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

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Price Isn't Everything: Behavioral Response around Changes in Sin Taxes

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ABSTRACT

Taxes change behavior. But how does this change arise? In traditional economic models, change is achieved through the price channel: assuming all else is held constant, taxes increase prices and thus decrease demand. However, the assumption that all else is held constant may be violated in the course of a legal change, in part because the process by which laws are changed often involves the provision of information, attempts at persuasion, and the deployment of alternative dissuasive tools. We examine violations of this assumption in a particular policy domain: discouraging smoking with cigarette taxes. We document a marked increase in related media coverage, lobbying efforts, place-based smoking restrictions, and anti-smoking appropriations in the time period surrounding a tax law change. The intensity of these factors is directly associated with decreases in cigarette consumption in a manner that could be confused with price effects. Our results suggest that price effects may have a surprisingly small role in the behavioral response that occurs around tax law changes.

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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 commodity 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. Changing the law typically requires making the case that the law *should* change, and interested parties attempt to influence the process with the provision of information, persuasive appeals, and alternative non-price interventions. 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).¹ Although an important role for such expressive powers is prominent in the legal literature,² this concept is rarely

¹ Different legal theories exist to explain how the expressive function of law operates. For example, an attitudinal theory of expressive law posits that laws signal the underlying social attitudes towards a behavior, and citizens who are motivated to avoid societal disapproval change behavior in response to laws updating their beliefs (McAdams, 2000a); an informative theory of expressive law posits that the lawmaking process aggregates information on the costs and benefits of a behavior, and citizens change behavior in response to laws updating their beliefs (Dharmapala and McAdams, 2003); and a focal point theory of expressive law posits that laws influence expectations about how citizens will behave and thus guide the behavior of citizens with a common interest in coordinating (Posner, 1996; Strauss, 1996; McAdams, 2000b). A large literature in law analyzes how laws can and should change behavior through social norms (McAdams, 1997; Cooter, 1998; Posner and Rasmusen, 1999; Posner, 2000; McAdams and Rasmusen, 2007). Social norms could change behavior either by informing citizens of others' behavior in a morally neutral way (Picker, 1997; Posner, 2000; Mahoney and Sanchirico, 2001) or by representing unwritten rules of behavior and thus signaling moral disapproval (Ellickson, 1991; Cooter, 1996; Kahan, 1996, 1998; McAdams, 1997; Kaplow and Shavell, 2002).

² See, e.g., Pildes and Niemi, 1993; Sunstein, 1994; Lessig, 1995; Sunstein, 1996; Robinson and Darley, 1997; Cooter, 1998; Pildes, 1998; Mazzone, 1999; Posner, 2000; Alder, 2000; Nadler, 2005; Dana, 2007; McAdams, 2015; Schauer, 2015; Linos and Twist, 2016; Nadler, 2017; Richards, 2017. Following a line of work on expressive law (e.g., Kornhauser, 1998; Adler, 2000; McAdams, 2000a, 2000b; Dharmapala and McAdams, 2003), we distinguish the expressive effect of law as defined

considered in economic applications.³ Relatively little empirical work measures the importance of a tax's expressive effect as compared to its price effect.⁴

In this article, we assess the relative importance of a tax's price effect as compared to a set of *non-price factors*. Our set of non-price factors consists of potentially important dimensions of expressive effects, broadly defined to include effects generated throughout the lawmaking process.⁵ Our measures approximate the information provision in the social debate taking place through media outlets, persuasive activities undertaken in the political process including campaign contributions and targeted appropriations, and attempts to dissuade the targeted behavior through alternative laws or policies. We acknowledge that the precise measurement of expressive effects is extremely challenging, and that the list of proxies we consider is incomplete. However, even with imperfectly measured proxies for the activities pursued in the process of legal change, we find substantial evidence that non-price factors relating to expressive effects can be just as powerful in changing behavior as the tax itself.

above from a related concept of expressive law dealing with how law can have moral value independent of whether it changes behavior because of what it expresses (e.g., Adler 2000a, 2000b; Anderson and Pildes, 2000). In particular, we are interested in what McAdams (2015, pgs. 13-17) refers to as an *expressive theory of law's effects*, which McAdams distinguishes from other categories of expressive claims made in the legal literature.

³ 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 and not through changing demand itself.

⁴ For some measurement of expressive effects, see, e.g., McAdams and Nadler, 2005; Funk, 2007; Wittlin, 2011; Dwenger et al., 2016; Fabbri and Hoepfner, 2018.

⁵ As discussed in Section 2, these factors are related to the idea of anti-smoking sentiment, the importance of which has been documented (e.g., DeCiccia et al., 2008).

In our empirical analysis, we 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 interest because of its dramatic consequences for infant health (Evans, Ringel, and Stech 1999). Furthermore, in prominent papers on the response to sin taxation, this behavior has been used as a proxy for smoking more broadly (e.g., Gruber and Köszegi 2001), in part because of the unusual availability of high-frequency measurements of smoking that are comparable across states and over time. We use the smoking information that is reported on the U.S. Standard Certificate of Live Birth,⁶ a permanent legal record required for all live births.⁷ 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-path of four non-price factors during the lawmaking process: place-based legal restrictions on cigarette smoking (such as bans on smoking in restaurants or workplaces), news headlines related to cigarettes, anti-smoking appropriations (used to fund anti-smoking advertising, among other things), and tobacco industry spending on political donations to state politicians. We document substantial changes in these factors during the lawmaking process. Compared to states not

⁶ See Figure A1 for an example.

⁷ The manner in which these data are collected help alleviate concerns of survey response bias: the procedure for collecting smoking information starts with using medical records, physician reports, or observed behaviors; if these sources are unavailable, smoking information is recorded by the mother's report (National Center for Health Statistics, 1987). 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.

facing tax law changes, the period before a tax law change is marked by a sizable increase in newspaper coverage and political donations. This illustrates that tax changes occur during time periods of substantial information provision and attempts at persuasion. The period before and after a tax law change is also marked by an increase in both place-based legal restrictions and anti-smoking appropriations, illustrating the simultaneous use of other dissuasive policies. These findings establish that potentially important non-price factors are not held constant in the lawmaking process.

Second, we examine the relative importance of price and non-price factors on the decrease in cigarette consumption that occurs around changes in tax law. We demonstrate that this decrease in consumption is partly explained by the adoption of place-based legal restrictions, increases in newspaper coverage, and increases in anti-smoking appropriations, even after accounting for the changes in cigarette taxes. Controlling for even our partial list of non-price factors dramatically influences the estimated importance of price effects: the inclusion of these controls reduces the estimated responsiveness to the tax by roughly half.

Taken together, these results suggest that a meaningful degree of the decrease in demand occurring during a sin tax change arises from non-price factors. 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, 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 diverges well in advance of the tax change.⁸ Furthermore, the degree of this anticipatory decrease in demand is uncorrelated with the size of the tax change that is ultimately enacted, suggesting that the responses are not well explained by expectations of future price increases.

In sum, our results support the view that the process of changing tax laws changes more than just prices. While this process does unambiguously shape financial incentives, its impact on informational, psychological, and sociological factors influencing demand may be of at least comparable quantitative importance. Furthermore, changes in sin taxes are often only one prong of a multi-pronged attempt to dissuade the target behavior. A complete economic theory of sin taxation must accommodate these factors. A complete legal theory of expressive effects must better account for those that occur as a result of the ideas communicated by all the activities and debate that surround and accompany the process of legal change.

The paper proceeds as follows. Section II provides a conceptual framework for understanding the role of non-price factors. Section III describes the

⁸ For passed tax laws, these findings bear some resemblance to existing empirical tests of “rational addiction” (Becker and Murphy, 1988). Gruber and Koszegi (2001) test the hypothesis that demand will decline after a tax change is passed, but before it is effective, resulting from rationally addicted smokers reducing their degree of addiction in anticipation of a known future increase in price (see also Taylor et al. (2018) for comparable findings regarding the implementation of the Berkeley soda tax). While such forces may be active, models of rational addiction require significant modification to explain our finding that much of the suppression in demand occurs not only before the date of proposed tax changes, but also well before the moment of voting to enact the change. Furthermore, the finding that this anticipatory decline is uncorrelated with the tax change that will ultimately be enacted suggests that it may not be rationalized by rational expectations of future price changes.

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 in the model of Reif (2018), 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:

$$\begin{aligned} \max_{a_t, c_t} \quad & \sum_{t=1}^{\infty} \beta^{t-1} (U(a_t, c_t, x_t, S_t) + G(a_t, E_t[\bar{a}_t])) \\ \text{s. t.} \quad & A_0 = \sum_{t=1}^{\infty} (1+r)^{-(t-1)} (c_t + p_t a_t) \\ & S_{t+1} = (1-d)(S_t + a_t) \end{aligned}$$

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) equals lifetime discounted wealth (A_0).

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.⁹ 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 ($E_t[\bar{a}_t]$).¹⁰ 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.¹¹

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

⁹ 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).

