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## **Crime and political effects of a concealed carry ban in Brazil**

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# **Crime and political effects of a concealed carry ban in Brazil**

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## **Abstract**

This paper studies the effects of legislation in Brazil that banned concealed carry nationwide in 2003, and provided for a voter referendum 22 months later 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. Other crimes involving guns also declined. There is no evidence of substitution effect as non-gun-related homicides were not affected. Two pieces of evidence suggest that the mechanism explaining this result is a decrease in the number of people carrying gun in response to the legislation: first, the number of illegal gun carrying decreased and second, only gun-related homicides taking place outside the residence were reduced. 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.

JEL: D72, H11, I12, J17, K14

**Keywords:** Gun laws; Right-to-carry concealed weapons; Gun-related death; Voting Behavior.

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## 1. Introduction

How do laws that regulate the carrying of concealed weapons affect levels of violence in society? Is violence reduced by enacting laws that allow citizens to carry concealed weapons, or by laws that forbid the carrying of concealed guns? On one hand, allowing citizens to carry concealed weapons 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 the carrying of concealed weapons 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 concealed weapons, but it has proved to be a very difficult subject of study; empirical results are sensitive to minor variations in the data and model specifications, delivering mixed conclusions (Manski and Pepper 2018).<sup>1</sup> As a result, the broad impacts of such laws are not clear, and little is known about who, if anyone, benefits from such legislation, and how this relates to the prospects for and public views of gun legislation.

This paper approaches the question about how concealed weapons laws affect violence by measuring the impact of a nationwide law that banned the carrying of concealed weapons. I examine gun-carrying restrictions that were passed by the National Congress of Brazil, and implemented in December 2003. The legislation prohibited carrying concealed 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. Using a regression discontinuity design analysis, I study the impact of the legislation on crime rates in various communities and populations throughout Brazil. I then use a least square regression to examine and

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<sup>1</sup>See, for example: Lott and Mustard 1997; Ayres and Donohue 1999 and 2003; Black and Nagin 1998; Duggan 2001; Ludwig 1998; Aneja et al. 2011; Donohue and Levitt 1998.

compare voter support for the ban on weapons in municipalities that had varying reductions in levels of gun-related violence.

Provisions of the Brazilian legislation present a rare opportunity to identify the effects of a ban on carrying concealed weapons in a way that avoids some of the problems that have surfaced in analyzing the effects of gun legislation elsewhere. Most research on the impact of right-to-carry concealed weapons 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 concealed weapon. Though extensive research has been conducted, results are inconclusive. Another shortcoming of this literature stems from endogeneity problems, such as gun regulations potentially enacted in response to crime. The Brazilian law, by contrast, *prohibited* the right to carry concealed weapons, and required people to comply immediately - thus allowing for better identification of the law's impacts. This contrasts with the situation confronting researchers analyzing the impacts of laws that authorize the carrying of concealed weapons; even if an applicant meets all requirements and seeks a permit for a concealed weapon license right after the law passes, obtaining the permit and the weapon takes time.<sup>2</sup>

I first analyze the impacts of the gun carrying prohibition following Davis's (2008) empirical strategy of using time as the assignment variable in a regression discontinuity design (RDD).<sup>3</sup> As the law prohibiting the right to carry concealed weapons is a deterministic function of time, there are no

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<sup>2</sup> The literature on the effects of right-to-carry-concealed-weapons laws is concentrated in the United States, where the time to obtain a license varies from state to state, and from place to place within certain states. For instance, 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, according to the California Department of Consumers Affairs.

<sup>3</sup> Lucas Davis investigated the impact that restricting automobile usage in Mexico City had on air pollution. For additional examples of empirical strategies using time as the assignment variable in RDD, see: Anderson 2014; Auffhammer and Kellogg 2011; Bento et al. 2014; Busse et al. 2006 and 2010; Gallego et al. 2013; Carr and Packham 2017.

confounding variables other than time itself, and endogeneity problems are less of a concern.<sup>4</sup> Moreover, because the same law was imposed on all Brazilian municipalities, there is no need for comparisons between treatment and control groups.<sup>5</sup> This eliminates the need to rely on certain controversial assumptions that have hampered the previous literature.<sup>6</sup>

My results show that prohibiting the carrying of concealed weapons decreased gun-related crimes and the economic value of the law, calculated by using the most conservative measures of the value of statistical life in Brazil, 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 were not affected by the law. Using monthly data on non-homicide crimes at the municipal level, provided by the São Paulo state, I show that the prohibition of the right to carry concealed weapons led to a decrease in robberies, and illegal gun carrying.<sup>7</sup> Non-gun-related crimes such as rape, and theft remained unchanged.

I then utilize the subsequent referendum, which asked citizens to decide whether to ban all weapons and ammunition, to examine whether places that experienced greater reductions in gun violence are more likely to support the gun prohibition. I use an ordinary least-square regression (OLS)

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<sup>4</sup> One problem of measuring the effect on crime from laws that give people the right to carry concealed weapons is dealing with the potential endogeneity of such laws (see Durlauf, Navarro and Rivers 2016).

<sup>5</sup> Before the law was enacted, all Brazilian municipalities authorized the carry of concealed weapon.

<sup>6</sup> Manski and Pepper (2018) argue that researchers studying the effects of right to carry concealed weapons laws on crimes in the United States had to rely on strong assumptions such as the assumption that states that enacted right-to-carry-concealed-weapons laws (treatment group) had identical propensities and environments for criminality as those of states that did not enact such laws (control group).

<sup>7</sup> Notice that before the prohibition of right to carry concealed weapons, “illegal gun carrying” referred to the unauthorized carrying of weapons, but after the prohibition, the carrying of weapons – carrying guns either openly or in a concealed way – was prohibited. São Paulo state is the only Brazilian state to provide monthly data on these types of crimes since 2001. I thank *Secretaria de Segurança do Estado de São Paulo* for sending me these data after a formal request.

and find that exposure to gun violence is more important than experiencing gains from the gun carrying prohibition to explain higher levels of support for the referendum on banning guns.<sup>8</sup>

## 2. Related Literature and the Gun Legislation

### 2.1 What are the effects of laws restricting concealed carry? Who are their beneficiaries?

Research by Lott and Mustard (1997) reached the controversial conclusion that laws that gave people the right to carry concealed weapons reduced crime rates in the United States.<sup>9</sup> This finding was challenged by Ayres and Donohue (1999 and 2003) and Black and Nagin (1998), on the grounds that the 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. Other studies have showed empirically that concealed weapon laws do not reduce criminality (Duggan, 2001; Ludwig, 1998; Aneja et al. 2011). 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 concealed weapons laws increased or decreased crimes.

