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

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Governments implemented smoking bans in bars 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 percent) increase in intensive-margin alcohol consumption and no economically meaningful effects on smoking. Effects on alcohol consumption are concentrated among current and former smokers. 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, 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 detriment

[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 (for example, cancer, liver disease) and acute (for example, suicide and motor vehicle crashes) causes (CDC 2021). These deaths constitute 2.8 million years of potential life lost (CDC 2021).

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]

This paper focuses on behavioral responses to banning smoking in bars, though nearly every jurisdiction with a smoking ban in bars either simultaneously or previously implemented a smoking ban in restaurants. The causal effect I am therefore interested in identifying is the effect of a smoking ban that covers both bars and restaurants on alcohol consumption.[5] 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.[6] I estimate a difference-in-differences model where my identifying variation is

[3]for example, "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]I control for restaurant-only smoking bans and provide more detail on the construction of these variables in Section 2.2.

[6]Likely location of alcohol consumption can be ascertained if one assumes total alcohol consumption

the date of implementation of a smoking ban in bars. During the sample period, 25 states and 651 local governments (cities or counties) implemented a smoking ban in bars, providing a wealth of spatial and temporal variation in treatment status.[7]

Conditional on drinking in the past 30 days, smoking bans in bars lead to an increase of one serving of alcohol per 30 days (5 percent increase). Average alcohol consumption for current and former smokers increases by 4-8 percent. I find no economically meaningful effects on the total quantity of alcohol purchased for home consumption in the past month. These results imply at least some of the increase in total alcohol consumption is coming from increases in on-premises alcohol consumption. With respect to their effect on extensive-margin smoking and cigarette purchases, I find no economically meaningful effects of smoking bans on smoking prevalence or purchases of cigarettes. These results highlight the importance of considering 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 several literatures, including the one on the impact of smoking bans on alcohol consumption. Earlier studies find mixed results of state-level smoking bans on alcohol consumption. Using the 1992 to 2002 waves of the Health and Retirement Study, Picone, Sloan, and Trogdon (2004) find that general state-level smoking bans lead to reductions in alcohol consumption for women over the age of 50. In contrast, Koksal and Wohlgenant (2016) find that restaurant smoking bans lead to increases in restaurant alcohol consumption and decreases in at-home alcohol consumption using the 2002 to 2008 waves

equals alcohol consumption at home plus alcohol consumption at bars or restaurants.

[7]During the same period, 28 states and 751 municipalities implemented a smoking ban in restaurants.

of the Consumer Expenditure Survey. I extend this literature in four key ways. First, I incorporate city 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. Second, I study a later time period when bar and restaurant smoking bans are more prevalent and cover a broader population. If there are heterogeneous treatment effects across jurisdictions or time, my results will better capture the ultimate effects of this policy, which is more relevant for policy analysis. Third, compared to work studying the impact on older adults (Picone, Sloan, and Trogdon 2004), I use data that are more representative of both the population most affected by a smoking ban in a bar (younger adults) and the U.S. population more broadly. Fourth, I use more direct measures of alcohol consumption compared to other work that imputes quantities from dividing expenditures by a calculated price index (Koksal and Wohlgenant 2016); using direct measures of quantities is particularly important given the potential for smoking bans to impact on-premises alcohol prices.

A separate strand of literature has indirectly analyzed the effects of smoking bans on bar and restaurant alcohol consumption through their effects on the employment and fiscal health of this industry. This literature has found mixed results that vary depending on the country or countries studied, as well as the time period analyzed. While prior work concludes that a nationwide smoking ban in Ireland led to a 4.6 percent reduction in the quantity of bar sales (Cornelsen and Normand 2012), other work finds the adoption of state-level smoking bans in Germany did not lead to a reduction in revenues, the number of establishments, or demand for bar and restaurant alcohol (Wissmann 2022). Smoking bans in other European countries similarly yielded null effects on revenues, profits, and employment for the bar and

restaurant industry (Pieroni and Salmasi 2017).

In the U.S. context, Adams and Cotti (2007) find reductions in employment for bars but not restaurants using the 2001-2004 Quarterly Census of Employment and Wages following the implementation of smoking bans in bars. In line with the restaurant-ban results, Kim and Yörük (2015) find restaurant smoking bans lead to reductions in expenditures on dining out for households with smokers but a more-than-offsetting increase in expenditures on dining out for nonsmoking households using the 1999-2009 waves of the Panel Survey of Income Dynamics. Cowling and Bond (2005) find relative increases in restaurant and bar tax revenues in California after the imposition of California's statewide restaurant and bar smoking bans. A meta-analysis finds smoking bans led to a reduction in absolute sales for bars, largely in countries outside the U.S., while they had a null effect on the ratio of bar and restaurant sales to all other retail sales and employment in bars (Cornelsen et al. 2014). While at first glance my findings of increased alcohol consumption may appear inconsistent with some of this earlier research, my sample period has limited overlap with many of these studies. For example, Adams and Cotti's (2007) sample period runs from 2001 to 2004 while mine is from 2004 to 2012. Moreover, their work identifies an immediate, short-run drop in employment whereas my event studies suggest an increasing treatment effect over a longer time horizon. Further, as bar and restaurant smoking bans have become more widely adopted they cover a larger geographic area, increasing the time cost to smokers of driving to jurisdictions without a ban. My paper contributes to this strand of the literature by providing an explanation for why many of these studies find null or positive effects: bars and restaurants will not suffer adverse employment or sales outcomes if smoking bans lead to increases in bar and restaurant alcohol consumption. Finally, I contribute to a broader

literature in health economics on policies that target smoking and drinking, their respective effects on cigarette and alcohol consumption, and effects on related externalities.[8, 9]

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 alternative estimators (stacked difference-in-differences and DiD imputation estimator) and section 4 describes the results. 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, which are the years that have reliable county identifiers in both datasets. The Nielsen Consumer Panel does not start until 2004, and starting with the 2013 wave, BRFSS stopped publicly reporting county-level identifiers in the aggregated dataset due to privacy concerns.[10] Summary statistics by treatment status are in Table 1 and Appendix Table D.1 for the BRFSS data and Appendix Table D.2 for the Nielsen data.

[8] 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).

[9] 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).

[10] I use the same years as 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.

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 consumption. For my sample period, 2004-2012, 80-90 percent of observations in the BRFSS contain county identifiers.[11] 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-

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

tion.[12,13] 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 (for example, 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.[14] 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

[12]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 (for example, 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.

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

[14]This conversion is not exact as a serving of alcohol depends also on alcohol by volume (ABV). Twelve ounces of 5 percent ABV beer constitutes one serving of alcohol, 5 ounces of 12 percent ABV wine constitutes one serving, and 1.5 ounces of 40 percent ABV liquor constitutes one serving (NIAAA). Higher ABV beers have become more common in recent years. Some liquor has a higher ABV than 40 percent, such as Absinthe (at least 45 percent ABV), while others have a lower ABV, such as Irish Cream (15-20 percent 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.

(or stockpiled) purchases of cigarettes. I use scanned-in cigarette purchases to measure 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).[15]

3 Methods

To identify causal effects of smoking bans in bars on alcohol consumption, smoking, and alcohol consumption by smoking status, I start by estimating a traditional two-way-fixed-effects difference-in-differences model. I exploit variation in the timing of effective dates of

[15]Summary statistics for the control variables are shown in Appendix Table D.1.

these smoking bans, incorporating bans implemented at the city, county, and state level. In recent years, there has been a growing awareness of potential sources of bias arising from the traditional two-way-fixed-effects difference-in-differences methods when treatment is staggered and in the presence of heterogenous treatment effects (for example, Goodman-Bacon 2021, Callaway and Sant'Anna 2021). Specifically, comparing newly treated units to already-treated units can bias the difference-in-differences coefficient if treatment effects are dynamic or vary across treatment units. Indeed, the event studies described in Section 4.1 show effects on total and intensive-margin alcohol consumption that are increasing over time, which would suggest that the traditional two-way-fixed-effects estimator yields attenuated effect sizes. To address these potential issues, I implement two alternative difference-in-differences estimators as robustness checks: the stacked difference-in-differences estimator (following Deshpande and Li 2019) and the DiD imputation estimator (Borusyak, Jaravel, and Speiss, forthcoming). The former focuses on "clean" comparisons between newly treated units and units that are either not yet treated or never treated, while the latter uses the untreated observations to calculate unit and time fixed effects and project counterfactual outcomes for the treated observations. Both estimators yield larger effect sizes than the traditional two-way-fixed-effects estimator.

3.1 Difference-in-Differences Identification and Assumptions

Three assumptions are needed for a classic two-way-fixed-effects difference-in-differences estimate to capture a causal effect:

1. *Parallel trends:* in the absence of smoking bans in bars and restaurants, trends in out-

comes, conditional on control variables, would be the same across treated and untreated counties

2. *No concurrent shocks:* 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

3. *Constant and immediate treatment effect:* any effect must occur immediately after the policy is implemented and must be constant across jurisdictions and over time, conditional on the control variables

Section 3.4 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 and more formally test for parallel pre-trends with an F-test for the joint significance of the pre-period coefficients. The event studies also provide a test of the validity of the third assumption; the increasing effect of treatment over time motivates the use of the two alternative estimators.

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 C 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 county c. I also control for the fraction subject to a smoking ban in restaurants but not bars, 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 exclu-

sive categories for Hispanic, Black, Asian, American Indian/Alaska Native, Native Hawaiian/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.[16] 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 Census-region-by-month-year ($\delta_{r,t}$) fixed effects. I cluster the standard errors, $\varepsilon_{i,c,t}$, at the county level, and weight the regressions by the sample weights provided by BRFSS or Nielsen.

In Section 4.4, 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 calendar year (to account for potential stockpiling or infrequent cigarette purchases).

[16] 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 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. I test for parallel pre-trends with an F-test of the joint significance of the pre-period coefficients ($t-4$, $t-3$, and $t-2$). For nearly every outcome, the F-statistics are too small ($p > .10$) to reject the null hypothesis that the pre-period coefficients are jointly equal to zero.[17] I report the F-statistics in the figure notes for each event study.

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.[18,19]

[17]The F-statistic for the pre-period coefficients on intensive-margin cigarette purchases equals 2.39 (marginally significant). The coefficients are negative but increasing in the pre-period, suggesting there may be a pre-trend and the results should be interpreted with caution. The F-statistic of the pre-trend for extensive-margin alcohol consumption using the DiD imputation estimator equals 2.87, but the pre and post-period coefficients are of a similar magnitude, suggesting the true effect is a null effect.

[18]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.

[19]Out of all observations corresponding to counties with at least one smoking ban, 83 percent were covered by laws that affected at least half the county population that were implemented in the same year as the first law. 10 percent 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 percent 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.

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. The control variables and fixed effects are the same as in the original specification, aside from using years instead of months; standard errors are again clustered at the county level; and regressions are weighted using the sample weights.

3.4 Potential Endogeneity of Smoking Type

A primary motivation 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 (for example, Evans et al. 1999, Bharadwaj et al. 2014) and prevent others from initiating smoking (Liu 2010). If smoking bans in bars have effects on smoking 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 by smoking status 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, but they 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 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 Appendix B, but during this time period (2004-2012), smoking bans in bars do not have an economically meaningful effect on the prevalence of smoking.

3.5 Alternative Difference-in-Differences Estimators

Numerous papers have highlighted potential issues with staggered-timing difference-in-differences models. Using always treated or already-treated groups as controls for later-treated groups can yield biased effect sizes, particularly in the presence of treatment effects that vary over time or by locale (Goodman-Bacon 2021, Callaway and Sant'Anna 2021). For example, if treatment effects are positive and increasing over time, using already-treated groups as controls for later-treated groups may attenuate the effect size.

To address this issue, I conduct robustness checks using two alternative estimators: the stacked difference-in-differences estimator and the DiD imputation estimator. With the stacked difference-in-differences method, 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. For the DiD imputation estimator, I use the method outlined in Borusyak, Jaravel, and Speiss (forthcoming).

For the stacked difference-in-differences specification, I create eight different datasets, one for each treatment year from 2005 to 2012. I drop units that were treated in 2004 or earlier as there are no pre-treatment observations for them. Each dataset includes observations from up to 4 years before a smoking ban is implemented in bars through up to 5 years after for the treated group.[20] For the untreated group, I include observations that are either not treated during the sample period or treated more than 5 years after the treatment year. Event time is relative to the treatment year. Each of these datasets is identified by the treatment year, or stack. I append the eight datasets to each other, which allows for the same observation 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 interacted with stack fixed effects, county-by-stack fixed effects, year-by-stack fixed effects, and region-by-year-by-stack fixed effects. The standard errors are clustered at the county-by-stack level and regressions are probability weighted by the sample weights.

