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Interacting Effects of State Cigarette Taxes on Smoking Participation

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Abstract

A state cigarette tax increase may deter some residents from smoking, but other residents may avoid the higher tax by purchasing cigarettes from another state. Using U.S. health survey microdata from 1999 to 2012, this paper measures how border-crossing opportunities affect the smoking deterrence achieved by a cigarette tax increase. I estimate by two-way fixed effects regression that a \$1 state cigarette tax increase decreases the smoking rate by an additional 0.58 percentage points for each dollar of cigarette tax in the nearest lower-tax state. However, each successive \$1 tax increase decreases the smoking rate by 0.38 fewer percentage points than the last. I show that the signs of these terms can be theoretically derived without parametric assumptions. I observe that, as both home and nearest lower taxes rose from 1999 to 2012, the mean effectiveness of a home state tax increase remained roughly constant over the period. My results imply that the lowest-tax states are those with the greatest power to reduce the national smoking rate.

Keywords: Cigarette Taxes; Smoking; Tax Avoidance; Border-crossing

JEL Classification: I12, I18, H26, H73

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1 Introduction

From a public health perspective, smoking deterrence is vital. Anti-smoking policies saved an estimated 157 million years of life from 1964 to 2012 (Holford et al., 2014). From an economic perspective, smoking deterrence is socially beneficial because it limits negative externalities such as secondhand smoke and litter. There may also be "internalities" (Herrnstein et al., 1993; Gruber and Koszegi, 2001) associated with smoking, whereby consumers, especially adolescents, are fundamentally unable to assess the full present and future costs of consuming an unhealthy and addictive product. Cigarette taxes are a pervasive method of correcting for these market failures as well as raising public revenue. Every state in the U.S. has collected a cigarette tax since 1970, and there have been over 120 increases in state cigarette taxes in the 21st century.

A common political argument against state cigarette tax increases is that consumers will react by traveling to another state with a lower tax and buying cigarettes there. Many states have outlawed such "casual smuggling" if it exceeds one or two cartons per trip. Larger-scale organized smuggling also arises from interstate tax differences. The Contraband Cigarette Trafficking Act of 1978 made smuggling more than 50 cartons (equivalent to 500 packs, or 10,000 cigarettes) a federal offense with a penalty of up to five years in prison.

The existence of both casual and organized smuggling implies that a consumer's decision to smoke may depend not only on the price of cigarettes in his home state, but the price in another state and the cost of travel. Thus, depending on the attractiveness of border crossing, a tax increase in one state may impact the effectiveness of a tax increase in another state.[1] Measuring these interacting effects of state cigarette taxes on smoking participation is the main purpose of this paper.

I model the decision to smoke as depending on the tax in the home state, the distance to the nearest state with a lower tax, and the value of the nearest lower tax. I use smoking microdata from the Behavioral Risk Factor Surveillance System (BRFSS) and take advantage

[1] In this paper, the "effectiveness" of a cigarette tax increase in either the home state or another state refers to the drop in smoking participation it causes in the home state.

of abundant variation across states in the timing and magnitude of cigarette tax increases from 1999 to 2012. In line with my theoretical predictions, I find that the effectiveness of a home state cigarette tax increase rises with the nearest lower tax, but that, keeping the nearest lower tax constant, successive tax increases diminish in effectiveness. I estimate that these two effects on the effectiveness of a home state tax increase cancel out when the nearest lower tax rises at about two-thirds the rate of the home state tax, which is roughly what occurred at the yearly means over the sample period. Lastly, I show my findings to be robust to various alternative specifications and sample restrictions.

My findings imply that, because it would become a destination for smuggled cigarettes, a state may not be able to acheive its smoking deterrence goals simply by raising its own tax to some prohibitive threshold. On the other side, a state that is a source of smuggled cigarettes exerts a positive externality on proximal states when it raises its cigarette tax. Thus, the power to reduce smoking in the U.S. is especially in the hands of states with low cigarette taxes. Therefore, even if it is certain that cigarette taxes will continue to rise, where they rise will be an important determinant of how much the national smoking rate responds.

2 Literature

While policymakers are interested in cigarette taxation for the purposes of both smoking deterrence and revenue generation, the literature has focused on the latter. A vast set of theoretical and empirical works, rooted in Kanbur and Keen (1993), model the behavior of revenue-maximizing governments that share a crossable border. Leal et al. (2010) provide a survey of this literature.

Models of border crossing rely on the intuitive hypothesis that, all else equal, living closer to a lower-tax jurisdiction increases the likelihood of cigarette smuggling. Testing this hypothesis is complicated by the irregularity of state borders and population distributions. Some authors simplify the problem by focusing on one tax increase in one state. For example, Emery et al. (2002) investigate the extent of tax avoidance in California, while Hanson and Sullivan (2009) measure tax incidence in Wisconsin. Stehr (2005) measures cigarette tax avoidance without explicitly considering border geography by comparing cigarette consumption data from the BRFSS to state sales data and interpreting the difference as coming from cross-border purchases.

Few individual-level surveys provide the geographical identification necessary to estimate models of cross-border cigarette purchases. The Tobacco Use Supplements to the Current Population Survey (TUS-CPS) is the most prominent such survey in the literature (Lovenheim, 2008; Chiou and Muehlegger, 2008; and DeCicca et al., 2013a; 2013b). A key advantage of the TUS-CPS is that it asks the respondent whether their most recent cigarette pack purchase was in a state other than their state of residence. A key limitation is that the finest identified level of geography for most observations in the TUS-CPS is the metropolitan statistical area (MSA), which is generally a group of one or more counties economically linked with an urban core.

Nationwide data with greater detail than the TUS-CPS is rare, but available. Harding et al. (2012) use Nielsen Homescan data from 2006 and 2007 to measure the effects of proximity to lower-tax states on cigarette tax incidence. For consumers across the United States, the Nielsen dataset provides their census tract of residence and the zip codes of the stores at which they shop. This is a vast improvement in geographical identification over the TUS-CPS. In their main specification, the authors interact the log of the distance from the consumer's home state to the closest lower-tax state with the difference in cigarette taxes between them. In an alternative specification, they generalize the marginal effect of distance by using interactions of distance range dummy variables and the tax difference. They find that the consumer burden of a cigarette tax rises with distance to the border, especially for households with annual income greater than $30,000.

While many authors use microdata to estimate price-participation elasticities for smoking (Gallet and List, 2003), few account for cross-border purchases. The first to do so is Lovenheim (2008), who uses the TUS-CPS waves from 1992 to 2002. Lovenheim estimates statistically insignificant price-participation mean elasticities between -0.02 and -0.06, depending on the use of a year trend and accounting for the possibility of the nearest lower-tax jurisdiction being an Indian reservation rather than another state.[2] However, he finds that doubling the distance to a lower-tax state reduces the price-participation elasticity by about 0.2. Thus, he provides evidence that those with a greater opportunity to buy cigarettes across the border are less responsive to tax increases. Reinforcing that casual smuggling is an extensive problem, he estimates that between 13% and 25% of smokers buy cigarettes across a border.

To allow the dependent variable to be interpreted as either total consumption or the smoking rate, the theoretical model in Lovenheim (2008) relies on parametric assumptions for the cigarette demand function and the share of smokers that smuggle. In particular, Lovenheim assumes that increasing the home tax and the nearest lower tax by the same percentage has no effect on the smuggling share, which allows for estimation of the smuggling share as a function of the distance to the nearest lower tax. In this paper, I show that, for smoking participation in particular, the signs of the first- and second-order effects of the home and nearest lower taxes can be derived without parametric assumptions.

Another paper that considers cross-border purchases, though not chiefly, is Callison and Kaestner (2014) (hereafter C&K). Using the TUS-CPS waves from 1995 to 2007 with state and survey wave fixed effects, they estimate that a one dollar increase in the cigarette tax changes the probability that a person smokes by -0.7 percentage points. They estimate this effect to be -1.0 percentage point but statistically insignificant when they restrict the sample to only the 19 states with the largest tax increases over the period, and to be -0.3 percentage points when they use a paired difference-in-difference approach on the restricted sample.

