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PRICE ISN'T EVERYTHING:
BEHAVIORAL RESPONSE AROUND CHANGES IN SIN TAXES

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ABSTRACT

In traditional economic models, taxes change behavior by changing prices. In empirical analyses, factors other than price are thought to be relevant, but any non-price factors are usually assumed to be held constant as taxes vary. We contend that violations of this assumption are expected when laws are passed changing sin taxes. In support of this claim, we document that state-level cigarette tax increases are concomitant with increases in anti-smoking appropriations, media coverage on smoking, lobbying efforts, and place-based smoking restrictions. The influence of these non-price factors is easily confused with price effects, and we find evidence suggesting that controlling for them substantially reduces the estimated demand responsivity to the tax itself.

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I. INTRODUCTION

Taxes are often deployed to shape the behavior of individuals and institutions. But how does this behavioral change arise? Nearly universally in economic models, taxes' influence on behavior is attributed to their impact on prices. Concretely, a tax on a good raises its price, and the law of demand then implies that the quantity demanded should decrease. In the course of changing a tax law, however, more than just prices may change. Especially in cases where taxes are intended to dissuade consumption (e.g., taxes on alcohol, carbon, or cigarettes), changing the law typically requires making the case that consumption *should* be dissuaded. Interested parties may attempt to influence the legislative process with the provision of information and persuasive appeals, and they may attempt to simultaneously deploy other interventions to achieve their goal of dissuasion. Furthermore, the act of changing the law may itself have an *expressive effect,* whereby it directly influences beliefs, emotions, or behavior purely by condoning or condemning an activity (McAdams, 2015). In short, while tax changes do typically influence price, they may also be expected to occur alongside significant changes to potentially important non-price factors.

In this article, we document the significant changes to non-price factors that occur as cigarette taxes are changed. We further document that accounting for non-price factors is critical in estimating the behavioral response to these tax changes. In our context, failure to control for our set of non-price factors would lead a researcher to significantly inflate estimates related to tax elasticity. This omission of controls could also lead a researcher to infer that behavioral response preceding a tax change is unambiguous evidence of the anticipatory response present in rational addiction models, when it may instead be driven by the coevolution of non-price factors.

In our empirical analysis, we specifically examine how the consumption of cigarettes by pregnant women evolves throughout the process of state-level cigarette tax law changes. Dissuading smoking by pregnant women is of particular policy interest because of smoking's consequences for infant health (Evans, Ringel, and Stech, 1999; DeCicca, Kenkel, and Lovenheim, 2021). Prominent papers on the response to sin taxation use this behavior as a proxy for smoking more broadly (see, e.g., Gruber and Kőszegi 2001), in part because of the unusual availability of high-frequency measurements of smoking that are comparable across states and over time. These data come from the smoking information that is reported on the U.S. Standard Certificate of Live Birth, a permanent legal record required for all live births. Using these data, we examine how cigarette consumption evolves in the time period surrounding 150 state-level tax law changes occurring between 1989 and 2009.

Our empirical analysis proceeds in three steps. First, we examine the time-paths of four non-price factors: tobacco industry spending on political donations to state politicians, anti-smoking appropriations (which are used to fund anti-smoking advertising, among other things), news headlines related to cigarettes, and place-based legal restrictions on cigarette smoking (such as bans on smoking in restaurants or workplaces). We document substantial changes in these factors in the window surrounding a tax change. Compared to states not facing tax changes, states facing a change face a sizable increase in political donations and newspaper coverage in the lead up to the reform. This illustrates that cigarette tax changes occur during time periods of substantial information provision and attempts at persuasion. The period before and after a cigarette tax change is also marked by an increase in both anti-smoking appropriations and place-based legal restrictions, illustrating the simultaneous deployment of other dissuasive policies. These findings establish that potentially important non-price factors are not held constant throughout the lawmaking process.

Second, we examine the role of non-price factors as explanations of the decrease in cigarette consumption that occurs around changes in tax law. We demonstrate that this decrease in consumption is partly explained by increases in anti-smoking appropriations, increases in newspaper coverage, and the adoption of place-based legal restrictions, even after accounting for the changes in the taxes themselves. The inclusion of these controls leads to quantitatively important changes in the behavioral response attributed to the tax itself: it reduces the estimated responsivity to the tax by roughly half. Under reasonable assumptions of the importance of unobserved non-price factors, the estimated responsivity decreases further.

This analysis demonstrates that the within-state variation in demand occurring around a tax change is significantly influenced by variation in non-price factors even when accounting for variation in taxes themselves. Combined with the findings that non-price factors change markedly in the window around a tax change, this finding poses a challenge for identification strategies that make use of within-state variation in smoking and taxes. Within-state variation is a common source of variation used for identification in the sin tax literature, and we are unaware of any observational study of a sin-tax elasticity where non-price factors are reasonably controlled. Caution is therefore needed in mapping estimates from the existing literature into notions of elasticity derived purely from price effects.

In our third set of analyses, we investigate additional predictions that arise if non-price factors are at work. Because the increase in non-price factors often occurs well in advance of the tax change, the decrease in demand due to these factors should occur well before taxes become relevant for prices. Directly comparing the time-path of demand in states facing a tax change to that in states facing no tax change, we find that demand is better explained as a smooth and gradual decrease preceding the change than it is by a function with discontinuity when tax changes are announced or enacted. These patterns contrast with the predictions of standard models where consumers only respond to the current tax and with the predictions of rational addition models where consumers respond to announced future taxes. We additionally document that a classic piece of evidence in support of rational addiction models—the anticipatory response to tax increases between the moment of announcement and enactment found by Gruber and Kőszegi (2001)—does not survive the inclusion of controls for our non-price factors. Our analysis does not establish the *absence* of the anticipatory rational-addiction response discussed by Gruber and Kőszegi (2001), but rather makes clear that compellingly testing for its *presence* is very difficult when accounting for non-price factors.

In sum, our results support the view that the process of changing tax laws changes more than just prices. Perhaps surprisingly, the quantitative impact of informational, psychological, and sociological factors influencing demand may be of at least comparable quantitative importance for understanding behavior during a tax change. Some of role of these non-price factors falls under the umbrella of "expressive powers of the law"—that is, the power of the law or lawmaking process to influence behavior through what it expresses and communicates, and not merely through its direct influence on rules and incentives. Such expressive effects have a prominent role in the legal literature, but this concept is rarely considered in economic applications,[1] and relatively little empirical work measures its importance.[2] While few would disagree with the conceptual point that

[1] This is true not only in classical economic models with rational agents (e.g., Pigou, 1920; Ramsey, 1927; Harberger, 1964), but also in economic models of sin taxation in behavioral public finance (e.g., O'Donoghue and Rabin, 2003, 2005, 2006; Farhi and Gabaix, 2017; Allcott, Lockwood, and Taubinsky, 2019). Although these behavioral-economic models involve agents with some degree of "irrationality," they continue to model taxes as affecting quantity demanded through prices.
[2] For some discussion and measurement of expressive effects, see, e.g., McAdams and Nadler, 2005; Funk, 2007; Wittlin, 2011; Dwenger et al., 2016; Fabbri and Hoeppner, 2018.

these factors need to be held constant in assessments of behavioral response, the results of this paper suggest that holding these factors constant is more challenging and more important than is commonly appreciated.[3]

The paper proceeds as follows. Section II provides a conceptual framework for understanding the role of non-price factors. Section III describes the construction of our dataset. Section IV presents empirical analysis. Section V concludes.

II. CONCEPTUAL FRAMEWORK

Our empirical analyses evaluate the extent to which changes in cigarette consumption around tax changes are attributed to changes in prices. We formalize the potential theoretical role of non-price factors using the model of Reif (2019), which combines and generalizes the forward-looking features of the rational addiction model of Becker and Murphy (1988) and social interaction features in the spirit of Brock and Durlauf (2001).

Consider an individual facing the following utility maximization problem:

$$\max_{\{a_t, c_t\}\forall t} \sum_{t=1}^{\infty} \beta^{t-1}\left(U(a_t, c_t, x_t, S_t) + G(a_t, E_t[\bar{a}_t])\right)$$

$$\text{s.t.} \;\; A_0 = \sum_{t=1}^{\infty} (1+r)^{-(t-1)}(c_t + p_t a_t)$$

$$S_{t+1} = (1-d)(S_t + a_t)$$

Utility in period t is governed by the individual's discount rate ($\beta < 1$), private utility (U), and social utility (G). Utility is maximized subject to a budget constraint requiring that lifetime discounted expenditures (at an interest rate r) equal lifetime discounted wealth (A_0).

[3] Our findings are reminiscent of DeCicca et al. (2008), who document a positive cross-sectional correlation between cigarette prices and anti-smoking sentiments and show that the correlation leads to the overestimation of price elasticities inferred from comparisons across states with different tax regimes. Our point is related but distinct: that a variety of factors relating to and shaping anti-smoking sentiments rapidly change within a state in the lead up to a tax change. While these findings are clearly related, our results more directly inform empirical strategies based on within-state variation.

This model has four basic features. First, the individual chooses between smoking a_t at price p_t and other consumption c_t (with price normalized to 1), resulting in substitution between these goods governed by their relative prices. Standard sensitivity to contemporaneous prices arises. Second, the individual is influenced by addiction to nicotine (achieved by smoking). Addiction is modeled with a "stock" of addictive capital S_t that decays each period by the rate of depreciation $d \in (0,1)$. The larger the addictive stock, the higher the marginal utility of smoking. Through this channel, the amount of nicotine consumption today will influence how much an individual wants to smoke in the future; therefore, prices that will be faced in the future become relevant to consumption decisions today. Third, the utility of smoking is influenced by the behavior of other individuals through social utility (G), such as where conformity to group smoking norms is valued or where smoking generates spillovers.[4] Social utility is governed by the relationship of the individual's smoking (a_t) and the individual's expectation of the average smoking of others $(E_t[\bar{a}_t])$.[5] Fourth, utility is influenced by a catch-all component for "non-price factors" x_t which accounts for the level of education, advertising, and other factors that influence both utility directly and utility achieved from smoking.[6]

In theoretical or empirical approaches using models of this variety, the term x_t is normally viewed as a confounding factor. For convenience, it is typically assumed to be held constant in the course of a tax change. We argue that a variety of factors that evolve in the course of a tax change are naturally accommodated by this term. We follow Reif (2019) and represent individual utility (after substituting optimal c_t into the equation) as:

$$V(a_t, x_t, S_t, E_t[\bar{a}_t])) = -\frac{1}{2}(b_{aa}a_t^2 + b_{SS}S_t^2 + b_{xx}x_t^2) + b_{aS}a_tS_t$$
$$+ b_{ax}a_tx_t + b_{Sx}S_tx_t + b_a a_t + b_S S_t + b_x x_t + b_k + G(a_t, E_t[\bar{a}_t])$$

This representation allows utility to depend on both linear and quadratic terms for smoking (a_t), addictive stock (S_t), and other factors (x_t). Utility also depends on interactions between all three, and a more general functional form of social utility $(G(a_t, E_t[\bar{a}_t]))$. For further details and restrictions on these terms, see Reif (2019).

[4] As noted by Reif, social utility of these types have been employed in significant prior work (Binder and Pesaran, 2001; Blanchflower et al., 2009; Brock and Durlauf, 2001; Glaeser and Scheinkman, 2002).
[5] Reif considers several ways of specifying this function, and assumes that the group is sufficiently large that the individual's contribution to the mean is negligible.
[6] For ease of exposition, we adopt Reif's treatment of x_t as a scalar. However, we note that its replacement with a vector poses no conceptual problem.

This representation highlights the manner in which non-price factors may become relevant. In the context of this model, such persuasive effects may operate through several channels. First, through the terms b_{xx} and b_x, x_t directly affects utility. This could capture phenomena such as a direct aversion to graphic warning labels,[7] and it could capture direct aversion to anti-smoking sentiment (as in, e.g., DeCiccia et al., 2008). Second, through the term b_{ax}, x_t influences the marginal utility generated by additional smoking. This could capture news coverage influencing beliefs about the marginal health consequences of smoking or direct increases in the marginal costs of smoking from non-tax dissuasive policies such as place-based restrictions. Third, through the term b_{Sx}, x_t influences the marginal utility of smoking generated by addictive stock. This could capture advertisements that remind the individual of smoking more often, policies that restrict exposure to smoking in public areas, or increased information about or accessibility of smoking cessation aids.[8]

The degree to which smokers actively choose to manage their addiction in a forward-looking manner remains a topic of debate. However, as documented in Reif (2019), convenient cigarette demand equations result from this framework regardless of whether consumers are myopic or forward-looking. The smoking demand equation for a myopic individual is:

$$a_t = \alpha^1 p_t + \alpha^2 S_t + \alpha^3 \bar{a}_t + \alpha^4 x_i + k_m$$

The smoking demand equation for a forward-looking individual is:

$$a_t = \beta^1 p_t + \beta^2 p_{t+1} + \beta^3 S_t + \beta^4 \bar{a}_t + \beta^5 \bar{a}_{t+1} + \beta^6 x_t + \beta^7 x_{t+1} + k$$

In both cases, demand is linear in the parameters of interest, with the coefficients (α, β) and the constants (k_m, k) depending on the specifics of the social interaction model.