The DiD imputation estimator provides another way to address the issue of what the authors call "forbidden comparisons": comparing newly treated units to already-treated units. The estimator imputes group and time fixed effects (in this case, county and month fixed effects) by fitting regressions solely on untreated observations. These fixed effects are then used to impute the counterfactual untreated outcomes for the treated observations (what the values would have been in the absence of treatment). The estimator then calculates a weighted average of the estimated treatment effects for each treated observation to generate

[20]Not every treatment year has 4 years of pre-period data or 5 years of post-period data; for example, the 2006 treatment year will only have 2 years of pre-period data and the 2009 treatment year will only have 3 years of post-period data, as it is an unbalanced panel.

the corresponding difference-in-differences coefficient. Further detail on the estimator is available in Borusyak, Jaravel, and Speiss (forthcoming).

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, in addition to the restaurant-only ban variable. Column 2 adds demographic controls, region-by-month-year fixed effects, and policy controls: an indicator for the blood alcohol concentration limit being .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 drops individuals residing in the handful of places that implemented a smoking ban in bars before a smoking ban in restaurants. Column 5 presents results using the stacked difference-in-differences approach (with annual stacks). Column 6 presents results using the DiD imputation estimator detailed in Borusyak, Jaravel, and Speiss (forthcoming). 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 lead to an increase of .6 servings of alcohol consumed per month, a 5.3 percent increase. This effect is significant at the 1 percent level. The larger point estimates for the alternative estimators, 1.2 servings for the stacked estimator and 1.1 servings for the DiD imputation estimator (both significant at the 1 percent level), are consistent with the increasing treatment effects shown in the event study in the top panel of Figure 2, as a positive and increasing treatment effect over time leads to attenuation bias in a traditional two-way-fixed-effects specification. Over time the post-period coefficients increase from $\frac{1}{2}$ to $1\frac{1}{2}$ drinks per month, and starting 1 year after implementation they are individually statistically significant at the 5 percent level. The pre-period coefficients are close to zero and not statistically significant.

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 4 columns: precisely estimated null effects. In the preferred specification (Column 2), smoking bans are associated with a .2 percentage point increase in the probability of drinking any alcohol in the past 30 days, a .4 percent increase that is not statistically significant. The event study results are consistent with this estimate: the coefficients are small and generally not statistically significant (bottom panel of Figure 2). The stacked difference-in-differences and DiD imputation point estimates are statistically significant at the 5 percent level and larger compared to the other estimates, although still economically small (.5 to .7 percentage points; a little more than a 1 percent 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 1 serving of alcohol, a 4.7 percent increase that is significant at the 5 percent level. For the corresponding event study (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 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 percent level. This graph is consistent with the point estimate in Table 2, Column 2, and with the slightly larger estimates for the alternative estimators. The stacked difference-in-differences point estimate is 1.9 and the DiD imputation estimate is 1.4, representing a 6 to 9 percent increase that is statistically significant at the 1 percent level.

4.2 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 (Table 3). In my preferred specification (Column 2), the implementation of smoking bans in bars is associated with an average decrease in the quantity of servings of alcohol purchased for off-premises consumption of .3 drinks per month (Panel A), an approximately 2 percent decline that is not statistically significant. The stacked difference-in-differences coefficient is a precise null (.05), while the DiD imputation estimate is similar in magnitude to the preferred specification but positive (.4). Neither of these estimates are statistically significant. Smoking bans in bars do not appear to have an

effect on the total quantity of alcohol purchased by households 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 two-way-fixed-effects coefficient is very small and negative (-.2, a .8 percent decline) while the stacked and DiD imputation coefficients are small and positive (.4 to .5, a 1 to 2 percent increase). The stacked difference-in-differences coefficient is statistically significant but the others are not, and all effects are economically small.

Along the intensive margin, the primary two-way-fixed-effects specification finds small negative effects: -.5 servings of alcohol per month, approximately a 1 percent decrease. The stacked difference-in-differences coefficient is a precise null (.2, or .3 percent). Neither is statistically significant. The DiD imputation estimate, on the other hand, is positive and statistically significant at the 1 percent level: 3.1 drinks per month, a 5.7 percent 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* (.4 servings of alcohol per month; Panel A Column 6) is markedly smaller than the effect size for total alcohol consumed by *individuals* in the BRFSS (1.1; Panel A Column 6 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.

4.3 Event Studies Robustness Checks

The stacked and DiD imputation estimator event studies for alcohol consumption are shown in the Appendix and are similar to the standard event studies. For the total amount of alcohol consumed in the stacked event study specification (top-left panel of Appendix Figure D.1), there is no pre-trend, while in the years after a smoking ban in bars is implemented, alcohol consumption increases by approximately 1 to 2 drinks per month several years after taking effect. The coefficients for years 2 to 5 are individually statistically significant at the 1 percent 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 (top-right panel of Appendix Figure D.1), the coefficients in both the pre and post periods look similar to each other and are generally not statistically significant. These results are also similar to the traditional event study results shown in the bottom panel of Figure 2. For the intensive margin (bottom panel of Appendix Figure D.1), the pre-period coefficients are small, negative, and not statistically significant, while in the years following a smoking ban, alcohol consumption increases by 1 to 3 drinks per month. The post-period coefficients are individually statistically significant starting in year 2 and slightly larger than the traditional event study results shown in 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 led to increases in alcohol consumption.

The DiD imputation event studies, shown in Appendix Figure D.2, also show increases in alcohol consumption, although the pre-period standard errors are relatively large. For overall alcohol consumption (top-left panel of Appendix Figure D.2), 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 (bottom panel of Appendix Figure D.2) also increases to 2-3 drinks per 30 days several years after smoking bans, while the pre-period coefficients are small and just below zero.

4.4 Heterogeneous Effects of Smoking Bans

The net increases in alcohol consumption in Section 4.1 may mask heterogeneous effects for different groups, such as smoking status, age, or sex. 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. These differences may be particularly pronounced for smokers in colder weather, when the cost of stepping outside to smoke is higher. Younger adults and men are more likely to drink and drink more alcohol than older adults and women, respectively, so a smoking ban in a bar may have more of an effect on them. Younger adults in particular may also be more likely to frequent bars. 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.

Disaggregating effects on alcohol consumption by smoking status shows that increases in alcohol consumption are concentrated among current and former smokers (Table 4). Current smokers increase their alcohol consumption by approximately 1.5 drinks per 30 days (7.4

percent increase), which is marginally statistically significant. Smoking bans lead former smokers to drink a little over .5 additional drinks per 30 days (4.5 percent increase), which is also marginally significant.[21] There is a marginally significant but very small decline in the extensive margin of alcohol consumption for current smokers (1 percentage point, a 1.6 percent decrease). Along the intensive margin, current smokers increase their alcohol consumption by a little over 3 drinks per 30 days (9 percent increase), an effect that is statistically significant at the 5 percent 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 (for example, the smokers who drank the least were the ones who quit drinking). Former smokers' alcohol consumption increases by 1 drink over 30 days, a 4.6 percent increase that is also significant at the 5 percent level.

Turning to alcohol purchases, the effects of smoking bans by smoking status are similar to the overall results for alcohol purchases. After the implementation of smoking bans in bars, smoking households' monthly alcohol purchases for off-premises consumption do not change (-.01 servings of alcohol) (Panel A, Column 1 of Table 5) and nonsmoking households' purchases decline by .25 servings of alcohol (Column 2). Neither of these effects are statistically significant and they are economically small, representing a .1 percent and 2.2 percent decline, respectively. Along the extensive margin, smoking households are .9 percentage points less likely to purchase alcohol after a smoking ban (3 percent decrease) and nonsmoking households are .05 percentage points more likely to purchase alcohol (.6 percent increase). Neither of these effects are statistically significant. Along the intensive margin,

[21] Event studies, shown in Appendix Figure D.3, are consistent with these results.

smoking households' alcohol purchases, conditional on purchasing alcohol, increase by 2.7 servings of alcohol per month. This effect is not statistically significant and is small (4 percent 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 .8 servings per month, a 1.7 percent decrease that is not statistically significant.

Smoking bans in bars may have heterogeneous effects by climate, particularly for smokers. Stepping out of a bar to smoke a cigarette may be more miserable during the winter months in a cold or snowy climate. I test whether smoking bans differentially affect alcohol consumption and smoking in cold climates in an alternative specification that interacts the smoking ban variables with an indicator for cold weather.[22] The effects of smoking bans on alcohol consumption are generally similar across climates, as seen in Table 6. The effects on total and intensive-margin alcohol consumption for current smokers are larger in cold climates, an increase of 2.1 versus 1.3 drinks per 30 days for total alcohol consumption, although these effect sizes are not statistically significantly different from each other. For alcohol purchases (Table 7), effects by climate generally mirror the results for alcohol purchases in Tables 3 and 5. A notable difference is for extensive-margin alcohol purchases: effects for all households and smoking households are negative and statistically significant for smoking bans in cold weather, and these effects (as well as the one for nonsmoking households) are statistically

[22]Cold weather is defined as the fall and winter months (October through March) for the Mountain, West North Central, East North Central, Middle Atlantic, and New England Census divisions.

significantly different (smaller) than the effects of smoking bans in "not cold" weather.

The effects of smoking bans on alcohol consumption are concentrated among younger (of-age) adults, as shown in Table 8. Adults between the ages of 21 and 34 increase their alcohol consumption by approximately 1 drink per 30 days, a nearly 8 percent increase. This effect is marginally statistically significant. There is no change along the extensive margin and a marginally significant 1.9-drink-per-30-days increase along the intensive margin (8 percent). Alcohol consumption also increases for 35-54 year olds but by a lesser amount: .5 drinks per 30 days (4.2 percent 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.

Smoking bans may have differential effects on alcohol consumption by smoking status for men and women, given gender disparities in both smoking and alcohol consumption. Effects for women are concentrated among current smokers (Column 1 of Table 9), who increase their total alcohol consumption by .9 drinks per 30 days, an 8 percent increase that is marginally significant. This effect corresponds to an intensive-margin increase of over 1.5 drinks per 30 days, an 8 percent increase that is also marginally significant. For men the effects are driven by current and former smokers. Male current smokers increase their total alcohol consumption by 1.7 drinks per 30 days, which represents a nearly 6 percent increase although it is not statistically significant. They are also 1.5 percentage points less likely to drink alcohol post-smoking bans, which is economically small (2.3 percent decrease) but statistically significant at the 5 percent level. The resulting intensive-margin increase of 3.6 drinks per 30 days (8 percent increase and marginally significant) may thus be a result of compositional changes in the pool of men who smoke and drink (for example, perhaps the

male smokers who drank the least quit drinking). Male former smokers increase their alcohol consumption by .9 drinks per 30 days, a 5.3 percent increase that is marginally statistically significant. This translates to a 1.6 drink-per-30-days increase along the intensive margin, a nearly 6 percent increase that is statistically significant at the 5 percent level. Effects for gender (not by smoking status) are included in Appendix Table D.3 and document broadly similar results.

Appendix C 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, more on the days they drink, or both? Understanding the effects at a more detailed level can illustrate whether these changes in drinking behavior may have negative health consequences.[23] Appendix C also includes results from a state-level analysis, to more directly compare my results with earlier work on smoking bans' effect on alcohol consumption. The results are consistent with my main results.

To provide further support for the changing environment of bars being the reason for increases in alcohol consumption, I present effects of restaurant-only smoking bans in Appendix Table D.4 for the main measures of alcohol consumption and purchases. These effects come from the preferred specification (Column 2 of Tables 2 and 3) and represent the effect of a restaurant-only smoking ban relative to no bar or restaurant smoking ban. If the coefficient on the restaurant-only smoking ban is of a similar magnitude and sign as the coefficient on the bar (and restaurant) smoking ban, it would imply that smoking bans in bars have no marginal effect on alcohol consumption. If, however, the restaurant-only coefficient for

[23]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).

consumption is 0 or negative, that indicates that smoking bans in bars do have an effect on alcohol consumption independent of smoking bans in restaurants.