¹⁰ 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.

¹¹ For ease of exposition, we adopt Reif’s treatment of x_t as a scalar, but note that its replacement with a vector generates no problems with the analysis that follows.

of factors that evolve in the course of a tax change are naturally accommodated by this term. We follow Reif (2018) and represent individual utility (after substituting optimal c_t into the equation) as:

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

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 (2018).

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,¹² 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

¹² 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).

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.¹³

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 (2018), 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:

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

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 equations

¹³ It is worth noting that 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 more public locations, so these restrictions could shape perceptions of smoking prevalence (Hamilton et al. 2008).

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 (Becker and Murphy, 1988; Köszegi and Gruber, 2001).¹⁴

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. In contrast to the typical treatment of variation in the non-price factors as a small confound that must be approximately controlled for, we examine whether these factors could potentially be a primary channel through which behavioral change operates in the course of a tax change.

III. DATA SOURCES AND SAMPLE DEFINITION

¹⁴ The model assumes that future prices are known. In practice, future prices are uncertain and unavailable, and some of the non-price factors we examine may additionally influence future price expectations.

Our data analysis relies on the cigarette consumption of pregnant mothers, the timing of state-level proposed tax law changes, and potential measures of non-price factors of demand suppression. 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. Research on cigarette demand typically uses one of two data sources: direct measurement of cigarette sales or survey measures of consumption. The advantage of using cigarette sales data is their clear reflection of actual purchasing behavior, often providing high-frequency measures at the state, local, retailer, household, or individual level. For many research questions, including ours, the main drawback of sales data is its limited availability for longer time series, and the concern that cigarette purchasing and consumption might diverge surrounding tax changes. As an example of the latter concern, stockpiling of cigarettes around cigarette tax changes has been well documented (Chiou and Muehlegger, 2014). Additionally, tax changes may induce smokers to travel to other tax territories for purchases (e.g., DeCicca et al., 2013a, DeCicca et al., 2013b, Lovenheim 2008, Chiou and Muehlegger 2008).

Although consumption data overcomes issues of stockpiling and travel for purchase, it comes with multiple drawbacks. First, most consumption datasets are based on surveys, and how people respond to surveys could be influenced by the policy intervention being studied. Second, very few datasets have enough coverage to establish credible measures of cigarette consumption at a granular time level.

Gruber and Köszegi (2001) discuss this issue at length. Their solution, which we adopt, is to use cigarette consumption of mothers who gave birth as measured in the Vital Statistics Detailed Natality Data Files. For the main analysis, we follow their approach to construct a measure of cigarette consumption at the state-year-month level. Like Gruber and Köszegi (2001), we note that although pregnant women are not a representative population, they are an important group to study because of the dramatic consequences of maternal smoking on infant health. Indeed, the impact of maternal smoking on infant health is thought to be the leading externality associated with smoking (Evans, Ringel, and Stech 1999).

For most states and in most years, the Natality files have recorded every birth since 1989. We follow the treatment of the smoking variable in Gruber and Köszegi (2001) and assume that it represents the average rate of consumption in the month before delivery. Unfortunately for our purposes, the details of the measure of cigarette consumption changed in the mid-2000s, and states transitioned to using this new measure in a staggered manner between 2003 and 2009. Because the new measures are not directly comparable to the old measures, we drop the state from our dataset once the older measure is no longer available.

The measures of interest are either unavailable or unreliable in California, Indiana, South Dakota, and New York, so we follow Gruber and Köszegi (2001) and drop these states. For the remaining states, we construct the state-year-month average cigarette consumption beginning in 1989 and ending at the point where the survey elicitation was changed. Figure 1 reports the window of available data for each included state, as well as the timing of tax law change events discussed below.

B. Timing of Tax Laws

We augment our data on monthly state-specific cigarette consumption with data on the state cigarette taxes in place, as well as the exact timing of all changes in state tax law. We use two datasets to identify the timing of each tax law change, including year-month of the enactment and the year-month that the tax law change becomes effective: the Centers for Disease Control’s State Tobacco Activities Tracking and Evaluation System and the National Conference of State Legislature’s Ballot Measures Database.¹⁵

C. Intensity of Political Activities and Social Debate

We form three different measures of political and social debate to serve as non-price factors. We view these as measures of activities intended to inform or influence the behaviors of either voters or lawmakers.

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

¹⁵ 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. We checked the reliability of the database on tax changes via referendum against cigarette referendums in Ballotpedia, an online encyclopedia of U.S. elections that also provides information on state referendums. In the small number of cases with discrepancies, we went to primary sources to verify the dates to use.

from 1979 to 2014.¹⁶ Our measure draws from the “contributions database” within DIME, and focuses specifically on donations to state-level candidates. DIME includes the date on which the donation was made and the state of the candidate or committee that the donation was made to, which we use to 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.¹⁷ 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 the appropriations measure 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.¹⁸ To construct the dataset, we scraped headlines from

¹⁶ 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>.

¹⁷ Because the appropriations data are available only since 1991, we code appropriations in 1989 and 1990 as those in 1991.

¹⁸ 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.

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 strength of a state’s place-based restrictions on smoking activity using the State Tobacco Activities Tracking and Evaluation System from the Centers for Disease Control and Prevention, 2018.¹⁹ This data contains historical information on all state-level legislation, including the enactment date and effective date of laws related to smoking in bars, day care centers, housing, public transportation, restaurants, workplaces, and other areas.²⁰ Our place-based legal restriction index is the average number of legal changes in each of the available domains described above.

¹⁹ The data are available at <https://chronicdata.cdc.gov/Legislation/CDC-STATE-System-Tobacco-Legislation-Smokefree-Ind/32fd-hyzc>.

²⁰ The database contains laws related to “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.” We group several of the separate classifications. Day care centers includes commercial and home-based. Casinos includes casinos, bingo halls, and racetracks. Hospitals includes hospitals and hospital campuses. Mental health facilities includes mental health outpatient and residential facilities. Substance use facilities includes outpatient and residential facilities.

E. Summary of Dataset

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.

IV. EMPIRICAL ANALYSIS

In this section, we present analysis that tests for the importance of non-price factors in explaining the decrease in cigarette consumption around tax law changes. We proceed in two steps. First, we examine the time-path of the candidate non-price factors around tax law changes. Second, we examine the relative importance of price and non-price factors in the decrease in consumption that occurs.

In these analyses, we will often employ a “stacked event study” approach. When employing this approach, we will define some window of time around a tax-change event. Considering a single event in isolation, we then proceed by examining a variable of interest across that window, comparing the “treated” state with a tax change to “control” states experiencing no tax change in that window. We define event time as the month relative to the month a tax change became effective. We create a stacked dataset containing each such event. Table 1 reports descriptive statistics of the original panel data and the stacked-event-study dataset.

The data structure in the stacked event study approach allows us to test for the significance of changes in an outcome in the treated state relative to the control states in a difference-in-differences approach.²¹ We regress an outcome on event

²¹ A difference-in-differences approach only produces unbiased estimates if the change in law is exogenous. Dating back to at least Besley and Case (2000), the possibility of endogenous policies leading to biased estimates has been acknowledged. Malani and Reif (2015) build on this literature

time indicator variables and examine the interaction between these indicators and the state with a cigarette tax change. We do not include event-time indicators for the period 2 to 3 years prior to the tax change, using the mean difference across states at this time as the pre-period in the difference-in-difference design. All analyses include state-event fixed effects to draw on within-state-event variation. The coefficients of interest are on the interaction terms between time and treatment status.

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).²² This introduces correlation between the error terms for these two control observations in the data. This is accommodated by clustering standard errors by state,²³ which additionally accounts for residual within-state variation occurring across events and across time. Second, there can be common shocks experienced by all states at given moments in time, such as due to 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

by distinguishing endogenous policies from anticipation matters and show how such distinction can matter in estimating treatment effects.

²² 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.

²³ 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.

concerned about correlation occurring across all observations within a given event, as can arise due to our selection of control states at the event level. We perform multi-way clustering using the approach of Correia (2016).

This approach to clustering is conservative and significantly reduces statistical power. Although clustering along fewer dimensions would substantially increase statistical power, we feel our approach best reflects the statistical uncertainty that exists. In essence, although sample sizes for our regressions appear large, our conceptual sample size is no larger than the 150 events which we observe. The manner in which we cluster reflects this attitude towards inference in our sample.

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

We begin by examining the time-path of the factors around tax law changes: place-based legal restrictions, cigarette news headlines, anti-smoking appropriations, and political donations made by associates of the tobacco industry. As described above, we examine a stacked event study. 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—the “control” states.²⁴

²⁴ As we will see, the process of changing the tax law decreases cigarette consumption even before the law is changed, which could contaminate the trend in any control state if that state changed tax law outside of the event window. We therefore take a conservative approach and exclude from the control states any state that witnessed a tax law change six months prior to the event window and six months subsequent to the event window. Similar results arise if we do not make this exclusion.

