Tobacco Consumption Habits in Argentina: Causal Evidence from a New Regulation

Victoria Barone *
UCLA

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Abstract

This paper assesses the effectiveness of indoor smoking bans and graphic tobacco warnings in the context of low- and middle-income countries, where 80% of smokers worldwide live. I study Argentina's 2011 anti-smoking law. I exploit differences in previous state-level legislation to estimate the causal effect of the non-price policy on smoking prevalence, alcohol consumption, and health outcomes. I find that the probability of being a current smoker decreased by 6.17 percentage points due to the law implementation. Nonetheless, when looking at heterogeneous effects, my results suggest that the policy disproportionately benefited more educated and richer individuals.

Keywords: risky behaviors, tobacco control regulation, event study, developing countries
JEL codes: I12, I18, H75, D12

*Department of Economics, University of California-Los Angeles, 9262 Bunche Hall, Los Angeles, CA 90095. Contact: mvbarone@g.ucla.edu. I am grateful to Maurizio Mazzocco, Michela Giorcelli, Adriana Lleras-Muney, and Manisha Shah for advice and support. I thank Matias Morales Cerda, Fernanda Rojas-Ampuero, and participants in the Albert Family Proseminar in Applied Microeconomics at UCLA for helpful comments and conversations.

I Introduction

Modifiable risky behaviors, such as smoking tobacco, are a major determinant of premature death in both developed and developing countries (Blecher, 2008; Cawley and Ruhm, 2011). The World Health Organization (WHO) estimates that tobacco use is the leading risk factor in high-income countries, accounting for 18% of deaths.[1] In middle-income countries, where 80% of smokers worldwide live, tobacco use is the second most important risk factor and is responsible for about 11% of deaths. Moreover, Goodchild, Nargis and d'Espaignet (2018) estimate that 40% of the health care expenditure due to smoking-attributable diseases occurs in developing countries, highlighting the substantial burden these countries face.

Although tobacco has long been heavily taxed, non-price policies have become increasingly common in the last two decades. The most frequently implemented non-price policies are place-based bans and warning labels. For instance, by 2016, at least 105 countries required graphic warnings to be printed on cigarette packages.[2] Non-price policies are particularly attractive in middle- and low-income countries, where tobacco use is concentrated among low-income households.[3]

In this paper, I examine the effect of non-price interventions on smoking, drinking, and health outcomes in the context of a middle-income country, Argentina, where smoking is responsible for 13.2% of deaths, with a direct cost equal to 0.75% of its GDP—or, equivalently, 7.5% of the 2015 federal health budget (Alcaraz et al., 2016). In 2011, the Argentinean government implemented an anti-smoking law that banned smoking in public spaces, including bars and restaurants, and in public and private workplaces; restricted cigarette sales; and regulated the inclusion of graphic tobacco warnings on cigarette packages. The Argentinean case offers a good setting for this research because (i) the policy implementation was not accompanied by changes in taxation or cigarette prices, so it represents a *pure* non-price policy, and (ii) Argentina is a federal country, like the US, which allows me to exploit state-level differences in the stringency of tobacco regulation before

[1] WHO, 2009

[2] In 2001, Canada became the first country in the world to introduce graphic warnings on cigarette packages and was quickly followed by many others (Azagba and Sharaf, 2012). In the US, the 2009 Tobacco Control Act required that cigarette packaging includes graphic warnings, but implementation has been delayed by legal challenges from cigarette manufacturers. For example; on March 2, 2021, the U.S. District Court for the Eastern District of Texas granted a motion by the plaintiffs in the case of R.J. Reynolds Tobacco Co. et al. v. United States Food and Drug Administration et al., No. 6:20-cv-00176, to postpone the effective date of the "Required Warnings for Cigarette Packages and Advertisements" final rule by an additional 90 days. The new effective date of the final rule is April 14, 2022.

[3] Tobacco use rises to 29% in households in the lowest income quintile versus 20% in the richest households; see Crawfurd and Nestour (2019) and Fuchs, Gonzalez Icaza and Paz (2019).

2011 as a source of exogenous variation.

The contribution of this paper is threefold. First, this paper contributes to a vast literature in health economics that evaluates place-based bans and has yielded mixed results. North America-based research shows mixed impacts of smoking bans on smoking behavior (Adda and Cornaglia, 2010; Bitler, Carpenter and Zavodny, 2010; Carpenter, Postolek and Warman, 2011; Burton, 2020), while research in Europe suggests a systematic reduction of small magnitude in smoking behavior (Buonanno and Ranzani, 2013; Pieroni et al., 2013; Sureda et al., 2014). The local context of smoking behavior could help explain differences in estimates: how popular and accepted smoking is, whether it is strongly associated with other behaviors, and whether the accessibility of smoking cessation programs among others would contribute to individuals' responses to anti-tobacco policies. Nonetheless, the effectiveness of smoking restrictions in the context of middle- and low-income countries, where 80% of tobacco smokers worldwide live, has been much less studied. My research fills this gap by analyzing the anti-tobacco law passed by the Argentinean federal government in 2011. Unlike other studies of the effects of smoking bans in the context of middle- and low- income countries that have relied on cross-country (e.g, Blecher, 2008; Abascal et al., 2012) or before-and-after comparisons (e.g., Thrasher et al., 2010), I employ an identification strategy that provides causal estimates of the impacts of this policy on smoking, drinking, and health outcomes.

Second, this paper contributes to the literature on the impacts of graphic tobacco warnings. Cigarette warning labels are well-established policies, but research based on actual consumption behavior is limited. The effectiveness of this policy has been assessed by randomized controlled laboratory experiments (e.g., Hammond, 2011) and cross-country comparisons (e.g, Blecher, 2008). The main empirical challenge in evaluating this policy is that graphic warnings have often been enacted as part of broader anti-smoking campaigns (DeCicca, Kenkel and Lovenheim, 2020). To overcome this challenge and disentangle the effect of graphic tobacco warnings and place-based bans, I combine (i) a simple framework that imposes structure on the effect of a bundled policy and (ii) additional variation among early and late adopters of place-based bans. My paper is closest to Kuehnle (2019) who uses individual-level panel data from Australia to examine the association between pictorial warnings and smoking behavior—prevalence, quitting,initiating and relapsing. Nonetheless, one limitation of their study is that it cannot separate the effect of pictorial warnings from the effects of the introduction of Quitline reference, and mass media campaigns that accompanied the policy.

Third, I aim to advance the economic research on tobacco regulation effects on health outcomes. The main empirical challenge in measuring the effects of regulatory changes

on health outcomes originates in the fact that many health conditions associated with smoking—such as, heart disease, lung cancer, and overall premature mortality—are processes that develop over long periods of time making it difficult to know the appropriate lag between policy implementation and disease onset and progression (DeCicca, Kenkel and Lovenheim, 2020). To overcome this limitation, I exploit data on hospital discharges, this approach has two advantages. On one hand, when studying hospitalization events rather than mortality, the lag required to observed health effects is shorter. On the other hand, hospitalization data is well suited to capture health gains, such as less severe symptoms associated with smoking cessation, that might not translate into mortality declines. For example, in the case of chronic obstructive pulmonary disease (COPD), it has been shown that smoking cessation and avoidance of cigarette smoke reduce the excess lung function decline (Eklund et al., 2012). [4]

Data for this paper come from three main sources. First, to estimate the effects of this non-price intervention on smoking and drinking, I use individual-level data from two national surveys for the years 2008 to 2013.[5] These data include geographic location, demographic characteristics, and cigarette and alcohol consumption, which I use to construct the smoking status and participation and drinking behaviors of smokers. Second, to study the effects on health outcomes, I use restricted-access administrative data on hospital discharges provided by the Argentinean National Center for Health Statistics, which I aggregate at the state level. Third, I compile data on tobacco regulation from state statutes and laws, coding a total of 47 state-level regulations to characterize the regulatory environment of each state before the 2011 national law was enacted.

My identification strategy relies on state-level differences in the strength of regulation on tobacco products before the national law was passed. States with more lenient restrictions were more exposed to the effects of the new regulation than states with strict regulations. I compare the smoking and drinking behavior of individuals in lenient states (treated individuals) with that of individuals in strict states (comparison states). I show that before 2011, treated and comparison states were comparable in observable characteristics, including smoking prevalence and demographic composition. I also show that individuals in treated and comparison states were observationally equivalent before 2011.

I find that the anti-tobacco regulation effectively reduced smoking participation: The

[4]Chronic obstructive pulmonary disease (COPD) is a chronic inflammatory lung disease that causes obstructed airflow from the lungs. Symptoms include breathing difficulty, cough, mucus (sputum) production and wheezing. The main cause of COPD is tobacco smoke and smoking accounts for up to 8 out of 10 deaths (US Department of Health, 2014)

[5]The National Survey on Risk Factors (ENFR) and the National Survey on Consumption Prevalence of Psychoactive Substances (EnPreCoSP); data are available for the years 2008, 2009, 2011, and 2013.

probability of being a current smoker decreased by 22% among people aged 18 to 65. On the intensive margin, the share of individuals smoking fewer than 5 cigarettes a day decreased by 0.16 percentage point, suggesting that the reduced probability of being a current smoker is driven by former smokers whose consumption was on the lower end of the distribution. But importantly, the 2011 national law has regressive implications. Non-price policies are expected to be less regressive than price policies, because they do not increase the economic burden of consumption on poorer smoker; who make up a large proportion of the smoking population in developing countries. Despite not changing the monetary cost of smoking, I find that the policy disproportionately benefited more educated and richer individuals. Specifically, an elementary school graduate is 4.37 percentage points less likely to be a current smoker, while a college graduate is 9.86 percentage points less likely after the policy implementation. A similar pattern is observed across the income gradient: an individual who belongs to the highest income quintile is 8.92 percentage points less likely to be a smoker after the national law was introduced—3 percentage points greater in absolute values than the change for the lowest income quintile.

Understanding whether alcohol and tobacco are consumed as complements or substitutes is crucial in determining the side effects of tobacco control policies, especially when these policies target venues such as bars and restaurants where smoking and drinking are usually combined. Smoking bans in bars and restaurants represent a change in the non-price determinant of demand for alcohol consumed in bars; under the hypothesis that these goods are substitutes, an effective anti-smoking policy would backfire and lead to an increase in alcohol consumption. I find that the new tobacco regulation induced a decrease in the abusive consumption of beer (3 percentage points), wine (2.74 percentage points), and binge drinking (5.44 percentage points). The direction of these changes is consistent with a complementary relationship when it comes to the consumption of these pairs of goods. [6]

The impacts on extensive and intensive margin outcomes are reflected in slightly better health outcomes. The rate of hospital discharges due to diagnoses of chronic obstructive pulmonary disease (COPD) decreased in the short run, but I find no effects on rates of hospital discharges due to lung cancer. I interpret these results as suggestive of an improvement in population health correlated with more strict regulation of tobacco products, but due to data limitations, I cannot perform a longer term analysis of these out-

[6] I find that, in contrast to beer and wine, smoking and spirits—vodka, gin, tequila, rum, and whiskey—have a substitution relationship on consumption. One potential explanation of this result is that smokers who drink spirits are different from other smokers who abuse alcohol. I find that this result is driven by single young people (less than 25 years) with more education who are increasing their consumption of alcohol.

comes or identify whether the results are driven by fewer individuals being diagnosed or the same number of individuals being diagnosed with less severe symptoms and requiring fewer hospitalizations.

The rest of the paper is organized as follows. Section II presents background information on tobacco regulation in Argentina. Section III describes the data. Section IV presents the research design and provides evidence supporting the identification assumption. Section V examines the effects of the national regulation on smoking, drinking, and health outcomes. Section VI discusses the mechanisms through which the law operates and section VII concludes.

II Non-price Regulation and the Argentinean National Law

Argentina's 2011 national law has three main components: (i) it bans smoking in public spaces, such as restaurants, bars, educational institutions, and public and private work sites; (ii) it bans sales in schools, hospitals, public buildings, and means of public transportation; and (iii) it regulates advertising of tobacco products and mandates the incorporation of graphic tobacco warnings. These warnings consist of short messages about smoking's health consequences and side effects displayed with a shocking picture on one side of the package (see Appendix Figure A.1 for examples). Images are designed by the National Department of Health and are updated once every 18 months in order to attenuate potential wear-out effects of graphic warnings. After the law was approved, producers had up to 6 months to incorporate the designs on their packages and could not adapt the warnings to reduce their effectiveness. [7]

Argentina is a federal country; as in the US, states have autonomy to implement different regulations. States do not have control over cigarette prices, and the taxation of tobacco-related products is exclusive to the national government. However, each state has the autonomy to regulate smoking and access to tobacco products. I compile data on tobacco regulation from state statutes and laws for each of the 23 Argentinean states and the capital city to characterize the regulatory environment of each state before the 2011 national law was enacted. Then, I use this information to construct a legislation index, which is a discrete and bounded index that summarizes the state-level regulations.[8]

[7] An advantage of this setting is that individuals are *almost* randomly assigned to graphic warnings. Cigarettes packages are mainly sold at convenience stores, where consumers cannot directly access the cigarette package and must ask the cashier to retrieve it, leaving very little room to pick which warning is printed on the package they are buying. This means that the effects I estimate are an average effect of the graphic warnings selected by the Argentinean government, but, I cannot provide evidence about which warning is more effective.

[8] This attempt to assemble data on the enactment of tobacco regulation policies is similar to the Pre-

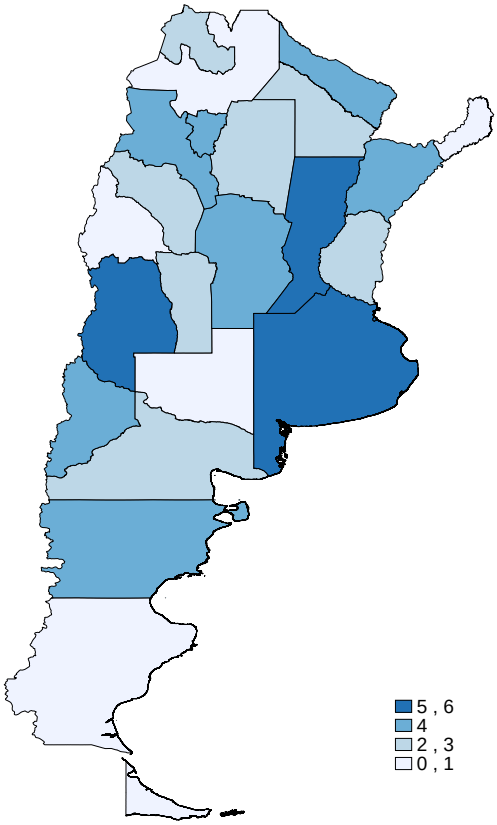
Higher values on the legislation index represent strong regulation of tobacco products. A value of 0 indicates that a given state has only banned cigarette sales to minors. A value of 1 indicates that the state has also banned consumption in public means of transportation. A value of 2 indicates that a state has banned consumption in educational and health care institutions or that a state has banned some type of advertising—e.g., event sponsorship. A value of 3 indicates that the state has banned consumption in additional venues, such as bars and restaurants. Each subsequent unit increase indicates a tightening in the regulation. The highest value of the index before 2011 was 6, which implies limiting advertising in public spaces, sport events and other venues; banning indoor smoking in several venues (bars, restaurants, educational and health care institutions); and allowing cigarette sales only in specialty or authorized shops. A value of 7 was reached by all states in 2011 when the national law was passed, which demonstrates the fact that no state had legislation as strict as the new national law. The legislation index allows for a convenient summary of the regulatory apparatus, but its weakness is that each unit increase does not reflect the same change in regulation. The data construction section in Appendix B. provides details on the criteria and definitions used.

Figure 1 shows the regional variation across states in 2009. Darker shades on the map (higher values of the legislation index) indicate stricter regulation, and lighter shades (lower values of the legislation index) indicate less strict regulation. The 2011 law impacted the regulation of tobacco products in every state with different intensity. For example, in a state in which only sales to minors were banned (a state with a legislation index equal to 0), the national law introduced place-base bans, advertising limitations and graphic warnings. On the other hand, in a state in which tobacco was heavily regulated—such as a state with a legislation index of 6,—the main change was the incorporation of graphic warnings. Nonetheless, every state experienced tightened regulation after introduction of the 2011 national law. The ideal research design would exploit the intensity of treatment—i.e., the difference between the value of the legislation index for state s and the maximum value of the index. This exercise would be informative regarding the effectiveness of the policy for different increments in strength of regulation. Unfortunately, the number of states and years observed is relatively small, which limits the implementation of this design. Hence, I use the legislation index to identify lenient vs strict states, and use the latter as the comparison or untreated group. In the rest of the paper, I refer to "lenient" states as those with low legislation index value (less than or

scription Drug Abuse Policy System, which aims to track state laws related to prescription drug abuse. Researchers have widely used these data to study the effects of such laws on opioid use and mortality (see Buchmueller and Carey, 2018)

equal to 3) and to the other states as "strict" or comparison states. As a robustness check, I replicate the main analysis using an alternative definition of treated and comparison states, and find similar results (see robustness Section C.1).

Figure 1: Legislation Index in 2009



Notes: The legislation index summarizes the strength of regulations at the state level; higher index values (darker shades) indicate stricter regulation and lighter shades indicate more lenient regulation. See Appendix B. for details on construction of the legislation index.

III Data and Descriptive Statistics

To measure tobacco and alcohol consumption, I use individual-level data from the National Survey on Risk Factors (ENFR) and the National Survey on Consumption Prevalence of Psychoactive Substances (EnPreCoSP). The surveys provide information on self-reported consumption of tobacco and alcohol within the last year for individuals aged 18 to 65.[9] Importantly, these survey data include state identifiers and are available for the

[9]Both surveys use the same questionnaire for smoking and alcohol consumption. The main difference between the surveys is that the ENFR also asks individuals about their health, diet, and physical activity, among other behaviors, while the EnPreCoSP also asks individuals about consumption of other drugs, such as marijuana.

years before and after the policy was implemented: 2008 , 2009, 2011, and 2013. I study two outcomes of smoking behavior. First, I construct extensive margin outcomes, defined as the probability that an individual is a current smoker and the probability that an individual has never smoker. I consider an individual to be a current smoker if she has ever smoked more than 100 cigarettes and if, at the moment of the survey, she smokes every day or some days. I consider an invidual to be a never smoker if she has never smoked befor or if, she has smoked less than 100 cigarettes. These definitions are close to the one used by Carpenter, Postolek and Warman (2011) with Canadian survey data. Second, I study an intensive margin measure given by the number of daily cigarettes smoked in the last month; this measure is informative regarding whether the distribution of smoked cigarettes was responsive to the national law.

I recognize two potential limitations of these survey data. First, *social smokers* could be classified as non-smokers. This would imply that my results provide a conservative estimate of the true parameters of interest, since restaurants and bars are where social smokers are more likely to smoke and drink and the policy directly bans consumption in these venues. Second, extensive- and intensive- margin measures are based on self-reported data, which are not free of measurement error; this would arise if smokers deny their habit when surveyed or incorrectly report how much they smoke. I use data on sales to provide evidence on the direction of measurement error in Table A1. Consumption time series from sales and survey data show a similar trend and suggest that (i) individuals underreport their consumption and (ii) under-reporting is stable across the years, i.e. it did not change after the implementation of the national law.[10] Smoking behavior outcomes are measured with some error in the survey data; however, the direction of this error is consistent with my results providing a lower bound rather than being driven by changes in reporting after the policy.

To document whether cigarettes and alcohol are substitutes or complements, I construct measures of alcohol consumption among smokers. I study alcohol consumption in the last month and two measures of harmful alcohol consumption: abusive consumption and binge drinking. I define alcohol abuse as an indicator of having more than eight drinks of beer, five drinks of wine or three drinks of spirits in a given day.[11] Binge drinking is defined as consuming five or more drinks on a single occasion in the past 30 days, on either a weekend or a weekday.

[10]Survey data captures about 58% of consumption relative to sales data for every year that is available. Unfortunately, sales data is not available at the state level so I can not reproduce my main results using these data.

[11]Spirits include vodka, gin, tequila, rum, and whiskey. These thresholds are defined following the guidelines from the World Health Organization (WHO) and the Center for Diseases Control (CDC).

Last, I study effects on health outcomes. I use restricted-access administrative data on hospital discharges to compute the prevalence rate of hospitalization from COPD and respiratory-system-related cancer.[12] These data come from the Argentinean National Center for Health Statistics and are provided at the hospitalization level.[13] In the data, one observation corresponds to a hospitalization event for which I observed the individual's gender, age, and the main diagnosis and a state identifier. The main limitation of these data is that I do not observe individual identifiers or their smoker status. For this reason, my analysis is only suggestive with respect to how the population's health changed after the national law and cannot distinguish whether there are fewer diagnoses or less severe ones.

Table 1: Summary Statistics, Pre-policy Period

Variable (mean)	All states	Strict states	Lenient states	Equality of means
Never-smokers	0.560	0.561	0.558	0.777
Current smokers	0.286	0.281	0.290	0.587
Mean cigarettes per day				
0 to 5	0.373	0.397	0.352	0.024
6 to 10	0.266	0.260	0.270	0.513
11 to 20	0.297	0.282	0.310	0.073
more than 20	0.064	0.060	0.068	0.382
Prevalence rate (per 1,000)				
COPD	0.203	0.196	0.209	0.765
Lung cancer	0.069	0.061	0.076	0.330
Number of states	24	11	13	

Notes: This table presents summary statistics for the outcomes of interest for the pre-policy period. I pooled 2008 and 2009 for consumption outcomes and 2008 to 2010 for health outcomes. The last column reports the p-value for the test of equality of means for strict and lenient states. The category more than 20 cigarettes per day includes smokers with consumption strictly greater than 20 and less than or equal to 50 cigarettes a day.

Table 1 shows mean outcomes before the national law was enacted. Strict states (high values in the legislation index) and lenient states (low values in the legislation index) have comparable proportions of never and current smokers. But, the distribution of smoked cigarettes per day in lenient states has a heavier right tail; i.e., the proportion of more-

[12]COPD is defined using ICD 10th revision codes J41, J42, J43, and J44. Respiratory-system-related cancer is defined using ICD 10th revision codes C30, C33, and C34.

[13]In Spanish, Direccion de Estadisticas e Informacion de Salud (DEIS). This agency is analogous to the U.S. National Center for Health Statistics.

than-10-cigarettes-per-day smokers is higher in these states. The difference in the distribution of smoked cigarettes per day—statistically significant at the 5- and 10-percent level—suggests that strict and lenient states might have been on different trends before 2011. I directly test for the presence of pre-trends in intensive-margin outcomes in Table 3. I find no statistically significant differences in the proportion of smokers whose consumption is between one and five cigarettes per day, or between 10 and 15 cigarettes a day. I find a small difference in the consumption group 15 to 20 cigarettes per day, which is statistically significant at a 10-percent significance level and not economically meaningful. Importantly, strict and lenient states show similar prevalence rates in the diagnosis directly affected by tobacco smoking.

IV Empirical Strategy

The 2011 law impacted the regulation of tobacco products nationally, but I exploit the fact that states with more lenient restrictions were more exposed to the effects of the new regulation, by comparing more lenient with stricter states. To do so, I define a dichotomous variable ($Treat_s$) that equals 1 if the legislation index for state s is less than or equal to 3 before 2011; i.e., states with legislation strictly greater than 3 serve as comparison states.[14] I estimate the causal effect of the national regulation via the following regression, which is run over the sample of individuals in treated and comparison states two periods before and one period after the regulation was implemented:

$$y_i = \sum_{\tau=-2}^{1} \delta_\tau \left[Treat_s \cdot (Years\ After\ Treat = \tau) \right] + \beta' X_{is} + \Gamma' X_{st} + \alpha_s + \alpha_t + \varepsilon_i \,, \qquad (1)$$

where s indexes the state in which individual i is observed in year t,and y_i is the outcome of interest. The variable $Years\ After\ Treat = \tau$ indexes time relative to the law's implementation.[15] The variable $Treat_s$ indicates whether state s is a comparison state ($Treat_s = 0$) or a treated state ($Treat_s = 1$). In this specification, lagged coefficients (δ_{-2} and δ_{-1}) test whether the outcome of interest y in comparison and treated states followed similar trends before the policy was introduced. The coefficient for the lead period, δ_1 captures the effect of the national law 1 year after implementation. The omitted coefficient is δ_0, which corresponds to the year of the treatment. X_{is} is a vector of control variables that comprises individual-level characteristics—e.g., age, gender, educational

[14] I present estimates of the main results defining $Treat_s = 1$ if the legislation index is strictly less than 3 in the robustness checks section (see Section C.) and arrive at similar conclusions.

[15] I normalize time relative to the law implementation to match the years when I observe outcome variables: i.e., $\tau = -2$ corresponds to 2008, $\tau = -1$ to 2009, $\tau = 0$ to 2011, and $\tau = 1$ to 2013.

attainment, and income. I also include state-time varying controls: private employment and population. States' fixed effects (α_s) control for variation in outcomes across states that is constant over time. Time fixed effects (α_t) control for variation in outcomes over time that is common across all states. The variable ε_i is an individual error term. Standard errors are block-bootstrapped at the state level with 200 replications.[16]

My identifying assumption is that comparison states would have been on the same trend as treated states absent the national legislation. The remainder of this section provides evidence in support of the research strategy and discusses the identifying assumption.

A. *Why Did State Governments Regulate Tobacco Consumption?*

As shown in Table 1, treated and comparison states are comparable in terms of the outcomes of interest before the national law was enacted. Nonetheless, identifying the causal effect of the 2011 national law on tobacco consumption requires that treatment and comparison status at the state level be exogenous to outcomes of interest. In order to better understand why some states chose to regulate tobacco more than others before 2011, I regress the probability of having enacted strong regulation before 2011 on a set of covariates that include the political affiliation of the state government, an indicator of whether a state is a tobacco producer, measures of the prevalence of tobacco consumption and hospitalizations due to COPD, a measure of private employment that proxies the economic well-being of a state, and demographic characteristics. These results are presented in Table A2. The Peronist party was responsible for enacting the 2011 national law; nonetheless, I find that the it was not more likely to regulate tobacco consumption at the state level than opposition parties. Similarly, I find that states that are tobacco producers did not have a smaller probability of being strict states, although one would expect these states to be more lenient regarding the consumption of tobacco products. States with a higher prevalence of COPD diagnoses were 0.394% more likely to have enacted a stricter regulation; although statistically different from zero, this coefficient indicates that for the probability of enacting a strong regulation to increase in 1% the prevalence of COPD has to increase 2.53 times. Finally, states with a greater share of individuals between 15 and 25 years were more likely to have a stricter regulation. In my estimates, I

[16]Using cluster-robust standard errors permits both error heteroskedasticity and flexible error correlation within clusters. With a small number of clusters, cluster-robust standard errors are downward biased (Cameron, Gelbach and Miller, 2008); block-bootstrap procedures yield more accurate cluster-robust inference when there are few clusters. Block-bootstrap standard-error estimation procedure maintain the autocorrelation structure within groups—states in this specific application—by keeping observations that belong to the same group together in a block, as it samples groups instead of observations.

control for state fixed effects, which partially purge initial differences across states in the prevalence of COPD and demographic composition.

B. *Were Individuals in Treated and Comparison States Observationally Equivalent?*

I test whether individuals in treated and comparison states were statistically indistinguishable in terms of their observed characteristics in 2008 and 2009.[17] Balancing test results are presented in Table 2. The estimated differences indicate that individuals are statistically indistinguishable in terms of gender, age, marital status, employment status, and educational attainment. I find evidence that on average, individuals in comparison states are poorer than individuals in treated states. The proportion of households in the second quintile is bigger in comparison states, while the proportion of households in the third and fourth quintiles is smaller in comparison states. To account for this difference in observable characteristics, I include controls for household income in every specification, as well as age, gender, employment status, and other individual characteristics.

[17] I present pooled results only. Conclusions on balance on observable characteristics do not change when the analysis is done by year.

Table 2: Balance in Terms of Individual Characteristics in Strict versus Lenient States

Variable (mean)	All states	Strict states	Lenient states	Equality of means
Average age	37.719	37.745	37.698	0.896
Male	0.491	0.495	0.487	0.581
Young (< 25 years old)	0.236	0.239	0.234	0.749
Married or cohabitant	0.579	0.570	0.587	0.316
Employed	0.701	0.702	0.700	0.878
Educational level				
Elementary school	0.196	0.195	0.196	0.973
High school	0.237	0.234	0.239	0.598
College	0.146	0.150	0.141	0.451
Income category				
First quintile	0.134	0.131	0.136	0.605
Second quintile	0.294	0.355	0.242	0.000
Third quintile	0.214	0.199	0.227	0.038
Forth quintile	0.222	0.181	0.257	0.000
Fifth quintile	0.136	0.135	0.137	0.876
Observations	60,449	23,830	36,619	
Number of states	24	11	13	

Notes: This table presents summary statistics for demographic characteristics for the pre-policy period. I pooled 2008 and 2009. Demographic characteristics other than age indicate the proportion of individuals with a given attribute. The last column reports the p-value for the test of equality of means for strict and lenient states.

C. *Did State Governments Enforce the 2011 National Law?*

Before examining the effects of the policy on outcomes of interest, I provide evidence of compliance with the law using data on exposure to environmental tobacco smoke in closed venues. In the 2009 and 2013 surveys, non-smokers were asked whether they notice someone smoking inside their home, the workplace, an educational institution, a hospital, or in bars and restaurants. With the exception of bars and restaurants, all states banned consumption in these venues before implementation of the national law. I use these data to document how exposure to tobacco smoke changed across venues before and after 2011. Figure A.2 shows that the share of non-smokers not exposed to environmental tobacco smoke increased by 5% in homes, workplaces, educational institutions and hospitals; whereas the share of non-smokers not exposed in bars and restaurants increased by 10%. These results suggest that exposure to tobacco smoke in bars and restaurants decreased more than in other venues. I argue that, since smoking in the workplace,

educational institutions, and hospitals was already regulated in comparison and treatment states before the 2011 law, the differential response in restaurants is informative of law enforcement and compliance.

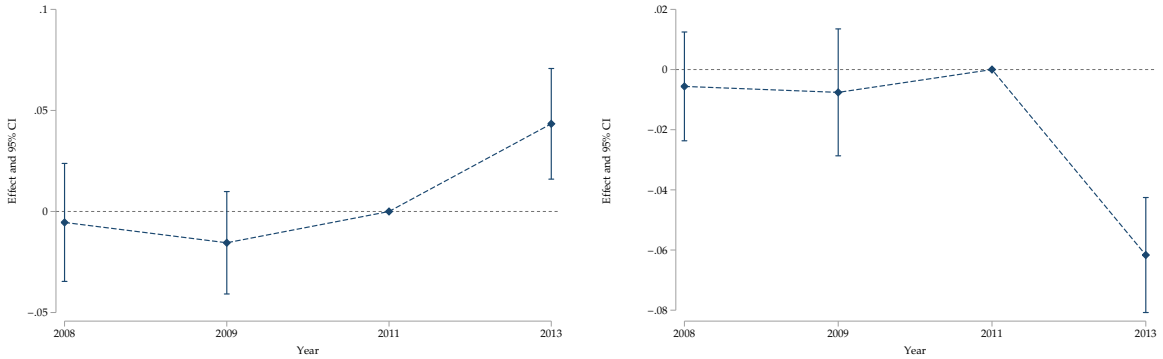
V Results

My empirical analysis proceeds in three steps. First, I estimate the causal impact of the 2011 national law on smoking behavior, by examining smoking participation and intensity among smokers. These exercises provide evidence on the effectiveness of the policy. Second, to assess potential side-effects of the tobacco control policy, I ask whether alcohol and tobacco are consumed as complements or substitutes . Finally, I study health outcomes in the short run.

A. *Effects of the 2011 National Law on Tobacco Consumption*

Extensive margin outcomes. Smoking participation decreased as a result of the stricter regulation implemented in 2011. To understand how the policy affected smoking participation, I study its effects on (i) the probability of individuals' having never smoked, and (ii) the probability of individuals' being current smokers. The former increased by 4.34 percentage points. This suggests that the regulation curbed tobacco initiation: The law caused a roughly 10% increase in the probability of an individual's being a never-smoker. This result is reinforced by a reduction of 6.17 percentage points in the probability of being a current smoker, which represents a 22% reduction in this outcome. Taken together these effects suggest that the law caused a reduction on smoking participation helping individuals to quit smoking and curving smoking initiation. Figure 2 shows these results and Appendix Table A3 presents point estimates and standard errors. These event-study graphs also show that comparison and treated states were on similar trends regarding cigarette consumption before the national law was passed. The estimates for $\delta_{\tau=-2}$ and $\delta_{\tau=-1}$ are not statistically distinguishable from zero, which provides additional support to the identification assumption.

Figure 2: Effects on Extensive Margin Outcomes



(a) Prob. Never Smoker (b) Prob. Current Smoker

Notes: This figure presents point estimates and symmetric percentile-t confidence intervals of the causal effect of the national regulation on the probability of being a never smoker (left panel) and the probability of being a current smoker (right panel). The omitted year corresponds to 2011, the year the law was passed. Standard errors are block-bootstrapped at the state level with 200 replications.

Intensive Margin Outcomes. I examine whether there is a reduction in the number of daily cigarettes smoked. To measure smoking intensity, I generate bins (b) of consumption in five-unit increments up to 20 cigarettes a day, and 10-unit increments for higher quantities. These intervals are open to the left: (0,5] (5, 10] (10, 15] (15, 20] (20, 30] (30,40] and (40,50].[18] For example, b_1 is a dummy variable that takes the value of one if the number of cigarettes smoked is positive but less than or equal to five. Results are presented in Table 3.

I find evidence that the distribution of cigarettes smoked daily shifts to the right; i.e., it is more likely that smokers consume more than 5 cigarettes per day and less likely that they consume fewer than 5 cigarettes per day. Specifically, the proportion of smokers who consume between one and five cigarettes per day decreased by 16 percentage points, while the proportion consuming between 5 and 20 cigarettes a day increased by 13%. Notice that about 93% of the consumers have consumption in the first four bins—i.e., up to a pack per day. Thus, estimating the effects for bins above 20 cigarettes a day requires a bigger sample or sizable effects to avoid power-related limitations.

Extensive and intensive margin results indicate that the reduction in the probability of being a current smoker is driven by quitters whose consumption was less than five cigarettes per day. Thus, after the law was passed, the pool of smokers becomes more negatively selected.

[18] In Table 3, I present the proportion of smokers for each bin in 2011. I did not include smokers who report smoking more than 50 cigarettes a day; they represent the 3% of the sample in 2011. Note that the shares of smokers in each bin add up to one and that the coefficients for each year add up to zero, since they represent the changes in accumulated mass in the distribution of smoked cigarettes.

Table 3: Effects on Intensive Margin Outcomes

Cigarettes smoked	(0,5]	(5, 10]	(10, 15]	(15, 20]	(20, 30]	(30 , 40]	(40 , 50]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2008	-0.0201	0.0225	-0.0006	0.0003	0.0015	-0.0042	0.0006
	(0.0293)	(0.0161)	(0.0085)	(0.0143)	(0.0077)	(0.0064)	(0.0009)
2009	-0.0052	0.0603	0.0008	-0.0379*	-0.0097*	-0.0101*	0.0018
	(0.0276)	(0.0227)	(0.0092)	(0.0201)	(0.0047)	(0.0052)	(0.0018)
2013	-0.1652***	0.0584*	0.0102*	0.0786***	0.0216	-0.0035	-0.0001
	(0.0295)	(0.0141)	(0.0087)	(0.0150)	(0.0050)	(0.0035)	(0.0009)
Mean dep. var.	0.3500	0.2708	0.0966	0.2143	0.0385	0.0272	0.0024
Observations	40,651	40,651	40,651	40,651	40,651	40,651	40,651
R-squared	0.0716	0.0108	0.0071	0.0324	0.0138	0.0181	0.0017
Correctly predicted	0.6242	0.6949	0.8627	0.7526	0.9077	0.9181	0.9398

Notes: This table presents estimated effects on cigarettes smoked in the last 30 days. The omitted category corresponds to 2011, the year the federal law was passed. All regressions include individual-level controls: age, gender, educational attainment, employment status, and income category of the household. State-time varying controls include total private employment and total population. All regressions include state and time fixed effects. Correctly predicted indicates the percentage of times the predicted outcome matches the actual outcome. Standard errors are block-bootstrapped at the state-level with 200 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

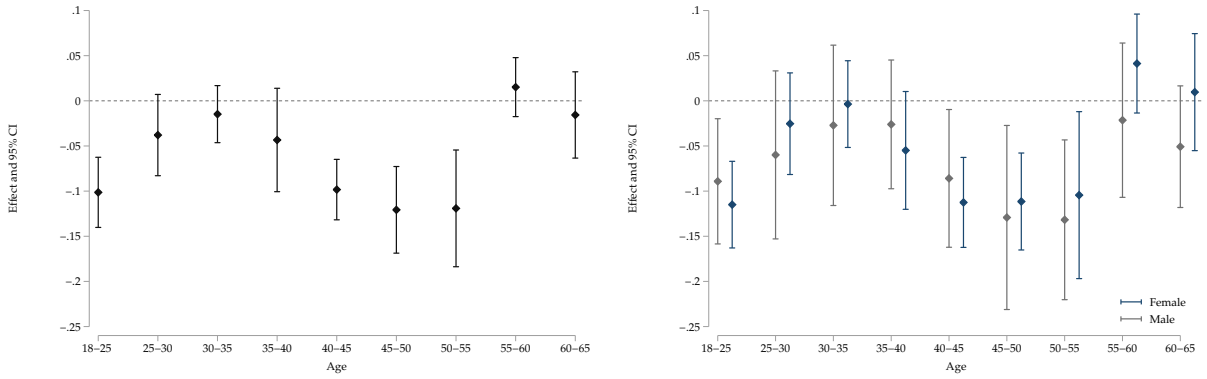
Heterogeneity in Smoking Participation. Do the effects of stricter restrictions on tobacco consumption vary across populations? To tackle this question, I estimate equation (1) restricting the sample by age group, gender, educational attainment, and household income. Figures 3 and 4 plot estimates of the causal effect of the national law by demographic group. Tables A4, A5, A6, and A7 complement these figures and present estimates of the pre-policy coefficients which are statistically indistinguishable from zero and provide additional support for the parallel trends assumption.

The reduction in the probability of being a current smoker is driven by individuals aged 18 to 25 years old and individuals aged 40 to 55 years old. Although the estimated effects are comparable in magnitude, the mechanisms behind them are quite different. The reduction in current smokers among the younger adults is driven by an increase in non-smokers and shows how the policy curbed tobacco initiation; whereas the result among adults aged 40 to 55 is driven by an increase in former smokers. In 2013, the percentage of never-smokers aged 18 to 25 was 66.54% and this figure for adults aged 50 to 55 was 46.89%, a 20-point difference. At the same time, the percentage of current smokers was comparable in the two age groups—26.66% and 28.81%, respectively, suggesting that

the group of younger non-smokers had a higher share of never-smokers, while the group of older non-smokers had a higher share of former smokers.

Adults aged 55 to 65 did not respond to the increase in the cost of smoking induced by the national law. A potential explanation is that people might find quitting more difficult the older they are and the longer they have been addicted to nicotine. The length of addiction mechanically increases as consumers age if they do not quit. Using intention-to-quit data, I find that 67% of smokers aged 50 to 55 have intended to quit smoking, while only 60% of smokers aged between 55 and 60 have intended to quit. Hence, the hypothesis of discouraged smokers is a plausible explanation for the null effects estimated for these groups. Other papers have find similar results, e.g., Kuehnle (2019) find that the smoking behavior of individuals aged 50 and over was not affected by the introduction of tobacco graphic warnings.

Figure 3: Extensive Margin Effects by Age and Gender



(a) Age groups

(b) Age-gender groups

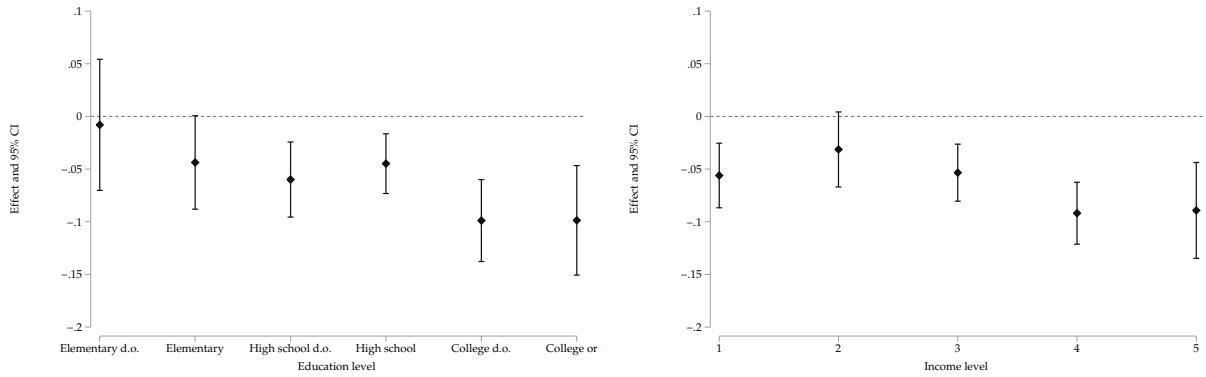
Notes: This figure presents point estimates and symmetric percentile-t confidence intervals of the causal effect of the national regulation on the probability of being a smoker in 2013 by subgroups. Standard errors are block-bootstrapped at the state-level with 200 replications.

Women are less likely to smoke than men, but they are equally responsive to increases in the cost of smoking induced by the national law. Panel (b) of Figure 3 presents estimated effects by age and gender and tests the null hypothesis that the effect on women of a given age is equal to the effect on men of the same age group. I do not find evidence to reject this hypothesis for any age group (see Table A5 for the respective p-values).

Cigarette taxes would be regressive with respect to income if poorer and richer consumers smoked at the same rate, since the same price increase entails a higher economic

burden for poorer consumers; this regressive effect is exacerbated when smoking prevalence is inversely related to income, as is the case in most developing countries. In contrast, non-price policies are particularly attractive in middle- and low-income countries, where tobacco use is concentrated among low-income households. These policies change the non-monetary costs of smoking and have the comparative advantage of not increasing the economic burden for consumption on poorer smokers. Nonetheless, I find that the 2011 national law has regressive implications. The causal impacts of the law on tobacco consumption are higher in absolute values for more educated and richer individuals (see Figure 4).

Figure 4: Extensive Margin Effects by Education and Income



(a) Educational attainment

(b) Income

Notes: This figure presents point estimates and symmetric percentile-t confidence intervals of the causal effect of the national regulation on the probability of being a smoker in 2013 by subgroups. Standard errors are block-bootstrapped at the state-level with 200 replications. Panel (a) presents results by educational attainment, in which d.o. stands for drop-out; panel (b) presents results by household income ranking. The 1^{st} quintile corresponds to the lowest 20% of households in the income distribution. Analogously, the 5^{th} quintile corresponds to the highest 20% of households in the income distribution.

I document that an elementary school graduate is 4.37 percentage points less likely to be a current smoker after the policy, while a college graduate is 9.86 percentage points less likely to be a current smoker after the policy implementation. Also, an individual who belongs to the highest income quintile is 8.92 percentage points less likely to be a smoker after the national law was introduced. This change is 3 percentage points higher in absolute values than the change for the lowest income quintile (a 5.61 percentage points decrease).

Robustness checks. The main results on extensive and intensive margin outcomes are robust to a change in the definition of the treatment. The baseline specification defines

the treatment group as those states with a legislation index less than or equal to 3 before 2011. I consider an alternative definition and use as a treatment group those states with a legislation index strictly less than 3 before 2011. This change implies that the pool of treated states has on average a more lenient regulation. I present the results of this exercise in Appendix Tables A12, A13, and A14. Regarding extensive margin outcomes, I find that (i) estimates of the coefficients for 2008 and 2009 are not statistically distinguishable from zero, and (ii) results on the probability of being a never-smoker and the probability of being a current-smoker are smaller in absolute value but still significantly different from zero. Regarding intensive margin outcomes, I find reductions in the probability of smoking up to 5, up to 10 and up to 15 cigarettes per day. These results imply that the distribution of cigarettes smoked daily shift to the right, similarly to the main results.

I also consider two additional robustness checks. First, I test whether the results are driven by one state by estimating the main outcomes of interest in samples that drop one state at a time. I present these results in Figure A.3, which results indicate that the estimated effect is not driven by one state with particularly strong effects. Second, another concern is whether the trend in cigarette price confounds my results. I present evidence that for the period of analysis, there is no sharp changes in the price of the cheapest cigarette package (see Figure A.4). I pay special attention at the minimum price because smokers could easily substitute between brands to avoid price changes in the more expensive brands.

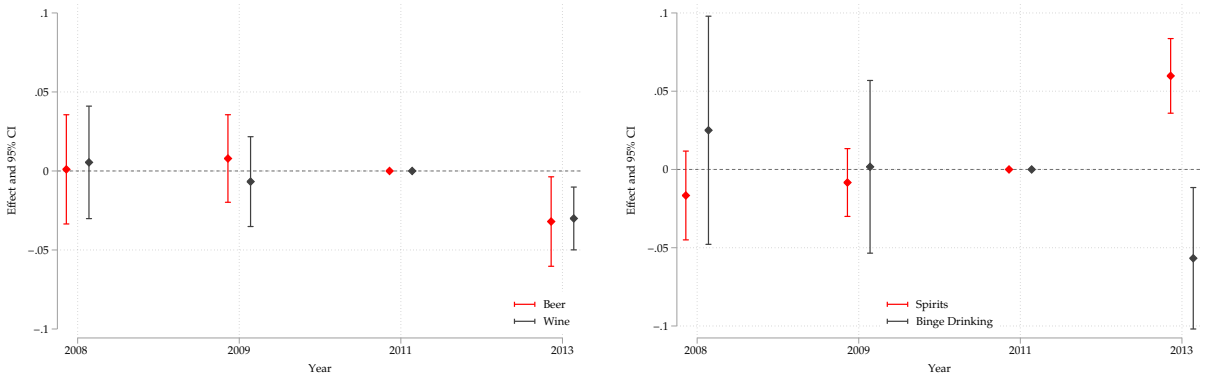
B. *Effects of the 2011 National Law on Alcohol Consumption*

Policies that ban the consumption of tobacco in bars and restaurants could have important effects on a smoker's alcohol consumption, since a smoking ban could lower a smoker's utility from drinking in a bar. Also, the question of whether alcohol and tobacco are consumed as complements or substitutes is crucial for determining the side effects of tobacco control policies. Under the hypothesis that these goods are substitutes, an effective anti-smoking policy would backfire and lead to an increase in alcohol consumption, all else equal. Conversely, under the hypothesis that they are complementary goods, an effective anti-smoking policy would lead to a decrease in alcohol consumption. Most of the literature has studied the responses of alcohol consumption to a price policy by exploiting changes in cigarette prices (e.g., Decker and Schwartz, 2000; Tauchmann et al., 2013; Krauss et al., 2014; Shrestha, 2018). A notable exception is Burton (2020), who finds that smoking bans in bars and restaurants lead to an increase of one serving of alcohol a month, though these bans do not have an effect on smoking participation. In this

paper, I study alcohol consumption and alcohol abuse; the latter is particularly important in assessing the potential negative side effects of tobacco control policies.

Figure 5 reports estimates of the marginal effects on alcohol consumption from the estimation of equation (1) using a probit model. Appendix Table A8 presents the estimates of the (raw) coefficients. Marginal effects can be interpreted as the percentage-point change in the relevant outcome relative to the average level of such outcomes. Standard errors are clustered at the state level. The estimated coefficients for 2008 and 2009 are statistically indistinguishable from zero; this supports the identifying assumption of parallel trends in the consumption of alcoholic beverages between comparison and treated states.

Figure 5: Effects on the Risky Consumption of Alcoholic Beverages



(a) Beer and Wine

(b) Spirits and Binge Drinking

Notes: This figure presents estimates of the marginal effects and confidence intervals of the 2011 national law on the probability of abusive consumption of beer, wine, and spirits and binge drinking across smokers. Abusive consumption thresholds and binge drinking are defined in the main text. The omitted category corresponds to 2011, the year the federal law was passed. Standard errors are clustered at the state level.

I find no evidence of changes in the average consumption of alcohol by smokers (column 1 of Table A8), but measures of extreme consumption did change as a result of stricter tobacco regulation, i.e., I do find a reduction in unhealthy alcohol drinking behaviors, such as, alcohol abuse and binge drinking. In particular, cigarette smoking and the abusive consumption of beer and wine are complementary goods, i.e., the consumption of wine and beer decreases after an increase in the non-monetary cost of smoking. Similarly, tobacco consumption and binge drinking also change in a direction that suggests a complementary relationship. The stricter regulation of tobacco consumption caused a reduction in smoking participation and a decrease in the abusive consumption of beer of 3.20 percentage points; of 3.00 percentage points for wine, and of 5.67 percentage points

for binge drinking.

Tobacco and spirits—vodka, gin, tequila, rum, and whiskey—have a substitution relationship. The consumption of spirits increased by 5.98 percentage points after the stricter regulations on tobacco consumption and advertising went into effect. The average level of abusive consumption of spirits in 2011 was 3.18%, and thus the estimated effect suggests a huge increase in this risky behavior. A potential explanation of this result is that smokers who drink spirits are different from other smokers who abuse alcohol. Smokers who abuse spirits are more likely to be single young people (less than 25 years) and are more educated and wealthier than those who abuse other drinks; see appendix Table (A9). Thus, the difference in sign of the coefficient could be attributable to young people who were induced to substitute smoking for drinking in bars and pubs. This seems to be especially true for young men; the policy effect on spirits consumption is three times larger for men than for women.

C. *Effects of the 2011 National Law on Health Outcomes*

Reduced smoking in public places could entail important health benefits on both nonsmokers and smokers. Smoking can cause lung disease by damaging the airways and the small air sacs (alveoli) in the lungs. COPD, which includes emphysema and chronic bronchitis, is one of the most common lung diseases caused by smoking. Also, cigarette smoking causes most cases of lung cancer. Estimates from the US indicate that smoking causes about 90% of all lung cancer deaths and 80% of all deaths from COPD.[19] In the Argentinean setting, Alcaraz et al. (2016) estimate that cigarette smoking caused 75% of COPD deaths and 82% of lung cancer deaths, while 33% of other cancers could be attributed to cigarette smoking.

I estimate the effects of the clean-indoor-air policy and the introduction of graphic tobacco warnings using hospital discharge data. I construct the prevalence rate for disease c for age group a as the ratio between the number of cases with diagnoses c in age group a and the total population aged a of state s in year t times 1,000. I run the following regression over the sample of treated and comparison states for the period 2008-2014 by cause of discharge c:

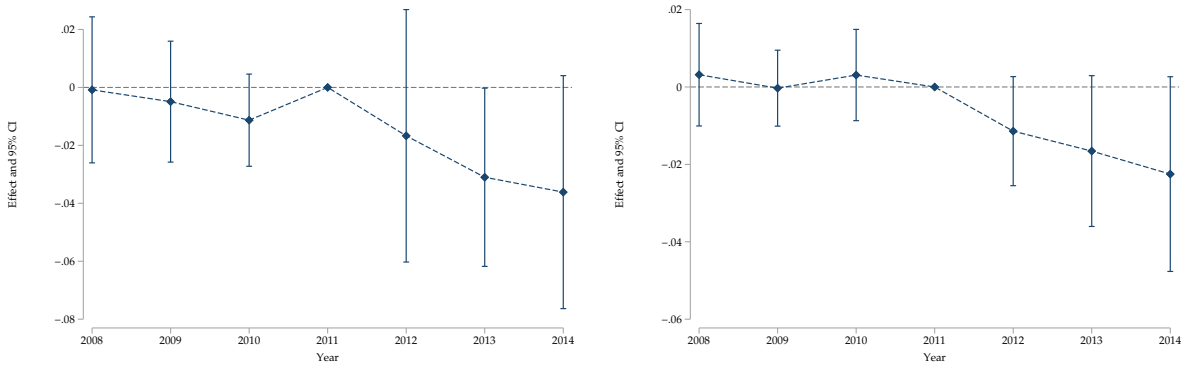
$$y_a = \sum_{\tau=-3}^{4} \delta_\tau \left[Treat_s \cdot (Years\ after\ treat = \tau) \right] + \Gamma' X_{st} + \alpha_a + \alpha_s + \upsilon t + \epsilon_a, \qquad (2)$$

where $Years\ after\ treat = \tau$ is equal to the difference between the calendar year and the

[19] See Department of Health and Human Services, *The Health Consequences of Smoking—50 Years of Progress: A Report of the Surgeon General, 2014.*

year the national law was passed. The variable $Treat_s$ is defined as before. X_{st} is a vector of control variables that comprise state-level characteristics. I include age-group fixed effects (α_a), and thus δ_τ is identified by within age-group differences between lenient and strict states over time. The linear time trend t controls for changes in population health over time that are constant across states. Standard errors are block-bootstrapped at the state level with 200 replications. The coefficients can be interpreted as a change in the prevalence rate of diagnosis c when the legislation was tightened.

Figure 6: Effects on Health Outcomes



(a) COPD (b) Lung cancer

Notes: This figure presents point estimates and symmetric percentile-t confidence intervals of the causal effect of the national regulation on the prevalence rate of COPD (left panel) and lung cancer (right panel). The omitted year corresponds to 2011, the year the law was passed. Standard errors are block-bootstrapped at the state level with 200 replications.

The left (right) panel in figure 6 shows the estimated effect on COPD (lung cancer) prevalence rate for the population aged 18 to 65—Table A10 presents point estimates and standard errors. I estimate that by 2014, the prevalence of COPD decreased about 3.6 percentage points relative to 2011, while respiratory system cancer decreased by about 2.2 percentage points. However, the latter result is not statistically different from zero. The effect for each year after the national law was passed is greater in absolute value than the previous one, which suggests that the policy could have important long-term effects.[20] The results on health outcomes could be driven by fewer individuals being diagnosed with these diseases; the same number of individuals being diagnosed with less severe symptoms and requiring fewer hospitalizations, or a combination of both. Although I can not shed light on which channel is the most important, my results suggest an improvement in population health correlated with stricter regulation of tobacco products.

[20]Unfortunately, the Argentinean National Center for Health Statistics stopped collecting hospital discharge data after 2014, so conducting a longer term analysis is not feasible.

My results are consistent with the fact that many health conditions associated with smoking—such as, heart disease, lung cancer, and overall premature mortality—are processes that develop over long periods of time making it difficult to know the appropriate lag between policy implementation and disease onset and progression (DeCicca, Kenkel and Lovenheim, 2020). Nonetheless, in the case of chronic obstructive pulmonary disease (COPD), it has been shown that smoking cessation and avoidance of cigarette smoke reduce the excess lung function decline (Eklund et al., 2012). Thus, the reduction on hospitalization from COPD could reflect the health gains from smoking cessation and reduced exposure to cigarette smoke induce by the law. In contrast, the null effects on cancer diagnoses reflect the fact that reverting the onset of this disease requires a longer time horizon.

VI Bans or Graphic Tobacco Warnings?

The implementation of Argentina's 2011 policy features a common characteristic across countries: The government implemented two anti-tobacco policies at the same time, i.e., the clean-indoor-air policy and the mandate for graphic tobacco warnings. In particular, graphic warning labels have often been enacted as part of broader anti-smoking campaigns (DeCicca, Kenkel and Lovenheim, 2020). One example is the 2001 Canadian Federal Tobacco Control Strategy, which proposed raising tobacco taxes in addition to the incorporation of graphic tobacco warnings (Health Canada, 2002).[21] Another example of joint implementation of policies is the Taiwan Tobacco Hazards Prevention Amendment Act of 2009, which extended smoke-free areas to almost all enclosed workplaces and public places, added graphic health warnings to cigarette packages, and banned tobacco advertisement (see, Chang et al., 2010).

The joint-implementation setting poses two empirical challenges to the identification of policy effects. The first challenge is that researchers cannot exploit either regional or temporal variation to disentangle the effects of each policy because these are implemented at the same time. The second challenge is the presence of spillover effects between policies. If these are nonzero, the estimated effect of the policy bundle would be a function of the effects of each policy and the spillover effects. To overcome these identification challenges, I exploit an additional source of variation in my data and impose structure on how the two policies interact. This exercise helps in understanding what mechanisms are

[21] Beginning in April 2001, the federal government implemented a sequence of tax hikes. The excise tax was first raised to $10.99 per carton in May 2001, and then to $12.62 by the end of 2001. In mid-2002, the federal tax was further raised to $13.86 per carton and then to $15.85 in July 2002 (Gabler and Katz, 2010).

driving the extensive margin outcomes.

Effects of place-based bans. I exploit the fact that, while tobacco graphic warnings were implemented nationally in 2011, place-based bans were adopted by some states before 2011. Thus, the comparison between states that have implemented place-based bans before the national implementation—bans early adopters—and the states that have not implemented bans until the national law—bans late adopters—provides useful variation to identify the effect of place-based bans.[22] Under the assumption that conditional on time and fixed effects the two groups of states would have followed the same trend absence the 2011's federal policy; the following regression provides a causal estimate of δ^{bans}:

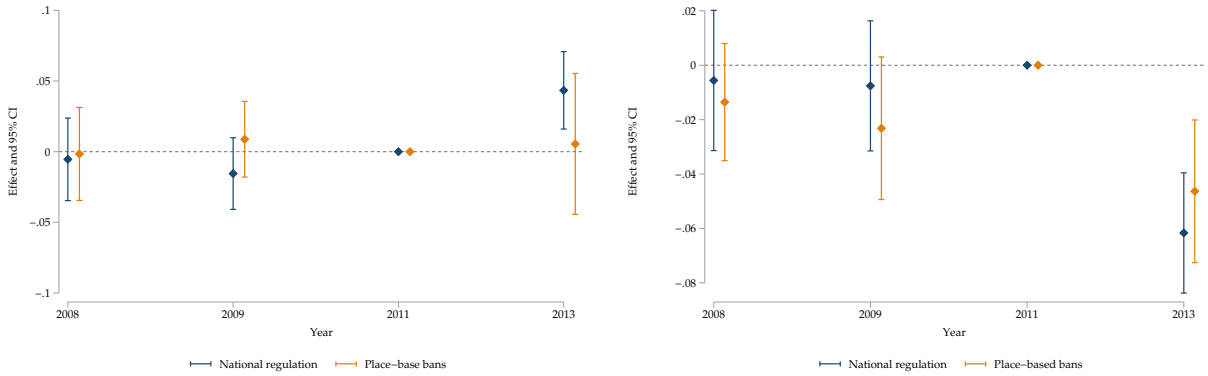
$$y_i = \sum_{\tau=-2}^{1} \delta_{\tau}^{bans} \left[Bans_s \cdot (Years\ after\ treat = \tau) \right] + \beta' X_{is} + \Gamma' X_{st} + \alpha_s + \alpha_t + \varphi_i, \quad (3)$$

where $Bans_s$ is a dummy variable that equals 1 if the state s is a late-adopter of place-based bans; and the rest of the parameters are defined as in equation 1. Figure 7 shows the results of this estimation and replicates the main results for comparison.[23] I find no effect of place-based bans on the proportion of non-smokers, which highlights the fact that non-smokers do not experience the cost of place-based bans—i.e., non-smokers are not compelled to go outside to smoke. On the other hand, roughly 40% of the change in the proportion of current smokers can be attributed to place-based bans, suggesting that the reduction in the probability of being a current smoker is driven by both components of the policy.

[22] Importantly, the group of states that adopted bans before the national implementation is not the same as those states classified as strict states. These two groups differ because the legislation index has multiple dimensions. Thus, a strict state could have ban cigarette sales, advertising, and consumption in schools and other venues but not in restaurants. This state will be a control state in the regression that estimates δ but a treated state in the regression that estimates $\tilde{\delta}^{bans}$.

[23] Appendix Table A11 presents point estimates and standard errors.

Figure 7: Effects of Place-based Bans



(a) Prob. never smokers

(b) Prob. current smokers

Notes: This figure presents point estimates and symmetric percentile-t confidence intervals of the causal effect of the national regulation and place-base bans on the probability of being a never smoker (left panel) and the probability of being a current smoker (right panel). The omitted year corresponds to 2011, the year the law was passed. Standard errors are block-bootstrapped at the state level with 200 replications.

Effects of graphic tobacco warnings. To construct estimates of the effects of tobacco graphic warnings, I make two additional assumptions regarding the effects of the policy intervention (i) I assume that treatment effects are constant across states and time, and (ii) I assume that the effects of each policy branch are additive. Under these assumptions, the total effect of the policy (δ) can be written as a linear combination of the effects of each branch of the policy: tobacco graphic warnings (δ^{tgw}) and place-based bans (δ^{bans}):[24]

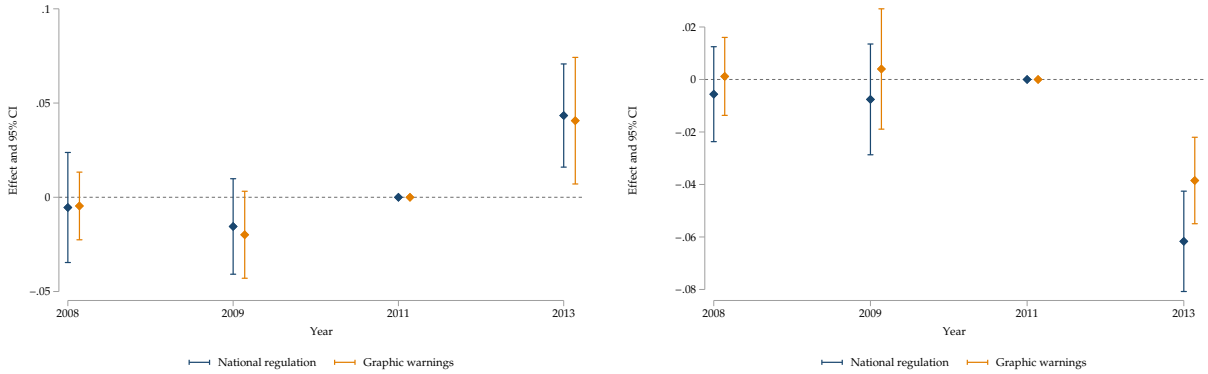
$$\delta = \delta^{tgw} + \omega\, \delta^{bans}\,, \tag{4}$$

where ω is the proportion of states that have not implemented a place-based ban before the national regulation, and δ^{bans} is the effect of place-based bans, and estimates of these effects can be used to construct estimates of the effect of graphic tobacco warnings. The left panel of Figure 8 shows that graphic tobacco warnings are an effective policy instrument to deter smoking initiation. I find that most of the effect on the probability of being a never-smoker is explained by the introduction of these warnings. The rationale behind pictorial warnings on tobacco products is to inform smokers and non-smokers about the documented health risks of smoking. When non-smokers were asked about graphic warnings, 73% reported having seen them in the last 30 days. Studies in experimental settings have documented that graphic warnings elicit emotional responses, such as fear or disgust (DeCicca, Kenkel and Lovenheim, 2020). My results indicate that these

[24]See Appendix D. for a detailed derivation of this equation.

emotional responses may be an effective tool to keep non-smokers away from cigarettes. For current smokers, around 60% of the effect can be attributed to the incorporation of tobacco graphic warnings (Figure 8, right panel). I estimate that graphic warnings reduce the probability of being a current smoker by 3.85 percentage points, these results are in line with Kuehnle (2019) who finds that the introduction of pictorial warnings on cigarette packages reduced smoking by around 4% within the first year of the policy in Australia.

Figure 8: Effects of Graphic Tobacco Warnings



(a) Prob. never smokers

(b) Prob. current smokers

Notes: This figure presents point estimates and symmetric percentile-t confidence intervals of the causal effect of the national regulation and tobacco graphic warnings on the probability of being a never smoker (left panel) and the probability of being a current smoker (right panel). The omitted year corresponds to 2011, the year the law was passed. Standard errors are block-bootstrapped at the state level with 200 replications.

Interpretations. The previous exercise helps in understanding what mechanisms are driving the extensive margin outcomes; nonetheless, some caveats are in place. First, spillover effects may arise from complementarities in policy intervention and these effects can not be disentangled from the effect of place-based bans. If these spillover effects are positive—for example, because the effectiveness of place-based bans is enhanced by the implementation of the advertising restriction as the latter helps to raise awareness of the side effects of smoking—the estimates of δ^{bans} would provide an upper-bound estimate of the true effect. Second, the derivation of Equation 4 relies on the assumption that effects are additive. If this where not the case, this expression would still provide a useful decomposition of the total effect but we would need to assume that interaction terms are small.

VII Conclusions and Discussion

Smoking bans and advertising regulations have played an increasingly important role in tobacco control policies over the past 20 years. These policies are particularly attractive in middle- and low-income countries, where tobacco use is concentrated among low-income households. This paper provides evidence of the effectiveness and potential limitations of these policies by studying the Argentinean case. Argentina's 2011 national law has two main components: (i) it implements indoor smoking bans and (ii) it regulates advertising of tobacco products and mandates the incorporation of graphic tobacco warnings. I exploit regional variation in the leniency of tobacco regulation before 2011 to identify the effects of this new regulation on various outcomes. I find that the new regulations effectively curbed smoking initiation and consumption. Nonetheless, when looking at heterogeneous effects, my results suggest that the policy disproportionately benefited more educated and richer individuals, and thus potentially widens health disparities between these groups. This paper also highlights the incorporation of graphic tobacco warnings as a valuable policy instrument. Well-designed package warnings are a highly cost-effective means for increasing awareness of the smoking's effects on health and deterring individuals from smoking.

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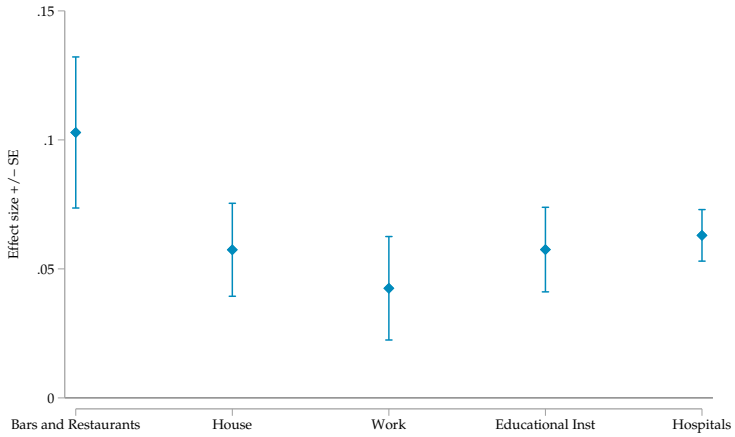
A Appendix

A. Additional Figures and Tables

Figure A.1: Examples of law-regulated packaging.

Notes: This figure provides an example of the Tobacco Graphic Warnings printed on cigarette packages. Big font messages: (i) smoking reduces years of life, (ii) smoking causes cancer, (iii) smoking might cause leg amputation, (iv) pregnant women who smoke harm her child (v) smoking causes death by suffocation. Small font messages: (i) tobacco drives half of smoker's deaths, (ii) every cigarette poisons you, (iii) smoking causes gangrene, (iv) every cigarette damages your respiratory capacity.

Figure A.2: Exposure to Environmental Tobacco Smoke



Notes: This figure presents estimates of the change in the share of non-smokers exposed to environmental tobacco smoke after the implementation of the national law. Each coefficient corresponds to a separate regression of the share of individuals reporting to not have noticed someone smoking inside a given venue. All coefficients are expressed as effect sizes to ease the comparison of the effects across venues.

Table A1: Evolution of Cigarette Sales and Consumption

	Sales data		Survey data	
Year	Consumption	Percent change	Consumption	Percent change
2008	181.06		106.75	
2009$^{(a)}$	177.64	-1.89%	103.42	-3.11%
2010	174.86			
2011$^{(b)}$	182.65	1.40%	105.84	1.16%
2012	178.36			
2013$^{(b)}$	174.07	-2.38%	99.39	-3.09%

Notes: Consumption measures are expressed in millions of 20 cigarettes packages a month. Sales data is collected by the Ministry of Agriculture, time series are expressed in 20 cigarettes packages in a year. Consumption from survey data is constructed as the average consumption per day multiplied by 30. Summary statistics indicates that 62% of smokers smoke every day, this is the modal frequency. Percent changes are: (a) relative to the previous calendar year; (b) relative to the $n-2$ year.

Table A2: Determinants of Strong Regulation prior to 2011

Dependent variable:	Indicator for Enacting a Strong Regulation	
	Coefficient	Marginal Effect
Peronist party (=1)	0.390	0.133
	(0.266)	(0.090)
State is tobacco producer (=1)	0.598	0.204
	(0.372)	(0.128)
Ln employment	-0.172	-0.058
	(0.516)	(0.177)
Unemployment rate	4.769	1.626
	(5.197)	(1.783)
Proportion of smokers	-0.327	-0.112
	(3.060)	(1.044)
Prevalence of COPD	1.156***	0.394***
	(0.378)	(0.130)
Share population 0 - 14 years old	0.066	0.022
	(16.370)	(5.584)
Share population 15 - 24 years old	48.93***	16.687***
	(17.580)	(5.832)
Share population 25 - 44 years old	20.400	6.958
	(16.510)	(5.682)
Share population 45 - 64 years old	44.210	15.077
	(30.110)	(10.360)
Ln population	0.542	0.185
	(0.529)	(0.183)
Observations	144	144
Pseudo R^2	0.2196	

Notes: This table presents the results of a probit model estimating the determinants of enacting a strong regulation before 2011. The second column presents estimated coefficients from the probit model and the last column presents the corresponding marginal effects computed at the means. The Peronist party was responsible of enacting the 2011 National Law; thus, the coefficient of this dummy tests whether the Peronist party is more likely to regulate tobacco consumption at the state level than the opposition parties. States that are considered tobacco producers are Jujuy (36% of the country's tobacco production), Misiones (29%), Salta (25%), Tucuman (7%), Catamarca (1%), Corrientes (1%) and Chaco (1%). Standard errors are robust to heteroskedasticity. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3: Effects on Extensive Margin Outcomes

Probability of	Never smokers	Current smokers
	(1)	(2)
2008	-0.0054	-0.0056
	(0.0147)	(0.0104)
2009	-0.0155	-0.0076
	(0.0162)	(0.0087)
2013	0.0434**	-0.0617***
	(0.0175)	(0.0103)
Mean dep. var. in 2011	0.4608	0.2825
Observations	153,093	153,093
R-squared	0.0329	0.0259
Correctly predicted	0.5536	0.6881
Individual controls	Yes	Yes
State $\times$ time controls	Yes	Yes
State FE	Yes	Yes
Time FE	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment is a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. Individual-level controls include age, gender, educational attainment, employment status and income category of the household. State $\times$ time controls include total private employment and total population. Standard errors are block-bootstrapped at the state-level with 200 replications.

Table A4: Probability of being a current smoker by age group

Age:	18-25	25-30	30-35	35-40	40-45	45-50	50-55	55-60	60-65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2008	0.0009	-0.0252	-0.0363	-0.0212	0.0125	-0.0253	0.0074	0.0177	0.0365
	(0.0293)	(0.0267)	(0.0235)	(0.0319)	(0.0303)	(0.0267)	(0.0257)	(0.0179)	(0.0201)
2009	0.0038	0.0089	-0.0209	-0.0363	-0.0216	0.0163	-0.0227	-0.0038	-0.0088
	(0.0157)	(0.0225)	(0.0212)	(0.0224)	(0.0166)	(0.0249)	(0.0302)	(0.0201)	(0.0158)
2013	-0.1014***	-0.0379	-0.0148	-0.0434	-0.0983***	-0.1207***	-0.1191***	0.0152	-0.0156
	(0.0307)	(0.0251)	(0.0200)	(0.0217)	(0.0163)	(0.0250)	(0.0275)	(0.212)	(0.0187)
Mean dep. var. in 2011	0.27	0.31	0.29	0.29	0.26	0.30	0.32	0.27	0.23
Observations	24,822	19,409	20,329	18,804	16,121	14,205	13,576	11,992	13,835
R-squared	0.0497	0.0278	0.0377	0.0341	0.0348	0.0247	0.0253	0.0232	0.0240
Correctly predicted	0.6899	0.6604	0.6564	0.7059	0.5085	0.6758	0.6591	0.7331	0.8005
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ time controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. Individual-level controls include gender, educational attainment, employment status and income category of the household. State $\times$ time controls include total private employment and total population. Standard errors are block-bootstrapped at the state-level with 200 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A5: Probability of being a current smoker by age group and gender

Age:	18-25	25-30	30-35	35-40	40-45	45-50	50-55	55-60	60-65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2008									
Female	-0.0448	-0.0507	-0.0252	-0.0123	-0.0247	-0.0055	-0.0322	-0.0174	0.0612
	(0.0338)	(0.0296)	(0.0212)	(0.0455)	(0.0375)	(0.0341)	(0.0433)	(0.0359)	(0.0356)
Male	0.0485	0.0146	-0.0442	-0.0204	0.0560	-0.0446	0.0423	0.0510	0.0035
	(0.0359)	(0.0316)	(0.0406)	(0.0341)	(0.04145)	(0.0499)	(0.0671)	(0.0342)	(0.0379)
2009									
Female	0.0142	-0.0257	-0.0111	-0.0047	-0.0068	-0.0330	-0.0276	-0.0362	0.0153
	(0.0257)	(0.03212)	(0.0367)	(0.225)	(0.0218)	(0.0359)	(0.0414)	(0.0294)	(0.0266)
Male	-0.0056	0.0422	-0.0312	-0.0669	-0.0338	0.0680	-0.0238	0.0220	-0.0391
	(0.0247)	(0.0329)	(0.0315)	(0.0363)	(0.0344)	(0.0306)	(0.0327)	(0.0274)	(0.0272)
2013									
Female	-0.1150	-0.0253	-0.0036	-0.0549	-0.1125	-0.1115	-0.1044	0.0413	0.0096
	(0.0252)***	(0.0381)	(0.0386)	(0.0266)	(0.0259)***	(0.0308)***	(0.0401)***	(0.0343)	(0.0283)
Male	-0.0891	-0.0599	-0.0271	-0.0261	-0.0859	-0.1292	-0.1318	-0.0215	-0.0508
	(0.0510)***	(0.0384)	(0.0342)	(0.0282)	(0.0324)***	(0.0473)***	(0.0304)***	(0.0437)	(0.0348)
$H_0 : \delta_{FEMALE} = \delta_{MALE}$	.432	.1235	.6888	.9247	.5955	.9301	.9443	.8385	.3941
Mean dep. var. in 2011									
Female	0.27	0.31	0.29	0.29	0.26	0.30	0.32	0.27	0.23
Male	0.33	0.36	0.36	0.35	0.32	0.35	0.37	0.30	0.25
Observations									
Female	13,107	10,700	11,360	10,451	8,859	7,666	7,464	6,697	8,193
Male	11,715	8,709	8,969	8,353	7,262	6,539	6,112	5,295	5,642
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ time controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. Individual-level controls include educational attainment, employment status and income category of the household. State $\times$ time controls include total private employment and total population. Standard errors are block-bootstrapped at the state-level with 200 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A6: Probability of being a current smoker by educational attainment

Educational level:	Elementary d/o	Elementary	HS d/o	HS	College d/o	College +
	(1)	(2)	(3)	(4)	(5)	(6)
2008	-0.0312	-0.0310	0.0030	0.0336	0.0036	-0.0093
	(0.4700)	(0.0175)	(0.0272)	(0.0212)	(0.0260)	(0.0238)
2009	-0.0630***	0.0093	-0.0177	-0.0141	0.0049	0.0167
	(0.0210)	(0.0170)	(0.0139)	(0.0179)	(0.0220)	(0.0223)
2013	-0.0080	-0.0437*	-0.0599***	-0.0448***	-0.0989***	-0.0986***
	(0.0361)	(0.0185)	(0.0216)	(0.0303)	(0.0296)	(0.0193)
Mean dep. var. in 2011	0.31	0.30	0.35	0.28	0.24	0.228
Observations	12,261	30,388	27,877	35,538	21,195	23,811
R-squared	0.0713	0.0434	0.0258	0.0217	0.0199	0.0134
Correctly predicted	0.5073	0.6906	0.6370	0.6173	0.7181	0.7607
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ time controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. Individual-level controls include age, gender, educational attainment, employment status and income category of the household. State $\times$ time controls include total private employment and total population. HS stands for High School and d/o stands for drop-out. Standard errors are block-bootstrapped at the state-level with 200 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A7: Probability of being a current smoker by income

Income category	1^{st} quintile (1)	2^{nd} quintile (2)	3^{rd} quintile (3)	4^{th} quintile (4)	5^{th} quintile (5)
2008	-0.0239	-0.0288	-0.0080	0.0282	-0.0028
	(0.0246)	(0.0241)	(0.0201)	(0.0195)	(0.0318)
2009	-0.0433	-0.0513**	0.0297	0.0093	-0.0213
	(0.0211)	(0.0216)	(0.0200)	(0.0146)	(0.0404)
2013	-0.0561***	-0.0313*	-0.0534***	-0.0919***	-0.0892***
	(0.0135)	(0.0187)	(0.0137)	(0.0183)	(0.0207)
Mean dep. var. in 2011	0.29	0.29	0.31	0.28	0.25
Observations	23,427	33,418	32,291	37,006	16,507
R-squared	0.0530	0.0291	0.0248	0.0231	0.0217
Correctly predicted	0.6846	0.7049	0.7032	0.7057	0.7023
Individual controls	Yes	Yes	Yes	Yes	Yes
State $\times$ time controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Notes: The 1^{st} quintile corresponds to the lowest 20 percent households in the income distribution. Analogously, the 5^{th} quitile corresponds to the highest 20 percent households in the income distribution. The omitted category corresponds to 2011, the year the federal law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. Individual-level controls include age, gender, educational attainment, and employment status. State $\times$ time controls include total private employment and total population. Standard errors are block-bootstrapped at the state-level with 200 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A8: Probability of risky alcohol consumption - Estimates from the Probit model

Dependent variable	Alcohol consumption	Beer abuse	Wine abuse	Spirits	Binge drinking
	(1)	(2)	(3)	(4)	(5)
2008	-0.0845	-0.0085	0.0132	-0.2020	0.0534
	(0.0591)	(0.1297)	(0.1108)	(0.1645)	(0.1109)
2009	0.0091	0.0342	-0.0807	-0.1013	-0.0277
	(0.0741)	(0.0985)	(0.0901)	(0.1329)	(0.0816)
2013	0.0288	-0.2256**	-0.1527**	0.6968***	-0.1526**
	(0.0355)	(0.1079)	(0.0556)	(0.1553)	(0.0730)
Marginal Effects (at means)					
2008	-0.0268	-0.0011	0.0021	-0.0171	0.0179
	(0.0187)	(0.0162)	(0.0174)	(0.0139)	(0.0373)
2009	0.0029	0.0043	-0.0127	-0.0086	-0.0093
	(0.0235)	(0.0123)	(0.0142)	(0.0111)	(0.0274)
2013	0.0091	-0.0282**	-0.0241**	0.0589***	-0.0512**
	(0.0113)	(0.0133)	(0.0089)	(0.0117)	(0.0246)
Mean dep. var. in 2011	0.7232	0.1095	0.1291	0.0318	0.3605
Observations	29,391	21,561	21,561	21,561	21,561
Pseudo R-squared	0.0534	0.1768	0.1035	0.1613	0.1054
Correctly predicted	0.9816	0.8605	0.8663	0.9258	0.6909
Individual controls	Yes	Yes	Yes	Yes	Yes
State $\times$ time controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. The sample is restricted to current smokers who have reported drinking alcohol in the last month. Individual controls include age, gender, educational attainment, employment status and income category of the household. State $\times$ time controls include total private employment and total population. Standard errors in parentheses are clustered at the state-level.

Table A9: Demographic Characteristics of Smokers by Alcohol Beverages

	Abusive consumption			Binge drinking
	Beer	Wine	Spirits	
Female	0.14	0.13	0.30	0.19
	(0.01)	(0.01)	(0.04)	(0.01)
Young (<25 years old)	0.45	0.29	0.61	0.35
	(0.02)	(0.02)	(0.04)	(0.01)
Less than high school	0.53	0.61	0.44	0.53
	(0.02)	(0.02)	(0.04)	(0.01)
Single	0.54	0.36	0.74	0.48
	(0.02)	(0.02)	(0.03)	(0.01)
High income hh	0.06	0.03	0.09	0.07
	(0.01)	(0.01)	(0.02)	(0.01)
Population share	0.12	0.13	0.03	0.38

*Notes:*This table presents demographic characteristics of smokers who consumed alcohol in the last month. Abusive consumption thresholds are specific to the alcohol beverage, see the main text for details. Binge drinking is defined as consuming 5 or more drinks during a single occasion in the last 30 days, either during the weekend or during a week day.

Table A10: Health Outcomes

Diagnosis	COPD	Lung cancer
	(1)	(2)
2008	-0.00085	0.00317
	(0.01393)	(0.00658)
2009	-0.00492	-0.00031
	(0.01240)	(0.00518)
2010	-0.01130	0.00309
	(0.01043)	(0.00641)
2012	-0.01670	-0.01140
	(0.01955)	(0.01019)
2013	-0.031**	-0.0166
	(0.01810)	(0.01387)
2014	-0.0361*	-0.0255
	(0.02235)	(0.01704)
Mean dep. var.	0.1061	0.0644
Observations	1,512	1,512
R-squared	0.447	0.539
Time varying controls	Yes	Yes
State FE	Yes	Yes
Linear trend	Yes	Yes

Notes: This table presents estimated effects on health outcomes. The omitted category corresponds to 2011, the year the national law was passed. Standard errors are block-bootstrapped at the state-level with 200 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table A11: Mechanisms

Policy	National regulation (δ)		Place-based bans (δ^{bans})		Graphic Warnings (δ^{tgw})	
Probability of	Never smokers	Current smokers	Never smokers	Current smokers	Never smokers	Current smokers
	(1)	(2)	(3)	(4)	(5)	(6)
2008	-0.0054	-0.0056	-0.00162	-0.0135	-0.00461	0.0012
	(0.0147)	(0.0104)	(0.0149)	(0.0115)	(0.0103)	(0.0083)
2009	-0.0155	-0.0076	0.00879	-0.02318*	-0.01988*	0.0040
	(0.0162)	(0.0087)	(0.0168)	(0.0087)	(0.0164)	(0.0092)
2013	0.0434**	-0.0617***	0.00546	-0.04635***	0.04067*	-0.038525***
	(0.0175)	(0.0103)	(0.0020)	(0.0080)	(0.0207)	(0.0102)
Observations	153,093	153,093	153,093	153,093	153,093	153,093
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
State x time controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents point estimates of the effects of the federal law, the effects of place-based bans, and the incorporation of graphic tobacco warnings following the decomposition proposed in equation (4). The first column reproduces results presented in Figure 2. Standard errors are block-bootstrapped at the state level with 200 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

B. Construction of Legislation Index

Argentina has 23 states and one federal district.[25] Each state has the autonomy to dictate laws in their territory as long as these laws are not contrary to the federal laws. Broadly speaking, taxes on tobacco-related products are implemented at the national level but non-price policies have been implemented at the regional level before the 26,687 law was passed.

I coded a total of 47 regional laws and two national laws. The three main categories that composed the index are tobacco advertising, sales, and consumption. Each category is further divided into sub-categories, to allow for a better understanding of the regulation and to be able to assess the importance of each particular aspect of the law. The subcategories were defined following González-Rozada (2006) report on the status of tobacco legislation in Argentina.

The advertising category includes the sub-categories: publicity, advertising in radio, in television, to a certain audience, regulation on the content of advertising, events sponsoring, brand stretching, inclusion and size of tobacco graphic warnings, and inclusion of contact information about anti-smoking public services. Sales category—defined as bans on sales—includes the next sub-categories: sales to under 18 years old individuals, elementary school, high school, education institutions in general, hospitals or health institutions, government buildings, public transportation means, and sales by the unit. Finally, the consumption category includes the sub-categories: government buildings, workplaces, health institutions, elementary schools, high schools, universities, public transportation means, restaurants, bars, entertainment centers.

To construct the legislation index I summarize the previous categories with a dummy variable. The advertising category is summarized by a dummy that takes the value of 1 if the state has passed some regulation regarding advertising. The sales category is restricted to bans in sales to underage individuals. Consumption sub-categories are grouped regarding similarities of the environments: public means of transportation, educational institutions, health institutions, and restaurants, bars, and other entertainment places together. Thus, the maximum value the index can take before the implementation of the national law is 6. I define strict states as those with an index strictly greater than 3.

[25] I refer to this administrative unit as another state since the distinction between state and federal district is not relevant for this paper.

C. Robustness Checks

C.1. Alternative Definition of Comparison and Treated States

My identification strategy relies on state-level differences in the strength of regulations on tobacco products before the national law was passed. I define the treatment as a dichotomous variable, $Treat_s$, that equals 1 if the legislation index for state s is less than or equal to 3 before 2011. In this section, I present the main results of my analysis using an alternative definition. I define the treatment as a dichotomous variable that equals 1 if the legislation index for state s is strictly less than 3 before 2011.

Table A12 presents balancing test results. As in the main analysis, estimated differences indicate that individuals are statistically indistinguishable in terms of gender, age, marital status, employment status, and educational attainment. I find evidence that on average individuals in comparison states are poorer than individuals in treated states.

I reproduce results for the extensive margin in Table A13. The estimates of the coefficients for 2008 and 2009 are not statistically distinguishable from zero, meaning that the comparison and treated states were on similar trends regarding cigarette consumption before the national law was passed. I find that results on the probability of being a never smoker and the probability of being a current smoker are smaller in absolute value and still significantly different from zero under the more strict definition of treatment.

I present results for the intensive margin in Table A14. The estimated coefficients are the percentage change in the probability of observing average daily consumption in bin b. I find evidence that the distribution of daily cigarettes smoked shifts to the right, i.e. it is more likely that smokers consume more than 15 cigarettes per day and less likely that they consume fewer than 15 cigarettes per day. The proportion of smokers who consume between zero and five cigarettes per day decreases by 23 percentage points. This estimate is slightly greater than the one found with the original treatment definition.

Table A12: Pre Policy Balance Individual Characteristics

	Control	Treatment	Difference
Average age	37.66	37.71	-0.05
	(13.47)	(13.31)	(0.42)
Male	0.49	0.49	0.01
	(0.50)	(0.50)	(0.02)
Young (< 25 years old)	0.24	0.23	0.01
	(0.43)	(0.42)	(0.01)
Married or cohabitant	0.57	0.59	-0.01
	(0.49)	(0.49)	(0.02)
Employed	0.70	0.70	0.00
	(0.46)	(0.46)	(0.02)
Educational level			
Elementary school drop out	0.08	0.08	0.01
	(0.27)	(0.26)	(0.01)
Elementary school	0.20	0.19	0.01
	(0.40)	(0.39)	(0.01)
High school drop out	0.19	0.20	-0.01
	(0.39)	(0.40)	(0.01)
High school	0.23	0.24	-0.01
	(0.42)	(0.43)	(0.02)
College drop out	0.14	0.13	0.00
	(0.34)	(0.34)	(0.01)
College	0.14	0.14	0
	(0.35)	(0.34)	(0.01)
Income category			
First quintile	0.15	0.13	0.02***
	(0.34)	(0.32)	(0.01)
Second quintile	0.35	0.23	0.12***
	(0.47)	(0.41)	(0.01)
Third quintile	0.20	0.23	-0.03***
	(0.40)	(0.42)	(0.01)
Forth quintile	0.17	0.27	-0.09***
	(0.38)	(0.43)	(0.01)
Fifth quintile	0.13	0.14	-0.01
	(0.32)	(0.32)	(0.01)
Observations	27,348	33,101	60,449
Number of states	15	9	24

Notes: This table presents the mean and standard deviation of individual characteristics for comparison and treated states in columns 1 and 2. Column 3 presents estimated coefficients and standard errors from an OLS regression of the mean difference. Estimates use pooled data from national surveys on tobacco use for 2008 and 2009. The lowest quintile corresponds to the lowest 20% of households in the income distribution. Analogously, the highest quintile corresponds to the highest 20% of households in the income distribution.

Table A13: Effects on Extensive Margin Outcomes

Probability of	Never smokers	Current smokers
	(1)	(2)
2008	-0.0205	-0.0055
	(0.0149)	(0.0120)
2009	-0.0238	-0.0138
	(0.0165)	(0.0099)
2013	0.0402**	-0.0594***
	(0.0187)	(0.0100)
Mean dep. var. in 2011	0.4608	0.2825
Observations	153,093	153,093
R-squared	0.0328	0.0259
Correctly predicted	0.5536	0.6881
Individual controls	Yes	Yes
State $\times$ time controls	Yes	Yes
State FE	Yes	Yes
Time FE	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is strictly less than 3 before 2011. After 2011, all states *are treated* thus $Treat_s$ equals one for all states s after 2011. Individual controls include age, gender, educational attainment, employment status and income category of the household. State $\times$ time controls include total private employment and total population. Standard errors are block-bootstrapped at the state-level with 200 replications.

Table A14: Effects on Intensive Margin Outcomes

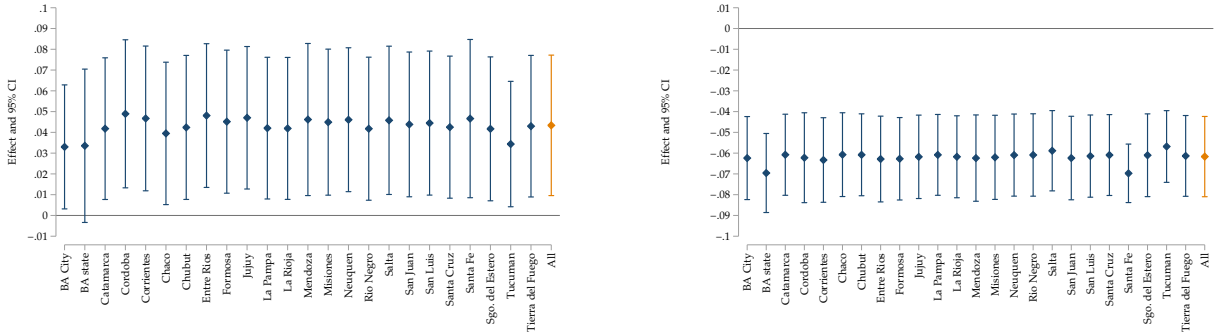
Cigarettes smoked	(0,5]	(5, 10]	(10, 15]	(15, 20]	(20, 30]	(30 , 40]	(40 , 50]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2008	0.0017	0.0237	-0.0033	0.0084	0.0051	-0.0100	0.0010
	(0.0317)	(0.0185)	(0.0104)	(0.0123)	(0.0067)	(0.0062)	(0.0011)
2009	-0.0839**	0.0394*	0.0020	-0.0182	-0.0111***	-0.0097*	0.0008
	(0.0370)	(0.0201)	(0.0056)	(0.0150)	(0.0035)	(0.0054)	(0.0014)
2013	-0.2335***	-0.0224	-0.0147**	0.0110	0.0073	-0.0073**	-0.0003
	(0.0233)	(0.0159)	(0.0069)	(0.0144)	(0.0045)	(0.0034)	(0.0007)
Mean dep. var. in 2011	0.3397	0.2628	0.0937	0.2080	0.0374	0.0265	0.0024
Observations	45,585	45,585	45,585	45,585	45,585	45,585	45,585
R-squared	0.0797	0.0114	0.0069	0.0257	0.0108	0.0170	0.0014
Correctly predicted	0.8013	0.7775	0.7333	0.7634	0.7211	0.7182	0.7123
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ time controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:*The omitted category corresponds to 2011, the year the federal law was passed. Treatment is a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is strictly less than 3 before 2011. After 2011, all states *are treated* thus $Treat_s$ equals one for all states s after 2011 Individual controls include age, gender, educational attainment, employment status and income category of the household. State $\times$ time controls include total private employment and total population. Standard errors are block-bootstrapped at the state-level with 200 replications.

C.2 Removing One State at a Time

Are the effects of the policy driven by one particular state? I address this question by performing a simple exercise: I estimate the effect on extensive margin outcomes in a sub-sample of states where I exclude one state at a time. I present the results of this exercise in Figure A.3, the category "All" replicates the effects discussed in section **??**. This figure presents estimates of the marginal effect and confidence intervals of the 2011 national law on the probability of never smokers (panel a) and current smokers (panel b). I find evidence that point estimates are robust to the exclusion of one state from the sample.

Figure A.3: Extensive Margin Outcomes



(a) Never Smokers

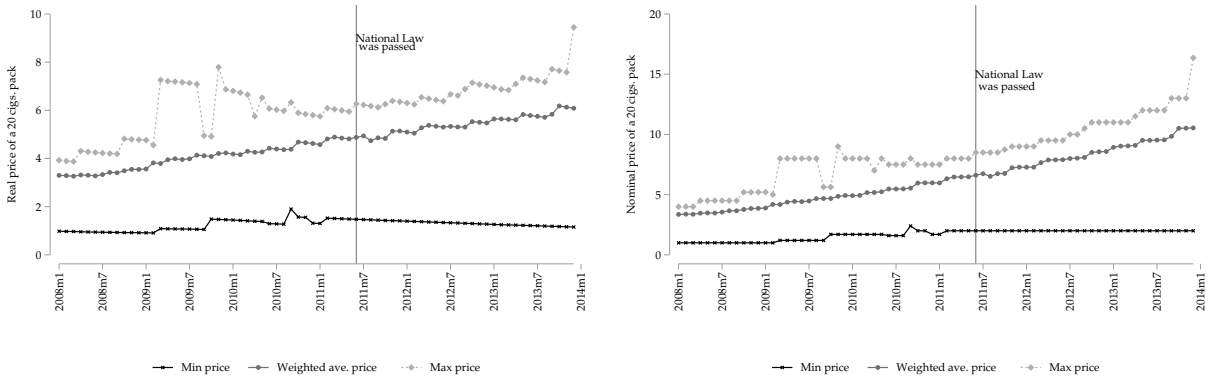
(b) Current Smokers

*Notes:*This figure presents estimates of the marginal effect and confidence intervals of the 2011 national law on the probability never smokers (panel a) and never smokers (panel b). Treatment is a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. All regressions include individual-level controls: age, gender, educational attainment, employment status, and income category of the household. The category "All" refers to the sample that includes all the states, see table **??**. Standard errors are clustered at the state-level.

C.3 Prices and Industry

Did the 2011 policy change prices of cigarettes? I use data on prices to address this question. The Ministry of Agriculture follows the sales of cigarettes at the national level and provides information on sales by the price paid by the consumer. I focus on three price ranges: the minimum price paid in the cigarette market, the average price paid and the highest price paid. Data are at the price-range month level so I can closely follow the evolution around the dates of the change in the regulation. Figure A.4 presents time series of real and nominal prices. I use the price index constructed by Cavallo (2013) to construct the real price time series.[26] There is no evidence of sharp changes in the price of the cigarette package. I pay closer attention to the minimum price because smokers could *easily* substitute among brands to avoid price changes in the more expensive brands.

Figure A.4: Cigarette Prices - Time



Notes: This figure presents time series of real and nominal prices paid by consumers split into three price ranges: the minimum price, the average price, and the highest price. I use the price index constructed by Cavallo, 2013) to deflate prices. Data is at the price-monthly level and comes from price series constructed by the Ministry of Agriculture.

[26]During the period under study, the Argentinean economy experienced high inflation. The lack of reliability on official estimates of the Consumer Price Index challenges the use of the official Consumer Price Index series, for a discussion see, Cavallo (2013).

D. **Derivations: Bans or Tobacco Graphic Warnings?**

The goal of this section is to discuss a framework that allows the identification of the effect of each policy branch: tobacco graphic warnings and place-based bans. The exogenous variation induced by the 2011's anti-tobacco law alone does not allow me to separately identify the effect of each policy branch. Thus, to disentangle the effects, I exploit an additional source of variation and put some structure on how each policy interacts.

Let y_{0i} be the smoking status of individual i in state s in absence of the federal policy and let y_{1i} be the smoking status of individual i in state s if the federal policy is implemented. I assume that the effect of the policy is constant across states (s) and time (t), so the conditional mean function $E\left[y_{1i}|s,t\right]$ can be written as: [27]

$$E\left[y_{1i}|s,t\right] = E\left[y_{0i}|s,t\right] + \delta \ , \tag{A.1}$$

where the parameter δ is the effect of the national regulation, i.e., the effects of the bundle policy. I argue that this effect is a linear combination of the effects of each branch of the policy: tobacco graphic warnings (δ^{tgw}) and place-based bans (δ^{bans}). While tobacco graphic warnings were implemented nationally in 2011, place-based bans were adopted by some states before 2011. Define S_1 as the subset of states that have implemented place-based bans before the national implementation—bans early adopters—and S_2 as the subset of states that have not implemented such bans until the national law—bans late adopters. Under the assumption that the effects of each policy branch are additive and allowing for the presence of spillover effects among the clean-indoor-air policy and the tobacco graphic warnings, the outcomes if the implementation happens are:

$$\begin{aligned}
&S_1 \quad \text{states:} \quad E\left[y_{1i}|s \in S_1,t\right] = E\left[y_{0i}|s \in S_1,t\right] + \delta^{tgw} \ , \\
&S_2 \quad \text{states:} \quad E\left[y_{1i}|s \in S_2,t\right] = E\left[y_{0i}|s \in S_2,t\right] + \delta^{bans} + \delta^{tgw} + f(\delta^{bans},\delta^{tgw}) \ ,
\end{aligned} \tag{A.2}$$

where $f(\delta^{TGW},\delta^{bans})$ is the spillover effect of implementing the policies jointly. Thus, from A.1 it follows that the effect of the bundle policy can be written as:

$$\begin{aligned}
\delta =& E\left[y_{1i}|s,t\right] - E\left[y_{0i}|s,t\right] \ , \\
\delta =& E\left[y_{1i}|s \in S_1,t\right] P(s \in S_1) + E\left[y_{1i}|s \in S_2,t\right] P(s \in S_2) - E\left[y_{0i}|s \in S_1,t\right] P(s \in S_1) - E\left[y_{0i}|s \in S_2,t\right] P(s \in S_2) \ .
\end{aligned}$$

[27] In the rest of the derivation I implicitly condition on a vector of covariates $\boldsymbol{X}$ that includes age, educational achievement, among other observable characteristics. I omit this conditioning to ease notation.

Let $P(s \in S_1) = \omega_1$ and $P(s \in S_2) = \omega_2$ and using the expressions in A.2:

$$\begin{aligned}\delta =& \omega_1 \left(E\left[y_{1i} | s \in S_1, t \right] - E\left[y_{0i} | s \in S_1, t \right] \right) + \omega_2 \left(E\left[y_{1i} | s \in S_2, t \right] P(s \in S_2) - E\left[y_{0i} | s \in S_2, t \right] \right) \ , \\ \delta =& \delta^{tgw} + \omega_2 \delta^{bans} + \omega_2 f(\delta^{bans}, \delta^{tgw}) \ . \end{aligned} \tag{A.3}$$

Where ω_2 can be estimated as the proportion of states that have not implemented a place-based ban before the national regulation, and ω_1 can be estimated as the proportion of states that have implemented a place-based ban before the national regulation. Expression A.3 shows that under the assumptions of (i) homogeneity of policy effects, and (ii) additive policy effects; the effect of the policy bundle (δ) is a linear combination of the effects of each branch of the policy. With this expression at hand, the next step is to ask whether each of these parameters are identified.

The effect of place-based bans is identified by the comparison of early adopters of place-based bans with late adopters. But, it can not be disentangled from the spillover effects. Put another way, we can identified and estimate the parameter $\tilde{\delta}^{bans} = \delta^{bans} + f(\delta^{bans}, \delta^{tgw})$. The effect of the national policy is identified by the comparison between lenient states and strict states as discussed before. Thus, I can re-write A.3 as follows:

$$\delta = \delta^{tgw} + \omega_2 \tilde{\delta}^{bans} \ . \tag{A.4}$$

Estimates of ω_2, δ and $\tilde{\delta}^{bans}$ can be used to construct estimates of the effect of tobacco graphic warnings. The running equation to estimate δ is (1), replicated here omitting individual and state-time varying control variables to ease exposition:

$$y_i = \sum_{\tau=-2}^{1} \delta_\tau \left[\text{Treat}_s \cdot (\text{Years after treat} = \tau) \right] + \alpha_s + \alpha_t + \varepsilon_i \ . \tag{A.5}$$

Next, the proportion of states that have not implemented a place-based ban before the national regulation is observed in the data: $\omega_2 = P(s \in S_2)$ and can be estimated as the number of states that are late-adopters of clean-indoor-air bans relative to the total number of states. Finally, the running equation to estimate $\delta^{bans} + f(\delta^{bans}, \delta^{tgw})$ is:

$$y_i = \gamma_0 + \gamma_1 \times 1\{Year = 2013\} + \gamma_2 \times 1\{s \in S_2\} + \tilde{\delta}^{bans} 1\{Year = 2013 \ , s \in S_2\} + u_i \ . \tag{A.6}$$

This regression can be augmented by leads and lags of relative treatment time in an

event-study framework:

$$y_i = \sum_{\tau=-2}^{1} \tilde{\delta}_{\tau}^{bans} \left[\text{Bans}_s \cdot (\text{Years after treat} = \tau)\right] + \alpha_s + \alpha_t + u_i \,, \tag{A.7}$$

where $Bans$ is a dummy variable that equals 1 if the state s is a late-adopter of place-based bans.