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Crime and Political Effects of a Right-to-Carry Ban in Brazil

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Crime and political effects of a right-to-carry ban in Brazil*

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Abstract: This paper studies the effects of legislation in Brazil that banned the right-to-carry guns and provided for a voter referendum regarding whether to ban the sale of all firearms. Using a regression discontinuity design, I find that gun-related homicides decreased by 12.2 percent, with the reduction especially pronounced in high-crime areas and among black males. There is no evidence of substitution effect as non-gun-related homicides were not affected. Analysis of the subsequent voter referendum, which was defeated by a wide margin, shows stronger support for the complete weapons ban in the areas more affected by gun violence.

Keywords: gun policy, right-to-carry guns, gun-related homicides, voting behavior
JEL Codes: D72, H11, I12, J17, K14

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1 Introduction

How do laws that regulate the right-to-carry guns affect crime? On one hand, allowing citizens to carry guns may deter criminals from committing a crime because they may think that their intended victims could be armed. On the other hand, laws that forbid carrying may decrease violence by reducing the odds of serious injury or death occurring during criminal encounters or in disagreements that escalate. Extensive research has been conducted to understand the impact of laws that allow citizens the right to carry weapons, but there is no consensual conclusion (e.g., Lott and Mustard 1997; Donohue and Ayres 1999; Ayres and Donohue III 2002; Black and Nagin 1998; Ludwig 1998; Aneja et al. 2011; Donohue and Levitt 1998). As stated by Manski and Pepper (2018), empirical results are sensitive to minor variations in the data and model specifications, delivering mixed conclusions. Therefore, the broad impacts of such laws are not clear, and little is known about who benefits from such legislation.

This paper aims to fill this gap in the literature by measuring the impact that a law that banned the right to carry weapons in Brazil had on crime. Brazil is a country with high gun-related homicides rate (18.2 per 100,000), ranked seventh in the world (Naghavi et al., 2018), and that experienced a sharp increase of 204% in this rate between 1980 (when data series begin) and 2003 (when the law passed and this rate started to decrease), going from 7.3 to 22.2 gun-related homicides per 100,000.[1] Gun-related homicides in Brazil consistently increased since 1992 until 2003, which motivated gun-carrying restrictions that were passed by the National Congress of Brazil, and implemented in December 2003. The legislation prohibited carrying weapons, and provided for a subsequent referendum 22 months later to allow voters to decide whether to implement a more stringent law to completely ban the ownership of weapons and ammunition. I study the impact of the legislation on crime rates in various communities and populations throughout Brazil and analyze voter support for the

[1] Gun-related homicides corresponds to close to 70% of all homicides in Brazil, this number was close to 40% in 1980.

ban on weapons and whether it relates to gun violence.

The impact of right-to-carry (RTC) laws has been extensively studied, however, results are mixed and inconclusive. Lott and Mustard (1997), for instance, concluded that laws giving people the right to carry guns reduced crime rates in the United States. However, their findings was challenged by Ayres and Donohue III (2002) and Black and Nagin (1998), on the grounds that their empirical models were not robust to reasonable changes in the model specifications, and that these models were sensitive to the correction of several coding errors. Aneja et al. (2014) describe a National Research Council panel discussion in 2004 that invited specialists to study county-level crime data from 1977-2000 in the United States, and concluded that it was impossible to state whether RTC laws increased or decreased crimes.

Manski and Pepper (2018) revisit the United States' context and show that, when analyzing the impact of RTC laws in this country, it is possible to find contradictory results even when using similar data and empirical strategies. This can happen because data on crime cannot reveal counterfactual outcomes, which authors commonly solve by making "invariance assumptions asserting that specified features of treatment response are constant across space or time (p.3)." Yet, the literature on RTC laws does not find a consensus on credible assumptions regarding crime rates trends. Relaxing invariance assumptions, Manski and Pepper show that there are no simple conclusions, and that it is not possible to identify with certainty the sign of the impact of RTC laws on crime.[2]

This paper, however, approaches the question about how RTC laws affect violence by measuring the impact of a nationwide law that *banned*, instead of authorized, the right to carry, allowing me to identify and examine an immediate and precise break in crime trend. Most research on the impact of RTC laws has been conducted in the United States, using variations in state gun legislation to find the impact on crimes. Nearly all of this legislation expanded the right to carry weapon. The Brazilian law, by contrast, prohibited the right

[2]Although the literature on the effect of RTC laws on crime is inconclusive, many authors find a positive relationship between the number of guns and crimes (see, for instance, Cerqueira et al. 2013; Leigh and Neill 2010; Duggan 2001; Stolzenberg and D'alessio 2000; McDowall 1991; McDowall et al. 1995; Cook et al. 2005; Cook and Ludwig 1998; Cook and Ludwig 2002; Newton and Zimring 1969)

to carry, and required people to comply immediately, thus allowing for better identification of the law's impacts because it created a sharp change in incentives to discourage people to carry guns. Authorizing the right to carry can create a change in trend, however, it does not impose a clear break in behavior.[3] Imposing people to immediately respond to the law, allows me to use monthly data on crime and construct a regression discontinuity model where time is the running variable. Restricting the window of time addressed by my analysis enhances the credibility of the assumption that the only differences in crimes trends after the gun carrying prohibition take place in response to the law.[4]

I follow the recent literature that uses regression discontinuity in time, also known as interrupted time series, to investigate event study and economic policies (e.g., Anderson 2014; Gallego et al. 2013; Davis 2008; Auffhammer and Kellogg 2011; Busse et al. 2006; Carr and Packham 2019). As Davis (2008) explain, the advantage of using a regression discontinuity to examine an event study is that this method can better control for unobserved factors changing over time that may cause the error term to be correlated with time producing biased estimates of the policy analyzed. Regression discontinuity can address this issue by considering an arbitrarily narrow window of time around the implementation of the law. Within this interval, the unobserved factors influencing gun-related homicides are likely to be similar so that observations before the enactment of the law regulating access to guns provide a comparison group for observations after it. In addition, as explained by

[3] Researchers analyzing the impacts of laws that authorize carrying of weapons cannot rely on a clear break in trend for two main reasons. First, it is unlikely that everyone willing to carry a gun will immediately apply for a permit and meet all requirements. Second, even if all applicants meet all requirements and seek a permit for a weapon license right after the law passes, obtaining the permit and the weapon takes time and may vary within States and individuals. For instance, in the United States the time to obtain a license varies from state to state, and from place to place within certain states (e.g, in Florida, the state division of licensing has up to 90 days to review an application for a concealed weapon license, while in Texas, the maximum time allowed to review a license application is 60 days; and within the state of California, the time to obtain a weapon can vary from four months to six months).

[4] Another problem of measuring the effect on crime from laws that give people the right to carry weapons is dealing with the potential endogeneity of such laws (Durlauf et al. 2016). The Brazilian law analyzed in this paper was enacted across the country (the sample contains information on homicides for all 5,597 Brazilian municipalities). This fact mitigates endogeneity issues, such as when a particular State decides to enact a law on guns in response to an endogenous variable (e.g., crime rate; voters' political ideology; mass shootings; immigration).

Auffhammer and Kellogg (2011), regression discontinuity in time by adding a polynomial function of the running variable (i.e., time) allows unobserved variables related to gun-related homicides to vary non-linearly. Moreover, because the same law was imposed on all Brazilian municipalities, there is no need to rely on certain controversial assumptions that have hampered the previous literature.[5] Nonetheless, I also take advantage of the fact that non-gun-related crimes are not affected by the legislation examined and alternatively propose a difference-in-differences model using these type of crimes as control group and find similar results.

The empirical analysis show that prohibiting gun carrying decreased gun-related crimes and the economic value of the law is estimated to be close to $3.4 billion in one year. Using monthly data (available across the country) on homicides, I find that gun-related homicides decreased by 4,406 (a 12.2 percent reduction) in the year following the law, and that the reduction was most pronounced among young black males and in high-crime areas, in addition, only gun-related homicides outside the residence were affected. Non-gun-related homicides and gun-related suicides were not affected by the law. Using monthly data on non-homicide crimes at the municipal level, provided by the State of São Paulo, I show that the prohibition of the right to carry concealed weapons led to a decrease in robberies, and illegal gun carrying.[6] Non-gun-related crimes such as rape, and theft remained unchanged. Combined, these results suggest that the mechanism explaining the reduction in gun-related crimes is a reduction in gun carrying. Finally, I analyze the subsequent referendum, which asked citizens to decide whether to ban all weapons and ammunition, and find that exposure to gun violence is positively related to support for the referendum on banning guns.

[5]The following case illustrates an example of a controversial assumption discussed by Manski and Pepper (2018): Virginia enacted law conferring the right to carry weapon in 1989, but Maryland did not. Therefore, one needs to assume that in the absence of such law, Virginia and Maryland would experience the same changes in crimes between 1988 and 1990.

[6]The sample contains information on non-homicide crimes for all 645 municipalities in the State of São Paulo.

2 The gun carry prohibition in Brazil

The law prohibiting gun carrying in Brazil became effective in 2003 under President Luiz Inácio Lula da Silva's administration. More specifically, the federal legislation was signed into law in December 22nd, 2003 (Law number 10.826) and was called *Estatuto do Desarmamento* (Disarmament Statute). This law prohibited citizens from carrying a gun outside of their residences or places of business. Brazil is a country in which close to 200 laws passes every year in congress. More relevant to this work, between January 2003 and January 2004, there were 201 laws passed including the prohibition of gun carrying I study. However, none of them should have a sharp impact in homicides starting in January 2004. 21% of these laws have very low to no impact in people's lives (e.g., laws that decide on the name of a public airport or laws that add a famous personality to the "Heroes of the nation" book). 60% contain information on budgetary decisions (mostly on reallocation of money from the primary surplus). 16% are laws regulating or adding minor changes to existing regulations related to professions, copyrights and taxes. Finally, the remaining 3% were regarding programs related to drought, domestic violence and poverty. They were all either enacted prior to October 2003 or not enacted. The exception is a law helping young people to get their first job that was enacted in 2004 and could be confounding the results. Nonetheless, as Andrade (2005) explains, this program started as a trial throughout 2004 and only affected six municipalities.[7]

The gun carrying prohibition became effective one day after being signed into law (i.e. December 23rd, 2003). This fact was highly emphasized by the media. For instance, in December 23rd, 2003, the daily newspaper with biggest circulation in Brazil, Folha de S. Paulo, had in its front page an article stating that the Disarmament Statute became effective that day. The same newspaper also reported cases of citizens being arrested for carrying their guns after the law became effective. For instance, one day after the enactment of the

[7] The database containing all the laws passed in Brazil can be accessed at `https://www2.camara.leg.br/atividade-legislativa`

legislation, Folha de S. Paulo reported the case of a car wash employee that, after being stopped and searched by police officers, was arrested for carrying a gun (Folha (2003)).

