The Impact of Smoking Bans in Bars and Restaurants on Alcohol Consumption and Smoking*

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Governments implemented bar and restaurant smoking bans to target smoking-related externalities, but these bans may also affect drinking. This paper studies smoking bans' effects on alcohol consumption and smoking behavior. I estimate a difference-in-differences model that exploits spatial and temporal variation in smoking bans. Bans result in a 1-drink-per-month (5%) increase in intensive-margin alcohol consumption and no economically meaningful effects on smoking. These results imply that smoking bans lead to unintended consequences in the form of increased alcohol consumption.

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1 Introduction

Externalities are a classic example of a market failure that governments have long regulated. Smoking cigarettes is an example of a good that both generates significant negative externalities and constitutes a major public health problem in the United States.[1] Ever since the 1964 Surgeon General's report linked smoking cigarettes to adverse health consequences, federal, state, and local governments have implemented policies such as cigarette taxes, tobacco minimum purchasing ages, and smoking bans, to minimize the prevalence of smoking and mitigate the externalities generated by secondhand smoke.[2]

In this paper I study whether smoking bans in bars and restaurants, regulations ostensibly targeted at smoking behavior, affect alcohol consumption. As a secondary analysis, I examine their effects on smoking, the likely location of alcohol consumption, and alcohol consumption by smoking status. Smoking bans in bars represent a change in a non-price determinant of demand for alcohol consumed in bars, which may differentially affect smokers and nonsmokers. If nonsmokers derive disutility from cigarette smoke, then a smoking ban in a bar increases nonsmokers' utility of drinking in a bar and increases their bar alcohol consumption, ceteris paribus. In contrast, if smokers derive utility from being able to smoke while they drink at a bar, then a smoking ban would lower smokers' utility from drinking in a bar and decrease their bar alcohol consumption. Indeed, many bar owners predicted that a smoking ban would cause smokers to substitute drinking at bars for drinking at home (to the

[1] Approximately one in five deaths (480,000) annually in the U.S. are a result of cigarette smoking (CDC, 2020). Of those, 41,000 are a result of exposure to secondhand smoke (CDC, 2020).

[2] Excessive alcohol consumption constitutes its own public health problem and also creates negative externalities. Annually, over 95,000 people die due to excessive alcohol consumption in the U.S., from both chronic (e.g. cancer, liver disease) and acute (e.g. suicide and motor vehicle crashes) causes (CDC, 2021). These deaths constitute 2.8 million years of potential life lost (CDC, 2021).

detriment of bar owners' bottom lines).[3] An additional consideration is that individuals may derive utility from the presence of other patrons. If a smoking ban encourages nonsmokers to spend more time at bars, then both smokers and nonsmokers may find the bar to be a more enjoyable place. The effect of smoking bans on total alcohol consumption is also uncertain, as any change in marginal utility from drinking at a bar will change the marginal rate of substitution between drinking at a bar and drinking at home.[4]

Given the potentially heterogeneous effects of smoking bans on alcohol consumption, my analysis of the effects of smoking bans on alcohol consumption examines how the effects vary for smokers and non-smokers. I also investigate whether these bans affect the prevalence of smoking, as some individuals may change their smoking behavior after these bans are implemented.

I use the 2004-2012 waves of the Behavioral Risk Factor Surveillance System (BRFSS) and the Nielsen Consumer Panel. The BRFSS measures smoking status and alcohol consumption, and the Nielsen data include cigarette purchases and alcohol purchased for home consumption.[5] I estimate a difference-in-differences model where my identifying variation is the date of implementation of a smoking ban in bars and restaurants. During the sample period, 25 states and 651 local governments (cities or counties) implemented a smoking ban in bars, and 28 states and 751 municipalities implemented a smoking ban in restaurants, providing a wealth of spatial and temporal variation in treatment status.

[3] e.g., "I was extremely worried about how the ban would affect my tavern, as probably 75 percent of my customers were smokers."—Teri Regano, owner of the Roman Coin (Milwaukee Record, 2015) and "There will probably be a lot more homebodies."—Mark O'Brien, bartender at Who's Bar (Passi, 2010).

[4] Nonsmokers may substitute away from alcohol consumed at home to alcohol consumed at a bar. Alternatively, through habit formation or addiction, individuals may drink more at bars without reducing how much they drink at home.

[5] Likely location of alcohol consumption can be ascertained if one assumes total alcohol consumption equals alcohol consumption at home plus alcohol consumption at bars or restaurants.

Conditional on drinking alcohol in the past 30 days, smoking bans in bars and restaurants lead to an increase of one serving of alcohol per 30 days (a 5% increase). Average alcohol consumption for current and former smokers increases by 5-8%. I find no economically meaningful effects on the total quantity of alcohol purchased for home consumption in the past month. Taken together, these results imply that at least some of the increase in total alcohol consumption may be coming from increases in bar and restaurant alcohol consumption. With respect to smoking bans in bars and restaurants' effect on extensive-margin smoking and cigarette purchases, I find no economically meaningful effects of smoking bans on smoking prevalence or purchases of cigarettes.

The results highlight the importance of thinking about the substitutability or complementarity of risky health behaviors when targeting one particular health behavior (in this instance, smoking). Changing the environment of bars to make smoking more difficult may have made bars more enjoyable places to drink.

This paper contributes to a literature in health economics on policies that target smoking and drinking, their respective effects on cigarette and alcohol consumption, and effects on related externalities.[6, 7] Earlier studies find mixed results of state-level smoking bans on alcohol consumption. Picone, Sloan, and Trogdon (2004) find that general smoking bans lead to reductions in alcohol consumption for older women while Koksal and Wohlgenant (2016) find that restaurant smoking bans lead to increases in restaurant alcohol consumption and decreases in at-home alcohol consumption. I extend this literature by incorporating city

[6]Other papers on smoking study the effects of policies such as cigarette taxes, smoking bans, and clean indoor air laws (Adda and Cornaglia, 2006; Adda and Cornaglia, 2010; Anger et al., 2011; Cotti, Nesson, and Tefft, 2016; Evans et al., 1999; Kvasnicka et al., 2018, and many others).

[7]Other papers on alcohol consumption study the effect of policies such as the Minimum Legal Drinking Age and restrictions on the sale of off-premises alcohol on Sundays (Carpenter et al., 2016; Lovenheim and Steefel, 2011; Nilsson, 2017; and many others).

and county-level smoking bans, which reduces the measurement error in the treatment status. Many of the early laws were implemented at the county and city level, and states typically implement smoking bans after some of their cities or counties.

The next section of the paper (section 2) describes the BRFSS and Nielsen data sources and provides information on smoking bans in bars and restaurants. Section 3 details the difference-in-differences framework and section 4 describes the results, including specifications that use the stacked difference-in-differences method and the DiD imputation estimator. Section 5 concludes.

2 Data

Measures of "treatment" (effective dates of smoking bans in bars and restaurants) come from the American Nonsmokers' Rights Foundation, which I match to outcomes using county-level geographic identifiers. Measures of alcohol consumption and smoking status come from the 2004 to 2012 waves of the Behavioral Risk Factor Surveillance System (BRFSS) and the Nielsen Consumer Panel.[8] Summary statistics by treatment status are in Table 1 and Appendix Table C.1 for the BRFSS data and Appendix Table C.2 for the Nielsen data.

2.1 Alcohol Consumption and Smoking Status

The BRFSS measures an individual's self-reported smoking status and frequency and amount of alcohol consumption (measured in servings of alcohol), but not location of alcohol

[8]These years have reliable county identifiers in both datasets. The Nielsen Consumer Panel does not start until 2004. Starting with the 2013 wave, BRFSS stopped publicly reporting county-level identifiers in the aggregated dataset due to privacy concerns. I use the same years in case smoking bans have heterogeneous effects over time; using different years for different datasets might lead to erroneous conclusions, particularly when comparing outcomes from the BRFSS and the Nielsen data.

consumption. For my sample period, 2004-2012, 80-90% of observations in the BRFSS contain county identifiers.[9] During this period, with the exception of Hawaii in 2004, all states (and Washington, D.C.) participate in the BRFSS each year. The BRFSS is designed to be representative at the state level.

I create alcohol-related outcomes from the BRFSS data using responses to four different questions: 1) whether individuals drank any alcohol during the past 30 days (extensive margin), 2) how many days in the past 30 individuals drank alcohol, 3) the average number of drinks consumed on the days an individual drank alcohol, and 4) the maximum number of drinks consumed on one occasion. Multiplying the number of days by the average amount consumed per day yields the total amount of alcohol consumed in the past 30 days (for individuals who drink), which measures intensive-margin consumption. Adding in non-drinkers' zero drinks to the intensive-margin measure yields the total amount of alcohol consumed in the past month. Smoking status comes from two questions: 1) whether individuals have smoked at least 100 cigarettes during their lifetime, and 2) if yes, whether they smoke every day, some days, or not at all. Respondents answering no to the first question are classified as "never smokers". I classify individuals who report smoking every day or some days as current smokers and those who report smoking not at all as former smokers.

The Nielsen data contain scanned-in household-level cigarette and alcohol purchases from grocery stores, convenience stores, liquor stores, and other sources of off-premises consump-

[9]BRFSS suppresses county identifiers if fewer than 50 respondents live in the same county.

tion.[10,11] I use the county-level geographic identifier in the Nielsen data. Nielsen's sampling procedures are designed such that the data are representative at the national level. The scanned-in alcohol purchases provide details on both the quantity purchased and the UPC code (e.g., a 6-pack of Blue Moon wheat beer or 1 bottle of Chateau Ste. Michelle Cabernet Sauvignon wine). I convert alcohol purchases into servings of alcohol to make them comparable. Twelve ounces of beer, 5 ounces of wine, or 1.5 ounces of liquor are one serving of alcohol.[12] The Nielsen data do not included alcohol purchased for on-premises consumption, such as alcohol purchased and consumed at a bar.

The three alcohol-related outcomes from the Nielsen data are the total quantity of alcohol purchased, the prevalence of purchasing alcohol, and the quantity of alcohol purchased for households that purchased any alcohol. The first is the total servings of all types of alcohol purchased in a month, while the second is a measure of whether a household purchased any alcohol for off-premises consumption in a month. For regressions of alcohol consumption by smoking status, I infer smoking status by whether the household scanned in any cigarettes in the current calendar year. I use the year instead of the same month to allow for infrequent (or stockpiled) purchases of cigarettes. I use scanned-in cigarette purchases to measure

[10]Participating households are provided UPC scanners and instructed to scan all of their purchases that are intended for at-home consumption. Scanned-in purchases could underreport alcohol and cigarettes; for example, if an item is consumed before the panelist arrives home (e.g., a bottle of wine bought for dinner at a friend's house). Another source of underreporting is the purchase of alcohol and cigarettes by underage consumers. It is not problematic that purchases by teenagers are excluded because I am estimating the effect of smoking bans on adults' behavior, so it would make the Nielsen data more comparable to the BRFSS.

[11]Other papers use the Nielsen Consumer Panel data to measure cigarette and or alcohol purchases (e.g., Cotti, Dunn, and Tefft, 2015; Cotti, Nesson, and Tefft, 2018; and Janssen and Parslow, 2021).

[12]This conversion is not exact as a serving of alcohol depends also on alcohol by volume (ABV). Twelve ounces of 5% ABV beer constitutes one serving of alcohol, 5 ounces of 12% ABV wine constitutes one serving, and 1.5 ounces of 40% ABV liquor constitutes one serving (NIAAA). Higher ABV beers have become more common in recent years. Some liquor has a higher ABV than 40%, such as Absinthe (at least 45% ABV), while others have a lower ABV, such as Irish Cream (15-20% ABV). While my conversion process generates some measurement error, as long as the purchase of beer or liquor with non-standard ABV is uncorrelated with the implementation of smoking bans, it does not present a problem for my analysis.

extensive and intensive-margin purchases. The extensive margin is measured as whether the household purchased any cigarettes in the past month, and the intensive margin is measured as the number of packs of cigarettes a smoking household purchased in the past month.

2.2 Smoking Bans

The map in Figure 1 shows the timing of smoking bans in bars that were implemented prior to December 31, 2012 (the end of my sample period). Always-treated counties, earlier adopters, later adopters, and never-treated counties are shaded different colors. Earlier adopters are concentrated in the West and the Northeast, while later adopters are primarily in the upper Midwest. The South had a mix of early and late adopters.

Incorporating city-level smoking bans is important because in the South, many cities implemented smoking bans in bars in the absence of legislation at the county or state level. An analysis that excludes city-level bans will consider most of the South as untreated, when much of that population is actually subject to a smoking ban in bars. As indicated in the map, there is quite a bit of spatial and temporal variation in the implementation of the laws.

My measure of treatment is the fraction of the county population that is subject to a smoking ban in both bars and restaurants. If a county has implemented a smoking ban, or the corresponding state, the treatment variable takes a value of 1. If some but not all cities in a county have implemented a smoking ban, the treatment variable takes a value strictly between 0 and 1. As a control variable, I include the fraction of the county that is subject to a smoking ban in restaurants only. I have constructed the variables in this way because there are very few places that have smoking bans in bars but not restaurants. Except for a handful

of small cities, every jurisdiction that implemented a smoking ban in bars prior to December 2012 had either previously implemented a smoking ban in restaurants or implemented such a ban simultaneously. The policy-relevant regulation, because I am focusing on behavioral responses to banning smoking in bars, is therefore smoking bans in bars and restaurants.

2.3 Control Variables

Demographic characteristics for the BRFSS regressions come from the BRFSS. I control for age, marital status, sex, race, educational attainment, and employment status. In lieu of demographic characteristics for the Nielsen regressions, I use household fixed effects, which capture any time-invariant household characteristics.

I also include measures of state-level alcohol and tobacco policies. I use the state-level legal blood alcohol concentration (BAC) limit for driving under the influence from the Alcohol Policy Information System (APIS), a database compiled by the National Institute on Alcohol Abuse and Alcoholism (NIAAA). State-level cigarette taxes come from the Tax Burden on Tobacco (TBOT).[13]

3 Methods

To identify causal effects of smoking bans in bars and restaurants on alcohol consumption, smoking, and alcohol consumption by smoking status, I estimate a difference-in-differences model. I exploit variation in the timing of effective dates of these smoking bans, incorporating bans implemented at the city, county, and state level.

[13]Summary statistics for the control variables are shown in Appendix Table *C*.1.

