SPILLOVER EFFECTS OF RISKY HEALTH BEHAVIORS AND CRIME

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State and local governments implement many policies to address issues or solve problems within their jurisdictional boundaries. Areas within the purview of state and local government policymaking include public-health-related activities like smoking and drinking, as well as policies related to the criminal justice system. These policies often have unintended (or intended) spillover effects, both in terms of externalities and spatial spillovers to nearby jurisdictions. In this dissertation I examine three different policies or innovations that were implemented at the state or local level: smoking bans in bars and restaurants, ban-the-box policies, and the introduction of ridesharing services.

In Chapter 1 I examine the impact of smoking bans in bars and restaurants on the amount and location of alcohol consumption, smoking status, and the incidence of alcohol-related externalities such as drunk driving and violent crime. I exploit differential timing in the adoption of these smoking bans across state and local jurisdictions between 2004 and 2012. I find that smoking bans in bars and restaurants lead to an increase of one drink per month, on average, for individuals who drink alcohol. These increases are concentrated among occasional and former smokers. Smoking bans do not have an effect on whether individuals smoke during my sample period. I also do not find effects of smoking bans on violent crime, but there is a 4% increase in fatal drunk-driving crashes in areas with a high prevalence of smoking. These results imply that smoking bans in bars and restaurants lead to unintended consequences in the form of increased alcohol consumption and drunk driving.

In Chapter 2 my coauthor and I revisit the labor market effects of ban-the-box policies, which are policies that prevent employers from asking job applicants about their previous contact with

the criminal justice system. These policies are intended to reduce the stigma associated with incarceration, so as to help individuals who were recently incarcerated get their "foot in the door" to find formal-sector employment. Some of the existing research on BTB policies, however, finds that these policies lead to broad discrimination against young, non-college-educated minority men (the groups employers may believe to be most likely to have had prior contact with the criminal justice system). Using variation in exposure to these policies both within and across metropolitan statistical areas, we find no evidence of discrimination and evidence of positive employment effects for some subgroups of Hispanic men.

In Chapter 3 I analyze how the introduction of ridesharing services such as Uber and Lyft into the most populous U.S. cities affected fatal motor vehicle crashes, particularly drunk-driving-related crashes. I exploit the differential introduction of Uber and Lyft into these cities during the time period 2006 to 2016. I find small declines in drunk-driver-related fatal motor vehicle incidents and small increases in overall fatal motor vehicle incidents, but I am unable to reject the null hypothesis of no effect of Uber or Lyft on these outcomes. Event studies indicate that both drunk-driver-related and overall fatal motor vehicle incidents decline over the medium-to-long term, which is consistent with both a delayed adoption of this new transportation option by riders and or drivers, and with returns to experience in driver skill for Lyft or Uber drivers.

BIOGRAPHICAL SKETCH

Anne McKay Burton earned a Master of Arts in Economics from Cornell University in Ithaca, New York. She graduated summa cum laude with a Bachelor of Arts in Economics and Government from Colby College in Waterville, Maine, with Honors in Economics and Distinction in Economics and Government. While at Colby, she was inducted into Phi Beta Kappa's Beta Chapter of Maine. She earned a high school diploma from Lakeside Upper School in Seattle, Washington. She attended Lakeside Middle School in Seattle, Washington from fifth through eighth grade, and the Hurray for Me! School in Shoreline, Washington from second through fourth grade. She spent first grade at Syre Elementary School in Shoreline, Washington, and split her kindergarten time between Sunset Elementary School in Shoreline, Washington and Eastview Elementary School in Avon Lake, Ohio. She attended preschool at Jack and Jill Nursery School in Avon Lake, Ohio and daycare at Brownstone Day School in Lakewood, Ohio.

I dedicate this dissertation to the memory of my grandma, Nancy McKay Burton. Nancy McKay Burton graduated cum laude from Whitman College in Walla Walla, Washington in 1953 with a Bachelor of Arts in Social Sciences. At a time when society expected the few women who were lucky enough to be able to attend college to graduate with an MRS degree, she instead went to Whitman to get an education (meeting my grandpa was a pleasant surprise). She was inducted into Phi Beta Kappa's Beta Chapter of Washington and later studied history at Stanford University. While raising her four children, she became the first woman on the King County Planning Commission. She later went on to work as a Facility Planner and Policy Analyst for the Seattle School Board, where she was a passionate and diligent researcher and problem solver. She was a woman before her time, and without her support, inspiration, and encouragement over the years, I would not be where I am today.

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Last but certainly not least, I thank my pack, Willie, Waylon, and Cooper, for unconditionally loving me and always being happy to see me. They are good dogs.

Researcher's own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

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CHAPTER 1

THE IMPACT OF SMOKING BANS IN BARS AND RESTAURANTS ON ALCOHOL CONSUMPTION, SMOKING, AND ALCOHOL-RELATED EXTERNALITIES

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Governments implemented bar and restaurant smoking bans to target smoking-related externalities, but these bans may also affect drinking. This paper studies smoking bans' effects on alcohol consumption, smoking, and alcohol-related externalities. I estimate a difference-in-differences model that exploits spatial and temporal variation in smoking bans. They result in a 1-drink-per-month increase in intensive-margin alcohol consumption, driven by changes in bar and restaurant consumption. I find a 4% increase in fatal drunk-driving crashes in areas with a high smoking prevalence but no effects on violent crime or extensive-margin smoking. Increased alcohol consumption and drunk driving are unintended consequences of smoking bans.

1.1 Introduction

Governments have long intervened to correct market failures, often through the regulation of goods that generate externalities. 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. 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).

Ever since the 1964 Surgeon General's report linked smoking cigarettes to adverse health consequences, federal, state, and local governments have implemented numerous policies to minimize the prevalence of smoking and mitigate the externalities generated by secondhand smoke. Such policies include cigarette taxes and minimum purchasing ages for tobacco. This paper focuses on smoking bans in bars and restaurants, which are regulations ostensibly targeted at smoking behavior that may also affect alcohol consumption. Excessive alcohol consumption constitutes its own public health problem and also creates negative externalities. Annually, over 95,000 people die due to excessive alcohol consumption in the U.S., from both chronic (e.g. cancer, liver disease) and acute (e.g. suicide and motor vehicle crashes) causes (CDC, 2021). These deaths constitute 2.8 million years of potential life lost (CDC, 2021).

This paper is about how regulations can have unintended consequences, especially when these regulations target risky health behaviors, which may interact with each other. More specifically, the main research question this paper asks is, do smoking bans in bars and restaurants affect overall alcohol consumption and are there heterogeneous effects for smokers and nonsmokers? Analyzing whether there are heterogeneous effects for smokers and nonsmokers is complicated by the potential endogeneity of smoking status, as some individuals may change their smoking behavior after these bans are implemented. If smoking bans lead to changes in smoking status, and if such changes are correlated with alcohol consumption, then estimates of the effect of smoking bans on alcohol consumption *by smoking status* would be biased. To address this potential endogeneity issue, this paper also investigates whether these bans affect the prevalence of smoking. Given that I find no effect of smoking bans on extensive-margin smoking, I can more plausibly interpret the effects on alcohol consumption by smoking status as causal. In a secondary analysis, I examine whether smoking bans lead to changes in the incidence of alcohol-related externalities, such as violent crime and motor vehicle crashes.

This paper contributes to a vast literature in health economics on policies that target smoking and drinking, their respective effects on cigarette and alcohol consumption, and their effects on related externalities.[1,2] It also relates to an important but understudied literature on the interaction of risky health behaviors and their externalities (exceptions include Adams and Cotti, 2008; and Anderson, Hansen, and Rees, 2013).

Unlike earlier studies of the effects of state-level smoking bans on alcohol consumption (e.g. Picone, Sloan, and Trogdon, 2004; and Koksal and Wohlgenant, 2016), 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. States have tended to implement smoking bans after some of their cities or counties implemented such bans. My measure allows for incorporating the effects of the spatial heterogeneity in the laws, which is crucial for understanding the potential behavioral responses. Spatial heterogeneity in the laws generates multiple margins along which individuals can behaviorally respond. If smokers want to avoid the ban, they can travel to a nearby city or county without a smoking ban in order to smoke while drinking at the bar.[3]

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

[2] 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).

[3] Similarly, in some jurisdictions, smokers can substitute bars for gambling facilities. In some localities, gambling facilities are not included in bar and restaurant smoking bans. Other jurisdictions are near Native American reservations that have gambling facilities, which are not subject to state and local smoking bans because they are sovereign.

This paper also contributes to the economics of crime literature on the relationship between alcohol consumption and crime and to the public economics literature on the spatial spillover effects of local policies and the optimal regulation of externalities.[4,5] The spatial heterogeneity in the laws and the potential changes in the amount and location of alcohol consumption give rise to competing externalities. On the one hand, smoking bans in bars and restaurants address the secondhand-smoke externalities that smokers impose on bar and restaurant workers and the other patrons. On the other hand, smoking bans may also generate changes in the incidence of alcohol-related externalities. If individuals drink outside the home more as a result of smoking bans, they may engage in more social interactions, some of which generate negative externalities (e.g. bar fights and sexual assaults). Drinking outside the home may also be associated with increases in the prevalence of drunk driving. Drinking at home may lead to increases in domestic violence.

Smoking bans in bars and restaurants represent a change in a non-price determinant of demand for alcohol consumed in bars, which may differentially affect smokers and nonsmokers.[6] If nonsmokers derive disutility from cigarette smoke, then a smoking ban in a bar increases a nonsmoker's utility of drinking in a bar, ceteris paribus. For smokers, revealed preference suggests that they derive utility from being able to smoke while they drink at a bar. In this case, a smoking ban would lower a smoker's utility from drinking in a bar, ceteris paribus. Indeed, many bar owners predicted that a smoking ban would cause smokers to substitute drinking at bars for drinking at home (to the detriment of bar owners' bottom lines).[7] However, the newfound inability to smoke in the bar is not the only change occurring. If individuals derive utility from the presence of other patrons, and if a smoking ban encourages nonsmokers to visit bars more often, then both smokers and nonsmokers may find the bar to be a more enjoyable place because more friends are there.[8]

[4] Anderson et al. (2018), Hansen (2015), Lindo et al. (2018), Markowitz and Grossman (1998), Tomé (2019).

[5] e.g. Beard et al. (1997), Beatty et al. (2009), Cawley et al. (2019), Lovenheim (2008), Lovenheim and Slemrod (2010), Ogawa and Wildasin (2009), and Stehr (2007).

[6] One can also think of them as a transfer of the property rights over the air in bars from smokers to nonsmokers.

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

[8] Conversely, for those who view increasing numbers of patrons in a bar as a congestion externality, a more crowded

As with smoking bans' effect on utility from drinking at a bar, their effect on total alcohol consumption is a priori uncertain. 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. Nonsmokers, for example, 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. Therefore, the effect of smoking bans on total alcohol consumption is also ambiguous.

To estimate the effects of smoking bans in bars and restaurants on smoking and drinking, 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. Under the assumption that total alcohol consumption equals alcohol consumption at home plus alcohol consumption at bars or restaurants, I can estimate the effect on alcohol consumption at bars and restaurants by smoking status.[9] I estimate a difference-in-differences model where my identifying variation is the date of implementation of a smoking ban in bars and restaurants.

I find that, conditional on drinking alcohol in the past 30 days, smoking bans in bars and restaurants lead to an increase of one serving of alcohol per 30 days (a 4% increase). The effects are driven by changes in alcohol consumption for occasional and former smokers. I find small declines in the total quantity of alcohol purchased for home consumption in the past month. These declines occur for both smokers and nonsmokers, although these results are not necessarily statistically significant. Taken together, these results imply that the increase in total alcohol consumption is driven by increases in bar and restaurant alcohol consumption. With respect to smoking bans in bars and restaurants' effect on extensive-margin smoking, I find no effect of smoking bans on

bar may be less enjoyable.

[9]Using BRFSS data, I define frequent smokers as those who smoke every day. Occasional smokers report smoking some days. Never smokers have never regularly smoked cigarettes (which in the BRFSS means they have smoked fewer than 100 cigarettes in their lifetime). Former smokers used to smoke every day or some days but they have quit.

smoking prevalence.

In a secondary analysis, I use the Uniform Crime Reports (UCR) and the Fatality Analysis Reporting System (FARS) for 2004-2012 to estimate the effects of these smoking bans on alcohol-related externalities. I am unable to reject the null hypothesis of no effect of smoking bans on the crime rate for various violent crimes. In this context, a small (4%) increase in alcohol consumption does not appear to translate into increases in violent crime. However, I do find small increases (4%) in fatal drunk-driving crashes in areas with a high prevalence of smoking.

The small increases in alcohol consumption that I find highlight the importance of thinking about the substitutability or complementarity of risky health behaviors when targeting one particular risky health behavior (in this instance, smoking). Changing the environment of bars to make smoking more difficult had the unintended effect of making bars more enjoyable places to drink. These results also highlight the role of spatial heterogeneity in local policies, which may have led to unintended increases in drunk driving. One possible explanation for the increase in drunk driving in areas with a high prevalence of smoking is that after smoking bans were implemented, smokers drove to nearby jurisdictions without smoking bans to smoke at the bar, then they drove home drunk. Lastly, it is worth noting that I do not find increases in violent crimes as a result of smoking bans. These results suggest that while in general, increases in alcohol consumption are associated with increases in violent crime, small increases in alcohol consumption do not correspond to increases in violent crime. The policy implication of this result is that it may be optimal to design policies to minimize heavy drinking but not all drinking, so as to mitigate negative externalities in the least restrictive way.

The next section of the paper (section 3.3) describes the data sources and institutional details of bar and restaurant smoking bans. Section 1.3 details the methods, key assumptions, and potential violations of key assumptions for the effects on smoking and drinking. My main results of the effect of smoking bans in bars and restaurants on alcohol consumption, smoking, and alcohol consumption disaggregated by smoking status are in section 1.4. Section 1.5 outlines the reduced-form models and results for alcohol-related externalities. In section 1.6, I estimate a variety of alternative specifications and robustness checks. Section 1.7 concludes.

1.2 Data

I use several data sources in my analysis, all of which are described in further detail below. The measures of "treatment" (effective dates of smoking bans in bars and restaurants) come from the American Nonsmokers' Rights Foundation. For measures of alcohol consumption and location of alcohol consumption by smoking status, I use the Behavioral Risk Factor Surveillance System (BRFSS) and the Nielsen Consumer Panel. For data on alcohol-related crimes, I use the Uniform Crime Reports (UCR). I use the Fatality Analysis Reporting System (FARS) for measures of drunk-driver-related motor vehicle fatalities.

All of my smoking and drinking-related data contain county-level geographic identifiers. My sample period is 2004-2012 because those are the years I have consistent county identifiers for all of the datasets I use. I use the same years in case there are heterogeneous effects of smoking bans over time; if I used different years for different datasets, I might draw erroneous conclusions, particularly when comparing outcomes from the BRFSS and the Nielsen data. The Nielsen Consumer Panel does not start until 2004, which I why I start my sample period then. I end in 2012 because starting with the 2013 wave, BRFSS stopped publicly reporting county-level identifiers in the aggregated dataset due to privacy concerns. For the crime data, I use the same years for consistency, but my geographic unit is the agency level. I aggregate the motor vehicle crash data to the county-month level.

1.2.1 Measures of Alcohol Consumption and Smoking Status

I use measures of alcohol consumption from the Behavioral Risk Factor Surveillance System (BRFSS) and the Nielsen Consumer Panel dataset.[10] The BRFSS data contain information on self-reported smoking status and frequency and amount of alcohol consumption (measured in servings of alcohol). The BRFSS data do not include information on location of alcohol consumption. The raw BRFSS data are at the individual level. For my sample period, 2004-2012, 80-90% of observations in the BRFSS contain county identifiers (see Figure 1.1).[11] During this period, nearly all states (and Washington, D.C.) participate in the BRFSS each year.[12] The BRFSS is designed to be representative at the state level.

I create several alcohol-related outcomes from the BRFSS data using responses to four different questions. The first question concerns whether individuals drank any alcohol during the past 30 days, which provides an extensive-margin measure of alcohol consumption. The second question asks how many days in the past 30 individuals drank alcohol. The third question asks about the average number of drinks consumed on the days an individual drank alcohol. I use these questions for the number of days and the average amount consumed. 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 is a measure of intensive-margin alcohol consumption. Adding in the non-drinkers' zero drinks to the intensive-margin measure yields the total amount of alcohol consumed in the past month. The fourth question asks about the maximum number of drinks consumed on one occasion. Smoking status comes from two questions. The first asks whether individuals have smoked at least 100 cigarettes during their lifetime. If respondents answer no, they are classified as "never smokers". If respondents answer yes, they are asked whether they

[10]Summary statistics by treatment status for the BRFSS data are in Appendix Tables A.1 and A.2 and summary statistics by treatment status for the Nielsen data are in Appendix Table A.3.

[11]Not all observations have the county-level geographic identifiers because BRFSS suppresses county identifiers if fewer than 50 respondents live in the same county.

[12]Hawaii did not participate in the BRFSS in 2004.

smoke every day, some days, or not at all. I classify "every day" smokers as frequent smokers, "some days" smokers as occasional smokers, and "not at all" smokers as former smokers.

The Nielsen data contain self-scanned cigarette and alcohol purchases at the household level from grocery stores, convenience stores, liquor stores, and other sources of off-premises consumption.[13,14] 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.[15] The scanned-in alcohol purchases provide details on both the quantity purchased and the UPC code (e.g. a 6-pack of Blue Moon wheat beer or 1 bottle of Chateau Ste. Michelle Cabernet Sauvignon wine). In order to make the alcohol purchases comparable, I convert everything into approximate servings of alcohol. I consider 12 ounces of beer (a 6-pack of beer would thus be six servings of alcohol), 5 ounces of wine, and 1.5 ounces of liquor to be one serving of alcohol.[16] The Nielsen data do not included alcohol purchased for on-premises consumption, such as alcohol purchased and consumed at a bar.

The two alcohol-related outcomes from the Nielsen data are the total quantity of alcohol purchased and the prevalence of purchasing alcohol. The former is the total servings of all types of alcohol purchased in a given month, while the latter is a measure of whether a household purchased any alcohol for off-premises consumption in a month. I infer smoking status by whether

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

[14] Several other papers use the Nielsen Consumer Panel data to measure cigarette and or alcohol purchases (e.g. Cotti, Dunn, and Tefft, 2014; Cotti, Nesson, and Tefft, 2018; and Janssen and Parslow, 2021).

[15] Nielsen balances their sample on the following nine demographic characteristics: household size, household income, household head age, female head education level, male head education level, whether there are children in the household, race, Hispanic ethnicity, and household head occupation type.

[16] This conversion is not exact as a serving of alcohol depends also on the alcohol by volume (ABV). Twelve ounces of 5% ABV beer constitutes one serving of alcohol, 5 ounces of 12% ABV wine constitutes one serving, and 1.5 ounces of 40% ABV liquor constitutes one serving (NIAAA). In recent years, breweries have released some higher ABV beers. Further, some liquor has a higher ABV than 40%, such as Absinthe (at least 45% ABV), while others have a lower ABV, such as Irish Cream (15-20% ABV). To the extent that my conversion process generates 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.

the household scanned in any cigarettes in the current calendar year. I use the year instead of the same month to allow for infrequent (or stockpiled) purchases of cigarettes.

The BRFSS and the Nielsen Consumer Panel have mismatched observational units (BRFSS is individual level but Nielsen is household level). In order to make my estimates and data consistent across datasets, I use the respective sample weights and aggregate each dataset to the county-month level.

Table 1.1 shows summary statistics for alcohol consumption for smokers and nonsmokers (BRFSS data). Smokers drink more than nonsmokers. The average adult reports consuming 12 servings of alcohol in the past 30 days; the average smoker consumes 17 drinks over 30 days while the average nonsmoker consumes 9 drinks in 30 days (the difference is statistically significant at the 1% level). Smokers are also slightly more likely to report drinking any alcohol in the past 30 days (the extensive margin). While 48% of adults report drinking any alcohol in the past 30 days, the prevalence of drinking is 54% for smokers (and 45% for nonsmokers; the difference between smokers and nonsmokers is statistically significant at the 1% level). Among individuals who drink (the intensive margin), the gap in the total number of drinks consumed in the past 30 days is even larger for smokers and nonsmokers. On average, conditional on drinking in the past 30 days, individuals report drinking 24 drinks in the past 30 days (slightly less than 1 drink per day), while smokers report drinking 33 drinks (slightly more than 1 drink per day) and nonsmokers report drinking 19 drinks (approximately 2 drinks every 3 days; statistically significantly different than smokers' consumption at the 1% level).

1.2.2 Measures of Alcohol-Related Externalities

The Uniform Crime Reports (UCR) contain measures of various crimes reported to police.[17] I focus on violent crimes because of the well-documented relationship between alcohol consumption and violence (Anderson, Crost, and Rees, 2018; Markowitz and Grossman, 1998; Tomé, 2019). Specifically, I analyze effects of smoking bans on crime rates for violent crime, murder, rape, aggravated assault, and simple assault. Following the literature, I aggregate the data to the agency-year level.[18] As noted in the literature, the Uniform Crime Reports contain numerous record errors (Evans and Owens, 2007; Chalfin and McCrary, 2018; Maltz and Weiss, 2006; Mello, 2019). In order to use the data, record errors and outliers must be identified in the data (typically done using a regression-based approach) and replaced with imputed values. I use Mello's cleaned version of the Uniform Crime Reports data.[19] He describes the data-cleaning procedure in detail in his paper (Mello, 2019).

For measures of drunk driving, I use various measures of fatal motor vehicle incidents from the Fatality Analysis Reporting System (FARS).[20] The Fatality Analysis Reporting System is a database maintained by the National Highway Traffic and Safety Administration (NHTSA) that records characteristics of every fatal motor vehicle incident on public roadways in the United States. These characteristics include the number of fatalities, the time of day and day of week of the crash, whether the driver(s)'s blood alcohol concentration was tested and the results, and numerous other factors.

[17]Summary statistics by treatment status for the UCR data are in Appendix Table A.4.

[18]According to Mello (2019), instead of reporting monthly crime counts, many agencies report the full year's statistics all at once, making the monthly counts inaccurate. Therefore, I use annual crime estimates.

[19]I am very grateful to Steven Mello for sharing his data-cleaning code and cleaned Uniform Crime Reports data with me.

[20]Summary statistics by smoking prevalence for the FARS data are in Appendix Table A.5.

1.2.3 Measures of Smoking Bans ("Treatment")

The American Nonsmokers' Rights Foundation compiles the effective dates of indoor smoking bans in restaurants and bars for cities, counties, and states in the U.S. The map in Figure 1.2 reflects the general timing of smoking bans in bars that were implemented prior to December 31, 2012 (the end of my sample period). The light blue areas reflect places that implemented smoking bans in bars prior to the start of my sample period (2004). The areas shaded blue-gray implemented bans in the first half of the sample period (2004-2007), while the areas in navy blue implemented them in the second half of the sample period (2008-2012). The jurisdictions shaded white reflect the never-treated group, as they did not implement smoking bans in bars and restaurants prior to the end of my sample period (December 2012). Earlier adopters tended to be states in the West and the Northeast, while later adopters tended to be in the upper Midwest. The South had a mix of early and late adopters.

The importance of incorporating city-level smoking bans is reflected in the map in Figure 1.3. This map excludes city-level bans. In the South, many cities implemented smoking bans in bars in the absence of legislation at the county or state level. An analysis that does not include city-level bans (or that only analyzes state-level bans) will consider most of the South as being part of the control group, when in reality, much of the population in the South is subject to a smoking ban in bars. As indicated in both maps, there is quite a bit of spatial and temporal variation in the implementation of the laws.

Smoking bans differ along several other dimensions as well. Some bans were passed by the state legislature or the local equivalent while others were passed by voters. Some went into effect very soon after being approved while others were phased in several months later. The enforcement authorities also vary: some laws are enforced by the Attorney General, others by local health directors, and others by fire marshals. There is also a range of penalties for violating the law. Penalties can be imposed on the business or the smoker. Typically, the business has an obligation to ensure that its employees do not smoke inside the bar or restaurant, and also to notify customers of the law by posting signs. If a customer starts smoking inside, the business generally has an obligation to alert the customer that smoking is not permitted by law. Once the business has met that obligation, if the customer continues to smoke, then the customer may incur the penalty. Typically the penalty is a fine of around \$50 to \$100, but for repeat offenders in some jurisdictions the fine may be as steep as \$1,500 (Maine State Legislature, 2004, 2018; North Carolina General Assembly, 2010; North Dakota Legislative Branch, 2012; Van Ells, 2012).

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, then the treatment variable takes a value of 1. If some but not all of the cities in a county have implemented a smoking ban, then 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 smoking ban variables in this way because with the exception of a handful of small cities, every jurisdiction that implemented a smoking ban prior to the end of my sample period (December 2012) had either previously implemented a smoking ban in restaurants or implemented such a ban simultaneously. In other words, there are essentially no places that have smoking bans in bars but not restaurants. The policy-relevant regulation, because I am focusing on the behavioral responses to banning smoking in bars, is therefore smoking bans in bars and restaurants. For this analysis, I do not control for other provisions of smoking bans, such as enforcement mechanisms or possible exemptions.

1.2.4 Control Variables

Annual county-level demographic characteristics come from the U.S. Census Bureau. Specifically, I control for annual county-level population percentages by sex (male, female), race and ethnicity (non-Hispanic black; non-Hispanic Asian; non-Hispanic white; Hispanic; and other races, which aggregates individuals identifying as belonging to non-Hispanic American Indian and Alaska Native, Native Hawaiian and other Pacific Islander, or two or more racial groups), and age (less than 15, 15-24, 25-34, 35-44, 45-64, and 65 and above).

I also include measures of state-level policies regarding alcohol and tobacco. 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). I use state-level cigarette taxes from the Tax Burden on Tobacco (TBOT).

In some specifications, I test for heterogeneous effects of smoking bans based on the underlying smoking prevalence. To avoid the potential endogeneity of smoking, I use a pre-period measure of smoking prevalence. The prevalence estimates in the 1992 Tobacco Use Supplement to the Current Population Survey (TUS) are well suited for this exercise as 1992 is before all but a handful of jurisdictions implement smoking bans (1992 is the earliest wave of the TUS). Unfortunately, the TUS does not contain reliable county-level geographic identifiers, so I use state-level measures of smoking prevalence. I split states into thirds, denoted by high, medium, and low smoking prevalence. States with low smoking have a smoking prevalence of 15-20%. Smoking prevalence in medium-smoking states is 20-25%, and smoking prevalence in high-smoking states is 25-30%.

1.2.5 Potential Sources of Measurement Error in the BRFSS and Nielsen

All data sources have their limitations. For 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, these limitations include social desirability bias. Recall bias is another potential issue. Social desirability bias could manifest as individuals underreporting their consumption of cigarettes or alcohol (both on the extensive and intensive margins), because there is stigma in some social circles associated with the consumption of these goods. A constant level of underreporting would not be an issue for my identification strategy; what would be problematic is if the level of underreporting is correlated with the implementation of smoking bans in bars and restaurants. With respect to self-reported smoking behavior, my prior is that if individuals are going to change how they self report their smoking status (or quantity of cigarettes smoked), they would be more likely to underreport (or underreport to a greater extent) after the implementation of a smoking ban (as the smoking ban reflects an increase in the stigma surrounding smoking). Given my results in section 1.4.3, I doubt think that is occurring in this context.

With respect to self-reported alcohol consumption and social desirability bias, so long as individuals are consistently misreporting their alcohol consumption it is not a problem. It is possible that local jurisdictions are implementing other policies concurrently with bar and restaurant smoking bans, and these other policies (especially if they directly target alcohol consumption) could affect the level of misreporting. To the extent that my specifications fail to control for these other policies, my estimates could suffer from omitted variable bias. Alternatively, if the increased stigmatization of one risky health behavior (smoking cigarettes) makes the stigma of other risky health behaviors (drinking alcohol) more salient, then smoking bans in bars and restaurants could be associated with increased underreporting of alcohol consumption even in the absence of simultaneously implemented alcohol policies.

In terms of potential underreporting in the Nielsen Consumer Panel data, Cotti, Dunn, and Tefft (2015) compare alcohol expenditures for off-premises consumption in the Nielsen to reported expenditures in the Consumer Expenditure Survey (CEX) diary data. They find that reported expenditures on beer and wine in the Nielsen are approximately half of reported expenditures in the CEX. However, this difference is almost exclusively due to more households in the Nielsen underreporting on the extensive margin; that is, they do not report any alcohol purchases. When restricting the sample to the intensive margin (households that ever report alcohol expenditures), the weekly expenditures are very similar. These results suggest that smoking bans would have to change whether households ever report any alcohol purchases in order for there to be an issue with social desirability bias specific to the Nielsen data. In terms of underreporting of cigarette purchases, DeCicca, Kenkel, and Lovenheim (forthcoming) suggest that the extent of measurement error in the Nielsen data is probably not changing with tobacco control policies such as smoking bans.

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

1.2.6 Actual Location of Alcohol Consumption is Unobserved

I am unable to directly observe location of alcohol consumption. I am able to observe self-reported (via the household's barcode scanner) purchases of alcohol for off-premises consumption, which I am considering a proxy for home consumption. Off-premises alcohol consumption does not have to occur at one's own home. Individuals could also consume the alcohol they purchased in a grocery or liquor store at, say, somebody else's home (e.g. a house party or a dinner party). The alcohol purchased for off-premises consumption is likely not being consumed at a bar but it could be consumed at some restaurants in some jurisdictions (e.g. ones which permit patrons to bring their own wine). So long as any changes in the prevalence of bringing one's own alcohol (BYOB) to restaurants are not correlated with the implementation of smoking bans in bars and restaurants, my estimates for the alcohol-consumption measures will be unaffected.

Another, perhaps more likely, behavioral response to bar and restaurant smoking bans is that individuals may be more or less likely to substitute socializing at a house party for socializing at a bar. For example, perhaps prior to smoking bans, nonsmokers did not want to socialize in a smoke-filled bar so they socialized at house parties instead. After the smoking ban, they now socialize at the bar more. This substitution does not affect my estimates of the effect of bar and restaurant smoking bans on location of alcohol consumption by smoking status, but it does affect the interpretation of my estimates of the prevalence of alcohol-related externalities. If individuals were driving home drunk from their friend's house before, and now they're driving home drunk from the bar, they're still driving drunk in both cases. Consequently, my estimates will capture the net effect of smoking bans in bars and restaurants on alcohol-related externalities. I will not be able to separately identify changes in externalities due to changes in drinking at one's own home, changes in drinking at other off-premises locations (e.g. house parties), and changes in drinking at bars and restaurants.

1.3 Reduced-Form Empirical Models of Smoking and Drinking

To identify the causal effect of smoking bans in bars and restaurants on the amount and location (at home or in a bar or restaurant) of alcohol consumption, smoking, and alcohol consumption by smoking status, I estimate a difference-in-differences model. I exploit the variation in the timing of the effective dates of these smoking bans at the county level, incorporating bans implemented at the city, county, and state level.

1.3.1 Difference-in-Differences Identification and Assumptions

There are two assumptions needed for a differences-in-differences estimate to capture a causal effect.

1. *Parallel trends:* in the absence of the smoking bans in bars and restaurants, trends in alcohol consumption, conditional on the control variables, would be the same across treated and untreated counties
2. That at the time of the implementation of smoking bans in bars or restaurants, there are no other changes occurring in the treated jurisdictions that affect alcohol consumption, conditional on the control variables

Sections 1.2.6 and 1.3.3 outline potential instances in which these assumptions may not be satisfied. To partially assess the validity of the parallel trends assumption, I conduct event studies, which are shown in section 3.4.1.

1.3.2 Reduced-Form Regression Equation: Alcohol Consumption

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

$$Y_{c,m} = \alpha + \beta \cdot ban_{c,m} + \mathbf{X}_{c,m} \cdot \gamma + \delta_c + \rho_m + \varepsilon_{c,m} \tag{1.1}$$

$Y_{c,m}$ denotes the alcohol-related outcome for individuals in county c at time (month-year) m. In terms of measures of overall alcohol consumption (BRFSS data), my primary measures are the probability of self reporting any alcohol consumption in the past 30 days (extensive margin) and the total amount of self-reported alcohol consumed in the past 30 days by self-reported drinkers (intensive margin). In sections 1.6.1 and 1.6.2, I disaggregate the measure of total alcohol consumption in the past 30 days into the number of self-reported days drinking in the past 30 and the average amount of alcohol consumed on days when an individual drank. With respect to alcohol purchased for off-premises consumption (Nielsen Consumer Panel data), my primary measures are whether the household scanned in any alcohol purchases in the past month (extensive margin) and the total quantity of alcohol purchased for off-premises consumption in the past month. The extensive-margin measure is a proxy for whether alcohol was consumed at home and the total quantity measure is a proxy for the amount of alcohol consumed at home.

In my main specification, $ban_{c,m}$ represents the fraction of individuals subject to a smoking ban in both bars and restaurants at time m in county c. I also control for a smoking ban being effective in county c at time m for restaurants only, which is included in the vector $\mathbf{X}_{c,m}$ along with other characteristics that vary at the county level over time (described in more detail below). The omitted category is "no smoking ban effective in bars or restaurants".

$\mathbf{X}_{c,m}$ represents a vector of characteristics that vary at the county level over time. Specifically, I include the percentages of the population in county c at time m that are male, non-Hispanic black, non-Hispanic Asian, Hispanic, other (non-Hispanic and non-white) races, under the age of 15, 15-24, 35-44, 45-64, and 65 or older; the state-level legal limit for blood alcohol concentration for operating a motor vehicle; and the state-level cigarette tax.[21] The omitted categories for the demographic variables are the percentages of the population that are female, non-Hispanic white, and individuals between the ages of 25 and 34. I include the state-level policy variables because

[21] 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.

anti-smoking measures, such as cigarette taxes and smoking bans, are frequently implemented in conjunction with each other. By controlling for these other policy variables I can 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) and time (month-year pair, ρ_m) fixed effects. I cluster the standard errors, $\varepsilon_{c,m}$, at the county level. I use the county population as probability weights, which makes my results interpretable as the effects of smoking bans on alcohol consumption for the average person as opposed to the average county.

In section 1.4.4, I estimate the effects of smoking bans on alcohol consumption separately by smoking status:

$$Y_{s,c,m} = \alpha_s + \beta_s \cdot ban_{c,m} + \mathbf{X}_{c,m} \cdot \gamma_s + \delta_{s,c} + \rho_{s,m} + \varepsilon_{s,c,m} \tag{1.2}$$

In this specfication, $Y_{s,c,m}$ denotes the alcohol-related outcome for individuals self reporting smoking status s in county c at time (month-year) m. The coefficient of interest, β_s, represents the effect of bar and restaurant smoking bans on alcohol-related outcomes for individuals with smoking status s. Running the regressions separately by smoking status also allows the coefficients for the control variables to differ by smoking status.

Smoking status s varies between the Behavioral Risk Factor Surveillance System (BRFSS) data and the Nielsen Consumer Panel data. With the BRFSS data I am able to distinguish between (self reported) individuals who smoke every day (frequent smokers), those who smoke some days (occasional smokers), those who have never smoked consistently (never smokers), and individuals who used to smoke but no longer do so (former smokers). With the Nielsen data, I infer smoking status (smoker or nonsmoker) from whether the household scanned in cigarettes at all during the same calendar year.

1.3.3 Potential Endogeneity of Smoking Type

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

For example, suppose the null hypothesis that smoking bans have no effect on a smoker's alcohol consumption were true. Also suppose that the smoking bans in bars and restaurants induced the smokers who were the heaviest drinkers to quit smoking, thereby switching from the "frequent smoker" type to the "former smoker" type. Average alcohol consumption among frequent smokers would mechanically decrease and I might erroneously conclude that the smoking bans induced smokers to quit drinking when in reality, the smoking bans induced the drinkers to quit smoking.

To address this potential endogeneity issue, I directly test the effects of smoking bans in bars and restaurants on smoking status. Specifically, I estimate the following linear probability model regression using the Behavioral Risk Factor Surveillance System (BRFSS) data:

$$Y_{s,c,m} = \alpha_s + \beta_s \cdot ban_{c,m} + \mathbf{X}_{c,m} \cdot \gamma_s + \delta_{s,c} + \rho_{s,m} + \varepsilon_{s,c,m} \tag{1.3}$$

I describe the results in more detail in section 1.4.3, but in general they indicate that during this time period (2004-2012), bar and restaurant smoking bans do not have an effect on the prevalence of smoking.

1.4 Main Results: Alcohol Consumption and Smoking

1.4.1 Effect of Smoking Bans on Overall Alcohol Consumption (BRFSS)

I first show the effects of bar and restaurant smoking bans on various measures of alcohol consumption, regardless of smoking status. Table 1.2 show these results using the BRFSS data. Column 1 of Table 1.2 shows the effect of smoking bans in bars and restaurants on the average amount of alcohol consumption (measured by the self-reported total number of servings of alcohol consumed in the past 30 days): individuals consume an additional 0.52 drinks over 30 days. This effect is statistically significant at the 1% level and represents a 4.48% increase in alcohol consumption on average, which implies that smoking bans in bars and restaurants lead to small to moderate increases in alcohol consumption.

Column 2 of Table 1.2 shows what happens to the prevalence of drinking any alcohol in the past 30 days (the extensive margin, which is measured by the percentage of individuals reporting any alcohol consumption in the past 30 days). Smoking bans in bars and restaurants are associated with a 0.20 percentage point reduction (-0.37%) in the percentage of individuals who report drinking alcohol in the past 30 days, which is not statistically significantly different than 0. I interpret this result as a precisely estimated null effect of smoking bans in bars and restaurants on extensive-margin alcohol consumption in the past 30 days.

The effect of smoking bans in bars and restaurants on the intensive margin of alcohol consumption (amount of drinks consumed in the past 30 days conditional on drinking any alcohol in the past 30 days) is shown in the third column of Table 1.2. The implementation of smoking bans in bars and restaurants results in an average increase of 0.91 servings of alcohol consumed over the past 30 days among those who drink. This effect is statistically significant at the 1% level and it represents a 4.17% increase in alcohol consumption among drinkers, indicating that smoking bans lead to small-to-moderate intensive-margin increases in alcohol consumption.

The amount of alcohol consumed over 30 days is a function of the number of days an individual drank alcohol and the average amount of alcohol the individual consumed on each day the individual drank. Studying the effects on these outcomes can illuminate how individuals are responding to smoking bans: are they drinking more often, consuming more alcohol when they drink, or both? Columns 4 and 5 of Table 1.2 disaggregate the effects on intensive-margin alcohol consumption into these two components. For individuals who drank alcohol in the past 30 days, smoking bans in bars and restaurants are associated with an increase in the number of days spent drinking (out of the past 30 days) of 0.06 days (0.73%), on average (Column 4). I cannot reject the null hypothesis of no effect of smoking bans in bars and restaurants on the number of days spent drinking. Individuals do not appear to be drinking more often, on average, after smoking bans are implemented.

Conditional on drinking that day, smoking bans in bars and restaurants result in a 0.06-serving increase in the average amount of alcohol individuals consume (Column 5). This effect is statistically significant at the 1% level, and it represents a 2.31% increase in the average amount of alcohol consumed per day.

The last column of Table 1.2 (Column 6) shows the effect of smoking bans in bars and restaurants on the maximum amount of alcohol consumed on one occasion (conditional on drinking alcohol in the past 30 days). Analyzing the effect on the maximum amount of alcohol consumed can indicate whether there are potentially unhealthy changes in drinking, such as binge drinking. The implementation of smoking bans in bars and restaurants leads to an increase in the maximum amount of alcohol consumed of 0.08 servings, on average. This effect is statistically significant at the 1% level, and it represents a 2.25% increase in maximum alcohol consumption.

Overall, these results are consistent with smoking bans in bars and restaurants leading to small-to-medium increases in alcohol consumption among individuals who drink. Given that the magnitudes of the effect sizes for the average amount consumed per day and the maximum amount consumed on one occasion are similar, it appears that, on average, individuals are drinking more on each day versus drinking the same amount most days and a lot more on 1 day.

1.4.2 Effect of Smoking Bans on Alcohol Purchases for Off-Premises Consumption (Nielsen)

Next I analyze the effect of smoking bans on off-premises alcohol purchases (a proxy for alcohol consumed at home) using the Nielsen data, which can provide some insight into how smoking bans affect the location of alcohol consumption (Table 1.3). The first column shows the effect of smoking bans in bars and restaurants on the total quantity of alcohol purchases, measured by the number of servings of alcohol. The implementation of smoking bans in bars and restaurants is associated with an average decrease in the amount of servings of alcohol purchased for off-premises consumption of 0.35 drinks per month, which is statistically significantly different than 0 at the 5% level. This effect size corresponds to a 6.61% decrease in the quantity of servings of alcohol purchased. Smoking bans in bars and restaurants are associated with small-to-moderate declines in the quantity of alcohol purchased for off-premises consumption.

The second column of Table 1.3 shows the effects on extensive-margin off-premises alcohol purchases, or whether households purchase any alcohol for off-premises consumption. I find that smoking bans in bars and restaurants lead to a 0.30-percentage-point reduction in the prevalence of past-month off-premises alcohol purchases (-1.15%), which is not statistically significant at conventional levels. There is a precisely estimated null effect of smoking bans on the prevalence of past-month alcohol purchases for off-premises consumption.

Comparing these results with those from the BRFSS, I find small-to-medium increases in alcohol consumption in the BRFSS but no corresponding increases in alcohol purchased for off-premises consumption in the Nielsen. Therefore, the aggregate increases in alcohol consumption are most likely due to increases in bar and restaurant alcohol consumption as opposed to at-home consumption.

1.4.3 Effect of Smoking Bans on Extensive-Margin Cigarette Smoking (BRFSS)

Before disaggregating the effect of smoking bans on alcohol consumption by smoking status, I will first present results for the effect of smoking bans on smoking. These results can indicate whether the potential endogeneity of smoking status is likely to be a concern. Table 1.4 shows the results for the effect of smoking bans in bars and restaurants on extensive-margin smoking (whether individuals smoke cigarettes) for the prior 30 days using the BRFSS data. After the implementation of a bar and restaurant smoking ban, there are no meaningful changes in the prevalence of each smoking status.

The prevalence of self-reported frequent (every day) smoking increases by 0.13 percentage points (0.98%), which is not statistically significant. The prevalence of self-reported occasional (some day) smoking increases by 0.22 percentage points, which is marginally statistically significant (10% level). As the proportion of individuals who identify as occasional smokers is quite small (5.25% of the sample), this effect size represents an increase in occasional smoking of 4.19%.

The prevalence of self-reported never smoking declines by 0.09 percentage points (-0.15%) while the prevalence of individuals self identifying as former smokers declines by 0.26 percentage points (-1.04%), neither of which are statistically significant.

Overall, there is essentially no effect of smoking bans on extensive-margin smoking during this time period (2004-2012). There may be slight increases in occasional smoking, but I can rule out moderate increases in the prevalence of occasional smoking. I can also rule out economically meaningful changes in the prevalence of frequent, never, and former smoking.

1.4.4 Disaggregating the Effects on Alcohol Consumption by Smoking Status

In section 1.4.1 I showed that, on average, individuals drink more after smoking bans in bars and restaurants are implemented. In the following sections, I analyze the effect of smoking bans on alcohol consumption by smoking status. 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.

As mentioned earlier, a smoking ban in a bar likely has differential effects on the non-price determinants of demand (e.g. the atmosphere of the bar) for smokers and nonsmokers, which means that they may respond in different ways to this policy. Therefore, 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.

1.4.5 Effect of Smoking Bans on Extensive-Margin Alcohol Consumption by Smoking Status (BRFSS)

Table 1.5 shows the results for the effect of smoking bans in bars and restaurants on extensive-margin alcohol consumption for the prior 30 days using the BRFSS data (interpretable as the percentage of individuals in the county who drank any alcohol). As with the unconditional (with respect to smoking status) results for extensive-margin alcohol consumption, I am able to rule out meaningful changes in the prevalence of past-30-day alcohol consumption. After the implementation of a smoking ban in a bar and a restaurant, the fraction of frequent smokers who self report drinking in the past 30 days declines by 0.24 percentage points (-0.41%). Smoking bans in bars and restaurants are associated with a 0.35 percentage point decrease in the prevalence of drinking in the past 30 days for occasional smokers (-0.53%). For never smokers, the prevalence of drinking increases by 0.15 percentage points (0.30%). For former smokers, the prevalence of drinking declines by 0.67 percentage points (-1.16%). None of these estimates are statistically significantly different than 0.

Overall, these results are precisely estimated null effects. Smoking bans in bars and restaurants do not lead to changes in the prevalence of alcohol consumption for any smoking status.

1.4.6 Effect of Smoking Bans on Intensive-Margin Alcohol Consumption by Smoking Status (BRFSS)

Table 1.6 shows the results for the effect of smoking bans in bars and restaurants on intensive-margin alcohol consumption for the prior 30 days using the BRFSS data. All of the results in this section are conditional on drinking any alcohol in the past 30 days. One might expect never smokers to increase their alcohol consumption at bars and restaurants the most, as a smoking ban might represent the greatest improvement in bar atmosphere from the perspective of nonsmokers relative to other smoking status groups. However, I actually find that never smokers increase their alcohol consumption the least.

After smoking bans in bars and restaurants are implemented, frequent smokers drink an additional 1.15 drinks per month (a 3.19% increase). This effect is not statistically significant, which means I cannot reject the null hypothesis of no effect of bar and restaurant smoking bans on intensive-margin alcohol consumption for frequent smokers. I can, however, rule out economically meaningful reductions (greater than 4%) in alcohol consumption for frequent smokers at the 5% significance level. Occasional smokers drink an additional 2.20 drinks per month after the implementation of smoking bans in bars and restaurants, which is statistically significant at the 5% level. This effect size corresponds to a 7.91% increase in the number of drinks consumed per month, on average.

Never smokers drink 0.28 more drinks per month (an increase of 1.73%) after the implementation of the smoking bans; this coefficient is not statistically significantly different than 0 at conventional levels. I can rule out economically meaningful declines in alcohol consumption for never smokers (greater than 2%) with the 95% confidence interval. Former smokers drink an additional 1.36 drinks per month. This effect size is statistically significantly different than 0 at the 1% level and corresponds to a 5.99% increase in the average number of drinks consumed per month for former smokers.

On average, people are not drinking less after smoking bans in bars and restaurants are implemented, and occasional and former smokers increase their drinking by small-to-moderate amounts. I discuss potential explanations for these results in the next section.

1.4.7 Effect of Smoking Bans on Alcohol Purchased for Off-Premises Consumption by Smoking Status (Nielsen)

Table 1.7 shows the results for the effect of bar and restaurant smoking bans on the total quantity of alcohol purchased for off-premises consumption in the past month using the Nielsen data. After the implementation of smoking bans in bars and restaurants, smokers' monthly off-premises alcohol purchases decline by 0.20 servings of alcohol, on average (a 10.33% decrease). This effect is marginally statistically significant (10% level). I can rule out economically meaningful increases in purchases for off-premises consumption with the 95% confidence interval.

Nonsmokers' monthly off-premises alcohol purchases decline by 0.14 servings of alcohol (-4.00%) after smoking bans are implemented in bars and restaurants, which is not statistically significant at conventional levels. I can rule out economically meaningful increases (larger than 2%) in the quantity of past-month alcohol purchases.

Turning to the extensive margin (the prevalence of off-premises alcohol purchases), which is shown in Table 1.8, smoking bans in bars and restaurants lead to a 0.73-percentage-point decrease in the prevalence of off-premises alcohol purchases for smokers (a 2.31% decline). This effect is not statistically significant. I can rule out economically meaningful increases (larger than 3%) in smokers' prevalence of purchasing alcohol for off-premises consumption.

For nonsmokers, smoking bans in bars and restaurants are associated with a 0.11 percentage point decline in the prevalence of past-month off-premises alcohol purchases (a decline of 0.46%). This effect is not statistically significantly different than 0 at conventional levels. I can rule out economically meaningful increases (larger than 3%) in the prevalence of nonsmokers purchasing alcohol for off-premises consumption.

Overall, I am able to rule out increases in both the total quantity of alcohol purchased for off-premises consumption as well as the prevalence of purchasing alcohol for off-premises consumption for both smokers and nonsmokers. Therefore, the observed increases in alcohol consumption for occasional and former smokers that I find using the BRFSS data are most likely driven by changes in bar and restaurant (on-premises) alcohol consumption for these groups.

Why might occasional and former smokers increase their bar and restaurant alcohol consumption the most? Prior to smoking bans, former smokers may have been avoiding smoke-filled bars and restaurants, for fear that being around other people smoking may trigger them to take up smoking again (this behavior is similar to the model of behavior described in Bernheim and Rangel, 2004).[22] After smoking bans are implemented, however, former smokers may feel more comfortable going out to bars and restaurants, or more comfortable staying there longer, hence their bar and restaurant alcohol consumption would increase. Occasional smokers represent a very small portion of the U.S. adult population (5.25% in my sample), so caution is warranted in interpreting results for this group. However, this group likely consists of many individuals who only smoke after they have been drinking, so they may be drinking more now that more of their former smoker friends are going to bars with them or staying at bars longer.

1.4.8 Event Studies

As a way to test whether the parallel trends assumption is likely to be satisfied, I conduct event studies of several smoking and drinking outcomes using the BRFSS data. Event studies can also highlight whether there may be dynamic treatment effects; for example, if it takes time for people to modify their behavior. To reduce the noise that may arise from monthly observations, I aggregate the data to a quarterly level. I use a pre-period window of 8 quarters (2 years) and a post-period window of 20 quarters (5 years), and I bin the end-points. I omit the quarter prior to implementation as the reference point.

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

In my main difference-in-differences estimates, I use the fraction of the county population subject to a smoking ban in bars and restaurants as my treatment variable. With an event study, it is not possible to use this fractional variable, because I need to identify one specific date where treatment starts. Therefore, I consider the quarter of implementation of a smoking ban in a bar to be the first quarter where any part of the county has implemented a smoking ban. This definition, along with any other definition of treatment, will create some measurement error in my treatment variable. There is no getting around the fact that for some time periods, only parts of some counties are covered by a smoking ban, but I have to consider the county as fully or not-at-all treated in an event-study framework. The reason for using "any" as opposed to half the county or the entire county is that I do not want to include treated individuals in the pre period.[23] However, this definition means that there are some untreated individuals in the post period. Therefore, the post-period coefficients may be attenuated, because the treatment group is only partially treated.

The event-study equation is as follows:

$$Y_{c,q} = \alpha + \sum_{k=-8,k\neq-1}^{k=20} \beta_k \cdot ban_{k,c,q} + \mathbf{X}_{c,q} \cdot \gamma + \delta_c + \rho_q + \varepsilon_{c,q} \tag{1.4}$$

$Y_{c,q}$ represents the smoking or drinking-related outcome for county c in quarter q. $ban_{k,c,q}$ equals 1 if a smoking ban in a bar has been in place in any part of county c for k quarters as of quarter-year q (k ranges from -8 to 20, and $k = -1$ is omitted). The control variables and fixed effects are the same as in the primary difference-in-differences specification (with quarter-year fixed effects as opposed to month-year fixed effects), standard errors are again clustered at the county level, and regressions are weighted by the county population.

[23] Out of all the observations corresponding to counties with at least one smoking ban, 78% were covered by laws that affected at least half the county population that were implemented in the same quarter as the first law. 14% 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 8% of observations had laws that covered at least half the county population that were implemented some time 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.

Figure 1.4 shows the event study for overall alcohol consumption. In the pre period, the coefficients are small and fluctuate around zero. After a smoking ban is implemented, the coefficients increase slightly to around $\frac{1}{2}$ to 1 drinks per month, and are almost always positive, although the individual coefficients are generally not statistically significantly different than 0. While the coefficients fluctuate to some extent, there appears to be a general level increase as opposed to an increase in the trend. The graph shows similar results as the difference-in-differences point estimate for overall alcohol consumption.

Figure 1.5 shows the event study for extensive-margin alcohol consumption. In both the pre and post period, the coefficients are small, fluctuate around zero, and are not statistically significant. This graph is consistent with the difference-in-difference null result for extensive-margin alcohol consumption.

Figure 1.6 shows the event study for intensive-margin alcohol consumption. In the pre period, the coefficients are a touch negative but they are small and not statistically significantly different than zero. In the post period there is a noticeable increase in the effect size; the coefficients fluctuate around 1 drink per month, although the individual coefficients are generally not statistically significantly different than 0. This graph is consistent with the difference-in-differences point estimate of an increase of approximately 1 drink per month.

The event studies for the number of days in the past month, the average amount consumed per day, and the maximum amount consumed on one occasional are shown in Appendix Figures $A.1$ through $A.3$. In each of these graphs, the pre-period coefficients are small and fluctuate around zero, and the post-period coefficients are also small, fluctuate around zero or have barely perceptible increases, and are generally not statistically significant. These results are consistent with the difference-in-differences point estimates of small effects that are sometimes statistically significant.

Figures 1.7 through 1.10 show the results for extensive-margin smoking. In Figures 1.7 and

1.8, which show the effects of smoking bans on frequent and occasional smoking, the pre-period coefficients are small and fluctuate around zero. In the post period, the coefficients are also small, fluctuate around zero, and are not individually significant. These results are consistent with the difference-in-differences point estimates of essentially no effect of smoking bans on the prevalence of frequent or occasional smoking. Figure 1.9 shows the effect for never smoking. Here the pre-period coefficients are also close to zero, but there may be a slight downward trend in the prevalence of never smoking. However, the post-period coefficients are similar in magnitude to the pre-period coefficients, and they are individually not statistically significantly different than 0. Figure 1.10 shows the results for former smoking. In the pre period, the coefficients are small but positive. In the post period, the coefficients are small, fluctuate around zero, and not individually statistically significant.

The event studies for extensive-margin alcohol consumption by smoking status are shown in Appendix Figures $A.4$ through $A.7$. These graphs are a little noisier but they show that in the pre period, the coefficients are generally small and fluctuate around zero. In the post period, the coefficients are also small and fluctuate around zero, which is consistent with the difference-in-differences null results for extensive-margin alcohol consumption for each smoking status.

Figures 1.11 through 1.14 show the results for intensive-margin alcohol consumption by smoking status. In Figure 1.11, the graph for intensive-margin alcohol consumption for frequent smokers, the coefficients in the pre period are small and fluctuate around zero yet they may exhibit a slight downward trend. In the post period, the coefficients are slightly larger and generally positive, although they are not individually statistically significant. For occasional smokers (Figure 1.12), the pre-period coefficients are close to zero yet may reflect a slight upward trend. In the post period, the coefficients are larger and continue on the slight upward trend, although they are not individually significant. For never smokers (Figure 1.13), the pre and post-period coefficients are small and fluctuate around zero. There may be a barely perceptible increase in the post period, although the coefficients are not individually significant. For former smokers (Figure 1.14), the pre-period coefficients are small and fluctuate around zero. In the post period, there are small increases in the effect size, although the coefficients are generally not individually significant. These results are broadly consistent with the difference-in-differences point estimates: small increases for frequent, occasional, and former smokers (although the point estimate for frequent smokers was not statistically significant), and very small increases for never smokers (again not significant).

I also conduct event studies for the outcomes in the Nielsen data, shown in Figures 1.15 through 1.20, for the quantity of alcohol purchases and the prevalence of alcohol purchases overall, for smokers, and for nonsmokers. Bar and restaurant smoking bans do not have an effect on the quantity of alcohol purchased (Figure 1.15); the coefficients are small, fluctuate around 0, and are not statistically significant. There is likewise no effect of smoking bans on purchases for smokers (Figure 1.17). For nonsmokers, there may be slight increases in alcohol purchases after several years but the coefficients are not individually statistically significant (Figure 1.18). Bar and restaurant smoking bans also do not have an effect on the prevalence of purchasing alcohol in the past month (Figure 1.16); the coefficients are small but generally negative, and not statistically significant. There is generally no effect of smoking bans on smokers' and nonsmokers' prevalence of purchasing alcohol for home consumption (Figures 1.19 and 1.20), with the estimates for nonsmokers being more attenuated and precise.

1.5 Effect of Smoking Bans on Alcohol-Related Externalities

Do these small-to-moderate increases in alcohol consumption translate into increases in crime or drunk driving? Prior research in economics has documented the relationship between alcohol consumption and violent crime, but it is possible that these small increases may have different effects.[24] I test whether these changes in alcohol consumption correspond to changes in crime by analyzing the effect of smoking bans on violent crime measured by the Uniform Crime Reports. Prior research has also shown that smoking bans lead to increases in drunk driving (Adams and Cotti, 2008), so I also test whether smoking bans have the same effects in my sample, using the Fatality Analysis Reporting System data on fatal crashes.

1.5.1 Reduced-Form Regression Equation: Crime

I aggregate various measures of violent crime to the agency-year level. Consistent with the literature, I use the number of crimes per 10,000 people as my outcome variable. I consider the agency treated if its corresponding city, county, or state has implemented a smoking ban in bars and restaurants. I estimate the following equation:

$$Y_{a,t} = \alpha + \beta \cdot ban_{a,t} + \mathbf{X}_{a,t} \cdot \gamma + \delta_a + \rho_t + \varepsilon_{a,t} \tag{1.5}$$

$Y_{a,t}$ is defined as the crime rate (per 10,000 people) for agency a in year t. $ban_{a,t}$ represents the fraction of year t for which the agency's jurisdiction is subject to a smoking ban in bars and restaurants.[25] The vector $\mathbf{X}_{a,t}$ includes control variables for a smoking ban in restaurants only, the state-level blood alcohol concentration limit, and the state cigarette tax.[26]

δ_a denotes agency-level fixed effects and ρ_t represents year fixed effects. I cluster the standard errors, $\varepsilon_{a,t}$, at the agency level. I weight the regressions by the population estimates for the agency's jurisdiction, which allows me to interpret the results as the effect of smoking bans on crime rates experienced by the average person as opposed to the average agency.

[24]e.g. Anderson et al. (2018), Hansen (2015), Lindo et al. (2018), Markowitz and Grossman (1998), Tomé (2019).

[25]For most years, $ban_{a,t}$ will equal 0 or 1. In the year it was implemented, $ban_{a,t}$ represents the number of months out of the year the ban was in place.

[26]I don't include controls for population demographics because agency-level demographics are not available.