Prior to the legislation, the Brazilian government authorized citizens to carry a gun if they met all requirements to carry and applied for a license. At the time, citizens caught carrying a gun without a permit or that possessed a gun inside their residence without registering it, would face a penalty of one to two years in jail. More important for this research, they would have the possibility of bail and wait for trial outside prison. Nonetheless, after the legislation was enacted, all citizens (except for hunters, private security employees, and police officers) were prohibited to carry a gun. Additionally, the penalty for carrying became two to four years of incarceration, and it also became a "no bail" offense. Possession of guns inside the residence or place of business, on the other hand, would still be permitted and although illegal possession also had its penalty increased to two to four years of incarceration, the perpetrator would still have the possibility of bail.[8] Given that the average wait for criminal trial in Brazil is 4 years (Ribeiro et al. (2012)), eliminating the possibility of bail should create a much larger incentive to avoid illegal carrying than illegal possession.[9]

The Brazilian gun regulation not only imposed high penalties for those individuals who were arrested carrying a gun but also denied them the possibility of bail. Therefore, it should be expected a reduction in the occurrences of gun carrying after 2003. According to a study conducted in 2010 investigating the effectiveness of the Disarmament Statute (Paz (2010)), the reduction in gun violence observed after the law passed was driven by a reduction in gun carrying. The study mentions the case of the State of São Paulo, where the number of illegal gun carrying decreased after the gun carrying prohibition, even though police officers intensified the search for it. Using crime data provided by the State of São Paulo I find that

[8]See Chapter 4, articles 12 and 14, of the Disarmament Statute on the penalties for illegal gun possession and carrying: `http://www.planalto.gov.br/ccivil_03/LEIS/2003/L10.826.htm`)

[9]It is important to emphasize that the Disarmament Statute replaced the federal law number 9,437 of February 1997 regulating guns. Therefore, most of the sections contained in the former was already part of the latter. The main changes are the ones described in this paragraph, together with the extra requirements to possess a gun (i.e., no criminal record; be employed; show proof of residence and of technical and psychological ability to use guns).

illegal gun carrying was reduced by 14.3% in the year following the gun carrying prohibition, which corroborates the study narrative.[10]

Theoretically, this reduction in the number of people carrying a gun could increase or decrease crimes. On one hand, non-homicides crimes that more often involve guns would decrease if perpetrators become less likely to carry guns, and gun-related homicides would decrease if either or both perpetrators (reduction in felony murder) and regular citizens (reduction of the odds of death occurring in disagreements that escalate) are less likely to carry. On the other hand, all types of crimes would increase if the deterrence effect hypothesis is correct. That is, if allowing citizens to carry weapons deter criminals from committing a crime as they may think that their intended victims could be armed. My empirical analysis supports the first case - i.e., reduction in gun carrying reduces gun-related crimes and have no effect on non-gun-related crimes. In addition, consistent with the fact that gun ownership was not as much affected by the law, I find no impacts of the gun carrying ban on gun-related suicides and gun-related homicides that take place inside the residence. This provides further evidence that less people carrying guns is the mechanism explaining the reduction in gun-related crimes.

Finally, an important and unique feature of the legislation was its 35th section, which set the stage for a national referendum to take place in October 2005 (22 months after the initial legislation was passed into law), to allow Brazilian citizens to vote on an even more restrictive weapons law. The law put forward in the referendum stipulated that the sale of any guns and ammunition would be completely prohibited in the country (again, with exceptions for hunters and those with security-related jobs). More specifically, voters were asked the following question: Should the commerce of firearms and ammunition be prohibited in Brazil? Therefore, the referendum did not propose to change the previously passed legislative statute, but proposed to go further, by prohibiting the sale of all firearms. Analyzing the referendum I provide insights on the type of voters that are more willing to

[10]This result is obtained in Table 4, which will be discussed in the subsection 4.2.

support gun control.

3 Data and Empirical Strategy

I first analyze the effects of prohibiting gun carrying on homicides, and then I examine its effects on non-homicide crimes. Monthly homicide data at the municipality level, for all 5,597 Brazilian municipalities, have been available since 1996 in the Brazilian National System of Mortality Records (DATASUS). Monthly data at the municipality level on non-homicide crimes, however, are only available for the state of São Paulo, which contains 645 municipalities.[11] In Figure 1, top graph, I show the total monthly number of gun-related and non-gun-related homicides in Brazil between 1996 and 2014. As one can notice, there is a positive trend in gun-related homicides prior to the the law that banned gun carrying became effective and, after its enactment (i.e., after the red vertical line), there is a break in the trend. Non-gun-related homicides, on the other hand, do not seem to be affected by the law. In the bottom graph in Figure 1, I aggregate the annual number for both variables within the same time range and the vertical line represents the year of 2003, i.e., the last one before the enactment of the law. The advantage of this approach is that it makes it easier to see the drop in gun-related homicides in Brazil after the enactment of the law. Once again, this graph suggests a break in the positive trend in the number of gun-related homicides in the country after the policy became effective. Gun-related homicides increased annually in Brazil since 1992 until the enactment of the gun carrying prohibition in 2003, however, there was a sharper increase in this type of violent crime since 1995. Dix-Carneiro et al. (2018) explain this phenomenon by showing that the Brazilian large-scale unilateral trade liberalization between 1990-1995, which generated exogenous negative shocks in the labor market, is partially responsible for it.

Table 1 shows descriptive statistics, considering the year of 2003, of homicides in Brazil and non-homicide crimes in the state of São Paulo. In Brazil, 70% of all homicides are gun-

[11]This data is provided by the Secretaria de Segurança Pública de São Paulo since 2002.

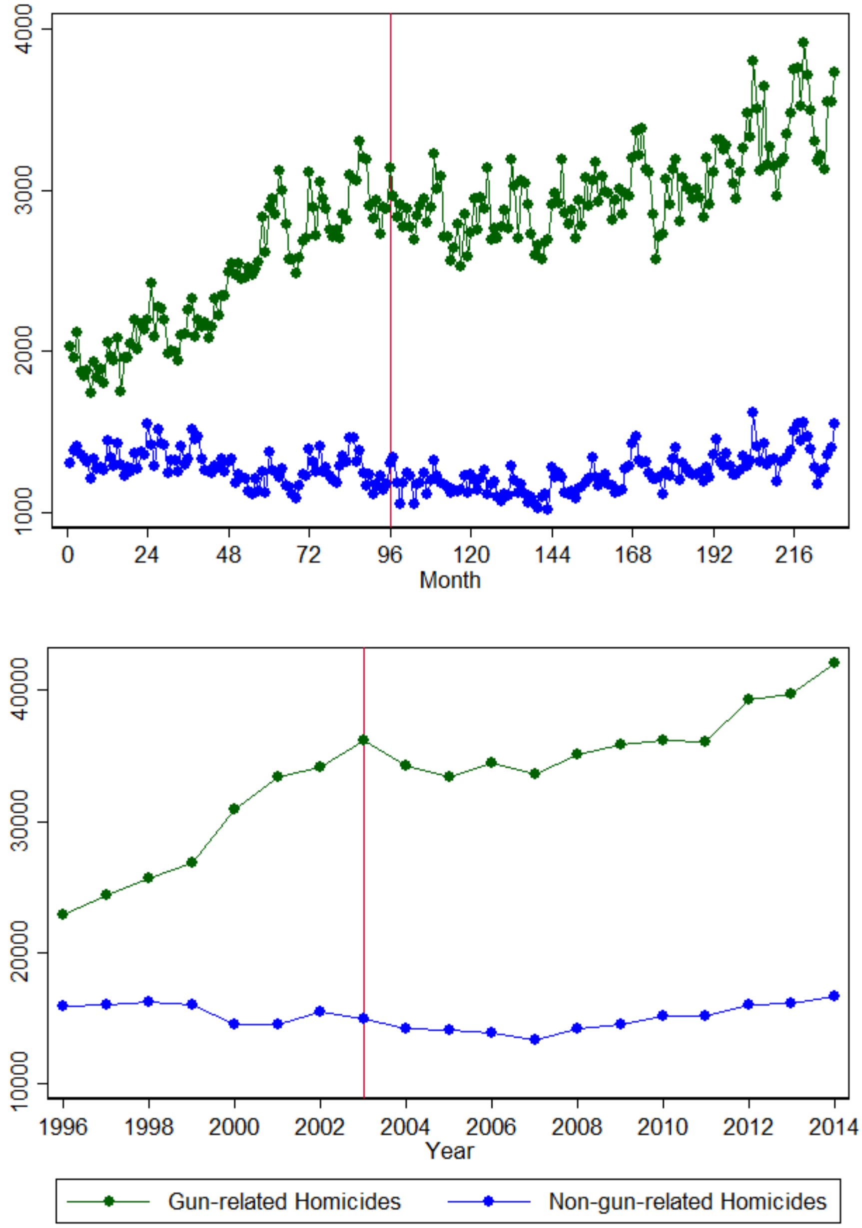

Figure 1: The top (bottom) graph shows the monthly (annual) number of gun-related and non-gun-related homicides in Brazil surrounding the enactment of the gun carrying prohibition law (i.e., red vertical line).

related homicides and even though Brazilian population at the time had 50% and 48% of its population declaring themselves as white and black respectively, the black population is over-represented as victims of gun-related homicides. Also, it is possible to notice that males and young people between 15 and 29 years old are more likely to be victims of gun-related homicides and, out of the total gun-related homicides that had their location identified, 85% of the cases took place outside the residence or place of business. Non-homicides crimes, which are only available for the state of São Paulo, has theft as its most frequent occurrence, followed by robbery. Robberies, in contrast to thefts, involve criminal and victims' interaction with force, intimidation, and/or coercion, so criminals often use guns in these situations. Therefore, if the concealed carry ban were effective, one would expect gun-related homicides, robberies and illegal gun carrying to be more affected than other non-gun-related crimes.[12]

I use a regression discontinuity where time is the running variable to evaluate the impact of the concealed carry prohibition on crimes. This method, also known as an Interrupted Times Series (ITS), has been widely used to estimate the effects of economic policies (e.g., Carr and Packham 2019; Auffhammer and Kellogg 2011; Davis 2008; Gallego et al. 2013). ITS can be interpreted as a sub-type of regression discontinuity, in which time is the assignment variable, and the cutoff is defined as the date when a new policy is implemented. Formally, my empirical model is constructed as the following:

$$GRH_{mt} = \sum_{k=0}^{p} \lambda_k r_t^k + D_t \sum_{k=0}^{p} \gamma_k r_t^k + \theta_{mt} + \omega_m + \delta_w + \epsilon_{mt}, \tag{1}$$

such that: $(c - h) \geq r_t \leq (\ c + h)$.

