A Tale of Two Policies: Alcohol Restrictions and Public Safety*

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Abstract

This paper explores the effects of two recent alcohol policy changes in Austin, Texas, on crime and traffic accidents. The first policy, implemented statewide, expanded the permitted hours for Sunday alcohol sales, allowing sales to begin at 10:00 AM instead of 12:00 PM. The second policy lifted restrictions on public drinking in specific neighborhoods in East Austin. Using a difference-in-discontinuity (DRD) model for the first policy, this paper finds no significant effects on either traffic accidents or crime. For the second policy, a difference-in-differences (DiD) model suggests that allowing public drinking led to substantial reductions in violent and public order crimes, with no increase in traffic crashes. These findings challenge conventional assumptions about public drinking, suggesting that liberalized alcohol restrictions may not compromise public safety.

Keywords: Alcohol Policy, Crime, Regression Discontinuity, Difference-in-Differences, Public Safety

JEL: K14, I18, R41

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1 Introduction

Dating back to the Prohibition era of the 1920s, the role of government in addressing excessive alcohol consumption has been a subject of enduring controversy. While some view the Prohibition as a cautionary tale that underscores the dangers of overreach in regulating alcohol consumption, others regard it as a public health success (Thornton, 1991; Blocker, 2006). Even economic evaluations of the Prohibition offer mixed conclusions. Miron (1999) estimated that the Prohibition increased homicide rates by 25–75 percent, whereas Livingston (2016) found evidence of a short-term decline in homicides with no long-term effects (Miron, 1999; Livingston, 2016).

Despite debates about the optimal role of government, some intervention to mitigate the harms of excessive alcohol use is widely seen as necessary. Alcohol is implicated in 33 percent of all criminal offenses, 60 percent of murders, and between 30 and 90 percent of rapes, with offenders often intoxicated at the time of the crime (Rand et al., 2010; Dilulio Jr., 1996). Excessive alcohol consumption also contributes to 79,000 deaths annually in the United States and incurs economic costs of $223.5 billion, primarily from productivity losses, healthcare expenses, and criminal justice expenditures (Bouchery et al., 2011). Nevertheless, 19 percent of Americans who drink alcohol admit to consuming more than they believe they should, highlighting a significant disconnect between awareness of harm and behavioral change (Schaeffer, Hoynes and DeSilver, 2024).

Mechanistically, alcohol consumption disrupts decision-making and impairs impulse control, driven by neurochemical imbalances in brain regions responsible for working memory and executive function (Sullivan, Harris and Pfefferbaum, 2010; Brevers et al., 2014). In light of these risks, policymakers have implemented a range of strategies to regulate alcohol consumption, from outright bans to more targeted interventions.

A common regulatory approach involves limiting the time and location of alcohol sales. For example, in Germany, banning late-night off-premise alcohol sales reduced hospitalizations by 7 percent among adolescents and 6 percent among young adults (Marcus and Siedler, 2015). Furthermore, in Virginia, permitting the sale of package liquor on Sundays led to a 10 percent increase in alcohol-related Group A crimes (Heaton, 2012). Similarly, in Kansas, a 10 percent increase in locations licensed to sell alcohol was associated with a 3 to 5 percent rise in violent crime (Anderson, Crost and Rees, 2018). An outright alcohol ban in Australia yielded significant reductions in non-natural mortality, rape, and homicides (Barron et al., 2024).

However, the effects of alcohol policies are often highly context-dependent. In Texas, for instance, counties transitioning from "dry" to "wet" (allowing alcohol sales) experienced a reduction in drug arrests of 9 to 30 percent, suggesting that alcohol and other drugs may act as substitutes (Conlin, Dickert-Conlin and Pepper, 2005). Moreover, the allowance of beer and wine sales in Texas reduced traffic accidents by 8 percent, likely due to shorter travel distances for alcohol purchases (Baughman et al., 2001). These heterogeneous effects underscore the need to evaluate the consequences of specific alcohol interventions in order to avoid unintended consequences. This paper examines the effects of two such alcohol policy changes in Austin, Texas, on crime and traffic accidents.

The first policy, implemented statewide, allowed alcohol sales on Sundays to start two hours earlier, shifting the permitted time from 12:00 PM to 10:00 AM. To estimate the effects of the policy, I utilize a difference-in-regression discontinuity model, comparing crimes and traffic accidents around the 10:00 AM cutoff before and after the policy change. Besides utilizing a novel source of variation in the crime literature, this approach extends the placebo-based tuning method that Goldin et al. (2024) proposed to the crime context. Using this methodology, I find no evidence of decreased crimes or vehicle crashes.

The second policy lifted restrictions on public drinking in certain East Austin neighborhoods (designated areas 2, 4, and 6) while maintaining public drinking restrictions in other designated areas (1, 3, and 5). This variation provides an ideal natural experiment for studying the impact of public drinking ordinances on public safety outcomes. Utilizing a difference-in-differences model, I find that the policy changes decreased the number of daily crimes in the city of Austin, with notable reductions in violent and public order offenses. I suspect that the mechanism underlying this reduction is that the public drinking allowance shifted alcohol use to more visible and socially moderated environments. However, I found no evidence of any changes in driving behavior. Together, these two results challenge the conventional wisdom that liberalizing alcohol access threatens public safety.

2 Data

This study combines public crime and traffic accident data from Austin, Texas, to evaluate the effects of two distinct alcohol policies.

The crime data comes from the Austin Police Department's Crime Reports, which details the time, the type of location, the primary offense and the latitude and longitude of all crimes committed in the city of Austin.[1] I utilize the crime and location type classifications from Lee, Lal and Query (2024) to classify crimes as violent, public order, and other crimes, as well as inside and outside crimes.

Traffic accident data was obtained from the Texas Department of Transportation's Crash Level Records.[2] For each crash, these records provide information on the number of injuries and whether the crash produced any fatalities.

3 Identification

3.1 Sunday Alcohol Sales Extension: Regression Discontinuity Design

Before the policy change, purchasing alcohol was restricted both at 9:59 AM (just before 10:00 AM) and at 10:01 AM (just after 10:00 AM, but before 12:00 PM). After the policy change, however, alcohol sales remained illegal at 9:59 AM but became legal at 10:01 AM. This sharp policy discontinuity provides an ideal setting for a difference-in-regression-discontinuity model (DRD). Thus, I estimate the following equation:

$$Y_{it} = \alpha + \beta \mathbf{1}\{t \geq 10:00\} + g_1(\text{Score}_i) + g_2(\text{Score}_i)\mathbf{1}\{t \geq 10:00\} + \varepsilon_{it}, \tag{1}$$

where Y_{it} represents the outcome variable (e.g., total crimes or crashes) that occurred during a given 10-minute interval on a given Sunday t, $\mathbf{1}\{t \geq 10:00\}$ represents a treatment indicator for times after 10:00 AM, and Score_i measures the number of minutes an observation is from distance from 10:00 AM. $g_1(\text{Score}_i)$ and $g_2(\text{Score}_i)$ allow for polynomials of order 0, 1, or 2. In this context,

[1] The crime reports records can be found: $https://data.austintexas.gov/Public-Safety/Crime-Reports/fdj4-gpfu/about_data$

[2] The crash level records can be found: $https://data.austintexas.gov/Transportation-and-Mobility/Austin-Crash-Report-Data-Crash-Level-Records/y2wy-tgr5/about_data$

comparing β before and after the policy change produces our causal estimate.