1.5.2 Results: Crime

Smoking bans in bars and restaurants do not have an effect on various measures of violent crime, as shown in Table 1.9. None of the effects are statistically significant, but I will briefly discuss the point estimates to show that they are also not economically significant. For all violent crimes, smoking bans in bars and restaurants lead to a reduction in the violent crime rate of 0.55 crimes per 10,000 people, a decrease of 1.03%. Smoking bans are associated with a reduction of 0.01 murders per 10,000 people, a 2.27% decrease. Reported rapes increase by 0.05 per 10,000 people after smoking bans are implemented, an increase of 1.67%. Aggravated assaults decline by 0.34 per 10,000 people, a 0.95% decrease. Smoking bans lead to a reduction of 0.82 simple assaults per 10,000 people, a 0.83% decrease. All of the observed changes in crime rates are quite small. The effects on violent crimes other than murder are relatively precisely estimated. Given the rarity of murder, those effects are less precisely estimated (the lower bound of the 95% confidence interval represents a 10% decrease in murder). Although smoking bans in bars and restaurants lead to small-to-moderate increases in alcohol consumption, there is no corresponding increase in violent crime. These results suggest that small increases in alcohol consumption do not lead to increases in violent behavior.

1.5.3 Event Studies: Crime

To assess the likelihood of the parallel trends assumption being satisfied, I conduct event studies using the UCR data. The equation is similar to the event-study equation for the BRFSS outcomes. As with the simple difference-in-differences estimation, the data are aggregated to the agency-year level. I use a pre-period window of 4 years and a post-period window of 5 years, and I bin the end-points. I omit the year prior to implementation as the reference point. I consider the year of implementation of a smoking ban in a bar to be the first year where a city, county, or state that covers the agency's jurisdiction has implemented a smoking ban in both bars and restaurants for any part of the year.

The event-study equation is as follows:

$$Y_{a,t} = \alpha + \sum_{k=-4,k\neq-1}^{k=5} \beta_k \cdot ban_{k,a,t} + \mathbf{X}_{a,t} \cdot \gamma + \delta_a + \rho_t + \varepsilon_{a,t} \tag{1.6}$$

$Y_{a,t}$ represents various crime rates using crimes reported to agency a in year t. $ban_{k,a,t}$ equals 1 if a smoking ban in a bar has been in place for a city, county, or state that covers agency $a's$ jurisdiction for k years as of year t (k ranges from -4 to 5, and $k = -1$ is omitted). The control variables and fixed effects are the same as in the primary difference-in-differences specification, standard errors are again clustered at the agency level, and regressions are weighted by the population of the agency's jurisdiction.

Figure 1.21 shows the effect of smoking bans in bars and restaurants on the violent crime rate. In both the pre and post periods, the coefficients are small, fluctuate around zero, and not statistically significantly different than 0. The event study is consistent with the difference-in-differences point estimate of null effects of smoking bans in bars and restaurants on reported violent crime.

Appendix Figures $A.8$ through $A.11$ show the effects of smoking bans on the rates of reported murders, rapes, aggravated assaults, and simple assaults. The results for homicide, sexual assault, and aggravated assault are similar to the results for violent crime: null effects and no evidence of dynamic effects. For simple assaults, there may be a small decrease in the year of and the year after implementation of a smoking ban, followed by a slight upward trend in assaults, but the coefficients are generally not individually statistically significant and they are small.

1.5.4 Reduced-Form Regression Equation: Drunk Driving

Consistent with the smoking and drinking outcomes, I aggregate fatal drunk-driving crashes to the county-month level. I define drunk-driving crashes as those in which at least one vehicle driver had a recorded blood alcohol concentration of at least 0.08 g/dL, which is the legal limit for Driving Under the Influence for adults for all states during the majority of my sample period. Given the large differences in population size across counties, I use the log of crashes so that the effect size is measured in percent changes, which is more comparable across counties than the level of crashes. For many counties, there are no fatal crashes in a given month, so I take the log of 1 plus fatal crashes.

I also interact the treatment variable with indicators for smoking prevalence (high, medium, and low smoking prevalence) from the TUS. I estimate the following equation:

$$log(Y_{c,m} + 1) = \alpha + ban_{c,m} \cdot \mathbb{I}\{smk\ prevalence\}_{c,m} \cdot \beta + \mathbf{X}_{c,m} \cdot \gamma + \delta_c + \rho_m + \varepsilon_{c,m} \tag{1.7}$$

$Y_{c,m}$ denotes fatal drunk-driving crashes in county c at time (month-year) m. As in section 1.3.2, $ban_{c,m}$ represents the fraction of individuals subject to a smoking ban in both bars and restaurants at time m in county c. $\mathbb{I}\{smk\ prevalence\}_{c,m}$ represents indicator variables for high, medium, and low smoking prevalence. The vector $\mathbf{X}_{c,m}$ contains the same control variables as in section 1.3.2.[27] δ_c denotes the county-level fixed effects and ρ_m denotes the time (month-year pair) fixed effects.

I cluster the standard errors, $\varepsilon_{c,m}$, at the county level. I use the county population as probability weights, which makes my results interpretable as the effects of smoking bans on drunk driving crashes for the average person as opposed to the average county.

[27] The fraction of the county population subject to a smoking ban in a restaurant only, the percentages of the population in county c at time m that are male, non-Hispanic black, non-Hispanic Asian, Hispanic, other (non-Hispanic and non-white) races, under the age of 15, 15-24, 35-44, 45-64, and 65 or older; the state-level legal limit for blood alcohol concentration for operating a motor vehicle; and the state-level cigarette tax.

1.5.5 Results: Drunk Driving

I find that smoking bans in bars and restaurants have differential effects on fatal drunk driving crashes by smoking prevalence, as shown in Table 1.10. Across all jurisdictions, smoking bans in bars and restaurants have no effect on fatal drunk-driving crashes. However, in areas with a high prevalence of smoking, smoking bans in bars and restaurants lead to approximately a 4% increase in fatal drunk-driving crashes. This effect is statistically significant at the 5% level. Conversely, smoking bans do not have an effect on drunk driving crashes in areas with medium and low smoking prevalence. They are associated with a 2% decline in crashes in medium-smoking-prevalence areas and a 1% decline in crashes in low-smoking-prevalence areas. Neither effect is statistically significant, and I can rule out economically meaningful changes in the prevalence of drunk-driving crashes for both.

These results are consistent with prior work by Adams and Cotti (2008), which also finds an increase in drunk driving after smoking bans are implemented in bars and restaurants. As Adams and Cotti explain, these results are consistent with smokers driving to nearby jurisdictions without smoking bans so they may smoke and drink at a bar, then driving home drunk. That I only find an effect on drunk driving in areas with a high smoking prevalence lends further support to this hypothesis, as it is consistent with smokers being the ones driving drunk.

1.5.6 Event Studies: Drunk Driving

I also conduct event studies using the FARS data. The equation is similar to the event-study equation for the BRFSS outcomes, except that I am using the log of (1 +) drunk-driving crashes. I aggregate the data to a quarterly level. I use a pre-period window of 8 quarters (2 years) and a post-period window of 20 quarters (5 years), and I bin the end-points. I omit the quarter prior to implementation as the reference point. I consider the quarter of implementation of a smoking ban in a bar to be the first quarter where any part of the county has implemented a smoking ban. I estimate separate regressions for the overall prevalence of drunk driving, as well as drunk driving in high, medium, and low smoking areas.

The event-study equation is as follows:

$$log(Y_{c,q} + 1) = \alpha + \sum_{k=-8,k\neq-1}^{k=20} \beta_k \cdot ban_{k,c,q} + \mathbf{X}_{c,q} \cdot \gamma + \delta_c + \rho_q + \varepsilon_{c,q} \tag{1.8}$$

$Y_{c,q}$ represents fatal drunk-driving crashes for county c in quarter q. $ban_{k,c,q}$ equals 1 if a smoking ban in a bar has been in place in any part of county c for k quarters as of quarter-year q (k ranges from -8 to 20, and $k = -1$ is omitted). The control variables and fixed effects are the same as in the primary difference-in-differences specification (with quarter-year fixed effects as opposed to month-year fixed effects), standard errors are again clustered at the county level, and regressions are weighted by the county population.

Figure 1.22 shows the effect of smoking bans in bars and restaurants on overall fatal drunk-driving crashes. The coefficients in the pre period are small and not statistically significantly different than zero. The post-period coefficients are of a similar magnitude and likewise generally individually not statistically significant. Figure 1.23 shows the effect of smoking bans in bars and restaurants on fatal drunk-driving crashes in areas with a high prevalence of smoking. The coefficients in the pre and post period are noisy, of a similar magnitude, almost universally positive, and individually not statistically significant. The 4% increase in drunk-driving crashes in my primary specification does not show up in the event study, in the sense that there is not a visible increase in drunk-driving crashes after smoking bans are implemented. This inconsistency could reflect a false positive, or it could also be due to the measurement error in my treatment variable: I count a county as being treated as soon as any part of the county is subject to a smoking ban. Given the rarity of fatal drunk-driving crashes; however, it could be that an increase in drunk driving will not show up in the data until more of the county is truly treated, which would attenuate my post-period coefficients.

Figures $A.12$ and $A.13$ show the effects on fatal drunk-driving crashes in medium and low-smoking-prevalence areas. In both graphs, the coefficients in the pre and post periods are close to zero and individually not statistically significant. These results are consistent with the difference-in-differences point estimates of null effects.

1.6 Alternative Specifications and Robustness Checks: Alcohol Consumption and Smoking

In sections 1.6.1 through 1.6.3 I turn to more disaggregated measures of alcohol consumption in order to analyze along what margins individuals are changing their alcohol consumption. Are they drinking on more days throughout the month, are they drinking more alcohol on the days they drink, or are they doing both? Understanding the effects at a more detailed level can illustrate whether these changes in drinking behavior may have negative health consequences. For example, taking up binge drinking and going from two to six drinks one night each week may have different health effects than drinking two drinks each on an additional two days per week (even though the total change in weekly alcohol consumption, four additional drinks, is the same). Binge drinking is associated with negative health effects such as alcohol poisoning and other unintentional injuries (CDC, 2019a).

In section 1.6.4, I interact smoking bans with indicators for smoking prevalence, similar to the drunk-driving results. Behavioral responses to smoking bans in bars and restaurants may depend not only on an individual's smoking status but also on friends' or neighbors' smoking status. Alternatively, if individuals, particularly smokers, are avoiding the bans by traveling to bars in nearby jurisdictions, then these results may be picking up any changes in alcohol consumption arising from those trips (e.g. staying longer and drinking more because of the increased travel cost).

In section 1.6.5, I compare the results from my primary specification, which includes city-level bans, to results from an alternative specification where I only use state-level bans. Prior research on smoking bans and alcohol consumption only analyzes the effect of state-level bans, so this robustness check allows me to compare my results to the previous literature.

1.6.1 Effect of Smoking Bans on Number of Days Drank (BRFSS)

Appendix Table $A.6$ shows the results for the effect of smoking bans in bars and restaurants on the number of days individuals consumed alcohol (over the past 30 days, conditional on drinking). After the implementation of smoking bans in bars and restaurants, frequent smokers drink alcohol on 0.03 additional days per month, a 0.29% increase. Occasional smokers drink alcohol on an additional 0.28 days per month, an increase of 3.28%. Never smokers drink on 0.03 additional days per month, which is a 0.37% increase. Former smokers see a 0.06-day increase in the number of days they drank (0.60%). None of these estimates are statistically significantly different than 0.

Overall, bar and restaurant smoking bans are not associated with economically meaningful changes in the number of days per month individuals of any smoking status drink alcohol (conditional on drinking in the past 30 days). For frequent, never, and former smokers, I find precisely estimated null effects of smoking bans on the number of days drank in the last month. For occasional smokers, I can rule out small decreases and moderate increases in the number of days spent drinking.

1.6.2 Effect of Smoking Bans on Average Amount of Alcohol Consumed on Drinking Days (BRFSS)

Appendix Table *A*.7 shows the results for the effect of smoking bans in bars and restaurants on the average amount of alcohol individuals consumed on days they drank (conditional on drinking during the past 30 days). After smoking bans in bars and restaurants are implemented, frequent smokers drink, on average, an additional 0.03 drinks on days they drink (a 0.85% increase). For occasional smokers, bar and restaurant smoking bans are associated with an increase of 0.07 drinks on average, on days they drink alcohol (an increase of 2.15%). Never smokers drink an additional 0.03 servings of alcohol, on average, on days they drink (a 1.21% increase). None of these effects are statistically significantly different than 0. In contrast, former smokers drink an additional 0.10 drinks per day on days they drink alcohol, which is statistically significantly different than 0 at the 1% level. This effect size corresponds to a 4.72% increase.

Overall, I can rule out economically meaningful declines in the number of drinks consumed on days individuals drank alcohol for each smoking status. I find that former smokers experience small-to-moderate increases in their average alcohol consumption on days they drink.

1.6.3 Effect of Smoking Bans on Maximum Amount of Alcohol Consumed on One Occasion (BRFSS)

Appendix Table *A*.8 shows the results for the effect of bar and restaurant smoking bans on the maximum amount of alcohol consumed on one occasion in the past 30 days. For frequent smokers, the implementation of smoking bans in bars and restaurants is associated with a 0.08-drink increase in the maximum amount of alcohol consumed on one occasion (a 1.75% increase). Occasional smokers see essentially no change (+0.01 drinks) in the maximum amount of alcohol consumed on one occasion (a 0.22% increase). Similarly, for never smokers, bar and restaurant smoking bans are associated with essentially no change in the maximum amount of alcohol consumed on one occasion (+0.02 drinks, an increase of 0.61%). None of those estimates are statistically significant. In contrast, for former smokers, smoking bans in bars and restaurants are associated with a 0.09-drink increase in the maximum amount of alcohol consumed on one occasion. This effect size is statistically significant at the 5% level and corresponds to a 2.8% increase in the maximum amount of alcohol consumed on one occasion.

Overall, smoking bans in bars and restaurants have no effect on the maximum amount of alcohol consumed by frequent, occasional, and never smokers. For former smokers, these bans are associated with small but not economically meaningful increases in the maximum amount of alcohol consumed on one occasion.

1.6.4 Effects of Smoking Bans on Alcohol Consumption and Smoking for Areas with High, Medium, and Low Smoking Prevalence

Appendix Table *A*.9 shows the results for the effect of bar and restaurant smoking bans on alcohol consumption by statewide smoking prevalence. In areas with a high prevalence of smoking, there are small but not statistically significant increases in alcohol consumption (0.58 drinks per month overall, a 6.77% increase, and 0.98 drinks per month on the intensive margin, a 4.27% increase). The average number of drinks per day increases slightly, by 0.08 drinks or 3.14% (significant at the 5% level), as does the maximum amount of alcohol consumed on one occasion (0.13 drinks, a 3.73% increase, which is significant at the 5% level). In areas with a medium prevalence of smoking, the effects are even smaller and still not statistically significant (increases of 0.18 drinks per month overall, or 1.57%, and 0.41 drinks per month on the intensive margin, a 1.82% increase).

Areas with a low prevalence of smoking show the largest increases in alcohol consumption: 1.02 additional drinks per month overall (significant at the 1% level, corresponding to an 8.16% increase in consumption), and 1.64 additional drinks per month on the intensive margin (also significant at the 1% level, and reflecting a 7.35% increase). Disaggregating the intensive-margin effects, individuals in low-smoking-prevalence areas drink on an additional 0.19 days per month (a 2.06% increase; significant at the 1% level) and drink an additional 0.07 servings of alcohol on days they drink (a 2.89% increase, which is significant at the 5% level). The maximum amount of alcohol consumption also increases slightly (0.05 drinks, or 1.33%), but the effect is not statistically significant.

Overall, the increases in alcohol consumption appear to be coming primarily from areas with a low prevalence of smoking. This result is consistent with smoking bans in bars creating a more enjoyable environment for most people. It only takes one smoker to fill the bar with a cloud of smoke, and so a smoking ban in areas with a low prevalence of smoking will only inconvenience a few people while improving the atmosphere for many.

Appendix Tables $A.10$ and $A.11$ show results for alcohol consumption by both smoking status and statewide smoking prevalence. For example, how does alcohol consumption change for former smokers who live in areas with a high prevalence of smoking relative to former smokers who live in areas with a low prevalence of smoking? There are no statistically or economically significant changes along the extensive margin (Appendix Table $A.10$), in line with the other extensive-margin results.

Along the intensive margin (Appendix Table $A.11$), again the largest increases in alcohol consumption generally come from individuals who live in areas with a low prevalence of smoking. In low-smoking areas, frequent smokers drink an additional 4.15 drinks per month (an 11.55% increase, which is significant at the 1% level), occasional smokers drink an additional 3.15 drinks per month (an 11.32% increase, which is marginally statistically significant), never smokers drink an additional 0.70 drinks per month (a 4.21% increase; significant at the 5% level), and former smokers drink an additional 0.76 drinks per month (an increase of 3.19%, which is marginally statistically significant). The large increase for frequent smokers is consistent with both a substitution effect and a peer effect; now that smokers cannot smoke while sitting at the bar, they may switch to drinking more. They may also drink more because now more of their friends are going out to bars with them (e.g. their never-smoker and former-smoker friends).

In areas with a medium prevalence of smoking, the effects of smoking bans on alcohol consumption for frequent and never smokers are small and not statistically significant. There are moderate increases in alcohol consumption for occasional (2.36 drinks per month, an 8.79% increase) and former (1.35 drinks per month, a 5.42% increase) smokers. These effects are marginally statistically significant.

In areas with a high prevalence of smoking, there are small declines in alcohol consumption for frequent smokers (0.94 drinks per month, or 2.60%), although the effect is not statistically significant. Occasional and never smokers' alcohol consumption increases by very small amounts in high-smoking prevalence areas but these effects are not statistically significant: 0.25 drinks per month for occasional smokers (a 0.96% increase) and 0.41 drinks per month for never smokers (a 2.63% increase). Former smokers, however, drink an additional 2.50 drinks per month if they live in areas with a high prevalence of smoking (significant at the 1% level), which corresponds to a 10.89% increase. This last result is consistent with these former smokers feeling more comfortable going out to bars after smoking bans are implemented, because they no longer have to be worried about being surrounded by smokers (which was more likely to occur in areas with a higher prevalence of smoking).

The results for alcohol purchases for home consumption are presented in Appendix Table $A.12$. In areas with a high smoking prevalence, there are small decreases in the total quantity of alcohol purchases and the prevalence of purchasing alcohol, which are driven by smokers. In areas with a medium smoking prevalence, there are small declines in the total quantity of alcohol purchases, largely due to reductions in the amount purchased by smokers. The prevalence of purchasing alcohol declines a bit for smokers and increases a very small amount for nonsmokers (neither are statistically significant). In areas with a low prevalence of smoking, there is essentially no effect on the total quantity of alcohol purchased due to offsetting changes for smokers and nonsmokers. Smokers' alcohol purchases increase by 0.30 drinks per month (9.47%, marginally statistically significant), and nonsmokers' alcohol purchases decline by 0.32 drinks per month (7.36%, not statistically significant). Nonsmokers are slightly less likely to purchase any alcohol for off-premises consumption though (1.48 percentage points, or 6.04%, which is significant at the 5% level).

Comparing the BRFSS with the Nielsen results yields suggestive evidence that the increases in alcohol consumption are coming at least partially from bars and restaurants. In the BRFSS there are small increases in alcohol consumption in high, medium, and low-smoking-prevalence areas (Column 1 of Appendix Table $A.9$), although only the effect in low-smoking areas is statistically significant. In the Nielsen there are small declines in the quantity of alcohol purchases in high and medium-smoking areas and no changes in low-smoking-prevalence areas. Further, the confidence intervals for the BRFSS and Nielsen estimates of alcohol consumption and purchases in low-smoking areas do not overlap, suggesting that there must be an increase in bar and restaurant consumption. The increase in intensive-margin alcohol consumption for frequent and occasional smokers in low-smoking areas (Columns 2 and 3 of Appendix Table $A.11$) is partially due to increases in purchases for at-home consumption (Column 2 of Appendix Table $A.12$), but it is likely also due in part to increases in bar and restaurant consumption for smokers. Similarly, the increase for never and former smokers is also likely due in part to increases in bar and restaurant consumption.

Appendix Table $A.13$ shows the effect of bar and restaurant smoking bans on smoking status by statewide smoking prevalence. Consistent with the other results on smoking status, the coefficients are close to zero and generally not statistically significant at conventional levels. There are some small increases in the prevalence of occasional smoking in areas with a high or medium prevalence of smoking (a 0.3 percentage-point increase, or 6%). The effect in medium smoking prevalence areas is marginally statistically significant. Again though, the magnitude of the coefficient is quite small. In high, medium, and low smoking prevalence areas, smoking bans do not lead to changes in smoking status during this time period (2004-2012).

1.6.5 Effect of State-Level Smoking Bans Only

To more directly compare my results with the previous literature on smoking bans and alcohol consumption, I run an alternative specification where I only use state-level bans. Any jurisdiction without a state-level ban (even if it is covered by a city or county-level ban) is considered part of the untreated group. The effects are similar to my main specification, although the disaggregated results by smoking status are slightly different. Appendix Figures $A.14$ and $A.15$ show the main results. Smoking bans in bars and restaurants lead to an average increase in alcohol consumption of 0.60 drinks over the past 30 days (a 5.15% increase), which is statistically significant at the 1% level. There is a corresponding 1.13-drink increase along the intensive margin (an increase of 5.16%), which is also statistically significant at the 1% level. There is essentially no effect of smoking bans on the extensive margin of alcohol consumption (-0.27 percentage points, a 0.51% decrease, which is not statistically significantly different than 0). There are small increases in the number of days individuals drank, the average amount they drank per day, and the maximum amount of alcohol they drank on one occasion, although the effect on the number of days is only marginally statistically significant.

For alcohol purchases, there are small declines in the total quantity of alcohol purchased for home consumption (-0.21 drinks, a 3.94% decrease), but the effect is not statistically significant.

After smoking bans are implemented, the prevalence of purchasing alcohol for home consumption declines by 1.50 percentage points (a 5.82% decrease), which is statistically significant at the 5% level. Given the overlapping confidence intervals for the effects on overall alcohol consumption and the total quantity of alcohol purchases, I cannot say definitively that the increases seen in the BRFSS are coming from increases in bar and restaurant alcohol consumption.

Appendix Figures $A.16$ through $A.19$ show the effects on alcohol consumption disaggregated by smoking status. Figure $A.16$ shows the effects on extensive-margin alcohol consumption. There is no effect of smoking bans in bars and restaurants along the extensive margin for frequent or never smokers. There is a 1.75-percentage-point decline in the prevalence of drinking for occasional smokers (a decline of 2.70%), which is marginally statistically significant. There is a 0.75-percentage-point decline for former smokers (a 1.30% decrease), which is also marginally statistically significant. Although marginally significant, both of these effects are small.

Appendix Figure $A.17$ shows the effects on intensive-margin alcohol consumption. Bar and restaurant smoking bans lead to an increase of 2.24 drinks per month for frequent smokers, which is statistically significant at the 5% level and corresponds to a 6.24% increase. They are associated with a 1.61-drink increase for occasional smokers (a 5.77% increase), although the effect is not statistically significant. Smoking bans lead to a 0.58-drink increase for never smokers (3.61% increase), which is statistically significant at the 5% level. They lead to a 1.36-drink increase for former smokers, which is significant at the 1% level and corresponds to a 5.97% increase. These results are qualitatively similar to my main specification (where I include city and county-level bans). The notable difference is that when I only use state-level bans, I find statistically significant increases in intensive-margin alcohol consumption for frequent, never, and former smokers, whereas in my main specification I find statistically significant increases for occasional and former smokers.

Appendix Figures $A.18$ and $A.19$ show the results for alcohol purchases by smoking status.

Smoking bans are associated with small declines in the total quantity of alcohol purchased for home consumption for both smokers and nonsmokers (-0.17 and -0.16 drinks per month, or 8.85% and 4.55% declines, respectively), although neither effect is statistically significant. Smoking bans are associated with a 1.96 percentage point decrease in the prevalence of purchasing alcohol for home consumption in the past month for smokers (6.21% decrease), although this effect is not significant. They are associated with a 1.39 percentage point decrease in the prevalence of purchasing alcohol for nonsmokers, which is marginally statistically significant and corresponds to a 5.71% decrease. These results are generally consistent with the specification that includes county and city-level bans.

1.7 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. Smoking bans in bars and restaurants have made some individuals better off with respect to smoking and secondhand-smoke-related health outcomes (e.g. Anger, Kvasnicka, and Siedler, 2011; Bharadwaj, Johnsen, and Løken, 2014; Jones et al., 2015; and Kvasnicka, Siedler, and Ziebarth, 2018).

In this paper, I test whether smoking bans in bars and restaurants have unintended consequences with respect to alcohol consumption and externalities such as drunk driving. I use the Behavioral Risk Factor Surveillance System and the Nielsen Consumer Panel to analyze whether smoking bans affect alcohol consumption, alcohol purchases for consumption at home, and the prevalence of smoking. I use the Uniform Crime Reports and Fatality Analysis Reporting System to analyze whether these bans are associated with alcohol-related externalities such as violent crime and drunk driving. To identify causal effects of smoking bans on these outcomes, I estimate a difference-in-differences model where my identifying variation is variation in the effective dates of smoking bans in bars and restaurants at the city, county, and state level.

I find that smoking bans in bars and restaurants result in average increases in alcohol consumption of approximately 1 drink per month (conditional on drinking), or 4%. These increases appear to be concentrated among occasional smokers (2.20 drinks per 30 days) and former smokers (1.36 drinks per 30 days). These small increases in alcohol consumption probably do not have negative health effects. They are also most likely driven entirely by changes in bar and restaurant consumption, as purchases for off-premises consumption are flat or decline after smoking bans in bars and restaurants are implemented. Smoking bans in bars and restaurants also lead to a 4% increase in fatal drunk-driving crashes in areas with a high prevalence of smoking. It is worth noting that the likely reason for increased drunk driving, smokers driving to nearby jurisdictions where they can smoke and drink at the bar and then drive drunk home, is a feature of the spatial heterogeneity in the law. A federal smoking ban in bars and restaurants may not have the same effects on drunk driving.

Why didn't smoking bans lead to greater increases in alcohol consumption for nonsmokers relative to other smoking status groups? One might expect nonsmokers to experience greater disutility from cigarette smoke than current or former smokers, so their behavior should have perhaps changed the most. One possible explanation is that nonsmokers are the largest smoking status group by far (57% of adults in the BRFSS sample report being never smokers) and there may be heterogeneous effects of smoking bans on alcohol consumption within the nonsmoking group. If an individual is never going to go to a bar under any circumstances, it does not matter whether that bar has a smoking ban. If some nonsmokers are drinking more at bars but most are not changing their behavior, the average effect will appear very small.

I contribute to the literature on smoking bans and alcohol consumption by incorporating city and county level smoking bans. Like Koksal and Wohlgenant (2016), who analyze state-level smoking bans in restaurants, I find that state and local bans in bars and restaurants lead to increases in bar and restaurant alcohol consumption. Similar results for state-level bans and local bans were not a foregone conclusion: local bans are generally much more avoidable than state-level bans. Smokers may avoid local bans by driving elsewhere to smoke and drink at the bar, which may not lead to a decrease in alcohol consumed at a bar. With a state-level ban, they may instead substitute toward consuming alcohol at home.

The crime results contribute to the literature on alcohol consumption and crime by showing that small increases in alcohol consumption do not necessarily lead to increases in violent crime. This result is interesting from a policy perspective because it suggests that an optimal alcohol-regulation and crime-prevention policy may target heavy drinking or binge drinking as opposed to all drinking.

How do these effect sizes compare to other policies that affect alcohol consumption? The overall effect on alcohol consumption (0.52 drinks per month) is quite a bit smaller than the change in alcohol consumption at the minimum legal drinking age in Canada. When young adults are legally able to consume alcohol, their monthly alcohol consumption increases by approximately 5 drinks per month (reported as 1.16 drinks per week), which is ten times larger than the effect of smoking bans that I find (Carpenter, Dobkin, and Warman, 2016). Stehr (2007) finds that repeal of a ban on Sunday alcohol sales leads to a 2.4% increase in beer sales and a 3.5% increase in liquor sales. Assuming sales are a good proxy for alcohol consumption, the effect of a Sunday sales ban thus appears to be slightly smaller but similar to the effect of a smoking ban (4.5% increase in overall alcohol consumption).

The fact that smoking bans do not appear to lead to a decline in alcohol consumption at bars and restaurants raises an interesting question: why didn't more bars and restaurants voluntarily adopt smoking bans? To start, many bar owners didn't think that a smoking ban would be good for business (Milwaukee Record, 2015). In addition, they faced a Prisoner's Dilemma-type situation.

The best financial outcome might be for all bars to be smoke free, but if one bar were to defect, they would capture all of the smokers' business. That concern was in fact raised by bar owners when the enactment of these bans was being debated (Maine State Legislature, 2004). In this instance, smoking bans served to solve a coordination failure among private businesses.

An interesting direction for future research would be to test for heterogeneity in the policy impacts. In particular, it may be worth exploring 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. There may also be differences based on the level at which the policy is implemented, e.g. a state-level policy versus a city-level policy. Additionally, given the notable regional differences in smoking prevalence, it would be interesting to determine whether there are heterogeneous effects by geographic region. Future research could also conduct a cost-benefit analysis of this policy by comparing the health and mortality benefits of secondhand smoke avoided with the lives lost from drunk driving.

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 estimate the effect on bar and restaurant alcohol consumption by comparing the effect on total alcohol consumption, as measured by the BRFSS, with the effect on alcohol purchased for at-home consumption, as measured by 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. Another limitation is that my measure of crime only includes crimes reported to law enforcement agencies. Certain crimes, particularly sexual assaults, are heavily underreported to law enforcement, and the effect of smoking bans on reported crime might not be the same as the effect of smoking bans on unreported crime. With respect to drunk driving, my measure of drunk driving is fatal crashes, but there could be effects on nonfatal crashes or the incidence of drunk driving more broadly (e.g. DUIs) that I fail to pick up with this limited measure.

The results of this paper show that when risky health behaviors are substitutes or complements, a policy change targeting one risky health behavior can have spillover effects on another risky health behavior. In this instance, a policy ostensibly aimed at minimizing smoking and secondhand smoke had unintended consequences on alcohol consumption and drunk driving. Optimal policy regarding risky health behaviors and their externalities needs to anticipate the behavioral responses arising from the substitutability or complementarity of risky health behaviors.

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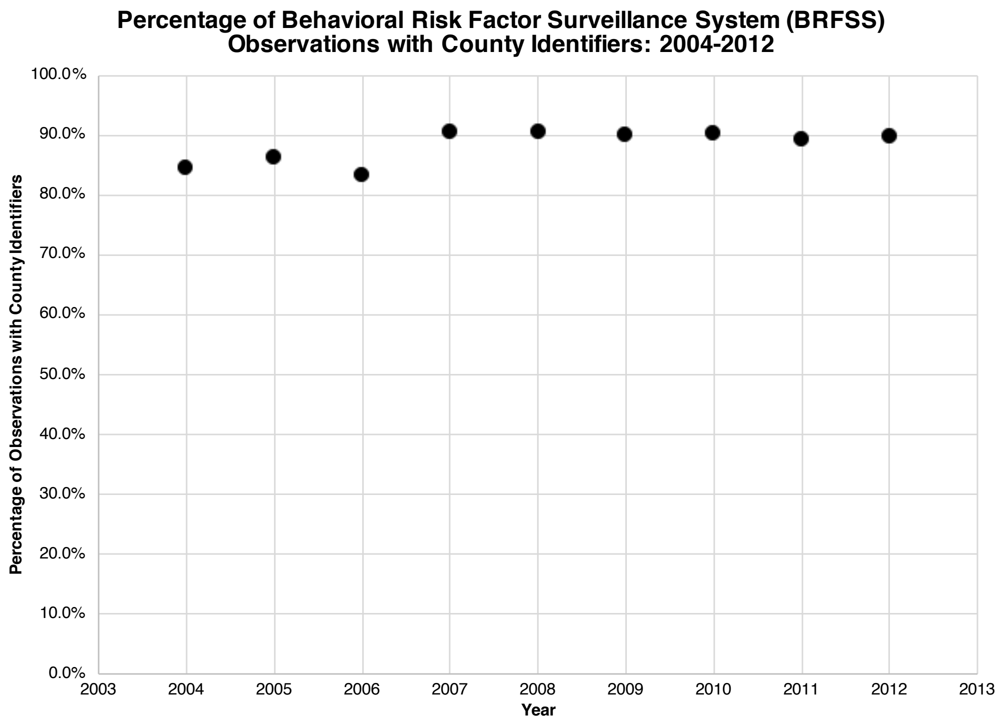
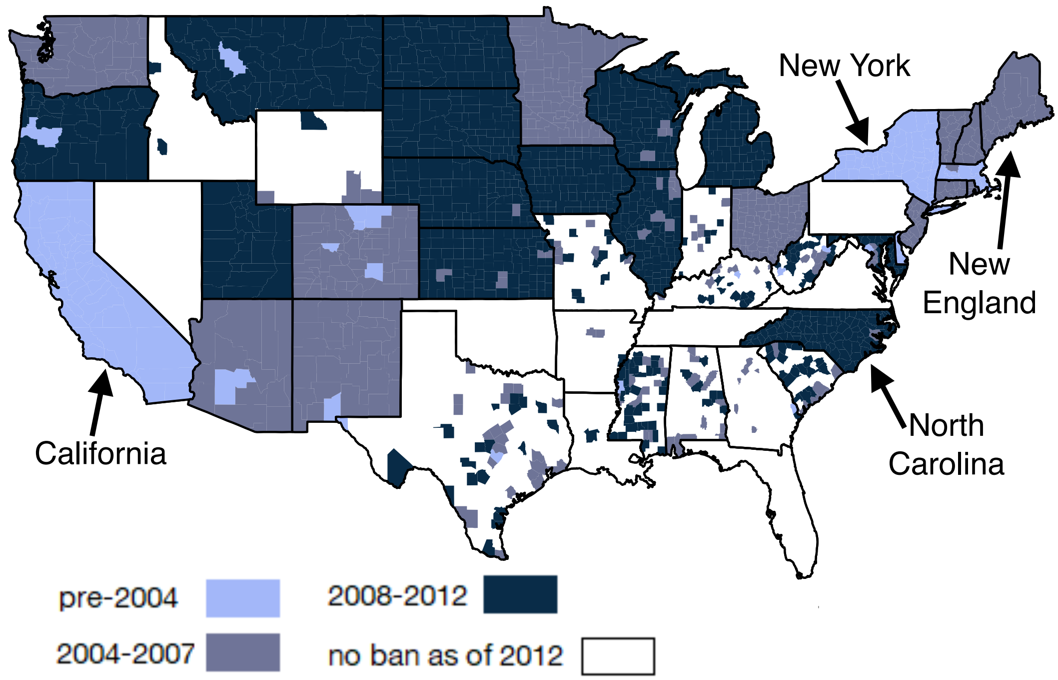
1.9 Figures and Tables

Figure 1.1



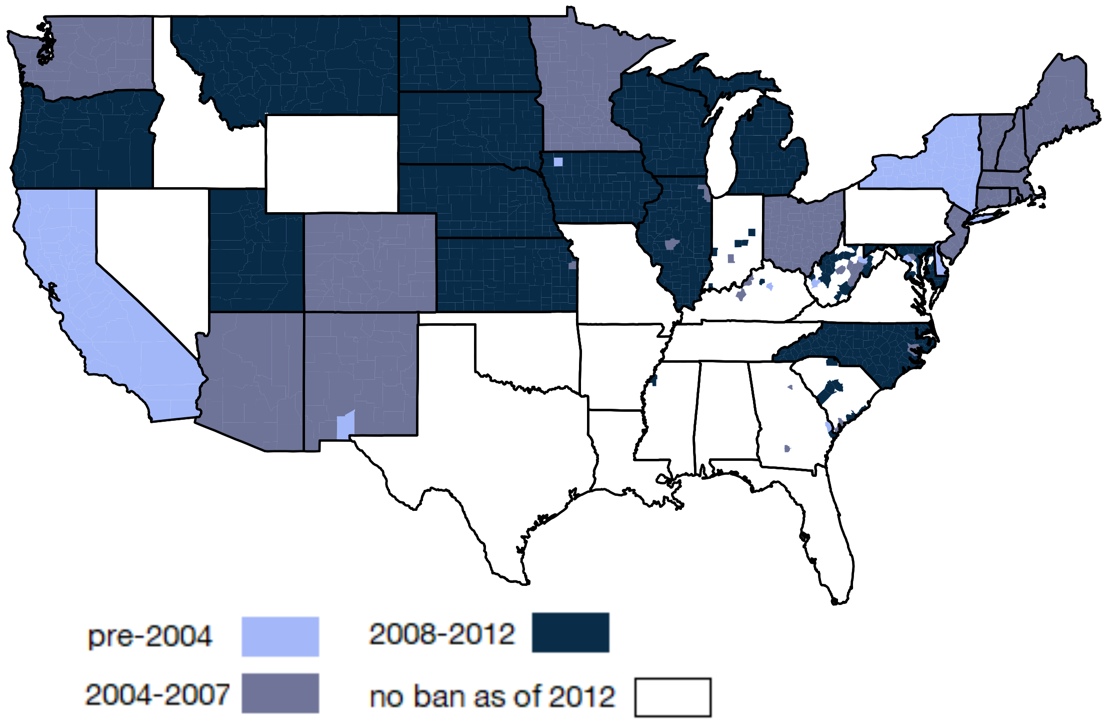
Data source: BRFSS 2004-2012.

Figure 1.2: Map of Smoking Bans in Bars Implemented by Cities, Counties, and States by December 31, 2012



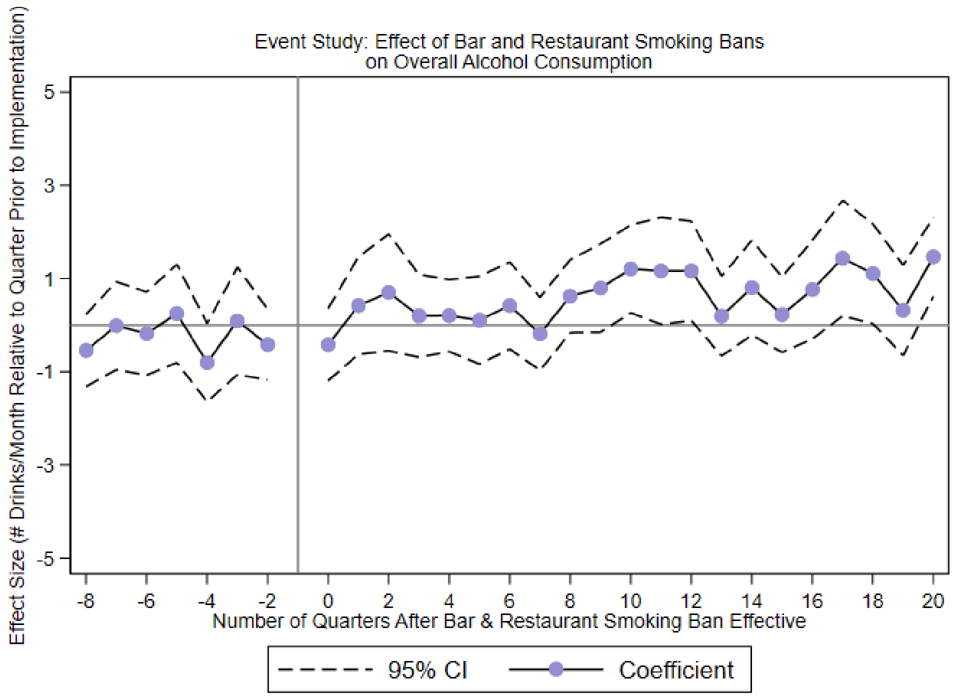
Data Source: American Nonsmokers' Rights Foundation

Figure 1.3: Map of Smoking Bans in Bars Implemented by Counties and States by December 31, 2012



Data Source: American Nonsmokers' Rights Foundation

Figure 1.4



Note: Results from the estimation specified in Equation 1.4. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.5

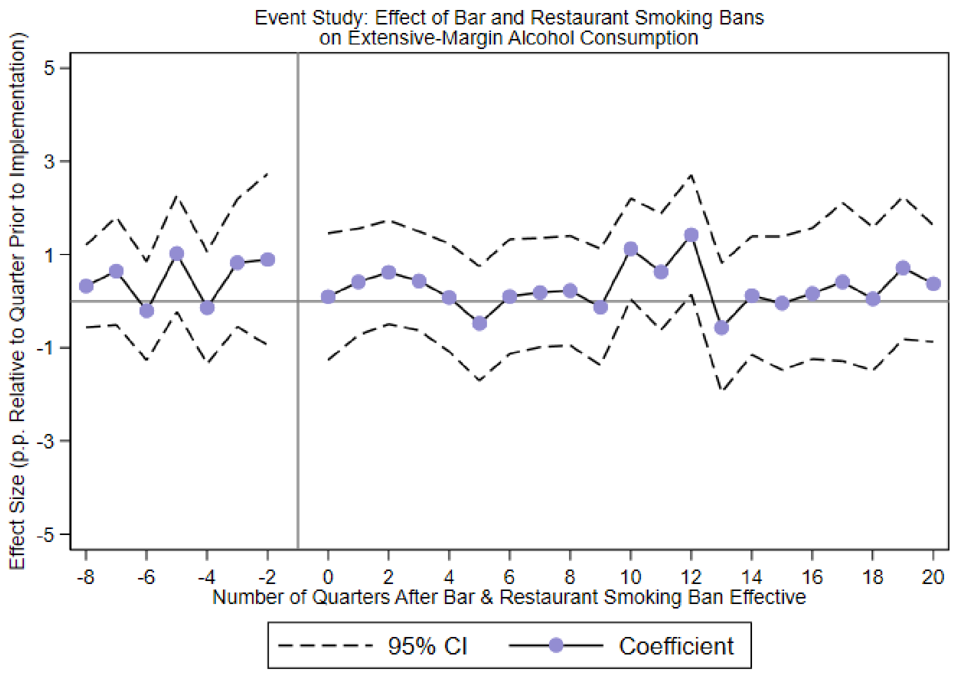


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Figure 1.6

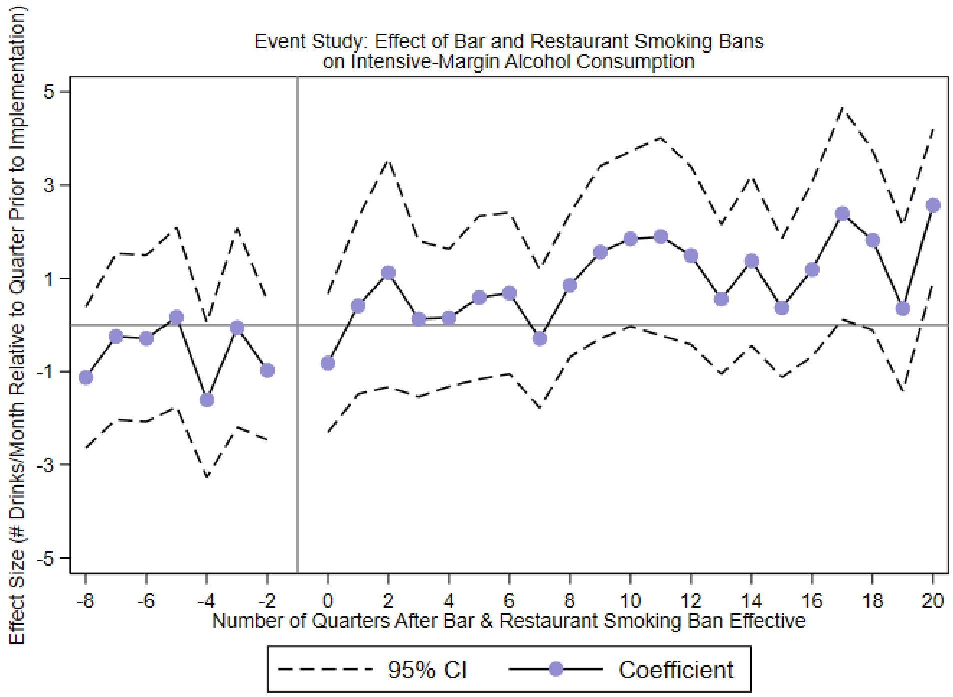

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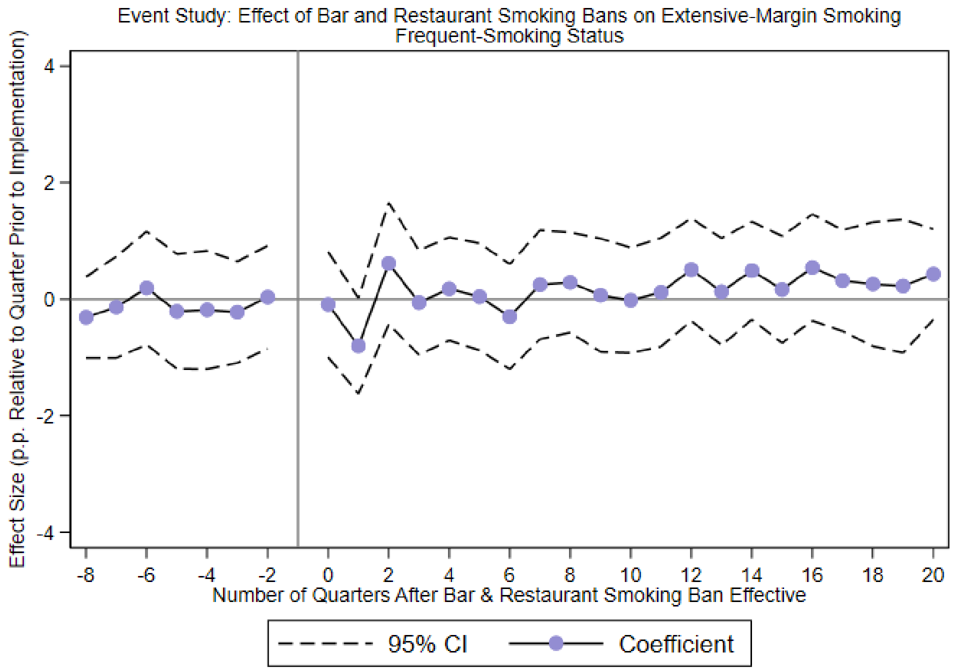
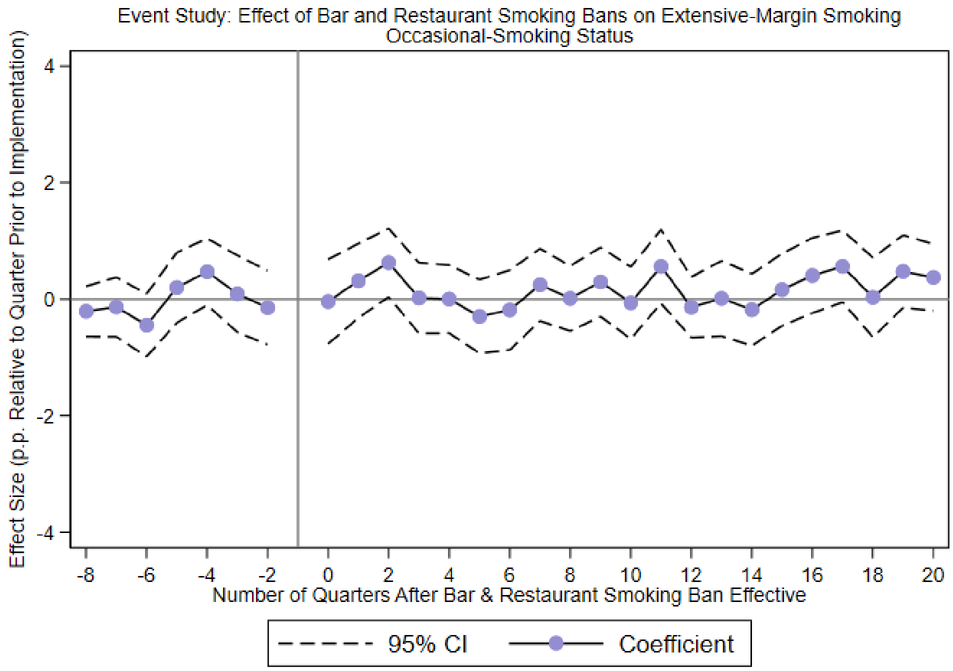
Figure 1.7



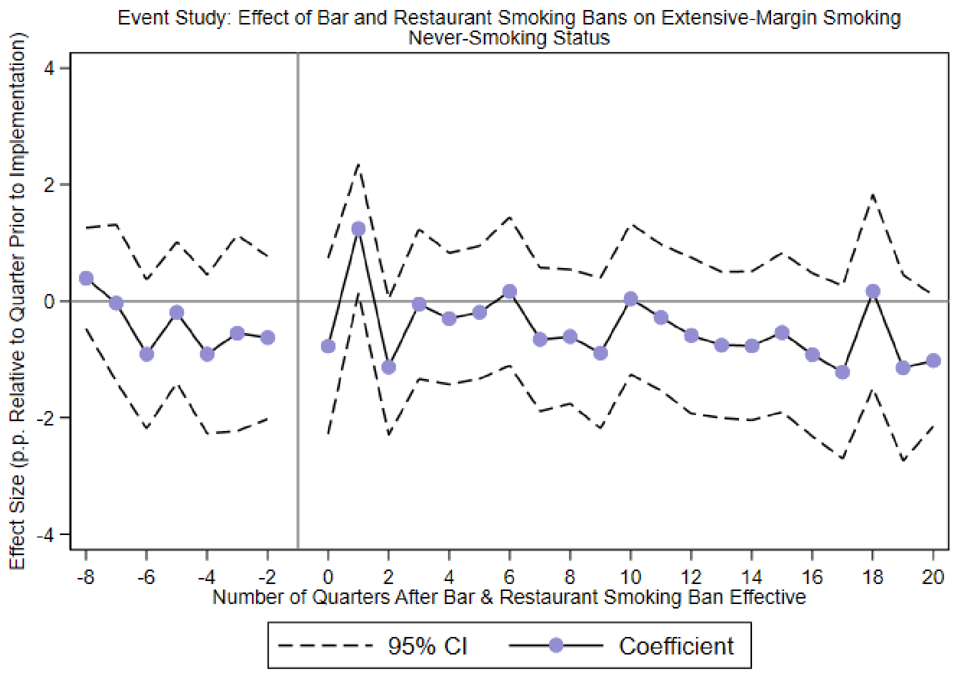
Note: Results from the estimation specified in Equation 1.4. Sample restricted to frequent smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.8



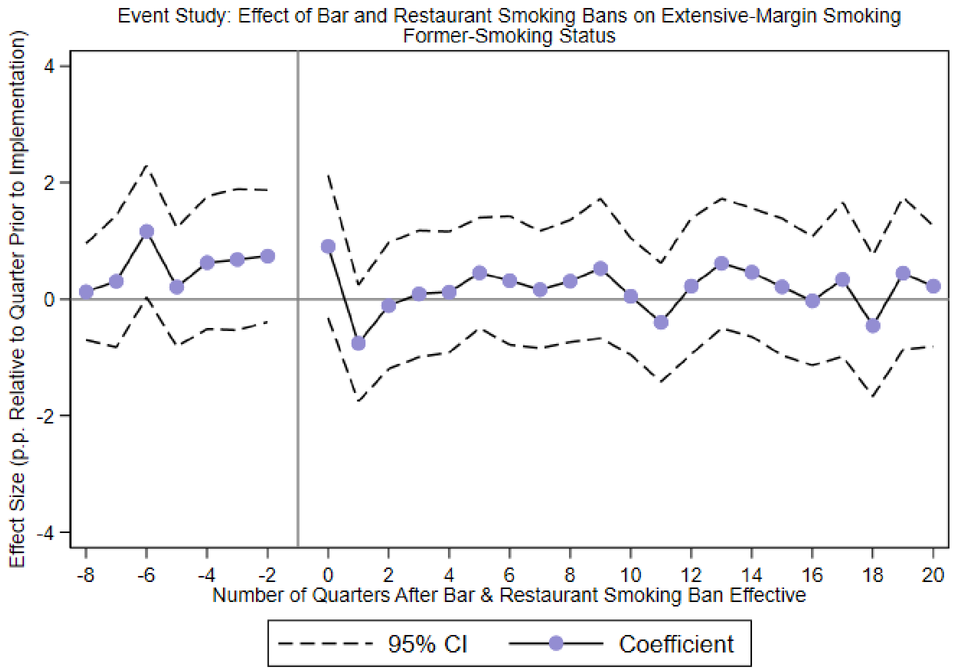
Note: Results from the estimation specified in Equation 1.4. Sample restricted to occasional smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.9



Note: Results from the estimation specified in Equation 1.4. Sample restricted to never smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.10

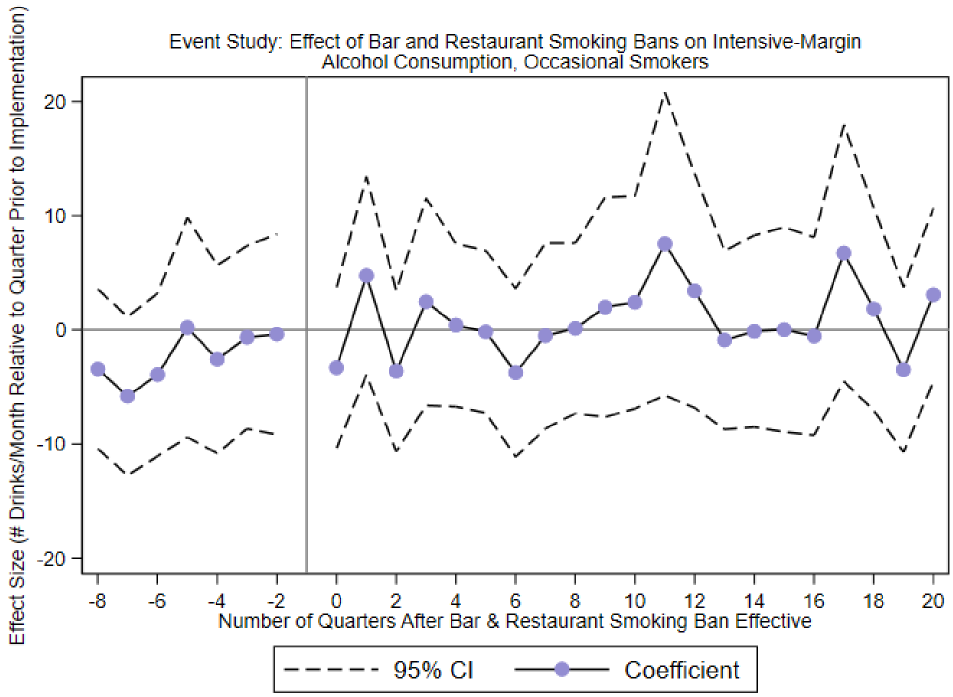


Note: Results from the estimation specified in Equation 1.4. Sample restricted to former smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.11

Note: Results from the estimation specified in Equation 1.4. Sample restricted to frequent smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.12



Note: Results from the estimation specified in Equation 1.4. Sample restricted to occasional smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.13

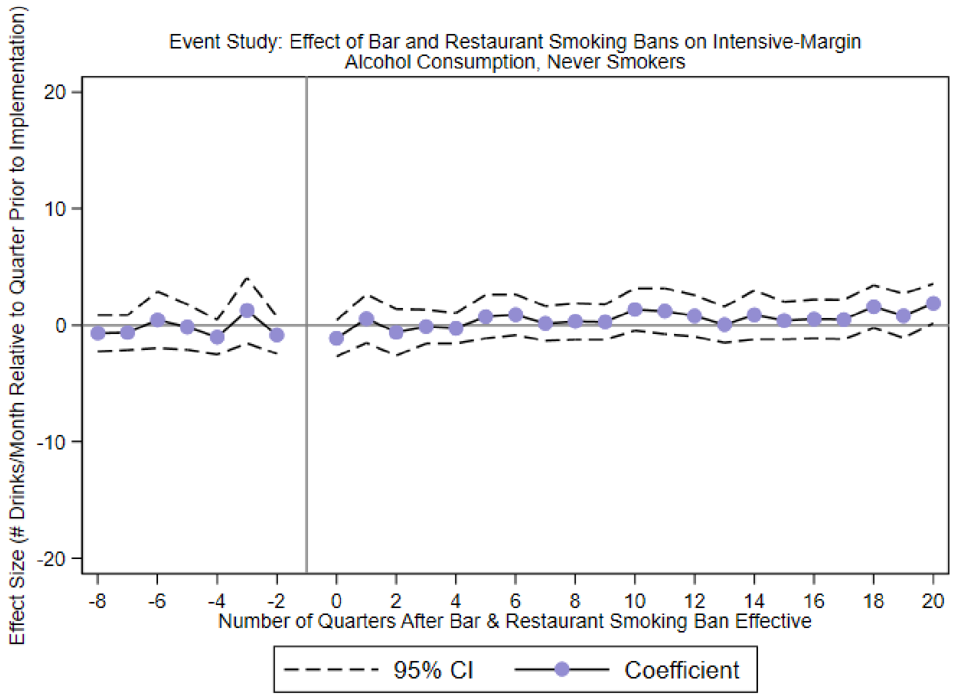


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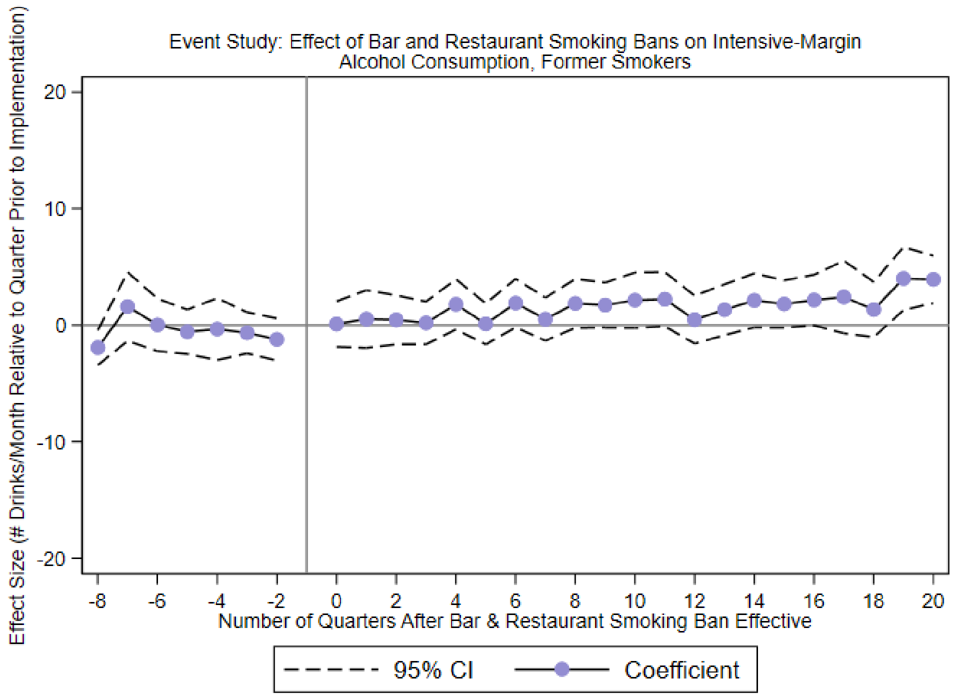
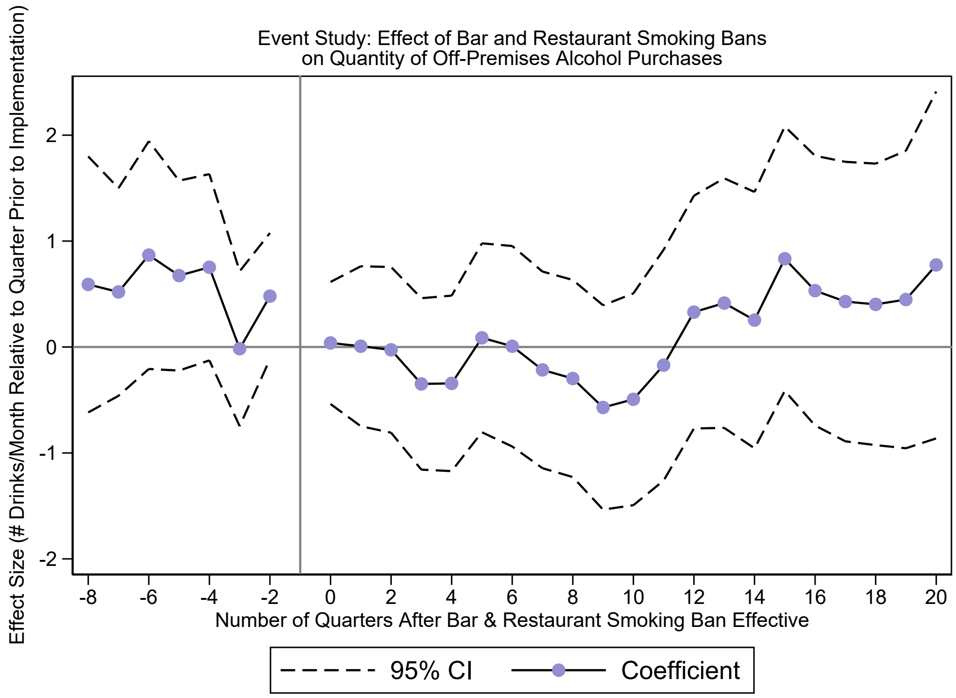
Figure 1.14