I find restaurant-only smoking bans lead to marginally significant reductions in alcohol consumption of .6 drinks per month overall and 1.1 drinks per month along the intensive margin (5 percent reductions), with no effect on extensive-margin consumption. While these reductions are comparable in magnitude to the increases in consumption from bar and restaurant smoking bans, they are not offsetting because the "bar and restaurant" smoking ban and "restaurant-only" smoking ban are mutually exclusive by definition. For alcohol purchases, restaurant-only smoking bans (compared to no bar or restaurant smoking bans) are associated with small and statistically insignificant increases in purchases along all margins, which shows that the decline in consumption resulting from restaurant-only smoking bans is likely not from declines in off-premises consumption. Taken together, the results for restaurant-only smoking bans imply the reductions in consumption are from changes in on-premises consumption. They also provide support for the increases in alcohol consumption from smoking bans in bars and restaurants being driven by the changing environment of bars.

Nearly half of individuals and three-quarters of households do not consume or purchase any alcohol (for off-premises consumption) in the past month, which means there are many zeroes for these variables. In a robustness check, I test whether the results for total alcohol consumption and purchases are similar under a Poisson specification, which is better equipped to handle count data with many zeroes (Appendix Table D.5).[24] Comparing Panel

[24]For the Nielsen regressions, I replace household fixed effects with controls for age of the eldest household head, marital status, race/ethnicity, highest level of educational attainment for a household head, whether either head is employed, number of adults in the household, an indicator for whether minor children live in the household, and indicators for an unmarried female or male head of household. I am unable to use

A, Column 1 (total alcohol consumption for all individuals) to Panel A, Column 2 of Table 2 (my preferred specification), the results are identical: an increase of .6 drinks per 30 days, significant at the 1 percent level. Effect sizes are also quantitatively similar for alcohol consumption by smoking status (Columns 2 to 4 of Appendix Table D.5, Panel A versus Columns 1 to 3 of Table 4, Panel A). For alcohol purchases, the Poisson specification yields effect sizes that are larger in magnitude and statistically significant for all household and smoking households, and similar in magnitude for nonsmoking households (Panel B of Appendix Table D.5 versus Table 3, Panel A, Column 2 and Table 5, Panel A).

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.[25] In this paper, I use the Behavioral Risk Factor Surveillance System and the Nielsen Consumer Panel to test whether smoking bans in bars 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 that uses variation in effective dates of smoking bans in bars at the city, county, and state level.

Smoking bans in bars result in average increases in alcohol consumption of approximately 1 drink per month (conditional on drinking), or 5 percent. These increases occur for current

household fixed effects in the Poisson specification because of the statistical separation problem (Correia, Guimarães, and Zylkin 2020; Correia, Guimarães, and Zylkin 2021).

[25] 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 (for example, Anger, Kvasnicka, and Siedler 2011; Bharadwaj, Johnsen, and Løken 2014; Jones et al. 2015; and Kvasnicka, Siedler, and Ziebarth 2018).

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. Regardless of their motivations for drinking more at bars, the results for current smokers are consistent with prior research that found smokers, particularly smokers who drink, increased their use of smokeless tobacco following the implementation of smoking bans in bars (Adams, Cotti, and Furhmann 2013). Taken together, these results imply that smokers with a sufficiently inelastic demand for drinking at bars responded to bar smoking bans by substituting alcohol and smokeless tobacco for cigarettes, not by substituting drinking at home for drinking at a bar, contrary to the fears of many bar owners before these bans were implemented.

Former smokers may have an additional reason for increasing their alcohol consumption. Prior to smoking bans, they may have avoided smoke-filled bars and restaurants, for 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).[26] After smoking bans are implemented, former smokers may feel more comfortable going out to bars, or more comfortable staying there longer, hence increasing their alcohol consumption.

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

How do these effect sizes compare to other policies that affect alcohol consumption? The overall effect on alcohol consumption (.6 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 percent increase in beer sales and a 3.5 percent 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 percent 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. Such an analysis would permit a test of the mechanisms by which Adams and Cotti (2008) find that drunk-driving fatalities increased following the implementation of smoking bans in bars and restaurants: is the effect due to an increase in alcohol consumption at bars and restaurants or is it due to smokers driving further to drink at bars where they can smoke?

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 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 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. However, taking these effect sizes at face value, a back-of-the-envelope calculation yields an estimated average increase in on-premises consumption of .75 servings per month, a 17 percent increase in imputed on-premises alcohol sales.[27] While this back-of-the-envelope estimate implies a sizeable percentage increase in on-premises sales, it reflects an increase of less than one drink per month, which may not necessitate increases in bar and restaurant employment. Given that much of the literature on the effects of smoking bans on economic outcomes for bars and restaurants finds null or small positive effects, this estimate is qualitatively in line with those findings.

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.

[27] I find a .6-serving increase in alcohol consumption and a .3-serving decline in off-premises alcohol purchases per household. An average of 2 adults per household in the Nielsen sample implies a .15-serving decrease per person. Assuming purchases are a good proxy for consumption implies a .6- -.15 = .75-serving increase in on-premises consumption per month. Assuming total consumption equals total sales, subtracting per-person off-premises alcohol purchases (14/2 = 7) from total consumption (11.4) yields imputed on-premises consumption or sales of 4.4-servings per month. Dividing .75 by 4.4 yields a 17 percent increase in on-premises sales.

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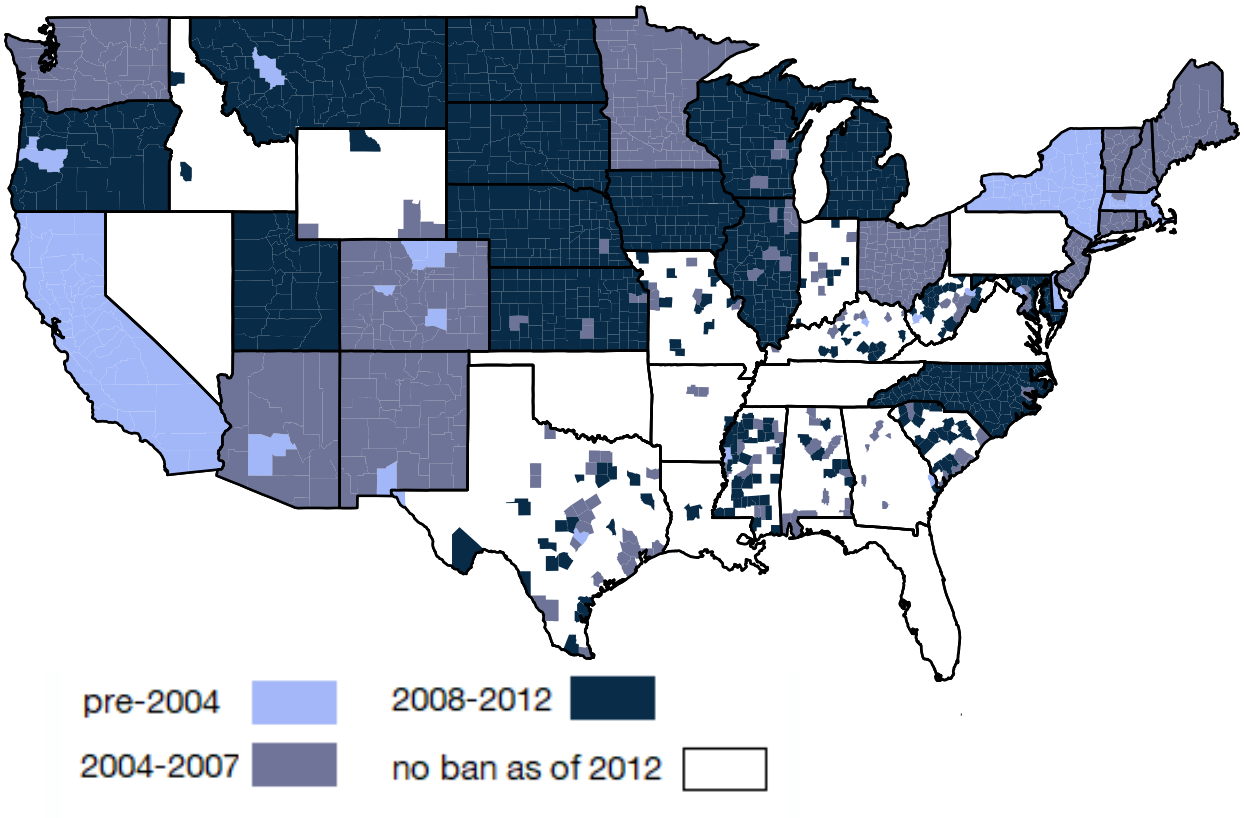
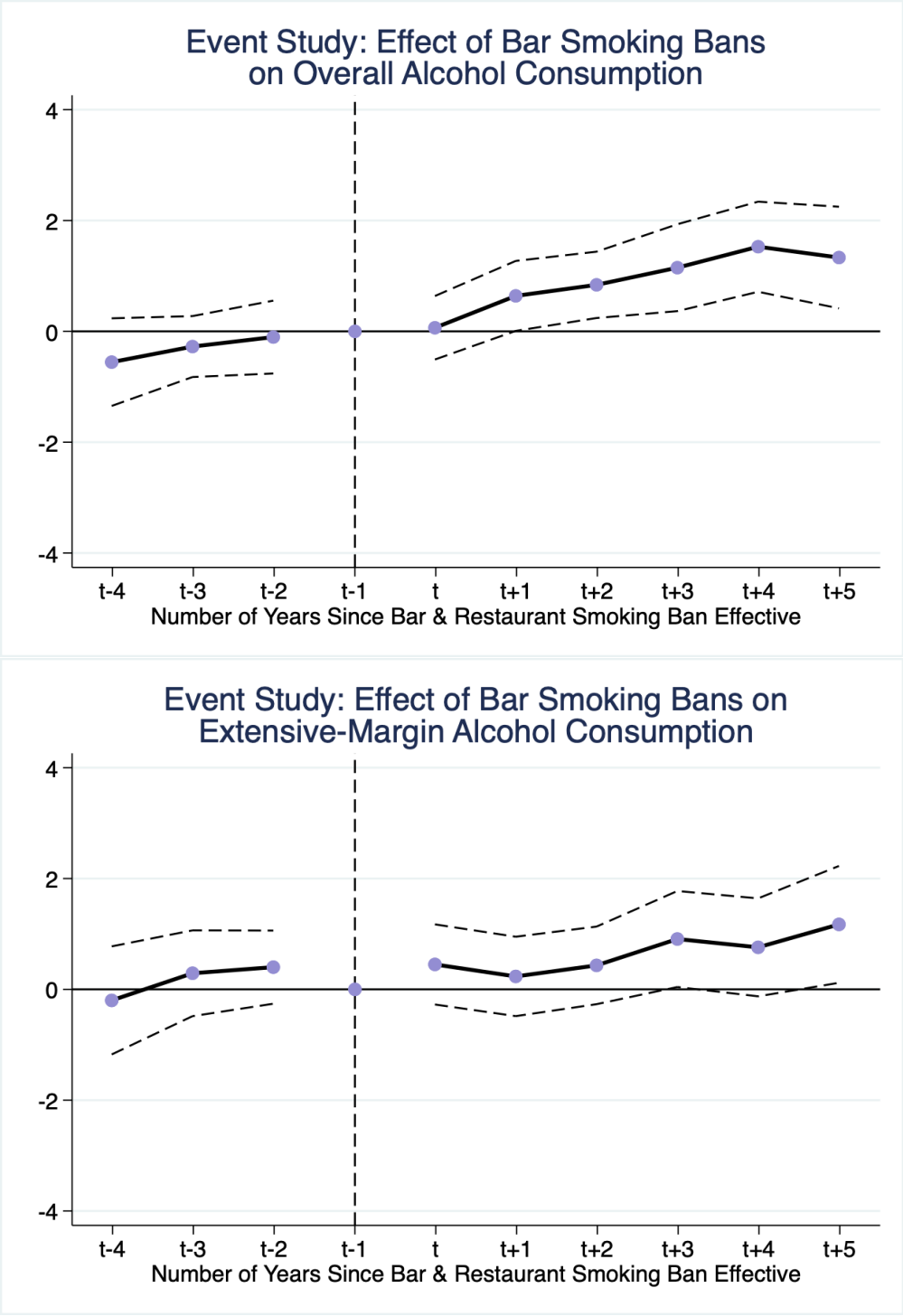
7 Figures and Tables