However, systematic differences in crimes committed before and after the cutoff may exist. Thus, I utilize the placebo-based tuning method proposed by Goldin et al. (2024). Specifically, I optimize the model and bandwidth to produce a precise zero for the Saturday estimates.[3] This entails grouping crimes and traffic accidents into 10-minute bins and separately estimating the RMSE every Saturday between September 2021 and September 2022. Because we expect placebo values to be zero, selecting the bandwidth and polynomial based on the specification best produces a zero treatment effect offers an out-of-sample method to determine the model. In the Monte Carlo tests offered by Goldin et al. (2024), this approach outperforms CCT estimates regardless of whether the true effect is 0 or not.

Due to the potential that police officers assign crimes and traffic accidents at unknown times to the top of the hour, I repeat the optimization with and without a 10-minute donut around 10:00 AM. The polynomial comparisons can be found in Figure A.1). The model comparisons with and without the donut can be found in Figures A.2 and A.3.

As demonstrated by Figure 1, the optimal bandwidth for the crime outcome is produced by a polynomial zero RDD model, henceforth referred to as a "levels" model with a 200-minute bandwidth before the cutoff. The optimal bandwidth for traffic accidents is similarly produced by a levels model but with a bandwidth of 320 minutes and a 10-minute donut around 10:00 AM (Figure 2).

The identifying assumption for this analysis is the existence of parallel trends, meaning the difference between the treatment and control groups remains stable during the pre-period. If this condition holds, it is reasonable to infer that the trends would remain parallel in the post-period as well in the absence of treatment. As demonstrated by Figures 3 and 4, the parallel trend assumption appears to hold.

3.2 Public Drinking Allowance: Difference-in-Differences

Before October 1, 2020, designated areas 1-6 all restricted public drinking (Figure 5). However, after October 1, 2020, designated areas 2, 5, and 6 repealed their restriction. This treatment and control comparison makes this policy a natural application of a difference-in-differences model. As

[3] Because the whole period from 10:00 AM to 12:00 PM is affected by the policy change, I only optimize for the left side of the bandwidth.

long as parallel trends exist, allowance of public drinking can be identified by:

$$Y_{it} = \alpha + \delta(\text{Post}_t) + \gamma(\text{Treated}_i) + \beta(\text{Treated}_i \times \text{Post}_t) + \lambda_t + \varepsilon_{it}, \tag{2}$$

where Y_{it} is the crime or crash outcome for designated area i at day t, Post_t is an indicator for the post-policy period, Treated_i is an indicator for neighborhoods where public drinking was legalized, and λ_t refers to date fixed effects. Treatment effects are represented by β.

The identifying assumption for the difference-in-differences model is parallel trends (i.e., stable differences between treatment and control in the pre-period). However, as demonstrated in Figure A.4, crime and traffic accidents fell disproportionately in designated areas 1, 3, and 4 due to the Covid-19 pandemic. Thus, I exclude the lockdown period from our sample. As indicated by Figures 6 and 7, after the lockdown period, the treatment and control groups are parallel.[4]

4 Results

This study examines two alcohol policy changes in Austin, Texas, focusing on their impacts on crime and traffic accidents. The first policy extended Sunday alcohol sales hours from 12:00 PM to 10:00 AM, and its effects were evaluated using a difference-in-regression discontinuity model. The second policy lifted public drinking restrictions in select areas, and its effects were analyzed through a difference-in-differences model.

4.1 Sunday Alcohol Sales Extension

The extension of Sunday alcohol sales hours did not significantly change overall crime rates or traffic accidents (Table 1). Further analysis categorized crimes into violent, public order, and other types (Table 2). While there was a 67 percent reduction in other crimes, this finding appears to be a statistical anomaly due to multiple testing. In future work, I plan to break down the results for the other crimes category further. Analysis by crime location (indoors vs. outdoors) also revealed no significant differences (Table 3).

Traffic accidents similarly showed no measurable change following the policy (Table 4). Despite

[4]Because daily data had too many events, the event study plots were aggregated to smooth out the curves despite the analysis being at the daily level. Plots of the pre-period with daily data can be found in Figure A.5.

initial concerns that increased alcohol availability might encourage riskier driving behaviors, the data provides no evidence to support this hypothesis. One potential explanation is that alcohol purchases occurred predominantly in private spaces, reducing the need for travel. Together, these findings suggest that extending sales hours had little observable impact on public safety.

4.2 Public Drinking Allowance

The second policy, which lifted public drinking restrictions in East Austin, had more pronounced effects. This policy resulted in a significant reduction of approximately two crimes per day, affecting both indoor and outdoor crime locations (Table 7). These findings suggest that individuals could be transitioning from private drinking to drinking in more socially regulated public spaces. Indeed, the peer pressure in these settings could be the source of moderated drinking and conflicts. Notably, the decreases in crime appear to be driven by violent and public order crimes (Table 6), which tend to impose disproportionate societal costs. However, like with the extension of alcohol sales, I find no evidence of any changes in traffic accidents (Table 8). These findings seem to suggest that concerns that loosening alcohol restriction increases traffic accidents are overstated and unsupported by the data.

5 Conclusion

Conventional wisdom dictates a trade-off between time-and-place alcohol regulations and public safety. Yet, this study finds no evidence that public safety needs to be sacrificed to provide consumers with more options.

Indeed, both policies—the extension of Sunday alcohol sales hours and the allowance of public drinking—show that crime did not increase, and in the case of public drinking allowances, crime decreased. Furthermore, no significant effects were observed for any of the traffic accident outcomes for either policy. These findings provide strong evidence that fears that liberalized alcohol laws cause traffic accidents are overstated.

However, these results should be interpreted with caution. First, as the heterogeneous effects of both policy changes suggest, these results may not be representative of all alcohol policy interventions. Indeed, some alcohol laws may be safe to repeal, while others could pose safety risks. Thus, using empirical evidence to tailor specific alcohol regulations remains essential.

Second, the change in public drinking policy occurred during the peak of the COVID-19 pandemic. Despite the sample starting after the lockdown policies relaxed, COVID-19 could have differentially affected areas that experienced the relaxation of the public drinking ordinances. Although the trends appear parallel, further study with more sophisticated controls is needed.