Where GRH_{mt} is the number of gun-related homicides at municipality m and period t (i.e., period t is defined by combining a specific month w of a given year y), c represents the cutoff (January 2004), r_t indicates the periods surrounding the cutoff, D_t is a dummy indicating that the prohibition of right to carry concealed weapons became effective, p indicates the order of the polynomial and h represents the selected bandwidth (in months). γ_0 captures the law effect and γ_1 captures the slope change after the reform when the model is

[12]The annual average, at the municipality level, of gun-related homicides and non-gun-related homicides in Brazil in 2003 was 6.45 and 2.66 respectively. The annual average, at the municipality level, of robbery, theft, rape, drug trafficking and illegal gun carrying in São Paulo in 2003 was 515.2; 1000.8; 6.16; 21.6 and 26.7 respectively.

Table 1: Descriptive statistics of crime in Brazil and the state of São Paulo in 2003

	Crime	Total
Homicides - Brazil		
	Gun-Related Homicides	36,115
	Non-Gun-Related Homicides	14,928
Characteristics of Gun-Related Homicides' Victims		
Race		
	Black	56.2%
	White	36.6%
	Other	7.2%
Age		
	Less than 15	1.3%
	Between 15 and 29	59.2%
	More than 29	39.5%
Locality		
	Residence or place of business	8%
	Out of residence or place of business	45.3%
	Non-identified	46.7%
Gender		
	Male	94%
	Female	6%
Non-Homicides - São Paulo		
	Robbery	332,229
	Theft	645,529
	Rape	3,978
	Drug Trafficking	13,935
	Illegal Gun Carrying	17,253

estimated using p=1 (I also estimate a quadratic and cubic models, i.e., p=2 and p=3, to allow unobserved variables to vary non-linearly). θ_{mt} contains monthly data for temperature and rainfall accumulation for each municipality m at period t.[13] ω_m and δ_w are, respectively,

[13] I control for rainfall and temperatures because weather is related to crime (Cohn 1990). Monthly rainfall and temperature data were collected from Matsuura and Willmott (2009). The authors provide estimations

municipalities and calendar months fixed effects to capture time-invariant unobservables and any seasonal effect. Finally, ϵ_{mt} contains the error term for each observation.[14]

I estimate equation (1) using a negative binomial regression given the count nature of crime data (for a detailed discussion on best estimator for crime data, see Osgood 2000). I do not choose Poisson regression because the variance of homicides is larger than its mean. However, all results are consistent with a choice of Poisson regression or OLS.[15]

4 Results

4.1 Homicides - Brazil

Using the regression proposed in equation (1), I estimate the impact of the law on total homicides, gun-related homicides and non-gun-related homicides. Figure 2 shows a graphical result considering a deseasonalized data for each crime and three different bandwidths (48, 24 and 12 months). Explicitly, I first regress on calendar months and weather variables each one of the dependent variables analyzed in this figure. Then, I collect the part of this analysis that cannot be explained by calendar months and weather variations and use it as the dependent variable.[16] For each type of crime and bandwidth, an RD estimation of the law's impact, taking seasonality into account, is created. This figure indicates that

of monthly worldwide precipitation and temperature data at the 0.5 x 0.5 degree level. Each point is characterized by a specific geographic coordination (latitude and longitude), and the monthly precipitation and average temperature for each point is associated with the rainfall and temperature data collected from its 20 closest weather stations.

[14]Studies examining crimes usually restrict their sample because of few occurrences. For instance, Cook and Ludwig (2006) examine the impact of number of guns on crimes using gun-related suicides to suicides ratio as proxy for number of guns and consider only the 200 counties with the largest populations in the United States. Because the number of homicides is not as uncommon as suicides, I consider municipalities with more than 10,000 inhabitants. Municipalities with more than 10,000 inhabitants account for 92.4 percent of the total Brazilian population; nearly all, 98 percent, of gun-related homicides occur in these areas. I show in the Appendix (Table A1) that choosing different threshold options (no population restrictions or municipalities with more than 50,000 inhabitants) does not change my results.

[15]Negative binomial, as well as Poisson regression, drop municipalities that contains all zero outcomes, mitigating concerns about small size municipalities that rarely experience homicide.

[16]The main goal of this approach is to remove seasonality from the variation in homicides, this exercise is similar to Kleven et al. (2014) and Schneider et al. (2019), however, these authors focus on removing pre-trend rather than season from their analysis.

the reduction in homicides that followed the prohibition of gun carrying was driven by gun-related homicides. In contrast, non-gun-related homicides, which captures potential confounding variables related to crime that could be changing simultaneously with the law, are continuous around the cutoff. This mitigates concerns of endogeneity problems and also suggest that there was no weapon substitution effects, i.e., there is no evidence that criminals responded to the higher cost of using guns to commit homicides by replacing them with knives or other cutting instruments.

Table 2 reports results of the effect of the law considering changes in the slope of the time-series after its enactment and selection of different bandwidths and polynomial order of the running variable.[17] Before proceeding further with Table 2 analysis, it is important to comment on two facts. First, gun-related suicides were not affected by the law that prohibited gun carrying, which reinforces the argument that a lower number of people carrying guns is the mechanism explaining the reduction in gun-related homicides.[18] Second, not taking seasonality into account decreases the magnitude and significance of the gun-related homicides coefficient, suggesting that seasonality plays an important role: the decrease in gun-related homicides in January (when the law became effective, coinciding with the beginning of summer in Brazil), a month in which this variable would usually reach its annual peak, shows the strength of the law.

The results on Table 2 show a strong relationship between the law and gun-related homicides. Panel A, Column 3 of the first row, indicates that the legislation decreased gun-related homicides by 12.2%.[19] As in 2003 Brazil had 36,115 gun-related homicides, then close to

[17]The optimal bandwidth as suggested by Calonico et al. (2017) is 22 months. The results are very close to the one obtained in Table 2, Panel B (i.e. 24 months bandwidth). I use 12 months bandwidth as my main estimation to guarantee that the gun carrying prohibition is the solely responsible for the decrease in gun-related homicides.

[18]The coefficient measuring the impact of prohibiting carrying concealed weapons on gun-related suicides was -0.18 (p-value = 0.3) for a bandwidth of 12 and a sample restricted to municipalities with more than 50,000 people. As gun-related suicides are a rare event (average of 1,364 cases per year), I also restricted the sample to municipalities with more than 100,000 people and obtained similar coefficient. This result contrasts with the findings of Leigh and Neill (2010) showing that the gun buyback in Australia reduced gun-related deaths, but mostly as a result of a sharp decline in suicides.

[19]As the coefficient of interest is a dummy variable, the interpretation of the negative binomial estimation is intuitive. The percentage change in gun-related homicides is equal to $e^{\hat{\beta}-1}$.

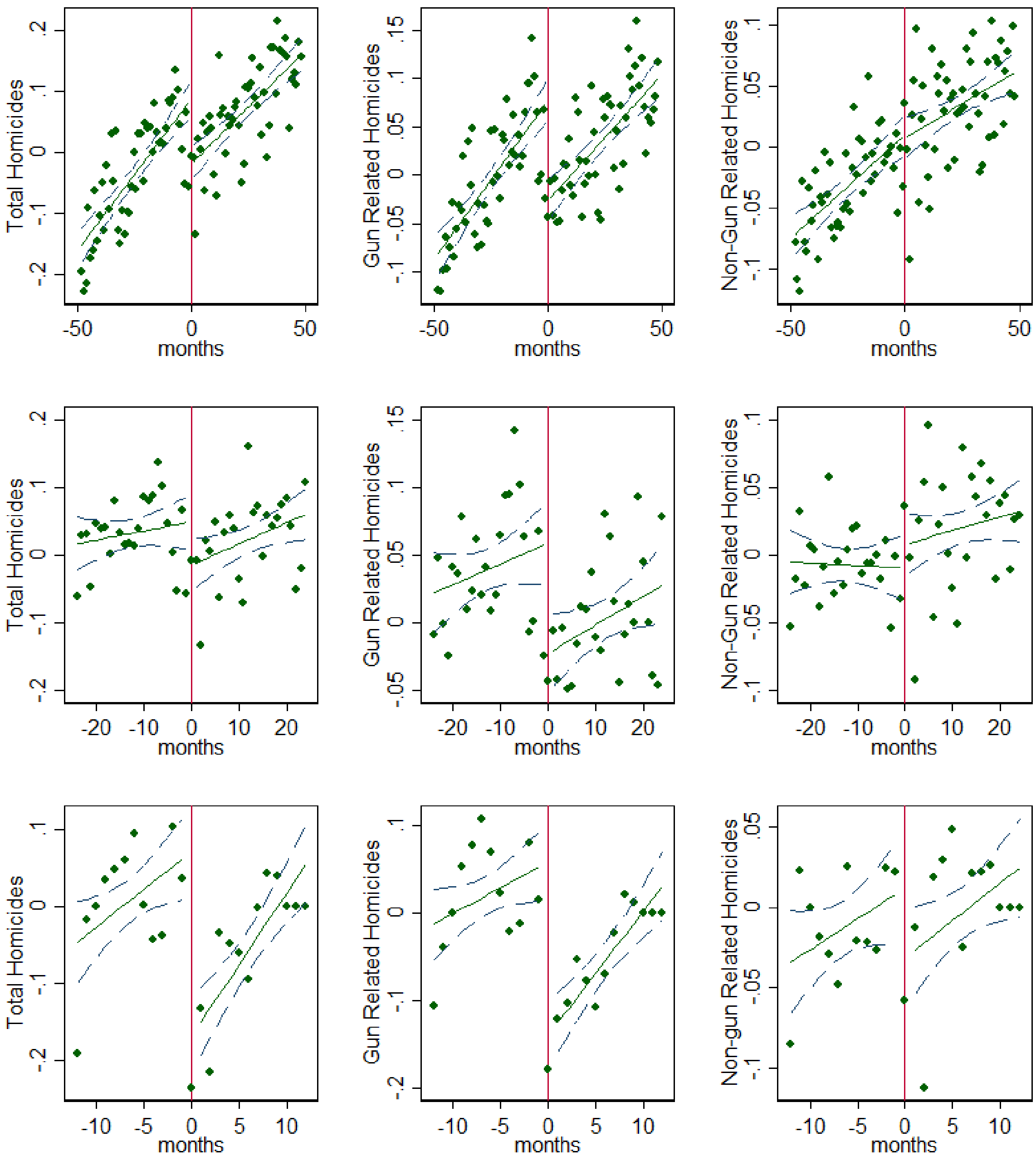

Figure 2: The solid line is fitted separately on each side of the threshold, and the dashed line represents the 95% confidence interval. The scatter plots show monthly averages. I regress the predicted residuals after regressing the dependent variables on calendar months, monthly rainfall and temperatures to take seasonality into account, with standard errors clustered at the municipality level.

Table 2: RDD estimating the concealed carry prohibition effect on Gun and Non-Gun-related homicides

VARIABLES	(1) Total Homicides	(2) Non-gun Related Homicides	(3) Gun Related Homicides
Panel A - 12 Months			
$\hat{\gamma_0}$ - Law Effect, linear	-0.076***	0.040	-0.130***
	(0.028)	(0.049)	(0.034)
$\hat{\gamma_1}$ - Slope	0.005**	0.004	0.006**
	(0.002)	(0.004)	(0.003)
$\hat{\gamma_0}$ - Law Effect, quadratic	-0.063	-0.011	-0.096*
	(0.042)	(0.073)	(0.051)
$\hat{\gamma_0}$ - Law Effect, cubic	-0.186***	-0.123	-0.226***
	(0.052)	(0.090)	(0.063)
Panel B - 24 Months			
$\hat{\gamma_0}$ - Law Effect, linear	-0.053***	-0.014	-0.070***
	(0.014)	(0.023)	(0.017)
$\hat{\gamma_1}$ - Slope	-0.003***	-0.000	-0.005***
	(0.001)	(0.001)	(0.001)
$\hat{\gamma_0}$ - Law Effect, quadratic	-0.026	0.069**	-0.071***
	(0.021)	(0.034)	(0.025)
$\hat{\gamma_0}$ - Law Effect, cubic	-0.028	0.073	-0.075*
	(0.032)	(0.052)	(0.039)
Panel C - 48 Months			
$\hat{\gamma_0}$ - Law Effect, linear	-0.077***	-0.034**	-0.099***
	(0.010)	(0.016)	(0.012)
$\hat{\gamma_1}$ - Slope	-0.004***	-0.003***	-0.005***
	(0.000)	(0.001)	(0.000)
$\hat{\gamma_0}$ - Law Effect, quadratic	-0.043***	-0.006	-0.063***
	(0.015)	(0.024)	(0.019)
$\hat{\gamma_0}$ - Law Effect, cubic	-0.022	0.051	-0.064**
	(0.021)	(0.033)	(0.026)

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis. Each Panel shows different sets of bandwidth selection. In each set, the first two coefficients come from the same regression while coefficients in the third and fourth lines (Law Effect, quadratic and Law Effect, cubic) come from two other regressions allowing the running variable to vary, respectively, quadratically and cubically. Panel A, columns 1, 2 and 3, contain, respectively, 63,406, 56,558 and 54,131 observations. Panel B, columns 1, 2 and 3, contain, respectively, 133,181, 125,978 and 121,198 observations. Panel C, columns 1, 2 and 3, contain, respectively, 272,537, 266,385 and 257,186 observations. The number of observations changes across columns because the Negative Binomial model excludes municipalities that had zero cases of homicides within the time frame analyzed. All regressions control for municipalities fixed effects, calendar months, rain and temperatures. All municipalities with more than 10,000 people are considered. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

4,400 lives were saved in 2004 due to the implementation of the law. Extending the window of my analysis, as shown in the fifth and ninth rows of Table 2, attenuates the effects of the law on gun-related homicides to an annual reduction of 6.8 to 9.4 percent. The coefficient that captures the change in the slope of the homicides trend (i.e., γ_1) after the enactment of the law shows a positive change in Panel A, however, when allowing for a larger window of analysis (i.e., Panels B and C), a significant and negative coefficient is estimated suggesting that the gun legislation decelerated the gun-related homicides growth in the country. Table 2 also shows the results for modeling equation (1) using polynomials of order 2 and 3, third and fourth row of each Panel, and the impact of the law on gun-related homicides remained negative and significant across these models specifications.

In the appendix, Table A1, I find that the estimations are not sensitive to changes in the population size restrictions, different choices of regression models, aggregating homicides at the national level and adding the monthly average crime for the entire period as control. I also show an event study exercise, which results are presented in Figure A2, that analyze the yearly change in gun-related homicides and find similar results to the one reported in Table 2. Finally, Table A3 in the appendix shows that non-fatal gunshots intended to kill were also reduced (by 16.3 percent) in response to the gun legislation, but accidental non-fatal gunshots were not affected suggesting that prohibiting the carrying of concealed weapons affects not only fatal, but also non-fatal shootings intended to kill.

The proposed regression discontinuity approach has an identification assumption that is not as strong as a difference-in-differences model. While the latter imposes that unobserved variables related to crime vary linearly, the former allows unobserved factors to act nonlinearly overtime. Thus, the regression discontinuity model allows me to eliminate potential endogenous relationship between the error term and the time of the enactment of the law by allowing the function of the running variable to vary flexibly across the cutoff. This empirical strategy, however, needs to rely on the assumption that there were no downward time trend in crime prior to the enactment of the law. I check the validity of this requirement following Carr and Packham (2019). I randomly select a date prior to treatment, (i.e., from 1996-2003) and use it as a cutoff. Then, I analyze how often a 1,000 random draws yield absolute treatment effects smaller than the one estimated in my main specification.[20] Figure 3 shows the distribution of the placebo estimates, where only 4.2% of them are smaller than the main coefficient (i.e. -0.13).[21]

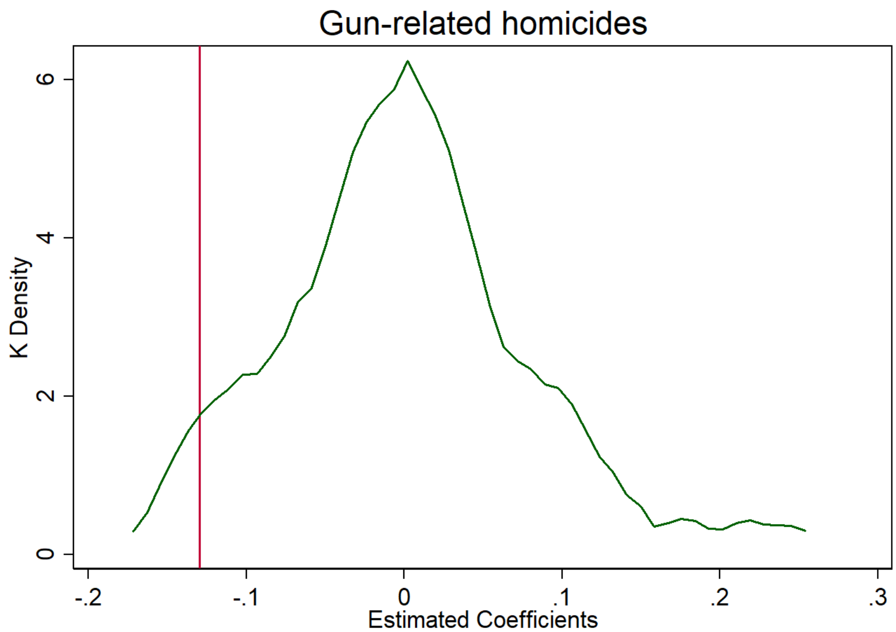

Figure 3: The figure plots the distribution of 1,000 randomly drawn placebo estimates from the regression discontinuity specification in Equation 1 using pre-period gun-related homicides data and a 12 months' bandwidth. The vertical redline represents the baseline estimate reported in the first row (third column) of Table 2 (i.e. -0.13). Only 4.2% of these estimates are smaller than the baseline.

Next, I examine which population group benefited the most from the gun carry prohibition. I use the same RDD proposed in equation (1), but split the sample by age and race of gun-related homicides' victims, and also by the location where gun-related homicides took place. The reduction in gun-related homicides was especially pronounced among young black

[20] Notice that I estimate equation (1) using a randomly assigned *placebo* month of the enactment of the law out of the 96 months contained in the dataset prior to the law. I then repeat this random process 1,000 times to construct the distribution analyzed in this work.

[21] In Figure A1 in the appendix, I run the regression described in equation (1) but using January of other years as cut-offs (1996-2012) and show that, except for January 2004 when the law became effective, there was no significant decrease in gun-related homicides.

males and within occurrences that take place outside the residence.

Table 3, Panel A, shows that the gun legislation effect on gun-related homicides is driven by blacks.[22] Although *only* 56 percent of the victims of gun-related homicides are blacks (Table 1), the effects of the concealed carry prohibition surface almost exclusively among this segment of the population that had a reduction of 15.9% in the number of gun-related homicides. This result is similar to Williams Jr (2017)'s work investigating the impact of weakening (instead of strengthening, as this work does) gun-control laws (in Missouri, U.S.), which caused an increase in homicides exclusively among blacks. The author explains this result using O'Flaherty and Sethi (2010)'s theoretical model showing that making access to guns easier, when cases involving black victims of homicides are less likely to be solved and less aggressively prosecuted than cases where the victims are white, increase the murder rate among the former group disproportionately more than the latter. This scenario reflects the U.S. as well as the Brazilian reality where blacks are more likely to be victims of crime with impunity (Ozemela et al., 2019). Thus, the result presented in Table 3 is consistent with the prediction that one should find a disproportionately larger decrease in murder rate among blacks when gun access becomes stricter. Panel B of Table 3 suggests that young people (between 15 and 29 years of age) benefited more from the law, and experienced a 14.6% decline in gun-related homicides. Finally, Panel C of Table 3 shows that only gun-related homicides happening in "out of home" locations were affected by the concealed carry prohibition (16% reduction). This should be expected because the law did not prohibit gun ownership, but carrying them outside the residence or place of business.

In the appendix, Table A2, I show that the effect of the prohibition of gun carrying is related to baseline crime rates. I split off the sample between quartiles according to the distribution of gun-related homicides per 100,000 residents between 1996 and 2003. The effects of the legislation are driven by the last quartile that splits off the highest 75 percent of municipalities according to gun-related homicides rates. This result is reinforced by a

[22]The number of observations, located in the last row, changes across columns because the Negative Binomial model excludes municipalities that had zero cases of homicides within the time frame analyzed.

Table 3: Gun-related homicides' victims by race, age and locality

	Panel A		
Variables	(1) White	(2) Black	(3) Other
Concealed Carry Ban	-0.054	-0.173***	0.251
	(0.054)	(0.044)	(0.554)
Observations	38,921	41,660	2,600

	Panel B		
Variables	(1) 15 to 29 years	(2) More than 29 years	(3) Less than 15 years
Concealed Carry Ban	-0.158***	-0.068	-0.128
	(0.043)	(0.048)	(0.233)
Observations	43,108	45,171	7,399

	Panel C	
Variables	(1) Out of home	(2) Residence
Concealed Carry Ban	-0.174***	-0.001
	(0.052)	(0.111)
Observations	39,020	24,635

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis. Bandwidth is equal to 12 months. All regressions control for municipalities fixed effects, calendar months, rain and temperatures. All municipalities with more than 10,000 people are considered. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

difference-in-differences model constructed in Table A2 where crime rate is used as a continuous treatment variable. Combined, these results suggest that the effects of the concealed carry prohibition were pronounced among young black males living in crime-ridden areas, and within crimes taking place outside the residence.

4.2 Non-Homicides - São Paulo

In Brazil, each state is responsible for providing its own public security and São Paulo is the only state to provide monthly data on crime since 2002. The data provides information on theft, robbery, homicides, rape, drug trafficking and illegal gun carrying. If the mechanism explaining the reduction in the number of gun-related homicides is a decrease in the number of people carrying a gun, then one should observe a reduction in illegal gun carrying, as well as crimes that more often involve firearms, such as robberies. Crimes less likely to involve guns such as theft and rape should not be affected if there are no confounding variables changing with the law. Figure 4 shows a regression discontinuity estimation for each crime considering a two-year window around the treatment start date and corroborates the aforementioned hypothesis.

Table 4 shows the impact of the law on the six crimes considered. As one can notice, the concealed carry prohibition decreased the number of crimes related to guns.[23] The monthly data show that illegal gun carrying decreased by 14.3%, robberies by 12.5% and homicides by 13.6%. Findings reported in Table 4 corroborates the Disarmament Statute analysis (Paz 2010) claiming that there was a reduction in gun carrying. This is a particularly strong result because, according to the study, police officers intensified the search for illegal gun carrying after the law passed. This suggests that the mechanism explaining the decrease in gun-related homicides is a decrease in gun-carrying. Although the sample covers only

[23] In the appendix, in Figure A3, I show evidence that there was no sharp increase in arrests in the State of São Paulo after the enactment of the law mitigating concerns with the possibility that the mechanism explaining the results is through an increase in incarceration with no possibility to bail right after the law was passed and, that way, gun-related crimes would have been avoided by arresting perpetrators before they commit the crime.

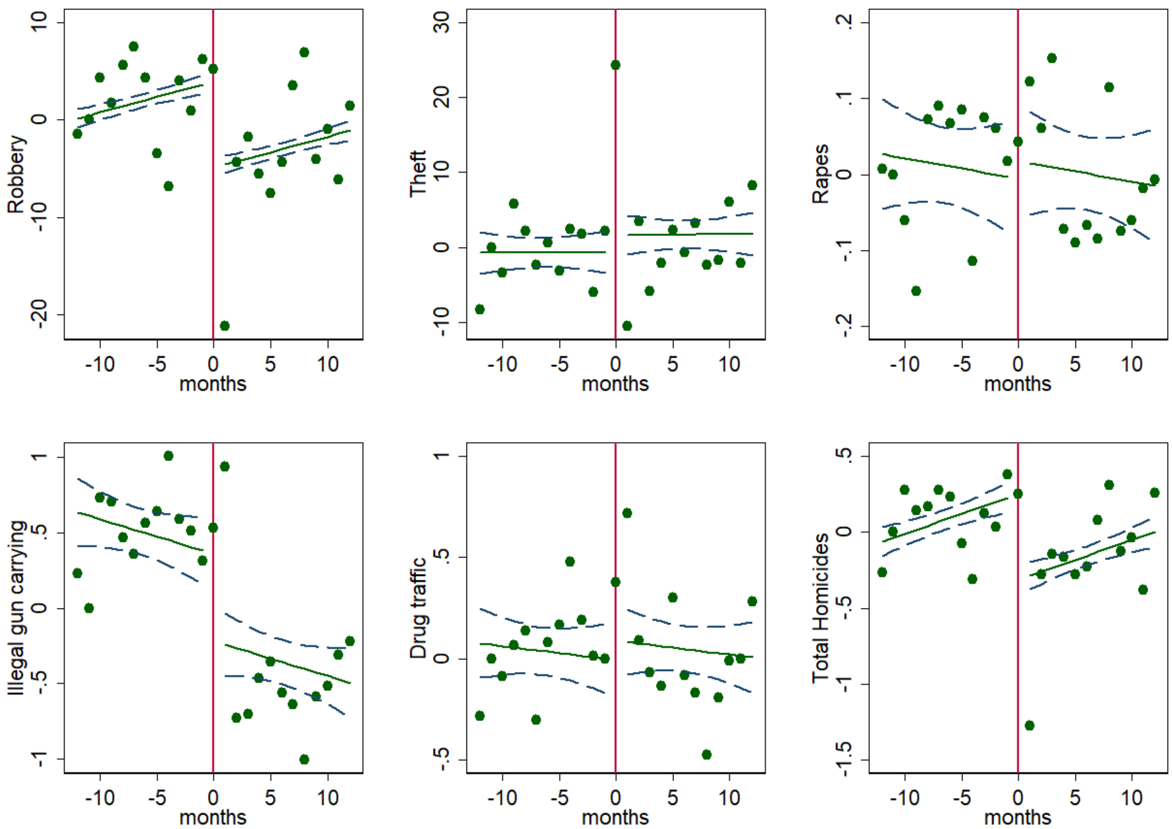

Figure 4: The solid line is fitted separately on each side of the threshold, and the dashed line represents the 95% confidence interval. The scatter plots show monthly averages. I regress the predicted residuals after regressing the dependent variables on calendar months, monthly rainfall and temperatures to take seasonality into account, with standard errors clustered at the municipality level.

São Paulo state, which is not representative of the entire country (relatively rich state and the most populous of Brazil), one can gain some insights into the mechanism explaining the reduction in gun-related homicides, especially because the decrease in homicides observed in this state is similar to the one found across the country.

As additional robustness check, I provide in the appendix an analysis that aggregate the municipal data at the State level (Table A4) and another one that allows the response to the law to vary within the types of crimes considered (Table A5). Aggregating the data at the State level (Table A4) do not alter the results, however, it allows me to add a control for State expenditure on security (in Brazil, States are responsible for proving security for all municipalities within their territories), which also do not alter the main results.[24] Finally, in Table A5, I present a difference-in-differences analysis that use non-gun-related crimes as control group and show that, compared to the control group, gun-related crimes decreased in response to the law.

Combined, the results presented in this section show insights on gun carrying regulation that add, in four different ways, to the previous literature due to the design of the Brazilian law and the availability of high-frequency, granular data on crime. First, by studying a law that prohibited gun carrying and imposed an immediate response on law obeying citizens, it was possible to use a regression discontinuity design that through the analysis of a short time-window better identifies the impact of the law (Davis, 2008) and lessen the strength of the identification assumption by allowing unobservable variables to vary non-linearly across time (Auffhammer and Kellogg, 2011). Second, using data on many types of crimes, gun deaths and crime location, it was possible to show evidence of a mechanism explaining the reduction in gun-related homicides. Namely, a decrease in gun carrying. Not only the empirical evidence shows that after the law less people were caught carrying a gun by the police (even though police were searching more for this type of crime), but it also shows evidence consistent with this proposed mechanism as non-gun-related crimes were not affected and gun-related deaths (homicides and suicides) inside the residence, as people could still own a gun, did not decrease.

Third, by analyzing gun-related homicides victims, it was possible to learn that young black males were disproportionately benefited by the law as this population had a larger decrease in gun-related homicides. Lastly, results were robust across different model specifications. Similar results were found using different types of regression models (OLS, Poisson and Negative Binomial), population and time restrictions, aggregation of the dependent variable and three different econometric designs (RDD, DID and event study).

[24]The State analysis in Table A4 also allows me to use monthly labor market outcomes, only available at the State level, to test whether there was a break in the trend of employment when the law was enacted. However, I do not find any significant effects.

Table 4: Estimating the concealed carry prohibition effect on non-homicides crimes

VARIABLES	(1) Robbery	(2) Theft	(3) Rape	(4) Illegal Carry	(5) Drug Traffic	(6) Homicides
Concealed Carry Ban	-0.134***	-0.006	-0.078	-0.154**	-0.096	-0.147**
	(0.029)	(0.018)	(0.098)	(0.061)	(0.059)	(0.070)
Observations	8,826	8,826	7,863	8,826	8,714	8,226

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis. Bandwidth is equal to 12 months. All regressions control for municipalities fixed effects, calendar months, rain and temperatures. All municipalities with more than 10,000 people are considered. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Finally, as aforementioned, the last part of the gun legislation examined in this paper set the stage for a national referendum to take place in October 2005 (22 months after the initial legislation was passed into law). This referendum allowed Brazilian citizens to vote on whether owning guns and ammunition should be prohibited across the country. In the appendix, Section C, I provide an analysis of the results of this referendum at the municipality level, together with a survey at the individual level that allows me to tie individual characteristics with support for stricter gun legislation. Although this exercise does not establish causality, it provides suggestive evidence that exposure to gun violence is strong and positively related to support for laws that preclude access to guns.

5 Discussion and concluding remarks

Many countries have gun regulations, and measuring their impact is both important and extremely difficult. Laws that give people the right to carry guns are the most-studied gun regulations (Leigh and Neill 2010). Nonetheless, as Manski and Pepper (2018) argue, it is not possible to make any conclusions about the effects of such laws without making strong assumptions. Showing that different assumptions lead to different conclusions about the impact of gun laws on crime rates, they conclude by saying "...we do not report findings with incredible certitude: there are no simple conclusions." However, certain aspects of Brazil's gun legislation allow one to circumvent problems that have plagued other natural experiments, and, thus, allow for a window onto the issue that offers clearer insights and conclusions.