3.1 Difference-in-Differences Identification and Assumptions

Two assumptions are needed for a difference-in-differences estimate to capture a causal effect:

1. *Parallel trends:* in the absence of smoking bans in bars and restaurants, trends in outcomes, conditional on control variables, would be the same across treated and untreated counties

2. At the time of the implementation of smoking bans in bars or restaurants, there are no other changes occurring in treated jurisdictions that affect the outcomes, conditional on the control variables

Section 3.3 and Appendix *A* outline potential instances in which these assumptions may not be satisfied. To partially assess the validity of the parallel trends assumption, I conduct event studies, which are described in section 4.3.

3.2 Reduced-Form Regression Equation

I estimate the following reduced-form Ordinary Least Squares equations for various measures of alcohol consumption and smoking:

$$Y_{i,c,t} = \alpha + \beta \cdot ban_{c,t} + \mathbf{X}_{i,c,t} \cdot \gamma + \delta_c + \rho_t + \delta_{r,t} + \varepsilon_{i,c,t} \tag{1}$$

$Y_{i,c,t}$ denotes the alcohol (or smoking-related) outcome for individual i living in county c in month t. My primary measures of alcohol consumption using the BRFSS data are the total amount of alcohol consumed in the past 30 days, whether an individual consumed any

alcohol in the past 30 days (extensive margin), and the total amount of alcohol consumed in the past 30 days if the individual drinks (intensive margin). In Appendix *B*.1 I disaggregate the measure of total alcohol consumption in the past 30 days into the number of days an individual drank (in the past 30 days) and the average amount of alcohol consumed on days an individual drank. For alcohol purchased for off-premises consumption (Nielsen Consumer Panel data), my primary measures are the total quantity of alcohol purchased in the past month, whether the household scanned in any alcohol purchases in the past month (extensive margin), and how much alcohol was scanned in for households that purchased any alcohol (intensive margin). The total quantity measure is a proxy for the amount of alcohol consumed at home and the extensive-margin measure is a proxy for whether alcohol was consumed at home. My measures of smoking using the BRFSS data are whether an individual reports being a current, never, or former smoker. For the Nielsen data, smoking measures are whether a household purchased any cigarettes in the past month, and the number of packs of cigarettes purchased by smoking households.

In my main specification, $ban_{c,t}$ represents the fraction of the county population subject to a smoking ban in both bars and restaurants in month t in individual i's county c. I also control for the fraction subject to a smoking ban in restaurants but not bars in county c and month t, which is included in the vector $\mathbf{X}_{i,c,t}$. The omitted category is "no smoking ban in bars or restaurants".

For the BRFSS regressions, $\mathbf{X}_{i,c,t}$ represents a vector of demographic characteristics and policy variables. I include age (in 5-year bins), marital status (never married, married, widowed, separated, divorced, and unmarried but cohabitating), sex, race (mutually exclusive categories for Hispanic, Black, Asian, American Indian/Alaska Native, Native Hawai-

ian/Pacific Islander, multiracial, white, or other races), education (less than high school, high school or equivalent, some college, or college degree), and employment status. In the Nielsen regressions, the demographic controls are replaced with household fixed effects. In both regressions, policy variables are the state-level legal limit for blood alcohol concentration for operating a motor vehicle; and the state-level cigarette tax.[14] I include state-level policy variables because anti-smoking measures, such as cigarette taxes and smoking bans, are frequently implemented in conjunction with each other. I control for these other policies to ensure that I am not conflating the effects of smoking bans with the effects of other anti-smoking policies.

The equation also includes county (δ_c), month-year (ρ_t), and region-by-month-year ($\delta_{r,t}$) fixed effects. I cluster the standard errors, $\varepsilon_{i,c,t}$, at the county level. I weight the regressions by the sample weights provided by BRFSS or Nielsen.

In section 4.6, I estimate the effects of smoking bans on alcohol consumption for each smoking status, using Equation 1 and restricting the sample to the relevant smoking status. Smoking status varies between the Behavioral Risk Factor Surveillance System (BRFSS) data and the Nielsen Consumer Panel data. With the BRFSS data I distinguish between current, never, and former smokers. With the Nielsen data, I infer smoking status (smoker or nonsmoker) from whether the household scanned in any cigarettes during the year (to account for potential stockpiling or infrequent cigarette purchases).

[14] Counties are subsets of states, which is why I can include time-varying state-level characteristics in a vector of time-varying county-level characteristics.

3.3 Potential Endogeneity of Smoking Type

One of the primary motivations of these bans was to induce smokers to quit. If they were effective, some individuals would quit smoking and others would not initiate smoking, which would cause the smoking and nonsmoking groups to change over time. Prior research finds anti-smoking policies lead some people to quit smoking (e.g., Evans et al., 1999; Bharadwaj et al., 2014) and prevent others from initiating smoking (Liu, 2010). If smoking bans in bars and restaurants are having these effects on smokers or would-be smokers during my sample period, the untreated groups would not be valid counterfactuals for the treated groups. My estimates of the effect of the smoking bans on alcohol consumption for each type would be biased if alcohol consumption was correlated with an individual's propensity to quit (or not initiate) smoking.

For example, suppose smoking bans have no effect on smokers' alcohol consumption. Also suppose that smoking bans in bars and restaurants induced the smokers who were the heaviest drinkers to quit smoking, thereby switching from "current smoker" to "former smoker". Average alcohol consumption among current smokers would mechanically decrease, making it appear that smoking bans induced smokers to quit drinking when in reality, smoking bans induced drinkers to quit smoking.

To address this potential endogeneity issue, I directly test the effect of smoking bans in bars and restaurants on smoking status by Equation 1 with indicators for smoking status on the left-hand side using the BRFSS data. I also estimate the effect on any cigarette purchases and smokers' quantity of cigarette purchases in the Nielsen data. I describe the results in more detail in Section 4.2, but during this time period (2004-2012), bar and restaurant

smoking bans generally do not have an economically meaningful effect on the prevalence of smoking.

3.4 Stacked Difference-in-Differences Method

Numerous papers in recent years have highlighted potential issues with difference-in-differences models that rely on staggered timing. Using always treated or already-treated groups as controls for later-treated groups can yield biased effect sizes (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). To address this issue, I conduct a robustness check using a stacked difference-in-differences method, where for each treated group, the control group consists of not-yet-treated or never-treated units. I follow Deshpande and Li (2019) in constructing the dataset and estimating the regression.

I create four different datasets, one for each treatment year, that include observations from 3 years before a smoking ban is implemented in bars through 2 years after for the treated group.[15] For the untreated group, I include observations that are treated more than 2 years in the future or not at all during the sample period, keeping the same years as the treated group. Event time is relative to the treatment year. Each of these datasets is identified by the treatment year, or stack. I then append the four datasets to each other, which allows for a county-year to appear in the final dataset multiple times (as both treated and untreated units for different treatment years). The event study specification regresses the outcome on event time indicators interacted with treatment indicators, the controls listed earlier, county-by-stack fixed effects, year-by-stack fixed effects, and region-by-year

[15]The four treatment years are 2007, 2008, 2009, and 2010. Those are the only years that have both a 3-year pre-period and a 2-year post-period in the sample, which runs from 2004 to 2012.

fixed effects. The standard errors are clustered at the county-by-stack level and regressions are probability weighted by the sample weights.

4 Results

4.1 Alcohol Consumption (BRFSS)

Table 2 presents the baseline results for the effect of smoking bans on alcohol consumption using the BRFSS data. Each column presents results from a slightly different specification. Column 1 is a "plain" regression that only includes county and time (month-year) fixed effects. Column 2 adds demographic controls, region-by-month-year fixed effects, and policy controls: an indicator for the blood alcohol concentration limit being 0.08 and the per-pack cigarette tax; this is my preferred specification. Column 3 switches the treatment variable from the fraction of the county that is treated to an indicator, with 1 indicating that any part of the county has a smoking ban. Column 4 presents results using the stacked difference-in-differences approach (with annual stacks). Column 5 presents results using the DiD imputation estimator detailed in Borusyak, Jaravel, and Speiss (2022). The results are robust to clustering standard errors at the state level instead of the county level.

Panel A shows the effect on total alcohol consumption, measured as the total servings of alcohol consumed in the past 30 days. The effect on total alcohol consumption is stable across specifications, although the stacked difference-in-differences and imputation estimator specifications have larger point estimates. In my preferred specification (Column 2), smoking bans in bars and restaurants lead to an increase of 0.58 servings of alcohol consumed per

month, a 5.09% increase. This effect is significant at the 1% level. The stacked difference-in-differences point estimate is 0.84, a 7.38% increase that is also significant at the 1% level. The DiD imputation estimator coefficient is 1.08, which is a 9.42% increase that is significant at the 1% level.

Panel B shows the effect on the prevalence of drinking any alcohol in the past 30 days (extensive margin). The estimates are stable across the first 3 columns: precisely estimated null effects. In the preferred specification (Column 2), smoking bans are associated with a 0.18 percentage point increase in the probability of drinking any alcohol in the past 30 days, a 0.33% increase that is not statistically significant. The stacked difference-in-differences and DiD imputation point estimates are statistically significant at the 5% level and larger compared to the other estimates, although still economically small (0.71 percentage points; a 1.33-1.38% increase).

Effects on the intensive margin of alcohol consumption (servings of alcohol per 30 days for individuals who drink) are in Panel C. The estimates are again relatively stable across specifications, although the stacked and imputation difference-in-differences estimates are slightly larger. In the preferred specification, smoking bans lead to an increase of 0.97 servings of alcohol, a 4.49% increase that is significant at the 5% level. The stacked difference-in-differences point estimate is 1.29 and the DiD imputation estimate is 1.42, representing a 5.77-6.57% increase that is statistically significant at the 1% level.

4.2 Smoking (BRFSS)

Estimating the effect of smoking bans on smoking is important in its own right and can also indicate whether the potential endogeneity of smoking status with respect to alcohol consumption is likely to be a concern. For the most part, I estimate precise null effects of smoking bans on smoking status, with the exception of the stacked difference-in-differences specification. In the preferred specification (Column 2 of Table 3), bar and restaurant smoking bans are associated with a 0.23 percentage point increase in the prevalence of current smoking (1.13%), a 0.27 percentage point decrease in never smoking (0.48%), and a 0.04 percentage point increase in former smoking (0.15%). None of these effects are statistically significant. On the other hand, the stacked difference-in-differences specification implies that some former smokers become current smokers: the prevalence of current smoking increases by 0.62 percentage points (3.00%) while the prevalence of former smoking declines by 0.72 percentage points (2.94%). These effects are significant at the 5 and 1% level, respectively. The DiD imputation estimates are more in line with the TWFE specification: current smoking increases by 0.39 percentage points (1.93%), never smoking declines by 0.27 percentage points (0.48%), and former smoking declines by 0.13 percentage points (0.52%). None of those estimates are statistically significant. Even if one gives more credibility to the stacked difference-in-differences estimates, any effect of smoking bans on smoking is economically small. Nevertheless, they do indicate that effects on alcohol consumption by smoking status should be interpreted with some caution as any effects may be driven by compositional changes in the smoking status groups.

These results may seem different at first glance than earlier work that finds (workplace)

smoking bans reduce the prevalence of smoking (e.g., Evans, Farrelly, and Montgomery, 1999). However, these earlier papers study earlier time periods when smoking prevalence was higher and the marginal smoker presumably had a more elastic demand for cigarettes.[16] Furthermore, the stacked difference-in-differences estimates could be consistent with a model of cue-triggered decision processes (Bernheim and Rangel, 2004). Prior to smoking bans, former smokers may have avoided bars because they did not want to be around people smoking, for fear that could trigger them to start smoking again. After smoking bans are implemented, they may feel more comfortable going to bars but accidentally encounter smoking (e.g., people smoking right outside the bar), leading them to take up smoking again.

4.3 Event Studies

Event studies test whether parallel pre-trends hold and also highlight dynamic treatment effects. I use a pre-period window of 4 years, a post-period window of 5 years, and omit the year prior to implementation as the reference point.

In my primary specifications, I use the fraction of the county population subject to a smoking ban in bars and restaurants as my treatment variable. However, an event study requires one implementation date. Therefore, I consider the year of implementation of a smoking ban in a bar to be the first year where any part of the county has implemented a smoking ban.[17,18]

[16] Figures 2 and 3 in DeCicca, Kenkel, and Lovenheim (2022) document the precipitous decline in smoking prevalence in the U.S. in the last 55-60 years.

[17] Any definition of treatment will create measurement error in my treatment variable, as for some time periods, only parts of some counties are covered by a smoking ban. I must consider the county as fully or not-at-all treated in an event-study framework. The reason for using "any" law is that I do not want to include treated individuals in the pre period. As a result, there are untreated individuals in the post period, which may attenuate the post-period coefficients.

[18] Out of all observations corresponding to counties with at least one smoking ban, 83% were covered by

The event-study equation is

$$Y_{i,c,t} = \alpha + \sum_{k=-4, k\neq -1}^{k=5} \beta_k \cdot ban_{k,c,t} + \mathbf{X}_{i,c,t} \cdot \gamma + \delta_c + \rho_t + \delta_{r,t} + \varepsilon_{i,c,t} \quad (2)$$

$Y_{i,c,t}$ represents the smoking or drinking-related outcome for individual i living in county c in year t. $ban_{k,c,t}$ equals 1 if a smoking ban in a bar has been in place in any part of county c for k years as of year t (k ranges from -4 to 5, and $k = -1$ is omitted). The control variables and fixed effects are the same as in the original specification, standard errors are again clustered at the county level, and regressions are weighted using the sample weights.

For the total quantity of alcohol consumption, the pre-period coefficients are close to zero and not statistically significant (top panel of Figure 2). After a smoking ban is implemented, the coefficients become positive and increase to $\frac{1}{2}$ to $1\frac{1}{2}$ drinks per month. Starting 1 year after implementation they are individually statistically significant at the 5% level. The effects increase slightly over time. The graph shows similar results as the difference-in-differences point estimate for overall alcohol consumption.

For extensive-margin consumption (bottom panel of Figure 2), the coefficients are small and generally not statistically significant, which is consistent with the difference-in-differences null result for extensive-margin alcohol consumption. For intensive-margin alcohol consumption (top panel of Figure 3), the pre-period coefficients are small and not statistically significant. The post-period effects are positive and steadily increasing over time, from no change

laws that affected at least half the county population that were implemented in the same year as the first law. 10% of the observations corresponding to counties with at least one smoking ban were never covered by laws that affected at least half the county population by the end of the sample period. The remaining 7% of observations had laws that covered at least half the county population that were implemented sometime after the first law. Using the date that half the county population was covered by a smoking ban as the date of implementation yields broadly similar results.

in the year of implementation to an increase of approximately 2 drinks by year 5. The individual coefficients for years 1 to 5 are statistically significant at the 5% level. This graph is consistent with the difference-in-differences point estimate of an increase of 1.1 drinks per month.

