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

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Abstract

I study the effect of cigarette tax increases 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 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. 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). I also estimate that the increase in cross-border shopping is substantially larger for those that live closer to a lower-tax border. 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.

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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 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.

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.[2] Cigarette tax avoidance strategies include cross-state border shopping for personal use (Lovenheim 2008, Harding et. al. 2012, DeCicca et. al. 2013) and many forms of organized smuggling by criminal organizations (Joossens and Raw 2012). Using the Tobacco Use Supplement of the CPS, Figure 1 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 the neighboring state governments may wish to consider coordinated adjustments to taxes to promote public health as well as to avoid the leakage in tax revenues in the tax-raising state.

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).[3] 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. To improve the measurement of the effect of cigarette tax hikes on cross-border shopping, I use a cellphone tracking dataset provided by SafeGraph. This data covers nearly every census block group (CBG)[4] 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

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

[3] 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.

[4] 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.

population.[5] 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 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 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). 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 2, 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 1, 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.

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,

[5] 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.

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 broad tax increases that are not limited to one area in the USA.

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.[6] 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:

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

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

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

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 3, 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 3. 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).

Literature Review

Many papers have measured either the totality of cigarette tax evasion[7] (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 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[8] 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 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.[9] 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

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

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

[9] 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.

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.[10] This paper and others like it have a similar drawback as papers that use cigarette sales as an outcome. 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 3 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 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 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

[10] A "noncompliance cigarette pack" is a pack that does not have the community's state tax stamp on it.

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 of potential cigarette retailers[11] 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.[12] 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.[13]

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 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 they 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 large increase in cross-border visits to these retailers for reasons besides cigarette purchasing around the time the cigarette tax is increased, my difference-in-differences estimation will be independent of the constant flow of cross-border visiting. This is the case as the main difference-in-differences parameter, the average treated on the treated, concerns the change in cross-border visits. I provide evidence that this overreporting is constant by using a placebo test where the outcome is the number of visitors to retailers in the industries I consider that do not sell cigarettes. 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

a. Main Analysis

[11] 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.

[12] 35 miles is the maximum distance in the first quartile of the distribution of these distances.

[13] 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.

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 Appendix Table 1. 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).[14]

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:

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

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.[15] This clustering decision was

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

[15] 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.

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.[16] The doubly-robust estimator only uses initial values of covariates 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, 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 4. 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 2 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.

b. 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.[17] 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 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.

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[16] 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.

[17] 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.

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.[18] 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 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)[19], 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[20], 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.

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

[19] 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.

[20] 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.

c. 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.[21] I then use most[22] 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 overcount 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, suggesting no significant change in shopping for other goods. 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.[23]

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 5. For many of the other related policies, there appears to be no change in their level that coincides with the state cigarette tax increases.[24] However, Figure 5 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.

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[21] These include ALDI, Natural Grocers, Whole Foods Market, Binny's Beverage Depot, Virginia ABC, CVS, Walmart Pharmacy, and Walgreen's Pharmacy.

[22] 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.

[23] 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:

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

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

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

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 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 6.[25] 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.[26]

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 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

a. Main Analysis

My main results are given in Table 3. 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

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

[26] 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.

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.[27]

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.

b. Extensions and Event Studies

The next six columns of Table 3 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 2 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.

Tables 4 and 5 present heterogenous effects by treated state and distance to the border. Table 4 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

[27] 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.

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 Table 5, 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, the estimates in Table 5 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 6 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 7 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 7 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 3 and 5 are presented in Figures 7 and 8. In Figure 7, I present the event studies for the full sample results presented in Table 3. 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.

Figure 8 presents event studies for the estimates concerning cross-border and in-state shopping by quartiles of distance to the border. As the results in Table 5 suggest, 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, much like displayed in Figure 2, there appears to be no permanent drop in in-state shoppers after the tax becomes effective.

c. Alternative Estimation Strategies

Table 8 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 Tables 2 and 3. 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.

d. 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[28]:

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

, 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 in this year.