These demand equations nest both the common considerations of a tax change present in the literature and the alternative non-price factors that we set out to study. First, both demand equations contain a standard, contemporaneous price effect (represented by the term α^1 or β^1). In traditional models of sin taxation that are meant to approximate (addiction-free) rational agents, this is the sole channel through which a tax change influences behavior. Next, the demand

[7] For recent empirical evidence on the effects of graphic warning labels on consumption, see Beleche et al. (2018). Graphic warning labels are used in other settings as well, and recent field-tests provide empirical evidence that graphic warning labels on sugary drinks can meaningfully decrease consumption (e.g., Roberto et al. 2016).

[8] Reif's model does not permit these other factors to influence social utility. However, in principle, the importance of these factors could arise through this channel as well. Anti-smoking campaigns have arguably increased the social stigma surrounding smoking, which itself can decrease demand (Stuber et al. 2009; Riley et al. 2017). Furthermore, place-based smoking restrictions can displace smoking to or from public locations, so these restrictions could shape perceptions of smoking prevalence (Hamilton et al. 2008).

equations contain intertemporal dependency on past and future prices. Past and future prices become indirectly relevant through the addictive component, captured by term α^2 in the myopic model or the term β^3 in the forward-looking model, and become directly relevant for the forward-looking consumer through the term β^2. These terms capture the forces of anticipatory price responses (as in Becker and Murphy, 1988; Gruber and Kőszegi, 2001).

We contend that typical discussion or analysis of tax changes imagines taxes operating through only contemporaneous and non-contemporaneous prices. Indeed, most discussion and analysis of tax changes typically focuses entirely on contemporaneous prices. However, we draw attention to the remaining components of the demand equations, which are governed directly by issues such as expectations of social behavior, information, and material changes to the costs or benefits of smoking itself. A tax change is almost always viewed purely as a price change, but we will present evidence suggesting that both the social expectations and information components captured in the remaining terms of the equation are directly affected.

III. Data Sources and Sample Definition

Our data analysis relies on the cigarette consumption of pregnant mothers, the timing of state-level proposed tax law changes, and measurement of our candidate non-price factors that could influence demand. We detail our construction of these data below.

A. Cigarette Demand: Natality Files

To conduct our analyses, we require a measure of cigarette consumption that is both high-frequency and comparable across states and over time. When facing these requirements, research on cigarette demand typically uses one of two data sources: direct measurement of cigarette sales or survey measures of consumption.

While sales data has clear appeal as a data source, it has three important limitations for the purposes of our study. First, rich sales data has limited availability for longer time series (e.g., Nielsen scanner data is not available for purchases before 2004). Second, cigarette purchasing diverges from cigarette consumption surrounding tax changes because of stockpiling (Chiou and Muehlegger, 2014). Finally, tax changes may induce smokers to travel to other tax territories for purchases, which leads sales data from a particular retailer to more imperfectly reflect a given consumer's consumption (see, e.g., DeCicca et al., 2013a, DeCicca et al., 2013b, Lovenheim 2008, Chiou and Muehlegger 2008).

Although survey measures of consumption can overcome these issues, they come with their own limitations. First, survey data is often criticized for selection into participation or imperfections in recorded responses. Second, few surveys are conducted in a manner that provides data on cigarette consumption at a granular time level.

To address these issues, we adopt the (partial) solution proposed by Gruber and Kőszegi (2001): we study the cigarette consumption of mothers who gave birth as recorded in the Vital Statistics Detailed Natality Data Files. The manner in which this dataset is constructed mitigates concerns present with sales data. Certificates of Live Birth[9] have significantly lower problems with non-completion than typical surveys, and the formal records are made with input and supervision from medical professionals.[10] Furthermore, the smoking measures included in these data elicit mothers' smoking behavior in the window prior to their child's birth. The fact that births are distributed across time, combined with the fact that these data were collected in a consistent way across a large group of states for a significant window of time, allows for the construction of a panel dataset spanning many tax change events while still having the needed granularity.

For most states, the Natality files record every birth since 1989 and contain a standardized elicitation of recent cigarette consumption as of that year. We follow the treatment of the smoking variable in Gruber and Kőszegi (2001) and assume that it represents the average rate of consumption in the month before delivery. Unfortunately for our purposes, the details of the elicitation of cigarette consumption changed in the mid-2000s, and states transitioned to using a new measure in a staggered manner between 2003 and 2009. Because the new measures are not directly comparable to the old measures, we drop states from our dataset once the older measure is no longer available. Moreover, information on cigarette consumption is either unavailable or unreliable in California, Indiana, South Dakota, and New York, so we follow Gruber and Kőszegi (2001) and drop these states. For the remaining states, we construct the state-year-month average cigarette consumption beginning in 1989 and ending when the survey elicitation changed in the state.

[9] See Appendix Figure A1 for an example of these forms.

[10] Smulian et al. (2001) find that 86 percent of hospital staff in New Jersey maternity facilities used either prenatal care records or maternal hospital medical records as the source of the smoking information. They also found that only 6 percent of hospital staff in New Jersey used the mother's report.

Like Gruber and Kőszegi (2001), we note that although pregnant women are not a representative population, they are an important group to study because of the important consequences of maternal smoking on infant health. Indeed, the impact of maternal smoking on infant health is thought to be one of the main externalities associated with smoking (Evans, Ringel, and Stech, 1999; DeCicca, Kenkel, and Lovenheim, 2021).

B. Timing of Tax Laws

For data on monthly state cigarette taxes in place and the timing of changes in state cigarette tax law, we use data from the Centers for Disease Control's State Tobacco Activities Tracking and Evaluation System. We identify the timing of each tax law change, including year-month of the enactment of the new law and the year-month that the law change becomes effective. We additionally validate the effective dates by comparing them to those reported in the Tax Burden on Tobacco dataset released by the Federation of Tax Administrators.

C. Intensity of Political Activities and Social Debate

We form three different measures of political and social debate to serve as some of our non-price factors. We view these as measures of activities intended to inform or influence the behaviors of either voters or lawmakers. These measures can be thought of as imperfect proxies for an expanded definition of the "expressive effects" (McAdams, 2015)—effects that occur as a result of the ideas communicated by all the activities and debate that surround and accompany the process of legal change.

First, we establish a proxy relevant to the intensity of *political* debate around cigarettes using tobacco industry spending on political donations to state politicians. To do so, we use a component of the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica 2013). DIME contains information on approximately 100 million donations made by individuals, political action committees, and corporations to candidates in local, state, and federal elections from 1979 to 2014.[11] Our measure draws from the "contributions database" within DIME, focusing specifically on donations to state-level candidates. DIME includes the date on which the donation

[11] Within this database, contribution records, candidate and committee filings, and election outcomes for state elections are provided by the National Institute on Money in State Politics and the Sunlight Foundation. More information can be found at http://data.stanford.edu/dime.

was made and the state of the recipient candidate or committee. Using this data, we build a panel of state-year-month donations made from the tobacco-related entities.

Second, to construct a measure of persuasive activity by anti-tobacco interests, we use state-level anti-smoking appropriations from the 2015 Health Communication Interventions, Best Practices Program Components from the Centers for Disease Control and Prevention.[12] The appropriations data capture funding from four major funding sources: federal funding, state funding, the Robert Wood Johnson Foundation, and the American Legacy Foundation. Note that this measure of appropriations only imperfectly reflects expenditures in a given time period because the appropriations are not necessarily expended. We assume that they are expended in a uniform manner throughout the funding period.

Third, we establish a proxy for the intensity of public debate around cigarettes by using headline appearances of smoking-related words in newspapers. This approach derives a measure of public debate by assuming that newspapers either respond to public demand for issues or promote debate about an issue by writing about it.[13] To construct the dataset, we scraped headlines from newslibrary.com from 1990 to 2015. According to newslibrary.com, it is "[t]he most complete archive available for [6504 newspapers and other news sources]". The website is searchable by state and search results contain the date of the news article. Our main measure is the number of monthly headlines including "cigarette." Newspapers enter and exit this dataset over time, with substantially more newspapers in the dataset in recent years, so we measure the intensity of debate using the number of cigarette-related headlines per state or local journal rather than the raw count of articles appearing.

D. Place-Based Legal Restrictions

We construct an index of the legislative activity related to place-based restrictions on smoking activity using the State Tobacco Activities Tracking and Evaluation System from the Centers for Disease Control and Prevention (2018). These data contain historical information on all state-level legislation, including the enactment date and the effective date. More importantly,

[12] Because the appropriations data are available only since 1991, we code appropriations in 1989 and 1990 as those in 1991.
[13] In showing that changes in physician behavior before tort reforms lead to a two-fold increase on estimated effects of the law on physician behavior, Malani and Reif (2015) document an increase in newspapers discussing medical malpractice reforms before it was adopted. Past work like this partially motivates our approach.

for a set of classes of locations (including restaurants, private worksites, day care centers, and more),[14] this dataset provides the citation of the statute that prohibited smoking at that location class. Our place-based legal restriction index is the average of the cumulative number of legal changes in each of the classes of locations that are independently tracked (fully enumerated in footnote 14).

E. Summary of Panel

The final sample consists of 10,022 state-year-month cells and spans 150 tax changes occurring in 43 of the 46 states in the sample. Figure 1 reports the window of available data for each state, as well as the timing of tax law change events. The data starts in 1990 for all but a few states. The data ends in 2009 for 23 states, the data ends between 2005 to 2009 for 12 states, and the data ends from 2003 to 2004 for the remaining 11 states. The size of the tax change ranges from a decrease of \$0.10 to an increase of \$1.00, with 75 percent of tax changes increasing by at least \$0.05, 50 percent increasing by at least \$0.15, and 25 percent increasing by at least \$0.37. The average size of the tax change was an increase of \$0.25.

IV. EMPIRICAL ANALYSIS

In this section, we investigate the importance of non-price factors in explaining the decrease in cigarette consumption around tax law changes. We proceed in three steps. First, we examine the time-path of non-price factors around tax law changes. Second, we examine the relative importance of price and non-price factors in explaining the decrease in consumption that occurs following tax law changes. Third, we revisit typical explanations for the timing of behavioral changes in the period surrounding a tax law change. The formal econometric specification varies across these three groups of analyses, and we present the relevant details when each specification is introduced. However, all three groups of analyses involve a "stacked event

[14] The full set of classes of locations recorded in the data is "Bars, Commercial Day Care Centers, Government Multi-Unit Housing, Government Worksites, Home-Based Day Care Centers, Hotels and Motels, Personal Vehicles, Private Multi-Unit Housing, Private Worksites, Restaurants, Bingo Halls, Casinos, Enclosed Arenas, Grocery Stores, Hospitals, Hospital Campuses, Malls, Mental Health Outpatient and Residential Facilities, Prisons, Public Transportation, Racetrack Casinos, Substance Abuse Outpatient and Residential Facilities." When counting the number of distinct changes, we group several similar classes. Our coding of day care centers includes commercial and home-based. Our coding of casinos includes casinos, bingo halls, and racetracks. Our coding of hospitals includes hospitals and hospital campuses. Our coding of mental health facilities includes mental health outpatient and residential facilities. Our coding of substance use facilities includes outpatient and residential facilities.

study" approach, which we describe here. For recent papers organizing data with similar procedures, see Cengiz, Dube, Lindner, and Zipperer (2019) or Deshpande and Li (2019).

To illustrate the organization of data in a stacked event study, we first describe the data structure of a single event considered in isolation. We then discuss the process whereby individual events are "stacked."

Imagine that we are interested in understanding the impact of a single event: the tax change that occurred in Louisiana in July of 2000. To measure the impact of this event, we would examine the evolution of smoking behavior in Louisiana in a window of time around that change, which we call an *event window*. Depending on our analysis, the event window may be divided into further sub-periods. In analyses geared towards modeling the evolution of non-price factors and consumption in the time before and after a tax change, we define a baseline period occurring well before the tax change and then examine how variables deviate from the baseline average in the time period more immediately before and after the tax change. Concretely, for our primary specifications in Sections IV.A and IV.C, we define the window 25-36 months (2-3 years) before the tax change as the baseline period and then examine how variables evolve relative to the average value during baseline period across the 24 months leading up to the tax change and the 12 months following it. In our regression analyses of tax responsivity which involve comparing average cigarette consumption before and after the tax change, we will define the event window by a symmetric period before and after the tax change. Concretely, for our primary specification in Section IV.B, we consider an event window covering 36 months before the tax change and 36 months after the tax change (although we additionally consider narrower and wider windows).

Conceptually, the behavior observed in Louisiana during the event window may be thought of as a demonstration of behavior in a state that has been "treated" with a tax change. In some of our analyses, we focus on understanding the predictors of within-treated-state variation in outcomes. In other analyses, we compare changes in treated states with changes in "control" states that did not experience a tax change. In forming our set of control states, we exclude any state that experienced a tax change during our defined event window. Additionally, and as we will see, non-price factors begin to evolve for a period before a tax change and some continue to evolve after, so we exclude any state that experienced a tax change in the six months immediately before or after the event window.[15]

For this single event, our event dataset would consist of observations for both Louisiana and for each control state for every month in the event window. To form our full "stacked event study" dataset, we append the event datasets for all of the tax changes considered in our analysis. For our graphical analyses, and in Table 1 below, we will restrict the dataset to the 108 events for which we observe all state-year-months in the full event window (effectively excluding tax changes at the very beginning or very end of the sampling periods plotted in Figure 1). In our regression analyses, we will consider all 150 tax change events occurring in our original dataset.

