The Effects of State-Level Legislative Smoking Bans in Private and Government Workplaces on Smoking Prevalence

by Katarina Michalyshyn

(7031525)

Major Paper presented to the
Department of Economics of the University of Ottawa
in partial fulfillment of the requirements of the M.A. Degree
Supervisor: Professor Abel Brodeur

ECO6999

Ottawa, Ontario
August 2018

Abstract: This paper examines the effects of state-level legislative smoking bans in private and government workplaces on smoking prevalence. Using data from the Behavioral Risk Factor Surveillance System and other sources, a difference-in-difference method is used. The main findings are that private and government workplace smoking bans are not consistently found to have an effect on whether a person is a smoker. However, when regressions are run on subsets of the population, or when the definition of smoker is changed to only include more intensive smokers, the coefficients for private bans, government bans, natural logarithm of real cigarette tax and bar bans are sometimes statistically significant. None of the effects are of large economic significance, and some have unexpected signs.

Section I: Introduction

Cigarette smoking is known to cause cancer and other health conditions. These health problems increase health expenditures, to the point where some governments have started suing tobacco companies for compensation for the health care expenditures incurred from smoking. Further, smoking harms not only the smoker themselves, but also those around them who are exposed to second-hand smoke. In fact, sometimes the person smoking will breathe in a lower amount of toxic substances compared to the amount of toxic compounds present in the second-hand smoke that they are producing (Government of Canada 2015). In Canada, more than 800 people who do not smoke die per year because of heart-related illnesses or lung cancer brought on by exposure to second-hand smoke (Government of Canada 2016). Because of the negative consequences on health and health care expenditures, governments have an interest in reducing the number of people who smoke. One initiative that governments have tried is to ban smoking in certain places, such as workplaces.

This paper looks at the effect of workplace smoking bans on smoking prevalence. It uses data from the 1990-2000 Behavioral Risk Factor Surveillance System (BRFSS), which is a representative cross-sectional survey of people living in the United States that is administered by the Center for Disease Control (CDC). Data about legislative smoking bans comes from the CDC's State Tobacco Activities Tracking and Evaluation (STATE) system (for 1995-2000) and from web searches. As well, data on cigarette taxes comes from the American Nonsmokers' Rights Foundation. A difference-in-difference approach is used.

The main results are that private and government smoking bans are generally not found to be statistically significantly related to the probability that a person is a smoker. The marginal effects of a probit regression confirm this. However, in linear regressions that decompose

the sample into subgroups based on sex and age group, some significant results are found. Private bans reduce the likelihood that a person is a smoker by 2.7 percent for men aged 35 to 54 and by 3.1 percent for women aged 18 to 34. These estimates are statistically significant at the 10% level. Among all people aged 55 and over, as well as among women alone of this age group, the natural logarithm of the real tax on cigarettes has a small, negative and significant (at the 5% level) effect. Bans on smoking in bars have small, positive and significant (at the 1% level) effects among men overall and men in the middle age category (35 to 54). A similar effect is found for males aged 18 to 34, but it is only significant at the 10% level. On the contrary, bar bans have a small, negative and significant (at the 5% level) effect on women in the middle age category.

A robustness check reveals that, when the definition of a smoker is changed so that only more intensive smokers are included, the private ban coefficients stay statistically insignificant but the government ban coefficients becomes statistically significant at the 5% level and are positive. The bar ban coefficients are all positive and statistically significant at the 1% level. This suggests that the effects of bans might be more effective for people who are more intensive smokers, but remain quite small.

My paper contributes to a growing literature on the effects of smoking policy on smoking behaviour (Adda and Cornaglia 2010; Bitler *et al.* 2010; Brodeur 2013; Carpenter *et al.* 2011; Cheng *et al.* 2017; Evans *et al.* 1999; Savage 2014; Tauras 2004; Tauras 2006). In the next section, I describe the methods and results found in the previous literature. The literature related to the effects of smoking ban legislation on smoking prevalence does not come to a consistent finding. I contribute to this literature by looking at the effects of private and public smoking bans and by using a synthetic control method.

The rest of this paper proceeds as follows. Section II provides a review of the literature. Section III presents the conceptual framework. Section IV includes the data and descriptive statistics. Section V describes the identification strategy. Section VI presents the results. Section VII shows two robustness checks. Section VIII concludes.

Section II: Literature Review

Empirical Papers

In what follows, I describe the methodologies and results of the empirical papers that are the most relevant to my work.

Evans *et al.* (1999) study the impact of smoking bans in workplaces on smoking prevalence. The data takes into account whether there is a ban in a workplace, but not whether it is due to smoking ban legislation or to an employer deciding themselves to impose a smoking ban on their workplace. The authors rely on the National Health Interview Survey (NHIS) and the Current Population Survey (CPS) and provide evidence of a drop of 5 to 6 percentage points in smoking prevalence, which is quite a large drop. In order to deal with endogeneity and omitted variables, the authors also employ an instrumental variables approach and come up with similar (although of a bit greater magnitude) coefficients for the influence of workplace bans on the proportion of the population that smokes.

Tauras (2004) looks at how the introduction of legislative smoking bans and increases in the cost of cigarettes influence smokers who smoke daily and smokers who smoke less regularly. Before this study, the literature had not looked much at smokers who do not smoke every day. The author relies on the NHIS (1991, 1993 and 1994) and uses a combination of probit equations and

generalized linear equations. Smoking ban laws that apply to private-sector workplaces or public locations have a smaller influence on smoking prevalence than do cigarette prices. Smoking laws that cover restaurants are associated with a drop in the smoking rate, while smoking laws covering shops increase the likelihood that a person smokes less regularly, instead of smoking daily. Further, if there is state-level legislation that prohibits lower levels of government from creating their own smoking restriction legislation, this is associated with a rise in the smoking rate.

Tauras (2006) looks at how legislation that limits smoking and how increases in the cost of cigarettes to smokers influence the smoking rate and the number of cigarettes that smokers smoke. Nine waves of the tobacco component of the monthly CPS from between 1992 and 1999 are looked at. This paper employs state and year fixed effects to take into account the fact that attitudes about smoking could differ between states and years, which was not done prior to this paper. The author finds no clear influence of smoking laws on the smoking rate, other than legislation that limits smoking in shopping centres, which is associated with a drop. Higher cigarette prices are associated with lower smoking rates and fewer cigarettes consumed per smoker.

Bitler *et al.* (2010) is one of the first papers to examine how workplace smoking restriction legislation changes the smoking prevalence of people employed at the types of workplaces that these laws impact. The data employed is the tobacco component of different waves of the CPS between the years of 1992 and 2007. The paper looks at workers who are under smoking bans while working at companies, the civil service, educational facilities, bars and restaurants. Using a difference-in-difference approach, the results are that a workplace smoking law is not associated with a lower smoking prevalence for any of these sectors of workers, except for those who work in bars.

Savage (2014) looks at the influence of a 2004 federal-level Irish legislative ban on smoking in workplaces that covers most workplaces that are inside of buildings. No other nation before Ireland had enacted this type of legislation at the federal level, making this an interesting policy to study the influence of. Employing a difference-in-difference method, the smoking rate of those who have jobs is determined to drop less than the smoking rate of those who do not have jobs.

Carpenter *et al.* (2011) look at whether the smoking prevalence and the second-hand smoke quantity that people experience change when smoking legislation changes in Canada. The data come from the master files of the Canadian Tobacco Use Monitoring Survey (2002-2008) and the Canadian Community Health Survey (2000-2008). Using a difference-in-difference approach, the paper's results are that the introduction of legislation restricting smoking in public locations does not influence the smoking rate or the degree to which smokers smoke. That said, the paper provides strong evidence that this legislation is associated with a drop in the amount of second-hand smoke experienced by people who frequent locations covered by the legislation. This drop is quite economically significant – drops of approximately 65% in bars and approximately 75% in restaurants – and is present among those who do not smoke, but also among those who do smoke.

Brodeur (2013) looks at how the smoking prevalence is influenced by county bans that cover workplaces, bars or restaurants. The author uses the BRFSS data from 1988 to 2010, which includes 3,751,652 observations. Using a difference-in-difference approach, the results are that county bans on smoking that cover workplaces, bars or restaurants are not correlated with a drop in smoking rates. These results hold even when the sample is decomposed by gender and age group.

Adda and Cornaglia (2010) look at how legislation that restricts and taxes cigarette smoking influences those who smoke and those who do not. This paper employs the cross-sectional BRFSS survey between 1984 and 2006 (3,215,506 individuals) and the panel National Health and Nutrition Examination Survey (NHANES) between 1988 and 1994 and between 1999 and 2006 (42,009 individuals), as well as some other data. They employ difference-in-difference, as well as triple difference, techniques. Specifically, they exploit differences in the times that smoking ban laws come into effect, differences in the geographical locations in which they come into effect, and differences in smoking behaviours on business days versus on weekends. They employ data about the percentage of the population that smokes and that quits smoking, about how people spend their time, as well as about the concentration of cotinine present in bodily substances. Cotinine is present in someone's blood or other bodily substance if they have been exposed to nicotine, which is in cigarettes and cigarette smoke. The results suggest that when smoking ban laws have been introduced that cover bars, restaurants and workplaces, those who do not smoke have experienced comparatively more second-hand smoke. The magnitude is especially notable among people who do not smoke but live in the same home as someone who smokes. The authors look at whether smoking ban laws push smokers to increase the amount of smoking they do in other settings in which people who do not smoke tend to be frequently present. By this logic, smoking bans would raise the amount of second-hand smoke that these non-smoking people would experience. The authors' data surrounding time spent in restaurants and bars following the introduction of a smoking ban suggests this prediction is correct. Laws that ban smoking do not significantly reduce the rate of smokers in the population, nor do they seem to push smokers to stop smoking or to try to stop smoking. On the other hand, if taxes on cigarettes rise, this lowers the amount of second-hand smoke that non-smokers experience. This paper looks at smoking ban legislation at the state,

county and municipal level. The cotinine measurements were done by medical professionals, so they are not prone to much measurement error compared to if a self-reported measure of second-hand smoke had been employed.

Cheng *et al.* (2017) examine how being under a legislative workplace smoking ban, whether it be from the municipality, county or state, or a combination of these three, influences the percentage of workers who smoke and the amount of second-hand smoke experienced by workers who do not smoke. The sample includes those who work inside of buildings. The paper employs the tobacco component of several waves of the CPS, ranging from 2001 to 2010, and the BRFSS data for 2000-2006. Using a method very similar to a typical difference-in-difference method, the results are that legislative workplace smoking bans are associated with a 12% lower smoking prevalence among workers and a drop of 28% in the amount of second-hand smoke that non-smoking workers experience.

Overall, the effects of smoking policies on smoking behaviour vary depending on the methodology and the dataset that a paper uses.

Theoretical Papers

My paper also relates to the literature about why people engage in smoking or quit smoking. By looking at the effects of legislative bans that cover different types of venues, I help shine light on the mechanisms that may push smokers to quit smoking or may prevent non-smokers from starting to smoke. These mechanisms will be further discussed in Section III.

Becker and Murphy (1988) propose a rational addiction framework, where rational people choose to engage with substances that they know they will become addicted to because doing so

will provide them with the greatest overall level of welfare through the course of their lives. Becker *et al.* (1994) find empirical evidence that individuals are rationally addicted to cigarettes.

However, Auld and Grootendorst (2004) find that if a paper's data is time-series, the typical econometric test used to determine whether individuals' consumption behaviour follows the rational addiction model is not always accurate. In the paper, the typical econometric test is run with cigarette data but also with data about oranges, eggs and milk. These last three substances are not considered as substances that individuals can become addicted to. The typical econometric test erroneously indicates that individuals' consumption behaviour for oranges, eggs and milk follows the rational addiction model. The paper finds that the typical econometric test, if the data is time-series, cannot dependably distinguish between a good that follows the rational addiction model and a good for which individuals consume serially correlated quantities. While the paper calls into question the validity of the typical empirical test that is employed to determine whether a good follows the rational addiction model, it does not question the validity of the rational addiction model itself.

Another theory suggests that smokers are myopic – that is, they do not realize (or do not take into account) that they will become addicted when they start smoking. Becker *et al.* (1994) suggest the myopic model as one potential addiction model, but then find results that discredit it. As well, if a person's preferences are time-inconsistent, they may begin smoking under the assumption that becoming addicted to smoking will increase their welfare (like in the rational addiction model), but once addicted, may wish they had never begun smoking (Hurley 2010).

Bernheim and Rangel (2004) construct an addiction framework in which addicted individuals often accidentally consume the item to which they are addicted. Previous consumption makes the person more predisposed towards elements of their surroundings which may provoke

accidental consumption. However, because these people are aware of this proneness, to a certain extent they exhibit nuanced behaviour.

Section III: Conceptual Framework

In this paper, the word "ban" is used to mean any smoking restriction, which includes: smoking restricted to designated areas, smoking restricted to separately ventilated areas, or a full ban. These varying degrees of smoking restrictions could mean that there are different levels of behaviour change among smokers. Presumably, the more restrictive bans would make it harder to smoke during work hours, and may therefore be more likely to cause a person to quit smoking or to not begin smoking. As well, a ban may increase the stigma surrounding smoking, which could push more smokers to quit and fewer non-smokers to begin smoking.

Banning smoking in some places might simply push smokers to substitute smoking in those places for smoking more in other places (Adda and Cornaglia 2010). By this logic, instead of pushing workers to stop smoking, a workplace ban may push smokers to smoke more in their own homes. This would subject other people in the smoker's household, including children, to more second-hand smoke. Thus, a workplace smoking ban would shift the negative externality of second-hand smoke from a person's colleagues to a person's household members, without necessarily reducing the size of the externality itself. As well, smoking bans at work could push smokers to instead smoke more at restaurants or bars, unless these locations have smoking bans (which is sometimes the case, especially for restaurants). This would subject other patrons to more second-hand smoke.

Along the lines of what Becker and Murphy (1988) propose with their rational addiction model, smokers may choose to smoke even if they know they will become addicted because consuming cigarettes gives them a higher current and future welfare level. If this is the case, making smoking less convenient to do during working hours would have a negative effect on smokers' welfare. Thus, the ban might lower the welfare associated with smoking. For some people, this might be enough of a drop in welfare for them to stop smoking or for a non-smoker to not start smoking.

Further, peoples' behaviour might follow what Bernheim and Rangel (2004) theorize. Smokers may be susceptible to triggers from their environment that increase the likelihood that they will then consume a cigarette. By this logic, a workplace smoking ban would reduce the exposure that smokers or former smokers have to stimuli that may push them to smoke. Therefore, smokers may find it easier to quit smoking, and former smokers may find it easier to not start smoking again.

As well, if a state bans smoking in government workplaces prior to banning smoking in private workplaces (many states have followed this pattern), it is possible that some government workers who are smokers will switch to jobs in the private sector following the introduction of a government workplace ban. If this happens, these smokers will continue smoking (at least until a smoking ban is instituted in their private sector workplaces), whereas if they were not able to leave their government jobs they would find it harder to continue smoking, and so they might be more likely to quit.

Section IV: Data and Descriptive Statistics

Most of the data for this paper comes from the BRFSS survey. This is an annual cross-sectional phone survey carried out in the United States by the CDC that asks for information about respondents' health, as well as information about demographics and socio-economic status. The respondents are all 18 and over. Individuals who live in an institution or are members of the military are not surveyed in the BRFSS. The BRFSS chooses respondents randomly, but because the people who actually end up being surveyed are not representative of the American population, the BRFSS uses weights. Thus, weighted summary statistics and regression results are representative of the American population. By the end of my study period the BRFSS collected data on all 51 states (50 states and District of Columbia), but there is no data available on all states for the earlier years of my study period. Therefore, there are more observations in the later years of my study period. For instance, in 1990 there were 81,557 individuals from 45 states, while in 2000, there were 184,450 individuals from 51 states and Puerto Rico. The raw dataset from 1990-2000 contained 1,340,241 individuals. After dropping observations from Puerto Rico and individuals with missing observations for any variables of interest, the dataset contained 1,098,405 individuals. However, individuals with extreme BMI values were dropped for the main regressions, bringing the final total to 1,080,866.

A large part of the data about smoking bans (private-sector workplaces, government workplaces, restaurants and bars) comes from the CDC's STATE system, which has data from 1995 onward. Data for previous years was found by searching on the internet for state legislation about smoking bans. In the midst of this search, I found that in some cases, the STATE data was inaccurate. When this was the case, I replaced the STATE observations with information I found online about when smoking ban legislation came into place. As well, the data about smoking bans specifies whether there is a complete ban, whether smoking is allowed only in separately ventilated

areas, whether smoking is allowed in designated areas, or whether there is no restriction on smoking at all. Because there is likely to be a fair amount of measurement error in this data due to laws sometimes being slightly different in different states, I construct a binary smoking ban variable that reflects whether or not there is any type of smoking restriction in a given type of place in a state. In the robustness checks I change the smoking variable slightly to see if there are different results. The data for the natural logarithm of real cigarette tax comes from the American Nonsmokers' Rights Foundation.

Like in many studies, Body Mass Index (BMI) is used as a proxy for whether a person is underweight, of normal weight, overweight or obese. In the main results, individuals whose BMI is extremely low (<13) or extremely high (>40) are omitted. This is because it is likely that many of these observations are erroneous and their presence could bias the results. A robustness check runs the regressions without excluding people with an extreme BMI value.

Table 1 presents the summary statistics. Smokers make up 23.8% of the population. The alternative definition of the smoker variable is used in a robustness check and will be discussed in the Section VII of this paper. The average natural logarithm of real cigarette tax is 2.913. Almost half of people (48.9%) are under a state private workplace smoking ban at some point, and 70.9% of people are under a state government workplace smoking ban at some point. State restaurant smoking bans cover 52.5% of people at some point, while bar bans only cover 7.3% of people at some point. Approximately 50% of the population is female (0.499). The racial decomposition is 77.7% white, 9.3% black, 9.2% hispanic and 3.8% other. For marital status, 60.5% of the population is married. Approximately a quarter of households have total incomes of $50,000 or more. There are almost the same proportions of people aged 18 to 34 (35.7%) as there are people aged 35 to 54 (37.4%). Approximately two thirds of people are working (65.7%). A little over a

third (36.2%) of people are overweight, while 15% are obese and only 2.4% are underweight. In terms of education, 32.7% of people have just high school or GED, while approximately the same proportions have some post-secondary (27.4%) or college or more (26.4%).

In the 1990-2000 period, 8 states had a private ban introduced and 16 states had a government ban introduced. Sixteen states had private bans for all 11 years, while 27 states did not have private bans for any of the years. Twenty-seven states had government bans for the whole time period, while 8 states had no government bans for any of the time period. However, since not all states have BRFSS data for all years, not all smoking bans are included in every part of the analysis in this paper.

Section V: Identification Strategy

This paper uses a difference-in-difference method to look at how the introduction of state-wide workplace smoking bans affects whether a person is a smoker. The difference-in-difference method is possible because the smoking bans are introduced at different times in different states. Both government and private-sector workplace bans are examined. Because the policies are at the state level, standard errors are clustered at the state level.

The econometric equation is

$$Smoker_{ist} = \alpha + \beta_s + \gamma_t + \varphi privateSB_{st} + \theta govtSB_{st} + \lambda X_{it} + \delta Z_{st} + \varepsilon_{ist},$$

where $Smoker_{ist}$ equals 1 if individual i in state s in year t is a smoker and zero otherwise, β_s is state fixed effects, γ_t is year fixed effects, $privateSB_{st}$ is a binary variable that equals 1 if the state has a law that bans smoking in private workplaces and zero otherwise, $govtSB_{st}$ is a binary variable that equals 1 if the state has a law that bans smoking in government workplaces and zero otherwise,

X_{it} are individual characteristics that can vary by year, Z_{st} are state characteristics that can vary by year (see Table 1 for a more complete list of the characteristics included in X_{it} and Z_{st}) . The coefficients φ and θ report the results of the difference-in-difference method. I present OLS estimates throughout, but show that the main findings are robust to non-linear models.

The BRFSS survey questions about a respondent's smoking status have different answer choices in different years. In 1990-1993, respondents who identify as current or irregular smokers are classified as being smokers in this paper. In 1994-1995, there are four categories of current smokers: those who smoked every day out of the previous 30 days, those who smoked 1-29 days out of the previous 30 days, those who smoked 0 days out of the previous 30 days, and those who smoked an unknown number of days out of the previous 30 days. In this paper, I consider all of the people in these categories to be smokers. I include respondents who identify as current smokers who did not smoke in the past 30 days in my smoker variable because identifying oneself as a former smoker is also an answer choice in the BRFSS question. Therefore, a person who had not smoked in the last month could choose to identify themselves as either still being a smoker or being a former smoker. Excluding those who identify as smokers who had not smoked in the past 30 days would potentially make my smoking variable have different meanings in 1994-1995 than in the others years, due to the different answer choices in different years. For 1996-2000, I consider smokers to be people who identify as current smokers who smoke every day and current smokers who smoke some days. A robustness check is run in which I change the smoker variable so that it only equals 1 if the person is a more intensive smoker.

Section VI: Results

Table 2 shows the regression results. In column 1, I include only the private ban and the government ban variables. In column 2, I also include the natural logarithm of real cigarette tax and the two dummies for whether bar and restaurant state bans are implemented. Columns 3 to 6 add demographic controls (columns 3-6), income controls (columns 4-6), employment and education controls (columns 5-6) and other personal characteristics (column 6).

The private ban estimates are always negative and small, ranging from -1.5 percent to -1.9 percent. Although the estimates have similar magnitudes across all specifications, only the estimate in the first column is statistically significant at the 10% level, with a value of -1.9 percent. The signs of the private ban coefficients are expected, since banning smoking in a workplace makes smoking less convenient to do and may push some people to quit or to not start smoking. The government ban coefficients are always small, positive and insignificant. The estimates for the natural logarithm of real cigarette tax and for restaurant ban are always very small, negative and insignificant. The coefficients for bar ban are always quite small and positive, but are only significant at the 5% level in columns 2 and 3. The bar ban signs are unexpected.

Table 3 shows the regression results of the preferred specification (column 6 from Table 2) when the sample is decomposed by sex, age category and by both simultaneously. For private bans, the estimates are all still negative and of similar size to the estimates obtained by using the full sample. Most of the subgroup private ban estimates are statistically insignificant. However, the private ban estimates for males aged 35 to 54 (-2.7 percent) and females aged 18 to 34 (-3.1 percent) are significant at the 10% level. The government ban coefficient is only significant at the 10% level among females 55 and over. All other government ban coefficients in Table 3 are small, positive and statistically insignificant, similar to the results of the regressions using the full sample.

The coefficients for the natural logarithm of real tax are only statistically significant at the 5% level in the regression with people ages 55 and over (-.012) and the regression with females 55 and over (-.013). These negative signs are to be expected because a higher tax rate would make cigarettes more expensive and this might push people to quit or to not start smoking. Most of the other of the natural logarithm of real tax coefficients are small, negative and insignificant, like in Table 2, although a few are positive or equal to zero. The restaurant ban coefficients in both Table 2 and Table 3 are all small, negative (except for females aged 18 to 34 in Table 3) and statistically insignificant.

While in the regression with the full sample the bar ban coefficient was .006 and statistically insignificant, in Table 3 the estimates for bar bans are in some cases statistically significant and are not all positive. The bar ban coefficients for males (.016) and males aged 35 to 54 (.020) are positive and statistically significant at the 1% level, while the coefficient for males aged 18 to 34 (.022) is statistically significant at the 10% level. The estimate for females aged 35 to 54 (-.016) is negative and statistically significant at the 5% level.

Table 4 shows the marginal effects of private bans and government bans. These were obtained by running probit models and then by computing the marginal effects. None of the coefficients are statistically significant in any of the specifications. These results provide further support for the conclusion that legislative smoking bans do not have much influence on smoking prevalence.

Figures 1 through 6 show the results of the synthetic control method being applied to six states that switch from having no private ban to having a private ban during the 1990-2000

timeframe[1]. The synthetic control method is useful when no one state is a good counterfactual for a treated state. A synthetic control state is constructed by weighting other states' data such that the resulting synthetic state follows, as closely as possible, the same trend ex-ante as the treatment state. After treatment, the synthetic control state shows what would have happened to the treated state if it had not been treated. Table A1 shows the smoker predictor means. The variables used to construct the synthetic control states are the means of the smoker variable in the two years preceding the treatment. Only states that did not have private bans at the time a given treated state became treated, and only states that had smoking data for all 11 years, were used to construct a given synthetic control state. Tables A2 through A7 show the state weights for each synthetic control region. In each of the graphs, the ex-post trends are very similar to each other. The only possible exception is that for Delaware the synthetic control state's smoking proportion goes up slightly for the two years after the treatment and then goes down the next year, while Delaware's smoking proportion stays constant for the two years after the treatment and then goes up slightly in the third year. However, these differing trends are not sustained.

Overall, the results of the synthetic control method suggest that private smoking bans do not affect state smoking rates in a noticeable way. These results confirm the results obtained earlier in this paper by the OLS method with regards to the impact of private smoking bans on smoking prevalence.

[1] There were eight states that switched from having no private ban to having a private ban in the 1990-2000 time period. For the synthetic control method, the predictor variables are the smoking prevalence in the treated state 1 and 2 years before the treatment. Thus, I could only use the synthetic control method for states whose bans were introduced in 1992 or later. Therefore, I could not use District of Columbia because its private ban was introduced in 1991. Unfortunately, I also could not obtain matching states for California, which had a private ban introduced in 1995.

Section VII: Robustness Checks

Table 5 shows the results of a robustness check that uses the same regressions as in Table 2 but includes individuals with extremely low and high BMI values. Compared to the results in Table 2, the results in Table 5 are of very similar magnitude and all of the coefficients retain their signs. The only coefficient that changes in significance level is that of bar ban in specification 2, which is significant at the 5% level in Table 2 but only at the 10% level in Table 5.

As another robustness check, Table 6 shows the regression results of the specifications from Table 2 but with a modified dependent variable. This new dependent variable still measures smoking status, but only equals 1 if the person is a more regular smoker. For 1990-1993, the new dependent variable only includes current smokers (and not irregular smokers, which the initial dependent variable included). For 1994-1995, the new dependent variable only includes those who have smoked all or 1-29 of the 30 preceding days (and not current smokers who smoked 0 or an unknown number of the past 30 days, which the initial dependent variable included). For 1996-2000, the only answer choices for smokers are to identify as a current smoker who smokes every day or a current smoker who smokes some days. In order to keep the meaning of the smoker variable for 1996-2000 compatible with its meaning in the other years, I consider people who choose either of these answer choices as smokers, even though this means that the new smoker variable is the same as the initial smoker variable for 1996-2000. Modifying the smoker variable in 1996-2000 just for the sake of change would not make the new smoker variable have the same meaning throughout the whole time period studied.

Table 1 shows that the mean of the new smoker variable is 23.4%. Since the new smoker variable includes the same people as the old smoker variable for 1996-2000, and there are

more observations for the later years of my dataset, it is not surprising that the mean of the new smoker variable is quite similar to the mean of the old smoker variable (23.8%).

Compared to the private ban coefficients in Table 2, the corresponding coefficients in Table 6 are still all negative, but are smaller and are all statistically insignificant. In both tables, the government ban coefficients are positive, but in Table 6 they are bigger (although still not very large) and are all statistically significant at the 5% level. The Table 6 coefficients for the natural logarithm of real tax and restaurant ban are insignificant, like in Table 2, but are even smaller than their corresponding coefficients in Table 2. The natural logarithm of real tax coefficients in Table 6 are positive, unlike the corresponding coefficients in Table 2. The restaurant ban coefficients in both tables are negative, unless they are equal to zero. The coefficients in Table 6 for bar ban are all statistically significant at the 1% level, whereas in Table 2 only the coefficients for the 2nd and 3rd columns are statistically significant at the 5% level. The Table 6 bar ban coefficients are also bigger than their corresponding coefficients in Table 2, although they are still not of large economic significance. However, like in Table 2, they are positive, which is unexpected by the conceptual framework.

Section VIII: Conclusion

This paper uses a difference-in-difference method to identify the effects of state-level legislative smoking bans on whether a person is a smoker. For the overall sample, the results are virtually all small and statistically insignificant. When regressions are run on subsets of the sample, some sex and age groups experience differing effects of workplace smoking bans, the natural logarithm of the real tax rate and bar smoking bans. When the definition of a smoker is modified

to only include more intensive smokers, the results for the effects of government bans and bar bans become statistically significant, although with unexpected positive signs. That being said, even when coefficients are statistically significant, the estimates are quite small. This suggests that smoking bans will have only very modest effects, if any, on whether someone is a smoker. As well, some coefficients have different signs than what is expected by the theoretical framework. Further investigation into the mechanisms causing these unexpected signs could be useful.

Even if workplace smoking bans are not consistently found to make someone less likely to be a smoker, these bans could improve the work environment for other workers by reducing their exposure to harmful second-hand smoke. Thus, workplace smoking bans may not be pointless legislation for governments to enact. However, if governments wish to enact legislation that will push people to quit smoking or to not start smoking, other policies may be useful to consider.

References:

Adda, Jérôme, and Francesca Cornaglia, "The Effect of Bans and Taxes on Passive Smoking," *American Economic Journal: Applied Economics*, 2 (2010), 1-32.

Auld, Christopher M., and Paul Grootendorst, "An empirical analysis of milk addiction," *Journal of Health Economics*, 23 (2004), 1117-1133.

Becker, Gary S., Michael Grossman, and Kevin M. Murphy, "An Empirical Analysis of Cigarette Addiction," *American Economic Review*, 84 (1994), 396-418.

Becker, Gary S., and Kevin M. Murphy, "A Theory of Rational Addiction," *Journal of Political Economy*, 96 (1988), 675-700.

Bernheim, B. Douglas and Antonio Rangel, "Addiction and Cue-Triggered Decision Processes," *American Economic Review*, 94 (2004), 1558-1590.

Bitler, Marianne P., Christopher S. Carpenter, and Madeline Zavodny, "Effects of Venue-Specific State Clean Indoor Air Laws on Smoking-related Outcomes," *Health Economics,* 19 (2010) 1425-1440.

Brodeur, Abel, "Smoking, Income and Subjective Well-Being: Evidence from Smoking Bans," CEP Discussion Paper No 1202, 2013.

Carpenter, Christopher, Sabina Postolek, and Casey Warman, "Public-Place Smoking Laws and Exposure to Environmental Tobacco Smoke (ETS)," *American Economic Journal: Economic Policy*, 3 (2011), 35-61.

Cheng, Kai-Wen, Feng Liu, Mariaelena Gonzalez, and Stanton Glantz, "The Effects of Workplace Clean Indoor Air Law Coverage on Workers' Smoking-related Outcomes," *Health Economics*, 26 (2017), 226-242.

Evans, William N., Matthew C. Farrelly, and Edward Montgomery, "Do Workplace Smoking Bans Reduce Smoking?" *American Economic Review*, 89 (1999), 728-747.

Government of Canada, "Dangers of second-hand smoke - Canada.ca," Last updated May 6, 2015. Retrieved on August 15, 2018. https://www.canada.ca/en/health-canada/services/smoking-tobacco/avoid-second-hand-smoke/second-hand-smoke/dangers-second-hand-smoke.html

Government of Canada, "Risks of smoking - Canada.ca," Last updated May 17, 2016. Retrieved on August 15, 2018. https://www.canada.ca/en/health-canada/services/smoking-tobacco/effects-smoking/smoking-your-body/risks-smoking.html

Hurley, Jeremiah E., *Health Economics*, 1st ed. (Toronto: McGraw-Hill Ryerson, 2010).

Savage, Michael, "Smoking outside: the effect of the Irish workplace smoking ban on smoking prevalence among the employed," *Health Economics, Policy and Law*, 9 (2014), 407-424.

Tauras, John A., "Public Policy and Some-day Smoking among Adults," *Journal of Applied Economics*, 7 (2004), 137-162.

Tauras, John A., "Smoke-free Air Laws, Cigarette Prices, and Adult Cigarette Demand," *Economic Inquiry*, 44 (2006), 333–342.

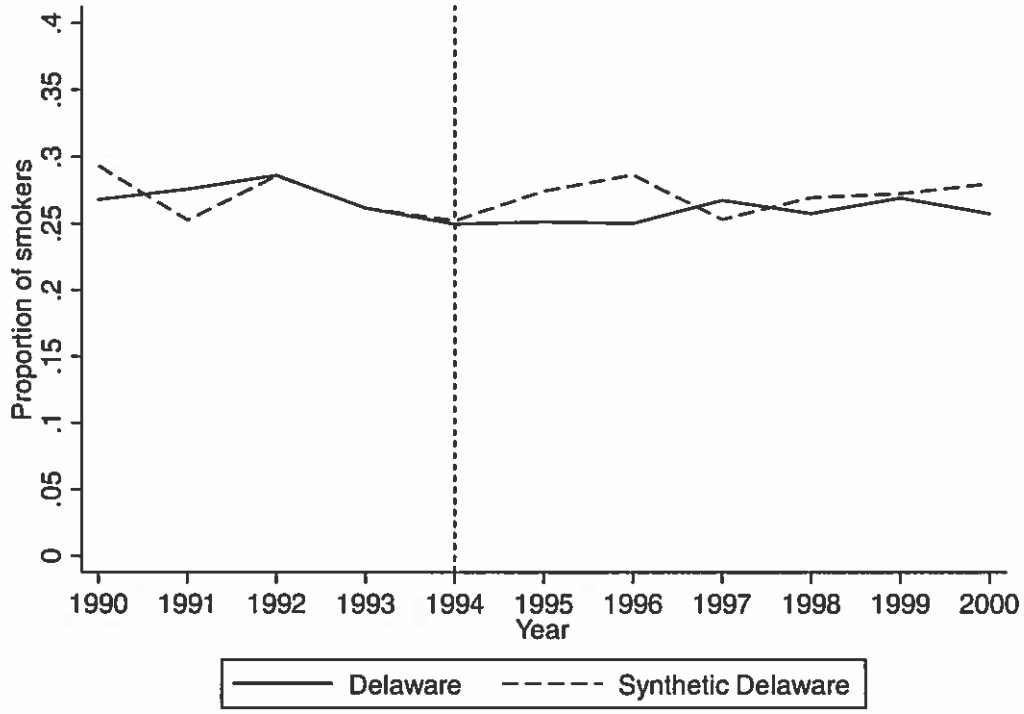

Figure 1: Proportion of Smokers in Delaware and a Synthetic Control Region by Year

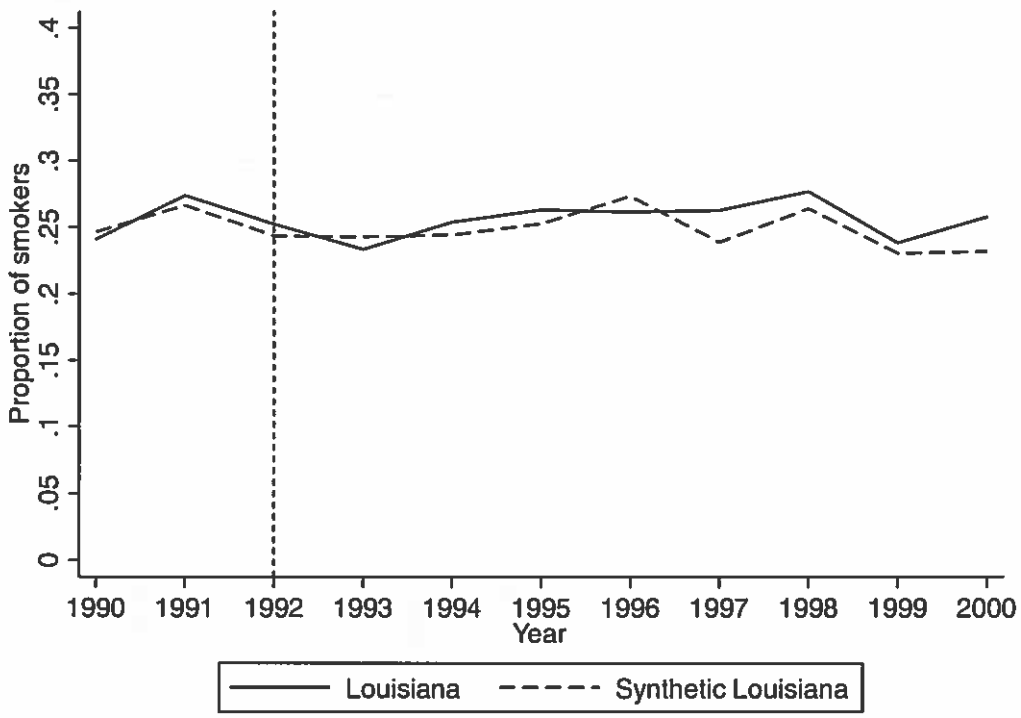

Figure 2: Proportion of Smokers in Louisiana and a Synthetic Control Region by Year

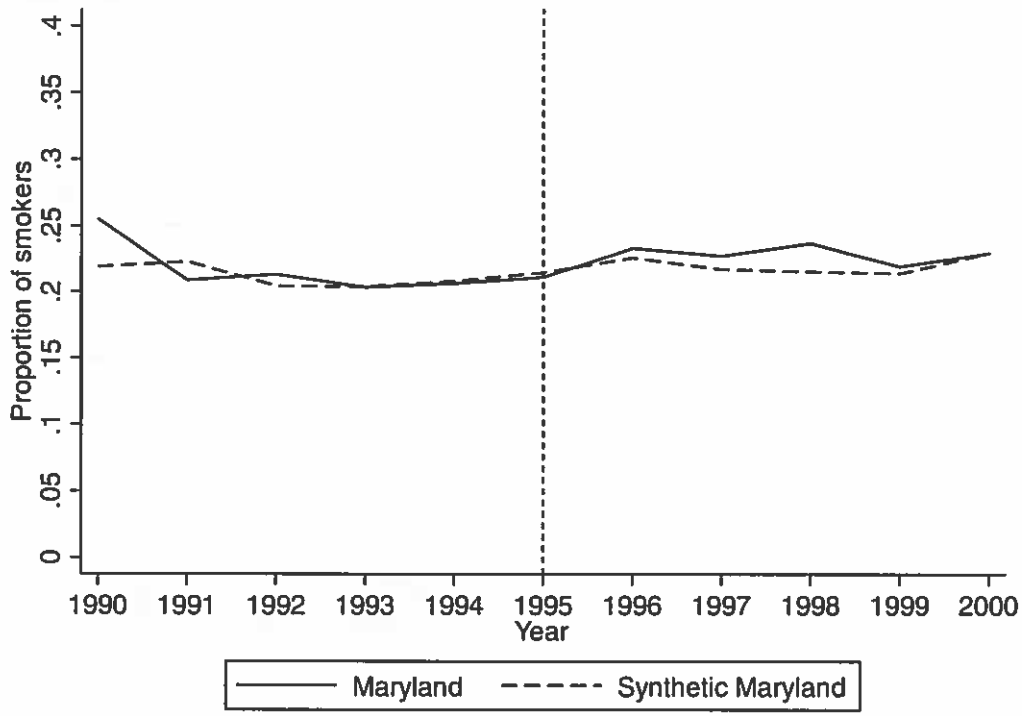

Figure 3: Proportion of Smokers in Maryland and a Synthetic Control Region by Year

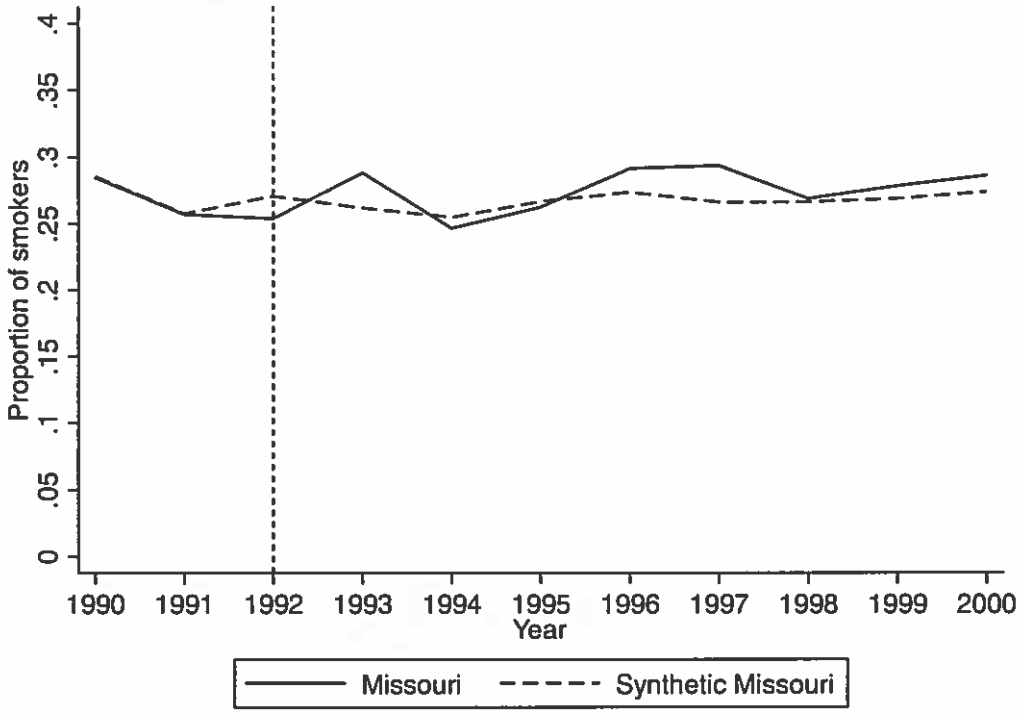

Figure 4: Proportion of Smokers in Missouri and a Synthetic Control Region by Year

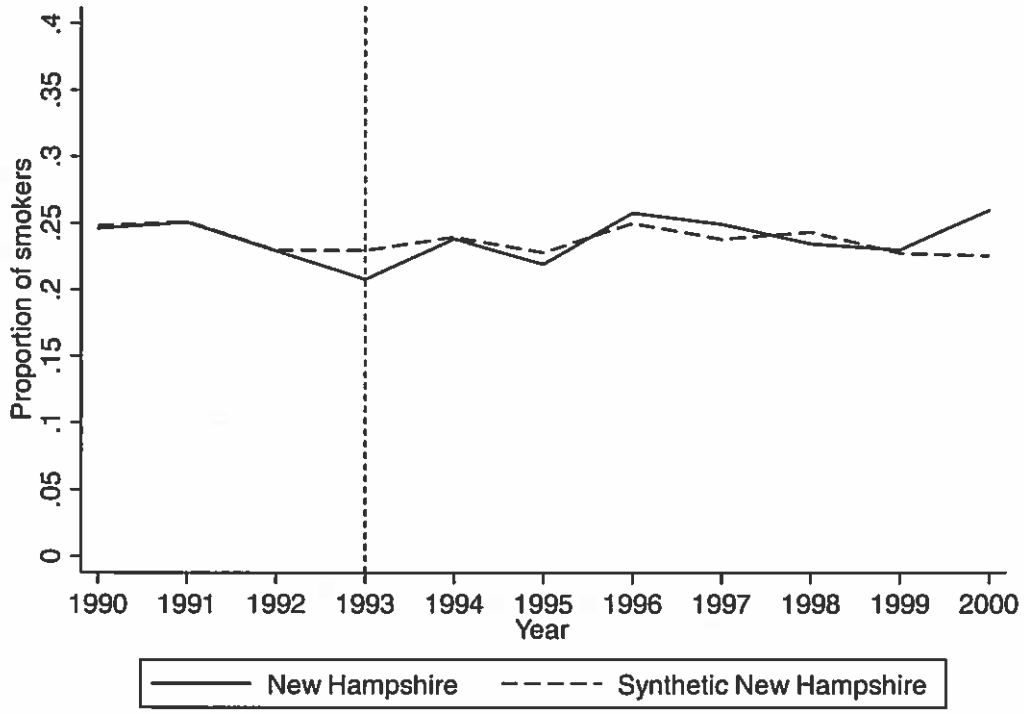

Figure 5: Proportion of Smokers in New Hampshire and a Synthetic Control Region by Year

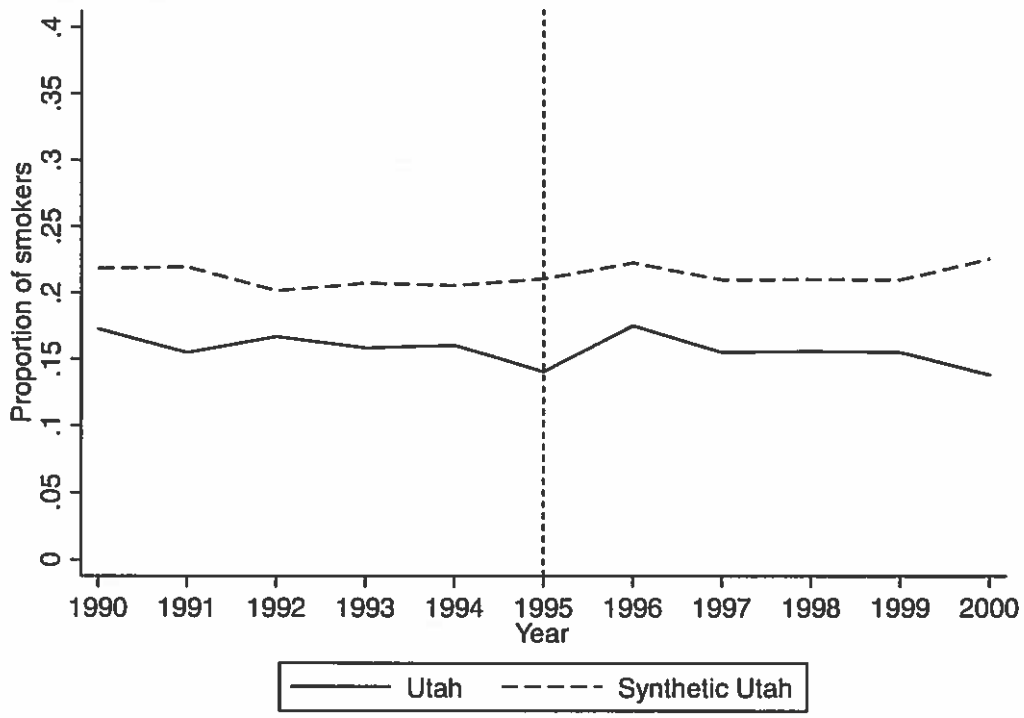

Figure 6: Proportion of Smokers in Utah and a Synthetic Control Region by Year

Table 1: Summary statistics

Variable	Mean	
Smoker	0.238	(0.426)
Smoker (alternative definition)	0.234	(0.423)
Natural logarithm of real cigarette tax	2.913	(0.760)
Private ban	0.489	(0.500)
Government ban	0.709	(0.454)
Restaurant ban	0.525	(0.499)
Bar ban	0.073	(0.261)
Female	0.499	(0.500)
Race		
White	0.777	(0.416)
Black	0.093	(0.290)
Hispanic	0.092	(0.289)
Other	0.038	(0.191)
Marital status		
Married	0.605	(0.489)
Divorced	0.094	(0.292)
Widowed	0.066	(0.249)
Separated	0.022	(0.148)
Never married	0.188	(0.390)
Unmarried couple	0.025	(0.156)
Household income		
Less than $10,000	0.094	(0.292)
$10,000 to $14,999	0.079	(0.270)
$15,000 to $19,999	0.093	(0.290)
$20,000 to $24,999	0.110	(0.313)
$25,000 to $34,999	0.173	(0.378)
$35,000 to $49,999	0.191	(0.393)
$50,000 or more	0.259	(0.438)
Age		
18 to 34	0.357	(0.479)
35 to 54	0.374	(0.484)
55 and over	0.270	(0.444)
Employment status		
Working	0.657	(0.475)
Unemployed (>1 year)	0.018	(0.134)
Unemployed (<1 year)	0.026	(0.159)
Homemaker	0.076	(0.265)
Student	0.041	(0.199)
Retired	0.159	(0.366)
Unable to work	0.022	(0.148)
BMI category		
Underweight	0.024	(0.153)

Normal	0.463	(0.499)
Overweight	0.362	(0.481)
Obese	0.150	(0.357)
Education		
Grade 8 or less	0.049	(0.215)
Some high school	0.086	(0.281)
High school or GED	0.327	(0.469)
Some post-secondary	0.274	(0.446)
College grad or more	0.264	(0.441)
Observations	1,080,866	

Notes: Means are weighted. Standard deviations are in parentheses.

Table 2: Regression results

	(1)	(2)	(3)	(4)	(5)	(6)
Private ban	-.019*	-.016	-.015	-.019	-.017	-.017
	(.010)	(.011)	(.012)	(.013)	(.013)	(.012)
Government ban	.013	.012	.013	.012	.011	.010
	(.009)	(.010)	(.010)	(.010)	(.009)	(.009)
Natural logarithm of real tax		-.004	-.004	-.004	-.003	-.004
		(.004)	(.004)	(.004)	(.003)	(.003)
Restaurant ban		-.007	-.008	-.007	-.005	-.004
		(.005)	(.005)	(.006)	(.006)	(.005)
Bar ban		.009**	.011**	.008	.006	.006
		(.005)	(.005)	(.006)	(.006)	(.005)
Demographic controls			Yes	Yes	Yes	Yes
Income controls				Yes	Yes	Yes
Employment and education control					Yes	Yes
Other personal characteristics						Yes
Observations	1,080,866	1,080,866	1,080,866	1,080,866	1,080,866	1,080,866

Notes: OLS estimates are presented. All regressions are weighted. Robust standard errors, clustered at the state level, are in parentheses. Demographic controls include a dummy variable for female, dummy variables for age (Age 55 and over is the omitted category), dummy variables for races (white non-hispanic is the omitted category). Income controls are dummy variables, where the income over $50,000 category is omitted. Employment and education controls are dummies about a person's employment status and education level. Working people and people with a high school diploma or GED but no more are the omitted categories. Other personal characteristics are the person's BMI category (normal weight people are omitted), marital status (single people are omitted). Alabama is the omitted state and 1990 is the omitted year. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 3: Regression results by subgroup

	Males	Females	Age 18 to 34	Age 35 to 54	Age 55 and over	Males			Females		
						Age 18 to 34	Age 35 to 54	Age 55 and over	Age 18 to 34	Age 35 to 54	Age 55 and over
Private ban	-.022	-.010	-.033	-.016	-.004	-.035	-.027*	-.003	-.031*	-.005	-.005
	(.014)	(.010)	(.021)	(.013)	(.008)	(.027)	(.014)	(.008)	(.017)	(.015)	(.011)
Government ban	.008	.011	.012	.012	.002	.011	.011	-.005	.013	.013	.009*
	(.011)	(.007)	(.014)	(.011)	(.004)	(.015)	(.014)	(.008)	(.014)	(.010)	(.005)
Natural logarithm of real tax	-.001	-.005	-.005	.003	-.012**	-.002	.005	-.011	-.007	.000	-.013**
	(.004)	(.003)	(.007)	(.004)	(.005)	(.008)	(.006)	(.007)	(.008)	(.006)	(.006)
Restaurant ban	-.005	-.003	-.002	-.006	-.004	-.005	-.005	-.006	.001	-.007	-.002
	(.007)	(.005)	(.014)	(.004)	(.004)	(.018)	(.005)	(.007)	(.011)	(.006)	(.005)
Bar ban	.016***	-.006	.015	.002	-.008	.022*	.020***	-.003	.006	-.016**	-.014
	(.005)	(.006)	(.010)	(.006)	(.006)	(.011)	(.006)	(.006)	(.012)	(.007)	(.008)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment and education control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other personal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	470,456	610,410	332,708	435,447	312,711	148,422	195,848	126,186	184,286	239,599	186,525

Notes: OLS estimates are presented. The preferred specification (specification 6 in Table 2) is run. All regressions are weighted. Robust standard errors, clustered at the state level, are in parentheses. Demographic controls include a dummy variable for female, dummy variables for age (Age 55 and over is the omitted category), dummy variables for races (white non-hispanic is the omitted category). Income controls are dummy variables, where the income over $50,000 category is omitted. Employment and education controls are dummies about a person's employment status and education level. Working people and people with a high school diploma or GED but no more are the omitted categories. Other personal characteristics are the person's BMI category (normal weight people are omitted), marital status (single people are omitted). * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 4: Probit marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)
Private ban	-.0002	-.003	-.003	-.006	-.005	-.003
	(.007)	(.007)	(.007)	(.007)	(.006)	(.006)
Government ban	.0004	.00003	.001	-.0005	-.0002	-.0001
	(.005)	(.005)	(.005)	(.006)	(.005)	(.005)
Demographic controls			Yes	Yes	Yes	Yes
Income controls				Yes	Yes	Yes
Employment and education control					Yes	Yes
Other personal characteristics						Yes
Observations	1,080,866	1,080,866	1,080,866	1,080,866	1,080,866	1,080,866

Notes: All regressions are weighted. Robust standard errors, clustered at the state level, are in parentheses. Demographic controls include a dummy variable for female, dummy variables for age (Age 55 and over is the omitted category), dummy variables for races (white non-hispanic is the omitted category). Income controls are dummy variables, where the income over $50,000 category is omitted. Employment and education controls are dummies about a person's employment status and education level. Working people and people with a high school diploma or GED but no more are the omitted categories. Other personal characteristics are the person's BMI category (normal weight people are omitted), marital status (single people are omitted). Alabama is the omitted state and 1990 is the omitted year.

Table 5: Robustness check – Regression results with extreme BMI values included

	(1)	(2)	(3)	(4)	(5)	(6)
Private ban	-.019*	-.017	-.016	-.019	-.018	-.017
	(.011)	(.012)	(.012)	(.013)	(.012)	(.012)
Government ban	.014	.013	.014	.013	.011	.010
	(.010)	(.010)	(.010)	(.010)	(.010)	(.009)
Natural logarithm of real tax		-.003	-.003	-.003	-.002	-.003
		(.004)	(.004)	(.004)	(.003)	(.003)
Restaurant ban		-.007	-.007	-.007	-.005	-.004
		(.004)	(.005)	(.006)	(.006)	(.005)
Bar ban		.009*	.010**	.007	.005	.005
		(.005)	(.004)	(.005)	(.005)	(.005)
Demographic controls			Yes	Yes	Yes	Yes
Income controls				Yes	Yes	Yes
Employment and education control					Yes	Yes
Other personal characteristics						Yes
Observations	1,098,405	1,098,405	1,098,405	1,098,405	1,098,405	1,098,405

Notes: OLS estimates are presented. All regressions are weighted. Robust standard errors, clustered at the state level, are in parentheses. Demographic controls include a dummy variable for female, dummy variables for age (Age 55 and over is the omitted category), dummy variables for races (white non-hispanic is the omitted category). Income controls are dummy variables, where the income over $50,000 category is omitted. Employment and education controls are dummies about a person's employment status and education level. Working people and people with a high school diploma or GED but no more are the omitted categories. Other personal characteristics are the person's BMI category (normal weight people are omitted), marital status (single people are omitted). Alabama is the omitted state and 1990 is the omitted year. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 6: Robustness check – Alternative definition of smoker

	(1)	(2)	(3)	(4)	(5)	(6)
Private ban	-.001	-.005	-.004	-.008	-.007	-.006
	(.007)	(.009)	(.009)	(.010)	(.010)	(.009)
Government ban	.024**	.022**	.023**	.022**	.021**	.020**
	(.009)	(.010)	(.010)	(.010)	(.010)	(.009)
Natural logarithm of real tax		.001	.001	.001	.002	.001
		(.005)	(.006)	(.005)	(.005)	(.005)
Restaurant Ban		-.003	-.003	-.003	-.001	.000
		(.003)	(.004)	(.005)	(.004)	(.004)
Bar ban		.016***	.017***	.014***	.012***	.012***
		(.003)	(.003)	(.003)	(.003)	(.003)
Demographic controls			Yes	Yes	Yes	Yes
Income controls				Yes	Yes	Yes
Employment and education control					Yes	Yes
Other personal characteristics						Yes
Observations	1,080,866	1,080,866	1,080,866	1,080,866	1,080,866	1,080,866

Notes: OLS estimates are presented. All regressions are weighted. Robust standard errors, clustered at the state level, are in parentheses. Demographic controls include a dummy variable for female, dummy variables for age (Age 55 and over is the omitted category), dummy variables for races (white non-hispanic is the omitted category). Income controls are dummy variables, where the income over $50,000 category is omitted. Employment and education controls are dummies about a person's employment status and education level. Working people and people with a high school diploma or GED but no more are the omitted categories. Other personal characteristics are the person's BMI category (normal weight people are omitted), marital status (single people are omitted). Alabama is the omitted state and 1990 is the omitted year. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Appendix:

Table A1: Predictor means

	Louisiana		Missouri		New Hampshire		Delaware		Maryland		Utah	
	1990	1991	1990	1991	1991	1992	1992	1993	1993	1994	1993	1994
Treated	0.241	0.274	0.285	0.257	0.250	0.229	0.286	0.261	0.204	0.206	0.158	0.160
Synthetic	0.246	0.266	0.286	0.257	0.251	0.229	0.286	0.261	0.204	0.208	0.207	0.206

Notes: The predictor variables are the smoking prevalence in the treated state 1 and 2 years before the treatment.

Table A2: State weights in synthetic Delaware

State	Weight	State	Weight
Alabama	0	Missouri	--
Arizona	0	New Hampshire	--
California	0	New Mexico	0
Colorado	0	North Carolina	0
Delaware	--	North Dakota	0
Georgia	0	Ohio	0
Hawaii	0	Oklahoma	0
Idaho	0	South Carolina	.444
Indiana	.556	South Dakota	0
Kentucky	0	Texas	0
Louisiana	--	Utah	0
Maryland	0	Virginia	0
Michigan	0	Washington	0
Mississippi	0	West Virginia	0

Notes: For ease of presentation, states that had a private ban in 1991 or earlier (and thus could not be used as part of a synthetic control state, since the predictor variables are the smoking prevalence in a treated state 1 and 2 years before the treatment and the dataset only contains observations from 1990 and later) are excluded from this table. States for which data was not available for every year of the 1990-2000 period are also excluded. The states for which a private ban came into effect between 1992 and the introduction of Delaware's private ban, in 1994, are included in the table but are not given a numerical value for weight. Delaware is included in the table but is not given a numerical value for weight. The states listed in the table that have numerical values for weight are the states that did not have a private ban in the year Delaware's private ban came into place.

Table A3: State weights in synthetic Louisiana

State	Weight	State	Weight
Alabama	0	Missouri	--
Arizona	.721	New Hampshire	0
California	0	New Mexico	0
Colorado	0	North Carolina	0
Delaware	0	North Dakota	0
Georgia	0	Ohio	0
Hawaii	0	Oklahoma	0
Idaho	0	South Carolina	0
Indiana	0	South Dakota	0
Kentucky	.279	Texas	0
Louisiana	--	Utah	0
Maryland	0	Virginia	0
Michigan	0	Washington	0
Mississippi	0	West Virginia	0

Notes: For ease of presentation, states that had a private ban in 1991 or earlier (and thus could not be used as part of a synthetic control state, since the predictor variables are the smoking prevalence in a treated state 1 and 2 years before the treatment and the dataset only contains observations from 1990 and later) are excluded from this table. States for which data was not available for every year of the 1990-2000 period are also excluded. The states for which a private ban came into effect between 1992 and the introduction of Louisiana's private ban, in 1992, are included in the table but are not given a numerical value for weight. Louisiana is included in the table but is not given a numerical value for weight. The states listed in the table that have numerical values for weight are the states that did not have a private ban in the year Louisiana's private ban came into place.

Table A4: State weights in synthetic Maryland

State	Weight	State	Weight
Alabama	.127	Missouri	--
Arizona	0	New Hampshire	--
California	--	New Mexico	0
Colorado	0	North Carolina	0
Delaware	--	North Dakota	0
Georgia	0	Ohio	0
Hawaii	0	Oklahoma	0
Idaho	.873	South Carolina	0
Indiana	0	South Dakota	0
Kentucky	0	Texas	0
Louisiana	--	Utah	--
Maryland	--	Virginia	0
Michigan	0	Washington	0
Mississippi	0	West Virginia	0

Notes: For ease of presentation, states that had a private ban in 1991 or earlier (and thus could not be used as part of a synthetic control state, since the predictor variables are the smoking prevalence in a treated state 1 and 2 years before the treatment and the dataset only contains observations from 1990 and later) are excluded from this table. States for which data was not available for every year of the 1990-2000 period are also excluded. The states for which a private ban came into effect between 1992 and the introduction of Maryland's private ban, in 1995, are included in the table but are not given a numerical value for weight. Maryland is included in the table but is not given a numerical value for weight. The states listed in the table that have numerical values for weight are the states that did not have a private ban in the year Maryland's private ban came into place.

Table A5: State weights in synthetic Missouri

State	Weight	State	Weight
Alabama	.008	Missouri	--
Arizona	.008	New Hampshire	.012
California	.01	New Mexico	.015
Colorado	.012	North Carolina	.107
Delaware	.015	North Dakota	.011
Georgia	.013	Ohio	.019
Hawaii	.012	Oklahoma	.015
Idaho	.009	South Carolina	.034
Indiana	.456	South Dakota	.009
Kentucky	.071	Texas	.015
Louisiana	--	Utah	.005
Maryland	.018	Virginia	.015
Michigan	.065	Washington	.013
Mississippi	.013	West Virginia	.023

Notes: For ease of presentation, states that had a private ban in 1991 or earlier (and thus could not be used as part of a synthetic control state, since the predictor variables are the smoking prevalence in a treated state 1 and 2 years before the treatment and the dataset only contains observations from 1990 and later) are excluded from this table. States for which data was not available for every year of the 1990-2000 period are also excluded. The states for which a private ban came into effect between 1992 and the introduction of Missouri's private ban, in 1992, are included in the table but are not given a numerical value for weight. Missouri is included in the table but is not given a numerical value for weight. The states listed in the table that have numerical values for weight are the states that did not have a private ban in the year Missouri's private ban came into place.

Table A6: State weights in synthetic New Hampshire

State	Weight	State	Weight
Alabama	.033	Missouri	--
Arizona	.224	New Hampshire	--
California	.013	New Mexico	.008
Colorado	.009	North Carolina	.01
Delaware	.008	North Dakota	.008
Georgia	.024	Ohio	.024
Hawaii	.018	Oklahoma	.011
Idaho	.106	South Carolina	.007
Indiana	.006	South Dakota	.012
Kentucky	.011	Texas	.013
Louisiana	--	Utah	.013
Maryland	.011	Virginia	.33
Michigan	.031	Washington	.053
Mississippi	.008	West Virginia	.011

Notes: For ease of presentation, states that had a private ban in 1991 or earlier (and thus could not be used as part of a synthetic control state, since the predictor variables are the smoking prevalence in a treated state 1 and 2 years before the treatment and the dataset only contains observations from 1990 and later) are excluded from this table. States for which data was not available for every year of the 1990-2000 period are also excluded. The states for which a private ban came into effect between 1992 and the introduction of New Hampshire's private ban, in 1993, are included in the table but are not given a numerical value for weight. New Hampshire is included in the table but is not given a numerical value for weight. The states listed in the table that have numerical values for weight are the states that did not have a private ban in the year New Hampshire's private ban came into place.

Table A7: State weights in synthetic Utah

State	Weight	State	Weight
Alabama	0	Missouri	--
Arizona	0	New Hampshire	--
California	--	New Mexico	0
Colorado	0	North Carolina	0
Delaware	--	North Dakota	0
Georgia	0	Ohio	0
Hawaii	0	Oklahoma	0
Idaho	1	South Carolina	0
Indiana	0	South Dakota	0
Kentucky	0	Texas	0
Louisiana	--	Utah	--
Maryland	--	Virginia	0
Michigan	0	Washington	0
Mississippi	0	West Virginia	0

Notes: For ease of presentation, states that had a private ban in 1991 or earlier (and thus could not be used as part of a synthetic control state, since the predictor variables are the smoking prevalence in a treated state 1 and 2 years before the treatment and the dataset only contains observations from 1990 and later) are excluded from this table. States for which data was not available for every year of the 1990-2000 period are also excluded. The states for which a private ban came into effect between 1992 and the introduction of Utah's private ban, in 1995, are included in the table but are not given a numerical value for weight. Utah is included in the table but is not given a numerical value for weight. The states listed in the table that have numerical values for weight are the states that did not have a private ban in the year Utah's private ban came into place.