The Impact of Local Liquor Sales Restriction on Birth Outcomes and Alcohol-related Crimes in Texas

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March 28, 2021

Abstract

After the repeal of National Prohibition in 1933, 30 states gave counties and municipalities the local option to continue alcohol restrictions. Citizens set alcohol control policies in their communities through jurisdiction-wide elections (i.e., local option elections). Currently, 10% of U.S. communities maintain a ban on some or all alcohol sales. Assessing the impact of local access to alcohol on alcohol-related outcomes such as birth weight, drinking under the influence, alcohol-related crimes, and so on is complicated by the potential non-random selection of liquor laws. I examine the causal effects of local access to alcohol on birth outcomes by comparing municipalities where referenda on legalizing liquor sales passed and failed by narrow margins. My results indicate that municipalities which were studied experienced higher incidence of low birth weight after legalizing the local sale of alcohol to the general public. The incidence of low birth weight rose by 4.5% for babies born within two years after the elections.

*Department of Economics, Fordham University (email: msun46@fordham.edu). I would like to thank Brendan O'Flaherty, Pietro Ortoleva, and Suresh Naidu for invaluable guidance, assistance and advice. I thank Jushan Bai, Alessandra Casella, Donald Davis, Mark Dean, Francois Gerard, Wojciech Kopczuk, Jonah Rockoff, and Miikka Rokkanen for discussions and comments that shaped the content of this paper. I thank Laxman Gurung, Sun Kyoung Lee, Xuan Li, Janis Priede, Tuo Chen, and Danyan Zha for their help. All errors are my own.

1 Introduction

In the past four decades, one of the major goals of US public policy has been to reduce the health, safety, and criminal risks associated with alcohol abuse. A number of policies are designed to restrict the alcohol consumption. Some, such as the minimum legal drinking age (MLDA), and preliminary-breath-test laws, have become noticeably more stringent in the past 30 years. An assumption underlying these social policies is that alcohol-related problems and behaviour can be controlled by restricting the availability of alcoholic beverages. An extensive empirical literature focuses on the impact of these polices on alcohol-related outcomes such as motor vehicle accidents, Driving Under the Influence, and violent crimes. For example, studies suggest that increases in federal or state excise taxes on alcohol discourage heavy drinking and reduce motor vehicle fatalities (Coate and Grossman 1988, Grossman et al. 1993, Laixuthai and Chaloupka 1993, Mullahy and Sindelar 1994). Research also concludes that changes in minimum drinking age law significantly reduced alcohol-related accident fatalities. Ironically, while these laws were being tightened, local alcohol control policies at city and county levels were relaxed in many parts of the country. After the repeal of Prohibition in 1933, 34 states passed local option laws, where county and municipal governments govern alcohol policies in terms of alcohol availability and distribution, mostly by popular vote.

Restricting alcohol availability at the local level may be a plausible prevention strategy for several reasons. Light to moderate drinkers, rather than alcoholics, are believed to contribute disproportionately to a community's alcohol-related problems (Bruun et al. 1975, Room 1984). Curbing all community members' alcohol consumption, not just consumption by the heaviest alcohol abusers, may decrease alcohol-related problems (Rose 1992). However, as local alcohol restrictions generally limit the availability of alcohol, these law changes also significantly increase the travel distance required to obtain the alcohol, change where the alcohol is consumed and change the type of alcohol consumed. Hence, the overall effect of local alcohol access policies is ambiguous.

In 26 states, alcohol restrictions differ across counties and municipalities (Alabama, Alaska, Arkansas, Florida, Georgia, Illinois, Kansas, Kentucky, Massachusetts, Michigan, Minnesota, Mississippi, Nevada, New Hampshire, New Jersey, New York, North Carolina, Ohio, Oregon, Pennsylvania, South Dakota, Tennessee, Texas, Virginia, Washington, and Wisconsin). A dry community is a community whose government forbids the sale of alcoholic beverages. A wet community is a community with no restrictions on local liquor sales. A moist community is a community on the “middle ground” between a dry community and a wet community. A moist community allows alcohol to be sold in certain situations, but has limitations on alcohol sales that a normal "wet" community would not have. Some prohibit on-premises sale, some prohibit off-premises sale, and some prohibit both.