Table 1 reports descriptive statistics of the original panel data and the stacked-event-study dataset. The stacked-event-study dataset analyzed here is constructed with an event window ranging from 36 months before the tax change to 12 months. This dataset contains 5,292 event-state-year-month cells for the treated group and 74,774 event-state-year-month cells for the control group. As this table reveals, treated states in the stacked-event-study dataset differ in clear ways from both their control states and from the overall panel average. Relative to control states, treated states have lower average cigarette consumption and higher average tobacco industry political donations, anti-smoking appropriations, and place-based legal restrictions indices. These differences are consistent with the general concern motivating our paper: that tax changes do not occur at random but rather during periods when a variety of activities related to the dissuasion of smoking occur. In the next section, we begin our analysis by examining these differences more formally.

A. Examining the Time-Paths of Non-Price Factors

We begin by examining the time-path of our candidate non-price factors around tax law changes. Within each event, we compare the evolution of each measure in the state facing a tax law change—the "treated" state—with states facing no tax law change within that window (or in the six months before and after it)—the "control" states. To assess this evolution, we calculate the average value of each measure in the baseline period (25-36 months before the tax change,

[15] While we believe this additional exclusion based on a six-month buffer is conceptually important, in practice it has little impact on the estimates that we present.

indicated with shading in Figure 2), then plot the deviation from this baseline average across the event window using a six-month moving average.

Using this methodology, Figure 2 documents a stark escalation of the four non-price factors in the time surrounding a tax law change. Panel A reports the tobacco industry political donations, Panel B reports the anti-smoking appropriations, Panel C reports the news headlines per journal, and Panel D reports the place-based legal restrictions index. For a particularly clear example of the nature of social and political discourse in the course of a tax law change, consider the time path for tobacco industry political donations. While the baseline level of donations is quite low, donation activity spikes in a narrow window leading up to the tax law change. The other measures exhibit conceptually similar differences between treated and control states, although the changes are less localized. In the time period leading up to the tax law change, there is a marked increase in newspaper headlines concerning cigarettes and in anti-smoking appropriations. In the 24 months leading up to a tax law change, there is a steady divergence between the treated and control states in the place-based legal restrictions index.

Moving beyond visual assessment to statistical analysis, we directly test for the significance of these patterns in a difference-in-differences approach. We estimate the following regression with the stacked-event-study data:

$$\mathrm{x}_{e,s,t} = \sum_{b\in B}\left\{\beta_b \times I(t \in b) + \gamma_b \times I(t \in b) \times T_{e,s}\right\} + \phi_{e,s} + \epsilon_{e,s,t} \tag{1}$$

for event e in state s and event-time t, where b denotes an event-time period bin and where B denotes the set of bins. In this equation, the outcome of interest is the value of a non-price factor $\mathrm{x}_{e,s,t}$ measured for event e in state s and event-time t. Variable $T_{e,s}$ takes the value of 1 if state s is the treated state for event e and is otherwise 0. Event time is grouped into six-month bins, spanning from 24 months prior to the event's tax change to 12 months after the events' tax change. The function $I(t \in b)$ takes the value of 1 if the event time under consideration falls in bin b and is otherwise 0. The term $\phi_{e,s}$ denotes an event-state fixed effect, measuring the average value of the non-price factor during the event-and-state-specific baseline period (25-36 months prior to the event's tax change). For each six-month bin, this regression estimates the average difference in the value of the non-price factor relative to its average value in the baseline period. For control states, this difference is captured by the term β_b. Term γ_b the difference in these differences between treatment and control states. This serves as the key difference-in-difference estimate of interest.

Statistical inference in this framework requires accounting for several dimensions of correlation in the error terms. First, because of the manner in which events are stacked, a given state-year-month can be present multiple times in our data. For example, Alabama in January of 2000 serves as a control observation for both Louisiana's July 2000 tax change and Arkansas's July 2001 tax change (as well as other tax changes).[16] This introduces correlation between the error terms for these two control observations in the data. Clustering standard errors by state accommodates this issue while additionally accounting for residual within-state variation occurring across events and across time.[17] Second, there can be common shocks experienced by all states at given moments in time, such as those from an increase in the federal cigarette tax or the death of the Marlboro man, motivating us to additionally cluster standard errors by year-month. Finally, one might be concerned about correlation occurring across all observations within a given event, as can arise due to event-level selection of control states. This motivates us to additionally cluster at the event level. We perform multi-way clustering using the approach of Correia (2016). We maintain this set of clustering practices for all analyses.

Figure 3 plots the estimated coefficients with 90 percent confidence intervals. The figure reveals that the patterns described above are generally statistically significant. Compared to control states, states facing a tax law change experience an increase in newspaper headlines in the lead up to the date of the tax law change, an increase in place-based legal restrictions in the years following the tax law change, and a (statistically insignificant) increase in anti-smoking appropriations throughout. The increase in tobacco industry political donations in the immediate lead up to the tax law change remains apparent, but the estimate does not reach statistical significance at conventional levels.

In summary, states experiencing a tax change also experience evolutions of related non-price factors, both when examined in isolation and also when benchmarked against control states.[18] If these non-price factors have their own direct influence, this demonstrates both that this evolution

[16] Note that the event time in which a given state-year-month will be a control observation differs across events. For example, Alabama in January of 2000 serves as a control observation in event time -6 for Louisiana's July 2000 tax change and event time -18 for Arizona's July 2001 tax change.

[17] Note that to deal with the issue of repeated cases of the same observation, it would be sufficient to cluster at the state-year-month level. However, this level of clustering is nested within the state level, and clustering at this coarser level is standard practice in this literature.

[18] It is worth emphasizing that the apparent pretrends observed in Figures 2 and 3 are not spuriously generated by earlier tax changes included in the event data. Similar patterns arise when Figure 3 is estimated from the subset of events with no other tax changes occurring within the event window (see Appendix Figure A2).

must be controlled for when analyzing the behavioral response to tax changes and also that it can't be controlled for by simple comparisons to non-treated states.

B. Testing for Impact of Non-Price Factors

Having established that non-price factors are not constant around tax law changes, we now assess whether this variation is relevant for understanding the change in cigarette consumption that happens within treated states as their laws change. To do so, we construct event windows with the same number of periods before and after a tax change and estimate how cigarette consumption over these windows responds to the change in cigarette taxes. We first consider a specification in which non-price factors are not controlled, representing the baseline analysis that would be done when the researcher does not have this data. We then add our non-price factors as controls and examine the consequences. Because we do not have exogenous variation in non-price factors, and because we view our non-price factor variables as an incomplete set of noisy proxies for the broader phenomenon of interest, we do not focus attention on their estimated coefficients and do not interpret their coefficients as causal estimates. Instead, we examine the impact of the inclusion of this partial set of controls on the estimated coefficients on the state tax.

To this end, in Table 2 we estimate versions of the following regression with stacked-event-study data:

$$\ln(\text{cigarettes})_{e,s,t} = \beta \text{ statetax}_{e,s,t} + \gamma\, x_{e,s,t} + \phi_{\,e,s} + \epsilon_{e,s,t} \qquad (2)$$

Subscripts again denote event e in state s and event-time t. In words, we regress the natural log of cigarette consumption on the current state tax, non-price factors $x_{e,s,t}$, and state-event fixed effects ($\phi_{s,e}$). Because we are interested in understanding the determinants of within-treated-state covariation in cigarette consumption and taxes, we restrict our data to only the treated state for each event.[19] This restrictions makes the state-event fixed effect equivalent to an event fixed effect. We initially estimate this model using an event window that contains 36 months of data both before and after the event. After presenting this initial model we explore robustness to shorter and longer event windows.

Column 1 of Table 2 illustrates a well-documented finding: consistent with the existence of price-responsivity, cigarette consumption is negatively associated with the state tax within the

[19] Despite this focus, we note that we also find significant coefficients on non-price factors in control states (see Appendix Table A1).

state facing a tax law change. Interpreting the magnitude of coefficients, a one dollar increase in the state tax is associated with a 0.264 log point (s.e. = 0.033)—i.e., 30 percent—decrease in cigarette consumption. In Columns 2 to 5, we separately add each of the non-price factors as controls. We find that the variation in anti-smoking appropriations, newspaper discussion of cigarettes,[20] and the place-based legal restrictions index all have strongly statistically significant associations with a decrease in cigarette consumption. We find qualitatively similar—but not statistically significant—results for tobacco industry political donations. Column 6 controls for all of the non-price factors. We again find strongly significant associations with all non-price factors except tobacco industry political donations.

Contrasting the estimated coefficient on the state tax across columns illustrates a striking consequence of these findings. Across Columns 2 to 5, we find that the inclusion of individual controls consistently reduces the estimated coefficient on State Tax (although in Columns 2 and 5 the 95 percent confidence intervals still include the Column 1 estimate). As seen in Column 6, including all of these controls reduces the estimated responsivity to the tax by 52 percent, from -0.264 (s.e. = 0.033) with no controls to -0.127 (s.e. = 0.023) with all the controls. The difference between these estimates is strongly statistically significant ($p < 0.001$).

These analyses suggest that a substantial portion of the change in demand that could be attributed to tax changes per se is better attributed to typically unobserved underlying trends in non-price factors. In the presence of this type of problem, a common solution is to examine particularly narrow windows of time around an event, relying on the logic that a rapid response must be better attributable to the candidate event than any cooccurring trends over time. To assess such a possibility, and to generally investigate the robustness of our findings, we reestimate our model with event windows of varying lengths. Figure 4 reports the coefficients on state tax estimated with and without controls for non-price factors (as in Columns 6 and 1 of Table 2), using event windows ranging in size from 12 months of pre- and post-event data to 60 months of pre- and post-event data. Estimated price effects are systematically larger for larger time windows,

[20] Headlines containing the word cigarette may matter through two main channels. First, the headline could contain information about non-price factors, including the health consequences of smoking and social norms. Second, the headline could contain information about the expected or actual future increase in the price of cigarettes. To explore this distinction, we separate the cigarette headlines into those that contain the word tax and those that do not contain the word tax. Appendix Table A2 shows that the estimated effect in Table 2 is driven exclusively by cigarette headlines that do not contain the word tax, providing no evidence that the headlines are changing behavior by providing information more relevant to taxes than to smoking itself.

reflecting the fact that over greater window the underlying trends in non-price factors have greater confounding effects. However, for all event window widths presented, the inclusion of non-price controls substantially reduces the estimated effect. Using the narrowest event window, the estimate decreases in magnitude from -0.124 without the non-price controls to -0.051 with non-price controls—a 59% decline. In summary, much of the behavioral response around a tax change is explained by variation in non-price factors regardless of whether short- or long-term pre/post comparisons are considered.

Table 2 and Figure 4 clearly demonstrate that controlling for our four non-price factors substantially attenuates the relationship between cigarette consumption and state taxes. Recall that we have argued that these four factors, while important, are only imperfect proxies for the full class non-price factors potentially influencing demand. To generate approximate bounds on how much attenuation would occur if all non-price factors were fully controlled, we apply the approach of Oster (2019). Conceptually, Oster's approach considers a case where the estimation of a treatment effect is confounded by both observable and unobservable controls. Given knowledge of the stability of the treatment effect coefficient as observed controls are added, and given assumptions on the comparative selection occurring from observed and unobserved controls and the total predictive power (R^2) that would be achieved if all controls were observed, Oster's approach yields an estimate of the fully unconfounded treatment effect.

Even with conservative assumptions on the predictive importance of unobserved non-price factors, Oster's approach suggests substantially more attenuation could be expected. Comparing the regressions from Columns 1 and 6 of Table 2, the inclusion of the four non-price factors increased the within-event R^2s from 0.217 to 0.383 (i.e., including our four non-price factors generated a 76 percent increase in explained variation from the baseline specification). For this exercise, we assume that all unobserved non-price factors incrementally explain no more variation than the four non-price factors that we do observe–that is, we assume that the inclusion of additional unobserved non-price factors could increase the explained variation by no more than an additional 10 percent (for a within-event R^2 of no more than 0.421). Even with this relatively conservative assumption on the importance of unobserved variables, the corrected magnitude of the coefficient on state tax could be as low as -0.060.[21] This estimate reflects a 77 percent decrease from the estimate without controls for non-price factors of -0.264. Of course, if the upper bound

[21] Bootstrapped standard error on bound: 0.019, based on 1000 bootstrap iterations.

on the potential explanatory power of remaining unobserved variables is further relaxed, the lower bound on the state tax coefficient would be even smaller.

In the appendix, we provide additional analyses and discussion relevant for assessing the robustness of the results of this section. In particular, we assess the importance of modeling effects as cumulative, the role of linearity assumptions, the consequences of reweighting observations based on the number of events in a state, and issues related to the selection inherent in a sample of pregnant women having live births. While all these issues are conceptually substantive, we present analyses suggesting that they do not confound the qualitative inferences that we draw in this paper. We also examine how our non-price factors individually contribute to reducing estimated responsivity. These results provide additional nuance to our discussion, but consistently support our overarching point that non-price factors have a large influence in these analyses.