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 $M \in \{\text{Kentucky, Oklahoma}\}$:

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

, 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.

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

$$\frac{\widehat{\partial\text{Cross-Border}}/\partial\text{Tax}}{\#\text{ of CBG Cell Phones}} \approx \frac{\partial\text{Cross-Border}/\partial\text{Tax}}{\text{CBG Population}}$$

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

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

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

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.

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 remains substantial in recent years (2018-19). Policy makers should keep cross-state evasion opportunities in mind when raising state cigarette taxes. 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 also be politically infeasible. Another option would be to enact cigarette tax increases on the federal level, which could eliminate any increase in cross-border shopping, at least within the USA.

Beyond coordination, states may wish to use other methods to decrease cigarette consumption in their state besides cigarette taxes. Several recent studies suggest (Callison and Kaestner 2014, Hansen et. al. 2017) that cigarette taxes have recently[29] had less impact on cigarette consumption than in the past. This may be due to remaining smokers in the current era having relatively price-inelastic demand due to hardened preferences, implying further tax increases will not impact the total quantity demanded for cigarettes substantially. In my own sample, I did find evidence of a decrease in shoppers to potential cigarette retailers in the state they reside in response to the tax increase. However, this could have been the result of smokers substituting geographical location of cigarette purchase as opposed to quitting.

[29] 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.

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.[30] 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 low regulation of less-harmful tobacco products such as e-cigarettes or General Snus, providing free or subsidized smoking cessation products such as nicotine patches, and/or expanding healthcare access so that individuals can receive prescription products such as Chantix more easily.

[30] See footnote 27.

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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. 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 2: 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 3: 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.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by State

	Kentucky	Oklahoma	Illinois
DD (Cross-Border)	0.478^{+}	0.616	0.968^{+}
	(0.247)	(0.454)	(0.524)
Obs	240,713	192,196	445,971
DD (In-State)	-2.541	-3.227	-8.941*
	(1.888)	(6.993)	(4.182)
Obs	240,713	192,196	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. “Cross-Border” or “In-State” outcomes are indicated in row titles.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by Minimum Distance of CBG to Lower-Tax Border Quartile

	0.12-18.73 Miles	18.74-61.17 Miles	61.17-142.69 Miles
DD (Cross-Border)	1.557***	0.222***	-0.008
	(0.397)	(0.037)	(0.096)
Obs	219,212	219,471	219,527
DD (In-State)	-8.538+	-6.120	2.014
	(5.145)	(5.363)	(5.254)
Obs	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 the number of 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 6: 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 7: 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