To account for border crossing, C&K interact the home state tax with three dummy variables for intervals of distance to the nearest lower-tax state. They find that the home state tax actually has a stronger negative impact on an individual's probability of smoking when his home MSA is within 120 miles of a lower tax state. C&K do not account for the

[2] Lovenheim obtains similar estimates with and without accounting for Indian reservations. I do not attempt to account for Indian reservations in this paper. In an earlier working paper version (Lovenheim, 2007), he details the many difficulties he had to overcome to do so. In short, states vary widely in their enforcement of tribal sales of cigarettes to non-members, and tribal taxes and/or market power may undermine the assumption that tribes sell cigarettes at the non-tax price.

magnitude of the nearest lower tax. This is problematic because MSAs with higher taxes have a larger range of potential values for the corresponding nearest lower tax. This means that for MSAs with higher taxes, the nearest lower tax will tend to be both larger and closer. Therefore, distance and the nearest lower tax are likely to be negatively correlated. Indeed, in the sample used in this paper, this correlation is -0.22. Thus, if the magnitude of the nearest lower tax does impact the decision to smoke, estimates using only distance will be biased.

To the best of my knowledge, this paper makes three novel contributions to the literature. First, I show that the signs of the first-order and second-order effects of the home tax, nearest lower tax, and distance to the nearest lower tax on smoking participation can be theoretically derived without parametric assumptions. Second, and most importantly, I empirically confirm the theory with the use of a larger, more recent, and more geographically detailed dataset than that used in previous papers. Third, I document a curious observation: due to the mean nearest lower tax rising at about two thirds the rate of the mean home tax over the sample period, the effectiveness of an increase in the home tax at the means of the variables has remained roughly constant over time. At the means in each sample year, my estimates imply that a $1 increase in the home tax deterred 0.8% to 0.9% of the adult population from smoking.

3 Data

I use smoking data from the BRFSS, an annual nationwide health survey. This is not the first paper to use the BRFSS to measure the responsiveness of smoking participation to cigarette taxes, examples being Sloan and Trogdon (2004) and DeCicca and McLeod (2008). However, I am not aware of any that takes advantage of the county-level geographical identification the BRFSS offers. The BRFSS asks respondents if they have smoked 100 cigarettes in their lifetime, and if so, whether they currently smoke every day, some days, or not at all. I define a smoker as a person who smokes every day.

I pool all BRFSS surveys from 1999 to 2012 and exclude respondents that are pregnant or at least 75 years of age.[3] The period from 1999 to 2012 comprises every year following the Tobacco Master Settlement Agreement (TMSA) for which counties are identified in the BRFSS. The TMSA, adopted in November of 1998, fundamentally altered the U.S. tobacco industry by requiring tobacco companies to cease a variety of marketing practices and to make annual payments to fund state smoking-related medical costs and an anti-smoking advocacy group. Therefore I find it appropriate to restrict the sample to the post-TMSA era.

Information on federal and state cigarette taxes comes from *The Tax Burden on Tobacco* (Orzechowski and Walker, 2014), which I organize into a monthly panel. Most cigarette tax changes came into effect on the first of the month. For those that did not, I record the tax change as coming into effect beginning with the following month. Cigarette taxes in the U.S. are levied as a dollar amount per pack of cigarettes. I convert all taxes to January 2015 dollars using the monthly CPI.

The period from 1999 to 2012 contains a wealth of variation in cigarette taxes over states and time. Every year in this period saw multiple states raise their cigarette taxes, and every state except Missouri and North Dakota raised their tax at least once.[4] The highest state cigarette taxes are concentrated in the Northeast, while the lowest are concentrated in the South. As of January 2015, the highest state cigarette tax in the U.S. was \$4.35 per pack in New York and the lowest was \$0.17 in Missouri. The federal cigarette tax rose from \$0.24 to \$0.34 on January 1, 2000, to \$0.39 on January 1, 2002, and to \$1.01 on April 1, 2009.

I also take into account local cigarette taxes in the five counties that compose New York City, in Cuyahoga County, OH, and in Cook County, IL.[5] A few cities in Cook County have their own cigarette taxes, including Chicago, which contains over half the population of Cook

[3]I choose the same age cutoff as C&K. Other papers such as DeCicca and McLeod (2008) restrict the sample to those younger than 65. In Table A1 I show that the results are slightly stronger when those 65 and older are dropped from the sample, consistent with the recent literature showing that older smokers are relatively unresponsive to cigarette tax increases (Ma, 2015; MacLean et al., 2015).

[4]Cigarette taxes only decreased twice in the period, in each instance by 10 cents. The first was the expiration in 2004 of a temporary 10 cent increase in Oregon. The second was in New Hampshire in 2011 and was repealed in 2013 as initally planned.

[5]County-level taxes introduce the possibility that the nearest lower tax may be in the same state. In my preferred specification, I require the nearest lower tax to be in a different state. In Table A1 I show that the results are insensitive to allowing a county in the same state to be the nearest lower-tax jurisdiction. This is reasonable especially because my treatment of NYC is unaffected; the five NYC counties are all closer to a New Jersey county than any non-NYC county in New York.

County. Because I am unable to identify the respondent beyond the county level, I use the mean cigarette tax in Cook County weighted by population. The only other localities in the sample with their own cigarette taxes are in Alabama, Missouri, and Virginia. Each of these three states has many different cigarette taxes at the county and/or city levels, for which I do not account. In the results section, I check that the estimates are robust to the exclusion of these three states.

I measure distances between counties using 2010 centers of population from the U.S. Census. I measure the distance from a county to a state as the distance from the county's center of population to the closest center of population of all counties in that state. I exclude Alaska and Hawaii, leaving 48 states and the District of Columbia. For each month, I exclude the state that has the lowest cigarette tax of all states, as the nearest lower tax and the distance to it are undefined.[6]

Table 1 presents the means of the variables of interest for each survey year. After excluding observations with missing values, the sample contains 3,366,814 observations.[7] The sample period is characterized by an upward trend in real cigarette taxes and a downward trend in smoking. However, Table 1 leaves unclear the relative and interacting roles of home and cross-border taxes in reducing smoking. The remainder of this paper theoretically and empirically untangles the relationships between these variables.

4 Theoretical Model

The following model generalizes the framework of Saba et al. (1995), in which the representative consumer chooses to either pay a higher price for cigarettes in his home state or pay the cost of traveling to a state with a lower price. I allow for consumer heterogeneity and focus on a third option: to not smoke at all. Essentially, the consumer compares his reservation price for the first pack of cigarettes to the price in his home state and to some

[6] In Table A1, I alternatively include the lowest-tax state in each month by defining that state's "nearest lower tax" to be equal to its own tax and the log of distance to be 0. The estimates are practically identical.

[7] Missing values come primarily from non-identified counties, which are especially prominent in the earlier years of the BRFSS. The percentage of the sample for which counties are not identified falls from about 25% in 1999 to 7% in 2012. As a robustness check, I restrict the sample to only those counties that are identified in every year, and show that the results are very similar.

function of the price in the other state and the distance to that state.

The home state is denoted as state 1 and the consumer is a distance d_2 from state 2. p_1 and p_2 are the prices of a pack of cigarettes in the respective states with $p_1 > p_2$. Let $x(p;\vec{\theta})$ be the consumer's demand function for cigarettes, where $\vec{\theta}$ is a vector of preference parameters. Let $p(x;\vec{\theta})$ be the inverse demand function. Define $p^0 = p(0;\vec{\theta})$, which represents the consumer's reservation price for the first pack of cigarettes. The consumer enjoys a surplus of $\int_{p_1}^{p^0} x(p;\vec{\theta})\ dp$ from buying cigarettes in the home state. He prefers buying cigarettes in the home state to buying no cigarettes at all if and only if the surplus is positive, which is true if and only if $p^0 > p_1$.