Manski and Pepper (2018) explain how authors find contradictory results using similar data and empirical strategies. The authors' answer to this puzzle is that 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)."<sup>10</sup> Yet, the literature on concealed weapons laws does not find a consensus on credible assumptions regarding

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<sup>8</sup> In the appendix, I corroborate these findings using a survey that took place two days before the referendum.

<sup>9</sup> Using cross-section panel data at the county level from 1977-1992, their findings showed that approximately 1,500 lives would be saved per year if in 1992 all U.S. states had adopted laws allowing people to carry concealed weapons.

<sup>10</sup> The following case illustrates an example of an invariance assumption: Virginia enacted law conferring the right to carry concealed weapon in 1989, but Maryland did not. Then, assume that in the absence of such law, Virginia and Maryland would experience the same changes in crimes between 1988 and 1990.

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 concealed weapons laws on crime.<sup>11</sup> This work, however, uses monthly data on crime and the enactment of legislation that prohibited the right to carry concealed weapons to construct an RDD 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.

Although effects of concealed carry laws are inconclusive, scholars agree that their impact might be sensitive to different environments. For instance, Duggan (2001) and Durlauf et al. (2016) believe that the underlying environments as well as rates of gun ownership and criminality can explain such laws' effects. The literature on gun prevalence and crimes sustain such an argument. Cook and Ludwig (2004) find that the prevalence of youths carrying guns is positively related to local rate of youth violence. They also find that blacks and Hispanics are more likely to carry a gun than others. Cook and Ludwig (2006) show that gun ownership is linked to higher rates of homicides, and this effect is accentuated in youth homicides. After identifying the effect of the concealed weapons prohibition on crime, I also investigate whether these effects vary according to race and age of victims of gun-related homicides, and to the level of gun violence in the municipality.

## 2.2 Legislation prohibiting the concealed carry in Brazil

The first year under President Luiz Inácio Lula da Silva's administration was marked by the implementation of many noticeable legislations and programs in Brazil. For instance, in 2003, this

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<sup>11</sup> Although the literature on the effect of concealed carry laws on crime is inconclusive, many authors find a positive relationship between the number of guns and crimes (see, for instance, Cerqueira and Mello 2013; Leigh and Neil 2010; Duggan 2001; Stolzenberg and D'Alessio 2000, McDowall 1991, 1995; Cook and Ludwig 1998, 2002, 2006; and Newton and Zimring 1969.)

administration launched a successful (see Glewwe and Kassouf 2012) cash transfer program called *Bolsa Família*, as well as a major anti-corruption program that exposed corrupt politicians (see Ferraz and Finan 2008). More important for the present work, however, is the federal legislation regulating firearms that was signed into law in December 22<sup>nd</sup>, 2003 (Law number 10.826) called *Estatuto do Desarmamento* (Disarmament Statute). The legislation prohibited citizens from carrying a gun outside of their residences or places of business; it provided exemptions for hunters (sporting or subsistence), private security employees, and police officers. The penalty for illegal possession (or carrying) increased from an incarceration period of one to three months, to two to four years and it became a “no bail” offense.<sup>12</sup> Finally, the statute made obtaining a gun permit more expensive, and imposed more stringent requirements that made the process more restrictive.<sup>13</sup> This package of measures was enacted to decrease gun violence.

Part of the legislation regulating guns was only implemented by decree in July 1<sup>st</sup>, 2004,<sup>14</sup> however, the piece of legislation prohibiting concealed carry became effective one day after being signed into law (i.e. December 23<sup>rd</sup>, 2003). This fact was highly emphasized by the media. For instance, in December 23<sup>rd</sup>, 2003, the daily newspaper with biggest circulation in Brazil, *Folha de S. Paulo*,<sup>15</sup> 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 legislation, *Folha de S. Paulo*

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<sup>12</sup> This penalty is harsher than most of the ones applied in the United States, where most states punish possession of gun without permit as a misdemeanor. For instance, in New York, possession without permit is punishable by up to one year in prison, a fine of up to \$1,000, or both

<sup>13</sup> An applicant should have no criminal record, be employed, show proof of residence, pay a fee close to \$1,000 attend a gun safety course, and pass a psychological exam. In addition, the minimum age to purchase guns increased from 21 to 25.

<sup>14</sup> For instance, the requirement to hand over unregistered guns to the police within 90 days.

<sup>15</sup> *Folha de S. Paulo*, *Estado de S. Paulo* and *O Globo* are considered the most influential daily newspapers in Brazil.



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

The Brazilian gun regulation imposed high penalties for those individuals who are arrested carrying a gun, 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 Institute (*Sou da Paz* 2010), there was a reduction in gun violence after the law passed because of a decrease 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 illegal gun carrying was reduced by 14.3% in the year following the gun carrying prohibition, which corroborates the study narrative.

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 concealed 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. As gun ownership was not immediately affected by the law and my window of analysis is small, I do not find changes in gun-related homicides taking place inside the residence providing 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.

### **3. Data and empirical strategy**

#### **3.1 The impact of prohibiting the carrying of concealed weapons, and who benefits from it**

I begin my study of the effects of the prohibition against carrying concealed weapons on homicides, and then I examine its effects on non-homicide crimes. Monthly homicide data at the municipality level have been available across the country since 1996 in the Brazilian National System of Mortality Records (DATASUS). Monthly data on non-homicide crimes are only available for the state of São Paulo provided by the *Secretaria de Segurança Pública de São Paulo* since 2001. Table 1 shows descriptive statistics, considering the year of 2003, of homicides in Brazil and non-homicide crimes in the state of São Paulo. Brazil had 36,115 gun-related homicides in this year (which rendered it the country with the largest annual number of gun-related homicides in the world). In Brazil, 70 percent of all homicides are gun-related homicides. Theft is the most common crime in the state of São Paulo, 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.

Following Davis (2008), I use an RDD where time is the running variable to evaluate the impact of the concealed carry prohibition on crimes.<sup>16</sup> This method, also known as an Interrupted Times Series (ITS), has been widely used to estimate the effects of policy changes (Gonzalez-Navarro 2013; Moscoe et al. 2015; Bernal et al. 2017; Carr and Packham 2017). As Moscoe et al. (2015) argue, ITS can be interpreted as a sub-type of RDD, in which time is the assignment variable, and the cutoff is defined as the date when a new policy is implemented.