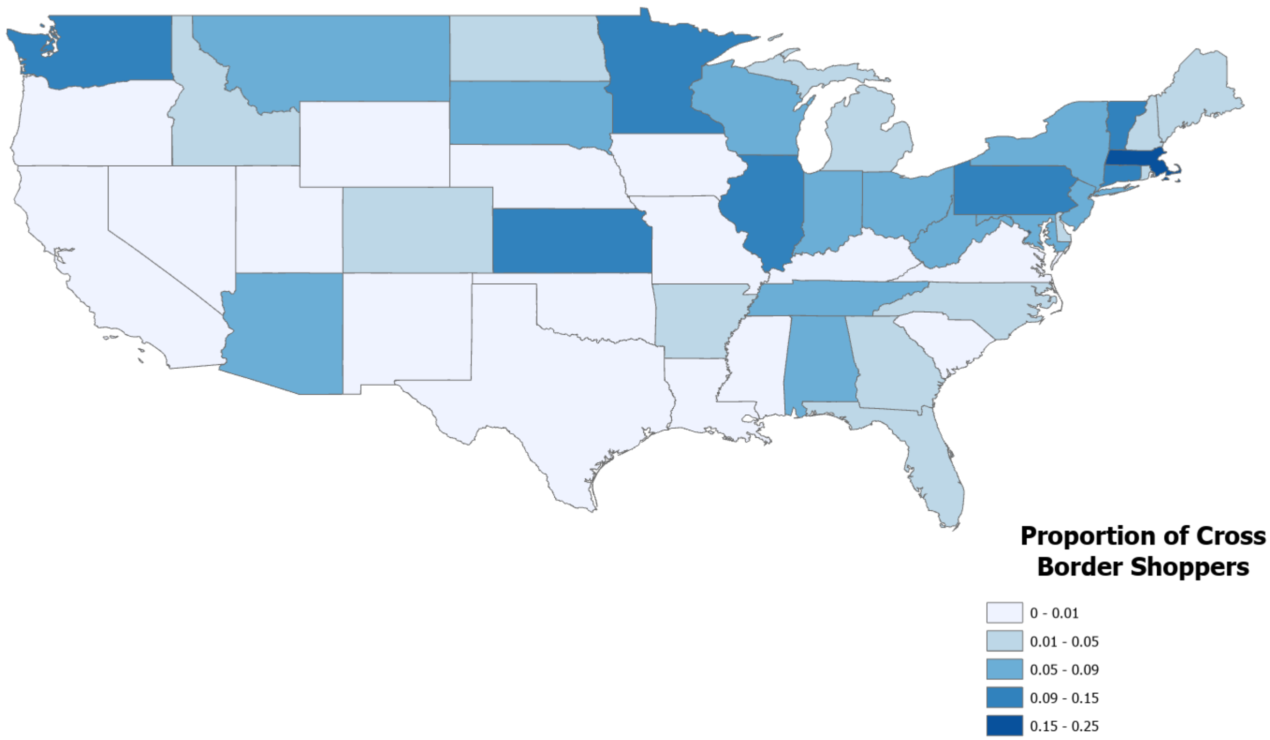
Notes: Bootstrapped standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as monthly cross-border shoppers per 100 cellphones active in a CBG. Each column splits the treatment group into a quartile of the 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 8: 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

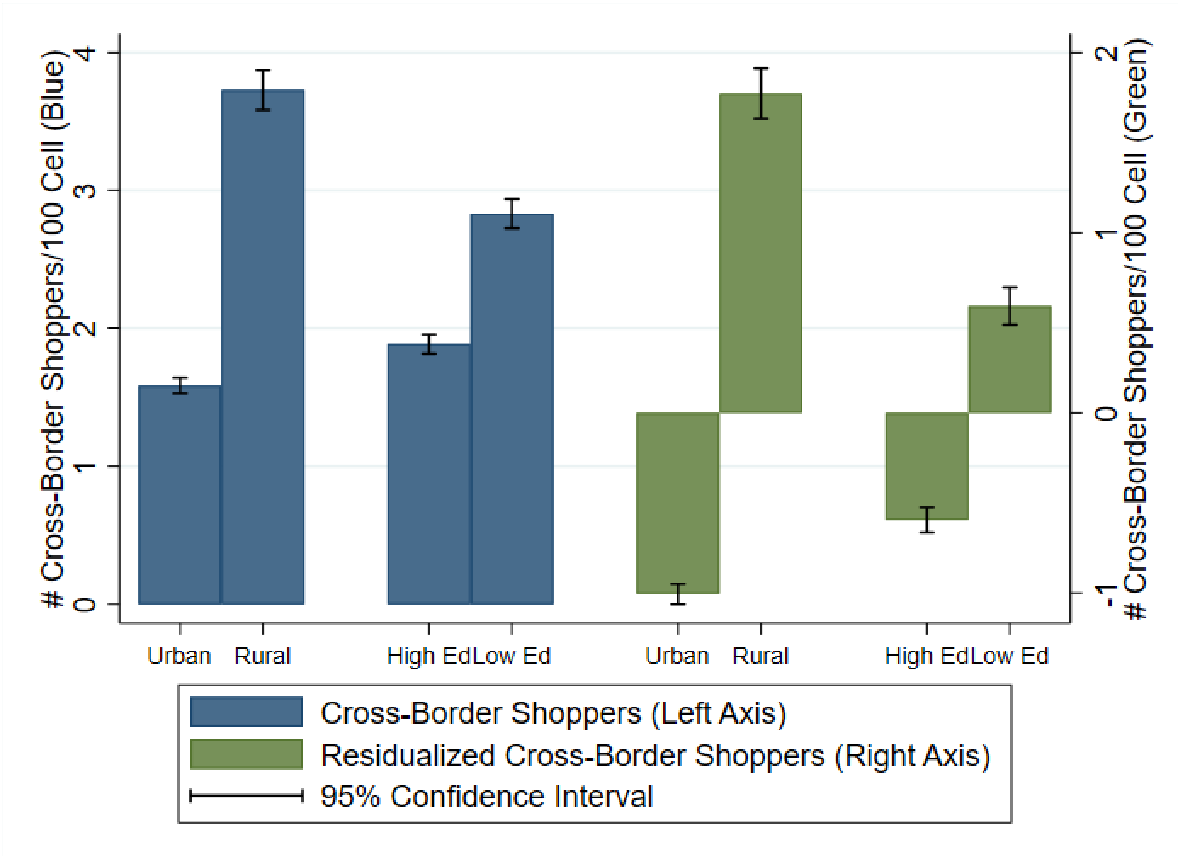
Notes: Standard errors in parentheses. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level for all columns except for the "TWMR" column, where they are clustered by CBG. Coefficients are read as monthly cross-border shoppers per 100 cellphones active in a CBG.The 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).