The consumer enjoys a surplus of $W(p_2, p^0;\vec{\theta}) = \int_{p_2}^{p^0} x(p;\vec{\theta})\ dp$ from buying cigarettes in the other state, but to do so must pay a transport cost $C(d_2)$ that increases with distance. He prefers buying cigarettes in the other state to buying no cigarettes at all if and only if the surplus exceeds the transport cost. Therefore there exists a "critical distance" $d^*(p_2, p^0;\vec{\theta}) = C^{-1}(W(p_2, p^0;\vec{\theta}))$ such that the net surplus $W - C$ is zero. Therefore the consumer prefers buying cigarettes in the other state to buying no cigarettes at all if and only if $d^* > d_2$. There also exists a "critical price" $p^*(d_2, p^0;\vec{\theta}) = W^{-1}(C(d_2), p^0;\vec{\theta})$ such that the consumer prefers buying cigarettes in the other state to buying no cigarettes at all if and only if $p^* > p_2$.

Consider $\vec{\theta}$ to be a vector of random variables, so that p^0 and p^* are dependent random variables with joint probability density function (pdf) $f(p^0, p^*; d_2)$ for a given value of d_2. Assume f is continuously differentiable and note that f is non-negative. Let S equal one if the agent smokes and zero otherwise. The probability that the agent smokes is the probability that at least one of the two states offers him a net surplus from purchasing cigarettes there:

$$\begin{aligned} P(S=1) = P(p^0 > p_1 \cup p^* > p_2) &= 1 - P(p^0 < p_1 \cap p^* < p_2) \\ &= 1 - \int_0^{p_1} \int_0^{p_2} f(p^0, p^*; d_2)\ dp^*\ dp^0 \end{aligned} \tag{1}$$

Differentiating shows how the probability of smoking and the effectiveness of price increases change with the prices in the two states:

$$\frac{\partial P(S=1)}{\partial p_1} = -\int_0^{p_2} f(p_1, p^*; d_2)\ dp^* \leq 0 \tag{2}$$

$$\frac{\partial P(S=1)}{\partial p_2} = -\int_0^{p_1} f(p^0, p_2; d_2)\ dp^0 \leq 0 \tag{3}$$

$$\frac{\partial^2 P(S=1)}{\partial p_1 \partial p_2} = -f(p_1, p_2; d_2) \leq 0 \tag{4}$$

$$\frac{\partial^2 P(S=1)}{\partial {p_1}^2} = -\int_0^{p_2} \frac{\partial f}{\partial p_1}(p_1, p^*; d_2)\ dp^* \tag{5}$$

$$\frac{\partial^2 P(S=1)}{\partial {p_2}^2} = -\int_0^{p_1} \frac{\partial f}{\partial p_2}(p^0, p_2; d_2)\ dp^0 \tag{6}$$

Equations 2 and 3 quantify the drop in the probability of smoking due to increases in the home price and cross-border price, respectively. Equation 4 quantifies the effect of an increase in one state's price on the effectiveness of an increase in the other state's price. The cross-partial derivative in Equation 4 is non-positive, implying that, if anything, an increase in the home state price is made more effective by a higher price in the other state. Intuitively, this is because a higher price in the other state makes it less likely that the consumer will switch to buying there in response to an increase in the home price, and thus makes it more likely that he will quit smoking instead.

The signs of the pure partial derivatives in Equations 5 and 6 can be determined under the conditions described by the following proposition:

Proposition 1. *Suppose, for some finite m and possibly infinite n, that $f(p)$ is a non-negative continuously differentiable function such that $\frac{\partial f}{\partial p}(p) \neq 0\ \forall p \in (m, n)$ and $\lim_{p \to n} f(p) = 0$. Then $\frac{\partial f}{\partial p}(p) < 0\ \forall p \in (m, n)$.*

Proof. Because $\lim_{p \to n} f(p) = 0$, there exists $y \in (m, n)$ such that $0 \leq f(y) \leq f(m)$. By the mean value theorem, there exists $a \in (m, y)$ such that $\frac{\partial f}{\partial p}(a) = \frac{f(y) - f(m)}{y - m} \leq 0$. Suppose that $\frac{\partial f}{\partial p}(b) \geq 0$ for some $b \in (m, n)$. By the intermediate value theorem, there exists c in the interval bounded by a and b such that $\frac{\partial f}{\partial p}(c) = 0$. But $c \in (m, n)$, a contradiction. Therefore

$\frac{\partial f}{\partial p}(p) < 0$ for all $p \in (m, n)$. □

To sign Equation 5, note that $f(p_1, p^*; d_2)$ is a non-negative continuously differentiable function of p_1, and that, by nature of being a pdf, $lim_{p_1 \to \infty} f(p_1, p^*; d_2) = 0$. Then, if $\frac{\partial f}{\partial p_1}(p_1, p^*; d_2)$ is not zero for all p_1 greater than some m, it must be negative for $p_1 > m$.[8] $\frac{\partial f}{\partial p_1}(p_1, p^*; d_2)$ being non-positive $\forall p^* \in (0, p_2)$ and negative on some subinterval means that both sides of Equation 5 are positive. Therefore, at a high enough home price, successive increases in the home price will diminish in effectiveness.

To sign Equation 6, note that $f(p^0, p_2; d_2)$ is a non-negative continuously differentiable function of p_2, and that, because the consumer is never willing to pay as much for cigarettes that require travel than ones that don't, $lim_{p_2 \to p^0} f(p^0, p_2; d_2) = 0$. Then, if $\frac{\partial f}{\partial p_2}(p^0, p_2; d_2)$ is not zero over some interval bounded above by p^0, it must be negative on that interval. $\frac{\partial f}{\partial p_2}(p^0, p_2; d_2)$ being non-positive $\forall p^0 \in (0, p_1)$ and negative on some subinterval means that both sides of Equation 6 are positive. This means that, at a high enough price in the other state, successive increases in that price will cause smaller and smaller drops in smoking in the home state.

The above logic merits some discussion. Proposition 1 states the conditions for a point of diminishing effectiveness, past which a function, such as a pdf, must decrease to zero. Up to that point, the second derivative with respect to a tax cannot be determined. This is because there may be disproportionately dense regions, or "spikes", in the pdf around certain reservation prices. Suppose, for illustration, that there happens to be a large share of people that would quit smoking if and only if the home price exceeded \$6.50. Raising the price from \$5 to \$6 is not very effective, but raising the price from \$6 to \$7 is super effective. Then, from \$5 to \$7, the second derivative of the probability of smoking with respect to the tax is negative. Proposition 1 ensures that, unless f is pathological, spikes in f must eventually be exhausted as the price increases. Therefore, at a high enough price, the second derivative cannot be negative.

[8]What if there is no such m? Then either f has zero density for all $p_1 > m$, or the derivative of f changes sign an infinite number of times. In the first case the derivative is zero. For an example of the second case, consider $f(p) = \frac{2sin^2(p)}{\pi p^2}$.

To derive the marginal effects of distance, recall that the probability of smoking can be stated in terms of the critical distance rather than the critical price:

$$\begin{aligned} P(S=1) = P(p^0 > p_1 \cup d^* > d_2) &= 1 - P(p^0 < p_1 \cap d^* < d_2) \\ &= 1 - \int_0^{p_1} \int_0^{d_2} g(p^0, d^*; p_2)\ dd^*\ dp^0 \end{aligned} \tag{7}$$

where $g(p^0, d^*; p_2)$, assumed to be continuously differentiable, is the pdf of p^0 and d^* for a given value of p_2. This makes it simple to show how the probability of smoking and the effectiveness of price increases change with distance:

$$\frac{\partial P(S=1)}{\partial d_2} = -\int_0^{p_1} g(p^0, d_2; p_2)\ dp^0 \leq 0 \tag{8}$$

$$\frac{\partial^2 P(S=1)}{\partial d_2 \partial p_1} = -g(p_1, d_2; p_2) \leq 0 \tag{9}$$

$$\frac{\partial^2 P(S=1)}{\partial d_2 \partial p_2} = -\int_0^{p_1} \frac{\partial g}{\partial p_2}(p^0, d_2; p_2)\ dp^0 \tag{10}$$

$$\frac{\partial^2 P(S=1)}{\partial {d_2}^2} = -\int_0^{p_1} \frac{\partial g}{\partial d_2}(p^0, d_2; p_2)\ dp^0 \tag{11}$$

Equation 8 quantifies the drop in the probability of smoking due to an increase in distance. Equation 9 quantifies the effect of an increase in distance on the effectiveness of an increase in the home price. The cross-partial derivative in Equation 9 is non-positive, implying that, if anything, an increase in the home state price is made more effective by a greater distance to the other state. Intuitively, this is because a greater distance to the other state makes it less likely that the consumer will switch to buying there in response to an increase in the home price, and thus makes it more likely that he will quit smoking instead.

