NBER WORKING PAPER SERIES

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

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Working Paper 25958
http://www.nber.org/papers/w25958

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2019

We thank Douglas Baird, Adam Chilton, Erin Delaney, Gerrit De Geest, Dhammika Dharmapala, Michael Frakes, Ezra Friedman, Daniel Hemel, Jim Hines, William Hubbard, Don Kenkel, Genevieve Lakier, Sarah Lawsky, Ben Lockwood, Anup Malani, Jonathan Masur, Richard McAdams, Adam Rosenzweig, Sarath Sanga, Max Schanzenbach, Joel Slemrod, Kate Smith, Dmitry Taubinsky, and seminar audiences at the American Economic Association Annual Meeting, the American Law and Economics Association Annual Meeting, the Conference on Empirical Legal Studies, the Junior Tax Scholars Workshop, George Mason University Antonin Scalia Law School, University of California Berkeley Haas School of Business, University of California Berkeley School of Law, University of California Hastings College of Law, University of Chicago Law School, Washington University School of Law, and Wharton for helpful comments. We thank Andrew Joung for excellent research assistance and the Wharton Dean's Research Fund for financial support. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 25958
June 2019
JEL No. D9,H2

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 decline.

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. 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.

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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; Lockwood and Taubinsky, 2017). Although the behavioral economics 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 Hoeppner, 2018.

In our empirical analysis, we examine how the consumption of cigarettes by pregnant mothers evolves throughout the process of state-level cigarette tax law changes. Dissuading this behavior 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 Koszegi 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,[5] a permanent legal record required for all live births.[6] 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

[5] See Figure A1 for an example.

[6] 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, illustrating 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 marked by an increase in both place-based legal restrictions and anti-smoking appropriations, illustrating the simultaneous use of other dissuasive policies. These findings illustrate 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 suppression of demand that occurs around changes in tax law. We demonstrate that the decline in consumption around cigarette tax law changes 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 responsivity to the tax by roughly half.

Taken together, these results suggest that a meaningful degree of the decline 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, decline 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.[7] Furthermore, the degree of this anticipatory decline 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 tax laws change more than prices. While a tax unambiguously does shape financial incentives, its impact on informational, psychological, and sociological factors shaping demand may be at least of 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

[7] 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:

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

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

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

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) 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.[8] 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 $(E_t[\bar{a}_t])$.[9] 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.[10]

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

[8] 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).

[9] 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.

[10] 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:

$$V(a_t, x_t, S_t, E_t[\bar{a}_t])) = -\frac{1}{2}(b_{aa}a_t^2 + b_{SS}S_t^2 + b_{xx}x_t^2) + b_{aS}a_tS_t$$

$$+ b_{ax}a_tx_t + b_{Sx}S_tx_t + b_aa_t + b_SS_t + b_xx_t + b_k + G(a_t, E_t[\bar{a}_t])$$

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.[11] 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

[11] 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).

smoking more often, policies that restrict exposure to smoking in public areas, or increased information about or accessibility of smoking cessation aids.[12]

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:

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

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 equations contain intertemporal dependency on past and future prices. Past and future prices

[12] 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).

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; Koszegi and Gruber, 2001).[13]

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

[13] 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 Koszegi (2001) discuss this issue at length. Their solution, which we adopt, is to use cigarette consumption of mothers who gave birth from 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 Koszegi (2001) at 1268, 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 Koszegi (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 Koszegi (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. (Centers for Disease Control's State Tobacco Activities Tracking and Evaluation System; National Conference of State Legislature's Ballot Measures Database).[14]

C. Intensity of Political Activities and Social Debate

We form three different measures of political and social debate to serve as non-price factors—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

[14] 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.[15] 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 Health Communication Interventions, Best Practices Program Components from the Centers for Disease Control and Prevention, 2015.[16] 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, and because 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

[15] 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.
[16] Because the appropriations data are available only since 1991, we code appropriations in 1989 and 1990 as those in 1991.

writing about it.[17] 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 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.[18] The 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.[19]

[17] 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.

[18] The data are available at https://chronicdata.cdc.gov/Legislation/CDC-STATE-System-Tobacco-Legislation-Smokefree-Ind/32fd-hyzc.

[19] 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.

Our place-based legal restriction index is the average number of legal changes in each of the available domains described above.

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 suppression of demand 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.[20] We regress an outcome on event 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).[21] This introduces correlation between the error terms for these two control observations in the data. This is accommodated by clustering standard errors by state,[22] 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

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

[21] 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.

[22] 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.

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 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 increases 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.[23]

[23] 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

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 witness 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

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.

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 the 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 thus 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 specification on the stacked-event-study data:

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

for 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.[24] 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.[25] As before, we conduct multi-dimensional clustering of standard errors, allowing for correlation within event, within state, and within century-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

[24] See Table A1.

[25] 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.

reported in Table 2. Figure 5 shows that the percent reduction in the estimated price effect is consistent using different time windows around the tax law change.

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,[26] and anti-smoking appropriations all have strongly statistically significant associations with a decrease in cigarette consumption. Furthermore, in each case, the estimated price responsivity is reduced due to the co-movement of these factors with the tax. Qualitatively similar—but not statistically significant—results are found 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.

[26] 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 that enhances the price effect.

Contrasting the estimated coefficient on the state tax in Column 6 and Column 1 illustrates a striking consequence of these findings: the inclusion of these controls reduces the estimated responsivity to the tax by roughly half.[27] As illustrated in Figure 5, the large reduction in the size of the price effect holds when conducting this analysis using pre/post periods of any year length from one to five.