The only option for residents of dry areas is to go to areas without alcohol restrictions, which can entail large distances and associated time costs, or risk incarceration through the illegal production of alcohol. Many dry communities do not prohibit the mere consumption of alcohol, which could potentially cause a loss of profits and taxes from the sale of alcohol to their residents in “wet” areas. Their main argument against alcohol sales is that unrestricted alcohol consumption in wet status may cause social and moral degradation, which in turn would lead to an increase in crime.

Local alcohol control policies have received much less attention from the mainstream media and have been examined less frequently in the literature. Currently, there are thousands of dry communities across the United States with about 18,000,000 people in the 10% of the area of the US that is dry (Hanson 2000). Almost one-half of the counties in Mississippi are dry with their own prohibition against the production, advertising, sale, distribution, or transportation of alcoholic beverages within their boundaries (Hanson 2000). As local prohibition affects a large portion of population living in dry areas, most of which with disadvantaged economic status, study of this policy is critical for local social outcomes such as alcohol-related accidents, alcohol-involved violent crimes, as well as maternal drinking during pregnancy.

This paper makes two contributions to the existing literature. The primary innovation of this paper is to examine the causal effect of local alcohol access (i.e., “wet” communities) on alcohol-related outcomes. Local option elections determine the types of alcoholic beverages which may be sold and how they can be sold by counties, cities, or individual justice of the peace precincts.

With universal 50% winning voting share threshold, close losers and close winners of local option elections provides quasi-random variation in winner status that can be used to overcome the endogeneity of local alcohol sales restrictions, since for narrowly decided races the outcome is unlikely to be correlated with other distinct characteristics as long as there is some unpredictable component of the ultimate vote.

Second, while the existing literature of local prohibition focuses on outcomes such as alcohol-related crimes and vehicle crashes, this paper examines the effect on birth outcomes, especially birth weight. Birth weight is the single most important indicator of infant health. It is a significant predictor of infant mortality and morbidity and of health and learning disabilities in later life. (see Currie and Cole [1991] for references). Babies born with low birth weight are more likely than babies born at a normal weight to have health conditions, including respiratory distress syndrome and bleeding in the brain as newborns and diabetes and obesity later in life. When a community goes from "dry" to "wet", there may be a higher risk of maternal drinking during pregnancy since buying liquor locally becomes easier. Drinking alcohol during pregnancy can result in embryonic developmental abnormalities such as low birth weight.

Table 1: Some Statistics of Dry versus Wet Communities in Texas

Variable	Dry Communities		Wet Communities	
	mean	*st dev*	*mean*	*st dev*
Per Capita Income	21.9	(4.6)	22.06	(6.16)
Percent Hispanic Pop	0.06	(0.10)	0.13	(0.20)
Percent Baptists	0.59	(0.14)	0.48	(0.20)
Percent Catholics	0.06	(0.08)	0.15	(0.19)
Pop Density (per sq mi)	78.4	(62.5)	106.21	(218.40)
Police Expend (000s)	3.1	(4.1)	9.11	(37.10)

2 Data

In this paper, I study the link between local restrictions on liquor sales and birth outcomes among local residents. I rely on two broad sources of data to identify: (1) date and location of local option elections as well as the number of vote for and against the issues voted on; (2) newborns' characteristics such as birth weight and mother's characteristics; and (3) .

2.1 Local Option Elections

The first source of data (and the reason I focus on Texas) is annual local option election data drawn from Texas Alcoholic Beverage Commission (TABC) Annual reports. The data include date of elections, alcoholic issues that were voted for, city and/or county of the election, number of vote for and against the issues, required vote shares for passage of the issues, and dry/wet status before and after the election. Our sample includes local option elections in counties and municipalities in Texas between 1979 and 2003.

Table 1 describes the number of local option elections observed between 1979 and 2003. Over this 24-year-period, there were 310 referenda, all trying to go from dry to wet. In total, 87 of the 254 counties in Texas were dry at the start of the period in 1975, and 33

Figure 1 shows the yearly number of local option elections between 1979 and 2003. Although there were elections about alcohol access laws over the entire period, the majority of elections took place before 1990 and after 2000.