The results of this section may be interpreted in two ways. First, and most directly, each regression presented above provides an estimate of behavioral responsivity to the tax (formally, a semielasticity) that is identified from within-state variation in a narrow time window and is valid under the assumption that the included non-price factors are sufficient to completely control for all relevant omitted variables. As we have cautioned, we believe that we likely are still only imperfectly measuring the necessary controls, so we view these estimates as revealing the (imperfect) inference a researcher would make when assuming a given set of observed controls was enough to proceed. Our results imply that relying on estimates without non-price factor controls would overstate the responsivity of cigarette consumption to taxes. [22]

Second, and more technically, these analyses reveal that the within-treated-state association between cigarette consumption and taxes is significantly confounded. This presents a challenge not only for our specific regressions, but for a broad class of quasi-experimental

[22] To benchmark our quantitative effects relative to existing literature, it is helpful to consider a log-log specification (i.e., an elasticity) rather than our default log-linear specification (i.e., a semielasticity). The log-log version of Table 2's analysis yields an estimated tax elasticity of -0.566 (s.e. = 0.068) without non-price-factor controls and of -0.286 (s.e. = 0.049) with all four non-price-factor controls. The uncontrolled estimate falls within the typical range of adult price elasticity estimates in this literature, and our fully controlled estimates reflect a greater degree of inelasticity than most estimates. For example, Chaloupka and Warner (2000) report a consensus price elasticity range of -0.4 to -0.7 and Gallet and List (2003) report a mean price elasticity of -0.48 from a meta analysis of 523 studies. These articles (and more) are discussed in the recent survey of DeCicca et al. (2021). Related to our findings, DeCicca et al. additionally document that lower elasticities are estimated in the relatively uncommon recent studies that attempt to control for anti-smoking sentiments.

analyses. Common identification strategies operate by contrasting the within-treated-state association with that in counterfactual predicted associations in which the relevant confounding is assumed to be controlled in some way. The difference is then interpreted as the causal effect of the tax itself. Our results illustrate that the most natural means of controlling for these confounding factors will be highly imperfect. For example, the fact that non-price factors evolve differently in states with and without tax changes, combined with the finding that controlling for the evolution of non-price factors strongly influences the association between cigarette consumption and state taxes, suggests that difference-in-difference designs can at best imperfectly account for these concerns. Moreover, the fact that substantial within-treated-state confounding exists even in a narrow window around a tax change suggests that controlling for all non-price factors with a general, state-specific time trend will be insufficient unless the time trend is sufficiently flexible to deviate from its broad trend is significant ways in a narrow period of time around the event. Convincingly establishing causation in the presence of such loosely specified state-specific time trends is extremely challenging and, in practice, is only successful when there is clear evidence of discontinuities in behavior at the time of an event. In the following section we present additional analysis geared towards examining evidence for such discontinuities.

C. Testing Additional Predictions of the Influence of Non-Price Factors

The results above suggest that much of the evolution in cigarette consumption that occurs around a cigarette tax change might be explained by the coevolution of non-price factors. This finding has strong implications for the predicted timing of the decrease in consumption. Because the evolution of non-price factors begins well before the tax is enacted, the decrease in consumption that occurs around a tax change would be expected to begin well in advance of when a tax change becomes relevant for prices.

To test the prediction, Figure 5 plots the time-path of cigarette consumption in treated states compared to control states. Formally, we estimate Equation 1 using the natural logarithm of cigarette consumption as the dependent variable and using 1-month bins. As in the analysis of Section IV.A, we, allow the period of time 36 months before the tax change to 25 months before the tax change to serve as the baseline period, then examine the evolution of our variable in the following 24 months prior to the tax change and the 12 months following the tax change. The figure reports our estimates of the differential change occurring in treated states in the event time.

Standard models of price sensitivity predict a discontinuous decrease in demand occurring precisely at the moment the tax changes. Figure 5 provides no clear evidence of such a discontinuity, and instead suggests that the decrease in demand occurs as a gradual decrease over a number of years.

Models of rational addiction predict some degree of anticipatory decrease in demand arising from a known future increase in prices. Prior work documents decreases in demand occurring before the tax is enacted but after a vote has occurred and use this difference to support the existence of rational addiction (see, e.g., Gruber and Kőszegi 2001). Note that the anticipatory effect we document here is occurring substantially earlier: the shaded region of Figure 5 illustrates the interquartile range of the enactment of tax changes in our data. Of course, a smoker who is aware of upcoming votes might hold a rational expectation of some future price increase even before the vote occurs, which could contribute to the earlier decrease in consumption that we observe.

Building on these observations, we investigate how accommodating the regression framework of Gruber and Kőszegi (2001) affects our results. The key innovation of their approach is to regress cigarette consumption on not only the tax rate that is in effect but also on the tax rate that has been enacted but is not yet effective. The coefficient on this latter term is interpreted as capturing anticipatory response to the future tax increase as in the rational addiction model.

Table 3 reports results for regressions like those in Table 2 but which includes a variable for the enacted but not yet effective tax. In the period between when a tax change is enacted and when it becomes effective, this variable takes the value of the size of the tax change (measured in dollars). At all other times, this variable takes the value of zero. Column 1 reproduces the qualitative result of Gruber and Kőszegi (2001), showing that 30 percent of the ultimate decrease in consumption that is attributed to the tax occurs in the window after the tax is enacted but before the tax goes into effect. Examining Columns 2 to 5, however, we see that even with the inclusion of this variable, the relevance of the non-price factors that we emphasized persists. In Column 6, which reproduces our main specification with all non-price controls included, we continue to find that the estimate on tax is reduced by over half relative to the baseline specification in Column 1, decreasing in magnitude from -0.266 (s.e. = 0.033) with no controls for non-price factors (Column

1) to -0.128 (s.e. = 0.023) with all the controls for non-price factors (Column 6). Furthermore, the coefficient on the Gruber and Kőszegi measure of anticipation of the price change becomes smaller and is not statistically significant at conventional levels, decreasing in magnitude from -0.081 (s.e. = 0.021) with no controls for non-price factors (Column 1) to -0.030 (s.e. = 0.021) with all the controls for non-price factors (Column 6). This suggests that the strength of the statistical evidence in support of Gruber and Kőszegi's account is meaningfully weakened when non-price factors are accommodated. However, it is important to note that the coefficient is imprecisely estimated, and the results do not reject meaningful anticipatory effects. We interpret these findings to illustrate the difficulty of causally attributing declines in consumption around tax changes to (even anticipatory) price events, but we emphasize that they do not firmly establish that no such causation occurs.

V. Conclusion

In the standard economic framework, sin taxes dissuade behavior through price effects. All else equal, a raise in taxes raises market prices, which in turn reduces demand. While this logic is correct and compelling, in this article we documented that all else is not held equal in the course of a tax law change, and that the behavioral response observed around a tax change might largely be attributable to non-price factors.

Within the sin tax literature, we highlight the importance of our findings in three ongoing debates. First, the results inform the ongoing policy discussion about the expected effects of sin taxes. While taxes on items like cigarettes and alcohol have long been employed, the recent surge of interest in taxing soda and sugary beverages has resulted in renewed interest, both conceptual and practical. As we proceed with a wave of attempts to impose new such taxes, understanding the key determinants of successful reduction in consumption is needed. Our results help inform the key channels through which these legal changes can achieve their goal and illustrate the potential for non-price channels to achieve similar effects.

Second, the results are relevant to the literature aimed at assessing the evidence of "rational addiction" in the spirit of Becker and Murphey (1988). Classic tests of this theory, such as Gruber and Kőszegi (2001), present evidence that cigarette consumption decreases in advance of the tax change. This is interpreted as evidence that forward-looking smokers reduce their degree of addiction in anticipation of cigarettes becoming more expensive. Our results suggest that a variety of other factors can contribute to decreases in consumption preceding the tax change. Our conceptual framework does not rely on the absence of any rational addiction motives—indeed, the provision of information that we document could interact with these motives in a manner that helps reduce demand. However, our results suggest that interpreting anticipatory decrease in consumption as clear evidence of rational addiction could only be done under the extremely stringent scenario in which all non-price factors are fully controlled for in empirical analysis.

Third, the results are relevant to the estimation and interpretation of sin tax elasticities. We documented that the inclusion of our candidate non-price factors as controls reduces the estimated degree of price sensitivity by roughly half. Furthermore, we emphasize that we have only a partial list of non-price factors, and our available measures are imperfect proxies for the underlying constructs of interest. As a result, the reduction in the estimated degree of price sensitivity is likely an underestimate. These findings build on prior research that has demonstrated that controls for survey measures of anti-smoking sentiments can reduce estimated price elasticities (e.g., DeCicca et al., 2008). Relative to these past findings, we contribute by directly studying a variety of the factors that we believe shape anti-smoking sentiments, demonstrating that these factors evolve rapidly in the course of a tax change, and by documenting that this rapid evolution may account for a large portion of the behavioral response occurring in the vicinity of tax changes.

Beyond the context of sin taxation, the results align with recent findings in several disparate domains demonstrating that more than financial incentives change in the course of a policy change. Gneezy and Rustichini (2000) find that introducing financial penalties for late pick-ups from childcare led to increased late pick-up, which is argued to be driven by the impact of the fine on perceptions of the social consequences of the behavior. Abouk and Adams (2013) find that the decrease of accidents after texting bans is better explained by the bans' announcements than the bans themselves. Malani and Reif (2014) find that physician labor supply anticipatory reactions to tort reform significantly affects the estimated effect of the reform on behavior. Richwine et al. (2019) find an increase in both public discourse about the benefits of vaccinations and vaccinations themselves in the lead up to a law that removed all nonmedical exemptions for school-mandated vaccinations. Taylor et al. (2019) find that the decline in soda consumption occurring during the Berkeley Soda Tax roll-out can be largely explained by impacts of election and social media coverage. Finally, Pratt (2020) finds that the impact of fines associated with water rationing can be partially explained by their impact on the perceived importance of conservation.

We empirically contribute to this literature by showing that the types of concerns raised in the previous work are likely to be first-order in models of optimal sin taxation. Conceptually, this diverse body of related examples can be unified through the legal literature concerning how laws work and why they are enacted. Legal scholars have long appreciated that laws do more than change financial incentives and can change behavior through what is commonly referred to as expressive effects. We view our work as supporting an expanded definition of expressive legal effects that incorporates responses to the ideas communicated by all the activities and debate that surround and accompany the process of legal change. Despite the prevalence of the view that law can work by expressing value, this article presents some of the first empirical evidence that begins to separate price effects from expressive effects. Our results suggests that, at least in the context of cigarette taxes, the manner in which tax laws operate through non-price channels should be a bigger consideration for both researchers and policy makers.

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Taylor, Rebecca, Scott Kaplan, Sofia Villas–Boas, and Kevin Jung (2019). “Soda Wars: The Effect of A Soda Tax Election on University Beverage Sales.” Economic Inquiry 57: 1480–1496.
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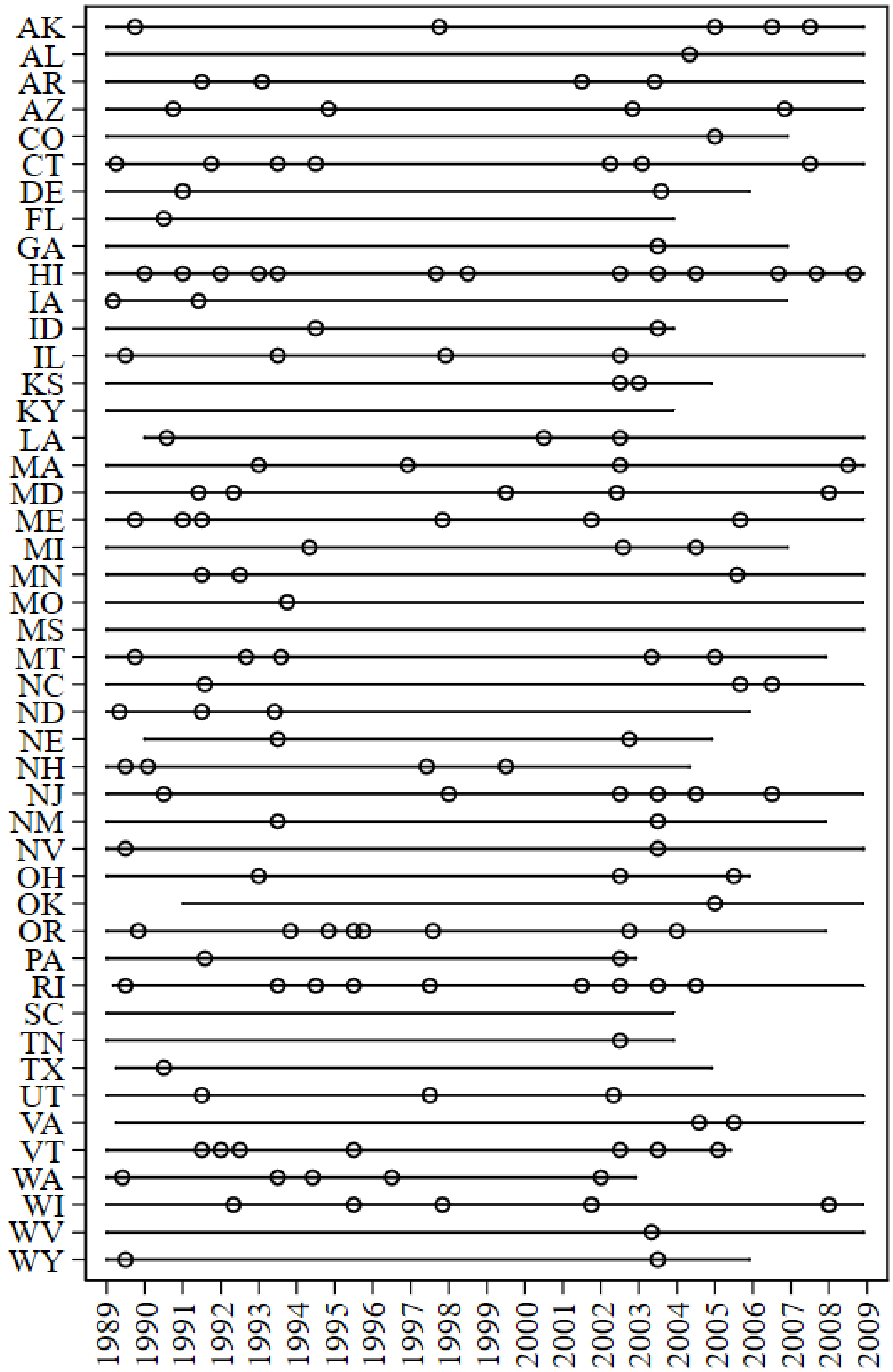
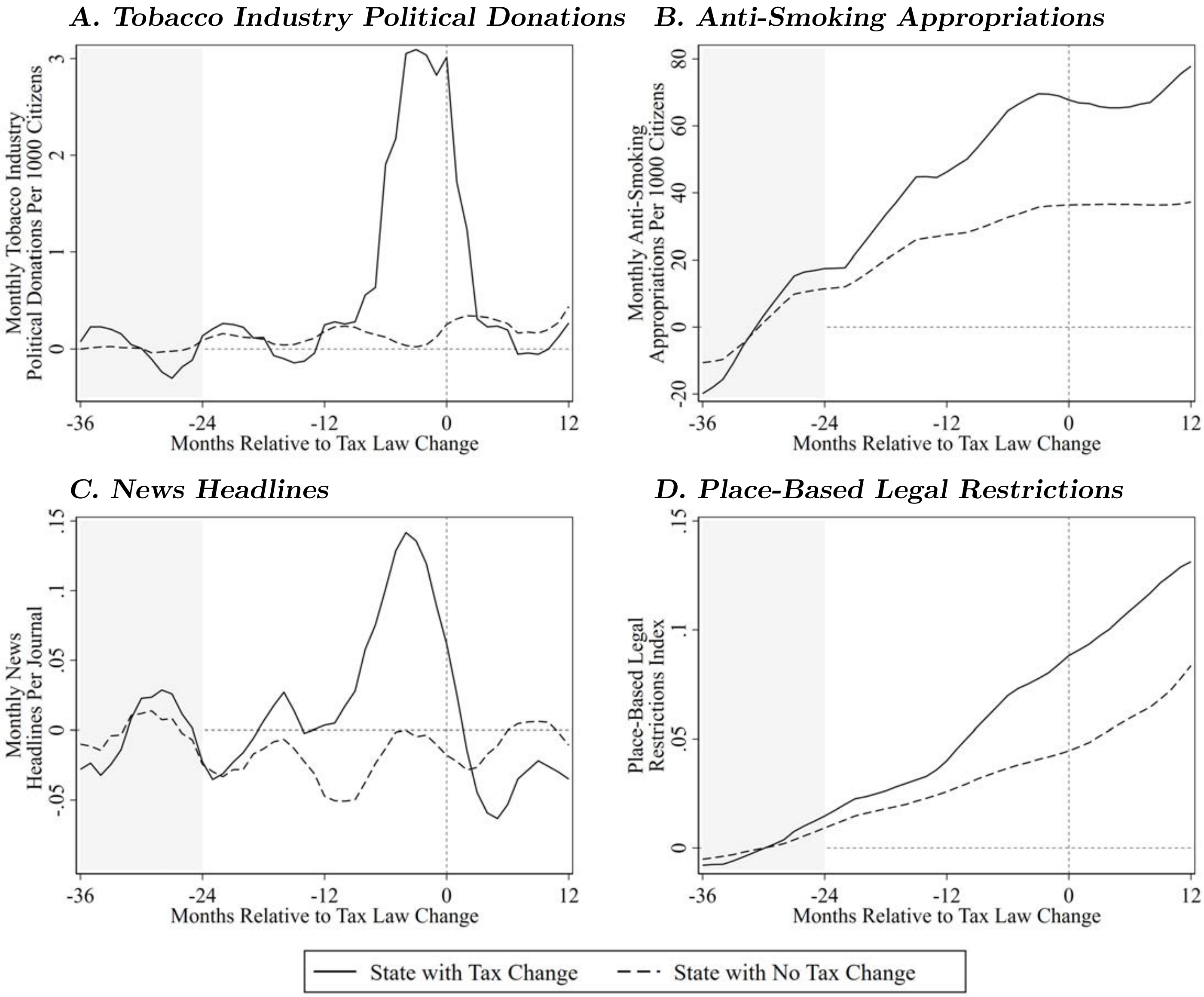
Yurekli, Ayda and Ping Zhang (2000). “The Impact of Clean Indoor–Air Laws and Cigarette Smuggling on Demand for Cigarettes: An Empirical Model.” Health Economics 9(2): 159–170.

Figure 1: Summary of State's Data Availability and Tax Law Changes



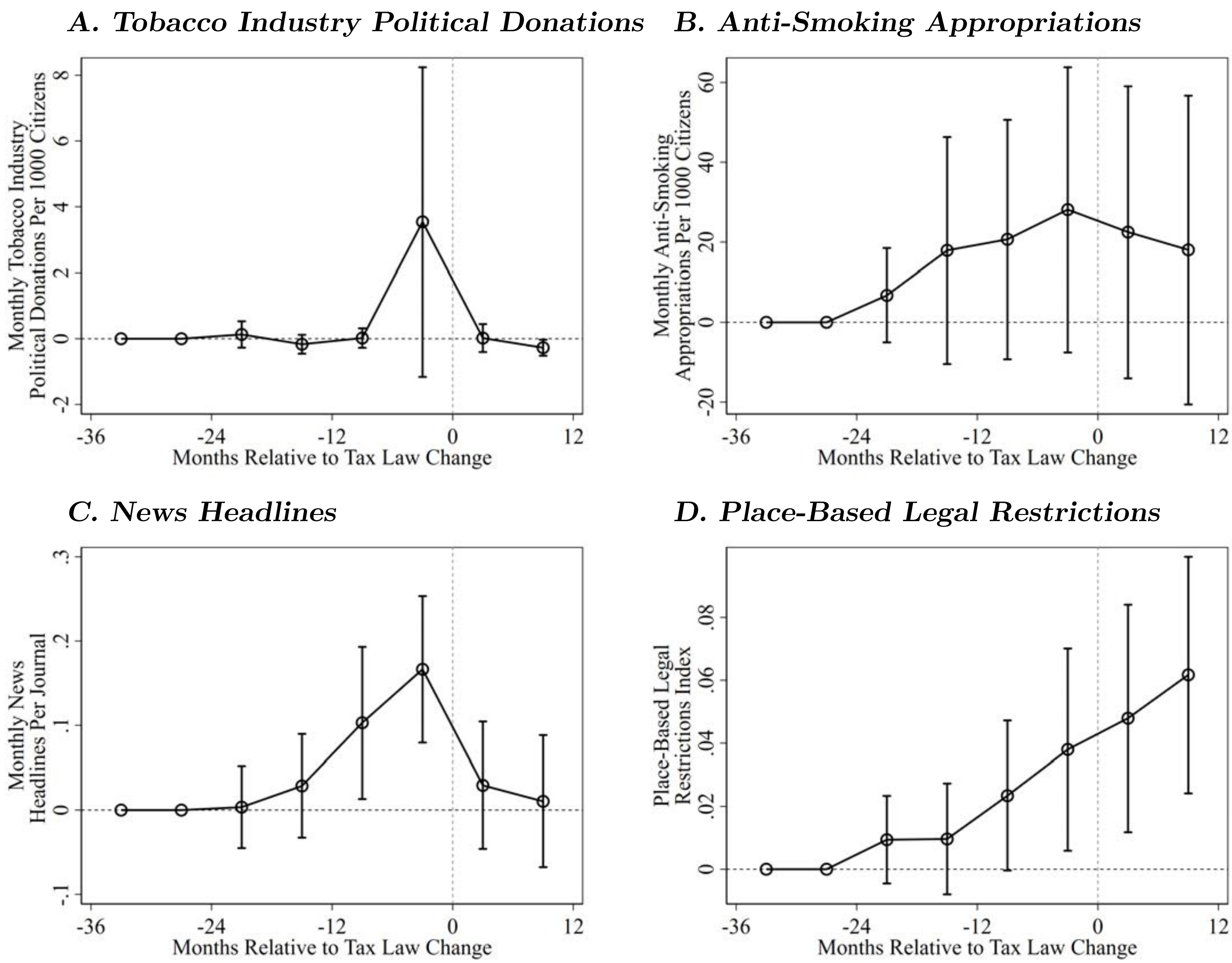
Notes: Each line indicates the years that the state is in the sample. The circle markers indicate the timing of cigarette tax changes.

Figure 2: Time-Paths of Non-Price Factors



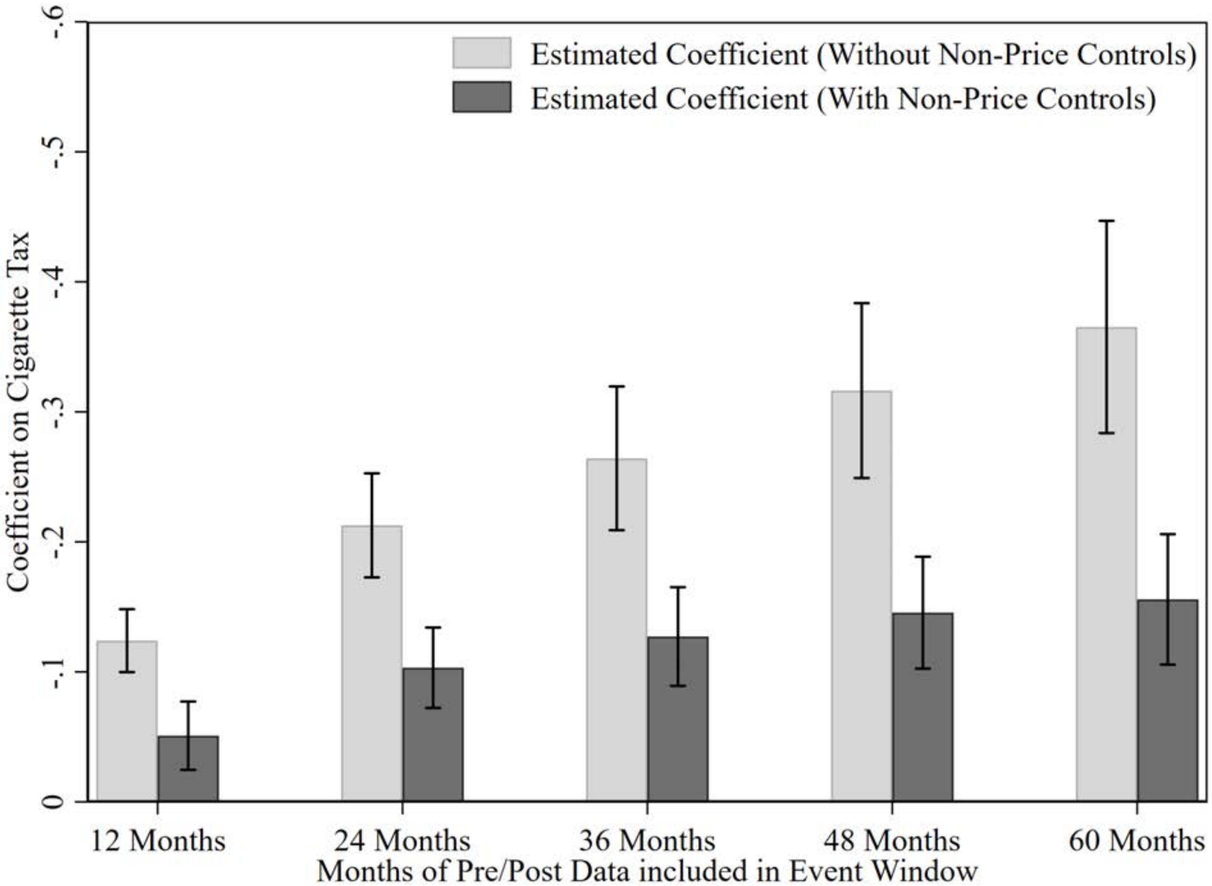
Notes: Each panel reports the evolution of a non-price factor, comparing "treatment" states with a tax change to "control" states with no tax change. We plot a six-month moving average, shifted such that the average of the non-price factor in the shaded window 25-36 months before the tax change is zero.

