

# **Measuring the Change in Cross-Border Shopping in Response to a Cigarette Tax: An Application Using Cellphone Tracking Data**

Maxwell Chomas<sup>1</sup>

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## **Abstract**

I study the effect of cigarette taxes on cross-state border shopping in the United States. To estimate this relationship, I use high-resolution census block group-by-month cellphone tracking data from Safegraph. I estimate 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. I find that, the median census block group (CBG) sent 0.83 more cross-border shoppers per month in response to a cigarette tax increase (48% of the mean). I also estimate that the increase in cross-border shopping is substantially larger for those that live closer to a lower-tax border. Finally, I find evidence that census block groups with more lower-educated adults send considerably more cross-border shoppers following tax increases. These results suggest that cross-border shopping remains an ongoing challenge for tobacco control policy efforts and for reducing tobacco-related education disparities.

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<sup>1</sup> Graduate Student, Department of Economics, Andrew Young School of Policy Studies, Georgia State University, Atlanta, mchomas1@gsu.edu.

## Introduction

Tobacco use leads to over 7 million deaths a year worldwide (World Health Organization 2017). Cigarettes are the most commonly used form of tobacco in the United States, with 12.5% of the adult population being users in 2020 (Cornelius et. al. 2022). Governments then have a strong incentive to reduce cigarette use to reduce public healthcare expenditures and improve public health. For example, in 2010, an estimated 15% of all Medicaid expenditures were attributed to cigarette smoking (Xu et. al. 2015). In response to the health and fiscal concerns engendered by cigarettes, many states have attempted to reduce cigarette use through taxation. In 2019 alone, state governments raised a combined \$17 billion in cigarette tax revenue.<sup>2</sup>

The health and fiscal benefits of cigarette taxation are contingent on individuals being unable to avoid paying higher cigarette taxes except by quitting or reducing cigarette consumption. Literature shows though that individuals use a variety of strategies to avoid taxes, including cross-state border shopping (Harding et. al. 2012, DeCicca et. al. 2013) and many forms of organized smuggling (Joossens and Raw 2012). For example, in 2019, it was estimated that 52.2% of cigarettes consumed in New York state did not collect tax revenue for the state.<sup>3</sup>

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<sup>2</sup> This figure is my personal calculation from “The Tax Burden on Tobacco, 1970-2019”, which is a publicly available dataset which can be found here: <https://chronicdata.cdc.gov/Policy/The-Tax-Burden-on-Tobacco-1970-2019/7nwe-3aj9>

<sup>3</sup> <https://taxfoundation.org/state-tobacco-tax-cigarette-smuggling/>

Little is known, however, how much cigarette users from both urban and rural areas engage in cross-border shopping across state lines in response to an increase of a cigarette tax. In figure 1, I show that there is strong heterogeneity in how many smokers cross-border shop by state in 2018 using the Tobacco Use Supplement of the Current Population Survey (TUS-CPS). In total, about 5% of smokers cross-border shopped for the most recent pack of cigarettes purchased before they were surveyed for the TUS-CPS in 2018.

Policymakers, however, may be more interested in how much cross-border shopping *changes* due to a cigarette tax increase. This may be true because if a large portion of individuals are cross-border shopping in response to a cigarette tax, this means they are not reducing cigarette consumption. This implies that the state government would not only be losing out on cigarette tax revenue, but also not reducing state medical expenditures. Further, cross-border shopping for cigarettes is done by many uncoordinated actors. If a large portion of the decline in sales is attributable to cross-border shopping, there is little any state could do unless it attempted to coordinate tax levels with its lower-tax border states.

Previous papers have had to focus on cross-border shopping from urban areas because of data limitations (Harding et. al. 2012, DeCicca et. al. 2013). I argue below that this will cause an undercount of cross-border shopping as rural residents are more likely to smoke and, conditional on smoking, more likely to be heavy smokers and cross-border shop. I will further argue that the inclusion of a time-varying, minimum distance to a lower tax border control in previous papers (Harding et. al. 2012, DeCicca et. al. 2013) introduced a downward-over-control bias (Cinelli et. al. 2022) for coefficients involving the home state cigarette tax level. The current paper will fill this gap in the literature by estimating how much cross-border shopping changes in response to a cigarette tax for both metropolitan *and* rural areas using a sample that covers nearly every census block group in the USA. I will further estimate conditional average treatment effects by and only control for static values of minimum distance to a lower tax border as

opposed to controlling for a time-varying version of this variable to avoid over-control bias. Finally, I will focus on education-based time expenditure inequalities via cross-border shopping engendered by cigarette taxation.

Focusing on the totality of cross-border shopping behavior for cigarettes is crucial as rural cigarette smokers are demographically different from their urban counterparts. I calculate differences in cigarette usage and cross-border shopping between urban and rural residents using the TUS-CPS from 2003-2019 in table 1. Here I find that rural residents are more likely to be an everyday smoker by 4 percentage points, more likely to smoke heavily (20 cigarettes or more a day) by 7 percentage points, and are more likely to cross-border shop by about 1 percentage point than urban residents. The results pertaining to smoking are in line with Darden (2021), who argues that the urban/rural disparity in smoking behavior is driven by low-skill workers choosing to remain in or migrate to rural areas.

I also concentrate on education-based time expenditure inequalities created by cigarette taxation. 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 population to be low-income and low-educated, suggesting a fiscal expenditure inequality. If certain low-educated individuals purchase most cigarettes and have a low income, they may be even more incentivized to cross-border shop in the face of a cigarette tax than their counterparts. These individuals may then not only face a fiscal expenditure inequality, but also a time expenditure inequality as they are more likely to spend the time to cross the border and purchase cigarettes when their state increases its cigarette tax.

Measuring the extent that cross-border shopping changes in response to a cigarette tax increase is challenging as many datasets do not ask about the location of cigarette purchases, do not inquire how often a cigarette user cross-border shopped, or are not specific about the smoker's location. To mitigate these issues, I use cell-phone tracking data provided by Safegraph to measure cross-border shopping. The

strengths of this data are many and allow me to address the estimation problems mentioned above. First, the data allows me to observe how many unique visitors<sup>4</sup> (from here on, just “visitors”) a point of interest received each month, along with the home census block group (CBG) of most visitors. I 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, 6-digit NAICS industry code along with a store name. Using this information, I construct a set of potential cigarette retailers and exclude stores that are in a cigarette retailer industry but do not sell cigarettes. 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.<sup>5</sup>

The weaknesses of the data are few but important. First, I cannot observe what the visitors purchased in the cigarette retailer. Secondly, I cannot follow an individual visitor over time. This means I do not know if a visitor is a cigarette user, nor do I know anything else about the visitor besides their home CBG. I attempt to combat this problem by constructing a monthly 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. As CBGs have a low population, I further use CBG characteristics as control variables and as an additional form of variation.

To estimate much cross-border shopping changes in response to a cigarette tax, I use a difference-in-differences model popularized by Callaway and Sant’Anna (2021) (CS21). This model is important to

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<sup>4</sup> A unique visitor is recorded when a traced cellphone enters a point of interest for the first time. For example, if a traced cellphone enters 3 different points of interest 1 time each over a month, the cellphone would be recorded as 1 unique visitor in each point of interest. On the other hand, if a traced cellphone enters the same point of interest 3 times in a month, the cellphone would only be counted as 1 unique visitor for that point of interest.

<sup>5</sup> 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.

use in my setting as the tax increases in my data become effective at distinct times. This kind of staggered policy implementation environment has been shown to cause estimation bias for two-way fixed effects models (Gibbons et. al. 2019, Goodman-Bacon 2021) and CS21 has been shown to alleviate this bias. This econometric model further allows me to add to the literature by exploring the parallel trends assumption along with dynamic effects after the tax has become effective.

I find strong evidence for an increase in cross-border shoppers in response to cigarette taxes. My main specification focuses on cross-border shopping to lower tax states from the state that raised its cigarette tax. This is an important distinction as cross-border shoppers should frequent these areas. I find that, for the CBG with the median amount of cellphones being traced, about 0.83 more monthly visitors from this CBG cross-border shopped in response to a cigarette tax increase. This magnitude is approximately a 50% increase from the before tax mean.

The result that the number of cross-border shoppers increases in response to a cigarette tax increase is robust to multiple specifications. I first show that those CBGs closer to a lower tax border send more cross-border shoppers in response to a cigarette tax than those farther away. Further, I show that there was a sharp decline in in-state shoppers for cigarette retailers near the border. I also construct two different partitions of my data based on adult educational attainment and whether a CBG is rural or not. I find that CBGs with many low-educated adults send substantially more cross-border shoppers than those with few low-educated adults. This result highlights that cigarette taxes increase the need for these individuals to cross-border shop. There is then an inequality between low and high educated adults not only in fiscal expenditures (Conlon et. al. 2021) but also in time expenditures. I further find that rural CBGs send substantially more cross-border shoppers than urban CBGs, which suggests that previous estimates that only used urban residents were undercounts. Finally, I find that CBGs that had a large change in the minimum distance to a lower-tax border sent significantly more cross-border shoppers than those that did

not have a large change. This result confirms that the decrease in minimum distance to a lower-tax border is an important mechanism through which cross-border shopping increases in response to a cigarette tax increase. It also implies that previous papers estimates may have suffered from an over-control bias when controlling for a time-varying minimum distance to a lower-tax border covariate.

## 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 their home state or over the 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 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.<sup>6</sup> 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:

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

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 if 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

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<sup>6</sup> This function is weakly decreasing in the home state's tax if  $i$  does not move from their CBG.

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 1 if the inequality in (1) holds and is 0 otherwise.

Now consider an increase  $\Delta$  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:

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

This change in the upper bound has two 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 terciles of distance 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. This confers the importance of considering rural areas as individuals in these areas have a higher probability of being heavy smokers and being a smoker. 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 I do not know who in my sample is a smoker, I test this assumption by dividing my sample between CBGs with a high portion of low-educated adults or is a rural CBG. I discuss in the methods section below that both groups are more likely to smoke and smoke more heavily. Comparing these groups to their counterparts will then allow me to assess whether more people cross-border shopped from areas with more smokers in general and heavy smokers conditional on smoking.

Further, note that:

$$D_i(t_0) - D_i(t_1) \geq 0 \tag{2}$$



, 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 2, conditional on the fact that a resident of a CBG did not migrate after the home tax was raised to a different CBG. 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 (2) 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 those in quartile four, the largest change in this distance, should have the largest increase 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 2. 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). This over-control bias should have been in the downward direction in previous papers as an increase in the home state's tax level decreases the minimum distance to a lower-tax border.

## Literature Review

Many papers have measured either the totality of cigarette smuggling<sup>7</sup> (Warner 1982, Baltagi and Levin 1986, Baltagi and Gold 1987, Saba et. al. 1995, Thursby and Thursby 2000, Ben Lakhdar et. al. 2016) or commercial cigarette smuggling (Yurekli and Zhang 2000) using state level cigarette sales data. These papers tend to find a strong presence of cigarette smuggling. 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, as mentioned in the introduction, there are many forms of cigarette smuggling. A decline in taxed cigarette sales in a home state due to a lower tax border state may occur due to a mixture of arbitrage and cross-border shopping. As policy responses differ based on the relative magnitude of both, providing separate measures for both arbitrage and cross-border shopping will be important. Further, arbitrage is potentially lucrative due to large discrepancies in state tax levels, which has caused a rise in “buttlegging” of cigarettes across state lines for profit.<sup>8</sup>

Stehr (2005) improves on previous papers by using both state-level sales and individual consumption data of cigarettes to estimate the magnitude of cross-border smuggling. To do this, he estimates how much the difference between log sales and log consumption, where consumption is estimated using the Behavioral Risk Factor Surveillance System, in the home state changes in response to border states having *higher* cigarettes taxes. This implies that Stehr is estimating how many cigarettes are exported from the home state. As the difference between sales and consumption changes in the home state should equal the amount of cigarette smuggling, Stehr estimates that this difference increases by .0322

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<sup>7</sup> This includes both commercial smuggling and cross-border shopping.

<sup>8</sup> One example of this comes from reselling cigarettes purchased in North Carolina to New York:

<https://www.washingtonpost.com/archive/local/1980/06/29/buttlegging/9419d999-059b-418b-b96e-a15f9953d8ee/>

log points in response to a 1 unit increase in his export incentive variable. However, Stehr still cannot differentiate between how much of this export behavior is due to arbitrage or cross-border shopping. Further, Stehr's estimate that 0.7% of total sales in 2001 were due to exporting is low considering more recent papers.

A similar paper by Lovenheim (2008) estimates how much of sales in a state are due to cross-border smuggling. To calculate this, he first estimates different price elasticities of cigarette demand depending on distance to a states border using the Tobacco Use Supplement of the Current Population Survey (TUS-CPS). He then uses these elasticities to estimate how much larger cigarette consumption is for those living near a lower tax border. Finally, this increase in consumption for those living near a low tax border is used to calculate that the percentage of sale due to cross-border smuggling is between 13 to 25 percent. However, Lovenheim's paper suffers from a similar issue in Stehr (2005) and other papers as he cannot differentiate between arbitrage or cross-border shopping in his calculation. Further, like all papers that use the TUS-CPS in this literature, Lovenheim must rely on urban smokers only so that he can calculate distance to a lower tax state's border.

Another paper using the TUS-CPS is DeCicca et. al. (2013), who measures the increase in the probability of cross-border shopping for smokers in states with higher cigarette taxes. As opposed to using the differences in price elasticities as in Lovenheim (2008) to tease out cross-border smuggling behavior, these authors use a question in the TUS-CPS which directly asks about cross-border shopping. The paper finds that a 1 dollar 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. One potential drawback of this paper is that the question inquiring about cross-border shopping only asks about the last pack of cigarettes purchased. This may then lead to an undercount of cross-border shopping behavior as a smoker

may mix cross-border shopping with in-state shopping and so report no cross-border shopping when they do cross-border shop quite frequently. Further, DeCicca et. al. (2013) must also only use urban smokers so that they can calculate distance to a lower-tax state's border.

A slew of other recent papers have taken the approach of estimating cross-border smuggling using littered cigarette packs (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.<sup>9</sup> This paper suffers from similar issues mentioned above in that noncompliance cigarette packs may either be the result of arbitrage or cross-border shopping. Furthermore, smokers who litter may not be representative of other smokers, which could cause bias in their estimates.

Finally, Harding et. al. (2012) uses Neilson Homescan data to estimate the increase in cross-border cigarette shopping due to higher cigarette taxes. The Neilson Homescan data is useful in this case as it records all purchases that the user scanned with the device given to them by Neilson. The authors find that a 1 cent increase in cigarette taxes increases cross-border shopping probability by 5.36%. This effect is lessened by 1.32% for each percentage increase in distance from the home state's border. However, as the authors point out, most cigarettes are purchased and then used immediately, so the user may not think to record the purchase. Further, like many papers before, the authors must only use urban residents as the Neilson Homescan data does not cover rural residents.

The current paper extends this literature in many ways. First, like Harding et. al. (2012) and DeCicca et. al. (2013), I give a direct estimate of cross-border shopping as opposed to the combined

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<sup>9</sup> A "noncompliance cigarette pack" is a pack that does not have the community's home state tax stamp on it.

estimate of cross-border shopping and arbitrage. I can make this distinction for two reasons. The first is that individuals engaging in arbitrage are unlikely to use a phone that can be traced when conducting illegal activity. Secondly, even if their phone is traceable, arbitrage should be concentrated among a few individuals, meaning that most of the cross-border shopping behavior I pick up will be for personal use.

Unlike these papers, my data does not rely on self-reporting. Further, my data is collected each month, which avoids underreporting issues described above for DeCicca et. al. (2013). In addition, I estimate conditional average treatment effects by terciles of distance to the border, which allows a non-linear effect on this margin. This further avoids issues with including a time-varying distance to a lower-tax border as a control variable, which I showed in figure 2 is a function of the home state tax. Both Harding et. al. (2012) and DeCicca et. al. (2013) include this time-varying control, which should put a downward bias on their coefficients involving the home state cigarette tax level. As I described above in the theoretical motivation section, the downward bias should come from the fact that this control is shutting off the causal path of increased cross-border shopping resulting from an individual living closer to a lower tax border after the home state tax increase.

My paper also uses data from nearly every census block group (CBG) in the states considered. This is an advantage over previous papers that only used urban smokers (Lovenheim 2008, Harding et. al. 2012, DeCicca et. al. 2013), which I show in table 1 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 as to who cross-border shops more in response to a cigarette tax by using the heterogeneity of CBG-level demographics available to me. This may be vital as low-educated individuals may not suffer a fiscal expenditure inequality when cigarette taxes increase, but also an inequality in the increase in time spent avoiding these taxes.

## 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 1<sup>st</sup>, 2018 and December 31<sup>st</sup>, 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 is distinct from the former as a visitor may make multiple visits to a POI over a month. However, the dataset does not record how many visits a visitor made to a specific POI. 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 5 minutes or more.

For each POI, the dataset details its name and NAICS industry code. Using each POI’s NAICS code, I have selected seven<sup>10</sup> NAICS codes that could be cigarette retailers. I further use the name of the POI to exclude certain retailers, such as CVS, who do not sell cigarettes but are a member of one of the seven NAICS groups.

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. 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, and visitors who entered a potential cigarette retailer outside of their state but within a border state. I

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<sup>10</sup> 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 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.

further assign each CBG a linear distance from its centroid to the closest lower-tax border state. I use this information to conduct analyses for only those CBGs that are close to the treated tax jurisdiction's border.

As mentioned in the introduction, my dataset has important limitations. First, I cannot observe what the visitors purchased in the cigarette retailer. Secondly, I cannot follow an individual visitor over time. This means I do not know if a visitor is a cigarette user, nor do I know anything else about the visitor besides their home CBG. This may cause issues if certain CBGs have more representation in the dataset than others, which could make my sample to be unrepresentative.

## Methods

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 policy change with no change in the same policy 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 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 3 state-level cigarette taxes in Illinois, Kentucky, and Oklahoma. Details on the effective date and control states chosen can be found in table 6.

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. As Goodman-Bacon (2021) has shown, the average treated on the treated estimate in TWFE models is comprised of all 2-by-2 difference-in-differences estimates that can

be estimated. These may lead to “bad” comparisons with dynamic treatment effect heterogeneity, such as early treated units being used as controls for later treated units. CS21 circumvents this issue by only estimating 2-by-2 difference-in-differences that compared treated units with (in my case) never-treated units. Their estimator then weights these 2-by-2 difference-in-differences estimates together to create useful summary parameters, such as the total average treated on the treated, or an average treated on the treated for each time a policy became effective.

Their 2-by-2 difference-in-differences estimates take the following form for any treated state  $g$  in period  $t$ :

$$ATT_{g,t} = E[Y_t - Y_{g^*-1} | G_g = 1] - E[Y_t - Y_{g^*-1} | C = 1]$$

, 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, and  $Y_t$  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  $t$  for treated state  $g$  is similar:

$$ATT_{g,t} = E[Y_t - Y_{t-1} | G_g = 1] - E[Y_t - Y_{t-1} | C = 1]$$

Notice that both equations do not have covariates. While the formula for these ATTs is similar with covariates, it is more complicated. To compensate for the inclusion of covariates, CS21 recommends using a doubly-robust estimator (Sant’Anna and Zhao 2020). CS21 also proposes their own cluster bootstrap to yield asymptotically valid standard errors. Per CS21’s recommendation, I will use both tools in my estimate. For the bootstrap, I cluster based on state of residence. 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 quartiles of minimum distance to a lower-tax



border over the whole sample period as covariates.<sup>11</sup> For the doubly robust estimator, only initial values of covariates are used and so are not time-varying.

I further run an event study within a balanced window to assess parallel trends and dynamic treatment effects. The window I choose, 6 months before the effective month and 5 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 post-period's coefficient in the event study by weighting together the ATTs for each treated state using the state's relative period to its own treatment. The weight in this case is chose to be the proportion of all the treated states observations that one treated group comprises.

My main results concern how the cigarette tax impacted cross-border shopping from the treated state 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.

I also divide my main results to estimate conditional average treatment effect by terciles of distance from the centroid of treated state CBGs to a lower-tax border state. 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. 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 20% of the distribution)

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<sup>11</sup> 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.

adults per capita with a high school degree or less and few (bottom 20% 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.<sup>12</sup> 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. 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. As described in the introduction, as low-educated people have an inequality in cigarette tax expenditure, this analysis allows me to comment on whether they also have an inequality in time expenditure to cross-border shop.

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

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<sup>12</sup> These calculations come from the TUS-CPS from the years 2003-2019.

## Results/Discussion

My main results are given in table 2. 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 CBGs send an additional 1.08 monthly cross-border shoppers per 100 devices active in response to a cigarette tax increase. This coefficient represents an increase of 48% from the dependent variable mean in the pre-period. Using the fact that the median CBG has 77 devices active over the sample period, this implies that, in sample, the median CBG sends an additional  $1.08 \times (77/100) \approx 0.83$  monthly cross-border shoppers. The second column estimates that a CBG sends 1.69 fewer monthly in-state shoppers per 100 devices active to cigarette retailers in response to a cigarette tax increase. However, while the sign on this coefficient is expected, it is not conventionally significant. On the other hand, the size of the coefficient is roughly symmetric with the number of cross-border shoppers a CBG sends out. This may make sense if cross-border shoppers are substituting away from in-state consumption as opposed to mixing consumption between border and home state.

The next two columns of table 2 estimate on a sample that has been divided by the adult proportion of a CBG that has at most a high school degree. Here I find, as predicted above, that CBGs with many lower educated adults send substantially more cross-border shoppers than CBGs with few lower educated adults. As a percentage of the pre-period mean, CBGs with few lower educated adults send about 30% more monthly border shoppers, while CBGs with many lower educated adults send 45% more. This result in particular highlights the education-related time expenditure inequality cause by cross-border shopping.

The final two columns in table 1 present estimates when dividing the sample by rural and urban CBG status. As predicted above, rural CBGs send substantially more cross-border shoppers than urban CBGs. 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.

Table 3 and 4 present heterogeneous effects by treated state and distance to the border. Table 3 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 than Oklahoma and Illinois, both of which raised their cigarette tax by \$1.00. Further, both Oklahoma and Illinois have similar estimates for this outcome. For in-state shoppers, I find that only Illinois saw a significant decrease for this margin, while Oklahoma and Kentucky did not see conventionally significant effects.

In table 4, 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 tercile (closest to the border) have the largest change in cross-border shoppers. As the tercile of distance from the border increases, while the coefficients remain positive and significant, they drop drastically compared to the first tercile. For example, the effect size for the second tercile is about a one fourth of the first tercile. However, the third tercile is close to the second tercile's estimate. 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, the estimates in table 3 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 by the third tercile of distance.

Table 5 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 to lower-tax border states whose tax level was surpassed by the home state. However, as this change in minimum distance grows for larger quartiles, so does the number of cross-border shoppers a CBG sends to the set of lower-tax border states mentioned earlier for this analysis. Further, these estimates at higher quartiles are conventionally significant, suggesting that lowering the minimum distance to a lower-tax border does spur more residents of the home state to cross-border shop. The results in table 5 then suggest that is an important mechanism that can explain why cross-border shopping may increase when the home 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 home state's cigarette tax can increase cross-border shopping.

The event studies for my results in tables 2 and 4 are presented in figures 3 and 4. In figure 3, I present the event studies for the full sample results presented in table 2. The figure presents a good case for parallel trends when the outcome is cross-border shoppers. Further, there appears to be a sustained increase in cross-border shoppers for up to 5 months after the tax becomes effective. 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.

Figure 2 presents event studies for the estimates concerning cross-border and in-state shopping by terciles of distance to the border. As the results in table 4 suggest, there is a strong, sustained increase in cross-border shopping for those in the first tercile of distance. As the terciles get larger, however, the initial increase becomes lower and generally does not last the full 5 months. Further, the event studies for

these results show good evidence of parallel trends. For in-state shoppers, much like displayed in figure 2, there appears to be no permanent drop in in-state shoppers after the tax becomes effective.

## **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 2018-2020. I found that the median census block group (CBG) sent about 0.83 more monthly cross-border shoppers in response to a cigarette tax increase. This magnitude is approximately a 50% increase from the before tax mean. I confirmed this result by showing larger increases in cross-border shopping 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 40 miles.

I further divided my sample by adult educational attainment in a CBG and rural CBG status. I 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. This result highlights that cigarette taxes create an education-based time expenditure inequality for who finds it conducive to cross-border shop in the face of a cigarette tax increase. I also estimated that rural CBGs send many more cross-border shoppers when faced with a cigarette tax than CBGs in urban areas. This result suggests that previous papers that relied solely on urban smokers likely undercounted the extent of the increase in cross-border shopping when a home state cigarette tax increases. I further suggested that previous papers may have downward biased their estimate by over-controlling for the time-varying minimum distance to a lower-tax border. This suggestion came from estimates that showed that the drop in this distance caused by the home state's cigarette tax increase was a significant driver in the increase in cross-border shopping.

In sum, my paper suggests that the increase in cross-border shopping in response to a cigarette tax increase is still a substantial thus far in the 21<sup>st</sup> century. Policy makers should keep in mind that when they go to raise their state's cigarette tax, it is likely that many smokers will simply choose to cross the border and shop out of state as opposed to reducing cigarette consumption. To avoid this issue, states may coordinate with border states and have them raise their own cigarette tax level by the same amount. Given the theoretical discussion earlier in this paper, this would cause no change in cross-border shopping. However, border states may be reluctant to do this as they would lose potential tax revenue from cigarette exports. One potential compromise could be to ask border states to have a slightly higher tax level only in the area near their border, but this differential tax would still lower than the home state's. This would have the effect of still allowing some cross-border shopping, but would reduce the amount as the relevant difference in tax levels would be smaller. Further, those in the home state would need to travel a great distance to reach the area of the border state with a much lower tax level, which would again disincentivize cross-border shopping.

A similar compromise to this was worked out with Native American tribes for New Mexico's cigarette tax increase in 2018. While these tribes were allowed to keep a lower tax level than the rest of New Mexico, they did agree to increase their tax level by an amount that was less than the rest of the state. This way, while the retailers on tribal land still had some business advantage, there was at least some disincentive for non-tribal residents of New Mexico to not cross-border shop.

Beyond these kinds of coordination, states may need to resort to other methods to decrease cigarette consumption in their state besides using increases in cigarette prices. One potential way to do this would be to support the Federal Drug Administrations push to remove most of the nicotine from

cigarettes.<sup>13</sup> This measure would make cigarettes much less addictive and could cause a large cessation in adults if passed.

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<sup>13</sup> <https://www.fda.gov/news-events/press-announcements/fda-announces-plans-proposed-rule-reduce-addictiveness-cigarettes-and-other-combusted-tobacco>



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**Table 1: 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.

<sup>+</sup>  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table 2: 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)
DD	1.082*** (0.135)	-1.690 (3.259)	0.363*** (0.105)	1.299** (0.501)	0.405*** (0.107)	1.863*** (0.539)
Obs	876,904	876,952	194,285	169,487	531,605	345,311
Dep Mean	2.21	227.70	1.22	2.86	1.78	2.90
# Clusters	8	8	8	8	8	8

*Notes:* Bootstrapped standard errors in parentheses. Standard errors are clustered on the state level. Coefficients are read as per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. “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.

<sup>+</sup>  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table 3: Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by State**

	Kentucky	Oklahoma	Illinois
DD (Cross-Border)	0.780*** (0.053)	1.210*** (0.028)	1.286*** (0.048)
Obs	574,608	567,114	727,174
DD (In-State)	-2.290 (3.288)	5.454 (4.386)	-6.939** (2.530)
Obs	574,656	567,162	727,222

*Notes:* Bootstrapped standard errors in parentheses. Standard errors are clustered on the state level. Coefficients are read as per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. “Cross-Border” or “In-State” outcomes are indicated in row titles.

<sup>+</sup>  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table 4: Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by Minimum Distance to Lower-Tax Border Tercile**

	Tercile 1	Tercile 2	Tercile 3
DD (Cross-Border)	1.728 <sup>*</sup>	0.463 <sup>***</sup>	0.398 <sup>***</sup>
	(0.831)	(0.048)	(0.034)
Obs	219,286	219,422	219,387
DD (In-State)	-6.624 <sup>+</sup>	-4.727 <sup>+</sup>	8.544
	(3.865)	(2.467)	(6.900)
Obs	219,311	219,422	219,410

*Notes:* Bootstrapped standard errors in parentheses. Standard errors are clustered on the state level. Coefficients are read as per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. “Cross-Border” or “In-State” outcomes are indicated in row titles.

<sup>+</sup>  $p < 0.1$ , <sup>\*</sup>  $p < 0.05$ , <sup>\*\*</sup>  $p < 0.01$ , <sup>\*\*\*</sup>  $p < 0.001$

**Table 5: Cross-Border Shoppers to Lower-Tax State by Change in Minimum Distance to Lower-Tax Border Quartile**

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
DD	0.174 (0.185)	0.715*** (0.212)	0.298* (0.139)	0.777* (0.305)
Obs	591,405	591,273	591,181	591,186
# Clusters	8	8	8	8

*Notes:* Bootstrapped standard errors in parentheses. Standard errors are clustered on the state level. Coefficients are read as per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. Each column splits the treatment group into a quartile of the change in minimum distance from the centroid of the CBG to the a lower-tax border state. Quartile 1 contains the lowest changes (including zero) and Quartile 4 contains the highest changes. Further, this table only considers cross-border shoppers to border states whose tax level was surpassed by the home state's cigarette tax level.

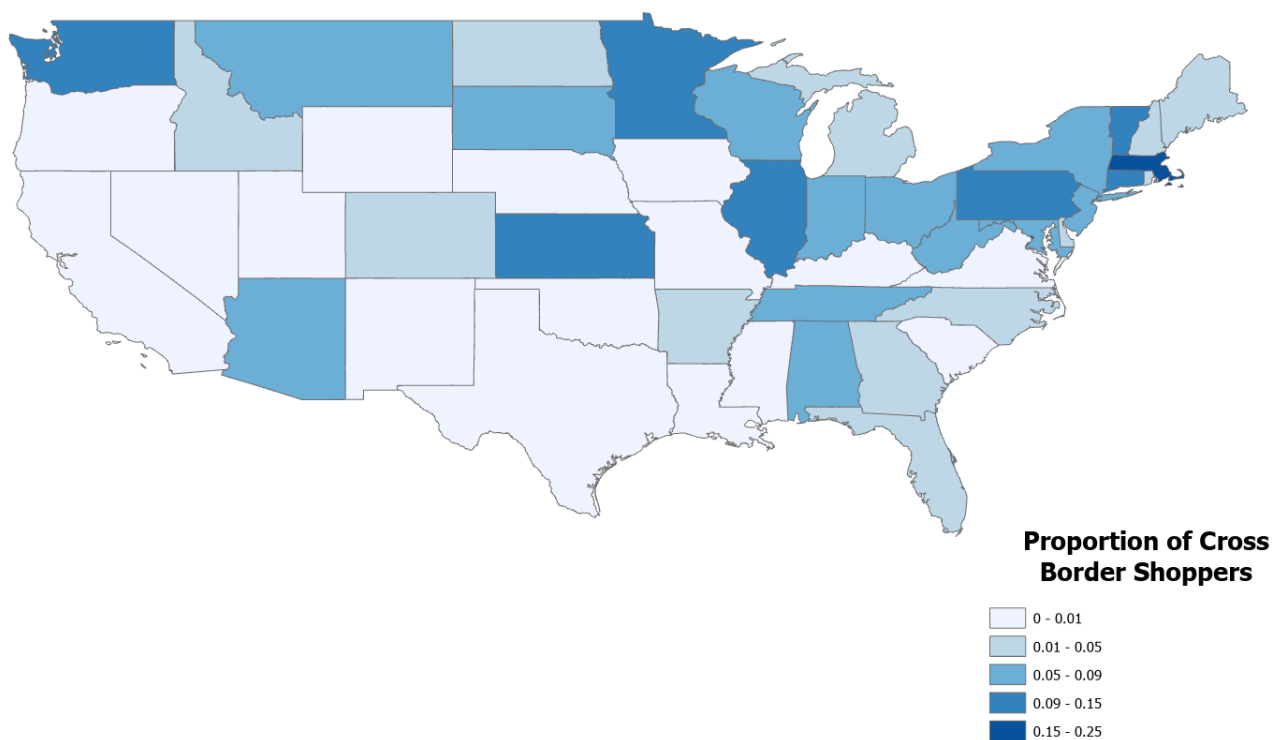
<sup>+</sup>  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table 6: Summary of Policies for Treated and Control States**

<b>Treated State</b>	<b>Border States (Controls)</b>	<b>\$ Increase</b>	<b>Effective Date</b>
Kentucky	Missouri, Virginia	0.5	July 1 <sup>st</sup> , 2018
Oklahoma	Colorado, Missouri	1	August 23 <sup>rd</sup> , 2018
Illinois	Indiana, Iowa, Missouri	1	July 1 <sup>st</sup> , 2019

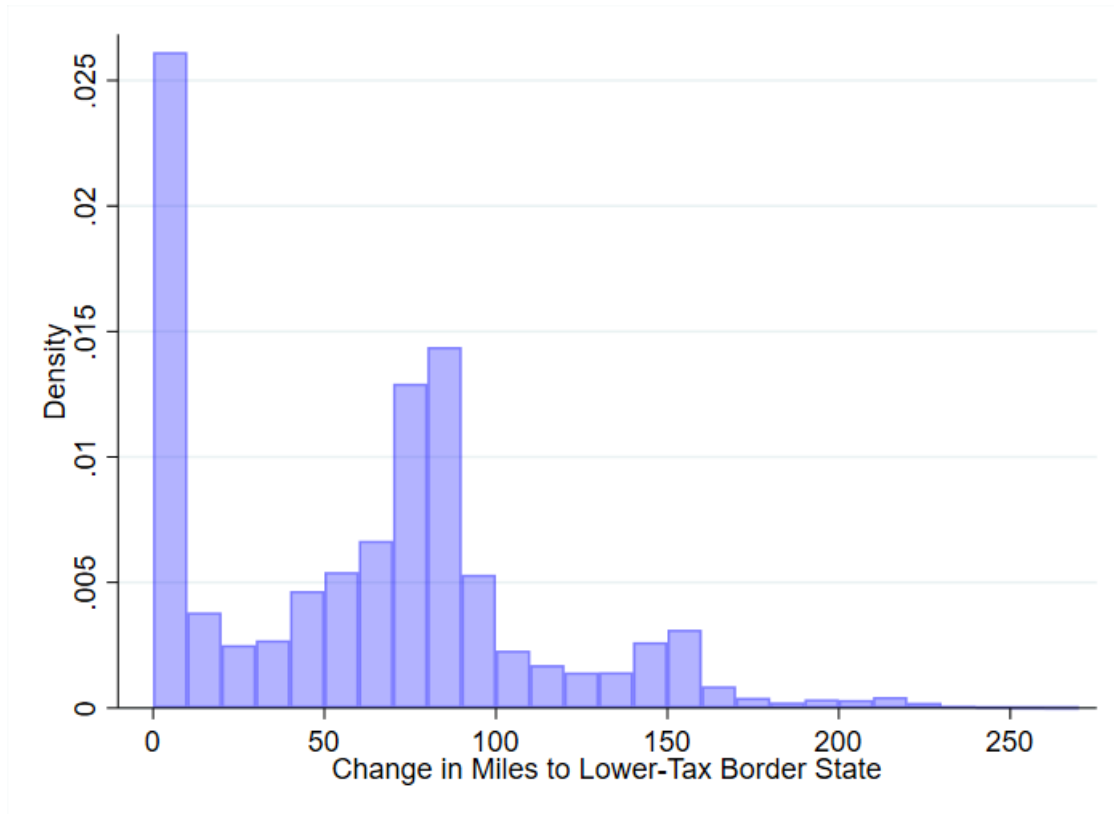


**Figure 1: Proportion of Cross-Border Shoppers by State in July, 2018**



*Notes:* This figure uses data from the Tobacco Use Supplement of the Current Population Survey in July of 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 2: Distribution of Changes in Distance to Lower-Tax Border State for Each Census Block Group**



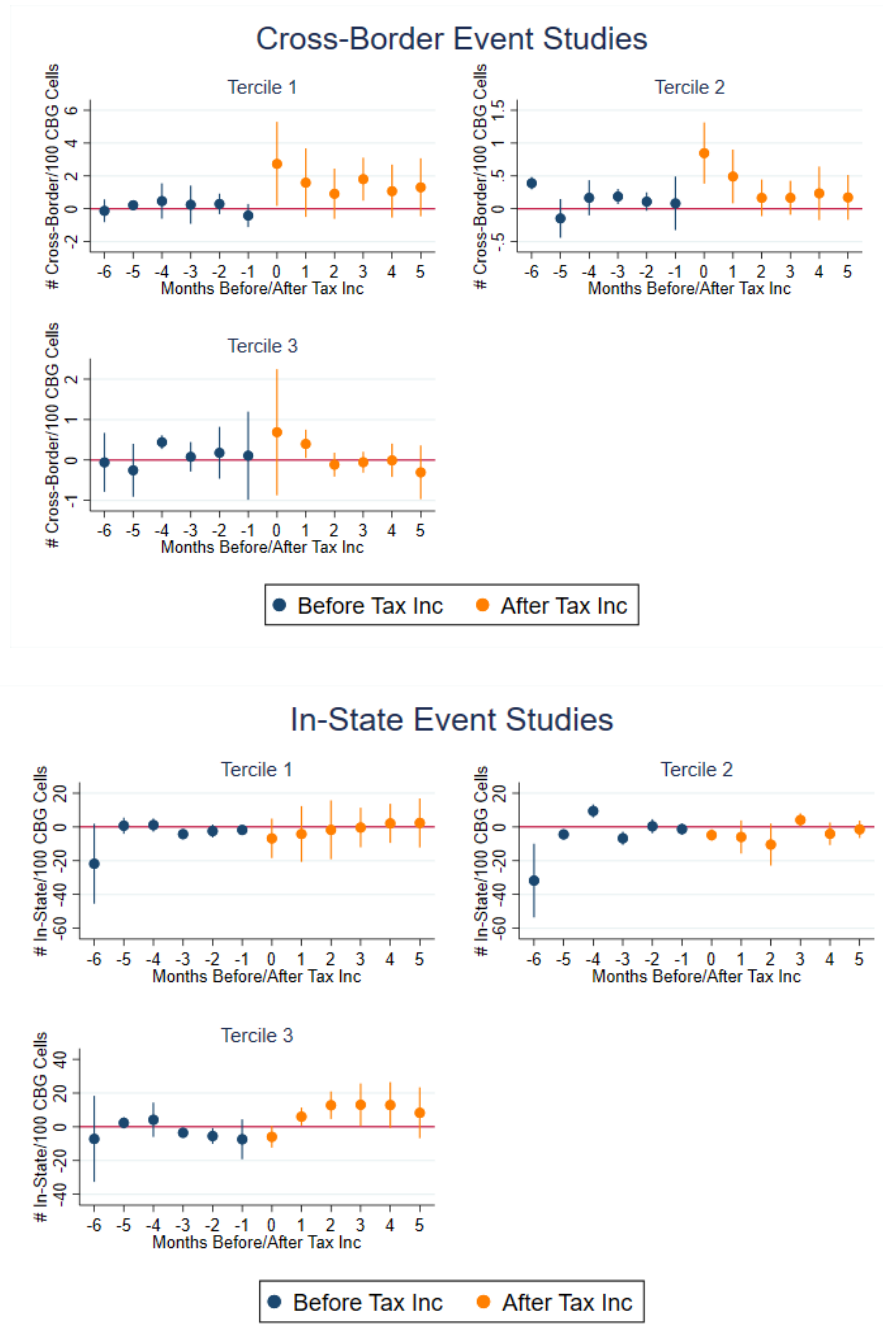
*Notes:* The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Change in distance to a lower-tax border state is calculated by taking the minimum distance from a centroid of each census block group to a lower-tax border state before the treated state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

**Figure 3: 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 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 4: Event studies For “Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by Minimum Distance to Lower-Tax Border Tercile”**



*Notes:* Bootstrapped confidence intervals are given as spikes on the point estimates presented as dots. Standard errors are clustered on the state 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. “Tercile 1” are distances closet to a lower-tax border and “Tercile 3” are distances furthest away from a lower-tax border.