To sign Equation 10, recall Proposition 1. Note that $g(p^0, d_2; p_2)$ is a non-negative continuously differentiable function of p_2, and that, because the consumer is never willing to travel to pay the maximum price he is willing to pay at home, $lim_{p_2 \to p^0} g(p^0, d_2; p_2) = 0$. Then, as long as $\frac{\partial g(p^0, d_2; p_2)}{\partial p_2}$ is not zero over some interval bounded above by p^0, it must be negative on that interval. $\frac{\partial g}{\partial p_2}(p^0, d_2; p_2)$ being non-positive $\forall p^0 \in (0, p_1)$ and negative on some subinterval means that both sides of Equation 10 are positive. Therefore, for a high enough price in the other state, an increase in that price has a smaller effect when it is farther away.

To sign Equation 11, note that $g(p^0, d_2; p_2)$ is a non-negative continuously differentiable function of d_2, and that, by nature of being a pdf, $lim_{d_2 \to \infty} g(p^0, d_2; p_2) = 0$. Then, as long as $\frac{\partial g}{\partial d_2}(p^0, d_2; p_2)$ is not zero for all d_2 greater than some m, it must be negative for $d_2 > m$. $\frac{\partial g}{\partial d_2}(p^0, d_2; p_2)$ being non-positive $\forall p^0 \in (0, p_1)$ and negative on some subinterval means that both sides of Equation 11 are positive. Therefore, for a far enough distance, successive increases in distance will have smaller and smaller effects on the probability of smoking.

To relate the entire discussion of prices to taxes, the only necessary assumption is that prices and taxes are positively related. As long as pass-through rates are entirely positive, all of the derivatives with respect to taxes have the same signs as the corresponding derivatives with respect to prices, even if pass-through rates vary across states or time. Thus the theory presented here guides an examination of the reduced form relationship between taxes and smoking participation. From a public policy perspective, this is not a disadvantage, since policymakers are generally unable to skip the intermediate causal steps between these two variables.

5 Methods

5.1 Econometric Model

Following DeCicca and McLeod (2008), I estimate linear probability models with two-way fixed effects. Initially, I regress an indicator for being a smoker on the cigarette tax in the respondent's county and a set of controls:

$$P(S_{icst} = 1) = \beta_0 T_{ct} + \vec{\alpha} \cdot \vec{X}_i + \zeta u_{st} + \delta_s + \gamma_t + \epsilon_{icst} \tag{12}$$

S_{icst} is equal to one if and only if respondent i in county c of state s at survey month t smokes every day and is otherwise equal to zero. T_{ct} is the sum of federal, state, and county cigarette taxes in county c at the start of month t in January 2001 dollars, centered at the sample mean. $\vec{X}_i$ is a set of individual characteristics comprised of dummy variables for marital status, employment status, education, sex, race, one, two, or three or more children in the household, and categories for income and age.[9] u_{st} is the unemployment rate in state s and month t and δ_s and γ_t are state and month-year fixed effects, respectively. β_0, the estimated effect of raising the tax by a dollar on an individual's probability of smoking, is the coefficient of interest. I expect β_0 to be negative by the law of demand. Alternatively, I allow the marginal effect to vary with the value of the tax by adding the square of the tax on the right hand side.

To take cross-border purchases into account, I include the cigarette tax in the nearest state to the respondent's county with a lower cigarette tax, which I denote T'_{ct}, the log of the distance in miles to that state, which I denote d_{ct}, and all second-order terms involving these two variables and the home state tax. I center the nearest lower tax and log of distance at their sample means.[10]

$$\begin{aligned} P(S_{icst}=1) &= \beta_0 T_{ct} + \beta_1 T'_{ct} + \beta_2 d_{ct} + \beta_3 (T_{ct} \times T'_{ct}) + \beta_4 (T_{ct} \times d_{ct}) \\ &+ \beta_5 (T'_{ct} \times d_{ct}) + \beta_6 T_{ct}^2 + \beta_7 T'^2_{ct} + \beta_8 d_{ct}^2 + \vec{\alpha} \cdot \vec{X}_i + \zeta u_s + \delta_s + \gamma_t + \epsilon_{icst} \end{aligned} \tag{13}$$

By Equations 4 and 9, I expect $\beta_3, \beta_4 < 0$. By Equations 2, 3, and 8, I also expect that the marginal effects of taxes and distance are also non-positive at all reasonable values of T, T', and d:

$$\frac{\partial P(S=1)}{\partial T_{ct}} = \beta_0 + \beta_3 \times T'_{ct} + \beta_4 \times d_{ct} + 2\beta_6 \times T_{ct} \leq 0 \tag{14}$$

$$\frac{\partial P(S=1)}{\partial T'_{ct}} = \beta_1 + \beta_3 \times T_{ct} + \beta_5 \times d_{ct} + 2\beta_7 \times T'_{ct} \leq 0 \tag{15}$$

$$\frac{\partial P(S=1)}{\partial d_{ct}} = \beta_2 + \beta_4 \times T_{ct} + \beta_5 \times T'_{ct} + 2\beta_8 \times d_{ct} \leq 0 \tag{16}$$

While Proposition 1 predicts that $\beta_5, \beta_6, \beta_7$ and β_8 are positive for large enough values of

[9] I include respondents that refused to answer, and, in the case of income, didn't know or were not sure (DK/NS). As shown in Table A2, I assign dummy variables to refusal and DK/NS entries as if they were simply another possible response outcome. In Table A1 I show that excluding all respondents with at least one refusal or DK/NS entry (about 13% of the sample) does not affect the results.

[10] Because centering only subtracts a constant (the sample mean) from each variable, it has no impact on the estimated marginal effects or their standard errors. However, it does allow the coefficients on the first-order terms to be interpreted as the marginal effects at the means of the variables.

T, T', and d, whether the values in the sample are "large enough" is unknown. A negative estimate does not contradict the theory, and a positive estimate raises the possibility, but does not prove, that the point of diminishing effectiveness has been reached.

5.2 Identification

The two-way fixed effects model is identified by variation in cigarette taxes across both states and months. That is, the model relies on the fact that states raised their taxes by different amounts and at different times. State fixed effects control for all time-invariant state characteristics, which is important because states have inherent differences that affect both smoking rates and tax policies.[11] Differences in cultural attitudes toward smoking, for example, may cause cigarette taxes and smoking rates to be negatively correlated independent of the effect of taxes on quantity demanded. However, time-varying characteristics such as economic conditions may also affect both smoking rates (Ruhm, 2005; Xu, 2013; Kenkel et al., 2014) and state tax policies (Maag and Merriman, 2003). In particular, a economic downturn at the state level may simultaneously reduce the demand for cigarettes and motivate the state government to use a cigarette tax increase to combat budgetary concerns. I control for the state unemployment rate in the survey month to address this issue. In Table A1, I show that the results are largely robust to not controlling for the unemployment rate.