Finally, the crime outcome only includes reported or discovered crimes. Thus, the observed results could be a product of changes in criminal activity or police reporting behaviors. This concern is partially alleviated because the most serious (violent) crimes tend to be reported at higher rates, but further study separating reporting effects is required.

However, despite these caveats, these results challenge assumptions about the loosening of alcohol restrictions. Thus, policymakers should carefully weigh the benefits of providing consumers with more options against any empirically established public safety costs when crafting such policies.

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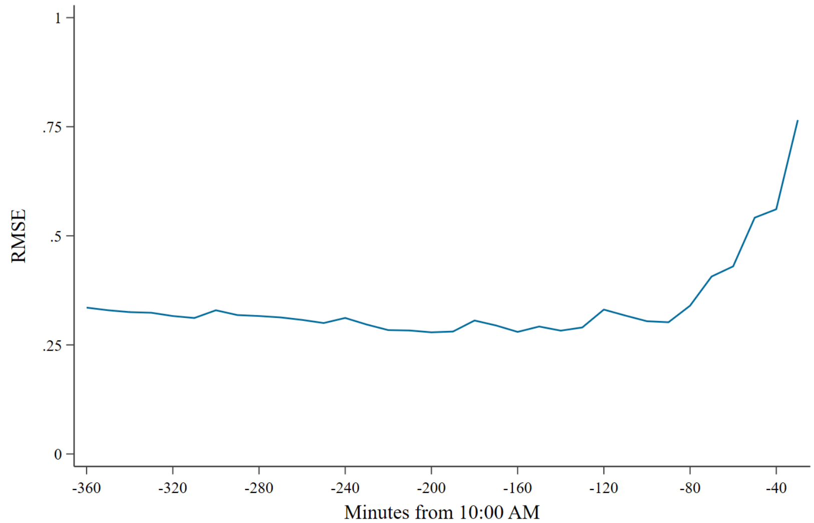
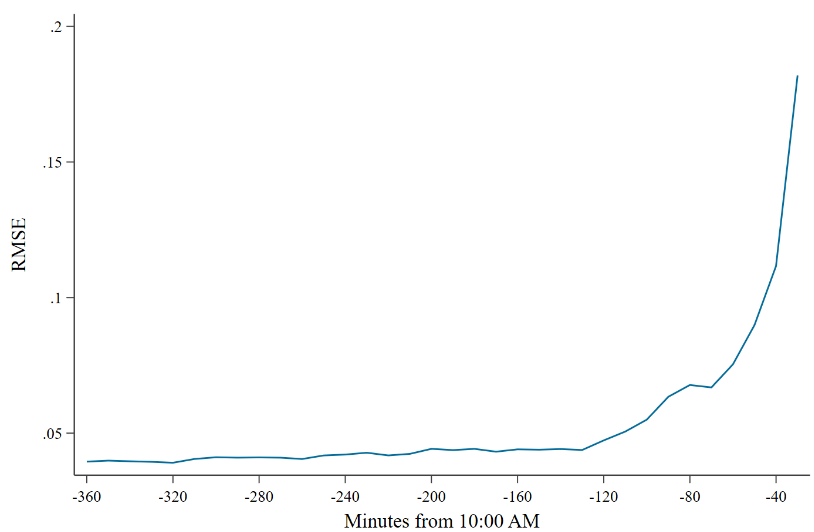
Figure 1: Crime RMSE Curve for the Levels model



Notes: The figure shows the Root Mean Squared Error (RMSE) for the crime outcome using the optimal model (levels). The results are obtained by estimating the treatment effect for each post-period Saturday (placebo) and averaging the squared coefficients. The optimal bandwidth, which occurs at the absolute minima, is -200 minutes.

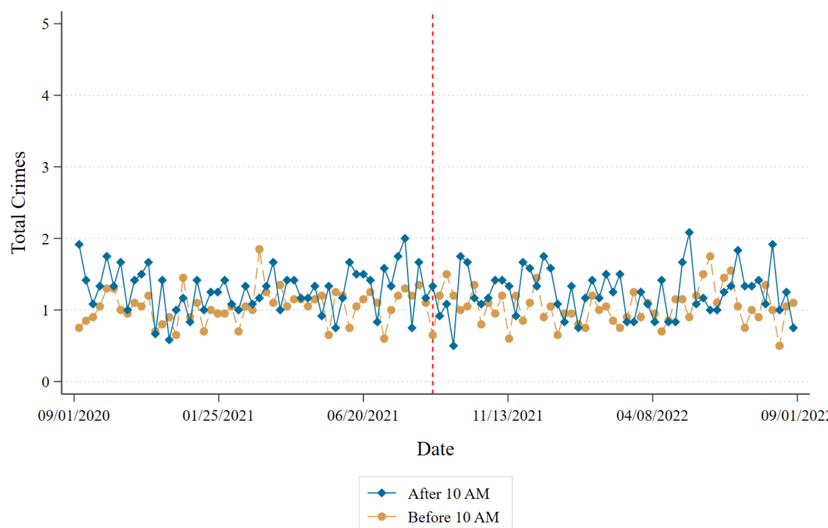
Figure 2: Traffic Accident RMSE Curve for the Levels with Donut model



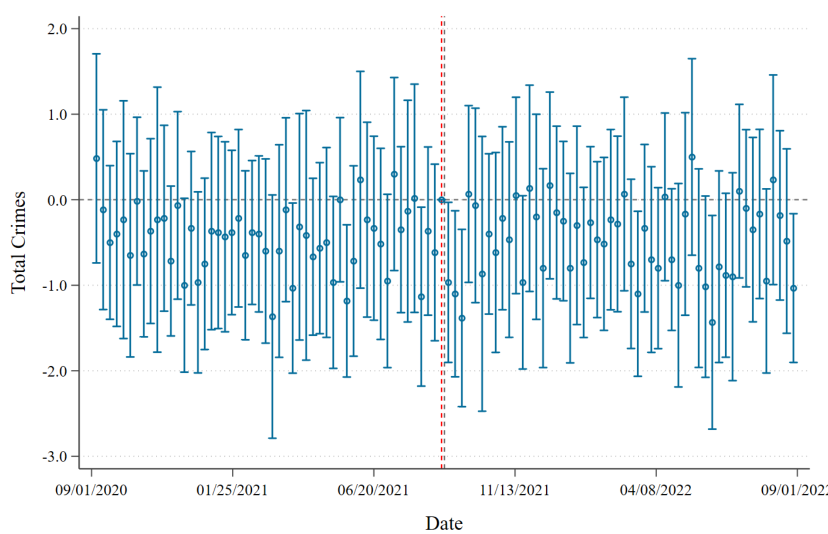
Notes: The figure shows the Root Mean Squared Error (RMSE) for the traffic outcome using the optimal model (levels with donut). The results are obtained by estimating the treatment effect for each post-period Saturday (placebo) and averaging the squared coefficients. The optimal bandwidth, which occurs at the absolute minima, is -320 minutes.