For the effects of smoking bans on current smoking (bottom panel of Figure 3), the pre-period coefficients are close to zero and not statistically significant. In the post period, the coefficients are also small (magnitude of less than $\frac{1}{2}$ percentage point) and not individually significant. These results are consistent with the difference-in-differences point estimates of no economically meaningful effect of smoking bans on the prevalence of current smoking. For never smoking (top panel of Figure 4), the pre-period coefficients are positive but close to zero. The post-period coefficients fluctuate around zero and are individually not statistically significant. For former smoking (bottom panel of Figure 4), both the pre and post-period coefficients tend to be negative but quite small and not statistically significant.

The event study for extensive-margin cigarette purchases (top panel of Figure 5) mirrors the one for current smoking (bottom panel of Figure 3). The pre-period coefficients are small, negative, and not statistically significant and the post-period coefficients are small, positive, and not significant. For intensive-margin cigarette purchases (bottom panel of Figure 5), the pre-period coefficients are small, negative, and not significant and the post-period coefficients are small, fluctuate around 0, and not significant. These event studies, using a different dataset, confirm that smoking bans in bars and restaurants did not have an effect on smoking.

4.4 Event Studies Robustness Checks

The stacked and DiD imputation estimator event studies are shown in the Appendix and are similar to the TWFE event studies. For the total amount of alcohol consumed in the stacked event study specification (top panel of Appendix Figure *C*.1), there is no pre-trend, while in the years after a smoking ban in bars is implemented, alcohol consumption increases by approximately $\frac{3}{4}$ of a drink per month. The coefficients for years 1 and 2 are individually statistically significant at the 10% level. These results are quite similar to the traditional difference-in-differences event study results in the top panel of Figure 2. For the extensive margin (bottom panel of Appendix Figure *C*.1), the coefficients in both the pre and post periods are small, but the post-period coefficients are statistically significant at the 10% level for years 0 and 2. These results are also similar to the traditional event study results shown in the bottom panel of Figure 2. For the intensive margin (top panel of Appendix Figure *C*.2), the pre-period coefficients are small, negative, and not statistically significant, while in the years following a smoking ban, alcohol consumption increases by a little over 1 drink per month. The post-period coefficients are also not individually statistically significant and are slightly attenuated relative to the traditional event study results shown in the top panel of Figure 3. Overall, the stacked difference-in-differences event study results are similar to the main findings and support the hypothesis that smoking bans in bars and restaurants led to increases in alcohol consumption.

The stacked event studies for smoking status are also broadly similar to the traditional event study specification. For current smoking (bottom panel of Figure *C*.2), the pre-period coefficients are small and negative while the post-period coefficients are small and positive,

and not statistically significant. For never smoking (top panel of Figure C.3), the pre-period coefficients are small and positive and the post-period coefficients are small and fluctuate around 0; none are individually significant. For former smoking (bottom panel of Figure C.3), the coefficients are small, negative, and not significant.

The DiD imputation event studies, shown in Appendix Figures C.4 to C.6, also show increases in alcohol consumption and small or no changes in smoking status, although the pre-period standard errors are relatively large. For overall alcohol consumption (top panel of Appendix Figure C.4), the pre-period coefficients are very close to 0 and the post-period coefficients are individually statistically significant and increase over time to about 2 additional drinks per 30 days 5 years after smoking bans are implemented. Intensive-margin alcohol consumption (top panel of Appendix Figure C.5) also increases to 2-3 drinks per 30 days several years after smoking bans, while the pre-period coefficients are small and just below 0. For each smoking status (bottom panel of Appendix Figure C.5 to bottom panel of Appendix Figure C.6), the pre and post-period coefficients are relatively similar, small in magnitude, and generally not individually significant.

4.5 Alcohol Purchases for Off-Premises Consumption (Nielsen)

Analyzing the effect of smoking bans on off-premises alcohol purchases (a proxy for alcohol consumed at home) using the Nielsen data can provide insight into how smoking bans affect the likely location of alcohol consumption (Appendix Table C.3). In my preferred specification (Column 2), the implementation of smoking bans in bars and restaurants is associated with an average decrease in the quantity of servings of alcohol purchased for off-

premises consumption of 0.34 drinks per month (Panel A), an approximately 2.5% decline that is not statistically significant. The stacked difference-in-differences coefficient is similar in magnitude (-0.28), while the DiD imputation estimate is identical in magnitude but positive. Neither of these estimates are statistically significant. Smoking bans in bars and restaurants do not appear to have an effect on the total quantity of alcohol purchased for off-premises consumption.

Similarly, smoking bans do not have much of an effect on whether households purchased any alcohol for off-premises consumption in the past month. The primary TWFE coefficient is very small and negative (-0.08, a 0.33% decline) while the stacked and DiD imputation coefficients are small and positive (0.39 and 0.38, respectively; a 1.5% increase). None of these estimates are statistically significant.

Along the intensive margin, the primary TWFE and stacked specifications find small negative effects: -0.56 and -0.76 servings of alcohol per month, approximately a 1% decrease. Neither is statistically significant. The DiD imputation estimate, on the other hand, is positive and statistically significant at the 1% level: 3.02 drinks per month, a 5.6% increase. This one result contrasts with every other alcohol purchases specification and outcome. Even if the intensive-margin DiD imputation estimate is taken at face value, the total quantity of alcohol purchased by households (0.34 servings of alcohol per month; Panel A Column 5) is markedly smaller than the effect size for total alcohol consumption in the BRFSS (1.08; Panel A Column 5 of Table 2), which provides suggestive evidence that even if intensive-margin purchases for alcohol consumed at home increase, total alcohol consumption is also likely increasing at bars and restaurants.

4.6 Heterogeneous Effects of Smoking Bans

The net increases in alcohol consumption in Section 4.1 may mask heterogeneous effects for different groups, such as age or smoking status. Younger adults are more likely to drink and drink more alcohol than older adults, so a smoking ban in a bar may have more of an effect on them. They may also be more likely to frequent bars. With respect to smoking status, a smoking ban in a bar likely has differential effects on the non-price determinants of demand (the bar atmosphere) for smokers and nonsmokers, which means they may respond differently to this policy. Understanding who is changing their behavior and in what ways is crucial for understanding the policy implications and the ways in which these results may generalize to other settings. Given that I do not find meaningful changes in smoking prevalence, the potential endogeneity of smoking status is likely not a concern in this context.

The effects of smoking bans on alcohol consumption are concentrated among younger (of-age) adults, as shown in Table 5. For 18-20 year-olds, who are not legally able to drink but may do so anyway, smoking bans in bars and restaurants have minimal effect on their alcohol consumption. Adults between the ages of 21 and 34 increase their alcohol consumption by approximately 1 drink per 30 days, a nearly 7.5% increase. This effect is marginally statistically significant. There is no change along the extensive margin and a marginally significant 1.7-drink-per-30-days increase along the intensive margin (7.4%). Alcohol consumption also increases for 35-54 year olds but by a lesser amount: 0.5 drinks per 30 days (4.3% increase that is marginally significant). The intensive-margin effect is slightly larger but not statistically significant. Adults 55 and older have minimal increases in alcohol consumption (not statistically significant) in response to smoking bans.

Disaggregating effects on alcohol consumption by smoking status shows that increases in alcohol consumption are concentrated among current and former smokers (Table 6). Current smokers increase their alcohol consumption by approximately 1.5 drinks per 30 days (7.4% increase), which is marginally statistically significant. Smoking bans lead former smokers to drink a little over 0.5 additional drinks per 30 days (4.5% increase), which is also marginally significant. There is a marginally significant but very small decline in the extensive margin of alcohol consumption for current smokers (0.9 percentage points, a 1.5% decrease). Along the intensive margin, current smokers increase their alcohol consumption by a little over 3 drinks per 30 days (9% increase), an effect that is statistically significant at the 5% level. Given the small but marginally significant reduction in extensive-margin drinking for current smokers, it is possible that at least some of the intensive-margin increase in alcohol consumption is a result of compositional changes (e.g., the smokers who drank the least were the ones who quit drinking). Former smokers' alcohol consumption increases by 1 drink over 30 days, a 4.5% increase that is also significant at the 5% level.

Appendix *B*.1 includes results for disaggregated measures of alcohol consumption in order to analyze along what margins individuals are changing their alcohol consumption. Are they drinking on more days throughout the month, are they drinking more alcohol on the days they drink, or are they doing both? Understanding the effects at a more detailed level can illustrate whether these changes in drinking behavior may have negative health consequences.[19]

Turning to alcohol purchases, the effects of smoking bans by smoking status are similar to

[19]Taking up binge drinking and going from two to six drinks one night each week has different health effects than drinking two drinks each on an additional two days per week (even though the total change in weekly alcohol consumption is the same). Binge drinking is associated with negative health effects such as alcohol poisoning and other unintentional injuries (CDC, 2019).

the overall results for alcohol purchases. After the implementation of smoking bans in bars and restaurants, smoking households' monthly alcohol purchases for off-premises consumption decline by 0.30 servings of alcohol (Panel A, Column 1 of Table C.4) and nonsmoking households' purchases decline by 0.29 servings of alcohol (Column 2). Neither of these effects are statistically significant and they are economically small, representing a 1.5% and 2.5% decline, respectively. Along the extensive margin, smoking households are 1.02 percentage points less likely to purchase alcohol after a smoking ban, which is marginally significant and represents a 3.4% decrease. Nonsmoking households are 0.14 percentage points more likely to purchase alcohol, although this effect is neither statistically significant nor economically meaningful (0.6% increase). Along the intensive margin, smoking households' alcohol purchases, conditional on purchasing alcohol, increase by 2.45 servings of alcohol per month. This effect is not statistically significant and is small (3.7% increase), and given the reduction in purchases along the extensive margin, could be driven by compositional changes. Households that did not buy as much alcohol may have been the ones to cut back on the extensive margin, which would make the intensive margin number mechanically increase. For nonsmoking households, intensive-margin alcohol purchases decreased by 0.86 servings per month, a 1.8% decrease that is not statistically significant.

5 Conclusion

The presence of externalities are a commonly accepted reason for governments to intervene in markets. In the case of cigarettes, the secondhand-smoke externality has well-

documented negative health consequences.[20] In this paper, I use the Behavioral Risk Factor Surveillance System and the Nielsen Consumer Panel to test whether smoking bans in bars and restaurants have unintended consequences with respect to alcohol consumption. To identify causal effects of smoking bans on these outcomes, I estimate a difference-in-differences model where my identifying variation is variation in effective dates of smoking bans in bars and restaurants at the city, county, and state level.

Smoking bans in bars and restaurants result in average increases in alcohol consumption of approximately 1 drink per month (conditional on drinking), or 5%. These increases occur for current and former smokers, are more pronounced for younger adults, and are driven by small increases in the average amount of alcohol consumed on each occasion. These small increases in alcohol consumption probably do not have negative health effects.

What are mechanisms by which current and former smokers would drink more as a result of smoking bans? If these increases are coming from bars and restaurants, a smoking ban may have made the bar more enjoyable for everybody, as even smokers may derive disutility from (other smokers') cigarette smoke. Additionally, if smoking ban in bars encourage new customers (nonsmokers) to go to bars, existing customers (smokers) may stay longer and drink more as bars have become more social places. Alternatively, smokers may have switched to drinking more at home so they could smoke while they drink. Smokers may then drink more overall because alcohol purchased for off-premises consumption is cheaper.

Former smokers may have an additional reason for increasing their alcohol consumption. Prior to smoking bans, they may have been avoiding smoke-filled bars and restaurants, for

[20] In other contexts, smoking bans in bars and restaurants led to improvements in population health with respect to smoking and secondhand-smoke-related health outcomes (e.g., Anger, Kvasnicka, and Siedler, 2011; Bharadwaj, Johnsen, and Løken, 2014; Jones et al., 2015; and Kvasnicka, Siedler, and Ziebarth, 2018).

fear that being around other people smoking may trigger them to take up smoking again (the cue-triggered model of decisionmaking described in Bernheim and Rangel, 2004).[21] After smoking bans are implemented, former smokers may feel more comfortable going out to bars and restaurants, or more comfortable staying there longer, hence their alcohol consumption would increase.

How do these effect sizes compare to other policies that affect alcohol consumption? The overall effect on alcohol consumption (0.58 drinks per month) is much smaller than the change in alcohol consumption at the minimum legal drinking age in Canada. Upon reaching legal age, young adults' monthly alcohol consumption increases by approximately 5 drinks per month, which is eight times larger than the effect of smoking bans (Carpenter, Dobkin, and Warman, 2016). Stehr (2007) finds that repeal of a ban on Sunday alcohol sales leads to a 2.4% increase in beer sales and a 3.5% increase in liquor sales. Assuming sales are a good proxy for alcohol consumption, the effect of a Sunday sales ban is slightly smaller than the effect of a smoking ban (5% increase in overall alcohol consumption).

An interesting direction for future research would be to test for heterogeneity in the policy impacts; for example, whether smokers are exploiting the spatial heterogeneity in the policy and avoiding the ban by accounting for border county policies or the distance to the nearest county with a different policy (following Adams and Cotti, 2008). One limitation of this paper is that I am unable to directly estimate the effect of smoking bans on the location of alcohol consumption. I provide some suggestive evidence on location of consumption by comparing the effect on overall alcohol consumption in the BRFSS to the effect on alcohol purchased for home consumption using the Nielsen. To the extent that there are

[21] Alternatively, they may have gone to bars and restaurants but not stayed very long.

differences in these datasets in terms of their accuracy in measuring alcohol servings or their representativeness, those differences could be contributing to the effect sizes that I estimate.

When risky health behaviors are substitutes or complements, a policy change targeting one risky health behavior can have spillover effects on another. In this instance, a policy ostensibly aimed at minimizing smoking and secondhand smoke had unintended consequences for alcohol consumption. Laws regarding risky health behaviors and their externalities need to anticipate the behavioral responses arising from their substitutability or complementarity.