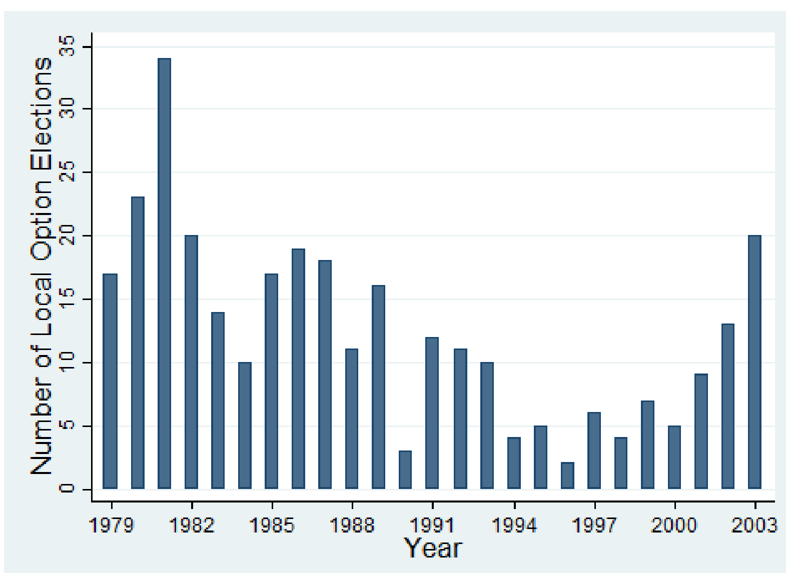


Figure 1: Number of Referenda By Year, 1979-2003 (Total 310)

2.2 Vital Statistics Records

The Vital Statistics Records, collected by Texas Department of State Health Services, correspond to 9.6 million birth certificates filed in hospitals within the 5,513 municipalities in Texas from 1979 to 2003. The final analysis of this paper will use a panel of newborns appearing within two years before and after the local option elections, reducing the sample to 279,270 birth records. The average birth weight in Texas for the period of study was 3,324 grams, and 7.04 percent of births were low birth weight.

The working dataset merges Vital Statistics Records with local option election data by mother's municipality of residence. Further details are given in a data appendix available from the authors on request. Data on birth weight are comparatively accurate.

In general, the election outcome may be correlated with other municipal characteristics that influence spending, so E[ujbj] = . If so, a simple regression of y_j on b_j will yield a biased estimate of .

3 Model

Suppose that municipality j considers a local option election to legalize the sale of alcoholic beverages and that this proposal receives vote share v_j (relative to the required threshold v^*). Let $b_j = 1(v_j \geq v^*)$ be a dummy indicator for local alcohol access. Suppressing time-related considerations, we can write some outcome y_j (incidence of low birth weight, for example) as

$$y_j = \alpha + b_j\beta + \mu_j,$$

where β is the causal effect of local alcohol access and μ_j represents all other determinants of the outcome (with E[μ_j]=0).

In general, the election outcome may be correlated with other municipal characteristics that influence local alcohol consumption and alcohol-related outcomes (i.e., E[$\mu_j b_j$] $\neq 0$). If so, a simple regression of y_j on b_j will yield a biased estimate of β. However, as Lee (2008) points out, as long as there is some unpredictable random component of the vote, a narrowly decided election approximates a randomized experiment. In other words, the correlation between the election outcome and unobserved municipal characteristics can be kept arbitrarily close to zero by focusing on sufficiently close elections.

Therefore, one can identify the causal effect of measure passage by comparing municipalities that barely passed an election (the "treatment group") with others that barely rejected an election (the "control group"). That is, if v_j is the vote share and v^* is the threshold required for passage, unobserved local characteristics μ_j may vary with v_j but should be similar for municipalities with $v_j = v^* + \epsilon$ and $v_j = v^* - \epsilon$ (for small ϵ). Formally, the required assumption is that E[μ_j|v_j] is continuous at $v_j = v^*$. Now we can write

$$E[y_j|v_j] = \alpha + E[b_j|v_j]\beta + E[\mu_j|v_j] = \alpha + b_j\beta + E[\mu_j|v_j],$$

The assumed continuity of $E[\mu_j|v_j]$ at v^* implies that:

$$\lim_{x\uparrow v^*} E[y_j|v_j] - \lim_{x\downarrow v^*} E[y_j|v_j] = \beta$$

I focus on an implementation of the RD strategy that involves approximating the regression functions above and below the cutoff by means of weighted polynomial regressions with weights computed by applying a kernel function on the distance of each observation's score to the cutoff. I present these kernel-based estimator using alternative bandwidth selectors and polynomial orders, as well as bias-corrected estimates per Calonico et al (Forthcoming) (henceforth CCT).