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



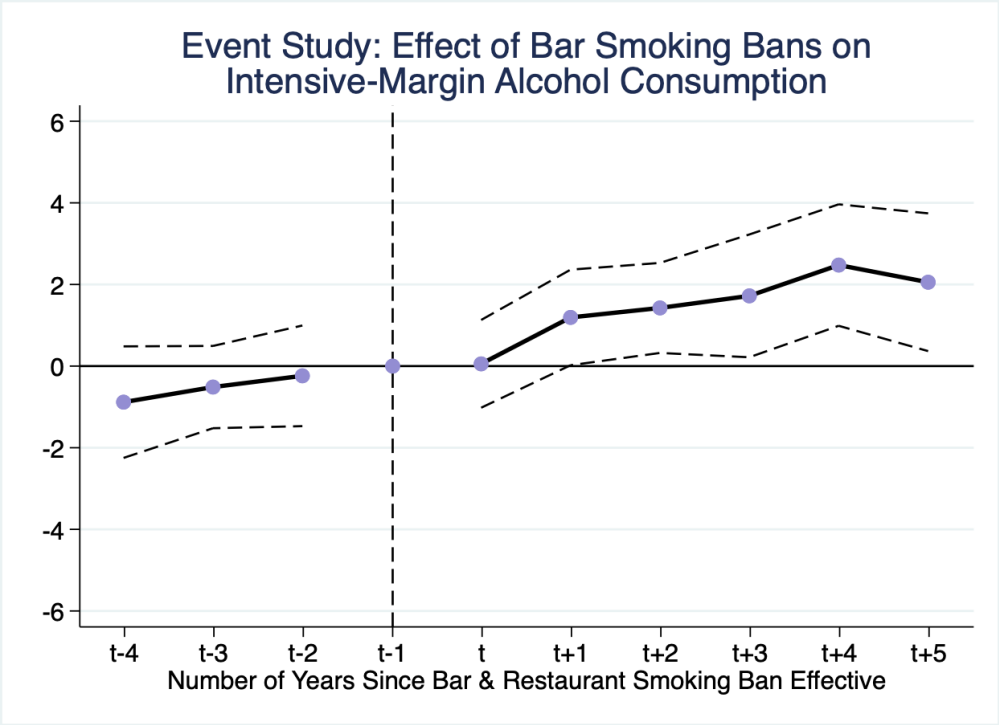
Data Source: American Nonsmokers' Rights Foundation

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 .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. F-statistics on joint significance of pre-period coefficients: top panel F = .67, bottom panel F = 1.21.

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 .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. Outcome: number of servings of alcohol consumed in the past 30 days for individuals who drink. Data source: BRFSS 2004-2012. F-statistic on joint significance of pre-period coefficients: F = .63.

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	0.54	0.00	0.00	0.75
	(0.50)	(0.00)	(0.00)	(0.43)
Ever bar ban	0.73	0.00	1.00	1.00
	(0.45)	(0.00)	(0.00)	(0.00)
Fraction restaurant-only ban	0.11	0.33	0.07	0.03
	(0.30)	(0.47)	(0.23)	(0.14)
Alcohol consumption:	11.96	11.29	11.44	12.22
total servings	(37.30)	(39.83)	(36.28)	(36.30)
Alcohol consumption:	53.44	48.59	53.44	55.28
extensive margin (p.p.)	(49.88)	(49.98)	(49.88)	(49.72)
Alcohol consumption:	22.56	23.49	21.57	22.25
intensive margin	(48.84)	(54.89)	(47.58)	(46.65)
Alcohol consumption:	8.34	8.35	7.95	8.34
# days	(8.59)	(8.71)	(8.39)	(8.55)
Alcohol consumption:	2.48	2.51	2.49	2.48
amount per day	(2.72)	(2.78)	(2.60)	(2.70)
Alcohol consumption:	3.67	3.65	3.74	3.68
max.	(3.71)	(3.76)	(3.74)	(3.69)
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	3,161,715	798,292	697,418	2,363,423

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 Smoking Bans on Alcohol Consumption (BRFSS)

Panel A: Total Alcohol Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	0.62***	0.60***	0.64***	0.65***	1.20***	1.06***
(standard error)	(0.21)	(0.22)	(0.19)	(0.22)	(0.20)	(0.28)
Pre-Ban Mean	11.44	11.44	11.44	11.43	11.48	11.44
% Effect	5.38%	5.28%	5.58%	5.72%	10.47%	9.30%
N	3,066,169	3,066,169	3,066,169	3,019,178	2,306,635	2,594,656
Panel B: Extensive-Margin Alcohol Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.13	0.20	0.10	0.21	0.54**	0.72**
(standard error)	(0.24)	(0.23)	(0.22)	(0.23)	(0.27)	(0.34)
Pre-Ban Mean	53.42	53.42	53.42	53.38	53.04	53.42
% Effect	-0.24%	0.37%	0.19%	0.39%	1.02%	1.36%
N	3,088,951	3,088,951	3,088,951	3,041,592	2,324,064	2,614,107
Panel C: Intensive-Margin Alcohol Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	1.10***	1.01**	1.13***	1.10***	1.88***	1.39***
(standard error)	(0.37)	(0.38)	(0.35)	(0.39)	(0.37)	(0.49)
Pre-Ban Mean	21.57	21.57	21.57	21.58	21.81	21.57
% Effect	5.08%	4.68%	5.23%	5.11%	8.63%	6.45%
N	1,520,926	1,520,926	1,520,926	1,492,243	1,096,722	1,259,070
Demographics		X	X	X	X	X
County & Time FE	X	X	X	X	X	X
Region-by-Time FE		X	X	X	X	X
Policy Controls		X	X	X	X	X
Fraction Treated	X	X		X	X	X
Drop Bar Ban 1st				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 (included in Column (1) regression), (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, except in Column (3) where it is a binary variable equal to 1 if any part of the county is covered. "Drop Bar Ban 1st" denotes a robustness check where I exclude individuals residing in counties that implemented a bar ban prior to a restaurant ban. 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 Smoking Bans on Alcohol Purchases (Nielsen)

Panel A: Total Quantity of Alcohol Purchases for Off-Premises Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.21	-0.31	-0.30	-0.29	0.05	0.35
(standard error)	(0.23)	(0.26)	(0.23)	(0.26)	(0.28)	(0.36)
Pre-Ban Mean	14.02	14.02	14.02	13.99	13.81	14.02
% Effect	-1.53%	-2.22%	-2.11%	-2.05%	0.35%	2.53%
N	5,752,884	5,752,884	5,752,884	5,632,932	4,068,084	3,823,212
Panel B: Extensive-Margin Alcohol Purchases for Off-Premises Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	0.01	-0.20	-0.07	-0.17	0.54**	0.38
(standard error)	(0.20)	(0.22)	(0.19)	(0.22)	(0.25)	(0.30)
Pre-Ban Mean	25.95	25.95	25.95	25.91	25.55	25.95
% Effect	0.05%	-0.76%	-0.26%	-0.67%	2.12%	1.45%
N	5,752,884	5,752,884	5,752,884	5,632,932	4,068,084	3,823,212
Panel C: Intensive-Margin Alcohol Purchases for Off-Premises Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.08	-0.49	-0.54	-0.36	0.16	3.07***
(standard error)	(0.69)	(0.76)	(0.71)	(0.77)	(0.87)	(1.06)
Pre-Ban Mean	54.01	54.01	54.01	54.02	54.04	54.01
% Effect	-0.15%	-0.90%	-1.00%	-0.67%	0.29%	5.68%
N	1,531,991	1,531,991	1,531,991	1,496,532	1,038,490	974,557
Household FE	X	X	X	X	X	X
County & Time FE	X	X	X	X	X	X
Region-by-Time FE		X	X	X	X	X
Policy Controls		X	X	X	X	X
Fraction Treated	X	X		X	X	X
Drop Bar Ban 1st				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 (included in Column (1) regression), (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, except in Column (3) where it is a binary variable equal to 1 if any part of the county is covered. "Drop Bar Ban 1st" denotes a robustness check where I exclude individuals residing in counties that implemented a bar ban prior to a restaurant ban. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: Nielsen Consumer Panel 2004-2012.

Table 4: Effect of Bar 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*	0.27	0.56*
(standard error)	(0.79)	(0.18)	(0.30)
Pre-Ban Mean	20.77	7.60	12.54
% Effect	7.38%	3.50%	4.49%
N	528,612	1,624,839	900,193
Panel B: Extensive-Margin Alcohol Consumption			
Smoking Status:	Current	Never	Former
	(1)	(2)	(3)
Smoking Ban	-0.96*	0.62*	-0.17
(standard error)	(0.54)	(0.32)	(0.39)
Pre-Ban Mean	61.63	48.69	57.46
% Effect	-1.56%	1.28%	-0.29%
N	535,750	1,633,683	906,694
Panel C: Intensive-Margin Alcohol Consumption			
Smoking Status:	Current	Never	Former
	(1)	(2)	(3)
Smoking Ban	3.14**	0.30	1.00**
(standard error)	(1.26)	(0.34)	(0.47)
Pre-Ban Mean	34.02	15.71	21.93
% Effect	9.22%	1.89%	4.55%
N	283,753	743,001	488,190
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 run on subsamples corresponding to the designated smoking status. 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 5: Effect of Bar Smoking Bans on Alcohol Purchases by Smoking Status (Nielsen)

Panel A: Total Alcohol Purchases		
Smoking Status:	Smoker (1)	Nonsmoker (2)
Smoking Ban	-0.01	-0.25
(standard error)	(0.78)	(0.24)
Pre-Ban Mean	20.95	11.77
% Effect	-0.06%	-2.16%
N	1,106,460	4,646,388
Panel B: Extensive-Margin Alcohol Purchases		
Smoking Status:	Smoker (1)	Nonsmoker (2)
Smoking Ban	-0.92	0.05
(standard error)	(0.62)	(0.24)
Pre-Ban Mean	31.26	24.23
% Effect	-2.94%	0.59%
N	1,106,460	4,646,388
Panel C: Intensive-Margin Alcohol Purchases		
Smoking Status:	Smoker (1)	Nonsmoker (2)
Smoking Ban	2.66	-0.83
(standard error)	(2.11)	(0.68)
Pre-Ban Mean	67.00	48.58
% Effect	3.96%	-1.70%
N	343,305	1,185,176
Household FE	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 run on subsamples corresponding to the designated smoking status. Smoker is defined as a household that purchased any cigarettes in the calendar year. 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 6: Effect of Bar Smoking Bans on Alcohol Consumption (BRFSS), by Weather

Panel A: Total Alcohol Consumption				
Smoking Status:	All	Current	Never	Former
	(1)	(2)	(3)	(4)
Smoking Ban: Cold Weather	0.65*	2.07*	0.01	0.59
(standard error)	(0.33)	(1.11)	(0.34)	(0.39)
Smoking Ban: Not Cold Weather	0.59***	1.29	0.38**	0.55*
(standard error)	(0.22)	(0.81)	(0.18)	(0.33)
N	3,066,169	528,612	1,624,839	900,193
Panel B: Extensive-Margin Alcohol Consumption				
Smoking Status:	All	Current	Never	Former
	(1)	(2)	(3)	(4)
Smoking Ban: Cold Weather	0.13	-1.03	0.74*	-0.53
(standard error)	(0.32)	(0.77)	(0.43)	(0.52)
Smoking Ban: Not Cold Weather	0.23	-0.94	0.57	-0.01
(standard error)	(0.25)	(0.58)	(0.36)	(0.43)
N	3,088,951	535,750	1,633,683	906,694
Panel C: Intensive-Margin Alcohol Consumption				
Smoking Status:	All	Current	Never	Former
	(1)	(2)	(3)	(4)
Smoking Ban: Cold Weather	1.13**	4.21**	-0.12	1.15*
(standard error)	(0.56)	(1.80)	(0.60)	(0.61)
Smoking Ban: Not Cold Weather	0.95**	2.64**	0.48	0.92*
(standard error)	(0.39)	(1.29)	(0.33)	(0.54)
N	1,520,926	283.753	743,001	488,190
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 a variation of the estimation specified in Equation 1, with the smoking ban variable interacted with an indicator for cold weather, and run on subsamples corresponding to the designated smoking status (Columns (2) through (4)). 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 7: Effect of Bar Smoking Bans on Alcohol Purchases (Nielsen), by Weather