Bernal et al. (2017) make a tutorial on when and how to use an ITS. They argue that this methodology is validated when the expected trend of the variable of interest, in the absence of the intervention (i.e. calculated using pre-intervention data), should be different than the one observed once the intervention is enacted. At the same time, the conditional expectation of confounding variables that can affect the variable of interest must be continuous around the intervention. Figure 1 shows satisfaction of this requisite. Gun-related homicides deviate from the trend after the prohibition of the right to carry concealed weapons. In contrast, non-gun-related homicides, which captures potential confounding variables related to crime that could be changing simultaneously with the law, follow the trend predicted using pre-intervention data and are continuous around the cutoff (January 2004).<sup>17</sup> This mitigates concerns of endogeneity problems.

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

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<sup>16</sup> For additional examples of empirical strategies using time as the assignment variable in regression discontinuity designs, see: Anderson 2014; Auffhammer and Kellogg 2011; Bento et al. 2014; Busse et al. 2006 2010; Gallego et al. 2013.

<sup>17</sup> As I only have access to monthly data, I defined January 2004 as my cutoff point. However, the last eight days of December 2003 are contaminated because the gun prohibition was already in effect. Nonetheless, if anything, this fact would underestimate my results.

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.<sup>18</sup>

My empirical model is constructed as the following:

$$GRH_{mt} = \alpha + \lambda D_t + \beta_1(r_t - c) + \beta_2(r_t - c) * D_t + X_{mt} + \Lambda_m + \gamma_t + \epsilon_{mt}, (1)$$

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

where  $GRH_{mt}$  is the number of gun-related homicides per 100,000 people at municipality  $m$  at month  $t$ ,  $c$  represents the cutoff (January 2004),  $r_t$  indicates the months surrounding the cutoff,  $D_t$  is a dummy indicating that the prohibition of right to carry concealed weapons became effective, and  $h$  represents the selected bandwidth (in months).  $\lambda$  captures the law effect.  $X_{mt}$  contains monthly data for temperature and rainfall accumulation for each municipality  $m$  at month  $t$ .<sup>19</sup>  $\Lambda_m$  and  $\gamma_t$  are, respectively, municipalities and time fixed effects to capture time-invariant unobservables and any seasonal effect. Finally,  $\epsilon_{mt}$  contains the error term for each observation.

I estimate equation (1) using a negative binomial regression because, given the count nature of crime data, Poisson-based regression models are a better fit. This is particularly the case for small municipalities (see Osgood 2000).<sup>20</sup> 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

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<sup>18</sup> 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.

<sup>19</sup> I control for monthly rainfall and temperatures because researchers have demonstrated that weather is related to crime (see Cohn, 1990 for a review of this literature). Monthly rainfall and temperature data were collected from Matsuura and Willmott (2009). The authors provide estimations 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.

<sup>20</sup> Poisson regression models drop municipalities that contains all zero outcomes, mitigating concerns about small size municipalities that rarely experience homicide.

regression or OLS. Using the RDD strategy proposed, I split the sample among different races and age of victims of gun-related homicides and among different localities where gun-related homicides took place. Finally, I examine whether varying criminality rates affect the effectiveness of the law.

3.2 Do places with larger reductions in gun violence show stronger support for the referendum banning gun sales?

To answer this question, I examine 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 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.<sup>21</sup> I also control for variables that are especially relevant in the literature on support for gun control.<sup>22</sup> However, my main independent variables are: rate of gun-related homicides before the enactment of the gun legislation and change in gun-related homicides after the law was passed. 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.

I estimate the following OLS regression to test the impact of policies on politics:

$$Y_m = \alpha + \beta_1 InitialGRH_m + \beta_2 ChangeGRH_m + X_m + \Lambda_s + \epsilon_m, (2)$$

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<sup>21</sup> More specifically, the control variables are mostly collected from the 2000 census and are composed by: per capita GDP (in 2005), total population (in 2005), percentage of people living in rural areas, distance to the state capital (which in Brazil is the main city in the state in terms of GDP and population), and the ratio of government-initiated agricultural land distributed to total agricultural land.

<sup>22</sup> For a discussion on why people support gun control, see Esposito and Finley, 2014; Carlson, 2012; Neiva, 2010; Kleck, Gertz and Bratton, 2009; Grafton and Permaloff, 2005; Kleck, 1996; Ellison, 1991. I included an index that measures the political ideology of the municipality based on the congressional elections (Fujiwara, 2015). I also included a dummy indicating land reform protest within a year of the referendum [source: Lab of Agriculture Geography (LAGEA)]. This is an important variable because farmers use guns to defend themselves against land invasions. Finally, I included a dummy indicating drought within one year of the election [source: Integrated System of Disaster Information (S2ID)]. Drought may increase landless peoples' propensities to invade land (see, for instance, Ralston 2013).

where  $Y_m$  is the dependent variable in municipality  $m$  (i.e. percentage of the vote in favor of the prohibition).  $InitialGRH_m$  is a variable controlling for the baseline level of gun-related homicides prior to the gun legislation (i.e. 2003), and  $ChangeGRH_m$  is the percentage change in gun-related homicides between two changes 2002 to 2003 and 2003 to 2004. That way, I can capture changes in gun-related homicides growth rates. The vector  $X_m$  includes all control variables relevant to explain support for gun control.  $\Lambda_s$  represents state fixed effects and  $\epsilon_m$  is the error term. The coefficient  $\beta_2$  is the parameter of interest that captures the effect of the policy on the dependent variable.

#### 4. The effects of the concealed carry prohibition on crime and who benefits from the law

##### 4.1 The effects of the law prohibiting concealed carry on gun-related homicides

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 sixteen-year window around the treatment start date. For each type of crime, on the left-hand side, an RD estimation of the law's impact taking seasonality into account is created, and, on the right-hand side, a description of the monthly number of occurrences (in thousands) is presented. This figure indicates that the reduction in homicides that followed the prohibition on carrying concealed weapons was driven by gun-related homicides.