To implement this, I begin by identifying each (j, t) combination with an election. I then select observations from municipality j in months t-24 through t + 24. Where a municipality has multiple elections in the same month, the same calendar month observation is used more than once. For example, if a municipality had elections in and 1997, the [t - 2, t + 6] windows are [1993, 2001] and [1995, 2003], respectively, and the 1995-2001 observations are included in each. Observations in the resulting data set are uniquely identified by the district, j, the date of the focal elec- tion, t, and the number of years elapsed between the focal election and the time at which the outcome was measured, r. We use this sample to estimate the following regressio

Specifically, assuming a homogeneous effect of the local option election passage on birth outcomes with universal 50% voting share for winning:

$$Y_{ijt} = \alpha + \gamma \mathbb{1}\{v_j \geq v^*\} + a(t) + X_{ij} + \mu_{ij}$$

where i indexed individuals and j indexed municipalities, Y_{ij} denoted the outcome of interest (for example, low birth weight) for individual i in city j, v_j was the voting share for local option election in city j, v^* was the distinct voting share for winning a local option election (i.e., 50%), a(.) is a flexible function of voting shares, X is a set of controls including gender and month-of-year fixed effects, and $E(\mu_{ij}|v_j) = 0$.

4 EVALUATING THE LOCAL OPTION REFERENDUM QUASI-EXPERIMENT

Our empirical strategy is to use close elections to approximate a true experiment. This requires that XXX be as good as randomly assigned, conditional on having a close election. In this section, we consider tests of this assumption. We also demonstrate that legalizing the local sale of alcohol in fact leads to decreased birth weight in subsequent birth cohorts.

4.1 Balance of Treatment and Control Groups

I examine three diagnostics for the validity of the RD quasi-experiment, based on the distribution of vote shares, preelection differences in mean characteristics, and differences in preelection trends. Tests of the balance of outcome variable means and trends before the election are possible only because of the panel structure of our data and provide particularly convincing evidence regarding the approximate randomness of measure passage.

Figure 2 shows a histogram of vote shares for local option elections among municipalities in Texas from 1979 to 2003. Discontinuous changes in density around the threshold can be an indication of endogenous sorting around this threshold, which would violate the RD assumptions (McCrary 2008). I see no evidence of such changes. In Figure 3, I follow McCrary (2008) and plot a discontinuous density function around the threshold (50% vote share). The figure demonstrates that the density just to the left of the cutoff is statistically indistinguishable from the density just to the right of the cutoff.