While DeCicca and McLeod (2008) maintain that state tax increases were motivated by budget shortfalls that are uncorrelated with smoking rates, other plausible motivations are similarly compatible with the identification strategy. Golden et al. (2014) consider a vast suite of explanatory variables and find that cigarette tax increases are most strongly associated with the political alignment of state governments. After the TMSA, Republican-controlled governments were less likely to raise cigarette taxes than mixed-party or Democrat-controlled governments. As the political alignment in a given state over time is unlikely to be correlated with the smoking rate, this explanation also suggests that the model is appropriate.

[11] A natural issue with using fixed effects is multicollinearity with the variables of interest. To test the level of multicollinearity, I regress each of the variables of interest on the other two in addition to state and month fixed effects. For the home tax, nearest lower tax, and log of distance, this results in variance inflation factors of 8.5, 6.29, and 1.21, respectively. Values below 10 are commonly considered acceptable.

Ultimately, though, I cannot rule out the possibility that one or more omitted variables are correlated with both state cigarette taxes and smoking rates. One step to dealing with this problem is to include state-specific linear time trends. This amounts to adding $\delta_s \times t$, the interaction between the state fixed effect and the month-year of the survey, to the right-hand side of Equation 13. Without this interaction term, I assume that unobserved differences across states are time-invariant, hence the state effects being "fixed." By including linear trends, I generalize the state effects to be a linear function of time.

6 Results

In the following tables of estimates, the estimated coefficients $\hat{\beta}_0$ through $\hat{\beta}_8$ are listed in order. Estimated coefficients on the controls for the preferred specification are provided in Table A2. I multiply all of the estimated coefficients by 100 to condense the tables and provide an intuitive interpretation of the estimates in terms of percentage points. Consistent with the previous literature, standard errors are clustered at the state level and observations are weighted by BRFSS sample weights. The results are not sensitive to the use of weights.

6.1 Main Results

Table 2 reports the main results. The first column estimates Equation 12, which ignores border crossing. I estimate that each $1 increase in the cigarette tax per pack decreases a person's probability of smoking by about 0.74 percentage points. Based on a recent CDC report that 13.7% of Americans smoke every day (Jamal et al., 2014), this translates to a 5.4% decrease in smoking participation per dollar. Based on a November 2014 average after-tax retail price of $5.84 for a pack of cigarettes (Orzechowski and Walker, 2014) and a pass-through rate of 1.11 (Keeler et al., 1996), this translates to a price-participation elasticity of -0.28, reasonably consistent with surveys of the literature (Chaloupka and Warner, 2000; Gallet and List, 2003).

Columns 2 and 3 respectively show that adding a quadratic term or controlling linearly for the costs of border crossing does not reveal any new conclusive results. Column 2 shows that, without accounting for border crossing, the effect of a home tax increase on the probability of smoking does not appear to vary much with the level of the tax. The quadratic term is small, negative, and statistically insignificant. Column 3 introduces the nearest lower tax and the log of the distance to that tax on the right hand side. The coefficient on the home tax is very similar to that in column 1. While they are not statistically significant at conventional levels, the estimated effects of the nearest lower tax and the log of distance are negative as predicted by Equations 3 and 8.

Column 4 reports estimates of Equation 13, my preferred specification. In contrast to columns 2 and 3, including a full set of interaction terms reveals significant implications of border crossing. As predicted by Equation 4, $\hat{\beta}_3$ is negative and significant at the 1% level, indicating that a tax increase is more effective the higher the tax in the lower-tax state. The point estimate implies that each dollar of the nearest lower tax causes a \$1 increase in the home state tax to decrease the probability of smoking by an additional 0.58 percentage points. $\hat{\beta}_6$ is positive and significant, indicating, in contrast to the result in column 2, that each successive increase in the home state tax causes a smaller drop in smoking than the last. $\hat{\beta}_7$ is also positive and significant, indicating that each successive increase in the nearest lower tax also causes a smaller drop in smoking than the last. Thus I report two main findings: 1) cigarette tax increases in the home state and the nearest lower-tax state complement each other as deterrents to smoking in the home state, and 2) the deterrence achieved by an increase in only one tax or the other diminishes as the tax rises.

$\hat{\beta}_4$ and $\hat{\beta}_5$ are not statistically significant and $\hat{\beta}_8$ is weakly significant. Therefore I do not observe compelling evidence of second-order effects involving distance. For the remainder of the paper, I focus on the implications and robustness of the second-order effects that do not involve distance.

$\hat{\beta}_0$, $\hat{\beta}_1$, and $\hat{\beta}_2$ in the preferred specification are the estimated marginal effects at the means of the variables over all years of the sample. For example, $\hat{\beta}_0$ implies that, at the means of the variables, a \$1 increase in the home tax decreases the smoking rate by 0.887

percentage points, which implies a price-participation elasticity of -0.34. However, the means of the tax variables generally increased over time as shown in Table 1, and so the estimated marginal effects may have changed as well. In Table 3, I use the year-specific means from Table 1, my preferred estimates from column 4 of Table 2, and Equations 14-16 to estimate marginal effects of increases in the home tax, nearest lower tax, and log of distance for each year of the sample. The values are all negative, as expected.

Home tax increases were remarkably stable in mean effectiveness over the period, only falling slightly after the federal tax increase in 2009. At each year's mean values, an increase in the home tax has a strongly significant negative effect on the probability of smoking, and the implied elasticities all fall between -0.315 and -0.345. These values are in accordance with the general cigarette tax literature (Chaloupka and Warner, 2000; Gallet and List, 2003), but are larger than the statistically insignificant elasticities reported by Lovenheim (2008). Many methodological differences could account for the differences in estimated elasticities at the means. Lovenheim uses an earlier dataset, restricts the sample to residents of MSAs, aggregates the data at the MSA level, uses a linear time trend rather than fixed effects, and uses a partial set of second-order terms justified by his stronger theoretical assumptions. However, the primary message of this paper is that cross-border purchasing opportunities impair the effectiveness of home tax increases. From this broader perspective, my results agree with those of Lovenheim. In any case, more interesting than the magnitudes of the mean elasticities is their apparent stability over time.

All else equal, $\beta_6 > 0$ implies that home tax increases would have diminished in effectiveness as they rose from 1999 to 2012. However, this was counteracted by the fact that cross-border taxes rose as well. From the first column of Table 3 it appears that these two effects approximately canceled out. I am not aware of a theoretical reason why the mean effectiveness of an increase in the home state tax should have necessarily been so stable as state taxes rose over time. Whether such stability persists will depend on the geographic pattern of future tax increases. Generally speaking, if the U.S. becomes more of a checkerboard of high and low taxes, home taxes will become less effective. If, on the other hand, taxes converge across states, home tax increases will become more effective. To quantify this, recall Equation 14, assume distance does not change, difference both sides, and insert the preferred estimates. The result is:

$$\Delta\left(\frac{\partial P(S=1)}{\partial T_{ct}}\right) = -0.577 \times \Delta T'_{ct} + 0.384 \times \Delta T_{ct} \tag{17}$$

Then the effectiveness of a home tax increase is unchanged if $\frac{\Delta T'_{ct}}{\Delta T_{ct}} = \frac{0.384}{0.577} = 0.666$. Therefore, the point estimates imply that successive home tax increases do not decrease in effectiveness as long as each increase in the home tax is accompanied by an increase in the nearest lower tax of two-thirds the size.

Like the home tax, point estimates of the marginal effects of the nearest lower tax and the log of distance are quite stable at the means from year to year. However, they are not as strongly significant. In particular, the nearest lower tax has a small and statistically insignificant effect at the means in each year. This suggests that, for many counties, taxes have become high enough that an increase in the nearest lower tax alone will not decrease the smoking rate. For two reasons, this does not imply that cross-border taxes do not matter. First, these are only mean effects. If the home tax is high enough above the mean and nearest lower tax is low enough below the mean, the effect of an increase in the nearest lower tax will be significant. Second, the main result of the paper still holds. Even if an increase in the nearest lower tax does not by itself have a significant effect on the home state smoking rate, it does have a significant effect when it accompanies an increase in the home tax.

