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

Victoria Barone *
UCLA

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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

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

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, tobacco use is the second most important risk factor, responsible for about 11% of deaths. Moreover, 80% of smokers worldwide live in low or middle-income countries. Preventing people from smoking and reducing their exposure to environmental tobacco smoke is a key public health priority of many governments.

Smokers impose negative internalities on themselves through decisions that fail to maximize lifetime utility. For example, present-biased smokers fail to place enough weight on the future health consequences of smoking. Smith et al. (2001) estimate that heavy smokers are more optimistic about their self-assessed longevity than their smoking behavior would warrant. Smoking creates direct externalities through second-hand smoke and fiscal externalities through the worse health, increased medical needs, and reduced productivity of smokers. Alcaraz et al. (2016) estimate that smoking is responsible for 13.2% of deaths in Argentina with a direct cost equal to 0.75% of GDP or, equivalently, 7.5% of the 2015 federal health budget. To curb tobacco consumption, governments have implemented a variety of anti tobacco policies. Although tobacco has long been taxed and regulated, the scope and extent of such regulations have increased markedly in the 21st century (DeCicca, Kenkel and Lovenheim, 2020), and a broader set of regulations, including advertising bans, public-smoking bans, and restrictive sales practices, have become increasingly popular (Blecher, 2008).

In this paper, I examine the effect of non price interventions on smoking habits and health outcomes. I do so in the context of the most ambitious anti tobacco law passed by the Argentinean federal government in 2011. This law banned smoking in public spaces, including bars and restaurants, and in public and private workplaces. It also regulates packaging design by incorporating tobacco graphic warnings (TGW) about the risks and side effects of tobacco consumption. I use individual-level data on smoking and drinking behavior to study the effect of this non price intervention on consumption habits and state-level data on hospitalizations by diagnosis to assess the impact of the law on health outcomes. My identification strategy relies on state-level differences in the strength of regulation on tobacco products before the national law

[1] WHO, 2009

was passed. Argentina is a federal country; as in the US, states have *some* autonomy to implement different regulations. Before 2011, states differed in their leniency regarding tobacco regulation, I exploit this regional variation to identify the causal effect of the non price intervention.

By 2016, at least 105 countries had begun requiring graphic warnings to be printed on packages.[2] Well-designed package warnings are a highly cost-effective means for increasing awareness of smoking's effects health.[3] A one-pack-per-day smoker is potentially exposed to graphic warnings up to 20 times a day (Hammond, 2011). The rationale for this attribute-based product regulation relies on making tobacco products less attractive and could effectively reduce the demand of price-inelastic consumers that value non-price attributes. Measures of the effectiveness of this type of advertising have relied on laboratory experiments on the impact of warning labels (Byrne et al., 2015). The measures considered include desire to quit, knowledge of tobacco's health risks, and ability to recall the messages. Although these measures may predict future behavior, research based on actual consumption behavior is limited (Hammond, 2011; Azagba and Sharaf, 2012). Research design challenges make it difficult to quantify graphic warnings' effectiveness. The regulations typically are enacted at the national level, so studies have not been able to estimate state-level difference-in-difference models (DeCicca, Kenkel and Lovenheim, 2020). I overcome these identification challenges by exploiting state-level differences in the strength of regulation on tobacco products before the graphic warnings were implemented in an event-study design.

Several studies have examined the impact of smoking restrictions on cigarette demand in the United States and elsewhere (see Chaloupka and Warner, 2000; Carpenter, Postolek and Warman, 2011; and DeCicca, Kenkel and Lovenheim, 2020, for a review of these studies). North-America based research shows mixed impact of smoking bans on smoking behavior, while European based research suggests a systematic reduction of small magnitude in smoking behavior. In this paper, I study the effectiveness of smoking restrictions in the context of developing countries using individual-level data and exploiting state-level variation. I find a reduction in smoking participation of 6.17

[2]Reported by the Canadian Cancer Society annual report, 2016. 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). See also Byrne et al. (2015).

[3]Guidelines to implement Article 11 (packaging and labeling) adopted under the WHO Framework Convention on Tobacco Control (FCTC)

percentage points, this result is stronger in magnitude than research from the 2005 Italian ban (Buonanno and Ranzani, 2013 and Pieroni et al., 2013). Differences in estimates may be driven by the local context of smoking behavior: how popular and accepted smoking is, whether is strongly associated with other behaviors, the accessibility to smoking cessation programs among others. My results are similar to Abascal et al. (2012), they find that Uruguay's comprehensive nationwide anti smoking campaign was associated with a substantial decrease in tobacco use. During 2005–2011, per capita, cigarette consumption decreased by 4.3% per year. Importantly, Uruguay and Argentina are culturally similar countries, but Argentina had higher smoking rates. [4]

The question of whether alcohol and tobacco are complements or substitutes is crucial for determining the side effects of tobacco control policies. If cigarettes and alcohol are substitutes, policies implemented to reduce smoking may create adverse health effects due to increases in alcohol consumption. Whether alcohol or marijuana are substitutes to or complements with tobacco is an open question in the literature: cigarettes can be a gateway to consuming alcohol (or other drugs), or cigarette policies (e.g. taxation) can induce substitution (DeCicca, Kenkel and Lovenheim, 2020). Empirical research has addressed this issue by estimating demand systems for alcohol and tobacco and subsequently calculating cross-price effects. Tauchmann et al. (2013) using German survey data and an instrumental-variable strategy, estimate that a reduction in tobacco consumption results in a moderate reduction in alcohol consumption. Using individual-level data from the Behavioral Risk Factor Surveillance System, Decker and Schwartz (2000) suggests that higher cigarette prices tend to decrease smoking participation but increase drinking. Shrestha (2018) studies the effect of increases in cigarette prices following the Master Settlement Agreement in 1998, on alcohol consumption among 18-to-24-year-olds and finds a substitution relationship between alcohol and cigarettes.[5] The literature on substitution behaviors have mainly exploit variation in tobacco prices induced by taxation or other policies (such as the MSA). This paper is the first to study the effects of a non price variation in the costs of smoking on the consumption of alcoholic beverages.

[4]Smoking prevalence for individuals older than 15 years old was 23% in Uruguay by 2011 (World Development Indicators) and 28% in Argentina during the same year (Argentina's National Survey on Risk Factors).

[5]The Master Settlement Agreement (MSA) was an agreement whereby forty-six states and the four largest tobacco companies resolved these states' legal claims against these cigarette manufacturers for smoking-related expenses incurred via their Medicaid programs. The original settlement called for payment of roughly $200 billion over a twenty-five year period. (Levy and Meara, 2006) estimate that the MSA led to a roughly forty-five cent increase in cigarette price.