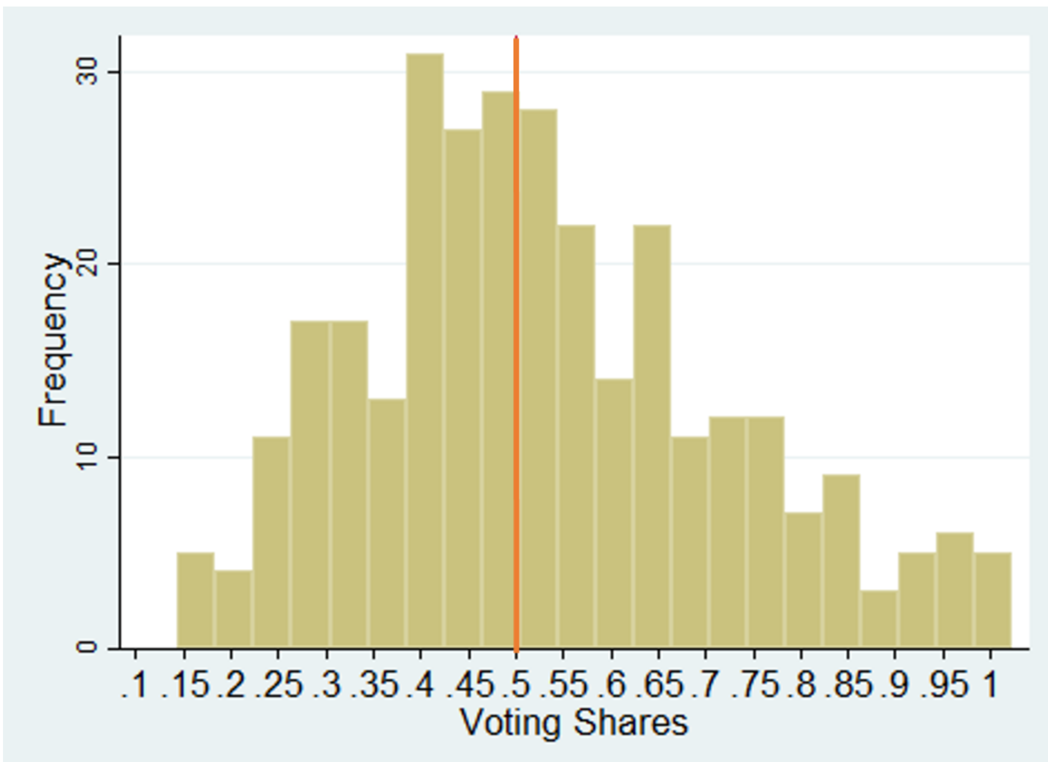


Figure 2: Histogram of Voting Shares in Local Option Elections, 1979-2003

Figure 5 presents graphical analyses of mean municipal incidence (probability) of low birth weight by the margin of victory or defeat, for births in 1-12 months (left panel) and in 13-24 months (right panel) before the election. I show average outcomes in one-percentage-point bins defined by the vote share relative to the threshold. Thus, the leftmost point represents measures that failed by between nine and ten percentage points, the next measures that failed by eight to

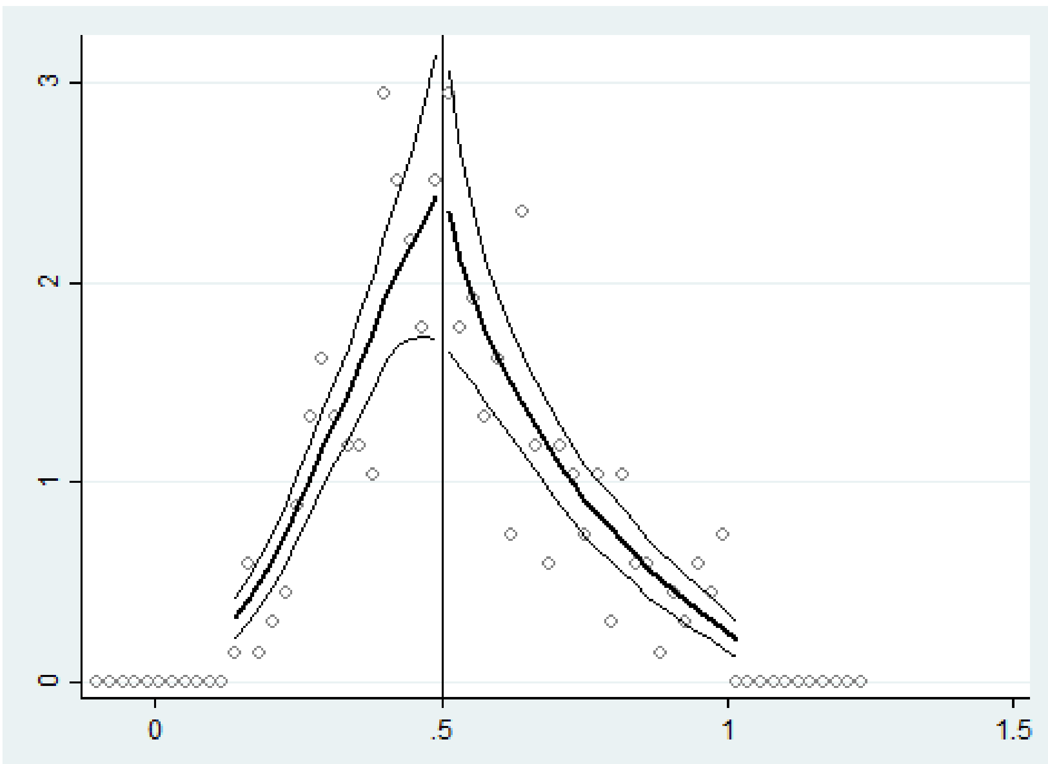


Figure 3: McCrary's Test for Discontinuity around threshold (50%)

Note: The x-axis represents the vote share. The y-axis represents the density. Solid lines are estimates and the dashed lines represent the 95% confidence interval around the estimate.

nine points, and so on. As expected, there is no sign of a discontinuity in probability of low birth weight for newborns before the election.