Figure 2 documents a stark escalation of these four non-price factors in the time surrounding a tax law change. Each panel reports the evolution of a six-month moving average. Panel A reports the place-based legal restrictions index, Panel B reports the news headlines per journal, Panel C reports the anti-smoking appropriations, and Panel D reports the tobacco industry political donations. 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, a marked spike in donation activity is seen in a narrow window leading up to the tax law change. Figure 3 plots the difference between the treated and control states, normalized by the standard deviation of this variable in control states for this window. Due to the large response in political donations, we report it separately on the right y-axis (of a larger scale than the left, which applies to all other factors). It shows that the increase in donation activity is large in magnitude compared to fluctuations in donation activity that occur in control states—it constitutes an increase by more than 10 standard deviations in the window immediately prior to a tax change.

The other measures exhibit conceptually similar differences between treated and control states, but the changes are less dramatic and 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 two years 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 the difference-in-differences approach described above. We use our stacked-event-study data and construct 6-month bins in the three years before and one year after the tax law change. We regress each of the non-price factors on the bins and the interaction between the bins and the state with a cigarette tax change (the treated state). The coefficients of interest are on the interaction terms between treated states and 6-month bins, which estimate the differential change occurring in treated states over different six-month windows. Figure 4 plots the estimated coefficients with 90 percent confidence intervals, and shows that the patterns described above are generally statistically significant. Compared to control states, states facing a tax law change witness 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.

B. Testing for Impact of Non-Price Factors

Having established that non-price factors are not constant around tax law changes and documented that the process of changing cigarette taxes explains some of the variation in the size of the debate around changes in taxes, we now assess how changes in these factors may confound inference about price effects. We focus the analysis on understanding the contribution of within-treated-state variation in

non-price factors for predicting within-treated state variation in cigarette consumption. We estimate the following regression with the stacked-event-study data:

$$\ln(\text{cigarettes})_{e,s,t} = \alpha + \beta \text{statetax}_{s,t} + \gamma X_{s,t} + \phi_{e,s} + \epsilon_{e,s,t}.$$

Subscripts denote event e in state s and year-month t . Because we are concerned only with the contribution of the non-price factors in explaining within-treated-state variation, the primary regression only includes data from the state facing a tax law change in each stacked event. We relaxed this restriction and found qualitatively similar results, illustrating that the influence of these variables is not unique to treated states.²⁵ We regress the natural log of cigarette consumption on the current state tax, non-price factors $X_{s,t}$, and state-event fixed effects ($\phi_{s,e}$). We define news headlines, anti-smoking advertising, and political donations as the cumulative amount leading up to the current month, thus assuming persistent effects of the non-price factors.²⁶ As before, we conduct multi-dimensional clustering of standard errors, allowing for correlation within event, within state, and within year-month. Because we are interested in the association of these factors with the high-frequency variation in demand occurring near a tax law change, we estimate this regression from data on the three years before and after the event. The results are reported in Table 2.

²⁵ See Table A1.

²⁶ Table A2 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.

The first column of Table 2 illustrates an unsurprising finding: consistent with the existence of price-responsivity, smoking demand is negatively associated with the state tax within the state facing a tax law change. Interpreting the magnitude of coefficients, a one dollar increase in the state tax is associated with a 26 percent decrease in cigarette consumption. In Columns 2 to 5, we separately add as controls the non-price factors that we have already seen are not constant around the tax law change. We find that the variation in place-based legal restrictions index, newspaper discussion of cigarettes,²⁷ and anti-smoking appropriations all have strongly statistically significant associations with a decrease in cigarette consumption. Qualitatively similar—but not statistically significant—results are found for tobacco industry political donations. Column 6 controls for all 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 2-5, we find that the inclusion of individual controls consistently reduces the estimated coefficient on State Tax (although in columns 2 and 5 the 95% confidence intervals

²⁷ Cigarette headlines can operate in two main ways. 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. If headlines provide information about expected future prices, the headlines could be changing behavior by increasing the salience of the future prices. We distinguish these two channels by separating the cigarette headlines into those that contain the word tax and those that do not contain the word tax. Table A3 shows that the estimated effect in Table 3 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.

still include the column 1 estimate). As seen in column 6, including all of these controls reduces the estimated responsiveness to the tax by roughly half.²⁸

B.1 Robustness Considerations

Role of the use of a 3-year time window: The regressions in Table 2 are estimated from the examination of a 3-year window around tax-change events. We find that the estimated responsiveness to the tax is substantially diminished when non-price controls are included. This finding is robust to the use of different time windows around the event. Figure 5 reports the coefficients on state tax estimated with (as in column 6 of Table 2) and without (as in column 1 of Table 2) non-price controls, using time windows ranging from one to five years. Although estimated price effects are systematically larger for larger time windows, the inclusion of non-price controls substantially reduces the estimated effect in all time windows considered.

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 driven 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

²⁸ This interpretation requires the assumption that the increase in cigarette prices from the increase in taxes takes effect in the month the tax increase becomes effective, which is supported by strong evidence that cigarette prices are stable until the month of the tax change (Rozema 2018) and even until the week of the tax change (Harding et al. 2012). This assumption does require the absence of anticipatory price effects. As discussed below, we accounted for anticipatory response to known future tax increases in alternative specifications by controlling for the enacted tax (e.g., Gruber and Koszegi, 2001; Taylor et al., 2018).

state tax variable with a cubic spline.²⁹ Figure 6 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 responsiveness in a manner that does not rely on the log-linear specification of our primary regressions.

Unique responsiveness of pregnant smokers: The analyses thus far have considered the smoking behavior of pregnant women. We study this group because of intrinsic interest in this “sin” behavior and because there are desirable data properties described in Section III. 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

²⁹ 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).

general populace of the U.S.³⁰ 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 A4 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 responsiveness.

Role of place-based legal restrictions: It is worth emphasizing that some research has estimated price responses after controlling for place-based legal restrictions in some way (e.g., Yurekli and Zhang 2000, Callison and Kaestner 2013, MacLean, Kessler, and Kenkel 2016, Nesson 2017).³¹ If the large reduction in the price effect witnessed from controlling for all the non-price factors is mainly driven by the place-based legal restrictions, prior estimates of the price effect would be close to the true price effect. Column 2 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. Figure A2 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

³⁰ 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.

³¹ 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).

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.

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 7 plots the evolution of the time-path of cigarette consumption in treated states compared to control states. The figure is a version of the stacked-event study regression design described above. We regress the natural log of cigarette consumption on event time indicators from 2 years before the tax change to 1 year after, allowing the period of time 3 years before the tax change to 2 years before the tax change to serve as the pre-period. The figure reports coefficients on the interactions between the event time indicators and the state with a cigarette tax change, which estimate the differential change occurring in treated states in the event time. Although the 90 percent confidence intervals are generally large given the conservative approach to clustering standard errors, the

divergence in demand between treatment and control states appears to be a gradual process beginning over a year in advance of the tax change.

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 (e.g., Gruber and Köszegi 2001). Note that the anticipatory effect we document here is occurring substantially earlier: the shaded region of Figure 7 illustrates the interquartile range of when votes on tax changes occurred 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.

Building on this observation, we investigate how accommodating the regression framework of Gruber and Köszegi (2001) affects our results. The key element of their approach is regressing cigarette consumption not only on 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 that has already been enacted.

Table 3 presents results for regressions like those in Table 2 including a single additional variable. 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 Köszegi (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 2-5, however, we see that even with the inclusion of this variable, the relevance of the non-price factors that we have 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. Furthermore, the coefficient on the Gruber and Köszegi measure of anticipation of the price change becomes smaller and is not statistically significant at conventional levels. This suggests that the strength of the statistical evidence in support of Gruber and Köszegi'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.

In short, the nature of the anticipatory consumption response to tax changes is consistent with non-price factors having a direct influence on demand. Furthermore, the nature of this response is difficult to rationalize through existing accounts of rationally-addicted anticipation of price effects.

V. DISCUSSION

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 have documented that all else is not held equal in the course of a tax law change, and that behavioral response may also be attributed to non-price factors.

The results align with recent findings in several disparate domains, each demonstrating that more than financial incentives change in the course of a policy change. In a particularly clear example, Abouk and Adams (2013) find that the decline of accidents after texting bans is better explained by the bans' announcements than the bans themselves. Relatedly, Richwine et al., (2019) present evidence that showing that vaccinations drastically increased in the lead up to a law that removed all nonmedical exemptions for school-mandated vaccinations—a window of time in which there was much public discourse about the benefits of vaccinations. Finally, Malani and Reif (2014) find that Physicians labor supply reacts in anticipation of tort reform, and show that accounting for their evolving expectations significantly affects the estimated impact of the policy. Our findings show that conceptually similar concerns exist in the domain of sin taxation and, in this context, are quite severe. We highlight the importance of these features in four ongoing academic 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 Murphy (1988). Classic tests of this theory, such as Gruber and Köszegi (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 be expected to cause declines 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 decline 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 have 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. Prior research has demonstrated that controls for non-price elements such as anti-smoking sentiments can mitigate estimated elasticities (e.g., DeCicca et al., 2008). We contribute to this debate by organizing the variety of components into a single conceptual framework and documenting that this

framework results in meaningfully different quantitative estimates of price responsiveness.