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).[28] 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 extent that the reduction in price effects from only the three non-price factors not used in previous research have the same consequence on the price effects by estimating the change in price effects with different sets of controls. Figure A2 reports the estimated coefficient on cigarette taxes with different sets of controls as indicated on the x-axis (with 90 percent confidence intervals), and shows that the three other non-price factors

[27] 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).

[28] Gruber and Koszegi (2001) do not control for place-based legal restrictions in the main analysis, but not 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).

reduce the price effect by nearly 50 percent. Consistent with Table 2, it also shows that controlling for news headlines and appropriations is responsible for the largest reductions in the estimated price effect.

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 may be explained by the coevolution of non-price factors. This finding has strong implications for the predicted timing of the decline in consumption. Because the evolution of non-price factors begins well before the tax is enacted, the decline 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 6 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 decline in demand arising from a known future increase in prices. Prior work documents declines in demand occurring before the tax is enacted, but after a vote has occurred (e.g., Gruber and Koszegi 2001). Note that the anticipatory effect we document here is occurring substantially earlier: the shaded region of Figure 6 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. We test this possibility in Figure 7. The figure reports the correlation between the size of the tax change and the size of the decline in consumption in the months before and after the tax change. If smokers are rationally responding to the expected future price increase, the change in consumption and the size of the tax increase should be negatively correlated. For treatment and control states in each event, we calculate the difference between cigarette consumption in a state-year-month and the average of that state's consumption in the window between 3 and 2 years prior to the tax change. We then calculate the event-specific difference-in-difference measure of the decline in consumption at each event time and estimate the correlation between it and the size of the tax change that will ultimately occur. As documented in the figure, once the tax change is enacted, such correlation does arise, suggesting that consumption responds more to larger tax increases after the tax has been changed. However, in the lead up to the tax change, correlations are approximately mean zero, inconsistent with a rational anticipatory response.

In short, these anticipatory results are consistent with non-price factors having a direct influence on demand and are 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. Next, Richwine et al., (2019) present evidence that showing that vaccinations drastically increased in the lead up to a law that removed all nonmedical exemptions, a 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 Murphey (1988). Classic tests of this theory, such as Gruber and Koszegi (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 the quantitative importance of the framework.

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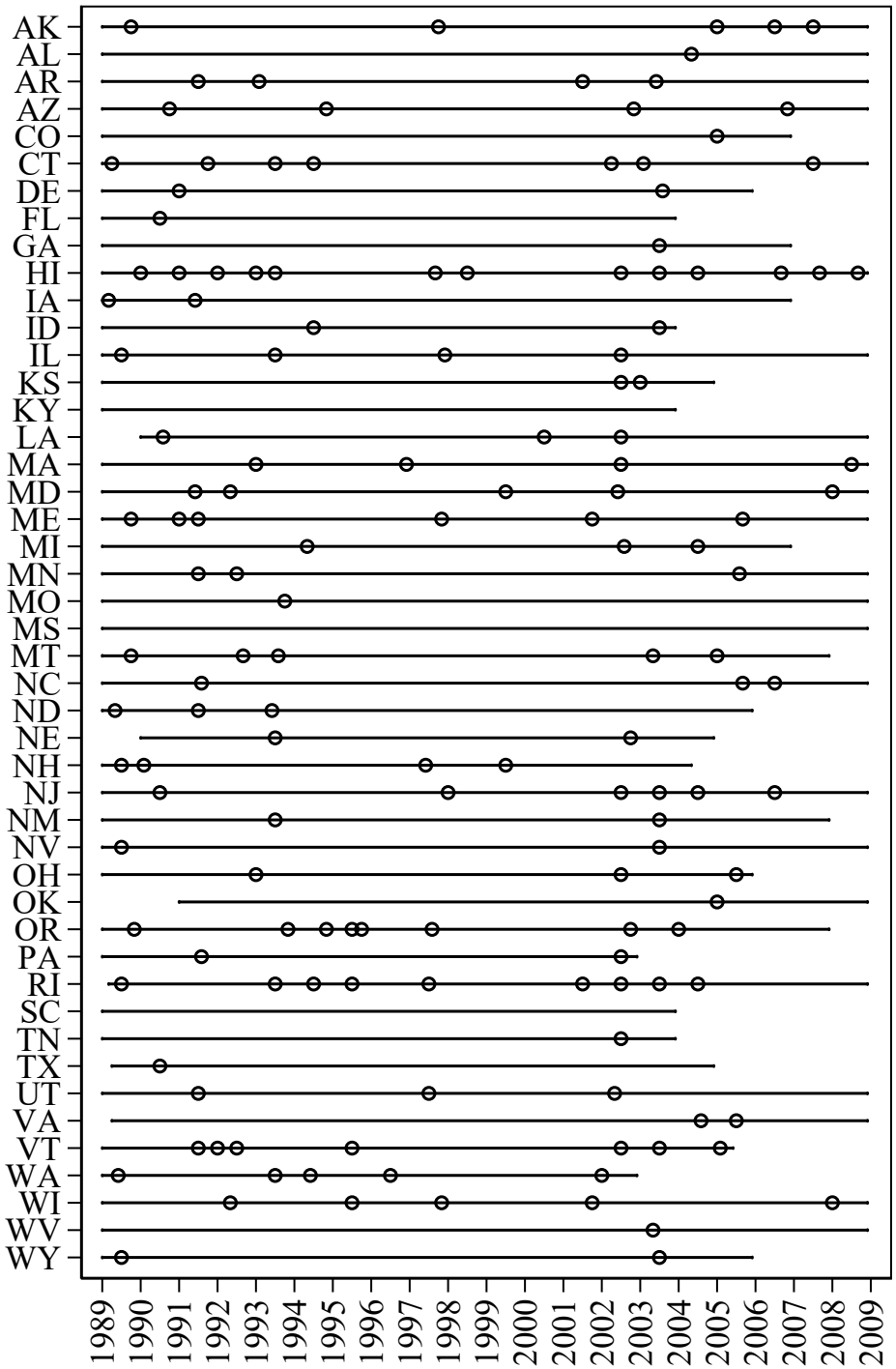
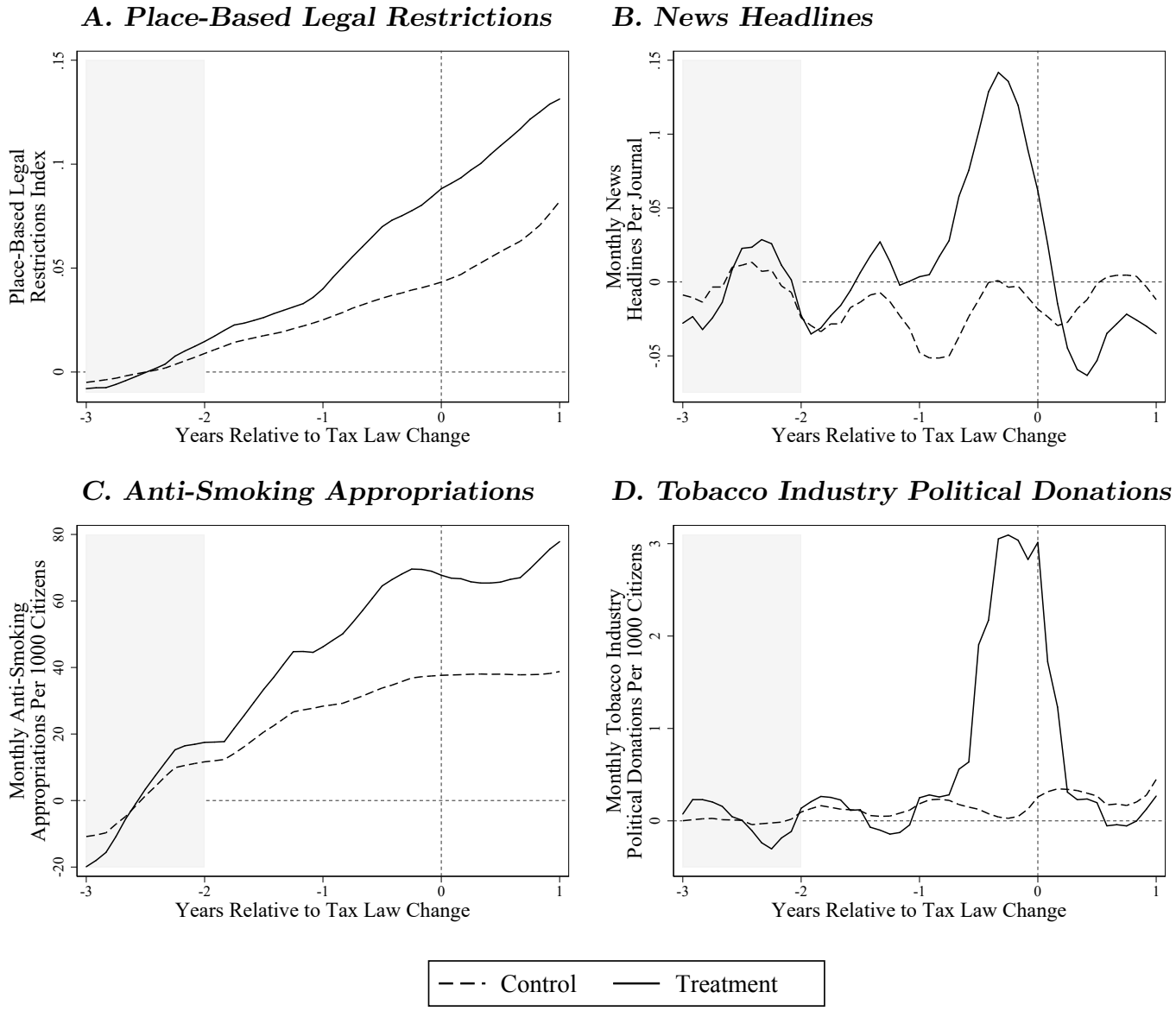
Figure 1: Summary of State's Data Availability and Tax Law Changes



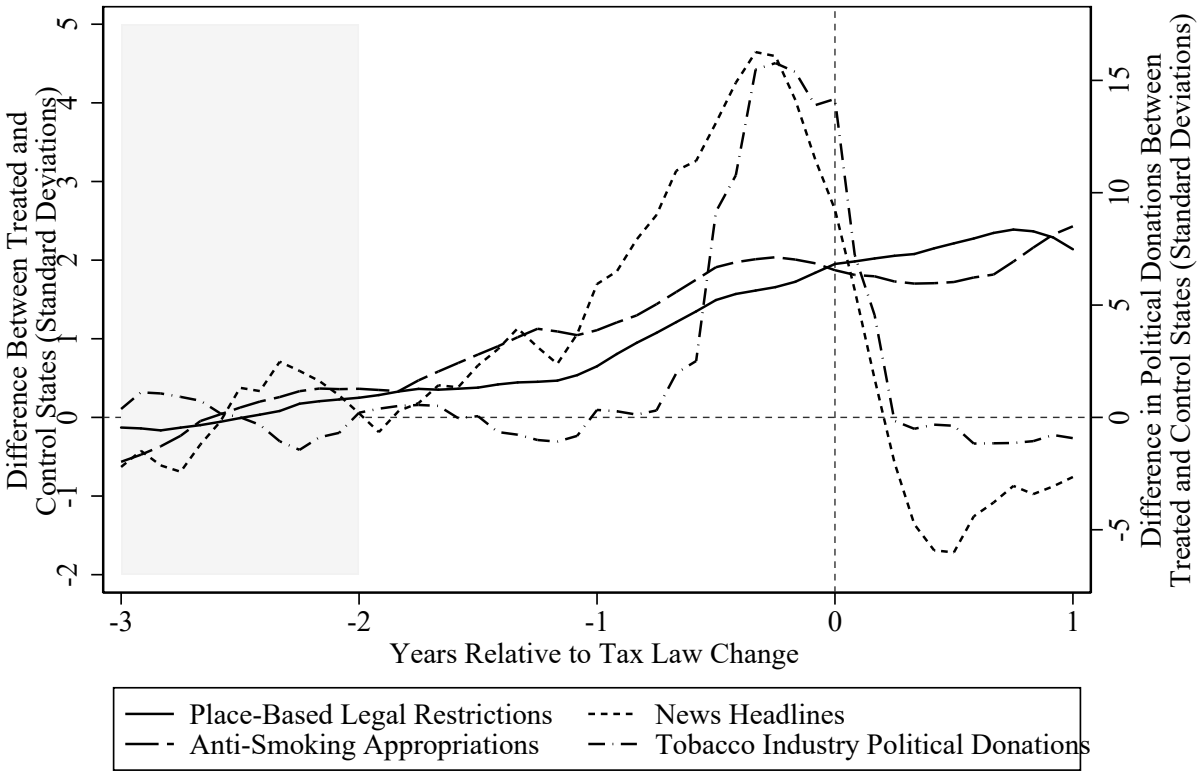
Notes: Each line indicates the years that the state is in the sample. The circle markers indicate the timing of cigarette tax changes.

Figure 2: Time-Paths of Non-Price Factors



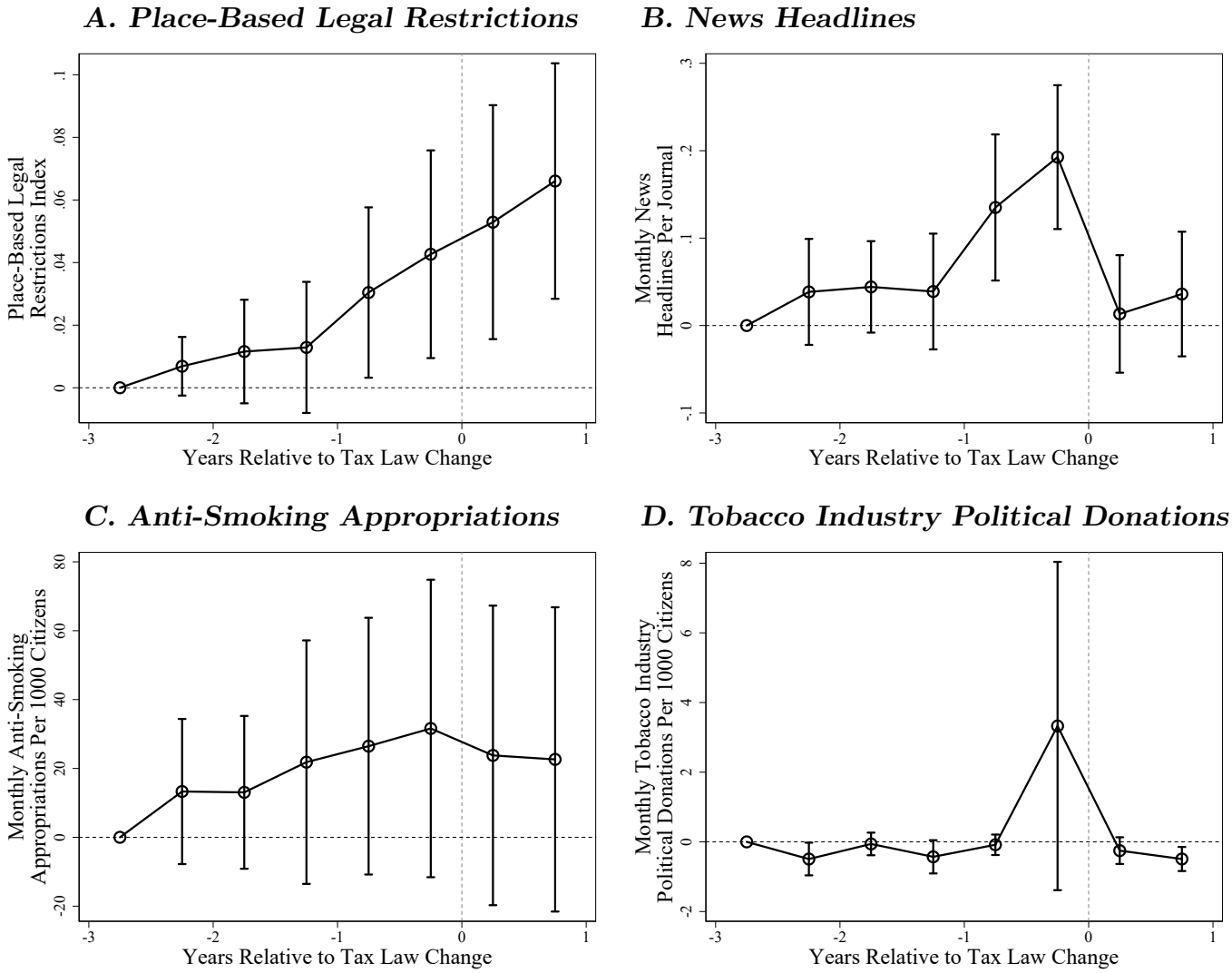
Notes: Each panel reports the evolution of a non-price factor, comparing "treatment" states with a tax change to "control" states with no tax change in the window of consideration. We plot a six-month moving average, shifted such that the average of the non-price factor in the shaded window two-to-three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



