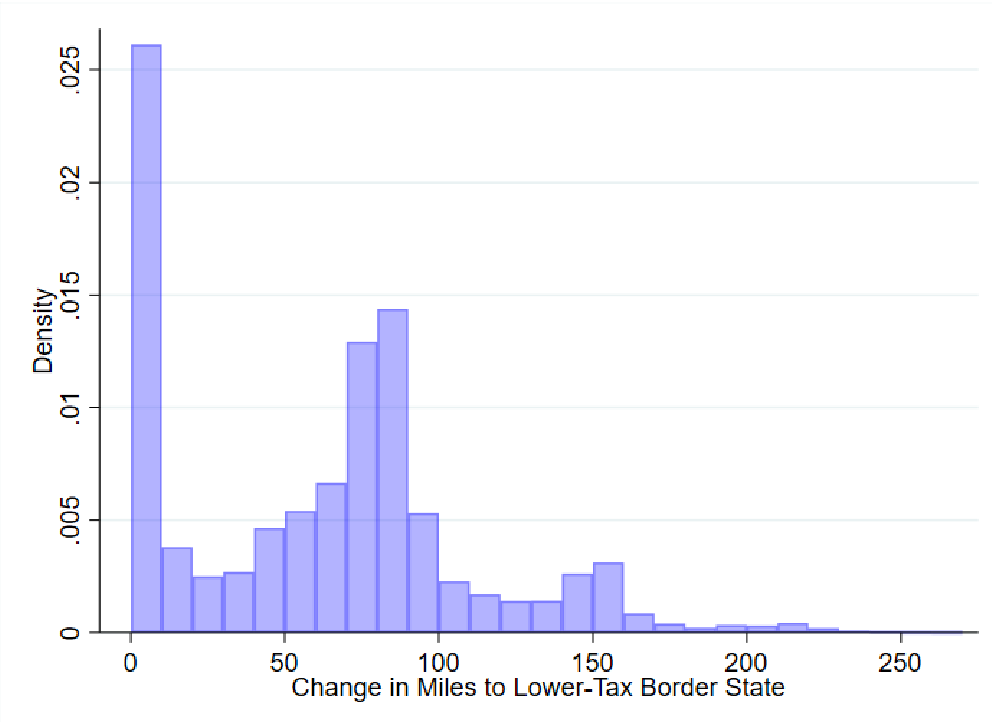
Appendix C. Chapter I Supplemental Figures

Figure C1. Proportion of Cross-Border Shoppers by State in July 2018



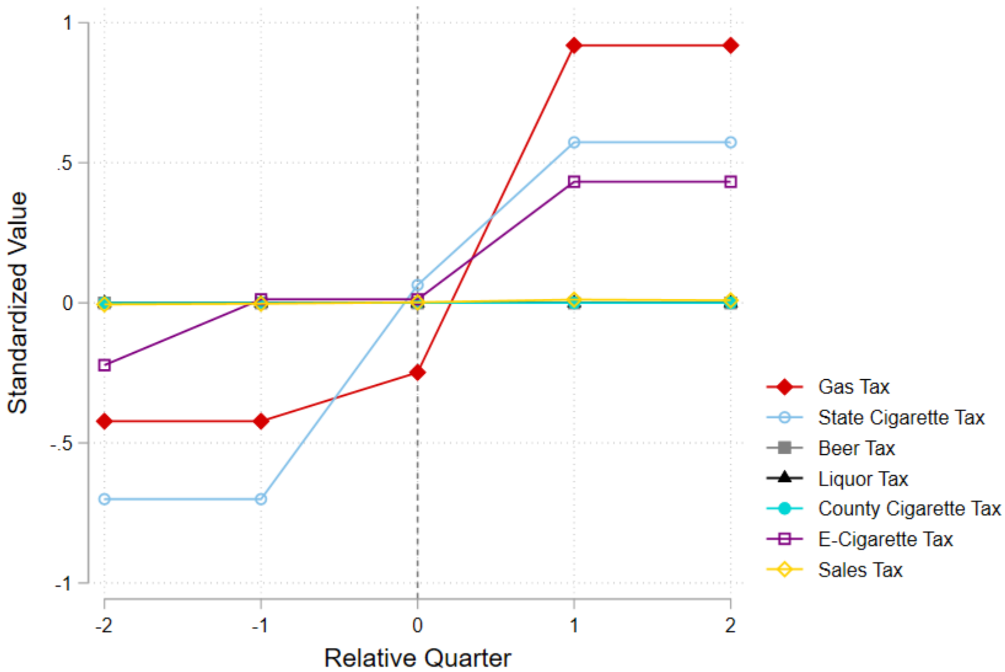
Notes: This Figure uses data from the Tobacco Use Supplement of the Current Population Survey in July 2018. The proportion of cross-border shoppers is calculated for each state by taking the number of individuals who reported purchasing their last pack of cigarettes in a state they do not reside in and then dividing by the number of every and some day smokers in the state.