Figure 3: Extended Alcohol Sales:
Event Study Plots for Crime Data

(a) Averages



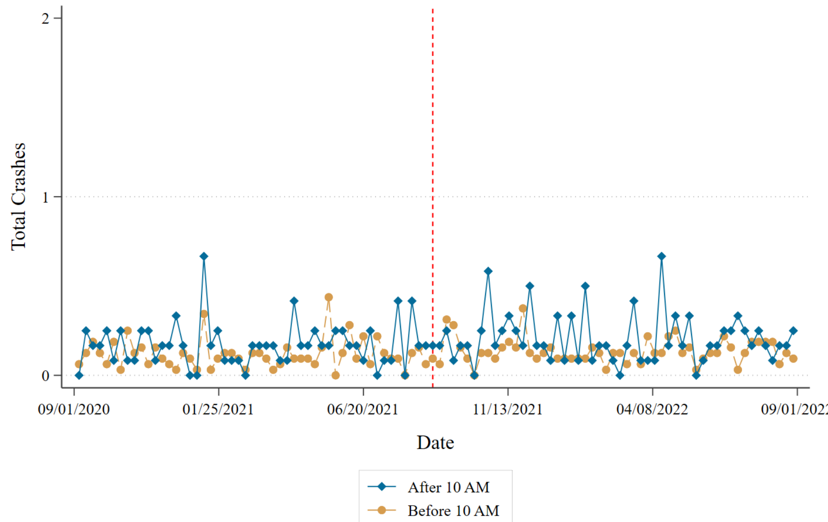
(b) Coefficient Plot



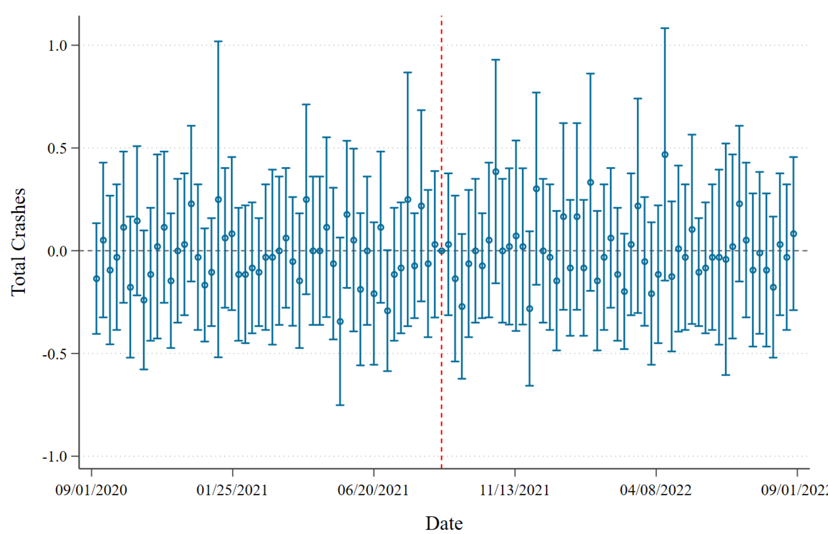
Notes: In panel A, the figure compares the average number of crimes in a 10 minute interval for the treatment (after 10 AM) and control groups (before 10 AM). In panel B, the figure reports the difference between these groups and the bars represent the 95 percent confidence interval. The dotted red line represents the Sunday before treatment.

Figure 4: Extended Alcohol Sales:
Event Study Plots for Traffic Accident Data

(a) Averages

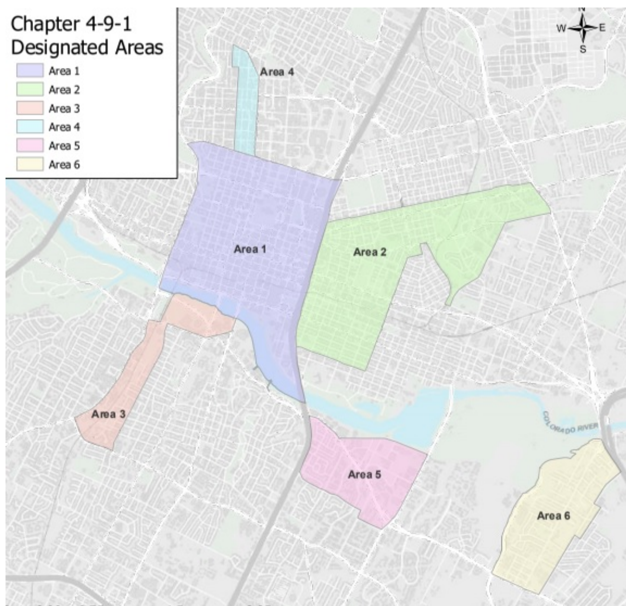


(b) Coefficient Plot



Notes: In panel A, the figure compares the average number of traffic accidents in a 10 minute interval for the treatment (after 10 AM) and control groups (before 10 AM). In panel B, the figure reports the difference between these groups and the bars represent the 95 percent confidence interval. The dotted red line represents the Sunday before treatment.