Table 2 reports results considering the selection of different bandwidths, and suggests that the short-run effect of the law was larger than the long-run effect.<sup>23</sup> Before proceeding further with Table 2 analysis, it is important to comment on two facts. First, gun-related suicides were not affected by the

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<sup>23</sup> The optimal bandwidth as suggested by Calonico, Cattaneo, Farrell, and Titiunik (2017) is 22 months. The results are very close to the one obtained in Table 2, second row (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.

law that prohibited the carrying of concealed weapons.<sup>24</sup> 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 (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 shows a strong relationship between the law and gun-related homicides. Column 3 of the first row indicates that the legislation decreased gun-related homicides by 12.2%.<sup>25</sup> In 2003, Brazil had 36,115 gun-related homicides, so close to 4,406 lives were saved in 2004 due to the implementation of the law.<sup>26</sup> Extending the window of my analysis, as shown in the second and third 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.<sup>27</sup> This result suggests that the law had a larger effect in its first year; however, it also indicates that the gains provided by the concealed carry ban did not vanish within those years. In the appendix (Table A1), I find that the estimations are not sensitive to allowing the running variable to vary quadratically or cubically nor to changes in the population size restrictions. Results are also robust to different choices of regression models. Finally, Table A2 in the appendix shows that non-fatal gunshots intended to kill were also reduced (by 16.3 percent) in response to the gun legislation.

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<sup>24</sup> 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 Neil (2010) showing that the gun buyback in Australia reduced gun-related deaths, but mostly as a result of a sharp decline in suicides.

<sup>25</sup> 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  $e^{\hat{\beta}} - 1$ .

<sup>26</sup> This result is close to the one found by Waisekfisz (2016) using a linear trend of annual gun-related homicides, at the country level, between 1997-2003. The author concluded that, based on the previous trend, there should have been 4,391 more gun-related homicides in Brazil in 2004 than the number that were reported, and he attributed this positive impact to the gun-control legislation. The similarity of our results mitigates concerns that my results are driven by large standard deviations of crime in small municipalities.

<sup>27</sup> The coefficient that captures the change in the slope of the trend in gun-related homicides after the enactment of the law becomes significant and negative when the bandwidth is larger than 20 months. This suggests that the gun legislation decelerated the gun-related homicides growth.

The proposed regression discontinuity in time only works if there are no time trends in crime. I check the validity of this requirement by assessing treatment effects where no policy change happened. That is, I simulate different dates for the beginning of the gun control regulation. Table 3 shows coefficients estimated from these simulations. The only negative significant result is obtained when I consider the correct date in which the prohibition of concealed carry took effect, i.e., January 2004. It is interesting to notice that point estimates are mostly positive indicating, if anything, that there was a positive trend in gun-related homicides. This suggests that the reduction in crime estimated is a lower bound of the true effect. As a further robustness check, following Carr and Packham (2017), I randomly select a date 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. 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).

#### 4.2 What is the mechanism explaining the reduction in gun-related homicides?

To answer this question, I use monthly data on non-homicide crimes that are provided by the state of São Paulo only.<sup>28</sup> 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.

Table 4 shows the impact of the law on five crimes, which are: illegal gun carrying, drug trafficking, rapes, robbery and theft. As one can notice, the concealed carry prohibition decreased the

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<sup>28</sup> In Brazil, each state is responsible for providing its own public security. São Paulo is the only state to provide monthly data on crime since 2001.



number of crimes related to guns.<sup>29</sup> The monthly data show that illegal gun carrying decreased by 14.3%, and robberies by 12.5%.

Findings reported in Table 4 corroborates the Disarmament Statute analysis (*Sou da 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 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.

In table 5, I establish a relationship between illegal gun carrying and gun-related homicides. I estimate a regression measuring how the change in the number of illegal gun carrying from 2003 to 2004 impacted the change in the number of homicides, non-gun-related homicides and gun-related homicides in the same period. Controlling for the baseline level of each type of homicide in 2003 I find that,<sup>30</sup> on average, for each twelve and a half less cases of illegal gun carrying there is one less gun-related homicide. Non-gun-related homicides were not affected by the change in illegal gun carrying.<sup>31</sup> Next, I return to the data on gun-related homicides and expand my analysis to Brazil to investigate the conditions explaining the effectiveness of the concealed carry prohibition.

#### 4.3 Heterogeneous effects of the law

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<sup>29</sup> Robbery, in contrast to theft, involves criminal and victims' interaction with force, intimidation, and/or coercion. As a result, criminals often use guns in these situations.

<sup>30</sup> Including the baseline level of each type of homicide is important for causal interpretation as places with higher level of homicides are more likely to experience a larger drop in crime because of mean reversion, and/or stricter compliance with the gun legislation. Therefore, its inclusion mitigates the possibility that the change in illegal gun carrying captures these spurious effects.

<sup>31</sup> Results are similar if I exclude municipalities that did not have a change in homicides within 2003 and 2004, or if I exclude municipalities with zero homicides in the baseline.

I propose a demographic division to better understand which population group benefitted the most from the concealed 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. Before showing the results, I present descriptive statistics in Table 6. It shows the number of gun-related homicides in 2003 divided across race, age and locality.<sup>32</sup>

The reduction in gun-related homicides was especially pronounced among young black males and within occurrences that take place outside the residence. Table 7, Panel A, shows that the gun legislation effect on gun-related homicides is driven by blacks. Although *only* 56 percent of the victims of gun-related homicides are blacks (Table 6), 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.<sup>33</sup> Panel B of Table 7 suggests that young people (between 15 and 29 years of age) benefitted more from the law, and experienced a 14.6% decline in gun-related homicides. Finally, Panel C of Table 7 shows that gun-related homicides happening in what I call “out of home” locations were solely 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.

I then examine 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 8 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. Combined, these results

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<sup>32</sup> I chose not to focus on gender because mostly of the victims of gun-related homicides are males (about 94% of the total).

<sup>33</sup> This could be explained by the fact that, in Brazil, blacks, compared to whites, are disadvantaged in terms of education, income, and security, and are the main victims of violent crimes (Waisekfisz 2012).

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.

## **5. Policy feedback: the 2005 Brazilian referendum case**

This section investigates whether areas that benefitted most from the concealed weapon-carrying prohibition had greater levels of support for the subsequent referendum banning all firearm sales in Brazil. I test this hypothesis using the regression proposed in equation (2).

Table 9 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. 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) indicating that places having an increase in gun-related homicides after the gun control legislation were more likely to support the referendum. Therefore, results suggest that being exposed to gun violence is more important than experiencing gains from the concealed carry prohibition to explain support for gun control.

Next, I further investigate whether being exposed to gun violence is more important to explain support for the referendum than the impact of the gun legislation per se. In the appendix, I analyze 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 (Table A3). Additionally, I also present in the appendix (Tables A4 and A5) an analysis showing that the closer to the referendum that an abnormal positive deviation in gun-related homicides takes place, the more it positively affects support for the gun ban. Combined, these results suggest that being exposed to gun violence, independent of experiencing gains from concealed carry prohibition, increase support for stricter gun legislation.

## 6. Discussion and conclusions

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 Neil, 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 concealed weapons bans on crime. Following a ban on carrying of concealed weapons in Brazil, 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 analysis shows. The paper shows that young black males living in high-crime areas disproportionately benefitted from the regulation because the reduction in gun-related homicides was particularly pronounced among that population. The research here 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. I estimate that for each 12.5 less occurrences of illegal gun carrying, there is one less gun-related homicide. Finally, examining the location of these homicides 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.

Lastly, this work establishes a link between the legislation that prohibited the carrying of concealed weapons, passed into law in December 2003, and a voter referendum to prohibit the sale of all weapons and ammunition that took place in October 2005. My results suggest that being exposed to gun violence positively explain the support for the referendum that proposed a complete gun ban, while experiencing the benefits of the gun carrying prohibition was not relevant.

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 (Ortiz, Markandya and Hunt, 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 Neil, 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.<sup>34</sup>

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<sup>34</sup> This calculation is also likely to be underestimated because I find (appendix) 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).

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 Neil (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.

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

Crime	Total
<i>Homicides - Brazil</i>	
Gun-related homicides	36,115
Non-Gun-related homicides	14,928
<i>Other Crimes – São Paulo</i>	
Robbery	332,229
Theft	645,529
Rape	3,978
Drug Trafficking	13,935
Illegal Gun Carrying	17,253

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
Concealed Carry Prohibition 12 months bandwidth	-0.076*** (0.028)	0.040 (0.049)	-0.130*** (0.034)
Concealed Carry Prohibition 24 months bandwidth	-0.053*** (0.014)	-0.014 (0.023)	-0.070*** (0.017)
Concealed Carry Prohibition 48 months bandwidth	-0.077*** (0.010)	-0.034** (0.016)	-0.099*** (0.012)

Each line shows different bandwidth selection. First row regressions, columns 1, 2 and 3, contain, respectively, 63,406, 56,558 and 54,131 observations. Second row regressions, columns 1, 2 and 3, contain, respectively, 133,181, 125,978 and 121,198 observations. Third row regressions, columns 1, 2 and 3, contain, respectively, 272,537, 266,385 and 257,186 observations. All regressions control for municipalities fixed effects, calendar months, rain and temperatures. All municipalities with more than 10,000 people are considered. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table 3 – Falsification test

Cutoffs	(1) Gun-related homicides	(2) Observations	(3) Number of municipalities
Concealed Carry Prohibition Cutoff – January 2004	-0.130*** (0.034)	54,131	2,174
Concealed Carry Prohibition Cutoff – January 2003	0.058 (0.036)	53,103	2,130
Concealed Carry Prohibition Cutoff – January 2002	0.020 (0.037)	51,761	2,074
Concealed Carry Prohibition Cutoff – January 2001	-0.041 (0.039)	49,785	2,021
Concealed Carry Prohibition Cutoff – January 2000	0.074* (0.042)	48,232	1,936
Concealed Carry Prohibition Cutoff – January 1999	0.058 (0.044)	47,431	1,903
Concealed Carry Prohibition Cutoff – January 1998	-0.057 (0.046)	46,853	1,880
Concealed Carry Prohibition Cutoff – January 1997	0.157** (0.053)	45,410	1,829

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. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 4 - RDD estimating the concealed carry prohibition effect on non-homicides crimes

VARIABLE	(1) robbery	(2) theft	(3) rape	(4) illegal carry	(5) drug traffic
Concealed Carry Prohibition	-0.134*** (0.029)	-0.006 (0.018)	-0.181 (0.138)	-0.154** (0.061)	-0.096 (0.059)
Observations	8,826	8,826	7,863	8,826	8,714

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. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 5 – OLS estimating the impact of the change in illegal gun carrying on the change in homicides, gun-related homicides and non-gun-related homicides

VARIABLES	(1) Change (2003 - 2004) in Total Homicides	(2) Change (2003 - 2004) in Non-gun- related Homicides	(3) Change (2003 - 2004) in Gun- related Homicides
Change (2003 - 2004) in Illegal Gun Carrying	-0.098*** (0.020)	-0.021 (0.013)	-0.080*** (0.019)
Baseline - 2003	-0.334*** (0.010)	-0.310*** (0.017)	-0.340*** (0.013)
Number of observations	645	645	645
R-squared	0.991	0.915	0.986

All regressions control for population. The change in illegal gun carrying, as well as all other crimes analyzed in this table, refers to the annual change between 2003 and 2004. In the second row, columns 1, 2 and 3 show, respectively, the rate of total homicides, non-gun-related homicides and gun-related homicides in 2003. The sample is restricted to the State of São Paulo – only state to provide data on illegal gun carrying for the period analyzed. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table 6 – Descriptive statistics of gun-related homicides

Race	Gun-related homicides	Age	Gun-related homicides	Locality	Gun-related homicides
White	13,224	Less than 15	495	Non-identified	16,915
Black	20,291	Between 15 and 29	21,371	Detention center	126
Other	2,600	More than 29	14,249	Residence	2,503
				Farm	183
				Out of home	16,388
Total	36,115	Total	36,115	Total	36,115

Note: The descriptive statistics correspond to the year of 2003. Race is divided in three groups: white, black (composed by black and a race denominated “pardo” in Brazil, commonly translated by mulatto), and other (composed by yellow, Indians and not-identified). Locality is divided in five groups: out of home (composed by places where people drive on and places where people go to walk, work, study, shop, practice sport, enjoy leisure and so on), detention center (composed by prison, youth detention center, orphanage, hospice, nursing home), residence (gun-related homicides inside the residence), farm (gun-related homicides inside the farm), and non-identified.

Table 7 – Gun-related homicides by race, age and locality

## Panel A - race

VARIABLE	(1) Gun-related Homicides White	(2) Gun-related Homicides Black	(3) Gun-related Homicides Other
Concealed Carry Prohibition	-0.054 (0.054)	-0.173*** (0.044)	0.251 (0.554)
Observations	38,921	41,660	2,600

## Panel B - age

VARIABLE	(1) Gun-related Homicides 15 – 29 years' old	(2) Gun-related Homicides More than 29 years' old	(3) Gun-related Homicides Less than 15 years' old
Concealed Carry Prohibition	-0.158*** (0.043)	-0.068 (0.048)	-0.139 (0.233)
Observations	43,108	45,171	7,424

## Panel C - locality

VARIABLE	(1) Gun-related Homicides Out of home	(2) Gun-related Homicides Detention center	(3) Gun-related Homicides Residence	(4) Gun-related Homicides Farm
Concealed Carry Prohibition	-0.174*** (0.052)	-0.214 (0.517)	-0.001 (0.111)	-0.155 (0.399)
Observations	39,020	2,050	24,635	4,475

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. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



Table 8 – Quartile analysis

VARIABLE	(1) < 25%	(2) >25% and <50%	(3) >50% and <75%	(4) >75%
Concealed Carry Prohibition	-0.006 (0.302)	-0.110 (0.171)	-0.103 (0.099)	-0.130*** (0.038)
Observations	7,412	13,075	16,272	17,372

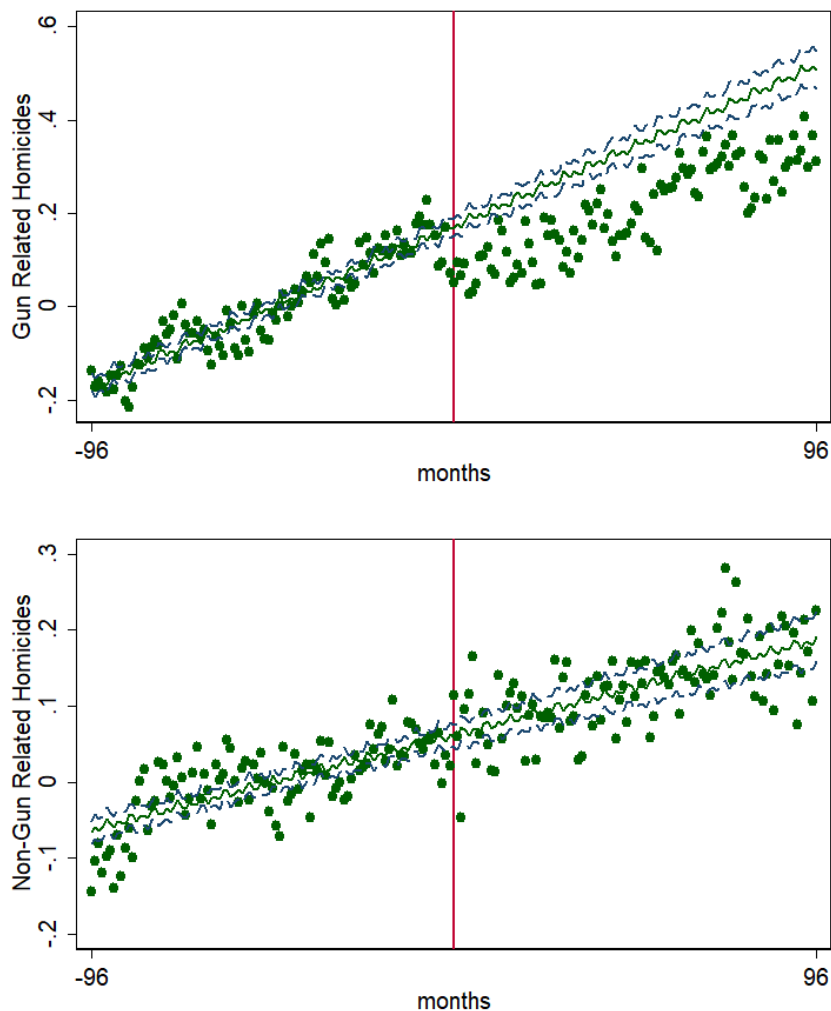
Bandwidth is equal to 12 months. Column 1 splits off the lowest 25% municipalities according to gun-related homicide rates. Column 2 splits off municipalities with gun-related homicide rates larger than the lowest 25%, but smaller than the highest 50%. Column 3 splits off municipalities with gun-related 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. This analysis considers only municipalities with more than 10,000 people. All regressions control for municipalities fixed effects, calendar months, rain and temperatures. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 9- OLS regression using vote in favor of gun prohibition as the dependent variable

VARIABLES	(1) Vote in favor of the prohibition	(2) Vote in favor of the prohibition	(3) Vote in favor of the prohibition
Baseline	0.938*** (0.337)	0.902*** (0.161)	0.480*** (0.165)
Change in gun-related homicides	0.257* (0.141)	0.294*** (0.068)	0.160** (0.069)
Socio-economic controls	No	No	Yes
State fixed effects	No	Yes	Yes
Observations	5,560	5,560	5,505
R-squared	0.007	0.657	0.668

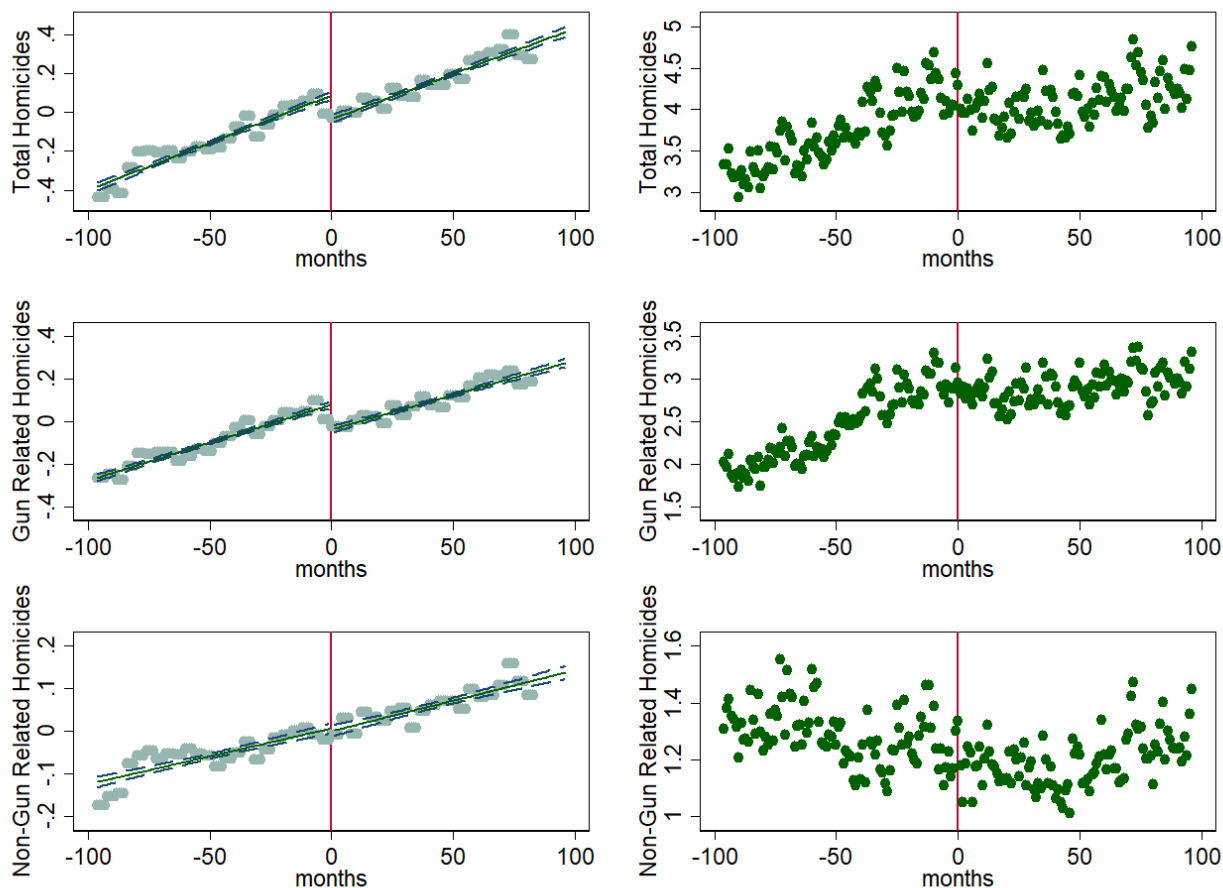
Robust standard errors clustered at the microregion (557 total) level are in parenthesis. Baseline controls for the baseline level of gun-related homicides in 2003. Change in gun-related homicides is the percentage change in gun-related homicides between two changes 2002 to 2003 and 2003 to 2004. The socio-economic controls contain 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Figure 1 – Gun-related and non-gun-related homicides trends



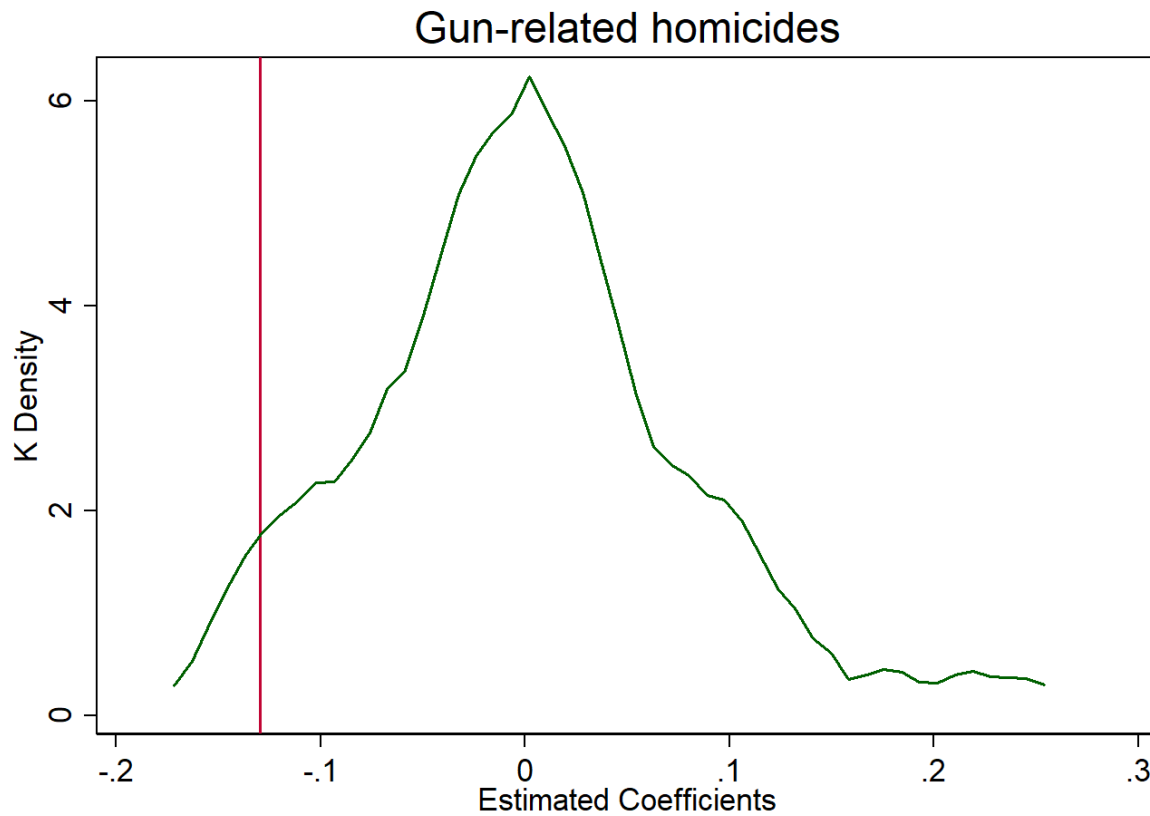
Notes: The top graph shows scatter plots representing the monthly gun-related homicides per 100,000 people and the bottom graph shows scatter plots representing the monthly non-gun-related homicides per 100,000. The vertical line at month zero represents the intervention. The solid function is fitted using an OLS regression and the dashed line represents the 95% confidence interval. The part of the function after the intervention contains predicted values using the pre-intervention data. I first regress the dependent variables on calendar months to take seasonality into account. Then, I regress the predicted residuals on time and pairs of cosine and sine functions.

Figure 2 – Effect of the concealed carry prohibition on total homicides, gun-related homicides and non-gun-related homicides per 100,000 people



Notes: On the left-hand side, figure 2 shows three time-varying functions using a 96 months' bandwidth and a vertical red line representing the cutoff point (January 2004). 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 averages for a period of four months. I regress the predicted residuals after regressing my dependent variables on calendar months, monthly rainfall and temperatures to take seasonality into account. On the right-hand side, figure 2 shows the monthly raw data (in thousands) for each type of homicides analyzed.

Figure 3 – Placebo estimates



Notes: 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.

## **Appendix**

### **A1 – Model specifications and restricting the bandwidth**

In this section I show that the impact of prohibiting concealed carry on gun-related homicides is not sensitive to varying the model presented on Table 2. Table A1, rows 2 and 3, shows that removing population size restrictions or adding more population size restrictions do not substantially change the results. In addition, allowing the running variable to vary quadratically (row 4) attenuate the gun legislation effect, while allowing it to vary cubically (row 5) accentuate the gun legislation effect. The cubic specification better fit the model suggesting that the main findings are, if anything, a lower bound of the true effect. Using an OLS regression I find that gun-related homicides were reduced by 8.4%.<sup>35</sup> Finally, using Poisson regression 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.

### **A2 – Gunshot wounds**

The subsequent analysis investigates data on monthly gunshot wounds at the municipality level, which are classified as “accidental” or “intended to kill.” Table A2 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.

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<sup>35</sup> 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.

### A3 – Using a survey data as robustness check

To increase confidence in my results showing that exposure to gun violence explain vote in the referendum, I use a public opinion survey asking voters whether they would vote in favor of or against the gun prohibition. This survey took place two days before the referendum. 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, if they were robbed at least once, if they would vote even if it was not mandatory to vote, and if they ever considered buying a gun to protect themselves. 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 benefitted by the concealed weapon ban (i.e. people more exposed to gun violence), voted more in favor of the gun prohibition.

Table A3 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. In accordance to the argument defended in this paper, people exposed to gun violence were 1.48 times more likely to vote in favor of the prohibition.<sup>36</sup> Additionally, income, gun ownership, and ever considering buying a gun was negatively related to voting in favor of the gun ban. Blacks were more likely to support the gun prohibition and the variable “would vote” showed that those willing to vote in the referendum, even if vote was not mandatory, were 1.76 times more likely to support the gun ban. This shows that people supporting the gun prohibition were more willing to politically participate in the referendum.

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<sup>36</sup> 1.48 represents the ratio of the odds for being exposed to gun violence to the ratio of the odds for not being exposed, which is calculated by exponentiating the coefficient for being exposed to gun violence (0.393).

#### A4, A5 – Does timing matter?

This subsection investigates whether having an increase in gun-related homicides close to the election is important in explaining the vote for the gun ban. 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. 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.<sup>37</sup> 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. Table A4 presents the estimated coefficient and shows that one deviation from the mean increases the support for gun prohibition by .73 percentage points. Table A5 shows 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 increase support for stricter gun regulations. In addition, they suggest that positive shocks in gun violence have a short-term effect favoring policies that limit gun ownership.

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<sup>37</sup> Formally, this variable is constructed as follows:  $Std.Homicides_m = \frac{(\sum_{t=1}^{12} homicides_{mt}) - Yearly\ Historical\ Average_m}{Standard\ Deviation_m}$ , where  $homicides_{mt}$  indicates the number of gun-related homicides for each municipality  $m$  at month  $t$ . More specifically,  $deaths_{m12}$  represents the number of gun-related homicides, in municipality  $m$ , and at the month in which the referendum took place (defined as  $t=12$ ). 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.



Table A1 – RDD estimating the gun carrying prohibition effect on total homicides and Gun and Non-Gun related homicides

VARIABLES	(1) Total Homicides	(2) Non-gun Related Homicides	(3) Gun-Related Homicides
Baseline	-0.076*** (0.028)	0.040 (0.049)	-0.130*** (0.034)
No restriction	-0.070** (0.028)	0.029 (0.048)	-0.117*** (0.034)
50,000	-0.080** (0.032)	0.017 (0.058)	-0.116*** (0.038)
Square	-0.063 (0.042)	-0.011 (0.073)	-0.096* (0.051)
Cube	-0.186*** (0.052)	-0.123 (0.090)	-0.226*** (0.063)
OLS	-0.162** (0.071)	-0.011 (0.047)	-0.151*** (0.053)
Poisson	-0.046** (0.023)	0.055 (0.042)	-0.089*** (0.027)

Robust standard errors clustered at the municipality level are in parenthesis. Bandwidth is equal to 12 months. All regressions 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 allow for the running variable to vary, respectively, quadratically and cubically (in addition to on either side of the cutoff). Row 6 and 7 use, respectively, OLS and Poisson regression model with municipality fixed effects (OLS model uses homicides per 100,000 people instead of counts as dependent variable). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table A2 – Impact of prohibiting concealed carry on gunshot wounds (by intention)

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

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. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table A3 – Logistic regression showing the relationship between exposure to gun injury and voting in favor of the prohibition

VARIABLES	Vote in favor of the prohibition
Monthly household income	-0.103*** (0.039)
Blacks	0.299*** (0.100)
Have gun	-1.287*** (0.215)
Injured by a gun	0.393*** (0.106)
Age	0.003 (0.003)
Men	-0.044 (0.101)
Would vote	0.569*** (0.097)
Considered buying a gun for protection	-0.952*** (0.127)
Robbed	-0.094 (0.116)
Observations	1,925

Robust standard errors (in parenthesis). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table 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 referendum

VARIABLE	Vote in favor of the prohibition
gun-related homicides std.	0.730*** (0.267)
Observations	5,505
Number of Microregion	557

The regression use microregion fixed effects and robust standard errors are adjusted for clusters at the microregion level. It is additionally 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. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table A5 – 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 column of the table

VARIABLE	(1) Oct. 2005	(2) Sep. 2005	(3) Aug. 2005	(4) Jul. 2005	(5) Jun. 2005	(6) May. 2005	(7) Apr. 2005	(8) Mar. 2005	(9) Feb. 2005	(10) Jan. 2005
gun-related homicides std.	0.73*** (0.26)	0.70** (0.27)	0.64** (0.28)	0.56* (0.29)	0.59** (0.27)	0.45 (0.27)	0.48* (0.26)	0.21 (0.26)	0.22 (0.28)	0.30 (0.28)
Observations	5,505	5,505	5,505	5,505	5,505	5,505	5,505	5,505	5,505	5,505
Number of Microregion	557	557	557	557	557	557	557	557	557	557

The regressions use microregion fixed effects and robust standard errors are adjusted for clusters at the microregion level. Each column measures the impact of an abnormal increase in gun-related homicides, within one year from the date reported in each column, on the referendum outcome. Notice that the first column captures the impact of a deviation in gun-related homicides within one year of the referendum (which took place in October 2005). 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

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