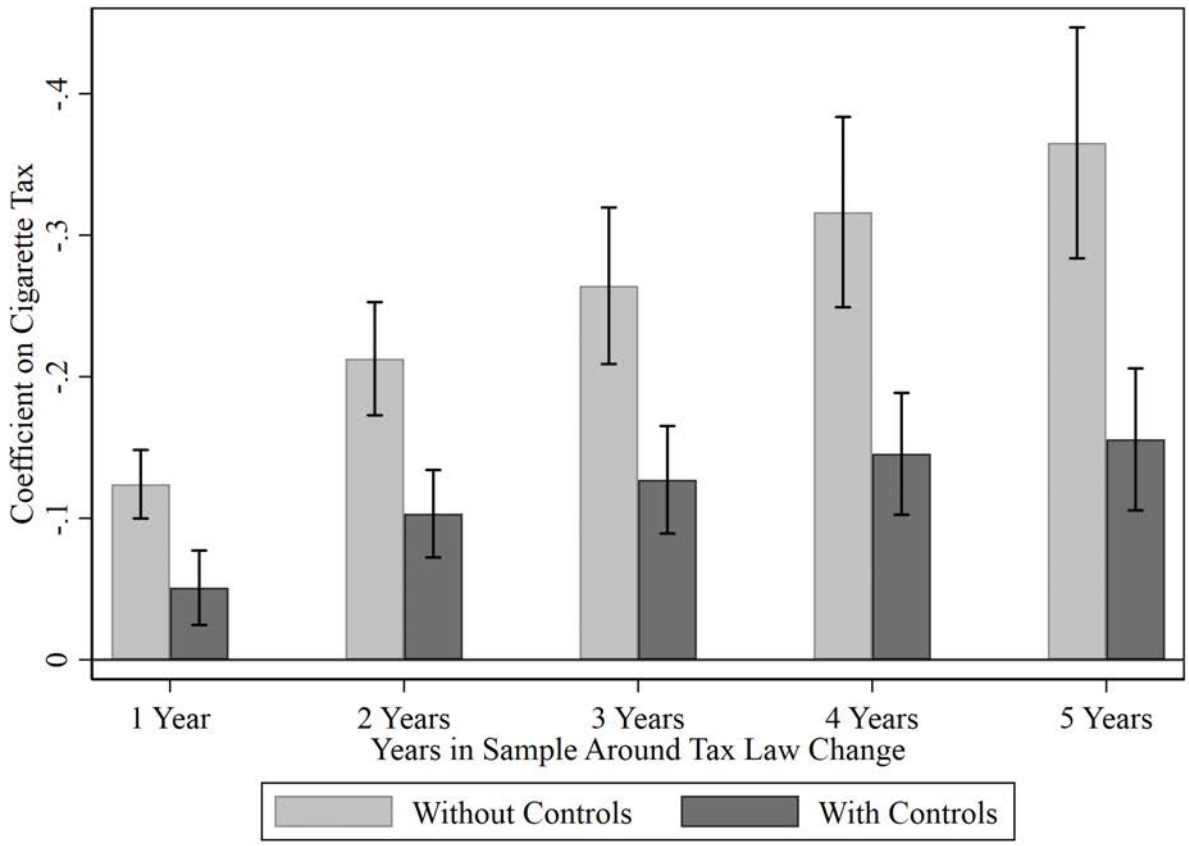
Notes: This figure reports the evolution of the non-price factors plotted in Figure 2, taking the difference between "treatment" states with a tax change to "control" states with no tax change in the window of consideration. We plot a six-month moving average of the difference, normalized by the standard 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



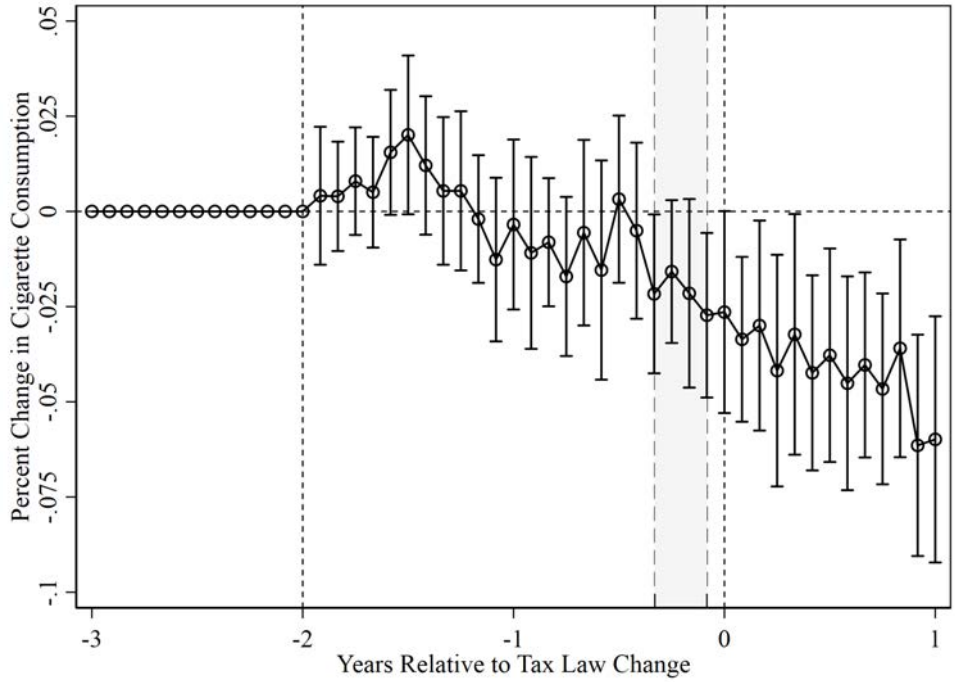
Notes: This figure reports the estimated coefficients from difference-in-difference regressions. The estimated coefficients come from the stacked-event-study data where we construct 6-month bins in the three years before and one year after the tax law change. We then regress each of the non-price factors 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



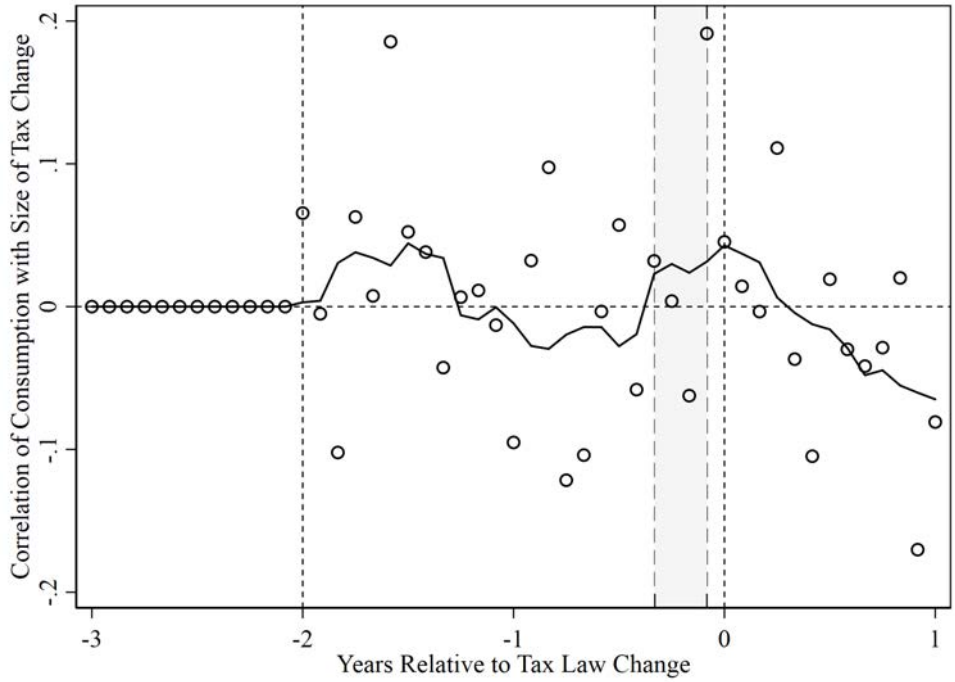
Notes: This figure reports the estimated responsivity of demand to the state tax level derived from the analysis in Section IV.B. Across columns, we vary the width of the time interval examined, ranging from a narrow window examining one year pre- and post-tax change to a wide window examining five years 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: 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.

Figure 7: Correlation of Consumption with Size of Tax Change in Different Time Periods



Notes: This figure plots the correlation between the cigarette consumption and the size of the tax change that will ultimately occur. Cigarette consumption is measured with our difference-in-difference approach, with the period from two-to-three years prior serving as the pre-period. Each dot in the figure illustrates this correlation in a particular month relative to the time the tax change becomes effective. The line illustrates a six-month moving average. 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

		Stacked Events		
	Panel	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

Notes: 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.

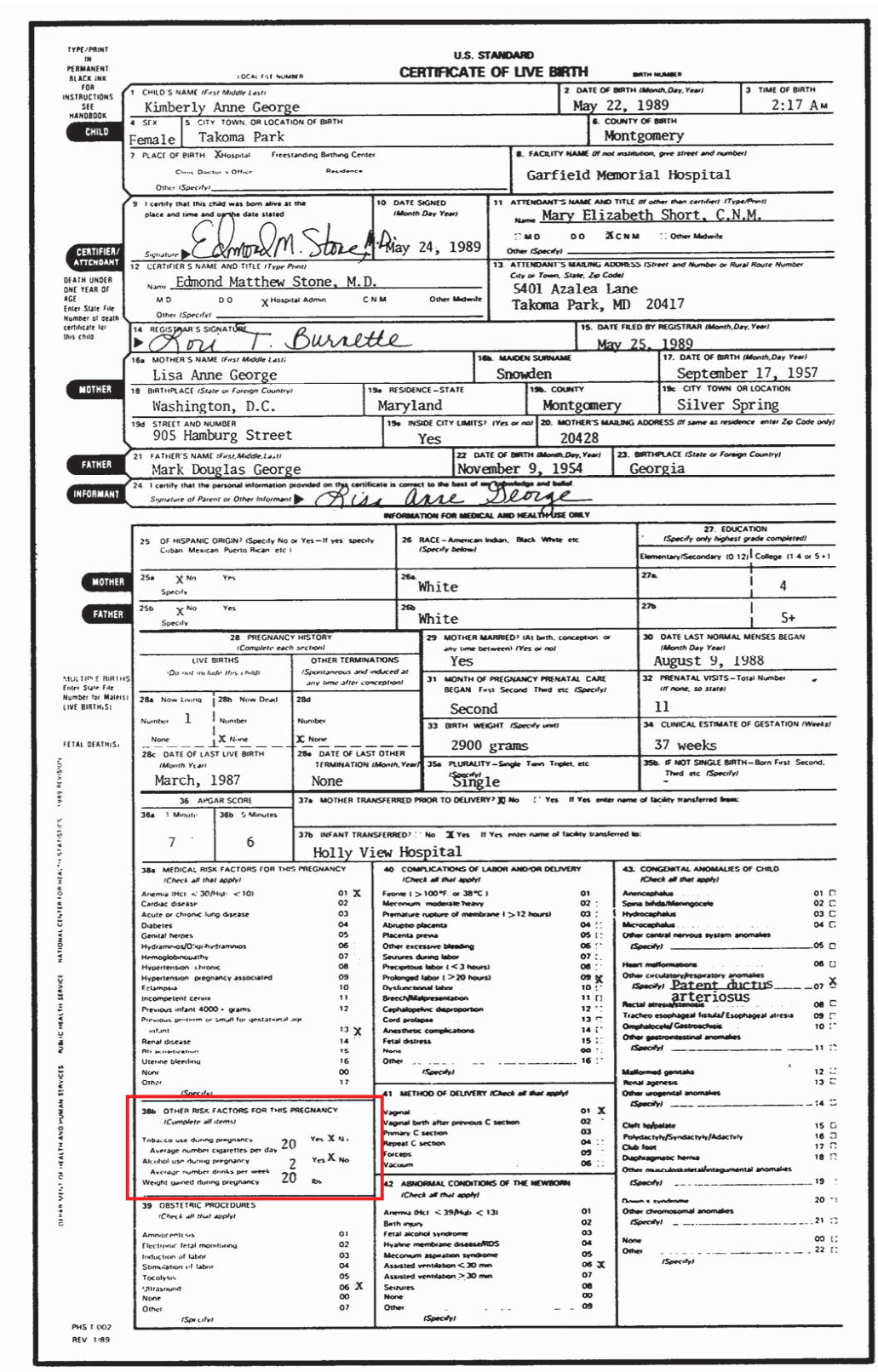
Table 2: Within-Tax-Change-Event Predictors of Cigarette Consumption

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

Notes: Standard errors 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



U.S. STANDARD
CERTIFICATE OF LIVE BIRTH

1 CHILD'S NAME: Kimberly Anne George
2 DATE OF BIRTH: 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: 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: May 24, 1989
11 ATTENDANT'S NAME AND TITLE: Mary Elizabeth Short, C.N.M.
12 CERTIFIER'S NAME AND TITLE: Edmond Matthew Stone, M.D.
13 ATTENDANT'S MAILING ADDRESS: 5401 Azalea Lane, Takoma Park, MD 20417
14 REGISTRAR'S SIGNATURE: Lori T. Burnette
15 DATE FILED BY REGISTRAR: May 25, 1989
16a MOTHER'S NAME: Lisa Anne George
16b MAIDEN SURNAME: Snowden
17 DATE OF BIRTH: September 17, 1957
18 BIRTHPLACE: 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
20 MOTHER'S MAILING ADDRESS: 20428
21 FATHER'S NAME: Mark Douglas George
22 DATE OF BIRTH: November 9, 1954
23 BIRTHPLACE: 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 X No; 25b X No
26 RACE: 26a White; 26b White
27 EDUCATION: 27a Elementary/Secondary (0-12): 4; 27b College (1-4 or 5+): 5+
28 PREGNANCY HISTORY — LIVE BIRTHS: 28a Now Living: Number 1; None; 28b Now Dead: Number; X None; OTHER TERMINATIONS: 28d Number; 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 PRENATAL CARE BEGAN: Second
32 PRENATAL VISITS—Total Number: 11
33 BIRTH WEIGHT: 2900 grams
34 CLINICAL ESTIMATE OF GESTATION: 37 weeks
35a PLURALITY—Single, Twin, Triplet, etc.: Single
35b IF NOT SINGLE BIRTH—Born First, Second, Third etc.:
36 APGAR SCORE: 36a 1 Minute: 7; 36b 5 Minutes: 6
37a MOTHER TRANSFERRED PRIOR TO DELIVERY? X No
37b INFANT TRANSFERRED? X Yes — If Yes, enter name of facility transferred to: Holly View Hospital

38a MEDICAL RISK FACTORS FOR THIS PREGNANCY (Check all that apply)
Anemia (Hct < 30/Hgb < 10) 01 X
Previous infant 4000+ grams 12
Previous preterm or small for gestational age infant 13 X

38b OTHER RISK FACTORS FOR THIS PREGNANCY (Complete all items)
Tobacco use during pregnancy: Yes X No
Average number cigarettes per day: 20
Alcohol use during pregnancy: Yes X No
Average number drinks per week: 2
Weight gained during pregnancy: 20 lbs

39 OBSTETRIC PROCEDURES (Check all that apply)
Ultrasound 06 X

40 COMPLICATIONS OF LABOR AND/OR DELIVERY (Check all that apply)
Prolonged labor (>20 hours) 09 X

41 METHOD OF DELIVERY (Check all that apply)
Vaginal 01 X

42 ABNORMAL CONDITIONS OF THE NEWBORN (Check all that apply)
Assisted ventilation < 30 min 06 X

43 CONGENITAL ANOMALIES OF CHILD (Check all that apply)
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: Estimated Price Responsivity with Different Sets of Controls

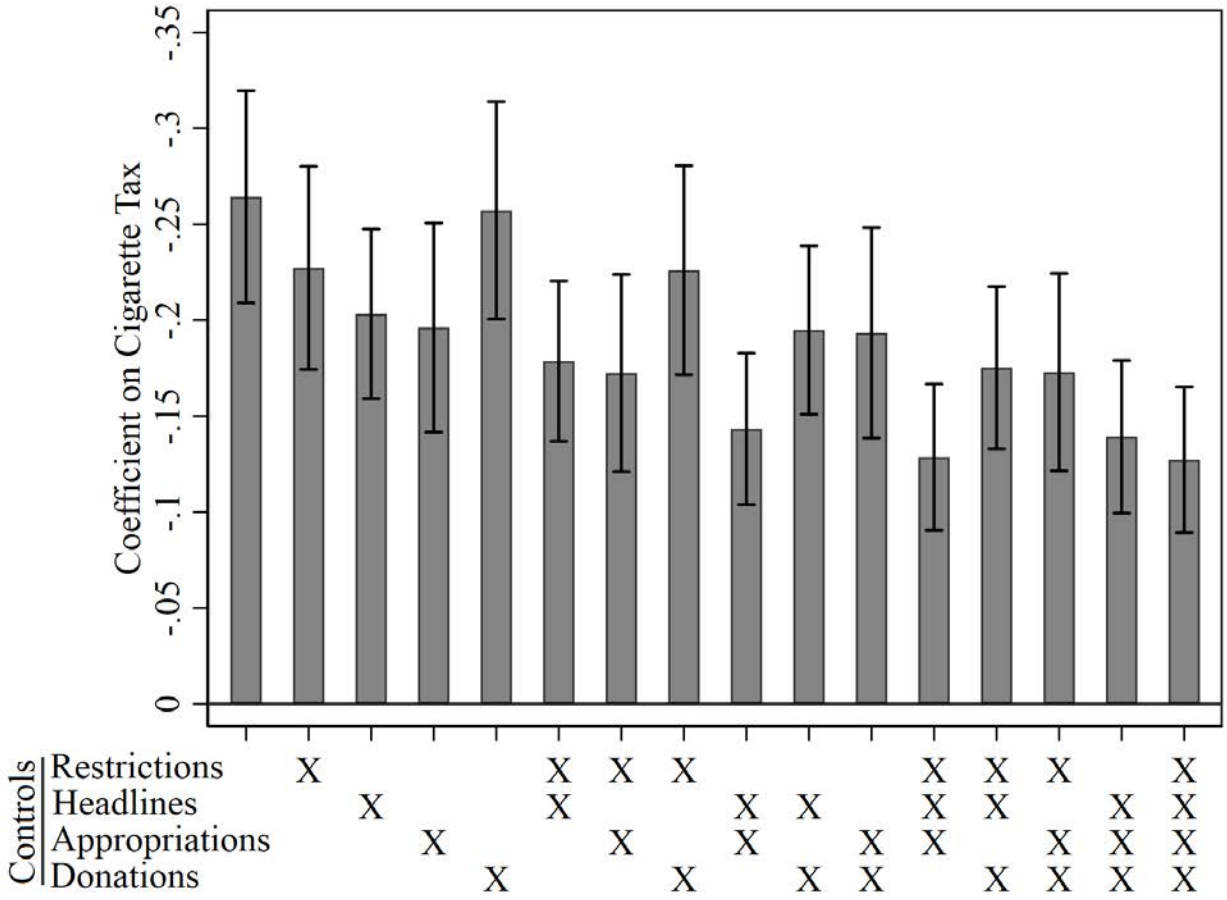


Table A1: Predictors of Cigarette Consumption in States without Tax Changes

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

Notes: Standard errors 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***	-0.227***	-0.259***	-0.253***	-0.264***	-0.217***
	(0.033)	(0.031)	(0.032)	(0.031)	(0.033)	(0.029)
Place-Based Legal Restrictions Index		-0.152***				-0.132***
		(0.031)				(0.031)
Monthly News Headlines Per Journal			-0.003			-0.002
			(0.005)			(0.005)
Monthly Anti-Smoking Appropriations Per 1000 Citizens				-0.147***		-0.139***
				(0.038)		(0.038)
Monthly Tobacco Industry Political Donations Per 1000 Citizens					-0.138*	-0.181**
					(0.072)	(0.074)
N	9,642	9,642	9,642	9,642	9,642	9,642

Notes: Standard errors 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***	-0.211***	-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)
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

Notes: Standard errors 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.