Figure 3: Time-Paths of Non-Price Factors: Difference-in-Difference Estimates



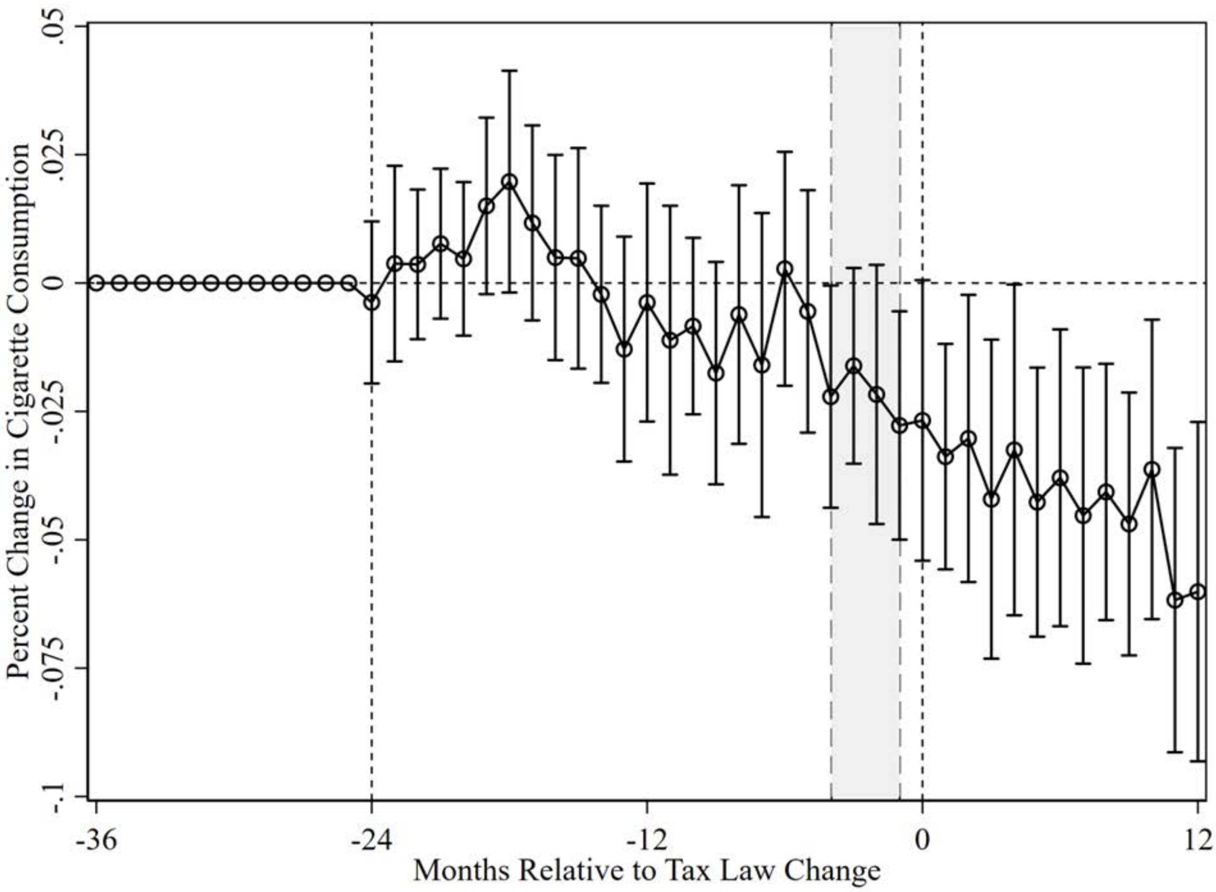
Notes: This figure reports the estimated coefficients associated with the difference-in-differences model presented in Equation 1. Capped lines indicate 90-percent confidence intervals.

Figure 4: Estimated Price Responsivity with and without Non-Price Controls



Notes: This figure reports the responsivity of demand to the state tax level from estimating Equation 2. Across columns we vary the width of the event window. Contrasting the light and dark gray bars illustrates the degree to which price responsivity declines when the non-price factors are included as control variables.

Figure 5: Time Path of Consumption Around Tax Changes



Notes: This figure reports difference-in-differences estimates illustrating the decline in cigarette consumption in states experiencing a tax change. The reported coefficients come from estimating Equation 1 with the natural log of the state tax as the dependent variable and with time binned at the month level. Capped lines indicate 90-percent confidence interval. The shaded region indicates the 25th to 75th percentile range of dates at which votes on tax law changes occurred.

Table 1: Descriptive Statistics

	Panel	Stacked Events All	Stacked Events Control	Stacked Events Treatment
Average Cigarettes Per Day	1.63	1.72	1.73	1.49
Monthly Tobacco Industry Political Donations Per 1000 Citizens	1.45	0.56	0.51	1.35
Monthly Anti-Smoking Appropriations Per 1000 Citizens	131.65	100.90	94.42	190.19
Monthly News Headlines Per Journal	0.36	0.35	0.35	0.35
Place-Based Legal Restrictions Index	0.44	0.34	0.33	0.47

Notes: Column 1 presents averages of our primary variables of interest in our panel data. Columns 2-4 present averages from our "stacked event study" dataset applying an event window covering 36 months before the tax change to 12 months after.

Table 2: Within-Tax-Change-Event Predictors of Cigarette Consumption

	ln(Cigarettes)					
	(1)	(2)	(3)	(4)	(5)	(6)
State Tax	-0.264***	-0.257***	-0.196***	-0.203***	-0.227***	-0.127***
	(0.033)	(0.034)	(0.032)	(0.026)	(0.031)	(0.023)
Tobacco Industry Political Donations		-0.070				-0.022
Per 1000 Citizens (Cumulative)		(0.089)				(0.054)
Anti-Smoking Appropriations			-0.005***			-0.004**
Per 1000 Citizens (Cumulative)			(0.002)			(0.001)
News Headlines				-0.005***		-0.005***
Per Journal (Cumulative)				(0.001)		(0.001)
Place-Based Legal					-0.152***	-0.087**
Restrictions Index					(0.031)	(0.034)
State-Event Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	9,642	9,642	9,642	9,642	9,642	9,642

Notes: This table reports the estimated coefficients from Equation 2. Standard errors are in parentheses and are corrected using multi-dimensional clustering that allows for correlation within event, within state, and within year-month. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Within-Tax-Change-Event Predictors of Anticipatory Cigarette Consumption Response

	ln(Cigarettes)					
	(1)	(2)	(3)	(4)	(5)	(6)
Effective State Tax	-0.266***	-0.259***	-0.198***	-0.205***	-0.229***	-0.128***
	(0.033)	(0.034)	(0.033)	(0.026)	(0.032)	(0.023)
Size of Enacted Tax Change	-0.081***	-0.076***	-0.069***	-0.050**	-0.062***	-0.030
(Prior to Being Effective)	(0.021)	(0.023)	(0.022)	(0.020)	(0.023)	(0.021)
Tobacco Industry Political Donations		-0.066				-0.021
Per 1000 Citizens (Cumulative)		(0.089)				(0.054)
Anti-Smoking Appropriations			-0.005***			-0.004**
Per 1000 Citizens (Cumulative)			(0.002)			(0.001)
News Headlines				-0.005***		-0.005***
Per Journal (Cumulative)				(0.001)		(0.001)
Place-Based Legal					-0.150***	-0.086**
Restrictions Index					(0.031)	(0.034)
State-Event Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	9,642	9,642	9,642	9,642	9,642	9,642

Notes: This table reports the estimated coefficients from Equation 2 with the inclusion of a variable for the tax rate that has been enacted but is not yet effective, labeled "Size of Enacted Tax Change (Prior to Being Effective)." Standard errors are in parentheses and are corrected using multi-dimensional clustering that allows for correlation within event, within state, and within year-month. * p<0.1, ** p<0.05, *** p<0.01.

Appendix Analyses

Assumption of cumulative effects: In our analyses, our measurements of news headlines, anti-smoking advertising, and political donations capture the cumulative amount leading up to the current month, thus assuming persistent effects of the non-price factors. We believe that the history of such activities affects current norms and sentiments regarding smoking, which justifies this cumulative treatment. For comparison, Appendix Table A3 reports the results for the current month non-price factors. We similarly find evidence that non-price factors predict consumption, although the quantitative reduction of estimated price effects is less when ignoring cumulative effects.

Role of functional form assumptions: The analyses in Table 2 assume a log-linear relationship between state taxes and cigarette consumption. Although this functional form assumption is common, it is possible that the estimates are influenced by specification error. To investigate the sensitivity of the estimates to functional form assumptions, we modify the regression framework to allow for a nonparametric relationship between taxes and cigarette consumption. In particular, we estimate the regressions in Columns 1 and 6 of Table 2 but replace the linear state tax variable with a cubic spline.[23] Appendix Figure A3 plots the estimated splines, which may be interpreted as estimated demand curves identified from within-event variation in taxes. The figure provides strong evidence that the curve estimated in the absence of controls is substantially steeper and spans a broader range of consumption levels than the curve with the controls. Overall, the figure indicates that controls for non-price factors substantially reduce estimated price responsivity in a manner that does not rely on the log-linear specification of our primary regressions.

Reweighting states based on number of tax changes: In our stacked event study design, states that have more tax changes correspondingly have more stacked events, and thus have more influence on the regression results. To explore the impact of this unequal influence on our regression estimates, we reproduce the results of Table 2 while inverse weighting all observations by the number of events in the state. Results are presented in Appendix Table A4. The differences between this table and Table 2 are extremely minor, leading us to conclude that our baseline approach is not unduly influenced by its inherent unequal assignment of influence.

[23] Because this spline can approximate a broad range of nonlinear relationships between taxes and consumption, we use the raw levels of cigarette consumption as our dependent variable (instead of its logarithmic transform).

Unique responsivity of pregnant smokers: The analyses thus far considered the smoking behavior of pregnant women. One concern that arises from this choice is that pregnant women might be especially responsive to non-price factors, rendering the results inapplicable to the broader population of smokers. A second concern is that the non-price factors could influence survey response bias, and pregnant women might be the group that responds the most to non-price factors by misreporting. To assess these concerns, we repeat the analyses in Table 2 using survey measurements of cigarette consumption available in the Behavioral Risk Factor Surveillance System (BRFSS). The BRFSS is a cross-sectional telephone survey overseen by the United States' Center for Disease Control, aimed to measure the health behaviors of the general populace of the United States.[24] Because the BRFSS discontinued its question on the number of cigarettes consumed in 2000, use of this dataset results in a substantially reduced number of usable tax-change events. Appendix Table A5 reports the results, which are broadly consistent with those reported above. In the log-linear regression with no non-price controls, the coefficient on State Tax is -0.381 (s.e.=0.070). With the inclusion of all non-price controls, this estimate is reduced to -0.136 (s.e.=0.055). As in Table 2, we find that the inclusion of non-price controls accounts for over half of the originally estimated price responsivity.

Potential for selection based on miscarriages: Beyond mere pregnancy, an additional form of selection influencing entry into our sample is the requirement of a live birth. Pregnancy terminating in miscarriage stops the would-be mother from entering our sample. In principle, because smoking is related to miscarriage, the nature of this selection could change in the windows before and after a tax change, and such a change can confound estimates. In practice, the effect sizes involved are sufficiently small that any such changes will have little quantitative effect. To illustrate, recall that 1) the average woman in our stacked event study smokes 1.72 cigarettes a day, 2) smoking decreased by 30 percent per dollar tax increase around the tax change, and 3) the average tax increase was \$0.25. The recent metaanalysis of Pineles et al. (2014) reports a 1 percent increase in the relative risk of miscarriage per cigarette smoked per day. Taking this all together, an average woman facing the average tax increase would decrease consumption by 0.13 cigarettes per day (30 percent response × \$0.25 average tax change × 1.72 average cigarettes a day). Based

[24] We match our stacked-event study dataset to the BRFSS dataset constructed by Goldin and Homonoff (2013), who study the relationship between cigarette tax salience and regressivity. See their paper for a complete description of the dataset.