Figure C2. Distribution of Changes in Distance to Lower-Tax Border State for Each Census Block Group



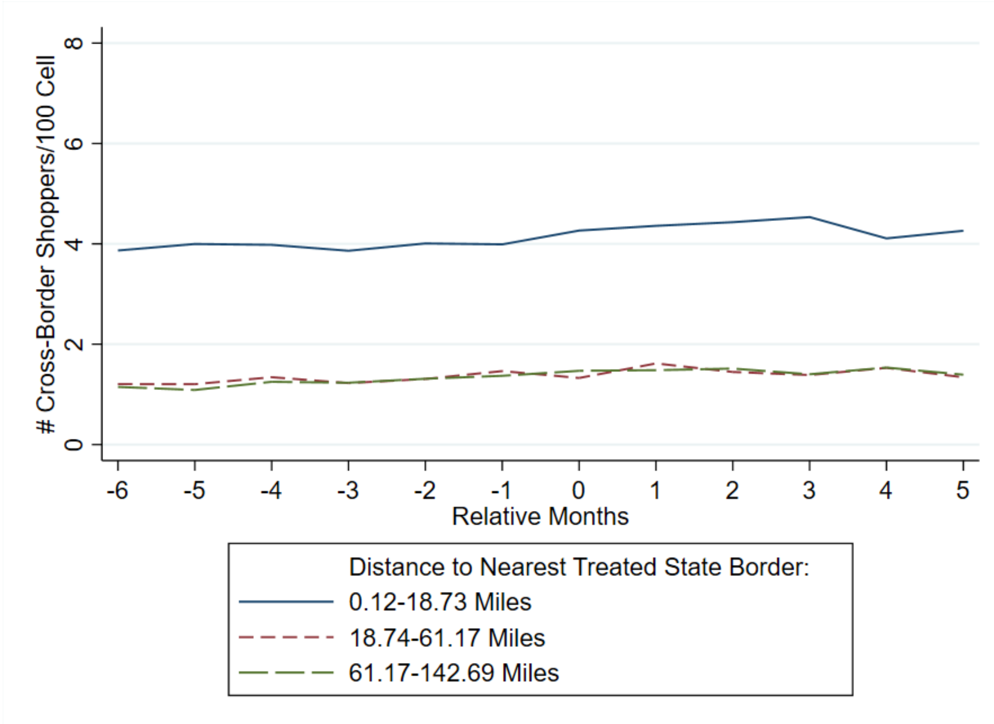
Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Change in distance to a lower-tax border state is calculated by taking the minimum distance from a centroid of each census block group to a lower-tax border state before the treated state increased its cigarette tax and subtracting from this the minimum distance to a lower-tax border state after the tax increase.

Figure C3. Change in Nominal Levels of State Cigarette Taxes and Other Related Tax Policies to Cross-Border Shopping



Notes: This graph shows the change in the standardized value of state cigarette taxes and other related tax policies that could cause a change in cross-border shopping to the retailers I've chosen. The relative quarter "0" is the quarter in which the state cigarette tax became effective for the three treated states I'm considering. The "County Cigarette Tax" also includes incorporated place-level cigarette taxes weighted up to the county level by their proportion of the county population. "E-Cigarette Tax" includes incorporated places and county-level taxes weighted up to the state level, as well as state level e-cigarette taxes.

Figure C4. Relative Months to Cigarette Tax Increase using Raw Data for Control States

