Does Easier Alcohol Access Affect Road Safety? Insights from Texas Municipal Elections

Somdeepa Das*   Tong Li†

April 2026

Draft version. Please do not quote or circulate.

Abstract

We examine the impact of expanded local alcohol access on traffic safety, exploiting close local option elections in Texas as a source of quasi-random variation. More than 300 cities in Texas voted to legalize alcohol sales between 2003 and 2019, leading to a large increase in liquor licenses — driven almost entirely by off-premise retailers such as liquor stores and grocery stores — and a reallocation of alcohol sales from bars and restaurants toward retail outlets. Despite these sizable market changes, we find no meaningful increase in total crashes, DUI crashes, or fatal crashes, and no evidence of spillover effects on neighboring cities. We also show that on-premise licenses are positively associated with crash rates while off-premise licenses are not, consistent with the absence of a crash effect when legalization primarily expands retail access.

Keywords: Alcohol, Drunk driving, Road safety

JEL Codes: H75, I18, R41

*Economics Department, Daniels School of Business, Purdue University
†Economics Department, Daniels School of Business, Purdue University

1 Introduction

Alcohol-related traffic incidents are a major public health issue in the United States, consistently contributing to a significant number of road fatalities each year. In 2023 alone, nearly 30% of all traffic-related deaths were alcohol-impaired driving fatalities, amounting to over 12,000 lives lost.[1] There are large social and economic costs of these accidents, making it critical to understand the underlying factors that influence drinking behavior and traffic safety. While the link between alcohol consumption and traffic fatalities is well established, much less is known about how the availability of alcohol, including through local policy, affects these outcomes.

Understanding how alcohol availability affects traffic accidents and corresponding fatalities is central to evaluating existing alcohol regulations. On one hand, greater access to alcohol may increase sales and consumption, leading to more impaired driving and, consequently, more accidents. On the other hand, increased alcohol availability may reduce impaired driving incidents, as people do not have to travel longer distances to wet areas to consume alcohol (Gary et al., 2003). Moreover, growth in on-premise drinking establishments such as bars and restaurants can raise local drunk driving risks, as individuals drive to and from these establishments after drinking (Hahn et al., 2010). These competing mechanisms make it an open empirical question whether easing restrictions on alcohol sales increases, decreases, or has no effect on traffic crashes and fatalities. The answer may also depend critically on the *type* of access that is liberalized: off-premise retail outlets — where alcohol is purchased for home consumption — may have very different implications for impaired driving than bars and restaurants where alcohol is consumed on the premises.

The causal literature on alcohol and traffic safety has largely studied enforcement and deterrence policies — zero tolerance drunk driving laws, stricter BAC limits, and enhanced DUI penalties — which predominantly affect teenagers or intervene at the enforcement stage rather than at the point of alcohol access (Carpenter, 2004; Dee, 1999; Hansen, 2015; Eisen-

[1] National Highway Traffic Safety Administration, 2023

berg, 2003). Our paper studies a distinct margin: spatial variation in whether alcohol can legally be purchased at all. This is economically relevant: even today, hundreds of US jurisdictions, particularly across the South, maintain some form of alcohol restriction, and local access laws remain an active and contested policy margin. Exploiting local referenda in Texas, we ask whether lifting these restrictions affects traffic safety for the general adult population, a question the prior literature has not addressed with clean causal identification.

The closest antecedent to our paper is Baughman et al. (2001), who study the effects of lifting alcohol access restrictions in Texas on traffic crashes using county-level panel data. Their results suggest that beer and wine legalization may reduce expected accidents, while the marginal effect of legalizing hard liquor points toward higher crash risk, but the underlying estimates are imprecise, and the identification strategy relies on county fixed effects and time trends without directly addressing election endogeneity. Anderson et al. (2018) study the lifting of alcohol prohibition in Kansas counties, focusing on violent crime. In their setting, the policy primarily expands on-premise outlets, and they find that an increase in bars and restaurants raises violent crime, pointing to on-premise establishments as a driver of risky behavior. Our paper provides a complementary result: when liberalization instead shifts off-premise retail, as we document in Texas, there is no corresponding increase in traffic crashes.

Our paper makes four contributions. We first provide city-level causal evidence on alcohol liberalization and traffic safety, working at the unit where the policy actually changes, rather than the county level where treatment is diluted. Second, we address the endogeneity of election timing using a regression discontinuity on close election outcomes, directly isolating quasi-random variation in legalization status.[2] Third, we document consistent short- and long-run effects using multiple identification strategies, including a stacked dynamic RD well-suited to settings with repeated elections and a staggered difference-in-differences estimator.

[2]Elections are endogenous: places seeking liberalization may differ systematically in ways that also predict driving risk. For example, rapidly growing suburban areas may be more likely to both pursue alcohol liberalization and attract younger, higher-risk driver populations.

Finally, we show that the type of access is the key mechanism: legalization primarily expands off-premise retail outlets and sales, and it is this channel that explains the absence of traffic safety consequences.

Texas provides an ideal setting to examine this question because local jurisdictions regularly hold referenda — known as local option elections — on whether to permit alcohol sales. These elections create plausibly exogenous variation in alcohol availability at the city level, enabling causal identification of the effects of local alcohol liberalization on public safety. To identify causal effects, we combine three complementary empirical approaches. First, we employ a standard regression discontinuity (RD) design that leverages close-election outcomes, comparing cities that narrowly passed versus narrowly failed, to identify the local causal effect of legalizing alcohol sales. Second, to estimate longer-run effects we employ a dynamic RD following Cellini et al. (2010), using the approach of Biasi et al. (2025) to handle repeated elections with clean control groups. Finally, we use the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021) as a benchmark. Together, these designs allow us to estimate both immediate and persistent effects of increased alcohol availability and to verify robustness across specifications.

Our results indicate that cities that lifted restrictions on alcohol sales experience a large and persistent increase in alcohol availability. In the short run, the number of establishments holding state-issued liquor licenses in a city increases by roughly 30% after legalization. This increase is driven almost entirely by off-premise outlets — liquor stores, grocery stores, and convenience stores selling alcohol for home consumption — which increase by roughly 48%, while on-premise establishments such as bars and restaurants show a small and statistically insignificant change.

Sales data confirm this reallocation of alcohol access and consumption: per-capita off-premise alcohol sales increase by roughly 40%, whereas on-premise sales decline by roughly 20%, pointing to a substantial shift toward retail purchases for home consumption. Despite these sizable changes in alcohol availability and consumption patterns, we find no meaningful increase in total traffic crashes, DUI crashes, or fatal crashes. This pattern is consistent with the nature of the first stage: since legalization primarily expands off-premise retail access, and off-premise outlets are less directly tied to drinking before driving than bars and restaurants, there is no reason to expect a large crash effect. We also find no evidence of spatial spillovers — cities near newly legalized neighbors do not experience increases in crashes — ruling out the possibility that effects are displaced across city borders rather than absent altogether.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 presents results. Section 6 concludes.

2 Background

Local option laws on alcohol have created substantial geographic variation in alcohol access regulations across the United States, producing a patchwork of "wet" and "dry" communities. This pattern is especially pronounced in the South and parts of the Midwest, where historical temperance movements, driven largely by Evangelical Protestant groups, shaped local attitudes toward alcohol and continue to influence policy even today. These religious and cultural factors help explain why many dry areas have persisted for decades after the repeal of Prohibition in 1933.

States differ in how they implement and modify alcohol regulations. Several (including Texas, Kentucky, Louisiana, and North Carolina) require local elections to impose or lift restrictions on alcohol sales, giving communities direct control over policy changes. Among these, Texas stands out because the Texas Alcoholic Beverages Commission maintains comprehensive public records of all local option elections. This makes Texas an especially valuable setting for studying how local shifts in alcohol availability affect economic and public safety outcomes.

A dry-to-wet transition, in which a previously dry jurisdiction legalizes alcohol sales, can influence a range of local outcomes, particularly traffic safety. Theoretically, such transitions can have opposing effects: increased access may raise alcohol consumption and impaired driving, but it may also reduce accidents if individuals no longer need to travel long distances to purchase alcohol. These competing mechanisms underscore the need for credible evidence on how local changes in alcohol policy affect traffic fatalities and related outcomes.

2.1 Alcohol Consumption and Traffic Accidents

Alcohol consumption has long been linked to public safety concerns, particularly through its role in impaired driving—one of the leading causes of traffic fatalities in the United States. According to the Texas Motor Vehicle Traffic Crash Facts (2024), 1,053 people were killed in crashes involving a driver under the influence of alcohol, accounting for roughly one-quarter of all traffic deaths in the state.[3]

Numerous studies discuss public health and safety concerns arising from alcohol consumption, exploring whether increased access to alcohol affects crime rates (Anderson et al., 2018; Bodenhorn, 2016), mortality (Norström et al., 2005), and highway accidents (Baughman et al., 2001), (Ramstedt, 2008). As a result, more concerns have emerged about whether policies related to alcohol access might impact public safety (Österberg, 2011). This raises a key question: does increased access to alcohol actually influence alcohol consumption, thereby affecting public safety outcomes such as drunk driving? For example, Castellari et al. (2017) found purchases of beer are higher on SNAP receipt days, but they also found that alcohol-related accidents with fatalities were substantially lower on those days (Cotti et al., 2016). The direct impact of alcohol access on alcohol consumption remains uncertain, making its potential indirect effects on issues such as drunk driving even less clear.

Alcohol access in the United States is shaped by a wide range of regulatory mechanisms that vary across states and localities. Prominent examples include the minimum legal drinking age (MLDA), Sunday sales bans, and local option laws. Each creates quasi-experimental

[3]Texas Department of Transportation, Texas Motor Vehicle Traffic Crash Facts, 2024.

variation that allows researchers to study the causal effects of alcohol regulation on public health and safety outcomes. Studies on the MLDA exploit sharp age discontinuities to estimate effects on traffic safety, health, and crime (Chalfin et al., 2023; Carpenter and Dobkin, 2015; Conover and Scrimgeour, 2013).[4] Similarly, studies on Sunday alcohol sales bans—such as Yörük and Lee (2018) and Bernheim et al. (2016)[5] – show that relaxing restrictions on alcohol availability can alter behavior in distinct ways. Yörük and Lee (2018) document a 16%–23% increase in Sunday violent and property crimes following legalization, though overall weekly crime remains unaffected. Bernheim et al. (2016), by contrast, find that consumers drink more when on-premise Sunday sales are extended, but not when off-premise hours are extended, suggesting channel-specific effects and limited self-control mechanisms. Their research offers empirical evidence linking changes in alcohol availability to behavioral and social responses, a mechanism central to our analysis of local alcohol policy effects.

Beyond age-based and temporal restrictions, local option laws give communities direct control over whether to permit alcohol sales. These laws create spatial variation in access that has been used to study the effects of liberalizing alcohol markets. A key study by Baughman et al. (2001) exploits dry-to-wet transitions in Texas using county-level panel data. Their results suggest that beer and wine legalization may reduce expected accidents, while the marginal effect of legalizing hard liquor points toward higher crash risk — but the estimates are imprecise and the identification relies on county fixed effects and time trends without directly addressing election endogeneity.

We build on this work by using a regression discontinuity (RD) design that compares cities narrowly voting for versus against legalization in local option elections with polynomials of vote margins, allowing us to address the potential endogenity. This approach leverages quasi-

[4]Also see Crost and Rees (2013), Yörük and Yörük (2011), Fertig and Watson (2009), and DiNardo and Lemieux (2001), who consistently find that minimum legal drinking age laws shape individual behavior and public health outcomes, curbing alcohol use among young people while inducing offsetting or unintended effects in related domains like marijuana use.

[5]Other empirical studies Carpenter and Eisenberg (2009), Heaton (2012), and Stehr (2010) show that easing Sunday sales bans tends to raise day-specific alcohol consumption and related harms such as crime or crashes, though overall drinking levels often remain unchanged.

random variation in election outcomes around the 50 percent threshold to identify the causal effects of increased alcohol availability on traffic safety. By focusing on close elections, we isolate policy-driven changes in access from pre-existing differences across cities.

A key contribution of our analysis is distinguishing between off-premise and on-premise access to alcohol, which may affect behavior in qualitatively different ways. Prior research, such as Anderson et al. (2018), shows that growth in on-premise establishments (bars and restaurants) can increase violent crime, while expansion of off-premise outlets such as liquor and grocery stores is often unrelated to such outcomes. In our setting, local legalization primarily expands off-premise outlets, with little change in bars and restaurants. This distinction allows us to identify how the type of alcohol access, not just its availability, influences traffic safety.

Finally, local option elections differ in the types of beverages they legalize – some permit only beer and wine, while others extend to liquor and mixed beverages with higher alcohol content. This heterogeneity enables us to examine whether expanding access to stronger alcoholic beverages leads to different effects on road safety. Our focus on smaller cities and municipalities further allows us to capture localized impacts that may differ from patterns observed in larger urban areas, providing a more precise estimate of how local changes in alcohol policy shape traffic outcomes.

2.2 Elections on Local Options in Texas

Texas utilizes local option laws, allowing municipalities to determine their own alcohol regulations through public referendums. These elections provide a valuable source of quasi-experimental variation for studying the effects of changes in alcohol availability. Over the past two decades, the state has experienced a pronounced shift from dry to wet jurisdictions, reflecting a broader national trend toward the liberalization of alcohol laws. In 2003, Texas had 35 completely wet counties and 51 completely dry ones (others are partially wet, which means permitting alcohol sales in only specific jurisdictions within the county); by 2019, 59 counties were entirely wet, while only five remained dry.

The prevalence of dry areas in Texas can be traced to the period following the repeal of Prohibition in 1933, when many communities – particularly those with strong Evangelical Protestant traditions – chose to maintain restrictions on alcohol sales. These religious and moral attitudes were central to the temperance movement and continue to shape local voting behavior. In recent decades, however, declining religious opposition and growing economic incentives have encouraged more municipalities to transition toward allowing alcohol sales. Alcohol sales generate both direct tax revenues and spillover economic activity, making legalization increasingly attractive to local governments.

For this study, we use election records from the Texas Alcoholic Beverages Commission (TABC) covering the period from 2003 to 2019. Over this time, there were 588 local option elections across 466 Texas cities, of which 326 transitioned their alcohol policy status (see Table 1). In addition, Figure 1 shows the distribution of these elections over time, illustrating the steady transition from dry to wet communities in this period.

Table 1: Summary of Local Option Elections During 2003–2019

	Total	Passed	Failed
On-premise	275	253	22
Off-premise	483	429	54
All	588	527	61

Notes: Counts of city-level local option elections. "On-premise" denotes permits for bars/restaurants; "Off-premise" denotes permits for retailers/grocery stores. Some elections bundle on- and off-premise issues (e.g., "legal sale of beer and wine"). Consequently, the sum across categories is larger than the total number of elections.

Local option elections in Texas follow a uniform legal procedure. A written application must be filed by at least ten qualified voters from the relevant jurisdiction—county, justice precinct, or municipality. Once approved, the county clerk issues a petition that must collect signatures from at least 25% of qualified voters in areas with fewer than 10,000 residents, or 20% in larger areas. Upon verification, the commissioners court orders the election, which

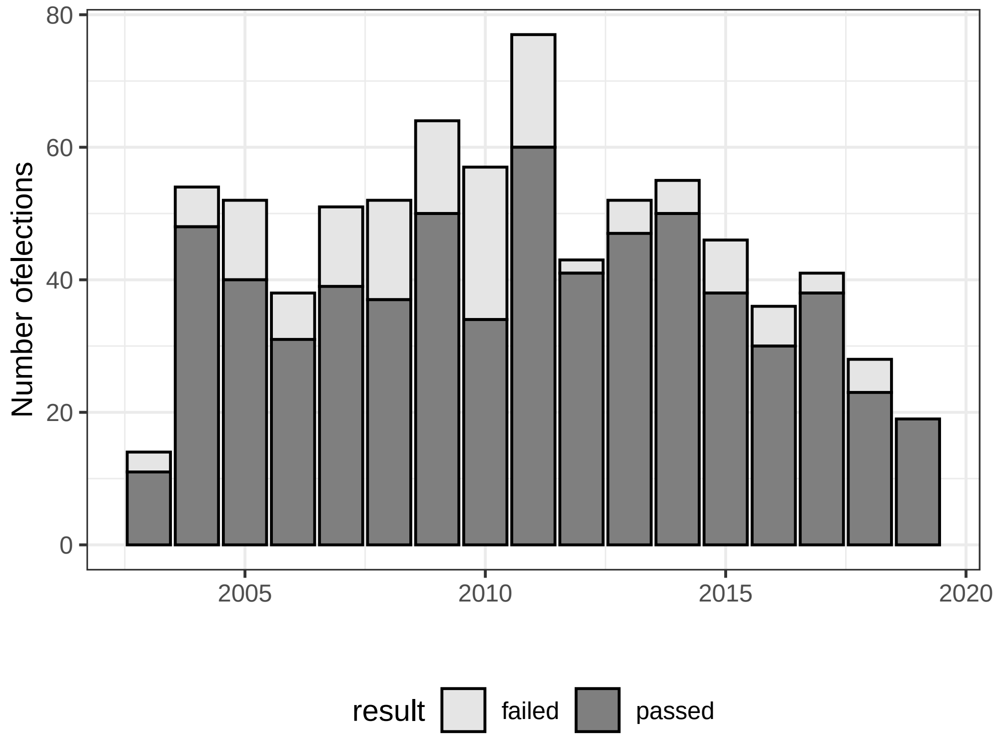


Figure 1: Local Options Elections, 2003-2019

Note: The figure plots the number of city-level local option elections held in Texas from 2003 to 2019, separately showing elections that passed and those that failed.

is held on uniform election dates prescribed by Texas law. If a majority votes in favor, the specified type of alcohol sale becomes legal; otherwise, it remains prohibited. These procedural rules ensure that the timing and outcomes of elections are citizen-driven and institutionally standardized across localities.

An important feature of Texas's system is that elections can lift restrictions on specific categories of alcohol sales. Off-premise licenses apply to liquor stores, grocery stores, and convenience stores that sell alcohol for consumption elsewhere, whereas on-premise licenses apply to bars and restaurants where alcohol is consumed at the point of sale. Elections concerning off-premise sales are slightly more common, and our results later show that these drive most of the observed increase in alcohol outlets following legalization. One likely reason is that existing grocery and convenience stores can more easily begin selling alcohol once restrictions are removed, while opening a new bar or restaurant often requires larger upfront investment and sufficient local demand.

3 Data

3.1 Local Option Elections, Liquor Establishments, and Alcohol Consumption

Our data on local option election records from 2003 to 2019 in Texas cities comes from the Texas Alcoholic Beverages Commission (TABC). To measure alcohol access, we focus on the direct effect of passing these elections, i.e. dry-to-wet transitions—on local alcohol businesses. These elections may occur at the county, city, or precinct level. We assign treatment at the city level. Over 80 percent of elections during our study period take place at the city level, and most of the Texas population resides in incorporated cities, so this captures the primary margin of variation.

Constructing city-year wet/dry status requires combining two sources. For cities that appear in the election records, the election history directly reveals their pre-election status: each election record indicates the city's wet/dry classification immediately before the vote, which can range from completely dry to partially wet (for example, a city that already allows beer and wine sales but is voting on whether to permit hard liquor). We assign city-year status based on this recorded pre-election classification, so cities are correctly coded as dry or partially wet in the years leading up to each election.[6] We assign wet status beginning in the calendar year of the first successful election, and cities remain treated thereafter.[7] For cities that hold multiple elections during the study period, we treat the first successful election as the treatment date. Prohibitory elections, in which a previously wet city votes to become dry again, are rare and excluded from the main analysis.

[6] Tex. Alco. Bev. Code § 251.73(1), .

[7] Liquor licenses can typically be obtained within 45–60 days of a successful election. Because our outcomes are measured annually and elections occur in May or November, we assign treatment in the calendar year of the election.

A complication arises because Texas jurisdictions overlap: a city sits within a county, and county- or precinct-level elections can change alcohol status independently of city-level votes. The Texas Alcoholic Beverage Code resolves this hierarchy explicitly — a city-level election overrides any conflicting county- or precinct-level outcome, so a city that holds its own election is governed solely by that result.[8] For cities that never hold their own election, however, county-level status can apply. This matters for how we treat cities with no election history in our data.

Cities that never hold an election during the study period are ambiguous: they could be cities that were already wet throughout the period (either through their own prior elections or through county-level status), or cities that remained completely dry. To distinguish between these two groups, we use the TABC's city-level wet/dry classification for 2019. Cities classified as wet in 2019 with no election history are coded as always-wet — whether because the county was wet or because of elections predating our sample. Cities classified as dry in 2019 with no elections are coded as always-dry. [9]

Because local option elections legalize alcohol sales rather than directly affecting consumption, the most immediate impact should be observed in the number and type of liquor establishments operating in these cities. To capture this, we collect administrative data on alcohol licenses from the TABC. This dataset allows us to distinguish between two types of licenses: off-premise and on-premise. Off-premise licenses cover establishments such as liquor stores, grocery stores, and convenience stores that sell alcohol for consumption elsewhere. On-premise licenses apply to bars and restaurants where alcohol is consumed on-site. Because liquor licenses in Texas must be renewed every two years, we can track the number of active licenses in each city-year.

To complement these supply-side measures, we incorporate two sources of data on alcohol sales to study changes in consumption. First, we obtain the universe of annual alcohol sales

[8] Texas Alcoholic Beverage Code 251.73(1), .

[9] Partially wet cities can allow sales of a particular category of alcohol (beer and wine vs. liquor) or restrict where it can be sold (off- vs. on-premise).

for bars and restaurants at the city-level from TABC. Second, we draw on the NielsenIQ Retail Scanner Data [10], which record off-premise alcohol sales from a large sample of retail outlets across 226 of Texas's 254 counties (approximately 89% coverage and about 5,000 retailers). Because the NielsenIQ files report only the first three digits of each store's ZIP code, off-premise sales can be measured reliably only at the county level. Together, these data allow us to examine, from the demand side, whether residents' alcohol consumption increased after alcohol sales were permitted.

3.2 Crashes in Texas

The annual motor vehicle crash dataset is a panel of yearly observations on Texas cities, compiled by the Texas Department of Transportation. It includes comprehensive crash records at the city and town levels, categorized by different injury severity levels.

Given our focus on alcohol availability policies, we specifically examine Driving Under the Influence (DUI) crashes [11], where alcohol consumption is identified as a contributing factor, along with the associated injury outcomes. Since the annual reports, available from 2003 onward [12], only include cities and towns with at least one reported crash, we assume that municipalities not listed in a given year experienced zero crashes for that calendar year.

Crash severity classifications changed over time, complicating direct year-to-year comparisons. Beginning in 2010, "Serious Injury" was redefined to include only "Incapacitating Injury," and further reclassification occurred in 2012. To maintain consistency across the full panel, we adopt a generalized scheme that groups crashes into fatal, any-injury, and non-

[10] Disclosure statement: Researchers' 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 researchers 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.

[11] The Texas Department of Transportation defines "DUI Alcohol" crashes as cases where the driver's BAC result is positive or a contributing factor is "Had Been Drinking" or "Under the Influence of Alcohol," excluding drug involvement.

[12] Data from 1998-2001 includes zero-crash municipalities, but as these reports cover only cities with populations over 2,500 and 2002 data is missing, our analysis starts from 2003.

injury categories without further distinguishing injury severity. All outcomes are normalized to crashes per 1,000 population.

Table 2: Summary Statistics

	Mean (Std. Dev.)
Number of Cities with elections	466
First Stage Outcomes	
All licenses (per 1,000, annual)	2.16
	(2.86)
On-premise licenses (per 1,000, annual)	0.92
	(1.24)
Off-premise licenses (per 1,000, annual)	1.24
	(2.24)
On-premise alcohol consumption (per capita, annual)	224.42
	(361.40)
Off-premise alcohol consumption (per capita, annual)	30.17
	(34.59)
Second Stage Outcomes	
All crashes (per 1,000, annual)	17.86
	(14.70)
DUI crashes (per 1,000, annual)	0.91
	(1.30)
Fatal crashes (per 1,000, annual)	0.16
	(0.58)
City Characteristics	
City population	12,936
	(34,245)
Protestant (% in county)	0.50
	(0.17)
Hispanic (% in county)	0.21
	(0.14)
Real GDP per capita (thousands of 2023 USD)	186.49
	(1,104.13)
Centerline road mileage (per 1,000)	44.04
	(61.83)
Total daily vehicle miles traveled (per 1,000)	38,032.64
	(48,531.05)
Vehicles registered (per 1,000)	1,252.41
	(2,723.16)
D/R vote ratio in presidential and gubernatorial elections (% in county)	0.46
	(0.33)
Population with high school diploma only (% in county)	0.30
	(0.07)
Unemployment rate (% in county)	5.50
	(1.80)

Notes: This table reports means with standard deviations in parentheses for cities with elections during the study period. Variables labeled (per 1,000) are measured per 1,000 population, and those labeled (% in county) represent county-level data.

Table 2 presents summary statistics for the full estimation sample. The sample includes 466 cities with at least one election during the study period, of which 326 transitioned their alcohol policy status. On the first-stage outcomes, cities average 2.16 alcohol licenses per 1,000 residents annually, with on-premise licenses considerably more common than off-premise licenses. For the second-stage outcomes, we focus on three traffic safety measures: all-cause crashes, DUI crashes, and fatal crashes, all measured per 1,000 population. We also control for a range of demographic, economic, and political characteristics at the city and county level that may confound the relationship between alcohol access and traffic safety.

4 Empirical Strategy

Our goal is to estimate the causal effect of transitioning from dry to wet status on two outcome stages: liquor establishments and crash rates. To identify these effects, we adopt multiple empirical strategies to identify the causal effects. First, we use a standard regression discontinuity design to estimate the immediate impact of the dry-to-wet transition, complemented by staggered difference-in-differences and event-study analyses to examine dynamic effects over time. Second, we implement a stacked dynamic RD framework that compares treated cities to "clean" control cities—those without subsequent elections within five years—to address potential heterogeneity and capture the effects of each election passage.

4.1 Regression Discontinuity Design

Our aim is to identify the effect of increased local availability of liquor on the incidence of traffic crashes in Texas cities. A dry city in Texas can choose to hold an election to decide whether to allow alcohol sales within its limits. If a majority of voters favor legalization, the city transitions from dry to partially or completely wet. We use these changes in alcohol sales regulations to identify their effect on traffic fatalities.

Before turning to our empirical design, it is important to consider potential sources of endogeneity in these elections. As discussed in Section 2.2, the timing and occurrence of these elections are not random. At least 20% of registered voters must petition the relevant authority to trigger an election, meaning that elections arise only where there is sufficient local interest or demand. As a result, both the decision to hold an election and its outcome may be correlated with underlying community characteristics that also influence traffic safety.

One clear example of this potential endogeneity comes from differences in religious composition across cities. Attitudes toward religiosity could be correlated with both alcohol-related behavior and voting outcomes. Elections are more likely to succeed in areas with a smaller Protestant population; while voter turnouts tend to be higher in cities with a larger Protestant share. Thus, religious composition of voters in Texas cities affects both both voting participation and election outcomes.

We turn to a regression discontinuity design to address these concerns. Our running variable is the margin of victory as defined by the difference between the percentage of votes in favor of legalizing alcohol sales and the percentage of votes against legalizing sales. Even if religious attitudes are correlated to voting outcomes, our empirical design is valid if these associations are continuous around the threshold margin of zero. By controlling polynomials of the vote margin, it is more likely that ex-ante characteristics of cities on either side of the threshold will be continuous around the win margin, allowing us to interpret any differences in outcomes as causal.

We estimate the effects of a successful election, or a dry-to-wet transition in a city with the following specification:

$$y_{c,k} = \alpha + \beta_0 \cdot \text{Pass}_c + \beta_1 \cdot \text{Margin}_c^n + \beta_2 \cdot \text{Margin}_c^n \cdot \text{Pass}_c + X_{c,k}\delta + \gamma_{t_c} + \varepsilon_{c,k} \tag{1}$$

where $y_{c,k}$ represents an outcome measured k years after the election in city c. We estimate equation 1 separately for different k, representing post-election years. Pass_c is an indicator that equals 1 if the election is successful; Margin_c refers to the running variable (the vote margin); and $X_{c,k}$ is a vector of covariates in year k.[13] The fixed effects γ_{t_c} capture common election year shocks, where t_c is the year the election occurred in city c. The treatment effect of a successful election, or a dry-to-wet transition, is identified by β_0. We estimate equation 1 using global polynomial regressions of order n. We also report the estimates with the optimal bandwidth selection method proposed by Calonico et al. (2014a,b, 2015) (in Table 4) and construct robust bias-corrected confidence intervals for inference.

To assess the validity of the regression discontinuity design, we test for both manipulation of the running variable and balance in pre-treatment characteristics. Following the density test of Cattaneo et al. (2018), we find no evidence of discontinuity in the distribution of election margins around the cutoff in Figure 2 (p = 0.5282), indicating that manipulation of election outcomes is unlikely. Pre-treatment covariates covering socio-economic, electoral, and traffic conditions also vary smoothly across the threshold, as shown in Table 3 (and appendix Figure A1). Together, these results suggest that cities narrowly passing and narrowly failing legalization elections are comparable, supporting the key identification assumption of the RD design.

4.2 Stacked Regression Discontinuity Design

Cities in Texas often hold legalization elections more than once. This can occur either because the first election fails, or when voters later consider expanding legality to additional types of liquor permits. During our study period, about 17% of cities held multiple elections, and roughly 12% passed more than one, with an average interval of four years between them. As a result, in the previous section, we restrict our attention to only the first successful election in a municipality.

[13] These include city population, the percentage of the population that is Protestant or Hispanic, real GDP per capita, the ratio of Democratic to GOP votes in presidential and gubernatorial elections, the proportion of the population with only a high school diploma, and the unemployment rate. We also include controls related to traffic conditions such as centerline miles, total daily vehicle miles traveled, the number of registered vehicles, and the average annual temperature range. Due to the lack of city-level data for some of these variables, we rely on county-level data and rescale them to per 1000 population metrics where applicable.

Table 3: Pre-treatment Covariate Balance Tests

	Optimal Bandwidth		Local Bandwidth	
	Linear	Quadratic	Linear	Quadratic
Socioeconomic Characteristics				
Log (Population)	0.43	0.33	0.37	0.29
	(0.29)	(0.39)	(0.49)	(0.65)
Unemployment rate	0.64	0.43	0.56	0.28
	(0.43)	(0.65)	(0.73)	(0.87)
GDP per capita (in $1000)	63.17	33.61	-31.60	-91.45
	(138.75)	(115.24)	(90.30)	(148.76)
High-school only share	-0.01*	-0.03	-0.03	-0.03
	(0.01)	(0.02)	(0.02)	(0.03)
Electoral Characteristics				
Election turnout	-0.01	-0.01	-0.03	-0.01
	(0.03)	(0.04)	(0.04)	(0.05)
Protestant share	-0.03	-0.06	-0.10	-0.12
	(0.04)	(0.06)	(0.07)	(0.11)
Hispanic share	0.04	0.08	0.10	0.15
	(0.03)	(0.05)	(0.08)	(0.11)
Democrat share	0.06	0.09	0.07	0.14
	(0.05)	(0.09)	(0.13)	(0.17)
Traffic Conditions				
Registered Vehicles per million	-0.02	-0.08	-0.10	-0.28**
	(0.04)	(0.06)	(0.07)	(0.13)
Vehicle Miles Traveled per million	-0.65	2.69	-7.08*	-8.40
	(2.97)	(4.10)	(4.39)	(6.27)
Annual Average Daily Temperature Range	-0.53	-0.96	-0.98	-0.64
	(0.69)	(1.05)	(1.64)	(2.21)

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Standard errors are in parentheses. The table reports estimates for covariate balance tests using equation 1. We use both data-driven (optimal) and fixed (full) bandwidths with triangular kernels.