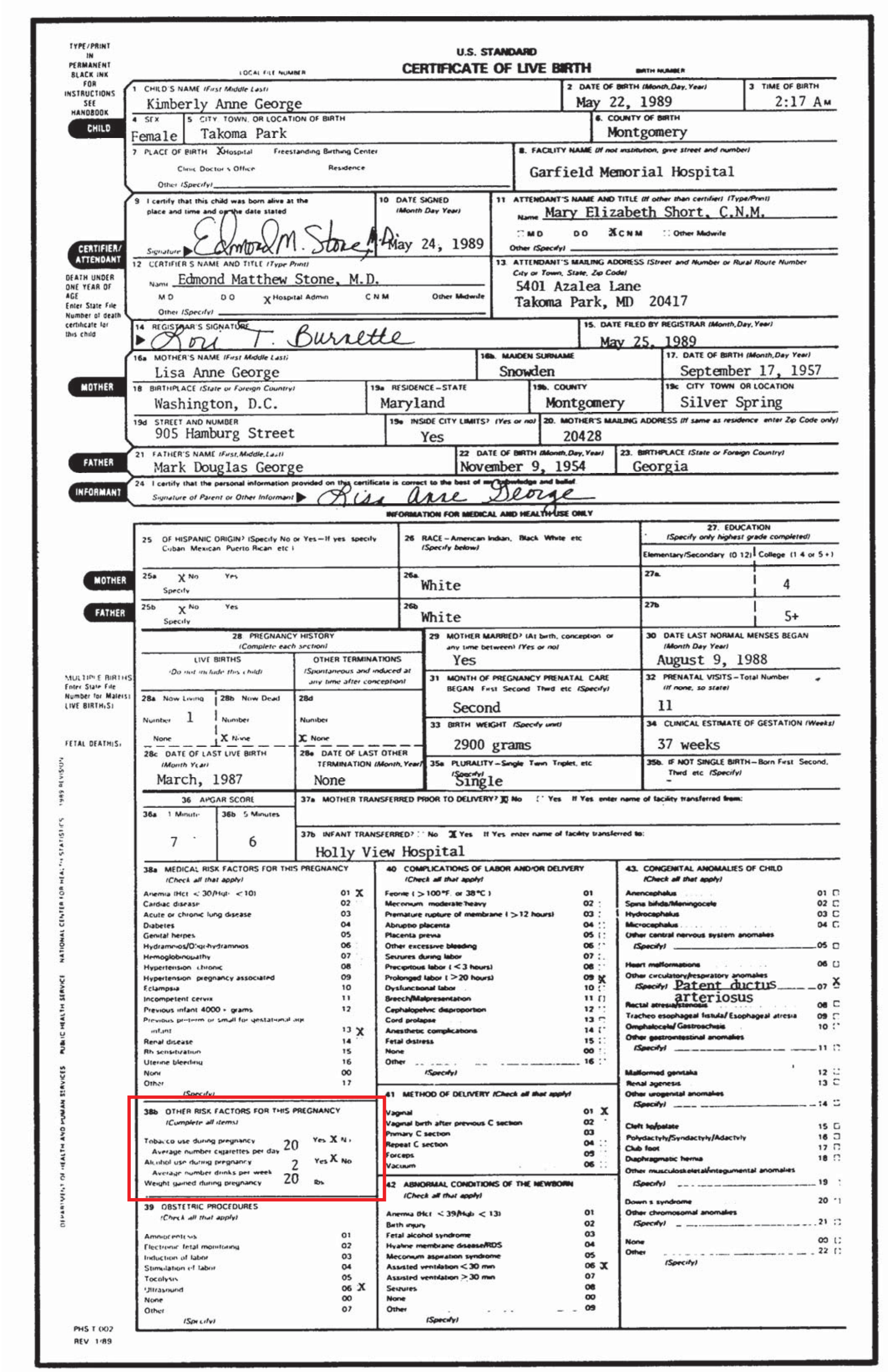
on the estimated 1 percent increase in the relative risk of miscarriage per cigarette per day, this decrease in consumption is expected to cause a 0.13 percent change in the relative risk in miscarriage. Thus, we expect selection arising from miscarriage to be quantitatively quite small.

Role of different non-price factors in reducing responsivity estimates: How do different sets of our non-price factors individually contribute to our results? And in particular, are our results captured by including single non-price factors that have been used in prior research? While most of our measures are not commonly applied, is worth emphasizing that some research has estimated price responses after controlling for place-based legal restrictions in some way (see, e.g., Yurekli and Zhang 2000, Callison and Kaestner 2013, MacLean, Kessler, and Kenkel 2016, Nesson 2017).[25] If the large reduction in estimated responsivity were mainly driven by including controls for place-based legal restrictions, these prior estimates may be relatively unconfounded by the issues we discuss. Column 5 of Table 2 suggests that only part of the reduction in price effect is explained by place-based legal restrictions. We further assess the comparative importance of place-based legal restrictions by re-running these regressions with different permutations of controls. Appendix Figure A4 reports the coefficients on cigarette taxes estimated from regressions with controls indicated on the x-axis. Controlling for the three other non-price factors reduce the price effect by nearly 50 percent relative to the baseline with only place-based legal restrictions controlled. Consistent with Table 2, the results of these analyses demonstrate that controls for news headlines and anti-smoking appropriations are responsible for the largest reductions in estimated price effects.

[25] Gruber and Koszegi (2001) do not control for place-based legal restrictions in the main analysis, but note that "controlling for the presence of various categories of clean air laws (using data described in Gruber [2000]) makes little difference to our results" (pg. 1274).

Appendix

Figure A1: Example U.S. Standard Certificate of Live Birth



U.S. STANDARD
CERTIFICATE OF LIVE BIRTH

1 CHILD'S NAME (First, Middle, Last): Kimberly Anne George
2 DATE OF BIRTH (Month, Day, Year): May 22, 1989
3 TIME OF BIRTH: 2:17 AM
4 SEX: Female
5 CITY, TOWN, OR LOCATION OF BIRTH: Takoma Park
6 COUNTY OF BIRTH: Montgomery
7 PLACE OF BIRTH: X Hospital
8 FACILITY NAME (If not institution, give street and number): Garfield Memorial Hospital
9 I certify that this child was born alive at the place and time and on the date stated. Signature: Edmond M. Stone, M.D.
10 DATE SIGNED (Month, Day, Year): May 24, 1989
11 ATTENDANT'S NAME AND TITLE: Mary Elizabeth Short, C.N.M. (X CNM)
12 CERTIFIER'S NAME AND TITLE: Edmond Matthew Stone, M.D. (X Hospital Admin)
13 ATTENDANT'S MAILING ADDRESS: 5401 Azalea Lane, Takoma Park, MD 20417
14 REGISTRAR'S SIGNATURE: Lori T. Burrette
15 DATE FILED BY REGISTRAR (Month, Day, Year): May 25, 1989
16a MOTHER'S NAME (First, Middle, Last): Lisa Anne George
16b MAIDEN SURNAME: Snowden
17 DATE OF BIRTH (Month, Day, Year): September 17, 1957
18 BIRTHPLACE (State or Foreign Country): Washington, D.C.
19a RESIDENCE—STATE: Maryland
19b COUNTY: Montgomery
19c CITY, TOWN, OR LOCATION: Silver Spring
19d STREET AND NUMBER: 905 Hamburg Street
19e INSIDE CITY LIMITS? (Yes or no): Yes
20 MOTHER'S MAILING ADDRESS (If same as residence, enter Zip Code only): 20428
21 FATHER'S NAME (First, Middle, Last): Mark Douglas George
22 DATE OF BIRTH (Month, Day, Year): November 9, 1954
23 BIRTHPLACE (State or Foreign Country): Georgia
24 I certify that the personal information provided on this certificate is correct to the best of my knowledge and belief. Signature of Parent or Other Informant: Lisa Anne George

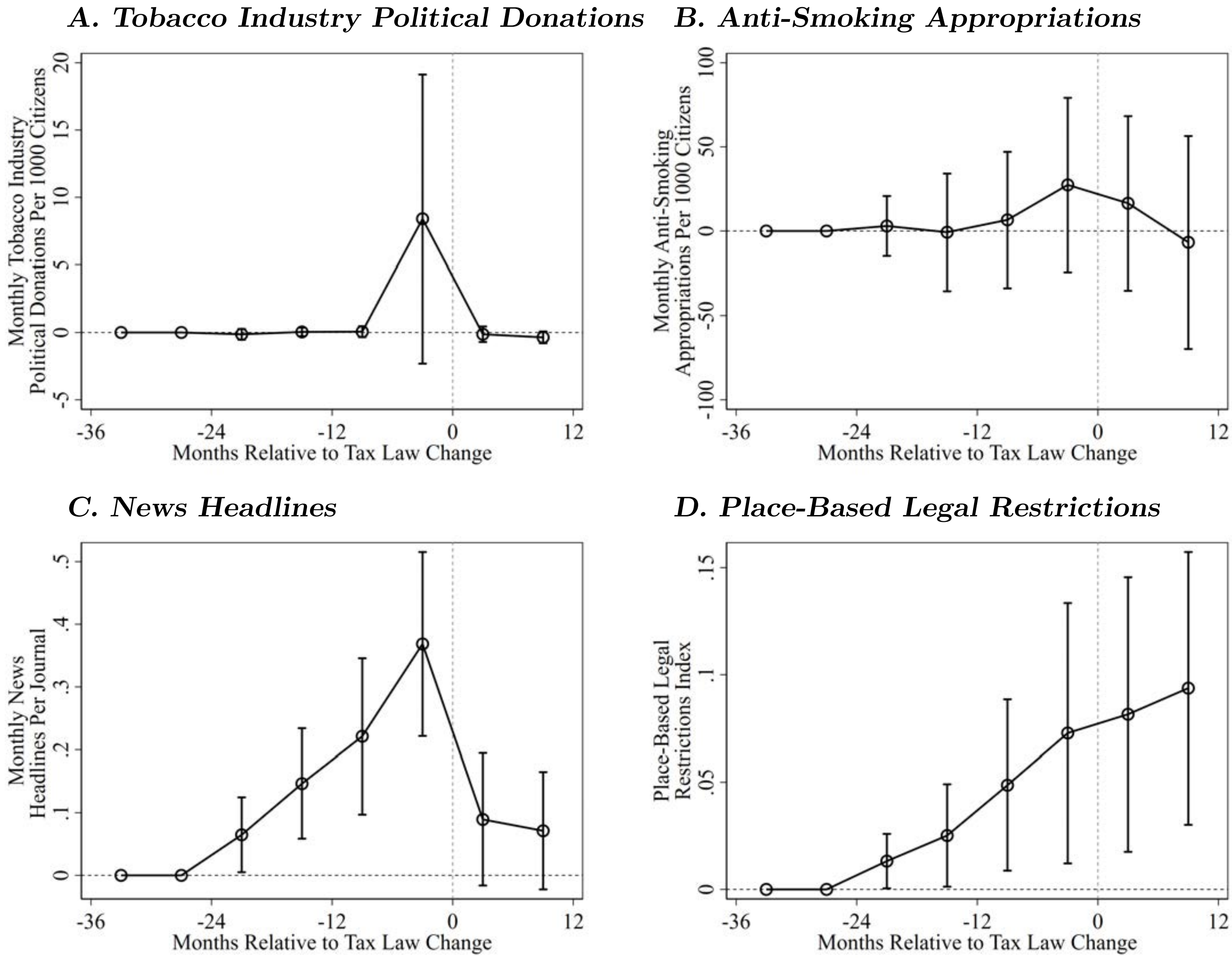
INFORMATION FOR MEDICAL AND HEALTH USE ONLY

25 OF HISPANIC ORIGIN? 25a (Mother): X No; 25b (Father): X No
26 RACE: 26a (Mother): White; 26b (Father): White
27 EDUCATION (Specify only highest grade completed): 27a (Mother): College 4; 27b (Father): College 5+
28 PREGNANCY HISTORY: 28a Now Living: Number 1; 28b Now Dead: X None; 28d Other terminations: X None; 28c DATE OF LAST LIVE BIRTH: March, 1987; 28e DATE OF LAST OTHER TERMINATION: None
29 MOTHER MARRIED? Yes
30 DATE LAST NORMAL MENSES BEGAN: August 9, 1988
31 MONTH OF PREGNANCY PRENATAL CARE BEGAN: Second
32 PRENATAL VISITS—Total Number: 11
33 BIRTH WEIGHT: 2900 grams
34 CLINICAL ESTIMATE OF GESTATION (Weeks): 37 weeks
35a PLURALITY: Single
36 APGAR SCORE: 36a 1 Minute: 7; 36b 5 Minutes: 6
37a MOTHER TRANSFERRED PRIOR TO DELIVERY? X No
37b INFANT TRANSFERRED? X Yes — Holly View Hospital
38a MEDICAL RISK FACTORS FOR THIS PREGNANCY: Anemia (Hct. < 30/Hgb. < 10) 01 X; Previous preterm or small-for-gestational-age infant 13 X
38b OTHER RISK FACTORS FOR THIS PREGNANCY: Tobacco use during pregnancy Yes X; Average number cigarettes per day 20; Alcohol use during pregnancy Yes X; Average number drinks per week 2; Weight gained during pregnancy 20 lbs
39 OBSTETRIC PROCEDURES: Ultrasound 06 X
40 COMPLICATIONS OF LABOR AND/OR DELIVERY: Prolonged labor (> 20 hours) 09 X
41 METHOD OF DELIVERY: Vaginal 01 X
42 ABNORMAL CONDITIONS OF THE NEWBORN: Assisted ventilation < 30 min 06 X
43 CONGENITAL ANOMALIES OF CHILD: Other circulatory/respiratory anomalies (Specify) Patent ductus arteriosus 07 X

