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

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Abstract

Modifiable risky behaviors such as smoking tobacco are a major determinant of premature death in developed and developing countries. In this paper, I study the effects of non price interventions on smoking habits in the context of Argentina's 2011 anti smoking law. I interact previous state-level legislation with the national law to identify the effect of the policy on smoking prevalence and average daily cigarette consumption. I find that the probability of being a current smoker decreased by 6.17 percentage points. I explore whether alcohol and tobacco are consumed as complements or substitutes to assess the side effects of non price policies. Graphic warning labels have often been enacted as part of broader anti-smoking campaigns. I propose a framework to disentangle the effect of place-based bans from the effect of tobacco graphic warnings.

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

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I Introduction

Modifiable risky behaviors such as smoking tobacco are a major determinant of premature death in both developed and developing countries (Blecher, 2008 and Cawley and Ruhm, 2011). The World Health Organization estimates that tobacco use is the leading risk factor in high-income countries, accounting for 18% of deaths.[1] In middle-income countries, were 80% of smokers worldwide live; tobacco use is the second most important risk factor, 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.

Preventing individuals from smoking and reducing their exposure to environmental tobacco smoke is a key public health priority of many governments. To this end, governments have implemented a variety of regulations, which include cigarette taxation, place-based smoking bans in areas ranging from bars and restaurants to workplaces, and regulations designed to make tobacco products less desirable, e.g., tobacco graphic warnings.[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 GDP or, equivalently, 7.5% of the 2015 federal health budget (Alcaraz et al., 2016). In 2011, the Argentinean government implemented a 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 incorporation of tobacco graphic warnings in cigarette packages. The Argentinean case represents 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 difference in the stringency of tobacco regulation before 2011 as a source of exogenous variation.

Data for this research comes from three main sources. First, to estimate the effects

[1] WHO, 2009

[2] Tobacco graphic 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 figure A.1 in the appendix for examples).

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

of this non price intervention on smoking and drinking I use individual-level data from two national surveys for the years 2008 to 2013.[4] These data include geographic location, demographic characteristics, and cigarette and alcohol consumption, which I use to construct smoking status and participation, and drinking behaviors among smokers. Second, to study 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 states statutes and laws, I coded a total of 47 state-level to characterize the regulatory environment on 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 therefore 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 the intervention.

I find that the anti tobacco regulation effectively reduced smoking participation; the probability of being a current smoker decreased by 22% among people aged 18 to 65. On the intensive margin, the share of individuals smoking less than 5 cigarettes a day decreased in 0.16 percentage points; suggesting that the reduced probability of being a current smoker is driven by quitters whose consumption was on the lower end of the distribution. Regardless of its effectiveness, the 2011 national law has regressive implications. Non-price policies are expected to be less regressive than price policies as they do not increase the economic burden of consumption on poorer smokers; who make a large proportion of the smoking population on 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 to be a current smoker 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 national law was

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

introduced, this effect is 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 for 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 a 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 leading to an increase in alcohol consumption. I find that the new tobacco regulation induced a decrease in 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. [5]

The impacts on extensive an intensive margins outcomes are reflected in better health outcomes. The rate of hospital discharges due to diagnoses of chronic obstructive pulmonary disease (COPD) and hospital discharges caused by lung cancer diagnosis decreased in the short run. I interpret these results as suggestive of an improvement in population health correlated with more strict legislation on tobacco products, but due to data limitation I can not identify whether the results are driven by less individuals being diagnosed these diseases or the same number of individuals being diagnosed less severe symptoms and requiring less hospitalizations.

This paper contributes to a vast literature in health economics that evaluates tobacco taxation and regulation.[6] Although tobacco has long been heavily taxed; non-price policies has became increasingly common in the last two decades. The most commonly implemented non-price polices are place-based bans and warning labels, e.g., by 2016 at least 105 countries had required graphic warnings to be printed on cigarette boxes.[7]

[5] I find that, contrary to beer and wine, smoking and spirits have a substitution relationship on consumption. One potential explanation of this result is that smokers who drink spirits are different than the rest of smokers who abuse alcohol. In fact, I find that this result is driven by single young people (less than 25 years) with more education who are substituting outside smoking by inside drinking in bars and pubs.

[6] DeCicca, Kenkel and Lovenheim (2020) provide the most recent review of this literature, see also Carpenter, Postolek and Warman (2011) and Chaloupka and Warner (2000).

[7] See, Canadian Cancer Society Annual Report, 2016. In 2001, Canada became the first country int he world to introduce graphic warnings in cigarette boxes.(Azagba and Sharaf, 2012), and was quickly followed by many others. By 2010, 35 countries have introduced graphic warnings and this number rises to 77 in 2015. In the US, the 2009 Tobacco Control Act requires that cigarette packaging and advertisements include graphic warnings, but the implementation has been delayed by legal challenges from cigarette manufacturers. For example the case R.J. Reynolds Tobacco Co. et al. v. the United States Food and Drug Administration et al. in the U.S. District Court for the Eastern District of Texas has its date of a final rule on Oct. 16, 2021 (fda.gov/tobacco-products).

Researches have found different answers to whether place-based bans are effective. North-America based research shows mixed impact of smoking bans on smoking behavior (Adda and Cornaglia, 2010, Bitler, Carpenter and Zavodny, 2010, Carpenter, Postolek and Warman, 2011, and Burton, 2020), while European based research suggests a systematic reduction of small magnitude in smoking behavior (Buonanno and Ranzani, 2013, Pieroni et al., 2013, and Sureda et al., 2014). The local context of smoking behavior could help explain differences in estimates: how popular and accepted smoking is, whether is strongly associated with other behaviors, the accessibility to smoking cessation programs among others would contribute to individuals responses to anti tobacco policies. Nonetheless, 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 in cross-country (e.g, Blecher, 2008, Abascal et al., 2012) or before-and-after comparisons (e.g., Thrasher et al., 2010); I employ a stronger identification strategy and provides causal estimates of the impacts of this policy on smoking, drinking and health outcomes.

This paper also contributes to the literature that studies the impacts of tobacco graphic warnings. Cigarette warning labels are very well-established policies but research based on actual consumption behavior is limited. The effectiveness of this policy has been assess from randomized controlled laboratory experiments (e.g., Hammond, 2011) and cross-country comparisons (e.g, Blecher, 2008). The main empirical challenge to evaluate this policy is that graphic warning 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 tobacco graphic warnings and place-based bans, I combine: (i) a simple framework that puts structure to the effect of a bundle policy and (ii) additional variation among early and late adopters of place-base bans. I find that tobacco graphic warnings explain about 60% of the impact on the probability of being a current smoker.