This paper provides the first regression discontinuity design analysis of the impact of a gun carrying ban on crime. I find that gun-related homicides fell by 4,406 (12.2 percent of the total number of such homicides in the country) in the year following the regulation. The paper shows that young black males living in high-crime areas disproportionately benefited from the regulation because the reduction in gun-related homicides was particularly pronounced among that population. The research also shows that non-gun-related homicides were not affected by the regulation.

The most plausible mechanism explaining the reduction in gun-related homicides is a decrease in the number of people carrying guns. Analyzing non-homicides data, I find that the gun legislation decreased illegal gun carrying, and robbery (type of crime that more often involve guns). However, reported rapes, thefts and drug-trafficking incidents were not affected. Finally, when analyzing homicides data and their location, I find that only the ones occurring outside the residence were affected by the law, corroborating the narrative that less people carrying guns explain the reduction in gun-related deaths (the finding that gun-related suicides were not affected also corroborates the narrative).

The economic value of the regulation I study can be estimated using the literature on the value of a statistical life. In Brazil, estimations of the value of statistical life vary from $0.77 million to $6.1 million (Arigoni Ortiz et al. 2009). Using the most conservative value and my estimation for the reduction in gun-related homicides caused by the regulation, I can make the following claim: The prohibition of the right to carry concealed weapons generated an economic value of $3.4 billion in one year. This number is about six times the value of the Australian gun buyback (Leigh and Neill 2010). Although, the decrease in the number of gun-related deaths per year attributed to the gun buyback in Australia was much smaller and different in nature (200 and mostly suicides) than the decrease estimated in this work (4,406 and mostly gun-related homicides), the value of statistical life in Australia is close to $2.5 million, i.e., 3.2 times larger than the amount I use to generate my estimation for Brazil. My calculation, therefore, could be understated because I considered only the most conservative value of statistical life.[25]

The impact of prohibiting concealed carry measured in this work could be larger in a context such as those with easier border controls and more effective policing. Leigh and Neill (2010) conclude their work by saying that extrapolating their results to other countries is not trivial. First, Australia does not have land borders, making it easier to control illegal firearm imports, and secondly, its government and policing services are highly organized and effective. Brazil, on the other hand, does not have these advantages. Therefore, prohibiting gun carrying in a country with easier border controls and more effective policing could provide a larger decrease in gun-related homicides.

[25]This calculation is also likely to be underestimated because I find (appendix, Table A3) that gunshot wounds intended to kill were reduced by 16.3 percent in 2004. The total health spending in gunshot wounds intended to kill in 2003 was 13.2 million Brazilian Reais (equivalent to $4.6 million at that time). Therefore, the law generated an additional economic value of $750 thousand through this channel, which does not consider the days of work missed by the gunshot wounds' victims while they were hospitalized and during their post-hospital recovery, nor it does consider the rehabilitation costs (such as medical drugs).

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Appendix A: Placebo and Alternative Specifications

In this section I show a placebo test followed by an analysis corroborating that the impact of prohibiting concealed carry on gun-related homicides is not sensitive to varying the model presented on Table 2. Figure A1 shows estimated coefficients of the dummy for enactment of the law banning gun carrying following the specification in equation 1. Using a 12-months bandwidth, but varying the enactment of the law to consider all months of January in the sample as "placebo" enactments of the law, I find, except for January 2004 when the law became effective, that there was no significant decrease in gun-related homicides in the placebo enactments. Table A1, test for alternative specifications. Rows 2 and 3, reports that removing population size restrictions or adding more population size restrictions do not substantially change the results. Using an OLS regression with fixed effects at the municipality level (row 4) I find that gun-related homicides were reduced by 8.4%.[26] Using Poisson regression (row 5) attenuates the impact of the gun legislation, however, negative binomial regression is more appropriate for the present work analysis because the variance of homicides is larger than its mean. Finally, both alternative specifications proposed in row 6, i.e., controlling for the monthly average of the type of homicide studied for the entire period and row 7, i.e., aggregating the monthly data at the national level (row 7) provide results that are similar to the main specification.

In order to capture the dynamic effect of the gun carrying prohibition law on yearly gun-related homicides, I present an event study as follows:

$$GRH_{mt} = \sum_{k=-4}^{4} \beta_k 1_{year=k} + \omega_m + \delta_w + \epsilon_{mt} \tag{2}$$

Where GRH_{mt} is the number of gun-related homicides in municipality m and month t, k is the distance to the law from 4 years before to 4 years after its implementation, $1_{year=k}$

[26]The OLS estimate indicates that the legislation decreased the monthly (yearly) gun-related homicides per 100,000 people by 0.151 (1.812) on average. In 2003, Brazil had 167,546,532 people living in municipalities with more than 10,000 inhabitants, so close to 3,035 lives were saved in 2004 due to the implementation of the law, which corresponds to 8.4 percent of the total gun-related homicides in 2003.

is a dummy identifying each year k. ω_m and δ_w control for municipality and month fixed effects respectively. In this specification, the sample is restricted to the 48 months before and 48 months after the law was passed and the year 2002 is set as the reference period.[27] The estimated $\hat{\beta}_k$, for each year k, are presented in Figure A2 which shows that, compared to 2002, gun-related homicides increase in 2003, i.e., 1-year before the enactment of the law, and decrease in 2004, i.e., 1-year after the law was passed. The difference between 2003 and 2004 is significant at the 99% level and equivalent to a reduction of 8.6% in gun-related homicides after the law was passed.[28]

[27] I thank the anonymous referee for proposing this exercise.

[28] The difference between 2003 and 2004 is equal to 0.068 and, as the average of gun-related homicides per 100,000 people in 2003 was 0.79, then this reduction is equivalent to 8.6% from the mean.

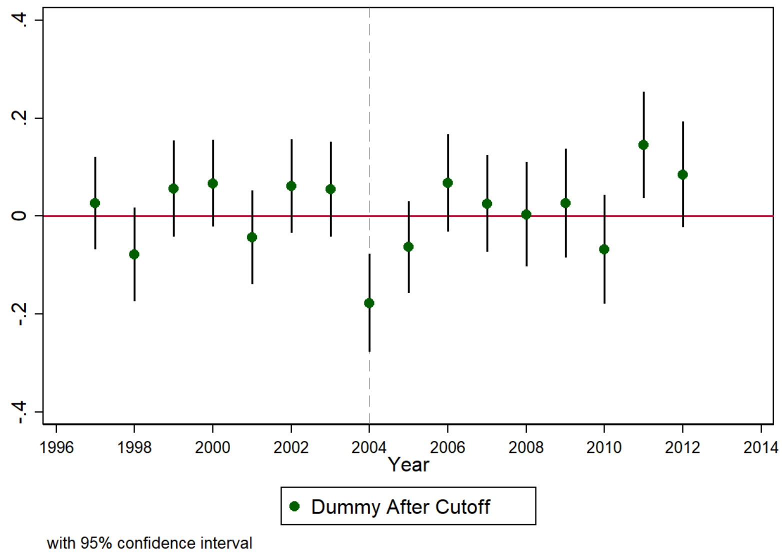

Figure A1: Each point shows, with standard errors clustered at the municipality level, the estimated effect of the gun carrying prohibition law using a 12-months bandwidth and assuming that the law was enacted in January of the respective year described in the horizontal axis.

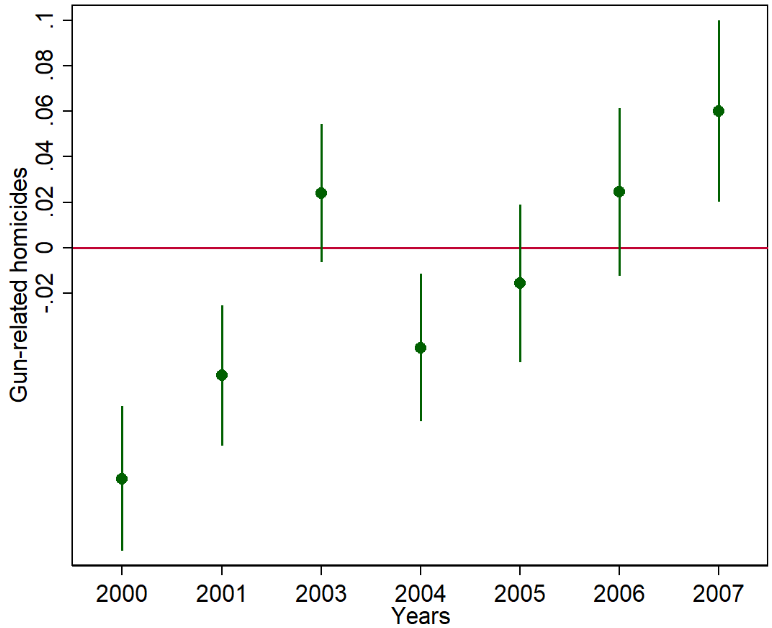

Figure A2: Each point shows, with 95% confidence interval and standard errors clustered at the municipality level, the estimated $\hat{\beta}_k$ for each year k in equation 2 compared to the baseline year (i.e., 2002).

Table A1: RDD estimating the concealed carry prohibition effect on Gun and Non-Gun-related homicides

VARIABLES	(1) Total Homicides	(2) Non-gun Related Homicides	(3) Gun Related Homicides
Baseline	-0.076***	0.040	-0.130***
	(0.028)	(0.049)	(0.034)
No restriction	-0.070**	0.029	-0.117***
	(0.028)	(0.048)	(0.034)
50,000	-0.080**	0.017	-0.116***
	(0.032)	(0.058)	(0.038)
OLS	-0.162**	-0.011	-0.151***
	(0.071)	(0.047)	(0.053)
Poisson	-0.046**	0.055	-0.089***
	(0.023)	(0.042)	(0.027)
Monthly Average	-0.070**	0.047	-0.120***
	(0.028)	(0.049)	(0.034)
National Level	-0.080**	0.019	-0.123***
	(0.033)	(0.056)	(0.034)

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis in rows 1, 2, 3, 5, 6 and 7. Robust standard errors, clustered at the municipality level, are reported in parenthesis in row 4 (i.e, OLS regression). Bandwidth is equal to 12 months. All regressions use municipalities fixed effects and control for calendar months, rain and temperatures. Row 1 uses the baseline estimation presented on Table 2. Row 2 use the same baseline estimation, but do not add population size restriction. Row 3 estimation also use the baseline estimation but restricts the sample to municipalities with more than 50,000. Row 4 and 5 use, respectively, OLS and Poisson regression model (OLS model uses homicides per 100,000 people instead of counts as dependent variable). Finally, row 6 add the average monthly of the type of homicide analyzed in each column for the entire period and row 7 aggregate the monthly data at the national level. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Appendix B: Quartile Analysis, gunshot wounds and additional robustness checks

The subsequent analysis investigates whether the effect of the prohibition of carrying concealed weapons is related to baseline crime rates. To test this hypothesis, I split off the sample between quartiles according to the distribution of gun-related homicides per 100,000 residents between 1996 and 2003. As Table A2 columns 1 to 4 shows, the effects of the concealed carry prohibition are driven by the last quartile that splits off the highest 75 percent of municipalities according to gun-related homicides rates.