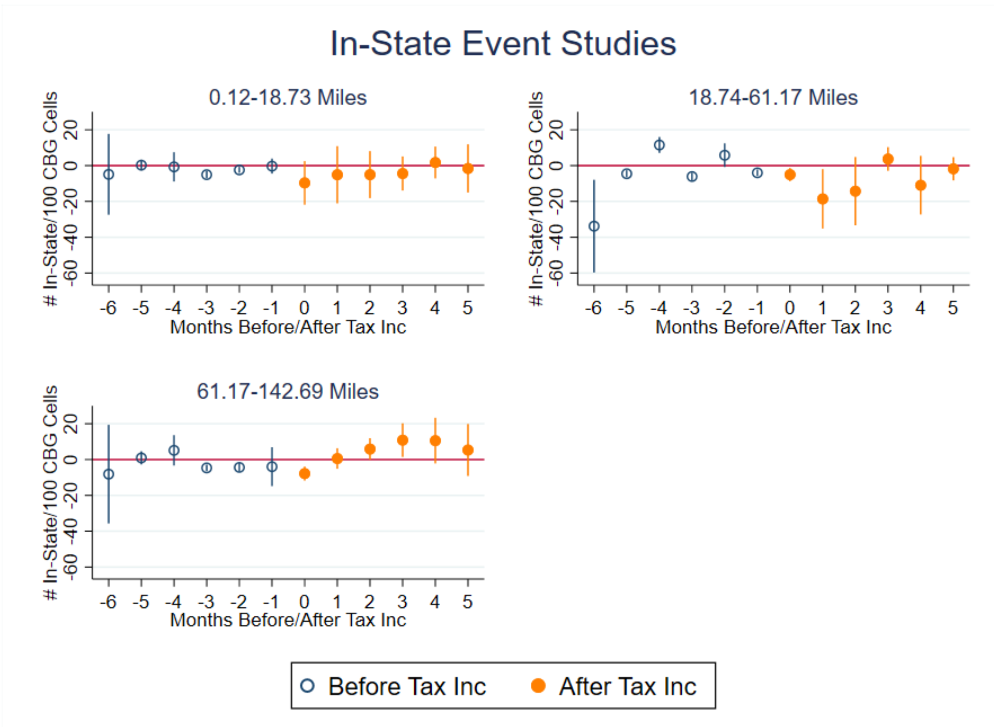


Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Each point on the graph is calculated by taking the average of cross-border shoppers per 100 devices in all the census block groups (CBGs) for each control state. The relative month of "0" is when the cigarette tax became effective in each state. Each division of distance from a lower-tax border is made by considering quartiles of distance from a lower-tax border as described in the methods section. As the last quartile does not contain any treated units, I dropped it from consideration.

Figure C5. Event Studies For Cross-Border Shoppers to Lower-Tax State and In-State Shoppers

Notes: Bootstrapped confidence intervals are given as spikes on the point estimates presented as dots. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as per 100 cellphones active in a CBG. The outcome concerns monthly shoppers. “Cross-Border” or “In-State” outcomes are indicated in separate Figure titles.

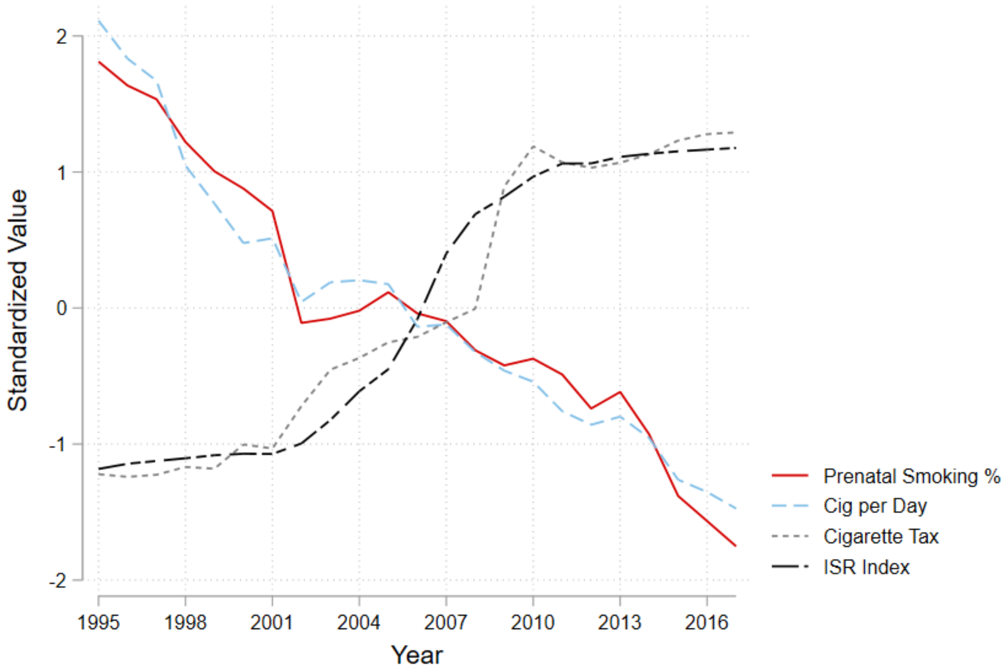
Figure C6. Event studies For In-State Shoppers by Minimum Distance to Lower-Tax Border Quartile



Notes: Bootstrapped confidence intervals are given as spikes on the point estimates presented as dots. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as per 100 cellphones active in a CBG. The outcome concerns monthly “in-state” shoppers.