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



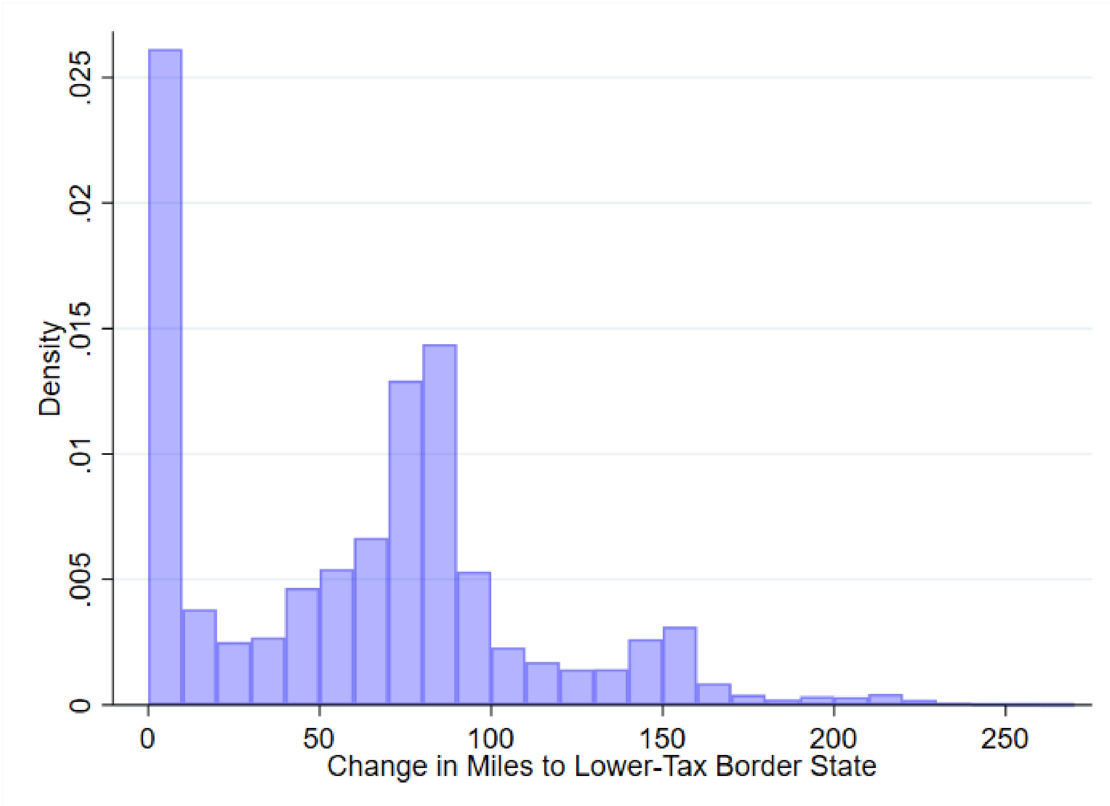
Notes: This Figure uses data from the Tobacco Use Supplement of the Current Population Survey in July 2018. The proportion of cross-border shoppers is calculated for each state by taking the number of individuals who reported purchasing their last pack of cigarettes in a state they do not reside in and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