PHS-T-002
REV. 1/89

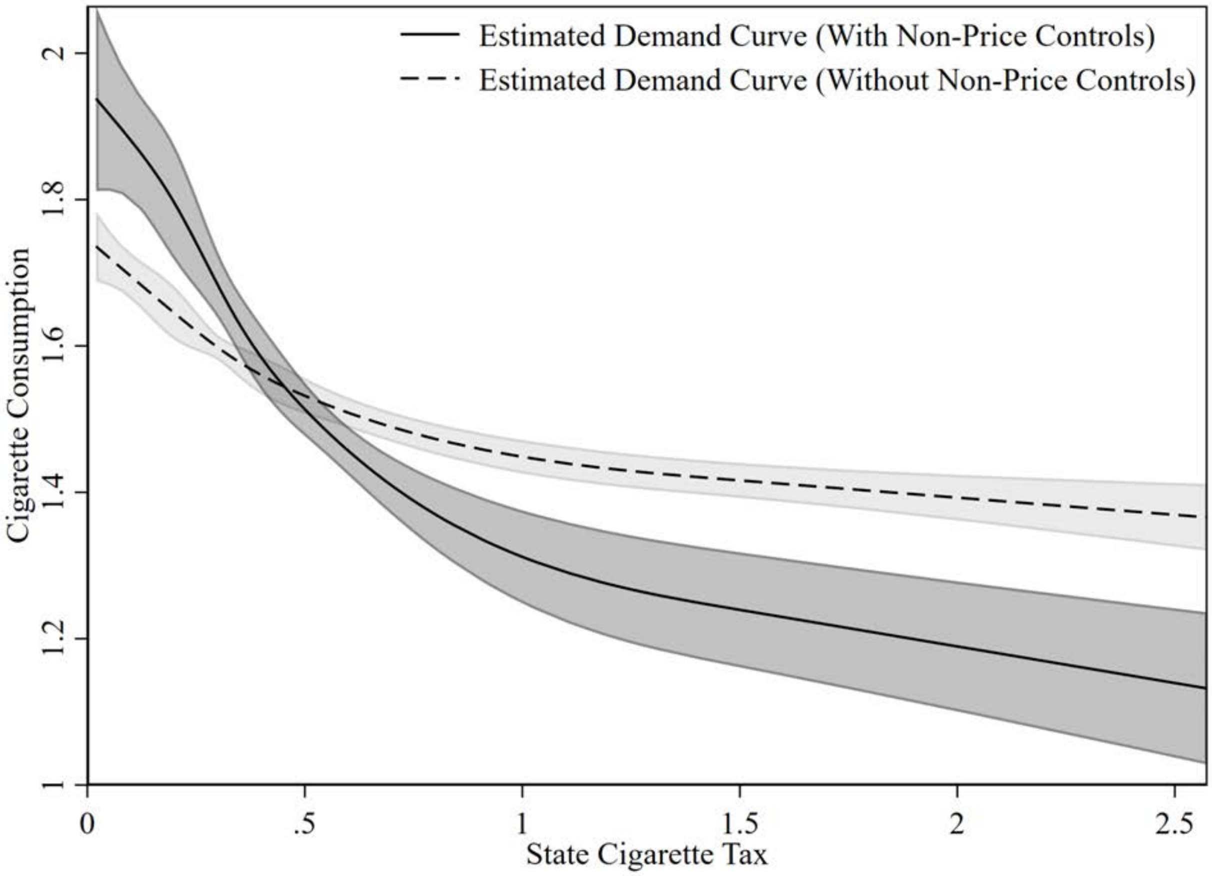
Source: National Center for Health Statistics (1987).

Figure A2: Time-Paths of Non-Price Factors With Non-Overlapping Events



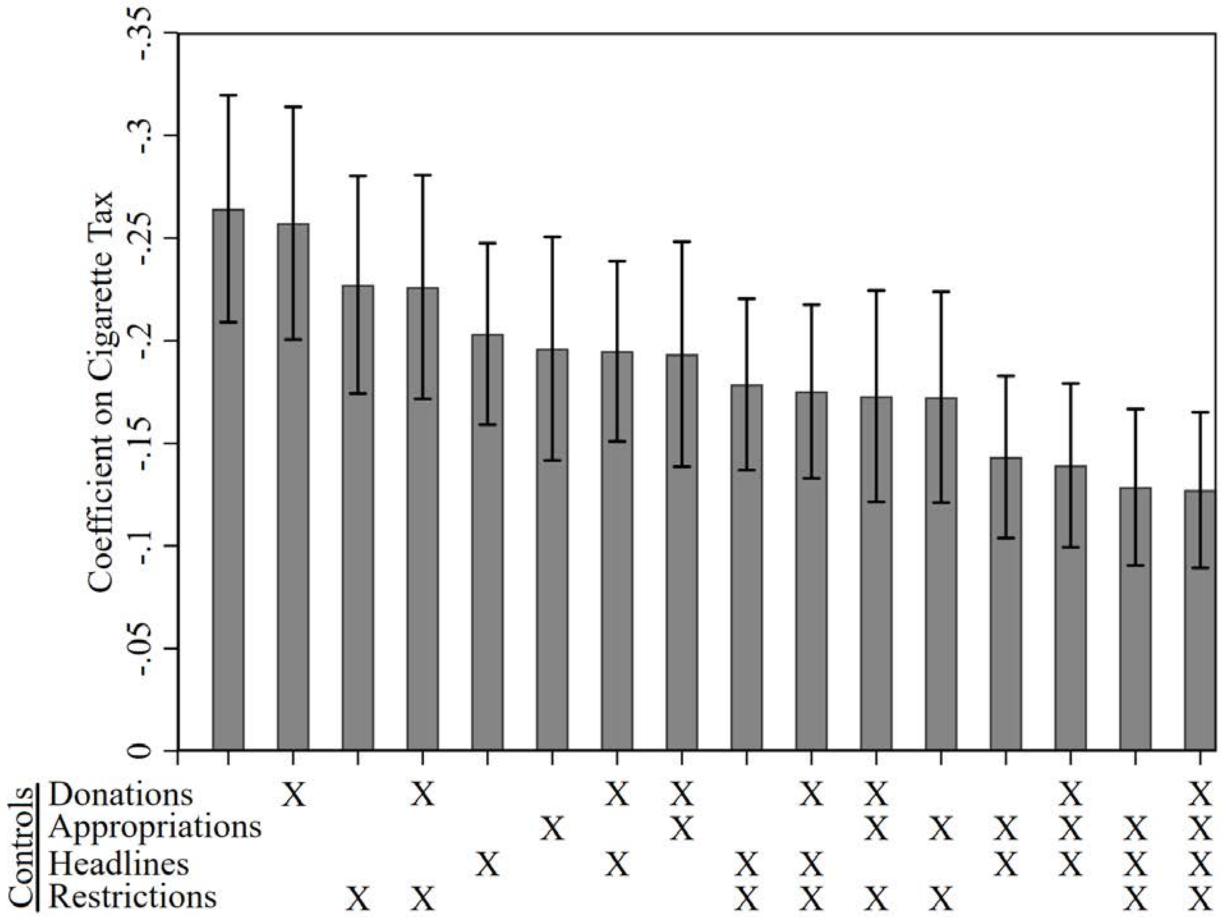
Notes: This figure reproduces the analysis of Figure 3 while excluding all tax-change events that have another tax change fall within their event window. Capped lines indicate 90-percent confidence intervals.

Figure A3: Cigarette Demand With and Without Non-Price Controls



Notes: This figure plots semiparametric estimates of the cigarette demand curve with or without non-price controls. We estimate Equation 2 while replacing the linear "statetax" term with a cubic spline. As in Table 2, estimates are identified from within-state variation with data restricted to a 3-year window surrounding a tax change. Estimates "with controls" linearly control for all non-price terms considered in Table 2. The shaded region reports 90 percent confidence intervals.

Figure A4: Specification Curve: Estimated Price Responsivity with Different Sets of Controls



Notes: This figure reports a specification curve of the responsivity of demand to the state tax level from estimating Equation 2. The bottom of the figure indicates the set of control variables included in the specification. The specifications are sorted on the estimated coefficient. Capped lines indicate 90-percent confidence intervals.

Table A1: Predictors of Cigarette Consumption in States without Tax Changes

	ln(Cigarettes)				
	(1)	(2)	(3)	(4)	(5)
Tobacco Industry Political Donations	-3.095***				-1.543***
Per 1000 Citizens (Cumulative)	(0.139)				(0.090)
Anti-Smoking Appropriations		-0.008***			-0.003***
Per 1000 Citizens (Cumulative)		(0.001)			(0.001)
News Headlines			-0.007***		-0.006***
Per Journal (Cumulative)			(0.000)		(0.000)
Place-Based Legal				-0.323***	-0.106***
Restrictions Index				(0.041)	(0.023)
State-Event Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	114,067	114,067	114,067	114,067	114,067

Notes: This table reports the estimated coefficients from Equation 2. Unlike Table 2, which presents estimates derived from a sample of only treated states, these estimates are derived from a sample of only control states. Control states are those that have no tax change during the event window (which covers 36 months before and after the event) or in the six months immediately before or after the event window. The state tax variable is excluded because it is constant within these control states. Standard errors are in parentheses and are corrected using multi-dimensional clustering that allows for correlation within event, within state, and within year-month. * p<0.1, ** p<0.05, *** p<0.01.

Table A2: Within-Tax-Change-Event Predictors of Cigarette Consumption: News Headlines with and without the Word Tax

	ln(Cigarettes)			
	(1)	(2)	(3)	(4)
State Tax	-0.210***	-0.211***	-0.208***	-0.130***
	(0.027)	(0.027)	(0.026)	(0.023)
News Headlines Per Journal (Cumulative)				
Cigarette with Tax	-0.012***		-0.002	-0.001
	(0.002)		(0.003)	(0.003)
Cigarette without Tax		-0.007***	-0.006***	-0.007***
		(0.001)	(0.001)	(0.001)
Tobacco Industry Political Donations Per 1000 Citizens (Cumulative)				-0.027
				(0.054)
Anti-Smoking Appropriations Per 1000 Citizens (Cumulative)				-0.004***
				(0.001)
Place-Based Legal Restrictions Index				-0.083**
				(0.033)
State-Event Fixed Effects	Yes	Yes	Yes	Yes
N	9,642	9,642	9,642	9,642

Notes: This table reproduces the results of Table 2 but separates our measure of the number of headlines containing the word "cigarette" into two measures depending on whether the headline also contains the word "tax." Standard errors are in parentheses and are corrected using multi-dimensional clustering that allows for correlation within event, within state, and within year-month. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A3: Non-Price Factors Measured Contemporaneously

	ln(Cigarettes)					
	(1)	(2)	(3)	(4)	(5)	(6)
State Tax	-0.264***	-0.264***	-0.253***	-0.259***	-0.227***	-0.217***
	(0.033)	(0.033)	(0.031)	(0.032)	(0.031)	(0.029)
Tobacco Industry Political Donations		-0.138*				-0.181**
Per 1000 Citizens		(0.072)				(0.074)
Anti-Smoking Appropriations			-0.147***			-0.139***
Per 1000 Citizens			(0.038)			(0.038)
News Headlines				-0.003		-0.002
Per Journal				(0.005)		(0.005)
Place-Based Legal					-0.152***	-0.132***
Restrictions Index					(0.031)	(0.031)
State-Event Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	9,642	9,642	9,642	9,642	9,642	9,642

Notes: This table reproduces the results of Table 2, replacing all cumulative non-price factor measures with their contemporaneous counterparts. Standard errors are in parentheses and are corrected using multi-dimensional clustering that allows for correlation within event, within state, and within year-month. * p<0.1, ** p<0.05, *** p<0.01.

Table A4: Within-Tax-Change-Event Predictors of Cigarette Consumption (Inverse Weighting for the Number of Events in the State)

	ln(Cigarettes)					
	(1)	(2)	(3)	(4)	(5)	(6)
State Tax	-0.260***	-0.258***	-0.182***	-0.200***	-0.221***	-0.126***
	(0.033)	(0.032)	(0.030)	(0.028)	(0.033)	(0.020)
Tobacco Industry Political Donations		-0.012				0.018
Per 1000 Citizens (Cumulative)		(0.091)				(0.078)
Anti-Smoking Appropriations			-0.006***			-0.004**
Per 1000 Citizens (Cumulative)			(0.002)			(0.002)
News Headlines				-0.004***		-0.004***
Per Journal (Cumulative)				(0.001)		(0.001)
Place-Based Legal					-0.146***	-0.076***
Restrictions Index					(0.030)	(0.028)
State-Event Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	9,642	9,642	9,642	9,642	9,642	9,642

Notes: This table reproduces the results of Table 2 while inverse-weighting all observations based on the number of tax-change events in the state. Standard errors are in parentheses and are corrected using multi-dimensional clustering that allows for correlation within event, within state, and within year-month. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A5: Reproducing Table 2 Using BRFSS Data

	ln(Cigarettes)					
	(1)	(2)	(3)	(4)	(5)	(6)
State Tax	-0.381***	-0.330***	-0.360***	-0.189***	-0.373***	-0.136**
	(0.070)	(0.073)	(0.070)	(0.058)	(0.070)	(0.055)
Tobacco Industry Political Donations		-1.407***				-1.353***
Per 1000 Citizens (Cumulative)		(0.360)				(0.360)
Anti-Smoking Appropriations			-0.003**			-0.002***
Per 1000 Citizens (Cumulative)			(0.001)			(0.000)
News Headlines				-0.002***		-0.002***
Per Journal (Cumulative)				(0.000)		(0.000)
Place-Based Legal					-0.039	0.054
Restrictions Index					(0.059)	(0.045)
State-Event Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	4,980	4,980	4,980	4,980	4,980	4,980

Notes: This table reproduces the results of Table 2 using data from the Behavioral Risk Factor Surveillance System (BRFSS). Standard errors are in parentheses and are corrected using multi-dimensional clustering that allows for correlation within event, within state, and within year-month. * p<0.1, ** p<0.05, *** p<0.01.