Appendix D. Chapter II Supplemental Figures

Figure D1. Standardized Outcomes and Policy Variables Time Series



Notes: The data in this figure is for pregnant women giving their first birth between 1995-2017. ISR stands for “Indoor Smoking Restriction”. “Prenatal Smoking %” is the proportion of pregnant women who smoked each year. “Cig per Day” is the average of the average number of cigarettes smoked per day over the course of a pregnancy unconditional on smoking status. “Cigarette Tax” is a real value measured in 1984 dollars. “ISR Index” is an index of ISR coverage over bars, restaurants, and workplaces as described above in the “Data” section.

Figure D2. Heterogenous Effects for Cigarette Tax Increase

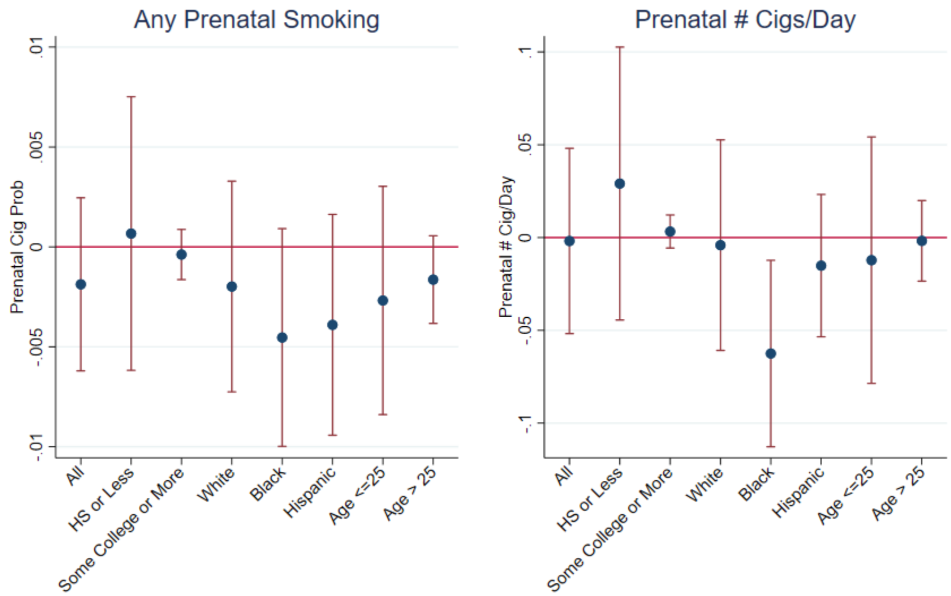
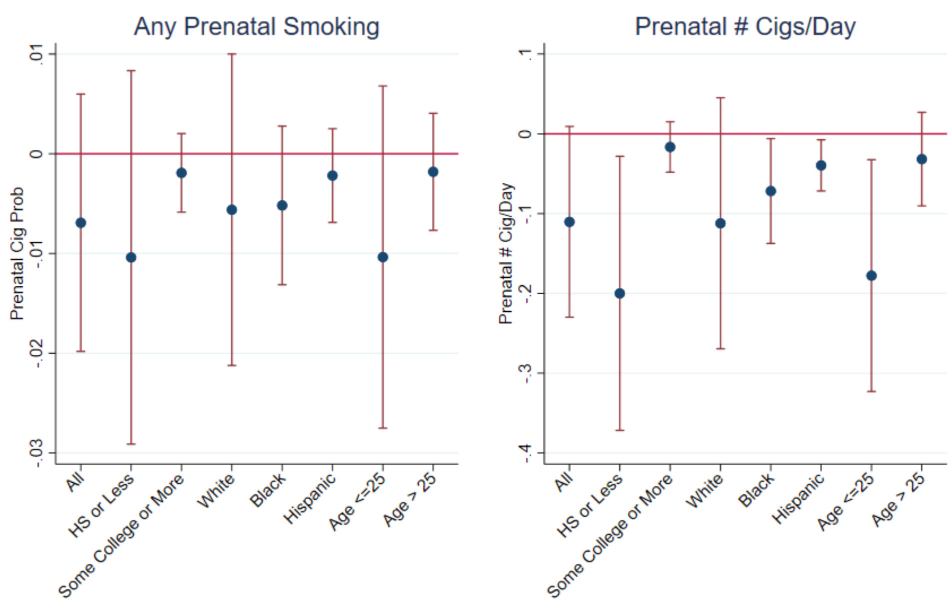


Figure D3. Heterogenous Effects for Indoor Smoking Restrictions



Notes: The outcomes in these figures show results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in Table 1 or 2. "Any Prenatal Smoking" refers to whether a pregnant woman smoked or not during pregnancy and "Prenatal # Cig/Day" is the unconditional average number of cigarettes per day during pregnancy. All models use stack-state FEs and time is controlled by stack-year-quarter FEs. All models also control for the variables mentioned in the summary statistics table.