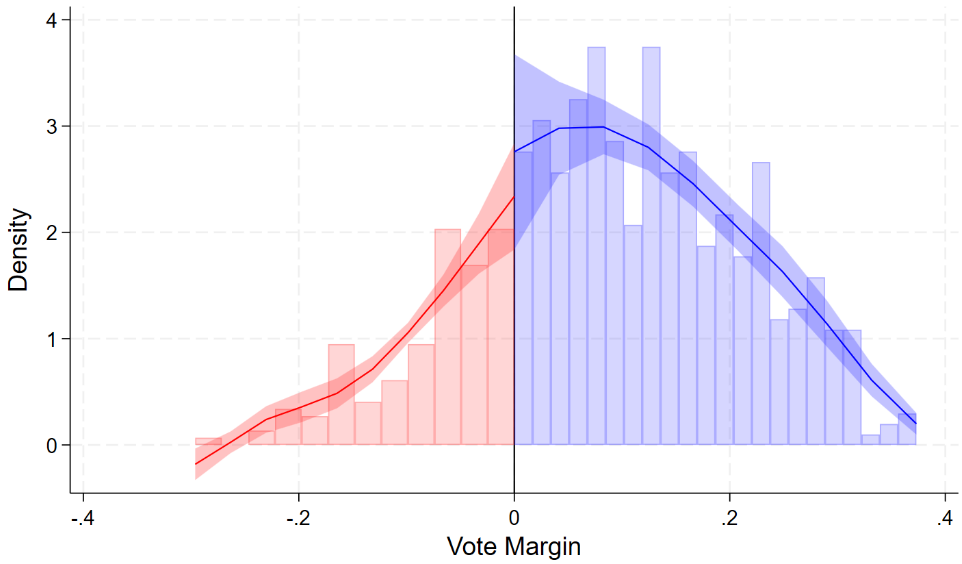


Figure 2: Testing Continuity of the Vote Margin in Elections

Note: This figure plots the density of the running variable, implementing the manipulation test from Cattaneo et al. (2018). The p-value from this test is p=0.5282.

To incorporate information from all elections that occur during our study period while maintaining clean comparisons across treatment cohorts, we adapt the dynamic regression discontinuity framework of Cellini et al. (2010) and Biasi et al. (2025). This approach allows us to treat each election as a separate treatment episode while accounting for both the timing and the sequence of elections, following the "dynamic" and "stacked" regression discontinuity estimators proposed by these authors. To do this, we construct a stacked city–year panel that tracks city outcomes relative to each election year, controls for prior election history, and compares narrowly passing cities to narrowly failing cities that do not pass another election in the near future.

We first define a treatment cohort g as all cities that narrowly passed an election in year g. For each cohort, the control group includes cities that narrowly failed in the same year and remained dry for at least five subsequent years (to avoid contamination from future successful elections). For both treated and control cities, we retain data for $k \in [-5, 5]$, corresponding to the five years before and after the election, and stack these datasets to form the final panel indexed by city, cohort, and year.

We estimate the following specification:

$$y_{cgt} = \alpha_{cg} + \gamma_{gt} + \varepsilon_{cgt} + \sum_{\substack{k\in[-5,5] \\ k\neq-1}} \left[\beta_k \,\text{Pass}_{cg,t-k} + \sigma_k \,\text{Held}_{cg,t-k} + P^n(\text{Margin}_{cg,t-k},\, \delta_k^n)\right] \tag{2}$$

where α_{cg} are city-by-cohort fixed effects and γ_{gt} are cohort-by-year fixed effects. $\text{Pass}_{cg,t-k}$ is an indicator equal to 1 if city c passed an election k years prior to year t, and 0 otherwise. The coefficients β_k trace the dynamic treatment effects of narrowly passing an election, relative to narrowly failing, across the event-time horizon.

Here we control for n-th order polynomials of the vote margin in each lead and lag period (set to 0 in non-election years) denoted by $P^n(\text{Margin}_{cg,t-k}, \delta_k^n)$, which helps isolate the identifying variation coming from elections, rather than by underlying trends correlated with the vote share. $\text{Held}_{cg,t-k}$ controls for a city's election history—it takes the value 1 if a city held an election k years before t [14]. This approach ensures that each treated city is compared only to nearly-failing control cities from the same election year with similar election history. Standard errors are clustered at the city level, and, following Biasi et al. (2025), we weight observations by population in the baseline year for each cohort.

4.3 Staggered Difference-in-differences Design

We also implement a staggered difference-in-differences strategy to estimate the effect of increased alcohol availability with greater precision and to capture the dynamic evolution of these effects over time. Unlike the previous sections, the control group consists of cities that remain dry and have not been treated (i.e., have not transitioned to wet status) during the study period, while the treatment group includes cities that first transition from dry to wet during this period. Cities that already allowed some form of alcohol sales at the start of the study are excluded from the sample, as they are already treated units and do not contribute

[14] This differs from $\text{Pass}_{cg,t-k}$, which takes the value 1 when a city passed an election rather than simply held one.

to identifying the effect of a dry-to-wet transition.

Our starting point is the following two-way fixed effects (TWFE) difference-in-differences regression:

$$y_{ct} = \alpha_c + \gamma_t + \beta \times \text{Wet}_{ct} + X_{ct}\delta + \varepsilon_{ct} \tag{3}$$

where y_{ct} is an outcome of interest in city c at calendar year t; Wet_{ct} is an indicator that equals 1 if city c is wet at year t; X_{ct} is a vector of time-varying control variables; α_c and γ_t are city and year fixed effects; and ε_{ct} is a city-clustered error term. When a successful election takes place in a city in year t, $\text{Wet}_{ct} = 1$, and stays 1 for all years $\geq t$. The identifying assumption here is that cities that remove the restrictions on alcohol sales are similar to the cities that remain dry, in terms of the pre-period trends in the outcome variables. To investigate the validity of this assumption we also estimate event study regressions with the following specification:

$$y_{ct} = \alpha_c + \gamma_t + \sum_{\substack{k\in[-5,5] \\ k\neq -1}} \beta_k \times \text{Post}^k_{ct} + X_{ct}\delta + \varepsilon_{ct} \tag{4}$$

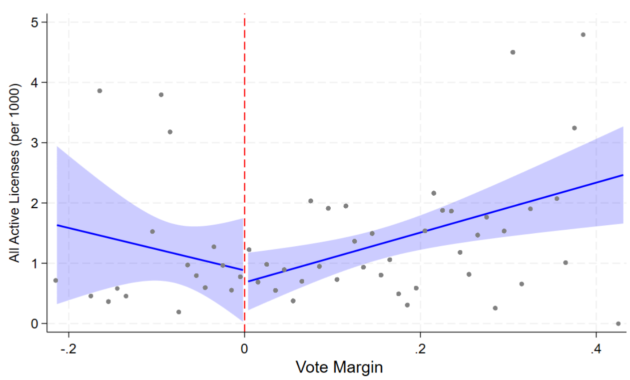
Here, Post^k_{ct} is an event-time dummy which equals 1 when city c is k years away from a dry-wet transition and 0 otherwise; it is also trivially zero for all cities which are never-treated (i.e., never change their dry status in the study period). We also include city and year fixed effects, time-varying covariates, and a city-clustered error term similar to equation 3. In this specification, showing that $\beta_k = 0$ when $k \leq -2$ provides support for the parallel trends condition as it shows that the treated group (cities which have already transitioned) are statistically similar to the control group (cities which have not-yet transitioned) in the pre-treatment period. In addition, β_k when $k \geq 0$ identifies the dynamic treatment effects relative to the year of the dry-wet transition.

We also estimate equations 3 and 4 using the Callaway and Sant'Anna (2021) estimator to make our analysis robust to the issues inherent in a staggered difference-in-differences setup. We focus on cities that have at least 4 years of pre-treatment data, which means that we do not consider status changes (i.e. successful elections) that occur before 2007.

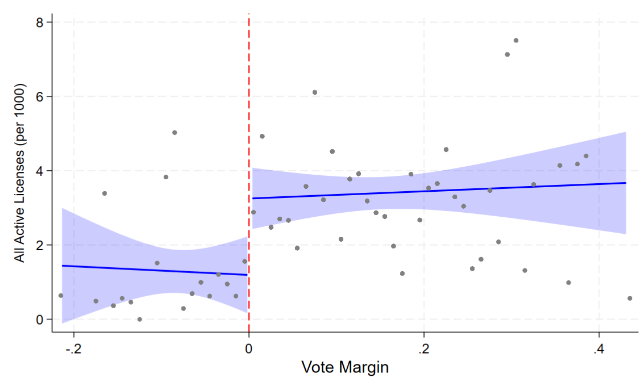
5 Results

5.1 Effect of Successful Elections on Liquor Establishments

We start by identifying the direct effect of a successful transition to wet in a city on alcohol access. When a dry city passes an election to allow alcohol sales, it lifts existing restrictions on alcohol sales, so we should expect the number of liquor establishments to increase. Figure 3 illustrates this pattern clearly, showing a distinct discontinuity around the win margin one year after the election, in contrast to the absence of any difference one year before.



(a) Event time $t = -1$



(b) Event time $t = +1$

Figure 3: All liquor licenses per 1000 population (pre-treatment vs. post-treatment)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before and after the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019.

Table 4 presents the regression discontinuity estimates from equation 1 across multiple specifications. The table is organized into two panels. The first panel reports first-stage outcomes that capture changes in alcohol access—total liquor licenses, on-premise and off-premise licenses, and corresponding sales measures. The second panel reports second-stage outcomes, including various categories of crash rates. Each column corresponds to a distinct RD specification: local versus global bandwidths, with and without covariates, and linear versus quadratic polynomial controls for the vote margin. All coefficients represent intent-to-treat effects and are measured per 1,000 population. Standard errors are clustered at the city level. The first column reports the global regression discontinuity specification that includes the full set of covariates and employs a linear polynomial in the vote margin. We regard this specification as preferred. The linear polynomial offers an appropriate level of functional-form flexibility without sacrificing precision, and the use of a global bandwidth mitigates the loss of effective sample size that arises in the local RD specifications.[15] Under this specification, the estimated treatment effect indicates that, one year after passing the election, cities issued approximately 2.5 additional liquor licenses per 1,000 population per year compared with cities where the measure failed, a sizable and statistically significant increase in alcohol access following a successful transition to wet status.[16]

We find that this increase is driven by the increase in off-premise liquor establishments (see Figure 4), including liquor stores, grocery stores, and convenience stores that sell alcohol for consumption elsewhere. We also compare on- and off-premise alcohol sales before and after the election using data from different sources, which further support these findings: on-premise sales show an insignificant increase of about \$12 per capita per year, whereas off-premise sales rise significantly by roughly \$21 per capita per year. Given that a larger share of elections in our study period lifted restrictions on off-premise sales rather than on-premise consumption, this is also consistent with an absence of statistically or economically significant increases in the number of on-premise licenses, meaning bars and restaurants are not driving the observed increase in liquor establishments.

5.2 Effect of Successful Elections on Crashes

We now turn to effect of increasing alcohol availability, resulting from a successful local option election, on traffic crashes. The bottom panel of Table 4 reports the estimated effects

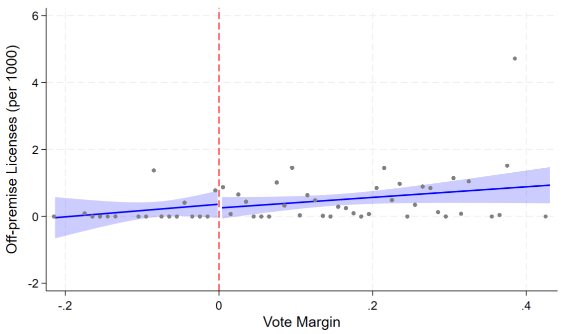
[15] In the local bandwidth estimates, the number of observations on either side of the cutoff can shrink to as few as 10–30, limiting statistical power and stability.

[16] In the sample, four cities—DISH, Jolly, Coffee City, and Emhouse—were excluded due to incorporation adjustments or irregular year-to-year changes in reporting.

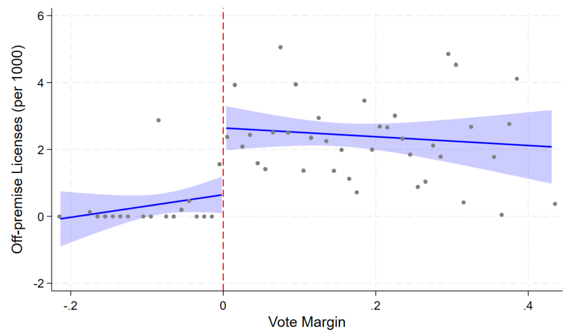
Table 4: Regression Discontinuity Results

	Mean	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First-stage outcomes									
All licenses	1.2933	2.5746***	3.4067***	1.3355***	2.2952***	2.0467*	1.4959	3.8942***	3.0678**
		(0.5788)	(0.9561)	(0.3433)	(0.6755)	(1.1942)	(1.1900)	(1.3272)	(1.4311)
On-premise licenses	0.8484	0.3130	0.7282***	0.4606***	0.9809**	0.8755**	0.2849	1.2782***	1.301***
		(0.2098)	(0.2571)	(0.1695)	(0.2894)	(0.2869)	(0.3667)	(0.2459)	(0.3784)
Off-premise licenses	0.4449	2.2616***	2.6785***	0.8748***	1.3143**	0.9424	0.2021	3.0651***	1.969
		(0.5187)	(0.8588)	(0.2904)	(0.5320)	(0.9807)	(0.9705)	(1.2513)	(1.2793)
On-premise sales	221.2596	12.385	73.394	48.371	329.71***	162.16**	179.04**	43.532*	132.28*
		(50.451)	(69.533)	(49.641)	(108.27)	(63.931)	(78.762)	(25.658)	(76.958)
Off-premise sales	10.7025	21.171**	25.238	55.092*	55.092	29.07	39.357	29.07	39.357
		(9.1728)	(17.732)	(32.702)	(33.529)	(19.383)	(27.687)	(19.383)	(27.687)
Second-stage outcomes									
All crashes	17.0000	2.1171	9.1623**	6.0302**	11.841***	9.5003**	13.748***	10.076***	14.222***
		(0.488)	(4.061)	(2.784)	(4.5575)	(4.5034)	(4.9853)	(3.4923)	(3.8965)
DUI crashes	0.9655	-0.3256	0.1952	0.2331	0.1418	0.4781	0.9886	0.4672	0.4641
		(0.3937)	(0.5603)	(0.1524)	(0.2837)	(0.5178)	(0.6704)	(0.4530)	(0.5897)
Non-DUI crashes	16.0345	2.4427	8.9671**	5.7971**	11.279**	8.4034*	12.423**	9.4945***	13.295***
		(2.8659)	(3.7905)	(2.7143)	(4.4705)	(4.3828)	(4.8183)	(3.2516)	(3.5981)
Fatal crashes	0.8425	0.1999	0.3203**	0.0614**	0.1586***	0.1164	0.1107	0.2688**	0.3479***
		(0.1252)	(0.1486)	(0.0294)	(0.0416)	(0.1243)	(0.0928)	(0.1256)	(0.1036)
Non-fatal crashes	16.4429	2.0275	8.6979**	6.7707**	12.225***	9.1214	13.087***	9.7169***	13.633***
		(2.9809)	(3.9321)	(2.6457)	(4.3855)	(4.4358)	(4.9367)	(3.4608)	(3.8777)
DUI fatal crashes	0.0307	0.0607	0.0843	0.0370***	0.0613***	0.1085	0.1130	0.1120	0.1287
		(0.0386)	(0.0717)	(0.0112)	(0.0162)	(0.1147)	(0.1286)	(0.1092)	(0.1276)
DUI non-fatal crashes	0.9103	-0.3829	-0.0973	0.2307	0.5138**	0.5641	0.8866	0.3100	-0.0040
		(0.3920)	(0.5554)	(0.0156)	(0.2328)	(0.4034)	(0.6729)	(0.3649)	(0.5815)
Bandwidth		Global	Global	Global	Global	Local	Local	Local	Local
Polynomial Order		Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Covariates		✓	✓	✓	✓	X	X	X	X
Population weights		X	X	✓	✓	X	X	X	X

Notes: * p<0.10, ** p<0.05, *** p<0.01. The coefficients are obtained from separate regressions of equation 1, each using a different outcome measured per 1,000 population. We use data-driven bandwidths as optimal bandwidths. Robust standard errors are clustered at city level. Licensing and on-premise sales data are obtained from the Texas Alcoholic Beverage Commission, off-premise sales data are from NielsenIQ, and crash data are from the Texas Department of Transportation. The covariates are the city characteristics in Table 2. The Mean column reports the pre-period mean of the dependent variable, computed as the average of each outcome measured one year before the election.

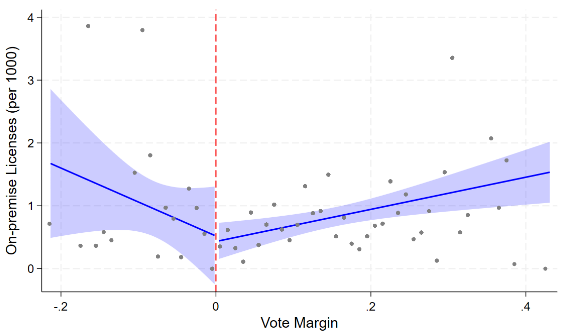


Event time $t = -1$

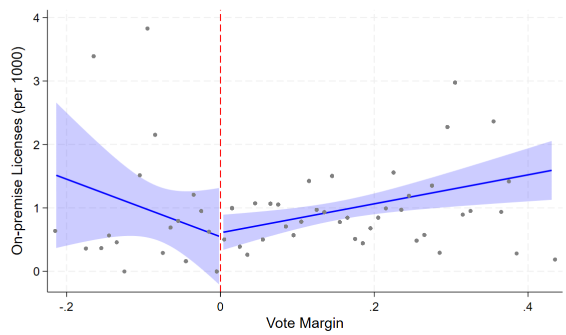


Event time $t = +1$

(a) Off-premise liquor licenses



Event time $t = -1$



Event time $t = +1$

(b) On-premise liquor licenses

Figure 4: Off- and on-premise liquor licenses per 1,000 population (pre- vs. post-treatment)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before and after the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019.

of election passage on traffic crashes. Across most specifications, we observe positive and statistically significant coefficients on total crashes, ranging from roughly 6 to 14 additional crashes per 1,000 population depending on bandwidth and polynomial order. Taken at face value, this would suggest that legalization increases crash rates. We argue, however, that this pattern does not reflect a causal effect of the policy.

The key evidence is in Figure 5. The left column plots crash outcomes one year *before* the election against the vote margin, and the right column does the same one year *after*. For total crashes, the two figures are nearly identical: cities that narrowly passed the election already had higher crash rates than cities that narrowly failed, even before the vote took place. A causal treatment effect cannot produce a pre-election discontinuity. This pattern instead reflects a pre-existing difference between the two groups: cities that favor alcohol legalization could be more commercially active and higher-traffic, and therefore have higher baseline crash rates independent of alcohol policy. The significant total crash coefficients in the RD table are thus best understood as an artifact of this pre-existing heterogeneity that the vote-margin polynomial does not fully absorb, rather than evidence of a direct policy effect.

The causally relevant outcome is DUI crashes — the direct mechanism through which alcohol access would affect road safety. In contrast to total crashes, DUI crashes show no discontinuity at either $t = -1$ or $t = +1$ in Figure 5, and the estimated coefficients are small in magnitude and statistically indistinguishable from zero across all specifications in Table 4. Fatal crashes are also null across most specifications, with the occasional significant coefficient concentrated in quadratic polynomial specifications and local bandwidth estimates, where the effective sample falls to as few as 10–30 observations.[17] Together, the absence of any effect on DUI crashes — the outcome most directly tied to the mechanism of interest, is the most informative result, and it is robust across all specifications. Appendix Figures A6 and A8 replicate the license and crash RD plots under a quadratic polynomial and confirm

[17]Most cities in the sample report zero fatal crashes in a given year, making point estimates in the tails of the distribution highly sensitive to a small number of observations.

the same pattern.

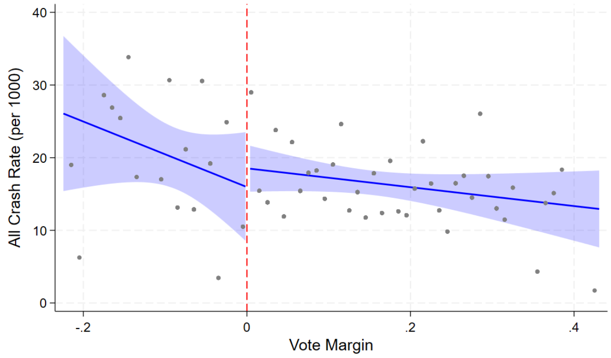
As a further check, Table 4 breaks crashes down by type and severity. Non-DUI crashes drive the positive total crash coefficients, consistent with the interpretation that the discontinuity in total crashes reflects pre-existing traffic differences rather than alcohol-related behavior. DUI crashes and fatal crashes are null across all specifications. We return to this finding in the context of the stacked dynamic RD and staggered difference-in-differences results in the next section, where the null on crashes is confirmed using designs with cleaner control groups and a longer event window.

5.3 Dynamic Effects of Successful Elections

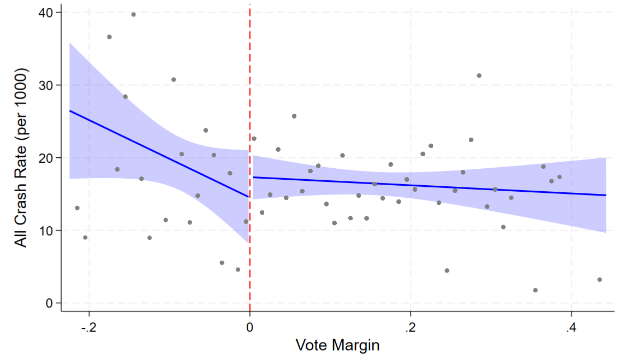
Building on Biasi et al. (2025), we employ a stacked dynamic regression discontinuity framework, conditioning on each city's election history and restricting the control group to "clean" cities that did not approve any further elections within five years following the focal one. This approach treats each election in a given city as a separate event, allowing us to stack multiple local RD estimates and trace the dynamic effects of election passages over time. By leveraging variation in election timing within cities and across cities, this method improves precision and enables a clearer assessment of how policy impacts evolve in the years following each election.

Table 5 and Table 6 present the dynamic effects of relaxing liquor sales restrictions on alcohol establishments and sales over the short term (0–2 years) and the longer term (3–5 years). All observations are weighted by city population in the baseline year. The regressions include city-by-cohort and cohort-by-year fixed effects, with the exception of off-premise sales, which are available only at the county level. We find strong evidence that legalization substantially increases the number of licensed outlets, particularly for off-premise retailers. Within two years after the election, the total number of alcohol establishments rises by roughly 0.59 per 1,000 residents — approximately 30% above the pre-treatment mean of 1.96 — driven almost entirely by off-premise licenses, which increase by roughly 48% above

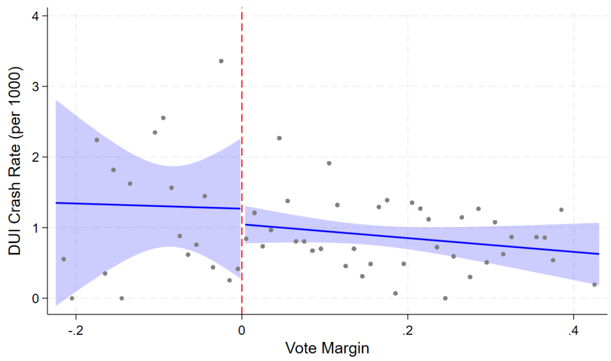
Event time $t = -1$ **Event time $t = +1$**



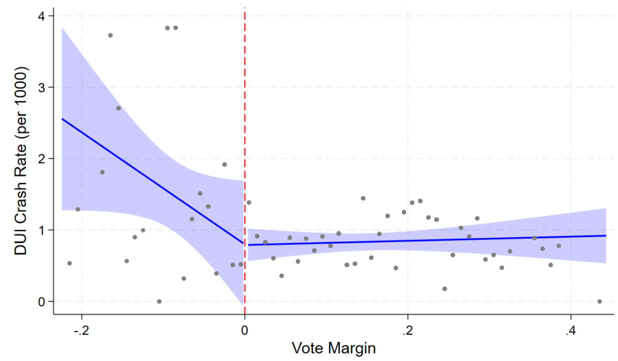
(a) All crashes



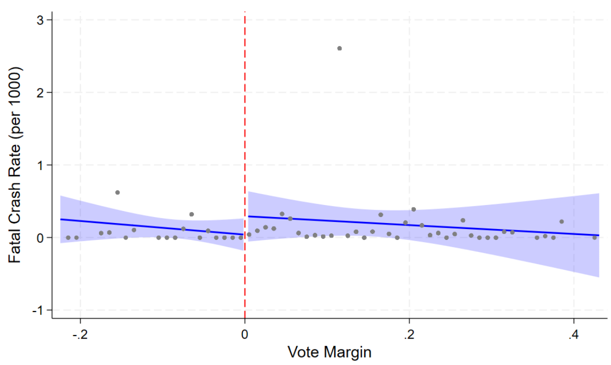
(b) All crashes



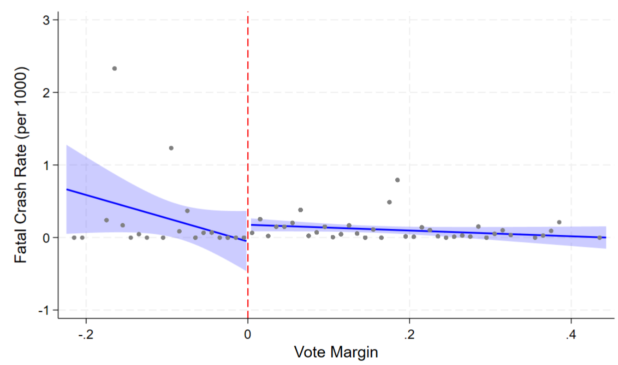
(c) DUI crashes



(d) DUI crashes



(e) Fatal crashes



(f) Fatal crashes

Figure 5: Crash outcomes per 1,000 population by event time ($t = -1, +1$)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before ($t = -1$) and after ($t = +1$) the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019. For total crashes, the near-identical pattern at $t = -1$ and $t = +1$ indicates that cities narrowly passing elections had higher crash rates prior to the vote, reflecting pre-existing differences in traffic exposure rather than a treatment effect of legalization.

Table 5: Stacked RD Estimates of Election Passage on Alcohol Establishments

	All (1)	On-Premise (2)	Off-Premise (3)
Average Effect (years after election):			
0–2 years	0.5903***	0.0020	0.5884***
	(0.1256)	(0.0439)	(0.1021)
3–5 years	0.6820***	0.1008	0.5813***
	(0.1389)	(0.0698)	(0.1047)
City–cohort fixed effects	X	X	X
Year–cohort fixed effects	X	X	X
Pre-treatment mean	1.9649	0.7344	1.2306
Adj. R^2	0.8858	0.8969	0.8363
N	5,657	5,657	5,657

Notes: This table reports stacked RD estimates and standard errors of linear combinations of β_k from equation 2. The dependent variables are: (1) total number of alcohol establishments, (2) number of off-premise establishments, and (3) number of on-premise establishments, each expressed as the number per 1,000 population. Observations are weighted by the city population at the start of the analysis period. All specifications include city-cohort and year-cohort fixed effects. Standard errors, shown in parentheses, are clustered at the city level. Licensing data are obtained from the Texas Alcoholic Beverage Commission.

Table 6: Stacked RD Estimates of Election Passage on Alcohol Sales

	On-Premise (1)	Off-Premise (2)
Average Effect (years after election):		
0–2 years	-42.3981***	10.6167***
	(15.9567)	(3.7406)
3–5 years	-12.8851	10.4004**
	(28.3626)	(4.7504)
City–cohort fixed effects	X	X
Year–cohort fixed effects	X	X
Pre-treatment mean	210.07	25.5482
Adj. R^2	0.9692	0.8959
N	3,941	4,184

Notes: This table reports stacked RD estimates and standard errors of linear combinations of β_k from equation 2. The dependent variables are: (1) on-premise alcohol consumption, (2) off-premise alcohol consumption, each measured in per-capita dollars (adjusted to 2023 dollars). Observations are weighted by city population at the start of the analysis period. All specifications include city–cohort and year–cohort fixed effects. Standard errors, shown in parentheses, are clustered at the city level. On-premise sales data are obtained from the Texas Alcoholic Beverage Commission and off-premise sales data are from NielsenIQ.

their pre-treatment mean, while on-premise licenses show no significant change. The effects persist and slightly strengthen three to five years after the election, suggesting that the number of active establishments continues to increase as markets adjust to the new regulatory environment (Appendix Figure A4 shows the full event-study grid for licenses and crashes). In contrast, Table 6 shows asymmetric effects on alcohol sales: on-premise sales decline sharply—by about \$42 (per capita per year) in the first two years—while off-premise sales rise by about \$10 (per capita per year) during the same period. This pattern suggests a reallocation of alcohol consumption from bars and restaurants toward retail outlets, consistent with easier availability and potentially lower retail prices following legalization (Appendix Figures A4c and A4d display the corresponding event-study plots).

On the crash side, Table 7 reports no evidence that expanded alcohol access led to higher accident rates. The estimated effects on total, DUI, and fatal crashes are small in magnitude and statistically indistinguishable from zero both in the short run (0–2 years) and the long run (3–5 years). These results indicate that while legalization spurred the growth of the retail alcohol market and shifted consumption toward off-premise settings, these changes did not translate into measurable increases in alcohol-related traffic incidents. Overall, the stacked dynamic RD evidence suggests that local alcohol liberalization primarily affected market structure and sales channels rather than generating adverse public safety consequences.

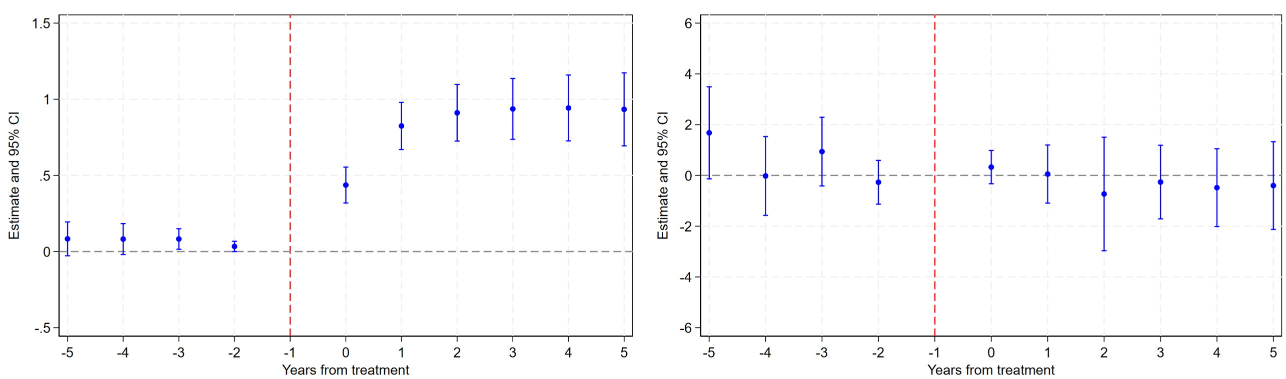
As a complementary analysis, we estimate a staggered difference-in-differences model following Callaway and Sant'Anna (2021), focusing on cities that transitioned from dry to wet between 2003 and 2019. The results largely mirror those from the stacked dynamic RD framework, with one minor difference: on-premise licenses show a statistically significant but economically modest increase relative to the much larger expansion in off-premise licenses. Table 8 presents the average treatment effects, reporting both the preferred Callaway and Sant'Anna (2021) (CSDID) estimates and the two-way fixed effects (TWFE) results for comparison, while Figures 6 and 7 display the corresponding event-study estimates.

The apparent discrepancy for on-premise results across specifications reflects differences in

Table 7: Stacked RD Estimates of Election Passage on Traffic Accidents

	All Crashes (1)	DUI Crashes (2)	Fatal Crashes (3)
Average effect (years after election):			
0–2 years	-0.4698	0.0479	-0.5177
	(0.7978)	(0.0762)	(0.7414)
3–5 years	-0.7628	0.0583	-0.8211
	(0.9269)	(0.0662)	(0.9021)
City–cohort fixed effects	X	X	X
Year–cohort fixed effects	X	X	X
Pre-treatment mean	14.2302	0.7486	0.1083
Adj. R^2	0.8635	0.6519	0.8637
N	5,992	5,992	5,992

Notes: This table reports estimates and standard errors of linear combinations of β_k from equation 2. The dependent variables are: (1) total traffic accidents, (2) DUI-related crashes, and (3) fatal crashes, each expressed as rates per 1000 population. Observations are weighted by city population at the start of the analysis period. All specifications include city–cohort and year–cohort fixed effects. Standard errors, shown in parentheses, are clustered at the city level. Crash data are from the Texas Department of Transportation.



(a) All licenses

(b) All crashes

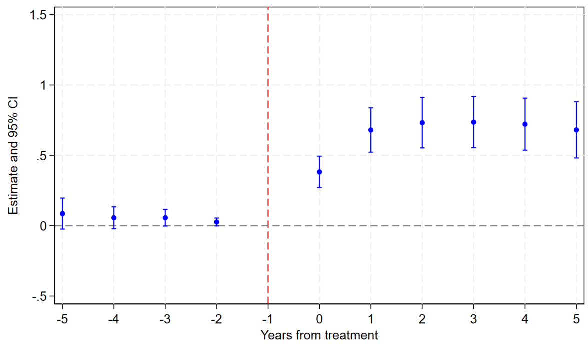
Figure 6: All licenses and all crashes (per 1,000 population)

Note: The figure presents event-study coefficients, calculated by averaging group-time treatment effects using the Callaway and Sant'Anna (2021) estimator. The sample includes only cities that had a dry-to-wet transition between 2003 and 2019.

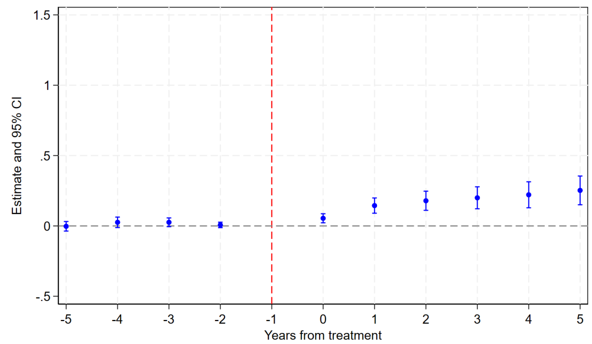
Table 8: Difference-in-Differences Estimates

		Estimation Approaches	
		TWFE	CSDID
First-stage outcomes			
All Licenses		0.8664***	0.6085**
		(0.1109)	(0.2529)
Pre-period mean: 1.2706	N: 6,279		
On-Premise Licenses		0.0903	0.1962**
		(0.0564)	(0.0774)
Pre-period mean: 0.8220	N: 6,279		
Off-Premise Licenses		0.7761***	0.4123*
		(0.0818)	(0.2237)
Pre-period mean: 0.4486	N: 6,279		
On-Premise Sales		23.5293	41.0610
		(20.5820)	(0.141)
Pre-period mean: 213.63	N: 3,720		
Off-Premise Sales		8.0795**	10.8458***
		(3.7758)	(3.3455)
Pre-period mean: 10.7024	N: 88		
Second-stage outcomes			
All Crashes		-1.7451**	1.4722
		(0.7630)	(1.1406)
Pre-period mean: 16.6597	N: 6,699		
DUI Crashes		-0.0675*	0.0818
		(0.0404)	(0.0840)
Pre-period mean: 0.8814	N: 6,699		
Non DUI Crashes		-1.6777**	1.3904
		(0.7396)	(1.0778)
Pre-period mean: 15.7778	N: 6,699		

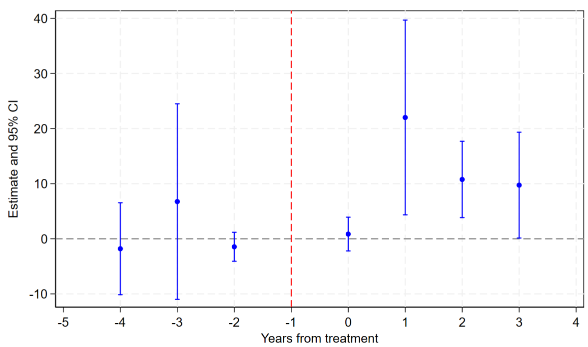
Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The coefficients in column 1 are obtained from separate regressions of equation 3, each using a different outcome measured per 1,000 population. Data are mainly from Texas Department of Transportation and Texas Alcoholic Beverage Commission. No Covariates are included.



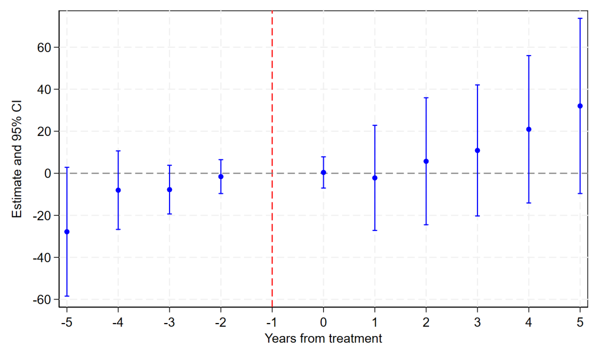
(a) Off-premise licenses



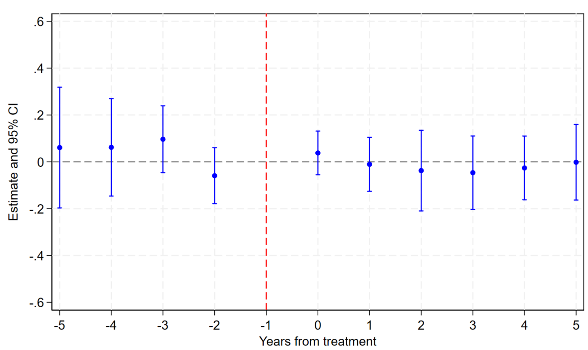
(b) On-premise licenses



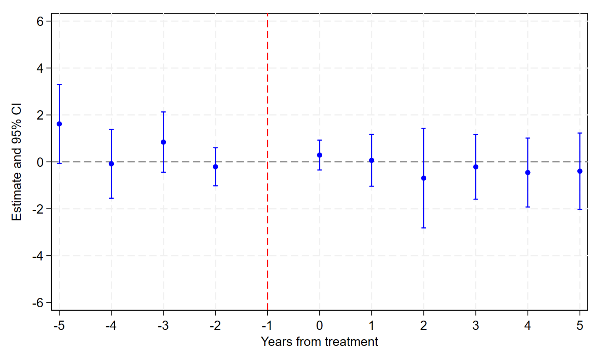
(c) Off-premise sales



(d) On-premise sales



(e) DUI crashes



(f) Non-DUI crashes

Figure 7: Licenses, sales, and crash outcomes (per 1,000 population)

Note: The figure presents event-study coefficients, calculated by averaging group-time treatment effects using the Callaway and Sant'Anna (2021) estimator. The sample includes only cities that had a dry-to-wet transition between 2003 and 2019.

the comparison sets. The CSDID estimator compares newly legalized ("first-time wet") cities with those that remain dry throughout the period. These cities often begin with no legal on-premise sales, so any legalization mechanically generates some on-premise activity. In contrast, the stacked regression-discontinuity design compares cities narrowly passing versus narrowly failing a given election, conditional on already participating in repeated local-option referenda. These municipalities frequently permit limited alcohol sales under beer-and-wine or private-club provisions prior to the election. For them, further liberalization mainly expands retail off-premise access rather than increasing bar or restaurant sales, producing a decline in on-premise revenues relative to off-premise channels.

5.4 Mechanism

The absence of measurable effects on DUI or total crashes can be explained by the nature of the policy change: the elections primarily liberalized off-premise alcohol sales, such as those in grocery and convenience stores, which are less directly associated with immediate drinking before driving than on-premise establishments like bars or restaurants. In contrast, crashes, especially DUI crashes, are more closely tied to on-premise consumption, as individuals who drink at bars and then drive home are the primary contributors to alcohol-related accidents. Consequently, expanding off-premise sales is unlikely to generate substantial changes in DUI or overall crash rates.

To support our interpretation, we run a simple two-way fixed effect regression:

$$y_{ct} = \alpha \cdot \text{OnPrem}_{ct} + \beta \cdot \text{OffPrem}_{ct} + \alpha_c + \gamma_t + \varepsilon_{ct}, \tag{5}$$

where y_{ct} is the crash outcome of interest, and OnPrem_{ct} and OffPrem_{ct} measure the number of on-premise and off-premise alcohol licenses in city c and year t, respectively. The model includes city fixed effects (α_c) and year fixed effects (γ_t) to account for time-invariant heterogeneity and common shocks.

Table 9 reports the estimated effects of on- and off-premise alcohol licenses on crash rates.[18] The results show that on-premise licenses are positively associated with crash outcomes: a one-unit increase in on-premise licenses per 1,000 residents per year is linked to roughly 1.17 additional total crashes and 1.07 additional non-DUI crashes per 1,000 residents per year , both statistically significant at the 5 percent level. The effect on DUI crashes is also positive but smaller in magnitude, at about 0.10 crashes per 1,000 residents per year. In contrast, off-premise licenses exhibit no significant relationship with any crash category. The findings indicate that traffic accidents are more closely associated, in a correlational sense, with the presence of on-premise drinking venues such as bars and restaurants. In contrast, the expansion of off-premise retail access through grocery and convenience stores shows no measurable relationship with road safety in these exploratory regressions, consistent with our earlier interpretation.

Table 9: Effects of Alcohol Licenses on Crash Rates

	Crash Rate (per 1,000 population)		
	All	DUI	Non-DUI
On-premise licenses (per 1000)	1.1749**	0.1050**	1.0699**
	(0.4836)	(0.0450)	(0.4491)
Off-premise licenses (per 1000)	0.1867	0.0018	0.1850
	(0.1338)	(0.0174)	(0.1318)
City FE	X	X	X
Year FE	X	X	X
Observations	7,140	7,140	7,140
R^2	0.7494	0.3537	0.7440

Notes: Each column reports estimates from equation 5. Standard errors (in parentheses) are clustered at the county level. The dependent variable is the number of crashes per 1,000 residents. All covariates are included.

[18] In this specification, we use the number of alcohol licenses rather than sales as the primary explanatory variables, since on- and off-premise sales are drawn from different data sources and the off-premise sales data do not capture all retail establishments.

5.5 Spatial Spillovers Across Cities

Local alcohol-option elections can affect not only the voting city but also adjacent municipalities. Residents in dry areas may cross city borders to buy or consume alcohol, and the point of purchase may differ from the point of consumption; people may buy alcohol in a wet city and consume it while driving through a neighboring dry city. If so, spillovers could generate crash effects in dry cities even absent local legalization, potentially masking our main null result. We therefore examine whether a city's outcomes respond to the share of neighboring cities that are wet.

We estimate the following two-way fixed effects specification:

$$y_{ct} = \beta_1 \text{Wet}_{ct} + \beta_2 \text{WetShare}_{ct} + \beta_3 \text{Wet}_{ct} \times \text{WetShare}_{ct} + \alpha_c + \gamma_t + \varepsilon_{ct}, \tag{6}$$

where WetShare_{ct} is the share of neighboring cities within 20 miles that are wet in year t, and α_c and γ_t are city and year fixed effects. The coefficient β_2 captures the spillover effect for dry cities, while $\beta_2 + \beta_3$ captures the total spillover effect for wet cities. The sample excludes cities that were dry throughout the entire observation period with no elections, as these are structurally distinct municipalities, predominantly rural with limited commercial activity, that are unlikely to serve as meaningful comparison units for cities where alcohol policy is actively contested.[19]

Table 10 reports results for the main 20-mile exposure specification. For licenses, wet cities with more wet neighbors show a negative, though imprecise, association with license counts, consistent with a reallocation of alcohol establishments toward newly wet neighbors as markets integrate. For dry cities, we find a positive and sometimes significant association between wet neighbor share and on-premise licenses, which is difficult to interpret causally and may reflect unobserved characteristics of cities in transitional regulatory environments rather than a true cross-city spillover.[20]

[19] Including always-dry cities does not change the crash results but introduces noise in the license estimates.

[20] This result is concentrated among cities that eventually legalize and is absent for cities that are always

For traffic crashes — the primary focus of the analysis — we find no evidence of meaningful spillover effects. Estimated effects on total crashes and DUI crashes are small and statistically indistinguishable from zero for both dry and wet cities. We observe positive and significant coefficients on fatal crashes, but we do not interpret these as evidence of meaningful road safety spillovers: the baseline rate of fatal crashes is very low (mean $\approx$ 0.028 per 1,000 population), making the estimates sensitive to a small number of observations, and the pattern does not extend to total or DUI crashes, which are the more informative measures of alcohol-related road safety. Appendix Table A1 reports results across binned distance rings (0–10, 10–20, and 20–50 miles) and confirms the absence of consistent spillover effects on crashes across specifications. Combining this evidence with the null result in the main analysis, we conclude that local alcohol liberalization does not generate measurable traffic safety consequences, even when accounting for potential cross-border effects.

Table 10: Spillover Estimates with 20-Mile Exposure

	Licenses			Crashes		
	All	Off	On	All	DUI	Fatal
Spillovers to dry cities	0.896	0.233	0.675***	-0.022	-0.032	0.039**
	(0.669)	(0.636)	(0.187)	(0.122)	(0.123)	(0.015)
Spillovers to wet cities	-1.450	-1.690	0.379	0.111	0.085	0.040**
	(1.530)	(1.500)	(0.319)	(0.133)	(0.134)	(0.018)
Mean of DV	3.680	2.300	1.950	0.809	0.823	0.028
N	18,623	18,623	18,623	18,623	18,623	18,623

Notes: This table reports the coefficients from the spillover regression (Equation 6) with own wet status, exposure to wet neighbors, and their interaction. The first coefficient row reports the spillover effect of having wet neighbors for dry cities only, and the second row reports the total spillover effect on wet cities as a linear combination of the exposure and interaction coefficients. Exposure is the share of neighboring cities within 20 miles that are wet in a given year. All outcomes are measured per 1,000 population. The sample includes all cities that are already wet before 2003, and cities that change status during the study period. All columns include city and year fixed effects, and standard errors clustered at the city level are shown in parentheses.

dry throughout the sample period, suggesting it does not represent a structural spillover effect.

5.6 Heterogeneity by Alcohol Strength

We next explore how the effects of local option elections differ by the type of alcohol legalized and by the type of beverage sold. To do so, we draw on two complementary pieces of evidence.

First, we disaggregate alcohol sales by product category—beer and wine versus hard liquor—using the baseline stacked RD specification in equation 2. Specifically, we estimate separate regressions for on-premise beer and wine sales and for on-premise liquor sales, and similarly for off-premise sales.

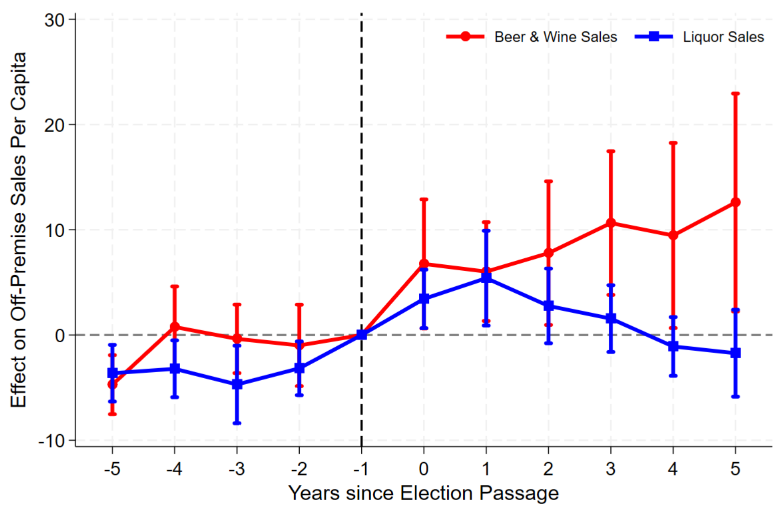
Second, we examine heterogeneity in the effects of local option elections by the type of alcohol legalized. As described in our dynamic RD framework, we extend equation 2 to allow the treatment effect coefficients to vary by the category of election e, where e indexes the type of alcohol and consumption setting legalized (for example, beer and wine for off-premise sales, or hard alcohol for on-premise sales). The estimating equation follows Biasi et al. (2025):

$$\begin{aligned} y_{cgte} &= \alpha_{cg} + \gamma_{gt} \\ &\quad + \sum_{\substack{k\in[-5,5] \\ k\neq -1}} \left[\beta_{k,e}\,\mathrm{Pass}_{cg,t-k,e} + \sigma_{k,e}\,\mathrm{Held}_{cg,t-k,e} + P^{n}(\mathrm{Margin}_{cg,t-k,e},\ \delta^{n}_{k,e})\right] + \varepsilon_{cgte}, \end{aligned} \tag{7}$$

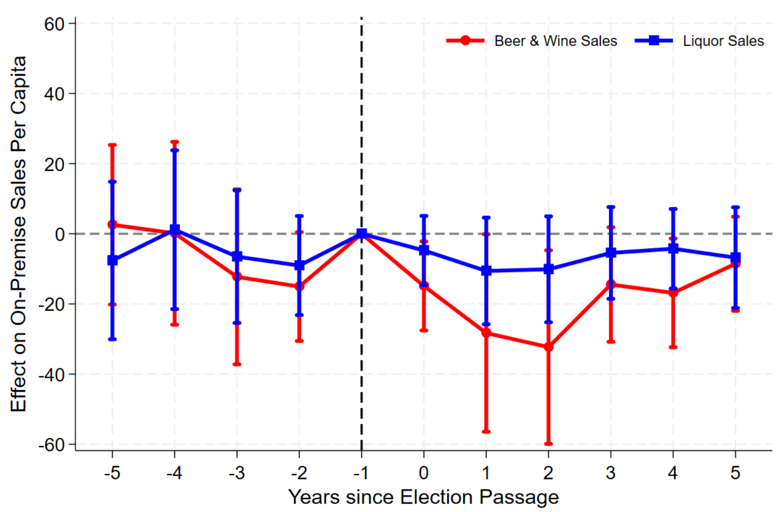
where all notation follows equation 2, but the indicators $\mathrm{Pass}_{cg,t-k,e}$, $\mathrm{Held}_{cg,t-k,e}$, and $\mathrm{Margin}_{cg,t-k,e}$ are now indexed by election type e. Each regression is estimated separately by election type, allowing treatment effects to differ across categories of alcohol legalization.

We distinguish between two main groups of elections: (1) Beer and wine elections, which legalize only low-alcohol beverages, and (2) Hard liquor elections, which legalize all forms of alcohol, including distilled spirits and mixed beverages.

In dry areas, on-premise alcohol availability is extremely limited, with only a small number of private social clubs permitted to sell alcohol. After the election passage, on-premise sales of beer and wine decline (Figure 8(a)). In contrast, off-premise sales of these beverages rise by a broadly similar magnitude (Figure 8(b)), suggesting that consumers shift purchases away from bars and restaurants and toward take-home retail outlets. Liquor sales, however, show little to no systematic change after the elections. Taken together, these patterns point to substitution within beverage categories, from on-premise to off-premise consumption, rather than an expansion in overall alcohol demand.



(a) Off-premise sales (by category)



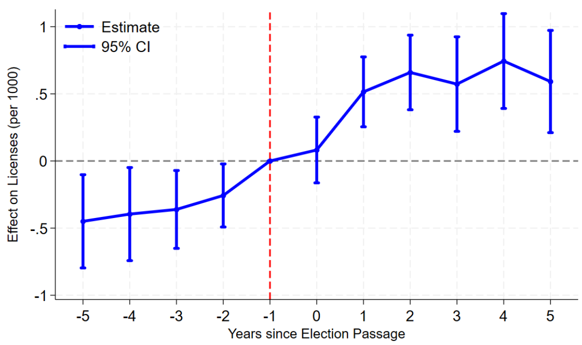
(b) On-premise sales (by category)

Figure 8: Stacked RD Estimates of Elections on Sales by alcohol category

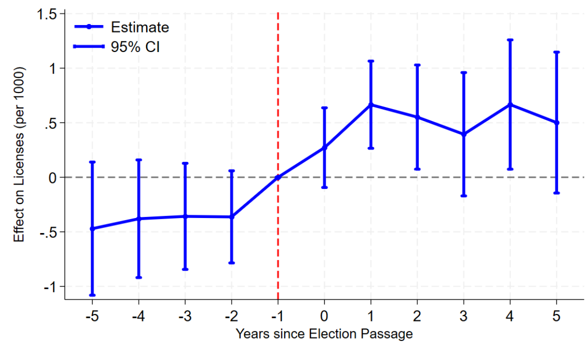
Note: The figure presents coefficients and confidence intervals of the coefficient of election passage indicators in equation 2, following Biasi et al. (2025). The specification includes city×cohort and cohort×year fixed effects. We weight observations by baseline-year population for each cohort and cluster standard errors at the city level. For off-premise sales, data are available only at the county level; accordingly, the unit of analysis and clustering are at the county level.

When we disaggregate by type of election (see Figure 9), the stacked dynamic RD results show that beer and wine elections have no measurable effect on total or alcohol-related crashes. The estimated coefficients remain close to zero across the event window, consistent with the substitution effects described above. In contrast, following liquor elections—which expand access to higher-strength beverages and typically allow both on- and off-premise sales—we observe a modest increase in total crash rates only under the optimal bandwidth specification, which echoes the result from Baughman et al. (2001). Like us, they find that beer and wine liberalization has negligible effects on accidents, while the marginal effect of adding hard liquor access points toward higher crash risk, though their estimates are also imprecise and statistically insignificant.

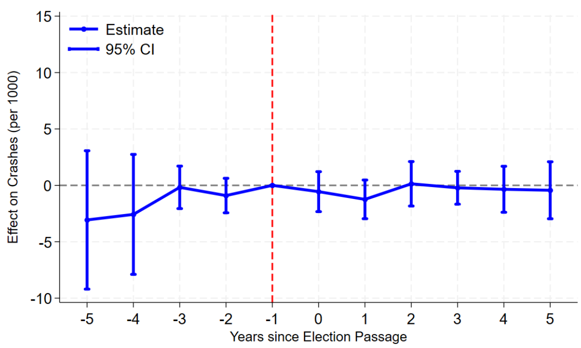
Together, these findings suggest that in our setting alcohol legalization mainly reallocates consumption of low-alcohol-content beverages from on-premise to off-premise venues, reducing the need to drive after drinking and thereby explaining the absence of any measurable increase in traffic crashes.



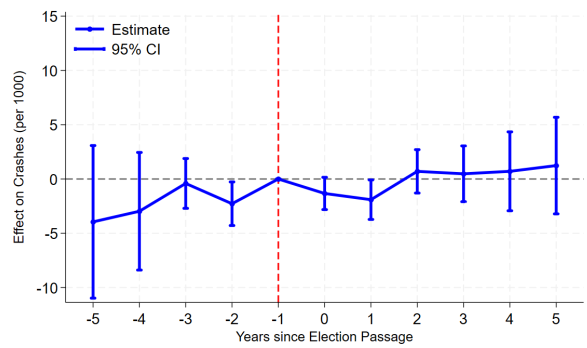
(a) Licenses — Beer & Wine



(b) Licenses — Hard Alcohol



(c) Crashes — Beer & Wine



(d) Crashes — Hard Alcohol

Figure 9: Stacked RD Estimates of Beer & Wine vs. Hard Alcohol Elections on Licenses (top) and Crashes (bottom)

Note: The figure presents coefficients and confidence intervals of the coefficient of election passage indicators in equation 2, following Biasi et al. (2025). The specification includes city×cohort and cohort×year fixed effects. We weight observations by baseline-year population for each cohort and cluster standard errors at the city level.

6 Discussion

This study examines how local liberalization of alcohol laws affects road safety in Texas municipalities. Using election variation and transitions from dry to wet status, we isolate causal effects of increased alcohol availability on both market structure and traffic outcomes. Across complementary research designs, our findings reveal a consistent pattern: legalizing alcohol sales leads to a large and sustained expansion in the number of liquor establishments—driven primarily by off-premise outlets such as liquor stores and grocery retailers—but has no measurable impact on total, DUI-related, or fatal traffic crashes.

The distinction between on-premise and off-premise alcohol access is central to understanding these results. Following legalization, off-premise establishments rise sharply while on-premise sales decline, suggesting a substitution from bar- or restaurant-based drinking to retail purchases for home consumption. This reallocation of alcohol access helps explain the absence of higher crash rates: when alcohol is purchased for off-premise consumption, the need to drive immediately after drinking is reduced.

The dynamic results reinforce this interpretation. Using staggered difference-in-differences and stacked RD designs, we find persistent increases in off-premise licenses and retail sales, but stable or slightly declining trends in on-premise activity and alcohol-related crashes. Moreover, our mechanism analysis — regressing crash outcomes on on- and off-premise license counts within a two-way fixed effects model — shows that on-premise licenses are positively associated with crashes, while off-premise licenses show no significant relationship with any crash category. This corroborates the interpretation that the effects of alcohol availability on public safety depend on where alcohol is consumed, not merely whether it is available.

These findings challenge the policy assumption that easier access to alcohol inevitably increases drunk driving and related harms. Instead, the results suggest that local liberalization primarily reshapes the geography of alcohol markets—expanding retail supply without increasing risky drinking behavior. From a policy perspective, this suggests that the safety consequences of alcohol liberalization depend on the type of access being expanded — a consideration that blanket wet/dry classifications obscure.

More broadly, this study highlights the value of leveraging fine-grained local variation to disentangle the mechanisms linking “sin good” regulation and public outcomes. Future research could build on these results by examining complementary channels, such as crime, health, or tax revenue, to provide a more comprehensive picture of the welfare implications of local alcohol policy.

References

D. Mark Anderson, Benjamin Crost, and Daniel I. Rees. Wet Laws, Drinking Establishments and Violent Crime. *The Economic Journal*, 128(611):1333–1366, June 2018. ISSN 0013-0133. . URL .

Reagan Baughman, Michael Conlin, Stacy Dickert-Conlin, and John Pepper. Slippery when wet: the effects of local alcohol access laws on highway safety. *Journal of Health Economics*, 20(6):1089–1096, November 2001. ISSN 0167-6296. . URL .

B. Douglas Bernheim, Jonathan Meer, and Neva K. Novarro. Do Consumers Exploit Commitment Opportunities? Evidence from Natural Experiments Involving Liquor Consumption. *American Economic Journal: Economic Policy*, 8(4):41–69, November 2016. ISSN 1945-7731. . URL .

Barbara Biasi, Julien Lafortune, and David Schönholzer. What works and for whom? effectiveness and efficiency of school capital investments across the us. *The Quarterly Journal of Economics*, page qjaf013, 2025.

Howard Bodenhorn. Blind Tigers and Red-Tape Cocktails: Liquor Control and Homicide in Late-Nineteenth-Century South Carolina, December 2016. URL .

Brantly Callaway and Pedro HC Sant'Anna. Difference-in-differences with multiple time periods. *Journal of econometrics*, 225(2):200–230, 2021.

Sebastian Calonico, Matias D. Cattaneo, and Rocio Titiunik. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326, 2014a. URL .

Sebastian Calonico, Matias D. Cattaneo, and Rocio Titiunik. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326, 2014b. . URL .

Sebastian Calonico, Matias D. Cattaneo, and Rocío Titiunik. rdrobust: An R Package for Robust Nonparametric Inference in Regression-Discontinuity Designs. *The R Journal*, 7 (1):38–51, 2015. . URL .

Christopher Carpenter. How do zero tolerance drunk driving laws work? *Journal of health economics*, 23(1):61–83, 2004. URL . Publisher: Elsevier.

Christopher Carpenter and Carlos Dobkin. The minimum legal drinking age and crime. *Review of economics and statistics*, 97(2):521–524, 2015. URL . Publisher: The MIT Press.

Christopher S. Carpenter and Daniel Eisenberg. Effects of Sunday Sales Restrictions on Overall and Day-Specific Alcohol Consumption: Evidence From Canada. *Journal of Studies on Alcohol and Drugs*, 70(1):126–133, January 2009. ISSN 1937-1888. . URL . Publisher: Alcohol Research Documentation, Inc.

Elena Castellari, Chad Cotti, John Gordanier, and Orgul Ozturk. Does the timing of food stamp distribution matter? a panel-data analysis of monthly purchasing patterns of us households. *Health Economics*, 26:1380–1393, 2017. . URL .

Matias D Cattaneo, Michael Jansson, and Xinwei Ma. Manipulation testing based on density discontinuity. *The Stata Journal*, 18(1):234–261, 2018.

Stephanie Riegg Cellini, Fernando Ferreira, and Jesse Rothstein. The value of school facility investments: Evidence from a dynamic regression discontinuity design. *The Quarterly Journal of Economics*, 125(1):215–261, 2010.

Aaron Chalfin, Benjamin Hansen, and Rachel Ryley. The Minimum Legal Drinking Age and Crime Victimization. *Journal of Human Resources*, 58(4):1141–1177, July 2023. ISSN 0022-166X, 1548-8004. . URL . Publisher: University of Wisconsin Press Section: Articles.

Emily Conover and Dean Scrimgeour. Health consequences of easier access to alcohol: New Zealand evidence. *Journal of Health Economics*, 32(3):570–585, May 2013. ISSN 0167-6296. . URL .

Chad Cotti, John Gordanier, and Orgul Ozturk. Eat (and drink) better tonight: Food stamp benefit timing and drunk driving fatalities. *American Journal of Health Economics*, 2(4): 511–534, 2016. URL .

Benjamin Crost and Daniel I. Rees. The minimum legal drinking age and marijuana use: New estimates from the NLSY97. *Journal of Health Economics*, 32(2):474–476, March 2013. ISSN 0167-6296. . URL .

Thomas S. Dee. State alcohol policies, teen drinking and traffic fatalities. *Journal of public Economics*, 72(2):289–315, 1999. URL . Publisher: Elsevier.

John DiNardo and Thomas Lemieux. Alcohol, marijuana, and American youth: the unintended consequences of government regulation. *Journal of health economics*, 20(6):991–1010, 2001. URL . Publisher: Elsevier.

Daniel Eisenberg. Evaluating the effectiveness of policies related to drunk driving. *Journal of Policy Analysis and Management*, 22(2):249–274, March 2003. ISSN 0276-8739, 1520-6688. . URL .

Angela R. Fertig and Tara Watson. Minimum drinking age laws and infant health outcomes. *Journal of Health Economics*, 28(3):737–747, May 2009. ISSN 0167-6296. . URL .

Marco Francesconi and Jonathan James. None for the Road? Stricter Drink Driving Laws and Road Accidents. *Journal of Health Economics*, 79:102487, September 2021. ISSN 0167-6296. . URL .

Marco Francesconi and Jonathan James. Alcohol Price Floors and Externalities: The Case of Fatal Road Crashes. *Journal of Policy Analysis and Management*, 41(4):1118–1156, 2022. ISSN 1520-6688. . URL . _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1002/pam.22414.

Sarah Lynn Schulte Gary, Lisa Aultman-Hall, Matt McCourt, and Nick Stamatiadis. Consideration of driver home county prohibition and alcohol-related vehicle crashes. *Accident Analysis & Prevention*, 35(5):641–648, 2003.

Robert A Hahn, Jennifer L Kuzara, Randy Elder, Robert Brewer, Sajal Chattopadhyay, Jonathan Fielding, Timothy S Naimi, Traci Toomey, Jennifer Cook Middleton, Briana Lawrence, et al. Effectiveness of policies restricting hours of alcohol sales in preventing excessive alcohol consumption and related harms. *American journal of preventive medicine*, 39(6):590–604, 2010.

Benjamin Hansen. Punishment and deterrence: Evidence from drunk driving. *American Economic Review*, 105(4):1581–1617, 2015. URL . Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Paul Heaton. Sunday liquor laws and crime. *Journal of Public Economics*, 96(1):42–52, February 2012. ISSN 0047-2727. . URL .

Thor Norström, Thor Norström, Mats Ramstedt, Thor Norström, and Mats Ramstedt and. Mortality and population drinking: a review of the literature. *Drug and Alcohol Review*, 24(6):537–547, 2005. . URL . PMID: 16361210.

Mats Ramstedt. Alcohol and fatal accidents in the united states—a time series analysis for 1950–2002. *Accident Analysis & Prevention*, 40(4):1273–1281, 2008. ISSN 0001-4575. . URL .

Mark F. Stehr. The Effect of Sunday Sales of Alcohol on Highway Crash Fatalities. *The B.E. Journal of Economic Analysis & Policy*, 10(1), August 2010. ISSN 1935-1682. . URL . Publisher: De Gruyter.

Baris K. Yörük and Jungtaek Lee. Did Legalization of Sunday Alcohol Sales Increase Crime in the United States? Evidence From Seven States. *Journal of Studies on Alcohol and Drugs*, 79(6):816–825, November 2018. ISSN 1937-1888. . URL . Publisher: Alcohol Research Documentation, Inc.

Barış K. Yörük and Ceren Ertan Yörük. The impact of minimum legal drinking age laws on alcohol consumption, smoking, and marijuana use: Evidence from a regression discontinuity design using exact date of birth. *Journal of health economics*, 30(4):740–752, 2011. URL . Publisher: Elsevier.

Esa L. Österberg. Alcohol tax changes and the use of alcohol in europe. *Drug and Alcohol Review*, 30(2):124–129, 2011. . URL .

Appendix

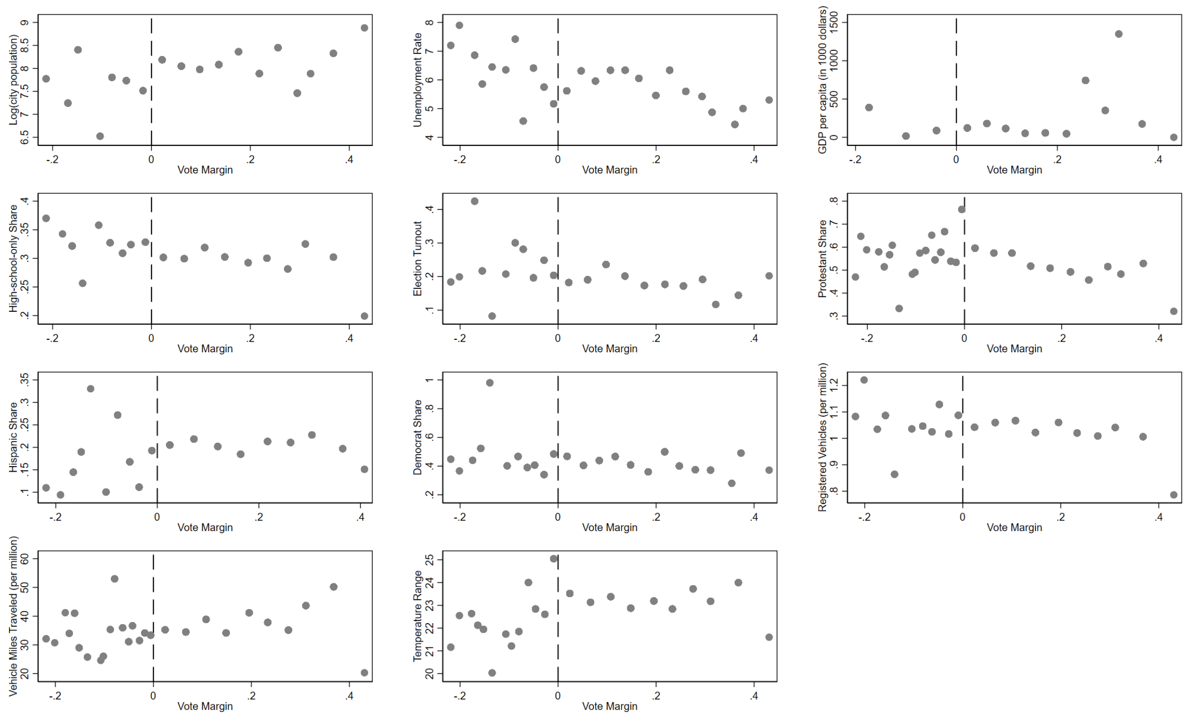
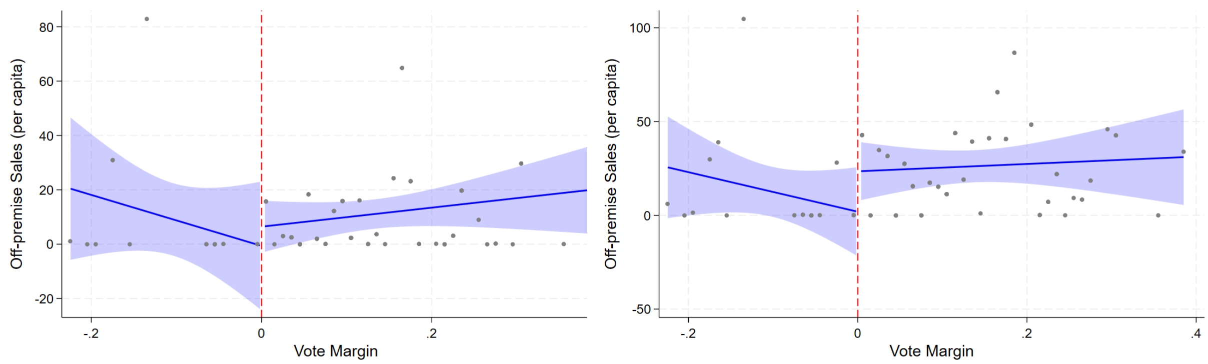


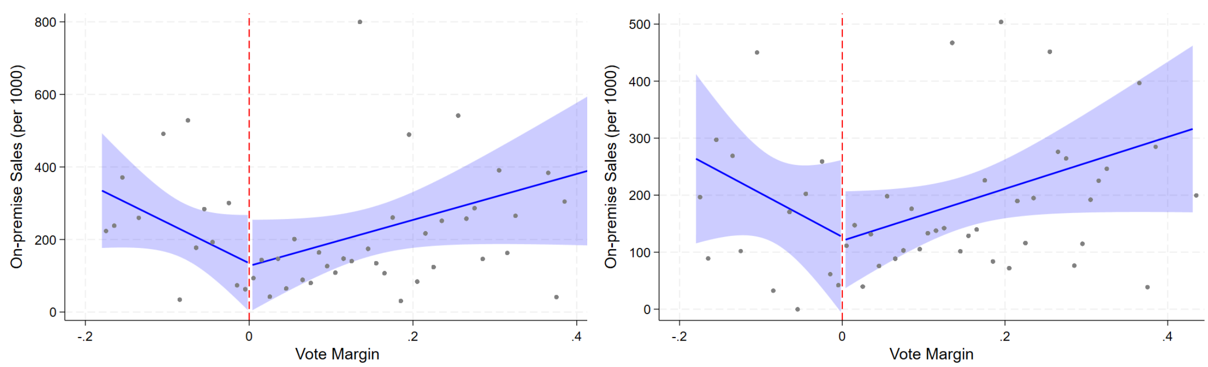
Figure A1: Covariate Balance Tests

Note: Each panel plots a pre-treatment covariate against the vote margin using binned means. No systematic breaks at the cutoff are evident, suggesting that treated and control cities are comparable near the threshold.



Event time $t = -1$ Event time $t = +1$

(a) Off-premise liquor sales

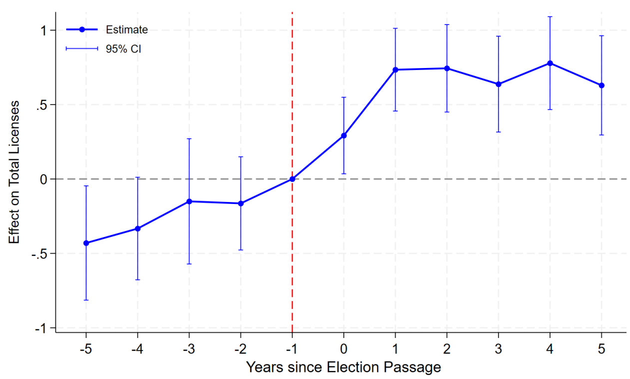


Event time $t = -1$ Event time $t = +1$

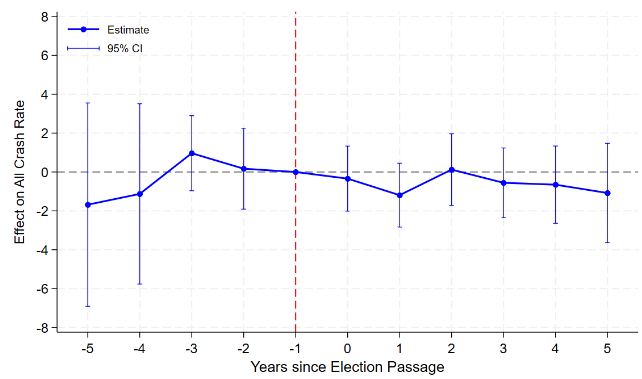
(b) On-premise liquor sales

Figure A2: Off- and on-premise liquor sales per 1,000 population (pre- vs. post-treatment)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before and after the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019.



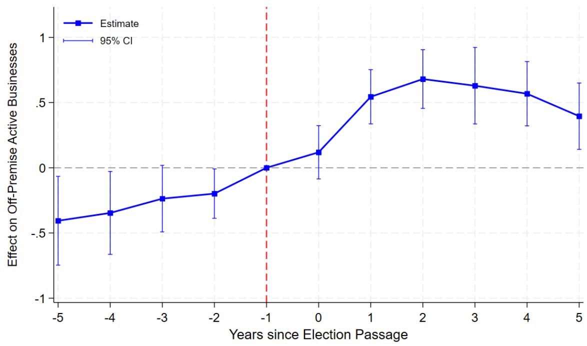
(a) All licenses



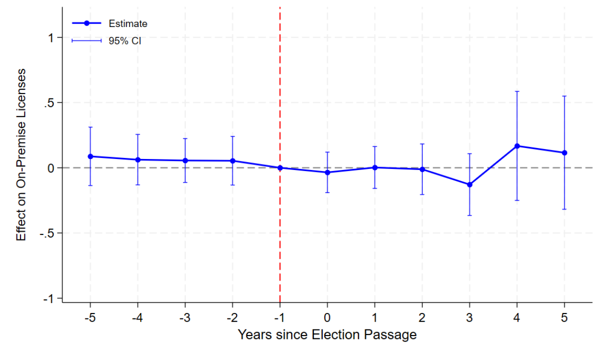
(b) All crashes

Figure A3: Stacked RD: all licenses and all crashes (per 1,000 population)

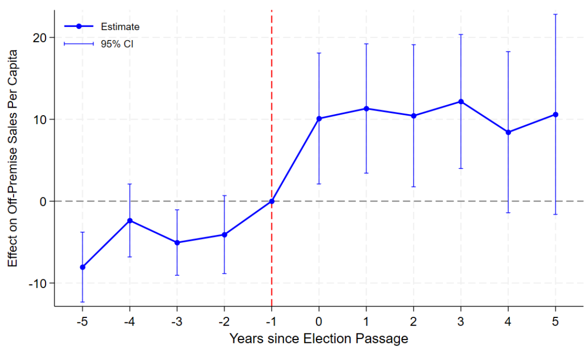
Note: The figure presents coefficients and confidence intervals of the coefficient of election passage indicators in equation 2, following Biasi et al. (2025). The specification includes city×cohort and cohort×year fixed effects. We weight observations by baseline-year population for each city and cluster standard errors at the city level.



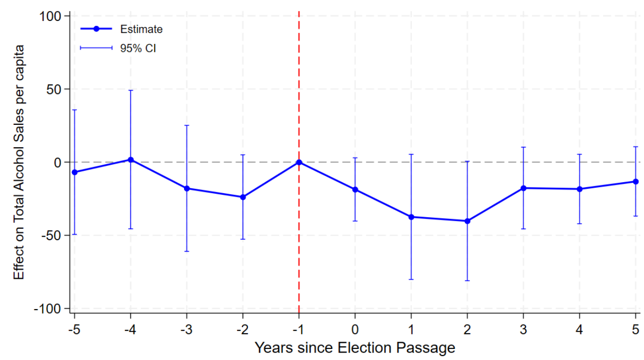
(a) Off-premise licenses



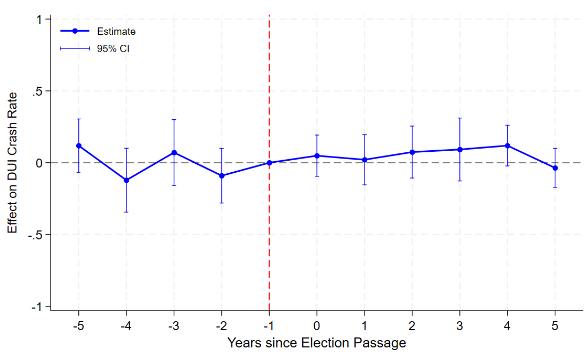
(b) On-premise licenses



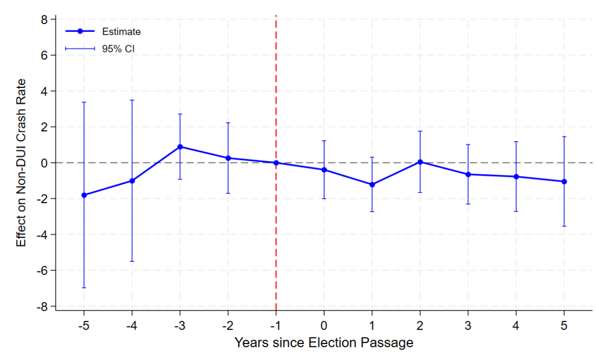
(c) Off-premise sales



(d) On-premise sales



(e) DUI crashes

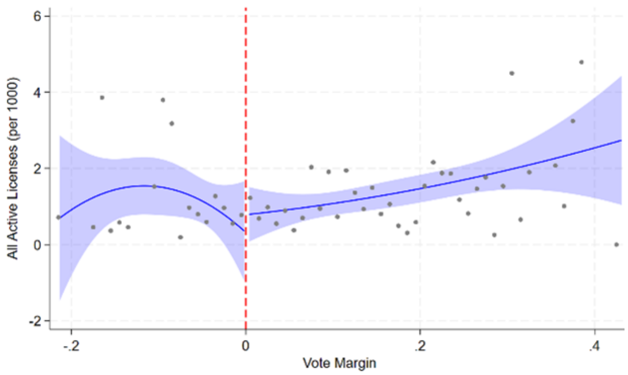


(f) Non-DUI crashes

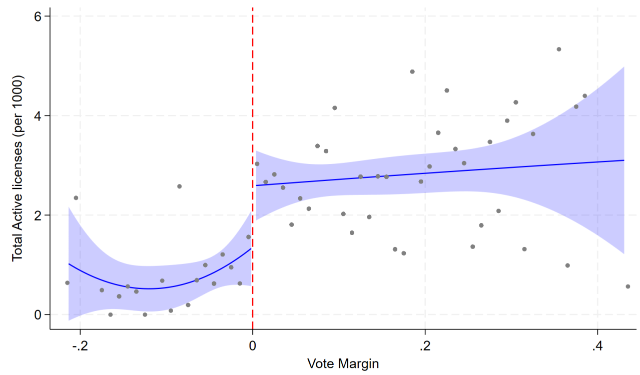
Figure A4: Stacked RD: licenses, sales, and crash outcomes (per 1,000 population)

Note: The figure presents coefficients and confidence intervals of the coefficient of election passage indicators in equation 2, following Biasi et al. (2025). The specification includes city×cohort and cohort×year fixed effects. We weight observations by baseline-year population for each cohort and cluster standard errors at the city level. For off-premise sales, data are available only at the county level; accordingly, the unit of analysis and clustering are at the county level.

The figures below present quadratic fitted lines from the regular regression discontinuity analyses.



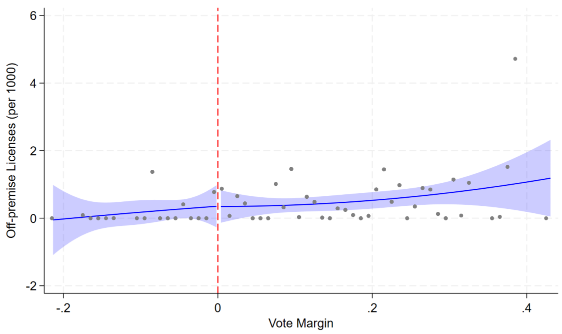
(a) Event time $t = -1$



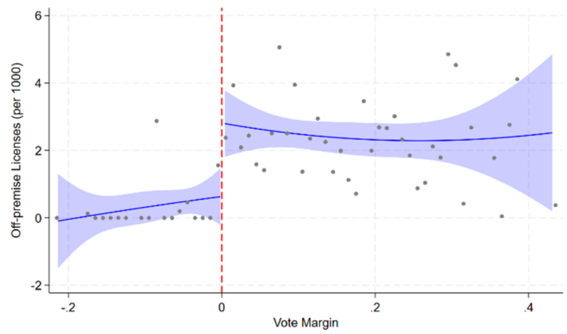
(b) Event time $t = +1$

Figure A5: All liquor licenses per 1000 population (pre-treatment vs. post-treatment)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before and after the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019.

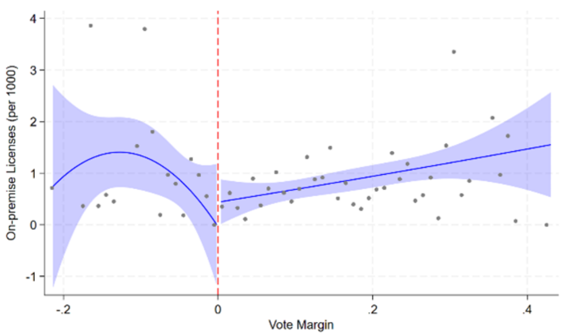


Event time $t = -1$

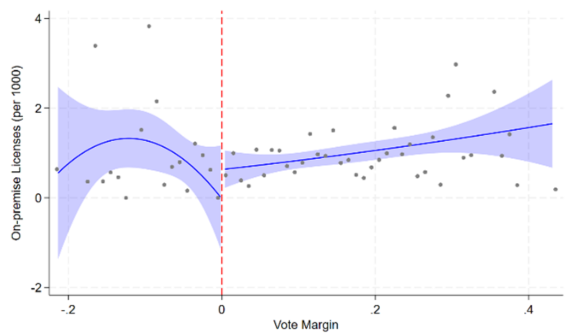


Event time $t = +1$

(a) Off-premise liquor licenses



Event time $t = -1$

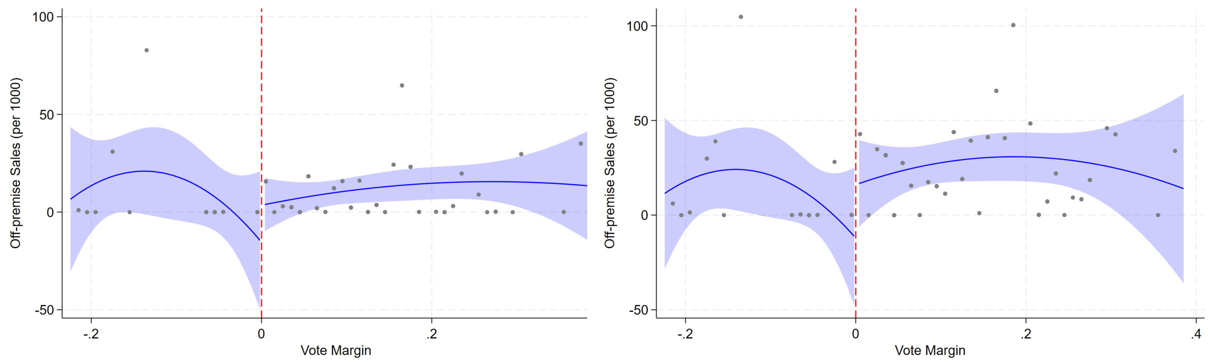


Event time $t = +1$

(b) On-premise liquor licenses

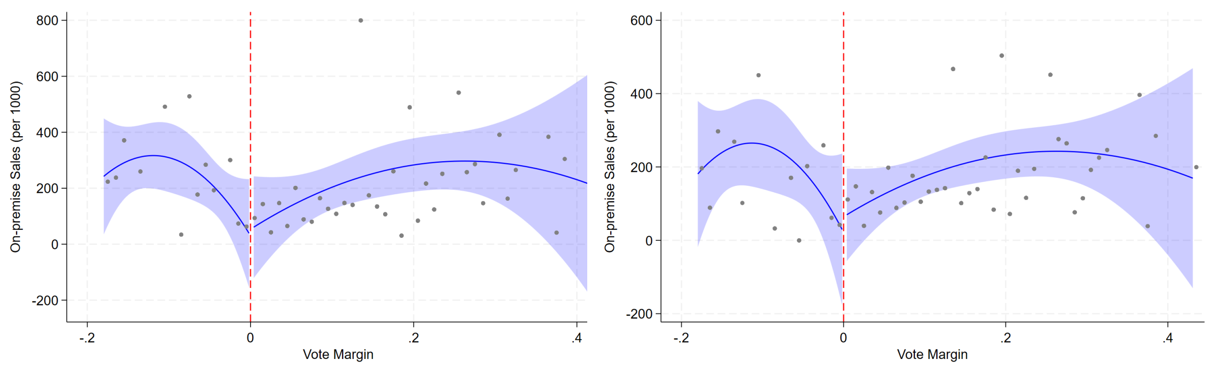
Figure A6: Off- and on-premise liquor licenses per 1,000 population (pre- vs. post-treatment)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before and after the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019.



Event time $t = -1$ Event time $t = +1$

(a) Off-premise liquor sales



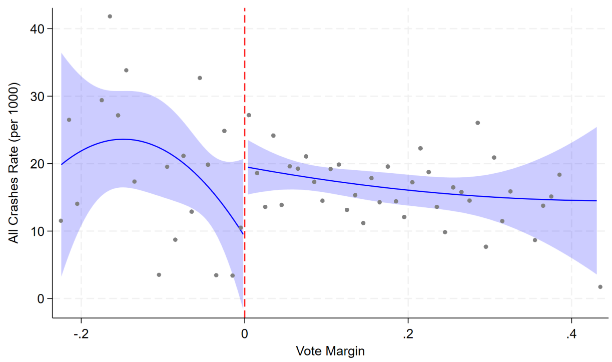
Event time $t = -1$ Event time $t = +1$

(b) On-premise liquor sales

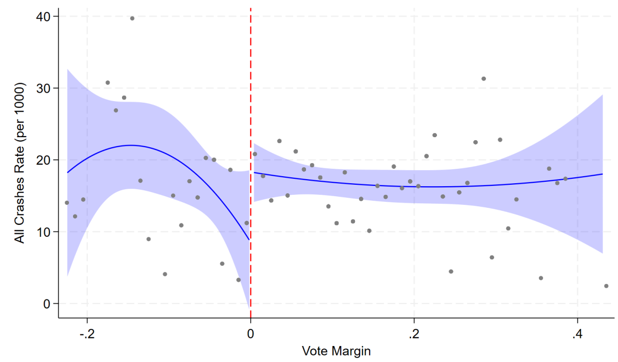
Figure A7: Off- and on-premise liquor sales per 1,000 population (pre- vs. post-treatment)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before and after the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019.

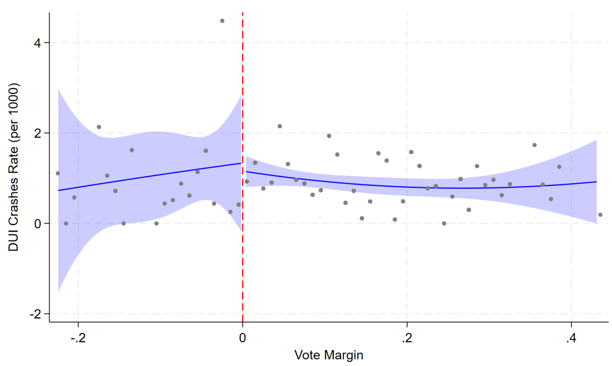
Event time $t = -1$ | **Event time $t = +1$**



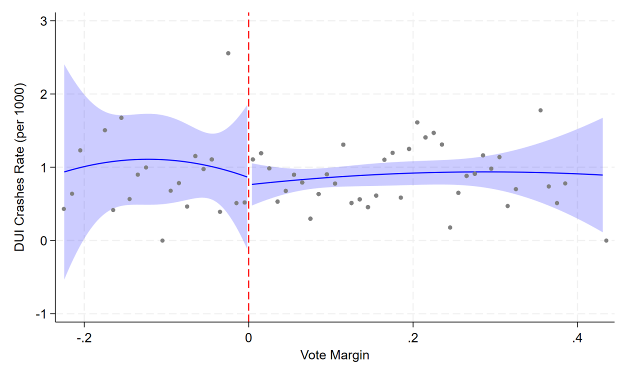
(a) All crashes



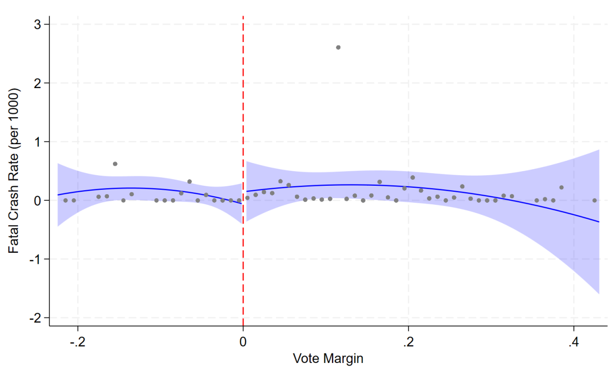
(b) All crashes



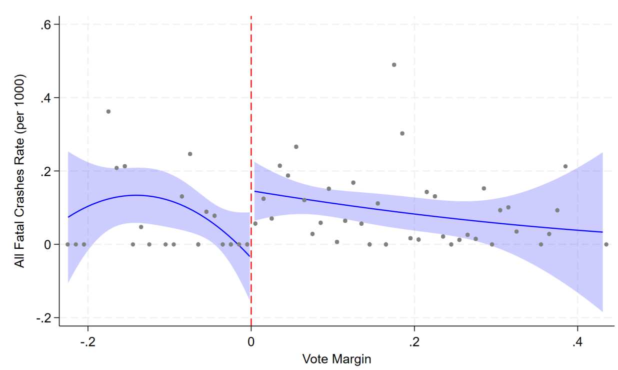
(c) DUI crashes



(d) DUI crashes



(e) Fatal crashes



(f) Fatal crashes

Figure A8: Crash outcomes per 1,000 population by event time ($t = -1, \ +1$)

Note: The figure shows binned averages of pre- and post-election outcomes by the win margin of passing elections. The outcomes are measured one year before and after the election. We consider all elections in dry cities that have not previously held an election between 2003 and 2019.

Table A1: Spillover Estimates by Distance Bin

	0–10 miles	10–20 miles	20–50 miles
Panel A. Spillovers to dry cities			
Licenses			
All licenses	0.199	0.856	1.690**
	(0.714)	(0.578)	(0.806)
Off-premise licenses	-0.040	0.213	0.400
	(0.696)	(0.545)	(0.649)
On-premise licenses	0.412**	0.596***	1.190***
	(0.205)	(0.164)	(0.389)
Sales			
On-premise sales	-5.650	-9.840	-10.200
	(10.300)	(10.500)	(18.900)
Off-premise sales (county)	-9.260	17.700	72.500**
	(18.900)	(19.700)	(31.300)
Crashes			
All crashes	0.184*	-0.083	-0.312
	(0.106)	(0.139)	(0.250)
DUI crashes	0.184*	-0.092	-0.303
	(0.106)	(0.139)	(0.251)
Fatal crashes	0.018	0.041***	0.040
	(0.013)	(0.015)	(0.027)
Panel B. Spillovers to wet cities			
Licenses			
All licenses	-0.619	-1.010	-2.030
	(1.080)	(1.230)	(1.560)
Off-premise licenses	-0.732	-1.310	-2.350*
	(1.070)	(1.200)	(1.420)
On-premise licenses	0.384	0.290	0.222
	(0.300)	(0.237)	(0.585)
Sales			
On-premise sales	-5.530	5.200	8.980
	(5.570)	(4.010)	(9.430)
Off-premise sales (county)	11.100	8.860	24.900*
	(9.990)	(7.430)	(13.700)
Crashes			
All crashes	0.049	0.119	-0.116
	(0.110)	(0.114)	(0.341)
DUI crashes	0.044	0.098	-0.107
	(0.111)	(0.116)	(0.343)
Fatal crashes	0.016	0.035**	0.045
	(0.011)	(0.016)	(0.031)

Notes: Each cell comes from a separate regression (Equation 6) including own wet status, one bin-specific spillover exposure, and their interaction. Panel A reports the spillover coefficient for dry cities; Panel B reports the linear combination of exposure and interaction coefficients for wet cities. Exposure is the share of neighboring cities in the indicated distance bin that are wet. License and crash outcomes are per 1,000 population; sales are per-capita dollars. Off-premise sales are county-level, all other outcomes are city-level. Sample excludes always-dry cities with no elections. All regressions include city/county and year fixed effects; standard errors clustered at city or county level.