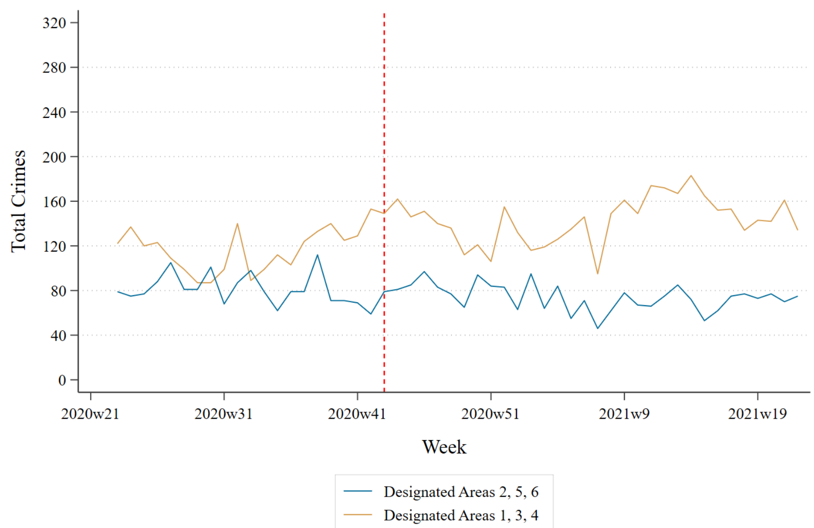
Figure 5: Map of Districts



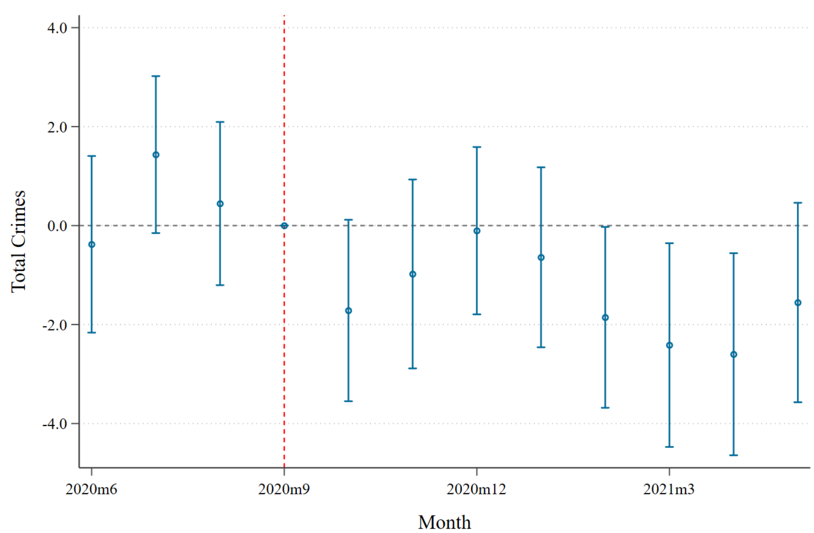
Notes: The map displays designated areas 1 through 6. Designated Areas 2, 4, and 6 are the treatment areas and Designated Areas 1, 3, and 5 are the control areas. The map retrieved from the City of Austin's X account (https://x.com/austintexasgov/status/1311722249234284550).

Figure 6: Public Drinking Allowance:
Event Study Plots for Crime Data

(a) Averages



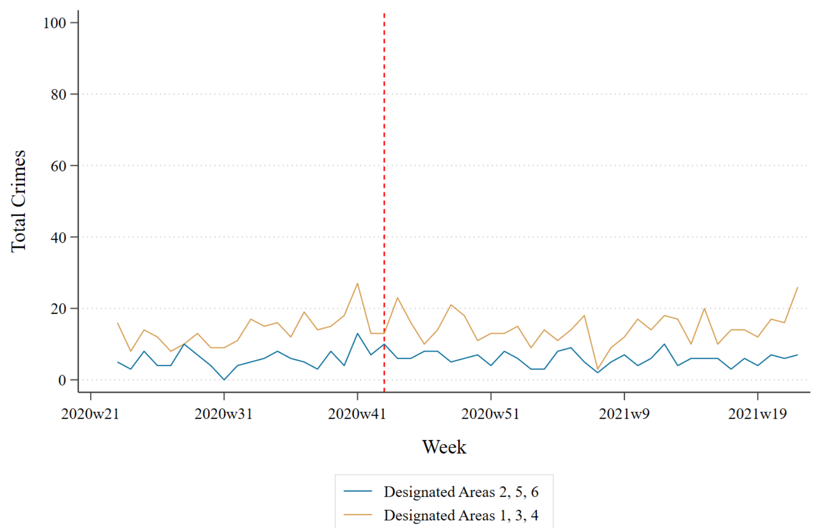
(b) Coefficient Plot



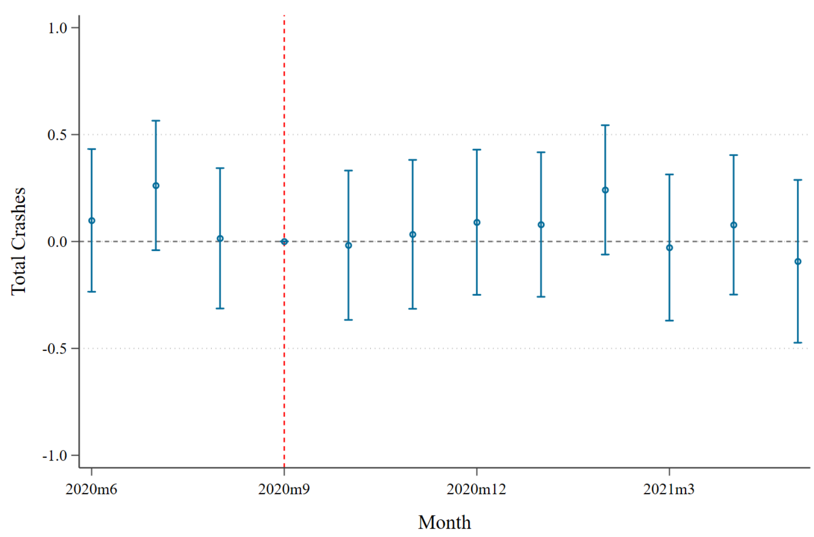
Notes: In panel A, the figure compares the weekly number of crimes for the treatment (Designated Areas 2, 4, and 6) and control groups (Designated Areas 1, 3, 5). In panel B, the figure reports the difference between these groups at the monthly level and the bars represent the 95 percent confidence interval. The dotted red line represents the unit before treatment.

Figure 7: Public Drinking Allowance:
Event Study Plots for Traffic Accident Data

(a) Averages



(b) Coefficient Plot



Notes: In panel A, the figure compares the total number of accidents for the treatment (Designated Areas 2, 4, and 6) and control groups (Designated Areas 1, 3, 5). In panel B, the figure reports the difference between these groups at the monthly level and the bars represent the 95 percent confidence interval. The dotted red line represents the unit before treatment.

Table 1: Effect of Extended Alcohol Sales on Crime and Traffic Crashes

	(1)	(2)	(3)
	Pre-Treatment (9/2020-8/2021)	Post-Treatment (9/2021-9/2022)	Diff-in-Diff [(2) - (1)]
Panel A: Number of Crimes			
Coefficient	0.240***	0.196***	-0.044
	(0.065)	(0.063)	(0.091)
95% CI	[0.11, 0.37]	[0.07, 0.32]	[-0.22, 0.13]
Control Mean	1.0	1.0	1.0
Percent Change	22.9	18.7	-4.2
Panel B: Number of Crashes			
Coefficient	0.072***	0.088***	0.016
	(0.022)	(0.023)	(0.032)
95% CI	[0.03, 0.12]	[0.04, 0.13]	[-0.05, 0.08]
Control Mean	0.1	0.1	0.1
Percent Change	63	65	13

Notes: The outcome in panel A is the average number of crimes committed in 10 minutes, while the outcome in Panel B is the average number of crashes that occurred every 10 minutes in Austin. Panel A includes crimes committed 200 minutes before 10 AM to 120 minutes after 10 AM. Panel B includes a 10 minute donut around 10 AM and includes crashes 320 minutes before 10 AM to 120 minutes after 10 AM. Columns 1 and 2 compare these outcomes before and after 10 AM. Column 3 is the difference between columns 1 and 2. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Effect By Crime Type