Figure 2: Before Tax Increase Time Expenditure Inequality in Treated States by CBG-Level Adult Educational Attainment per Capita and Rural Status



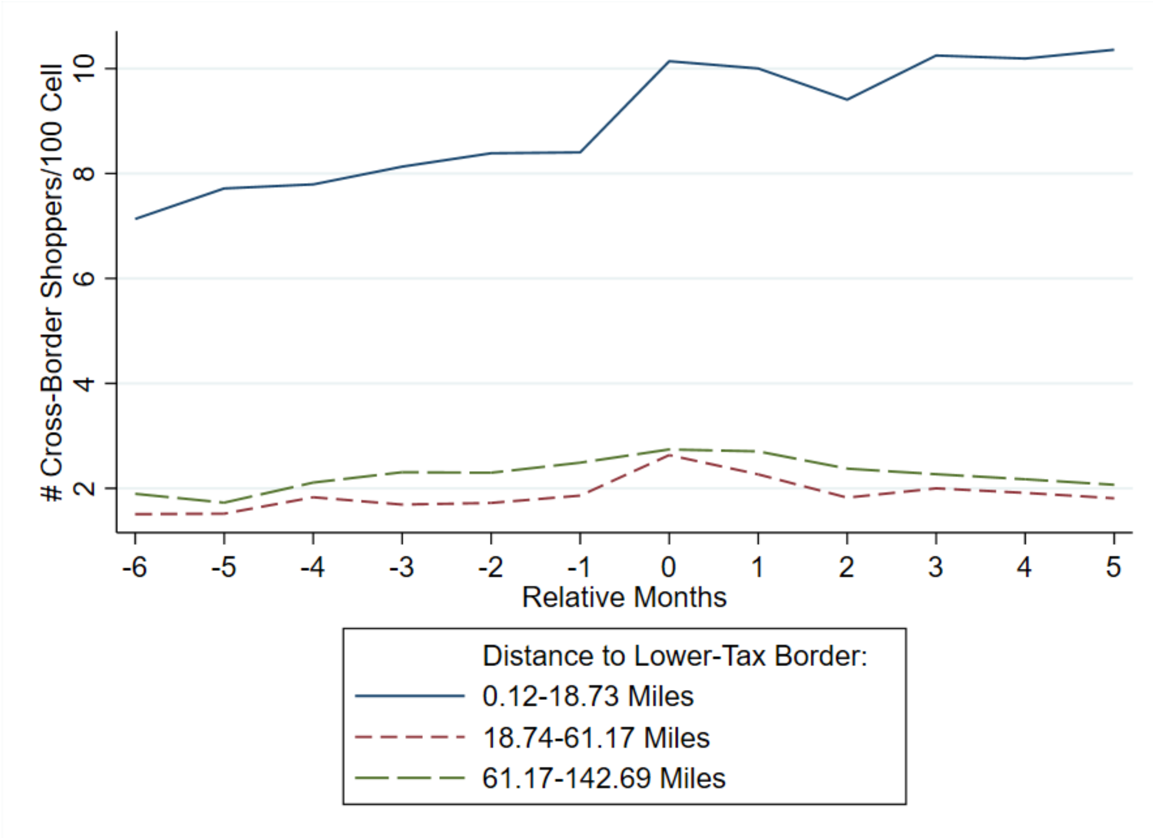
Notes: Levels of cross-border shopping are read as per 100 cellphones active in a census block group (CBG). These levels also concern monthly shoppers in the treated state before any treated state in the sample raised its cigarette tax. Each bar is the average number of cross-border shoppers over all 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.