Panel A: Total Quantity of Alcohol Purchases for Off-Premises Consumption			
Smoking Status:	All	Smoker	Nonsmoker
	(1)	(2)	(3)
Smoking Ban: Cold Weather	-0.38	0.18	-0.33
(standard error)	(0.27)	(0.26)	(0.62)
Smoking Ban: Not Cold Weather	-0.28	-0.13	-0.21
(standard error)	(0.26)	(0.79)	(0.25)
N	5,752,884	1,106,460	4,646,388
Panel B: Extensive-Margin Alcohol Purchases for Off-Premises Consumption			
Smoking Status:	All	Smoker	Nonsmoker
	(1)	(2)	(3)
Smoking Ban: Cold Weather	-0.64**	-1.46**	-0.36
(standard error)	(0.25)	(0.62)	(0.27)
Smoking Ban: Not Cold Weather	0.02	-0.65	0.26
(standard error)	(0.23)	(0.65)	(0.24)
N	5,752,884	1,106,460	4,646,388
Panel C: Intensive-Margin Alcohol Purchases for Off-Premises Consumption			
Smoking Status:	All	Smoker	Nonsmoker
	(1)	(2)	(3)
Smoking Ban: Cold Weather	0.09	3.85*	-0.36
(standard error)	(0.78)	(2.26)	(0.72)
Smoking Ban: Not Cold Weather	-0.77	2.05	-1.04
(standard error)	(0.81)	(2.15)	(0.72)
N	1,531,991	343,305	1,185,176
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, with the smoking ban variable interacted with an indicator for cold weather, and run on subsamples corresponding to the designated smoking status (Columns (2) and (3)). Smoker is defined as a household that purchased cigarettes at any point during the calendar year. 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 8: Effect of Bar Smoking Bans on Alcohol Consumption (BRFSS), by Age Group

Age:	18-20	21-34	35-54	55+
Panel A: Total Alcohol Consumption				
	(1)	(2)	(3)	(4)
Smoking Ban	0.15	1.11*	0.49*	0.27
(standard error)	(1.30)	(0.59)	(0.28)	(0.19)
Pre-Ban Mean	10.25	14.04	11.77	8.98
% Effect	1.49%	7.91%	4.20%	3.04%
N	49,215	392,675	1,077,213	1,546,818
Panel B: Extensive-Margin Alcohol Consumption				
	(1)	(2)	(3)	(4)
Smoking Ban	1.11	-0.19	0.36	0.15
(standard error)	(1.40)	(0.54)	(0.33)	(0.32)
Pre-Ban Mean	37.53	60.66	58.03	44.13
% Effect	2.97%	-0.31%	0.62%	0.35%
N	49,914	396,799	1,084,753	1,557,238
Panel C: Intensive-Margin Alcohol Consumption				
	(1)	(2)	(3)	(4)
Smoking Ban	-1.78	1.85*	0.68	0.43
(standard error)	(3.29)	(0.96)	(0.46)	(0.40)
Pre-Ban Mean	28.08	23.33	20.38	20.51
% Effect	-6.32%	7.95%	3.34%	2.08%
N	16,964	232,201	607,890	665,380
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 run on subsamples corresponding to the designated age groups. 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 9: Effect of Bar Smoking Bans on Alcohol Consumption by Smoking Status and Gender (BRFSS)

Smoking Status:	Current	Current	Never	Never	Former	Former
Gender:	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total Alcohol Consumption						
Smoking Ban	0.87*	1.71	0.12	0.52	0.19	0.87*
(standard error)	(0.47)	(1.40)	(0.11)	(0.38)	(0.22)	(0.52)
Pre-Ban Mean	10.85	29.37	4.28	11.85	7.69	16.48
% Effect	8.03%	5.81%	2.74%	4.37%	2.51%	5.30%
N	311,600	216,984	1,089,251	535,581	487,709	412,465
Panel B: Extensive-Margin Alcohol Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.01	-1.54**	0.36	0.92*	0.01	-0.48
(standard error)	(0.63)	(0.78)	(0.39)	(0.54)	(0.54)	(0.57)
Pre-Ban Mean	54.11	68.12	42.21	56.95	52.79	61.23
% Effect	-0.01%	-2.26%	0.86%	1.62%	0.01%	-0.78%
N	314,735	220,986	1,094,108	539,569	490,503	416,173
Panel C: Intensive-Margin Alcohol Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	1.56*	3.62*	0.07	0.57	0.34	1.59**
(standard error)	(0.80)	(1.98)	(0.27)	(0.62)	(0.38)	(0.76)
Pre-Ban Mean	20.26	43.48	10.21	20.95	14.63	27.05
% Effect	7.68%	8.32%	0.72%	2.71%	2.31%	5.88%
N	149,778	133,876	436,558	306,392	246,287	241,827
Demographics	X	X	X	X	X	X
County & Time FE	X	X	X	X	X	X
Region-by-Time FE	X	X	X	X	X	X
Policy Controls	X	X	X	X	X	X
Fraction Treated	X	X	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 run on subsamples corresponding to the designated gender and smoking status. Demographic controls are fixed effects for 5-year age bins, marital status, 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.[28] 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 3.4. 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.

[28]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 zero.

B 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. I generally find precise null or economically small effects of smoking bans on smoking status. In the preferred specification (Column 2 of Appendix Table D.6), bar smoking bans are associated with a .25 percentage point increase in the prevalence of current smoking (1.2 percent), a .3 percentage point decrease in never smoking (.5 percent), and a .05 percentage point increase in former smoking (.2 percent). 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 .5 percentage points (2 percent) while the prevalence of former

smoking declines by .6 percentage points (2 percent). These effects are significant at the 5 and 1 percent level, respectively. The DiD imputation estimates are more in line with the two-way-fixed-effects specification: current smoking increases by .45 percentage points (2.2 percent), never smoking declines by .3 percentage points (.5 percent), and former smoking declines by .15 percentage points (.6 percent). 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. The event studies in Appendix Figure D.4 are consistent with the point estimates in Column 2.

The effect of smoking bans on cigarette purchases (extensive and intensive-margin) are consistent with the results for smoking status. Appendix Table D.7 documents null effects for both measures across various specifications. The event study for extensive-margin cigarette purchases (top panel of Appendix Figure D.5) mirrors the one for current smoking (top-left panel of Appendix Figure D.4) and is also consistent with the point estimate in Column 2. For intensive-margin purchases, there appears to be a pre-trend (p-value of F-statistic for test of joint significance $< .07$), suggesting these results need to be interpreted with caution, but the event study shows null effects in the post period. These results, using a different dataset, suggest that smoking bans in bars did not have an effect on smoking.

Turning to heterogenous effects of smoking bans on smoking by weather, I find null effects of smoking bans in both cold and "not cold" climates on smoking and cigarette purchases (Appendix Table D.8). To the extent that habit formation and addiction play a key role in

smoking, the wintry months may be insufficient in length or misery to induce smokers to quit smoking or reduce their cigarette consumption long term. Further, time spent at bars is likely a relatively small fraction of smokers' time use; even if they do smoke less at bars they can compensate by smoking more later.

These results may seem different at first glance than earlier work that finds (workplace) smoking bans reduce the prevalence of smoking (for example, 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.[29] In addition, smokers' ability to substitute smokeless tobacco for cigarettes while at bars reduces their incentive to quit smoking because they are not effectively prohibited from consuming nicotine (Adams, Cotti, and Furhmann 2013).

C Additional Robustness Checks

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

Panels A and B of Appendix Table D.9 disaggregate the effects on intensive-margin alcohol consumption into these two components. In my primary specification, for individuals who drank alcohol in the past 30 days, smoking bans in bars are associated with an increase in the number of days spent drinking (out of the past 30 days) of .1 days (1 percent), on

[29]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.

average, which is marginally statistically significant (Panel A, Column 2). This effect is similar in the stacked specification and attenuated in the DiD imputation specifications. Combined, these results imply smoking bans have at most a very small effect on the number of days individuals drink alcohol, because even the statistically significant results are not economically meaningful.

The implementation of smoking bans in bars results in a .06-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 percent level and represents a 2 percent increase. The stacked and DiD imputation estimates are slightly larger, at .09 and .07 drinks per day, a 3 to 4 percent increase. These effects are significant at the 1 percent and 5 percent 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 leads to an increase in the maximum amount of alcohol consumed of .05 servings, on average, in the primary specification (Panel C, Column 2). This effect is marginally statistically significant and it represents a 1.4 percent increase in maximum alcohol consumption. The effect sizes for the stacked and DiD imputation estimators are similar in magnitude but the latter is not statistically significant. These results suggest that any increases in the maximum amount of alcohol consumed on one occasion are very small, which is not concerning from a public health perspective.

Turning to alcohol purchases, overall null effects of smoking bans on alcohol purchased for off-premises consumption could mask heterogeneous effects by type of alcohol (beer, wine,

and liquor). The Nielsen Consumer Panel contains detailed product characteristics, allowing me to test for these heterogeneous effects (Appendix Table D.10). After the implementation of smoking bans in bars, household purchases of beer (Panel A) decline by statistically and economically insignificant amounts across most specifications. The stacked difference-in-differences estimate is the sole exception, with an estimated effect size over twice as large as the preferred specification (-.6 servings of beer per month, a 7 percent reduction) that is statistically significant at the 5 percent level. Across all specifications, household purchases of wine are flat after smoking bans are implemented (Panel B), an effect that is precisely estimated. The effect of smoking bans on household purchases of liquor (Panel C) is a precise null effect for most specifications; here the exceptions are for the stacked and DiD imputation estimators, which generate an estimated increase of .6 to .7 servings of liquor purchased per month, a 6 to 7 percent increase that is statistically significant at the 5 percent level. Overall, smoking bans do not yield large changes in the composition of household alcohol purchases.

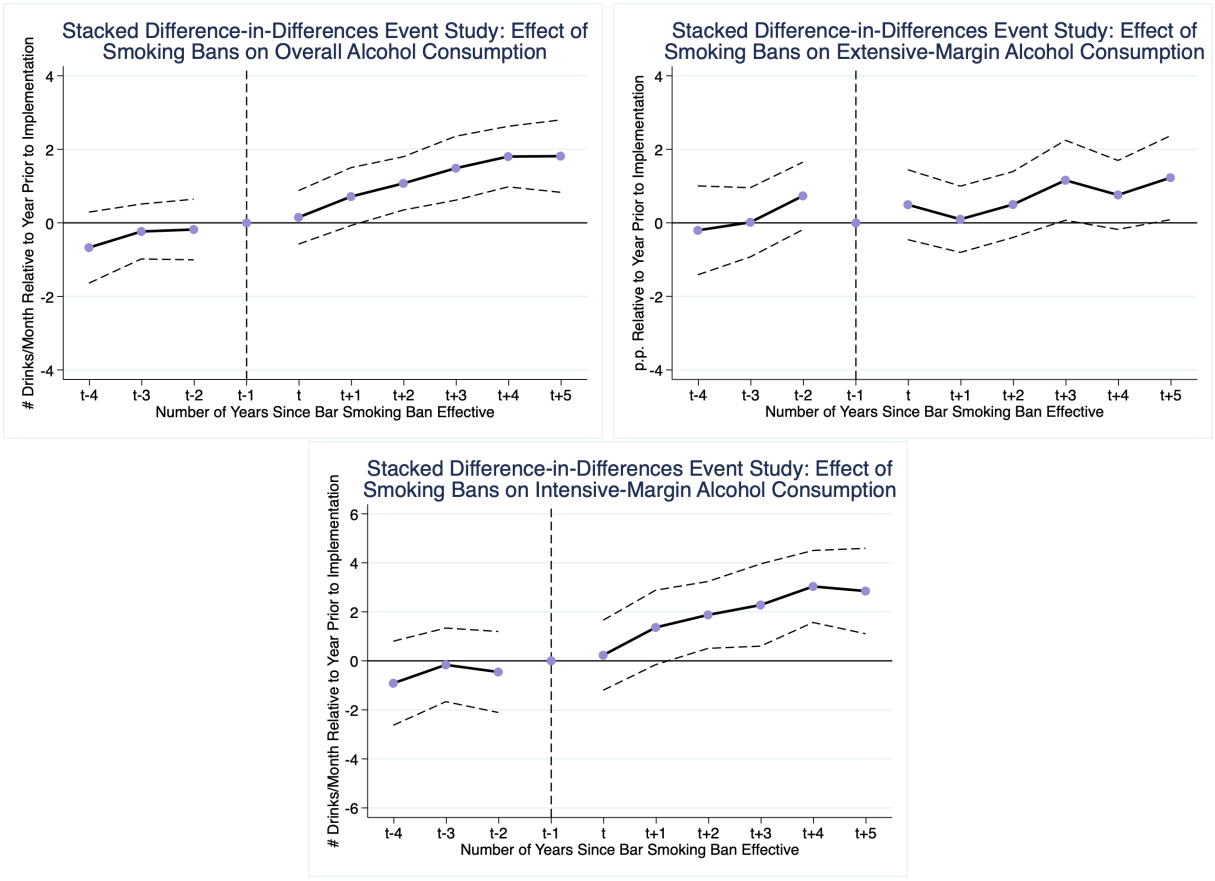
Finally, to more directly compare my results with earlier research on smoking bans and alcohol consumption, I run an alternative specification where I assign treatment based on whether the state has implemented a smoking ban. Individuals residing in jurisdictions without a state-level ban (even if they are covered by a city or county-level ban) are considered part of the untreated group.[30] The results for alcohol consumption and purchases are shown in Appendix Tables D.11 and D.12. The estimated effect sizes are quantitatively very similar to those from the preferred specification in Tables 2 to 5 and the interpretation remains the same: smoking bans in bars lead to increases in alcohol consumption, particularly along