The rest of this 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 operated. 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 transport means; and (iii) it regulates advertising of tobacco products and mandates the incorporation of tobacco graphic 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 figure A.1 in the appendix for examples). Images are designed by the National Department of Health and are updated once every year and a half in order to attenuate potential wear-out effects of graphic warnings. After the law was approved, producers had up to six months to incorporate the designs on their packages and could not adapt the warnings to reduce their effectiveness. [8]

Argentina is a federal country; as in the US, states have *some* autonomy to implement different regulations. States do not have control over cigarette prices, and taxation of tobacco-related products is exclusive to the national government. But, each state has the autonomy to regulate smoking and access to tobacco products. I compile data on tobacco regulation from states statutes and laws for each of the 23 Argentinean states and the capital city to characterize the regulatory environment on 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 regulation.[9] Higher values on the legislation index represent strong regulation on 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 also banned consumption in public transportation or bars and restaurants. Each subsequent unit increase indicates a tightening in the regulation. The highest value of the index before 2011 is 6, which implies limiting ad-

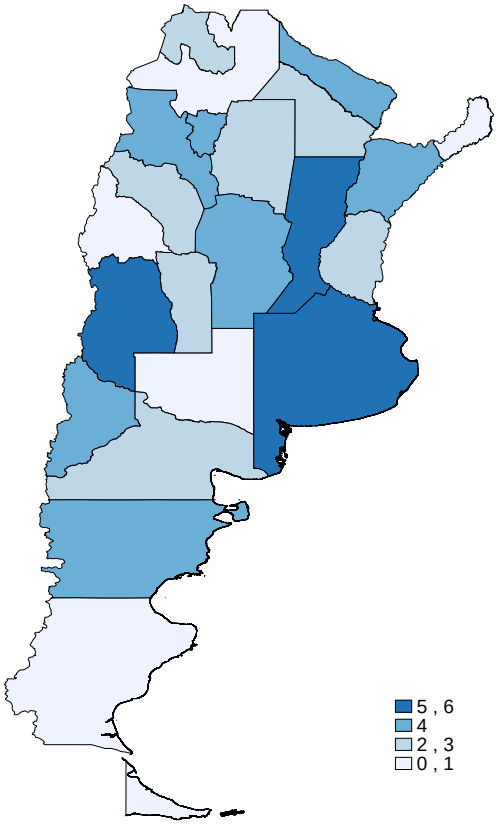
[8] An advantage of this setting is that individuals are *almost* randomly assigned to graphic warnings. Cigarette boxes are mainly sell at convenience stores where consumers can not directly access the cigarette box but have to ask for a box from the cashier, leaving very little room to pick which warning is printed in the box 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 can not provide evidence about which warning is more effective.

[9] This attempt to assemble data on the enactment of tobacco regulation policies is similar to the Prescription Drug Abuse Policy System, which aims to track key state laws related to prescription drug abuse. Researchers have widely used these data to study the effects of such laws on the opioid use and mortality (see; Buchmueller and Carey, 2018)

vertising in public spaces, sport events and other venues, indoor smoking bans in several venues (bars, restaurants, educational and health care institutions), and cigarette sales are allowed only in specialty or authorized shops. A value of 7 is reached by all states in 2011 when the national law was passed, which reflects 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 a unit increase does not reflect a specific change in regulation. The data construction section in the 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 while 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 where only sales to minors were banned (a state with legislation index equal to 0), the national law introduced place-base bans, advertising limitations and the graphic warnings. On the other hand, in a state where tobacco was heavily regulated, such as a state with legislation index 6, the main changed was the incorporation of graphic warnings. Nonetheless, every state experienced a tightened regulation after the introduction of the 2011 national law. The ideal research 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 inform researchers about the effectiveness of the policy for different increments in strength of regulations. 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 value of legislation index (less than or equal to 3); and to the other states as strict or comparison states. As a robustness check, I replicate my analysis with 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 while lighter shades indicate more lenient regulation. See Appendix A.ii for details about the 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 Prevalence of Consumption 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 years old.[10] Importantly, these survey data includes state-identifiers and are available for the years before and after the policy was implemented: 2008 , 2009, 2011 and 2013.

I construct two outcomes of smoking behavior. First, I construct an extensive margin

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

outcome, defined as the probability that an individual is a current smoker. I consider an individual to be a current smoker if she has ever smoked more than 100 cigarettes or if at the moment of the survey she smokes every day or some days. My definition is close to the one implemented 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 of 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 ban consumption in these venues. Second, extensive and intensive margin measures are based on self-reported data, which is not free of measurement error. Measurement error might arise because smokers deny their habit when surveyed or they report incorrectly 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 under-report their consumption and (ii) under-reporting is stable across the years, i.e. it did not change after the implementation of the national law.[11] Even tough smoking behavior outcomes are measured with some error in the survey data, the direction of this error is consistent with my results providing a lower bound and not 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 two measures of harmful alcohol consumption: abusive consumption and binge drinking. I defined alcohol abuse as an indicator of having more than eight drinks of beer, five drinks of wine and three drinks of spirits in a given day.[12] Binge drinking is defined as consuming five or more drinks during a single occasion in the past 30 days, either over the weekend or during a weekday.

Last, I study effects on health outcomes. I use restricted-access administrative data on hospital discharges to compute the prevalence rate of hospitalization from Chronic Obstructive Pulmonary Disease (COPD) and respiratory-system-related cancer.[13] These

[11]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.

[12]Spirits include vodka, gin, tequila, rum, and whiskey. These thresholds are defined following the guidelines from the WHO and the CDC.

[13]COPD is defined using ICD 10th revision codes J41, J42, J43 and J44. Respiratory system related cancer

data come from the Argentinean National Center for Health Statistics and are provided at the hospitalization-level.[14] In the data one observation corresponds to an hospitalization event for which I observed individual's gender, age, the main diagnosis, and a state identifier. The main limitation of these data is that I do not observe individual identifiers nor their smoker status. For this reason, my analysis is just suggestive on how population health changed after the national law but can not distinguish whether there are fewer diagnosis or less severe ones.

Table 1 shows mean outcomes and control variables before the national law was enacted. Strict states (high values in the legislation index) have similar proportion of never and current smokers than lenient states (low values in the legislation index). The distribution of smoked cigarettes per day in lenient states has a heavier right tail, i.e. the proportion of heavy smokers is higher in these states. Importantly strict and lenient states show similar prevalence rates in the diagnosis directly affected by tobacco smoking.

Table 1: Summary Statistics, pre-policy period

Variables (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 10	0.573	0.640	0.517	0.000
11 to 20	0.265	0.275	0.257	0.281
more than 20	0.161	0.085	0.226	0.000
Prevalence rate (per 1,000)				
COPD	0.203	0.196	0.209	0.765
Lung cancer	0.069	0.061	0.076	0.330
Observations	60,449	23,830	36,619	
Number of states	24	11	13	

Notes: This table presents summary statistics for 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 presents the p-value for test of equality of means.

is defined using ICD 10th revision codes C30, C33 and C34.

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

IV Empirical Strategy

The 2011 law impacted the regulation of tobacco products nationally, but I exploit that states with more lenient restrictions were more exposed to the effects of the new regulation, comparing more lenient to more strict states. To implement this comparison, 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 a legislation strictly greater than 3 serve as comparison states.[15] 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 where individual i is observed in year t, y_i is the outcome of interest. The variable $Years\ after\ treat = \tau$ indexes time relative to the law implementation.[16] The variable $Treat_s$ that indicates if state s serves as a comparison state ($Treat_s = 0$) of as a treated state ($Treat_s = 1$). In this specification, lagged coefficients (δ_{-2} and δ_{-1}) capture if the outcome of interest y in comparison and treated states was on similar trends before the policy changed. The coefficient for the lead period, δ_1 captures the effect of the national law one year after the implementation. The omitted coefficient is δ_0, which corresponds to the year of the treatment. X_{is} is a vector of control variables that comprise individual-level characteristics, e.g., age, gender, educational attainment, 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.[17]

[15] 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 to similar conclusions .

[16] I normalize time relative to the law implementation to match the years when I observe outcome variables: e.g, I consider $\tau = -2$ for the estimation using outcomes in 2008, $\tau = -1$ for 2009, $\tau = 0$ for 2011 and $\tau = 1$ for 2013.

[17] Using cluster-robust standard errors permit both error heteroskedasticity and flexible error correlation within clusters. With a small number of clusters the 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-errors estimates 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.

My identifying assumption is that comparison states would have been on the same trend as treated states absence 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 the timing of regulation at the state level to be exogenous to potential outcomes. 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 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 of enacting the 2011 National Law; nonetheless I find that the Peronist party was not more likely to regulate tobacco consumption at the state level than the 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 this states to be more lenient towards consumption of tobacco products. States with a higher prevalence of COPD diagnosis were 0.394% more likely to have enacted a stricter regulation; although statistically different from zero, this coefficient is very small. 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 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.[18] 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

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

sates 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.

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

Variables (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 presents the p-value for test of equality of means.

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

Before examining the effects of the policy on outcomes of interest, I provide evidence of law compliance using data on exposure to environmental tobacco smoke in closed venues. In 2009 and 2013 surveys, non-smokers were asked if they notice someone smoking inside their home, work place, educational institution, a hospital or at bars and restaurants. With exception of the latter, all states banned consumption in these venues before the 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, work places, educational institutions and hospitals; while the share of non-smokers exposed in bars and restaurants increased in 10%. These results suggests that exposure to tobacco smoke in bars and restaurants decreased more than in other venues. Importantly, these other venues were already regulated in comparison and treatment states before the 2011 law; therefore, the differential response in restaurants is informative of law enforcement.

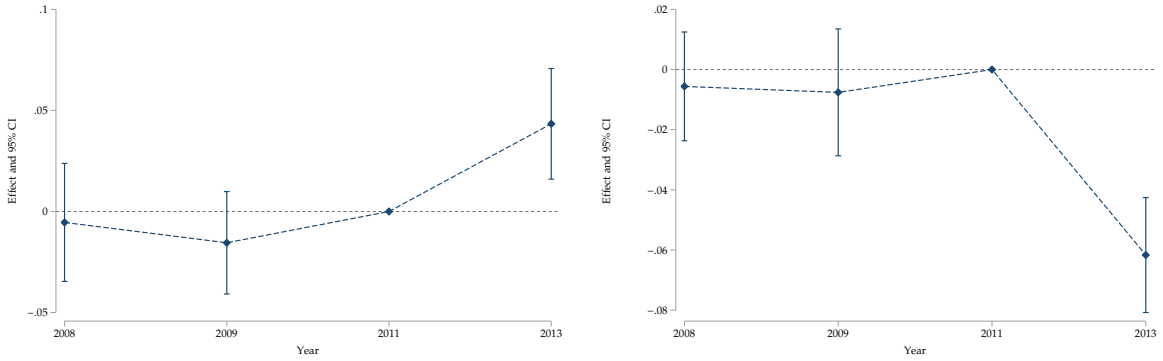
V Results

My empirical analysis proceeds in three steps. First, I estimate the causal impact of the 2011 national law on smoking behaviors, I look at smoking participation and intensive. These exercise provide evidence on the effectiveness of the policy. Second, I ask whether alcohol and tobacco are consumed as complements or substitutes to assess potential side-effects of the tobacco control policy. 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 more strict 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 result suggests that the law curbed tobacco initiation: the law caused a roughly 10% increase the probability of an individual having never smoked. This result in reinforced by a reduction of 6.17 percentage points in the probability of being a current smoker. This represents a sharp 22% reduction in this outcome, suggesting that the national law effectively helped individuals to quit smoking. Figure 2 shows these results. 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, providing 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 in 2013 (left panel) and the probability of being a current smoker (right panel). 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 ask 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-units 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].[19] For example, b_1 is a dummy variable that takes the value of one if the number of cigarettes smoked is between one and five. Results are presented in Table 3.

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 5 cigarettes per day and less likely that they consume fewer than 5 cigarettes per day. Specifically, the proportion of smokers who consume between zero 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 4 bins, i.e. up to a pack per day. Thus, capturing effects for bins above 20 cigarettes requires a bigger sample or sizable effects to avoid power-related limitations. Taken together, 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.

[19]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: The omitted category corresponds to 2011, the year the federal law was passed. Individual controls include 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 percentage of times the predicted outcome matches the actual, (see Wooldridge, 2010). Standard errors are block-bootstrapped at the state-level with 200 replications.*** p<0.01, ** p<0.05, * p<0.10

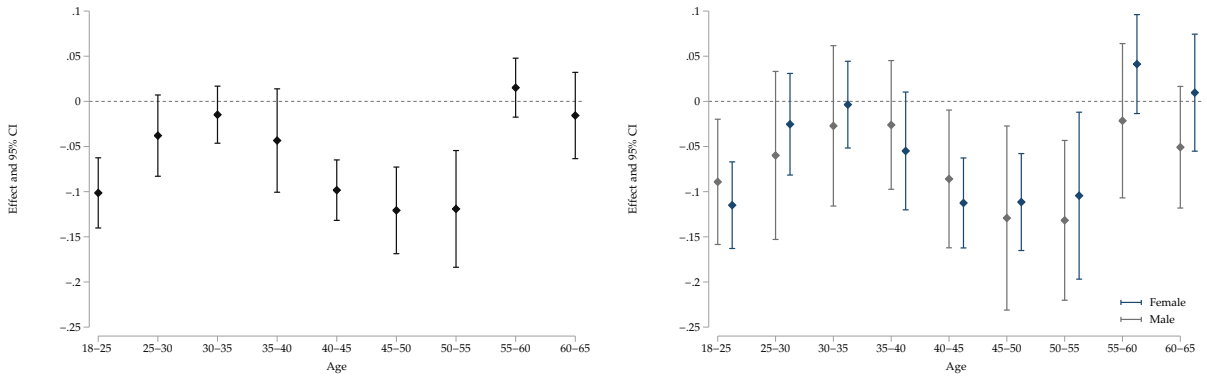
Heterogenity in Smoking Participation.—Do the effects of stricter restrictions on tobacco consumption vary across populations? To tackle this question I estimate equation (1) restricting the sample by age group, gender, educational attainment, and household income. Figures 3 and 4 plot the estimates 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 providing additional support for the parallel trend assumption.

The reduction on 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 estimated effects are statistically comparable, the mechanism behind them is quite different: an increase in non-smokers among the younger adults while an increase in former smokers among adults aged 40 to 55. In 2013, the percentage of never smokers aged 18 to 25 was 66.54%, whereas 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 among these 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. One 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 intend to 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 to the null effects estimated for these groups.

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 test 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-value).

Figure 3: Extensive Margin Effects by Age and Gender



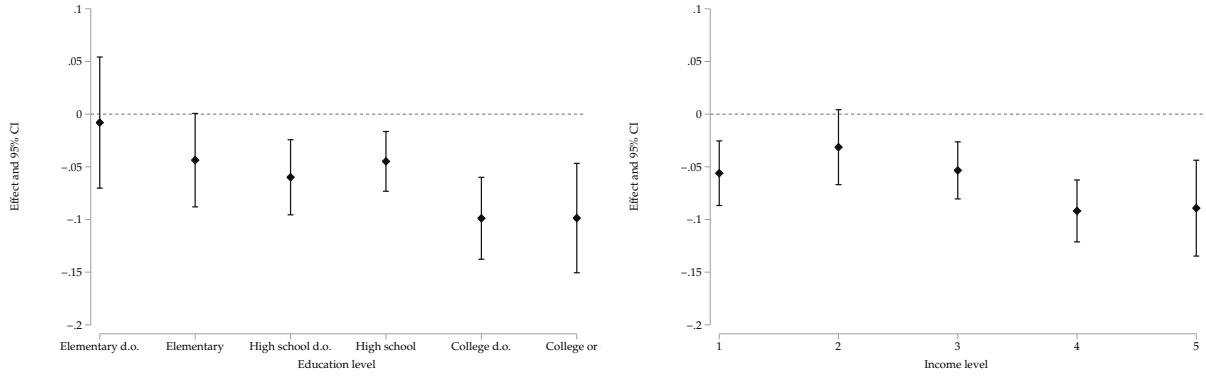
(a) Age groups (b) Age-gender groups

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.

Non-price policies are expected to be less regressive than price policies. In particular, cigarette taxes would be regressive with respect to income if poorer and richer consumers smoked at the same rate as 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. Policies that change the costs of smoking but not the monetary price of cigarettes have the comparative advantage of not increasing the economic burden of 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). I document that 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 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 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).

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 and panel (b) presents results by household income ranking. d.o. stands for drop-out. The 1^{st} quintile corresponds to the lowest 20% households in the income distribution. Analogously, the 5^{th} quintile corresponds to the highest 20 % households in the income distribution.

Robustness checks.—The main results on extensive and intensive margin outcomes are robust to a change in the definition of the treatment. As a robustness check, I define the treatment as a dichotomous variable that equal 1 if the legislation index for state s is strictly less than 3 before 2011. This change implies that the pool of states used as controls are stricter states. I reproduce table 2 and 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. These results change in a direction that one would expect given the change in the composition of the comparison

group: as more strict states are used as comparison, the changes induced by the 2011 law are smaller.

I also consider two additional robustness checks, I test if the results are driven by one state by estimating the main outcomes of interest in samples that drop one state at the time. I present these results in figure A.3. Results indicate that the estimate effect is not driven by one state with particularly strong effects. An additional concern is whether the trend in prices of cigarettes confound my results. I present evidence that for the period of analysis there is no evidence of sharp changes in the price of the cheapest cigarette box. I look at the minimum price because smokers could *easily* substitute among brands to avoid price changes in the more expensive brands.

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

Policies that ban consumption of tobacco on bars and restaurants could have important effects on smoker's alcohol consumption as 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 leading 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 responses of alcohol consumption from a price policy exploiting changes in cigarette prices (e.g. Decker and Schwartz, 2000; Tauchmann et al., 2013; Krauss et al., 2014; Shrestha, 2018). One 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.

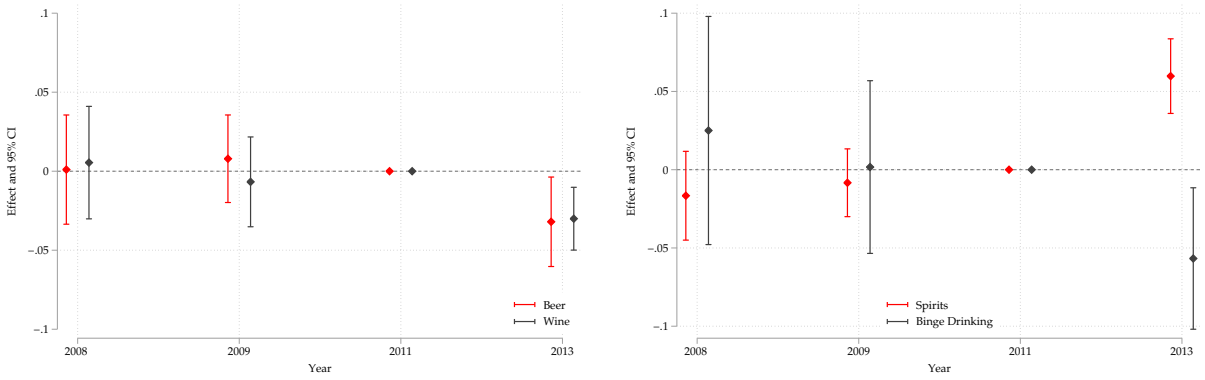
Figure 5 reports estimates of the marginal effects on alcohol consumption from the estimation of equation (1) using a Probit model. Appendix Table A8 presents the estimates of the (raw) coefficients. Marginal effects can be interpret as the percentage point change in the relevant outcome relative to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

I find evidence that tobacco and abusive consumption of beer and wine are complement goods, i.e., 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 on smoking participation and a decrease in abusive consumption of beer of 3.20 percentage points; 3.00 percentage points for wine, and 5.67 percentage points in binge drinking.

Tobacco and spirits have a substitution relationship. Consumption of spirits increased 5.98 percentage points after the stricter regulations on tobacco consumption and advertising when into effect. The average level of abusive consumption of spirits in 2011 was 3.18%, thus the estimated effect suggests a huge increase in this risky behavior. One potential explanation of this result is that smokers who drink spirits are different than the rest of 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 wealthy than those who abuse other drinks.[20] Thus, the difference in sign of the elasticity could be attributable to young people substituting outside smoking by inside drinking in bars and pubs. This seems to be specially true for young men, the policy effect on spirits consumption is three times larger for men than for women.

Figure 5: Effects on risky consumption of alcoholic beverages



(a) Beer and Wine

(b) Spirits and Binge Drinking

Notes: This figure presents estimates of the marginal effect 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 are specific to the beverage, see the main text for details. Binge drinking is defined as consuming five or more drinks during a single occasion in the past 30 days, either over the weekend or during a weekday. Standard errors are clustered at the state level.

[20] I present conditional sample means of observable characteristics in Appendix Table (A9).

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

Reduced smoking in public places could convey important health benefits to both nonsmokers and smokers. Smoking can cause lung disease by damaging the airways and the small air sacs (alveoli) found in the lungs; lung diseases caused by smoking include COPD, which includes emphysema and chronic bronchitis. 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.[21] For 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 tobacco graphic warning using hospital discharge data. I construct the prevalence rate of diagnose c for age group a as the ratio between the number of cases with diagnoses cause 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:

$$y_a = \sum_{\tau=-3}^{4} \delta_\tau \left[\text{Treat}_s \cdot (\text{Years after treat} = \tau)\right] + \Gamma' X_{st} + \alpha_a + \alpha_s + t + \varepsilon_{st}, \tag{2}$$

where $Years\ after\ treat = \tau$ is equal to the difference between the calendar year and the year the national law was passed. X_{st} is a vector of control variables that comprise state-level characteristics. State fixed effects (α_s) control for variation in outcomes across states that is constant over time. I include age-group fixed effects (α_a), 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 is constant across states. Standard errors are robust to heteroskedasticity.[22] The coefficients can be interpreted as a change in the prevalence rate of diagnose c when the legislation was tightened.

The first column in table 4 shows the estimated effect on COPD prevalence rate for the population aged 18 to 65 years old. I estimate that by 2014 the prevalence of COPD decreased about 3.6 percentage points relative to 2011, while the respiratory system cancer decreased by about 1.4 percentage points. The effect for each year after the national

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

[22] I refrain from using clustered standard errors at the state level because the number of observations within each cluster is small.

law was passed is greater in absolute value than the previous one hinting that the policy could have important long term effects.[23] I interpret the results on health outcomes as suggestive evidence of an improvement on population health correlated with a more strict legislation of tobacco product. These results could be driven by less individuals being diagnosed these diseases; the same number of individuals being diagnosed less severe symptoms and requiring less hospitalizations, or a combination of both; but I can not shed light on what channel is the most important.

Table 4: Health Outcomes

Diagnosis	COPD	Lung cancer
	(1)	(2)
2008	-0.00085	-0.00189
	(0.0113)	(0.0065)
2009	-0.00492	-0.0032
	(0.0107)	(0.0071)
2010	-0.01130	0.00232
	(0.0113)	(0.0085)
2012	-0.01670	-0.00758
	(0.0184)	(0.0051)
2013	-0.0310	-0.0106
	(0.0170)	(0.0050)
2014	-0.0361	-0.0144
	(0.0206)	(0.0049)
Mean dep. var.	0.1061	0.0644
Observations	1512	1512
R-squared	0.447	0.539
State x times controls	Yes	Yes
State FE	Yes	Yes
Linear trend	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the national law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals 1 if the legislation index for state s in moment t is less than or equal to 3 before 2011. After 2011, all states *are treated* thus $Treat_s$ equals 1 for all states s after 2011. State $\times$ time controls include total private employment. Standard errors are robust to heteroskedasticity.

[23]Unfortunately the Argentinean National Center for Health Statistics stopped collecting hospital discharge data after 2015, so doing a longer term analysis is not feasible.

VI Bans or Tobacco Graphic Warnings?

The implementation of the 2011 Argentina's 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 incorporation of tobacco graphic 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 tobacco graphic warnings (Health Canada, 2002).[24] Other 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. First, as policies are implemented at the same time researchers can not exploit either regional or temporal variation to disentangle the effects of each policy. The second challenge refers to the presence of spillover effects between policies. If this are non zero, the estimated effect of the policy bundle would be a combination of the effects of each policy and a spillover effect. To overcome these identification challenges I propose the following framework. Let the parameter δ be the effect of the policy bundle on the outcome of interest, δ^{bans} the effect of place-based policy, and δ^{TGW} the causal effect of the tobacco graphic warnings. The parameter δ can be written as a linear combination of the effects of each policy and the spillover effect: [25]

$$\delta = \delta^{tgw} + \omega\, \delta^{bans} + \omega\, f(\delta^{tgw},\delta^{bans}) \ , \tag{3}$$

where ω is the proportion of states that have not implemented a place-based ban before the national regulation and can be estimated from the data. $f(\delta^{tgw},\delta^{bans})$ is the spillover effect of implementing the policies jointly, which is assumed to be a function of each policy effects. I define $\tilde{\delta}^{bans} = \delta^{bans} + f(\delta^{bans},\delta^{tgw})$ and rewrite δ as: $\delta = \delta^{tgw} + \omega\, \tilde{\delta}^{bans}$. To estimate this parameter, I exploit the comparison between states that have implemented a place-based ban before 2011 (early adopters) with those where bans where not imple-

[24] In January 2001, Canada became the first county in the world to introduce pictorial warning messages on cigarette packs (Azagba and Sharaf, 2012). Begging 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).

[25] See appendix section D. for a detailed derivation of this equation.

mented until 2011 (late adopters). Under the assumption that the early adopter are a good comparison for late adopters, the following regression provides a causal estimate of $\tilde{\delta}^{bans}$:

$$y_i = \sum_{\tau=-2}^{1} \tilde{\delta}_{\tau}^{bans} \left[\text{Bans}_s \cdot (\text{Years after treat} = \tau)\right] + \beta' X_{is} + \Gamma' X_{st} + \alpha_s + \alpha_t + \varphi_i, \qquad (4)$$

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 have been previously defined. I argue that the estimate of $\tilde{\delta}^{bans}$ provides an upper bound estimation of δ^{bans} if one is willing to assume that spillover effects due to join implementation are positive.[26] Estimates of δ and $\tilde{\delta}^{bans}$ can be used to construct estimates of the effect of tobacco graphic warnings, I present these restuls in Table VI. This exercise helps to understand what are the mechanisms driving the extensive margin outcomes.

Tobacco graphic 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. To see why this might be the case, notice that when non-smokers are asked about graphic warnings, 73% of them reported having seen them in the last 30 days.[27] Also, studies using experimental settings have documented that graphic warnings elicit emotional responses, such as fear or disgust, (DeCicca, Kenkel and Lovenheim, 2020). These emotional responses might be particularly effective on non-smokers. That is, the warnings make the health's costs of smoking more salient to individuals who have not derived utility from smoking which could compensate the costs related to smoking.[28]

Among current smokers, around 60% of the effect can be attributed to the incorporation of tobacco graphic warnings. I estimate that the incorporation of tobacco graphic warning reduce the probability of being a current smoker by 3.85 percentage points. When asked about the warnings, 96% of smokers reported that they have seen graphic warnings on cigarettes boxes and about 54% of smokers answered positively. Although the intention or thought of quitting smoking might not reflect actual behavior, these answers are consistent with smokers being affected by the graphic warnings.

[26] There is no empirical evidence on the sign of these spillover effects. I hypothesize that, if different from zero these spillover effects are positive.

[27] This figure is computed from the results of the 2013 National Survey on Risk Factors.

[28] When an individual smokes a cigarette it only takes six to ten seconds for the nicotine to reach her brain. This makes smoking tobacco very addictive and difficult to stop. Also, nicotine speeds up a person's reaction time and increases his or her attention and focus. Many smokers report that they enjoy the ritual of smoking and that smoking gives them a pleasurable feeling. American Thoracic Society (2017)

Finally, I find a small effects of place-based bans on the proportion of non-smokers smokers, which highlights that non-smokers do not face the cost of place-based bans, i.e. they do not find themselves being displaced outside venues to smoke. On the other hand, roughly 40% of the change in the proportion of current smokers can be attributed to place based bans and spillover effects arising from join implementation of the policies. Suggesting that the reduction on the probability of being a current smoker is driven by both components of the policy.

Table 5: Mechanisms

Policy	National Regulation (δ)	Place-based Bans ($\tilde{\delta}^{bans}$)	Graphic Warnings (δ^{tgw})
Never smoker	0.0434	0.00546	0.04067
	(0.0175)**	(0.0220)	(0.0207)*
Current smokers	-0.0617	-0.04635	-0.038525
	(0.0103)***	(0.0080)***	(0.0102)***

Notes: This table presents point estimates of the effects of the federal law, the effects of place-based bans, and the incorporation of tobacco graphic warnings following the decomposition proposed in equation (3). The first column reproduces results presented in A3. Standard errors are block-bootstrapped at the state-level with 200 replications. *** p¡0.01, ** p¡0.05, * p¡0.10.

VII Conclusion

Preventing people from smoking and reducing their exposure to environmental tobacco smoke is a key public health priority of many governments. Bernheim and Rangel (2004) model of addiction places a high value on policies that improve opportunities for self-regulation and identifies a central role for "cognitive" policies, including the suppression of certain environmental cues (e.g., through limitations on advertising), and the dissemination of counter cues. This paper provides empirical support to this model, I find that public-place smoking restrictions and the incorporation of tobacco graphic warnings, are promising policies for decreasing smoking prevalence.

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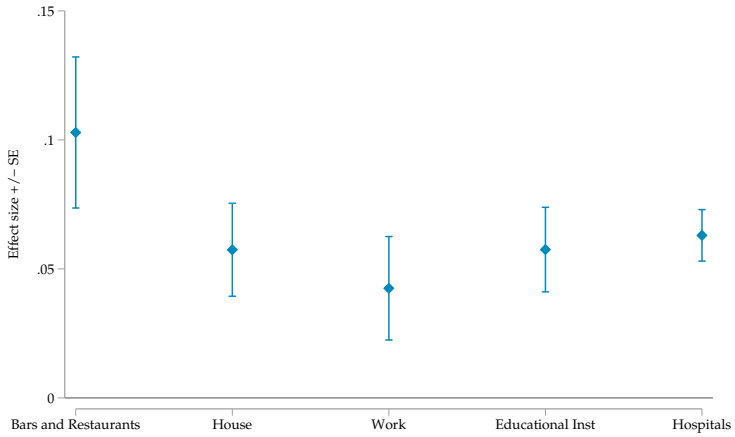
A Appendix

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 boxes. 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. All coefficients are expressed as effect sizes to ease the comparison of the effects across venues. Each coefficient corresponds to a separate regression of the share of individuals reporting to not have noticed someone smoking inside a given venue.

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, I converted them in monthly figures. Consumption from survey data is computed based on average consumption per day multiply by 30. Smokers are also asked how many days they smoke per month, though this variable has many missing values. 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)
Sh. Population 0 - 14 years old	0.066	0.022
	(16.370)	(5.584)
Sh. Population 15 - 24 years old	48.93***	16.687***
	(17.580)	(5.832)
Sh. Population 25 - 44 years old	20.400	6.958
	(16.510)	(5.682)
Sh. 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%), Tucumán (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. 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 in squared brackets 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. 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 in parentheses are clustered at the state-level and the number of states is 24. Standard errors are block-bootstrapped at the state-level with 200 replications.

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
Correctly Predicted									
Female	0.7467	0.7163	0.7162	0.7691	0.5735	0.7051	0.6879	0.7688	0.8351
Male	0.6317	0.5235	0.5972	0.6281	0.4677	0.6400	0.6307	0.6871	0.7359
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. 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 A6: Probability of being a current smoker by educational attainment

Educational level:	Elementary d/o (1)	Elementary (2)	HS d/o (3)	HS (4)	College d/o (5)	College + (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. 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. HS stands for High School and d/o stands for drop-out. Standard errors are block-bootstrapped at the state-level with 200 replications.

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. 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 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. After 2011, all states *are treated* thus $Treat_s$ equals one for all states s after 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 and the number of states is 24.

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

Table A10: Effects on consumption of alcoholic beverages by gender among smokers

Dep. variable	Alcohol consumption (1)	Beer abuse (2)	Wine abuse (3)	Spirits (4)	Binge drinking (5)
2013					
Female	0.0100	-0.0225***	-0.0127	0.0261***	-0.0120**
	(0.0170)	(0.0058)	(0.0111)	(0.0075)	(0.0234)
Male	0.0097	-0.0341	-0.0334*	0.0786***	-0.0722**
	(0.0201)	(0.0237)	(0.0194)	(0.0188)	(0.0299)
$H_0 : \delta_{Fem} = \delta_{Male}$	0.0001	0.0000	0.0000	0.0000	0.000
Mean dep. var. in 2011					
Female	0.6191	0.0461	0.0497	0.0297	0.2108
Male	0.7864	0.1399	0.1671	0.0328	0.432
Observations					
Female	12,471	7,994	7,994	7,994	7,994
Male	16,920	13,567	13,567	13,567	13,567
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:*This table presents estimates of the causal effect of the national law on alcohol consumption behavior across smokers by gender, a complete set of results is presented in table [add ref]. The omitted category corresponds to 2011, the year the federal law was passed. 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. Treatment is a binary variable that takes the value of 1 if the legislation index for state s in moment t is less than or equal to 3 before 2011 and for every state s after 2011. Standard errors are clustered at the state level and the number of states is 24.

Table A11: Effects on COPD prevalence rate

Age	20 - 65	20 - 25	25 - 40	40 - 55	55 - 65
	(1)	(2)	(3)	(4)	(5)
2008	-0.000845	-0.00312	-0.018	0.0143	0.00338
	(0.0113)	(0.0021)	(0.0227)	(0.0120)	(0.0250)
2009	-0.00492	-0.00134	-0.0268	0.0154	-0.00434
	(0.0107)	(0.0018)	(0.0225)	(0.0092)	(0.0210)
2010	-0.01130	-0.00178	-0.02250	-0.00922	-0.02100
	(0.0113)	(0.0010)	(0.0257)	(0.0062)	(0.0184)
2012	-0.01670	0.00021	-0.00993	-0.02020	-0.03000
	(0.0184)	(0.0012)	(0.0492)	(0.0147)	(0.0146)
2013	-0.0310	0.0013	-0.0522	-0.0194	-0.0329
	(0.0170)	(0.0018)	(0.0425)	(0.0197)	(0.0174)
2014	-0.0361	0.0027	-0.0597	-0.0246	-0.0375
	(0.0206)	(0.0021)	(0.0511)	(0.0247)	(0.0211)
Mean dep. var.	0.1061	0.0135	0.0596	0.0852	0.2533
Observations	1512	168	504	504	336
R-squared	0.447	0.803	0.005	0.473	0.702
State x times controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Linear trend	Yes	Yes	Yes	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the national law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals 1 if the legislation index for state s in moment t is less than or equal to 3 before 2011. After 2011, all states *are treated* thus $Treat_s$ equals 1 for all states s after 2011. State $\times$ time controls include total private employment. Standard errors are robust to heteroskedasticity.

Table A12: Effects on COPD prevalence rate

Age	20 - 65	20 - 25	25 - 40	40 - 55	55 - 65
	(1)	(2)	(3)	(4)	(5)
2008	-0.00189	-0.0009	-0.00397	-0.00158	0.002
	(0.0065)	(0.0004)	(0.0058)	(0.0099)	(0.0162)
2009	-0.0032	-0.00071	-0.0051	-0.00022	-0.00508
	(0.0071)	(0.0004)	(0.0057)	(0.0100)	(0.0155)
2010	0.00232	-0.00075	-0.00527	-0.00371	0.02450
	(0.0085)	(0.0004)	(0.0059)	(0.0095)	(0.0220)
2012	-0.00758	-0.00039	-0.00805	-0.01020	-0.00783
	(0.0051)	(0.0004)	(0.0087)	(0.0062)	(0.0114)
2013	-0.0106	-0.0007	-0.0093	-0.0116	-0.0181
	(0.0050)	(0.0006)	(0.0090)	(0.0064)	(0.0128)
2014	-0.0144	-0.0008	-0.00847	-0.0114	-0.0374
	(0.0049)	(0.0006)	(0.0091)	(0.0068)	(0.0161)
Mean dep. var.	0.0644	0.0016	0.0236	0.0640	0.1575
Observations	1512	168	504	504	336
R-squared	0.539	0.802	0.081	0.527	0.736
State x times controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Linear trend	Yes	Yes	Yes	Yes	Yes

Notes: The omitted category corresponds to 2011, the year the national law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals 1 if the legislation index for state s in moment t is less than or equal to 3 before 2011. After 2011, all states *are treated* thus $Treat_s$ equals 1 for all states s after 2011. State $\times$ time controls include total private employment. Standard errors are robust to heteroskedasticity.

B. Construction of Legislation Index

Argentina has 23 states and one federal district.[29] 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 is done at the national level but non price related policies have been put in place at the regional level before the law 26,687 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, sells, 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 next 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. Sells category—defined as bans on sells, includes the next sub-categories: sells to under 18 years old individuals, elementary school, high school, education institutions in general, hospitals or health institutions, government buildings, public transportation means and sells by the unit. Finally, the consumption category includes the following 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. Sells category is restricted to bans in sells to underage individuals. Consumption sub-categories are group 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.

C. Robustness Checks

C.1 Alternative Definition of Comparison and Treated States

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

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 equal 1 if the legislation index for state s is strictly less than 3 before 2011. This change implies that the pool of states used as controls are stricter states.

Table A13 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 sates are poorer than individuals in treated states.

I reproduce results for the extensive margin in Table A14. 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. This is the direction that we expect given the change in the composition of the comparison group: as more strict states are used as comparison, the changes induced by the 2011 law are smaller.

I present results for the intensive margin in Table A15. 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. This result suggests that as more strict states are used as comparison, the pool of smokers is more negatively selected.

Table A13: 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			
Elemetary school drop out	0.08	0.08	0.01
	(0.27)	(0.26)	(0.01)
Elemetary 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 A14: 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 A15: Effects on Intensive Margin Outcomes

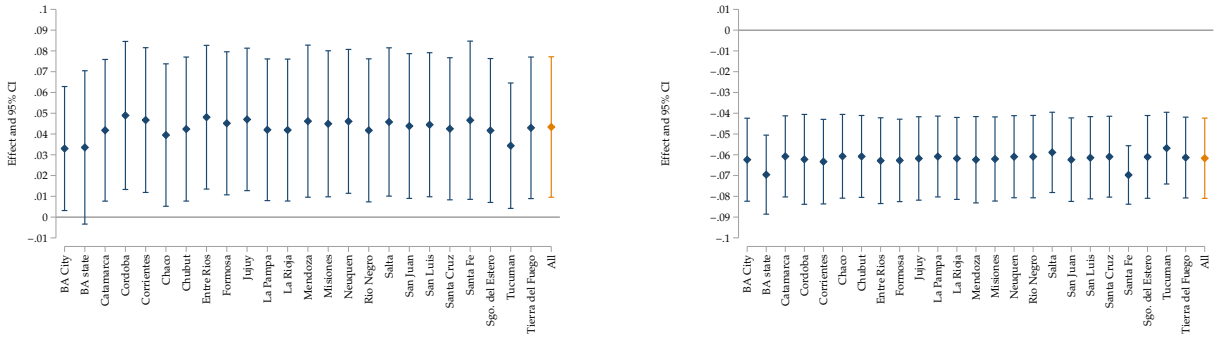
Cigarettes smoked	(0,5]	(5, 10]	(10, 15]	(15, 20]	(20, 30]	(30 , 40]	(40 , 50]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2008	0.0017	0.0237	-0.0033	0.0084	0.0051	-0.0100	0.0010
	(0.0317)	(0.0185)	(0.0104)	(0.0123)	(0.0067)	(0.0062)	(0.0011)
2009	-0.0839**	0.0394*	0.0020	-0.0182	-0.0111***	-0.0097*	0.0008
	(0.0370)	(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 policy's effects driven by one particular state? I address this question by performing a very 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 never smokers (panel a) and never smokers (panel b). I find evidence that point estimates are robust to the exclusion of one state from the sample, although there is some evidence that the state of Buenos Aires (the biggest state in terms of population) might drive the effect on never smokers. The 95% confidence interval includes the zero when the state of Buenos Aires is excluded, nonetheless, the point estimates are very close to the ones find when every state is included. To alleviate this concern, I use weighted OLS in all the regressions presented so far so that estimates are representative at the country level.

Figure A.3: Extensive Margin Outcomes



(a) Never Smokers

(b) Current Smokers

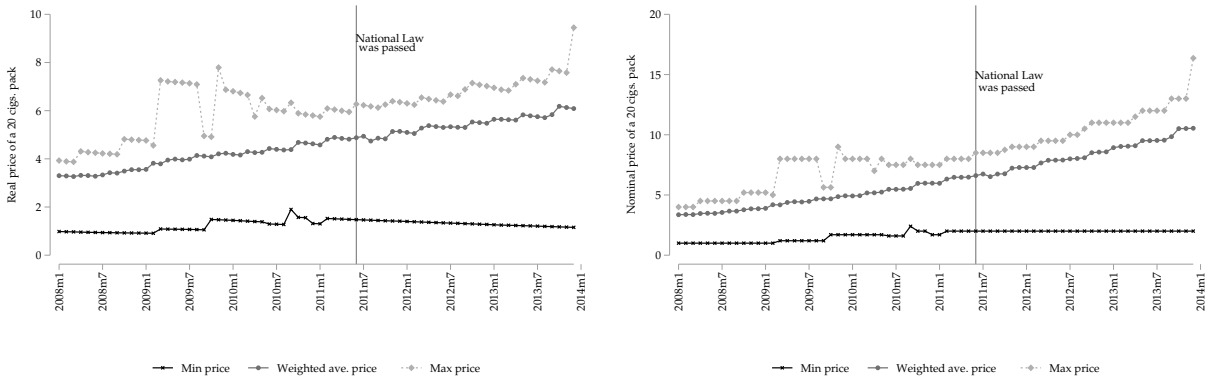
*Notes:*This figure presents estimates of the marginal effect and confidence intervals of the 2011 national law on the probability never smokers (panel a) and never smokers (panel b). Treatment is a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. 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. The category "All" replicates the effects in the main body, see table A3. 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, they provide information on sales by price range. I focus on three price ranges: the minimum price paid (by consumers) in the cigarette market, the average price paid and the highest price paid. Data is 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).[30] There is no evidence of sharp changes in the price of the cheapest cigarette box. I look at the minimum price because smokers could *easily* substitute among brands to avoid price changes in the more expensive brands. This substitution patterns are a very interesting per se and a potential mechanism through which the law operated but I can not speak to this mechanisms with the data that I have.

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

Figure A.4: Cigarette Prices - Time



Notes: This figure presents time series of real and nominal prices faced by consumers divide in 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.

D. Derivations: Bans or Tobacco Graphic Warnings?

In this section I present a framework for identification of the effects of each policy branch. The identification assumption put in place is that; conditional on state and time fixed effects, more lenient and more strict states would have followed the same trend in the absence of the 2011's federal policy. Define y_{0i} as 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 . In absence of this policy, smoking outcomes can be written as the sum of a state effect that is fixed over time and a year effect that is common to all states. The conditional mean function is restricted in a particular way, under the assumption that the effect of the policy is constant across states and time: [31]

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

The parameter δ is the causal effect of the national regulation. I argue that this effect is a linear combination of the effects of each branch of the policy. To see this, define S_1 as the subset of states that have implemented place-based bans before the national implementation and S_2 as the subset of states that have not implemented such bans. The counterfactual outcomes for each subset of states show that some states *have already been*

[31] I implicitly condition on covariates in the next derivations, I omit this conditioning to ease on notation.

treated:

$$\begin{aligned} S_1 \quad \text{states:} \quad & E\left[y_{0i}|s \in S_1,t\right] = \alpha_s + \alpha_t + \delta^{bans} \\ S_2 \quad \text{states:} \quad & E\left[y_{0i}|s \in S_2,t\right] = \alpha_s + \alpha_t \end{aligned}$$

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 TGW when they are jointly implemented, the outcomes if the implementation happens are:

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

Where δ^{bans} is the causal effect of the place-based policy on the outcome of interest, δ^{TGW} is the causal effect of the tobacco graphic warnings, and $f(\delta^{TGW},\delta^{bans})$ is the spillover effect of implementing the policies jointly. Then, the effect of the federal policy can be written as a function of the effect of each branch of the policy:

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

Let $P(s \in S_1) = \omega_1$ and $P(s \in S_2) = \omega_2$ thus :

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

Where ω_2 can be estimated as the proportion of states that have not implemented a place-based ban before the national regulation. Alternatively, ω_1 is the proportion of states that have implemented a place-based ban before the national regulation. Can the effect of place-based policies be told apart from the effect of tobacco graphic warnings? The effect of place-based bans is identified by the comparison of early adopters of place-based bans with late adopters. Early adopters imposed place-based bans before 2011, while late adopters imposed place-based bans once the 2011 national law was passed. Using the previous results:

$$\delta^{bans} + f(\delta^{bans},\delta^{tgw}) = \{E\left[y_{1i}|s \in S_2,t\right] - E\left[y_{0i}|s \in S_2,t\right]\} - \{E\left[y_{1i}|s \in S_1,t\right] - E\left[y_{0i}|s \in S_1,t\right]\}$$
$$\tilde{\delta}^{bans} = \{E\left[y_{1i}|s \in S_2,t\right] - E\left[y_{0i}|s \in S_2,t\right]\} - \{E\left[y_{1i}|s \in S_1,t\right] - E\left[y_{0i}|s \in S_1,t\right]\}$$

Where I have defined $\tilde{\delta}^{bans} = \delta^{bans} + f(\delta^{bans},\delta^{tgw})$.
The effect of the national policy can be now written in terms of $\tilde{\delta}^{bans}$:

$$\begin{aligned}\delta =& \delta^{tgw} + \omega_2 \left[\delta^{bans} + f(\delta^{bans},\delta^{tgw})\right] \\ \delta =& \delta^{tgw} + \omega_2 \tilde{\delta}^{bans}\end{aligned}$$

Where δ, ω_2 and $\tilde{\delta}^{bans}$ can be estimated. That is, I can use the estimates of δ and $\tilde{\delta}^{bans}$ 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:

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

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:

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

This regression can be augmented by leads and lags of relative treatment time in an event-study framework:

$$y_i = \sum_{\tau=-2}^{1} \tilde{\delta}_\tau^{bans} \left[\text{Bans}_s \cdot (\text{Years after treat} = \tau)\right] + \alpha_s + \alpha_t + u_i \tag{5}$$

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