Figure 3: Distribution of Changes in Distance to Lower-Tax Border State for Each Census Block Group



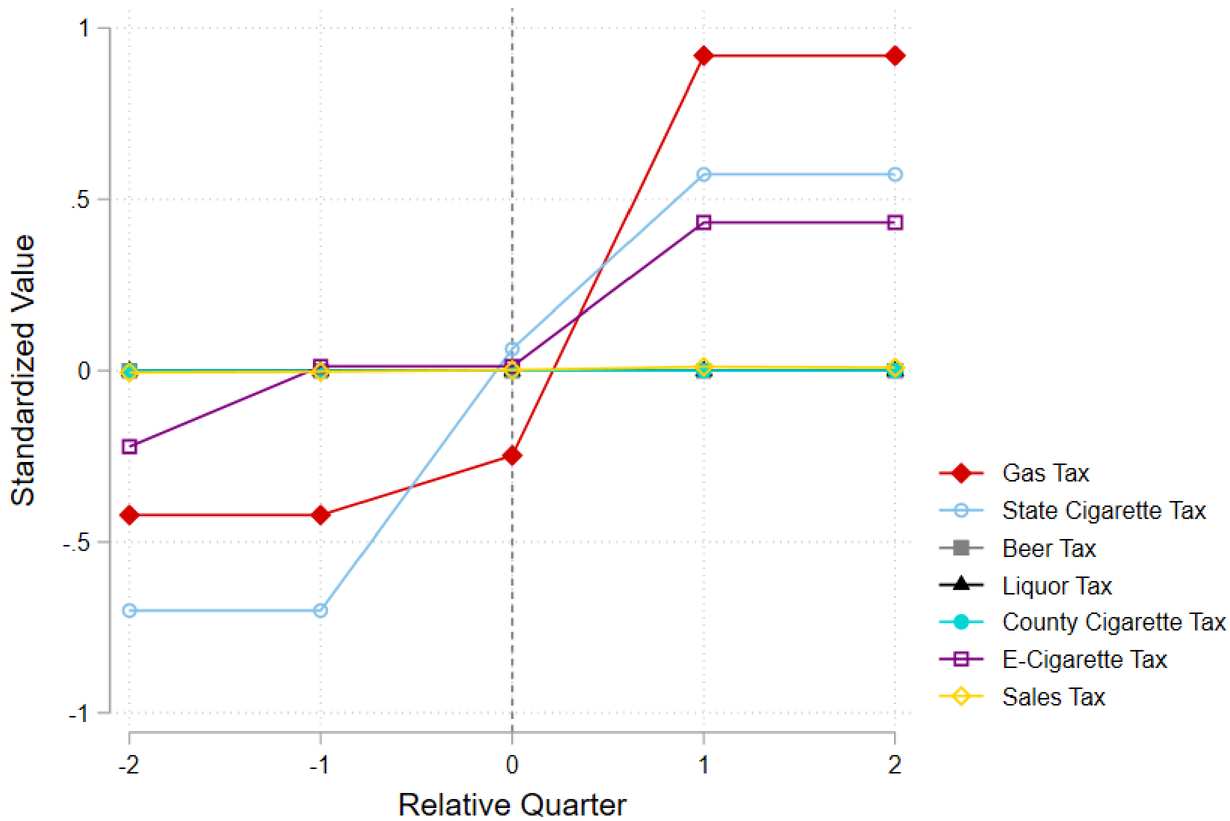
Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Change in distance to a lower-tax border state is calculated by taking the minimum distance from a centroid of each census block group to a lower-tax border state before the treated state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

Figure 4: Relative Months to Cigarette Tax Increase using Raw Data for Treated Groups