[30]State fixed effects replace county fixed effects and the standard errors are clustered at the state level instead of the county level in this specification.

the intensive margin for current and former smokers. These effects do not show up for off-premises alcohol purchases, suggesting that the increase in consumption is occurring at bars and or restaurants. The similarity of the results between the county and state-level analyses was not a foregone conclusion, as assigning treatment at the state level and excluding local smoking bans generates non-classical measurement error in treatment status, which could in theory affect the sign or magnitude of the estimated effect sizes. One potential explanation for the similarity of the results is that several populous states implemented smoking bans, minimizing the extent of measurement error by assigning treatment at the state instead of county level.

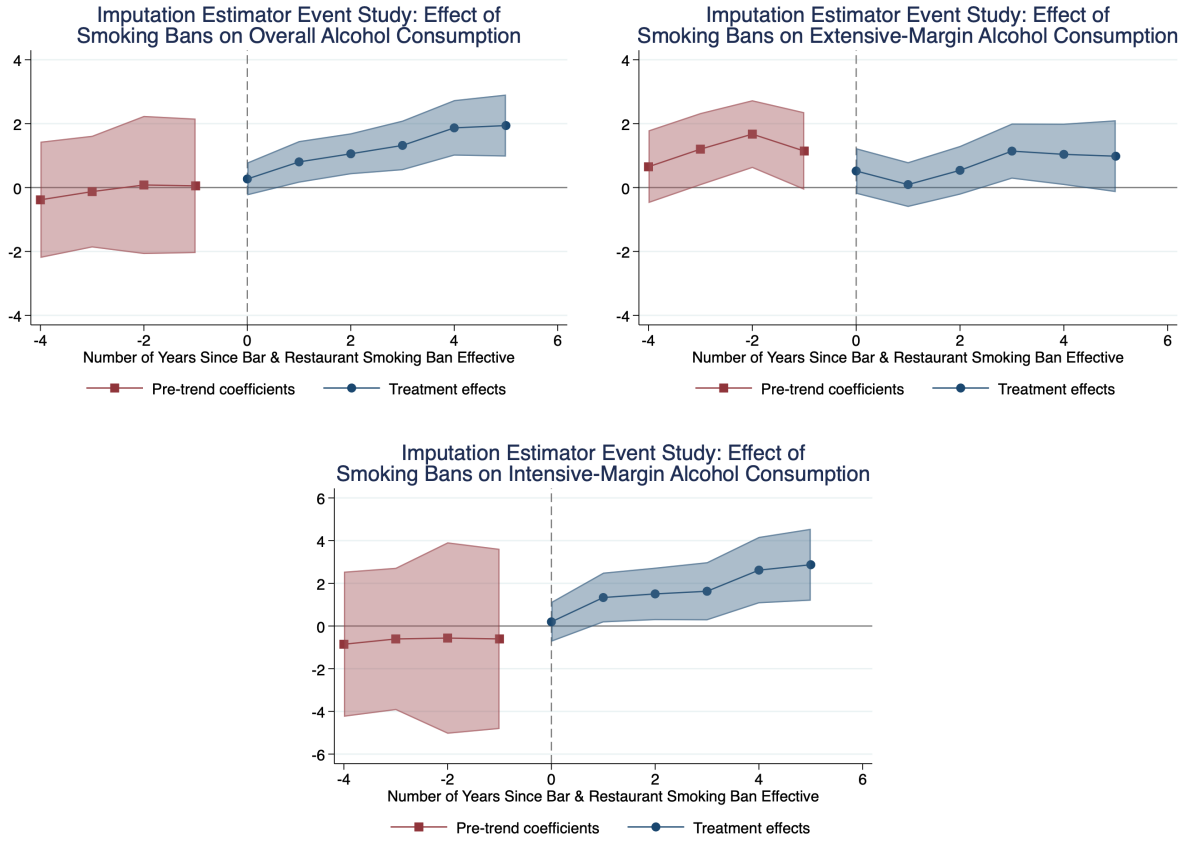
D Additional Figures and Tables

Figure D.1



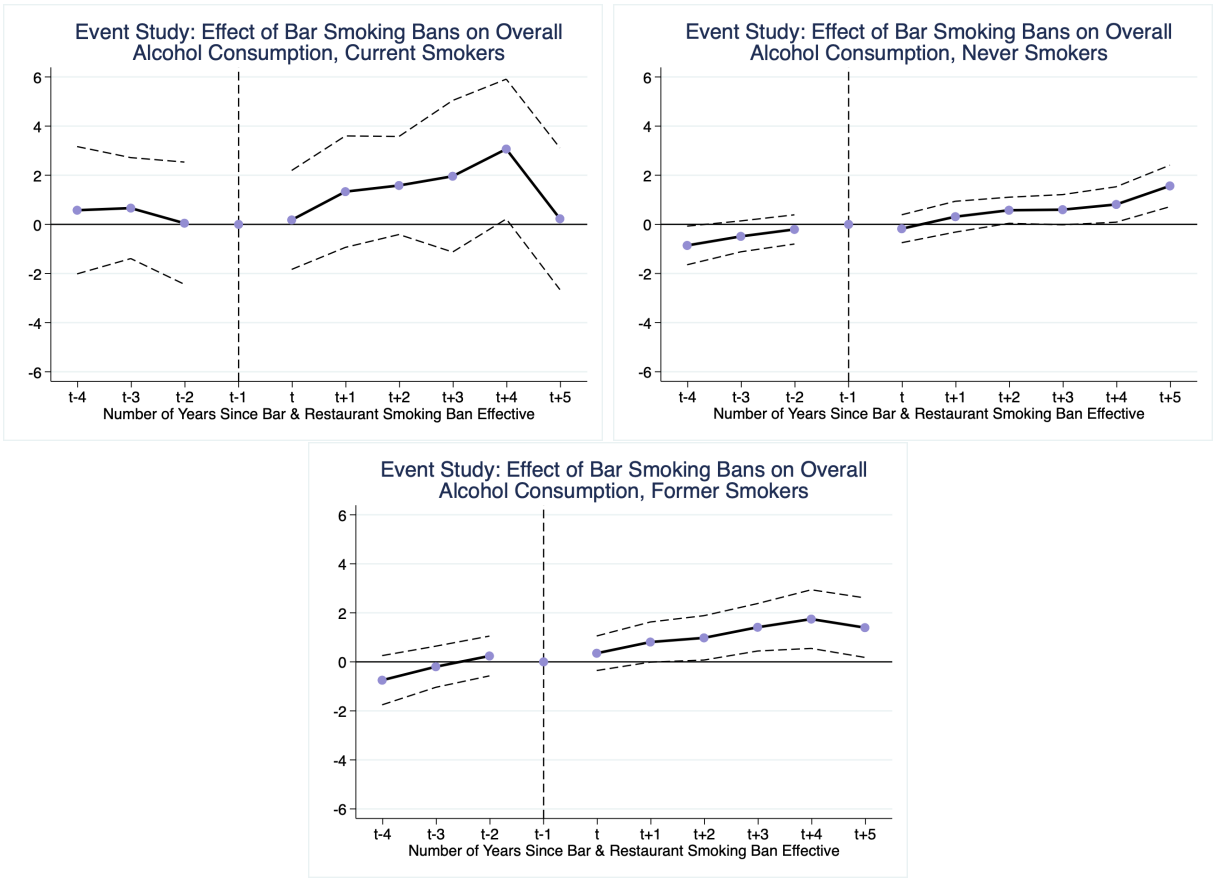
Note: Results from the estimation described in Section 3.5 for the stacked DiD estimator. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status, all interacted with stack fixed effects. Policy controls are (1) whether the 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 .08, and (3) the state cigarette tax per pack. The latter two are interacted with stack fixed effects. Controls also include county-by-stack, year-by-stack, and region-by-year-by-stack 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-left panel outcome: number of servings of alcohol consumed in the past 30 days. Top-right panel outcome: whether any alcohol was consumed in the past 30 days (percentage points). Bottom panel outcome: number of servings of alcohol consumed in the past 30 days for individuals who drink. Data source: BRFSS 2004-2012. F-statistics on joint significance of pre-period coefficients: top-left panel F = .63, top-right panel F = 1.06, bottom panel F = 0.42.

Figure D.2



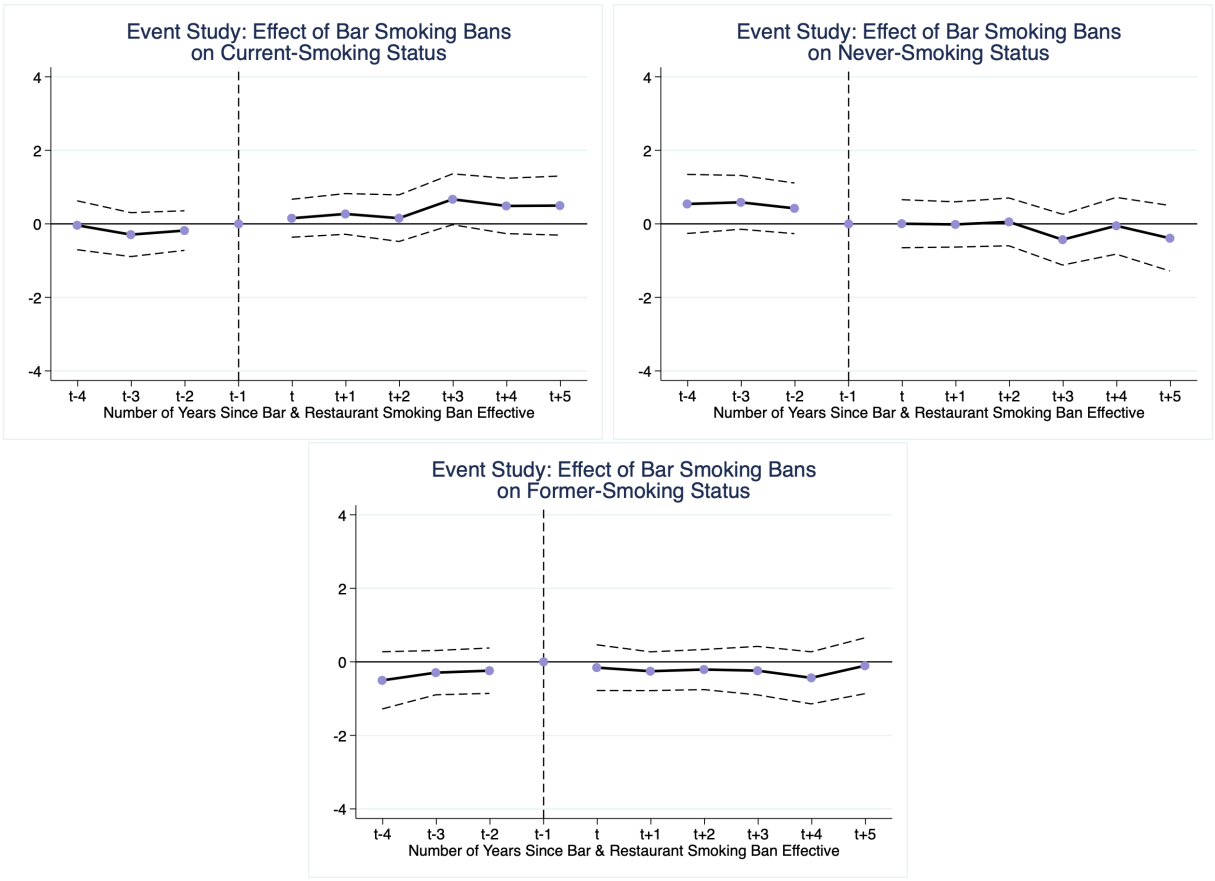
Note: Results from the estimation described in Section 3.5 for the DiD imputation estimator. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county is subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is .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-left panel outcome: number of servings of alcohol consumed in the past 30 days. Top-right panel outcome: whether any alcohol was consumed in the past 30 days (percentage points). Bottom panel outcome: number of servings of alcohol consumed in the past 30 days for individuals who drink. Data source: BRFSS 2004-2012. F-statistics on joint significance of pre-period coefficients: top-left panel F = .32, top-right panel F = 2.87, bottom panel F = .13.

Figure D.3



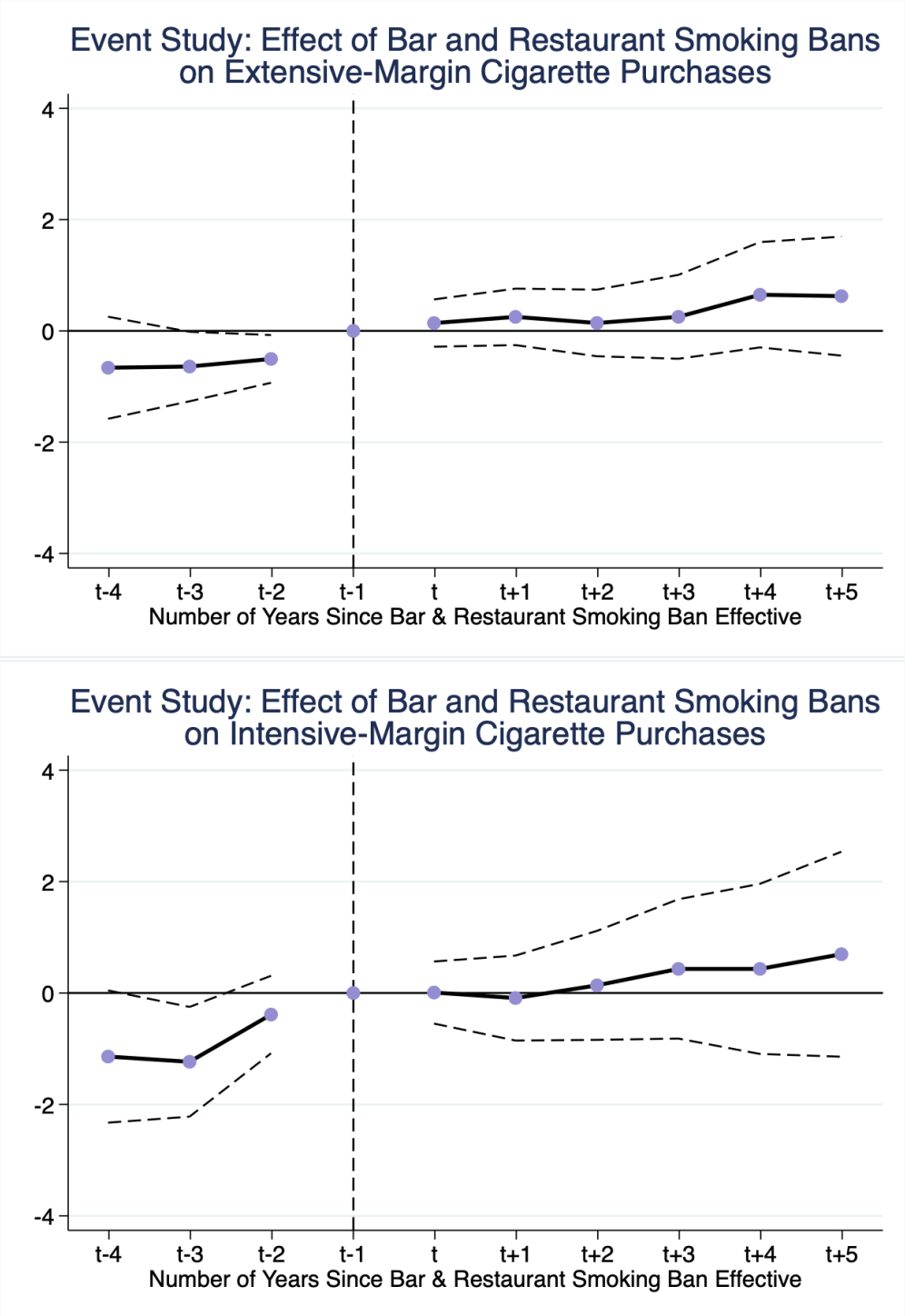
Note: Results from the estimation specified in Equation 2 run on subsamples corresponding to the designated smoking status. Demographic controls are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) whether the county 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 .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-left panel outcome: number of servings of alcohol consumed in the past 30 days for current smokers. Top-right panel outcome: number of servings of alcohol consumed in the past 30 days for never smokers. Bottom panel outcome: number of servings of alcohol consumed in the past 30 days for former smokers. Data source: BRFSS 2004-2012. F-statistics on joint significance of pre-period coefficients: top-left panel F = .15, top-right panel F = 2.02, bottom panel F = 1.08.

Figure D.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 .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-left panel outcome: whether the individual reports being a current smoker (percentage points (p.p.)). Top-right panel outcome; whether the individual reports being a never smoker (p.p.). Bottom panel outcome: whether the individual reports being a former smoker (p.p.). Data source: BRFSS 2004-2012. F-statistics on joint significance of pre-period coefficients: top-left panel F = .46, top-right panel F = 1.18, bottom panel F = .62.

Figure D.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 .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. Smoking households are defined as those that purchased any cigarettes in the calendar year. Data source: Nielsen Consumer Panel 2004-2012. F-statistics on joint significance of pre-period coefficients: top panel F = 1.92, bottom panel F = 2.39.

Table D.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	0.07	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)	(0.29)
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	0.69	0.73	0.75	0.67
	(0.46)	(0.44)	(0.43)	(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	0.31
	(0.46)	(0.45)	(0.46)	(0.46)
Fraction age 35-54	0.39	0.38	0.39	0.39
	(0.49)	(0.49)	(0.49)	(0.49)
Fraction age 55+	0.32	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	0.61	0.61
	(0.49)	(0.50)	(0.49)	(0.49)
BAC 0.08%	1.00	1.00	0.98	1.00
	(0.06)	(0.02)	(0.13)	(0.07)
Cigarette tax per pack ($)	1.91	1.47	1.47	2.07
	(0.97)	(0.66)	(0.75)	(1.02)
Observations	3,161,715	798,292	697,418	2,363,423

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 D.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:	13.69	12.64	14.02	14.14
total servings	(41.75)	(40.05)	(41.31)	(42.46)
Alcohol purchases:	25.65	23.59	25.96	26.53
extensive margin (p.p.)	(43.67)	(42.45)	(43.84)	(44.15)
Alcohol purchases:	53.38	53.61	54.02	53.29
intensive margin	(68.41)	(67.86)	(66.42)	(68.61)
Fraction smoking	0.22	0.25	0.24	0.21
households	(0.42)	(0.43)	(0.43)	(0.41)
Cigarette purchases:	10.46	12.21	11.58	9.71
extensive margin (p.p.)	(30.61)	(32.73)	(32.00)	(29.61)
Cigarette purchases:	15.40	17.09	16.80	14.49
intensive margin	(18.03)	(19.44)	(18.86)	(17.15)
Observations	5,752,884	1,679,628	1,024,356	4,073,256

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 D.3: Effect of Bar Smoking Bans on Alcohol Consumption (BRFSS), by Gender

Panel A: Total Alcohol Consumption		
Gender:	Women	Men
	(1)	(2)
Smoking Ban	0.26**	0.97**
(standard error)	(0.12)	(0.42)
Pre-Ban Mean	6.20	17.00
% Effect	4.21%	5.70%
N	1,896,414	1,169,750
Panel B: Extensive-Margin Alcohol Consumption		
Gender:	Women	Men
	(1)	(2)
Smoking Ban	0.25	0.13
(standard error)	(0.30)	(0.35)
Pre-Ban Mean	46.61	60.63
% Effect	0.54%	0.21%
N	1,907,349	1,181,598
Panel C: Intensive-Margin Alcohol Consumption		
Gender:	Women	Men
	(1)	(2)
Smoking Ban	0.39	1.50**
(standard error)	(0.25)	(0.63)
Pre-Ban Mean	13.39	28.25
% Effect	2.94%	5.29%
N	836,115	684,805
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 run on subsamples corresponding to the designated gender. Demographic controls are fixed effects for 5-year age bins, marital status, 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 D.4: Effect of Restaurant-Only Smoking Bans on Alcohol Outcomes

Panel A: Total Alcohol		
Outcome:	Consumption (BRFSS)	Purchases (Nielsen)
	(1)	(2)
Smoking Ban	-0.60*	0.53
(standard error)	(0.33)	(0.43)
Pre-Ban Mean	11.30	14.24
% Effect	-5.28%	3.73%
N	3,066,169	5,752,884
Panel B: Extensive-Margin Alcohol		
Outcome:	Consumption (BRFSS)	Purchases (Nielsen)
	(1)	(2)
Smoking Ban	-0.06	0.43
(standard error)	(0.49)	(0.42)
Pre-Ban Mean	51.58	26.30
% Effect	-0.11%	1.62%
N	3,088,951	5,752,884
Panel C: Intensive-Margin Alcohol		
Outcome:	Consumption (BRFSS)	Purchases (Nielsen)
	(1)	(2)
Smoking Ban	-1.11*	0.57
(standard error)	(0.59)	(1.39)
Pre-Ban Mean	22.11	54.14
% Effect	-5.00%	1.04%
N	1,520,926	1,531,191
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 for the restaurant-only ban variable. Column (1) presents coefficients corresponding to the regression shown in Table 2, Column (2). Column (2) presents coefficients corresponding to the regression shown in Table 3, Column (2). The restaurant-only ban variable is defined as the fraction of the county population subject to a smoking ban in restaurants only. Demographic controls for Column (1) are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls are (1) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (2) the state cigarette tax per pack. Controls also include county, month, and region-by-month fixed effects. Column (2) includes household fixed effects. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Column (1) data source: BRFSS 2004-2012. Column (2) data source: Nielsen Consumer Panel 2004-2012.

Table D.5: Effect of Bar Smoking Bans on Alcohol Consumption and Purchases by Smoking Status (Poisson)

Panel A: Total Alcohol Consumption, BRFSS				
Smoking Status:	All	Current	Never	Former
	(1)	(2)	(3)	(4)
Smoking Ban	0.61***	1.48**	0.25	0.56*
(standard error)	(0.22)	(0.75)	(0.18)	(0.30)
Pre-Ban Mean	11.44	20.77	7.60	12.54
% Effect	5.32%	7.11%	3.32%	4.50%
N	3,066,166	528,606	1,624,815	900,138
Panel B: Total Alcohol Purchases, Nielsen				
Smoking Status:	All	Smoker	Nonsmoker	
	(1)	(2)	(3)	
Smoking Ban	-0.92***	-2.55**	-0.44	
(standard error)	(0.35)	(1.10)	(0.34)	
Pre-Ban Mean	14.02	20.95	11.77	
% Effect	-6.56%	-12.16%	-3.77%	
N	5,747,052	1,099,944	4,638,265	
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 a variation of the estimation specified in Equation 1, estimated using a Poisson specification and run on subsamples corresponding to the designated smoking status (Columns (2) through (4)). Results presented are marginal effects calculated at the mean. Smoker in Panel B is defined as a household that purchased any cigarettes in the calendar year. Demographic controls for Panel A are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Demographic controls for Panel B are fixed effects for age bins, marital status, race and ethnicity, education, employment, number of adults in the household, presence of children, female unmarried head, and male unmarried head, coded using the highest value for the household where values differ across household members (e.g., educational attainment). 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. Panel A data source: BRFSS 2004-2012. Panel B data source: Nielsen Consumer Panel 2004-2012.