	(1)	(2)	(3)
	Violent	Public Order	Other
Coefficient	-0.061	0.064	-0.046**
	(0.045)	(0.070)	(0.020)
95% CI	[-0.15, 0.03]	[-0.07, 0.20]	[-0.09, -0.01]
Control Mean	0.3	0.7	0.1
Percent Change	-19	10	-67

Notes: Coefficients represent the Difference-in-Differences result comparing crimes of a certain type before and after 10 AM. Column 1 considers violent crime including sex crimes. Column 2 considers public order crimes including drug crimes. Column 3 considers crimes that do not fit into the previous two categories. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effect By Crime Location

	(1)	(2)
	Inside	Outside
Coefficient	-0.099	0.057
	(0.070)	(0.044)
95% CI	[-0.24, 0.04]	[-0.03, 0.14]
Control Mean	0.7	0.3
Percent Change	-14	19

Notes: Coefficients represent the Difference-in-Differences result comparing crimes before and after 10 AM. Column 1 considers crimes with a location type corresponding to an inside location, while Column 2 includes crimes with a location type corresponding to outside or partially outside location. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Alcohol Sales Extension: Heterogeneity Tests by Traffic Crashes

	(1)	(2)
	Count of Injuries	Number of Fatal Crashes
Coefficient	-0.003	0.016
	(0.035)	(0.032)
95% CI	[-0.07, 0.07]	[-0.05, 0.08]
Control Mean	0.1	0.1
Percent Change	-5	13

Notes: Coefficients represent the Difference-in-Differences result comparing traffic accidents before and after 10 AM. The outcome for column 1 is the number of daily injuries from car crashes. The outcome for column 2 is the number of crashes that resulted in fatalities. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Impact of Eased Public Drinking Restrictions on Crime and Traffic Crashes

	(1)	(2)	(3)
	Pre-Treatment (6/2020-9/2020)	Post-Treatment (10/2020-6/2021)	Diff-in-Diff [(2) - (1)]
Panel A: Number of Crimes			
Coefficient	-1.403***	-3.275***	-1.872***
	(0.309)	(0.276)	(0.415)
95% CI	[-2.01, -0.79]	[-3.82, -2.73]	[-2.69, -1.06]
Control Mean	5.4	6.7	6.3
Percent Change	-26.1	-48.5	-29.7
Panel B: Number of Crashes			
Coefficient	-0.361***	-0.411***	-0.049
	(0.060)	(0.048)	(0.077)
95% CI	[-0.48, -0.24]	[-0.50, -0.32]	[-0.20, 0.10]
Control Mean	0.6	0.7	0.7
Percent Change	-59	-59	-7

Notes: The outcome in panel A is the number of crimes, while the outcome in Panel B is the number of crashes that occurred daily in Austin. Columns 1 and 2 compare these outcomes in Designated Areas 2, 5, and 6 with the number of daily crimes in Designated Areas 1, 3, and 4 before and after public drinking was allowed. Column 3 is the difference between columns 1 and 2. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Public Drinking Allowance Effects By Crime Type

	(1)	(2)	(3)
	Violent	Public Order	Other
Coefficient	-0.488***	-1.322***	-0.062
	(0.153)	(0.297)	(0.049)
95% CI	[-0.79, -0.19]	[-1.90, -0.74]	[-0.16, 0.03]
Control Mean	2.0	4.0	0.3
Percent Change	-25	-33	-19

Notes: Coefficients represent the Difference-in-Differences result. Column 1 considers violent crime including sex crimes. Column 2 considers public order crimes including drug crimes. Column 3 considers crimes that do not fit into the previous two categogries. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Public Drinking Allowance Effects By Crime Location

	(1)	(2)
	Inside	Outside
Coefficient	-0.984***	-0.856***
	(0.194)	(0.254)
95% CI	[-1.36, -0.60]	[-1.35, -0.36]
Control Mean	2.8	3.0
Percent Change	-35	-28

Notes: Coefficients represent the Difference-in-Differences results. Column 1 considers crimes with a location type corresponding to an inside location, while Column 2 includes crimes with a location type corresponding to outside or partially outside location. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Public Drinking Allowance: Traffic Accident Heterogeneity Tests

	(1)	(2)
	Count of Injuries	Number of Fatal Crashes
Coefficient	-0.026	-0.049
	(0.071)	(0.077)
95% CI	[-0.16, 0.11]	[-0.20, 0.10]
Control Mean	0.4	0.7
Percent Change	-6	-7

Notes: Coefficients represent the Difference-in-Differences result. The outcome for column 1 is the number of daily injuries from car crashes. The outcome for column 2 is the number of crashes that resulted in fatalities. Parentheses include robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

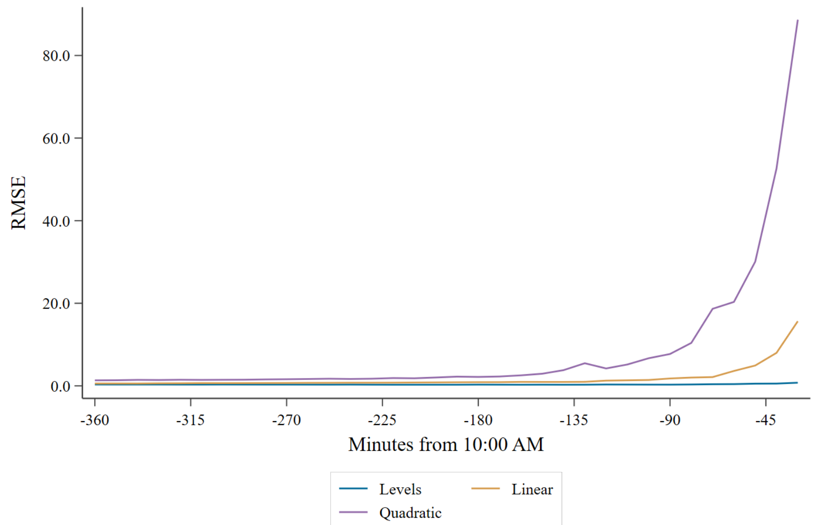
Online Appendix to Alcohol and Driving Behavior

Neel Lal

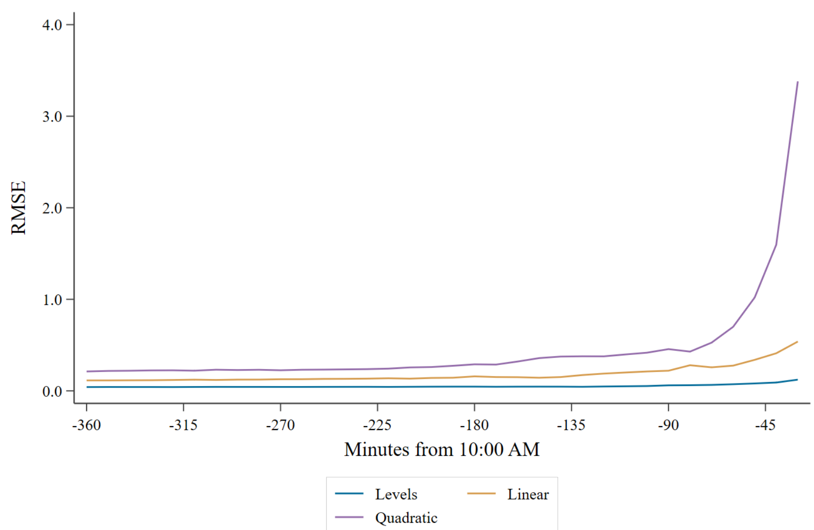
A Appendix Tables and Figures

Figure A.1: RMSE Comparison

(a) Crime



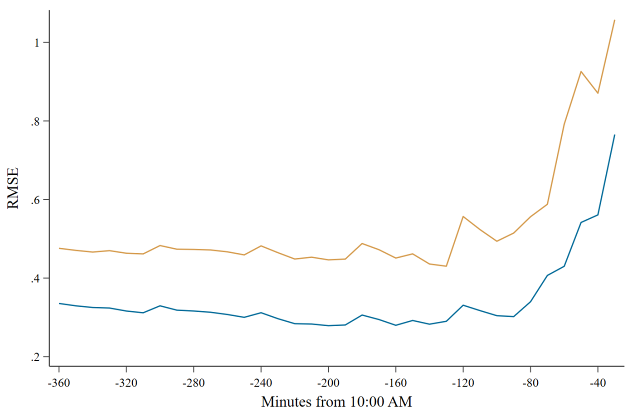
(b) Traffic Accidents



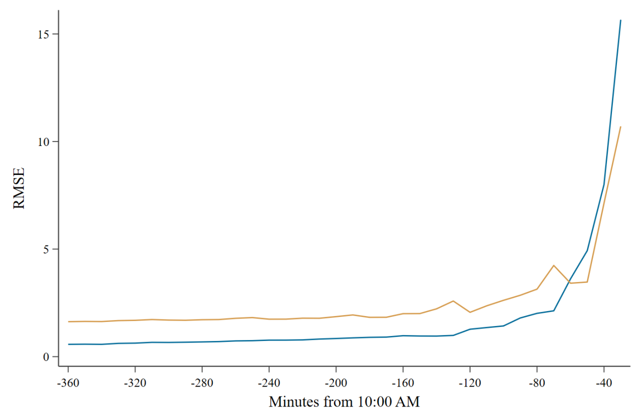
Notes: The figure shows the Root Mean Squared Error (RMSE) for the crime (panel a) and traffic accident outcomes (panel b) by model. The results are obtained by estimating the treatment effect for each post-period Saturday (placebo) and averaging the squared coefficients.

Figure A.2: Crime RMSE by Donut Specification and Bandwidth

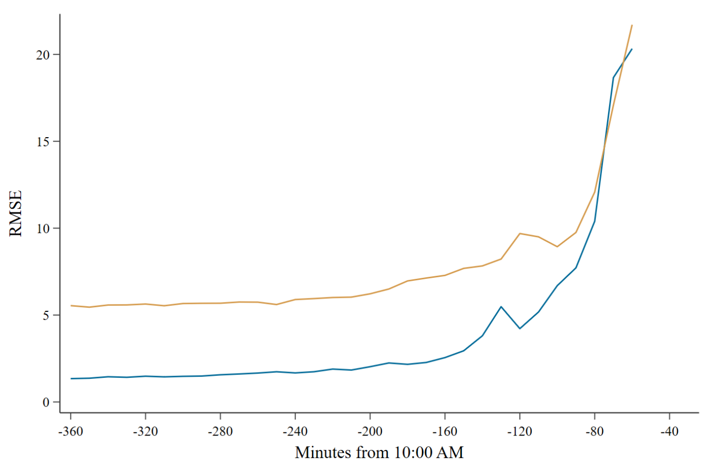
(a) Levels



(b) Linear



(c) Quadratic

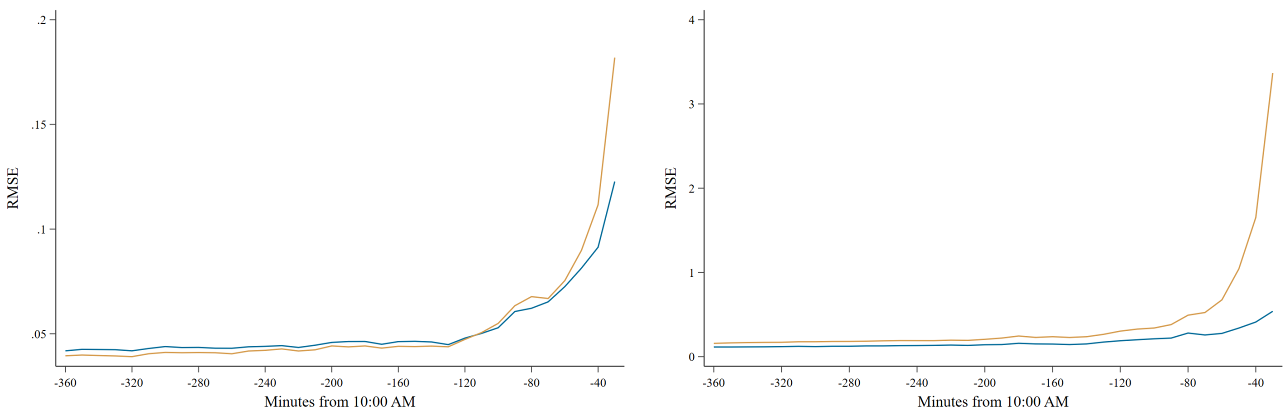


Notes: The figure shows the Root Mean Squared Error (RMSE) for the crime outcome with and without a 10-minute donut. The results are obtained by estimating the treatment effect for each post-period Saturday (placebo) and averaging the squared coefficients.