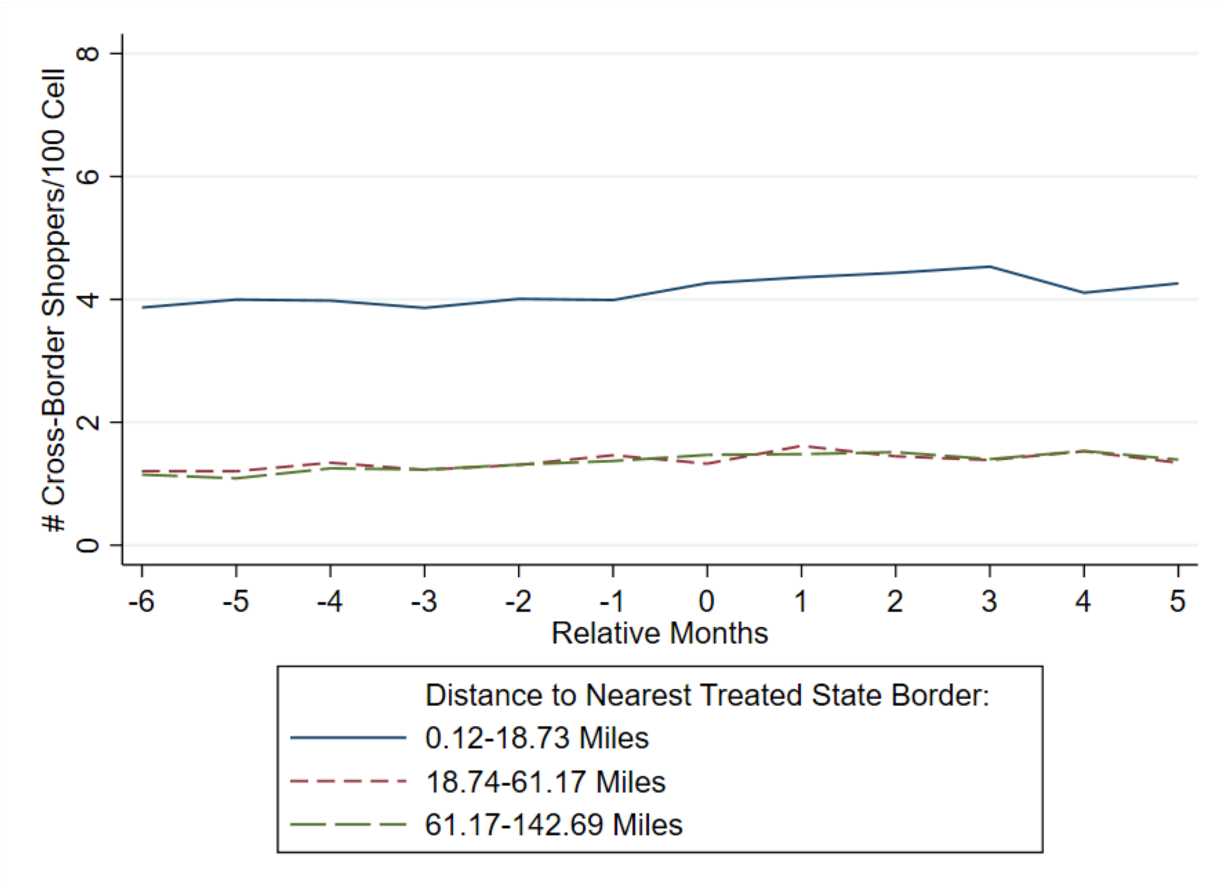
Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Each point on the graph is calculated by taking the average of cross-border shoppers per 100 devices in all the census block groups (CBGs) for each treated state. The relative month of "0" 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.