Finally, the results contribute to an active debate in the legal literature about how laws work and why they are enacted. Legal scholars have long appreciated that laws do more than change financial incentives. We show the value of an expanded definition of expressive 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, we are aware of only a handful of papers that directly estimate the magnitude of any expressive effects. This article presents some of the first empirical evidence that begins to separate price effects from expressive effects and suggests that, at least with cigarette taxes, the view of how the law can work through non-price channels should play a bigger role in lawmaking.

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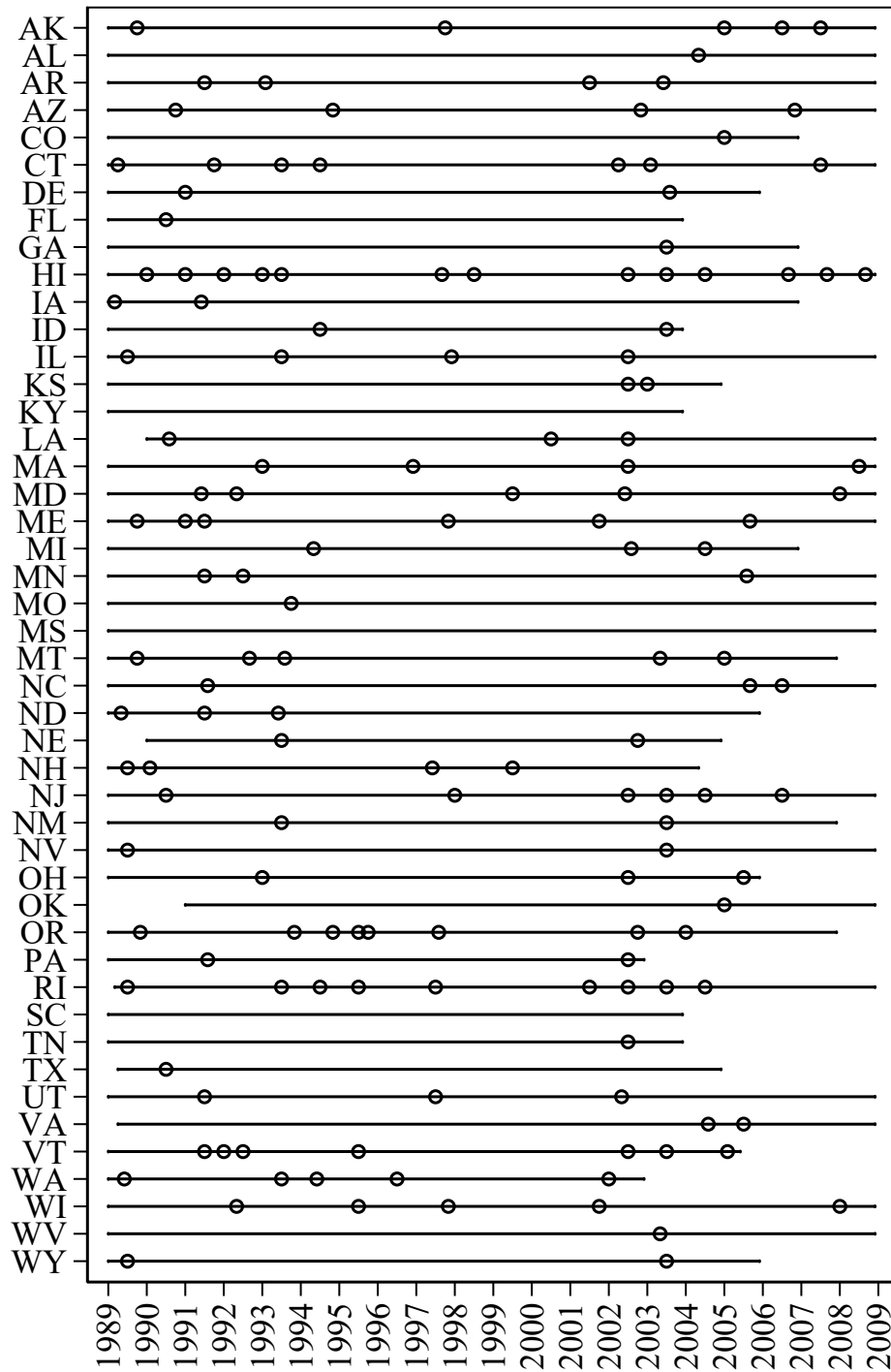
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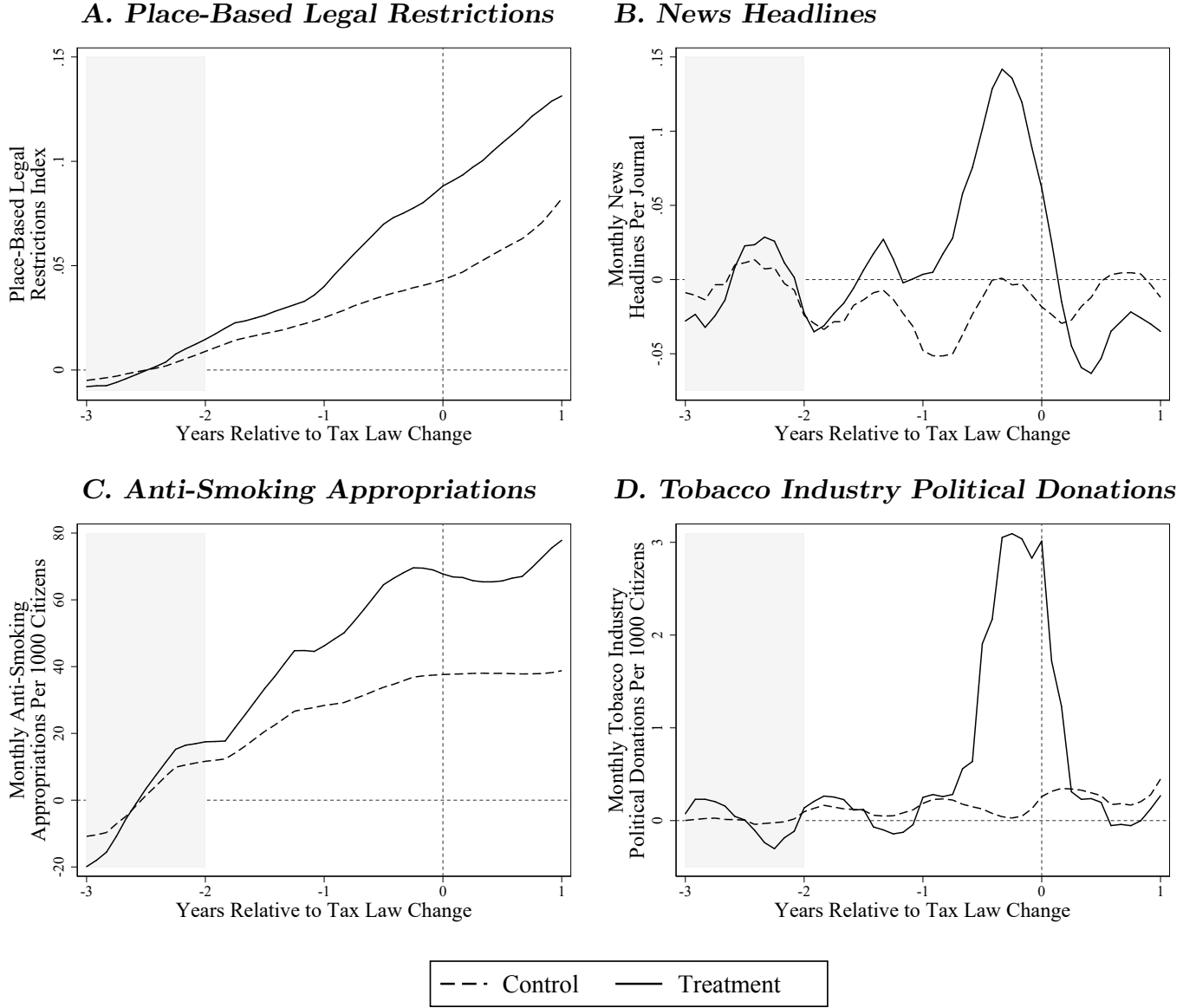
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Figure 1: Summary of State's Data Availability and Tax Law Changes