Figure D4. Sensitivity Analysis for Cigarette Taxes

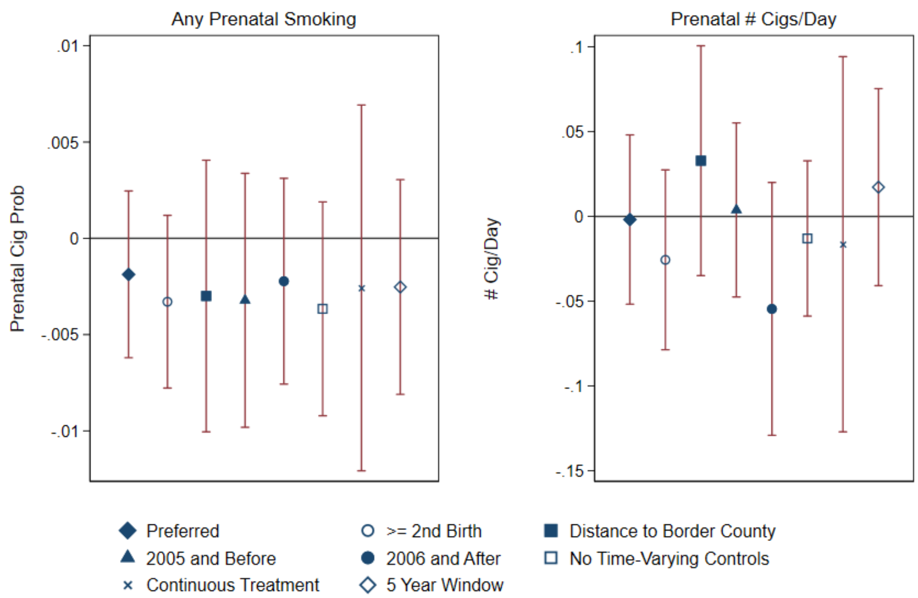
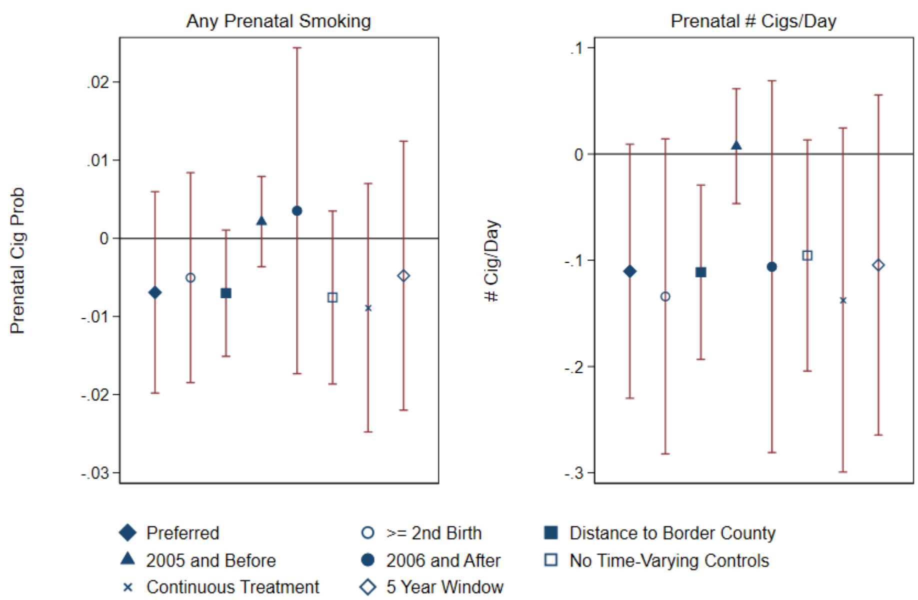
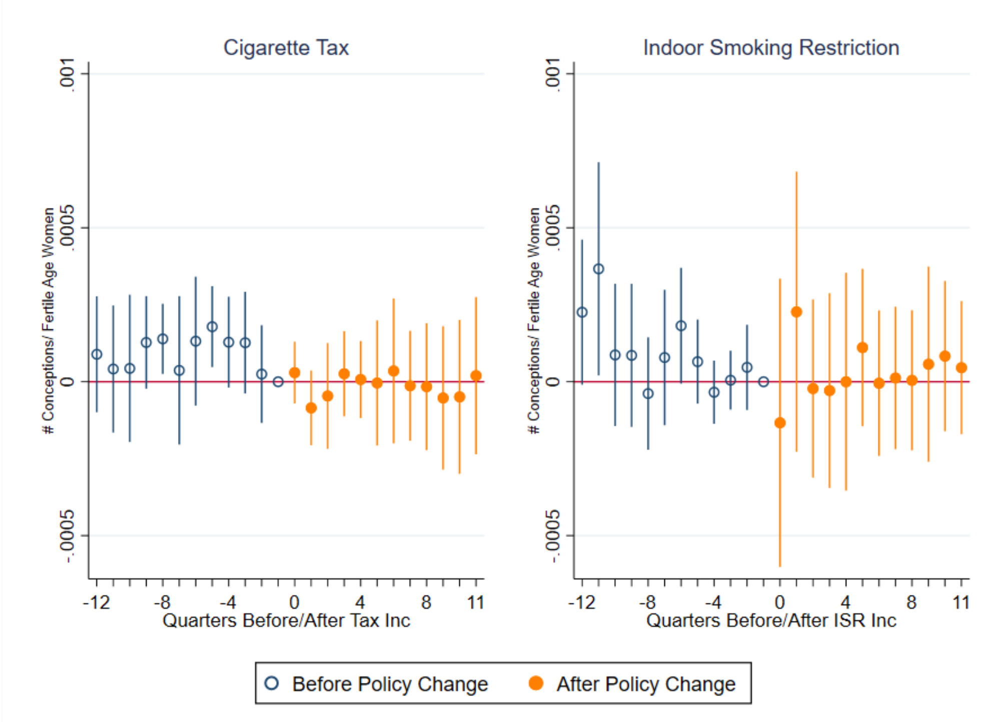