Figure 5: 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 6: Relative Months to Cigarette Tax Increase using Raw Data for Control Groups

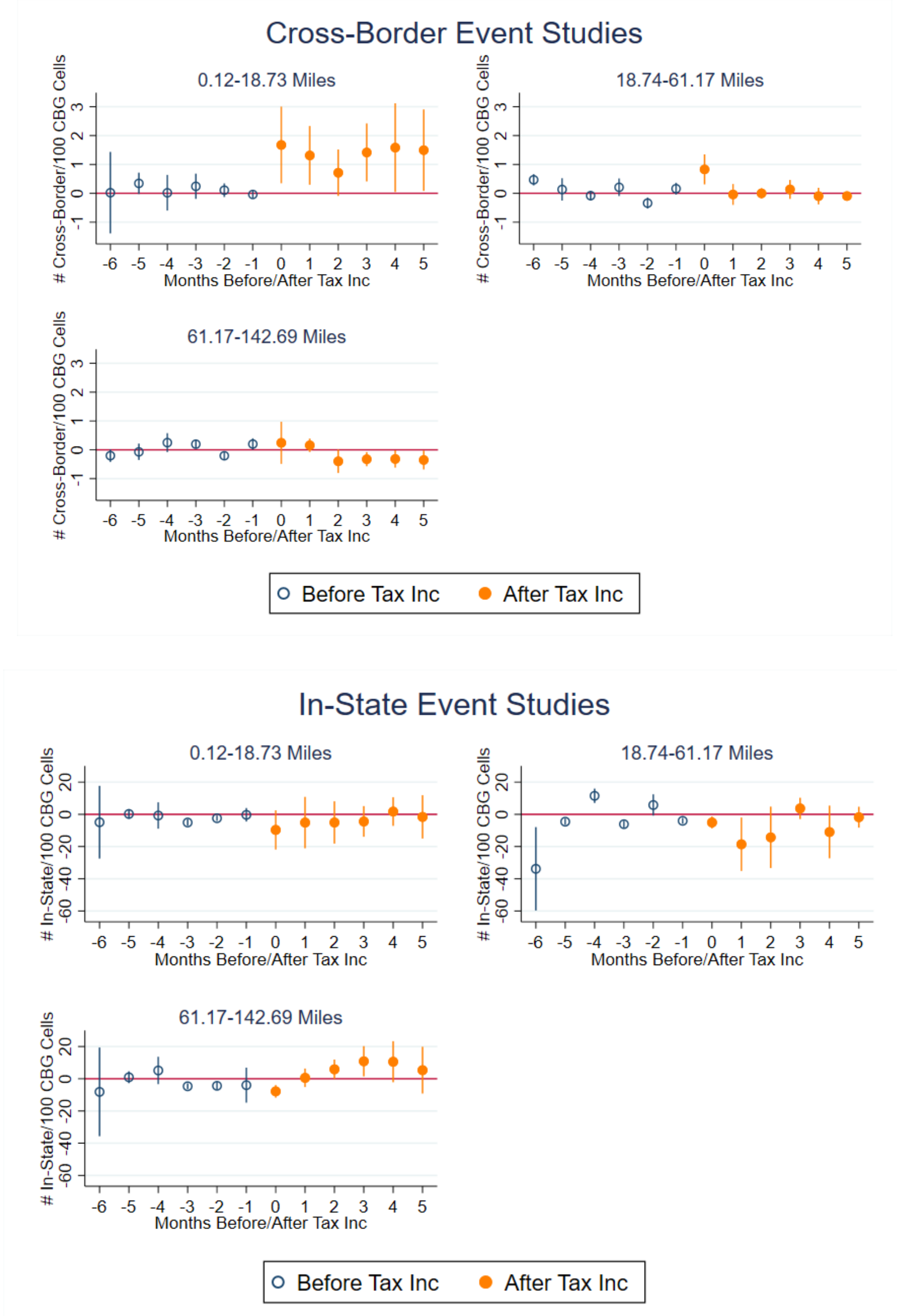


Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Each point on the graph is calculated by taking the average of cross-border shoppers per 100 devices in all the census block groups (CBGs) for each control state. The relative month of "0" 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 7: 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 8: 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 shoppers. "Cross-Border" or "In-State" outcomes are indicated in separate Figure titles.

Appendix

Table 1: 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.