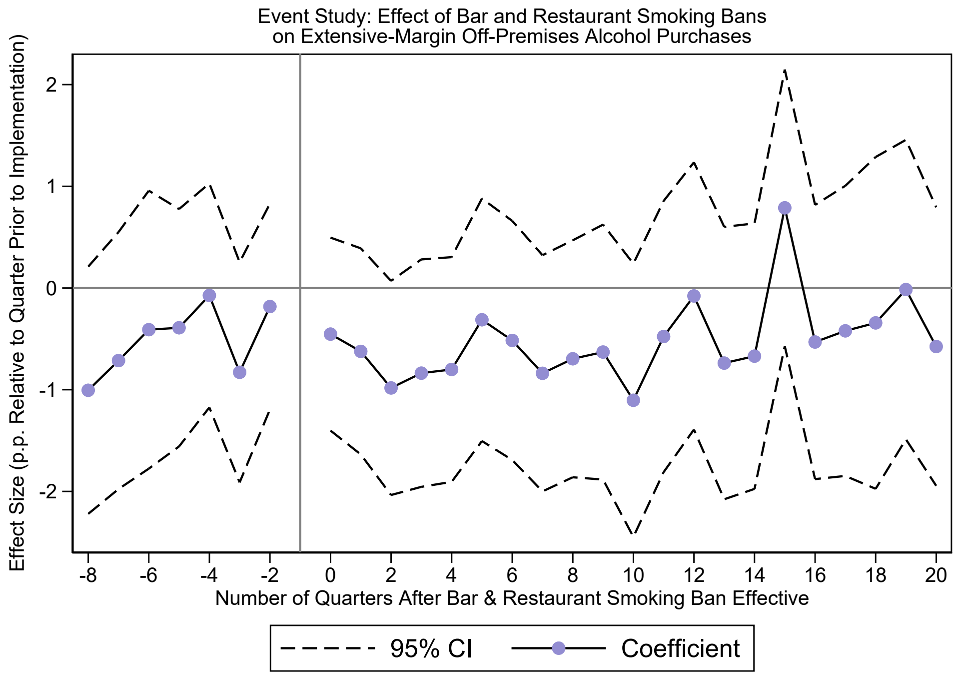
Note: Results from the estimation specified in Equation 1.4. Sample restricted to former smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure 1.15



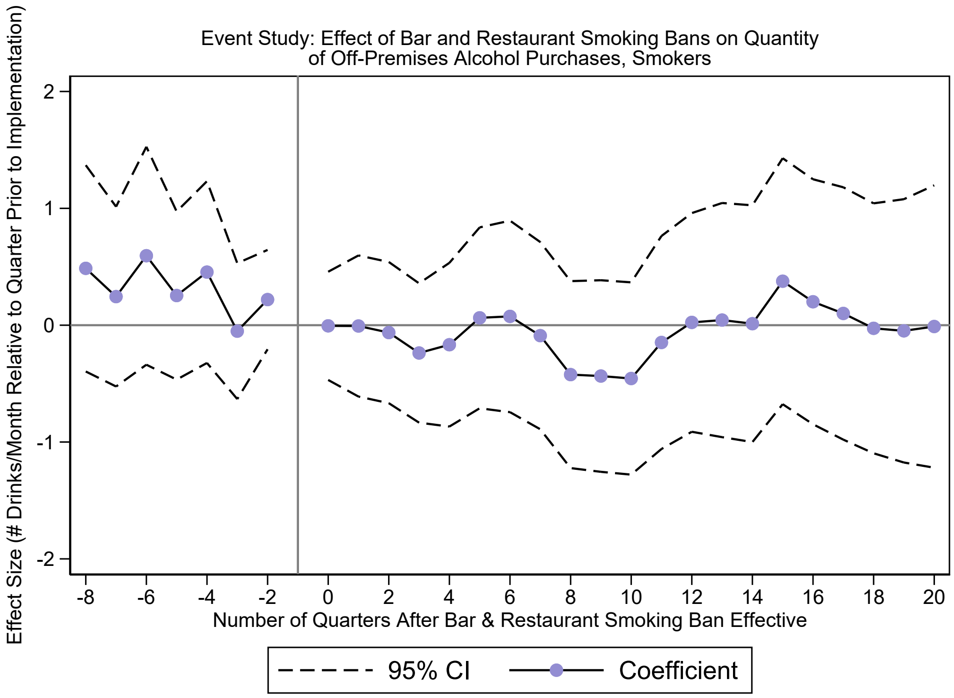
Note: Results from the estimation specified in Equation 1.4. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure 1.16



Note: Results from the estimation specified in Equation 1.4. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure 1.17

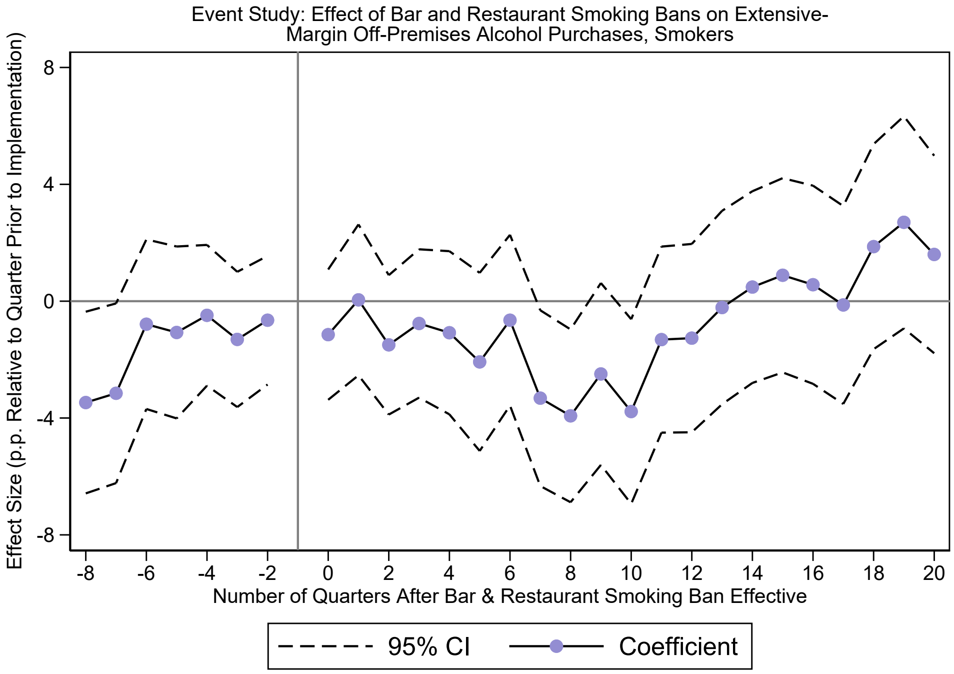


Note: Results from the estimation specified in Equation 1.4. Sample restricted to smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure 1.18

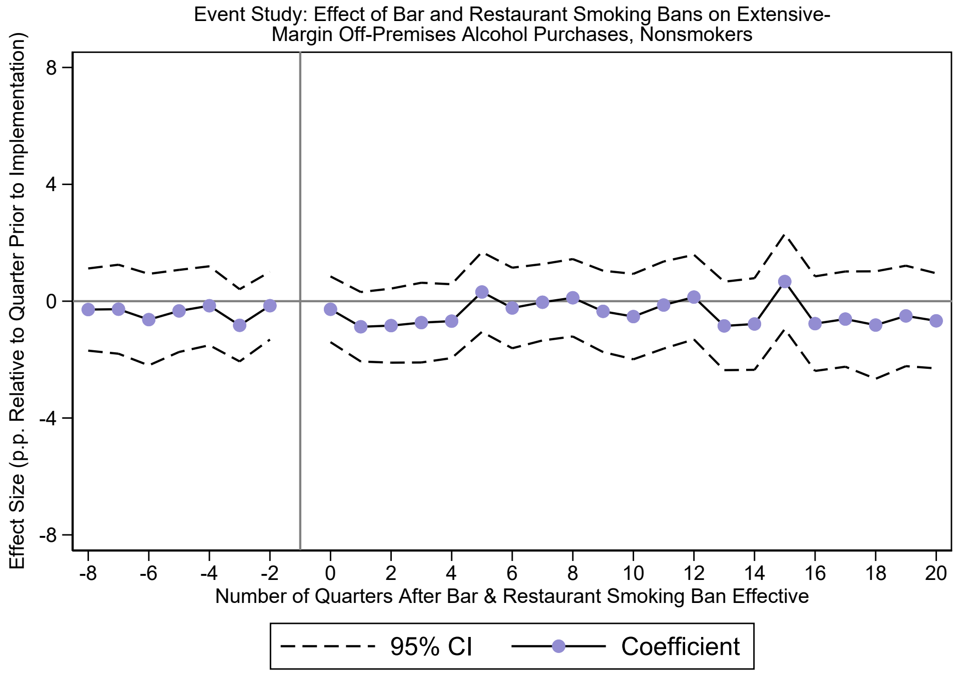
Note: Results from the estimation specified in Equation 1.4. Sample restricted to nonsmokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure 1.19



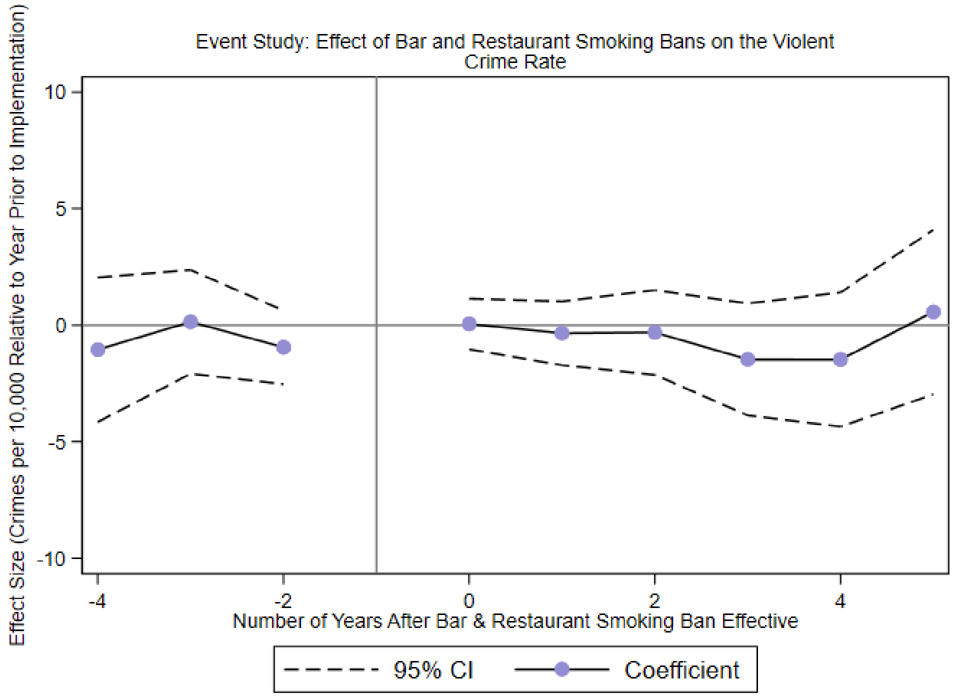
Note: Results from the estimation specified in Equation 1.4. Sample restricted to smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure 1.20



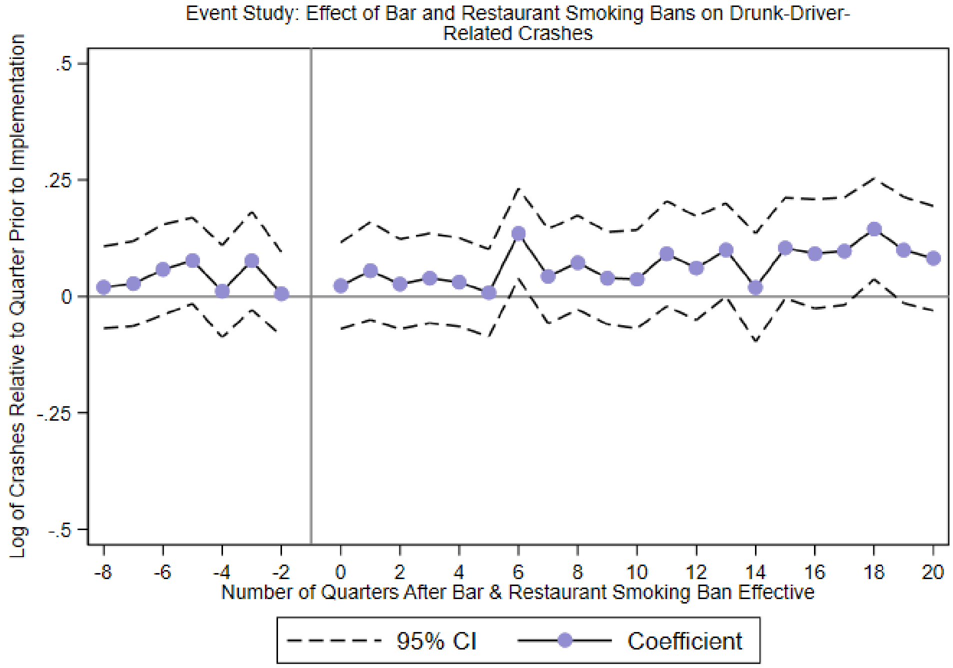
Note: Results from the estimation specified in Equation 1.4. Sample restricted to nonsmokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure 1.21



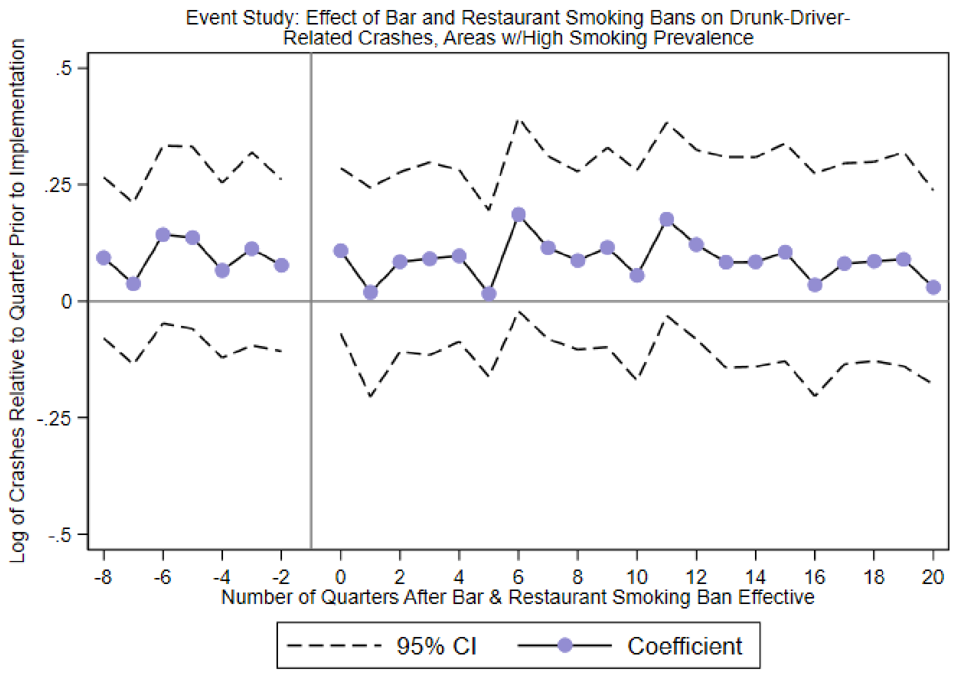
Note: Results from the estimation specified in Equation 1.6. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include agency and year fixed effects. Treatment is defined as whether the agency's jurisdiction is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the agency level. Regressions are probability weighted using the agency population. Data source: UCR 2004-2012.

Figure 1.22



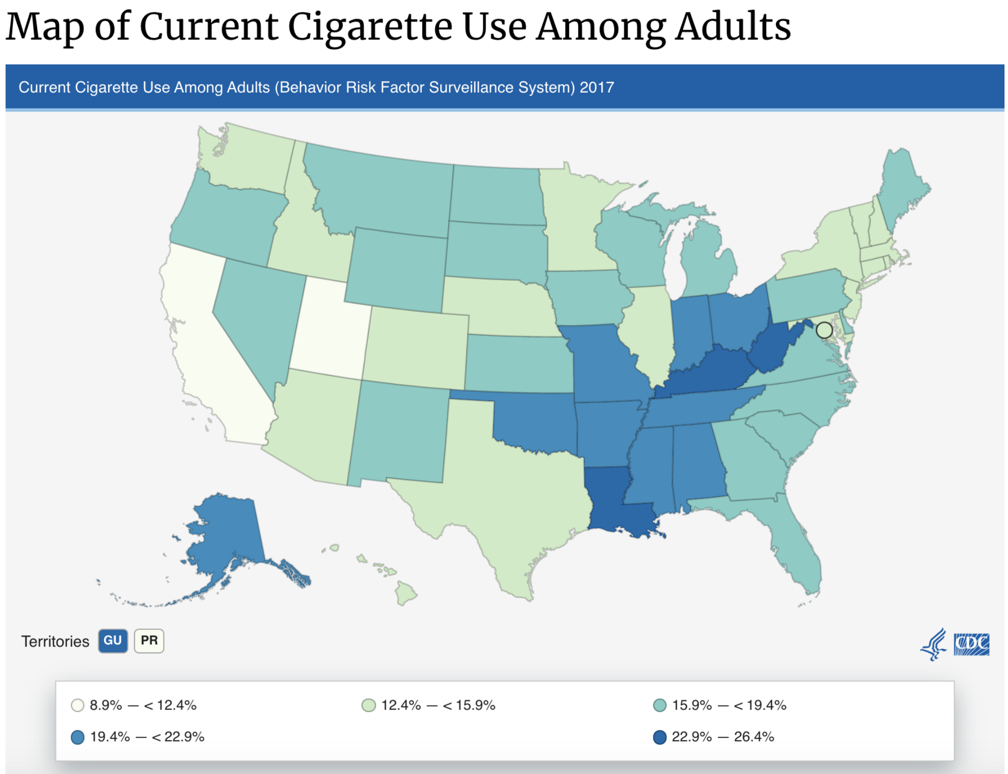
Note: Results from the estimation specified in Equation 1.8. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: FARS 2004-2012.

Figure 1.23



Note: Results from the estimation specified in Equation 1.8. Sample restricted to counties with a high prevalence of smoking. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: FARS 2004-2012.

Figure 1.24



Source: Centers for Disease Control, State Tobacco Activities Tracking and Evaluation (STATE) System

Table 1.1: Summary Statistics of Alcohol Consumption by Smoking Status (BRFSS, Past 30 Days)

Smoking Status	Overall (1)	Smoker (2)	Nonsmoker (3)	(2) - (3) (4)
Overall	12.03	17.34	8.94	8.39***
# Drinks (per month)				
N	516,064	189,934	326,130	
Extensive Margin	48.22	53.67	45.03	8.64***
percentage pts.				
N	517,610	191,047	326,563	
Intensive Margin	24.08	32.68	19.41	13.27***
# Drinks (per month) | Drinking				
N	375,055	132,034	243,021	

Note: Column (4) represents the alcohol-related outcome for smokers minus the alcohol-related outcome for non-smokers. *** denotes $p < 0.01$ for a t-test of the difference in means between smokers and nonsmokers (assuming unequal variances). Data source: BRFSS 2004-2012.

Table 1.2: Effect of Bar and Restaurant Smoking Bans on Alcohol Consumption (BRFSS)

	Overall (1)	Extensive Margin (2)	Intensive Margin (3)	# Days (4)	Avg. per Day (5)	Max. (6)
Bar/Restaurant Ban	0.52***	-0.20	0.91***	0.06	0.06***	0.08***
(standard error)	(0.18)	(0.27)	(0.31)	(0.05)	(0.02)	(0.03)
[95% confidence interval]	[0.18, 0.87]	[-0.72, 0.33]	[0.30, 1.52]	[-0.03, 0.16]	[0.01, 0.10]	[0.02, 0.14]
Dep. Var. Mean	11.66	53.19	21.91	8.36	2.41	3.51
% of Mean	4.48%	-0.37%	4.17%	0.73%	2.31%	2.25%
R^2	0.04	0.26	0.03	0.09	0.05	0.05
N	189,660	189,791	161,421	162,125	161,824	148,054

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

Note: Results from the estimation specified in Equation 1.1. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table 1.3: Effect of Bar and Restaurant Smoking Bans on Alcohol Purchases (Nielsen)

	Total Quantity (1)	Extensive Margin (2)
Bar & Restaurant Ban	-0.35**	-0.30
(standard error)	(0.15)	(0.30)
[95% confidence interval]	[-0.65, -0.06]	[-0.88, 0.29]
Dep. Var. Mean	5.33	25.78
% of Mean	-6.61%	-1.15%
R^2	0.36	0.40
N	280,632	280,632

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

Note: Results from the estimation specified in Equation 1.1. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Table 1.4: Effect of Bar and Restaurant Smoking Bans on Extensive-Margin Past-Month Smoking (BRFSS)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar and Restaurant Ban	0.13	0.22*	-0.09	-0.26
(standard error)	(0.17)	(0.13)	(0.23)	(0.18)
[95% confidence interval]	[-0.20, 0.45]	[-0.04, 0.48]	[-0.53, 0.36]	[-0.62, 0.10]
Dependent Variable Mean	13.04	5.25	56.60	25.10
% of Mean	0.98%	4.19%	-0.15%	-1.04%
R^2	0.11	0.03	0.12	0.07
N	190,096	190,096	190,096	190,096

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

Note: Results from the estimation specified in Equation 1.3. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population.
Data source: BRFSS 2004-2012.

Table 1.5: Effect of Bar and Restaurant Smoking Bans on Extensive-Margin Past-Month Alcohol Consumption (BRFSS)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar and Restaurant Ban	-0.24	-0.35	0.15	-0.67
(standard error)	(0.70)	(1.17)	(0.32)	(0.44)
[95% confidence interval]	[-1.61, 1.13]	[-2.63, 1.94]	[-0.48, 0.77]	[-1.54, 0.19]
Dependent Variable Mean	58.63	64.74	48.94	57.72
% of Mean	-0.41%	-0.53%	0.30%	-1.16%
R^2	0.07	0.07	0.22	0.16
N	122,221	68,756	174,017	152,539

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

Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table 1.6: Effect of Bar and Restaurant Smoking Bans on Intensive-Margin Past-Month Alcohol Consumption (BRFSS)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar and Restaurant Ban	1.15	2.20**	0.28	1.36***
(standard error)	(1.31)	(1.11)	(0.32)	(0.42)
[95% confidence interval]	[-1.43, 3.72]	[0.02, 4.39]	[-0.35, 0.91]	[0.55, 2.18]
Dependent Variable Mean	35.92	27.88	16.06	22.78
% of Mean	3.19%	7.91%	1.73%	5.99%
R^2	0.03	0.03	0.03	0.03
N	85,645	46,161	129,394	113,598

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

Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table 1.7: Effect of Bar and Restaurant Smoking Bans on Total Quantity of Past-Month Alcohol Purchases (Nielsen)

Smoking Status:	Smoker (1)	Nonsmoker (2)
Bar and Restaurant Ban	-0.20*	-0.14
(standard error)	(0.12)	(0.10)
[95% confidence interval]	[-0.43, 0.03]	[-0.35, 0.06]
Dependent Variable Mean	1.92	3.52
% of Mean	-10.33%	-4.00%
R^2	0.32	0.34
N	198,570	267,973

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

Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Table 1.8: Effect of Bar and Restaurant Smoking Bans on Extensive-Margin Past-Month Alcohol Purchases (Nielsen)

Smoking Status:	Smoker (1)	Nonsmoker (2)
Bar and Restaurant Ban	-0.73	-0.11
(standard error)	(0.87)	(0.34)
[95% confidence interval]	[-2.44, 0.98]	[-0.79, 0.56]
Dependent Variable Mean	31.54	24.35
% of Mean	-2.31%	-0.46%
R^2	0.27	0.37
N	198,570	267,973

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

Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Table 1.9: Effect of Bar and Restaurant Smoking Bans on Violent Crimes per 10,000 People

Crime Type:	Violent (1)	Murder (2)	Rape (3)	Aggravated Assault (4)	Simple Assault (5)
Bar and Restaurant Ban	-0.55	-0.01	0.05	-0.34	-0.82
(standard error)	(0.96)	(0.02)	(0.04)	(0.75)	(1.09)
[95% confidence interval]	[-2.43, 1.33]	[-0.05, 0.03]	[-0.04, 0.13]	[-1.81, 1.12]	[-2.96, 1.32]
Dependent Variable Mean	53.18	0.54	2.89	35.94	98.80
% of Mean	-1.03%	-2.27%	1.67%	-0.95%	0.83%
R^2	0.91	0.84	0.77	0.86	0.94
N	104,766	104,766	104,766	104,766	104,766

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

Note: Results from the estimation specified in Equation 1.5. Policy controls are (1) whether the jurisdiction 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 agency and year fixed effects. Treatment is defined as an indicator for whether the agency's jurisdiction is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the agency level. Regressions are probability weighted using the population for the agency's jurisdiction. Data source: UCR 2004-2012.

Table 1.10: Effect of Bar and Restaurant Smoking Bans on Log of Fatal Drunk Driving Crashes

Smoking Prevalence	All (1)	High Smoking (2)	Medium Smoking (3)	Low Smoking (4)
Bar and Restaurant Ban	-0.00	0.04**	-0.02	-0.01
(standard error)	(0.01)	(0.02)	(0.02)	(0.02)
[95% confidence interval]	[-0.02, 0.02]	[0.01, 0.07]	[-0.05, 0.01]	[-0.04, 0.02]
R^2	0.70	0.70	0.70	0.70
N	339,264	339,264	339,264	339,264

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

Note: Results from the estimation specified in Equation 1.7. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: FARS 2004-2012.

CHAPTER 2

THE DIRECT AND INDIRECT EFFECTS OF BAN THE BOX

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Ban-the-box (BTB) policies prevent employers from asking job applicants about their criminal history until very late in the hiring process. The intent of these policies is to help individuals with criminal records find employment by reducing the stigma associated with arrest or conviction. Some of the existing evidence on BTB, however, indicates that it leads to broad discrimination against young, non-college-educated minority men. We revisit the employment effects of these policies using two different empirical strategies that are new to this literature. Using variation in exposure to BTB across metro areas, we find no evidence of discrimination as a result of BTB and some evidence of positive employment effects for some subgroups of Hispanic men. Under our primary specification, black men are 0.26% more likely to be employed once BTB is in place, Hispanic men are 0.83% more likely to be employed, and white men are 0.02% more likely to be employed. None of these results are statistically significant and they are relatively precisely estimated null effects: our 95% confidence intervals rule out declines in the probability of employment of more than 1 percentage point and increases in the probability of employment of more than 2 percentage points. We also estimate the effect of BTB on labor force participation, and earnings.

2.1 Introduction

In recent years, policies aimed at helping the large formerly incarcerated population reintegrate into society have garnered bipartisan support. One such example are Ban-the-Box (BTB) laws, which require covered firms to remove questions about criminal convictions from job applications and delay background checks until later in the hiring process (see Figure 2.1 for an example of "the box"). These laws are intended to make it easier for individuals with a criminal history to obtain employment, and are becoming increasingly common: as of 2018, 35 states (including the District of Columbia) and more than 150 cities and counties have adopted BTB or similar "fair-chance" policies, covering 75 percent of the U.S. population (Avery, 2019).

BTB delays the acquisition of information about job applicants until late in the hiring process. Economists have long argued that removing information available to employers can lead them to discriminate along other dimensions, such as race (Aigner and Cain, 1977). This pattern of discrimination has been found with other types of information on job applicants, such as job-testing (Autor and Scarborough, 2008), drug testing (Wozniak, 2015), and credit checks (Bartik and Nelson, 2020). There is also some evidence in the BTB literature that indicates this policy leads to discrimination. For example, Agan and Starr (2017) conduct a résumé audit study following passage of BTB in New York City and New Jersey and find that the black-white gap in callbacks increased six-fold after BTB. Doleac and Hansen (2020) estimate a difference-in-differences model using variation in the timing of adoption of BTB and find that young, non-college-educated black men are 5.1% less likely to be employed.[1] In contrast, Craigie (2020) estimates a triple-difference model including self-reported criminal history and finds large increases in public-sector employment for ex-offenders and no evidence of discrimination. Similarly, Rose (2021) finds no effect on employment for ex-offenders of any race using administrative data from Washington state, which

[1]This point estimate is from Doleac and Hansen (2020)'s preferred sample from the Current Population Survey. Our primary results use the American Community Survey, for which Doleac and Hansen (2020) estimate an effect of -2.4%.

implies the absence of so-called “statistical discrimination.”

We make several contributions to the broader literature on removing information available to employers and on BTB in particular. First, we revisit the estimates of Doleac and Hansen (2020) after correcting for coding errors in that analysis which separately affected both when certain places were coded as being treated and the estimates for white workers. These coding errors were not intentional but meaningfully affect the statistical and economic significance of the results in Doleac and Hansen (2020). Second, we use two additional empirical strategies that are new to the literature on BTB. Specifically, in light of a growing literature on the potential pitfalls of difference-in-differences estimates with staggered policy adoption, we use the estimator proposed by Callaway and Sant’Anna (2020), which allows for arbitrarily heterogeneous and dynamic treatment effects. In another empirical strategy, we exploit variation in treatment within labor markets by focusing on metro areas that cross state borders and therefore legal jurisdictions.

We find no evidence of discrimination following implementation of BTB. Using the same sample from the American Community Survey (ACS) and specification as Doleac and Hansen (2020, DH) and after accounting for the coding errors, we find that their preferred estimate of the likelihood of employment for young black men without a four-year college degree is +0.03 percentage points (p.p.) or 0.06% of the pre-BTB mean, which is not statistically significant, and not a marginally statistically significant -1.28 p.p. (or -2.43%). Meanwhile, the point estimate for white men changes sign and becomes -0.46 p.p. or -0.59%, compared to 0.30 p.p. (0.38%). The corrected estimate for Hispanic workers is attenuated at 0.81 p.p. (1.14%) relative to their earlier estimate of 1.55 p.p. (2.16%). Neither their original estimates for Hispanic and white men nor the corrected estimates are statistically significant.

In our preferred estimates, we use ACS data from 2005 to 2014 to compare metropolitan statistical areas (MSAs) that adopt BTB with those that have yet to or never adopt BTB. For each year, we consider an MSA to be treated for the fraction of the year (measured in months) a central city in the MSA is treated by a city, county, or state BTB policy.[2] We estimate that BTB leads to a 0.15 p.p. increase in the probability of employment for black (non-Hispanic) men ages 25-34 without a college degree (two-year or four-year). This effect corresponds to 0.26% of the pre-BTB mean and is neither statistically nor economically significant. Observably similar Hispanic men experience an increase in the probability of employment of 0.61 p.p. or 0.83%, which is not statistically significant. Meanwhile, white men experience a statistically insignificant increase in employment of 0.02 p.p. or 0.02% of the mean.

How do these results fit with the existing literature on employer discrimination? We argue that there are several possible explanations for our findings. First, the results from our preferred across-MSAs specification have a similar interpretation as the corrected Doleac and Hansen (2020) estimates: BTB policies do not have meaningful effects on the probability of employment for young black, Hispanic, and white men without a college degree. Second, delaying criminal background checks is fundamentally different from eliminating job-testing, drug tests, or credit checks because an applicant's criminal history is a matter of public record, unlike these other examples of screening mechanisms. In other words, employers can comply with BTB laws by removing questions about criminal records from job applications but still screen applicants based on this attribute by searching through public records.[3] They can also conduct background checks later on in the hiring process, which would delay but not eliminate the revelation of this information. Third, as pointed out by Rose (2021), applicants with a criminal history might direct their search to jobs or employers that they know do not require a clean record. Finally, the absence of a criminal record can also be credibly signaled by applicants without gaps in their employment history or recent employment in an occupation that requires a clean record.

The rest of the paper is structured as follows. Section 2.2 lays out our conceptual framework

[2] A central city is a city in the name of the MSA; e.g., the central cities for the Washington-Arlington-Alexandria, DC-VA-MD-WV MSA are Washington, D.C.; Arlington, VA; and Alexandria, VA.

[3] While this would mean that these employers are violating the law, we note that so too would employers who choose to screen applicants based on race.

and Section 3.3 describes our sample. Section 2.4 describes our empirical strategies and results for revisiting the effect of BTB on employment and earnings. In Section 2.5 we conduct robustness checks for the across-MSA specification. Section 2.6 includes our empirical strategy and results for the within-MSA specification. In Section 3.6 we discuss the policy implications of our findings and conclude.

2.2 Conceptual Framework, Institutions, and Related Literature

Ban-the-Box policies relate to important questions in microeconomics about the desirability of more information, signaling, and the optimal timing of revealing information. Economists often believe that more information is better because full information improves decision-making. However, if that information is measured with error, or if it is not incorporated appropriately into the decision-making process, it is not clear ex ante whether more information is better.

With respect to information about criminal history, some employers may not want to hire employees with a criminal history under any circumstances, but other employers may be hesitant because they are not concerned about criminal history per se, but about an employee's productivity or trustworthiness. In the latter case, employers may be using criminal history as a proxy for productivity and trustworthiness. Criminal history is not a perfectly informative signal of these characteristics, however, and if employers treat criminal history as perfectly informative, they may be making sub-optimal hiring decisions.

Ban-the-Box policies are intended to make it easier for individuals with a criminal history to find a job by preventing employers from discriminating on the basis of criminal history in the initial hiring process. By delaying information about criminal history, employees may be able to send more informative signals about productivity and trustworthiness, such as through an interview.

However, these policies work by removing information that is readily available to employers on applications. This type of action often leads employers to resort to discrimination based on observable characteristics that they perceive to by correlated with the missing information (Aigner and Cain, 1977). In the context of BTB and other policies intended to make the employment process more equitable, it is argued that employers will use race and ethnicity to discriminate against applicants. Previous research on these types of interventions has found negative effects for minority workers (Autor and Scarborough, 2008; Wozniak, 2015; Bartik and Nelson, 2020).

We argue that there are several potential reasons why BTB might not lead to broad discrimination against minority applicants. First, criminal records are quasi-public information, depending on the state or locality of the offense. This makes limiting information about criminal history on job applications different from limiting information about drug tests (Wozniak, 2015) or credit checks (Bartik and Nelson, 2020), for example, because that information is inherently private. An employer who is motivated enough to screen out applicants with a criminal record can do so by evading the law and accessing public records with a search engine. The same is not true for an employer motivated to eliminate drug users from their applicant pool. Employers can also legally obtain this information later on in the hiring process. BTB delays the reveal of information about applicants' criminal histories rather than completely eliminating it.

Second, applicants without a criminal history can plausibly signal to employers that they have a clean record. This can be done by showing no gaps in their employment history or recent employment in a job that requires a clean record. Finally, applicants with a criminal history can direct their search to job openings or employers that they know do not require a clean record. This point was first made by Rose (2021). Taken together, these arguments suggest that BTB might not *a priori* lead to substantial amounts of discrimination, and might even have a marginally positive effect for its intended beneficiaries.

Ban-the-Box policies may also affect earnings, migration, and spillovers into non-BTB jurisdictions. In terms of potential spillovers, we expect BTB to lead to a positive earnings effect if it creates a credible outside option for workers in non-BTB jurisdictions. And even if BTB policies do not perfectly meet their intended purposes, they signal less discrimination, in general, where they exist. Such a signal might attract minority workers residing elsewhere to begin searching for jobs in these areas. This change in search behavior could lead to higher wages for these workers because employers where they currently reside now must bid higher in order to retain or attract them. This positive earnings effect is predicted in models like Black (1995), in which discrimination by some employers gives monopsony power to non-discriminating employers because their minority workers have fewer outside options.

While BTB policies do not force employers to hire workers with a criminal record, they are intended to improve the probability of matching with *all* employers by helping affected workers get past a first hiring screen. As discussed above, they might also signal less discrimination in general. In the context of Black (1995), BTB induces search by workers at firms that had previously discriminated against them, thereby lowering their search costs. The model predicts that these lower search costs would translate into higher wages by reducing the monopsony power of the non-discriminating firms. Now consider an employed minority worker living in a non-BTB area that borders a BTB jurisdiction. His reservation wage increases once BTB is implemented because he can now search for jobs in both the non-BTB and the BTB jurisdiction, and in the latter jurisdiction, potential employers cannot immediately know whether he has a criminal history.[4] Employers in the non-BTB area would then have to bid higher in order to recruit or retain these workers. For now, though, we limit our attention to the effect of BTB on the probability of employment.[5]

One might object to this prediction by noting that some research finds discrimination increases

[4]We use "he" as we restrict our analysis to the labor market effects of BTB on men, who disproportionately comprise the formerly incarcerated population. In 2017, only 7% of prisoners were women (Bronson and Carson, 2019).

[5]Another potential employer response to BTB laws could be to relocate to the non-BTB area that borders the BTB jurisdiction, in order to continue asking about criminal history on the initial job application. If employers are responding to BTB policies in this way, we view that response as a mechanism by which BTB laws might affect employment.

following the implementation of BTB (Agan and Starr, 2017; Doleac and Hansen, 2020). Employers could anticipate such discrimination and not consider the employers covered by BTB as a credible outside option for the worker. However, it could also be the case that outside employers view the fact that the worker is currently employed as a sufficiently strong positive signal of his productivity or trustworthiness that overcomes any preference for discrimination.

2.3 Data

We use data from the American Community Survey (ACS) from 2004 to 2014 (Ruggles et al. 2020). Our main sample is similar to Doleac and Hansen (2020): 25-34 year old men without a college degree of any type (Associate or Bachelor) who are U.S. citizens.[6] We limit our attention to men that are either white non-Hispanic, black non-Hispanic, or Hispanic. We focus on BTB laws implemented between 2005 and 2014. Data on BTB laws come from Table 1 of Doleac and Hansen (2020), Avery and Lu (2020), local government websites, law firm websites, and news articles. Figure 2.2 maps the jurisdictions with BTB policies in place during our sample period.

Our definition of a treated MSA depends on the empirical strategy. For the across-MSAs design, we categorize a local labor market as treated for the fraction of the year (measured in months) BTB is in place for any central city in the MSA.[7,8,9,10] For example, for the Washington-Arlington-Alexandria, DC-VA-MD-WV MSA, when Washington, D.C. implements a ban-the-box

[6] In contrast, the DH sample consists of men ages 25-34 with at most an Associate degree who are U.S. citizens.

[7] Central cities are the cities that comprise the name of the MSA, e.g. Washington, D.C.; Arlington, VA; and Alexandria, VA for the Washington-Arlington-Alexandria, DC-VA-MD-WV MSA.

[8] We consider a month to be treated if BTB is in place on the 15th of the month.

[9] For one jurisdiction (New Haven, Connecticut) we could not find an effective date of their BTB law, which was enacted on February 17, 2009. Given that most jurisdictions' laws have a lag between enactment and implementation, we assumed that the New Haven law took effect one month after passage, which according to our definition of treatment would mean the New Haven, CT MSA was treated starting in April 2009.

[10] In robustness checks, we use alternative definitions of treatment based on the MSA being treated for the entire year, half the year, or any part of the year. We also estimate alternative specifications where the labor market is treated once any part of the MSA implements BTB, as in Doleac and Hansen (2020).

policy in 2011, we consider the entire MSA to be treated (Figure 2.6). For our within-MSAs design, we categorize the MSA-state unit as treated for the fraction of the year a central city (or the entire state) in the MSA-state has implemented BTB.[11,12] For example, for the Chicago-Naperville-Elgin, IL-IN-WI MSA, when the city of Chicago, Illinois implements a ban-the-box policy in 2007, we consider the Illinois portion of the Chicago-Naperville-Elgin, IL-IN-WI MSA to be treated, but not the Indiana or Wisconsin portions (Figure 2.7).

We assume that Metropolitan Statistical Areas (MSAs) are local labor markets. This assumption means that the geography at which labor market outcomes and the treatment variable are measured are not aligned. Cities, counties, and states are all legal jurisdictions that have implemented BTB policies. However, MSAs in the microdata are collections of Public Use Microdata Areas (PUMAs), which follow boundaries defined by groups of counties, individual counties, or Census "places" (Ruggles et al. 2020). Therefore, the level at which treatment is assigned (city, county, or state) does not exactly line up with the level at which we can observe labor market outcomes. This geographic mismatch introduces measurement error in the treatment variable. While this measurement error constitutes a limitation of our paper, every paper that analyzes policies whose jurisdictions do not neatly line up with how economic activity is organized has this same limitation. In the Chicago example, people working in the Illinois suburbs of Chicago will not be covered by a city-of-Chicago BTB policy, but we are considering them as treated. Unfortunately, there is no way to identify which individuals live (or work) in the city of Chicago as opposed to a Chicago suburb in the ACS. However, this source of measurement error is not likely to be as large of a concern as it might appear at first glance. Many people who live in the suburbs of Chicago work in the city. These people would be correctly coded as treated. This geographic mismatch

[11] Some MSA-state units do not have a central city of the MSA. There are a few instances of MSA-state units without a central city being covered by a BTB law. In these cases, we consider an MSA-state unit without a central city to be treated if the state implements a BTB law. Coincidentally, in every instance a state implemented a BTB law before any local jurisdiction, so our definition is equivalent to using the first BTB law in that MSA-state unit.

[12] We again estimate alternative specifications where treatment is defined based on BTB being effective in the MSA-state for the entire year, half the year, or any part of the year. We also estimate alternative specifications where the labor market is treated once any part of the MSA-state implements BTB.

between legal jurisdictions and labor markets partially motivates our primary treatment measure as being when one of the central cities in the MSA (or the state) implements a BTB law. Many people commute to work in a central city, so when a central city implements a law, a substantial portion of the MSA's population will be treated. Under our preferred across-MSAs definition, when we consider an MSA-year to be treated, in 2014, on average, 73.7% of the MSA's population lived in a legal jurisdiction with a BTB policy.[13] Under our preferred within-MSAs definition, when we consider an MSA-state-year to be treated, in 2014, on average, 78.0% of the MSA-state's population lived in a legal jurisdiction with a BTB policy.[14] These percentages are lower bounds for the percent of workers covered by BTB laws as again, many workers who do not live in a central city commute to a central city for work.

Summary statistics for men ages 25-34 (both for the full sample and those living in MSAs) are shown in Table 2.1. Following the literature on BTB, we limit our attention to white (non-Hispanic), black (non-Hispanic), and Hispanic men. Overall, 21% of this sample is covered by a BTB policy at some point from 2004-2014, and half (50%) live in a place that ever implements a BTB policy. 26% of the sample has at most a high school degree, while 5% has a GED, 24% has some college education but no degree, 8% has an Associate degree, and 29% has a Bachelor's degree. These men live throughout the country, but are primarily located in the South. Among those living in places that ever implement BTB, 80% of them are employed at some point during the sample period. This overall employment rate is slightly higher than for those living in places that never implement BTB (78%) and very similar to that before BTB goes into effect (82%). This group also has relatively low average annual earnings of $38,922. Among the young men that live in MSAs, 27% are covered by a BTB policy at some point and nearly two-thirds (64%) live in MSAs that ever implement such a policy. The demographic characteristics of men living in MSAs are similar to those in the overall sample, though they are slightly more likely to be Hispanic, have

[13] Using the "any law" definition, on average, 71.4% of the MSA's population lived in a legal jurisdiction with a BTB policy.

[14] Using the "any law" definition, in 2014, on average, 77.5% of the MSA-state's population lived in a legal jurisdiction with a BTB policy.

a Bachelor's degree, or live in the Northeast or the West, and less likely to be white or live in the Midwest or the South.

In Table 2.2, we compare the characteristics of these young men separately by race/ethnicity groups and by whether they live in a place that ever adopts BTB. There are important differences both across places and groups. Black and white men are less likely to have at most a high school diploma in BTB-adopting areas (35% for black men, 18% for white men) compared to those in areas that never adopt BTB (42% and 26% respectively). A similar difference across places does not hold for Hispanic men (37% in BTB-adopting areas and 38% in non-adopting areas). For each group, men living in BTB-adopting areas are much more likely to hold a Bachelors degree. However, this gap is quite a bit wider for white men (15 p.p.) than for black (7 p.p.) or Hispanic men (3 p.p.). The geographic distribution of these men is also meaningfully different across groups. Black, Hispanic, and white men in BTB-adopting areas are much more likely to live in an MSA. For black men, 97% who live in a BTB-adopting area live in an MSA while only 56% of black men living in a non-BTB-adopting area live in an MSA. For white men, 91% of those who live in a BTB-adopting area live in an MSA, while only 49% of those living in a non-BTB-adopting area live in an MSA. The gap is much smaller for Hispanic men (96% for BTB-adopting areas and 77% for non-BTB-adopting areas). There are also racial differences in Census region of residence. White men potentially exposed to BTB policies are dispersed relatively evenly across the country. Black men living in BTB-adopting areas are less likely to live in the West and more likely to live in the South. Hispanic men living in BTB-adopting areas are much more likely to live in the West.

A crucial component of our empirical strategy is the fact that BTB was not implemented at the same time in each place that has ever adopted the policy. Figure 2.3 shows, for each year, the number of MSAs that are newly covered by a BTB policy in that year. Most of the policies in our sample are first implemented in in 2010 and later, though some are treated for several years prior. We also show the cumulative number of MSAs and the fraction of the population treated over time in Figure 2.4. The fraction of the population covered by a BTB policy slowly increases through 2009, when about 12% of the population is affected; after 2009 there is a rapid increase in the affected population. By the end of the sample period approximately half of the population lives in an MSA that is at least partially covered by a BTB policy.

Finally, in Figure 2.5, we show the evolution of our main outcome of interest over time, separately for each group. This figure shows the time series of unadjusted employment rates for men ages 25-34 without a college degree (Associate or Bachelor) living in MSAs that either ever or never implement BTB. For each race/ethnicity group, employment rates decline for these men over the course of our sample and do not fully recover regardless of where they live, primarily because of the Great Recession. It is also worth pointing out the substantial differences in the level of employment across groups. In places that ever implement BTB, white men have an average employment rate of 85% over the sample period. Meanwhile, black men have an employment rate of 60%. The Hispanic men in our sample are employed at a somewhat higher rate of 75%, though still much lower than that of white men. Finally, foreshadowing our main findings, we note that the trends in employment rates across MSAs ever treated by BTB and never treated by BTB are very similar over time for each group.

2.4 Revisiting the Effects of BTB

2.4.1 Replicating Doleac and Hansen's Empirical Strategy

We begin our empirical analysis by analyzing the effect of BTB on labor market outcomes in metro areas which may be partially or entirely covered by these policies. In particular, we revisit estimates in the literature on the effect of BTB on the probability of employment for young men without a Bachelor's degree. We also provide new estimates of employment, labor force participation, and earnings effects that are informed by a growing literature on the econometrics of two-way fixed effect estimation strategies used to study outcomes caused by the staggered adoption of a given policy.

Our analysis begins by replicating results in Doleac and Hansen (2020) (DH) using data from the ACS. This empirical strategy assumes an entire local labor market, which is proxied by a metro area, is treated once BTB is implemented anywhere within the market. Following DH, we estimate regressions of the form:

$$y_i = \alpha+\beta_1 BTB_{m,t}\times Black_i+\beta_2 BTB_{m,t}\times Hispanic_i+\beta_3 BTB_{m,t}\times White_i+\theta X_i+\delta_m+\delta_{tr}+\delta_m\times t+\varepsilon_i \quad (2.1)$$

where y_i is a labor market outcome for worker i living in metro area m in year t. X_i includes fixed effects for the worker's age, fixed effects for his highest level of education, and an indicator for whether he is currently enrolled in school. We also include metro area fixed effects (δ_m), time-by-region fixed effects (δ_{tr}), and metro area-specific linear time trends ($\delta_m \times t$). The error term is given by ε_i, and standard errors are clustered at the state level.

The term $BTB_{m,t}$ indicates when a metro area has been treated by BTB for at least one full calendar year.[15] This treatment indicator is interacted with indicators for whether individual i belongs to each racial/ethnic group. The preferred specification fully interacts the right-hand side of (2.1) with these group indicators. The coefficients of interest are therefore β_1, β_2, and β_3. These coefficients identify the causal effect of BTB on outcomes for workers from each group under three assumptions. The first assumption is that the underlying treatment effect is constant across metro areas and over time. The second assumption is that, if treatment effects are constant, then the trends in outcomes for workers in the untreated metro areas serve as an accurate counterfactual for those in the treated metro areas (parallel trends). The third assumption is that there are no concurrent

[15] In other words, a metro area is considered treated once BTB has been implemented from January 1st through December 31st of a given year.

shocks to workers' outcomes in the treated group, conditional on the control variables. The validity of the first assumption has been the subject of a growing econometric literature [e.g. **? ? ?**]. We discuss the implications of this literature for estimates of BTB below.

2.4.2 Replicating and Correcting Doleac and Hansen's Results

Table 2.3 replicates the DH estimates of the effect of BTB on the probability of employment using data from the ACS. Specifically, Columns 4-7 of Table 2.3 correspond to Table A-13 of DH. With the help of their data and code, we are almost able to exactly match each point estimate and standard error from DH, with any differences limited to the third or fourth decimal place. Columns 1-4 of Table 2.3 use data from the full sample of black, Hispanic, and white men ages 25-34 with at most an Associate degree. Column 5 limits the sample to men living in a metro area, Column 6 shows results for men living in places that ever adopt BTB, and Column 7 limits the sample period to 2008-2014 to account for the fact that the wording of ACS questions about employment changed in 2008.[16]

In the course of replicating these results, we discovered two distinct coding errors made by DH which, together, substantially change their results. We do not believe these errors were intentional and we discuss them in more detail in the Appendix. The first coding error concerns the metro area linear trends. In their preferred specification, DH fully interact the right-hand side of (2.1) with indicators for each race/ethnicity group. However, in their implementation, the metro area linear trends were not interacted with the indicator for being white. This error only affects the estimates for white men.

The second coding error concerns the assignment of treatment status for 36 metro areas. The

[16] Guidance from the Census Bureau indicates that researchers should use caution when comparing data from before and after this change but does not indicate that a comparison is not possible. More information is available at: https://www.census.gov/programs-surveys/acs/guidance/comparing-acs-data/2008.html.

coding errors can be classified into four (sometimes-overlapping) types:

1. MSAs that span multiple states and have different treatement statuses for each MSA-state unit
2. MSAs that implemented a law on January 1 but were not coded as treated until the next year
3. MSAs that were coded as treated using a later law instead of the first law
4. MSAs that were otherwise incorrectly coded as treated or untreated

Table 2.4 presents results that correspond to those in DH but correct for these two coding errors. Comparing estimates from the DH preferred specification (Columns 4-7 in Tables 2.3 and 2.4), we see that each point estimate for black workers is closer to zero after correcting for the coding errors. This attenuation also causes DH's preferred estimate using ACS data (Column 7) to lose both statistical and economic significance and change sign. BTB increases the likelihood of employment for young black men with an Associate degree or less by 0.03 percentage points or 0.06%. Previously, DH estimated that BTB reduces the likelihood of employment for this group by 1.28 percentage points or 2.43%. The point estimates for Hispanic and white workers also change after fixing the coding errors. The effects on Hispanic workers all attenuate and some change sign. There is now a 0.75 percentage point (1.01%) decrease in the probability of employment among Hispanic men when the sample is restricted to MSAs only (Column 5), and a 0.81 percentage point increase in the probability of employment using their preferred specification (1.14%). None of these estimates are statistically or economically significant. For white workers, the sign of each point estimate also becomes negative and becomes larger in magnitude but they are neither statistically nor economically significant. The largest (in magnitude) effect for white men is -0.52 percentage points (-0.64%) when the sample is restricted to MSAs (Column 5).

These corrected results imply that BTB does not cause discrimination against young black and Hispanic men without a four-year college degree when looking at annual data on employment.

2.4.3 Across-MSAs Design and Results

Our preferred approach using difference-in-differences to estimate the effect of BTB on the entire metro area is intentionally similar to DH. We estimate a similar regression as equation (2.1) but with some different control variables, variable definitions, and sample restrictions. We use year-by-Census-division fixed effects instead of year-by-Census region fixed effects, as divisions are smaller and MSAs in the same Census division are more comparable than MSAs in the same Census region. We do not include metro-area-specific linear time trends, which is motivated by the raw time series plots in Figure 2.5. The analysis period is relatively short (11 years) and includes the Great Recession, which saw historically severe labor market effects which lasted for an extended period after the "official end" of the recession. A macroeconomic shock should not bias the estimates from a difference-in-differences regression provided that both the treatment and control groups were similarly affected. However, with our empirical strategy, this assumption must also hold for each race/ethnicity group as well. The trends in Figure 2.5 imply that this assumption is likely to hold for young white workers but less likely to hold for young black and Hispanic workers.

Our baseline results also exclude all workers with any college degree, not just those with a four-year college degree as in Doleac and Hansen (2020). This sample change is minor and does not meaningfully impact the results, which we show in our discussion of heterogeneous results by education (Table 2.11). Our definition of treatment also differs slightly: we count an MSA as treated when a central city, county that includes a central city, or state that includes a central city implements BTB, as opposed to any jurisdiction. This change is also minor as there were few cities, counties, or states that did not contain a central city that implemented BTB before a jurisdiction covering a central city did. We also only include individuals who live in MSAs, as we do not view individuals living outside of an MSA as a good counterfactual.

Our primary specification is the following:

$$y_i = \alpha + BTB_{m,t} \times Race_i \times \beta + X_i \times Race_i \times \theta + Race_i \times \delta_m + Race_i \times \delta_{td} + \varepsilon_i \tag{2.2}$$

where y_i is a labor market outcome for worker i living in metro area m in year t. X_i includes fixed effects for the worker's age, fixed effects for his highest level of education, and an indicator for whether he is currently enrolled in school. We also include metro area fixed effects (δ_m), and year-by-division fixed effects (δ_{td}). These control variables are all interacted with indicators for the individual's race and ethnicity (non-Hispanic black, Hispanic, and non-Hispanic white). The error term is given by ε_i, and standard errors are clustered at the state level.

The term $BTB_{m,t}$ indicates the fraction of the year (measured in months) at least one central city in a metro area has been treated by BTB.[17] This treatment indicator is interacted with indicators for whether individual i belongs to each racial/ethnic group. The coefficients of interest are therefore the vector β.

Our last difference concerns the specification that omits years prior to 2008 to account for changes in the way the ACS asked about employment. In addition to dropping these earlier years, we also drop any metro area that implemented a BTB policy in 2008 or earlier. We drop these areas because of the fact that including already-treated units as controls can cause substantial bias in difference-in-differences estimates identified off of staggered treatment timing (Goodman-Bacon, 2021; Baker et al., 2021). We omit the metro areas that implement BTB policies in 2008, even though the survey question had already changed, to ensure that each metro area has at least one pre-treatment period.

Table 2.5 presents our baseline results on the employment effects of BTB. Our preferred specifi-

[17] We define treatment for a given month as a BTB law being in effect on the 15th of the month. Treatment can be from a law implemented by a central city, a county that includes a central city, or a state that includes a central city.

cation is Column 3, which includes demographic controls and is fully interacted with race/ethnicity. For black men, we find that the implementation of BTB leads to a 0.15 percentage point increase in the probability of employment (0.26%). For Hispanic men, we find that BTB leads to an increase in the probability of employment of 0.61 percentage points (0.83%). For white men, we find that BTB leads to a 0.02 percentage point increase in the probability of employment (0.02%). None of these results are statistically significant. BTB does not negatively affect employment for black, Hispanic, or white men (our 95% confidence interval rejects effects larger in magnitude than -1 percentage point).

When we restrict the sample to only include MSAs that ever adopt a ban-the-box policy (Column 4), the effects on the probability of employment for black and Hispanic men increase while the effect for white men becomes negative, although none of these estimates are statistically significant. Black men are 0.81 percentage points (1.44%) more likely to be employed after BTB is adopted. Hispanic men are 1.53 percentage points more likely to be employed following BTB (2.06%). White men are 0.45 percentage points (0.55%) less likely to be employed after BTB is adopted, in places that ever adopt BTB. These employment effects are economically small (1-2%).

In Column 5 we restrict the sample to 2008 and later and exclude MSAs that implemented a ban-the-box policy in 2008 or earlier. As noted by Doleac and Hansen (2020), the ACS changed the wording of its question about employment status in 2008. Guidance from the Census Bureau indicates that researchers should use caution when comparing data from before and after this change but specifically does not indicate that a comparison is not possible.[18] Black men are now 0.61 percentage points (1.17%) less likely to be employed. For Hispanic men, the effect is attenuated: Hispanic men are 0.21 percentage points more likely to be employed, a 0.29% increase. For white men, the effect is again negative but it is attenuated relative to Column 4; they are 0.14 percentage points less likely to be employed after the adoption of BTB (0.18%). These effects across

[18] More information is available at: https://www.census.gov/programs-surveys/acs/guidance/comparing-acs-data/2008.html.

race/ethnicity are economically small and not statistically significant, suggesting that if BTB has any employment effects, they are small.

2.4.4 Event Studies

A crucial assumption in a difference-in-differences empirical strategy is that the trends in outcomes across treated and untreated groups, conditional on the control variables, would be parallel in the absence of a ban-the-box policy. While it is impossible to directly test the validity of this assumption, we can assess whether the pre-trends in outcomes are parallel. In addition, in a standard difference-in-differences framework, a constant treatment effect is assumed, but the effects may vary over time. Therefore, to assess whether there are parallel pre-trends in employment outcomes, and to test for heterogeneous treatment effects over time, we conduct an event study:

$$y_i = \alpha + \sum_{k \neq -1, k=-3}^{k=4} \beta_k \times BTB_{k,m,t} + X_i \times \theta + \delta_m + \delta_{td} + \varepsilon_i \tag{2.3}$$

$BTB_{k,m,t}$ is an indicator equal to 1 if BTB has been in effect for a central city in MSA m for k years as of time t. As we are primarily concerned with assessing the validity of the parallel pre-trends assumption, we define treatment based on whether BTB was in effect for any part of the year (as of December 15th). With this definition of treatment, there will be very few observations in the untreated group that are actually subject to BTB (this measurement error would only exist for observations interviewed after December 15 in MSAs where BTB was implemented between December 15 and December 31 of that year). Using alternative definitions of treatment (such as a full-year or half-year definition) would mean that more observations in the pre-period would be incorrectly classified, which would be problematic for analyzing the pre-trends.

Figure 2.8 shows the effect of BTB on the probability of employment for black men. The coefficients before and after the implementation of BTB are similar, small (1 or 2 percentage points), and generally not statistically significantly different than 0. The coefficients in the pre period fluctuate a bit but there does not appear to be a pre-trend in employment for black men. The event study is consistent with the average difference-in-differences effect of small, not statistically significant gains in employment for black men. Figure 2.9 presents the effect of BTB on Hispanic men's employment. All of these coefficients are 0, which is similar to the standard difference-in-differences estimate for Hispanic men: null effects. The effect of BTB on employment for white men is shown in Figure 2.10. The coefficients in the pre and post period are also 0, which is consistent with the standard estimate of very small and not statistically significant changes in employment for white men.

The broad takeaways from the event studies are that there are parallel pre-trends in the probability of employment, and the effect of BTB on employment for black, Hispanic, and white men is constant over time: specifically, there is no effect on employment.

2.4.5 Staggered Difference-in-Differences Robustness Checks

We next assess whether our estimates are biased by heterogeneity in the treatment effect over time or across jurisdictions. Goodman-Bacon (2021) shows that, in settings such as ours where treatment is staggered over time, estimates from a differences-in-differences (DID) regression with group and time fixed effects are a weighted average of a series of 2x2 DID estimates which compare early or later treated units to units that are never treated, early treated units to later treated units that have not yet been treated, and later treated units to units that have already been treated. This final constituent DID, in which already treated units serve as a counterfactual for later treated units, can cause the overall DID estimate to be too small or have the wrong sign if the earlier treated units have a larger absolute treatment effect than the later treated units (Goodman-Bacon, 2021; Baker et al., 2021).

To determine the extent of this bias, Goodman-Bacon (2021) suggests plotting the point estimate and weight of each constituent 2x2 DID that is included in the aggregate DID. Figure 2.11 includes scatter plots with this information. Because the diagnostic from Goodman-Bacon (2021) requires a balanced panel and does not allow for covariates, the scatter plots only include MSAs that are present throughout our sample and are broken out by race and ethnicity.[19] Appendix Table B.1 shows that the aggregate DID estimates from Equation (2.2) for this balanced panel subsample are very similar to those for the full sample.[20] The scatter plots and corresponding weights for the different types of constituent DIDs gives us confidence that our aggregate DID results are not driven by biases resulting from treatment effect heterogeneity. For the aggregate DID estimate for each race and ethnicity group, the weight on comparisons between newly treated MSAs and MSAs that have already been treated is less than 1%, meaning that the TWFE estimate is primarily identified using "clean controls."

We also estimate an alternative to the two-way fixed effect DID which is robust to the kinds of biases described in Goodman-Bacon (2021). Specifically, we use the estimator from Callaway and Sant'Anna (2020), which is of the form:

$$ATT(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)}\right]}(Y_t - Y_{g-1})\right)\right] \tag{2.4}$$

Here, $ATT(g,t)$ is the group-time average treatment effect for the group g that is first treated at time t. In our setting, groups are MSAs and the time unit is years. G_g is an indicator for observations belonging to treatment group g, C is an indicator for observations are that never

[19]MSAs appear and disappear during our sample because of changes in population and corresponding changes to official delineations.

[20]In our preferred specification in Column 3, black men are 0.28 p.p. (0.49%) more likely to be employed, Hispanic men are 0.68 p.p. (0.92%) more likely to be employed, and white men are 0.01 p.p. (0.02%) less likely to be employed. None of these point estimates are statistically significant.

treated, and Y is the outcome variable, which in our case is the employment rate among the workers in our sample in each MSA and year. Outcomes are weighted by the propensity score $p_g(X) = P(G_g = 1|X, G_g + C = 1)$, which gives the probability of treatment given covariates X and membership in either treatment group g or the never-treated group C. The covariates we include are the fraction of workers in each MSA and year that are of each one-year age group from 25-34, the fraction of workers with various education levels, and the fraction of workers currently enrolled in school.[21] The group-time average treatment effects are estimated using inverse probability weighting. Intuitively, this estimator compares outcomes for workers living in treated units and those living in untreated units, while placing more weight on untreated units that are observably similar to treated units. The point estimates from this estimator are distinct but conceptually similar to those from the TWFE differences-in-differences model.

Table 2.6 contains the results using the Callaway and Sant'Anna (2020) method, estimated separately for each race and ethnicity group. Column 1 indicates that black men are 1.01 p.p. (1.79%) more likely to be employed once BTB is in place. This estimate is not statistically significant and not economically meaningful. However, it is larger than the baseline point estimate from our preferred TWFE specification (0.15 p.p. or 0.26% of the mean). In contrast, Hispanic men (Column 2) are 0.71 p.p. (0.96%) less likely to be employed, an effect that is neither statistically nor economically significant and of the opposite sign of our baseline estimate of 0.61 p.p. (0.83%). Finally, in Column 3, we estimate that white men are 1.07 p.p. (1.33%) more likely to be employed. Once again, this effect is neither statistically nor economically significant, and, like the effect for black men, this point estimate is larger than our baseline TWFE estimate (0.02 p.p. or 0.02% of the mean). The pattern of somewhat larger or opposite-signed point estimates for the point estimates from the Callaway and Sant'Anna (2020) estimator compared to the TWFE estimates is consistent with the bias created by dynamic treatment effects described in Goodman-Bacon (2021). However, these differences are modest (no more than 1.6 p.p. for the coefficients) and the Goodman-Bacon

[21] These covariates are meant to parallel the fixed effects included in Equation (2.2).

(2021) decomposition above shows that very little weight is placed on the estimates responsible for the bias. These results confirm that our estimates of economically small and not statistically significant employment effects of BTB are not driven by bias inherent to TWFE regression.

2.5 Robustness Checks and Heterogeneous Treatment Effects: Across-MSA Specification

2.5.1 Assumptions about Treatment Timing

We also estimate alternative specifications that vary the definition of treatment timing, as it is not possible to tell whether BTB was in effect for an individual during the interview reference period if BTB was implemented during the same year. Our primary specification uses the fraction of the year (measured in months) that BTB was in place in the metro area (repeated in Column 1 for comparability purposes). In Table 2.7, we present results that convert treatment to a binary variable based on whether BTB was in effect on various dates. In Column 2, an MSA is treated if there was a BTB law in place as of January 15th of that year (the whole year). In Column 3, an MSA is treated if there was a BTB law in place for half the year (law effective as of July 2nd). In Column 4, an MSA is treated if there was a BTB law in place as of December 15th of that year (any part of the year).

The results are consistent with the estimates using the fractional definition of treatment (Column 1), and they are still small and not statistically significant. The effect sizes for black men range from increases in the probability of employment of -0.12 to 0.31 percentage points (-0.22 to 0.55%). For Hispanic men, the coefficients range from 0.13 to 0.77 percentage points (0.17 to 1.04%). White men are -0.12 to 0.12 percentage points more likely to be employed post-BTB adoption (-0.15 to 0.15%).

2.5.2 Alternative Definition of Treatment Implementation

Our preferred specification does not consider an MSA to be treated until a central city is covered by a BTB law. Our motivation for defining treatment in this way is that we do not want to consider an MSA to be treated until a substantial fraction of the workers in the MSA are covered by a BTB law. An alternative way to define treatment (which is how Doleac and Hansen (2020) define it) is to consider an MSA treated once any part of the MSA is subject to a BTB law. We present results using this alternative definition in Appendix Table *B*.2.

Column 1 presents results using our fractional definition of treatment (based on months of the year). Columns 2 through 4 present results using the binary January, half-year, and December definitions. The effects for all 3 groups are very similar to our preferred specification that uses the central-city definition of treatment, and none of them are statistically significant.

After the adoption of a BTB law anywhere in the MSA, black men are between -0.05 and 0.23 percentage points (-0.10 to 0.42%) more likely to be employed. Hispanic men are 0.14 to 0.96 percentage points more likely to be employed (0.19 to 1.31%). White men are -0.06 to 0.14 percentage points more likely to be employed following BTB (-0.07 to 0.17%).

Overall, the results are similar to our preferred, central-city specification: there are very small and not significant employment changes for black, Hispanic, and white men. The similarity is not surprising given that very few small cities implement BTB laws before jurisdictions that encompass a central city do.

2.5.3 Alternative Model Specifications

Our results from our preferred specification are slightly different than the corrected Doleac and Hansen (2020) results, so in Table 2.8, we present results from modifying our specification to determine whether any one change is driving these differences. In Column 1, we replace the year-by-Census-division fixed effects with year-by-Census region fixed effects. In Column 2, we add linear time trends. In Column 3, we use the full sample as opposed to restricting our sample to individuals who live in an MSA.

When we use year-by-Census-region fixed effects, the effects of BTB change sign for black and white men and become larger for Hispanic men, although none of the effects are statistically significant. They are also small (-0.76 to +2.06%). When we add linear time trends, the effects for all men change sign relative to our preferred specification, but none of these effects are significant and they are all economically small (-0.70 to -0.21%).

When we use the full sample, the effect sizes are larger for each group and become statistically significant for Hispanic men. The effects for black and white men are economically small (+0.47 to 0.51%). For Hispanic men, after BTB is implemented they are 1.69 percentage points more likely to be employed (2.32%), which is statistically significant at the 5% level.

Our results are very similar using the full sample versus restricting to MSAs only, and the corrected Doleac and Hansen (2020) results are similar when they use the full sample or MSAs only (although the coefficient for Hispanic men changes sign), suggesting that this particular specification choice is not driving the discrepancy in our results. Rather, it appears that the differences between our results and those of Doleac and Hansen (2020) is coming from the combination of the region fixed effects and the linear time trends.

2.5.4 Labor Force Participation and Annual Earnings

Labor market effects of ban-the-box policies may not be limited to employment. They may also affect labor force participation and earnings. With respect to labor force participation, if individuals, particularly those with a criminal history, believe that BTB represents a reduction in discrimination, they may rejoin the labor force and increase their search effort. Alternatively, if black and Hispanic men believe that BTB will result in employers discriminating against them on the basis of race, then they may become discouraged and drop out of the labor force. To the extent that ban-the-box policies reduce discrimination, especially against workers with a criminal history, workers may see increases in the match quality, as measured through annual earnings. Conversely, if ban-the-box policies result in employers discriminating against black and Hispanic men, these men may end up taking lower-paying jobs if they are shut out of higher-paying jobs.

Appendix Table *B*.3 shows the effects of BTB on labor force participation and log of annual earnings. Column 1 is our preferred specification and corresponds to the employment effects in Column 3 of Table 2.5. Column 2 of Panel A restricts our sample to MSAs that eventually adopt BTB. Column 3 restricts the sample to 2008 or later.

In all 3 specifications, the adoption of BTB leads to very small changed in labor force participation for black men (-0.84 to +0.01 percentage points, or -1.23 to +0.01%). Hispanic men experience very small increases in labor force participation (0.36 to 0.75 percentage points, or 0.44 to 0.91%). White men also experience very small changes in labor force participation, ranging from -0.40 to +0.12 p.p. (-0.46 to +0.14%). None of these estimates are statistically significant. Overall, BTB has precisely estimated null effects on labor force participation for these demographic groups. We can rule out changes in labor force participation of more than 3 percentage points with our 95% confidence intervals.

Panel B of Appendix Table *B*.3 shows the results for log of annual earnings. Again, Column 1 is our preferred specification. After BTB is adopted, black men's annual earnings decrease by approximately 0.69%, Hispanic men's annual earnings increase by approximately 1.74%, and white men's earnings decrease by roughly 2.23%. The effect for white men is marginally statistically significant.

When we only include MSAs that ever adopt BTB in our sample (Column 2), the effect for black men changes sign, the effect for Hispanic men triples in size and becomes marginally significant, and the effect for white men attenuates and is no longer significant. After the adoption of BTB, annual earnings for black men increase by 0.74%, annual earnings for Hispanic men increase by 5.41%, and annual earnings for white men decrease by 0.45%.

When we restrict the sample to 2008 and later (Column 3), the effects for black and white men are similar to Column 2 but a bit larger in magnitude, the effect for Hispanic men attenuates and changes sign, and the effect for white men is slightly larger in magnitude. However, none of these estimates are statistically significant. After BTB is implemented, black men's annual earnings increase by 2.84%, Hispanic men's annual earnings decrease by 0.62%, and white men's annual earnings decrease by 1.98%.

In our preferred specification, we find null or small effects of BTB on log of annual earnings. The effects for black and Hispanic men are not precisely estimated, but the effects for white men are.

2.5.5 Public-Sector Employment

We consider a metro area as treated when any type of BTB law is implemented (covering public sector, government contractors, or private-sector employers). However, the first type of law implemented is always a law governing public-sector employers.[22] Given that our treatment variable is effectively measuring public-sector BTB laws, it is natural to analyze the effect on public-sector employment, where there should in theory be first-order effects.

Our results for the effect of BTB on public-sector employment are in Table 2.9. Our preferred specification is Column 1. The adoption of BTB leads to a reduction in the probability of being employed in the public sector for black men of 0.02 percentage points (0.50%), which is not statistically significant. For Hispanic men, BTB adoption leads to a reduction in the probability of public-sector employment of 0.11 percentage points (3.10%), which is also not statistically significant. For white men, BTB leads to a reduction in the probability of public-sector employment of 0.39 percentage points (9.83%), which is statistically significant at the 1% level.

When we restrict our analysis to areas that ever adopt BTB, the effect on black men's public-sector employment becomes larger in magnitude and marginally statistically significant. Black men are 0.66 percentage points less likely to be employed by the public sector (17.12%). The percent effect is quite large because the base is small; only 4% of black men work in the public sector. For Hispanic men, the effect becomes positive (but is still small in level terms); their probability of public-sector employment increases by 0.35 percentage points (9.79%), which is not statistically significant. For white men, the effect is very similar; BTB decreases public-sector employment by 0.35 percentage points (8.94%), which is statistically significant at the 5% level.

When we restrict our analysis to 2008 and later and exclude metro areas that implemented BTB prior to 2009, none of the estimated effects are statistically significant. Black men are now 0.26 percentage points more likely to be employed in the public sector (7.39%). Hispanic men are 0.12 percentage points more likely to have public-sector employment (3.52%). The negative employment effect for white men is attenuated; they are now only 0.22 percentage points less likely to be employed in the public sector (5.79%). Overall, there may be some small (in levels) negative

[22]Occasionally a BTB law is implemented that covers multiple types of employment, but in these instances, public-sector employment is always included.

public-sector employment effects, particularly for white men.

2.5.6 Employment for Other Groups

The motivation for restricting our sample to men ages 25-34 with no college degree is that this group is most likely to have contact with the criminal justice system, so if employers respond to a ban-the-box policy by discriminating on the basis of race (in an attempt to screen out individuals with a criminal history), then this demographic group is the one they may be most likely to discriminate against. If BTB creates similar patterns for other demographic groups, then something else may be occurring.

In Table 2.10, we analyze employment effects for other demographic groups, specifically women ages 25 to 34 and older men, all without a college degree. Column 1 presents the results for women; none of these results are statistically significant. BTB leads to a reduction in the probability of employment for black women of 0.80 percentage points (1.21%). For Hispanic women, BTB leads to an increase in the probability of employment of 0.16 percentage points (0.25%). BTB leads to an increase in the probability of employment for white women of 0.45 percentage points, or 0.68%. All of these effects are economically small, suggesting that BTB did not have much of an effect on women's employment.

In Column 2, we estimate effects for men ages 35 to 44. Black men are 1.42 percentage points more likely to be employed after the implementation of BTB, a 2.25% increase that is not statistically significant. This effect is approximately an order of magnitude larger than our estimate for black men ages 25 to 34 in our preferred specification (Column 3 of Table 2.5), but it is still small. Hispanic men are 2.25 percentage points more likely to be employed after BTB is in place, a 2.88% increase that is statistically significant at the 5% level. This effect is about larger than the effect for younger Hispanic men but still small. White men are 0.08 percentage points more likely to be employed under a BTB policy, which reflects a 0.10% increase and is not statistically significant. This effect is similar to the effect for white men ages 25 to 34.

For men ages 45 to 54, the employment effects are slightly larger than the effects for 25 to 34 year-old men. BTB leads to an increase in the probability of employment of 0.49 percentage points (0.81%) for 45 to 54 year-old black men. The probability of employment for Hispanic men increases by 1.98 percentage points (2.61%). BTB leads to an increase in the probability of employment of 0.28 percentage points for white men (0.35%). The effect for Hispanic men is statistically significant at the 5% level but the effects for black and white men are not statistically significant. For Hispanic men ages 35 to 54, BTB has small positive impacts on employment.

For men ages 55 to 64, the effects of BTB on employment are attenuated and change sign. However, these men are nearer to retirement and have lower labor force attachment, which may explain different outcomes for them. Black men ages 55 to 64 are 0.08 percentage points less likely to be employed after BTB (0.16%). Hispanic men ages 55 to 64 are 0.10 percentage points less likely to be employed post-BTB implementation (0.17%). White men ages 55 to 64 are 0.34 percentage points less likely to be employed after BTB is adopted (0.54%). None of these effects are statistically significant.

2.5.7 Heterogeneous Effects by Education

Educational attainment differs by both race and criminal history. Of individuals released from state prisons between 2000 and 2013, 51% had less than a high-school degree, 41% had at most a high-school degree, 6% had some college but no degree, and only 1% had a college degree (Yang, 2017). Educational attainment is also observable to an employer because it is listed on a job applicant's resumé. Therefore, if employers are responding to BTB by attempting to screen out applicants with characteristics correlated with criminal history, then they should be discriminating on both race and educational attainment. To test this hypothesis, we conduct a robustness check where we separately estimate the effect of BTB for various levels of education. If employers are using proxies for criminal history, then BTB should have negative employment effects for men with a high-school degree or less education, as compared to men with at least some college.

Table 2.11 shows the results separated by different levels of educational attainment, for men ages 25 to 34. Column 1 shows the results for men with less than a high-school education and moving across columns shows results for progressively higher levels of educational attainment. White men with a GED are less likely to be employed following adoption of BTB and Hispanic men with some college or a Bachelor's degree are more likely to be employed.

The effects by educational attainment for black men are all small and not statistically significant, with point estimates ranging from -1.03 percentage points to 1.56 percentage points (-1.43 to 2.53%). Black men with a GED are 0.77 percentage points (2.53%) more likely to be employed following BTB but this effect is not statistically significant.

Hispanic men with less than a high-school degree are 2.13 percentage points (3.60%) more likely to be employed following BTB, those with some college are 1.96 percentage points more likely to be employed (2.38%), and those with a Bachelor's degree are 2.73 percentage points (3.04%) more likely to be employed. The effects for men with some college and men with a Bachelor's degree are significant at the 10% and 1% levels, respectively. Hispanic men with a GED see declines in the probability of employment of 3.74 percentage points (6.26%), which is economically meaningful but imprecisely estimated. Hispanic men with a high school or Associate degree are 1 to 2 percentage points (1 to 2%) less likely to be employed once BTB is adopted, but these estimates are also not statistically significant.

White men with a GED are 3.62 percentage points less likely (5.59%) to be employed after the implementation of BTB; this estimate is statistically significant at the 5% level. Effects for white men with other levels of educational attainment are small and not statistically significant, ranging from -0.37 percentage points to 0.31 percentage points (-0.40 to 0.51%).

Overall, BTB appears to have economically small and statistically insignificant employment effects for black and white men across the educational attainment spectrum, with the exception of white men with GEDs. For Hispanic men with some college or a Bachelor's degree, they see small employment gains with BTB. Hispanic men with a GED experience declines in employment although this effect is not precisely estimated and not statistically significant.

2.5.8 Heterogeneous Effects by Legal Jurisdiction of BTB Policy

BTB policies implemented at the city level will not cover the entire population of the MSA, so it is possible that imperfectly measured treatment status is attenuating the "true" effect of BTB policies on employment. To address this concern, we create separate BTB variables for city laws, county laws, and state laws (each type of law must cover a central city in the MSA to be counted). State laws will generally cover more of an MSA's population than city laws, so it is possible that state laws may have an effect on employment outcomes while city laws do not. As with our primary specification, we use the fraction of the year (measured in months) a BTB law is in place for each legal jurisdiction. In some MSAs, multiple legal jurisdictions have implemented BTB laws, and we allow for an MSA to be coded as having both a city law and a state law, for example.

Our results for this analysis are in Table 2.12. In our preferred specification (Column 1), city and county BTB laws have small, positive, and not statistically significant effects on employment outcomes for black, Hispanic, and white men. The effect sizes are not economically meaningful. For black men, city BTB laws increase the probability of employment by 0.40 p.p. (0.70%), county BTB laws increase the probability of employment by 0.88 p.p. (1.52%), and state laws decrease the probability of employment by 0.95 p.p. (1.87%). For Hispanic men, city BTB laws increase the probability of employment by 0.95 p.p. (1.30%), county BTB laws increase the probability of employment by 1.08 p.p. (1.43%), and state BTB laws increase the probability of employment by 0.24 p.p. (0.33%). For white men, city BTB laws increase the probability of employment by 0.03 p.p. (0.04%), county BTB laws increase the probability of employment by 0.69 p.p. (0.86%), and state laws decrease the probability of employment by 0.02 percentage points (0.03%).

When we restrict our sample to MSAs that are ever covered by a BTB policy (Column 2), the effect sizes generally become slightly larger in magnitude and the effect of county BTB laws for Hispanic men become marginally statistically significant. The effect of city BTB laws on employment for white men changes sign. The effect sizes are still relatively small, however, ranging from -0.77 p.p. to +3.03 p.p. (-1.52 to 4.02%).

When we restrict the sample to 2008 and later and exclude MSAs that were covered by a BTB policy prior to 2009 (Column 3), the broad interpretation of the results is the same as in our preferred specification. The estimates are not statistically significantly different than 0 and they are small in magnitude, ranging from -1.38 p.p. (-3.01%) to +2.49 p.p. (3.35%).

Our analysis of these results is that this particular type of measurement error in treatment status is not likely to be obscuring the true effect of ban-the-box policies on employment. If that were happening, we would expect state BTB policies to have consistently larger effects on employment than cities and counties, which the results do not support.

2.5.9 Sector of Employment Covered by BTB

We also investigate whether the sector covered by a BTB law matters for employment outcomes. Very few men ages 25-34 without a college degree are employed in the public sector, so a public-sector BTB law may not impact aggregate employment. Laws covering government contractors or the private sector may have an impact as many more employers and workers are affected by the policy.

Table 2.13 shows the results when we allow for a BTB policy to have different impacts based on whether it covers public-sector employers, public-sector employers and government contractors, or all employers. Column 1 represents our preferred specification. In Column 2, we restrict the sample to MSAs that ever implement any type of BTB policy. In Column 3, we restrict the sample to 2008 or later due to the change in the wording of the employment questions in the ACS, and we exclude MSAs that are covered by a BTB policy that was implemented prior to 2009.

Under our preferred specification, different types of BTB policies have small and not statistically significant effects on the probability of employment. A public-sector BTB law is associated with a 0.06-percentage-point reduction in the probability of employment for black men (0.11%), a 0.43-percentage-point increase in the probability of employment for Hispanic men (0.57%), and a 0.27-percentage-point increase in the probability of employment for white men (0.34%). Employment effects for a BTB law that covers both the public sector and government contractors are slightly larger and positive for black and Hispanic men and negative for white men, but still small. BTB laws that cover both of these sectors are associated with increases in the probability of employment of 1.04 percentage points (1.94%) for black men, 1.31 percentage points (1.87%) for Hispanic men, and a decrease of 0.65 percentage points (0.84%) for white men. When all employers are covered by a BTB policy, black men's probability of employment increases by 0.89 percentage points (1.70%), Hispanic men's probability of employment increases by 1.53 percentage points (2.14%), and white men's probability of employment decreases by 0.56 percentage points (0.70%).

When we restrict the sample to MSAs that ever implement any type of BTB policy, the effects for black men are positive and larger, and the results for laws that cover more than public-sector employment are economically meaningful. BTB policies that cover government and government contractors increase the probability of employment for black men by 2.44 percentage points (4.54%), although this effect is not statistically significant. When a BTB policy covers all sectors, black men are 3.73 percentage points (7.12%) more likely to be employed; this effect is marginally statistically significant. The effects for Hispanic men are broadly similar to our preferred specification, and the effects for white men are all negative and larger in magnitude, but still small.

When we restrict our analysis to 2008 and later and drop MSAs that were covered by any type of BTB policy prior to 2009, none of the effects are statistically significant. The effects on employment are relatively small for all races, ethnicities, and law types, ranging from a 1.17-percentage-point (1.71%) decrease in the probability of employment for Hispanic men under a BTB policy that covers all employers, to a 1.77-percentage-point (3.48%) increase in the probability of employment for black men under a BTB policy that covers both the public sector and government contractors.

Overall, under our preferred specification, different types of BTB policies do not harm minority workers' employment prospects and more expansive BTB policies may even help them, although the effect sizes are rarely statistically significant.

2.6 Within-MSA Specification

2.6.1 Within-MSAs Design

Our within-MSA empirical strategy is new to the BTB literature but has been used to study other labor market policies, most notably the minimum wage (Card and Krueger, 1994; Dube et al., 2010). Here, identification of the effect of BTB is based on differences in BTB policy within individual MSAs. We call the various state portions of MSAs that span multiple states "MSA-state units." There are 27 MSAs that include multiple states. There are 19 MSAs that have MSA-state units that become covered by BTB during our sample period and have individuals sampled in the ACS.[23] Similar to the across-MSAs design, we consider an MSA-state unit to be treated once a central city in the MSA-state is covered by BTB. In the few instances where a legal jurisdiction implemented BTB in an MSA-state unit that does not contain a central city, we consider the MSA-state unit to be treated once the state implements a BTB policy.[24] The rationale for separating MSAs into different state portions is that online job boards show only 28% of job searchers look for work out of state, and just 11% of applications are sent to postings in a different state (Indeed, 2014; Marinescu and Rathelot, 2018). It is also the case that some of these laws are implemented at the city or county level while others are implemented at the state level.

Figure 2.7 depicts an example of this treatment assignment for the Chicago-Naperville-Elgin, Illinois-Indiana-Wisconsin MSA. This MSA is comprised of the counties shaded either blue or orange. The blue counties constitute the Illinois portion of this MSA and are considered treated by Chicago's BTB policy, which was implemented in June 2007. The orange counties constitute the Wisconsin and Indiana portions of this MSA and serve as the control group for the Illinois portion.

We estimate regressions of the form:

$$y_i = \alpha + BTB_{m(j)t} \times Race_i \times \beta + X_i \times Race_i \times \theta + Race_i \times \gamma_{mt} + \varepsilon_i \tag{2.5}$$

Where y_i is a labor market outcome for worker i. $m(j)$ corresponds to the state j portion of MSA

[23] We drop the Minneapolis-St. Paul-Bloomington, MN-WI MSA from our sample for the within-MSA specification because there are no men ages 25-34 without a college degree living in the Wisconsin part of the MSA in the 2005-2012 waves of the ACS. The Minnesota part of the MSA is covered by BTB starting in December 2006, so there is no untreated comparison group when treatment starts in 2006.

[24] Coincidentally, in every MSA-state without a central city that implements BTB in our sample, a state law precedes any city or county laws, so in effect we are considering the MSA-state unit treated once any BTB law is implemented. In a robustness check we estimate effects by city, county, or state law, and in this specification we exclude the one local law to be consistent with our definition of treatment elsewhere (Prince George's County, Maryland, which took effect in December 2014).

m. X_i includes worker characteristics such as age, years of education, and an indicator for being currently enrolled in school. γ_{mt} are fixed effects for the MSA-year m,t. We fully interact the right-hand side of (2.5) with indicators for race and ethnicity (non-Hispanic black, Hispanic, and non-Hispanic white). Standard errors are clustered at the state level.

The coefficient of interest is β, which identifies the causal effect of BTB on worker outcomes under two standard assumptions. The first identification assumption is that the trend in outcomes for workers in the untreated MSA-state units serves as an accurate counterfactual for those in the treated MSA-state units, conditional on the control variables (parallel trends). The second identification assumption is that there are no unobserved shocks that correlate with the timing of BTB, conditional on the control variables. We test whether the first assumption is likely satisfied by estimating event studies for each of the outcomes we study.

2.6.2 Within-MSAs Results

Table 2.14 contains estimates of the effect of BTB on the probability of employment for black, Hispanic, and white men using our within-MSAs design. Individuals are considered treated if they live in an MSA-state unit in which BTB is in place for a central city in the MSA-state unit.[25] Identification of the effect of BTB is based on differences in BTB policy within individual MSAs over time because each specification includes MSA-year fixed effects. Our preferred specification is in Column 3 and corresponds to Column 3 of Table 2.5.[26]

In our preferred specification (Column 3), the adoption of BTB leads to a 4.84-percentage-point reduction in the probability of employment for black men (8.42%), although this effect is not statistically significant. For Hispanic men, BTB is associated with a 2.61-percentage-point reduc-

[25] Or, if the MSA-state unit does not contain a central city, if their state of residence has implemented BTB.

[26] Column 4 shows results when we restrict the sample to 2008 and later and exclude MSAs where an MSA-state unit was covered by BTB prior to 2009. Column 4 of Table 2.14 is analogous to Column 5 of Table 2.5.

tion in the probability of employment (3.52%), but this effect is also not statistically significant. For white men, the probability of employment decreases by 1.77 percentage points following BTB (2.19%), which is statistically significant at the 5% level. The estimate for black men is negative and the magnitude is economically meaningful but it is imprecisely estimated.

When we restrict our analysis to 2008 and later (Column 4), the results for black and white men are slightly attenuated but the result for Hispanic men more than doubles. Black men are 3.00 percentage points (5.40%) and white men are 1.25 percentage points (1.59%) less likely to be employed following BTB implementation; these results are not significant. Hispanic men are now 7.11 percentage points less likely to be employed (9.65%), which is statistically significant at the 1% level.

When looking within an MSA, there appear to be different employment effects on either side of the state border. Employment falls for men in the MSA-state unit that is covered by BTB relative to the MSA-state unit in the same MSA that is not covered by BTB. The sign changes relative to our across-MSA specification suggest that there may be employment spillovers across state lines when one part of the MSA becomes covered by BTB but the other part of the MSA does not. From the across-MSA specification, BTB has no effect or very small positive effects on employment in MSAs that are covered by BTB relative to MSAs that are not covered by BTB, but within the MSAs that become covered by BTB, employment falls in the MSA-state of implementation relative to the MSA-state(s) that did not implement BTB. Put another way, small decreases in employment in MSA-states that are covered by BTB appear to almost exactly offset small increases in employment in the parts of the MSA that are not covered by BTB.

How does one think about these negative employment effects in the within-MSA specification? Aside from the potential spillovers, reductions in employment for all young men without college degrees suggests that employers are not discriminating on the basis of race. If racial discrimination were occurring, then the point estimate for white men should not be statistically significant. It is possible that employers are discriminating against all young men without college degrees, but this is a different story of discrimination than one that is racially motivated.

2.6.3 Public-Sector Employment

We estimate the effects of BTB on public-sector employment using our within-MSA design, motivated by the fact that the first BTB law in any jurisdiction covers the public sector. Results for this analysis are shown in Table 2.15, and Column 1 is our preferred specification. When BTB covers an MSA-state unit, black men without a college degree are 0.48 percentage points less likely to be employed in the public sector (12.42%). Hispanic men are 0.14 percentage points more likely to be employed in the public sector (3.50%). White men are 0.02 percentage points more likely to be employed in the public sector (0.48%). None of these effects are statistically significant and they are small in magnitude (the percent effect for black men is moderate because the base is so small).

2.6.4 Heterogeneous Effects by Legal Jurisdiction of BTB Policy

We also test for heterogeneous effects of BTB policies based on whether the policy is implemented at the city, county, or state level using our within-MSA design. The motivation for this analysis is the same as with our across-MSA design. We again use the fraction of the year (measured in months) a BTB law is in place for each legal jurisdiction. In some MSA-state units, multiple legal jurisdictions have implemented BTB laws, and we allow for an MSA to be coded as having both a city law and a state law, for example.

Our results for this analysis are in Table 2.16. In our preferred specification (Column 1), for black and Hispanic men, city and state BTB laws have small-to-moderate negative but not statistically significant effects on employment, while county BTB laws have large positive effects on employment for black men (marginally significant) and small positive effects on employment for Hispanic men (not significant). For white men, all 3 law types have small negative effects on employment, with the effects of county and state laws being statistically significant at the 5% level.

Given that some MSA-state units have multiple types of legal jurisdictions with BTB laws, to calculate the predicted effect of BTB for a particular MSA-state unit it may be necessary to add some of the coefficients together. For example, the city of Alexandria, Virginia and Arlington County, Virginia both implement BTB policies in 2014 (they are part of the Virginia portion of the Washington-Arlington-Alexandria, DC-VA-MD-WV MSA). For the months where both jurisdictions have BTB, the predicted effect of BTB would be the effect for a city law plus the effect for a county law.

For black and Hispanic men, the state BTB coefficients are smallest in magnitude (out of city, county, or state). For white men, the state BTB coefficient is the largest in magnitude. If our primary within-MSA results suffered from attenuation bias due to measurement error in the treatment variable (resulting from less than 100% of individuals being covered by a BTB policy), we would expect the state coefficients to be the largest, because when the state in an MSA-state unit implements a BTB policy, 100% of workers living in that MSA-state unit are covered by the policy.[27]

2.7 Discussion

Reintegrating individuals with a criminal history into society has thus far proven difficult to accomplish: 68% of individuals who are released from prison will recidivate within three years of their release (Alper et al., 2018). If some of these individuals are recidivating due to a lack of

[27]Of course, to the extent that the men in our sample live and work in different states, there would still be measurement error in treatment status.

job opportunities in the formal sector, then any policy that can improve labor market outcomes for the formerly incarcerated might reduce recidivism. Additionally, any policy that can successfully improve labor market outcomes for individuals with a criminal history might also reduce racial disparities in the labor market, as black men are incarcerated at 5.7 times the rate of white men and Hispanic men are incarcerated at 3.2 times the rate of white men (Bronson and Carson, 2019). However, these policies could also have unintended consequences for the target population as well as other vulnerable populations. Previous theoretical and empirical work in economics has found that when information about job applicants that employers want to know is removed, employers resort to discrimination based on observable characteristics that employers perceive to be correlated with the unobservable characteristics (Aigner and Cain, 1977; Autor and Scarborough, 2008; Wozniak, 2015; Bartik and Nelson, 2021).

We revisit the possibility of this discrimination in a prominent example intended to help the formerly incarcerated have a fair chance at employment, Ban the Box.[28] In this paper, we test whether ban-the-box policies affect the probability of employment for young black, Hispanic, and white men without a college degree. We use three different difference-in-differences methods to answer this question. The first is a standard across-MSA design that compares employment outcomes in MSAs that eventually are covered by BTB to MSAs that are not covered by BTB. The second is the approach outlined in Callaway and Sant'Anna (2020) that accounts for potential biases when treatment is staggered. The third is a within-MSA design that is new to the literature on BTB but has been used to answer other questions in labor economics, and it compares portions of MSAs (MSA-state units) that are covered by BTB to other portions of the same MSA that are not covered by BTB.

Using our across-MSA specification, we find precisely estimated null effects of BTB on the probability of employment for black, Hispanic, and white men (+0.15 p.p. for black men, +0.61

[28]We again note that under U.S. employment law, it is illegal for employers to discriminate on the basis of race in hiring decisions. It is therefore ironic that in attempting to screen out individuals who have been convicted of a crime, employers may themselves be breaking the law.

p.p. for Hispanic men, and +0.02 p.p. for white men). These results are robust to the difference-in-differences advances and bias tests outlined in Goodman-Bacon (2021) and Callaway and Sant'Anna (2020). Using our within-MSA design, we find that ban-the-box policies have negative but generally imprecisely estimated effects on the probability of employment for black, Hispanic, and white men (-4.84 p.p. for black men; -2.61 p.p. for Hispanic men; -1.77 p.p. for white men, statistically significant at 5% level). These negative employment effects are not inconsistent with the across-MSA results: BTB may have different effects in different parts of an MSA that offset each other when examining the entire local labor market (the MSA).

We contribute to the literature on the labor market effects of ban-the-box policies by correcting and updating the results of the canonical paper in the ban-the-box literature, and we use two new-to-BTB empirical strategies outlined above. Accurately estimating the aggregate labor-market effects of a common policy such as BTB is important for designing both optimal labor market policies and optimal policies to help individuals recently released from prison reintegrate into society.

The implications of our results are perhaps anticlimactic. We find that a policy that delays the revelation of information about job applicants that employers may want to know (whether an applicant has been formerly incarcerated) does not have economically meaningful aggregate employment effects. In hindsight, this result is perhaps not surprising because while delaying this information may represent a small increase in search costs on the employer side, this information is ultimately publicly available if an employer is sufficiently motivated to break the law and search for an applicant's criminal history online. While we do not find aggregate employment effects, that is not to say that there are no effects for certain subgroups of the population.

The primary limitation of this paper is that we are unable to directly estimate employment effects of BTB for formerly incarcerated individuals, who are the group that this policy was intended to help. We designed our sample restrictions to focus on the demographic groups most likely to either be formerly incarcerated or to be perceived by potential employers as most likely to be formerly incarcerated, but many people in our sample have never been incarcerated and there exist people not in our primary sample who have been incarcerated. BTB could have small employment effects for formerly incarcerated individuals that we would be underpowered to detect (or there could be offsetting effects for formerly incarcerated and never-incarcerated individuals).

An important avenue for future work would be to directly estimate employment outcomes for individuals who are formerly incarcerated, perhaps by using the new Criminal Justice Administrative Records System (CJARS) data. Another interesting direction for future work would be to further investigate spillover effects of ban-the-box policies.

2.8 Works Cited

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2.9 Figures and Tables

Figure 2.1: Criminal Conviction Questions on Job Applications

Have you ever been convicted of a felony? ☐ Yes ☐ No

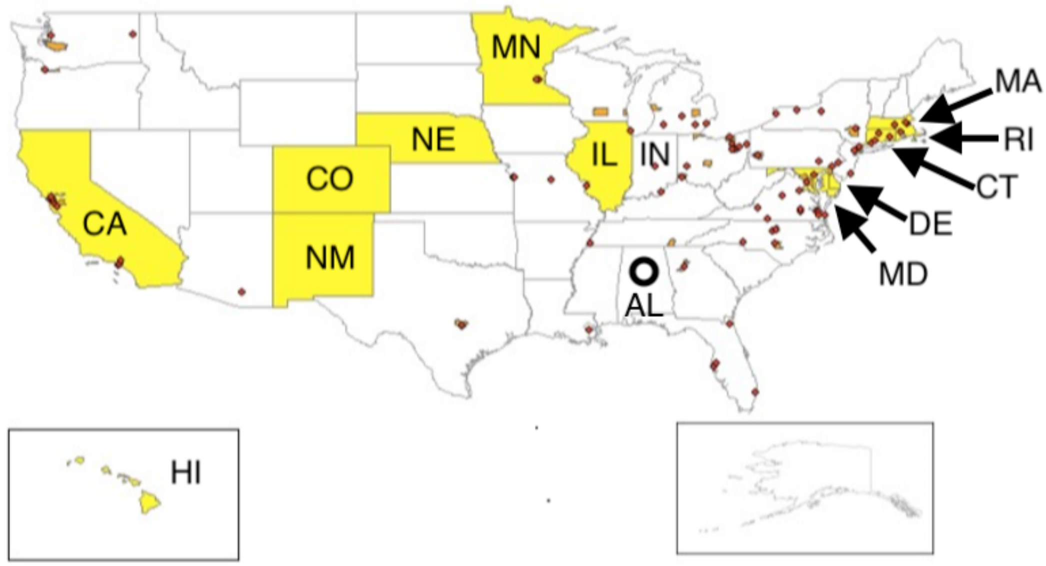
Date(s) / Nature of Offense(s): ____________________

Have you ever been convicted of a misdemeanor involving weapons, theft, dishonesty, or violence? ☐ Yes ☐ No

Date(s) / Nature of Offense(s) / Sentence Imposed: ____________________

Source: job-applications.com

Figure 2.2: Jurisdictions with Ban the Box Policies by December 2014

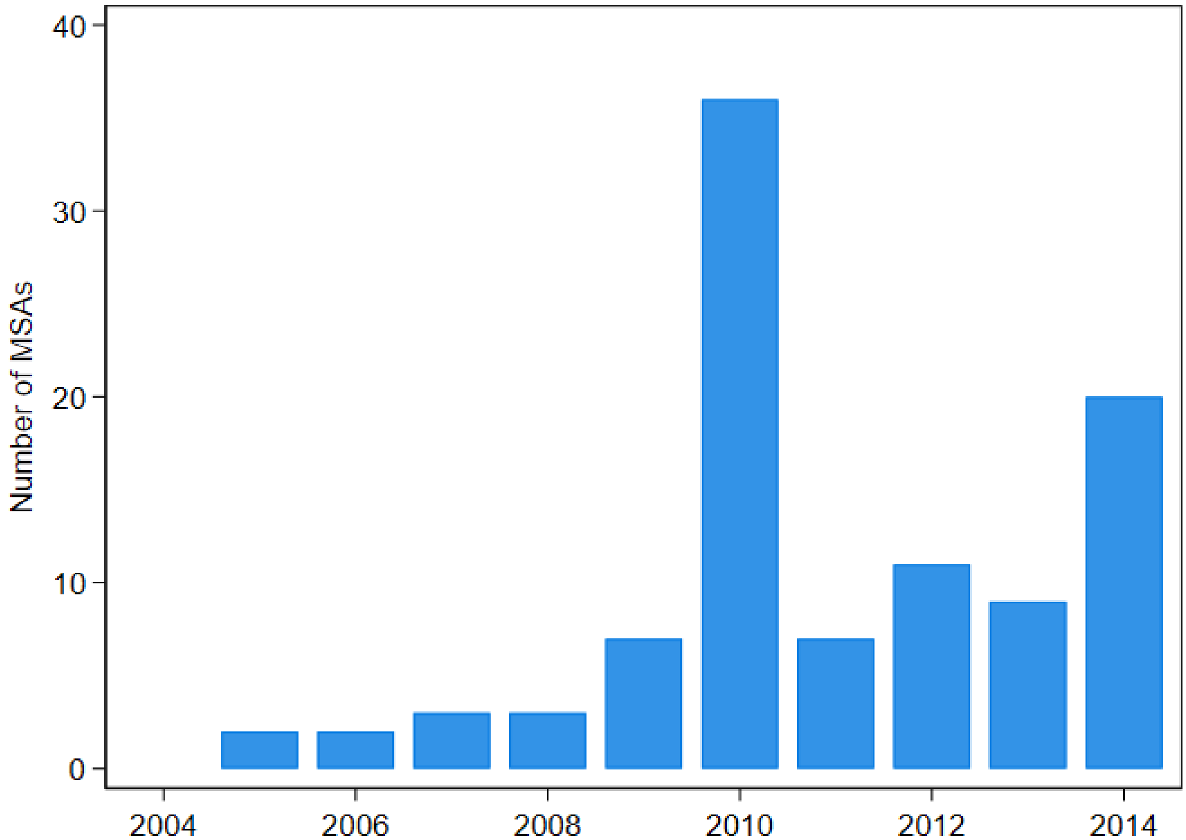


Jurisdictions with BTB policies are represented by yellow shading (state-level policies), orange shading (county-level policies), and red dots (city-level policies.)

Note: The shaded-in jurisdictions represent jurisdictions with any ban-the-box policy in place by December 2014 (the end of our sample period). The hollow black circle in Alabama approximates the Birmingham-Hoover, AL MSA, which is an example of an untreated observational unit.

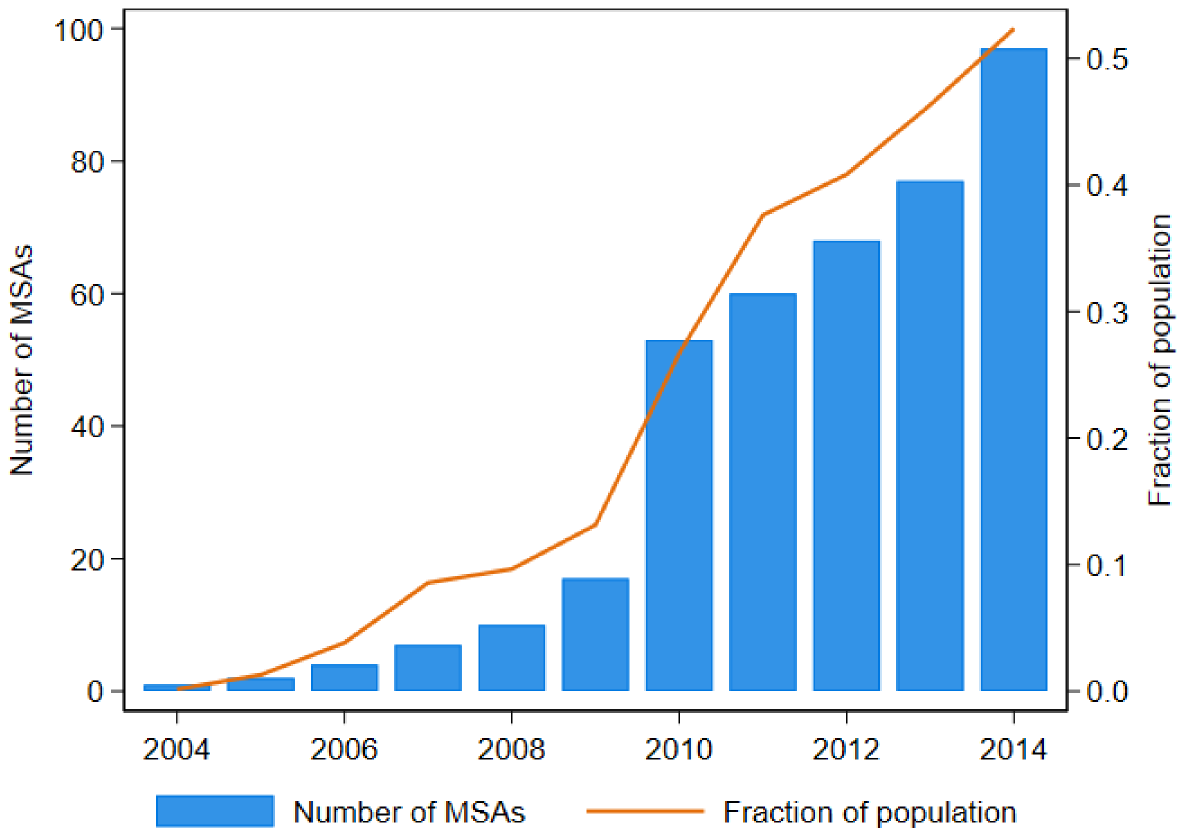
Source: Doleac and Hansen (2020) with labels and the hollow circle added by the authors.

Figure 2.3: Number of Metropolitan Statistical Areas Newly Covered by BTB



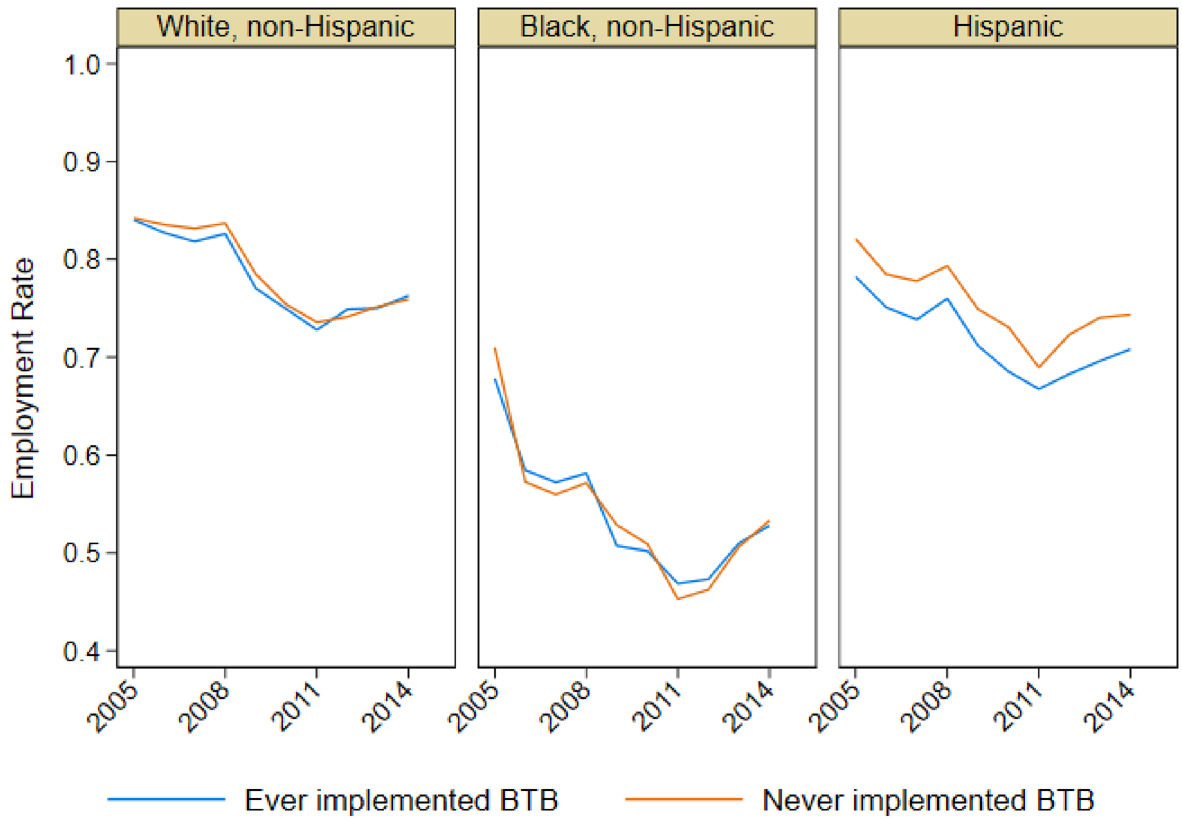
Note: Data from Table 1 of Doleac and Hansen (2020), Avery and Lu (2020), local government websites, law firm websites, and news articles. Metropolitan Statistical Areas are coded as treated by Ban the Box (BTB) if a legal jurisdiction that contains a central city has implemented the policy. Hawaii's BTB law is not shown in this graph as Hawaii implemented a statewide BTB in 1998.

Figure 2.4: Cumulative Number of Metropolitan Statistical Areas and Fraction of Population Covered by BTB



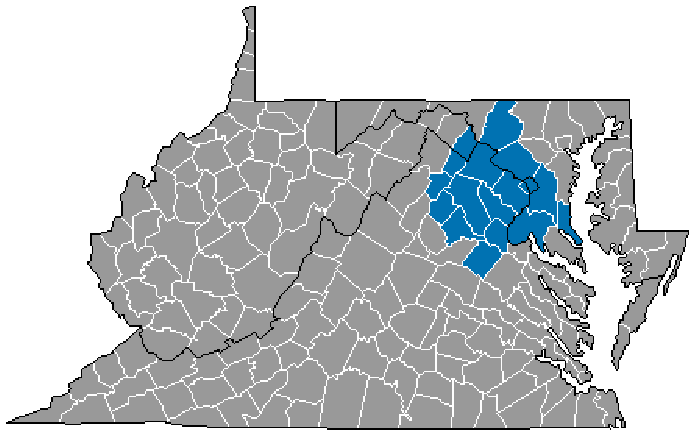
Note: Data from 2005-2014 waves of the American Community Survey, Table 1 of Doleac and Hansen (2020), Avery and Lu (2020), local government websites, law firm websites, and news articles. Metropolitan Statistical Areas are coded as treated by Ban the Box (BTB) if a legal jurisdiction that contains a central city has implemented the policy.

Figure 2.5: Employment Rates for Men Ages 25-34 Without a College Degree in Metropolitan Statistical Areas



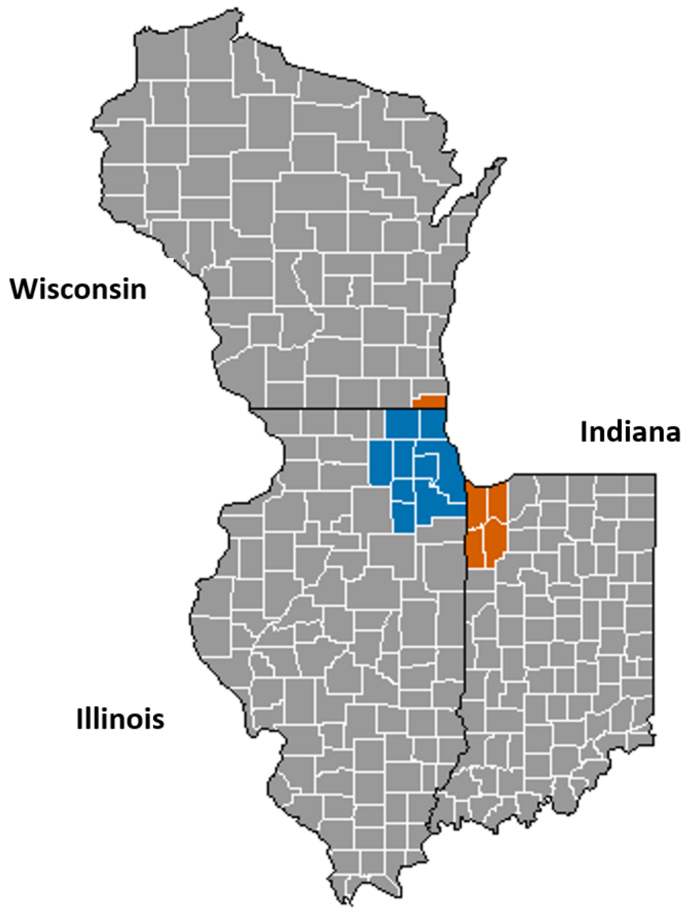
Note: Data from 2005-2014 waves of the American Community Survey. Sample consists of men ages 25-34 without a college degree (two-year or four-year) who reside in a Metropolitan Statistical Area (MSA). MSAs are categorized by whether at least one central city is covered by Ban the Box (BTB) during the sample period.

Figure 2.6: Map of Washington-Arlington-Alexandria, DC-VA-MD-WV Metropolitan Statistical Area



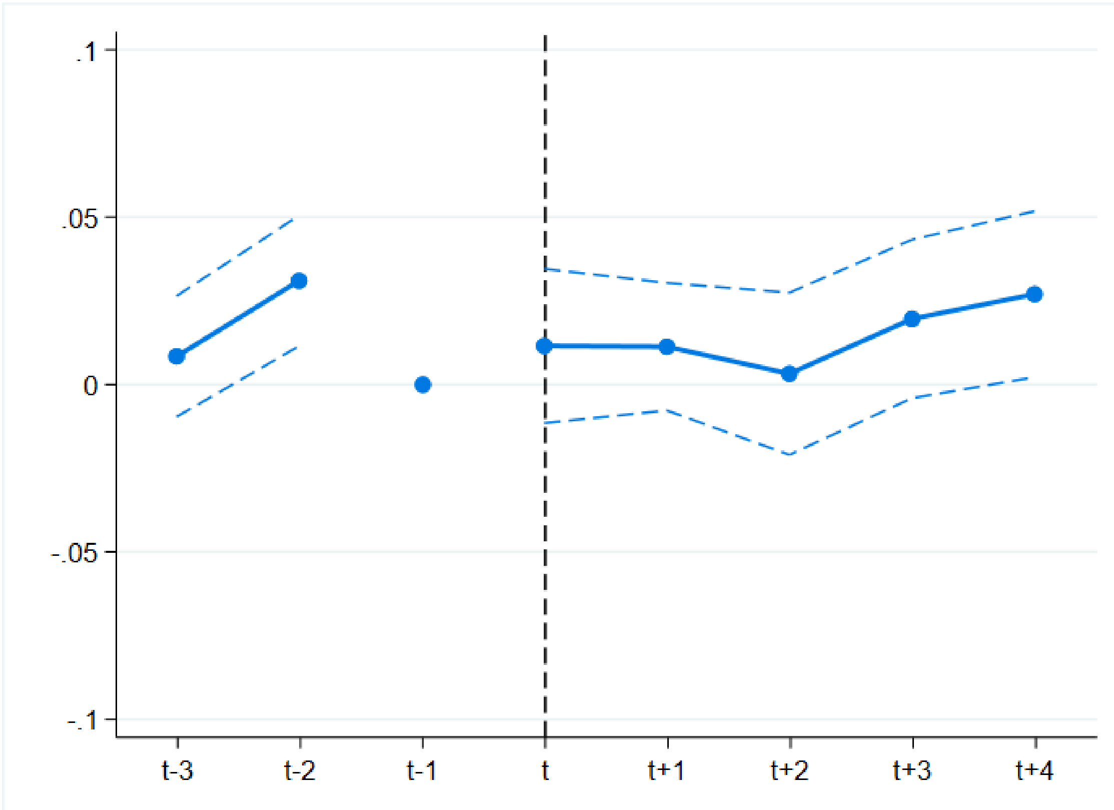
Note: Each region shaded in gray and outlined in white represents a county. The black lines delineate the state borders. The blue shaded counties represent the Washington-Arlington-Alexandria, DC-VA-MD-WV MSA, which comprises a treated observation in our across-MSA design.

Figure 2.7: Map of Chicago-Naperville-Elgin, IL-IN-WI Metropolitan Statistical Area



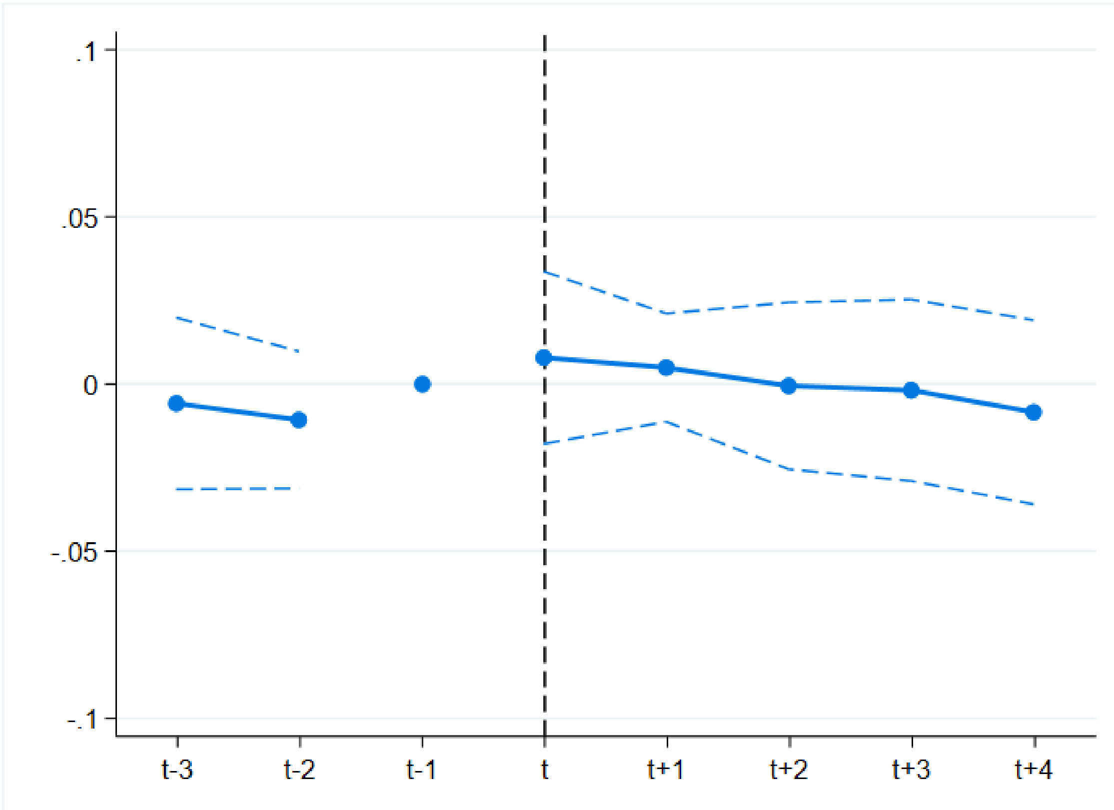
Note: Each region shaded in gray and outlined in white represents a county. The black lines delineate the state borders. The blue shaded counties represent the Illinois portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA. In our within-MSAs specification, the blue shaded counties are part of our treatment group. The orange shaded counties represent the Wisconsin and Indiana portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA. In our within-MSAs specification, the orange shaded counties represent part of the control group.

Figure 2.8: The Effect of BTB on the Probability of Employment: Black Men



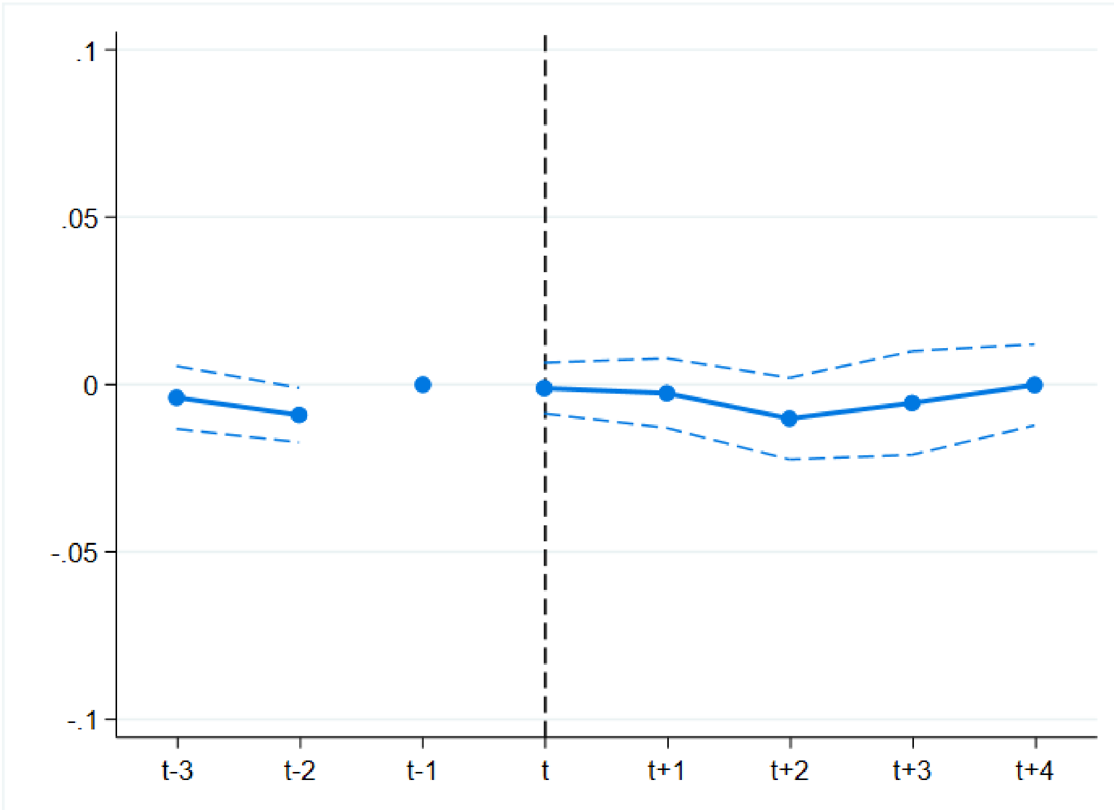
Note: This figure plots the coefficients and 95% confidence interval of the event study regression in Equation 2.3 for black men. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). The regression includes fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Individuals are considered treated if at least one central city in their MSA is covered by Ban the Box (BTB) as of December 15th of that year. Standard errors are clustered at the state level.

Figure 2.9: The Effect of BTB on the Probability of Employment: Hispanic Men



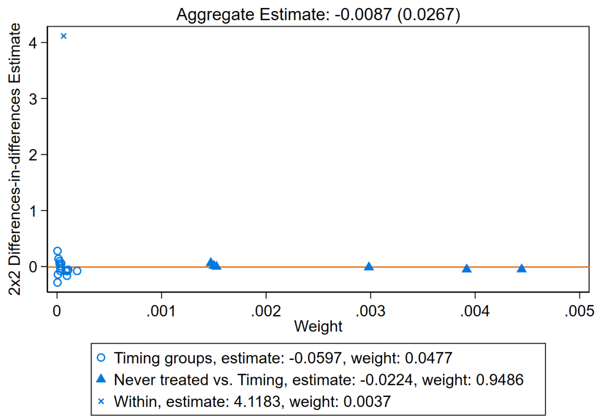
Note: This figure plots the coefficients and 95% confidence interval of of the event study regression in Equation 2.3 for Hispanic men. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Hispanic men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). The regression includes fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Individuals are considered treated if at least one central city in their MSA is covered by Ban the Box (BTB) as of December 15th of that year. Standard errors are clustered at the state level.

Figure 2.10: The Effect of BTB on the Probability of Employment: White Men

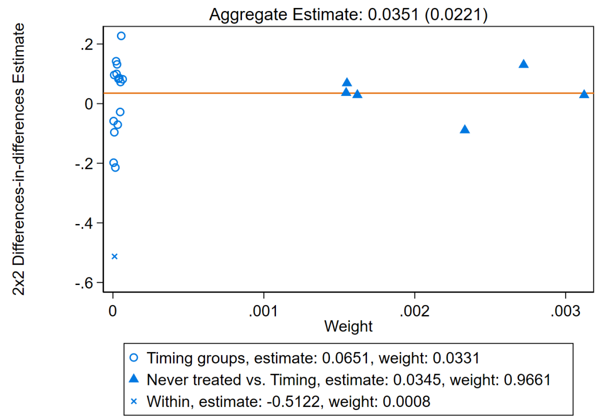


Note: This figure plots the coefficients and 95% confidence interval of of the event study regression in Equation 2.3 for white men. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). The regression includes fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Individuals are considered treated if at least one central city in their MSA is covered by Ban the Box (BTB) as of December 15th of that year. Standard errors are clustered at the state level.

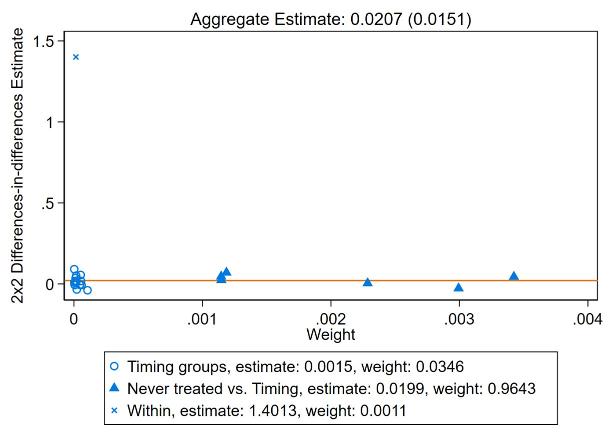
Figure 2.11: Goodman-Bacon (2021) Decomposition of Differences-in-Differences Components and Weights



(a) Black Men



(b) Hispanic Men



(c) White Men

Note: This figure plots the weights and constituent 2x2 differences-in-differences (DID) estimates from the decomposition in Goodman-Bacon (2021). Each panel includes data from a balanced panel of Metropolitan Statistical Areas (MSAs) with no covariates. “Timing groups” refers to comparisons between newly treated MSAs and not yet treated MSAs, “Never treated vs. Timing” refers to comparisons between treated MSAs and never-treated MSAs, and “Within” refers to comparisons between newly treated MSAs and MSAs that have already been treated. The horizontal line indicates the aggregate DID estimate, which is also given in each title.

Table 2.1: Summary Statistics of 25-34 Year Old Men, 2005-2014 American Community Survey

	(1) Full Sample	(2) MSAs Only
BTB	0.2059	0.2659
	(0.3930)	(0.4281)
Ever BTB	0.4992	0.6353
	(0.5000)	(0.4813)
White, non-Hispanic	0.7567	0.7242
	(0.4291)	(0.4469)
Black, non-Hispanic	0.1187	0.1253
	(0.3234)	(0.3311)
Hispanic	0.1246	0.1505
	(0.3302)	(0.3575)
Age	29.4887	29.4708
	(2.8776)	(2.8779)
Enrolled in school	0.1130	0.1236
	(0.3166)	(0.3291)
Less than high school	0.1040	0.0935
	(0.3053)	(0.2912)
High school diploma	0.1560	0.1412
	(0.3629)	(0.3482)
GED or alternative	0.0469	0.0407
	(0.2114)	(0.1977)
Some college	0.2366	0.2378
	(0.4250)	(0.4257)
Associate degree	0.0838	0.0807
	(0.2771)	(0.2723)
Bachelor's degree (or higher)	0.2888	0.3306
	(0.4532)	(0.4704)
Live in MSA	0.7281	1.0000
	(0.4449)	(0.0000)
Northeast	0.1717	0.1983
	(0.3772)	(0.3987)
Midwest	0.2380	0.1934
	(0.4259)	(0.3950)
South	0.3841	0.3693
	(0.4864)	(0.4826)
West	0.2061	0.2390
	(0.4045)	(0.4265)
Employed: ever BTB	0.8039	0.8058
	(0.3970)	(0.3955)
Employed: before BTB	0.8179	0.8210
	(0.3859)	(0.3834)
Employed: never BTB	0.7800	0.8037
	(0.4142)	(0.3972)
Annual earnings	38,921.67	41,111.12
	(37,051.64)	(39,582.47)
Observations	1,398,259	1,018,099

Note: Data are from the 2005-2014 waves of the American Community Survey. Sample consists of white (non-Hispanic), black (non-Hispanic), and Hispanic men ages 25-34. Each observation is an individual. Observations are coded as treated by Ban the Box (BTB) if they live in a metropolitan statistical area (MSA) in which at least one central city is covered by the policy. MSAs that are treated by BTB before or in 2005 are excluded from the sample. Annual earnings are conditional on being in the labor force.

Table 2.2: Summary Statistics of 25-34 Year Old Men by Race and Ethnicity and BTB Treatment Status, 2005-2014 American Community Survey

	White		Black		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)
BTB Status	Never	Ever	Never	Ever	Never	Ever
Age	29.5356	29.5211	29.4015	29.4007	29.3482	29.3166
	(2.8752)	(2.8753)	(2.8882)	(2.8842)	(2.8843)	(2.8704)
Enrolled in school	0.1021	0.1202	0.1088	0.1227	0.1201	0.1257
	(0.3027)	(0.3252)	(0.3114)	(0.3281)	(0.3251)	(0.3315)
Less than high school	0.0926	0.0571	0.2278	0.1631	0.1966	0.1875
	(0.2899)	(0.2320)	(0.4194)	(0.3695)	(0.3974)	(0.3903)
High school diploma	0.1670	0.1240	0.1943	0.1849	0.1818	0.1852
	(0.3730)	(0.3296)	(0.3957)	(0.3882)	(0.3857)	(0.3885)
GED or alternative	0.0510	0.0327	0.0877	0.0622	0.0593	0.0422
	(0.2199)	(0.1779)	(0.2829)	(0.2415)	(0.2361)	(0.2010)
Some college	0.2414	0.2220	0.2343	0.2621	0.2524	0.2542
	(0.4280)	(0.4156)	(0.4236)	(0.4398)	(0.4344)	(0.4354)
Associate degree	0.0932	0.0845	0.0523	0.0630	0.0793	0.0760
	(0.2907)	(0.2781)	(0.2225)	(0.2430)	(0.2702)	(0.2650)
Bachelor's degree (or higher)	0.2590	0.4116	0.1052	0.1726	0.1456	0.1758
	(0.4381)	(0.4921)	(0.3069)	(0.3779)	(0.3527)	(0.3806)
Live in MSA	0.4929	0.9135	0.5597	0.9704	0.7742	0.9565
	(0.4999)	(0.2811)	(0.4964)	(0.1694)	(0.4181)	(0.2040)
Northeast	0.1208	0.2483	0.0515	0.2324	0.0428	0.1980
	(0.3259)	(0.4320)	(0.2211)	(0.4224)	(0.2024)	(0.3985)
Midwest	0.2362	0.3155	0.0776	0.2459	0.0544	0.1104
	(0.4248)	(0.4647)	(0.2676)	(0.4306)	(0.2268)	(0.3134)
South	0.4949	0.2039	0.8325	0.3840	0.7075	0.1033
	(0.5000)	(0.4029)	(0.3734)	(0.4864)	(0.4549)	(0.3043)
West	0.1481	0.2323	0.0383	0.1376	0.1953	0.5883
	(0.3552)	(0.4223)	(0.1920)	(0.3445)	(0.3965)	(0.4921)
Employed	0.8238	0.8488	0.5095	0.5976	0.7552	0.7484
	(0.3810)	(0.3583)	(0.4999)	(0.4904)	(0.4300)	(0.4339)
Annual earnings	36,769	45,779	24,445	29,032	32,195	33,904
	(32,129)	(44,039)	(23,562)	(28,891)	(28,652)	(30,282)
Annual earnings (employed)	38,644	48,121	28,148	33,860	34,168	36,671
	(32,176)	(44,318)	(23,609)	(29,095)	(28,682)	(30,266)
In labor force	0.8841	0.9090	0.6150	0.7276	0.8215	0.8332
	(0.3201)	(0.2876)	(0.4866)	(0.4452)	(0.3829)	(0.3728)
Observations	545,079	513,024	81,413	84,539	73,703	100,501

Note: Data are from the 2005-2014 waves of the American Community Survey. Sample consists of white (non-Hispanic), black (non-Hispanic), and Hispanic men ages 25-34. Each observation is an individual. Observations are coded as treated by Ban the Box (BTB) if they live in a metropolitan statistical area (MSA) in which at least one central city is covered by the policy. MSAs that are treated by BTB before or in 2005 are also excluded. Annual earnings are conditional on being in the labor force.

Table 2.3: Replication of Doleac and Hansen (2020) Table A-13: Effect of BTB on the Probability of Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BTB x Black	-0.1934***	-0.1695***	-0.0044	-0.0051	-0.0049	-0.0050	-0.0128*
	(0.0134)	(0.0131)	(0.0048)	(0.0053)	(0.0047)	(0.0046)	(0.0071)
BTB x Hispanic	0.0175	0.0406***	0.0166*	0.0160	0.0130	0.0139	0.0155
	(0.0115)	(0.0095)	(0.0087)	(0.0108)	(0.0129)	(0.0132)	(0.0117)
BTB x White	0.0590***	0.0409***	0.0021	0.0034	0.0031	0.0023	0.0030
	(0.0065)	(0.0057)	(0.0032)	(0.0041)	(0.0046)	(0.0042)	(0.0048)
N	1,062,576	1,062,576	1,062,573	1,062,573	704,859	508,297	735,368
R^2	0.0353	0.1148	0.1550	0.1567	0.1404	0.1462	0.1652
Pre-Mean: Blk.	0.5617	0.5617	0.5617	0.5617	0.5758	0.5617	0.5266
Pre-Mean: Hisp.	0.7385	0.7385	0.7385	0.7385	0.7458	0.7385	0.7175
Pre-Mean: Wht.	0.8073	0.8073	0.8073	0.8073	0.8065	0.8073	0.7851
% Effect: Black	-34.44	-30.17	-0.79	-0.90	-0.85	-0.90	-2.43
% Effect: Hisp.	2.36	5.49	2.25	2.17	1.75	1.89	2.16
% Effect: White	7.31	5.06	0.26	0.43	0.38	0.29	0.38
MSA FE	X	X	X	X	X	X	X
Year-Region FE	X	X	X	X	X	X	X
Demographics		X	X	X	X	X	X
MSA lin. trends				X	X	X	X
Interact w/race			X	X	X	X	X
MSAs only					X		
BTB adopters						X	
2008 and later							X

Note: Results from the estimation specified in Equation 2.1. Data are from the 2004-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with at most an associate's degree. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. The wording of survey questions about employment was changed starting in 2008. Columns 4-7 replicate Table A-13 in Doleac and Hansen (2020). Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: Corrected Doleac and Hansen (2020) Table A-13: Effect of BTB on the Probability of Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BTB x Black	-0.1919***	-0.1669***	-0.0025	-0.0039	-0.0025	-0.0023	0.0003
	(0.0154)	(0.0147)	(0.0060)	(0.0078)	(0.0073)	(0.0076)	(0.0101)
BTB x Hispanic	0.0140	0.0395***	0.0094	0.0015	-0.0075	-0.0048	0.0081
	(0.0131)	(0.0106)	(0.0088)	(0.0065)	(0.0076)	(0.0072)	(0.0080)
BTB x White	0.0570***	0.0410***	0.0030	-0.0034	-0.0052	-0.0050	-0.0046
	(0.0064)	(0.0061)	(0.0036)	(0.0035)	(0.0044)	(0.0043)	(0.0042)
N	1,062,576	1,062,576	1,062,573	1,062,573	704,859	446,785	735,368
R^2	0.0355	0.1149	0.1550	0.1577	0.1415	0.1458	0.1664
Pre-Mean: Blk.	0.5648	0.5648	0.5648	0.5648	0.5813	0.5648	0.5271
Pre-Mean: Hisp.	0.7365	0.7365	0.7365	0.7365	0.7448	0.7365	0.7138
Pre-Mean: Wht.	0.8096	0.8096	0.8096	0.8096	0.8111	0.8096	0.7862
% Effect: Black	-33.98	-29.55	-0.44	-0.69	-0.43	-0.40	0.06
% Effect: Hisp.	1.90	5.36	1.28	0.20	-1.01	-0.65	1.14
% Effect: White	7.04	5.07	0.37	-0.42	-0.64	-0.61	-0.59
MSA FE	X	X	X	X	X	X	X
Year-Region FE	X	X	X	X	X	X	X
Demographics		X	X	X	X	X	X
MSA lin. trends				X	X	X	X
Interact w/race			X	X	X	X	X
MSAs only					X		
BTB adopters						X	
2008 and later							X

Note: Results from the estimation specified in Equation 2.1. Data are from the 2004-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with at most an associate's degree. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. The wording of survey questions about employment was changed starting in 2008. Estimates correct for coding discrepancies that are described in Section 2.4 of the paper. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5: The Effect of BTB on the Probability of Employment: Across-MSA Design

	(1)	(2)	(3)	(4)	(5)
BTB x Black	-0.2010***	-0.1802***	0.0015	0.0081	-0.0061
	(0.0160)	(0.0146)	(0.0069)	(0.0072)	(0.0088)
BTB x Hispanic	0.0301*	0.0561***	0.0061	0.0153	0.0021
	(0.0173)	(0.0150)	(0.0080)	(0.0129)	(0.0088)
BTB x White	0.0660***	0.0506***	0.0002	-0.0045	-0.0014
	(0.0073)	(0.0063)	(0.0043)	(0.0058)	(0.0042)
N	599,435	599,435	599,430	363,335	388,637
R^2	0.0364	0.1058	0.1384	0.1378	0.1425
Pre-BTB Mean: Black	0.5631	0.5631	0.5631	0.5631	0.5247
Pre-BTB Mean: Hispanic	0.7392	0.7392	0.7392	0.7392	0.7209
Pre-BTB Mean: White	0.8037	0.8037	0.8037	0.8037	0.7797
% Effect: Black	-35.70	-32.00	0.26	1.44	-1.17
% Effect: Hispanic	4.07	7.59	0.83	2.06	0.29
% Effect: White	8.21	6.29	0.02	-0.55	-0.18
MSA FE	X	X	X	X	X
Year-Division FE	X	X	X	X	X
Demographics		X	X	X	X
MSA linear trends					
Fully-interact with race			X	X	X
MSAs only	X	X	X	X	X
BTB-adopting only				X	
2008 and later					X

Note: Results from the estimation specified in Equation 2.2. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. The wording of survey questions about employment was changed starting in 2008. Column 5 omits all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: Group-Time Average Treatment Effects: Callaway and Sant'Anna (2020) Estimator

	(1) Black	(2) Hispanic	(3) White
BTB	0.0101	-0.0071	0.0107
	(0.0329)	(0.0260)	(0.0100)
N	2,822	2,853	3,077
Pre-BTB Mean	0.5631	0.7392	0.8037
% Effect	1.79	-0.96	1.33
MSA FE			
Year-Division FE			
Demographics	X	X	X
MSA linear trends			
Fully-interact with race			
MSAs only	X	X	X

Note: Results from the estimation specified in Equation (2.4). Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). Demographic controls include average age, years of education, and fraction currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. group-time average treatment effects estimated with inverse probability weighting. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: Robustness Checks: Treatment Timing Assumptions

	(1) Fraction	(2) January	(3) Half-Year	(4) December
BTB x Black	0.0015	0.0031	0.0001	-0.0012
	(0.0069)	(0.0067)	(0.0064)	(0.0060)
BTB x Hispanic	0.0061	0.0035	0.0013	0.0077
	(0.0080)	(0.0073)	(0.0073)	(0.0085)
BTB x White	0.0002	-0.0012	-0.0009	0.0012
	(0.0043)	(0.0039)	(0.0040)	(0.0039)
N	599,430	599,430	599,430	599,430
R^2	0.1384	0.1384	0.1384	0.1384
Pre-BTB Mean: Black	0.5631	0.5540	0.5599	0.5631
Pre-BTB Mean: Hispanic	0.7392	0.7304	0.7366	0.7392
Pre-BTB Mean: White	0.8037	0.7986	0.8022	0.8037
% Effect: Black	0.26	0.55	0.02	-0.22
% Effect: Hispanic	0.83	0.48	0.17	1.04
% Effect: White	0.02	-0.15	-0.11	0.15
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends				
Fully-interact with race	X	X	X	X
MSAs only	X	X	X	X
BTB-adopting only				
2008 and later				

Note: Results from the estimation specified in Equation 2.2. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Column 1 represents our preferred specification (Column 3 of Table 2.5). In Column 2, MSAs are treated if a BTB policy is implemented for a central city as of January 15th of that year. In Column 3, MSAs are treated if a BTB policy is in place in a central city for half the year (as of July 2nd). In Column 4, MSAs are treated if a BTB policy is implemented in a central city as of December 15th of that year. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.8: Robustness Checks: Model Specification

	Region FE (1)	Linear Time Trends (2)	Full Sample (3)
BTB x Black	-0.0043	-0.0038	0.0026
	(0.0074)	(0.0099)	(0.0076)
BTB x Hispanic	0.0152	-0.0016	0.0169**
	(0.0137)	(0.0144)	(0.0080)
BTB x White	-0.0023	-0.0056	0.0041
	(0.0043)	(0.0052)	(0.0036)
N	599,430	599,430	877,180
R^2	0.1379	0.1403	0.1563
Pre-BTB Mean: Black	0.5631	0.5631	0.5448
Pre-BTB Mean: Hispanic	0.7392	0.7392	0.7291
Pre-BTB Mean: White	0.8037	0.8037	0.8018
% Effect: Black	-0.76	-0.67	0.47
% Effect: Hispanic	2.06	-0.21	2.32
% Effect: White	-0.29	-0.70	0.51
MSA FE	X	X	X
Year-Division FE		X	X
Year-Region FE	X		
Demographics	X	X	X
MSA linear trends		X	
Fully-interact with race	X	X	X
MSAs only	X	X	
BTB-adopting only			
2008 and later			

Note: Results from the estimation specified in Equation 2.2 with modifications described below. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. In Column 1, year-by-Census-division fixed effects are replaced with year-by-Census-region fixed effects. Column 2 includes MSA-specific linear time trends interacted with race/ethnicity. Column 3 includes individuals who do not live in an MSA. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: The Effect of BTB on the Probability of Being Employed in the Public Sector: Across-MSA Design

	(1)	(2)	(3)
BTB x Black	-0.0002	-0.0066*	0.0026
	(0.0037)	(0.0034)	(0.0051)
BTB x Hispanic	-0.0011	0.0035	0.0012
	(0.0042)	(0.0062)	(0.0054)
BTB x White	-0.0039***	-0.0035**	-0.0022
	(0.0011)	(0.0015)	(0.0017)
N	599,430	363,335	388,637
R^2	0.0207	0.0196	0.0220
Pre-BTB Mean: Black	0.0384	0.0384	0.0348
Pre-BTB Mean: Hispanic	0.0354	0.0354	0.0341
Pre-BTB Mean: White	0.0395	0.0395	0.0384
% Effect: Black	-0.50	-17.12	7.39
% Effect: Hispanic	-3.10	9.79	3.52
% Effect: White	-9.83	-8.94	-5.79
MSA FE	X	X	X
Year-Division FE	X	X	X
Demographics	X	X	X
MSA linear trends			
Fully-interact with race	X	X	X
MSAs only	X	X	X
BTB-adopting only		X	
2008 and later			X

Note: Results from the estimation specified in Equation 2.2. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. The wording of survey questions about employment was changed starting in 2008. Column 3 omits all years prior to 2008 and all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10: Robustness Checks: Employment of Other Groups

	Women: Ages 25-34 (1)	Men: Ages 35-44 (2)	Men: Ages 45-54 (3)	Men: Ages 55-64 (4)
BTB x Black	-0.0080	0.0142	0.0049	-0.0008
	(0.0086)	(0.0093)	(0.0069)	(0.0072)
BTB x Hispanic	0.0016	0.0225**	0.0198**	-0.0010
	(0.0074)	(0.0099)	(0.0080)	(0.0117)
BTB x White	0.0045	0.0008	0.0028	-0.0034
	(0.0039)	(0.0039)	(0.0037)	(0.0041)
N	524,161	577,852	740,748	627,393
R^2	0.0572	0.1142	0.0941	0.0785
Pre-BTB Mean: Black	0.6608	0.6318	0.6072	0.4915
Pre-BTB Mean: Hispanic	0.6302	0.7805	0.7588	0.6125
Pre-BTB Mean: White	0.6608	0.8268	0.8022	0.6392
% Effect: Black	-1.21	2.25	0.81	-0.16
% Effect: Hispanic	0.25	2.88	2.61	-0.17
% Effect: White	0.68	0.10	0.35	-0.54
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends				
Fully-interact with race	X	X	X	X
MSAs only	X	X	X	X
BTB-adopting only				
2008 and later				

Note: Results from the estimation specified in Equation 2.2. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white individuals with no college degree (two-year or four-year). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. In Column 1, sample restricted to black, Hispanic, and white women ages 25-34 with no college degree (two-year or four-year). In Column 2, sample restricted to black, Hispanic, and white men ages 35-44 with no college degree (two-year or four-year). In Column 3, sample restricted to black, Hispanic, and white men ages 45-54 with no college degree (two-year or four-year). In Column 4, sample restricted to black, Hispanic, and white men ages 55-64 with no college degree (two-year or four-year). Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.11: Heterogeneous Employment Effects: Education

	Less than High Sch. (1)	GED or equiv. (2)	High Sch. Degree (3)	Some College (4)	Associates Degree (5)	Bachelors Degree (6)
BTB x Black	-0.0036	0.0077	0.0044	-0.0103	0.0156	-0.0101
	(0.0141)	(0.0274)	(0.0138)	(0.0124)	(0.0253)	(0.0115)
BTB x Hispanic	0.0213	-0.0374	-0.0096	0.0196*	-0.0149	0.0273***
	(0.0183)	(0.0242)	(0.0166)	(0.0102)	(0.0176)	(0.0084)
BTB x White	0.0008	-0.0362**	0.0041	-0.0004	0.0009	-0.0037
	(0.0112)	(0.0135)	(0.0060)	(0.0048)	(0.0068)	(0.0024)
N	95,202	41,416	143,690	242,065	82,063	336,492
R^2	0.1535	0.1232	0.0604	0.0624	0.0540	0.0649
Pre-BTB Mean: Black	0.2895	0.3044	0.5954	0.7236	0.7951	0.8797
Pre-BTB Mean: Hisp.	0.5912	0.5978	0.7742	0.8246	0.8680	0.8956
Pre-BTB Mean: White	0.6120	0.6471	0.8011	0.8604	0.8967	0.9252
% Effect: Black	-1.25	2.53	0.74	-1.43	1.96	-1.14
% Effect: Hispanic	3.60	-6.26	-1.24	2.38	-1.72	3.04
% Effect: White	0.13	-5.59	0.51	-0.04	0.10	-0.40
MSA FE	X	X	X	X	X	X
Year-Division FE	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
MSA linear trends						
Fully-interact with race	X	X	X	X	X	X
MSAs only	X	X	X	X	X	X
BTB-adopting only						
2008 and later						

Note: Results from the estimation specified in Equation 2.2. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and an indicator for being currently enrolled in school. In Column 1, sample restricted to individuals with less than a high-school degree. In Column 2, sample restricted to individuals with a GED and no additional education. In Column 3, sample restricted to individuals with a high-school degree and no additional education. In Column 4, sample restricted to individuals with some college but no degree. In Column 5, sample restricted to individuals with an Associate degree and no additional education. In Column 6, sample restricted to individuals with a Bachelor's degree. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.12: Heterogeneous Employment Effects: Legal Jurisdiction of BTB Policy

	(1)	(2)	(3)
City BTB x Black	0.0040	0.0143	-0.0049
	(0.0082)	(0.0093)	(0.0098)
City BTB x Hispanic	0.0095	0.0143	0.0060
	(0.0077)	(0.0111)	(0.0102)
City BTB x White	0.0003	-0.0017	-0.0029
	(0.0049)	(0.0059)	(0.0056)
County BTB x Black	0.0088	0.0112	0.0115
	(0.0159)	(0.0157)	(0.0203)
County BTB x Hispanic	0.0108	0.0303*	0.0249
	(0.0149)	(0.0164)	(0.0164)
County BTB x White	0.0069	0.0041	0.0040
	(0.0055)	(0.0066)	(0.0066)
State BTB x Black	-0.0095	-0.0077	-0.0138
	(0.0152)	(0.0195)	(0.0232)
State BTB x Hispanic	0.0024	0.0201	-0.0126
	(0.0131)	(0.0158)	(0.0117)
State BTB x White	-0.0002	-0.0045	0.0055
	(0.0063)	(0.0078)	(0.0070)
N	599,430	363,335	388,637
R^2	0.1385	0.1378	0.1425
Pre-City-BTB Mean: Black	0.5717	0.5717	0.5342
Pre-City-BTB Mean: Hispanic	0.7361	0.7361	0.7191
Pre-City-BTB Mean: White	0.8036	0.8036	0.7790
% Effect City: Black	0.70	2.51	-0.92
% Effect City: Hispanic	1.30	1.94	0.84
% Effect City: White	0.04	-0.21	-0.37
Pre-County-BTB Mean: Black	0.5805	0.5805	0.5512
Pre-County-BTB Mean: Hispanic	0.7551	0.7551	0.7429
Pre-County-BTB Mean: White	0.8009	0.8009	0.7778
% Effect County: Black	1.52	1.92	2.08
% Effect County: Hispanic	1.43	4.02	3.35
% Effect County: White	0.86	0.51	0.51
Pre-State-BTB Mean: Black	0.5079	0.5079	0.4602
Pre-State-BTB Mean: Hispanic	0.7369	0.7369	0.7097
Pre-State-BTB Mean: White	0.7997	0.7997	0.7735
% Effect State: Black	-1.87	-1.52	-3.01
% Effect State: Hispanic	0.33	2.73	-1.78
% Effect State: White	-0.03	-0.57	0.71

Note: Results from the estimation specified in Equation 2.2 with modifications described below. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in an MSA. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. For each race, there are 3 treatment variables (BTB) that equal the fraction of the year in which a ban-the-box policy was implemented by a central city, county covering a central city, or state covering a central city of the MSA, respectively. Column 2 restricts the sample to MSAs that are covered by BTB during our sample period. The wording of survey questions about employment was changed starting in 2008. Column 3 omits all years prior to 2008 and all MSAs that were covered by Ban the Box prior to 2009. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.13: Heterogeneous Employment Effects: Sector Covered by BTB

	(1)	(2)	(3)
Public Only BTB x Black	-0.0006	0.0049	-0.0108
	(0.0079)	(0.0083)	(0.0101)
Public Only BTB x Hispanic	0.0043	0.0166	0.0036
	(0.0087)	(0.0141)	(0.0097)
Public Only BTB x White	0.0027	-0.0023	-0.0014
	(0.0042)	(0.0059)	(0.0035)
Public + Contract BTB x Black	0.0104	0.0244	0.0177
	(0.0165)	(0.0179)	(0.0154)
Public + Contract BTB x Hispanic	0.0131	0.0110	0.0092
	(0.0131)	(0.0145)	(0.0159)
Public + Contract BTB x White	-0.0065	-0.0108	0.0031
	(0.0059)	(0.0068)	(0.0084)
Public + Contract + Private BTB x Black	0.0089	0.0373*	0.0123
	(0.0184)	(0.0190)	(0.0193)
Public + Contract + Private BTB x Hispanic	0.0153	0.0165	-0.0117
	(0.0138)	(0.0196)	(0.0122)
Public + Contract + Private BTB x White	-0.0056	-0.0127	-0.0058
	(0.0098)	(0.0125)	(0.0108)
N	599,430	363,335	388,637
R^2	0.1385	0.1378	0.1425
Pre-Public-BTB Mean: Black	0.5755	0.5755	0.5403
Pre-Public-BTB Mean: Hispanic	0.7521	0.7521	0.7325
Pre-Public-BTB Mean: White	0.8061	0.8061	0.7841
% Effect Public: Black	-0.11	0.85	-1.99
% Effect Public: Hispanic	0.57	2.21	0.49
% Effect Public: White	0.34	-0.29	-0.17
Pre-Contract-BTB Mean: Black	0.5372	0.5372	0.5085
Pre-Contract-BTB Mean: Hispanic	0.6984	0.6984	0.6950
Pre-Contract-BTB Mean: White	0.7806	0.7806	0.7506
% Effect Contract: Black	1.94	4.54	3.48
% Effect Contract: Hispanic	1.87	1.58	1.32
% Effect Contract: White	-0.84	-1.39	0.41
Pre-Private-BTB Mean: Black	0.5240	0.5240	0.5081
Pre-Private-BTB Mean: Hispanic	0.7171	0.7171	0.6804
Pre-Private-BTB Mean: White	0.7979	0.7979	0.7715
% Effect Private: Black	1.70	7.12	2.41
% Effect Private: Hispanic	2.14	2.30	-1.71
% Effect Private: White	-0.70	-1.59	-0.75

Note: Results from the estimation specified in Equation 2.2 with modifications described below. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in an MSA. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. For each race, there are 3 treatment variables (BTB) that equal the fraction of the year in which a ban-the-box policy covered public, public + contract, or public + contract + private-sector employment for a central city in the MSA, respectively. Column 2 restricts the sample to MSAs that are covered by BTB during our sample period. The wording of survey questions about employment was changed starting in 2008. Column 2 restricts the sample to MSA-state units that are covered by BTB during our sample period. The wording of survey questions about employment was changed starting in 2008. Column 3 omits all years prior to 2008 and all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.14: The Effect of BTB on the Probability of Employment: Within-MSA Design

	(1)	(2)	(3)	(4)
BTB x Black	-0.2676***	-0.2352***	-0.0484	-0.0300
	(0.0229)	(0.0218)	(0.0425)	(0.0543)
BTB x Hispanic	-0.0264	0.0144	-0.0261	-0.0711***
	(0.0330)	(0.0291)	(0.0262)	(0.0141)
BTB x White	0.0261*	0.0140	-0.0177**	-0.0125
	(0.0141)	(0.0133)	(0.0084)	(0.0078)
N	127,067	127,067	127,065	63,883
R^2	0.0349	0.1082	0.1403	0.1483
Pre-BTB Mean: Black	0.5745	0.5745	0.5745	0.5549
Pre-BTB Mean: Hispanic	0.7424	0.7424	0.7424	0.7366
Pre-BTB Mean: White	0.8069	0.8069	0.8069	0.7851
% Effect: Black	-46.59	-40.94	-8.42	-5.40
% Effect: Hispanic	-3.55	1.94	-3.52	-9.65
% Effect: White	3.24	1.74	-2.19	-1.59
MSA-Year FE	X	X	X	X
Demographics		X	X	X
Fully-interact with race			X	X
MSAs only	X	X	X	X
BTB-adopting only				
2008 and later				X

Note: Results from the estimation specified in Equation 2.5. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA) with at least 1 MSA-state unit that is covered by BTB during our sample period. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA-state unit in which the worker lives. If the MSA-state unit does not contain a central city, treatment equals the fraction of the year in which BTB is in effect for the state. The wording of survey questions about employment was changed starting in 2008. Column 4 omits all MSA-state units that are in MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.15: The Effect of BTB on the Probability of Being Employed in the Public Sector: Within-MSA design

	(1)	(2)
BTB x Black	-0.0048	-0.0014
	(0.0035)	(0.0038)
BTB x Hispanic	0.0014	-0.0006
	(0.0064)	(0.0030)
BTB x White	0.0002	-0.0015
	(0.0031)	(0.0043)
N	127,065	63,883
R^2	0.0208	0.0230
Pre-BTB Mean: Black	0.0389	0.0365
Pre-BTB Mean: Hispanic	0.0398	0.0433
Pre-BTB Mean: White	0.0423	0.0428
% Effect: Black	-12.42	-3.84
% Effect: Hispanic	3.50	-1.50
% Effect: White	0.48	-3.62
MSA-Year FE	X	X
Demographics	X	X
Fully-interact with race	X	X
MSAs only	X	X
BTB-adopting only		
2008 and later		X

Note: Results from the estimation specified in Equation 2.5. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA) with at least 1 MSA-state unit that is covered by BTB during our sample period. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA-state unit in which the worker lives. If the MSA-state unit does not contain a central city, treatment equals the fraction of the year in which BTB is in effect for the state. The wording of survey questions about employment was changed starting in 2008. Column 2 omits all years prior to 2008 and all MSA-state units in MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.16: Heterogeneous Employment Effects: Legal Jurisdiction of BTB policy: Within-MSA design

	(1)	(2)
City BTB x Black	-0.0605	-0.0495
	(0.0424)	(0.0567)
City BTB x Hispanic	-0.0186	-0.0732***
	(0.0275)	(0.0144)
City BTB x White	-0.0114	-0.0138
	(0.0074)	(0.0096)
County BTB x Black	0.1343*	0.1890**
	(0.0757)	(0.0767)
County BTB x Hispanic	0.0049	0.1705
	(0.0563)	(0.1329)
County BTB x White	-0.0137**	-0.0037
	(0.0059)	(0.0117)
State BTB x Black	-0.0176	0.0022
	(0.0400)	(0.0544)
State BTB x Hispanic	-0.0033	-0.1017***
	(0.0608)	(0.0349)
State BTB x White	-0.0375**	-0.0451***
	(0.0141)	(0.0156)
N	127,065	63,883
R^2	0.1404	0.1486
Pre-City-BTB Mean: Black	0.5623	0.5451
Pre-City-BTB Mean: Hispanic	0.7363	0.7269
Pre-City-BTB Mean: White	0.8016	0.7824
% Effect City: Black	-10.75	-9.08
% Effect City: Hispanic	-2.53	-10.07
% Effect City: White	-1.42	-1.77
Pre-County-BTB Mean: Black	0.5767	0.5573
Pre-County-BTB Mean: Hispanic	0.8140	0.8104
Pre-County-BTB Mean: White	0.8189	0.7966
% Effect County: Black	23.30	33.91
% Effect County: Hispanic	0.60	21.04
% Effect County: White	-1.68	-0.46
Pre-State-BTB Mean: Black	0.5424	0.5743
Pre-State-BTB Mean: Hispanic	0.7677	0.8105
Pre-State-BTB Mean: White	0.8026	0.7968
% Effect State: Black	-3.24	0.39
% Effect State: Hispanic	-0.43	-12.55
% Effect State: White	-4.67	-5.66

Note: Results from the estimation specified in Equation 2.5 with modifications described below. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA) with at least 1 MSA-state unit that is covered by BTB during our sample period. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. For each race, there are 3 treatment variables (BTB) that equal the fraction of the year in which a ban-the-box policy was implemented by a central city, county covering a central city, or state covering a central city of the MSA-state unit, respectively. If the MSA-state unit does not contain a central city, only the state law variable may be nonzero. The wording of survey questions about employment was changed starting in 2008. Column 2 omits all years prior to 2008 and all MSA-state units in MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

CHAPTER 3

DO UBER AND LYFT REDUCE DRUNK-DRIVING FATALITIES?

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This paper investigates whether Uber and Lyft lead to reductions in drunk driving, as measured by city-level drunk-driver-related motor vehicle fatalities and fatal crashes. I use a difference-in-differences method that exploits the variation in the timing of Uber and Lyft entry for the 100 most populous U.S. cities and a Poisson model to account for the fact that crashes and fatalities are count data. Using monthly city-level Fatality Analysis Reporting System (FARS) data for 2006 to 2016, I find small declines in drunk-driver-related fatal motor vehicle incidents and small increases in overall fatal motor vehicle incidents, but I cannot reject the null hypothesis of no effect of Uber or Lyft on these outcomes. Event studies suggest that drunk-driver-related and overall fatal motor vehicle incidents decline several years after the entry of Uber or Lyft into a city.

3.1 Introduction

Approximately 30% of motor vehicle fatalities in the U.S. involved a drunk driver between 2006 and 2016 (National Highway Traffic Safety Administration, 2017).[1] In 2016 alone, 10,497 people died from a motor vehicle crash involving a drunk driver (National Highway Traffic Safety Administration, 2017). In addition, motor vehicle fatalities are a leading cause of death for young people (Centers for Disease Control and Prevention, 2018). Among 15 to 24 year-olds in 2016, unintentional motor vehicle fatalities were the leading cause of death.[2] For 25 to 34 year-olds in 2016, unintentional motor vehicle fatalities were the third leading cause of death, behind unintentional poisoning and suicide (Centers for Disease Control and Prevention, 2018). Moreover, in 2016 over 1 million arrests were made for Driving Under the Influence (DUI) (Department of Justice, 2017). Drunk-driving crashes also generate an estimated cost of $44 billion per year (National Highway Traffic Safety Administration, 2017).

Drunk driving poses a significant negative externality in several ways. When a drunk driver crashes, there may be externalities in the form of deaths or injuries of other passengers in the drunk driver's car, or pedestrians, cyclists, and occupants of any other vehicles involved in the crash. Drunk driving also generates externalities from expenditures due to auto or health insurance claims (increases in costs are partially borne by higher premiums for everybody) and from increased expenditures on public safety (e.g. DUI enforcement). Negative externalities such as these have traditionally been an economic rationale for government intervention. But what if the "free market" could reduce the size of this negative externality? Uber often makes this claim. In a post on their website, Uber claims that as Uber use in Pennsylvania increases, DUI rates fall (Uber, 2014).

As part of their supporting evidence for the claim that Uber is associated with a reduction in

[1] A drunk driver is defined as someone with a blood alcohol concentration greater than or equal to 0.08 g/dL, which was the legal limit for driving under the influence (DUI) in all 50 states and Washington, D.C. during this time period.

[2] The next four leading causes of death were suicide, homicide, unintentional poisoning (includes drug and alcohol overdoses), and malignant neoplasms (cancer).

drunk driving, they provide a graph of Saturday night ride requests in Pittsburgh by time of day (see Figure 3.1). The graph does show a spike in requests around the time the bars close; however, that's not conclusive evidence that consumers are substituting toward Uber (or Lyft) and away from drunk driving. These ride requests could be coming from individuals who would have taken a taxi, walked, rode the bus, or bicycled, as opposed to driving drunk.

This paper answers the following question: have the introduction of ridesharing services such as Uber and Lyft led to a reduction in drunk driving, as measured by city-level drunk-driver-related motor vehicle fatalities and fatal crashes? I also examine whether ridesharing services affect overall crashes and fatalities, because if Uber and Lyft create more cars on the road then any effect on drunk driving could potentially be offset by heavier congestion.

Figure 3.2 illustrates a simplified, hypothetical market for drunk driving, with a downward-sloping "perceived marginal benefit of drunk driving" curve, an upward sloping "perceived marginal private cost of drunk driving" curve, and a much higher upward sloping "marginal social cost of drunk driving" curve. I have drawn the marginal cost curves as upward sloping to denote the fact that when there are more drunk drivers on the road, the roads become more dangerous (hence there is a higher marginal cost). The initial equilibrium quantity of drunk driving is represented by Q_0, while the initial socially efficient quantity of drunk driving is much lower and represented by Q_{soc}.[3] The entry of Uber or Lyft into a city represents a reduction in the price of a substitute for drunk driving. Under a comparative statics analysis, the entry of Uber or Lyft into a city will shift the "perceived marginal benefit of drunk driving" curve inward, leading to a new (partial) equilibrium quantity of drunk driving, denoted $Q_{post-Uber/Lyft}$.[4]

But what are the general equilibrium effects of Uber and Lyft? While these services represent

[3]Note: this illustration is stylized, and it is entirely possible that the actual marginal social cost of drunk driving is so high that the socially efficient quantity of drunk driving equals 0. The precise socially efficient quantity of drunk driving is beyond the scope of this paper.

[4]In this partial equilibrium scenario, the entry of Uber/Lyft does not affect the perceived marginal cost of drunk driving. Uber and Lyft may have a general equilibrium effect on the perceived marginal cost, which is described in more detail below.

a reduction in the price of a substitute to drunk driving, they also represent a reduction in the cost of drinking. There is evidence that Uber has led to increases in alcohol consumption (Teltser, Lennon, and Burgdorf, 2021; Zhou, 2020).[5] There may also be more cars on the road post-Uber and Lyft entry. The first effect may lead to an increase in the quantity of drunk driving. The second effect may lead to an increase in the quantity of drunk-driver-related motor vehicle crashes, as more cars on the road, ceteris paribus, means more cars for a drunk driver to potentially crash into. These offsetting potential effects make the impact of Uber and Lyft on drunk driving theoretically ambiguous and therefore an empirical question. A secondary economic motivation is that Uber and Lyft represent an unusual case of how health can be affected by firm entry and innovation.

Uber and Lyft are ridesharing services that operate through smartphone apps. Riders open the app, select their pickup location on a map, and request a ride. The driver transports the rider to the rider's destination. The apps require a credit card on file, and the app automatically charges the rider's credit card at the end of the ride. The main differences between Uber or Lyft and a taxi is that riders can request a ride through an easy-to-use app on their phone (they don't have to call a cab company or stand on a street corner), they can track the driver's realtime progress to the pickup location through the app, and payment occurs automatically, which means riders do not have to carry cash. In other words, Uber and Lyft reduce the time cost and increase the convenience of transportation.

Uber was founded in 2009, and in July 2010, it launched in San Francisco. The initial service only had black cars (known today as UberBlack), which are more expensive than taxis. In 2011 Uber expanded to New York City. In June 2012, Lyft started in San Francisco. Lyft typically enters cities after Uber, although it did launch before Uber in a few cities. In July 2012, Uber launched UberX, a cheaper version of Uber. UberX, when not in surge-pricing mode, is usually cheaper than

[5] Thinking of the general equilibrium version of Figure 3.2, increased alcohol consumption could lower the perceived marginal cost of drunk driving, which may lead to more drunk driving. This outcome could arise if people drive to the bar (sober), drink more because they can rely on Uber/Lyft to transport them home, but then drunkenly decide that they are capable of driving themselves.

a taxi. By early 2014, Uber had expanded to 50 of the 100 largest U.S. cities, and by late 2015, it had expanded to all but 2 of the 100 largest U.S. cities (Uber and Lyft, 2017).

Taking advantage of the staggered rollout of these ridesharing programs across cities, this paper uses a difference-in-differences approach to test whether there were reductions in drunk-driver-related motor vehicle fatalities and fatal crashes after the introduction of Uber and Lyft into a city. I use city-level motor vehicle fatality data for 2006 to 2016 from the Fatality Analysis Reporting System (FARS). Figure 3.3 shows the variation in Uber and Lyft entry across the 100 most populous U.S. cities over my sample period (2006 to 2016). There are two important features of the histogram to note. First, there is variation in the timing of Uber and Lyft entry. Second, there does not appear to be seasonality in the timing of Uber and Lyft entry (drunk driving fatalities and fatal crashes do exhibit some seasonality). One source of endogeneity would be if Uber and Lyft timed their entry into a city with peak drunk driving incidents. I test for parallel pre-trends with event studies to address whether this particular source of endogeneity is likely to be a concern.

This paper contributes to the literature on the effect of ridesharing on drunk driving. The most closely related paper estimates the impact of ridesharing on drunk driving for all U.S. cities with a population of at least 100,000 (Martin-Buck, 2017). He finds that for the period 2000-2014, ridesharing leads to reductions in drunk-driving-related crashes. Brazil and Kirk (2016) use a difference-in-differences method on county-level Fatality Analysis Reporting System (FARS) data for the counties containing the 100 largest metropolitan areas, and they do not reject the null hypothesis of no effect on motor vehicle fatalities. Dills and Mulholland (2018) use a difference-in-differences method on county-level FARS data for all U.S. counties, and they find that the decline in motor vehicle fatalities and fatal crashes becomes larger the longer Uber has been in a county. Greenwood and Wattal (2017) study the arrival of UberX in California and find that it leads to a 3.6% to 5.6% decline in motor vehicle fatalities per quarter. Peck (2017) finds a 25-35% reduction in the alcohol-related crash rate in New York City. In contrast, Barrios, Hochberg, and Yi (2020) find a 3% increase in overall traffic fatalities. In Brazil, Barreto, Neto, and Carazza (forthcoming) find that Uber leads to a 10% reduction in traffic fatalities.

Another strand of the literature has examined the effects of other drunk driving substitutes on measures of drunk driving. Chung, Joo, and Moon (2014) examine the impact of designated driver services in South Korea, and they find that an increase in the number of companies is associated with a reduction in alcohol-involved and overall traffic fatalities in 4 metropolitan areas and 8 provinces (Chung, Joo, and Moon, 2014). Jackson and Owens (2011) exploit the D.C. metro's late-night service expansions to examine the effect of public transportation on drunk driving. They find that the later operating hours of the metro reduced the probability of a DUI arrest in neighborhoods with bars near a Metro station, but that there was no effect on the probability of being arrested for DUI over all neighborhoods (Jackson and Owens, 2011).

This paper contributes to the existing literature in several ways. First, by restricting my sample to the 100 most populous U.S. cities, 98 of which have Uber or Lyft by the end of my sample period, I rely almost exclusively on the variation in the timing rather than whether entered. Cities that have never had Uber or Lyft might not be good controls: they are smaller, less population dense, and more rural than treated cities. Second, I examine city-level outcomes, which is arguably a more accurate measure of the treatment effect than county-level outcomes, because Uber and Lyft entry happens at the city level. Third, compared to most of the other papers on ridesharing in the U.S., (Brazil and Kirk, 2016; Dills and Mulholland, 2018; Martin-Buck, 2017) I use at least one additional year of data in the post-period. Finally, I contribute to the broader literature on determinants of drunk driving (Carpenter, Dobkin, and Warman, 2016; Lovenheim and Steefel, 2011; Dee, 1999; Eisenberg, 2003; Freeman, 2007; Hansen, 2015; Kenkel and Koch, 2001).

I find that the presence of Uber or Lyft in a city has mixed effects on motor vehicle fatalities and fatal crashes. Event study specifications provide suggestive evidence of longer-term effects of Uber and Lyft on fatal incidents, particularly for drunk-driver-related incidents (Figures 3.5

through 3.8). However, using the standard difference-in-differences method, I cannot reject the null hypothesis of no effect of Uber or Lyft on either drunk-driver-related or all fatal motor vehicle incidents.

The remainder of the paper proceeds as follows: Section 3.2 outlines the conceptual framework and the method I use, Section 3.3 describes the data, Section 3.4 presents the results of the difference-in-differences estimation and event studies, Section 3.5 incorporates robustness checks and alternative specifications, and Section 3.6 concludes.

3.2 Model, Identification & Methods

3.2.1 Model of Individual's Decision to Drive After Drinking

An individual's decision to drive drunk can be modeled with the following equation:

$$Prob(DD) = f(P_{DD}, P_{complements}, \mathbf{P_{substitutes}}, alc) \tag{3.1}$$

$Prob(DD)$ represents the probability an individual drives drunk. P_{DD} represents the implicit price of drunk driving, which includes the perceived risks of being arrested and crashing. $P_{complements}$ represents the implicit price of complements (e.g. alcohol). $\mathbf{P_{substitutes}}$ represents the implicit price of substitutes (e.g., walking, bicycling, taking public transit, hailing a taxi, **or using Uber or Lyft**). alc represents alcohol consumption, which could affect one's perception of one's cognitive and motor skills (e.g., perceived ability to drive safely). If the individual has already decided to drive, increasing alc increases the probability of driving drunk. Risk aversion affects P_{DD} by affecting the perceived risks of being arrested and crashing. The relative prices of drunk driving and its substitutes are also affected by the distance one has to travel.

I am unable to directly observe the probability that an individual drives drunk, but I do observe a measure of drunk-driving-related fatalities and fatal crashes. Drunk-driving fatalities can be modeled with the following equation:

$$DD\ fatalities = f(miles\ DD, \frac{fatal\ crash\ rate}{mile}, \frac{fatalities}{fatal\ crash}) \tag{3.2}$$

$DD\ fatalities$ represent drunk driving fatalities. $miles\ DD$ represent miles driven drunk. Increasing **Prob**(**DD**) leads to an increase in $\mathbb{E}$[**miles DD**]. I am able to observe the left-hand side of equation 3.2 as well as $\frac{fatalities}{fatal\ crash}$.

I estimate a reduced-form equation of equation 3.2:

$$DD\ F = f(ridesharing,\ city\ characteristics,\ city\ +\ time\ FE) \tag{3.3}$$

$city\ characteristics$ represents population characteristics and the unemployment rate. $city\ +\ time\ FE$ represent city and time fixed effects. I have not included alcohol consumption because alcohol consumption is an intermediate outcome (Uber and Lyft lead to increased alcohol consumption, which may lead to increases in drunk driving), and Uber or Lyft's effect on alcohol consumption could be a mechanism for how they affect drunk driving.[6]

3.2.2 Difference-in-Differences Identification and Assumptions

I estimate a difference-in-differences model in which an indicator for the presence of Uber or Lyft is my treatment variable and motor vehicle fatalities and fatal crashes are my outcome variables. Identification rests on two assumptions:

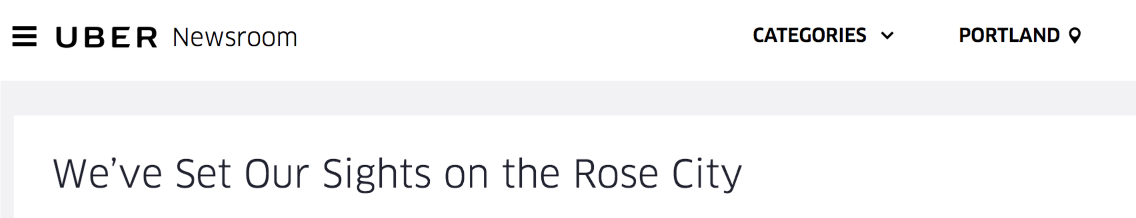
[6]Including intermediate outcomes in a regression can lead to collider bias, which would provide an inaccurate estimate of the effect of Uber and Lyft on fatal motor vehicle incidents.

1. *Parallel trends:* in the absence of Uber and Lyft, trends in motor vehicle fatalities and fatal crashes would be the same across treated and untreated cities
2. There are no other concurrent changes at the time of Lyft or Uber's entry into the treated cities that affect motor vehicle fatalities

As with all difference-in-differences studies, the greatest threat to identification is policy endogeneity. If Uber and Lyft are not entering cities randomly, and are in fact systematically targeting cities where drunk driving is increasing at faster rates than other cities, then the difference-in-differences model's results would be biased. However, if the pre-implementation trends in the outcome variables are parallel relative to cities without Lyft or Uber, then this type of policy endogeneity may not be not an issue.

According to an employee at Lyft, the decision to enter a given city was primarily influenced by the population density and response to competition from Uber (Gigante, phone interview, October 10, 2017). In some cities, Lyft decided to enter the market because the city explicitly welcomed ridesharing companies. In Indianapolis, the mayor's office and the chief of police were concerned with drunk driving and viewed ridesharing companies as a solution for reducing drunk driving.[7]

However, cities were not always in favor of Uber and Lyft, and in some cases, they banned them outright or created restrictions to delay their arrival. In these cities, Uber and Lyft wanted to operate months or years before they were legally allowed to do so. Portland, Oregon is one such example.



[7]If such a scenario were happening systematically, that would be concerning from a policy endogeneity standpoint.

https://newsroom.uber.com/us-oregon/hello_portland/

In July 2013, Uber *wanted* to operate in Portland but was barred due to regulations. 21 months later, in April 2015, Uber was legally able to begin operating after the regulations were revised. A similar situation arose in upstate New York: it was not until April 2017 that New York State passed a budget allowing Uber and Lyft to operate in upstate New York (Kim, 2017).

The fact that there are several cities where Uber and Lyft wanted to operate but were delayed while they worked with city officials or regulatory agencies introduces an element of randomness into the timing of their arrival. Even though the policy might be endogenous in some cities, in the aggregate, the timing of Uber and Lyft entry may not be correlated with trends in fatal drunk-driving incidents.

To test for the presence of policy endogeneity (and to test for dynamic treatment effects), I conduct event studies to test the validity of the parallel pre-trends assumption. Figures 3.5 through 3.8 shows the results of the event study specifications and are described in detail in Section 3.4.1.

3.2.3 Reduced-Form Drunk Driving Equation

The difference-in-differences model is a Poisson model estimated with control variables and city and month-year fixed effects. The main specification is equation 3.4.

$$\mathbb{E}[F_{it} \mid Ride,\ \mathbf{X}] = exp\{\alpha + \beta \cdot Ride_{it} + \mathbf{X}'_{it} \cdot \gamma + \eta_i + \delta_t\} \tag{3.4}$$

F_{it} represents the count of monthly city-level motor-vehicle fatalities or fatal crashes. $Ride_{it}$ represents a monthly city-level indicator for the presence of Uber or Lyft. $\mathbf{X}'_{it}$ represents a vector of characteristics that change over time. These include the monthly city-level unemployment rate as well as annual county-level demographic characteristics: the percent of the population that is African-American, Native American, Asian, or Hispanic, male, male aged 20 to 24, aged 20 to 24, 25 to 34, 35 to 54, and 55 and older. η_i represents city fixed effects. δ_t represents time fixed effects. The standard errors are clustered at the city level. I weight all regressions using the 2010 Census city population, so that the results are interpretable as the effect of Uber or Lyft on the average person, as opposed to the effect on the average city.

3.3 Data

3.3.1 Outcome Variables: Fatal Motor Vehicle Incidents

The outcome variables come from the National Highway Traffic Safety Administration (NHTSA) Fatality Analysis Reporting System (FARS) data. FARS contains the universe of motor vehicle crashes on public roadways in the United States (50 states and Washington, D.C.) that result in a fatality within 30 days of the crash (National Center for Statistics and Analysis, 2021). State governments send the crash data to the NHTSA each year, and NHTSA analysts aggregate and clean the data. FARS is the only source of data for fatal crashes for the entire United States. The case listings include information on the location and time of the crash, the number of fatalities, and the drivers' blood alcohol content, in addition to numerous other variables.

The sample includes monthly crash data from 2006 to 2016 for 99 of the 100 most populous U.S. cities (United States Census Bureau, 2012). I exclude San Juan, Puerto Rico because there are no FARS data for Puerto Rico.

I define fatalities and fatal crashes as drunk driver related if at least one vehicle driver had a blood alcohol concentration recorded in the FARS data of at least 0.08 g/dL (the legal limit for individuals 21+ for Driving Under the Influence in all 50 states and Washington, D.C. during my sample period, 2006 to 2016).

Given the findings that Uber and Lyft lead to increases in alcohol consumption (Teltser, Lennon, and Burgdorf, 2021; Zhou, 2020), I also examine all alcohol-related incidents. I define fatalities and fatal crashes as alcohol related if at least one vehicle driver had a recorded blood alcohol concentration greater than 0 g/dL. Note that this measure excludes fatalities and fatal crashes involving an intoxicated pedestrian, cyclist, or passenger.

If Uber and Lyft are primarily substitutes for drunk driving, then I would expect to see changes in drunk driving. However, if they are primarily substitutes for walking or bicycling home drunk, then an analysis of the effect of Uber and Lyft entry on drunk driving would not pick up the true effect of Uber and Lyft on drunk transportation choices.

Lyft and Uber may have an effect on fatal crashes if they lead to an increase in the number of cars on the road, independent of their effect on drunk driving. To address this possibility, I examine total (alcohol and non-alcohol-related) fatalities and fatal crashes.

Time of day of each crash is known, so I separate crashes into daytime and nighttime, classifying daytime crashes as occurring between 4 a.m. and 8 p.m and nighttime crashes as occurring between 8 p.m. and 4 a.m. This cutoff was chosen because the hours between 8 p.m. and 4 a.m. contain most of the alcohol-related crashes. In addition, they exclude standard rush hour when people would be commuting to or from work.

Figure 3.4 shows the distribution of monthly drunk-driver-related fatal crashes. Summary statistics for monthly city-level fatal motor vehicle *crashes* are shown in Table 3.1. Summary statistics for monthly city-level motor vehicle *fatalities* are shown in Table 3.2.

There are more drunk-driver related crashes at night compared to during the day. For nighttime hours, fatalities per fatal crash are slightly higher than daytime fatalities per fatal crash (0.88/0.76 = 1.16 for night; 0.26/0.24 = 1.08 for day).

Similar to drunk-driver-related crashes, the vast majority of alcohol-related crashes occur at night (between the hours of 8 p.m. and 4 a.m.). Alcohol-related crashes are slightly more lethal at night compared to during the day (1.15 fatalities per fatal crash vs. 1.10). In this sample of cities, drunk-driver-related crashes and fatalities make up 85% of alcohol-related crashes and fatalities.

Total fatal crashes are roughly evenly split between nighttime and daytime crashes, and there is quite a bit of variation across cities in the monthly number of fatal crashes. The means of daytime crashes and fatalities are slightly higher than nighttime crashes and fatalities, but fatalities per fatal crash are slightly higher for nighttime compared to daytime (1.09 vs. 1.06). Also of note is the fact that in this sample, 20% of fatal crashes and 21% of fatalities are drunk-driver-related, which is less than the national average of approximately 30% of motor vehicle fatalities during the same time period.

The three main limitations of the outcome variables/FARS data are that there is no information on the number of vehicle miles driven, alcohol-related crashes are measured with error, and there is no information on non-fatal crashes.

First, FARS does not contain information on the number of vehicle miles driven. To see why this is a problem, observe that the number of crashes in city i at time t can be represented by the following equation:

$$crashes_{it} = \frac{crashes_{it}}{vehicle\ mile\ driven_{it}} * (\#\ vehicle\ miles\ driven_{it}) \tag{3.5}$$

For example, if Uber and Lyft have no overall effect on the number of crashes, I could not distinguish between the following two scenarios. One, that Uber and Lyft have no effect on the number of crashes per vehicle mile driven and on the number of vehicle miles driven.[8] Two, that Uber and Lyft drivers are better drivers than average, leading to a reduction in the crash rate per vehicle mile driven, but they also lead to an increase in the number of vehicle miles driven, which exactly offsets the reduction in the crash rate.[9]

Second, there is measurement error for alcohol involvement in fatal crashes. One source of measurement error arises because states have different laws (and levels of enforcement) regarding BAC tests for drivers involved in fatal crashes (National Highway Traffic Safety Administration, 2012). Numerous states require probable cause for administering BAC tests to drivers involved in fatal crashes. As a result, the rates of known BAC test results vary across states (National Highway Traffic Safety Administration, 2012). However, when the alcohol test results are unknown, the National Highway Traffic Safety Administration estimates alcohol involvement (National Highway Traffic Safety Administration, 2017). After including the NHTSA alcohol-involvement estimates, only 0.3% of U.S. fatalities had an unknown or unreported highest driver blood alcohol concentration from 2006 to 2016. However, it is unclear how accurate the imputed BAC test results are, or how they may be biased.

Another source of measurement error comes from the breathalyzer tests themselves. An investigation by New York Times reporters found that breathalyzer machines in police departments across the country have not been properly calibrated or maintained, and the software in some of these machines had programming errors (Cowley and Silver-Greenberg, 2019). The combined effect of these errors yields breathalyzer test results that can be up to 40% higher than an individual's true blood alcohol concentration. A 40% overestimate of BAC would mean that an individual whose blood alcohol concentration was 0.057 g/dL (less than three-fourths the legal limit in all

[8]If this scenario were true, it would imply that for every mile driven by Lyft and Uber, one mile is not driven by taxis or individuals driving their own cars/carpooling. But recall that Uber and Lyft are cheaper than a taxi, on average, implying that a reduction in the price of transportation is associated with no change in quantity demanded, which would imply that demand for transportation is perfectly inelastic.

[9]Uber and Lyft drivers could alternatively be worse drivers on average, leading to increases in the crash rate, but if there is a decrease in the number of vehicle miles driven, that could offset the increase in the crash rate.

states during my sample period) could have a recorded BAC of 0.08 g/dL, rendering them drunk in the eyes of the law.

I address these measurement error issues in several ways. First, I separate crashes into nighttime and daytime, using nighttime crashes as a proxy for drunk-driver-related crashes. To address the measurement error issue arising from BAC imputation, I conduct a robustness check in which I restrict the sample to cities in the 18 states that test the BAC of at least 80% of deceased drivers (Kim et al, 2016; Appendix Table *C*.2). These results are presented in Section 3.5.2). To address the second source of measurement error, I estimate effects on alcohol-related fatal motor vehicle incidents. To the extent that errors in the breathalyzer machines are largely errors on the intensive margin, examining the effects on alcohol-related crashes avoids the issues arising from recorded BACs that are too high.[10]

Third, FARS does not contain information on non-fatal motor vehicle crashes. Suppose there are individuals who substitute away from drunk driving to Lyft and Uber, but they are the individuals who used to become involved only in non-fatal crashes. I would not be able to observe the reduction in non-fatal crashes in the data I have. Nevertheless, a reduction in non-fatal crashes is a desirable policy outcome. Consequently, the effect of Lyft and Uber on non-fatal crashes is beyond the scope of this paper.

3.3.2 Treatment Variable: Introduction of Ridesharing

I obtain data on the introduction of Uber or Lyft into a city from the respective company websites or from news articles. The month and year of each city's Uber or Lyft entry is listed in Appendix Table *C*.1.

[10]To the extent that toothpaste and mouthwash can also trigger a BAC above 0 for a roadside breathalyzer, there may still be some measurement error in alcohol-related crashes (Cowley and Silver-Greenberg, 2019).

The primary limitation of the treatment variable is that it requires the assumption of a constant, immediate treatment effect. The indicator may not accurately capture the effect of Uber and Lyft on drunk driving if, for example, it takes months or years for those services to become popular in a city. I conduct some robustness checks using alternative specifications to test the sensitivity of my results to this simplifying assumption. Nevertheless, assuming a constant treatment effect misses any measure of dose-response. The event studies in Section 3.4.1 provide suggestive evidence of longer-term effects.

3.3.3 Control Variables

I acquire monthly city-level unemployment data from the Bureau of Labor Statistics' Local Area Unemployment Statistics series.[11] Annual county-level population data come from the Surveillance, Epidemiology, and End Results (SEER) program. The population data break down the county-level populations by gender, race, and age. The 2010 city-level population data come from the U.S. Census Bureau. Summary statistics for selected control variables are shown in Table 3.3.

In this sample of cities, 30% of cities had Uber or Lyft in a given month-year. Also of note is the variation in city population size: of the 100 most populous U.S. cities, the average 2010 Census population was roughly 600,000 people. The 100th most populous city had a population just over 200,000, while the most populous city had nearly 8.2 million people.

The two main limitations of the SEER data are that county-level population estimates are imperfect measures of city-level population estimates, and annual population data are imperfect measures of monthly population data. Given the geographic mismatch of counties and cities, how

[11]The first six months of unemployment data for 2006 are unavailable for New Orleans, as the Bureau of Labor Statistics did not publish labor force estimates for New Orleans due to lingering data quality concerns from the effects of Hurricane Katrina (BLS, 2006). I therefore exclude the first six months of 2006 for New Orleans from my analysis.

closely the city population data line up with the county population data will vary across cities. With regard to annual population data, as long as the city population is not changing much month to month, the annual population data will be a good approximation of the monthly population.

In a robustness check I analyze heterogeneous effects of Uber and Lyft by public transit accessibility. I use rankings from a study conducted by WalletHub that ranked the 100 most populous U.S. cities on a variety of public transportation measures (McCann, 2019). One of the dimensions they ranked cities by was public transit accessibility and convenience. I define cities ranked 1-33 on this measure as "high accessibility," cities ranked 34-67 as "medium accessibility," and cities ranked 68-100 as "low accessibility". Appendix Table C.3 lists cities by accessibility type (high, medium, or low). The main limitation of these data are that they are from a study conducted in 2019, which is after my sample period.

3.4 Difference-in-Differences Results

Table 3.4 shows the results for monthly fatal motor vehicle incidents using a Poisson estimation. Panel A shows the results for drunk-driver-related fatal crashes. A fatal crash is defined as drunk driver related if the highest recorded Blood Alcohol Concentration of any involved driver was above the legal threshold for Driving Under the Influence (0.08 g/dL). The presence of Uber or Lyft is associated with a decrease in all drunk-driver-related fatal crashes of 0.03 crashes per month, a 3% decrease. Uber or Lyft lead to a reduction in nighttime drunk-driver-related fatal crashes (crashes recorded as occurring between 8 p.m. and 4 a.m.) of 0.01 crashes per month, a 1.3% decrease. They lead to a decrease in daytime drunk-driver-related crashes (recorded as occurring between 4 a.m. and 8 p.m.) of 0.02 crashes per month, an 8.3% decrease. None of these coefficients are statistically significantly different than 0.

Panel B of Table 3.4 shows the results for drunk-driver-related fatalities. Uber or Lyft leads to a reduction in all such fatalities of 0.09 people per month, an 8.0% decrease. They are associated with a 0.06-person-per-month decrease for nighttime drunk-driver-related fatalities (6.8%), and a 0.03-person-per-month decrease for daytime drunk-driver-related fatalities (11.9%). None of these effects are statistically significant, however.

Panel C shows the results for alcohol-related fatal crashes. A fatal crash is defined as alcohol related if the highest recorded Blood Alcohol Concentration of any involved driver was greater than zero. Uber or Lyft lead to monthly reductions of 0.02 alcohol-related crashes per month (1.2% decrease), 0.01 nighttime alcohol-related crashes (1.1% decrease), and 0.01 daytime alcohol-related crashes (3.3% decrease). Again, none of these effects are statistically significantly different than 0.

Panel D shows the results for alcohol-related fatalities. Uber or Lyft lead to reductions in all alcohol-related fatalities of 0.08 people per month, which is a 6% decrease. They are associated with 0.06 fewer nighttime alcohol-related fatalities per month (a 5.9% decrease) and 0.02 fewer daytime alcohol-related fatalities per month (a 6.1% decrease). None of these coefficients are statistically significant.

Panel E presents the results for all fatal crashes. Uber or Lyft leads to an increase of 0.15 fatal crashes per month (a 3% increase), a decrease of 0.03 nighttime fatal crashes per month (a 1.3% decrease), and an increase of 0.17 daytime fatal crashes per month (a 6.5% increase). However, none of these coefficients are significant.

Panel F presents the results for all fatalities. Uber or Lyft is associated with an increase of 0.16 fatalities per month (a 3% increase), no change in nighttime fatalities per month, and an increase of 0.15 daytime fatalities per month (a 5.5% increase). None of these effects are statistically significant.

3.4.1 Event Studies

A standard difference-in-differences model assumes a constant immediate treatment effect. However, this assumption may not be appropriate in the context of ridesharing. The adoption of this new technology takes time, meaning demand for Uber and Lyft may increase over time as more people learn about these apps and become more familiar with them. In addition, the number of drivers may change over time. Outward shifts in both the supply and demand curves for rides should in theory lead to an increase in the quantity of Uber and Lyft rides. As ridership increases, the effect of Uber and Lyft on drunk driving may change. Alternatively, driver skill may also play a role. If there are returns to experience for being a Lyft or Uber driver, whether through improved driving skill or improved knowledge of city geography (so drivers are not constantly checking their smartphones for driving directions), then there may not be a change in fatal incidents in the short run (there may even be an increase), but there could be longer-run changes.

To test for these heterogenous treatment effects, I conduct an event study. I focus on drunk-driver-related and overall fatal incidents because in the standard difference-in-differences specification, the results for alcohol-related incidents are quite similar to the results for drunk-driver-related incidents. To reduce the noise associated with monthly observations, I aggregate the data to the annual level. I use a pre-period window of 4 years and a post-period window of 6 years. I omit the year prior to Uber or Lyft's arrival in a city as the reference point. The Poisson event-study equation is as follows:

$$\mathbb{E}[(F_{iy}|\ Ride,\ \mathbf{X}] = exp\{\alpha + \sum_{k\neq-1,k=-4}^{k=6} \beta_k Ride_{kiy} + \mathbf{X}'_{iy}\gamma + \eta_i + \delta_y\} \tag{3.6}$$

$Ride_{kiy}$ is an indicator equal to 1 if Uber or Lyft has been in city i at year y for k years. β_k is the effect of Uber or Lyft having been in a city for k years.

The results indicate a delayed effect of Uber and Lyft on drunk driving. For drunk-driver-related fatal crashes (Figure 3.5), in the pre period the coefficients are small, positive, and not statistically significant. In the post period, the results become more negative and statistically significant the longer Uber or Lyft has been in a city. For drunk-driver-related fatalities (Figure 3.6), the results are nearly identical. The coefficients are small, positive, and not significant in the pre period, and trend downward and become statistically significant after several years in the post period. These event studies indicate that there are heterogeneous treatment effects of Uber and Lyft on drunk driving, consistent with a delayed adoption of this technology by riders and or drivers.

For overall crashes and fatalities (Figures 3.7 and 3.8), there may be a slight upward pre trend, as the coefficients are negative and generally not statistically significant but become more attenuated. In the first few years of the post period for both, the coefficients are small, positive, and not statistically significant, but over time they trend downward, though they are still generally not statistically significant.

3.5 Extensions

3.5.1 Quarterly and Annual Outcomes

Given that city-level fatal motor vehicle incidents are relatively rare occurrences, it is possible that the way one aggregates the data could matter for the results. Consequently, I conduct robustness checks where I aggregate the data to the quarterly or annual level as opposed to the monthly level. At these higher-level aggregations, there will be fewer zeroes in the outcome data.

The results for quarterly-level fatal incidents are shown in Table 3.5. There are some sign changes for drunk-driver-related fatal crashes and alcohol-related fatal crashes, but in general the estimated quarterly effects are similar to the monthly effects in percentage terms. None of the coefficients are statistically significantly different than 0.

For drunk-driver-related crashes (Panel A), the effect on all drunk-driver-related crashes changes sign relative to the monthly estimates (0.05 versus -0.03, the former of which is a 1.7% increase). The effect of Uber or Lyft on quarterly nighttime drunk-driver-related crashes also changes sign relative to the monthly estimate: it's an increase of 0.08 (3.3%) compared to -0.01 for the monthly estimate. For daytime drunk-driver-related crashes, the effect is attenuated (in percentage terms) relative to the monthly effect: -0.03 (-4.2%) compared to a monthly estimate of -0.02 (-8.33%). For drunk-driver-related fatalities (Panel B), Uber or Lyft leads to reductions of 0.19 drunk-driver-related fatalities (5.6% decrease), 0.12 nighttime drunk-driver-related fatalities (4.1% decrease), and 0.07 daytime drunk-driver-related fatalities (9.1% decrease) per quarter. These effects are similar to the monthly estimates in terms of percent changes.

Panel C shows the effect of Uber and Lyft on alcohol-related fatal crashes. There are increases of 0.09 fatal alcohol-related crashes per quarter (2.6% increase), 0.08 nighttime fatal alcohol-related crashes per quarter (3% increase), and 0.01 daytime fatal alcohol-related crashes per quarter (1.1% increase). Compared to the monthly estimates, these effects are small and positive as opposed to small and negative. For alcohol-related fatalities (Panel D), the quarterly effects are half as large as the monthly effects in percentage terms. Uber and Lyft are associated with reductions of 0.10 alcohol-related fatalities (2.5% decrease), 0.08 nighttime alcohol-related fatalities (2.7% decrease), and 0.02 daytime alcohol-related fatalities (3% decrease) per quarter.

For overall crashes (Panel E), Uber and Lyft lead to an increase of 0.58 fatal crashes per quarter (a 3.9% increase), a decrease of 0.03 nighttime fatal crashes (0.4% decrease), and an increase of 0.61 daytime fatal crashes (a 7.8% increase). The effects are similar in percentage terms to the monthly estimates. For overall fatalities (Panel F), Uber and Lyft again lead to small increases. All fatalities increase by 0.61 people per quarter (3.8% increase), nighttime fatalities increase by 0.04 people (a 0.5% increase), and daytime fatalities increase by 0.55 people (a 6.2% increase).

The effects are are again similar to the monthly estimates for overall fatalities when measured in percentage terms.

Table 3.6 presents the results for annual fatal incidents. The annual results are generally similar to the quarterly results, with the exception of the results for alcohol-related motor vehicle fatalities. None of the estimates are statistically significant.

For drunk-driver-related crashes (Panel A), Uber and Lyft lead to an increase of 0.40 crashes per year (a 3.3% increase). They are associated with an additional 0.65 nighttime drunk-driver-related fatal crashes per year, a 7.1% increase, and a reduction in daytime drunk-driver-related fatal crashes of 0.25 crashes per year (an 8.9% decrease). Panel B presents the results for drunk-driver-related fatalities. Uber or Lyft leads to reductions of 0.67 drunk-driver-related fatalities per year (4.9% decrease), 0.29 nighttime drunk-driver-related fatalities per year (2.8% decrease), and 0.39 daytime drunk-driver-related fatalities per year (12.7% decrease).

The results for alcohol-related crashes are shown in Panel C. Uber or Lyft is associated with an increase of 0.88 alcohol-related crashes per year (6.2% increase). They lead to an increase of 0.97 nighttime alcohol-related crashes per year (9.2% increase), and a decrease of 0.10 daytime alcohol-related crashes per year (2.8% decrease). Panel D shows the effect of Uber and Lyft on alcohol-related fatalities. They are associated with an increase of 0.09 alcohol-related fatalities per year (0.6% increase), an increase of 0.29 nighttime alcohol-related fatalities per year (2.4% increase), and a decrease of 0.22 alcohol-related fatalities per year (5.6% decrease). Compared to the monthly and quarterly effects, the sign changes for all and nighttime alcohol-related fatalities.

For overall crashes (Panel E), Uber and Lyft lead to increases of 3.54 crashes per year (6% increase), 0.62 nighttime crashes per year (2.2% increase), and 2.93 daytime crashes per year (9.4% increase). For overall fatalities (Panel F), Uber and Lyft again lead to increases: 7.12 fatalities per year (11.2% increase), 0.89 nighttime fatalities per year (2.9% increase), and 6.17 daytime fatalities per year (18.7% increase). The annual estimates for all fatalities are noticeably larger in percentage terms than the quarterly or monthly estimates; this difference is driven by larger effects on daytime fatalities.

3.5.2 Subsample of Majority Testing States

As mentioned previously, states vary in the percentage of drivers whose BAC they test, due to probable cause laws and inconsistent testing practices. Some states test the blood alcohol concentration of most deceased drivers, however (see Appendix Table *C*.2 for the list of states). These states tend to be on the West Coast, in the Mountain West, or in the Northeast, and the subsample of cities in these states have more fatal motor vehicle incidents than the sample average.

In theory, the effect of Uber and Lyft on overall crashes and fatalities in this subsample should be similar to the effect on the full sample, unless there are time-varying omitted variables that differentially affect motor vehicle crashes, or Uber and Lyft have heterogeneous effects across cities.[12] If the overall results are similar, then any difference in the effects on drunk-driver-related incidents may be due to the measurement error in BAC in the full sample.[13]

When I restrict the sample to the 18 states that test at least 80% of drivers who died in the motor vehicle crash (Table 3.7), the effects for nighttime fatal motor vehicle incidents, as well as overall crashes and fatalities, are similar to the monthly estimates (Table 3.4). The estimates for all and daytime drunk-driver-related and alcohol-related incidents change signs.

Uber or Lyft is associated with an increase in overall drunk-driver-related fatal motor vehicle crashes of 0.10 crashes per month (6.8%), which is not statistically significant (Panel A of Ta-

[12]The effects should be similar to the extent that overall crashes and fatalities do not suffer from measurement error in BAC.

[13]That is, the measurement error that arises from the NHTSA's BAC imputation procedure or from some states having greater discretion or probable cause requirements for law enforcement to enforce drunk-driving laws or measure BAC. Differences could also arise if Uber and Lyft have heterogeneous effects on drunk driving across cities.

ble 3.7). They are associated with a 0.03-crash decrease in nighttime drunk-driver-related fatal motor vehicle crashes (-2.7%), which is also not statistically significant. Daytime drunk-driver-related fatal crashes increase by 0.13 crashes per month (37.1%), which is marginally statistically significant (10% significance level) and a large effect in percentage terms. The coefficients for drunk-driver-related motor vehicle fatalities (Panel B of Table 3.7) are the same as the coefficients for fatal crashes (Panel A), although none of the coefficients are statistically significant.

For alcohol-related fatal motor vehicle crashes (Panel C), Uber and Lyft are associated with an increase of 0.06 crashes per month (3%), a decrease of 0.04 nighttime crashes per month (-3.1%), and an increase of 0.10 daytime crashes per month (22.7% increase). For alcohol-related fatalities (Panel D), Uber or Lyft leads to an increase of 0.04 fatalities per month (2% increase), a decrease of 0.04 nighttime fatalities per month (2.7% decrease), and an increase of 0.09 daytime fatalities per month (18.8% increase). None of the coefficients for alcohol-related fatal crashes or fatalities are statistically significant.

For overall crashes (Panel E), Uber or Lyft leads to an increase of 0.03 fatal crashes per month (0.4% increase), a decrease of 0.09 nighttime crashes per month (2.7% decrease), and an increase of 0.13 daytime fatal crashes per month (3.5% increase). For overall fatalities (Panel F), they are associated with an increase of 0.05 fatalities per month (0.7% increase), a decrease of 0.01 nighttime fatalities per month (0.3% decrease), and an increase of 0.06 daytime fatalities per month (1.6% increase). None of these coefficients are statistically significant.

The similarities in results for nighttime fatal motor vehicle incidents, and overall crashes and fatalities, while not statistically significant, suggest that Uber and Lyft may lead to small reductions in nighttime fatal incidents (alcohol related and overall), but small increases in daytime crashes and fatalities. The discrepancy for daytime drunk-driver-related and alcohol-related crashes (which drives the difference in total drunk-driver-related and alcohol-related incidents) could be a result of several factors. As mentioned above, the effect of Uber and Lyft on daytime drunk driving for the subsample of cities in majority testing states could be different than their effect on other cities. Alternatively, there could be measurement error in imputed BAC for daytime drunk drivers. For example, the imputed BAC may undercount the true extent of daytime drunk driving. Uber and Lyft may have an effect on the behavior of individuals who would always be recorded (or imputed) as having a BAC above 0.08, but they may not have an effect on individuals who would be undercounted in the imputation. It could be that Uber and Lyft are not substitutes for daytime drunk driving but they are substitutes for nighttime drunk driving, particularly if these daytime incidents are occurring in the early morning hours when people wake up drunk, believe they are sober, and drive home from wherever they spent the night.

3.5.3 Heterogeneity by Quality of Public Transportation

The effect of Uber and Lyft on motor vehicle crashes and fatalities may depend on the quality of potential substitutes to driving, as well as the quality of potential complements to Uber and Lyft. In cities that are more walkable or have more robust public transportation, people may not be substituting from driving their own car to Uber and Lyft. They may either substitute from using public transit, or they may not use Uber and Lyft much. In this instance, Uber and Lyft may not lead to declines in drunk driving and may even lead to increases in total crashes if there are more cars on the road. Conversely, Uber and Lyft combined with public transportation may encourage people to use their own cars less frequently, if they use Uber and Lyft to get from the bus or subway stop to their home, for example. Empirically, Uber appears to be a complement for public transit, particularly in larger cities and in cities with smaller transit agencies (Hall, Palsson, and Price, 2018). To analyze heterogeneity by public transportation, I split cities into three approximately equally sized groups: high, medium, and low public transit accessibility, using rankings from a study conducted by WalletHub (McCann, 2019).

The effects of Uber and Lyft on drunk driving by public transit accessibility are presented in Table 3.8 and the results for overall crashes and fatalities are presented in Table 3.9. Panel A of Table 3.8 shows the effect of Uber and Lyft on drunk-driver-related fatal crashes. For cities with high public transit accessibility, Uber and Lyft lead to a reduction of 0.06 drunk-driver-related fatal crashes per month (5%), 0.06 nighttime drunk-driver-related fatal crashes per month (6.5%), and 0.01 daytime drunk-driver-related fatal crashes per month (3.5%). None of these effects are statistically significant. For cities with medium public transit accessibility, Uber or Lyft is associated with an increase of 0.07 fatal drunk-driver-related crashes per month (8%), 0.09 nighttime fatal drunk-driver-related crashes per month (12.9%), and a decrease of 0.02 daytime drunk-driver-related fatal crashes per month (10.5%). These effects are also not statistically significant. For cities with low public transit accessibility, the entry of Uber or Lyft leads to a reduction of 0.08 drunk-driver-related fatal crashes per month (15.7%), an increase of 0.01 nighttime drunk-driver-related crashes per month (2.6%), and a reduction of 0.09 daytime fatal drunk-driver-related crashes per month (69.2%). The effect for daytime is marginally statistically significant.

Panel B of Table 3.8 presents results for drunk-driver-related fatalities. For cities with high public transit accessibility, Uber and Lyft lead to a reduction of 0.14 drunk-driver-related fatalities per month (10.1%), 0.12 nighttime fatalities per month (11.3%), and 0.02 daytime fatalities per month (6.3%). The effect on nighttime drunk-driver-related fatalities is statistically significant at the 5% level. For cities with medium public transit accessibility, the arrival of Uber or Lyft is associated with an increase of 0.04 drunk-driver-related fatalities per month (4%), 0.08 nighttime drunk-driver-related fatalities per month (10.1%), and a reduction of 0.04 daytime fatalities per month (20%). None of these effects are statistically significant. For cities with low public transit accessibility, Uber and Lyft lead to reductions of 0.15 drunk-driver-related fatalities per month (26.8%), 0.04 nighttime fatalities per month (9.5%) and 0.10 daytime fatalities per month (71.4%). The effect on daytime drunk-driver-related fatalities is marginally statistically significant.

Panel A of Table 3.9 shows results for overall crashes. For cities with high public transit accessibility, Uber and Lyft lead to an increase of 0.02 fatal crashes per month (0.3%), a decrease of 0.12 nighttime fatal crashes per month (-4%), and an increase of 0.13 daytime fatal crashes per month (3.8%). None of these coefficients are statistically significant. For cities with medium public transit accessibility, Uber or Lyft is associated with an increase of 0.47 fatal crashes per month (12.7%), 0.18 nighttime fatal crashes per month (9.7%), and 0.29 daytime fatal crashes per month (15.6%). The effects for overall and daytime crashes are statistically significant at the 5% level. For cities with low public transit accessibility, Uber and Lyft lead to increases of 0.26 fatal crashes per month (11.5%), 0.07 nighttime fatal crashes per month (6.5%), and 0.19 daytime fatal crashes per month (15.8%). None of these effects are statistically significant.

Results for overall fatalities are shown in Panel B of Table 3.9. For cities with high public transit accessibility, Uber and Lyft lead to a decrease of 0.01 fatalities per month (0.2%), a decrease of 0.12 nighttime fatalities per month (3.7%), and an increase of 0.10 daytime fatalities per month (2.8%). None of these effects are statistically significant. For cities with medium public transit accessibility, Uber and Lyft lead to an increase of 0.57 fatalities per month (14.3%), 0.27 nighttime fatalities per month (13.3%), and 0.29 daytime fatalities per month (14.9%). The effect on overall fatalities is significant at the 1% level, the effect on nighttime fatalities is marginally significant, and the effect on daytime fatalities is significant at the 5% level. For cities with low public transit accessibility, Uber and Lyft are associated with increases of 0.33 fatalities per month (13.6%), 0.11 nighttime fatalities per month (9.5%), and 0.23 daytime fatalities per month (18.3%). None of these effects are statistically significant.

In general, there are moderate declines for drunk-driver-related crashes and fatalities in cities with high or low public transit accessibility, and daytime incidents in cities with medium accessibility, although only some of these declines are statistically significant. The declines for daytime incidents in cities with low accessibility are quite large in percentage terms as the underlying number of crashes and fatalities in these cities is quite small. There are moderate increases in overall and nighttime drunk-driver-related incidents in cities with medium accessibility but these effects are not statistically significant.

For all crashes and fatalities, there are moderate increases in cities with medium or low public transit accessibility, although only the effects for medium cities are statistically significant. For cities with medium public transit accessibility, a potential mechanism for the increase in fatal incidents is more cars on the road as a result of Uber and Lyft. There are small declines for nighttime incidents in cities with high public transit accessibility that are offset by small increases in daytime incidents, although the effects are not statistically significant.

3.5.4 Negative Binomial Regression

The Poisson distribution is a special case of the negative binomial distribution. A Poisson model is appropriate when the outcome is a count variable and follows a Poisson distribution, which requires the mean and variance of the distribution to be equal. A negative binomial model is an alternative specification that does not require the mean to equal the variance. In my sample, the variance of fatal motor vehicle incidents is larger than the mean (Tables 3.1 and 3.2), so in this section I conduct an alternative specification using a negative binomial model. The equation for a negative binomial model is the same as the equation for the Poisson model (Equation 3.4), but assumes a different distribution of the outcome variable.

Table 3.10 presents the results from the negative binomial estimation. These estimates are unweighted because the estimates did not converge when I weighted by city population. The unweighted results for drunk-driver-related and alcohol-related fatal incidents are nearly identical to the population-weighted monthly Poisson estimates. The effects for overall fatal incidents are zero in the negative binomial specification so they are attenuated relative to the Poisson specification.

None of the coefficients are statistically significant.

Panel A shows the effect on drunk-driver-related fatal crashes. Uber or Lyft is associated with reductions of 0.03 drunk-driver-related fatal crashes (3% decrease), 0.04 nighttime drunk-driver-related fatal crashes (5.3% decrease), and 0.00 daytime drunk-driver-related fatal crashes. Panel B shows the effect on drunk-driver-related fatalities: a decrease of 0.08 drunk-driver-related fatalities (7.1% decrease), a decrease of 0.09 nighttime fatalities (-10.2%), and a decrease of 0.06 daytime fatalities (-23.1%).

The results for alcohol-related fatal crashes are in Panel C. Uber or Lyft leads to decreases of 0.02 alcohol-related fatal crashes per month (-1.7%), 0.02 nighttime alcohol-related fatal crashes per month (-2.3%), and no change for daytime alcohol-related fatal crashes. For alcohol-related fatalities (Panel D), they lead to reductions of 0.05 fatalities per month (-3.8%), 0.04 nighttime fatalities per month (-4%), and 0.01 daytime fatalities per month (-3%).

For overall crashes and fatalities, the point estimates are 0.00 for daytime crashes, overall fatalities, and nighttime fatalities. Uber and Lyft are associated with an increase of 0.01 crashes per month (0.2% increase). The estimation for nighttime crashes did not converge, so no result is reported for that specification. Uber and Lyft are associated with a reduction of 0.01 daytime fatalities per month (-0.4% decrease).

3.5.5 Log Regression

To analyze whether the results are consistent using other functional forms, in this section I estimate an OLS model using the log of monthly fatal motor vehicle incidents (plus 1) as the outcome. This specification assumes a lognormal distribution of the outcome variable, which occurs when the logarithm of a continuous variable is normally distributed.

$$log(F_{it} + 1) = \alpha + \beta \cdot Ride_{it} + \mathbf{X}'_{it} \cdot \gamma + \eta_i + \delta_t + \varepsilon_{it} \tag{3.7}$$

$log(F_{it} + 1)$ represents the log of 1 + monthly city-level motor vehicle crashes or fatalities. I add 1 to the count of incidents because many cities (fortunately) have 0 fatal incidents in a month, and the log of 0 is undefined. The treatment and control variables are the same as in Equation 3.4, and as before, the standard errors are clustered at the city level and I weight all regressions using the 2010 Census city population.

The results for the log specification are similar to the Poisson specification, although in some instances the log coefficients are simultaneously attenuated and more precisely estimated than the Poisson coefficients. The impact of Uber and Lyft on the log of fatal drunk-driver-related motor vehicle crashes is shown in Panel A of Table 3.11. The presence of Lyft or Uber in a city is associated with approximately a 2% decline in all drunk-driver-related motor vehicle crashes, a 3% decline in nighttime drunk-driver-related fatal crashes, and a 1% decline in daytime drunk-driver-related crashes declined. However, none of these effects are statistically significant. The log estimates are similar to the Poisson estimates for all and nighttime drunk-driver-related crashes (-2% vs. -3% and -3% vs. -1%), and they are attenuated for daytime crashes (-1% vs. -8%).

Turning to the results for the log of drunk-driver-related motor vehicle fatalities (Panel B of Table 3.11), I find that the presence of Lyft or Uber leads to a 4% decline in such fatalities, which is marginally statistically significant (10% significance level). This decline is driven by nighttime drunk-driver-related motor vehicle fatalities, which decline by 4% after Uber or Lyft entered a city (statistically significant at the 5% level). Daytime fatalities declined by 1%, although this difference is not statistically significant. The log estimates for all and nighttime drunk-driver-related fatalities are approximately half as large as the Poisson estimates (-4% vs. -8%, -4% vs.

-7%) and the daytime estimates are noticeably attenuated in the log specification (-1% vs. -12%).

The results for alcohol-related fatal motor vehicle crashes are smaller in magnitude and not statistically significant (Panel C of Table 3.11). The presence of Lyft or Uber leads to a 1% decline in all alcohol-related fatal motor vehicle crashes, a 1% decline in nighttime alcohol-related fatal crashes, and no decline in daytime alcohol-related fatal crashes. The estimates are similar to the Poisson specification for all and nighttime alcohol-related crashes (-1% for all), but attenuated for daytime crashes (no change vs. -3%).

For alcohol-related motor vehicle fatalities (Panel D of Table 3.11), Uber or Lyft leads to a 2% decline in all alcohol-related motor vehicle fatalities. They also lead to a 3% reduction in nighttime alcohol-related fatalities and a 1% decline in daytime alcohol-related fatalities. None of these coefficients are statistically significant and they are attenuated relative to the Poisson specification (-2% vs. -6%, -3% vs. -6%, and -1% vs. -6%).

For all fatal motor vehicle crashes (Panel E of Table 3.11), some of the coefficients become positive, but none of them are statistically significant. Uber or Lyft leads to a 2% increase in all fatal motor vehicle crashes, a 1% decrease in nighttime fatal motor vehicle crashes, and a 3% increase in daytime fatal motor vehicle crashes. The coefficients on motor vehicle fatalities (Panel F of Table 3.11) are virtually identical: Lyft or Uber leads to a 2% increase in all motor vehicle fatalities, a 1% decrease in nighttime fatalities, and a 2% increase in daytime fatalities. As with the fatal motor vehicle crash coefficients, none of these coefficients are statistically significant. The log estimates for all and nighttime crashes and fatalities are similar to the Poisson estimates (2% vs. 3%, -1% vs. -1%, 2% vs. 3%, and -1% vs. 0%) and attenuated for the daytime estimates (3% vs. 7% and 2% vs. 5%).

3.6 Discussion

The externalities associated with drunk driving are a serious problem in the United States. Consequently, many policies have been enacted to reduce the incidence of drunk driving, such as the Minimum Legal Drinking Age and a lower BAC limit. The former has been relatively successful at reducing motor vehicle fatalities (Carpenter, Dobkin, and Warman, 2016). The latter's effectiveness has been debated in the literature (Eisenberg, 2003 and Freeman, 2007).

In this paper, I analyze whether the "free market" may also have a role to play in combating drunk driving; specifically, whether the entry of Uber and Lyft into cities led to reductions in drunk-driver-related fatal motor vehicle incidents. I use Fatality Analysis Reporting System data from 2006 to 2016 for 99 of the 100 most populous cities in the U.S. to estimate a difference-in-differences model. In the standard difference-in-differences specification, I am unable to reject the null hypothesis of no effect of Uber and Lyft on monthly city-level drunk-driver-related fatalities or fatal crashes. The coefficients are small and negative, but they are not precisely estimated, meaning I cannot rule out moderate to large decreases or increases in fatal drunk-driving incidents. However, event study specifications indicate that there are statistically significant declines in annual drunk-driver-related crashes and fatalities 2-6 years after Uber or Lyft start operating in a city. The simple difference-in-differences results are similar when I estimate a negative binomial or a log specification. The small declines in drunk-driving-related fatal incidents are driven by cities with high or low public transit accessibility. When I restrict the sample to cities in states that test the BAC of at least 80% of deceased drivers, the effect on nighttime drunk-driving-related fatal incidents is similar but the effect on daytime drunk-driving-related fatal incidents becomes positive.

As a secondary analysis, I estimate the effect of Uber and Lyft on overall crashes and fatalities. I am unable to reject the null hypothesis of no effect of Uber or Lyft on overall crashes and fatalities.

The coefficients are small and generally positive but imprecisely estimated. The event studies show small increases in crashes and fatalities for the first couple of years after Uber and Lyft arrive in a city followed by large declines after 5 to 6 years. The simple difference-in-differences results are slightly attenuated when I estimate a negative binomial or log specification. The small increases in overall fatal incidents are concentrated in cities with medium public transit accessibility.

Given that the standard difference-in-differences coefficients are not statistically significant, caution is warranted when trying to translate the results of this paper into specific policy implications. That being said, the opposite-signed results for drunk-driving-related incidents and overall incidents suggest that there is the potential for certain regulations to keep the potentially beneficial effects of Uber and Lyft (reductions in nighttime drunk-driving fatal incidents) while minimizing the potentially detrimental effects (increases in overall fatal incidents). Longer-run declines in both drunk-driver-related and overall fatal incidents are consistent with both outward shifts in the supply and or demand curves for Uber and Lyft, as well as driver skill improving with experience. If the mechanism is one of driver skill, then stricter regulations concerning driver qualifications may be warranted as a way to reduce overall crashes and fatalities. If the increase in daytime incidents is occurring due to more cars on the road, then a tax on daytime rides may reduce demand for daytime Uber or Lyft rides (a similar concept to congestion pricing).

Future work should include more recent years of data to estimate longer-run effects. Another interesting avenue of inquiry would be to distinguish between possible mechanisms for the longer-run effects that I find, specifically increased adoption of ridesharing versus improvements in driver skill.

3.7 Works Cited

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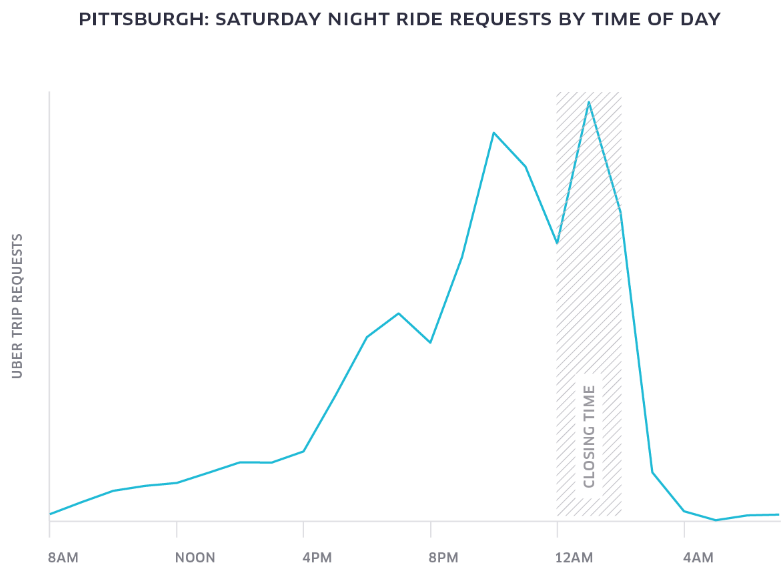
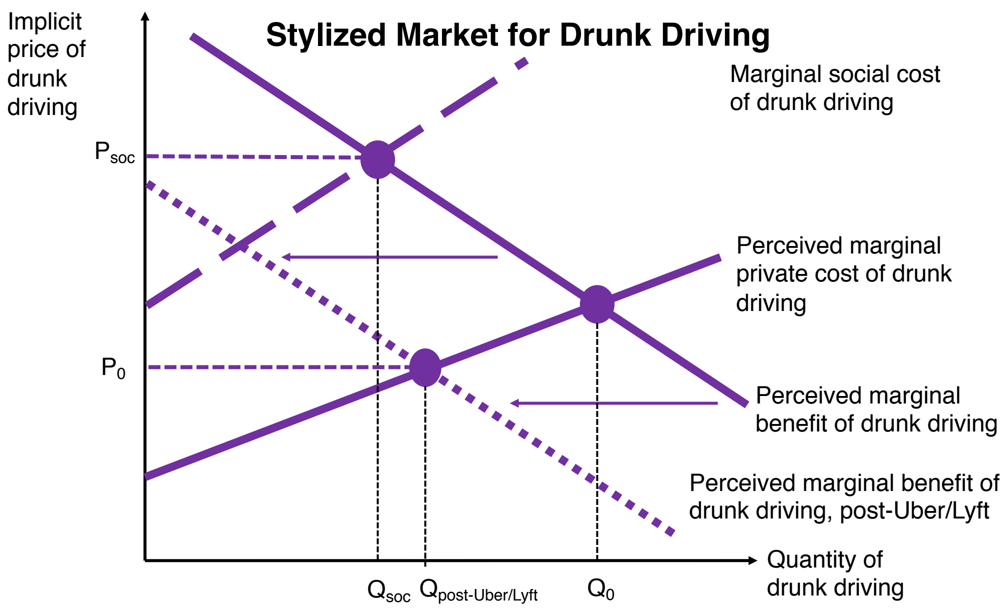
3.8 Figures and Tables

Figure 3.1



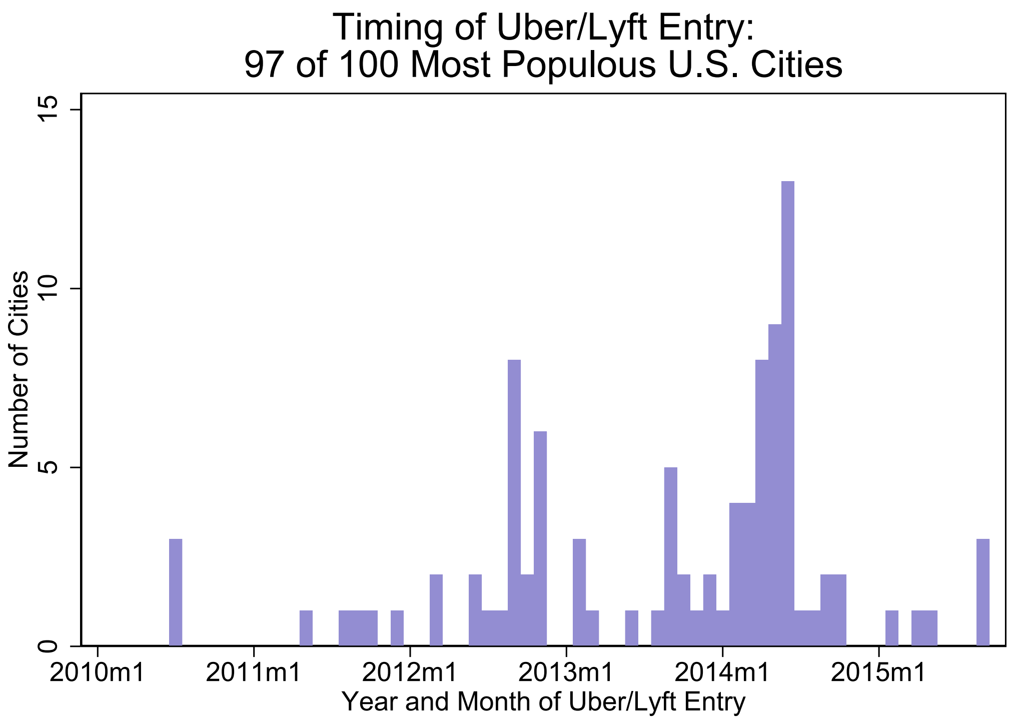
Source: Uber.

Figure 3.2



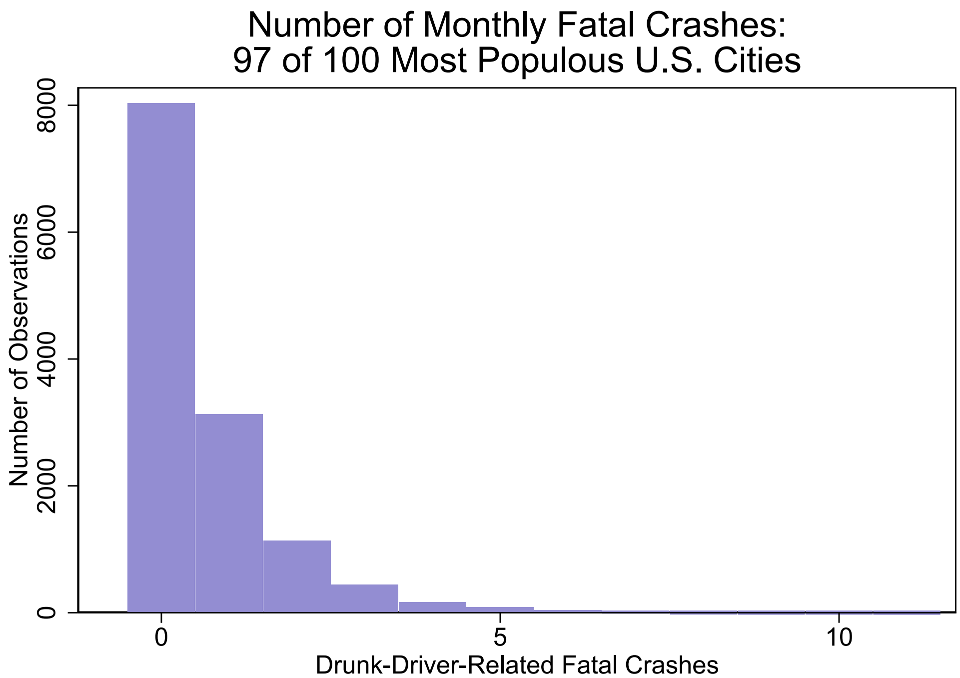
Source: Author's own illustration.

Figure 3.3



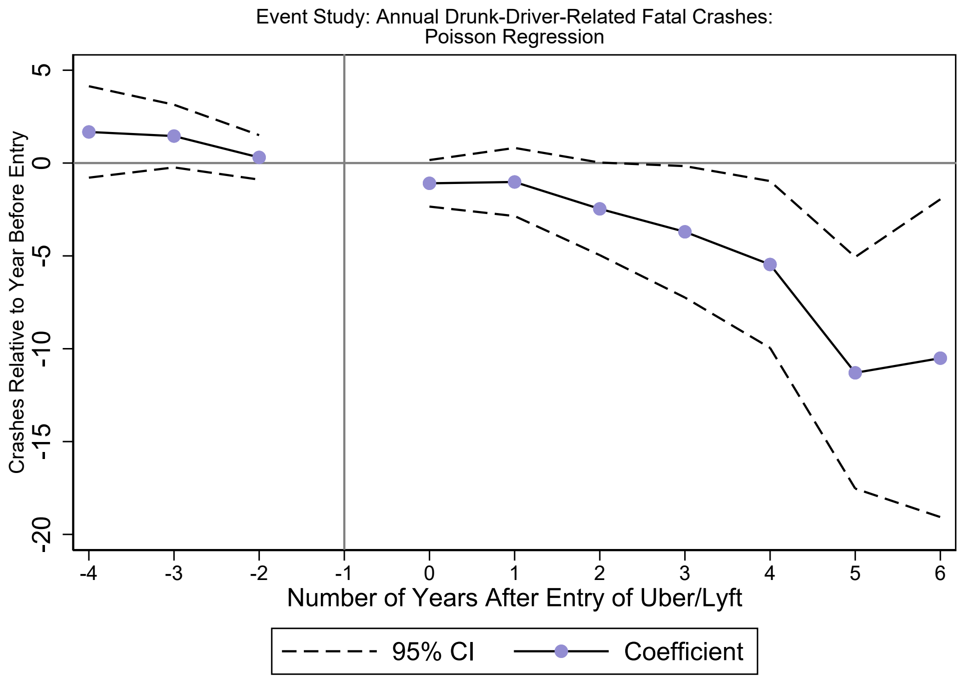
Data source: Author's hand-collected data from Uber, Lyft, and news articles.

Figure 3.4



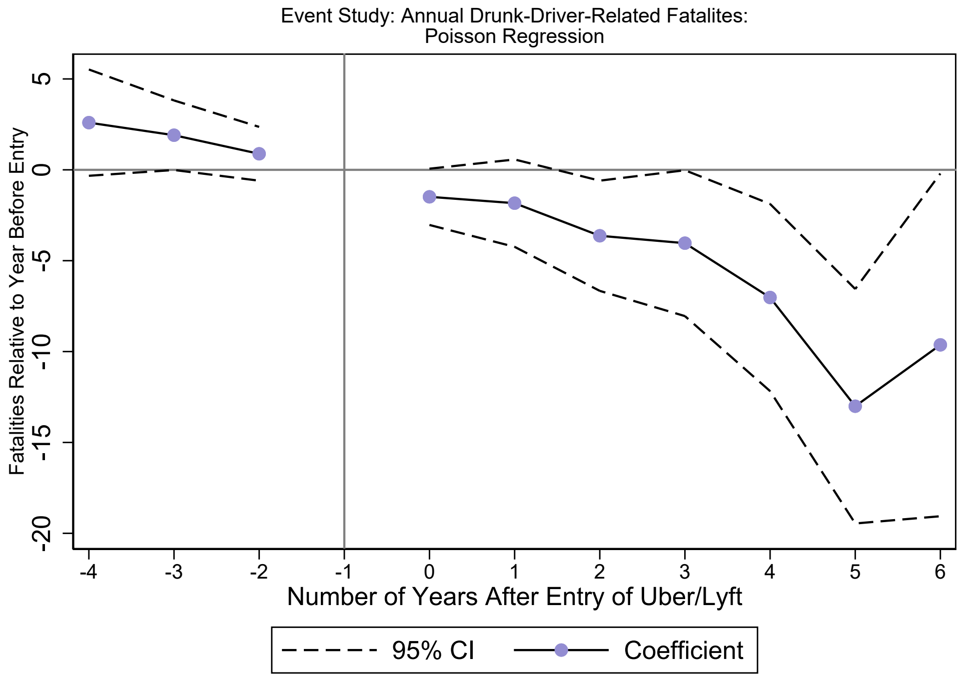
Data source: FARS 2006-2016.