Columns (1)–(2) of Table III present regressions of birth weight, mother's marriage status, and percentage of children born by mothers below age 21 variables measured in the year before alcohol referendum, on an indicator for whether the local option election was passed. The specifications are estimated from a sample that includes only observations from the year before the election. The first column controls for year effects and the required threshold. Like Table II, it reveals large premeasure differences in several outcomes. The second column adds a quadratic polynomial in the measure vote share. Comparing communities that barely passed an election with communities that barely failed eliminates the significant estimates, shrinking two of the point estimates substantially.

Columns (3)–(4) in Table III repeat my two first specifications, taking as the dependent variable the value in each outcome in the year t 2 (i.e., in two years before alcohol referendum). Although the model without controls shows some differences in trends between communities that pass and fail measures, these are eliminated when we include controls for the vote share.

Overall, there seems to be little cause for concern about the approximate randomness of the measure passage indicator in our RD framework. Once I control for a quadratic in the measure vote share, measure passage is not significantly correlated with pretreatment trends of any of the outcomes I examine. Further, in similar specifications (not reported in Table III), I find no evidence of "effects" on portion of infants who were born by mothers under the age of 21, portion of single mothers, or other covariates.

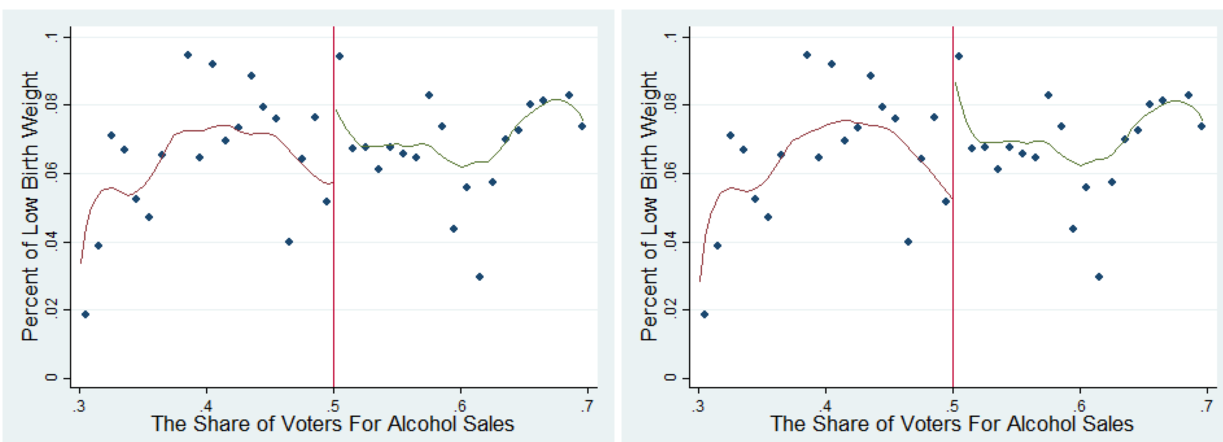
4.2 Intent-to-Treat Effects on Birth Weight

Figures present graphical analyses of mean municipal incidence (probability) of low birth weight by the margin of victory or defeat, for births in 1-9 months (left panel) and in 1-12 months (right panel) after the election. For municipalities where the measure just failed, there was no significant changes in the incidence of low birth weight. By contrast, after the election, municipalities where the measure just passed saw XXX (check the word). It is important to note that this result is obtained without adopting any controls. That is, I am only analyzing a simple difference between pre and post-election. Notice, however, that as the election becomes less random (i.e. the local option election wins by a margin of more than 5 percent), the change in the incidence of low birth weight returns to zero. This highlights the importance of using quasi-experimental methodology to deal with endogeneity.