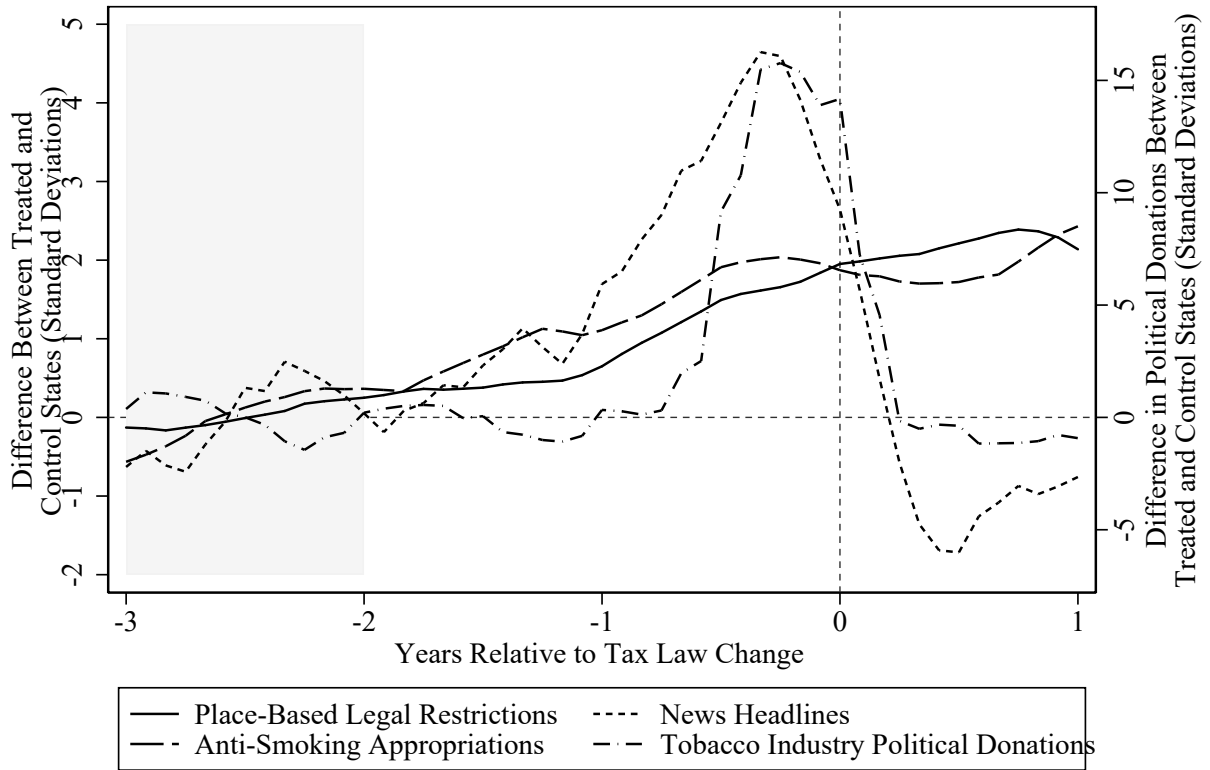
Notes: Each line indicates the years that the state is in the sample. The circle markers indicate the timing of cigarette tax changes.

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 in the window of consideration. We plot a six-month moving average, shifted such that the average of the non-price factor in the shaded window two-to-three years before the tax change is zero. The sample for this figure is restricted to the 112 tax changes for which a balanced panel is available over this window of consideration.

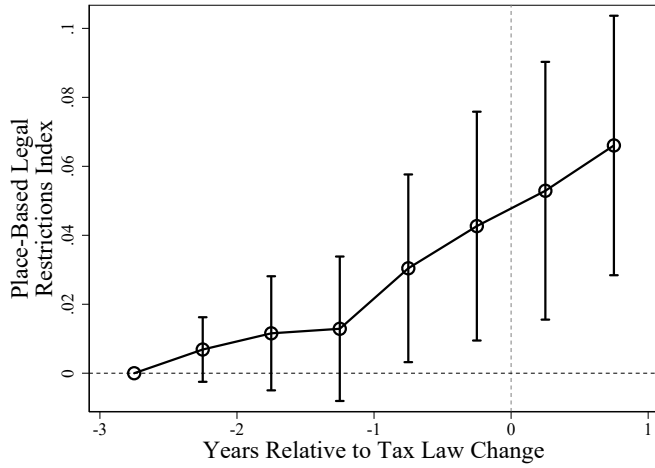
Figure 3: Time-Paths of Non-Price Factors: Difference from Control States



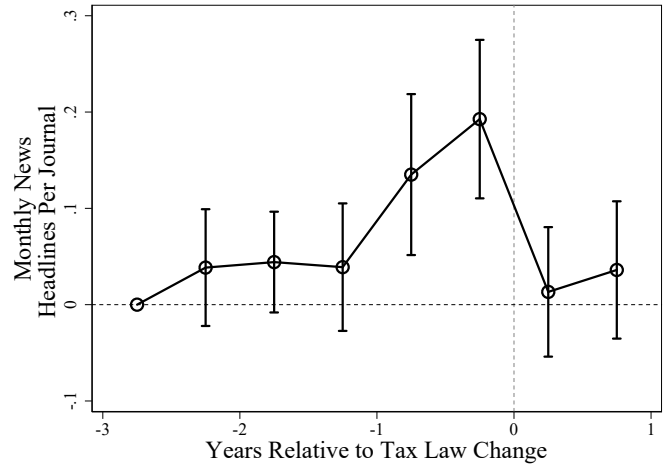
Notes: This figure reports the evolution of the non-price factors plotted in Figure 2, taking the difference between “treatment” states with a tax change to “control” states with no tax change in the window of consideration. We plot a six-month moving average of the difference, normalized by the standard deviation of this variable in control states and shifted such that the average of the non-price factor in the shaded window two-to-three years before the tax change is zero. The sample for this figure is restricted to the 112 tax changes for which a balanced panel is available over this window of consideration. Tobacco industry political donations are reported on the right y-axis.

Figure 4: Time-Paths of Non-Price Factors: Difference-in-Difference Estimates

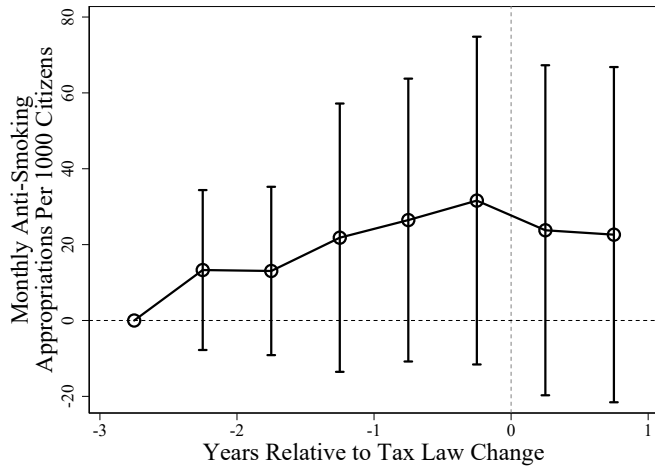
A. Place-Based Legal Restrictions



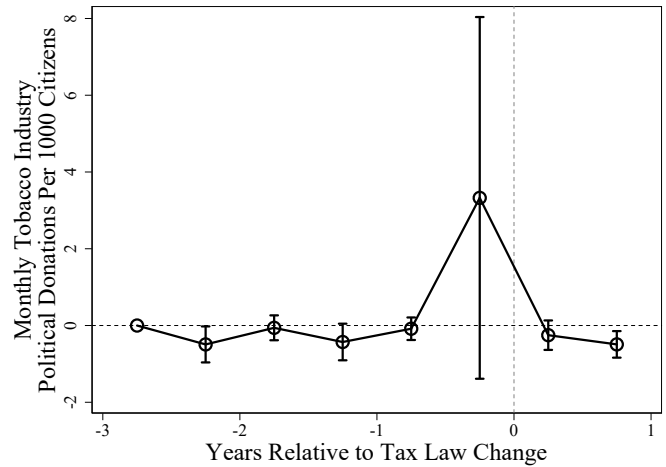
B. News Headlines



C. Anti-Smoking Appropriations

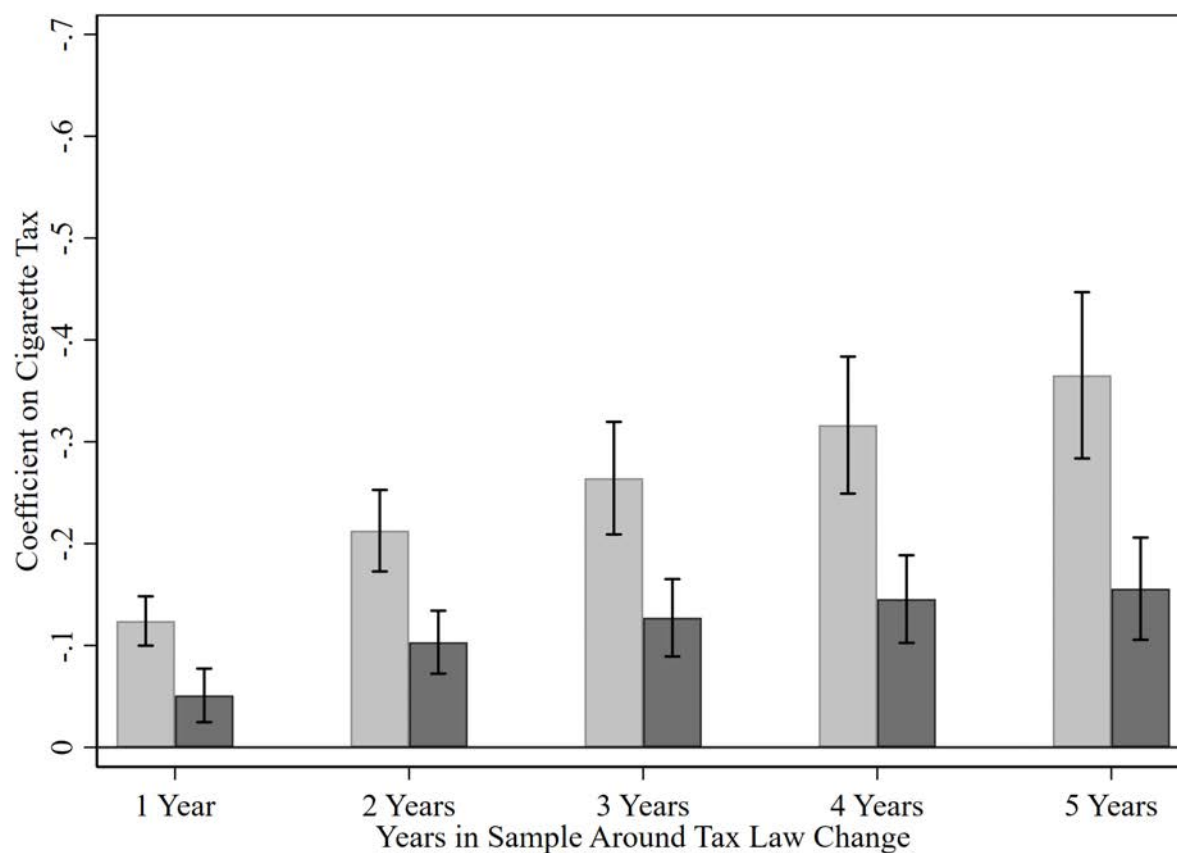


D. Tobacco Industry Political Donations



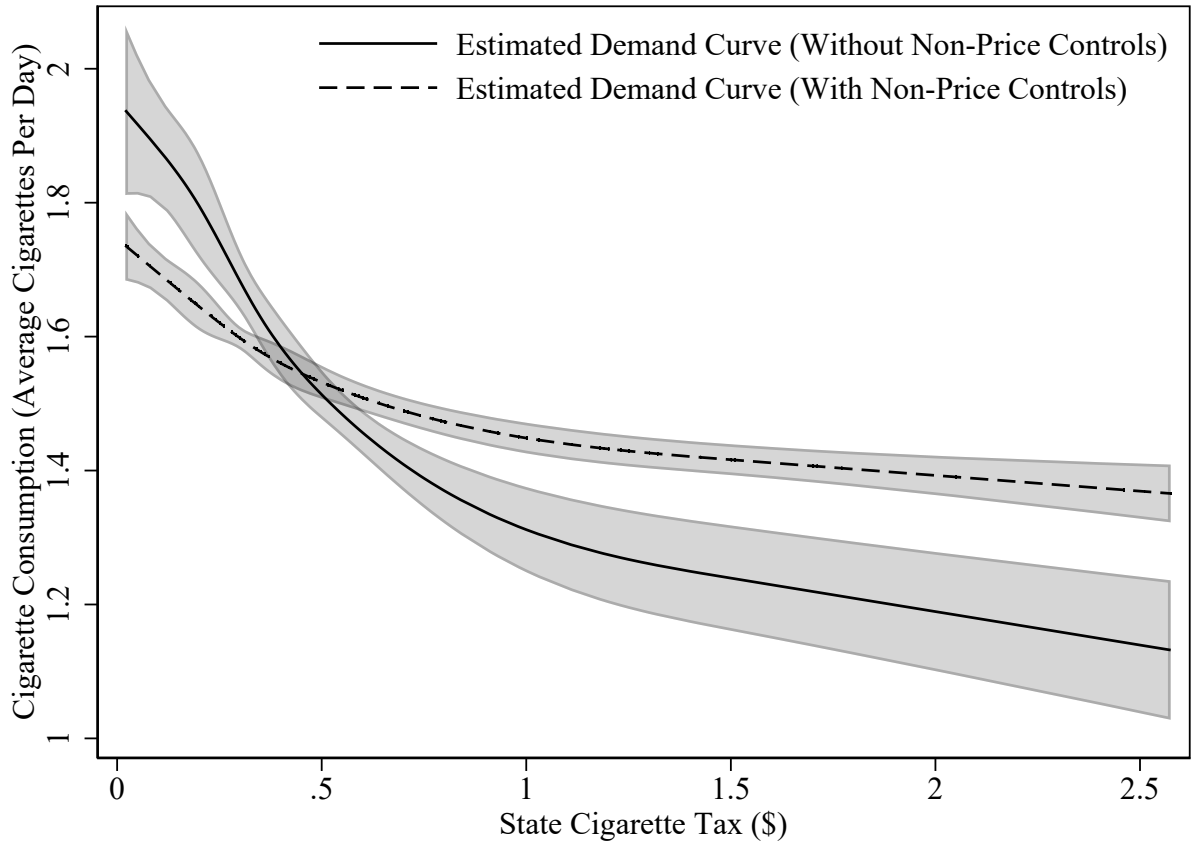
Notes: This figure reports the estimated coefficients from difference-in-difference regressions. The estimated coefficients come from the stacked-event-study data where we construct 6-month bins in the three years before and one year after the tax law change. We then regress each of the non-price factors on the bins and the interaction between the bins and the state with a cigarette tax change (the treated state). The reported coefficients are on the interaction terms between treated states and 6-month bins, which estimate the differential change occurring in treated states over different six-month windows. Capped lines indicate 90 percent confidence intervals.

Figure 5: Estimated Price Responsivity with and without Non-Price Controls



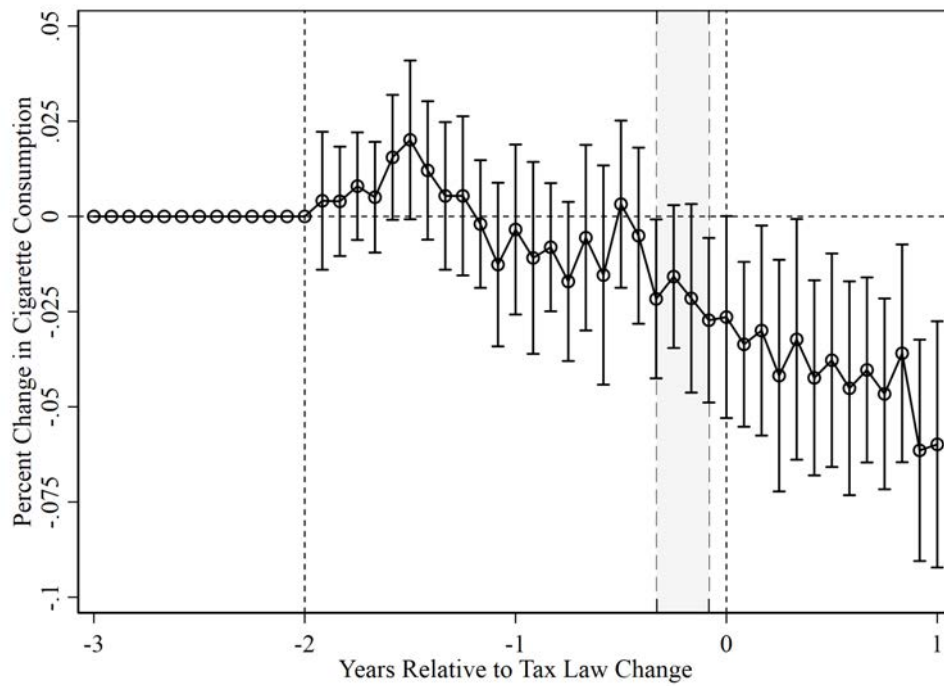
Notes: This figure reports the estimated responsivity of demand to the state tax level derived from the analysis in Section IV.B. Across columns, we vary the width of the time interval examined, ranging from a narrow window examining one year pre- and post-tax change to a wide window examining five years pre- and post-tax change. 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 6: 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 a cubic spline governing the within-state relationship between cigarette consumption and state taxes, 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. 90 percent confidence intervals are shaded behind each estimate.

Figure 7: Time Path of Consumption Around Tax Changes



Notes: This figure reports the estimated coefficients from our difference-in-difference regressions discussed in Section IV.C. Capped lines indicate 90 percent confidence intervals. The shaded region indicates the 25th to 75th percentile range of dates at which votes on the tax law changes occurred.