I find three key results. First, the anti tobacco regulation effectively reduced smoking; the probability of being a current smoker decreased by 6.17 percentage points in the people aged 18 to 65, relative to a prevalence of 28.25% in 2011, a sharp 22% reduction. Also, I find that the cumulative distribution of daily cigarettes smoked has shifted to the right, i.e. after the policy, it is less likely that smokers consume fewer than 15 cigarettes per day. Thus the reduced probability of being a current smoker is, at least partially, driven by quitters whose consumption was less than a pack per day. Second, 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, e.g., tobacco and alcohol. Nonetheless, tobacco and spirits have a substitution relationship on consumption, pointing to an unintended negative effect of tobacco regulation. Third, the law's effects on extensive and intensive margins 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.

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 TGW. Many countries implement anti tobacco policies simultaneously, e.g., in 2001, the Canadian Federal Tobacco Control Strategy proposed raising tobacco taxes, in addition to the incorporation of TGW (Health Canada, 2002).[6] I advance the literature and propose a framework to disentangle the effect of place-based bans from the effect of tobacco graphic warnings. To isolate the effect of the place-based bans, I compare early adopters of place-based bans with late adopters. This set of results helps to understand what is the role of place-based smoking bans and of tobacco graphic warnings on the extensive margin

[6]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). Other examples of joint implementation of policies are: Taiwan Tobacco Hazards Prevention Amendment Act in 2009 extended smoke-free areas to almost all enclosed workplaces and public places, added graphic health warnings to cigarette packages, and banned tobacco advertisement, promotion, and sponsorship (for details see, Chang et al. (2010)); Spain's anti smoking law in 2011 banned smoking on hospitality venues and selected outdoor areas (for details see, Sureda et al. (2014)).

outcomes. I find that tobacco graphic warnings play an important role in explaining the increase in the proportion of never smokers. The change in the proportion of current smokers is explained by both components of the policy; roughly 40% can be attributed to place-based bans and spillover effects arising from join implementation of the policies, while the remaining effect can be attributed to tobacco graphic warnings.

The rest of this paper is structured as follows. Section II presents background information on tobacco regulation in Argentina. Section III describes the data. Section IV presents the research design and my empirical strategy. Section V presents the main effects of the national regulation on extensive and intensive margin outcomes; and how these effects vary across populations. Section VI studies the interaction between the non price variation in the cost of smoking and abusive alcohol consumption. Section VII examines the effects on health outcomes highly correlated with smoking, e.g. COPD, asthma, and lung cancer. Section VIII discusses the mechanisms through which the law operated. Section IX 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.

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 leg-

islation index, which is a discrete and bounded index that summarizes the state-level regulation.[7] 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 advertising in public spaces, sport events and other venues, indoor smoking bans apply to 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 unit increases do not reflect a specific change in regulation. The data construction section in the appendix B. provides details on the criteria and definitions I used.

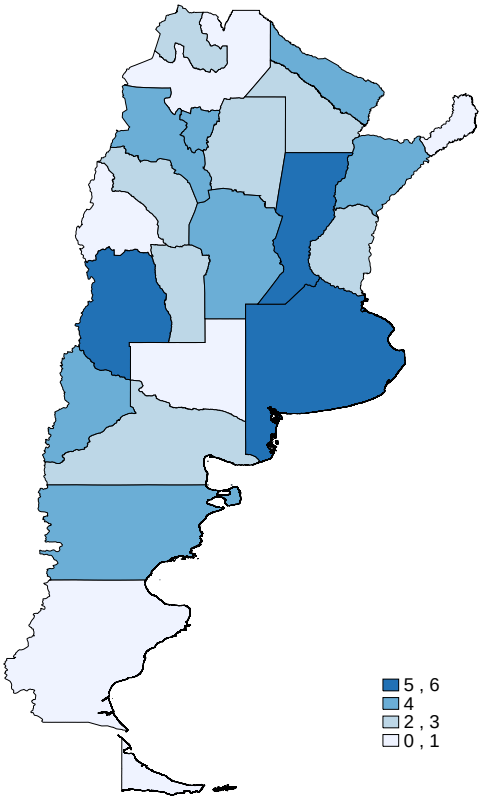
The 2011 law impacted the regulation of tobacco products in every state with different intensity. For example, in a state where only sales to minors was banned (a state with legislation index equal to 0), the national law introduced place-base bans, advertising limitations and the graphic warning. On 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.

Figure 1 shows the regional variation on regulation using the legislation index in 2009, I exploit this regional variation to identify the causal effect of the national policy. 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 and provide evidence on the empirical question of whether there is a tipping point in the level of regulation of tobacco products. Unfor-

[7] 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)

tunately, 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. Section IV discusses the implementation of this research design.

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. 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 contruct an extensive margin outcome, 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 the 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, restaurants and bars are where social smokers are most 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 picture similar trends 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.[8] 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

[8]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.

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.[9] 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.[10] These data come from the Argentinean National Center for Health Statistics and are provided at the hospitalization-level.[11] 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 speak to the 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.

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

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

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

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.

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. 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. 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.[12] 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 three years before and two years after the regulation was implemented:

[12]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 E.) and arrive to similar conclusions .

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

where s indexes the state where individual i is observed in year t, y_i is the outcome of interest. The variable $Years\ after\ treat = \tau$ is equal to the difference between the calendar year and the year in which the national law was passed and it is interacted with 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.[13]

My identifying assumption is that comparison states were on the same trend as treated states; that is, in absence of the national legislation, lenient states would have been on the same trend as strict states. 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?*

The research strategy of this paper is to compare states who have a strong regulation before the national law was passed to states with more lenient regulation. For this comparison to be valid, strict and lenient states need to be comparable in terms of the observable and unobservable characteristics. In order to better understand why some states chose to regulate tobacco more than others, I estimate a cross-sectional re-

[13]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.

gression at the state level in which I regress the probability of having enacted strong regulation before 2011 on a set of covariates:

$$Strict_s = \lambda_0 + \lambda_1 Peronist\ party_s + \lambda_2 Tobacco\ producer_s + \lambda' X_s + \nu_s, \tag{2}$$

where $Strict$ equals 1 if state s is strict state. I include an indicator for the political affiliation of the state government that indicates if the Peronist party was in power when the state regulated tobacco. The Peronist party was responsible of enacting the 2011 National Law; thus, the coefficient λ_1 tests whether the Peronist party is more likely to regulate tobacco consumption at the state level than the opposition parties. The variable $Tobacco\ producer$ indicates if the state produces tobacco, historically tobacco production is concentrated in seven states.[14] One would expect that states where tobacco is produced could be more lenient since regulation aim at curbing tobacco consumption could feed back into reduction in taxes and employment in the tobacco industry; also, these states could have been more exposed to the lobby of tobacco producers affecting regulation decisions.[15] The vector X_s includes measures of prevalence of tobacco consumption and prevalence of hospitalizations due to COPD as well as demographic characteristics; such as, population, gender and age composition, and a measure of private employment.