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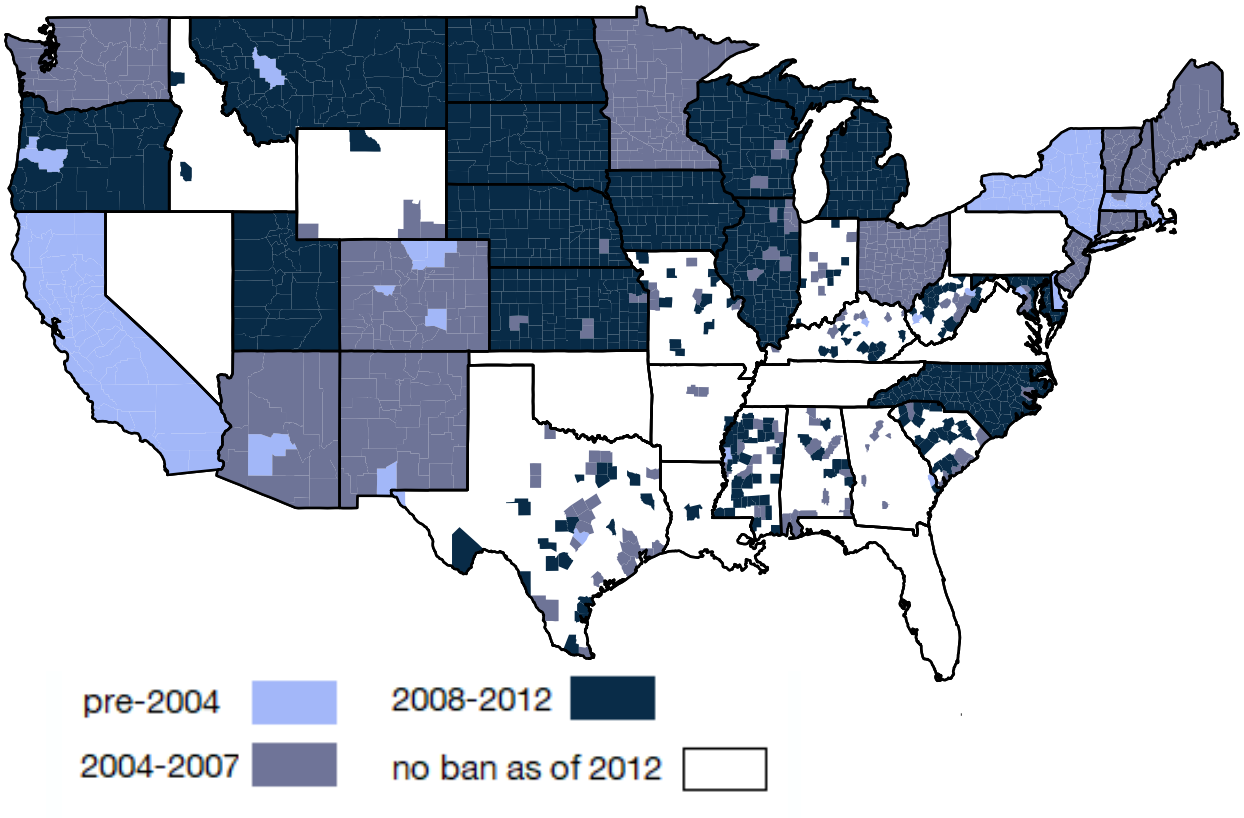
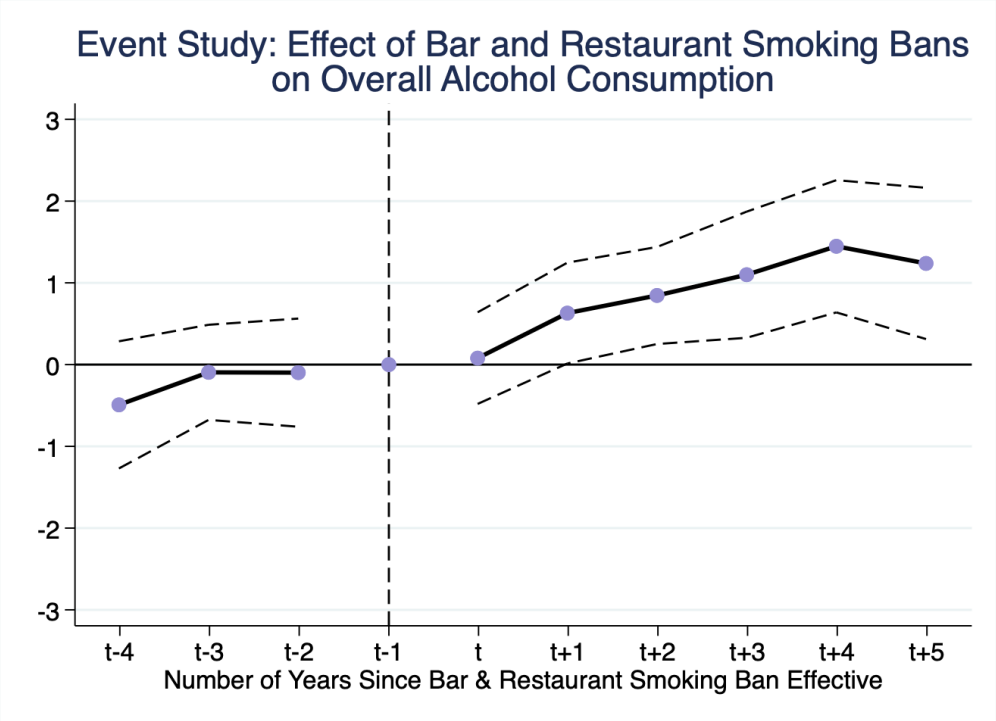
7 Figures and Tables

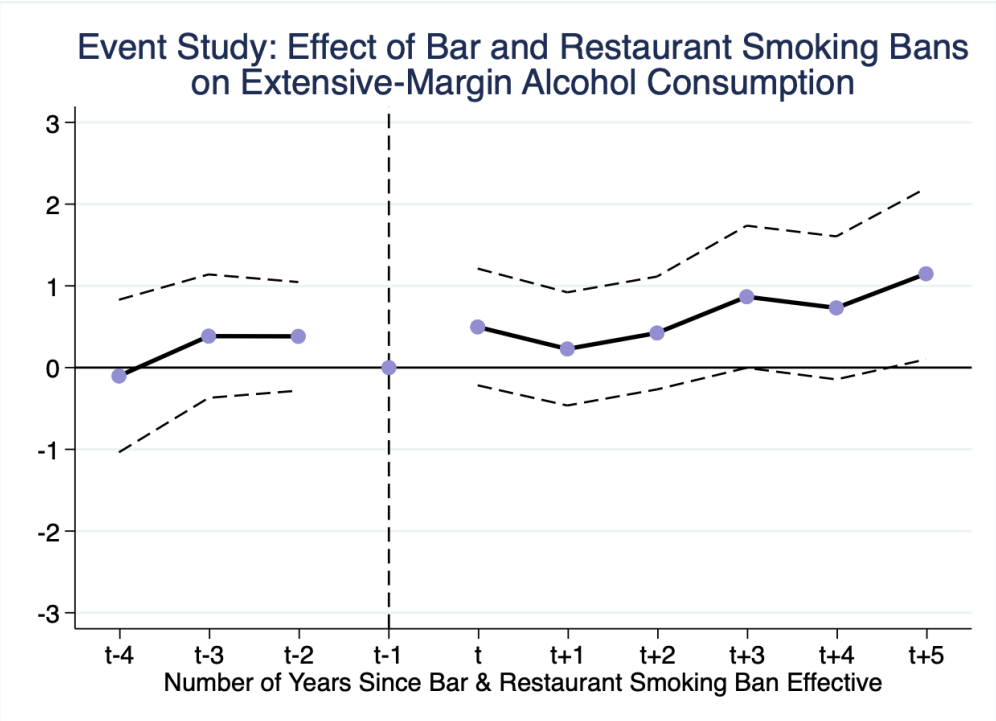
Figure 1: Map of Smoking Bans in Bars Implemented by Cities, Counties, and States by December 31, 2012



Data Source: American Nonsmokers' Rights Foundation

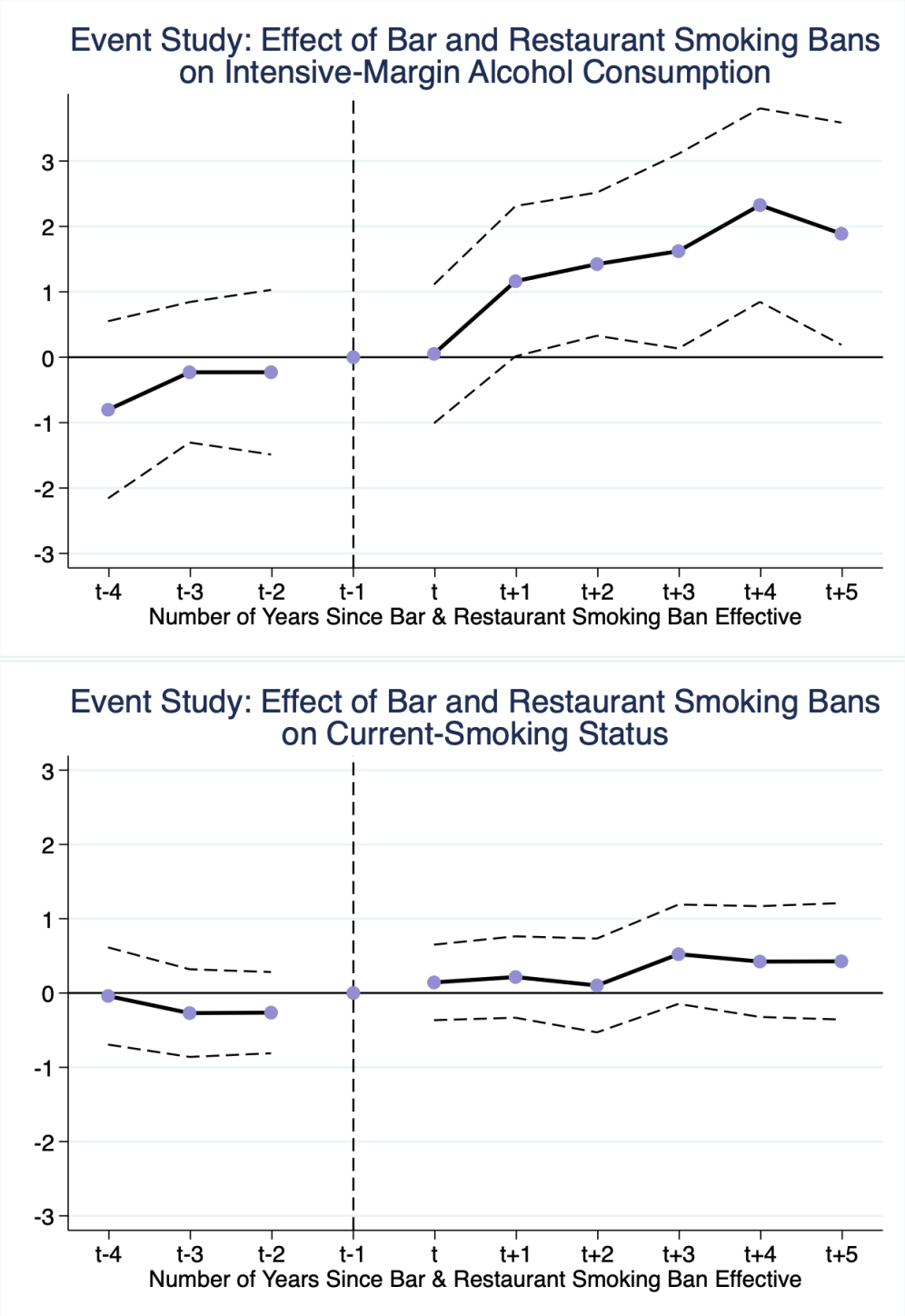
Figure 2





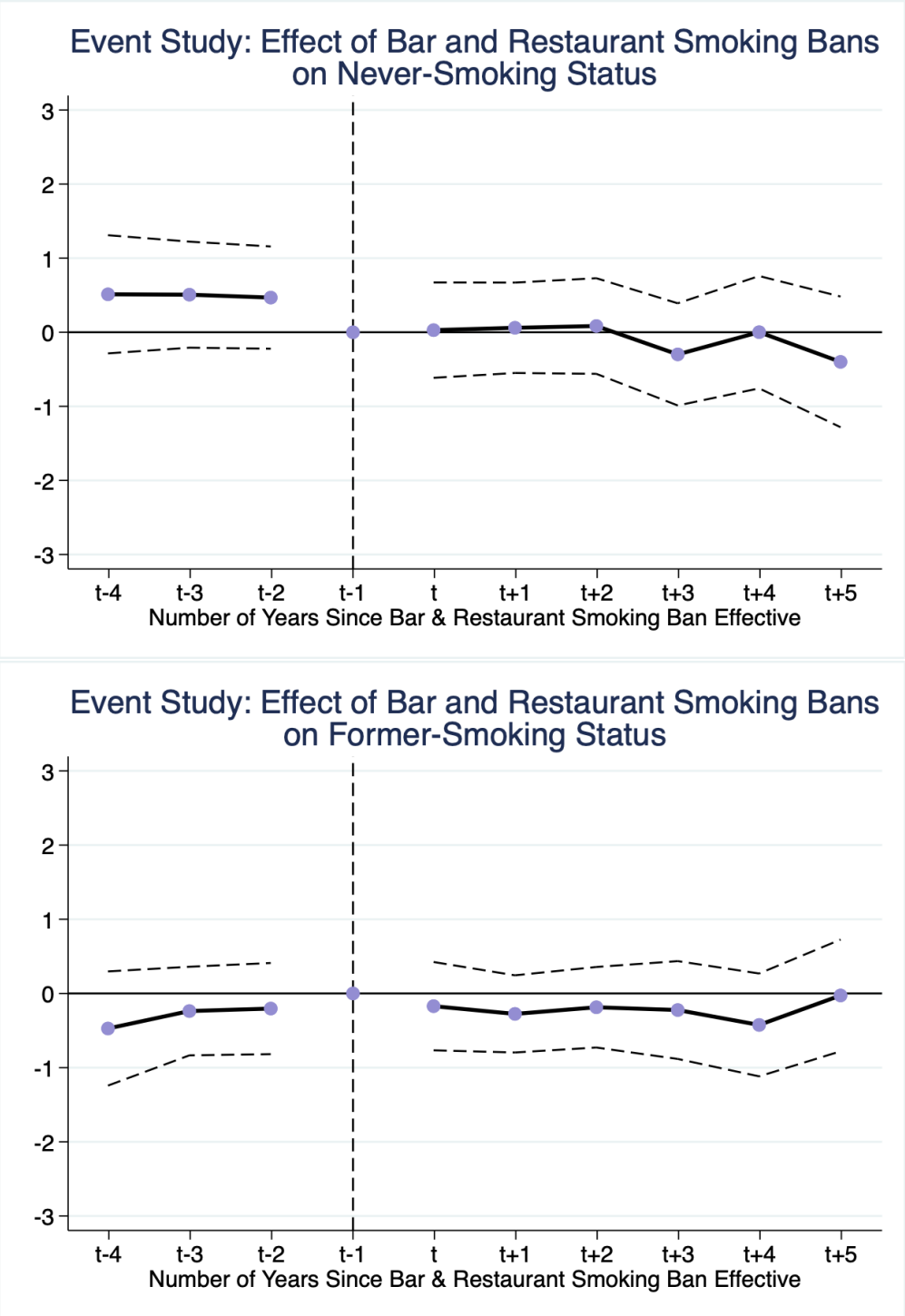
Note: Results from the estimation specified in Equation 2. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, year, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Top panel outcome: number of servings of alcohol consumed in the past 30 days. Bottom panel outcome: whether any alcohol was consumed in the past 30 days (percentage points). Data source: BRFSS 2004-2012.

Figure 3



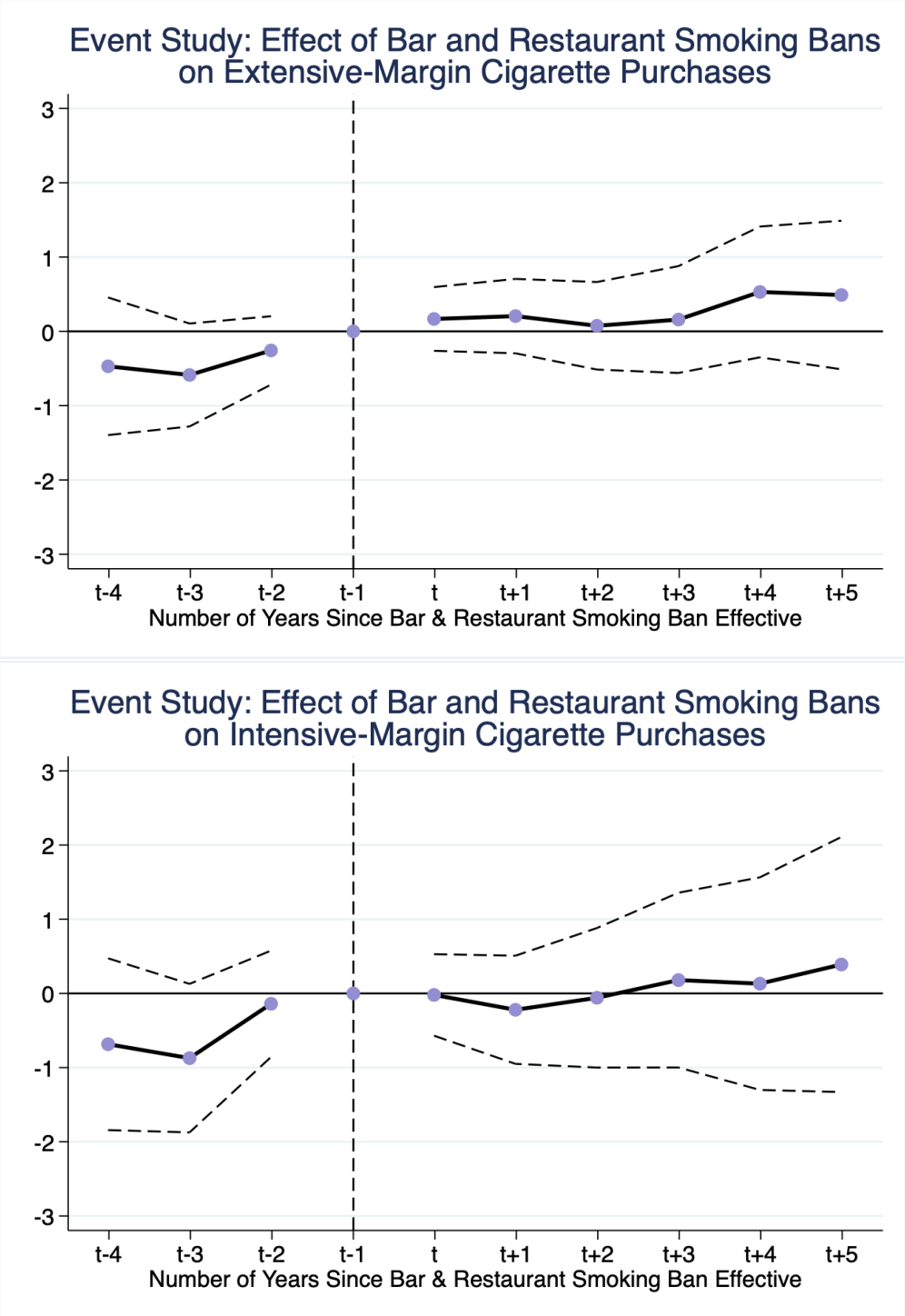
Note: Results from the estimation specified in Equation 2. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, year, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Top panel outcome: number of servings of alcohol consumed in the past 30 days for individuals who drink. Bottom panel outcome: whether the individual reports being a current smoker (percentage points). Data source: BRFSS 2004-2012.

Figure 4



Note: Results from the estimation specified in Equation 2. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, year, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Top panel outcome: whether the individual reports never having smoked cigarettes (percentage points). Bottom panel outcome: whether the individual reports being a former smoker (percentage points). Data source: BRFSS 2004-2012.

Figure 5



Note: Results from the estimation specified in Equation 2. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include household, county, year, and region-by-year fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants in a given year. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Top panel outcome: whether the household purchased any cigarettes in the past month (percentage points). Bottom panel outcome: the number of packs of cigarettes smoking households purchased in the past month. Data source: Nielsen Consumer Panel 2004-2012.

Table 1: Summary Statistics of Alcohol and Smoking Outcomes by Treatment Status, 2004-2012 Behavioral Risk Factor Surveillance System

	(1) Full Sample	(2) Never Smoking Ban	(3) Before Smoking Ban	(4) Ever Smoking Ban
Fraction bar ban	0.48	0.00	0.00	0.67
	(0.49)	(0.00)	(0.00)	(0.45)
Binary bar ban	0.55	0.00	0.00	0.75
	(0.50)	(0.00)	(0.00)	(0.43)
Ever bar ban	0.72	0.00	1.00	1.00
	(0.45)	(0.00)	(0.00)	(0.00)
Fraction restaurant-only ban	0.11	0.33	0.06	0.03
	(0.30)	(0.47)	(0.23)	(0.14)
Alcohol consumption:	11.92	11.27	11.40	12.17
total servings	(37.39)	(39.87)	(36.79)	(36.40)
Alcohol consumption:	53.25	48.43	53.24	55.08
extensive margin (p.p.)	(49.89)	(49.98)	(49.89)	(49.74)
Alcohol consumption:	22.57	23.52	21.59	22.25
intensive margin	(49.06)	(55.05)	(48.40)	(46.89)
Alcohol consumption:	8.34	8.36	7.95	8.34
# days	(8.60)	(8.73)	(8.40)	(8.56)
Alcohol consumption:	2.48	2.51	2.48	2.47
amount per day	(2.73)	(2.79)	(2.62)	(2.71)
Alcohol consumption:	3.67	3.65	3.73	3.67
max.	(3.72)	(3.77)	(3.74)	(3.70)
Fraction current smoker	0.19	0.21	0.20	0.18
	(0.39)	(0.41)	(0.40)	(0.38)
Fraction never smoker	0.56	0.54	0.56	0.57
	(0.50)	(0.50)	(0.50)	(0.49)
Fraction former smoker	0.25	0.25	0.24	0.25
	(0.43)	(0.43)	(0.43)	(0.43)
Observations	3,239,293	818,270	711,906	2,421,023

Note: Data are from the 2004-2012 waves of the Behavioral Risk Factor Surveillance System. Each observation is an individual and treatment is assigned at the monthly level. "Fraction bar ban" is defined as the fraction of the county population subject to a bar and restaurant smoking ban for that month in the individual's county of residence. "Binary bar ban" equals 1 if any part of the county is subject to a bar and restaurant smoking ban for that month. "Fraction restaurant-only ban" is defined as the fraction of the county population that is subject to a restaurant smoking ban but not a bar smoking ban for that month. Alcohol consumption is measured as the total number of servings of alcohol consumed in the past 30 days. The number of days is measured as the number of days out of the past 30 individuals reported drinking alcohol. Amount per day is measured as the average number of servings per day of alcohol individuals drank on days they drank alcohol. Maximum alcohol is the maximum number of servings of alcohol consumed on one occasion. Statistics are weighted by the sample weights.

Table 2: Effect of Bar and Restaurant Smoking Bans on Alcohol Consumption (BRFSS)

Panel A: Total Alcohol Consumption					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	0.60***	0.58***	0.63***	0.84***	1.08***
(standard error)	(0.22)	(0.22)	(0.19)	(0.25)	(0.27)
Pre-Ban Mean	11.40	11.40	11.40	11.43	11.40
% Effect	5.24%	5.09%	5.55%	7.38%	9.42%
N	3,135,416	3,126,968	3,126,968	3,157,136	2,645,643

Panel B: Extensive-Margin Alcohol Consumption					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	-0.15	0.18	0.06	0.71**	0.71**
(standard error)	(0.24)	(0.22)	(0.22)	(0.32)	(0.33)
Pre-Ban Mean	53.23	53.23	53.23	51.64	53.23
% Effect	-0.29%	0.33%	0.12%	1.38%	1.33%
N	3,159,469	3,150,831	3,150,831	3,182,318	2,666,007

Panel C: Intensive-Margin Alcohol Consumption					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	1.08***	0.97**	1.12***	1.29***	1.42***
(standard error)	(0.36)	(0.38)	(0.34)	(0.47)	(0.49)
Pre-Ban Mean	21.59	21.59	21.59	22.33	21.59
% Effect	5.02%	4.49%	5.18%	5.77%	6.57%
N	1,547,509	1,544,637	1,544,637	1,339,608	1,278,250
Demographics		X	X	X	X
County & Time FE	X	X	X	X	X
Region-by-Time FE		X	X	X	X
Policy Controls		X	X	X	X
Fraction Treated	X	X		X	X
Stacked DD				X	
DiD Imputation					X

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: BRFSS 2004-2012.

Table 3: Effect of Bar and Restaurant Smoking Bans on Smoking Status (BRFSS)

Panel A: Current-Smoking Status					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	0.36*	0.23	0.30	0.62**	0.39
(standard error)	(0.21)	(0.23)	(0.18)	(0.26)	(0.28)
Pre-Ban Mean	20.33	20.33	20.33	20.59	20.33
% Effect	1.76%	1.13%	1.48%	3.00%	1.93%
N	3,219,025	3,209,403	3,209,403	3,239,364	2,712,712
Panel B: Never-Smoking Status					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	-0.15	-0.27	-0.32	0.10	-0.27
(standard error)	(0.25)	(0.24)	(0.21)	(0.29)	(0.30)
Pre-Ban Mean	55.58	55.58	55.58	55.05	55.58
% Effect	-0.27%	-0.48%	-0.58%	0.18%	-0.48%
N	3,219,025	3,209,403	3,209,403	3,239,364	2,712,712
Panel C: Former-Smoking Status					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	-0.21	0.04	0.02	-0.72***	-0.13
(standard error)	(0.20)	(0.18)	(0.16)	(0.24)	(0.29)
Pre-Ban Mean	24.08	24.08	24.08	24.37	24.08
% Effect	-0.86%	0.15%	0.09%	-2.94%	-0.52%
N	3,219,025	3,209,403	3,209,403	3,239,364	2,712,712
Demographics		X	X	X	X
County & Time FE	X	X	X	X	X
Region-by-Time FE		X	X	X	X
Policy Controls		X	X	X	X
Fraction Treated	X	X		X	X
Stacked DD				X	
DiD Imputation					X

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: BRFSS 2004-2012.

Table 4: Effect of Bar and Restaurant Smoking Bans on Cigarette Purchases (Nielsen)

Panel A: Any Cigarette Purchases					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	0.07	0.06	0.03	0.19	0.38
(standard error)	(0.21)	(0.21)	(0.19)	(0.25)	(0.28)
Pre-Ban Mean	11.21	11.21	11.21	11.57	11.58
% Effect	0.63%	0.51%	0.24%	1.68%	3.29%
N	5,828,976	5,828,976	5,828,976	6,256,908	3,826,212

Panel B: Packs of Cigarettes (Intensive Margin)					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	-0.36	-0.20	-0.28	-0.44	0.22
(standard error)	(0.32)	(0.30)	(0.29)	(0.36)	(0.35)
Pre-Ban Mean	7.59	7.59	7.59	8.09	7.95
% Effect	-4.70%	-2.60%	-3.66%	-5.38%	2.79%
N	1,126,992	1,126,992	1,126,992	1,375,560	746,988
Demographics		X	X	X	X
County & Time FE	X	X	X	X	X
Region-by-Time FE		X	X	X	X
Policy Controls		X	X	X	X
Fraction Treated	X	X		X	X
Stacked DD				X	
DiD Imputation					X

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include household, county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: Nielsen Consumer Panel 2004-2012.

Table 5: Effect of Bar and Restaurant Smoking Bans on Alcohol Consumption (BRFSS), by Age Group

Panel A: Total Alcohol Consumption				
Age:	18-20	21-34	35-54	55+
	(1)	(2)	(3)	(4)
Smoking Ban	0.26	1.05*	0.50*	0.22
(standard error)	(1.29)	(0.59)	(0.28)	(0.21)
Pre-Ban Mean	10.37	14.05	11.76	8.97
% Effect	2.53%	7.45%	4.25%	2.40%
N	49,729	396,163	1,088,098	1,570,812
Panel B: Extensive-Margin Alcohol Consumption				
	(1)	(2)	(3)	(4)
Smoking Ban	1.18	-0.13	0.31	0.13
(standard error)	(1.38)	(0.54)	(0.33)	(0.31)
Pre-Ban Mean	37.42	60.57	57.97	43.93
% Effect	3.16%	-0.22%	0.54%	0.30%
N	50,441	400,390	1,095,860	1,581,601
Panel C: Intensive-Margin Alcohol Consumption				
	(1)	(2)	(3)	(4)
Smoking Ban	-1.39	1.73*	0.71	0.32
(standard error)	(3.27)	(0.96)	(0.46)	(0.44)
Pre-Ban Mean	28.50	23.39	20.39	20.58
% Effect	-4.89%	7.40%	3.48%	1.57%
N	17,109	232,069	613,123	673,057
Demographics	X	X	X	X
County & Time FE	X	X	X	X
Region-by-Time FE	X	X	X	X
Policy Controls	X	X	X	X
Fraction Treated	X	X	X	X
Stacked DD				
DiD Imputation				

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: BRFSS 2004-2012.

Table 6: Effect of Bar and Restaurant Smoking Bans on Alcohol Consumption by Smoking Status (BRFSS)

Panel A: Total Alcohol Consumption			
Smoking Status:	Current	Never	Former
	(1)	(2)	(3)
Smoking Ban	1.53*	0.23	0.56*
(standard error)	(0.82)	(0.18)	(0.29)
Pre-Ban Mean	20.77	7.57	12.49
% Effect	7.37%	3.08%	4.51%
N	537,485	1,658,599	917,516
Panel B: Extensive-Margin Alcohol Consumption			
Smoking Status:	Current	Never	Former
	(1)	(2)	(3)
Smoking Ban	-0.90*	0.60*	-0.20
(standard error)	(0.54)	(0.32)	(0.39)
Pre-Ban Mean	61.52	48.45	57.28
% Effect	-1.47%	1.23%	-0.35%
N	544,926	1,667,890	924,322
Panel C: Intensive-Margin Alcohol Consumption			
Smoking Status:	Current	Never	Former
	(1)	(2)	(3)
Smoking Ban	3.07**	0.24	0.99**
(standard error)	(1.31)	(0.34)	(0.47)
Pre-Ban Mean	34.09	15.73	21.92
% Effect	9.01%	1.55%	4.53%
N	288,009	754,286	496,091
Demographics	X	X	X
County & Time FE	X	X	X
Region-by-Time FE	X	X	X
Policy Controls	X	X	X
Fraction Treated	X	X	X
Stacked DD			
DiD Imputation			

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: BRFSS 2004-2012.

A Measurement Error

Data sources that contain self-reported measures of the consumption of stigmatized "goods" (cigarettes and alcohol), such as the BRFSS and the Nielsen Consumer Panel, may be subject to social desirability bias. This bias could manifest as individuals underreporting their consumption of cigarettes or alcohol (both on the extensive and intensive margins), because there is stigma in some social circles associated with the consumption of these goods. A constant level of underreporting would not be an issue for my identification strategy; what would be problematic is if the level of underreporting is correlated with the implementation of smoking bans in bars and restaurants.[22] If individuals are going to change how they self report their smoking status, they would be more likely to underreport after the implementation of a smoking ban (as the smoking ban reflects an increase in the stigma surrounding smoking). Underreporting of smoking status would not bias the unconditional estimates of smoking bans on alcohol consumption, but it would bias the results for smoking toward finding a reduction in smoking prevalence when one didn't exist. The effects on alcohol consumption by smoking status could also be biased as the composition of the smoking status groups would be wrong (some people in the never or former smoker group should really be in the current smoker group), which is problematic if the underreporting of smoking is correlated with alcohol consumption, as detailed in section 3.3. Given that I find a small but positively signed effect of smoking bans on the prevalence of smoking, social desirability bias is likely not a concern in this context.

[22]For the Nielsen data, Cotti, Dunn, and Tefft (2015) find that households underreport extensive-margin alcohol purchases but not intensive-margin purchases, and DeCicca, Kenkel, and Lovenheim (2022) suggest that the extent of measurement error in cigarette purchases is probably not changing with tobacco control policies such as smoking bans.

Recall bias is another issue with self-reported data, particularly for measures of alcohol consumption given that consuming sufficiently large quantities of alcohol can inhibit memory formation. Recall bias is the error in self-reported estimates of past behavior that arises because individuals cannot remember past events with complete accuracy. It could affect my estimates if smoking bans in bars and restaurants lead to sufficiently large increases in alcohol consumption for individuals to have no memory of how much alcohol they consumed. If individuals believe they drank less alcohol than their true consumption, then my estimates would be attenuated. Alternatively, if they do not remember how much alcohol they consumed, they could overestimate their alcohol consumption, in which case my results would be biased away from 0.

B Additional Robustness Checks

B.1 Disaggregated Measures of Alcohol Consumption (BRFSS)

The amount of alcohol consumed over 30 days is a function of the number of days an individual drank alcohol and the average amount of alcohol the individual consumed on each day the individual drank. Studying the effects on these outcomes can illuminate how individuals are responding to smoking bans: are they drinking more often, consuming more alcohol when they drink, or both?

Panels A and B of Appendix Table *C*.5 disaggregate the effects on intensive-margin alcohol consumption into these two components. In my primary TWFE specification, for individuals who drank alcohol in the past 30 days, smoking bans in bars and restaurants

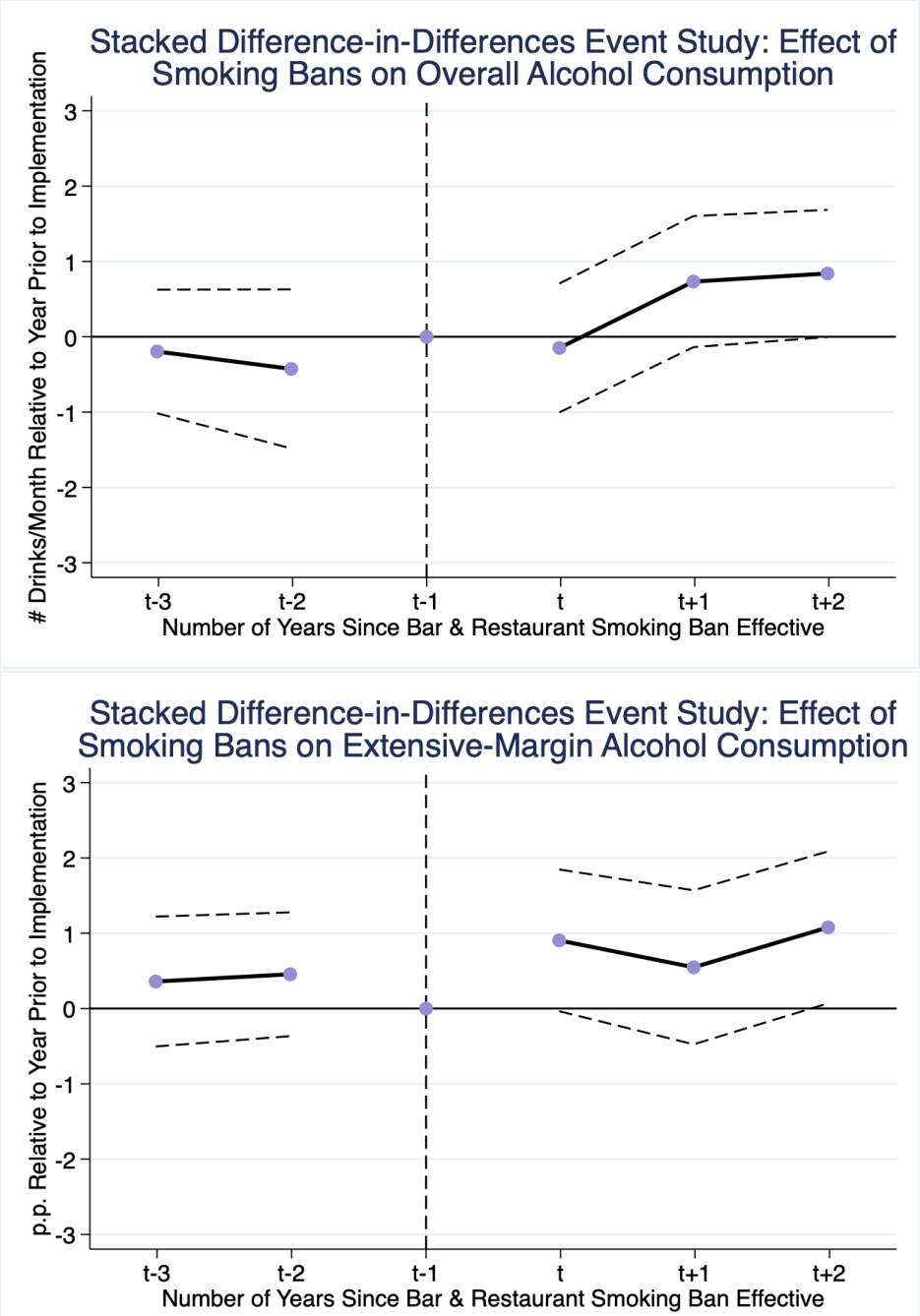
are associated with an increase in the number of days spent drinking (out of the past 30 days) of 0.08 days (1.05%), on average, which is marginally statistically significant (Panel A, Column 2). This effect is attenuated and not statistically significant in the stacked and DiD imputation specifications. Taken together, the results imply that smoking bans do not have an effect on the number of days individuals drink alcohol, because even the marginally significant result is not economically meaningful.

The implementation of smoking bans in bars and restaurants results in a 0.05-serving increase in the average amount of alcohol individuals consume, conditional on drinking (Column 2 of Panel B). This effect is statistically significant at the 5% level, and it represents a 2.21% increase. The stacked and DiD imputation estimates are slightly larger, at 0.09 and 0.07 drinks per day, on average, representing an approximately 3% increase. These effects are significant at the 1% and 5% level, respectively. The consistency of this result across specifications suggests that smoking bans lead to small increases in the average amount of alcohol consumed per day.

Analyzing the effect on the maximum amount of alcohol consumed can indicate whether there are potentially unhealthy changes in drinking, such as binge drinking. The implementation of smoking bans in bars and restaurants leads to an increase in the maximum amount of alcohol consumed of 0.05 servings, on average, in the primary TWFE specification (Panel C, Column 2). This effect is marginally statistically significant and it represents a 1.47% increase in maximum alcohol consumption. The effect sizes for the stacked and DiD imputation estimators are similar, and the latter is significant at the 5% level. These results indicate that there may be very small increases in the maximum amount of alcohol consumed on one occasion, which is not concerning from a public health perspective.

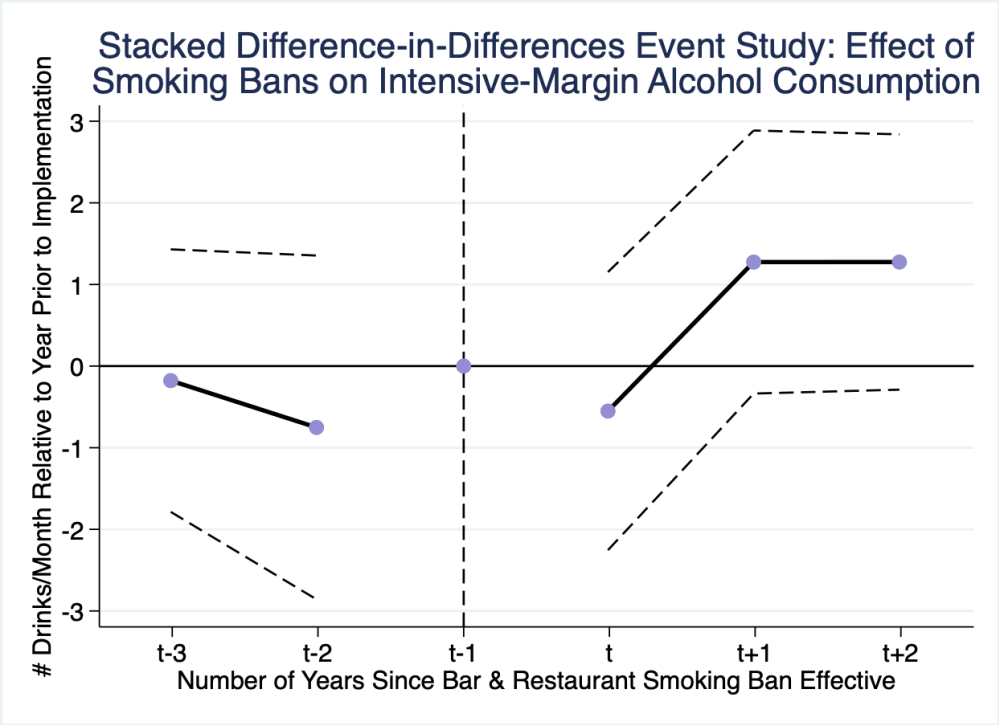
C Additional Figures and Tables

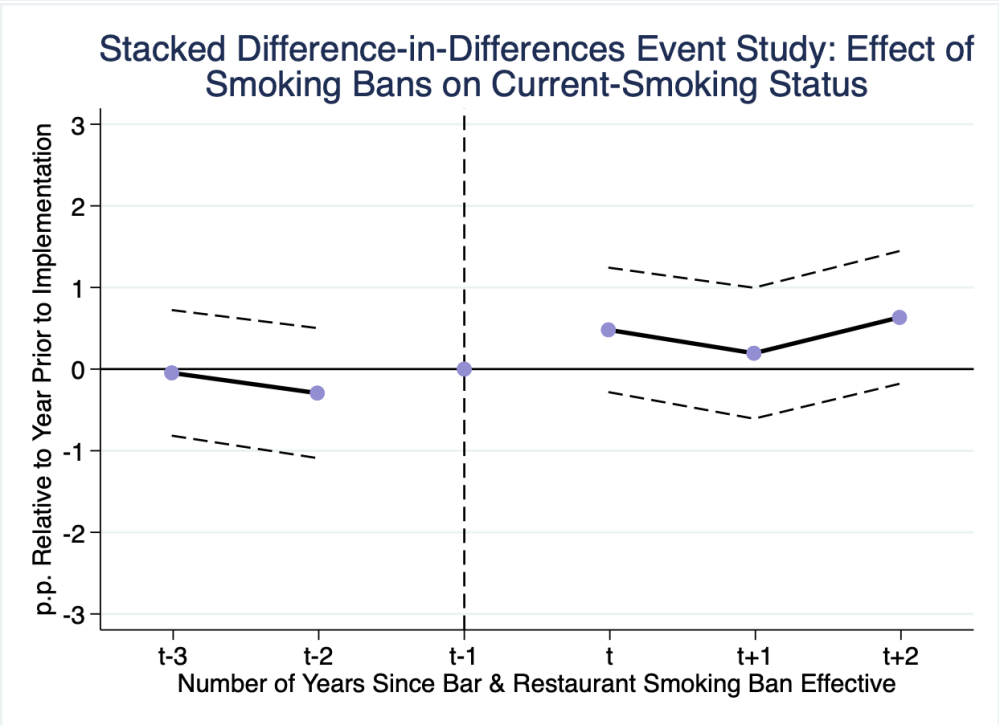
Figure C.1



Note: Results from the estimation described in Section 3.4. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county-by-stack, year-by-stack, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county-by-stack level. Regressions are probability weighted using the sample weights. Top panel outcome: number of servings of alcohol consumed in the past 30 days. Bottom panel outcome: whether any alcohol was consumed in the past 30 days. Data source: BRFSS 2004-2012.

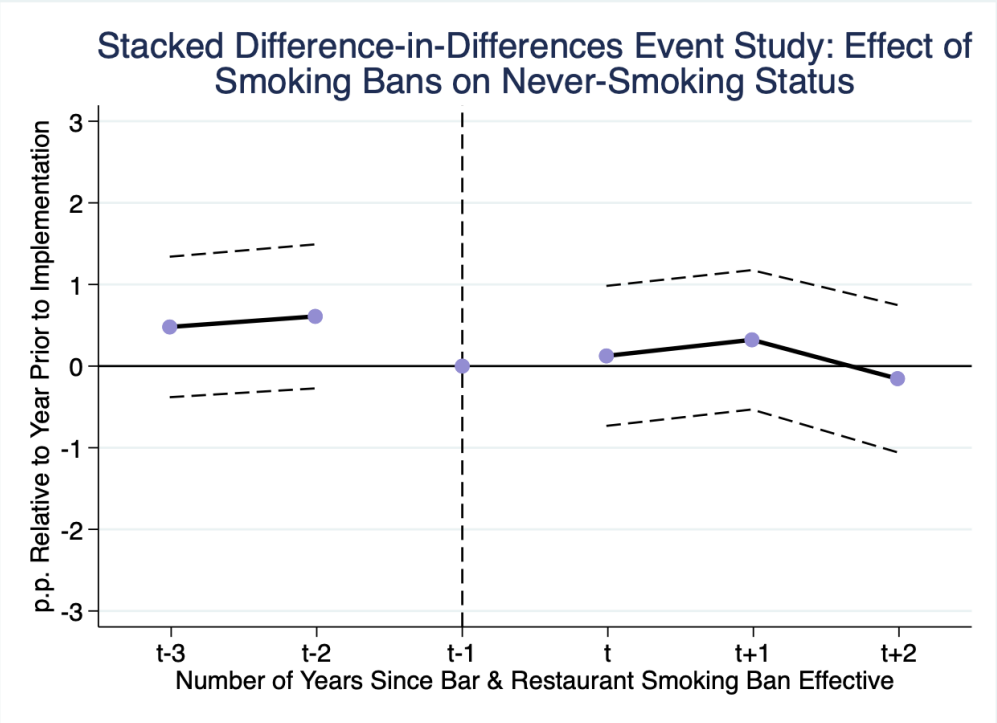
Figure C.2

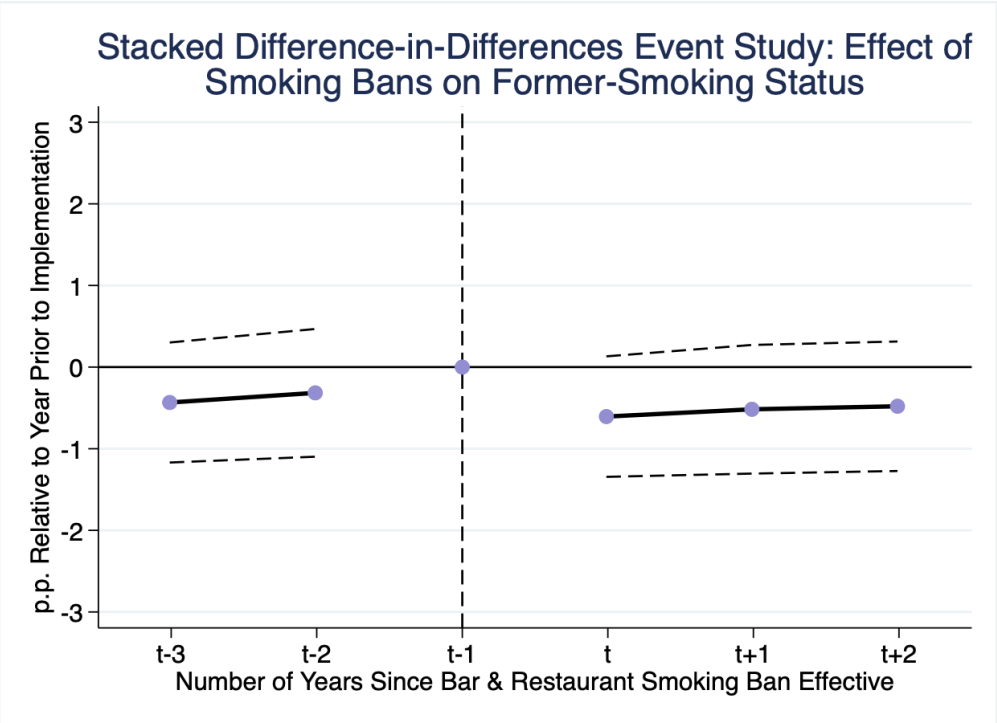




Note: Results from the estimation described in Section 3.4. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county-by-stack, year-by-stack, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county-by-stack level. Regressions are probability weighted using the sample weights. Top panel outcome: number of servings of alcohol consumed in the past 30 days for individuals who drink. Bottom panel outcome: whether the individual reports being a current smoker. Data source: BRFSS 2004-2012.

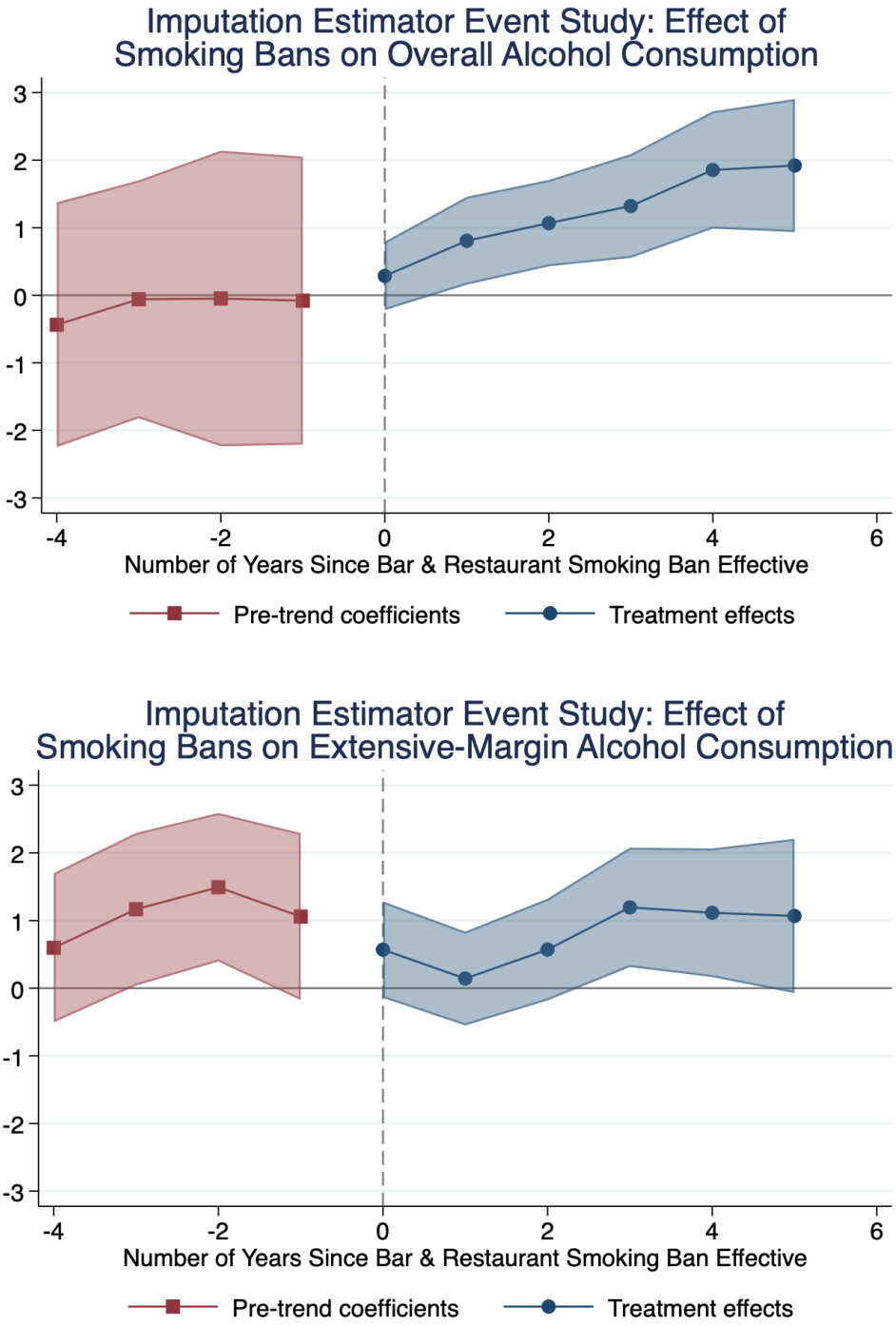
Figure C.3





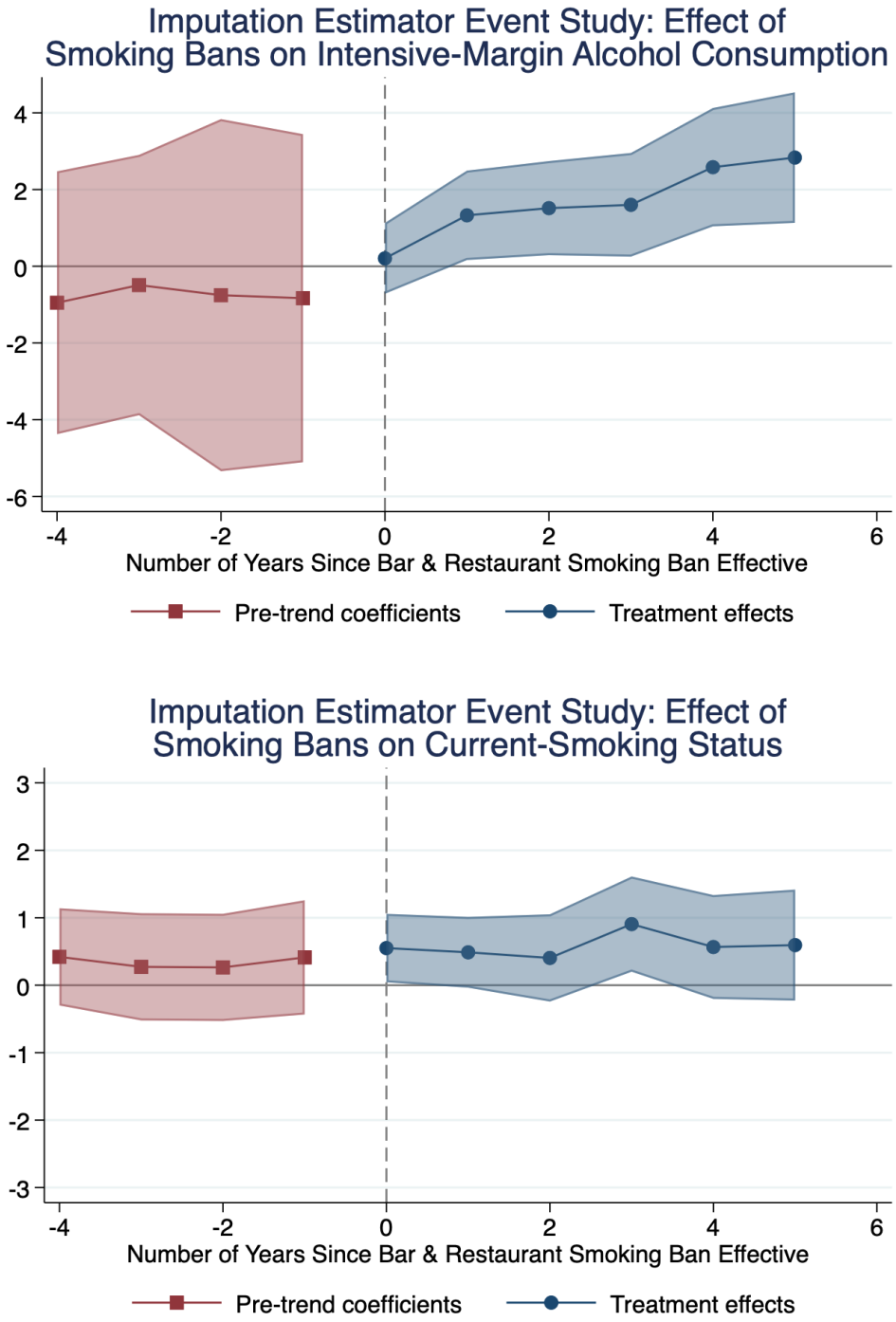
Note: Results from the estimation described in Section 3.4. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county-by-stack, year-by-stack, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county-by-stack level. Regressions are probability weighted using the sample weights. Top panel outcome: whether the individual reports never having smoked cigarettes. Bottom panel outcome: whether the individual reports being a former smoker. Data source: BRFSS 2004-2012.

Figure C.4



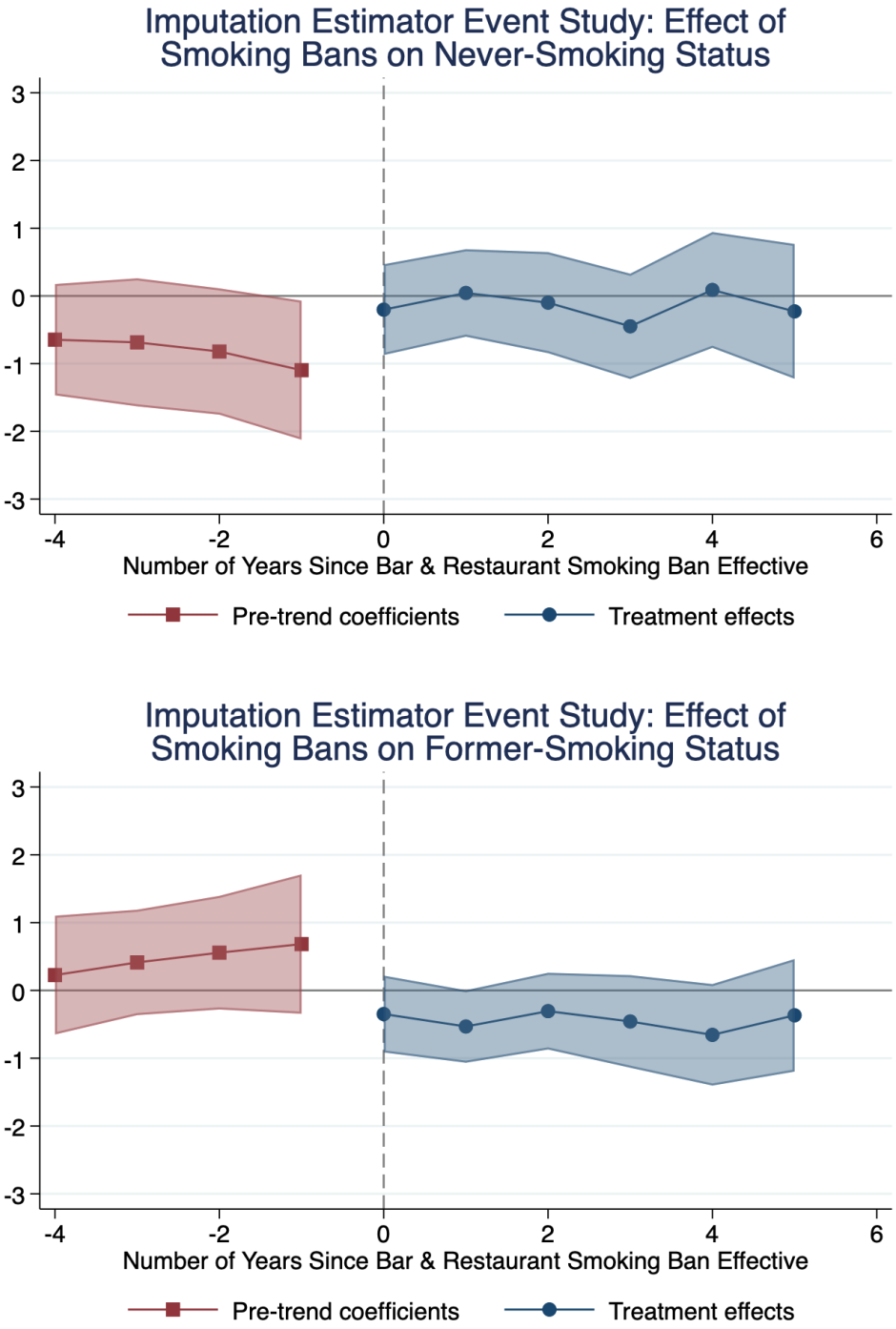
Note: Results from the estimation described in Section 3.4. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county-by-stack, year-by-stack, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county-by-stack level. Regressions are probability weighted using the sample weights. Top panel outcome: whether the individual reports never having smoked cigarettes. Bottom panel outcome: whether the individual reports being a former smoker. Data source: BRFSS 2004-2012.

Figure C.5



Note: Results from the estimation described in Section 3.4. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county-by-stack, year-by-stack, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county-by-stack level. Regressions are probability weighted using the sample weights. Top panel outcome: whether the individual reports never having smoked cigarettes. Bottom panel outcome: whether the individual reports being a former smoker. Data source: BRFSS 2004-2012.

Figure C.6



Note: Results from the estimation described in Section 3.4. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county-by-stack, year-by-stack, and region-by-year fixed effects. Treatment is defined as being effective when any part of the county population is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county-by-stack level. Regressions are probability weighted using the sample weights. Top panel outcome: whether the individual reports never having smoked cigarettes. Bottom panel outcome: whether the individual reports being a former smoker. Data source: BRFSS 2004-2012.

Table C.1: Summary Statistics of Control Variables by Treatment Status, 2004-2012 Behavioral Risk Factor Surveillance System

	(1) Full Sample	(2) Never Smoking Ban	(3) Before Smoking Ban	(4) Ever Smoking Ban
Fraction bar ban	0.48	0.00	0.00	0.67
	(0.49)	(0.00)	(0.00)	(0.45)
Fraction restaurant-only ban	0.11	0.33	0.06	0.03
	(0.30)	(0.47)	(0.23)	(0.14)
Fraction female	0.51	0.52	0.51	0.51
	(0.50)	(0.50)	(0.50)	(0.50)
Fraction Black	0.11	0.13	0.11	0.10
	(0.31)	(0.34)	(0.31)	(0.30)
Fraction Asian	0.03	0.02	0.02	0.04
	(0.18)	(0.12)	(0.14)	(0.20)
Fraction Hispanic	0.14	0.09	0.09	0.16
	(0.35)	(0.28)	(0.29)	(0.37)
Fraction white	0.68	0.73	0.75	0.67
	(0.46)	(0.44)	(0.44)	(0.47)
Fraction other race	0.04	0.04	0.03	0.03
	(0.18)	(0.19)	(0.18)	(0.18)
Fraction age 18-34	0.30	0.28	0.31	0.30
	(0.46)	(0.45)	(0.46)	(0.46)
Fraction age 35-54	0.38	0.38	0.39	0.38
	(0.49)	(0.49)	(0.49)	(0.49)
Fraction age 55+	0.31	0.34	0.30	0.31
	(0.46)	(0.47)	(0.46)	(0.46)
Fraction employed	0.59	0.57	0.62	0.60
	(0.49)	(0.49)	(0.49)	(0.49)
Fraction married	0.58	0.59	0.61	0.58
	(0.49)	(0.49)	(0.49)	(0.49)
Fraction high school or less	0.40	0.44	0.39	0.39
	(0.49)	(0.50)	(0.49)	(0.49)
Fraction some college or more	0.60	0.56	0.60	0.61
	(0.49)	(0.50)	(0.49)	(0.49)
BAC 0.08%	1.00	1.00	0.98	0.99
	(0.07)	(0.00)	(0.14)	(0.08)
Cigarette tax per pack ($)	1.91	1.47	1.47	2.08
	(0.98)	(0.66)	(0.75)	(1.03)
Observations	3,239,293	818,270	711,906	2,421,023

Note: Data are from the 2004-2012 waves of the Behavioral Risk Factor Surveillance System. Each observation is an individual and treatment is assigned at the monthly level. "Fraction bar ban" is defined as the fraction of the county population subject to a bar and restaurant smoking ban for that month in the individual's county of residence. "Fraction restaurant-only ban" is defined as the fraction of the county population that is subject to a restaurant smoking ban but not a bar smoking ban for that month. BAC 0.08% is defined as an indicator for a law mandating the BAC limit for driving under the influence is 0.08. Cigarette tax per pack is defined as the sum of the federal and state cigarette taxes per pack measured in dollars. Statistics are weighted by the sample weights.

Table C.2: Summary Statistics of Alcohol Outcomes by Treatment Status, 2004-2012 Nielsen Consumer Panel

	(1) Full Sample	(2) Never Smoking Ban	(3) Before Smoking Ban	(4) Ever Smoking Ban
Fraction bar ban	0.45	0.00	0.00	0.64
	(0.49)	(0.00)	(0.00)	(0.46)
Binary bar ban	0.51	0.00	0.00	0.72
	(0.50)	(0.00)	(0.00)	(0.45)
Ever bar ban	0.70	0.00	1.00	1.00
	(0.46)	(0.00)	(0.00)	(0.00)
Fraction restaurant-only ban	0.11	0.31	0.06	0.03
	(0.31)	(0.46)	(0.22)	(0.14)
Alcohol purchases:	13.68	12.64	14.02	14.13
total servings	(41.75)	(40.04)	(41.31)	(42.46)
Alcohol purchases	25.63	23.57	25.96	26.51
extensive margin (p.p.)	(43.66)	(42.45)	(43.84)	(44.14)
Alcohol purchases:	53.38	53.60	54.02	53.30
intensive margin	(68.42)	(67.87)	(66.42)	(68.64)
Fraction smoking	0.22	0.25	0.24	0.21
households	(0.42)	(0.43)	(0.43)	(0.41)
Cigarette purchases:	0.10	0.12	0.12	0.10
extensive margin	(0.31)	(0.33)	(0.32)	(0.30)
Cigarette purchases:	7.18	8.26	7.95	6.63
intensive margin (packs)	(14.51)	(15.99)	(15.45)	(13.67)
Observations	5,761,260	1,682,016	1,024,378	4,079,244

Note: Data are from the 2004-2012 waves of the Nielsen Consumer Panel. Each observation is a household and treatment is assigned at the monthly level. "Fraction bar ban" is defined as the fraction of the county population subject to a bar and restaurant smoking ban for that month in the household's county of residence. "Binary bar ban" equals 1 if any part of the county is subject to a bar and restaurant smoking ban for that month. "Fraction restaurant-only ban" is defined as the fraction of the county population that is subject to a restaurant smoking ban but not a bar smoking ban for that month. Alcohol purchases are measured as the total number of servings of alcohol purchased for off-premises consumption in the past month. Intensive-margin cigarette purchases are measured as the number of packs (of 20 cigarettes) purchased in the last month by smoking households (households that purchased any cigarettes in the calendar year). Statistics are weighted by the sample weights.

Table C.3: Effect of Bar and Restaurant Smoking Bans on Alcohol Purchases (Nielsen)

Panel A: Total Quantity of Alcohol Purchases for Off-Premises Consumption					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	-0.33	-0.34	-0.23	-0.28	0.34
(standard error)	(0.24)	(0.27)	(0.24)	(0.32)	(0.36)
Pre-Ban Mean	13.52	13.52	13.52	14.07	14.01
% Effect	-2.43%	-2.48%	-1.71%	-1.99%	2.41%
N	5,828,976	5,828,976	5,828,976	6,256,908	3,826,212

Panel B: Extensive-Margin Alcohol Purchases for Off-Premises Consumption					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	0.00	-0.08	0.08	0.39	0.38
(standard error)	(0.21)	(0.23)	(0.20)	(0.28)	(0.31)
Pre-Ban Mean	25.15	25.15	25.15	25.87	25.95
% Effect	0.02%	-0.33%	0.31%	1.49%	1.48%
N	5,828,976	5,828,976	5,828,976	6,256,908	3,826,212

Panel C: Intensive-Margin Alcohol Purchases for Off-Premises Consumption					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	-0.15	-0.56	-0.57	-0.76	3.02***
(standard error)	(0.68)	(0.76)	(0.71)	(0.98)	(1.06)
Pre-Ban Mean	53.77	53.77	53.77	54.38	54.01
% Effect	-0.28%	-1.04%	-1.05%	-1.39%	5.59%
N	1,537,418	1,537,418	1,537,418	1,561,112	975,284
Demographics		X	X	X	X
County & Time FE	X	X	X	X	X
Region-by-Time FE		X	X	X	X
Policy Controls		X	X	X	X
Fraction Treated	X	X		X	X
Stacked DD				X	
DiD Imputation					X

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include household, county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: Nielsen Consumer Panel 2004-2012.

Table C.4: Effect of Bar and Restaurant Smoking Bans on Alcohol Purchases by Smoking Status (Nielsen)

Panel A: Total Alcohol Consumption		
Smoking Status:	Smoker	Nonsmoker
	(1)	(2)
Smoking Ban	-0.30	-0.29
(standard error)	(0.77)	(0.25)
Pre-Ban Mean	19.97	11.40
% Effect	-1.51%	-2.51%
N	1,126,992	4,701,945
Panel B: Extensive-Margin Alcohol Consumption		
Smoking Status:	Smoker	Nonsmoker
	(1)	(2)
Smoking Ban	-1.02*	0.14
(standard error)	(0.62)	(0.24)
Pre-Ban Mean	29.86	23.60
% Effect	-3.43%	0.59%
N	1,126,992	1,667,890
Panel C: Intensive-Margin Alcohol Consumption		
Smoking Status:	Smoker	Nonsmoker
	(1)	(2)
Smoking Ban	2.45	-0.86
(standard error)	(2.11)	(0.67)
Pre-Ban Mean	66.87	48.33
% Effect	3.66%	-1.79%
N	343,949	1,189,556
Demographics	X	X
County & Time FE	X	X
Region-by-Time FE	X	X
Policy Controls	X	X
Fraction Treated	X	X
Stacked DD		
DiD Imputation		

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include household, county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: Nielsen Consumer Panel 2004-2012.

Table C.5: Effect of Bar and Restaurant Smoking Bans on Disaggregated Measures of Alcohol Consumption (Conditional on Drinking in Past 30 Days; BRFSS)

Panel A: Number of Days Spent Drinking in Past 30 Days					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	0.13***	0.08*	0.03	0.06	0.03
(standard error)	(0.05)	(0.05)	(0.05)	(0.06)	(0.08)
Pre-Ban Mean	7.95	7.95	7.95	7.91	7.95
% Effect	1.67%	1.05%	0.40%	0.79%	0.43%
N	1,571,562	1,568,500	1,568,500	1,364,790	1,298,629
Panel B: Average Alcohol Consumption per Drinking Day					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	0.05**	0.05**	0.07***	0.09***	0.07**
(standard error)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Pre-Ban Mean	2.49	2.49	2.49	2.55	2.49
% Effect	2.18%	2.21%	2.77%	3.53%	2.88%
N	1,560,968	1,557,997	1,557,997	1,356,634	1,289,775
Panel C: Maximum Alcohol Consumption on One Occasion					
	(1)	(2)	(3)	(4)	(5)
Smoking Ban	0.08**	0.05*	0.03	0.06*	0.07**
(standard error)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Pre-Ban Mean	3.73	3.73	3.73	3.78	3.73
% Effect	2.02%	1.47%	0.69%	1.64%	1.99%
N	1,406,853	1,404,214	1,404,214	1,280,967	1,093,806
Demographics		X	X	X	X
County & Time FE	X	X	X	X	X
Region-by-Time FE		X	X	X	X
Policy Controls		X	X	X	X
Fraction Treated	X	X		X	X
Stacked DD				X	
DiD Imputation					X

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 1. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include county, month, and region-by-month fixed effects. Treatment is defined as the fraction of the county population covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: BRFSS 2004-2012.