Figure 3.5



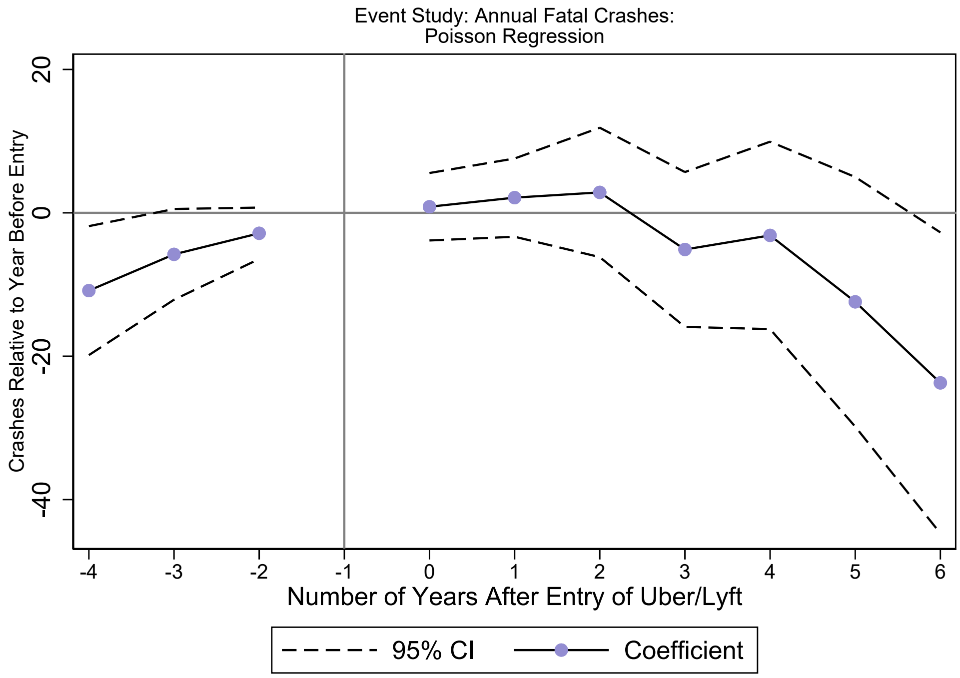
Note: Results from the estimation specified in Equation 3.6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Figure 3.6



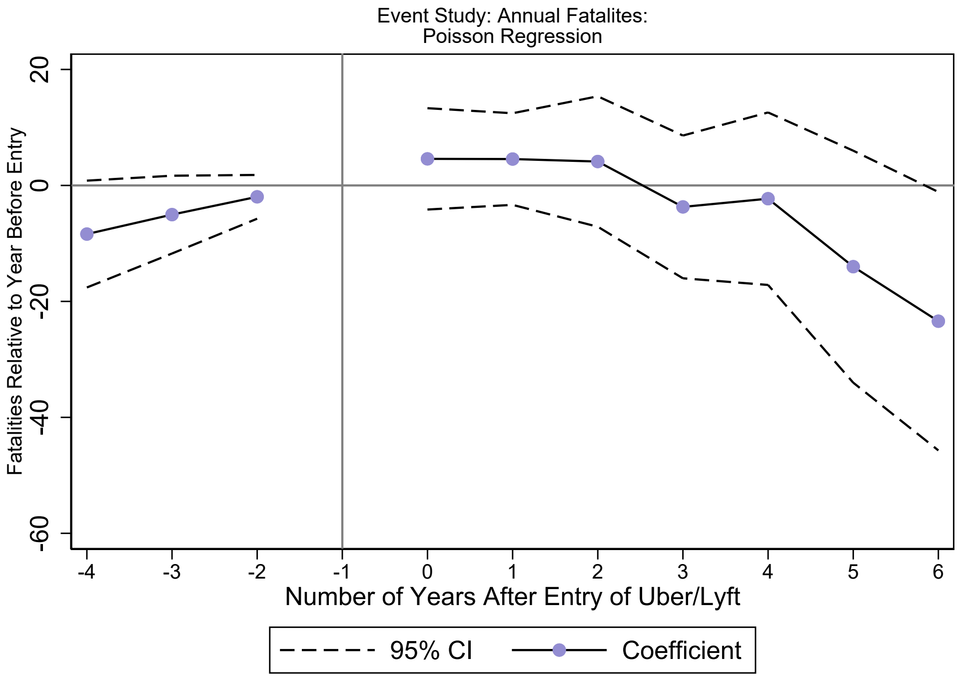
Note: Results from the estimation specified in Equation 3.6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Figure 3.7



Note: Results from the estimation specified in Equation 3.6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Figure 3.8



Note: Results from the estimation specified in Equation 3.6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Table 3.1: Summary Statistics of Fatal Motor Vehicle **Crashes:** 99 of the 100 Most Populous U.S. Cities

Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)	N (5)
Drunk Driver	**1.00**	1.50	0	11	13,062
Nighttime	0.76	1.23	0	9	13,062
Daytime	0.24	0.55	0	4	13,062
Alcohol-Related	**1.18**	1.73	0	13	13,062
Nighttime	0.88	1.36	0	9	13,062
Daytime	0.30	0.65	0	5	13,062
Total	**4.95**	5.66	0	34	13,062
Nighttime	2.35	2.91	0	20	13,062
Daytime	2.60	3.21	0	21	13,062

Note: each observation is a city-month-year, e.g. New York City, May 2006. Data are from 2006 to 2016 for the 100 largest U.S. cities excluding San Juan, Puerto Rico, which does not have FARS data. The first 6 months of 2006 for New Orleans are not included because the BLS did not publish labor force estimates due to lingering data quality concerns post-Hurricane Katrina (BLS, 2006). Statistics are weighted by the 2010 Census city population. Drunk driver means at least one driver was recorded as having a BAC ≥ 0.08 g/dL. Alcohol-related means at least one driver was recorded as having a BAC > 0.00 g/dL.
Data Source: FARS 2006-2016.

Table 3.2: Summary Statistics of Motor Vehicle **Fatalities:** 99 of the 100 Most Populous U.S. Cities

Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)	N (5)
Drunk Driver	**1.13**	1.74	0	12	13,062
Nighttime	0.88	1.45	0	10	13,062
Daytime	0.26	0.63	0	7	13,062
Alcohol-Related	**1.33**	1.99	0	14	13,062
Nighttime	1.01	1.60	0	10	13,062
Daytime	0.33	0.73	0	7	13,062
Total	**5.30**	6.08	0	36	13,062
Nighttime	2.55	3.22	0	21	13,062
Daytime	2.75	3.41	0	22	13,062

Note: each observation is a city-month-year, e.g. New York City, May 2006. Data are from 2006 to 2016 for the 100 largest U.S. cities excluding San Juan, Puerto Rico, which does not have FARS data. The first 6 months of 2006 for New Orleans are not included because the BLS did not publish labor force estimates due to lingering data quality concerns post-Hurricane Katrina (BLS, 2006). Statistics are weighted by the 2010 Census city population. Drunk driver means at least one driver was recorded as having a BAC ≥ 0.08 g/dL. Alcohol-related means at least one driver was recorded as having a BAC > 0.00 g/dL.
Data Source: FARS 2006-2016.

Table 3.3: Summary Statistics of Selected Control Variables (Unweighted): 99 of the 100 Most Populous U.S. Cities

Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)	N (5)
= 1 if Uber/Lyft	**0.30**	0.46	0	1	13,062
UE Rate (%)	6.98	3.05	1.50	28.40	13,062
% African-American†	17.09	14.53	0.21	65.62	13,062
% Asian†	7.70	9.22	0.53	71.10	13,062
% Hispanic†	23.35	18.78	1.27	95.73	13,062
% White†	51.08	17.35	3.47	87.47	13,062
% Male†	49.08	0.97	46.89	52.31	13,062
% Male 20-24†	3.91	0.93	2.54	9.47	13,062
% 20-24†	7.70	1.59	5.09	15.64	13,062
% 25-34†	15.30	2.05	10.61	23.59	13,062
% 35-54†	27.24	1.90	19.33	32.23	13,062
% 55+†	22.73	3.28	14.48	39.54	13,062
2010 Pop.	**602,532**	920,435	208,453	8,175,133	13,062

Note: each observation is a city-month-year, e.g. New York City, May 2006. Data are from 2006 to 2016 for the 100 largest U.S. cities excluding San Juan, Puerto Rico, which does not have FARS data. The first 6 months of 2006 for New Orleans are not included because the BLS did not publish labor force estimates due to lingering data quality concerns post-Hurricane Katrina (BLS, 2006). † refers to county population.
Data sources: Uber, Lyft, news articles, BLS, SEER, Census.

Table 3.4: Effect of Uber and Lyft on **Monthly** Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.03	-0.01	-0.02
	(0.06)	(0.04)	(0.04)
Dependent Variable Mean	1.00	0.76	0.24
% of Mean	-3.00%	-1.32%	-8.33%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.09	-0.06	-0.03
	(0.08)	(0.05)	(0.05)
Dependent Variable Mean	1.13	0.88	0.26
% of Mean	-7.96%	-6.82%	-11.94%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.02	-0.01	-0.01
	(0.06)	(0.04)	(0.04)
Dependent Variable Mean	1.18	0.88	0.30
% of Mean	-1.16%	-1.14%	-3.33%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.08	-0.06	-0.02
	(0.07)	(0.05)	(0.04)
Dependent Variable Mean	1.33	1.01	0.33
% of Mean	-6.02%	-5.94%	-6.06%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.15	-0.03	0.17
	(0.16)	(0.08)	(0.08)
Dependent Variable Mean	4.95	2.35	2.60
% of Mean	3.03%	-1.28%	6.54%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.16	0.00	0.15
	(0.16)	(0.08)	(0.13)
Dependent Variable Mean	5.30	2.55	2.75
% of Mean	3.02%	0.00%	5.45%
N	13,062	13,062	13,062

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

Note: Results from the estimation specified in Equation 3.4. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 3.5: Effect of Uber and Lyft on **Quarterly** Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of quarter)	0.05	0.08	-0.03
	(0.18)	(0.13)	(0.15)
Dependent Variable Mean	3.00	2.29	0.71
% of Mean	1.67%	3.29%	-4.23%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of quarter)	-0.19	-0.12	-0.07
	(0.24)	(0.15)	(0.17)
Dependent Variable Mean	3.40	2.63	0.77
% of Mean	-5.59%	-4.06%	-9.09%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of quarter)	0.09	0.08	0.01
	(0.18)	(0.13)	(0.15)
Dependent Variable Mean	3.54	2.63	0.91
% of Mean	2.54%	3.04%	1.10%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of quarter)	-0.10	-0.08	-0.02
	(0.23)	(0.15)	(0.17)
Dependent Variable Mean	4.00	3.02	0.98
% of Mean	-2.50%	-2.65%	-3.04%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of quarter)	0.58	-0.03	0.61
	(0.64)	(0.26)	(0.49)
Dependent Variable Mean	14.85	7.04	7.81
% of Mean	3.91%	-0.43%	7.81%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of quarter)	0.61	0.04	0.55
	(0.66)	(0.27)	(0.47)
Dependent Variable Mean	15.91	7.66	8.95
% of Mean	3.83%	0.52%	6.15%
N	4,354	4,354	4,354

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

Note: Results from the estimation specified in Equation 3.4 with quarterly instead of monthly observations. Controls include the quarterly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Table 3.6: Effect of Uber and Lyft on **Annual** Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of year)	0.40	0.65	-0.25
	(1.06)	(0.80)	(0.58)
Dependent Variable Mean	11.99	9.16	2.82
% of Mean	3.34%	7.10%	-8.87%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of year)	-0.67	-0.29	-0.39
	(1.33)	(0.93)	(0.65)
Dependent Variable Mean	13.59	10.52	3.07
% of Mean	-4.93%	-2.76%	-12.70%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of year)	0.88	0.97	-0.10
	(1.01)	(0.78)	(0.56)
Dependent Variable Mean	14.16	10.53	3.63
% of Mean	6.21%	9.21%	-2.75%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of year)	0.09	0.29	-0.22
	(1.28)	(0.91)	(0.63)
Dependent Variable Mean	16.00	12.08	3.92
% of Mean	0.56%	2.40%	-5.61%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of year)	3.54	0.62	2.93
	(2.86)	(1.30)	(2.43)
Dependent Variable Mean	59.40	28.15	31.25
% of Mean	5.96%	2.20%	9.38%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of year)	7.12	0.89	6.17
	(5.22)	(1.41)	(4.65)
Dependent Variable Mean	63.62	30.65	32.98
% of Mean	11.19%	2.90%	18.71%
N	1,089	1,089	1,089

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

Note: Results from the estimation specified in Equation 3.4 with annual instead of monthly observations. Controls include the annual city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 3.7: Effect of Uber and Lyft on Monthly Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression, Majority Testing Subsample (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.10	-0.03	0.13*
	(0.16)	(0.13)	(0.07)
Dependent Variable Mean	1.47	1.13	0.35
% of Mean	6.80%	-2.65%	37.14%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	0.10	-0.03	0.13
	(0.16)	(0.13)	(0.08)
Dependent Variable Mean	1.67	1.29	0.38
% of Mean	5.99%	-2.33%	34.21%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.06	-0.04	0.10
	(0.20)	(0.16)	(0.08)
Dependent Variable Mean	1.76	1.31	0.44
% of Mean	3.01%	-3.05%	22.73%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	0.04	-0.04	0.09
	(0.20)	(0.16)	(0.09)
Dependent Variable Mean	1.99	1.50	0.48
% of Mean	2.01%	-2.67%	18.75%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.03	-0.09	0.13
	(0.28)	(0.23)	(0.23)
Dependent Variable Mean	7.03	3.35	3.69
% of Mean	0.43%	-2.69%	3.52%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.05	-0.01	0.06
	(0.28)	(0.24)	(0.26)
Dependent Variable Mean	7.50	3.66	3.84
% of Mean	0.67%	-0.27%	1.56%
N	4,752	4,752	4,752

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

Note: Results from the estimation specified in Equation 3.4 using the subsample of cities in states that test the BAC of at least 80% of deceased drivers. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 3.8: Effect of Uber and Lyft on Monthly Drunk-Driver-Related Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression, Stratified by Public Transit Accessibility (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft High	-0.06	-0.06	-0.01
	(0.06)	(0.05)	(0.05)
Dependent Variable Mean	1.21	0.92	0.29
% of Mean	-4.96%	-6.52%	-3.45%
Uber and/or Lyft Medium	0.07	0.09	-0.02
	(0.09)	(0.06)	(0.05)
Dependent Variable Mean	0.88	0.70	0.19
% of Mean	7.95%	12.86%	-10.53%
Uber and/or Lyft Low	-0.08	0.01	-0.09*
	(0.15)	(0.12)	(0.05)
Dependent Variable Mean	0.51	0.38	0.13
% of Mean	-15.69%	2.63%	-69.23%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft High	-0.14	-0.12**	-0.02
	(0.08)	(0.05)	(0.06)
Dependent Variable Mean	1.38	1.06	0.32
% of Mean	-10.14%	-11.32%	-6.25%
Uber and/or Lyft Medium	0.04	0.08	-0.04
	(0.11)	(0.07)	(0.05)
Dependent Variable Mean	0.99	0.79	0.20
% of Mean	4.04%	10.13%	-20.00%
Uber and/or Lyft Low	-0.15	-0.04	-0.10*
	(0.16)	(0.12)	(0.06)
Dependent Variable Mean	0.56	0.42	0.14
% of Mean	-26.79%	-9.52%	-71.43%
N	13,062	13,062	13,062

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

Note: Results from the estimation specified in Equation 3.4 with treatment interacted with public transit accessibility. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 3.9: Effect of Uber and Lyft on Monthly Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression, Stratified by Public Transit Accessibility (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft High	0.02	-0.12	0.13
	(0.20)	(0.09)	(0.16)
Dependent Variable Mean	6.41	2.99	3.42
% of Mean	0.31%	-4.01%	3.80%
Uber and/or Lyft Medium	0.47**	0.18	0.29**
	(0.19)	(0.12)	(0.14)
Dependent Variable Mean	3.71	1.85	1.86
% of Mean	12.67%	9.73%	15.59%
Uber and/or Lyft Low	0.26	0.07	0.19
	(0.23)	(0.15)	(0.14)
Dependent Variable Mean	2.26	1.07	1.20
% of Mean	11.50%	6.54%	15.83%
Panel B. Overall Motor Vehicle Fatalities			
Uber and/or Lyft High	-0.01	-0.12	0.10
	(0.21)	(0.09)	(0.15)
Dependent Variable Mean	6.86	3.25	3.61
% of Mean	-0.15%	-3.69%	2.77%
Uber and/or Lyft Medium	0.57***	0.27*	0.29**
	(0.21)	(0.14)	(0.14)
Dependent Variable Mean	3.98	2.03	1.95
% of Mean	14.32%	13.30%	14.87%
Uber and/or Lyft Low	0.33	0.11	0.23
	(0.25)	(0.17)	(0.16)
Dependent Variable Mean	2.42	1.16	1.26
% of Mean	13.64%	9.48%	18.25%
N	13,062	13,062	13,062

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

Note: Results from the estimation specified in Equation 3.4 with treatment interacted with public transit accessibility. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 3.10: Effect of Uber and Lyft on Monthly Motor Vehicles Fatalities and Fatal Crashes: Negative Binomial Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.03	-0.04	0.00
	(0.06)	(0.07)	(0.13)
Dependent Variable Mean	1.00	0.76	0.24
% of Mean	-3.00%	-5.26%	0.00%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.08	-0.09	-0.06
	(0.07)	(0.07)	(0.14)
Dependent Variable Mean	1.13	0.88	0.26
% of Mean	-7.08%	-10.23%	-23.08%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.02	-0.02	-0.00
	(0.04)	(0.03)	(0.02)
Dependent Variable Mean	1.18	0.88	0.30
% of Mean	-1.69%	-2.27%	-0.00%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.05	-0.04	-0.01
	(0.05)	(0.04)	(0.02)
Dependent Variable Mean	1.33	1.01	0.33
% of Mean	-3.76%	-3.96%	-3.03%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.01	†	0.00
	(0.07)		(0.05)
Dependent Variable Mean	4.95	2.35	2.60
% of Mean	0.20%	†	0.00%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.00	0.00	-0.01
	(0.08)	(0.08)	(0.05)
Dependent Variable Mean	5.30	2.55	2.75
% of Mean	0.00%	0.00%	-0.36%
N	13,062	13,062	13,062

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

Note: Results from the estimation specified in Equation 3.4 using a negative binomial estimation instead of a Poisson estimation. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

†This specification did not converge so no estimate is reported.

Table 3.11: Effect of Uber and Lyft on Log of Monthly Motor Vehicles Fatalities and Fatal Crashes

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.02	-0.03	-0.01
	(0.02)	(0.02)	(0.02)
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.04*	-0.04**	-0.01
	(0.02)	(0.02)	(0.02)
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.01	-0.01	-0.00
	(0.02)	(0.02)	(0.02)
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.02	-0.03	-0.01
	(0.02)	(0.02)	(0.02)
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.02	-0.01	0.03
	(0.02)	(0.02)	(0.03)
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.02	-0.01	0.02
	(0.02)	(0.02)	(0.03)
N	13,062	13,062	13,062

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

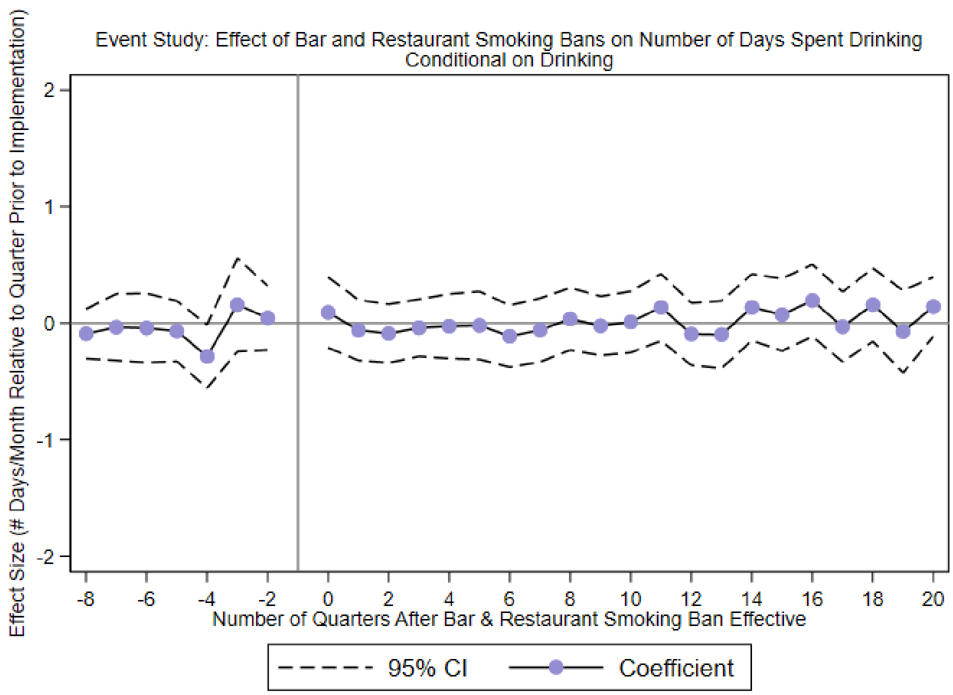
Note: Results from the estimation specified in Equation 3.7. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

APPENDIX A

APPENDIX FOR CHAPTER 1

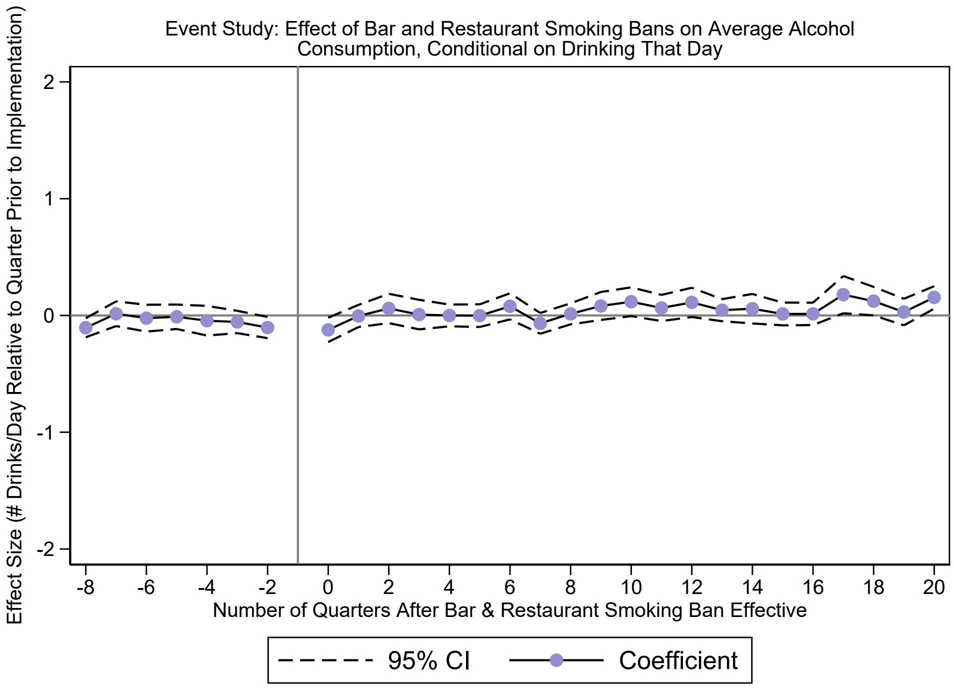
A.1 Additional Figures and Tables

Figure A.1



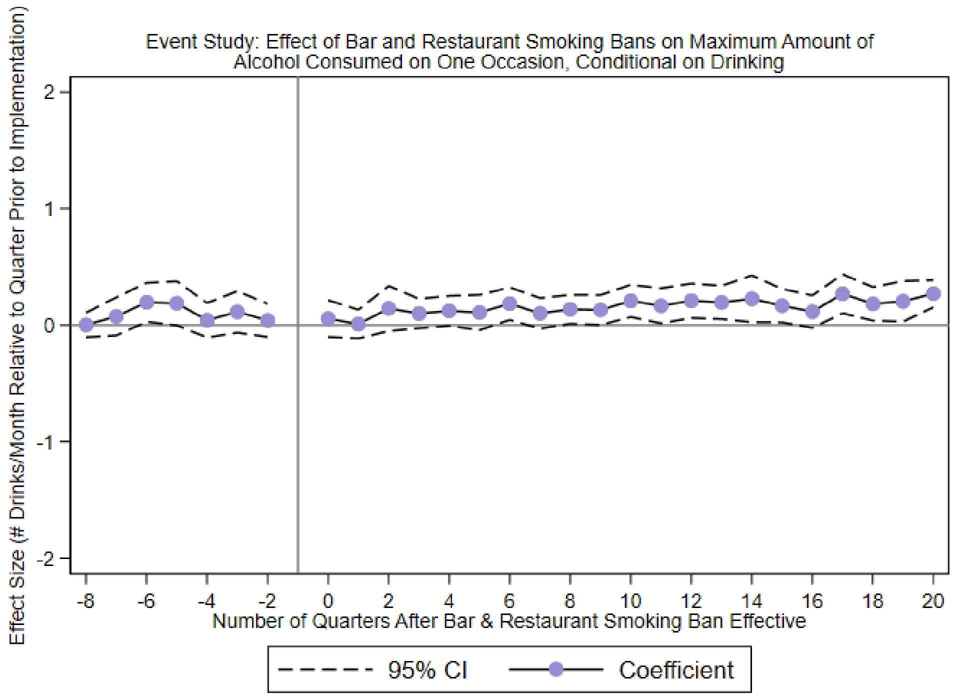
Note: Results from the estimation specified in Equation 1.4. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure A.2



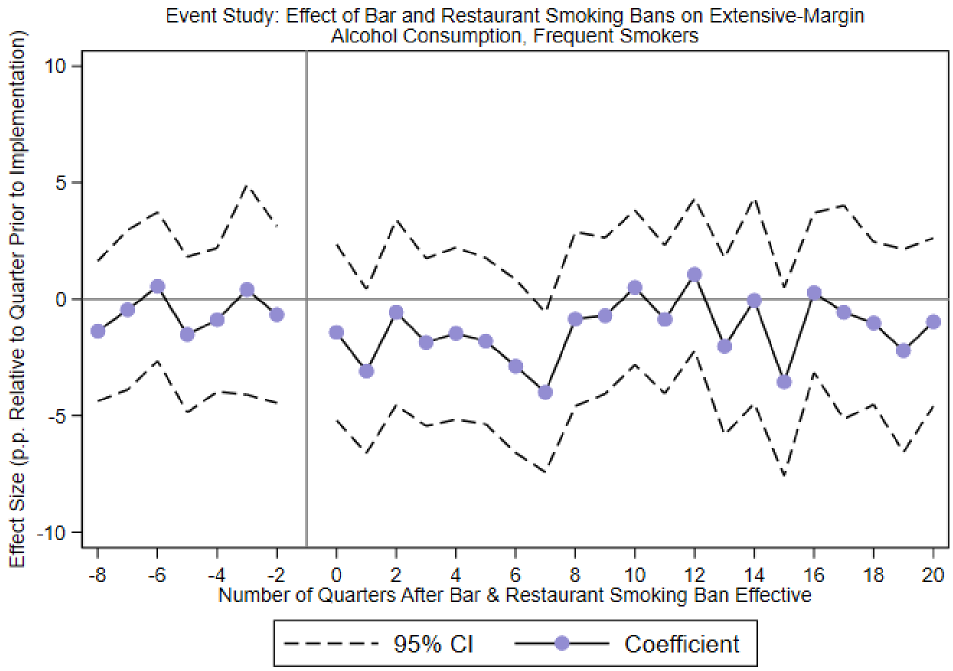
Note: Results from the estimation specified in Equation 1.4. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure A.3



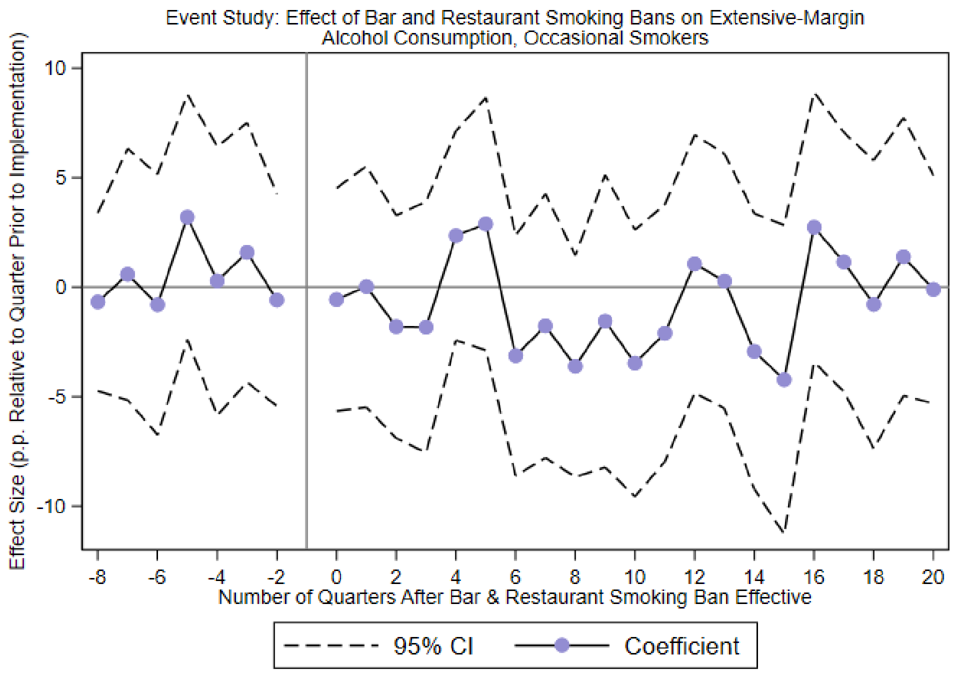
Note: Results from the estimation specified in Equation 1.4. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure A.4



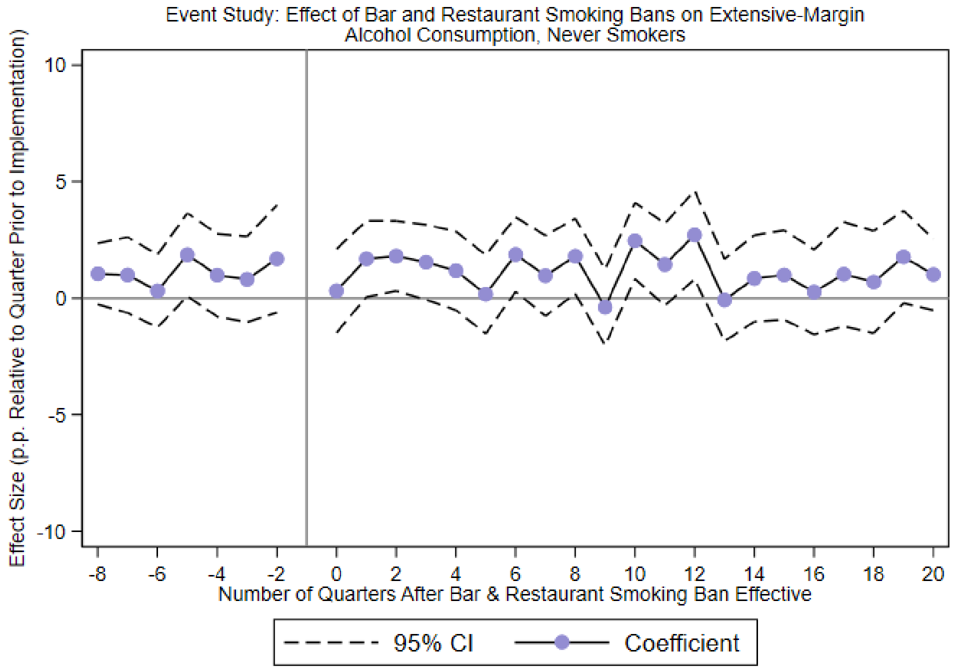
Note: Results from the estimation specified in Equation 1.4. Sample restricted to frequent smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure A.5



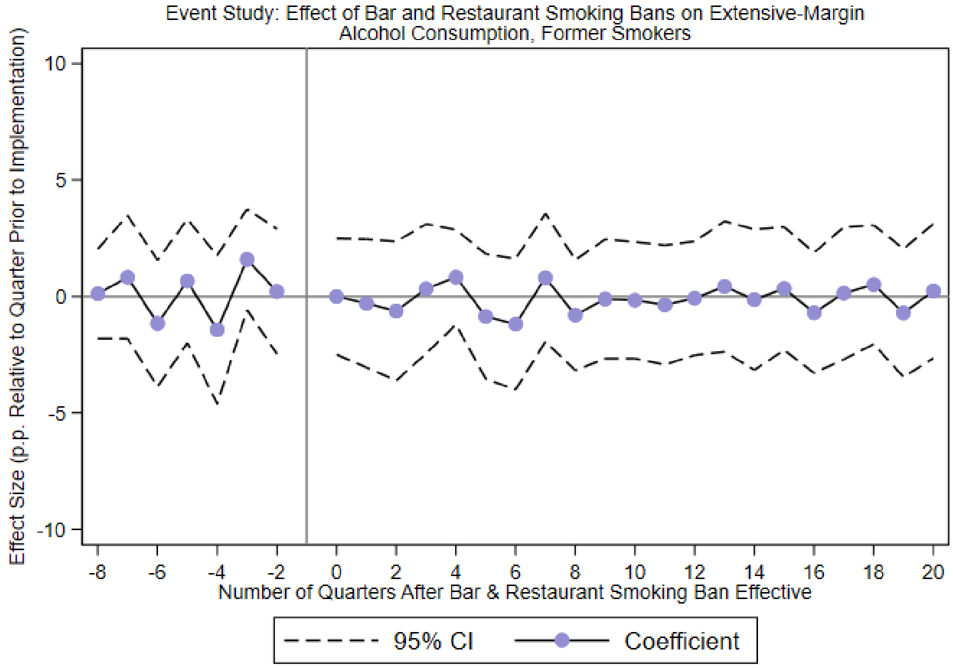
Note: Results from the estimation specified in Equation 1.4. Sample restricted to occasional smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure A.6



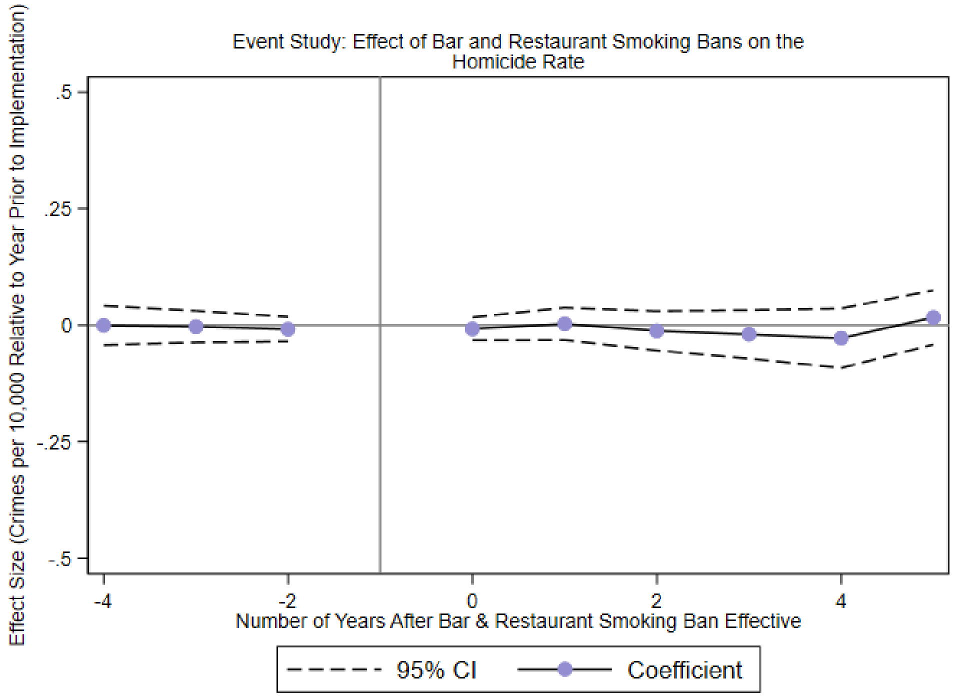
Note: Results from the estimation specified in Equation 1.4. Sample restricted to never smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure A.7



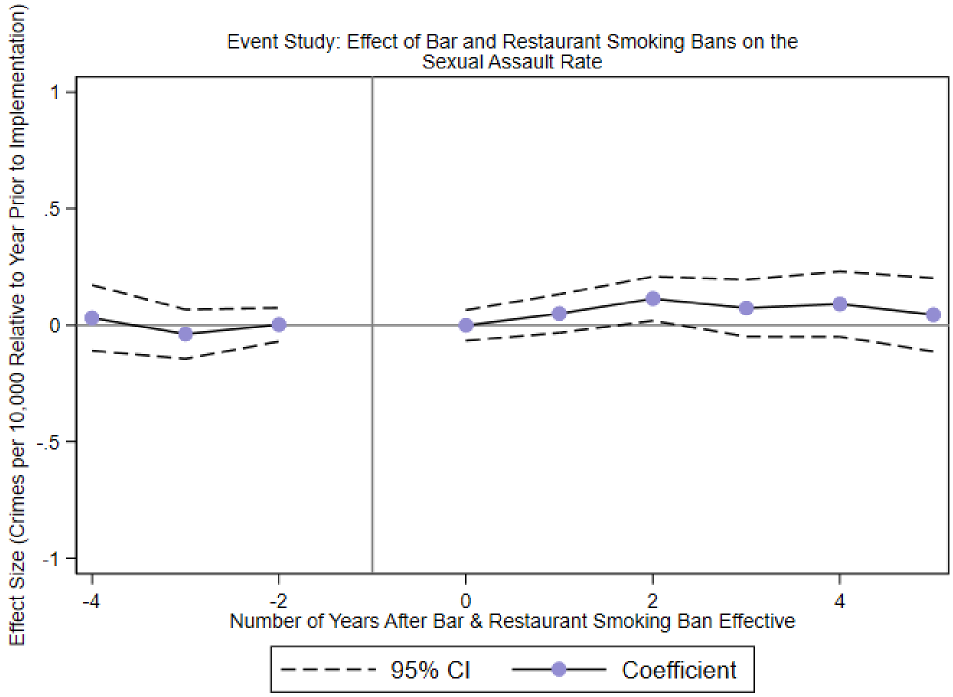
Note: Results from the estimation specified in Equation 1.4. Sample restricted to former smokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Figure A.8



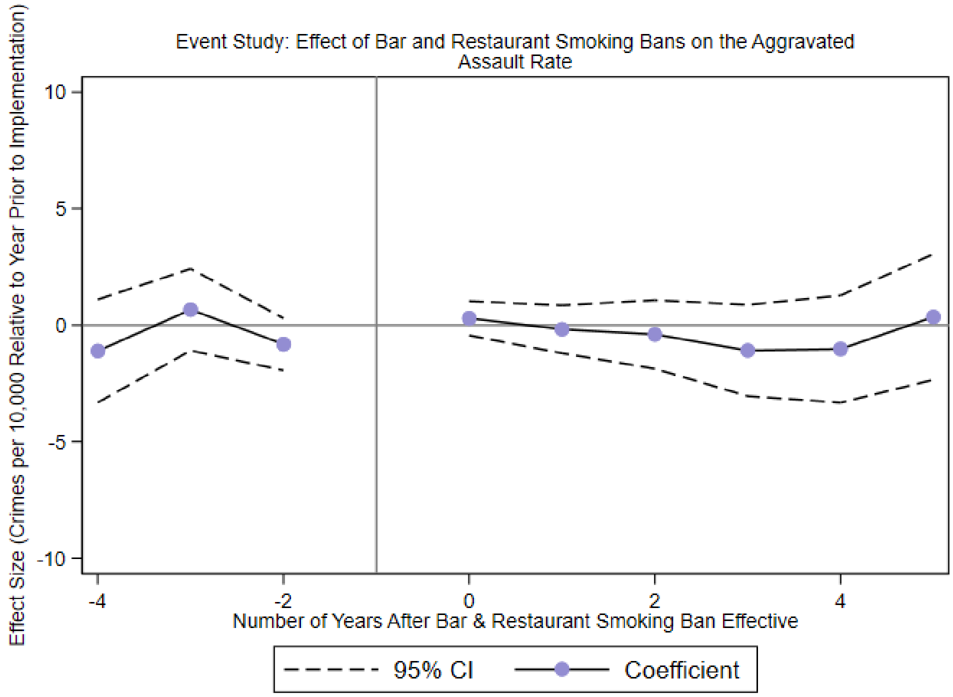
Note: Results from the estimation specified in Equation 1.6. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include agency and year fixed effects. Treatment is defined as whether the agency's jurisdiction is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the agency level. Regressions are probability weighted using the agency population. Data source: UCR 2004-2012.

Figure A.9



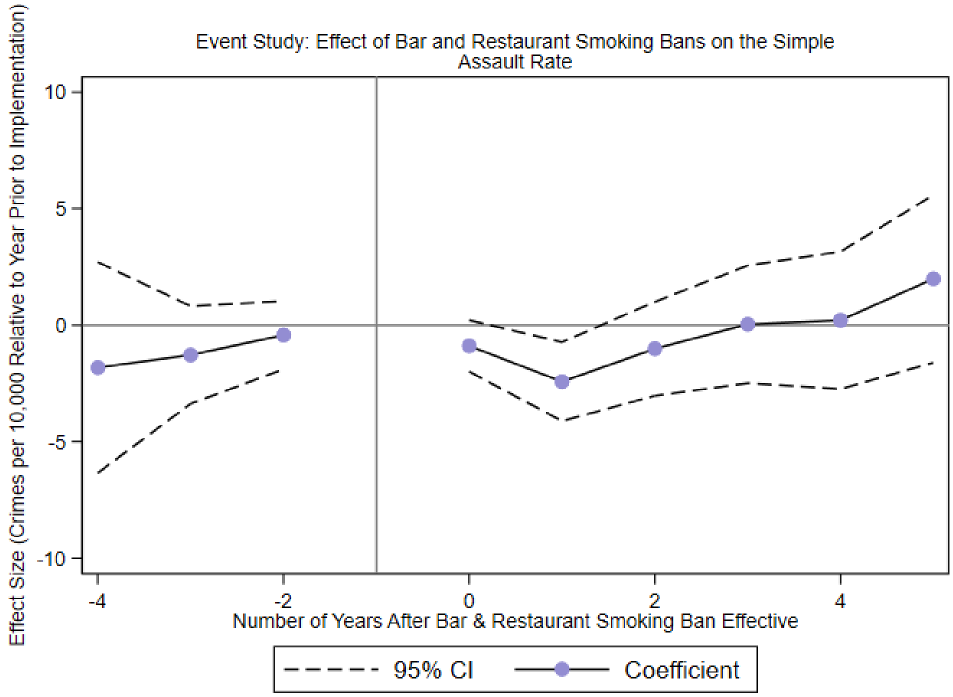
Note: Results from the estimation specified in Equation 1.6. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include agency and year fixed effects. Treatment is defined as whether the agency's jurisdiction is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the agency level. Regressions are probability weighted using the agency population. Data source: UCR 2004-2012.

Figure A.10



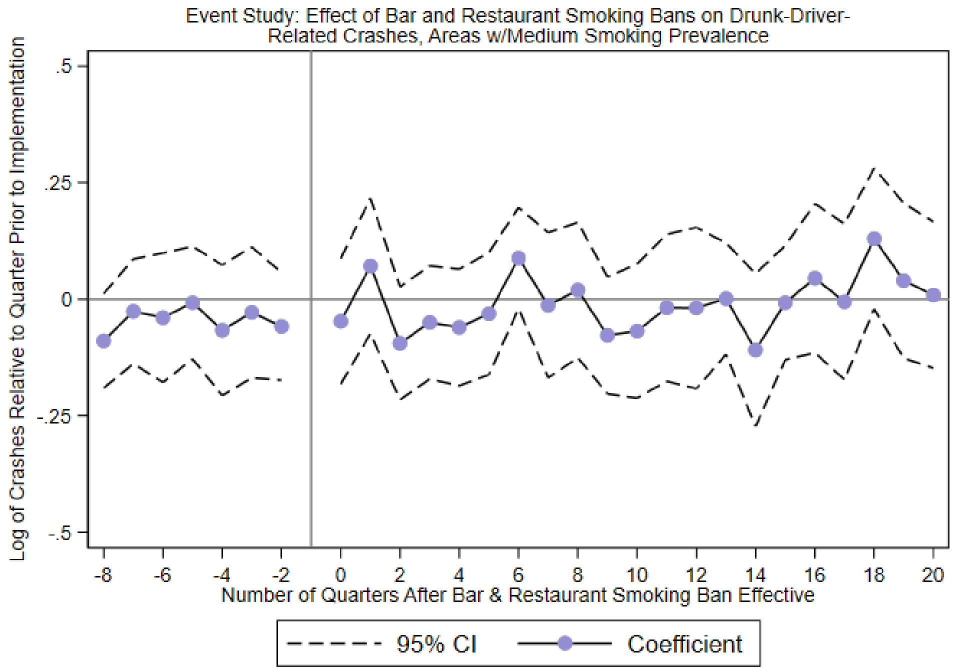
Note: Results from the estimation specified in Equation 1.6. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include agency and year fixed effects. Treatment is defined as whether the agency's jurisdiction is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the agency level. Regressions are probability weighted using the agency population. Data source: UCR 2004-2012.

Figure A.11



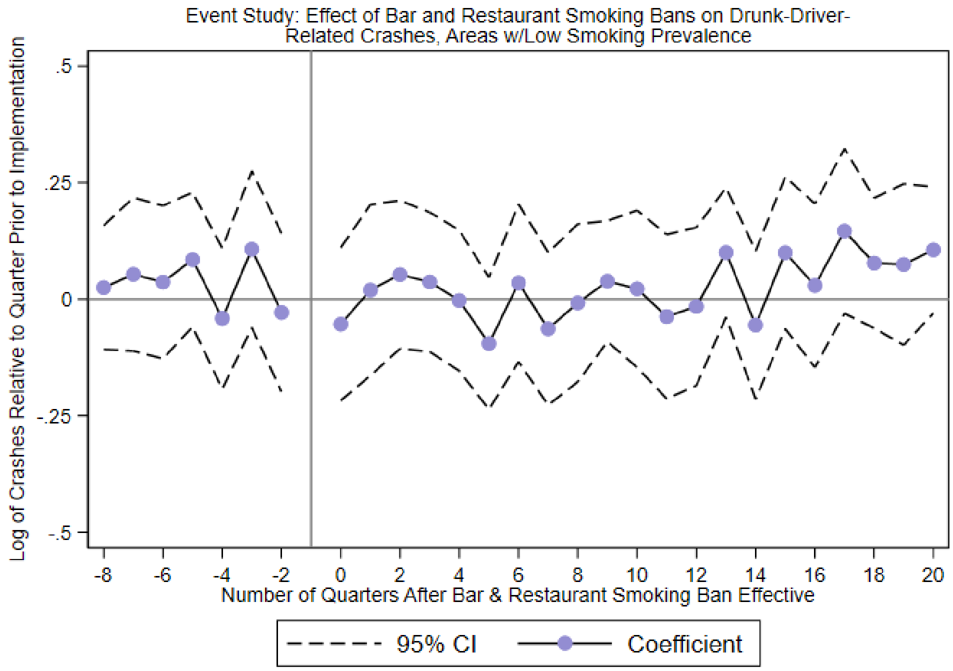
Note: Results from the estimation specified in Equation 1.6. Policy controls are (1) the fraction of the county population subject to a smoking ban in restaurants only, (2) an indicator for a law mandating the BAC limit for driving under the influence is 0.08, and (3) the state cigarette tax per pack. Controls also include agency and year fixed effects. Treatment is defined as whether the agency's jurisdiction is covered by a smoking ban in both bars and restaurants. Standard errors are clustered at the agency level. Regressions are probability weighted using the agency population. Data source: UCR 2004-2012.

Figure A.12



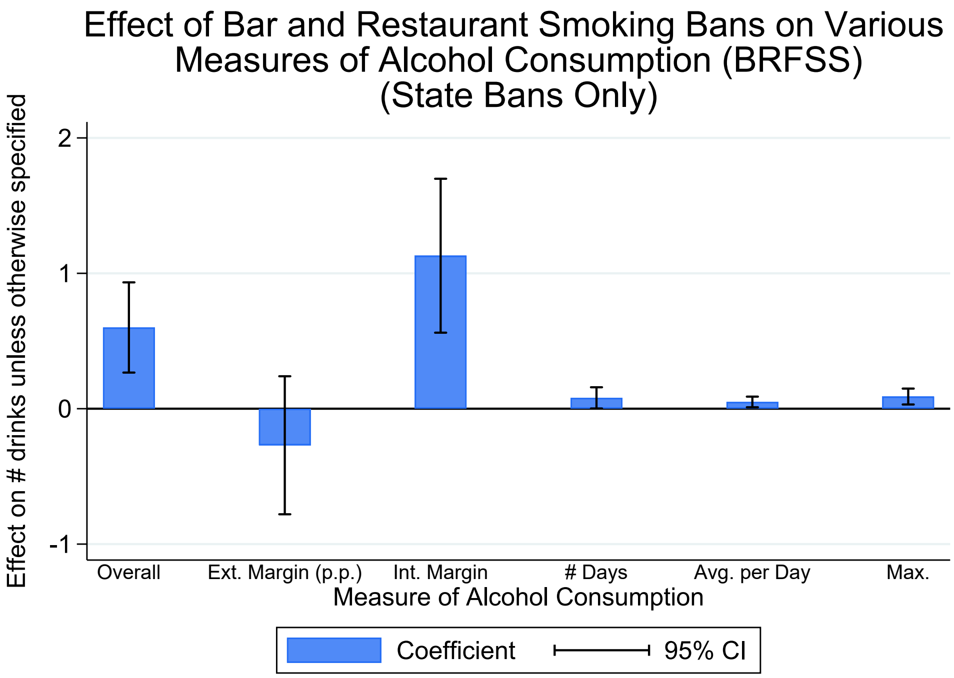
Note: Results from the estimation specified in Equation 1.8. Sample restricted to counties with a medium prevalence of smoking. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: FARS 2004-2012.

Figure A.13



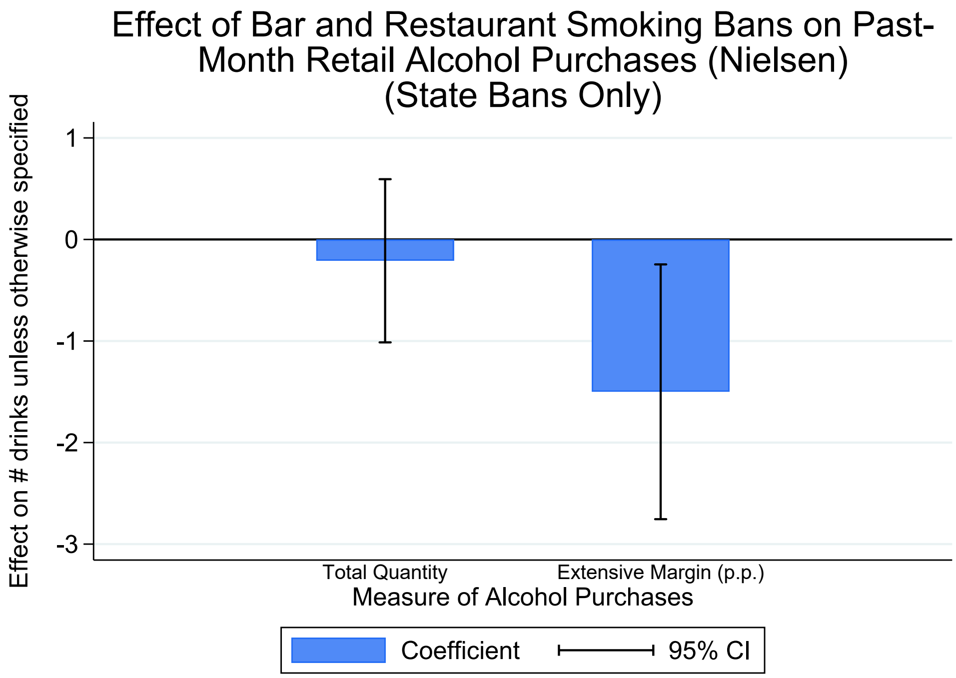
Note: Results from the estimation specified in Equation 1.8. Sample restricted to counties with a low prevalence of smoking. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: FARS 2004-2012.

Figure A.14



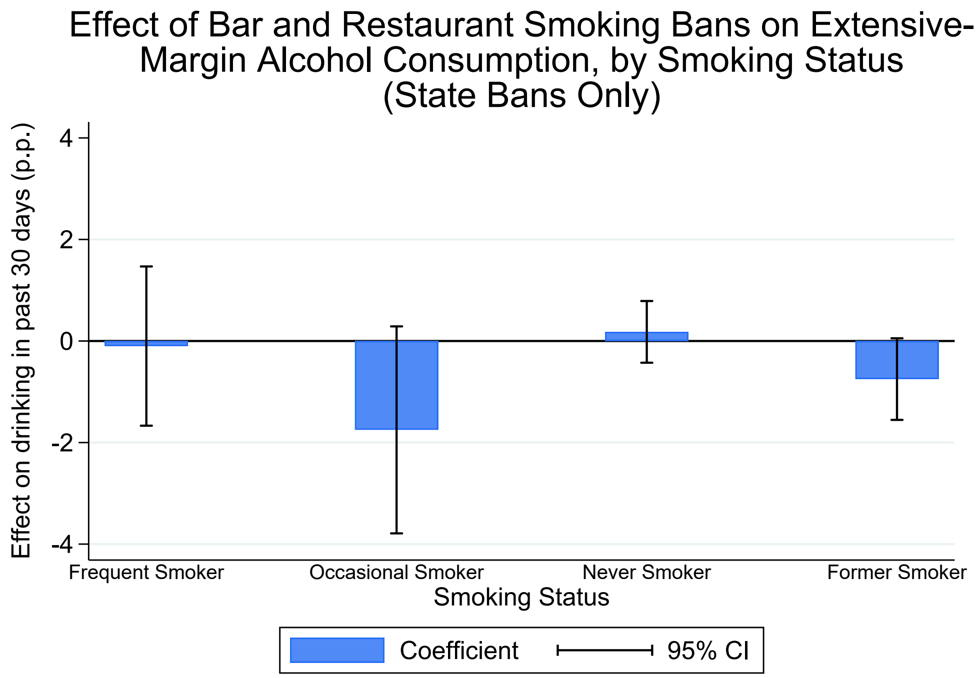
Note: Results from the estimation specified in Equation 1.1. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. Policy controls are (1) 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 county and month-year fixed effects. Treatment is defined as an indicator for whether the state has a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the county population. Data source: BRFSS 2004-2012.

Figure A.15



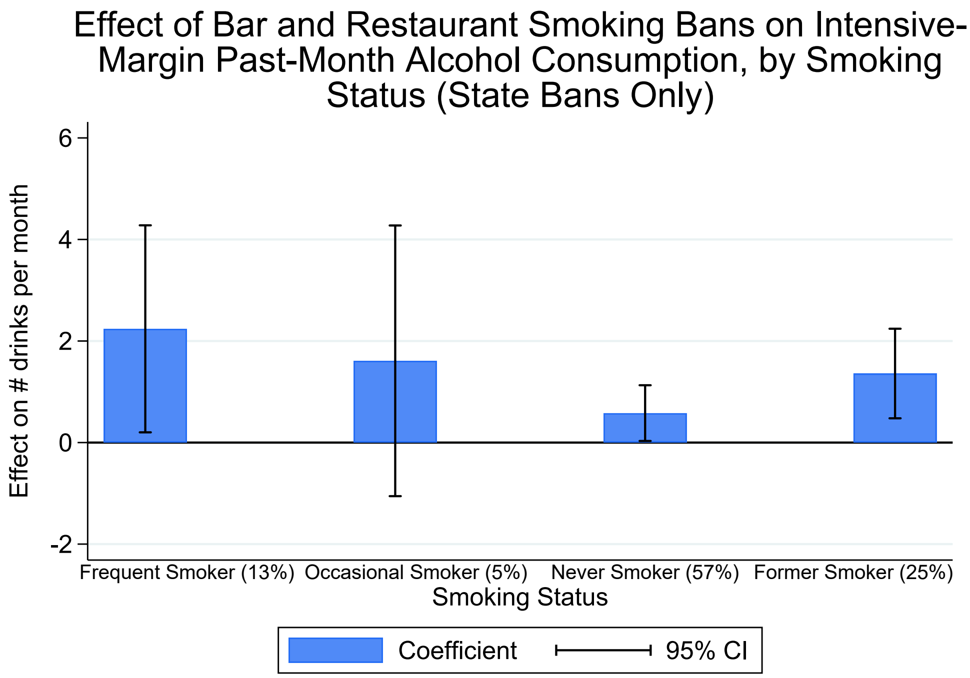
Note: Results from the estimation specified in Equation 1.1. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. Policy controls are (1) 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 county and month-year fixed effects. Treatment is defined as an indicator for whether the state has a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure A.16



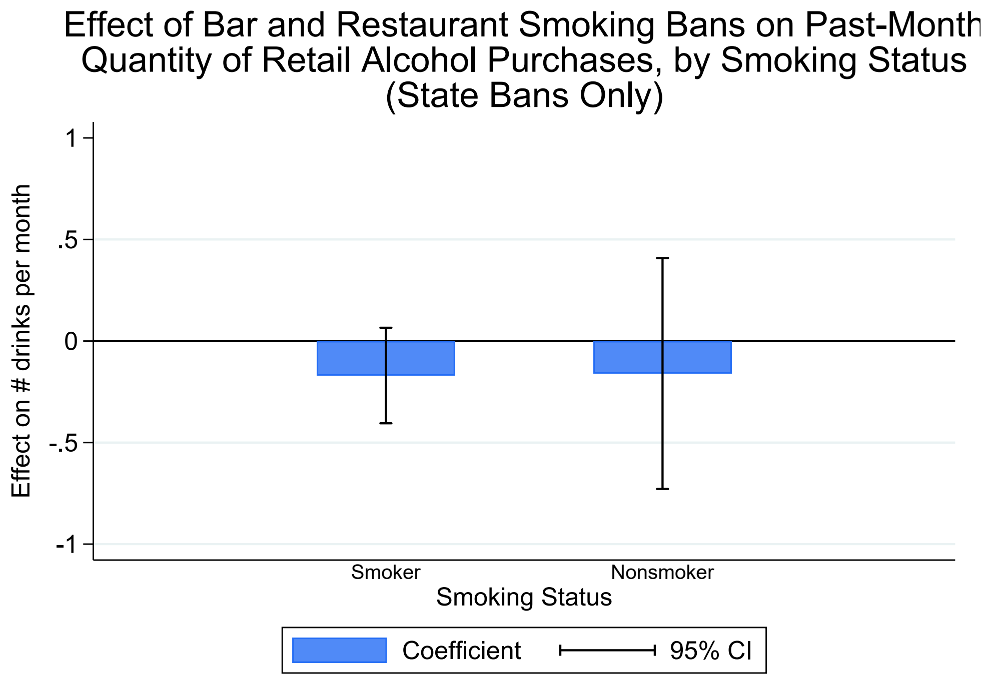
Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. Policy controls are (1) 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 county and month-year fixed effects. Treatment is defined as an indicator for whether the state has a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the county population. Data source: BRFSS 2004-2012.