Table 1: Descriptive Statistics

	Panel	Stacked Events		
		All	Control	Treatment
Average Cigarettes Per Day	1.63	1.79	1.81	1.55
Place-Based Legal Restrictions Index	0.44	0.34	0.33	0.48
Monthly News Headlines Per Journal	0.36	0.38	0.38	0.35
Monthly Anti-Smoking Appropriations Per 1000 Citizens	131.65	92.52	87.50	162.54
Monthly Tobacco Industry Political Donations Per 1000 Citizens	1.45	0.57	0.50	1.56
<i>Notes:</i> The first column presents averages of our primary variables of interest in our panel data. Columns 2-4 present averages calculated within our “stacked event study” dataset, comprised of a three-year window for all available states surrounding each tax change event.				

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

	ln(Cigarettes)					
	(1)	(2)	(3)	(4)	(5)	(6)
State Tax	-0.264*** (0.033)	-0.227*** (0.031)	-0.203*** (0.026)	-0.196*** (0.032)	-0.257*** (0.034)	-0.127*** (0.023)
Place-Based Legal Restrictions Index		-0.152*** (0.031)				-0.087** (0.034)
News Headlines Per Journal (Cumulative)			-0.005*** (0.001)			-0.005*** (0.001)
Anti-Smoking Appropriations Per 1000 Citizens (Cumulative)				-0.005*** (0.002)		-0.004** (0.001)
Tobacco Industry Political Donations Per 1000 Citizens (Cumulative)					-0.070 (0.089)	-0.022 (0.054)
N	9,642	9,642	9,642	9,642	9,642	9,642
<i>Notes:</i> 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*** (0.033)	-0.229*** (0.032)	-0.205*** (0.026)	-0.198*** (0.033)	-0.259*** (0.034)	-0.128*** (0.023)
Size of Enacted Tax Change (Prior to Being Effective)	-0.081*** (0.021)	-0.062*** (0.023)	-0.050** (0.020)	-0.069*** (0.022)	-0.076*** (0.023)	-0.030 (0.021)
Place-Based Legal Restrictions Index		-0.150*** (0.031)				-0.086** (0.034)
News Headlines Per Journal (Cumulative)			-0.005*** (0.001)			-0.005*** (0.001)
Anti-Smoking Appropriations Per 1000 Citizens (Cumulative)				-0.005*** (0.002)		-0.004** (0.001)
Tobacco Industry Political Donations Per 1000 Citizens (Cumulative)					-0.066 (0.089)	-0.021 (0.054)
N	9,642	9,642	9,642	9,642	9,642	9,642
<i>Notes:</i> 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

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

TYPE/PRINT IN PERMANENT BLACK INK FOR INSTRUCTIONS SEE HANDBOOK		U.S. STANDARD CERTIFICATE OF LIVE BIRTH			
LOCAL FILE NUMBER		BIRTH NUMBER			
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 A.M.	
4. SEX Female	5. CITY, TOWN, OR LOCATION OF BIRTH Takoma Park		6. COUNTY OF BIRTH Montgomery		
7. PLACE OF BIRTH <input checked="" type="checkbox"/> Hospital <input type="checkbox"/> Freestanding Birthing Center Clinic, Doctor's Office, Residence Garfield Memorial 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 date stated Edmond M. Stone May 24, 1989 Signature		10. DATE SIGNED (Month, Day, Year) May 24, 1989		11. ATTENDANT'S NAME AND TITLE (If other than certified (Type/Print) Mary Elizabeth Short, C.N.M. Name <input type="checkbox"/> M.D. <input type="checkbox"/> D.O. <input checked="" type="checkbox"/> C.N.M. <input type="checkbox"/> Other Midwife Other (Specify)	
12. CERTIFIER'S NAME AND TITLE (Type/Print) Edmond Matthew Stone, M.D. Name <input type="checkbox"/> M.D. <input type="checkbox"/> D.O. <input checked="" type="checkbox"/> Hospital Admin. <input type="checkbox"/> C.N.M. <input type="checkbox"/> Other Midwife Other (Specify)		13. ATTENDANT'S MAILING ADDRESS (Street and Number or Rural Route Number City or Town, State, Zip Code) 5401 Azalea Lane Takoma Park, MD 20417			
14. REGISTRAR'S SIGNATURE Ron T. Burnette		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		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? (Specify No or Yes—if yes specify Cuban, Mexican, Puerto Rican, etc.)		26. RACE—American Indian, Black, White, etc. (Specify below)		27. EDUCATION (Specify only highest grade completed) Elementary/Secondary (12) College (14 or 16) 4	
25a. <input checked="" type="checkbox"/> No <input type="checkbox"/> Yes Specify		26a. White		27a. 4	
25b. <input checked="" type="checkbox"/> No <input type="checkbox"/> Yes Specify		26b. White		27b. 5+	
28. PREGNANCY HISTORY (Complete each section) LIVE BIRTHS (Do not include this child) 28a. Now Living Number 1 28b. Now Dead Number None 28c. DATE OF LAST LIVE BIRTH (Month, Year) March, 1987		29. MOTHER MAILED? (At birth, conception, or any time between) (Yes or no) Yes 30. DATE LAST NORMAL MENSTRUATION BEGAN (Month, Day, Year) August 9, 1988		31. MONTH OF PREGNANCY PRENATAL CARE BEGAN First Second Third etc. (Specify) Second	
28d. DATE OF LAST LIVE BIRTH (Month, Year) March, 1987		32. PRENATAL VISITS—Total Number (If none, so state) 11		33. BIRTH WEIGHT (Specify unit) 2900 grams	
28e. DATE OF LAST LIVE BIRTH (Month, Year) March, 1987		34. CLINICAL ESTIMATE OF GESTATION (Weeks) 37 weeks		35. PLURALITY—Single Twin Triplet, etc. (Specify) Single	
36. APGAR SCORE 36a. 1 Minute 7 36b. 5 Minutes 6		37a. MOTHER TRANSFERRED PRIOR TO DELIVERY? <input checked="" type="checkbox"/> No <input type="checkbox"/> Yes If Yes enter name of facility transferred to: Holly View Hospital			
37b. INFANT TRANSFERRED? <input type="checkbox"/> No <input checked="" type="checkbox"/> Yes If Yes enter name of facility transferred to:					
38a. MEDICAL RISK FACTORS FOR THIS PREGNANCY (Check all that apply) Anemia (Hct < 30/Hgb < 10) <input checked="" type="checkbox"/> Cardiac disease <input type="checkbox"/> Acute or chronic lung disease <input type="checkbox"/> Diabetes <input type="checkbox"/> Genital herpes <input type="checkbox"/> Hydranmios/Dyshydramnios <input type="checkbox"/> Hemoglobinopathy <input type="checkbox"/> Hypertension—chronic <input type="checkbox"/> Hypertension—pregnancy associated <input type="checkbox"/> Eclampsia <input type="checkbox"/> Incompetent cervix <input type="checkbox"/> Previous infant 4000+ grams <input type="checkbox"/> Previous cesarean or small for gestational age infant <input checked="" type="checkbox"/> Renal disease <input type="checkbox"/> Rh sensitization <input type="checkbox"/> Uterine bleeding <input type="checkbox"/> None <input type="checkbox"/> Other <input type="checkbox"/> (Specify)		40. COMPLICATIONS OF LABOR AND/OR DELIVERY (Check all that apply) Fetus (1 > 100°F or 38°C) <input type="checkbox"/> Meconium moderate/heavy <input type="checkbox"/> Premature rupture of membrane (> 12 hours) <input type="checkbox"/> Abruptio placenta <input type="checkbox"/> Placenta previa <input type="checkbox"/> Other excessive bleeding <input type="checkbox"/> Seizures during labor <input type="checkbox"/> Prolonged labor (> 3 hours) <input type="checkbox"/> Prolonged labor (> 20 hours) <input type="checkbox"/> Dysfunctional labor <input type="checkbox"/> Breech/Malpresentation <input type="checkbox"/> Cephalopelvic disproportion <input type="checkbox"/> Cord entanglement <input type="checkbox"/> Anesthetic complications <input type="checkbox"/> Fetal distress <input type="checkbox"/> None <input type="checkbox"/> Other (Specify) <input type="checkbox"/>		43. CONGENITAL ANOMALIES OF CHILD (Check all that apply) Anencephalus <input type="checkbox"/> Spina bifida/Meningocele <input type="checkbox"/> Hydrocephalus <input type="checkbox"/> Microcephalus <input type="checkbox"/> Other central nervous system anomalies (Specify) <input type="checkbox"/> Heart malformations <input type="checkbox"/> Other circulatory/respiratory anomalies (Specify) Patent ductus arteriosus <input checked="" type="checkbox"/> Rectal atresia/stenosis <input type="checkbox"/> Tracheo-esophageal fistula/Esoophageal atresia <input type="checkbox"/> Omphalocele/Gastrochisis <input type="checkbox"/> Other gastrointestinal anomalies (Specify) <input type="checkbox"/> Malformed genitalia <input type="checkbox"/> Renal agenesis <input type="checkbox"/> Other urogenital anomalies (Specify) <input type="checkbox"/> Cleft lip/palate <input type="checkbox"/> Polydactyly/Syndactyly/Adactyly <input type="checkbox"/> Club foot <input type="checkbox"/> Diaphragmatic hernia <input type="checkbox"/> Other musculoskeletal/integumental anomalies (Specify) <input type="checkbox"/> Down's syndrome <input type="checkbox"/> Other chromosomal anomalies (Specify) <input type="checkbox"/> None <input type="checkbox"/> Other (Specify) <input type="checkbox"/>	
38b. OTHER RISK FACTORS FOR THIS PREGNANCY (Complete all items) Tobacco use during pregnancy 20 Yes <input checked="" type="checkbox"/> No <input type="checkbox"/> Average number cigarettes per day 2 Yes <input checked="" type="checkbox"/> No <input type="checkbox"/> Alcohol use during pregnancy 20 Yes <input checked="" type="checkbox"/> No <input type="checkbox"/> Average number drinks per week 20 Yes <input checked="" type="checkbox"/> No <input type="checkbox"/> Weight gained during pregnancy 20 Yes <input checked="" type="checkbox"/> No <input type="checkbox"/>		41. METHOD OF DELIVERY (Check all that apply) Vaginal <input checked="" type="checkbox"/> Vaginal birth after previous C section <input type="checkbox"/> Primary C section <input type="checkbox"/> Repeat C section <input type="checkbox"/> Forceps <input type="checkbox"/> Vacuum <input type="checkbox"/>		42. ABNORMAL CONDITIONS OF THE NEWBORN (Check all that apply) Anemia (Hct < 35/Hgb < 13) <input type="checkbox"/> Birth injury <input type="checkbox"/> Fetal alcohol syndrome <input type="checkbox"/> Hyaline membrane disease/RDS <input type="checkbox"/> Meconium aspiration syndrome <input type="checkbox"/> Assisted ventilation < 30 min <input checked="" type="checkbox"/> Assisted ventilation > 30 min <input type="checkbox"/> Seizures <input type="checkbox"/> None <input type="checkbox"/> Other (Specify) <input type="checkbox"/>	
39. OBSTETRIC PROCEDURES (Check all that apply) Amniocentesis <input type="checkbox"/> Electronic fetal monitoring <input type="checkbox"/> Induction of labor <input type="checkbox"/> Stimulation of labor <input type="checkbox"/> Tocolysis <input type="checkbox"/> Ultrasound <input checked="" type="checkbox"/> None <input type="checkbox"/> Other (Specify) <input type="checkbox"/>					

Source: National Center for Health Statistics (1987)

Figure A2: Estimated Price Responsivity with Different Sets of Controls

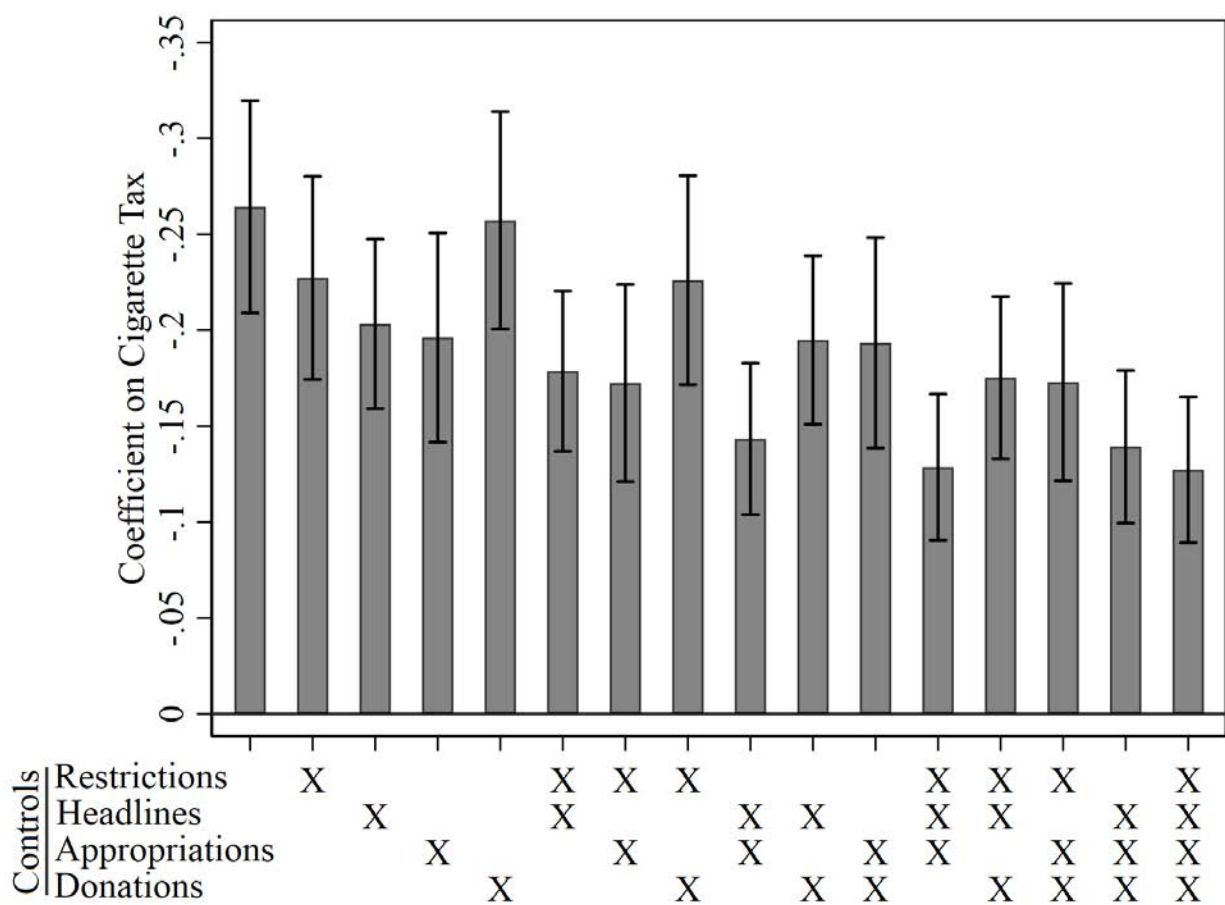


Table A1: Predictors of Cigarette Consumption in States without Tax Changes

	ln(Cigarettes)				
	(1)	(2)	(3)	(4)	(5)
Place-Based Legal Restrictions Index	-0.293*** (0.053)				-0.094** (0.038)
News Headlines Per Journal (Cumulative)		-0.006*** (0.001)			-0.005*** (0.001)
Anti-Smoking Appropriations Per 1000 Citizens (Cumulative)			-0.010*** (0.002)		-0.006*** (0.002)
Tobacco Industry Political Donations Per 1000 Citizens (Cumulative)				-0.833 (0.538)	-0.256 (0.316)
N	150,836	150,836	150,836	150,836	150,836
<i>Notes:</i> 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: Non-Price Factors Measured Contemporaneously

	ln(Cigarettes)					
	(1)	(2)	(3)	(4)	(5)	(6)
State Tax	-0.264*** (0.033)	-0.227*** (0.031)	-0.259*** (0.032)	-0.253*** (0.031)	-0.264*** (0.033)	-0.217*** (0.029)
Place-Based Legal Restrictions Index		-0.152*** (0.031)				-0.132*** (0.031)
Monthly News Headlines Per Journal			-0.003 (0.005)			-0.002 (0.005)
Monthly Anti-Smoking Appropriations Per 1000 Citizens				-0.147*** (0.038)		-0.139*** (0.038)
Monthly Tobacco Industry Political Donations Per 1000 Citizens					-0.138* (0.072)	-0.181** (0.074)
N	9,642	9,642	9,642	9,642	9,642	9,642
<i>Notes:</i> 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: 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*** (0.027)	-0.211*** (0.027)	-0.208*** (0.026)	-0.130*** (0.023)
News Headlines Per Journal (Cumulative)				
Cigarette with Tax	-0.012*** (0.002)		-0.002 (0.003)	-0.001 (0.003)
Cigarette without Tax		-0.007*** (0.001)	-0.006*** (0.001)	-0.007*** (0.001)
Place-Based Legal Restrictions Index				-0.083** (0.033)
Anti-Smoking Appropriations Per 1000 Citizens (Cumulative)				-0.004*** (0.001)
Tobacco Industry Political Donations Per 1000 Citizens (Cumulative)				-0.027 (0.054)
N	9,642	9,642	9,642	9,642
<i>Notes:</i> 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 Using BRFSS Data

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