Figure D5. Sensitivity Analysis for Indoor Smoking Restrictions



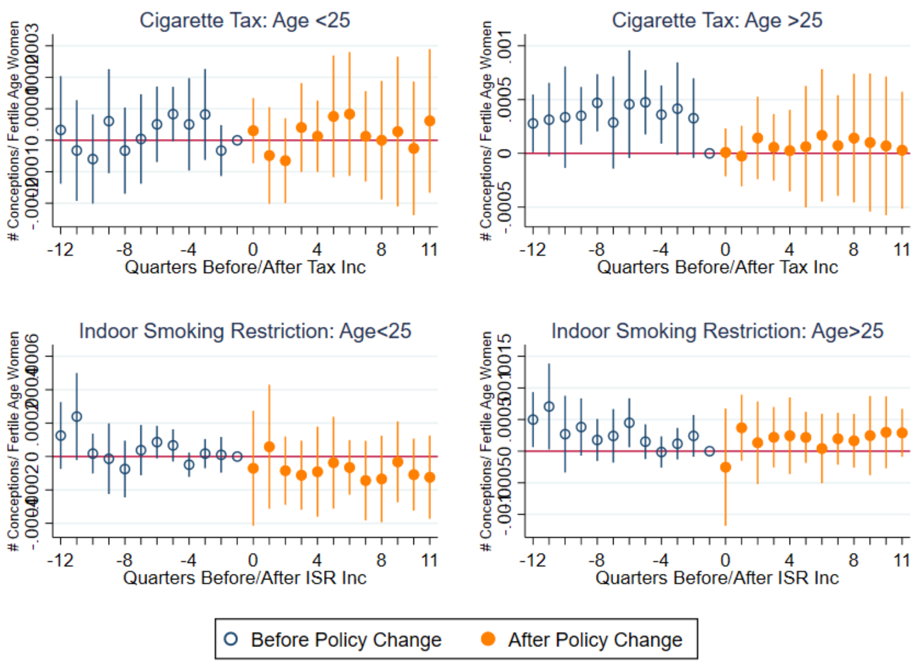
Notes: The outcomes in these figures show results for pregnant women giving their first birth or second and higher birth between 1995-2018 and belonging to one of the stacks identified in Table 1 or 2. "ISR" refers to an Indoor Smoking Restriction. "Any Prenatal Smoking" refers to whether a pregnant woman smoked or not during pregnancy and "Prenatal # Cig/Day" is the unconditional average number of cigarettes per day during pregnancy. All models use stack-state FEs and time is controlled by stack-year-quarter FEs. All models also control for the variables mentioned in the summary statistics table