6.2 Robustness Checks

Table 4 reports the results of various modifications to Equation 13, while Table 5 reports the results of various sample restrictions. My preferred estimates from column 4 of Table 2 are copied into the first column of both tables for comparison.

Column 2 of Table 4 replaces the state fixed effects with county fixed effects. In using state fixed effects, I assume that all unobserved characteristics that are correlated with the left- and right-hand side variables do not vary below the state level. Thus my preferred specification is vulnerable to unobserved differences across areas within the same state. For example, the northern part of a state may have different cultural, political, or personal health attitudes than the southern part. In column 2, I account for such differences at the county level, the finest identified level of geography in the BRFSS. The estimates are very similar to the baseline, confirming that the results are not driven by unobserved time-invariant differences across counties.

Column 3 includes state-specific linear trends in the probability of smoking. If linear changes in state characteristics over time are correlated with both cigarette taxes and smoking rates, then adding the trend term would affect the estimates. However, column 3 is very similar to the baseline, supporting the validity of the fixed effects model.

Column 4 removes federal taxes from the home and nearest lower taxes. In a linear specification, this would have no effect on the estimates because the federal tax is the same for all states at a given point in time, and is thus absorbed by the month fixed effects. However, because the results rely on the coefficients on the second-order terms, they may be sensitive to the exclusion of federal taxes. Column 4 confirms that the estimates are very similar whether or not federal taxes are excluded.

Column 5 adds three binary controls for state bans on indoor smoking. If such bans decrease smoking rates (Evans et al., 1999) and state governments that are more likely to enact them are also more likely to enact tax increases, the preferred estimates may confound the effects of taxes with the effects of bans. To address this, I use data on the effective dates of smoking ban legislation from the American Nonsmokers' Rights Foundation.[12] 34 states and the District of Columbia enacted bans on smoking in restaurants, bars, and/or non-hospitality workplaces over the sample period. I control separately for the legality of smoking in each of these three types of venues. Column 5 shows that doing so has practically no effect on the estimates.

Column 6 expands the definition of a smoker to include those who smoke some days as

[12] The data is available at http://www.no-smoke.org/pdf/EffectivePopulationList.pdf. I do not control for the thousands of local-level indoor smoking bans in the U.S.

opposed to only those who smoke every day. Roughly 5% of respondents reported smoking some days but not every day in each year of the sample.[13] Including some-day smokers checks that the results are not driven by people who transition from everyday smoking to some-day smoking in response to tax increases. If this was the case, then β_3, β_6, and β_7 would be smaller in absolute value in column 6 than in the baseline. I show that this is not the case, as these estimates are all larger in absolute value in column 6. Therefore the results appear to be driven by people who are deciding not to smoke at all, and who are thus fully capturing the intended individual benefits of cigarette taxes.

Table 5 reports the results of various sample restrictions. Column 2 excludes the West census region, which is characterized by large counties that may exacerbate the measurement error in the county-based distance calculations. Column 3 excludes the four states that share a border with Mexico: California, Arizona, New Mexico, and Texas. Cigarette smuggling from Mexico, as shown by Connelly et al. (2009), is evidence that the nearest lower tax and distance measures may overstate the costs of border crossing for those counties close to the Mexican border.[14] The estimates in columns 2 and 3 are muted compared to the baseline, but are qualitatively similar and remain statistically significant. This indicates that the results are not driven by large counties in the West or smuggling from Mexico.

Column 4 excludes Alabama, Missouri, and Virginia from the sample. These three states are characterized by many different cigarette taxes at the county and city level that I do not account for in this paper. Excluding them has practically no effect on the estimates.[15]

In column 5, I restrict the sample to those counties that are observed in every year from 1999 to 2012, which removes about 27% of the observations.[16] County identification in the BRFSS improved over the sample period, so that some counties are only observed in the later years. By removing those counties that are absent in one or more years, I check that

[13]The annual share of some-day smokers was highest in 2001 at 5.9% and lowest in 2010 at 4.7%, with no clear pattern over time.

[14]Connelly et al. (2009) find no association between state cigarette sales and sharing a border with Canada. This is sensible as cigarette taxes in Canada are higher than those in the U.S.

[15]The number of observations only falls by about 115,000. This is partly because Virginia was the lowest-tax state for the first 68 months of the sample and Missouri was the lowest-tax state for the last 30 months, so those states were already excluded in those periods, respectively.

[16]Each county-year with a positive number of observations has at least 21 observations.

the results are not affected by changes in the composition of the sample over time. Indeed, the estimates in column 5 are very similar to the baseline.

In sum, my results are robust to many alternative methods. The estimated coefficients on the square of the home tax, the square of the nearest lower tax, and their interaction, which form my main results, all remain statistically significant at conventional levels throughout. The estimates on the square of the nearest lower tax and the interaction term are especially robust, being significant at 1% in most specifications. Thus I provide evidence that border crossing can limit a state's ability to use taxes to deter its residents from smoking.

7 Conclusion

The existence of both casual and organized cigarette smuggling in the United States suggests that a consumer's decision to smoke does not only depend on the tax in his own state. In particular, it implies an interacting effect between state taxes, such that a tax increase is more effective when smuggling is less attractive. Intuitively, the marginal home state cigarette consumer, when faced with a home state tax increase, will either switch to buying cigarettes from a lower-tax state or quit smoking. The higher the tax in the lower-tax state, the smaller the surplus from buying there, and thus the more likely he is to quit instead.

Using BRFSS data from 1999 to 2012, I estimate that a $1 increase in a state's cigarette tax reduces a resident's likelihood of smoking by an additional 0.58 percentage points for every dollar of the nearest lower tax. This is a large impact: at the sample means, about a $1.50 increase in the nearest lower tax doubles the effectiveness of an increase in the home state tax. Such an increase in the nearest lower tax is not unheard of; the mean nearest lower tax in the sample rose by $1.23 (in January 2015 dollars) from 1999 to 2012.

I also find that successive tax increases, whether in the home state or the nearest lower-tax state, cause smaller and smaller drops in the home state smoking rate if the other tax does not change. I estimate that, especially after the federal cigarette tax increase in 2009, nearest lower taxes were high enough that an increase in the nearest lower tax alone would not decrease the mean survey respondent's probability of smoking. Home state tax increases, on the other hand, did not decrease in effectiveness over time at the yearly means. I estimate that maintaining the effectiveness of home state tax increases requires the nearest lower tax to rise at two-thirds the rate of the home tax.

My results suggest that one state's decision to raise its tax may make it worthwhile for a neighboring state to do the same. For example, state legislatures in Ohio and West Virginia have each considered $1 cigarette tax increases in 2015. Based on my estimates, a $1 tax increase in West Virginia would cause 0.58% of the adult population (or roughly 3.5% of everyday smokers) in eastern Ohio to respond to a $1 Ohio tax increase by quitting smoking instead of buying cigarettes from West Virginia. Ohio would lose no revenue from this group's decision to quit, since the group's alternative to quitting would be to buy across the border. In addition, some Ohio smokers who were already buying cigarettes in West Virginia may switch to buying in Ohio if the West Virginia tax rises. Thus a tax increase in West Virginia would be a gain for Ohio in terms of both revenue generation and smoking deterrence. This example also highlights that a cigarette tax increase will have differential effects across counties within the same state. For a given home state tax increase, those counties near a state with very low taxes will not experience as great a drop in smoking as those for which border crossing is not as attractive an option.

Thus interstate politics are entangled in the major public health hazard that is smoking. While smoking rates have fallen over the past decades, industry influence, innovations such as electronic cigarettes, and strong preferences of those who still smoke mean the issue will not go away quietly. States that wish to further curb smoking through higher taxes are to some extent at the mercy of the states that surround them. Greater disincentives to smoking on the part of either the most smoking-friendly states or the federal government may be required for smoking rates to continue to fall.