In Table A2, column 5, I run an alternative difference-in-differences (DID) model exploring this finding. To do so, I follow Acemoglu et al. (2004) and create a continuous treatment variable that measures exposure to gun violence at the municipality level, based on the gun-related homicides per 100,000 residents between 1996 and 2003. Formally, I run the following DID model:

$$GRH_{mt} = \gamma_0 + \gamma_1 D_t + \gamma_2 HomicideRate_m + \lambda D_t \times HomicideRate_m + \theta_{mt} + \omega_m + \delta_w + \epsilon_{mt}, \quad (3)$$

where GRH_{mt} is the number of gun-related homicides in municipality m and month t. D_t is a dummy identifying when the law became effective, $HomicideRate_m$ is the gun-related homicides per 100,000 residents between 1996 and 2003 in municipality m. λ captures the DID effect, i.e., whether gun-related homicides deferentially decreased within different levels of local gun violence. θ_{mt} contains monthly data for temperature and rainfall accumulation. ω_m and δ_w control for municipality and month fixed effects respectively and ϵ_{mt} represents the error term. In this specification, the sample is restricted to the 12 months before and 12 months after the law was passed.

The main coefficients of the regression proposed in equation 3, i.e., $\hat{\gamma_1}$, $\hat{\gamma_2}$ and $\hat{\lambda}$, are reported in the last column of Table A2. As expected, places with higher levels of pre-policy gun violence had a larger decrease in gun-related homicides. For each additional gun-related homicides per 100,000 people, there was a 0.1% larger reduction in gun-related homicides.

I then examine data on monthly gunshot wounds at the municipality level, which are classified as "accidental" or "intended to kill." Table A3 presents an RDD estimation showing that only the gunshots intended to kill were affected by the law. My estimation indicates that the law caused a reduction of 16.3 percent in the total gunshot wounds in the "intended to kill" category. This evidence suggests that prohibiting the carrying of concealed weapons affects not only fatal, but also non-fatal shootings intended to kill.

Next, I examine data at the State level. First, I show that aggregating the data at this level do not alter the main specification results as columns 1-3 of the first row in Table A4 show. Then, I take advantage of having the data at the State level and use monthly data that are only available at this level to show two additional results: First, when using the total number of monthly laid-offs and hires as dependent variable, there are no discontinuous jump after the enactment of the gun carrying ban as shown in the fourth and fifth columns of the first row in Table A4. Second, Figure A3 presents the evolution of the incarceration rate over the period of analysis and shows no indication of a sharp increase in incarceration after the law was enacted. This mitigates concerns with the possibility that the mechanism explaining the main results is through an increase in incarceration with no possibility to bail right after the law was passed and, that way, homicides would have been avoided by arresting potential perpetrators before they commit the crime. Finally, as States are responsible for proving security for all municipalities within their territories, I use the annual expenditure in security as a control variable and re-estimate the RDD model.[29] In columns 1-3 of the second row in Table A4, I find that my estimates were, if anything, underestimated if expenditure in security were not controlled for.

In the last robustness check proposed in this analysis, I use crimes not affected by the

[29]The State is the responsible for hiring police officers and allocate them across municipalities. The number of police officers allocated in each municipality is not publicly available.

law as control group. Formally, I estimate the following model:

$$Crime_{mtc} = \lambda_0+\lambda_1 r_t+T_{mtc}(\gamma_0+\gamma_1 r_t)+D_t[\alpha_0+\alpha_1 r_t+T_{mtc}(\beta_0+\beta_1 r_t)]+\theta_{mt}+\omega_m+\delta_w+\epsilon_{mt}, \quad (4)$$

where $Crime_{mtc}$ is the number of crime c at municipality m and period t, r_t is the running variable, D_t is a dummy indicating that the prohibition of right to carry concealed weapons became effective (i.e., equal one if after December 2003). T_{mtc} is a treatment dummy that equals one to identify a gun-related homicide and zero to identify a non-gun-related homicide.[30] β_0 captures the law effect on gun-related homicides compared to non-gun-related homicides. θ_{mt} contains monthly data for temperature and rainfall accumulation. ω_m and δ_w are, respectively, municipalities and calendar months fixed effects to capture time-invariant unobservables and any seasonal effect. Finally, ϵ_{mtc} contains the error term for each observation.[31]

The results are presented in Table A5. Panel A considers the entire country and the two types of crimes observed at this level. The first row shows that, gun-related homicides decreases relatively more than non-gun-related homicides after the enactment of the law prohibiting gun carrying. Panel B considers the State of São Paulo and shows that, robbery and illegal gun carrying decreases relatively more than thefts after the enactment of the law prohibiting gun carrying.

[30]When extending the analysis for the State of São Paulo, this variable identifies five different crimes (i.e., robbery, illegal gun carrying, homicides, rape and drug traffic) and the control group, or comparison crime, is theft.

[31]This model is similar to the "difference-in-discontinuities" (or diff-in-disc) proposed in Grembi et al. (2016).

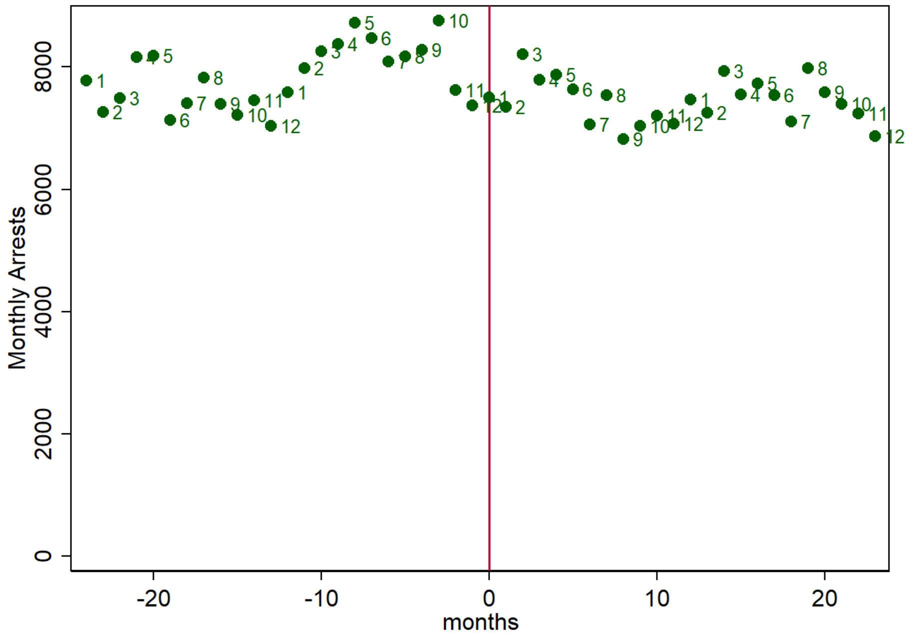

Figure A3: This figure plots the monthly number of total arrests in the State of São Paulo where the red vertical line indicates the enactment of the law banning gun carrying. The number indicates the order that each month appears in a calendar year (e.g., 1 indicates January).

Table A2: Quartile and DID Analysis

VARIABLES	(1) <25%	(2) >25% & <50%	(3) >50% & <75%	(4) >75%	(5) DID
Concealed Carry Ban	-0.006	-0.110	-0.103	-0.130***	0.010
	(0.302)	(0.171)	(0.099)	(0.038)	(0.016)
Homicide Rate					0.044***
					(0.008)
Interaction					-0.001***
					(0.000)
Observations	7,412	13,075	16,272	17,372	54,131

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis. Bandwidth is equal to 12 months. Column 1 splits off the lowest 25% municipalities according to gun-related homicide rates. Column 2 splits off municipalities with gun-related homicide rates larger than the lowest 25%, but smaller than the highest 50%. Column 3 splits off municipalities with gun-related homicide rates larger than the lowest 50%, but smaller than the highest 75%. Column 4 splits off the highest 75% municipalities according to gun-related homicide rates. Column 5 adds a continuous variable that measures pre-policy gun related homicides rate and its interaction with a dummy for enactment of concealed carry ban law. This analysis considers only municipalities with more than 10,000 people. All regressions control for municipalities fixed effects, calendar months, rain and temperatures. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table A3: Impact of prohibiting concealed carry on gunshot wounds (by intention)

VARIABLES	(1) Total gunshot wounds	(2) Accidental	(3) Intended to kill
Concealed Carry Ban	-0.127**	-0.071	-0.178**
	(0.064)	(0.123)	(0.081)
Observations	15,587	10,525	9,225

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis. Bandwidth is equal to 12 months. All regressions control for municipality fixed effects, calendar months, rain and temperatures. All municipalities with more than 10,000 people are considered. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table A4: State Analysis

VARIABLES	(1) Homicides	(2) Non-gun Homicides	(3) Gun Homicides	(4) Layoffs	(5) Hires
Concealed Carry Ban	-0.087**	0.027	-0.137***	0.000	0.067
	(0.038)	(0.058)	(0.045)	(0.051)	(0.064)
Concealed Carry Ban	-0.100***	0.008	-0.142***		
Security Control	(0.038)	(0.058)	(0.046)		
Observations	625	625	625	625	625

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis. Bandwidth is equal to 12 months. Columns 1, 2, 3, 4 and 5 have respectively, total homicides, non-gun related homicides, gun-related homicides, total number of lay-off and hires as dependent variables. All regressions control for State fixed effects, calendar months, rain and temperatures. Row 2 adds total yearly expenditure on security as control variable. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table A5: RDD analysis considering all types of crimes

Panel A - Brazil

Variables	(1) Each Crime Compared to Non-gun Related Homicides
Gun Related Homicides	-0.113***
	(0.040)
Observations	126,814

Panel B - São Paulo

Variables	(1) Each Crime Compared to Theft
Robbery	-0.062***
	(0.016)
Illegal Carry	-0.200***
	(0.025)
Homicides	-0.032
	(0.034)
Rape	0.013
	(0.040)
Drug Traffic	-0.026
	(0.028)
Observations	52,980

Note: Standard errors, calculated using observed information matrix, are reported in parenthesis. Bandwidth is equal to 12 months. Panel A consider all municipalities in Brazil and compare the reduction in gun-related homicides to non-gun-related homicides after the gun carrying prohibition. Panel B consider all municipalities in the State of São Paulo and compare the reduction in robbery, illegal gun carrying, homicides, rape and drug traffic to theft after the gun carrying prohibition. All regressions control for municipalities fixed effects, calendar months, rain and temperatures. All municipalities with more than 10,000 people are considered. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Appendix C: The 2005 Brazilian referendum case

This section investigates whether people exposed to gun violence had greater levels of support for the subsequent referendum proposing to ban all firearm sales in Brazil. I test this hypothesis analyzing data from the actual referendum's result as well as from a survey that took place two days before the referendum and find that people more exposed to gun violence showed greater support for weapons ban.