Figure D6.Fertility Composition Event Studies for All Births



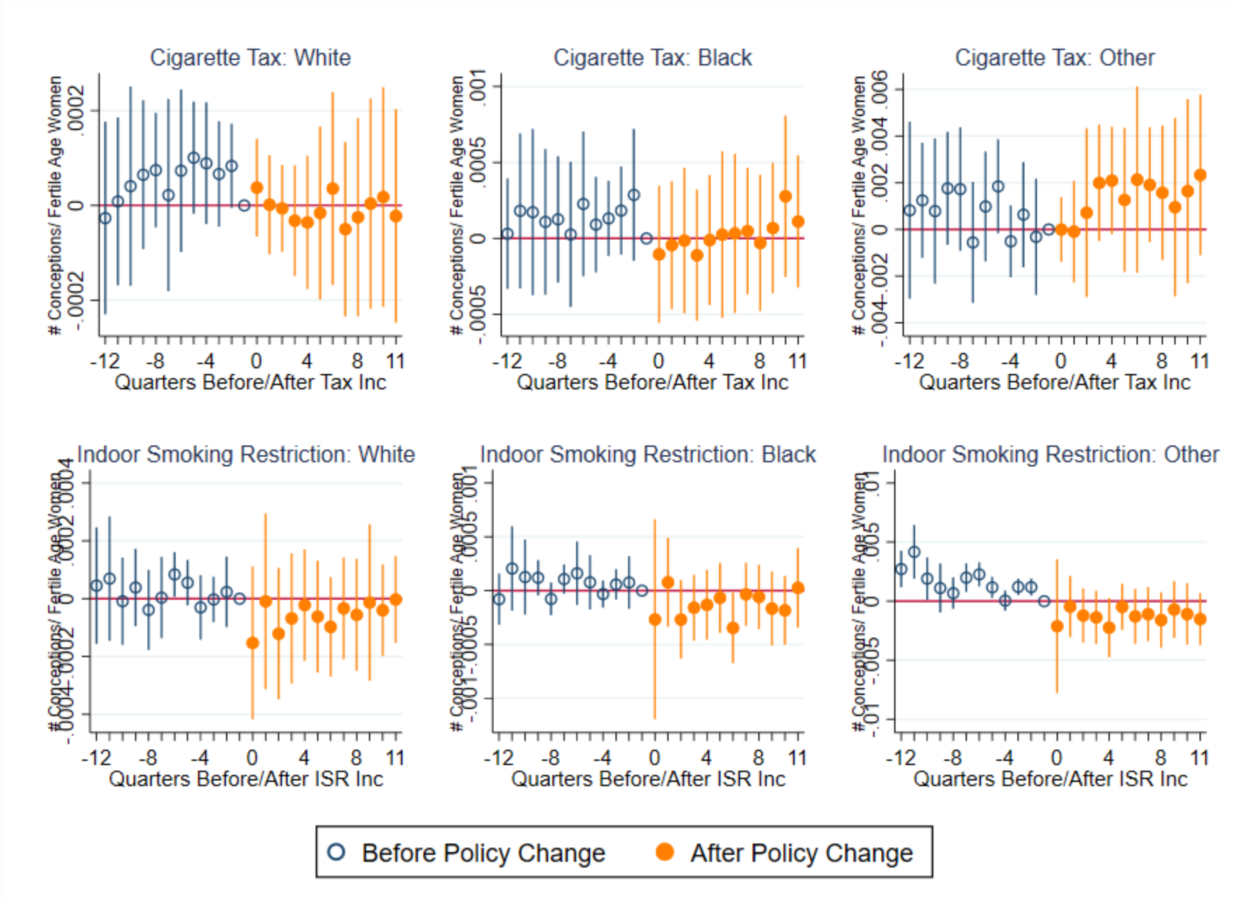
Notes: The outcomes in these figures show results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in Table 1 or 2. "ISR" refers to an Indoor Smoking Restriction. "# of Conceptions/Fertile Age Woman" refers to the number of conceptions in a state-quarter divided by the number of women residing in that state between the age of 15 and 49 in the corresponding year. All models use state FEs and time is controlled by national event-year-by-quarter FEs. All models also control for the variables mentioned in the summary statistics table.

Figure D7. Fertility Composition Event Studies by Age



Notes: The outcomes in these figures show results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in Table 1 or 2. "ISR" refers to an Indoor Smoking Restriction. "# of Conceptions/Fertile Age Woman" refers to the number of conceptions in a state-quarter divided by the number of women residing in that state between the age of 15 and 49 in the corresponding year. All models use state FEs and time is controlled by national event-year-by-quarter FEs. All models also control for the variables mentioned in the summary statistics table.

