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% of the pre-tax 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 that CBGs with many low educated adults and rural CBGs send substantially more cross-border shoppers than their respective counterparts. 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 limit 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 impacts 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 should consider a coordinated

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

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. 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 all the devices that it tracks, which is a precise definition of a consumer's home residence. This data is then well-suited for estimating how much cross-border shopping changes in response to a state-level cigarette tax for both urban and rural areas. Additionally, as the estimation is during a recent period (2018-2019) in the USA, e-cigarettes would have been widely available to all the smokers in my treated states. Previous studies examining cross-border shopping considered a time interval in the USA where e-cigarettes where not widely used by the population.[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.

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

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[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.

[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.

increase in cross-border shoppers in response to a state-level cigarette tax increase. Specifically, I estimate that the median CBG sent about 0.56 more monthly cross-border shoppers to a lower-tax border state in response to a cigarette tax increase. This magnitude is approximately 27% of the before tax mean. I further use my results to estimate that between 9% and 27% of missing sales in 2019 for a subset of my treated states were attributable to cross-border shopping. This back of the envelope calculation suggests that cross-border shopping does constitute a substantial portion of the decline in sales for a state. Exacerbated

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 receives cross-border shoppers. 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 had a lower tax before the tax hike. My results suggest that this is true for the treated states that surpassed the tax level of multiple border states. However, for the treated state that only surpassed one border state and already

had a higher tax level than the remaining border states, this intuition does not hold. Furthermore, the only significant increase in cross-border shoppers for this state were to border states that had lower tax levels than the treated state pre-tax increase. This implies that even if a state can raise its tax to a higher level than most surrounding states, further increases from this level will still incentivize more residents to cross-border shop in surrounding states. This result also confirms the need for border states to coordinate their tax levels to increase similarly with the tax-raising state.

Theoretical Motivation

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

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

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

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 1 if the inequality in (1) holds and is 0 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 to a lower tax border. This will test whether those closer to the border have a greater response to an increase in t than those further away.

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

As Conlon et. al. (2021) estimate, nearly all cigarette taxes are paid by a small fraction of individuals, who are much more likely than the general adult population to be low-income and low-educated. Building on this, Darden (2021) argues low-skilled workers either migrate to or remain in rural areas as they cannot compete in urban labor markets. Darden (2021) concludes that these migration patterns have caused rural areas to have more smokers than urban areas. His finding confirms the

importance of considering rural areas in any estimate of cross-border shopping as individuals in these areas have a higher probability of being heavy smokers and being a smoker. To incorporate these insights in my estimates, as I do not know who in my sample is a smoker, I divide my sample between CBGs with a high portion of low-educated adults or is a rural CBG. Comparing these groups to their counterparts will then allow me to assess whether more people cross-border shopped from areas with more smokers and heavy smokers conditional on being a smoker.

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

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

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

Further, note that:

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

, or, the change in the minimum distance to a lower tax border is non-negative when the home state increases its tax level. I show this relationship is true in my sample in Figure 3, 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 (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, as mentioned in the introduction, there are many forms of cigarette tax evasion. A decline in taxed cigarette sales in a home state due to a lower tax border state

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

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.[8]

Stehr (2005) improves on previous papers by using both state-level sales and individual consumption data of cigarettes to estimate the magnitude of cigarette tax avoidance. To do this, he estimates 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. Stehr estimates that differences between sales and consumption increases by 0.0322 log points in response to a 1 unit increase in the population and home state-radius weighted average cigarette tax differential between the home state and all surrounding border states. However, Stehr cannot differentiate between how much of this export behavior is due to arbitrage or cross-border shopping.

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 state's 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, the relatively higher level of consumption for those living near a low tax border is used to calculate that the percentage of sale due to cigarette 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.

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[8] 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/

Further, like all papers that use the TUS-CPS in this literature, Lovenheim uses only urban respondents so he can use the lowest geocode in the TUS-CPS – the Metropolitan Statistical Area FIPS code.

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 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. One potential limitation 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 they can use Metropolitan Statistical Area FIPS code.

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.[9] This paper suffers from similar issues mentioned above in that noncompliance 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.

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

Finally, Harding et. al. (2012) uses Nielsen Homescan data to estimate the increase in cross-border cigarette shopping due to higher cigarette taxes. The Nielsen Homescan data is useful in this case as it records all purchases whose Universal Product Code the user scanned with the device given to them by Neilson. 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 reduced as distance from the border increases. 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, the Nielsen Homescan data can at most identify a respondents home residence down to the census tract level.

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 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 and uses a high-resolution definition (census block group) of a respondent's home address. 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). 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 3 is a function of the home state tax.

My paper also uses data 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 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[10] 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 are far away from a treated state's border but still in a border state. 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.[11]

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 (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. This may further cause issues

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[10] 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.

[11] 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.

if certain CBGs have more representation in the dataset than others, leading 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 cigarette tax increase with no change in this tax 6 months before and 5 months after the effective month of the tax. This policy change occurs in what I call the "treated state". I then found states that were not treated over my period and designated a subset of these as the control states. All control states are bordering at least one treated state, do not have a tax change over the sample period, and have a lower tax level than the bordered treated state both before and after the tax effective date. I then consider cross-border shopping into the treated states as the control state's outcome. As their cigarette tax level is lower than the treated state's both before and after the treated state increases its tax, their cross-border shopping behavior should be unaffected. My final dataset considers three state-level cigarette taxes in Illinois, Kentucky, and Oklahoma. Details on the effective date and control states chosen can be found in Table 9.

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 $t > g^*$:

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

, 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 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:

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

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 $E(Y_t - Y_{g^*-1}|X, C = 1)$ 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.[12] This clustering decision was made as treatment not only varies by state, but also by distance to a lower-tax border in the state that raised its cigarette tax. This occurs as a cigarette retailer's passthrough of the tax to the final price is diminishing the closer they are to a lower-tax border (Harding et. al. 2012).

Concerning covariates, I use the quartiles of the proportion of a CBG that is white, has a high school education or less, and that drives to work. I also use whether the CBG is rural or urban and the first three quartiles of minimum distance to a lower-tax border over the whole sample period as

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[12] 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.

covariates.[13] 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 chose 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. My main results also only consider cross-border shopping to cigarette retailers in the border state but near the treated state's border. As I show in Table 2, border state cigarette retailers within the first quartile of distance to the treated state's border has a large, significant increase in cross-border visitors when the treated states raised their tax level. Further, cross-border shopping for personal use should be concentrated among cigarette retailers

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[13] 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.

in a border state but near the treated state's border, as individuals lose financial incentive to travel any further into the border state for tax evasion purposes than just across the state border.

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.[14] 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.

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.[15] 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

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[14] 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.

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

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.

As mentioned earlier, I removed retailers from my main analysis who do not sell cigarettes but are classified in a cigarette retailer industry.[16] I then use most[17] of these retailers I removed as a placebo test by only considering cross-border shopping to these non-cigarette selling retailers. A a null result in this regression where stores that are not believed to sell cigarettes are an outcome provides 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.

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

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

[17] All the stores in footnote 14, except for Walmart Pharmacy and Walgreen's Pharmacy.

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 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, in sample, 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 1.00 fewer monthly in-state shoppers per 100 devices active to cigarette retailers in response to a cigarette tax increase. Notice the size of the coefficient is not symmetric with the number of cross-border shoppers a CBG sends. This result may have occurred if cross-border shoppers condensed in-state trips to multiple cigarette retailers to an out-of-state trip to one or few cigarette retailers. It may also reflect a drop in consumption for treated state residents.

The next four 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 and by rural status of the CBG. 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 dodge increases in cigarette taxes.

b. Extensions and Event Studies

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 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 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.

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 5 and 6. In Figure 5, 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 6 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 those in the first quartile of distance. 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, along with 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 remaining columns in the table deal with various difference-in-differences estimators. The second 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 Cigarette Sales Decline

Using The Tax Burden on Tobacco data, I further compute how much of the "missing" sales in Oklahoma and Kentucky in 2019 are attributable to cross-border shopping. To determine this, I first compute the number of missing sales by regressing the number of cigarette sales in these states and the states surrounding them in 2019 on the number of sales for the same states in 2017. I then take the residual for Oklahoma and Kentucky after running this regression, thus providing an estimate of "missing" sales, or, the difference between the actual recorded sales in these states in 2019 and the predicted amount determined from the regression. Both estimated residuals are negative, suggesting that these states should have had more sales without the tax increase they implemented in 2018.

I then 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 manipulate the following approximation to get the change in cross-border shopping for each CBG[18]:

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

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[18] 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.

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

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

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

, 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.

Finally, 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 final line in the equation 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.

Putting all these parts together, I compute the proportion of missing sales attributable to cross-border shopping as follows for a state $M \in \{\text{Kentucky, Oklahoma}\}$:

$$Proportion\ of\ Missing\ Sales_M = \frac{\sum_{k=1}^{N} \sum_{b=1}^{12} z_{b,k,M} \times 30 \times \frac{\widehat{\beta_{M,ATT}^J}}{100} \times Population_{k,M}}{Missing\ Sales_M}$$

, 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 27% of the missing sales for Kentucky and about 9% of missing sales for Oklahoma can be attributed to cross-border shopping.

These results underly the impact cross-border shopping can have on tax revenue if a state decides to raise its cigarette tax.

Conclusion

In this paper, I estimated the change in cross-border shopping in response to a cigarette tax increase for three states in the USA over 2018-2019. I found that the median census block group (CBG) sent about 0.53 more monthly cross-border shoppers in response to a cigarette tax increase. This magnitude is approximately a 19% increase from the before tax mean. I confirmed this result by showing larger increases in cross-border shopping for CBGs closer to a lower-tax border. Event studies revealed that the only lasting change in cross-border shopping happened for CBGs whose minimum distance to a lower-tax border is less than 18 miles. Using my main estimates, I also found that 27% of the missing sales for Kentucky and about 9% of missing sales for Oklahoma can be attributed to cross-border shopping. This back of the envelope calculation showed that increases in cross-border shopping can substantially impact sales in states that raise their cigarette taxes.

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. I also 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 highlights that cigarette tax increases can exacerbate an spatial and education-based time expenditure inequality for who finds it conducive to cross-border shop.

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 21st 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 increase their tax level only in the area near their border, while keeping this tax lower than the treated state's. This would have the effect of still allowing some cross-border shopping but would reduce it as the relevant difference in tax levels would be smaller. Further, those in the treated 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 border state may agree to a deal like this if they project that additional revenue they bring in from the tax on the perimeter exceeds the loss in revenue from the reduced number of cross-border shoppers into their state. 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 Administration's push to remove most of the nicotine from

cigarettes.[19] This measure would make cigarettes much less addictive and could cause a large cessation among adults if passed.

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[19] 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. 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.
$^{+} p < 0.1, ^{*} p < 0.05, ^{**} p < 0.01, ^{***} p < 0.001$

Table 2: Cross-Border Shoppers to Lower-Tax State by Quartiles of Distance of POI in Border State to Tax-Raising State

	Quartile 1	Quartile 2	Quartile 3
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 per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. 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)
DD	0.690**	-1.004	0.465*	0.801*	0.306*	1.140*
	(0.254)	(3.329)	(0.181)	(0.354)	(0.142)	(0.498)
Obs	658,210	876,952	309,487	348,699	382,959	275,251
Dep Mean	3.61	227.70	2.48	4.67	2.01	3.42
# Clusters	23	23	23	23	22	23

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

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)	6.214	-6.981	-3.466
	(5.735)	(4.751)	(4.816)
Obs	574,656	567,162	727,222

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 per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. "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 to Lower-Tax Border Quartile

	Quartile 1	Quartile 2	Quartile 3
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 per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. "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 Minimum Distance to Lower-Tax Border Quartile for Each Treated State

	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 per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. 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

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
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 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.

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

Table 8: Alternative Difference-in-Differences Estimators and Placebo Regression Results

	Placebo	CS21	TWFE	Stacked	TWMR
DD	-0.048	0.690**	0.983*	0.655*	1.098***
	(0.047)	(0.254)	(0.430)	(0.251)	(0.197)
Obs	658,162	658,210	658,214	410,028	658,215
# Clusters	23	23	23	24	27,430

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 per 100 cellphones active in a CBG. The outcome also concerns monthly shoppers. 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). Finally, the "TWMR" column estimates a Two-Way Mundlak Regression popularized by Woolridge (2021).

Table 9: Summary of Policies for Treated and Control States

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

***** Controls may repeat in the table above, but they are only considered once in all analyses.

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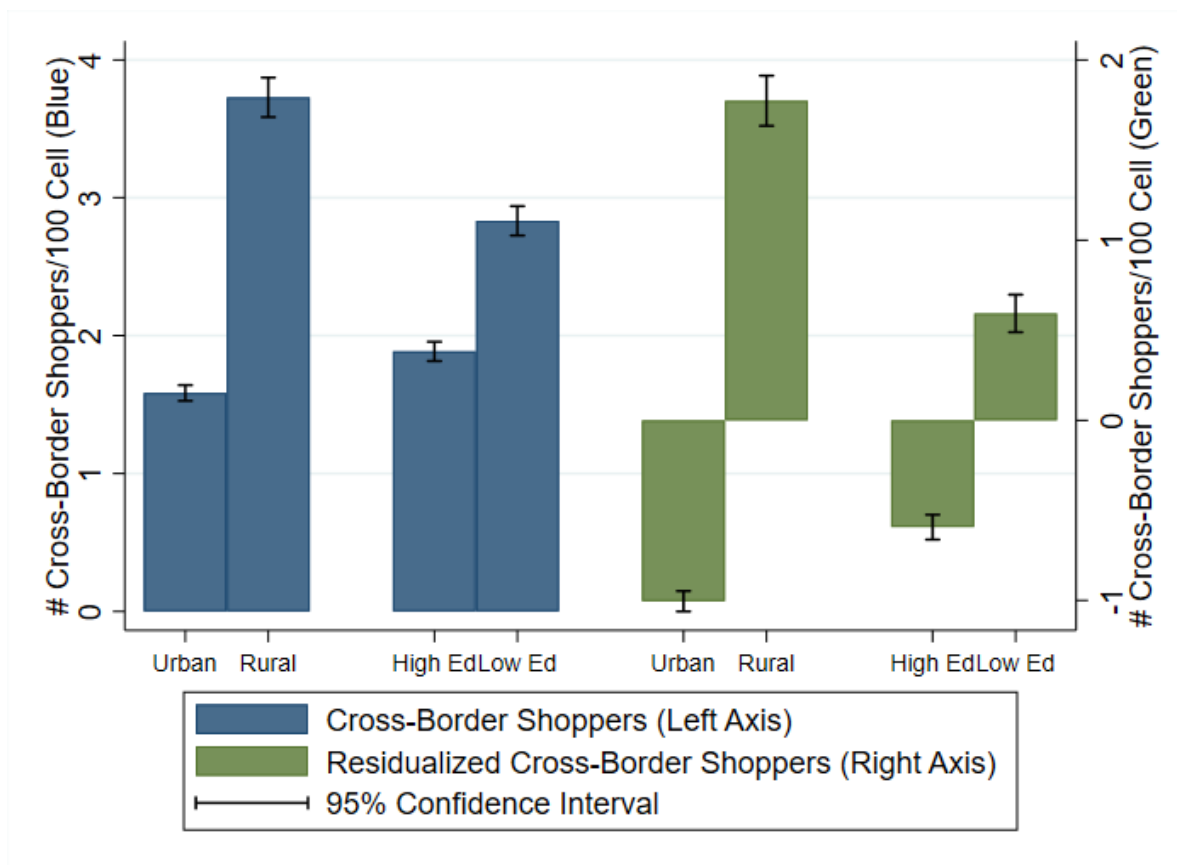

Notes: Levels of cross-border shopping are read as per 100 cellphones active in a census block group (CBG). These levels also concern monthly shoppers in the treated state before any treated state in the sample raised its cigarette tax. Each bar is the average number of cross-border shoppers over all 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
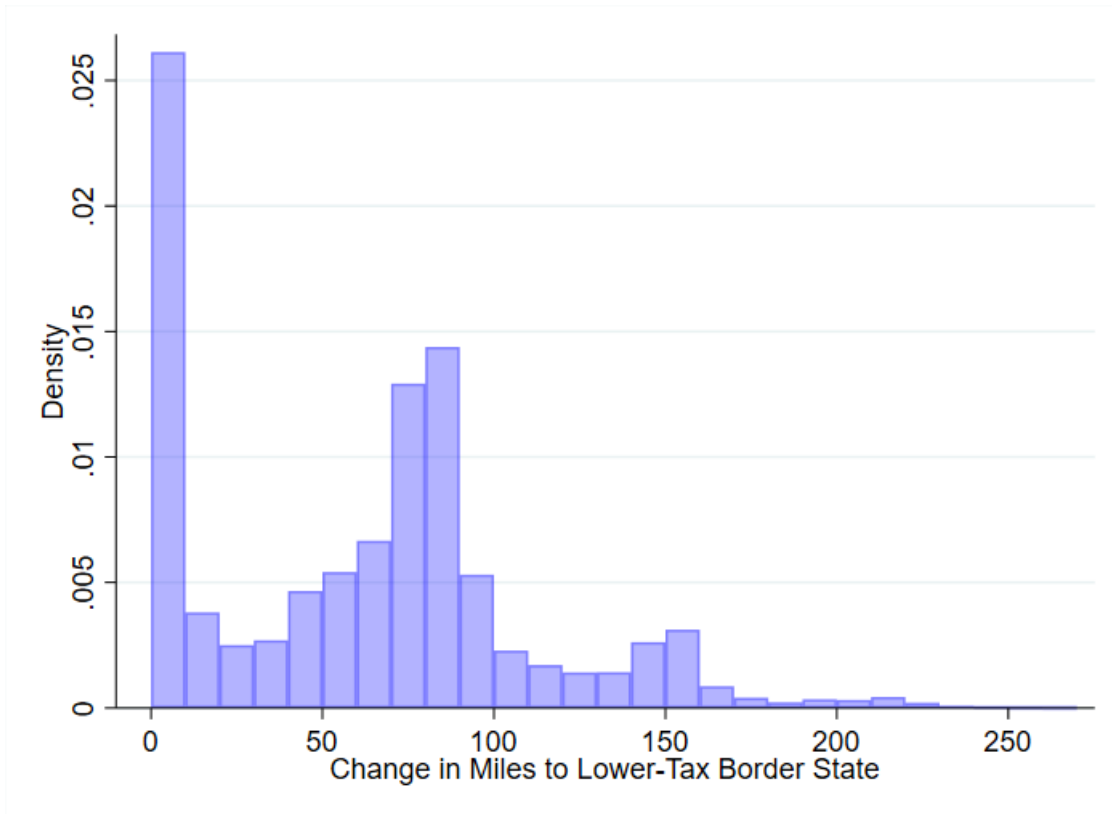
Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Change in distance to a lower-tax border state is calculated by taking the minimum distance from a centroid of each census block group to a lower-tax border state before the treated state 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
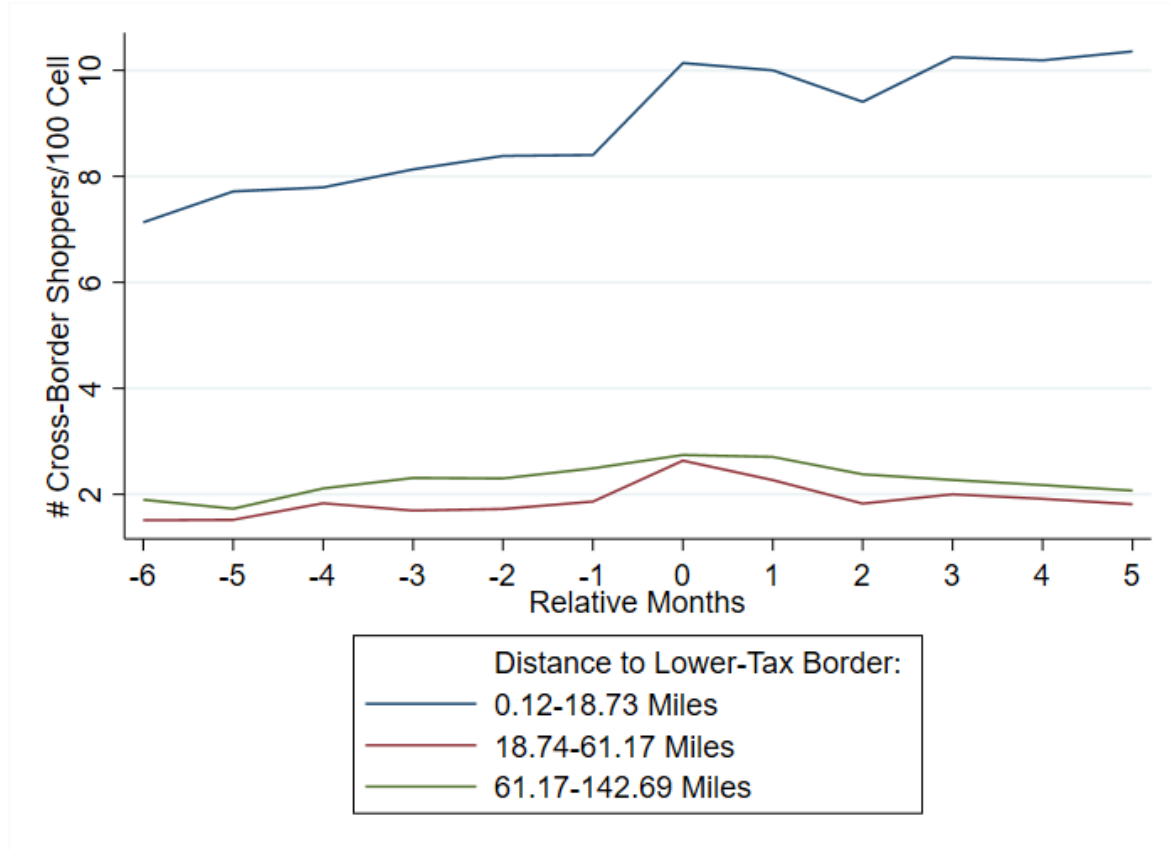

Notes: The data in the histogram above is from the treated states in my SafeGraph sample from 2018 through 2019. Each point on the graph is calculated by taking the average of cross-border shoppers per 100 devices in all the census block groups (CBGs) for each treated state. The relative month of "0" 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: 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 6: Event studies For "Cross-Border Shoppers to Lower-Tax State and In-State Shoppers by Minimum Distance to Lower-Tax Border Tercile"
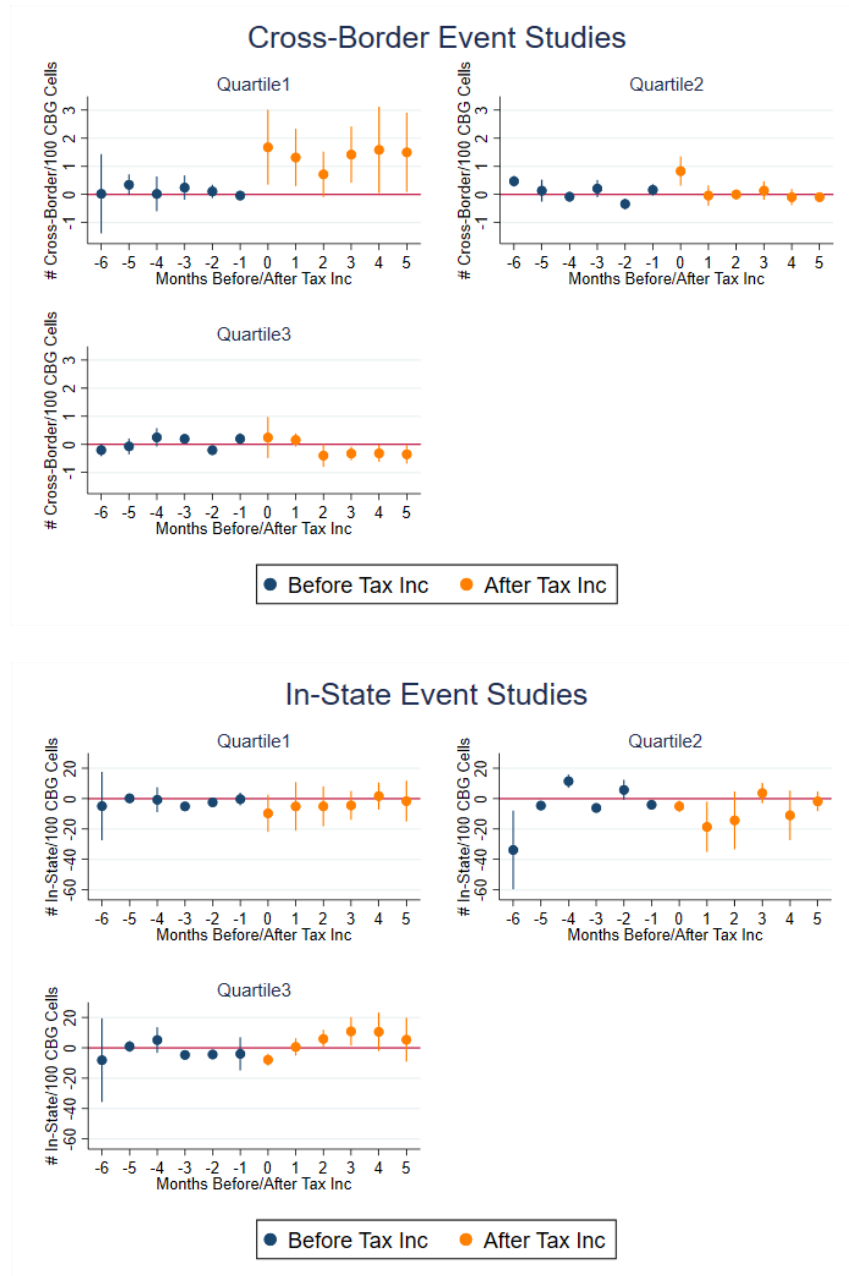
Notes: Bootstrapped confidence intervals are given as spikes on the point estimates presented as dots. Standard errors are clustered on the state-by-quartile of distance to a lower-tax border level. Coefficients are read as per 100 cellphones active in a CBG. The outcome concerns monthly shoppers. "Cross-Border" or "In-State" outcomes are indicated in separate Figure titles. "Tercile 1" are distances closet to a lower-tax border and "Tercile 3" are distances furthest away from a lower-tax border.