Table D.6: Effect of Bar Smoking Bans on Smoking Status (BRFSS)

Panel A: Current-Smoking Status						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	0.39*	0.25	0.33*	0.22	0.48**	0.45
(standard error)	(0.21)	(0.24)	(0.19)	(0.24)	(0.21)	(0.29)
Pre-Ban Mean	20.37	20.37	20.37	20.35	20.17	20.37
% Effect	1.93%	1.22%	1.60%	1.07%	2.39%	2.22%
N	3,143,293	3,143,293	3,143,293	3,095,140	2,364,490	2,657,459
Panel B: Never-Smoking Status						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.21	-0.30	-0.36*	-0.27	0.08	-0.30
(standard error)	(0.24)	(0.24)	(0.20)	(0.24)	(0.22)	(0.31)
Pre-Ban Mean	55.55	55.55	55.55	55.53	55.61	55.55
% Effect	-0.38%	-0.53%	-0.64%	-0.48%	0.15%	-0.54%
N	3,143,293	3,143,293	3,143,293	3,095,140	2,364,490	2,657,459
Panel C: Former-Smoking Status						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.19	0.05	0.03	0.05	-0.57***	-0.15
(standard error)	(0.20)	(0.18)	(0.16)	(0.19)	(0.19)	(0.29)
Pre-Ban Mean	24.08	24.08	24.08	24.12	24.22	24.08
% Effect	-0.77%	0.20%	0.12%	0.20%	-2.34%	-0.64%
N	3,143,293	3,143,293	3,143,293	3,095,140	2,364,490	2,657,459
Demographics		X	X	X	X	X
County & Time FE	X	X	X	X	X	X
Region-by-Time FE		X	X	X	X	X
Policy Controls		X	X	X	X	X
Fraction Treated	X	X		X	X	X
Drop Bar Ban 1st				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 (included in Column (1) regression), (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 D.7: Effect of Bar Smoking Bans on Cigarette Purchases (Nielsen)

Panel A: Any Cigarette Purchases						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	0.25	0.29	0.19	0.27	0.24	0.40
(standard error)	(0.21)	(0.21)	(0.19)	(0.21)	(0.21)	(0.28)
Pre-Ban Mean	11.58	11.58	11.58	11.59	11.47	11.58
% Effect	2.12%	2.53%	1.60%	2.30%	2.13%	3.44%
N	5,752,884	5,752,884	5,752,884	5,632,932	4,068,084	3,823,212
Panel B: Packs of Cigarettes (Intensive Margin)						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.20	-0.17	-0.26	-0.25	-0.24	0.23
(standard error)	(0.34)	(0.31)	(0.30)	(0.32)	(0.33)	(0.34)
Pre-Ban Mean	7.95	7.95	7.95	7.96	8.04	7.95
% Effect	-2.58%	-2.15%	-3.26%	-3.15%	-0.30%	2.90%
N	1,106,460	1,106,460	1,106,460	1,086,276	828,060	746,064
Household FE	X	X	X	X	X	X
County & Time FE	X	X	X	X	X	X
Region-by-Time FE		X	X	X	X	X
Policy Controls		X	X	X	X	X
Fraction Treated	X	X		X	X	X
Drop Bar Ban 1st				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 (included in Column (1) regression), (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. Analysis sample in Panel B restricted to households that purchased any cigarettes in the calendar year (smoking households). Data source: Nielsen Consumer Panel 2004-2012.

Table D.8: Effect of Bar Smoking Bans on Smoking Status and Cigarette Purchases by Weather

Panel A: Smoking Status, BRFSS			
Smoking Status:	Current	Never	Former
	(1)	(2)	(3)
Smoking Ban: Cold Weather	0.39	-0.13	-0.26
(standard error)	(0.30)	(0.34)	(0.26)
Smoking Ban: Not Cold Weather	0.19	-0.37	0.18
(standard error)	(0.25)	(0.26)	(0.21)
N	3,143,293	3,143,293	3,143,293
Panel B: Cigarette Purchases, Nielsen			
Cigarette Purchases:	Any	Packs	
	(1)	(2)	
Smoking Ban: Cold Weather	0.33	-0.22	
(standard error)	(0.22)	(0.31)	
Smoking Ban: Not Cold Weather	0.28	-0.15	
(standard error)	(0.21)	(0.32)	
N	5,752,884	1,106,460	
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, with the smoking ban variable interacted with an indicator for cold weather. Demographic controls for Panel A are fixed effects for 5-year age bins, marital status, sex, race, educational attainment, and employment status. Policy controls for Panels A and B 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 for Panels A and B also include county, month, and region-by-month fixed effects. Panel B includes household 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. Panel A data source: BRFSS 2004-2012. Panel B data source: Nielsen Consumer Panel 2004-2012.

Table D.9: Effect of Bar 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)	(6)
Smoking Ban	0.13***	0.09*	0.03	0.09*	0.13***	0.04
(standard error)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.08)
Pre-Ban Mean	7.94	7.94	7.94	7.95	7.96	7.94
% Effect	1.60%	1.10%	0.43%	1.18%	1.63%	0.56%
N	1,543,708	1,543,708	1,543,708	1,514,657	1,114,151	1,278,536
Panel B: Average Alcohol Consumption per Drinking Day						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	0.05**	0.06**	0.07***	0.06**	0.09***	0.07**
(standard error)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Pre-Ban Mean	2.49	2.49	2.49	2.49	2.50	2.49
% Effect	2.10%	2.21%	2.75%	2.38%	3.78%	2.68%
N	1,533,484	1,533,484	1,533,484	1,504,613	1,106,919	1,269,929
Panel C: Maximum Alcohol Consumption on One Occasion						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	0.07**	0.05*	0.03	0.06*	0.06*	0.06
(standard error)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Pre-Ban Mean	3.74	3.74	3.7 4	3.74	3.74	3.74
% Effect	1.88%	1.42%	0.68%	1.56%	1.68%	1.57%
N	1,382,153	1,382,153	1,382,153	1,356,266	1,008,679	1,077,069
Demographics		X	X	X	X	X
County & Time FE	X	X	X	X	X	X
Region-by-Time FE		X	X	X	X	X
Policy Controls		X	X	X	X	X
Fraction Treated	X	X		X	X	X
Drop Bar Ban 1st				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 (included in Column (1) regression), (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, except in Column (3) where it is a binary variable equal to 1 if any part of the county is covered. "Drop Bar Ban 1st" denotes a robustness check where I exclude individuals residing in counties that implemented a bar ban prior to a restaurant ban. Panel C uses the 2005-2012 waves of the BRFSS as that question was not asked in the 2004 wave. Standard errors are clustered at the county level. Regressions are probability weighted using the sample weights. Data source: BRFSS 2004-2012.

Table D.10: Effect of Bar Smoking Bans on Type of Alcohol Purchased for Off-Premises Consumption (Nielsen)

Panel A: Total Quantity of Beer Purchased for Off-Premises Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.34	-0.24	-0.19	-0.23	-0.60**	-0.24
(standard error)	(0.22)	(0.23)	(0.20)	(0.23)	(0.27)	(0.40)
Pre-Ban Mean	9.32	9.32	9.32	9.31	9.13	9.32
% Effect	-3.68%	-2.61%	-2.05%	-2.52%	-6.61%	-2.58%
N	3,388,311	3,388,311	3,388,311	3,320,477	2,409,164	2,258,982
Panel B: Total Quantity of Wine Purchased for Off-Premises Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	-0.06	-0.07	-0.09	-0.07	0.08	0.06
(standard error)	(0.13)	(0.13)	(0.15)	(0.14)	(0.15)	(0.17)
Pre-Ban Mean	4.49	4.49	4.49	4.49	4.52	4.49
% Effect	-1.33%	-1.60%	-2.09%	-1.57%	1.81%	1.36%
N	3,388,311	3,388,311	3,388,311	3,320,477	2,409,164	2,258,982
Panel C: Total Quantity of Liquor Purchased for Off-Premises Consumption						
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking Ban	0.11	0.02	-0.03	0.06	0.58**	0.72**
(standard error)	(0.26)	(0.29)	(0.27)	(0.29)	(0.28)	(0.29)
Pre-Ban Mean	10.26	10.26	10.26	10.23	9.98	10.26
% Effect	1.09%	0.16%	-0.30%	0.63%	5.83%	7.05%
N	3,388,311	3,388,311	3,388,311	3,320,477	2,409,164	2,258,982
Household FE	X	X	X	X	X	X
County & Time FE	X	X	X	X	X	X
Region-by-Time FE		X	X	X	X	X
Policy Controls		X	X	X	X	X
Fraction Treated	X	X		X	X	X
Drop Bar Ban 1st				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 (included in Column (1) regression), (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 D.11: Effect of State-Level Bar Smoking Bans on Alcohol Consumption (BRFSS)

Panel A: Total Alcohol Consumption				
Smoking Status:	All	Current	Never	Former
	(1)	(2)	(3)	(4)
Smoking Ban	0.68***	1.77**	0.40*	0.40*
(standard error)	(0.24)	(0.77)	(0.23)	(0.22)
Pre-Ban Mean	11.70	21.16	7.82	12.98
% Effect	5.79%	8.34%	5.09%	3.09%
N	3,066,172	528,618	1,624,843	900,195
Panel B: Extensive-Margin Alcohol Consumption				
Smoking Status:	All	Current	Never	Former
	(1)	(2)	(3)	(4)
Smoking Ban	0.33	-0.55	0.85**	-0.36
(standard error)	(0.35)	(0.67)	(0.35)	(0.40)
Pre-Ban Mean	56.07	63.77	51.43	60.42
% Effect	0.59%	-0.86%	1.66%	-0.59%
N	3,088,954	535,756	1,633,687	906,696
Panel C: Intensive-Margin Alcohol Consumption				
Smoking Status:	All	Current	Never	Former
	(1)	(2)	(3)	(4)
Smoking Ban	1.08**	3.30**	0.47	0.80**
(standard error)	(0.43)	(1.24)	(0.40)	(0.33)
Pre-Ban Mean	20.99	33.46	15.29	21.57
% Effect	5.15%	9.86%	3.11%	3.73%
N	1,520,929	283,777	743,010	488,208
Demographics	X	X	X	X
State & Time FE	X	X	X	X
Region-by-Time FE	X	X	X	X
Policy Controls	X	X	X	X
Fraction Treated				
Stacked DD				
DiD Imputation				

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

Note: Results from a variation of 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) an indicator for whether the state 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 state, month, and region-by-month fixed effects. Treatment is defined as whether the state is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the state level. Regressions are probability weighted using the sample weights. Data source: BRFSS 2004-2012.

Table D.12: Effect of State-Level Bar Smoking Bans on Alcohol Purchases (Nielsen)

Panel A: Total Alcohol Purchases			
Smoking Status:	All	Smoker	Nonsmoker
	(1)	(2)	(3)
Smoking Ban	-0.22	-0.11	-0.17
(standard error)	(0.31)	(1.05)	(0.27)
Pre-Ban Mean	14.58	21.55	12.46
% Effect	-1.49%	-0.53%	-1.36%
N	5,752,884	1,106,460	4,646,388
Panel B: Extensive-Margin Alcohol Purchases			
Smoking Status:	All	Smoker	Nonsmoker
	(1)	(2)	(3)
Smoking Ban	-0.35	-1.10*	-0.06
(standard error)	(0.24)	(0.60)	(0.22)
Pre-Ban Mean	26.46	31.70	24.87
% Effect	-1.31%	-3.46%	-0.23%
N	5,752,884	1,106,460	4,646,388
Panel C: Intensive-Margin Alcohol Purchases			
Smoking Status:	All	Smoker	Nonsmoker
	(1)	(2)	(3)
Smoking Ban	-0.05	2.20	-0.43
(standard error)	(0.90)	(2.69)	(0.83)
Pre-Ban Mean	55.09	67.98	50.09
% Effect	-0.09%	3.23%	-0.85%
N	1,531,998	343,321	1,185,183
Demographics	X	X	X
State & Time FE	X	X	X
Region-by-Time FE	X	X	X
Policy Controls	X	X	X
Fraction Treated			
Stacked DD			
DiD Imputation			

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

Note: Results from a variation of the estimation specified in Equation 1. Policy controls are (1) an indicator for whether the state 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, state, month, and region-by-month fixed effects. Treatment is defined as whether the state is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the state level. Regressions are probability weighted using the sample weights. Data source: Nielsen Consumer Panel, 2004-2012.