Figure D8. Fertility Composition Event Studies by Race



Notes: The outcomes in these figures show results for pregnant women giving their first birth between 1995-2018 and belonging to one of the stacks identified in Table 1 or 2. "ISR" refers to an Indoor Smoking Restriction. "# of Conceptions/Fertile Age Woman" refers to the number of conceptions in a state-quarter divided by the number of women residing in that state between the age of 15 and 49 in the corresponding year. All models use state FEs and time is controlled by national event-year-by-quarter FEs. All models also control for the variables mentioned in the summary statistics table.

Appendix E. SafeGraph Data Appendix for Chapter I

The SafeGraph dataset records foot traffic using Android and/or IOS device GPS tracing for over 40 million devices in the USA alone and over 3.6 million points of interest (POI) in both the USA and many other countries. A POI is potentially any location that can be visited besides a private residence. Foot traffic is recorded for a POI if a traced device enters the polygon boundary of the POI for at least 4 minutes. If POIs are close to each other (such as in a mall), they may share the same polygon boundary, implying that a trip within this shared boundary will count for both POIs. Parking lots are included in the polygon boundary for some but not all POIs. The dataset has a variable that details whether a POI has a parking lot included in its polygon boundary or not.

As stated above, the SafeGraph data records foot traffic by using GPS tracing of its included devices. These devices are traced by SafeGraph through partnering with Android and/or IOS application firms. However, there is currently no information on which application firms partner with SafeGraph to provide GPS tracing. Further, the dataset provides almost no demographic information on the owner of any device that it tracks. However, SafeGraph does use an algorithm to determine the home census block group (CBG) of most devices. The main idea of the algorithm is to assign a home CBG based on where the device is at night (between 6 PM and 7 AM) for many days over a six-week period. While a panel of devices is traced over time, devices in the panel drop in and out, implying the panel is unbalanced. Further, more devices are being traced by SafeGraph over time as the dataset grows larger. To account for this, SafeGraph provides the number of devices being traced in the dataset per CBG-week or CBG-month, depending on which level the statistic is needed.

When downloading data from SafeGraph, it can either come at the POI-week or POI-month level. For example, if you downloaded 6 months of monthly data for a single brand (say Starbucks) for the entire USA, each USA-based Starbucks location in the SafeGraph dataset would have six rows of data. The SafeGraph dataset further includes many variables for each POI per period of time (week or month), including the number of visitors and visits to the POI; total number of visitors who entered the POI during mutually exclusive intervals of time throughout the day; the POI's name, address, industry, longitude and latitude coordinates, city, postal code, and state (for the USA); related same day brands for visitors to that POI; and many more. Importantly for the current research, the data also gives how many visitors from each CBG came to that POI per period of time.

For tobacco researchers in particular, the researcher may be interested in the change in foot traffic over time within a tobacco retailer, or the change in the number of visitors to tobacco retailers by devices from a certain geographic region. Identification of tobacco retailers is possible in the SafeGraph data using the POI's name and/or industry. If the researcher is more interested in how foot traffic changed within tobacco retailers after the introduction of some policy, SafeGraph data in the form that it is downloaded (POI-week/month level) will work. This would allow the estimation of the change in foot traffic within each tobacco retailer over time. However, as was the case in the current paper, tobacco researchers may be more interested in the change of the number of visitors residing in a CBG (or other geographical unit) to tobacco retailers over time. Using the current paper as an example, I was interested in measuring the change in the number of visitors residing in a CBG to cross-state border tobacco retailers after a cigarette tax hike. While the SafeGraph dataset does not immediately lend itself to this kind of within CBG analysis, it can be reshaped to do so. This is possible as the dataset has a variable that details the number of visitors from a CBG to the POI per period of time. After downloading the data, the researcher would need to (1) split this variable so that in each period of time for a POI, the number of visitors to the POI from a CBG has its own row in the data and then (2) sum the number of visitors from each CBG in each period of time. The final result will be a panel data set on the CBG-week/month level that allows for within CBG estimation.

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Vita

Maxwell (Max) Chomas graduated from Grove City Area High School in May, 2011. After this time, Max went on to attend Washington & Jefferson College (W&J) where he majored in economics and mathematics. During his time at W&J, Max was a co-author on a paper later published in *Energy Policy* entitled "A Quantitative Description of State-Level Taxation of Oil and Gas Production in the Continental U.S" with Dr. Yongsheng Wang and Dr. Jeremy Weber. He graduated Magna Cum Laude from W&J in May, 2015. After this, Max received his M.A. in economics at University of California, Riverside, where he met and began dating his future wife. In May 2023, Max finally accomplished his dream of completing a Ph.D. in economics at Georgia State University. The topic of his dissertation, advised by Dr. Michael Pesko, was studying the impact of cigarette regulation on two main outcomes: prenatal smoking and cross-border shopping.