B. *Did State Governments Enforce the 2011 National Law Differently?*

Differential enforcement efforts can constitute a threat to identification if states more concerned about the harmful effects of smoking devote more resources to law enforcement and this additional effort differs across comparison and treated states. In particular, if all the comparison states, devote more effort than more lenient states the estimates would represent a lower bound for the effect of the national policy. If the direction of the selection is the opposite, i.e. if more lenient states devote more resources to law enforcement, the estimates would represent an upper bound for the effect of the national policy.

[14] These states are Jujuy (36% of the country's tobacco production), Misiones (29%), Salta (25%), Tucumán (7%), Catamarca (1%), Corrientes (1%) and Chaco (1%).

[15] Ideally, I would directly control for the role of the lobby from the tobacco industry in different states, nonetheless these data is not available at the state level for Argentina.

C. *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.[16] 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 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.

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

V Effects of the 2011 National Law

In this section, I examine the effects of the bans on consumption and the regulation of advertising Argentinean Federal government in 2011 on (i) extensive margin outcomes, (ii) intensive margin outcomes, and (iii) heterogeneous effects.

A. Extensive Margin Outcomes

A reduction in the number of current smokers can be explained by an increase in the number of former smokers or a decrease in the number of new consumers. To explore these two channels; in Table 3, I present results on the probability of being a never smoker and the probability of being a current smoker. The estimates suggest that the national law was effective in reducing smoke initiation and in helping people to stop smoking. The comparison and treated states were on similar trends regarding cigarette consumption before the national law was passed. The estimates for $\delta_{\tau=-2}$ and $\delta_{\tau=-1}$ correspond to the coefficients for 2008 and 2009, respectively and these estimates are not statistically distinguishable from zero.

The probability of being a never smoker increased by 4.34 percentage points, relative to the average of 46.08% in 2011, a roughly 10% increase in this outcome. This effect is driven by individuals who were on the margin between smoking and not smoking and decided not to smoke. The probability of being a current smoker decreased by 6.17 percentage points, relative to the average of 28.25% in 2011, a sharp 22% reduction. This effect is driven by former smokers and potential smokers who have not taken up smoking. Thus, the estimated effect is a weighted average of the treatment effect for these two groups.[17]

[17] These weights are a function of the proportion of each group in the population. To compute these weights, I need data about the number of quitters. I observe a measure of intention to quit, but its use entails two main challenges: (i) intention to quit might be an expression of purpose but not an actual behavior of individuals and (ii) many smokers report that they have tried to quit at least once. For example, in 2009, 82% of current smokers reported that they have tried to quit smoking at least once in their lives. Hence, intention to quit is a noisy measure of quitters and does not provide a useful variation to identify the share of quitters in the population.

Table 3: Effects on Extensive Margin Outcomes

Probability of	Never smokers (1)	Current smokers (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.

B. Intensive Margin Outcomes

In this section, I ask whether there is a reduction in the number of daily cigarettes smoked. I use data on self-reported smoking intensity in the last month. A challenge in using this measure is that smokers tend to report factors of five when asked about their daily consumption. One reason for this rounding is that cigarettes are sold in boxes of 10 or 20 as single-unit sales are illegal. To overcome this limitation, I discretize the number of cigarettes, i.e., I generate bins (b) of consumption in five-unit increments up to 20, and 10-units increments for higher quantities.[18] For example, b_1 is a dummy variable that takes the value of one if the number of cigarettes smoked is between one

[18]The intervals are open to the left: (0,5] (5, 10] (10, 15] (15, 20] (20, 30] (30,40] and (40,50]. In table 4; 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.

and five.

As the new legislation increases awareness of the side effects of smoking, smokers might (i) no change their behavior, (ii) reduce their consumption without quitting smoking, (iii) effectively quit smoking [19] or (iv) increase their consumption. If the effect operates entirely through channel (ii), the cumulative distribution of smoked cigarettes would then shift to the left; since the proportion of individuals in the "low" bins would increase. If the effect operates entirely through channel (iii), the change in the cumulative distribution of smoked cigarettes would thus depend on the composition of the group of quitters. If quitters come from the group of smokers with relatively low consumption, the cumulative distribution of smoked cigarettes would shift to the right, as the proportion of individuals in the "low" bins would decrease.

Table 4: 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
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 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.

Which direction dominates in the intensive margin is an empirical question that I

[19] Some smokers might attempt to quit but fail; I can not speak to this mechanism from the survey data I used in this paper.

address next. I estimate equation (1) using an indicator for daily number of cigarettes in bin b as the dependent variable. 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 16 percentage points. This result suggests that the pool of smokers is more negatively selected. Hence, the reduction in the probability of being a current smoker is partially driven by quitters whose consumption was less than five cigarettes per day. [20]

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 3 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 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 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.

[20] 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.

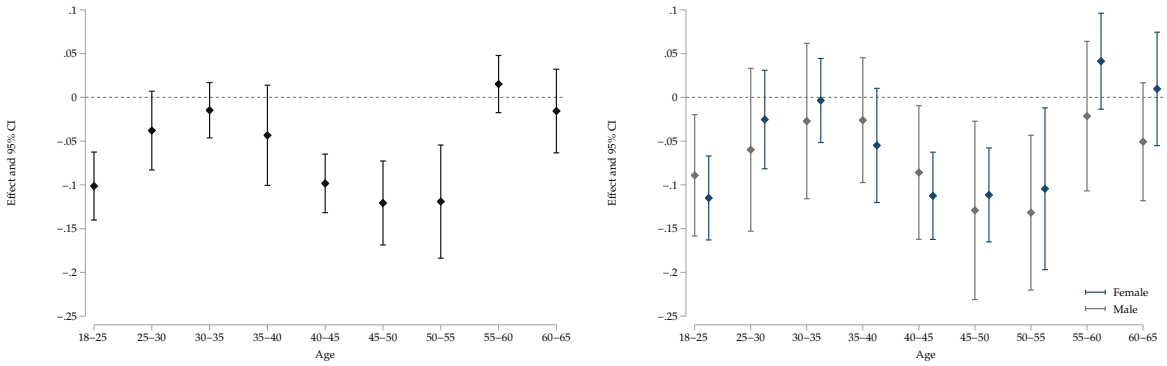
C. Heterogeneous Effects

Do the effects of stricter restrictions on tobacco consumption vary across populations? I address this question by analyzing the effects of the 2011 law on the probability of being a current smoker across populations. I estimate equation (1) restricting the sample by age group, gender, educational attainment, and household income. Figures 2 and 4 plot the estimates of the causal effect of the national law, i.e., the estimates for $\delta_{\tau=1}$ in equation (1) and the corresponding symmetric percentile-t confidence intervals. [21] Tables A5, A6, A7, and A8 complement these figures and present estimates of the remaining coefficients included in equation (1), estimates of $\delta_{\tau=-2}$ and $\delta_{\tau=-1}$ are statistically indistinguishable from zero .

The estimated effects on the probability of being a current smoker by age show an inverted-U shape pattern for ages 18 to 55 (panel (a), figure 2). Effects are statistically different from zero for individuals aged 18 to 25, and for individuals aged 35 to 55. This inverted-U shape is consistent with an increase in "never takers" among the youngest adults accompanied by 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. Thus, 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.

[21] Symmetric percentile-t confidence interval are construct as follows: define $\hat{\delta}_\tau$ as the point estimate of the parameter δ_τ, obtained employing OLS on equation (1), define $\hat{\delta}_\tau^{(b)}$ as the estimate of δ_τ from the bootstrap sample b. Then, the estimated confidence interval with confidence level α is: $\widehat{CI}_{boot} = [\hat{\delta}_\tau - \text{sd}(\hat{\delta}_\tau^{(b)}) \times q^*(1-\alpha/2), \hat{\delta}_\tau + \text{sd}(\hat{\delta}_\tau^{(b)}) \times q^*(1-\alpha/2)]$, where $q^*(1-\alpha/2)$ is estimated from a bootstrap simulation by sorting the bootstrap t-statistics and taking the $(1-\alpha)$ percentile. Symmetric percentile-t confidence intervals have the advantage of being centered in the point estimate of the coefficient of interest, whereas the confidence interval $\hat{CI}_E = [q^*(\alpha/2), q^*(1-\alpha/2)]$ proposed by Efron (1979) and widely used in the empirical practice, works poorly when the sample distribution of $\hat{\delta}_\tau$ is not symmetric about δ_τ (Hansen, 2018).

Figure 2: Extensive Margin Effects by Age and Gender



Notes: This figure presents point estimates and symmetric percentile-t confidence intervals (see footnote 21) of the causal effect of the national regulation on the probability of being a smoker in 2013 by subgroups. Estimates are constructed using a probability linear model (see equation 1) standard errors are block-bootstrapped at the state-level with 200 replications. Panel (a) presents results by age group and panel (b) presents results by age group interacted with gender. A complete set of results is presented in tables A5 and A6.

Adults aged 55 to 65 did not respond to the increase in the cost of smoking induced by the national law. The effect is statistically not different from zero, both for the entire population and for the subgroups by gender. I explore if the average consumption of the elder population is lower than their younger counterparts. To this end, I construct the cumulative distribution function of cigarettes per day (see figure A.5). At the threshold of 20 cigarettes per day consumption exhibits an age-sorting pattern as the cumulative distribution function (CDF) of older groups dominate the CDF of younger groups at this point and to its right. This sorting is less clear for lower levels of consumption.

I also explore an alternative explanation for the behavior of the elderly, 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.

As documented in table 2, women are less likely to smoke than men. In panel (b) of Figure 2, I explore whether the inverted-U pattern by age differs by gender. I estimate 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.e. $H_0 : \delta_{Female,\tau=1} = \delta_{Male,\tau=1}$. I do not find evidence to reject this hypothesis for any age group (see Table A6 for the respective p-value). These results indicate that although women are less likely to smoke, they are equally responsive to increases in the cost of smoking induced by the national law.

Cutler and Lleras-Muney (2010) study the relation between education and health behavior. Using data for the United States and the United Kingdom they find that each year of education is associated with a 3 percentage-point lower probability of smoking. A college graduate is 12 percentage points less likely to smoke than a high school graduate. Given that smoking is associated with a six-year shorter life expectancy (Cutler et al., 2002), this difference is immense. Following Cutler and Lleras-Muney (2010) approach, I estimate that the probability of smoking is negatively associated with education in my sample. Each additional year of education is associated with a 1.06 percentage point lower probability of smoking.[22] The smaller correlation indicates that smoking habits in developing countries are less linked to educational attainment, as the average rates of consumption are comparable across groups.[23]

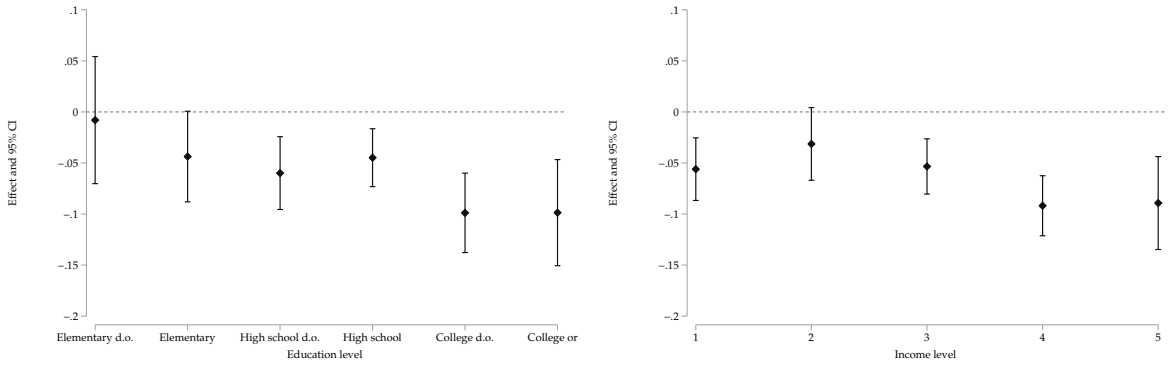
More educated individuals have been more responsive to the law, see panel (a) in Figure 4. Since the law's introduction, an elementary school graduate is 4.37 percentage points less likely to be a current smoker, while a college graduate is 9.86 percentage points less likely to be a current smoker. High-school completion is associated with an extra 5-percentage-point decrease in the probability of being a current smoker. Changes in the extensive margin can be explained, as argued before, by "never takers" of smoking as well as quitters. Consistently with the hypothesis of quitters, Cutler and Lleras-Muney (2010) find that better educated people are more successful at quitting smoking than less educated people, not because they try to quit more frequently or use different methods, but because they are more successful when they do try.

Cigarette taxes would be regressive with respect to income if poorer and richer consumers smoked at the same rate. This regressive effect is exacerbated when smoking prevalence is inversely related to income. Policies that change the costs of smoking but

[22] I run the following regression: $y_i = \beta_0 + \beta_1 EDUC_i + \Gamma' X_{is} + \alpha_s + \varepsilon_i$, where X_{is} is a vector of control variables that includes individual-level characteristics and α_s are state-level fixed effects to control for variation in outcomes across states that is constant over time. The estimate of β_1 is -0.0105 (0.0007) in 2008. Estimates for other years before the implementation of the national regulation are similar. This result should be interpreted as a simple correlation between the probability of smoking and educational attainment. Since I do not observe exact years of education, I convert the educational level observed to years of education using average school duration and rates of completion.

[23] Average rates of consumption for 2011 are reported in A7

Figure 3: Extensive Margin Effects by Education and Income



Notes: This figure presents point estimates and symmetric percentile-t confidence intervals (see footnote 21) of the causal effect of the national regulation on the probability of being a smoker in 2013 by subgroups. Estimates are constructed using a probability linear model (see equation 1). 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} quitile corresponds to the highest 20 % households in the income distribution. A complete set of results is presented in tables A7 and A8.

not the monetary price of cigarettes have the comparative advantage of not increasing the burden of consumption on poorer smokers. Bernheim and Rangel (2005) model of addiction explicitly cautions against strategies that tend to magnify economic burdens on those who become addicted, and underscores the benefits of policies that reduce these burdens. I explore heterogeneous effects by income in Panel (b) of Figure 4. In line with the previous findings on the education gradient, I document that individuals in richer households were more responsive to the new regulation. An individual who belongs to the highest income quintile was 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). Hence, the policy does not entail a regressive component, e.g. an increase in prices via taxation, but the benefits are higher for higher-income individuals.

VI Alcohol Consumption

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 they are substitute goods, 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 alchol consumption from a price policy exploiting changes in cigarette prices (e.g. Decker and Schwartz, 2000; Tauchmann et al., 2013; Shrestha, 2018) Argentina's 2011 law uses a non price policy inducing an increase in the cost of smoking. In this paper, I study the effect of an increase in the non monetary cost of smoking on abusive consumption of alcoholic beverages.

To study whether cigarettes ad alcohol are substitutes or complements, I estimate equation (1) using a Probit model. The outcomes of interest are constructed as follows: an indicator for heavy drinking of beer, an indicator for heavy drinking of wine, an indicator for heavy drinking of spirits, and an indicator for binge drinking. Heavy drinking or alcohol abuse is defined as a function of consumption per day: eight or more drinks of beer, five or more drinks of wine and three or more drinks of spirits.[24] 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. The sample is restricted to current smokers who have reported drinking alcohol in the last month. I also present results on alcohol consumption during the last month to characterize the frequency of this behavior.

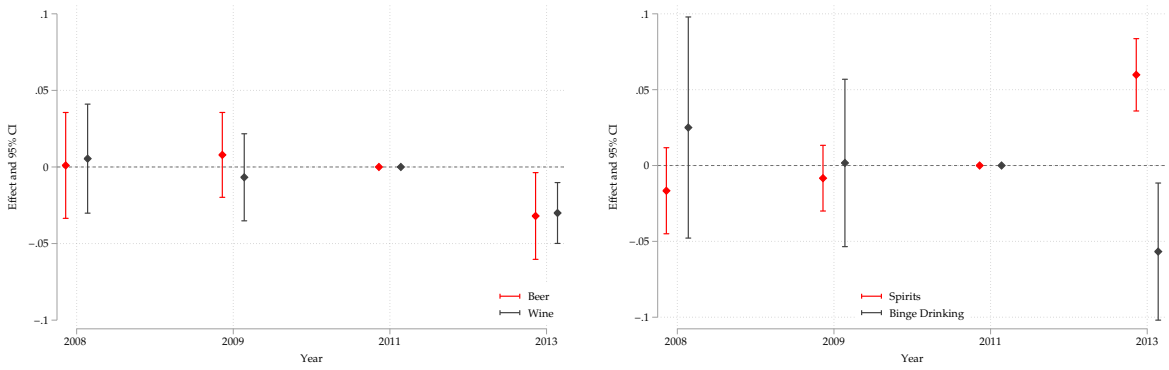
Figure 4 reports estimates of the marginal effects and Appendix Table A9 presents the estimates of the (raw) coefficients and the marginal effects. Marginal effects can be interpret as the percentage point change in the relevant outcome relative to the average level of such outcome. Standard errors are clustered at the state level.

The estimated coefficients for 2008 and 2009 are statistically indistinguishable from zero; they correspond to estimates of parameters $\delta_{\tau=-2}$ and $\delta_{\tau=-1}$ in equation (1). The null effects in the pre treatment period provide evidence 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 wine (and tobacco and abusive beer) consumption have a complementary relationship. Tobacco consumption and binge drinking also change in a direction that suggests a complementary relationship. However, tobacco and spirits have a substitution relationship. I document that a decrease in state leniency on tobacco regulation, effectively reduced smoking and induced a decrease

[24] Spirits include vodka, gin, tequila, rum, and whiskey.

Figure 4: Effects on risky consumption of alcoholic beverages



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. Estimates are constructed using a probit model (see equation 1). Standard errors are clustered at the state level. The complete results are presented in Table A9.

in abusive consumption of beer (3.20 percentage points), and wine (3.00 percentage points), and in binge drinking (5.67 percentage points). 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. Hence, an unintended effect of the tobacco regulation was an increase in abusive consumption of spirits.

Why are spirits substitutes with smoking while wine and beer are complements? One potential explanation is that smokers who drink spirits are different than the rest of smokers who abuse alcohol, at least on observable demographic characteristics. To illustrate this, I present conditional sample means of observable characteristics in Appendix Table (A10). 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. 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.

VII Health Outcomes

In this section, I ask whether the effects on extensive-margin outcomes, i.e. a reduction in the probability of being a current smoker and the increase in the probability of being a never smoker are in turn reflected in better health outcomes in the short run. I use data on the number of hospital discharges reported, by cause of diagnose at the state-age-group level from 2008 to 2014 to compute the prevalence rate of hospitalization by diagnosis.

Though smoking can potentially damage nearly every part of the human body, I limited the analysis to those health outcomes that exhibit a high correlation with smoking behavior. Smoking can cause lung disease by damaging the airways and the small air sacs (alveoli) found in the lungs. Cigarette smoking causes most cases of lung cancer. Lung diseases caused by smoking include COPD, which includes emphysema and chronic bronchitis. Estimates from the U.S. by the Department of Health and Human Services[25] indicate that smoking causes about 90% of all lung cancer deaths, and 80% of all deaths from COPD. Alcaraz et al. (2016) estimate that in 2015 in Argentina, cigarette smoking caused 75% of COPD deaths and 82% of lung cancer deaths, while 33% of other cancers could be attributed to cigarette smoking.

The outcomes of interest are defined 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. That is, the prevalence rate of diagnose c for age group a. [26] I estimate the effects of the clean-indoor-air policy and the introduction of TGW via the following equation, which I run over the sample of treated and comparison states for the period 2008-2014 by cause of discharge c for individuals aged 18 to 65 years as the main specification.

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

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

[25] See DHS, *The Health Consequences of Smoking—50 Years of Progress: A Report of the Surgeon General.*

[26] I compute the prevalence rates by age group since population estimates for the period of analysis are not available by single ages.

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.[27] 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 5 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. These effects are statistically different from zero at a 10% level of significance. The estimated coefficients indicate a negative gradient, as the effect for each year after the national law was passed is greater in absolute value than the previous one. Unfortunately, the discharge data is not available to analyze a longer term and study if these results fade out.

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

Table 5: 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.

I find a reduction in the number of hospitalization for these diagnosis. 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. Thus, these positive results on health outcomes must be taken as suggestive of an improvement on population health correlated with a more strict legislation of tobacco product.

Causally linking anti smoking policies to health outcomes is subject to the effects of potential confounders. The proposed specification purges the temporal trend in hospitalizations that is constant across states. Nonetheless, estimates are susceptible to secular trends in hospitalizations or correlated shocks from other policies or macroeconomic conditions. I propose two placebo tests.

VIII Bans or Tobacco Graphic Warnings: what works best?

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 TGW. Many countries implement anti tobacco policies simultaneously, e.g., in 2001, the Canadian Federal Tobacco Control Strategy proposed raising tobacco taxes, in addition to the incorporation of TGW (Health Canada, 2002).[28] Also, graphic warning labels have often been enacted as part of broader anti-smoking campaigns DeCicca, Kenkel and Lovenheim (2020). I advance the literature and study not only the effects the bundle policy, but also the effects of each policy separately.

The joint implementation setting poses two empirical challenges to the identification. The first challenge originates in the core of joint implementation; policies are implemented at the same time or within a short period which limits the opportunity to exploit either regional or temporal variation. The second challenge refers to the presence of spillover effects or complementarities between policies. Complementarities arise as policies provide incentives towards the same goal; deter individuals from smoking. The rationale of the joint implementation is to maximize the potential of each policy, i.e. the government's prior is that the combination of policies should *at least* be as effective as the individual or sequential implementation. In this section, I proposed a framework to disentangle the effect of place-based bans from the effect of tobacco graphic warnings.

To overcome the empiral challenges, I propose the following framework. Let the parameter δ be the causal effect of the national regulation identified of the comparison of lenient and strict states under the parallel trend assumption. Define two groups of states, those states who have implemented place-based bans before 2011, the early adopters of place-based bans, and those states with no (or very lenient) place-based bans before 2011, these are the late adopters states. I show in Appendix I., that this

[28] 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). Other examples of joint implementation of policies are: Taiwan Tobacco Hazards Prevention Amendment Act in 2009 extended smoke-free areas to almost all enclosed workplaces and public places, added graphic health warnings to cigarette packages and banned tobacco advertisement, promotion, and sponsorship (for details see, Chang et al. (2010)); Spain's anti smoking law in 2011 banned smoking on hospitality venues and selected outdoor areas (for details see, Sureda et al. (2014)).

parameter can be written as a linear combination of the effects of each branch of the policy and the spillover effects:

$$\delta = \delta^{tgw} + \omega_2 \, \delta^{bans} + \omega_2 \, f(\delta^{tgw},\delta^{bans}) \tag{4}$$

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 on the outcome of interest, ω_2 is the proportion of states that have not implemented a place-based ban before the national regulation and $f(\delta^{tgw},\delta^{bans})$ is the spillover effect of implementing the policies jointly.

Can the effect of place-based policies be told apart from the effect of tobacco graphic warnings? I exploit the comparison of early adopters of place-based bans with late adopters to identify the effect of place-based policies. Under the assumption that the early adopter are a good comparison for late adopters, the causal effect of place-based bans could be estimated via the following regression:

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

Where I use the notation $\tilde{\delta}^{bans} = \delta^{bans} + f(\delta^{bans},\delta^{tgw})$ to emphasize that the parameter being estimated is the effect of bans plus the potential spillover due to join implementation. $Bans$ is a dummy variable that equals 1 if the state s is a late-adopter of place-based bans. 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.[29]

Under the assumption that effects of bans and tobacco graphic warnings are constant across states and time and allowing for a spillover effect when these policies are jointly implemented, the effect of tobacco graphic warnings can be inferred from the following relation:

$$\delta = \delta^{tgw} + \omega_2 \tilde{\delta}^{bans} \tag{6}$$

[29]There is no empirical evidence on the sign of these spillover effects. I hypothesize that, if different from zero these spillover effects are positive as policy-makers are devoting efforts to both

Estimates of δ and $\tilde{\delta}^{bans}$ can be used to construct estimates of the effect of tobacco graphic warnings. I present the results of this exercise in table VIII. This set of results helps to understand what is the role of place-based smoking bans and of tobacco graphic warnings on the extensive margin outcomes. The change in the proportion of never smokers due to the national law is explained mainly by the incorporation of tobacco graphic warnings. The change in the proportion of current smokers is explained by both components of the policy; roughly 40% can be attributed to place based bans and spillover effects arising from join implementation of the policies, while the remaining effect can be attributed to tobacco graphic warnings.

Table 6: Mechanisms

Policy	National Regulation (δ)	Place-based Bans ($\tilde{\delta}^{bans}$)	Graphic Warnings (δ^{tgw})
Probability of			
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 (6). The first column reproduces results presented in 3. Standard errors are block-bootstrapped at the state-level with 200 replications.

The small effects of place-based bans on the proportion of non-smokers smokers reflects that non-smokers do not face the cost of place-based bans, i.e. non-smokers do not find themselves being displaced outside venues to smoke. My results suggest that tobacco graphic warnings are effective to deter smoking initiation, as most of the effect of the probability of being a never smoker is explained by these policy. But, how often are non-smokers exposed to these warnings? The 2013 National Survey on Risk Factors (ENFR) asked individuals if they have seen graphic warnings on cigarettes boxes on the last 30 days. A high proportion of the non-smoking population reported having seen the graphic warnings; among never smokers, 73% have seen graphic warnings. Experimental results from several studies find that labels elicit emotional responses, such as fear or disgust (DeCicca, Kenkel and Lovenheim, 2020). My results suggest that these emotional responses might be particularly effective to deter people from

smoking, probably because the warnings make the health's costs more salient to individuals who are not addictited to nicotine and who have not enjoyed the ritual of smoking and the physiological reactions to nicotine to *compensate* the costs related to smoking.[30] Also, anecdotal evidence suggest that smokers could avoid exposure to these warnings by purchasing cigarette cases, and might also develop some resistance to these warnings.[31]

Place-based bans and graphic warnings were effective in reducing the proportion of current smokers. Among current smokers, 96% reported that they have seen graphic warnings on cigarettes boxes. When asked if these images have made them consider quitting smoking, 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. Although these results show a promising effect of tobacco graphic warnings, there are still open questions that would help to better understand how tobacco graphic warnings work. The main two being: (i) do the effects of graphic warning wear off in the medium or short term and (ii) which of the warnings are more effective?. I provide estimates that are from consumption behavior in the short run. In particular, I look at individuals a year after the graphic warnings were printed. An advantage of the setting that I exploit is that individuals are (almost) randomly assigned to graphic warnings. The Argentinean law determine which warnings have to be printed and manufacturers did not have power to adapt the warnings to reduce the effectiveness. Also, cigarette boxes are usually sell at convenience stores where consumers can not directly access the box but have to ask for a box from the cashier. Thus, consumers have 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. [32]

[30] Nicotine is highly addictive. When a person uses tobacco, nicotine enters the body and activates nicotine receptors in the brain. The faster the delivery of nicotine to the body, the greater the addictive effect on the brain. Cigarettes have been designed to give high levels of nicotine to the brain very quickly. 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)

[31] Tobacco Graphic Warnings' laws usually regulates the packaging in a very detailed way, e.g. providing exact measures, places and pictures to be printed in the boxes. Also, these laws usually include frequent updates in printed pictures to mitigate the novelty of these pictures to wear off.

[32] On the one had, the ideal data to answer this question would link consumers' behavior with the actual warning they are more exposed to. On the other hand, if manufacturers distribute cigarette boxes with some warnings strategically in different locations estimates might be biased. There is no anecdotal

IX 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. Through the lens of this model, public-place smoking restrictions and regulation of advertising, especially tobacco graphic warnings, are promising policies for decreasing smoking prevalence. This research helps to broaden our understanding of the effectiveness of such policies in developing countries and supports the effectiveness of these policies as suggested by Bernheim and Rangel (2005).

I interact previous state-level legislation with the national law to identify the effect of bans on advertising and the implementation of clean-indoor-air policies by the national government. The probability of being a never smoker increased by 4.34 percentage points, relative to the average of 46.08% in 2011, a roughly 10% increase in this outcome. This effect is driven by potential smokers who decided not to smoke. The probability of being a current smoker decreased by 6.17 percentage points, relative to the average of 28.25% in 2011. Intensive margin results suggest that the distribution of daily cigarettes smoked shifted to the right, i.e. it became more likely that smokers would consume more than 15 cigarettes per day and less likely that they would consume fewer than 15 cigarettes per day. Thus, the pool of smokers is more negatively selected and the reduction in the probability of being a current smoker is partially driven by quitters whose consumption was less than a pack per day.

Policies implemented to deter smoking might affect alcohol consumption, depending on the complementary or substitutability between these goods. This paper finds evidence that tobacco and abusive wine (and tobacco and abusive beer) consumption have a complementary relationship. Tobacco consumption and binge drinking also change in a direction that suggests a complementary relationship. However, tobacco and spirits have a substitution relationship on consumption, pointing to a unintended negative effect of the anti-tobacco policy.

Finally, the probability of being a current smoker shows an inverted-U shape pattern for ages 18 to 55, consistent with a decrease in youth initiation of tobacco use and

evidence of such a strategic response from the manufacturers.

an increase in former smokers among adults aged 35 to 55. Also, adults aged 55 to 65, were less likely to intend to quit smoking and did not respond to the increase in the cost of smoking induced by the national law. Though the national law was effective in reducing consumption, it might have also increased health inequality. I find that completion of high school is associated with an extra 5 percentage points decrease in the probability of being a current smoker and that people in richer households were less likely to be a smoker after the introduction of the national law. Hence, though the policy does not entail a regressive component, e.g., as an increase in prices via taxation, the benefits are higher for higher-income individuals.

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 TGW. Graphic warning labels have often been enacted as part of broader anti-smoking campaigns DeCicca, Kenkel and Lovenheim (2020). I advance the literature and study not only the effects the bundle policy, but also the effects of each policy separately. I ask, what is the role of place-based smoking bans and of tobacco graphic warnings on the extensive margin outcomes? I find a small effects of place-based bans on the proportion of current smokers and an important role of tobacco graphic warnings on detering smoking initiation. Place-based bans and graphic warnings were effective in reducing the proportion of current smokers. My results show a promising effects of tobacco graphic warnings. I provide estimates that are from consumption behavior in the short run. In particular, I look at individuals a year after the graphic warnings were printed. An advantage of the setting that I exploit is that individuals are (almost) randomly assigned to graphic warnings. Nonetheless, there are still open questions that would help to better understand how tobacco graphic warnings work. The main two being: (i) do the effects of graphic warning wear off in the medium or short term and (ii) which of the warnings are more effective?.

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A Appendix

A. Tobacco Graphic Warnings

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.

B. Legislation Index Construction

Argentina has 23 states and one federal district.[33] 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 TGW and inclusion of

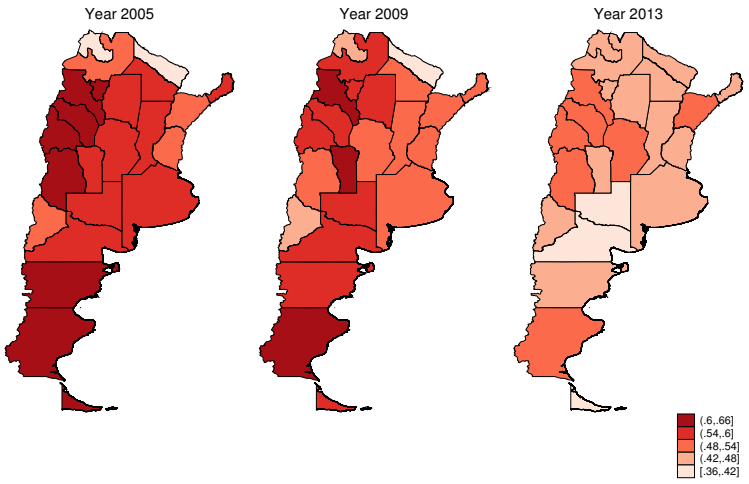
[33] I will refer to this administrative unit as another state since the distinction between state and federal district is not relevant for this paper.

contact information about anti-smoking public services. Sells category includes the next sub-categories [34] 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. 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. Evidence on law enforcement

Figure A.2: Exposure to Environmental Tobacco Smoke



*Notes:*Exposure to Environmental Tobacco Smoke is a self-reported measure. Each individual is asked if people regularly smoke around them in close venues such as: office, school, home. Thus, it measures the percentage of individuals expose to Environmental Tobacco Smoke in any indoor environment.

[34] All of them are defined as bans on sells in the described venue or establishment

D. Consumption trends and sales trends

Table A1: Evolution of 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.

E. Robustness Checks

E..1 Definition of the treatment

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 A2 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 A3. 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.