I first examine the outcome of the Brazilian 2005 referendum proposing a prohibition on the sale of all firearms and ammunition. My dependent variable is the percentage of votes in favor of the prohibition. This data is available, at the municipality level, from the Brazilian Superior Electoral Court (TSE). The control variables are collected from both IBGE and IPEADATA. They are composed of socioeconomic and demographic data.[32] My main explanatory variables are: the logarithm of gun-related homicides per 100,000 people in 2003 (i.e., the year prior to the law that prohibited gun carrying) and the percentage change in gun-related homicides between 2003 and 2004 to capture the first year effect of the law on this type of crime. Controlling for baseline levels of gun violence, I can examine whether places that had a larger reduction in gun-related homicides demonstrated higher levels of support for the referendum.

Table A6 presents an OLS regression using the baseline level of gun-related homicides and the change in this variable after the enactment of the gun legislation to explain the vote in favor of the prohibition. I find a positive relationship between the baseline gun-related homicides and support for gun prohibition suggesting that higher exposure to gun violence is positively related to support for the referendum. Nonetheless, the magnitude of the coefficient is highly sensitive to the inclusion of controls. The coefficient measuring the change in gun-related homicides is also positive (and sensitive to the inclusion of controls)

[32]More specifically, the control variables are mostly collected from the 2000 census and are composed by: population, percentage of people living in rural areas, per capita GDP, ideology, distance to state capital, dummy for drought, dummy for land reform protest, percentage of land bought by the government and redistributed to landless farmers.

indicating that places having an increase in gun-related homicides after the law was passed were more likely to support the referendum. This counter intuitive result can be explained by the possibility that having an increase in gun-related homicides close to the election can motivate people to vote *against* guns. Angatuba, a small town (20,000 inhabitants) in the countryside of the state of São Paulo serves as an anecdotal evidence. Angatuba showed the largest support for gun ban in the São Paulo state, and one way to explain this support is through the gun-related homicide that happened in this municipality one month before the referendum took place (i.e., September 2005). This is especially relevant in this case because Angatuba did not have gun-related homicides since August 2002.

To test this argument across the country, I propose a variable that measures gun-related homicides' annual deviation from the historical average. Formally, this variable is constructed as follows:

$$Std.Homicides_m = \frac{(\sum_{t=1}^{12} Homicides_{mt}) - YearlyHistoricalAverage_m}{StandardDeviation_m},$$

where $Homicides_{mt}$ indicates the number of gun-related homicides for each municipality m at month t. More specifically, $Homicides_{m12}$ represents the number of gun-related homicides, in municipality m, and at the month in which the referendum took place (i.e., t=12). The Yearly Historical Average and standard deviation considers the period between 1996 and 2005. The monthly data on gun-related homicides was collected at DATASUS.

This variable is constructed to measure the impact of an abnormal increase in gun-related homicides, within one year of the referendum, on the support for prohibiting gun ownership. Figure A4 presents the estimated coefficient and shows that one annual deviation, considering the sum of all gun-related homicides between November 2004 and October 2005, from the historical mean increases the support for gun prohibition by .73 percentage points. The remaining coefficients, September 2005 to January 2005, show that this effect vanishes as the gun-related homicides' deviation from the historical average takes place further from the referendum, which I test by simulating different months in which the referendum took place (in which October 2005 is the correct month). Combined, these results show that being exposed to gun violence is positively related to support for stricter gun regulations and that positive shocks in gun violence have a short-term effect favoring policies that limit gun ownership.

Finally, I complement the insights provided by the actual referendum result by examining a public opinion survey that took place two days prior to the referendum and asked voters whether they would vote in favor of or against the gun prohibition. The questionnaire also asked voters if they, themselves, were subjected to gun violence or if they had a family member or close friend who sustained a gun injury. The remaining survey questions relevant for this paper asked voters whether they had guns in their homes, and if they were robbed at least once. I also take race into account as blacks were disproportionately affected by the concealed carry prohibition.

As the dependent variable is binary, I use a logistic regression to assess whether groups more likely to be exposed to gun violence, voted more in favor of the gun prohibition. Table A7 shows how personally being exposed to gun injury or having a close relationship with someone exposed to gun violence is an important predictor of casting a vote in favor of the prohibition. People exposed to gun violence were 1.4 times more likely to vote in favor of the prohibition. Additionally, income, gun ownership, being male and at least once robbed were negatively related to voting in favor of the gun ban. Finally, Blacks were also more likely to support the gun prohibition.

Combined, these results suggest that being exposed to gun violence is positively related to support for stricter gun legislation. This is specially relevant in the Brazilian context because the campaign against the gun prohibition used exploitation of fear as its most effective argument against the referendum's proposition (da Cunha 2006). Once advertising campaigns were allowed to take place, three weeks prior to the referendum, the support for the gun ban dropped from 70% to 36% when votes were cast. According to da Cunha

(2006), after the campaign against the prohibition successfully advertised that citizens would become defenseless against criminals if firearms were banned, only people exposed to gun violence were willing to risk voting in favor of prohibiting guns.

The referendum outcome, therefore, underscore potential problems for direct democracy (i.e. referendums and initiatives put directly to voters rather than legislation passed by elected representatives). When the benefits of decreasing negative externalities, in this case gun-related externalities, are concentrated in a share of the population representing less than 50 percent of the voting public, these benefits might be ignored by the majority of voters, especially when they are comfortable with the status quo and/or afraid of changing it for the worse (Gerber 1999). If these externalities are large enough, ignoring them will result in an outcome with a lower social welfare. Therefore, in these situations, referendums should not be used (Maskin and Tirole 2004).

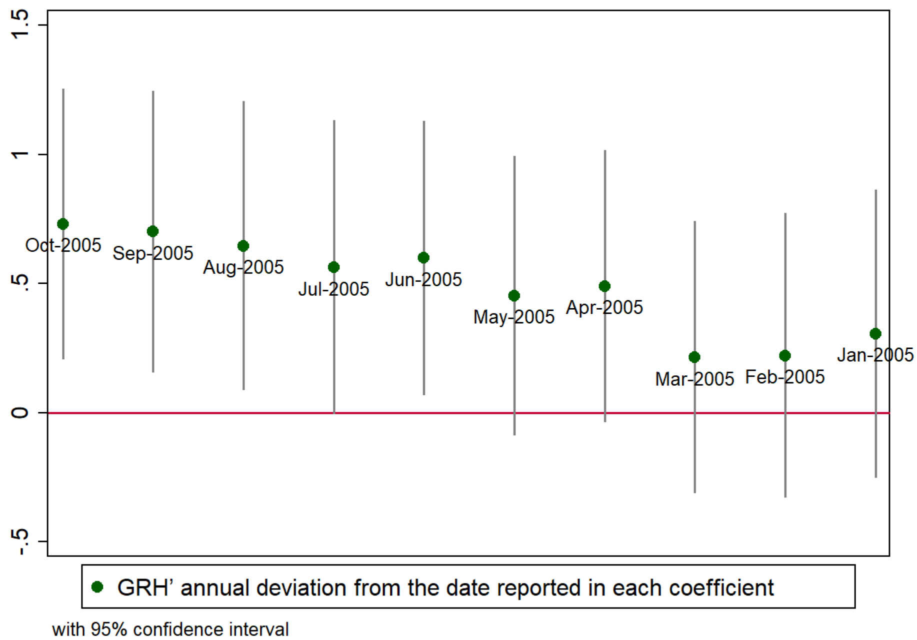

Figure A4: OLS regression showing the relationship between voting in favor of gun prohibition (dependent variable) and gun-related homicides' annual deviation from the historical average within one year from the date reported in each coefficient. All regressions are controlled for population, percentage of people living in rural areas, per capita GDP, ideology, distance to state capital, dummy for drought, dummy for land reform protest, percentage of land bought by the government and redistributed to landless farmers.

Table A6: OLS regression using vote in favor of gun prohibition as the dependent variable

VARIABLES	(1) Vote for Prohibition	(2) Vote for Prohibition	(3) Vote for Prohibition
Baseline	0.864***	0.708***	0.343**
	(0.297)	(0.141)	(0.138)
Change in gun-related homicides	0.357*	0.265***	0.115
	(0.198)	(0.099)	(0.096)
Socio-Economic Controls	NO	NO	YES
State Fixed Effects	NO	YES	YES
Observations	5,560	5,560	5,505

Note: Robust standard errors clustered at the microregion (557 total) level are in parenthesis. Baseline controls for the baseline level of gun-related homicides in 2003. Change in gun-related homicides is the percentage change in gun-related homicides between 2003 and 2004. Controls: population, percentage of people living in rural areas, per capita GDP, ideology, distance to state capital, dummy for drought, dummy for land reform protest, percentage of land bought by the government and redistributed to landless farmers.

Table A7: Logistic regression showing the relationship between exposure to gun injury and voting in favor of the prohibition

VARIABLES	(1) Vote in favor of the prohibition
Monthly household income	-0.098**
	(0.038)
Blacks	0.232**
	(0.097)
Have gun	-1.173***
	(0.204)
Injured by a gun	0.336***
	(0.102)
Age	0.003
	(0.003)
Men	-0.208**
	(0.096)
Robbed	-0.224**
	(0.113)
Observations	1,925

Note: The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.