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Table 1: Means of Variables

Year	Everyday Smoker	Home Tax	Nearest Lower Tax	Log(Distance)	N
1999	0.191	0.914	0.679	4.365	101,276
2000	0.187	1.059	0.819	4.350	117,786
2001	0.184	1.029	0.792	4.345	137,294
2002	0.185	1.218	0.893	4.337	165,580
2003	0.177	1.484	1.081	4.312	181,605
2004	0.163	1.513	1.076	4.335	219,549
2005	0.159	1.709	1.180	4.307	252,032
2006	0.154	1.699	1.168	4.267	238,523
2007	0.156	1.656	1.134	4.414	315,441
2008	0.141	1.786	1.241	4.334	300,636
2009	0.134	2.349	1.788	4.333	310,872
2010	0.130	2.683	1.988	4.354	326,121
2011	0.138	2.585	1.938	4.360	364,551
2012	0.132	2.580	1.913	4.342	335,548
Full Sample	0.152	1.922	1.400	4.341	3,366,814

Home and nearest lower taxes are the sum of federal, state, and county cigarette taxes in the respondent's county measured in January 2015 dollars. Distance is the number of miles to the nearest county in a lower-tax state.

Table 2: Estimated Effects of Cigarette Taxes on Smoking Participation

	(1)	(2)	(3)	(4)
Home Tax	-0.741***	-0.669***	-0.702***	-0.887***
	(0.187)	(0.240)	(0.178)	(0.247)
Lower Tax			-0.202	-0.129
			(0.142)	(0.228)
Log(Distance)			-0.084	-0.183*
			(0.136)	(0.096)
Home Tax × Lower Tax				-0.577***
				(0.188)
Home Tax × Log(Distance)				-0.004
				(0.089)
Lower Tax × Log(Distance)				0.187
				(0.150)
$(\text{Home Tax})^2$		-0.020		0.192**
		(0.034)		(0.080)
$(\text{Lower Tax})^2$				0.589***
				(0.137)
$(\text{Log(dist)})^2$				-0.155*
				(0.091)
N	3,366,814	3,366,814	3,366,814	3,366,814

The dependent variable in all regressions is an indicator equal to one only if the respondent smokes every day. Tax and distance variables are centered at their sample means and all reported coefficients are multiplied by 100 to aid the presentation of the results. All regressions include state fixed effects, month-year fixed effects, the state unemployment rate in the survey month, and individual-level controls for marital status, employment status, education, sex, race, the presence of one, two, or three or more children in the household, income category, and age category. Taxes are in January 2015 dollars and distance is in miles. All regressions are weighted by BRFSS sample weights and all standard errors are clustered by state.
*: Significant at 10%. **: Significant at 5%. ***: Significant at 1%.

Table 3: Marginal Effects at Year-Specific Means

Year	Home Tax	Lower Tax	Log(Distance)
1999	-0.905***	-0.188	-0.219**
	(0.309)	(0.305)	(0.104)
2000	-0.901***	-0.197	-0.211**
	(0.307)	(0.306)	(0.104)
2001	-0.900***	-0.204	-0.210**
	(0.308)	(0.310)	(0.104)
2002	-0.879***	-0.206	-0.211**
	(0.289)	(0.285)	(0.101)
2003	-0.881***	-0.142	-0.208**
	(0.270)	(0.242)	(0.101)
2004	-0.901***	-0.082	-0.208**
	(0.268)	(0.223)	(0.105)
2005	-0.898***	-0.067	-0.202*
	(0.260)	(0.209)	(0.108)
2006	-0.894***	-0.072	-0.200*
	(0.257)	(0.208)	(0.108)
2007	-0.881***	-0.080	-0.178
	(0.246)	(0.198)	(0.109)
2008	-0.886***	-0.052	-0.178
	(0.241)	(0.186)	(0.115)
2009	-0.876***	-0.055	-0.170
	(0.233)	(0.178)	(0.118)
2010	-0.839***	-0.093	-0.166
	(0.212)	(0.167)	(0.124)
2011	-0.830***	-0.106	-0.173
	(0.209)	(0.168)	(0.125)
2012	-0.838***	-0.096	-0.164
	(0.212)	(0.168)	(0.123)

Each value is calculated using the year-specific means from Table 1, the estimates in column 4 of Table 2, and Equations 14-16. Standard errors are calculated using the delta method.
*: Significant at 10%. **: Significant at 5%. ***: Significant at 1%.

Table 4: Robustness Checks: Alternative Specifications

	(1) Baseline	(2) County FEs	(3) State Trends	(4) No Fed. Taxes	(5) Control for Smoking Bans	(6) Some-day Smokers
Home Tax	-0.887***	-0.671**	-0.668***	-0.900***	-0.871***	-0.596**
	(0.247)	(0.322)	(0.246)	(0.274)	(0.233)	(0.254)
Lower Tax	-0.129	-0.128	-0.282	-0.089	-0.165	-0.187
	(0.228)	(0.303)	(0.218)	(0.232)	(0.234)	(0.248)
Log(Distance)	-0.183*	-0.057	-0.256***	-0.186*	-0.173*	-0.098
	(0.096)	(0.073)	(0.088)	(0.101)	(0.096)	(0.090)
Home Tax × Lower Tax	-0.577***	-0.501**	-0.570***	-0.501***	-0.555***	-0.944***
	(0.188)	(0.223)	(0.164)	(0.154)	(0.177)	(0.222)
Home Tax × Log(Distance)	-0.004	-0.044	0.015	0.019	0.007	0.117
	(0.089)	(0.086)	(0.090)	(0.088)	(0.084)	(0.115)
Lower Tax × Log(Distance)	0.187	0.165	0.075	0.156	0.171	-0.123
	(0.150)	(0.171)	(0.183)	(0.200)	(0.144)	(0.196)
$(\text{Home Tax})^2$	0.192**	0.200*	0.119*	0.176**	0.186**	0.238***
	(0.080)	(0.101)	(0.069)	(0.082)	(0.075)	(0.076)
$(\text{Lower Tax})^2$	0.589***	0.417**	0.510***	0.525***	0.577***	0.848***
	(0.137)	(0.170)	(0.133)	(0.166)	(0.129)	(0.212)
$(\text{Log(dist)})^2$	-0.155*	-0.005	-0.176*	-0.154	-0.152	-0.077
	(0.091)	(0.062)	(0.097)	(0.093)	(0.093)	(0.091)
N	3,366,814	3,366,814	3,366,814	3,366,814	3,366,814	3,366,814

The dependent variable in all columns except column 6 is an indicator equal to one only if the respondent smokes every day. In column 6, the dependent variable is also equal to one for those that smoke some days. Tax and distance variables are centered at their sample means and all reported coefficients are multiplied by 100 to aid the presentation of the results. All regressions include state fixed effects (or county fixed effects in column 2), month-year fixed effects, the state unemployment rate in the survey month, and individual-level controls for marital status, employment status, education, sex, race, the presence of one, two, or three or more children in the household, income category, and age category. Taxes are in January 2015 dollars and distance is in miles. All regressions are weighted by BRFSS sample weights and all standard errors are clustered by state.

*: Significant at 10%. **: Significant at 5%. ***: Significant at 1%.

Table 5: Robustness Checks: Sample Restrictions

	(1) Baseline	(2) No West Region	(3) No Mex. Border	(4) No AL, MO, or VA	(5) Yearly-Sampled Counties Only
Home Tax	-0.887***	-0.722***	-0.792***	-0.880***	-0.851***
	(0.247)	(0.243)	(0.237)	(0.254)	(0.277)
Lower Tax	-0.129	-0.119	-0.236	-0.151	-0.057
	(0.228)	(0.224)	(0.235)	(0.231)	(0.306)
Log(Distance)	-0.183*	-0.119	-0.046	-0.228**	-0.206*
	(0.096)	(0.084)	(0.078)	(0.099)	(0.108)
Home Tax × Lower Tax	-0.577***	-0.395**	-0.487**	-0.561***	-0.599***
	(0.188)	(0.161)	(0.182)	(0.189)	(0.194)
Home Tax × Log(Distance)	-0.004	0.050	0.029	0.025	0.009
	(0.089)	(0.088)	(0.083)	(0.088)	(0.082)
Lower Tax × Log(Distance)	0.187	0.174	0.084	0.163	0.050
	(0.150)	(0.163)	(0.145)	(0.153)	(0.163)
$(\text{Home Tax})^2$	0.192**	0.127*	0.138*	0.194**	0.190*
	(0.080)	(0.068)	(0.078)	(0.082)	(0.097)
$(\text{Lower Tax})^2$	0.589***	0.472***	0.579***	0.592***	0.613***
	(0.137)	(0.161)	(0.150)	(0.139)	(0.162)
$(\text{Log(dist)})^2$	-0.155*	-0.115	-0.097	-0.175*	-0.196*
	(0.091)	(0.096)	(0.098)	(0.099)	(0.098)
N	3,366,814	2,559,092	3,056,734	3,251,058	2,453,890

The dependent variable in all regressions is an indicator equal to one only if the respondent smokes every day. Tax and distance variables are centered at their sample means and all reported coefficients are multiplied by 100 to aid the presentation of the results. All regressions include state fixed effects, month-year fixed effects, the state unemployment rate in the survey month, and individual-level controls for marital status, employment status, education, sex, race, the presence of one, two, or three or more children in the household, income category, and age category. Taxes are in January 2015 dollars and distance is in miles. All regressions are weighted by BRFSS sample weights and all standard errors are clustered by state.
*: Significant at 10%. **: Significant at 5%. ***: Significant at 1%.

Appendix

Table A1: Further Robustness Checks

	(1) Baseline	(2) Age <65	(3) Same State Lower Tax	(4) Add Lowest Tax State	(5) No Unemp.	6 Exclude Refusers
Home Tax	-0.887***	-1.008***	-0.778***	-0.905***	-0.688**	-0.926***
	(0.247)	(0.290)	(0.271)	(0.251)	(0.284)	(0.249)
Lower Tax	-0.129	-0.079	-0.154	0.102	-0.223	-0.147
	(0.228)	(0.268)	(0.242)	(0.227)	(0.229)	(0.270)
Log(Distance)	-0.183*	-0.184*	-0.094	-0.124	-0.157	-0.171*
	(0.096)	(0.104)	(0.091)	(0.108)	(0.095)	(0.097)
Home Tax × Lower Tax	-0.577***	-0.670***	-0.617*	-0.574***	-0.465**	-0.595***
	(0.188)	(0.218)	(0.347)	(0.189)	(0.204)	(0.168)
Home Tax × Log(Distance)	-0.004	0.010	0.066	-0.005	-0.001	-0.039
	(0.089)	(0.105)	(0.086)	(0.086)	(0.090)	(0.083)
Lower Tax × Log(Distance)	0.187	0.219	0.120	0.181	0.154	0.208
	(0.150)	(0.164)	(0.166)	(0.136)	(0.149)	(0.155)
$(\text{Home Tax})^2$	0.192**	0.228**	0.197	0.201**	0.131	0.184**
	(0.080)	(0.096)	(0.124)	(0.082)	(0.087)	(0.080)
$(\text{Lower Tax})^2$	0.589***	0.700***	0.632***	0.499***	0.547***	0.594***
	(0.137)	(0.160)	(0.223)	(0.137)	(0.167)	(0.137)
$(\text{Log(dist)})^2$	-0.155*	-0.175*	-0.075	-0.118	-0.152	-0.143
	(0.091)	(0.098)	(0.062)	(0.092)	(0.093)	(0.090)
N	3,366,814	2,757,785	3,366,814	3,434,559	3,366,814	2,923,448

The dependent variable in all regressions is an indicator equal to one only if the respondent smokes every day. Tax and distance variables are centered at their sample means and all reported coefficients are multiplied by 100 to aid the presentation of the results. All regressions include state fixed effects, month-year fixed effects, the state unemployment rate in the survey month (except in column 5), and individual-level controls for marital status, employment status, education, sex, race, the presence of one, two, or three or more children in the household, income category, and age category. Taxes are in January 2015 dollars and distance is in miles. All regressions are weighted by BRFSS sample weights and all standard errors are clustered by state.
*: Significant at 10%. **: Significant at 5%. ***: Significant at 1%.

Table A2: Expanded Estimates From Preferred Specification

Taxes and Distance			**Race**		
Home Tax	-0.887***	(0.247)	White, non-Hispanic	Omitted	
Lower Tax	-0.129	(0.228)	Black, non-Hispanic	-7.574***	(0.850)
Log(Distance)	-0.183*	(0.096)	Other, non-Hispanic	-5.199***	(0.467)
Home Tax × Lower Tax	-0.577***	(0.188)	Multiracial, non-Hispanic	0.964*	(0.490)
Home Tax × Log(Distance)	-0.004	(0.089)	Hispanic	-13.174***	(0.498)
Lower Tax × Log(Distance)	0.187	(0.150)	Refused	-2.755***	(0.558)
$(\text{Home Tax})^2$	0.192**	(0.080)			
$(\text{Lower Tax})^2$	0.589***	(0.137)	**Children in Household**		
$(\text{Log(dist)})^2$	-0.155*	(0.091)	No Children	Omitted	
			One Child	-0.416*	(0.213)
Marital Status			Two Children	-1.833***	(0.229)
Married	Omitted		≥ Two Children	-1.937***	(0.357)
Divorced	9.157***	(0.323)	Refused	-2.926***	(0.713)
Widowed	4.976***	(0.279)			
Separated	9.135***	(0.418)	**Income**		
Never Married	3.286***	(0.232)	< $10,000	Omitted	
In Unmarried Couple	8.107***	(0.948)	$10,000-$15,000	0.171	(0.383)
Refused	3.947***	(0.883)	$15,000-$20,000	0.887**	(0.389)
			$20,000-$25,000	0.354	(0.341)
Employment Status			$25,000-$35,000	-1.196***	(0.424)
Employed For Wages	Omitted		$35,000-$50,000	-2.493***	(0.496)
Self-Employed	0.353*	(0.189)	$50,000-$75,000	-4.704***	(0.631)
Out of Work ≥ 1 Year	6.547***	(0.352)	≥ $75,000	-7.154***	(0.704)
Out of Work < 1 Year	6.241***	(0.465)	Don't Know/Not Sure	-3.401***	(0.653)
Homemaker	-1.088***	(0.240)	Refused	-6.922***	(0.496)
Student	-7.443***	(0.664)			
Retired	0.349**	(0.154)	**Age**		
Unable to Work	6.055***	(0.400)	18-24	Omitted	
Refused	-0.183	(1.012)	25-29	3.989***	(0.334)
			30-34	3.518***	(0.255)
Completed Education			35-39	3.930***	(0.259)
No School	Omitted		40-44	4.305***	(0.258)
Grade 1-8	1.641*	(0.848)	45-49	3.892***	(0.287)
Grade 9-11	11.905***	(1.151)	50-54	2.182***	(0.263)
High School	3.463***	(0.929)	55-59	-0.312	(0.328)
Some College	-1.223	(1.046)	60-64	-3.525***	(0.454)
College Degree	-9.345***	(1.205)	65-69	-6.841***	(0.706)
Refused	0.111	(1.495)	70-74	-10.276***	(0.836)
			Refused	-3.065***	(0.304)
Sex					
Male	Omitted		State Unemployment	-0.430***	(0.068)
Female	-2.603***	(0.252)			
r^2	0.090		N	3,366,814	

The dependent variable is an indicator equal to one only if the respondent smokes every day. Tax and distance variables are centered at their sample means and all reported coefficients are multiplied by 100 to aid the presentation of the results. Coefficients on state and month-year fixed effects are not reported. Taxes are in January 2015 dollars and distance is in miles. The regression is weighted by BRFSS sample weights and standard errors are clustered by state.
*: Significant at 10%. **: Significant at 5%. ***: Significant at 1%.