Figure A.17



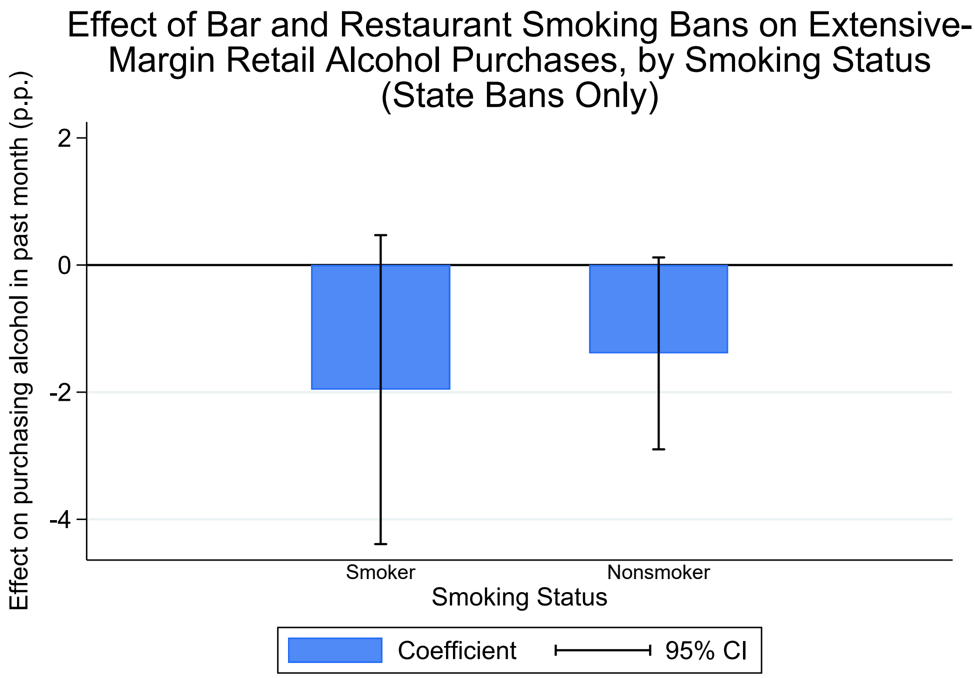
Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. Policy controls are (1) 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 county and month-year fixed effects. Treatment is defined as an indicator for whether the state has a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the county population. Data source: BRFSS 2004-2012.

Figure A.18



Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. Policy controls are (1) 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 county and month-year fixed effects. Treatment is defined as an indicator for whether the state has a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the county population. Data source: Nielsen Consumer Panel 2004-2012.

Figure A.19



Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. Policy controls are (1) 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 county and month-year fixed effects. Treatment is defined as an indicator for whether the state has a smoking ban in both bars and restaurants. Standard errors are clustered at the county level. Regressions are probability weighted using the county population. Data source: Nielsen Consumer Panel 2004-2012.

Table A.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	0.66
	(0.49)	(0.00)	(0.00)	(0.46)
Binary bar ban	0.55	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
	(0.44)	(0.00)	(0.00)	(0.00)
Fraction restaurant-only ban	0.11	0.34	0.07	0.03
	(0.31)	(0.47)	(0.23)	(0.14)
Alcohol consumption:	11.66	10.92	11.11	11.94
total servings	(14.03)	(17.59)	(12.48)	(12.45)
Alcohol consumption:	53.19	48.46	52.99	54.93
extensive margin (p.p.)	(19.89)	(23.77)	(19.79)	(17.93)
Alcohol consumption:	21.91	22.45	21.03	21.72
intensive margin	(23.06)	(29.74)	(23.16)	(20.23)
Alcohol consumption:	8.36	8.43	7.96	8.33
# days	(3.94)	(4.86)	(3.86)	(3.56)
Alcohol consumption:	2.41	2.38	2.41	2.42
amount per day	(1.26)	(1.54)	(1.22)	(1.15)
Alcohol consumption:	3.51	3.40	3.55	3.55
max.	(1.76)	(2.08)	(1.85)	(1.63)
% Frequent smokers	13.04	15.17	14.54	12.25
	(13.16)	(16.55)	(12.76)	(11.56)
% Occasional smokers	5.25	5.24	5.31	5.26
	(8.04)	(9.79)	(7.77)	(7.29)
% Never smokers	56.60	53.97	55.65	57.57
	(18.25)	(22.12)	(17.43)	(16.49)
% Former smokers	25.10	25.62	24.50	24.91
	(15.28)	(18.82)	(14.53)	(13.75)
Observations	190,230	83,988	39,465	106,242

Note: Data are from the 2004-2012 waves of the Behavioral Risk Factor Surveillance System. Each observation is a county-month and the data are aggregated from the individual level. “Fraction bar ban” is defined as the fraction of the county population subject to a bar and restaurant smoking ban for that month. “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 county population.

Table A.2: 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	0.66
	(0.49)	(0.00)	(0.00)	(0.46)
Fraction restaurant-only ban	0.11	0.34	0.07	0.03
	(0.31)	(0.47)	(0.23)	(0.14)
% Female	50.92	51.01	50.89	50.88
	(1.10)	(1.28)	(1.04)	(1.03)
% Black	12.60	15.77	13.29	11.43
	(12.74)	(14.73)	(13.78)	(11.70)
% Asian	4.90	2.46	2.96	5.80
	(5.87)	(2.67)	(3.83)	(6.44)
% Hispanic	16.29	10.95	11.67	18.26
	(16.32)	(13.02)	(14.79)	(16.96)
% White	63.66	68.48	69.59	61.88
	(21.56)	(19.97)	(19.97)	(21.85)
% Other race	2.56	2.34	2.49	2.64
	(3.51)	(3.20)	(4.13)	(3.61)
% Age < 15	20.06	19.40	20.87	20.30
	(2.61)	(2.43)	(2.48)	(2.64)
% Age 15-24	14.26	13.79	14.58	14.43
	(2.82)	(2.95)	(3.29)	(2.76)
% Age 25-34	13.46	12.85	13.38	13.69
	(2.30)	(2.22)	(2.05)	(2.28)
% Age 35-44	13.91	13.61	14.37	14.02
	(1.57)	(1.64)	(1.63)	(1.53)
% Age 45-64	25.63	26.25	24.94	25.40
	(2.69)	(2.39)	(2.73)	(2.76)
% Age 65+	12.68	14.10	11.86	12.16
	(3.36)	(4.33)	(2.91)	(2.74)
BAC 0.08%	1.00	1.00	0.98	0.99
	(0.07)	(0.00)	(0.14)	(0.08)
Cigarette tax	1.91	1.47	1.45	2.07
per pack ($)	(0.98)	(0.66)	(0.75)	(1.03)
County	1,176,145	506,095	617,510	1,423,842
population	(1,952,022)	(602,440)	(806,120)	(2,203,975)
Observations	190,230	83,988	39,465	106,242

Note: Data are from the 2004-2012 waves of the Behavioral Risk Factor Surveillance System. Each observation is a county-month and the data are aggregated from the individual level. “Fraction bar ban” is defined as the fraction of the county population 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. 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 county population.

Table A.3: 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	0.46	0.00	0.00	0.64
	(0.49)	(0.00)	(0.00)	(0.46)
Binary bar ban	0.52	0.00	0.00	0.73
	(0.50)	(0.00)	(0.00)	(0.44)
Ever bar ban	0.71	0.00	1.00	1.00
	(0.45)	(0.00)	(0.00)	(0.00)
Fraction restaurant-only ban	0.11	0.30	0.07	0.03
	(0.30)	(0.46)	(0.23)	(0.14)
Alcohol purchases:	5.33	5.19	5.59	5.38
total servings	(5.93)	(7.66)	(6.77)	(5.08)
Alcohol purchases:	25.78	23.77	25.92	26.58
extensive margin (p.p.)	(15.14)	(18.29)	(16.75)	(13.60)
% Smokers	21.96	24.52	24.25	20.95
	(14.99)	(18.96)	(16.85)	(12.95)
County	1,086,864	445,428	548,451	1,345,809
population	(1,894,091)	(584,151)	(777,035)	(2,159,856)
Observations	339,408	156,672	87,994	182,736

Note: Data are from the 2004-2012 waves of the Nielsen Consumer Panel. Each observation is a county-month and the data are aggregated from the household level. "Fraction bar ban" is defined as the fraction of the county population subject to a bar and restaurant smoking ban for that month. "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 is measured as the total number of servings of alcohol purchased for off-premises consumption in the past month. Statistics are weighted by the county population.

Table A.4: Summary Statistics of Crime Rates by Treatment Status, 2004-2012 Uniform Crime Reports

	(1) Full Sample	(2) Never Smoking Ban	(3) Before Smoking Ban	(4) Ever Smoking Ban
Bar ban	0.46	0.00	0.00	0.79
	(0.49)	(0.00)	(0.00)	(0.39)
Ever bar ban	0.65	0.00	1.00	1.00
	(0.48)	(0.00)	(0.00)	(0.00)
Restaurant-only ban	0.11	0.24	0.04	0.01
	(0.31)	(0.43)	(0.20)	(0.09)
BAC 0.08%	0.99	1.00	0.97	0.99
	(0.08)	(0.00)	(0.18)	(0.08)
Cigarette tax per pack ($)	1.87	1.64	1.80	2.22
	(1.00)	(0.72)	(0.78)	(0.98)
Violent crime	53.18	54.36	54.91	47.08
per 10,000 people	(43.15)	(51.66)	(84.19)	(67.94)
Murders	0.54	0.32	0.25	0.30
per 10,000 people	(0.72)	(0.79)	(0.62)	(0.64)
Rapes	2.89	2.30	2.58	2.09
per 10,000 people	(2.43)	(3.07)	(3.08)	(2.44)
Aggravated assaults	35.94	42.11	44.16	34.85
per 10,000 people	(31.93)	(42.61)	(79.51)	(58.95)
Simple assaults	98.80	82.59	90.09	68.18
per 10,000 people	(81.43)	(83.24)	(98.60)	(75.07)
Population of	556,840	43,973	69,291	92,710
agency jurisdiction	(1,491,802)	(144,739)	(253,375)	(375,634)
Observations	104,766	48,237	19,447	56,529

Note: Data are from the 2004-2012 years of the Uniform Crime Reports. Each observation is an agency-year. "Bar ban" is defined as the fraction of the year in months the agency's jurisdiction is subject to a bar and restaurant smoking ban. "Restaurant-only ban" is defined as the fraction of the year in months the agency's jurisdiction is subject to a restaurant smoking ban but not a bar smoking ban. 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 population in the agency's jurisdiction.

Table A.5: Summary Statistics of Drunk-Driver-Related Fatal Motor Vehicle Crashes by Treatment Status, 2004-2012 Fatality Analysis Reporting System

	(1) Full Sample	(2) High Smoking	(3) Medium Smoking	(4) Low Smoking
Fraction bar ban	0.46	0.21	0.43	0.77
	(0.49)	(0.38)	(0.49)	(0.42)
Binary bar ban	0.52	0.33	0.48	0.78
	(0.50)	(0.47)	(0.50)	(0.42)
Ever bar ban	0.71	0.58	0.67	0.91
	(0.45)	(0.49)	(0.47)	(0.29)
Fraction restaurant-only ban	0.11	0.08	0.21	0.01
	(0.30)	(0.23)	(0.41)	(0.08)
DD-related crashes	1.51	1.14	1.28	2.20
	(2.70)	(2.27)	(2.06)	(3.59)
DD-related crashes:	1.78	1.58	1.28	2.40
ever smoking ban	(3.04)	(2.72)	(2.27)	(3.71)
DD-related crashes:	0.98	1.39	0.77	0.53
before smoking ban	(1.89)	(2.46)	(1.40)	(0.93)
DD-related crashes:	0.83	0.53	1.28	0.22
never smoking ban	(1.38)	(1.18)	(1.55)	(0.52)
County population	1,086,788	571,776	976,785	1,788,932
	(1,893,918)	(878,637)	(1,242,809)	(2,912,905)
Observations	339,408	148,716	110,268	80,424

Note: Data are from the 2004-2012 years of the Fatality Analysis Reporting System. Each observation is a county-month. “Fraction bar ban” is defined as the fraction of the county population subject to a bar and restaurant smoking ban for that month. “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. Column (1) shows statistics for the full sample. Column (2) restricts the sample to counties in states with a high prevalence of smoking as measured by the 1992 Tobacco Use Supplement to the Current Population Survey. Column (3) restricts the sample to counties in states with a medium prevalence of smoking. Column (4) restricts the sample to counties in states with a low prevalence of smoking. Statistics are weighted by the county population.

Table A.6: Effect of Bar and Restaurant Smoking Bans on Number of Days Spent Drinking in Past 30 Days (Conditional on Drinking in the Past 30 Days)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar/Restaurant Ban	0.03	0.28	0.03	0.06
(standard error)	(0.14)	(0.18)	(0.06)	(0.09)
[95% confidence interval]	[-0.25, 0.31]	[-0.07, 0.63]	[-0.09, 0.14]	[-0.12, 0.25]
Dependent Variable Mean	9.76	8.55	7.18	10.20
% of Mean	0.29%	3.28%	0.37%	0.60%
R^2	0.04	0.05	0.08	0.06
N	86,904	46,853	130,017	114,293

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

Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table A.7: Effect of Bar and Restaurant Smoking Bans on Average Alcohol Consumption per Drinking Day (Conditional on Drinking in Past 30 Days)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar/Restaurant Ban	0.03	0.07	0.03	0.10***
(standard error)	(0.08)	(0.08)	(0.02)	(0.03)
[95% confidence interval]	[-0.12, 0.18]	[-0.08, 0.22]	[-0.02, 0.07]	[0.05, 0.15]
Dependent Variable Mean	3.26	3.10	2.07	2.16
% of Mean	0.85%	2.15%	1.21%	4.72%
R^2	0.04	0.05	0.04	0.04
N	86,162	46,568	129,767	114,055

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

Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table A.8: Effect of Bar and Restaurant Smoking Bans on Maximum Alcohol Consumption on 1 Occasion (Conditional on Drinking in Past 30 Days)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar/Restaurant Ban	0.08	0.01	0.02	0.09**
(standard error)	(0.11)	(0.10)	(0.03)	(0.04)
[95% confidence interval]	[-0.14, 0.31]	[-0.19, 0.21]	[-0.04, 0.07]	[0.02, 0.16]
Dependent Variable Mean	4.83	4.67	2.96	3.19
% of Mean	1.75%	0.22%	0.61%	2.80%
R^2	0.04	0.06	0.04	0.04
N	76,671	41,170	117,972	103,746

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

Note: Results from the estimation specified in Equation 1.2. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table A.9: Effect of Bar and Restaurant Smoking Bans on Alcohol Consumption, by Smoking Prevalence (BRFSS)

	Overall (1)	Extensive Margin (2)	Intensive Margin (3)	# Days (4)	Avg. per Day (5)	Max. (6)
Bar/Rest. Ban	0.58	0.11	0.98	0.02	0.08**	0.13**
High smoking	(0.39)	(0.51)	(0.70)	(0.10)	(0.04)	(0.05)
[95% CI]	[-0.18, 1.34]	[-0.89, 1.11]	[-0.39, 2.35]	[-0.17, 0.21]	[0.00, 0.15]	[0.03, 0.23]
Mean	8.64	37.01	22.95	8.13	2.45	3.48
% of Mean	6.77%	0.29%	4.27%	0.26%	3.14%	3.73%
Bar/Rest. Ban	0.18	-0.57	0.41	-0.00	0.04	0.07*
Medium smoking	(0.22)	(0.42)	(0.36)	(0.06)	(0.03)	(0.04)
[95% CI]	[-0.25, 0.61]	[-1.39, 0.25]	[-0.30, 1.13]	[-0.12, 0.11]	[-0.02, 0.09]	[-0.00, 0.14]
Mean	11.47	50.63	22.63	8.36	2.46	3.63
% of Mean	1.57%	-1.12%	1.82%	-0.05%	1.57%	1.93%
Bar/Rest. Ban	1.02***	0.16	1.64***	0.19***	0.07**	0.05
Low smoking	(0.23)	(0.33)	(0.37)	(0.06)	(0.03)	(0.04)
[95% CI]	[0.56, 1.48]	[-0.49, 0.82]	[0.92, 2.36]	[0.07, 0.31]	[0.02, 0.12]	[-0.03, 0.12]
Mean	12.50	55.86	22.29	9.11	2.28	3.44
% of Mean	8.16%	0.29%	7.35%	2.06%	2.89%	1.33%
R^2	0.04	0.26	0.03	0.09	0.05	0.05
N	189,660	189,791	161,421	162,125	161,824	148,054

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

Note: Results from the estimation specified in Equation 1.1 with treatment interacted with smoking prevalence indicators. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table A.10: Effect of Bar and Restaurant Smoking Bans on Extensive-Margin Alcohol Consumption, by Smoking Status and Smoking Prevalence (BRFSS)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar/Restaurant Ban	-0.93	2.18	0.69	-0.39
High smoking	(1.40)	(1.73)	(0.54)	(0.82)
[95% confidence interval]	[-3.67, 1.81]	[-1.21, 5.56]	[-1.37, 1.75]	[-2.01, 1.22]
Dependent Variable Mean	44.60	49.36	31.51	39.49
% of Mean	-2.08%	4.41%	2.18%	-0.99%
Bar/Restaurant Ban	-0.79	-1.02	-0.12	-0.92
Medium smoking	(1.12)	(1.75)	(0.43)	(0.66)
[95% confidence interval]	[-3.00, 1.41]	[-4.46, 2.42]	[-0.97, 0.72]	[-2.21, 0.37]
Dependent Variable Mean	55.44	60.44	46.22	54.09
% of Mean	-1.43%	-1.69%	-0.27%	-1.70%
Bar/Restaurant Ban	1.15	-1.02	0.15	-0.51
Low smoking	(0.76)	(1.42)	(0.43)	(0.49)
[95% confidence interval]	[-0.34, 2.63]	[-3.81, 1.76]	[-0.68, 0.99]	[-1.46, 0.45]
Dependent Variable Mean	58.29	64.94	52.70	60.02
% of Mean	1.97%	-1.58%	0.29%	-0.84%
R^2	0.07	0.07	0.22	0.16
N	122,221	68,756	174,017	152,539

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

Note: Results from the estimation specified in Equation 1.2 with treatment interacted with smoking prevalence indicators. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table A.11: Effect of Bar and Restaurant Smoking Bans on Intensive-Margin Alcohol Consumption, by Smoking Status and Smoking Prevalence (BRFSS)

Smoking Status:	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar/Restaurant Ban	-0.94	0.25	0.41	2.50***
High smoking	(2.62)	(2.11)	(0.76)	(0.68)
[95% confidence interval]	[-6.08, 4.20]	[-3.88, 4.38]	[-1.07, 1.90]	[1.17, 3.83]
Dependent Variable Mean	36.11	25.92	15.65	22.95
% of Mean	-2.60%	0.96%	2.63%	10.89%
Bar/Restaurant Ban	0.15	2.36*	-0.08	1.35*
Medium smoking	(1.47)	(1.35)	(0.41)	(0.64)
[95% confidence interval]	[-2.73, 3.04]	[-0.29, 5.02]	[-0.88, 0.72]	[-0.02, 2.51]
Dependent Variable Mean	35.48	26.86	16.29	22.99
% of Mean	0.43%	8.79%	-0.49%	5.42%
Bar/Restaurant Ban	4.15***	3.15*	0.70**	0.76*
Low smoking	(1.50)	(1.86)	(0.34)	(0.43)
[95% confidence interval]	[1.21, 7.09]	[-0.49, 6.79]	[0.04, 1.35]	[-0.10, 1.61]
Dependent Variable Mean	35.92	27.85	16.51	23.69
% of Mean	11.55%	11.32%	4.21%	3.19%
R^2	0.03	0.04	0.03	0.03
N	85,645	46,161	129,394	113,598

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

Note: Results from the estimation specified in Equation 1.2 with treatment interacted with smoking prevalence indicators. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

Table A.12: Effect of Bar and Restaurant Smoking Bans on Alcohol Consumption, by Smoking Prevalence (Nielsen)

	Total Quantity (1)	Total Quantity Smokers (2)	Total Quantity Non. (3)	Extensive Margin (4)	Extensive Margin Smokers (5)	Extensive Margin Non. (6)
Bar/Rest. Ban	-0.58*	-0.52**	-0.07	-0.42	-3.31**	0.85
High smoking	(0.30)	(0.23)	(0.19)	(0.55)	(1.46)	(0.60)
[95% CI]	[-1.18, 0.01]	[-0.96, -0.07]	[-0.44, 0.31]	[-1.49, 0.66]	[-6.16, -0.45]	[-0.32, 2.02]
Mean	4.36	2.62	2.66	19.12	25.28	17.31
% of Mean	13.38%	-19.68%	-2.52%	-2.17%	-2.04%	4.93%
Bar/Rest. Ban	-0.39**	-0.30*	-0.07	0.22	-0.90	0.29
Med. smoking	(0.19)	(0.17)	(0.14)	(0.41)	(1.25)	(0.47)
[95% CI]	[-0.77, -0.01]	[-0.62, 0.03]	[-0.33, 0.20]	[-0.58, 1.02]	[-3.35, 1.55]	[-0.63, 1.21]
Mean	5.66	2.75	3.81	23.80	29.28	22.31
% of Mean	-6.95%	-10.78%	-1.71%	0.91%	-3.07%	1.31%
Bar/Rest. Ban	-0.03	0.30*	-0.32	-0.94*	1.82	-1.48**
Low smoking	(0.25)	(0.17)	(0.20)	(0.55)	(1.29)	(0.62)
[95% CI]	[-0.51, 0.46]	[-0.03, 0.64]	[-0.71, 0.07]	[-2.01, 0.14]	[-0.71, 4.44]	[-2.69, -0.27]
Mean	6.31	3.20	4.32	25.80	30.23	24.50
% of Mean	-0.40%	9.47%	-7.36%	-3.63%	6.01%	-6.04%
R^2	0.36	0.32	0.34	0.40	0.28	0.37
N	280,632	197,484	268,296	280,632	197,484	268,296

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

Note: Results from the estimation specified in Equations 1.1 (Columns 1 and 4) and 1.2 (Columns 2, 3, 5, and 6) with treatment interacted with smoking prevalence indicators. Columns 2 and 5 show the results for the subsample of smokers, while Columns 3 and 6 show the results for the subsample of nonsmokers. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: Nielsen Consumer Panel 2004-2012.

Table A.13: Effect of Bar and Restaurant Smoking Bans on Extensive-Margin Smoking, by Smoking Prevalence

	Frequent (1)	Occasional (2)	Never (3)	Former (4)
Bar/Restaurant Ban	-0.02	0.31	0.17	-0.46
High smoking	(0.28)	(0.24)	(0.44)	(0.40)
[95% confidence interval]	[-0.57, 0.54]	[-0.17, 0.78]	[-0.69, 1.04]	[-1.25, 0.33]
Dependent Variable Mean	17.36	5.20	51.96	25.49
% of Mean	-0.11%	5.89%	0.33%	-1.81%
Bar/Restaurant Ban	0.26	0.30*	-0.35	-0.21
Medium smoking	(0.25)	(0.18)	(0.28)	(0.23)
[95% confidence interval]	[-0.23, 0.75]	[-0.05, 0.66]	[-0.91, 0.21]	[-0.67, 0.24]
Dependent Variable Mean	15.35	4.95	52.84	26.86
% of Mean	1.69%	6.14%	-0.66%	-0.79%
Bar/Restaurant Ban	0.02	0.02	0.13	-0.17
Low smoking	(0.21)	(0.13)	(0.31)	(0.24)
[95% confidence interval]	[-0.39, 0.42]	[-0.24, 0.28]	[-0.48, 0.74]	[-0.63, 0.30]
Dependent Variable Mean	13.36	4.68	54.27	27.69
% of Mean	0.13%	0.40%	0.24%	-0.60%
R^2	0.11	0.03	0.12	0.07
N	190,096	190,096	190,096	190,096

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

Note: Results from the estimation specified in Equation 1.3 with treatment interacted with smoking prevalence indicators. Demographic controls are the percentages of the county population that is male, Hispanic, non-Hispanic black, non-Hispanic Asian, non-Hispanic non-white other racial groups, younger than 15, 15 to 24, 35 to 44, 45 to 64, and 65 or older. The omitted categories for the demographic controls are the percentage female, percentage white, and percentage aged 25 to 34. 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 and month-year 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 county population. Data source: BRFSS 2004-2012.

APPENDIX B

APPENDIX FOR CHAPTER 2

B.1 BTB Treatment and Additional Results

Table B.1: The Effect of BTB on the Probability of Employment: Balanced Panel of Metropolitan Statistical Areas

	(1)	(2)	(3)	(4)	(5)
BTB x Black	-0.2011***	-0.1803***	0.0028	0.0088	-0.0060
	(0.0161)	(0.0147)	(0.0071)	(0.0073)	(0.0089)
BTB x Hispanic	0.0299*	0.0559***	0.0068	0.0145	0.0036
	(0.0173)	(0.0151)	(0.0076)	(0.0127)	(0.0078)
BTB x White	0.0660***	0.0506***	-0.0001	-0.0045	-0.0021
	(0.0074)	(0.0064)	(0.0044)	(0.0058)	(0.0043)
N	579,048	579,048	579,045	362,040	374,676
R^2	0.0358	0.1050	0.1370	0.1377	0.1410
Pre-BTB Mean: Black	0.5634	0.5634	0.5634	0.5634	0.5251
Pre-BTB Mean: Hisp.	0.7391	0.7391	0.7391	0.7391	0.7209
Pre-BTB Mean: White	0.8036	0.8036	0.8036	0.8036	0.7796
% Effect: Black	-35.69	-32.00	0.49	1.56	-1.13
% Effect: Hispanic	4.04	7.57	0.92	1.96	0.50
% Effect: White	8.21	6.30	-0.02	-0.56	-0.26
MSA FE	X	X	X	X	X
Year-Division FE	X	X	X	X	X
Demographics		X	X	X	X
MSA linear trends					
Fully-interact with race			X	X	X
MSAs only	X	X	X	X	X
BTB-adopting only				X	
2008 and later					X

Note: Results from the estimation specified in Equation 2.2 using a balanced panel of Metropolitan Statistical Areas (MSAs). Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in an MSA. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. The wording of survey questions about employment was changed starting in 2008. Column 5 omits all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Robustness Checks: Treatment Based on First BTB Law in the Metropolitan Statistical Area

	(1) Fraction	(2) January	(3) Half-Year	(4) December
BTB x Black	0.0010	0.0023	-0.0005	0.0001
	(0.0069)	(0.0068)	(0.0064)	(0.0062)
BTB x Hispanic	0.0065	0.0037	0.0014	0.0096
	(0.0081)	(0.0073)	(0.0073)	(0.0088)
BTB x White	0.0007	-0.0006	-0.0004	0.0014
	(0.0043)	(0.0039)	(0.0041)	(0.0039)
N	599,430	599,430	599,430	599,430
R^2	0.1384	0.1384	0.1384	0.1384
Pre-BTB Mean: Black	0.5631	0.5542	0.5601	0.5631
Pre-BTB Mean: Hispanic	0.7392	0.7304	0.7366	0.7392
Pre-BTB Mean: White	0.8037	0.7986	0.8023	0.8037
% Effect: Black	0.18	0.42	-0.10	0.02
% Effect: Hispanic	0.87	0.50	0.19	1.31
% Effect: White	0.09	-0.07	-0.04	0.17
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends				
Fully-interact with race	X	X	X	X
MSAs only	X	X	X	X
BTB-adopting only				
2008 and later				

Note: Results from the estimation specified in Equation 2.2. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment is based on the first BTB implemented anywhere in the MSA, not just in central cities. Column 1 is similar to our preferred specification (Column 3 of Table 2.5) and uses the fraction of the year (in months) BTB is in effect. In Column 2, MSAs are treated if a BTB policy is implemented as of January 15th of that year. In Column 3, MSAs are treated if a BTB policy is in place for half the year (as of July 2nd). In Column 4, MSAs are treated if a BTB policy is implemented as of December 15th of that year. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: The Effect of BTB on Labor Force Participation and Log Annual Earnings

	(1)	(2)	(3)
Panel A: Labor Force Participation			
BTB x Black	-0.0001	0.0001	-0.0084
	(0.0062)	(0.0080)	(0.0088)
BTB x Hispanic	0.0075	0.0036	0.0071
	(0.0077)	(0.0103)	(0.0084)
BTB x White	0.0012	-0.0040	-0.0002
	(0.0029)	(0.0038)	(0.0032)
N	599,430	363,335	388,637
R^2	0.1312	0.1290	0.1406
Pre-BTB Mean: Black	0.7065	0.7065	0.6833
Pre-BTB Mean: Hispanic	0.8233	0.8233	0.8210
Pre-BTB Mean: White	0.8813	0.8813	0.8739
% Effect: Black	-0.01	0.01	-1.23
% Effect: Hispanic	0.91	0.44	0.86
% Effect: White	0.14	-0.46	-0.02
Panel B: Log Annual Earnings	(1)	(2)	(3)
BTB x Black	-0.0069	0.0074	0.0284
	(0.0259)	(0.0381)	(0.0320)
BTB x Hispanic	0.0174	0.0541*	-0.0062
	(0.0212)	(0.0275)	(0.0220)
BTB x White	-0.0223*	-0.0045	-0.0198
	(0.0111)	(0.0120)	(0.0130)
N	480,558	289,945	302,846
R^2	0.1160	0.1157	0.1153
Pre-BTB Mean: Black	9.7256	9.7256	9.6848
Pre-BTB Mean: Hispanic	10.0472	10.0472	10.0385
Pre-BTB Mean: White	10.1778	10.1778	10.1333
MSA FE	X	X	X
Year-Division FE	X	X	X
Demographics	X	X	X
MSA linear trends			
Fully-interact with race	X	X	X
MSAs only	X	X	X
BTB-adopting only		X	
2008 and later			X

Note: Results from the estimation specified in Equation 2.2. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to black, Hispanic, and white men ages 25-34 with no college degree (two-year or four-year) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. The wording of survey questions about employment were changed starting in 2008. Column 3 omits all MSAs that implemented Ban the Box prior to 2009. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Ban the Box Laws by Type of Covered Employment and Legal Jurisdiction

(1) MSA	(2) Jurisdiction	(3) Public Date	(4) Contract Date	(5) Private Date
Akron, OH	Summit Co., OH	Sep. 1, 2012		
	Akron, OH	Oct. 29, 2013		
Ann Arbor, MI	Ann Arbor, MI	May 5, 2014		
Atlanta-Sandy Springs-Roswell, GA	Atlanta, GA	Jan. 1, 2013		
	Fulton Co., GA	Jul. 16, 2014		
Atlantic City-Hammonton, NJ	Atlantic City, NJ	Dec. 23, 2011	Dec. 23, 2011	
Austin-Round Rock, TX	Travis Co., TX	Apr. 15, 2008		
	Austin, TX	Oct. 16, 2008		
Baltimore-Columbia-Towson, MD	Baltimore, MD	Dec. 1, 2007	Aug. 13, 2014	Aug. 13, 2014
Boston-Cambridge-Newton, MA-NH	Boston, MA	Jul. 1, 2006	Jul. 1, 2006	
	Cambridge, MA	May 1, 2007	Jan. 28, 2008	
Bridgeport-Stamford-Norwalk, CT	Bridgeport, CT	Oct. 5, 2009		
Buffalo-Cheektowaga-Niagara Falls, NY	Buffalo, NY	Jun. 11, 2013	Jun. 11, 2013	Jun. 11, 2013
Canton-Massillon, OH	Stark Co., OH	May 1, 2013		
	Canton, OH	May 15, 2013		
	Massillon, OH	Jan. 3, 2014		
	Alliance, OH†	Dec. 1, 2014		
Charlotte-Concord-Gastonia, NC-SC	Charlotte, NC	Feb. 28, 2014		
Charlottesville, VA	Charlottesville, VA	Mar. 1, 2014		
Chattanooga, TN-GA	Hamilton Co., TN	Jan. 1, 2012		
Chicago-Naperville-Elgin, IL-IN-WI	Chicago, IL	Jun. 6, 2007	Nov. 5, 2014	Nov. 5, 2014
Cincinnati, OH-KY-IN	Cincinnati, OH	Aug. 1, 2010		
	Hamilton Co., OH	Mar. 1, 2012		
Cleveland-Elyria, OH	Cleveland, OH	Sep. 26, 2011		
	Cuyahoga Co., OH	Sep. 30, 2012		
Columbia, MO	Columbia, MO	Dec. 1, 2014	Dec. 1, 2014	Dec. 1, 2014
Columbus, OH	Franklin Co., OH	Jun. 19, 2012		
Detroit-Warren-Dearborn, MI	Detroit, MI	Sep. 13, 2010	Feb. 1, 2012	
Durham-Chapel Hill, NC	Durham, NC	Feb. 1, 2011		
	Carrboro, NC†	Oct. 16, 2012		
	Durham Co., NC	Oct. 1, 2012		
Fayetteville, NC	Cumberland Co., NC	Sept. 6, 2011		
	Spring Lake, NC†	Jun. 25, 2012		

Flint, MI	Genesee Co., MI	Jun. 1, 2014		
Hartford-West Hartford-East Hartford, CT	Hartford, CT	Aug. 9, 2009	Aug. 9, 2009	
Indianapolis-Carmel-Anderson, IN	Indianapolis, IN	Jun. 5, 2014	Jun. 5, 2014	
Jacksonville, FL	Jacksonville, FL	Nov. 10, 2008	Nov. 10, 2008	
Kalamazoo-Portage, MI	Kalamazoo, MI	Jan. 1, 2010		
Kansas City, MO-KS	Kansas City, MO	Apr. 4, 2013		
	Kansas City, KS	Nov. 6, 2014		
	Wyandotte Co., KS	Nov. 6, 2014		
Kingston, NY	Woodstock, NY[†]	Nov. 18, 2014		
Lancaster, PA	Lancaster, PA	Oct. 1, 2014		
Lansing-East Lansing, MI	East Lansing, MI	Apr. 15, 2014		
Los Angeles-Long Beach-Anaheim, CA	Compton, CA[†]	Jul. 1, 2011		
	Carson City, CA[†]	Mar. 6, 2012		
	Pasadena, CA[†]	Jul. 1, 2013		
Louisville/Jefferson Co., KY-IN	Louisville, KY	Mar. 25, 2014	Mar. 25, 2014	
Madison, WI	Dane Co., WI	Feb. 1, 2014		
Memphis, TN-MS-AR	Memphis, TN	Jul. 9, 2010		
Miami-Fort Lauderdale-West Palm Beach, FL	Pompano Beach, FL[†]	Dec. 1, 2014		
Milwaukee-Waukesha-West Allis, WI	Milwaukee, WI	Oct. 7, 2011		
Minneapolis-St. Paul-Bloomington, MN-WI	Minneapolis, MN	Dec. 1, 2006		
	St. Paul, MN	Dec. 5, 2006		
Muskegon, MI	Muskegon, MI	Jan. 12, 2012		
New Haven-Milford, CT	New Haven, CT	Apr. 2009[*]	Apr. 2009[*]	
New Orleans-Metairie, LA	New Orleans, LA	Jan. 10, 2014		
New York City-Newark- Jersey City, NY-NJ-PA	New York City, NY	Oct. 3, 2011	Oct. 3, 2011	
	Yonkers, NY	Nov. 1, 2014		
	Newark, NJ	Nov. 18, 2012	Nov. 18, 2012	Nov. 18, 2012
Norwich-New London, CT	Norwich, CT	Dec. 1, 2008		
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	Philadelphia, PA	Jun. 29, 2011	Jun. 29, 2011	Jun. 29, 2011
	Wilmington, DE	Dec. 10, 2012		
	New Castle Co., DE	Jan. 28, 2014		

Pittsburgh, PA	Pittsburgh, PA Allegheny Co., PA	Dec. 31, 2012 Nov. 24, 2014	Dec. 31, 2012	
Portland-Vancouver-Hillsboro, OR-WA	Multnomah Co., OR Portland, OR	Oct. 10, 2007 Jul. 9, 2014		
Providence-Warwick, RI-MA	Providence, RI	Apr. 1, 2009		
Richmond, VA	Richmond, VA Petersburg, VA[†]	Mar. 25, 2013 Sept. 3, 2013		
Rochester, NY	Rochester, NY	May 20, 2014	May 20, 2014	May 20, 2014
San Francisco-Oakland-Hayward, CA	East Palo Alto, CA[†] San Francisco, CA Oakland, CA Alameda Co., CA Berkeley, CA[†] Richmond, CA[†]	Jan. 1, 2005 Oct. 5, 2005 Jan. 1, 2007 Mar. 1, 2007 Oct. 1, 2008 Nov. 22, 2011	 Apr. 4, 2014 Jul. 30, 2013	 Apr. 4, 2014
San Jose-Sunnyvale-Santa Clara, CA	Santa Clara, CA	May 1, 2012		
Seattle-Tacoma-Bellevue, WA	Seattle, WA Pierce Co., WA	Apr. 24, 2009 Jan. 1, 2012	Nov. 1, 2013	Nov. 1, 2013
St. Louis, MO-IL	St. Louis, MO	Oct. 1, 2014		
Tampa-St. Petersburg-Clearwater, FL	Tampa, FL	Jan. 14, 2013		
Toledo, OH	Lucas Co., OH	Oct. 29, 2013		
Virginia Beach-Norfolk-Newport News, VA-NC	Newport News, VA Portsmouth, VA[†] Norfolk, VA Virginia Beach, VA	Oct. 1, 2012 Apr. 1, 2013 Jul. 23, 2013 Nov. 1, 2013		
Washington-Arlington-Alexandria, DC-VA-MD-WV	Washington, DC Alexandria, VA Arlington Co., VA Fredericksburg, VA[†] Prince George's Co., MD	Jan. 1, 2011 Mar. 19, 2014 Nov. 3, 2014 Jul. 2014[*] Dec. 4, 2014	Dec. 17, 2014	Dec. 17, 2014
Worcester, MA-CT	Worcester, MA	Sep. 1, 2009	Sep. 1, 2009	
Youngstown-Warren-Boardman, OH-PA	Youngstown, OH	Mar. 19, 2014		
state of California	California	Jun. 25, 2010		
state of Colorado	Colorado	Aug. 8, 2012		
state of Connecticut	Connecticut	Oct. 1, 2010		
state of Delaware	Delaware	May 8, 2014		
state of Hawaii	Hawaii	Jan. 1, 1998	Jan. 1, 1998	Jan. 1, 1998
state of Illinois	Illinois	Jan. 1, 2014	Jul. 19, 2014	Jul. 19, 2014
state of Maryland	Maryland	Oct. 1, 2013		
state of Massachusetts	Massachusetts	Aug. 6, 2010	Aug. 6, 2010	Aug. 6, 2010
state of Minnesota	Minnesota	Jan. 1, 2009	Jan. 1, 2009	May 13, 2013

state of Nebraska	Nebraska	Apr. 16, 2014		
state of New Mexico	New Mexico	Mar. 8, 2010		
state of Rhode Island	Rhode Island	Jul. 15, 2013	Jul. 15, 2013	Jul. 15, 2013

Note: Data are from Table 1 of Doleac and Hansen (2020), Avery and Lu (2020), local government websites, law firm websites, and news articles. †indicates the legal jurisdiction did not cover a central city in the corresponding MSA, meaning we do not use this law to define treatment in our primary specification. Columns 3, 4, and 5 denote the effective dates of BTB laws covering public-sector employers, government contractors, and private-sector employers, respectively, except where otherwise noted. *New Haven, CT's law was enacted on February 17, 2009 but we could not find an effective date. We therefore assumed that it took effect one month later (March 17, 2009), which would mean by our definition of treatment, the first effective month was April 2009. Fredericksburg, VA's law was enacted on June 5, 2014 but we could not find an effective date. We therefore assumed that it took effect one month later (July 5, 2014), which would mean by our definition of treatment, the first effective month was July 2014. Danville, VA implemented a public BTB on July 1, 2014 but Danville was a micropolitan statistical area according to the 2013 OMB MSA delineation file.

Table B.5: MSA-State Units Included in Within-MSA Sample

(1) MSA	(2) State	(3) 1st BTB Jurisdiction	(4) Sample Years
Boston-Cambridge-Newton,	Massachusetts	Boston, MA	2005-2014
MA-NH	New Hampshire	–	2005-2014
Charlotte-Concord-Gastonia,	North Carolina	Charlotte, NC	2005-2014
NC-SC	South Carolina	–	2005-2014
Chattanooga, TN-GA	Tennessee	Hamilton County, TN	2005-2014
	Georgia	–	2005-2014
Chicago-Naperville-Elgin,	Illinois	Chicago, IL	2005-2014
IL-IN-WI	Indiana	–	2005-2014
	Wisconsin	–	2005-2014
Cincinnati, OH-KY-IN†	Ohio	Cincinnati, OH	2005-2014
	Kentucky	–	2005-2014
Kansas City, MO-KS	Missouri	Kansas City, MO	2005-2014
	Kansas	Kansas City, KS*	2005-2014
Louisville/Jefferson County,	Kentucky	Louisville, KY	2005-2014
KY-IN	Indiana	–	2005-2014
Memphis, TN-MS-AR	Tennessee	Memphis, TN	2005-2014
	Mississippi	–	2005-2014
	Arkansas	–	2005-2011
New York-Newark-Jersey City,	New York	New York City, NY	2005-2012
NY-NJ-PA†	New Jersey	Newark, NJ	2005-2012
Omaha-Council Bluffs, NE-IA	Nebraska	Nebraska	2005-2014
	Iowa	–	2005-2014
Philadelphia-Camden-	Pennsylvania	Philadelphia, PA	2005-2014
Wilmington,	New Jersey	–	2005-2014
PA-NJ-DE-MD	Delaware	Wilmington, DE	2005-2014
	Maryland	Maryland	2005-2014
Portland-Vancouver-Hillsboro,	Oregon	Multnomah County, OR	2005-2014
OR-WA	Washington	–	2005-2014
Providence-Warwick,	Rhode Island	Providence, RI	2005-2010
RI-MA	Massachusetts	Massachusetts	2005-2010
St. Louis, MO-IL	Missouri	St. Louis, MO	2005-2014
	Illinois	Illinois	2005-2014
Salisbury, MD-DE	Maryland	Maryland	2005-2014
	Delaware	Delaware	2005-2014
Washington-Arlington-	District of Columbia	Washington, DC	2005-2014
Alexandria,	Virginia	Alexandria, VA	2005-2014
DC-VA-MD-WV†	Maryland	Maryland	2005-2014
Worcester, MA-CT	Massachusetts	Worcester	2005-2010
	Connecticut	Connecticut	2005-2010
Youngstown-Warren-	Ohio	Youngstown, OH	2005-2014
Boardman, OH-PA	Pennsylvania	–	2005-2014

Note: Data from the 2005-2014 waves of the ACS. N = 127,067. We drop the Minneapolis MSA because the WI portion is only covered in 2012-2014, which means we don't have an untreated comparison unit in the pre-treatment time periods. †The IN portion of the Cincinnati MSA, PA portion of the New York City MSA, and WV portion of the Washington MSA are not covered in our ACS sample. *Kansas City, KS and Wyandotte County, KS (which covers Kansas City, KS) implemented a BTB law on the same day. Column 3 lists the first legal jurisdiction in the MSA-state unit that covers a central city and implemented a BTB policy. When the MSA-state unit does not contain a central city of the MSA, we consider the MSA-state unit treated when the state implements BTB. Column 4 lists the our sample years, which includes years when our demographic groups of interest are sampled in the ACS, and excludes years when all MSA-state units in the MSA are covered by a BTB policy for the full year (in effect on January 15th).

B.2 Coding Discrepancies in Doleac and Hansen (2020)

In the course of replicating Doleac and Hansen (2020), we realized that the ban-the-box treatment variable was sometimes incorrectly coded for some MSAs. We do not believe these errors were intentional and we have catalogued them in the interest of transparency. There are multiple MSAs on the list with large populations (e.g. Boston, New York City, Philadelphia, and Seattle), so it is not surprising that re-coding the treatment variable changes the results. We describe the errors separately for the ACS and the CPS because there were quite a few differences in affected MSAs between the two datasets.

B.2.1 ACS

In their ACS specifications, we found coding errors for 36 MSAs. They can be classified into four (sometimes-overlapping) types:

1. MSAs that span multiple states and have different treatment statuses for each MSA-state unit
2. MSAs that implemented a law on January 1 but were not coded as treated until the next year
3. MSAs that were coded as treated using a later law instead of the first law (e.g. a state law instead of an earlier city law)
4. MSAs that were otherwise incorrectly coded as treated or untreated

For the first type, the following MSAs were affected:

- Boston-Cambridge-Newton, MA-NH
- New York-Newark-New Jersey City, NY-NJ-PA
- Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Providence-Warwick, RI-MA
- Salisbury, MD-DE.

These metro areas were not coded as treated once a portion of them implemented BTB. Instead, the different state portions were separately coded as treated according to each state portion's policies (e.g. the New Hampshire portion of the Boston-Cambridge-Newton, MA-NH MSA was coded as untreated while the Massachusetts part was coded as treated).

The following MSAs implemented a law on January 1 but weren't coded as treated until the following year:

- Atlanta-Sandy Springs-Roswell, GA
- Bloomington, IL
- Champaign-Urbana, IL

- Chattanooga, TN-GA
- Decatur, IL
- non-MSA part of Illinois
- Kalamazoo-Portage, MI
- Kankanee, IL
- non-MSA part of Minnesota
- Rockford, IL
- St. Cloud, MN
- St. Louis, IL
- San Francisco-Oakland-Hayward, CA
- Springfield, IL
- Washington-Arlington-Alexandria, DC-MD-VA-WV

The following MSAs were coded as treated with a later law:

- Akron, OH
- Boston-Cambridge-Newton, MA-NH
- Bridgeport-Stamford-Norwalk, CT
- Cincinnati, OH-KY-IN
- Hartford-West Hartford-East Hartford, CT
- New Haven-Milford, CT
- Norwich-New London, CT
- Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Providence-Warwick, RI-MA
- Seattle-Tacoma-Bellevue, WA
- Virginia Beach-Norfolk-Newport News, VA-NC
- Worcester, MA-CT

The following MSAs were otherwise incorrectly coded as treated or untreated:

- New Jersey part of Allentown-Bethlehem-Easton, PA-NJ
- Atlantic City-Hammonton, NJ
- Columbus, OH
- non-MSA part of New Jersey
- New Jersey part of New York City-Newark-Jersey City, NY-NJ-PA
- Ocean City, NJ
- New Jersey part of Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Trenton, NJ
- Vineland-Bridgeton, NJ

For Columbus, Ohio, Franklin County implemented a BTB law on June 19, 2012, but that law was not used to assign treatment status to the Columbus MSA. For all of New Jersey, the state of New Jersey is coded as having a BTB policy since December 2006, while New Jersey does not have a statewide BTB policy.

B.2.2 CPS

In their CPS specifications, we found coding errors for 19 MSAs. They can be classified into three (sometimes-overlapping) types:

1. MSAs that span multiple states and have different treatment statuses for each MSA-state unit
2. MSAs that were coded as treated using a later law instead of the first law
3. MSAs that were otherwise incorrectly coded as treated or untreated

The following MSAs had MSA-state unit mismatches:

- Boston-Cambridge-Newton, MA-NH
- Davenport-Moline-Rock Island, IA-IL
- Hagerstown-Martinsburg, MD-WV
- New York City-Newark-Jersey City, NY-NJ-PA
- Omaha-Council Bluffs, NE-IA
- Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- St. Louis, MO-IL

The following MSAs were coded as treated with a later law:

- Akron, OH
- Austin-Round Rock, TX
- Boston-Cambridge-Newton, MA-NH
- Cincinnati, OH-KY-IN
- Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Seattle-Tacoma-Bellevue, WA
- Virginia Beach-Norfolk-Newport News, VA-NC

The following MSAs were otherwise incorrectly coded as treated or untreated:

- New Jersey part of Allentown-Bethlehem-Easton, PA-NJ
- Atlantic City-Hammonton, NJ
- Columbus, OH
- non-MSA part of New Jersey
- New Jersey part of New York-Newark-Jersey City, NY-NJ-PA
- Ocean City, NJ

- New Jersey part of Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Trenton, NJ
- Vineland-Bridgeton, NJ

For Columbus, Ohio, Franklin County implemented a BTB law on June 19, 2012, but that law was not used to assign treatment status to the Columbus MSA. For all of New Jersey, the state of New Jersey is coded as having a BTB policy since December 2006, while New Jersey does not have a statewide BTB policy in place during the sample period.

B.2.3 Other Differences

We also found several MSAs where constituent legal jurisdictions had BTB policies with different effective dates than the effective dates in Table 1 of Doleac and Hansen (2020). The following MSAs were covered by city, county, or state BTB policies for which we found different effective dates than those listed in Table 1 of Doleac and Hansen (2020). We have listed the effective dates we found for each type of law, where they differed:

- Baltimore-Columbia-Towson, MD
  - Baltimore's contract and private BTB laws effective August 13, 2014
- Boston-Cambridge-Newton, MA-NH
  - Boston's contract BTB law effective July 1, 2006
- Danville, VA
  - Danville's public BTB law effective July 1, 2014
  - Danville is a micropolitan statistical area in the 2013 OMB delineation file so it is not included in our sample
- Detroit-Warren-Dearborn, MI
  - Detroit's contract law effective February 1, 2012
- Hartford-West Hartford-East Hartford, CT
  - Hartford's public and contract BTB laws effective August 9, 2009
- Indianapolis-Carmel-Anderson, IN
  - Indianapolis's public and contract BTB laws effective June 5, 2014
- Jacksonville, FL
  - Jacksonville's contract BTB law effective November 10, 2008
- Louisville/Jefferson County, KY-IN
  - Louisville's contract BTB law enacted March 13, 2014

- we could not find an effective date, but most BTB laws are not effective immediately, so we assumed the law took effect one month later (by our treatment definition, treatment would start in April 2014)

- New Haven-Milford, CT
  - New Haven's public and contract BTB laws enacted February 17, 2009
  - we could not find an effective date, but most BTB laws are not effective immediately, so we assumed the law took effect one month later (by our treatment definition, treatment would start in April 2009)
- New York City-Newark-Jersey City, NY-NJ-PA
  - Newark, NJ's public, contract, and private BTB laws effective November 18, 2012
- Pittsburgh, PA
  - Pittsburgh's contract BTB law effective December 31, 2012
- Seattle-Tacoma-Bellevue, WA
  - Seattle's contract and private BTB laws effective November 1, 2013
- Spokane-Spokane Valley, WA
  - The mayor of Spokane issued a directive on July 31, 2014 to draft a public-sector BTB policy
  - the public BTB policy was not effective until March 6, 2015, which makes the Spokane MSA untreated throughout the duration of our sample period (2005-2014)
- Washington-Arlington-Alexandria, DC-VA-MD-WV
  - Fredericksburg's public BTB law enacted June 5, 2014
  - we could not find an effective date, but most BTB laws are not effective immediately, so we assumed the law took effect one month later (by our treatment definition, treatment would start in July 2014)
- Worcester, MA-CT
  - Worcester's public and contract BTB laws effective September 1, 2009

Many of these laws cover the contract or private sector, so changing the effective dates would not affect the main results (because public-sector BTB laws are always the first to be implemented), but they would affect the public-contract-private robustness check. In the instances where we found different effective dates for public-sector BTB laws, the difference was usually by a month or two, which did not greatly affect the results.

APPENDIX C
APPENDIX FOR CHAPTER 3

C.1 Additional Tables

Table C.1: List of 100 Most Populous U.S. Cities, by Date of Lyft/Uber Entry

City	State	Month	Year	City	State	Month	Year
Oakland	CA	July	2010	San Francisco	CA	July	2010
San Jose	CA	July	2010	New York City	NY	May	2011
Seattle	WA	August	2011	Chicago	IL	September	2011
Boston	MA	October	2011	Washington	DC	December	2011
Long Beach	CA	March	2012	Los Angeles	CA	March	2012
Philadelphia	PA	June	2012	San Diego	CA	June	2012
Fremont	CA	July	2012	Atlanta	GA	August	2012
Arlington	TX	September	2012	Aurora	CO	September	2012
Dallas	TX	September	2012	Denver	CO	September	2012
Fort Worth	TX	September	2012	Garland	TX	September	2012
Irving	TX	September	2012	Plano	TX	September	2012
Minneapolis	MN	October	2012	St. Paul	MN	October	2012
Chandler	AZ	November	2012	Gilbert	AZ	November	2012
Glendale	AZ	November	2012	Mesa	AZ	November	2012
Phoenix	AZ	November	2012	Scottsdale	AZ	November	2012
Baltimore	MD	February	2013	Sacramento	CA	February	2013
Stockton	CA	February	2013	Detroit	MI	March	2013
Indianapolis	IN	June	2013	Honolulu	HI	August	2013
Anaheim	CA	September	2013	Charlotte	NC	September	2013
Chula Vista	CA	September	2013	Irvine	CA	September	2013
Santa Ana	CA	September	2013	Oklahoma City	OK	October	2013
Tucson	AZ	October	2013	Jersey City	NJ	November	2013
Columbus	OH	December	2013	Nashville	TN	December	2013
Jacksonville	FL	January	2014	Fresno	CA	February	2014
Houston	TX	February	2014	Milwaukee	WI	February	2014
Pittsburgh	PA	February	2014	Cincinnati	OH	March	2014
Madison	WI	March	2014	San Antonio	TX	March	2014
Tulsa	OK	March	2014	Albuquerque	NM	April	2014
Cleveland	OH	April	2014	Lincoln	NE	April	2014
Louisville	KY	April	2014	Memphis	TN	April	2014
Raleigh	NC	April	2014	St. Petersburg	FL	April	2014
Tampa	FL	April	2014	Chesapeake	VA	May	2014
Colorado Springs	CO	May	2014	Kansas City	MO	May	2014
Newark	NJ	May	2014	Norfolk	VA	May	2014
Omaha	NE	May	2014	Riverside	CA	May	2014
San Bernardino	CA	May	2014	Virginia Beach	VA	May	2014
Austin	TX	June	2014	Bakersfield	CA	June	2014
Corpus Christi	TX	June	2014	Durham	NC	June	2014

El Paso	TX	June	2014	Greensboro	NC	June	2014
Hialeah	FL	June	2014	Lexington	KY	June	2014
Lubbock	TX	June	2014	Miami	FL	June	2014
Orlando	FL	June	2014	Toledo	OH	June	2014
Winston-Salem	NC	June	2014	Baton Rouge	LA	July	2014
Wichita	KS	August	2014	Anchorage	AK	September	2014
New Orleans	LA	September	2014	Reno	NV	October	2014
St. Louis	MO	October	2014	Birmingham	AL	February	2015
Portland	OR	April	2015	Fort Wayne	IN	May	2015
Henderson	NV	September	2015	Las Vegas	NV	September	2015
North Las Vegas	NV	September	2015				

Note: Buffalo, NY and Laredo, TX did not have UberX before December 31, 2016. San Juan, Puerto Rico is excluded because there are no FARS data available.
Data source: Author's hand-collected data from Uber and Lyft's websites and news articles.

Table C.2: States that Test Blood Alcohol Concentration for at Least 80% of Deceased Drivers

State (1)	FIPS Code (2)
California	6
Colorado	8
Connecticut	9
Hawaii	15
Illinois	17
Maryland	24
Massachusetts	25
Montana	30
New Hampshire	33
New Jersey	34
North Dakota	38
Ohio	39
Pennsylvania	42
Rhode Island	44
Vermont	50
Virginia	51
Washington	53
West Virginia	54

Data source: Kim et al. (2016).

Table C.3: Cities by Public Transit Accessibility

High		Medium		Low	
City (1)	Rank (2)	City (3)	Rank (4)	City (5)	Rank (6)
San Francisco, CA	1	Houston, TX	34	Scottsdale, AZ	68
Boston,	2	Santa Ana, CA	35	Omaha, NE	69
Washington, D.C.	3	Durham, NC	36	Irvine, CA	70
Jersey City, NJ	4	Cincinnati, OH	37	Fresno, CA	71
New York City, NY	5	Long Beach, CA	38	Tulsa, OK	72
Chicago, IL	6	Pittsburgh, PA	39	St. Petersburg, FL	73
Seattle, WA	7	Buffalo, NY	40	Anaheim, CA	74
Minneapolis, MN	8	Raleigh, NC	41	Fort Worth, TX	75
Philadelphia, PA	9	Milwaukee, WI	42	Reno, NV	76
Oakland, CA	10	Fremont, CA	43	Toledo, OH	77
Albuquerque, NM	11	Las Vegas, NV	44	Plano, TX	78
Baltimore, MD	12	Columbus, OH	45	Colorado Springs, CO	79
Portland, OR	13	Madison, WI	46	Chandler, AZ	80
Denver, CO	14	Virginia Beach, VA	47	Aurora, CO	81
Los Angeles, CA	15	Norfolk, VA	48	Boise, ID	82
Newark, NJ	16	San Antonio, TX	49	Hialeah, FL	83
St. Louis, MO	17	Anchorage, AK	50	Garland, TX	84
Cleveland, OH	18	Sacramento, CA	51	Winston-Salem, NC	85
Austin, TX	19	Detroit, MI	52	Mesa, AZ	86
Miami, FL	20	Charlotte, NC	53	Indianapolis, IN	87
San Jose, CA	21	Corpus Christi, TX	54	Fort Wayne, IN	88
Atlanta, GA	22	Oklahoma City, OK	55	Chula Vista, CA	89
Kansas City, MO	23	Irving, TX	56	Stockton, CA	90
Lincoln, NE	24	Greensboro, NC	57	Baton Rouge, LA	91
Orlando, FL	25	Memphis, TN	58	Lubbock, TX	92
Phoenix, AZ	26	Bakersfield, CA	59	Chesapeake, VA	93
St. Paul, MN	27	San Bernardino, CA	60	North Las Vegas, NV	94
New Orleans, LA	28	El Paso, TX	61	Henderson, NV	95
Dallas, TX	29	Jacksonville, FL	62	Arlington, TX	96
Honolulu, HI	30	Lexington-Fayette, KY	63	Birmingham, AL	97
Nashville, TN	31	Louisville, KY	64	Glendale, AZ	98
San Diego, CA	32	Riverside, CA	65	Laredo, TX	99
Tucson, AZ	33	Wichita, KS	66	Gilbert, AZ	100
		Tampa, FL	67		

Note: Author's ranking of high, medium, and low public transit accessibility based on rankings conducted by WalletHub. Data source: McCann (2019).