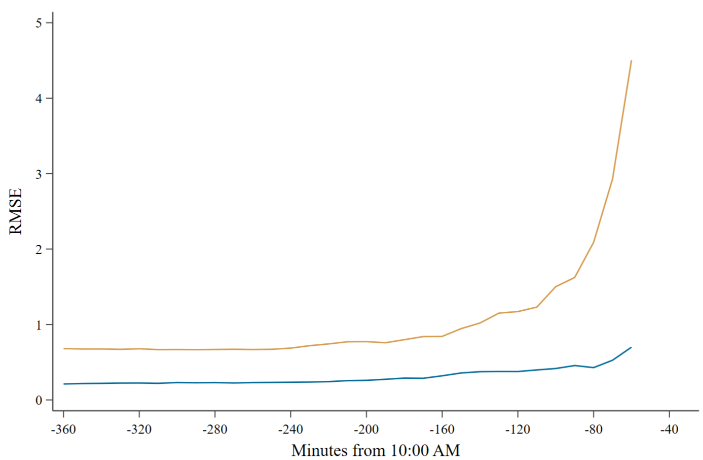
Figure A.3: Traffic Accident RMSE by Donut Specification and Bandwidth

(a) Levels

(b) Linear



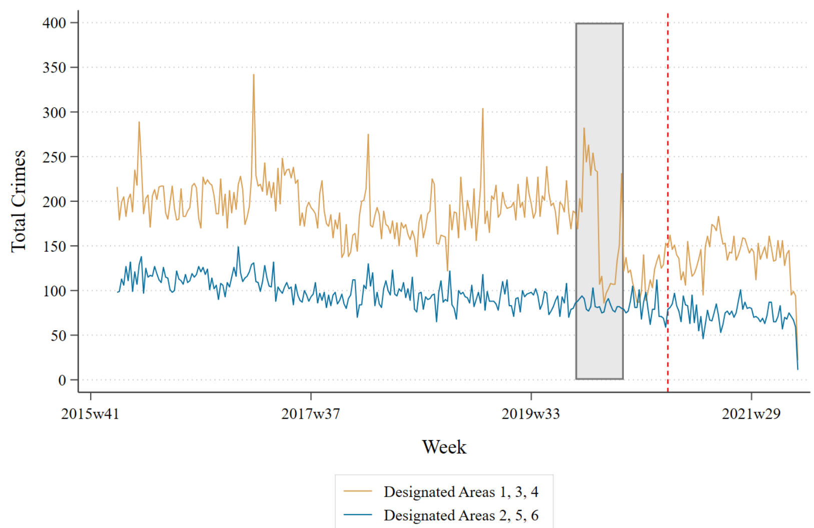
(c) Quadratic



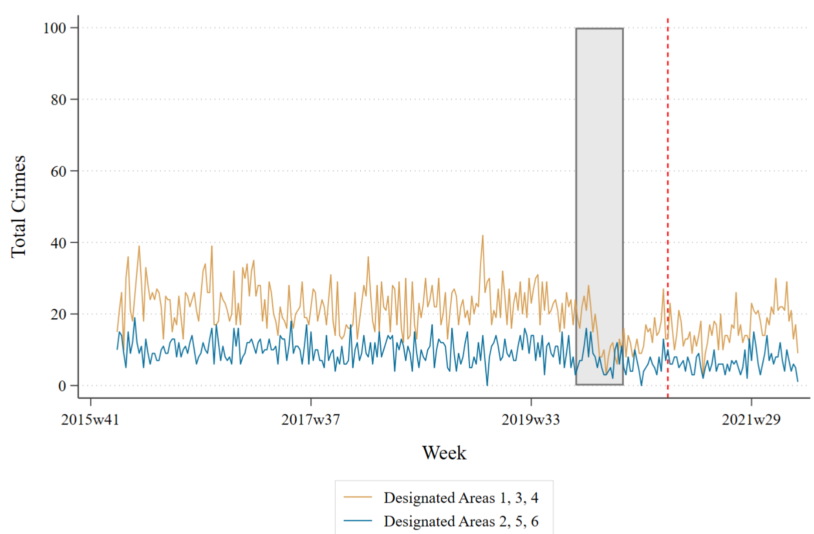
Notes: The figure shows the Root Mean Squared Error (RMSE) for the traffic accident outcome with and without a 10-minute donut. The results are obtained by estimating the treatment effect for each post-period Saturday (placebo) and averaging the squared coefficients.

Figure A.4: Event Study Plots for Public Drinking Allowance Including Covid-19

(a) Crime



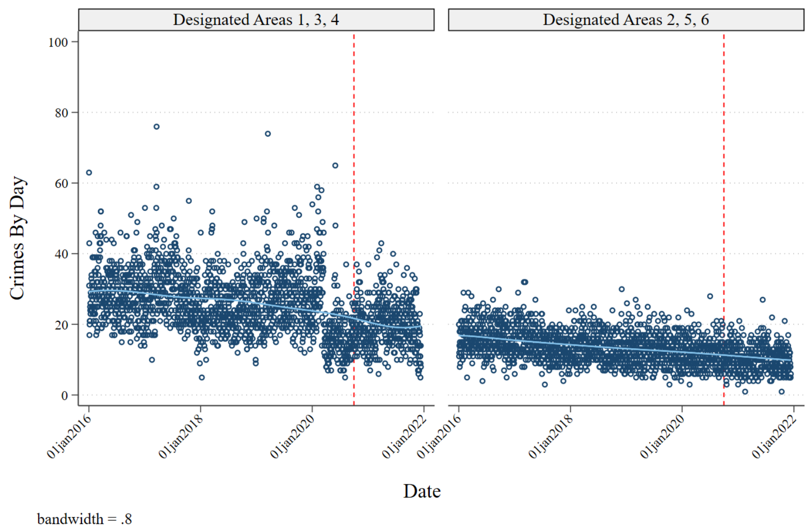
(b) Traffic Accidents



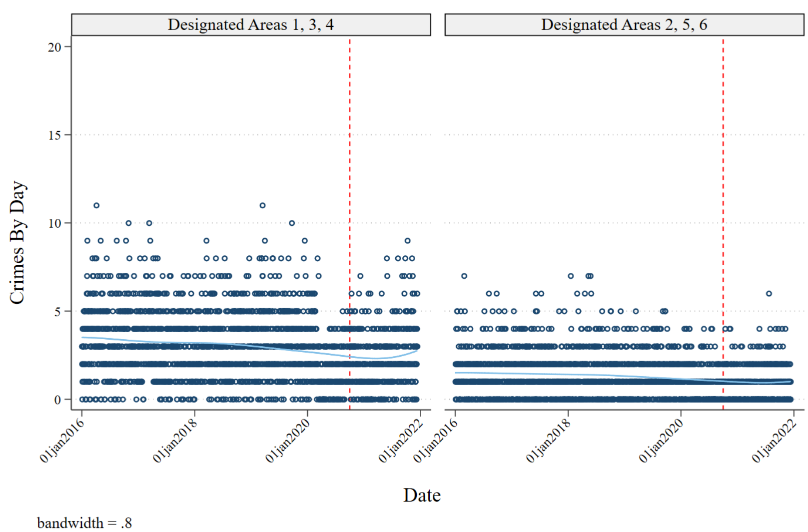
Notes: The figure compares the total number of crimes (panel a) and accidents (panel b) for the treatment (Designated Areas 2, 4, and 6) and control groups (Designated Areas 1, 3, 5). A gray box covers the period for which the lockdown were enforced. The dotted red line indicates the week of treatment.

Figure A.5: Lowess Plots for Public Drinking Allowance using Daily Data

(a) Total Crimes



(b) Traffic Accidents



Notes: The figure compares the total number of crimes (panel a) and accidents (panel b) for treatment and control groups. The light blue line is a fitted a Lowess curve.