The following table presents the mean and standard deviation of individual characteristics for comparison and treated states when treatment is defined as a dichotomous variable that equal 1 if the legislation index for state s is strictly less to 3 before 2011. Estimates use pooled data from national surveys on tobacco use for 2008 and 2009.

I present results for the intensive margin in Table A4. 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 A2: 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 A3: Effects on Extensive Margin Outcomes

Probability of	Never smokers (1)	Current smokers (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 A4: Effects on Intensive Margin Outcomes

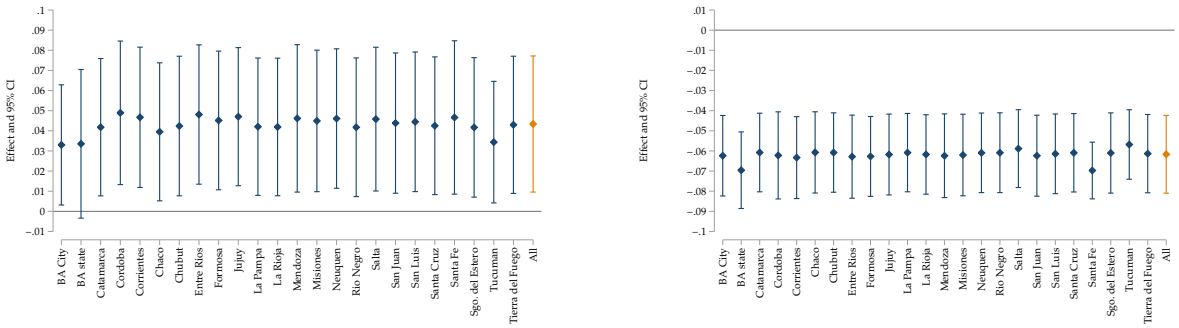
Cigarettes smoked	(0,5]	(5, 10]	(10, 15]	(15, 20]	(20, 30]	(30 , 40]	(40 , 50]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2008	0.0017	0.0237	-0.0033	0.0084	0.0051	-0.0100	0.0010
	(0.0317)	(0.0185)	(0.0104)	(0.0123)	(0.0067)	(0.0062)	(0.0011)
2009	-0.0839**	0.0394*	0.0020	-0.0182	-0.0111***	-0.0097*	0.0008
	(0.0370)	(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.

E..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 A.. 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

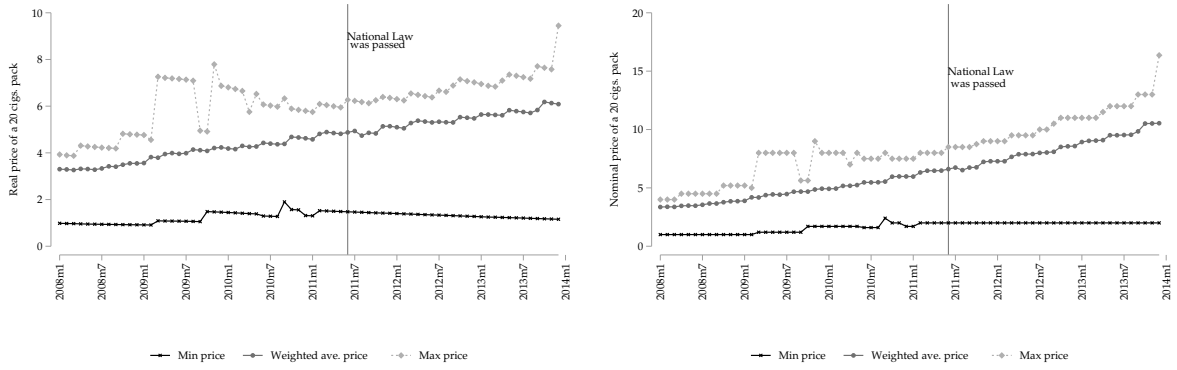
(b) Current Smokers

*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 3. Standard errors are clustered at the state-level.

E..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).[35] 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.

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.

[35] 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.

F. Heterogeneous effects

Table A5: 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

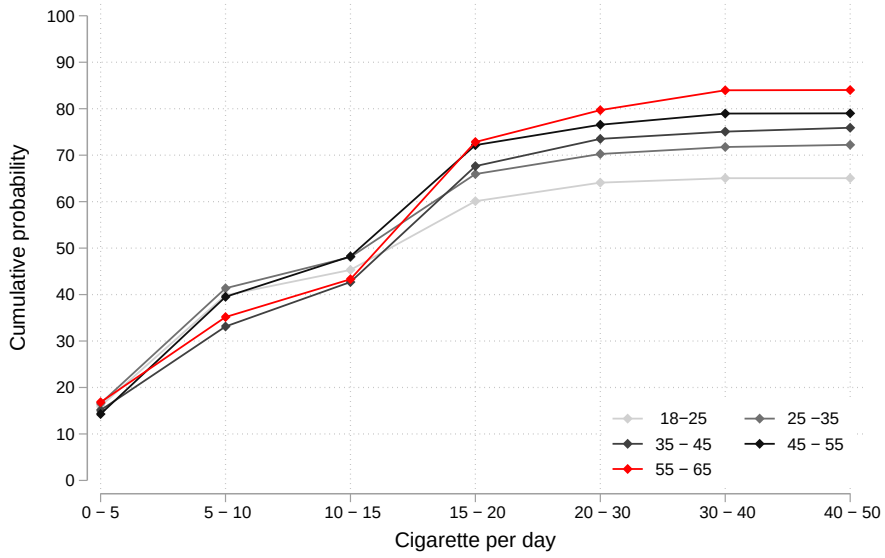
Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. After 2011, all states *are treated* thus $Treat_s$ 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 A6: 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.

Figure A.5: CDF of daily consumption by age group



Notes: Cumulative Distribution Function (CDF) of reported daily consumption of cigarettes in the last month by age group. Smokers tend to report factor of 5 integers when asked about their daily consumption. To overcome this rounding, I discretize the number of cigarettes, I construct open-to-the-left intervals of consumption: [0,5] (5, 10] (10, 15] (15, 20] (20, 30] (30,40] and (40,50]. For each interval, I compute the percentage of smokers of a given age that report a daily consumption in the interval values.

Table A7: Probability of being a current smoker by educational attainment

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

Notes: The omitted category corresponds to 2011, the year the federal law was passed. Treatment is defined as a dichotomous variable, $Treat_s$, that equals one if the legislation index for state s in moment t is less than or equal to 3 before 2011. 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 A8: 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.

G. Alcohol Consumption

Table A9: 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 A10: 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

H. Health outcomes

COPD

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.

Lung Cancer

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.

I. Appendix to what works best

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: [36]

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

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 treated*:

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

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:

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

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

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:

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

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:

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

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