Table 6 reports the results of my regression discontinuity estimates, which are consistent with the information displayed in Figure 3: For close winners, the changes in local restriction on local sales (i.e., going from “dry” to “wet”) highered the incidence of low birth weight by 3.5% and 4.2% for babies born within 10 months and within 12 months after the elections, respectively. The incidence of low birth weight rised by 4.5% for babies born 13-24 months after the elections. These results are most consistent with the Alesina et al. (1999) argument that

Figure 4

(a) 1-9 Months After (b) 1-12 Months After



Note:

Table 2: RD Estimates of the Local Alcohol Access on Birth Weight

Birth Weight	**Before the Elections**			
	(13-24 Months)		**(1-12 Months)**	
	Bandwidth	**Coefficient (std. error)**	**Bandwidth**	**Coefficient (std. error)**
Threshold				
2500g	0.110	0.016	0.102	0.020
		(0.015)		(0.014)
2250g	0.111	0.006	0.106	0.008
		(0.010)		(0.011)

Dependent Variable: binary indicator of birth weight. Robust standard errors in parentheses. The bandwidths are obtained by a variant of the Stata package described in Calonico et. al (2014).

*Note: - 0.1 * p < 0.05, ** p < 0.01, *** p < 0.001*

Table 3: RD Estimates of the Effects of Local Option Elections on Mother's age at Birth Delivery

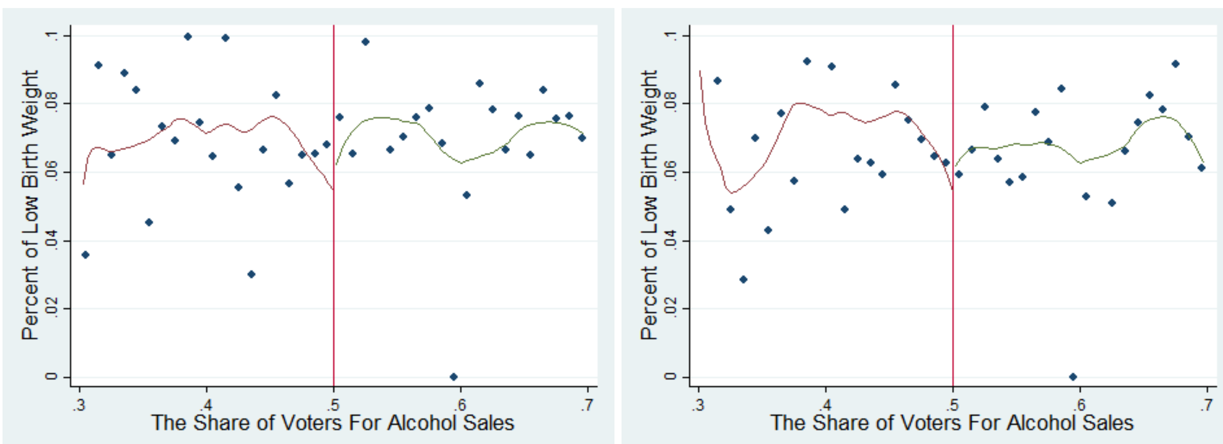
	12 Months Before the Vote		**Within 10 Months After the Vote**		**11-19 Months After the Vote**	
	Bandwidth (1)	**Coefficient (std. error) (2)**	**Bandwidth (3)**	**Coefficient (std. error) (4)**	**Bandwidth (5)**	**Coefficient (std. error) (6)**
Pass of Vote						
(0-vote share<50%	0.071	–.596	0.055	–.044 52	0.094	–3.0605***
1-vote share>=50%)		(.472 36)		(.588 19)		(.612)
Month of year FE		Yes		Yes		Yes
Observations		72762		52985		15580

Dependent Variable: Mother's age at birth delivery. Robust standard errors in parentheses. The bandwidths are obtained by a variant of the Stata package described in Calonico et. al (2014).

*Note: - 0.1 * p < 0.05, ** p < 0.01, *** p < 0.001*

Figure 5

(a) 13-24 Months Before (b) 1-12 Months Before



Note: Graph shows average probability of low birth weight for births in 1-12 months (left panel) and in 13-24 months (right panel) before the local option election, by the vote share. Local Option elections are grouped into bins one percentage point wide: measures that passed by between 0.001% and 1% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin.