ESSAYS IN PUBLIC POLICY AND SOCIAL OUTCOMES IN BRAZIL

BY

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DISSERTATION

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ABSTRACT

This dissertation consists of three essays. The first essay looks at the impact of a Brazilian law that first prohibited concealed carry and then provided a referendum asking voters whether guns should also be banned in the country. It exploits the abrupt change that prohibited all citizens to carry guns to identify the effects of this law on crimes and to understand which part of society are more likely to benefit from gun control. Once this connection is established, I investigate whether the part of the society that benefitted the most from the concealed carry ban were also more likely to support the gun prohibition referendum. The second and third essays investigate the impact of the phased-in introduction to electronic voting in Brazil. In particular, the second essay looks at whether electronic voting, by eliminating the possibility of fraud after voting, impacted the usage of an alternative electoral malfeasance named voter buying (i.e., paying outsiders to transfer electoral registration into a given candidate's district to increase votes for that candidate). It then examines whether clientelistic parties were hurt by the new voting system. The third and last essay investigates whether electronic voting, by *de facto* enfranchising poorer and less educated voters, also impacted public spending. Connecting all three essays is the fact that they all relate to Brazil and analyze the impact of public policies. Below are the title and individual abstracts for each of the three essays.

Chapter 1: Crime and political effects of a concealed carry ban in Brazil

This paper studies the effects of legislation in Brazil that banned the carrying of concealed weapons nationwide in 2003, and provided for a voter referendum 22 months later regarding whether to ban the sale of all firearms in Brazil. Using a regression discontinuity design, I find that in the wake of the law gun-related homicides decreased by 10.8 percent, with the reduction especially pronounced

among young black males living in high-crime areas. Other crimes involving guns (robberies) also declined, while crimes that did not involve guns were unaffected. Enrollment in adult education courses disproportionately increased in areas that saw the biggest drop in gun-related crimes. Economic benefits are estimated to exceed $3 billion. Analysis of the subsequent voter referendum, which was defeated by a wide margin, shows higher voter turnout and stronger support for the complete weapons ban in the areas that had experienced the greatest decline in gun-related homicides.

Chapter 2: Does voting technology affect clientelism?

This paper studies the phased-in introduction of electronic voting systems in Brazil to examine whether the technological shift affected clientelism. The new technology undercut voter fraud that had previously been shown to take place with paper ballots *after* voting (i.e., adding votes to tabulation sheets after voting has ended). At the same time, the new technology increased the relative appeal of voter fraud via *voter buying* (i.e., paying outsiders to transfer electoral registration into a given candidate's district to increase votes for that candidate). I find that municipalities using electronic rather than paper ballots experienced larger increases in the number of registered voters suggesting an increase in voter buying. Even though voter buying is a clientelistic strategy, electronic voting decreased support for clientelistic parties indicating that fraud after voting was a more effective electoral malfeasance, especially because voter buying requires compliance and I find that voting turnout was smaller in places using electronic voting.

Chapter 3: Electronic voting and Social Spending: The impact of enfranchisement on municipal public spending in Brazil

This article studies the effect of political participation on municipal level public spending. We use the gradual introduction to electronic voting in Brazil, which especially enfranchised poorer voters

in the legislative elections, to identify the causal effect of voting on public spending. A theoretic, political economy model suggests that, by *de facto* enfranchising the poor, electronic voting increases social spending. We test this prediction empirically using as instrument the introduction to electronic voting, which affects voting enfranchisement without directly influencing public spending. We first apply a two-stage least square regression and then we validate our estimation using a difference-in-differences methodology. We find in our preferred specification that an increase of 1 percentage point in the valid vote to turnout ratio for state representatives increases health spending by 1.42%; education by 1%; public employment by 1.28%; total spending by 1.26%; total revenue by 1.07% and intergovernmental transfer by 1.11%.

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To Aline, Ester, Paulo, Pedro and Maria José

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Chapter 1: Crime and Political Effects of a Concealed Carry Ban in Brazil

1.1 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 2016).[1] 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 (rather than authorized) the carrying of concealed weapons. I examine gun-carrying restrictions that were passed by the National Congress of Brazil, and implemented in December 2003 in Brazil. 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. The implementation of the law and the provision for the follow-up referendum

[1]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.

provide natural experiments that allow me to analyze the impact of the policy on crime and the political process. Using a regression discontinuity design analysis, I study the impact of the legislation on crime rates and on various communities and populations throughout Brazil. I then use a least square regression to examine and compare voter turnout and support for the ban on weapons in neighborhoods 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 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. Manski and Pepper (2016) explain this phenomenon by showing that empirical findings on the impacts of such laws are highly sensitive to controversial assumptions about crime rates trends. 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 far 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.[2]

[2] 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.

I construct an empirical model that overcomes challenges faced by the literature studying the impact of so-called "right-to-carry" laws on crime. I follow Davis's (2008) empirical strategy of using time as the assignment variable in a regression discontinuity design (RDD).[3] As the law prohibiting the right to carry concealed weapons is a deterministic function of time, there are no confounding variables other than time itself, and endogeneity problems are less of a concern.[4] Moreover, because the same law was imposed on all Brazilian municipalities, there is no need for comparisons between treatment and control groups. This eliminates the need to rely on certain controversial assumptions that have hampered the previous literature.[5]

My results show that prohibiting the carrying of concealed weapons decreased gun-related crimes; 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 billion in one year. Using monthly data (available across the country) on homicides, I find that gun-related homicides decreased by 3,900 (a 10.8 percent reduction) in the year following the law, and that the reduction was most pronounced in high-crime areas; 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,

[3] 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 regression discontinuity designs, see: Anderson 2014; Auffhammer and Kellogg 2011; Bento et al. 2014; Busse et al. 2006 and 2010; Gallego et al. 2013.

[4] 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).

[5] Manski and Pepper (2016) 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).

total arrests, and arrests for violations of weapon-carrying laws; non-gun-related crimes such as rape, drug trafficking, and theft remained unchanged.[6]

I use two different empirical strategies to investigate who benefits from the prohibition on carrying concealed weapons, and whether the prohibition impacts social outcomes. Using an RDD, I find that the reduction in gun-related homicides was especially pronounced among young black males, and in places with higher gun-related homicides rates. The regulation decreased gunshots that were categorized as "intending to kill," but did not affect gunshots categorized as "accidental." Using the fact that the prohibition shows heterogeneous effects, I construct a difference-in-differences (DID) model. My treatment group is composed of the population in areas that experienced steeper drops in crime in the wake of the law. The DID, which validates my RDD findings and thus bolsters internal validity, shows that the treatment group had higher levels of enrollment in young and adult education, compared to the control group living in less-affected areas.

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 turn out to vote and to support the gun prohibition. I use an ordinary least-square regression (OLS) and find that areas that had previously had high levels of gun violence and thus had benefitted most from the legislation that prohibited the right to carry concealed weapons had higher levels of voter turnout and higher levels of support for the referendum on banning guns.[7] These results suggest that people in areas exposed to a greater degree of gun-related violence care more about and show larger support for gun control policies. These findings, combined with the

[6] Notice that before the prohibition of right to carry concealed weapons, "illegal gun carrying" referred to the unauthorized carrying of open (unconcealed) 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.

[7] In the appendix, I corroborate these findings using a survey that took place two days before the referendum.

fact that the referendum failed, offer insights about why gun control legislation may be difficult to pass, even though utilitarian welfare gains seem large. Namely, these gains may be concentrated in a small share of the population.

How generalizable are the homicides findings to other countries? While is not possible to know for sure without similar legislature being applied in different contexts, one can hypothesize that laws restricting the number of guns, such as the one applied in Brazil, decrease the number of gun-related deaths.[8] In the Brazilian context, this effect was driven by gun-related homicides, especially the ones committed against young black males living in high-crime areas. Yet, it is possible that in other contexts, gun-related suicides would be affected as well. For instance, Leigh and Neill (2010) show that gun buybacks in Australia reduced gun-related suicides. Additionally, the 10.8 percent reduction in gun-related homicides that I find could be larger in a country in which policing and law enforcement are highly organized and effective, not the Brazilian case.

1.2 Related Literature and the Gun Legislation

1.2.1 What are the effects of gun laws? Who are their beneficiaries?

There is a vast literature investigating the first question, but no definitive answer to it. Without an answer to the first question, the second one is compromised. The main reason why the first question remains unanswered is because results showing the effects of "concealed carry" laws are sensitive to minor modifications on researchers' assumptions about crime trends. An overview of the literature underscores the difficulties that surface, and the debates that have ensued. Research by Lott and Mustard (1997) reached the controversial conclusion that laws that gave people the right

[8] The most generalizable finding of this work is on how laws regarding the right to carry concealed weapons affect homicides. Data on non-homicide crimes are provided only by the São Paulo state. This is the most populous and wealthy state of the country, and I do not claim it is representative of Brazil.

to carry concealed weapons reduced crime rates in the United States.[9] 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 (2016) 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)."[10] Yet, the literature on concealed weapons 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 concealed weapons laws on crime.

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. However, these findings also face challenges.[11] First, because data on the number of guns available are lacking,

[9] 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.
[10] 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.
[11] See Cerqueira and Mello 2013; Duggan 2001; Stolzenberg and D´Alessio 2000, McDowall 1991; Cook and Ludwig 1998, 2002, 2006; and Newton and Zimring 1969.

proxies are needed. For instance, to proxy for the number of firearms, Duggan (2001) uses the number of gun magazine subscriptions per county, and Cook and Ludwig (2006) use a ratio of gun-related suicides to suicides per county. Second, as Leigh and Neill (2010) point out, such research suffers from endogeneity problems. For instance, people who live in neighborhoods that have higher crime rates might buy more guns to protect themselves. Therefore, gun ownership could be related to current crimes or expectations of future crime rates.

In a study of a gun buyback program in Australia, Leigh and Neil (2010) used a comparison of the differences in the number of firearms surrendered in different states to show that gun-related suicides decreased by 80 percent due to this program (the effects on gun-related homicides were less precise). Nonetheless, their work has the same problem as the ones that studied the impact of concealed weapon laws in the United States. That is, the research assumes that all Australian states would have had the same gun-related death changes if they had bought back the same number of guns. It also relies on the assumption that the buyback rate in each state had no relationship with pre-existing trends.

The endogeneity problem faced by research examining the impact of concealed carry laws on crime, as well as the relationship between guns and crime, is not easily addressed. These studies need to rely on assumptions that pre-existing annual crime trends do not affect gun ownership, the implementation of anti-crime policies, or the effectiveness of these policies. 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 that overcomes both the endogeneity problems and the reliance on strong assumptions. The advantage of using an RDD model is that restricting the window of time addressed by my analysis enhances the credibility of the assumption that the

only differences in crimes trends after the concealed weapon 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.

1.2.2 Legislation prohibiting the right to carry concealed weapons and the referendum on a ban on all weapons and ammunition

Brazilian legislation barred the carrying of concealed weapons, and provided for a voter referendum on whether to ban weapons 22 months after the legislation's enactment. The former provision of the act allows me to measure whether prohibiting gun carrying decrease crimes, and the latter provision can help to establish a relationship between being affected by gun laws and showing support for them.

In the early 2000s, as Figure 1.1 illustrates, more than 30,000 gun-related homicides occurred in Brazil every year, and most of the victims were young.[12] This number was much smaller in the

[12] Yearly data from 1979 to 2013 are available at DATASUS (data from Brazilian Health Ministry).

1980s but sharply increased in the 1990s. Although 60 percent of the victims of gun-related homicides were young (15-29 years old), this population only represented close to 30 percent of the population. The number of gun-related homicides per 100,000 people for this age group increased from 27.6 in the 1990 to 42.2 in 2000 – while the number of gun-related homicides per 100,000 people for all ages rose from 14.3 to 20.6. Therefore, the sharp increase in gun-related homicides in Brazil in the 1990s disproportionately affected young people.

Motivated by this dramatic increase in the number of firearm-related deaths in Brazil, legislators passed nationwide firearm regulations in December 22nd, 2003 (Law number 10.826), in the form of the *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.[13] Finally, the statute made obtaining a gun permit more expensive, and imposed more stringent requirements that made the process more restrictive.[14] This package of measures was enacted to decrease gun violence.

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 hunter and those with security-

[13] 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

[14] 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.

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, which that prohibited the carrying of concealed weapons, but it proposed to go further, by prohibiting the sale of all firearms. In what follows, I describe the referendum campaign and its outcome.

As argued by De Vreese and Semetko (2004), political campaigning is more relevant in referendums than regular elections, especially because heuristics (e.g. ballot cues) are absent and political parties' attitudes may confuse voters. In the 2005 Brazilian referendum, the two main opposing parties in the political arena, the Workers Party (PT) and the Brazilian Social Democracy Party (PSDB), supported the campaign in favor of prohibiting the sale of guns. At the same time, the Liberal Front Party (PFL, an extreme right-wing party) worked together with the United Workers Socialist Party (PSTU, an extreme left-wing party) against the gun ban. The mixed signals coming from parties' political ideologies can explain why voters could not rely on typical political cues, and why the political campaign, conducted mostly through TV ads, gained importance.[15]

The campaign against the gun prohibition used exploitation of fear as its most effective argument against the referendum's proposition.[16] The televised advertisements argued that the inability of the Brazilian state to provide security would leave citizens defenseless against criminals if firearms were banned.[17] For instance, as noted by Lissovsky (2006), the second most televised ad of the campaign against the gun ban, which aired 38 times during three consecutive days,[18] showed a

[15] The government provided one hour daily (each side had half hour) of free electoral airtime on free-to-air television (all radio stations broadcast it simultaneously). In addition, each side had short TV ads available to them during the day.

[16] See Anastasia, Inacio and Novais 2006; Araújo and Santana 2006; Inacio 2006; Lissovsky 2006; Mota 2006; Cunha 2006; Esteves 2007; Goldstein 2007; Veiga and dos Santos 2008; Cavalcanti 2016.

[17] Cunha (2006) argues that vulnerability, sense of fear and uncertainty were the most common themes explored by the campaign against the gun ban, particularly during the last 10 (out of 20) days of campaign.

[18] These dates, October 15-17, are close to the last day of campaign, which was held on October 20, 2005.The timing suggests the relevance and appeal of the message.

citizen (representing a family man) placing a sign at his front door informing passersby that he did not possess any weapons. After the man installed the sign and admired his work, the soundtrack becomes dark. It becomes clear to the audience that he immediately regrets his decision. Consequently, he removes the sign while the speaker concludes: the problem is not for me not to have a gun; the problem is that the criminal will know for sure that I do not have one.[19] This exploitation of fear created uncertainty about citizen security in the case of the referendum succeed, which led many to vote in favor of the status quo.

Moreover, the campaign against the gun sale prohibition, as Lissovsky (2006) characterizes it, was well organized, had twice as much money,[20] and promoted a main message that was direct and focused: Prohibiting guns was an attempt of suppression of rights (even though possessing guns was never a constitutional right in Brazil as it is in the United States), which would increase citizens' vulnerability to crime. In addition, Cavalcanti (2016) argues that the National Rifle Association (NRA) provided the campaign against the gun ban with financial means and expertise.[21] It provided the campaign with strategic advice and propaganda materials that were previously used in the United States.

By contrast, the campaign in favor of the gun ban was supported by researchers and criminologists. However, as Soares (2006, p.75) argues, "(…) that tremendous and cognitive and factual advantage was not transformed into a political and electoral advantage." The most problematic issue with this campaign was its lack of organization. As Mota (2006) argues, one of the main coordinators of the campaign in favor of the gun prohibition, Ruben César Fernandes, admitted

[19] This advertisement can be accessed at: https://www.youtube.com/watch?v=Nu4okj8yPws

[20] The campaign against gun ban was financed by the gun industry and got $2 million, while the campaign in favor of gun ban got $960 thousand.

[21] The author claims that the NRA should be interested in the referendum because if the gun ban passed and provided evidence that the society was better after it, many other countries could attempt to do the same.

that he had no specific strategy. Another mistake, according to Mota (2006), was the usage of celebrities to deliver the campaign message. Common citizens' testimonies reporting their daily struggles with gun-related homicides might have better connected with the audience. For all of these reasons, it seems clear that the campaign against the gun ban was better organized than the campaign in favor of it, and these differences in campaign capabilities can help to explain the referendum's final outcome, in which 64 percent of the population voted against the gun ban. In section 5, I establish a link between voter support for the prohibition on the sale of firearms and the effectiveness of the earlier legislation that prohibited carrying concealed weapons; this link, which has been neglected by the literature thus far, can provide insights about which voter types believe they benefit from gun control legislations.

1.3 Data and empirical strategy

1.3.1 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 1.1 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 forcing variable to evaluate the impact of the concealed carry prohibition on crimes.[22] 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). 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 1.2 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).[23] This mitigates concerns of endogeneity problems.

[22] 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.

[23] 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.

Studies examining crimes usually restrict their sample because of few occurrences. For instance, Cerqueira and Mello (2013) study the impact of a gun law on crimes in the state of São Paulo. They use as the dependent variable the annual change in the number of gun-related suicides to total suicides, and argue that this variable is noisy in small municipalities because of low incidence. Therefore, they consider only municipalities with more than 50,000 inhabitants. Cook and Ludwig (2006) use a similar strategy 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.[24]

My empirical model is constructed as the following least square estimation:[25]

$$GRH_{mt} = \alpha + \lambda D + \beta_1(r-c) + \beta_2(r-c) * D + X_{mt} + \Lambda_m + \epsilon_{mt}, \quad (1)$$

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

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 indicates the months surrounding the cutoff, D is a dummy indicating that the prohibition of right to carry concealed weapons became effective, and h represents the selected bandwidth (in months). λ captures the law effect. X_{mt} contains monthly data for temperature and rainfall accumulation for each municipality m at month t.[26]

[24] 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 (50,000 and 100,000 inhabitants) does not change my results.

[25] I do not add municipal fixed effects because, as Lee and Lemieux (2010) argue, including fixed effects is unnecessary for identification in a RD design. Nonetheless, it is important to highlight that including fixed effects does not significantly change the results as reported in the appendix.

[26] 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.

Λ_m are dummies indicating each calendar month to capture any seasonal effect.[27] Finally, ϵ_{mt} contains the error term for each observation.

Using population and gunshot wounds data, I verify whether the prohibition of right to carry concealed weapons had heterogeneous effects, and whether its effects were driven by intentional gunshots. Using the RDD strategy proposed, I split the sample among different races and age of victims of gun-related homicides to study the law's effects on various populations. Then, using data on gunshot wounds, provided by the DATASUS, I examine whether gunshot wounds intended to kill were affected to a greater degree than accidental ones.

I then examine if the effects of the concealed carry prohibition are larger in places having more youth that are vulnerable to becoming criminals. Young people are overrepresented as both victims and perpetrators of violence, and the likelihood that someone carries a gun is larger in places with higher rates of youth violence and among high-risk groups (Cook and Ludwig 2004). I construct an index, at the municipality level, which I call the *vulnerability index,* to measure youth violence. I then assess whether the effects of the law that prohibited carrying concealed weapons varies in accordance with such an index.[28]

To measure how the vulnerability index relates to the law effects, I build on equation (1) and construct the following RDD analysis:

$$GRH_{mt} = \alpha + \lambda D + \beta_1(r - c) + \beta_2 vuln.index_m + \beta_3 vuln.index_m * D + X_{mt} + \Lambda_m + \epsilon_{mt}, \quad (2)$$

[27] In Brazil summer starts in December and ends in March. As showed by (Waisekfisz and Athias (2005) – Mapa da Violência SP), the number of homicides reaches its peak in the summer.

[28] Further details about this index is provided in section 4.

where β_3 is now my main coefficient of interest. It measures whether the effectiveness of the prohibition on carrying concealed weapons is related to the $vuln.index_m$ (vulnerability index in municipality m). My analysis indicates that there are heterogeneous effects of the law.

Lastly, I use a DID to validate my RDD findings in equation (2). Taking advantage of how the law's effects vary in accordance with the vulnerability index, I propose the following DID estimation:

$$GRH_{mt} = \alpha + \beta_1 Post_t + \beta_2 Post_t * vuln.index_m + X_{mt} + \Lambda_m + \epsilon_{mt}, \quad (3)$$

where $Post_t$ is a dummy variable equal to one when $t = 2004$ (and equal to zero when $t = 2003$), $vuln.index_m$ is a continuous treatment variable representing the vulnerability index in municipality m. Vector X_{mt} includes control variables that vary across time and municipalities. The dependent variable GRH_{mt} corresponds to the gun-related homicides in municipality m and year t.[29] Λ_m represents municipal fixed effects and ϵ_{mt} is the error term. The coefficient β_2 is the parameter of interest that captures the effect of the prohibition of carrying concealed weapons on gun-related homicides.

1.3.2 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 variables are the percentage of votes in favor of the prohibition, as well as the turnout-to-registered-voters' ratio. These data are available from the Brazilian Superior Electoral Court (TSE). The control variables are collected from both IBGE

[29] Using the DID methodology I can access whether the prohibition of the right to carry concealed weapons impacted educational outcomes. More specifically, I test whether places with higher vulnerability indices had a relatively larger young and adult education enrollment (i.e. young and adult education enrollment is my dependent variable in equation 3).

and IPEADATA. They are composed of socioeconomic and demographic data.[30] I also control for variables that are especially relevant in the literature on support for gun control.[31] However, my main independent variable is the vulnerability index. Because places with higher vulnerability indices were disproportionately affected by the prohibition on carrying concealed weapons, I can examine whether places that benefited most from the 2003 legislation had larger turnout and demonstrated higher levels of support for the referendum.

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

$$Y_m = \alpha + \beta_1 vuln.index_m + X_m + \Lambda_s + \epsilon_m, \quad (4)$$

where Y_m is the dependent variable in municipality m and can be both the percentage of the vote in favor of the prohibition as well as the turnout-to-registered-voters' ratio. The vector X_m includes all control variables relevant to explain support for gun control. Λ_s represents state fixed effects and ϵ_m is the error term. The coefficient β_1 is the parameter of interest that captures the effect of the policy on the dependent variable. As I show later, the $vuln.index_m$ variable explains the effectiveness of the prohibition on carrying concealed weapons and should, therefore, be related to political outcomes associated with the referendum.

[30] More specifically, the control variables are mostly collected from the 2000 census and are composed by: the ratio of the number of women to the number of men, per capita GDP (in 2005), total population (in 2005), percentage of people living in rural areas, years of schooling, percentage of households with TV access, the ratio of the number of households receiving government conditional cash transfer (Bolsa Família) to total population, distance to the state capital (which in Brazil is the main city in the state in terms of GDP and population), change in the income distribution (between 1991 and 2000), number of cattle per people living in rural areas, and the ratio of government-initiated agricultural land distributed to total agricultural land.

[31] 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).

1.4 Analyzing the effects of the concealed carry prohibition on crime, and determining who benefits from the law

I first investigate the impact of prohibiting the carrying of concealed weapons on crime, and then I show who benefits the most from the law. I focus on homicides because data on this type of crime are available across the country. Using population data, I investigate which groups were more affected by the concealed carry prohibition. Finally, I validate my RDD findings using a DID model, which also allows me to study whether places that experienced greater benefits from the law had any changes in enrollment in adult education programs.

1.4.1 The effects of the law prohibiting carrying concealed weapons 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. Following Davis (2008), I show on Figure 1.3 a graphical result considering an eight-year window around the treatment start date. This figure indicates that the reduction in homicides that followed the prohibition on carrying concealed weapons was driven by gun-related homicides. Table 1.2 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. Before proceeding further with Table 1.2 analysis, it is important to comment on two facts. First, gun-related suicides were only marginally affected by the law that prohibited the carrying of concealed weapons.[32] Second, not taking seasonality into account decreases the magnitude and significance of the gun-related homicides coefficient, suggesting that seasonality

[32] The coefficients measuring the impact of prohibiting carrying concealed weapons on gun-related suicides was -0.025, and the standard deviation was equal to 0.0145. 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. However, it is important to point out that in Brazil, different from Australia, gun-related suicides are rare events that represent just 3 percent of total gun-related deaths.

plays an important role: the decrease in gun-related homicides in January, a month in which this variable would usually reach its annual peak, shows the strength of the law.

The results on Table 1.2 shows a strong relationship between the law and gun-related homicides. Column 3 of the first row indicates that the legislation decreased the monthly gun-related homicides per 100,000 people by 0.191 on average.[33] In 2003, Brazil had 167,546,532 people living in municipalities with more than 10,000 inhabitants, so close to 3,900 lives were saved in 2004 due to the implementation of the law, which corresponds to 10.8 percent of the total gun-related homicides in 2003.[34] This result is close to the one found by Waisekfisz (2016) using a linear trend of gun-related homicides in Brazil between 1997-2003.[35] Extending the window of my analysis, as shown in the second and third rows of Table 1.2, attenuates the effects of the law on gun-related homicides to an annual reduction of 6.7 percent. 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 show that the estimations are not sensitive to model specifications.

As a robustness check, I present a falsification test where I simulate different dates for the beginning of the gun control regulation. Table 1.3 shows coefficients estimated from these simulations. The only significant result is obtained when I consider the correct date in which the prohibition of concealed carry took effect, i.e., January 2004.

[33] The mean of monthly gun-related homicides per 100,000 people mean is .75, and the standard deviation is 2.

[34] I obtained this number by multiplying 0.191 by 12 to get an annual measure. Next, I multiplied the outcome by 167,546,532 and divided by 100,000.

[35] Waisekfisz (2016) indicated that 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. My estimation, however, controls for weather and seasonality effects, uses monthly data at the municipality level, and examines a much shorter period than in Waisekfisz (2016) to overcome my inability to control for important economic and social changes that can affect my dependent variable.

1.4.2 What is the effect of the concealed carry prohibition on other crimes?

To answer this question, I use monthly data on non-homicide crimes that are provided by the state of São Paulo only.[36] I find that robbery, illegal gun carrying and total arrests were reduced while rape, drug trafficking and theft remained unchanged.

Figure 1.4 shows the impact of the law on five crimes plus total arrests, which are: illegal gun carrying, drug trafficking, rapes, robbery and theft. As one can notice, the concealed carry prohibition decreased the number of crimes related to guns as well as total arrests.[37] The monthly data show that illegal gun carrying per 100,000 people decreased by 0.94 (26 percent reduction); robberies per 100,000 people decreased by 5.52 (7.7 percent reduction); and arrests per 100,000 people decreased by 3.37 (16 percent reduction).[38]

I find evidence that the concealed carry prohibition affects gun-related crimes, but does not change the remaining (non-gun-related) crimes, indicating that the law inhibited criminals from carrying guns. These results should be interpreted with care as the sample covers only São Paulo state. I do not claim that São Paulo is representative of the entire country: it is a relatively rich state (largest GDP and second largest GDP per capita) and the most populous of Brazil. Nonetheless, it can provide some insights about the entire country, especially because the state of São Paulo had a similar reduction in gun-related homicides (9.91 percent) as observed for whole sample.[39] 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.

[36] 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.
[37] 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.
[38] Using a falsification test where I define the cutoff to be January 2003, I find no effects for illegal gun carrying, robberies and total arrests.
[39] This result is available upon request.

1.4.3 Who benefits the most from prohibiting the right to carry concealed weapons?

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 gun-related homicides by age and race of victims. Before showing the results, I present descriptive statistics in Table 1.4. It shows the number of gun-related homicides in 2003 divided across race and age.[40]

The reduction in gun-related homicides was especially pronounced among young black males. Table 1.5, Panel A, shows that the effect on gun-related homicides is driven by blacks. Although *only* 56 percent of the victims of gun-related homicides are blacks (Table 1.4), the effects of the concealed carry prohibition surface almost exclusively among this segment of the population. Waisekfisz (2012) performs an analysis of homicides victims in Brazil by race. The author argues that blacks, compared to whites, are disadvantaged in terms of education, income, and security, and that they are the main victims of violent crimes. Therefore, my results indicate that the concealed carry prohibition was more effective in areas that lack security, and have high rates of crime. Panel B of Table 1.5 suggests that young people (between 15 and 29 years of age) experienced greater benefits from the law; however, this should be expected because this group represents the majority of the victims of gun-related homicides (Table 1.4).

Table 1.5 indicates that the effect of the prohibition of carrying concealed weapons is related to 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 1.6 shows, the effects of the concealed carry prohibition are driven by the last quartile that splits off

[40] I chose not to focus on gender because most of the victims of gun-related homicides are male (about 94 percent of the total).

the highest 75 percent of municipalities according to gun-related homicides rates. Therefore, the effects of the concealed carry prohibition were pronounced among young black males living in crime-ridden areas. Next, I use hospitalization data to investigate the effect of the law on gunshot wounds.

The subsequent analysis investigates data on monthly gunshot wounds at the municipality level, which are classified as "accidental" or "intended to kill." As gunshot wounds happen less frequently than gun-related homicides (in 2003 there were 21,484 gunshot wounds), I restrict my sample to municipalities with more than 50,000 people.[41] Table 1.7 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 11.6 percent in the total gunshot wounds in the "intended to kill" category. This is additional evidence that prohibiting the carrying of concealed weapons affects victims of murder instead of victims of involuntary manslaughter.

1.4.4 The vulnerability index

To investigate the previous subsection indication that high-crime areas disproportionately benefitted from the law, I construct an index considering the level of at-risk youth in each municipality.[42] The goal of this index is to map the places that have more young people susceptible to becoming criminals. They, and the people living close to them, are more likely to be exposed to gun-related homicides. The index I construct is based on the index of vulnerable young people

[41] The decision to restrict the sample to municipalities with more than 50,000 people results in analysis of 65 percent of the Brazilian population, but that group includes 98 percent of gunshot wounds intended to kill and 93 percent of accidental gunshot wounds. The results are still significant, but less precise, if I consider municipalities with more than 10,000 people (92 percent of the total Brazilian population).

[42] Here, at-risk youth measures how unlikely young people are to avoid a life of crime.

developed by the SEADE Foundation (State System of Data Analysis) for the São Paulo city neighborhoods.[43] Formally, the index is constructed as follows:

$$vulnerability\ index_m = \frac{\sum_{i=1}^{5}\left(\left(\frac{var_{mi}-Min(var_i)}{Max(var_i)-Min(var_i)}\right)*100\right)+\left(100-\left(\frac{var_{m6}-Min(var_6)}{Max(var_6)-Min(var_6)}\right)*100\right)}{6} \quad (5)$$

where i represents the six variables described in Table 1.8.[44]

The decision to use such an index finds support in the literature. As Cook and Ludwig (2006) argue, young people comprise "a relatively high percentage of whom are killed in gang- and felony-related attacks by youthful criminals" (p.387). Young people are also overrepresented as the victims of gun-related homicides in Brazil (Figure 1.1), and they experienced more pronounced effects from the prohibition on carrying concealed weapons (Table 1.5). Also, by considering homicide rates, this index captures the effect presented in Table 1.6 showing that the law disproportionately benefitted high-crime areas. Thus, prohibiting the carrying of concealed weapons should disproportionately affect gun-related homicides in places with higher vulnerability indices.

Next, I show that the number of gun-related homicides disproportionately decreased in places where the vulnerability indices were larger. Table 1.9 uses the RDD proposed in equation (2) and finds that an increase of one unit in the vulnerability index intensifies the effect of the concealed carry prohibition by additionally reducing the annual gun-related homicides by 221. Therefore, the law provided more benefits to areas with higher levels of youth violence.

[43] See http://produtos.seade.gov.br/produtos/ivj/

[44] This index hypothetically ranges from zero to 100. However, the minimum and maximum values are respectively 11.49 and 58.32. Its average equals 32.10, and standard deviation equals 5.

1.4.5 Bolstering internal validity with a difference-in-differences (DID) model

The previous subsection shows that gun-related homicides disproportionately decreased in high-crime areas. This conclusion allows me to validate my RDD findings using a difference-in-differences model, where the continuous treatment group is composed of areas with higher vulnerability indices (the regions that were more affected by the law).

Strong internal validity is a great advantage of RDD models. However, one common criticism of the methodology is that internal validity is obtained at the expense of external validity. One feature of my analysis helps mitigate this concern: namely the fact that many municipalities (2,875) had more than 10,000 people, and so my sample contains 51.6 percent of Brazilian municipalities. In addition, to demonstrate the robustness of the findings, I estimate the DID proposed in equation (3).

Table 1.10 presents the results showing that an increase of one unit in the vulnerability index intensifies the law's effect by additionally reducing the annual gun-related homicides by 244. The estimated coefficient (β_3) is very close to the one estimated in Table 1.9, bolstering the internal validity of the RDD estimates. Taking advantage of this DID strategy, I show next an analysis using annual data on school enrollment as the dependent variable to check if there is any indication of larger school enrollment of young males in high-crime areas.

1.4.6 School Enrollment

The empirical evidence presented thus far indicates that young black males living in high-crime areas were disproportionately affected by the legislation. This group should, therefore, be participating more in alternative activities such as education. Using data of the *Censo Escolar*

(Brazilian school census),[45] I find empirical evidence that male enrollment in adult education increased more in high-crime areas after the concealed carry ban took effect. Adult education is a public program focused on giving young adults who dropped out of or never attended school the opportunity to finish their basic studies. In 2004, 63 percent of people enrolled in this program were between ages 15 and 29 (85 percent were between ages 15 and 39). Though collection of race-related data only began in 2005 − thus preventing a racial analysis, given my time window − the initial information from 2005 indicates that blacks used adult education more than other races; among the male students who declared their race, 67.4 percent were black.

Figure 1.5 illustrates my argument; it shows that male enrollment in adult education increased disproportionately more in places with an above-median vulnerability index (treatment group), while female enrollment did not change. I use female enrollment as a placebo because women are almost unaffected by gun-related homicides (94 percent of such victims are male). Schools release enrollment figures annually, at the beginning of the year. Therefore, the year 2005 captures the effect of the concealed carry ban at a time when the law had been in place for about a year.

Table 1.11 tests the significance of the results using the same methodology proposed in equation (3), but using enrollment in adult education per 100,000 people as the dependent variable. It shows that an increase of one unit in the vulnerability index amplifies the effect of the concealed carry prohibition on male enrollment by increasing it by 6.5 enrollments per 100,000 inhabitants.

To conclude, the main result of section 4 is that prohibiting the carrying of concealed weapons reduces gun-related homicides and that high-crime areas disproportionately benefitted from the regulation. In the next section, I show that high-crime areas were also more likely to turnout to vote in the referendum and to support the gun prohibition.

[45] Data for the Censo Escolar (Brazilian school census) can be found at: http://portal.inep.gov.br/censo-escolar

1.5 Policy feedback: the 2005 Brazilian referendum case

This section investigates whether areas that benefitted most from the concealed weapon-carrying prohibition had higher voter turnout and 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 (4). Places with high vulnerability indices disproportionately benefitted from the law; thus, I expect these places to have higher voter turnout, and for voters to show more support for the gun prohibition referendum.

As mentioned before, the vulnerability index was originally constructed to measure young people's vulnerabilities to crime in the neighborhoods of the São Paulo municipality. As São Paulo is the largest city of Brazil, the Superior Electoral Court makes electoral neighborhood-level data available for the São Paulo municipality. Taking advantage of these neighborhood-level data, Figure 1.6 presents the estimated relationship between voting in favor of the gun ban and the vulnerability index, after adjusting for income and population. As expected, the relationship is positive and strong. Next, I show that this relationship also exists across the country.

Table 1.12 presents an OLS regression using the vulnerability index to explain the vote in favor of the prohibition (equation 4). I find a positive relationship between vulnerability index and support for gun prohibition. The coefficient estimated in column 1 is remarkably close to the one estimated for the São Paulo city's neighborhoods (Figure 1.6); even after all control variables are added to the model, as Column 2 presents, the estimated vulnerability index impact remains close to the one estimated for São Paulo city's neighborhoods. One way to interpret the vulnerability index coefficient is to compare municipalities with the "best" and "worst" indices.[46] In moving from a municipality with the "best" index (11.49) to a municipality with the "worst" index (58.32), the

[46] The "best" index in this context means that the municipality had the lowest vulnerability index, and the "worst" had the highest vulnerability index.

likelihood of voting in favor of the prohibition increases by 12.27 percentage points. This is a relevant number as an increase of 13.94 percentage points for the "Yes campaign" would have been enough for the proposed weapons ban to win.

Although my estimations provide strong and expected results, they may suffer from omitted variable bias, especially due to the lack of control for the number of guns in the municipalities. This could explain the significance of the results. For instance, it could be the case that places without gun-related homicides are also places where many citizens have firearms and where firearms serve as a deterrent to violence.[47] To address this potential omitted-variable problem, I collect municipal data on the number of unlawful gun firings and unlawful gun carrying after the law's passage to serve as a proxy for the number of guns in the municipality.[48] Unfortunately, these data are only available for the state of São Paulo. As Column 3 of Table 1.12 shows, the number of guns in the municipalities is not driving my results. Once again, the vulnerability index coefficient remains close to those estimated for São Paulo's neighborhoods and the whole country. This confirms my previous results, and I further validate them in the appendix.[49] Next, I discuss the effect of the vulnerability index on voter turnout.

Before investigating the effect of vulnerability index on the turnout-to-electorate ratio, I discuss the turnout bias introduced by mandatory voting in Brazil. As Cepaluni and Hidalgo (2016) argue, in Brazil, a compulsory voting system increases inequality in turnout. The participation gap between poorer and wealthier voters is heightened by the Brazilian compulsory voting system

[47] This argument is assessed through a model of crime in Donohue and Levitt (1998).

[48] This data are aggregated by year, at the municipal level. The year considered in the sample is 2004.

[49] I use a survey that took place two days before the referendum to corroborate my argument that people who were more likely to benefit from the concealed weapon ban (i.e. people more exposed to gun violence) showed greater support for weapons ban (Tables A2). I also present in the appendix (Tables A3 and A4) an analysis showing that the closer to the referendum that a gun-related homicide takes place, the more it positively affects support for the gun ban.

because nonmonetary penalties for abstention disproportionately affect middle- and upper-class voters. Therefore, they turnout more to vote. As examples of these nonmonetary fines, the authors mention prohibiting violators from obtaining a passport and/or taking a civil service exam − services that are primarily used by members of middle and upper classes.

As a higher vulnerability index is associated with poverty, and voting turnout is biased toward upper classes, a negative relationship between this index and turnout should be expected. To solve this problem, I include the previous turnout-to-registered-voters' ratio (turnout in the 2004 elections) and interact this variable with the vulnerability index. Table 1.13 presents the results. As one can notice, given a certain level of the 2004 turnout-to-registered-voters' ratio, an increase of one unit in the vulnerability index increases voter turnout for the referendum by 1.3 percentage points, indicating that people living in areas with larger reductions in gun violence care more about gun-control policies.[50]

1.6 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 (2016) 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

[50] In the appendix I show (Table A2) that people who would have voted in the referendum even if voting were not mandatory were more likely to support the gun ban.

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 3,900 (10.8 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 – both because the reduction in gun-related homicides was particularly pronounced among that population, and because in the wake of the law young black men were more likely to enroll in public adult education. The research here also shows that non-gun-related homicides were not affected by the regulation, and that the number of gunshots intended to kill decreased after the law, but accidental gunshots were not affected.

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). Therefore, 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 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 (3,900 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. Additionally, as I showed in my analysis, gunshot wounds intended to kill were reduced by 12.3 percent. 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 $565.8 thousand through this channel.[51]

I also show that the legislation decreased illegal gun carrying, robbery and total arrests. However, reported rapes, thefts and drug-trafficking incidents were not affected. 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 show that areas that experienced larger decreases in gun-related homicides also experienced higher levels of voter turnout and showed greater levels of support for the referendum that proposed a complete gun ban.

The places that experienced larger decreases in gun-related violence following the enactment of the concealed carry ban were largely concentrated in regions that represent about 39 percent of the Brazilian population (i.e., places with above-median levels of vulnerability as measured by a vulnerability index). By comparison, 36 percent of voters cast ballots in favor of the gun ban. These findings 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. If these externalities are large enough, ignoring them

[51] This calculation is underestimated as it 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).

will result in an outcome with a lower social welfare. Therefore, in these situations, referendums should not be used (Maskin and Tirole, 2004).

My results could be even 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.

1.7 Tables and Figures

Table 1.1 – 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
Other Crimes – São Paulo	
Robbery	332,229
Theft	645,529
Rape	3,978
Drug Trafficking	13,935
Illegal Gun Carrying	17,253

Table 1.2 – 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	-0.227***	-0.036	-0.191***
12 months bandwidth	(0.070)	(0.046)	(0.053)
Concealed Carry Prohibition	-0.108***	0.003	-0.111***
24 months bandwidth	(0.029)	(0.018)	(0.022)
Concealed Carry Prohibition	-0.127***	-0.009	-0.119***
48 months bandwidth	(0.021)	(0.013)	(0.016)

Robust standard errors clustered at the municipality level are in parenthesis. Each line shows different bandwidth selection. First row regressions contain 71,420 observations. Second row regressions contain 139,925 observations. Third row regressions contain 277,129 observations. All regressions control for 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 1.3 – Falsification test

Cutoff	(1) Gun-related homicides	(2) Observations
Concealed Carry Prohibition Cutoff – January 2004	-0.191***	71,420
	(0.053)	
Concealed Carry Prohibition Cutoff – January 2003	0.059	71,224
	(0.050)	
Concealed Carry Prohibition Cutoff – January 2002	0.059	71,049
	(0.049)	
Concealed Carry Prohibition Cutoff – January 2001	-0.048	70,260
	(0.050)	
Concealed Carry Prohibition Cutoff – January 2000	0.084*	69,475
	(0.045)	
Concealed Carry Prohibition Cutoff – January 1999	0.048	69,379
	(0.050)	
Concealed Carry Prohibition Cutoff – January 1998	-0.073	69,446
	(0.049)	
Concealed Carry Prohibition Cutoff – January 1997	0.031	69,268
	(0.049)	

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 All municipalities with more than 10,000 people are considered. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4 – Descriptive statistics

Race	Gun-related homicides	Age	Gun-related homicides
White	13,224	Less than 15	495
Black	20,291	Between 15 and 29	21,371
Other	2,600	More than 29	14,249
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).

Table 1.5 – Gun-related homicides by race and age

Panel A - race

VARIABLES	(1) Gun Related Homicides White	(2) Gun Related Homicides Black	(3) Gun Related Homicides Other
Concealed Carry Prohibition	-0.007	-0.153***	-0.029*
	(0.029)	(0.037)	(0.016)
Observations	71,420	71,420	71,420

Panel B - age

VARIABLES	(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	-0.112***	-0.075**	-0.003
	(0. 035)	(0.034)	(0.006)
Observations	71,420	71,420	71,420

Robust standard errors clustered at the municipality level are in parenthesis. Bandwidth is equal to 12 months. All regression control for 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 1.6 – Quartile analysis

VARIABLES	(1) < 25%	(2) >25% and <50%	(3) >50% and <75%	(4) >75%
Concealed Carry Prohibition	-0.064 (0.057)	-0.044 (0.072)	-0.085 (0.102)	-0.499*** (0.154)
Observations	17,769	17,950	17,951	17,750

Robust standard errors clustered at the municipality level are in parenthesis. 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. Bandwidth is equal to 12 months. All regressions control for calendar months, rain and temperatures. *** p<0.01, ** p<0.05, * p<0.1

Table 1.7 – Gunshot wounds by intention

VARIABLES	(1) Gunshot wounds intended to kill	(2) Accidental gunshot wounds	(3) Total gunshot wounds
Concealed Carry Prohibition	-0.109***	-0.030	-0.114**
	(0.038)	(0.035)	(0.055)
Observations	13,738	13,738	13,738

Robust standard errors clustered at the municipality level are in parenthesis. Bandwidth is equal to 12 months. All regression control for calendar months, rain and temperatures All municipalities with more than 50,000 people are considered. *** p<0.01, ** p<0.05, * p<0.1

Table 1.8 – Socioeconomic variables used to construct the vulnerability index

i	Variables[52]	Mean	Std. Dev.	Min	Max
1	% of mothers between 15-17 years	8.5	6.5	0	57.9
2	% of people between 15-17 years, never attended school	2.1	3	0	34.3
3	% of people between 15-19 years	10.7	1.3	4.4	16
4	Male homicides per 100.000 people between 15-29 years	29	40	0	431
5	% of population growth between 1997 to 2001	6.1	13.6	-50.2	171.6
6	Monthly household per capita income (in Brazilian Reais)	170.8	96.4	28.3	954.6

[52] All variables contain 5507 observations. Variables 1 to 3 and variable 6 are collected from the 2000 Census obtained at IBGE (Brazilian Institute of Geography and Statistics). The remaining variables are obtained at IPEADATA (Institute of Applied Economic Research). Variable 4 calculates the average between 1996 and 2005 as this variable oscillates substantially across years.

Table 1.9 – Relationship between the concealed carry prohibition and the vulnerability index

VARIABLES	(1) Gun Related Homicides
Concealed Carry Prohibition*Vulnerability Index	-0.011*** (0.003)
Observations	71,420

Robust standard errors clustered at the municipality level are in parenthesis. Bandwidth is equal to 12 months. All regression control for 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 1.10 – DID analysis showing the effect of concealed carry prohibition on gun-related homicides

VARIABLES	(1) Gun Related Homicides
Post*Vulnerability Index	-0.146*** (0.039)
Observations	5,757

The regression is controlled by municipal fixed effects. Robust standard errors are in parenthesis. The regression is additionally controlled by population and income. All municipalities with more than 10,000 people are considered. *** p<0.01, ** p<0.05, * p<0.1

Table 1.11 – DID analysis showing the effect of concealed carry prohibition on school enrolment

VARIABLES	(1) School enrollment men	(2) School enrollment women
Post*Vulnerability Index	6.494***	3.036
	(2.066)	(2.241)
Observations	5,770	5,770

The regression is controlled by municipal fixed effects. Robust standard errors are in parenthesis. The regression is additionally controlled by population and income. The year dummy assumes the value of one when year equals to 2005 and zero when year equals to 2004. All municipalities with more than 10,000 people are considered. *** p<0.01, ** p<0.05, * p<0.1

Table 1.12 - OLS regression using vote in favor of gun prohibition as the dependent variable

VARIABLES	(1) Vote in favor of the prohibition Brazil	(2) Vote in favor of the prohibition Brazil	(3) Vote in favor of the prohibition São Paulo state
Vulnerability index	0.227***	0.262***	0.204***
	(0.046)	(0.044)	(0.068)
Socio-economic controls	No	Yes	Yes
Proxy for number of guns	No	No	Yes
State fixed effects	Yes	Yes	No
Microregion fixed effects	No	No	Yes
São Paulo state only	No	No	Yes
Observations	5,507	5,505	645
R-squared	0.650	0.682	0.532

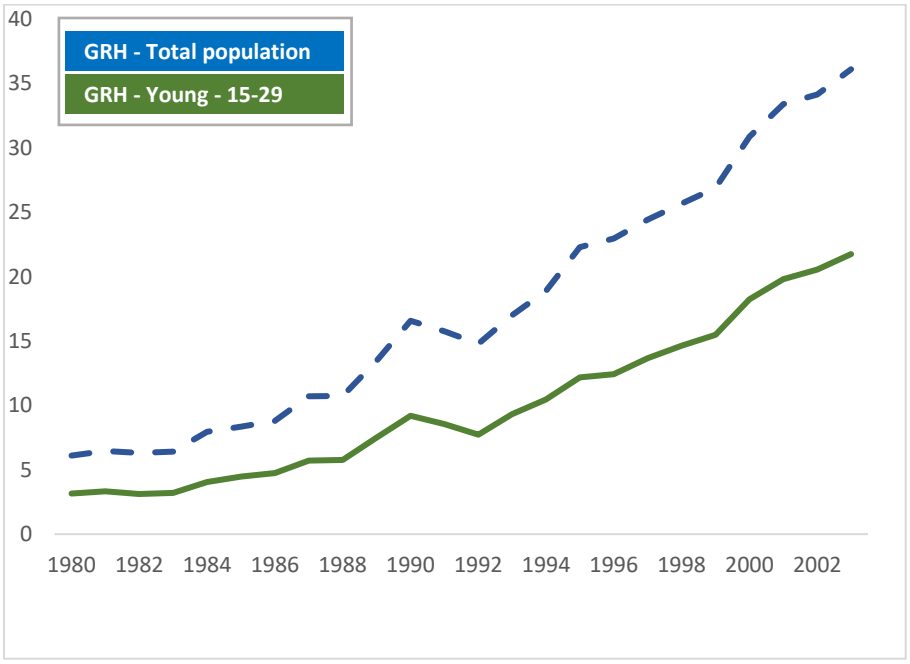
Robust standard errors clustered at the microregion (557 total) level are in parenthesis. The socio-economic controls contain population, percentage of people living in rural areas, per capita GDP, ideology, distance to state capital, per capita conditional cash transfer, women to men ratio, per capita number of cattle, dummy for drought, dummy for land reform protest, percentage of land bought by the government and redistributed to landless farmers. The proxies for number of guns are defines as the number of illegal gun carrying and illegal gun firing. *** p<0.01, ** p<0.05, * p<0.1

Table 1.13 - OLS regression using voting turnout in the referendum as the dependent variable

VARIABLES	(1) Voting Turnout
Voting Turnout in 2004	0.322***
	(0.090)
Vulnerability index	-0.014***
	(0.002)
Voting Turnout in 2004 * Vulnerability index	0.013***
	(0.003)
Observations	5,502
R-squared	0.729

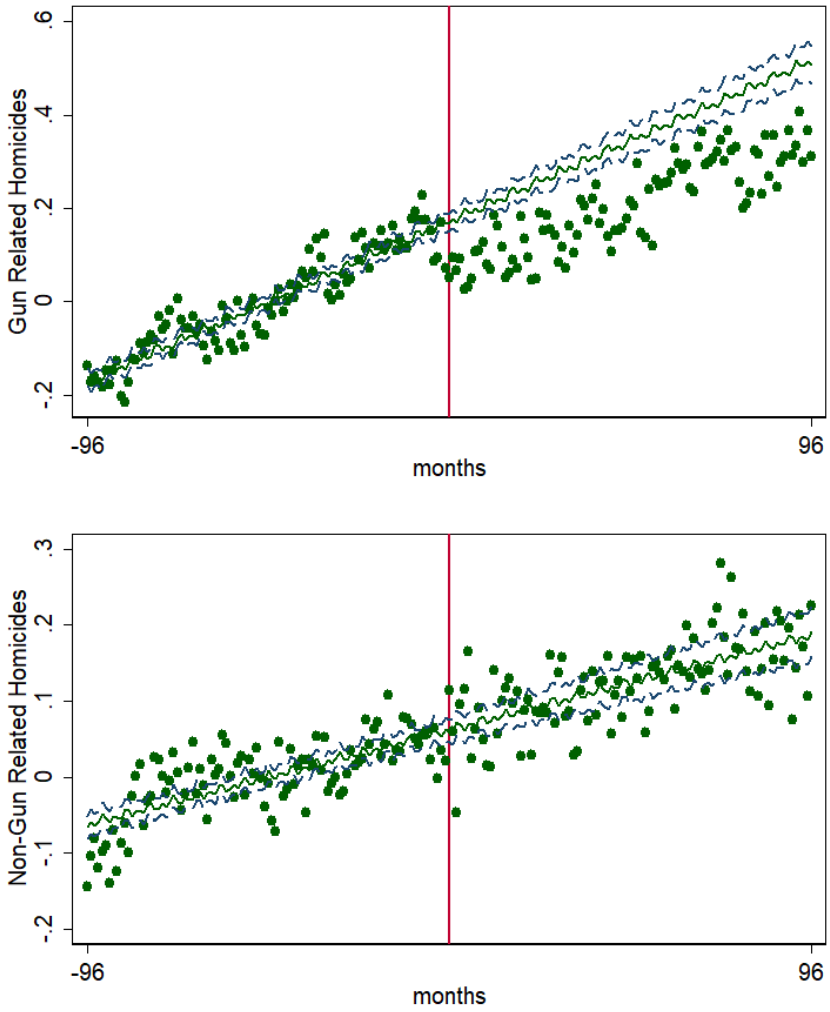
Robust standard errors clustered at the microregion (557 total) level are in parenthesis. The regression is additionally controlled by population and per capita GDP. *** p<0.01, ** p<0.05, * p<0.1

Figure 1.1 – Yearly gun-related homicides (GRH) in the Brazilian territory (in thousand).



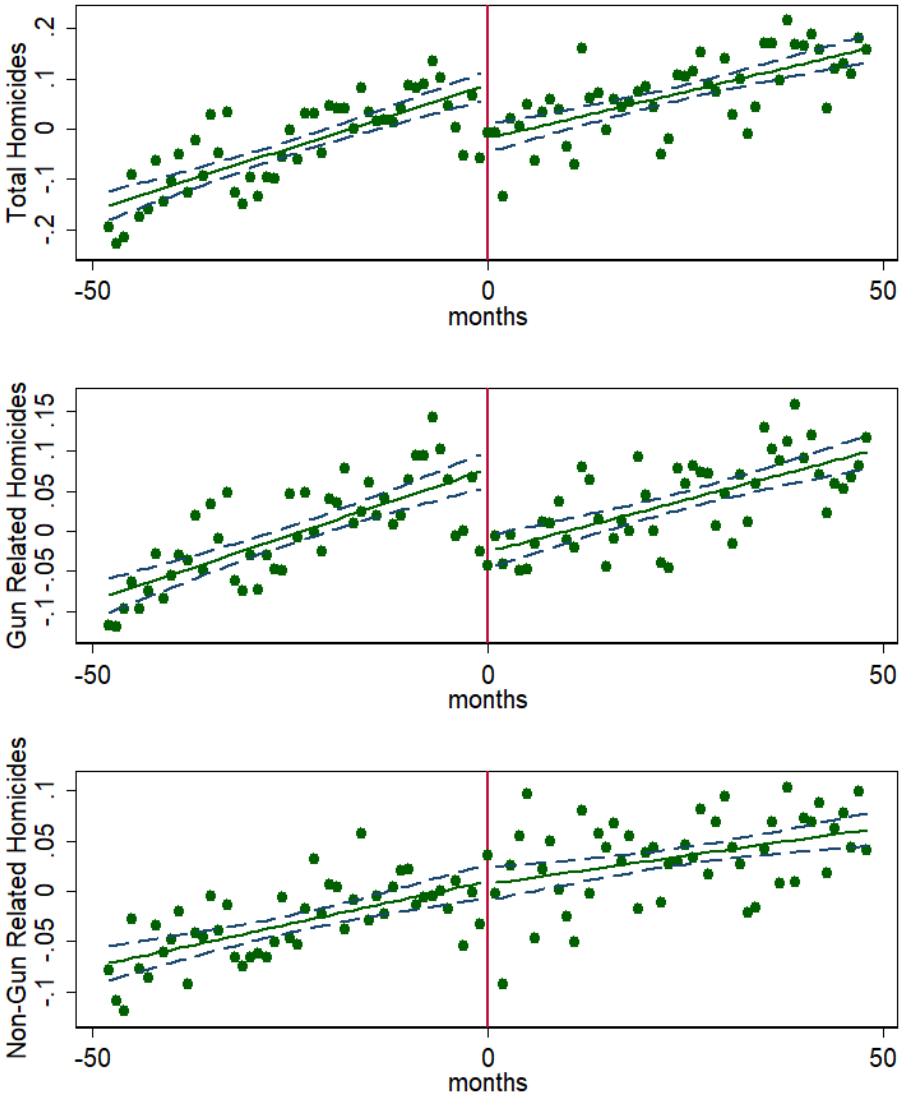
Notes: The data is available at DATASUS. The dashed line shows the total gun-related homicides, and the solid line shows the gun-related homicides concentrated on 15-29 years old people (close to 60% of the total gun-related homicides).

Figure 1.2 – Gun-related and non-gun related homicides trends



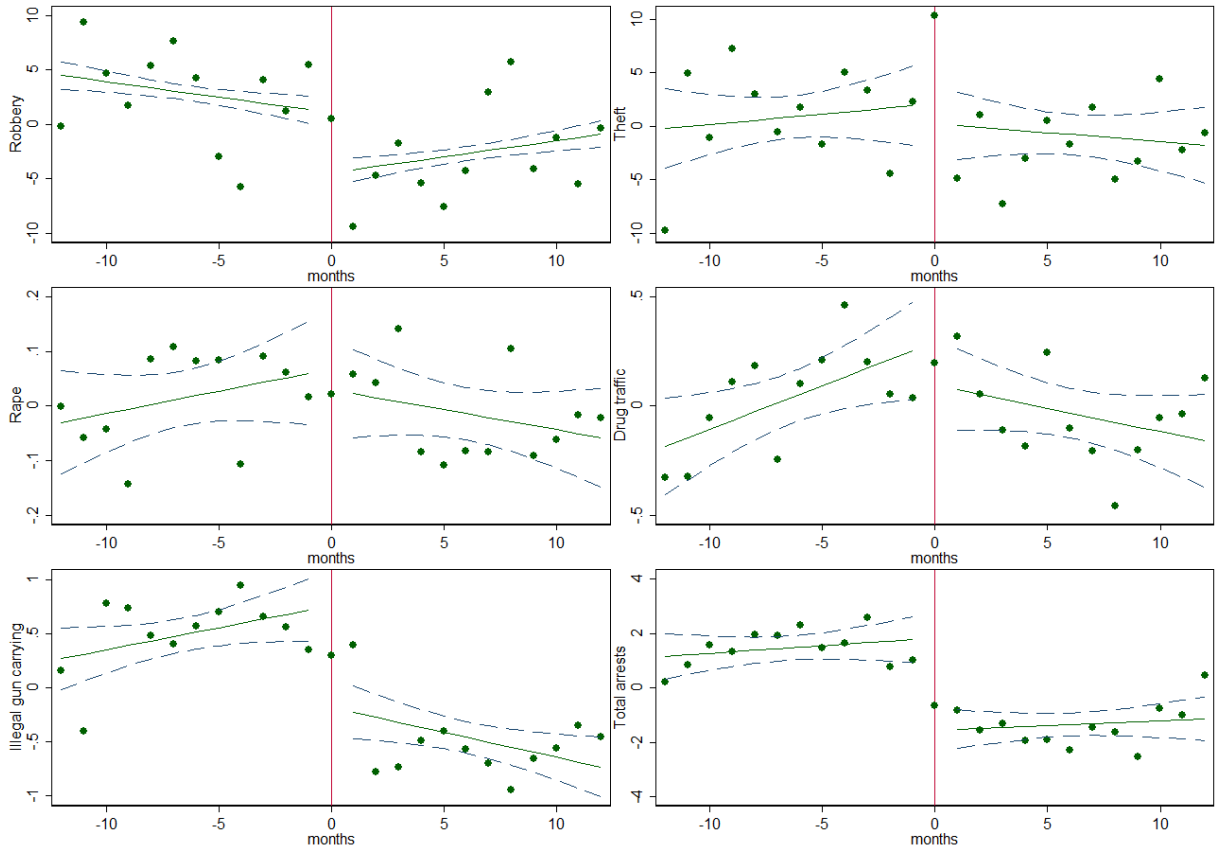
Notes: The top graph shows scatter plots representing the monthly gun-related homicides per 100,000 people and the bottom graph shows scatter plots representing the monthly non-gun related homicides per 100,000. The vertical line at month zero represents the intervention. The solid function is fitted 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 1.3 – Effect of the concealed carry prohibition on total homicides, gun-related homicides and non-gun-related homicides per 100,000 people



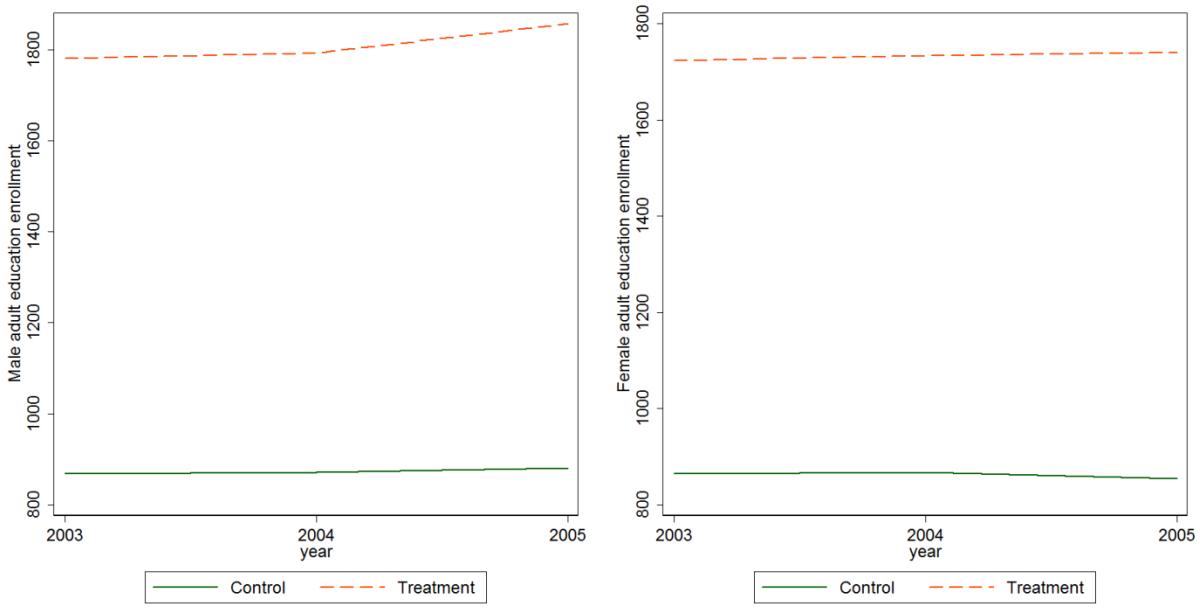
Notes: Figure 1.3 shows three time-varying functions using a 48 months' bandwidth and a vertical red line representing the cutoff point (January 2004). The solid line is fitted separately on each side of the threshold, and the dashed line represents the 95% confidence interval. The scatter plots show monthly averages. I regress the predicted residuals after regressing my dependent variables on calendar months, monthly rainfall and temperatures to take seasonality into account.

Figure 1.4 – Concealed prohibition effect on gun-related homicides (GRH), non-gun-related homicides (NGRH), total homicides and other crimes



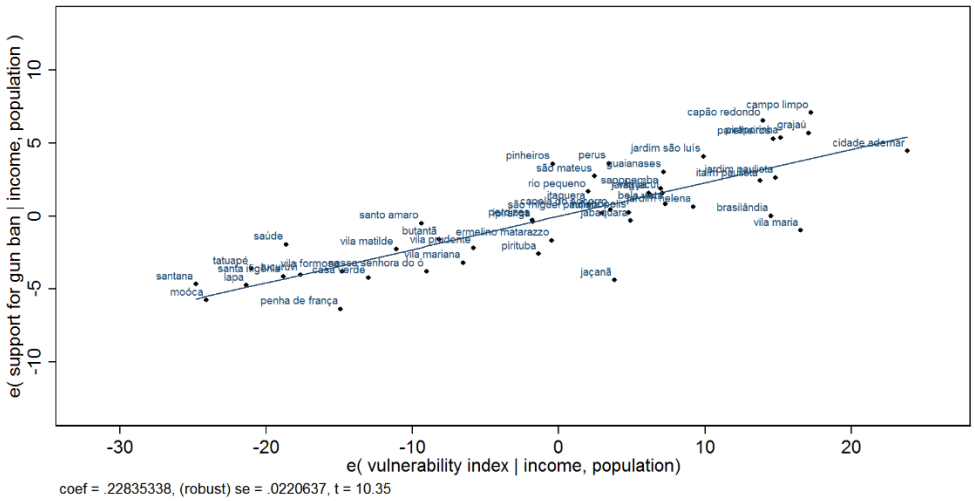
Notes: Figure 1.4 shows, for each crime, two time-varying function using a 12 months' bandwidth and a vertical red line representing the cutoff point (January 2004). The solid line is fitted separately on each side of the threshold, and the dashed line represents the 95% confidence interval. The scatter plots show monthly averages. I regress the predicted residuals after regressing my dependent variables on calendar months, monthly rainfall and temperatures.

Figure 1.5 – Average enrollment in adult education per 100,000 inhabitants



Notes: The dashed line represents the municipalities with above median vulnerability index. The solid line represents the municipalities with below median vulnerability index. All municipalities with more than 10,000 people are considered.

Figure 1.6 – Relationship between voting for the prohibition and vulnerability index



Notes: The dashed line represents the least square estimation of the relationship between the *residuals* of the linear regression of support for gun control on population and income and the *residuals* of the linear regression of the vulnerability index on population and income. The regression considers all 47 neighborhoods of the São Paulo municipality for which the TSE provides information on.

Chapter 2: Does Voting Technology Affect Clientelism?

2.1 Introduction

Electronic voting systems have been adopted by many democracies since the 1990s (Katz et al. 2011), and others are on the verge of using them (Alvarez and Hall 2008). However, introduction of such technologies need not be politically neutral. In fact, there is extensive evidence that differences in the design and the way information is displayed across these systems can have dramatic effects on electoral outcomes (Card and Moretti 2007; Herron and Wand 2007; Calvo et al. 2009; Katz et al. 2011). In this paper, I examine automated voting systems design and requirements and provide the first evidence that electronic voting affects clientelism.

One subtype of clientelistic strategy is *voter buying.* Hidalgo and Nichter (2016) define "voter buying" as the act of providing rewards to outsiders for transferring their electoral registrations into a given politician's district to vote for that candidate. As electronic voting eliminated the possibility of fraud after voting, voter buying became relatively more attractive. The phased-in introduction to electronic voting in Brazil in 1998 allows me to examine the impact of the new technology using a difference-in-differences methodology, where municipalities using electronic voting comprise my treated group. I establish the presence of voter buying by demonstrating that places using electronic voting show relatively inflated numbers of registered voters.

I then show the impact of electronic voting on the political support for clientelistic parties and voting turnout. I find that electronic voting reduced the vote share for parties with clientelistic tendencies, which I measure using the Democratic Accountability and Linkages Project (DALP) dataset, organized by the Duke University, that provide information about each Brazilian party's clientelistic tendencies. Finally, I show that electronic voting reduced voting turnout (i.e. turnout

to registered voters' ratio) especially in less developed areas that are historically dominated by political machines indicating that voters "bought" did not show up to vote.

This paper focuses on voter buying, a worldwide phenomenon also existent in Brazil.[53] I follow Hidalgo and Nichter (2016) empirical strategy and examine changes in the number of registered voters to show evidence of voter buying in Brazil. The authors find that mayors running for re-election in *small* municipalities pay voters from surrounding larger municipalities to register in their locality and to vote for them.[54] Empirical evidence from their work shows that small municipalities have inflated electoral registration levels during municipal elections.

Although there is evidence of voter buying in Brazilian municipal elections, no previous evidence of voter buying has surfaced in federal elections. The absence of voter buying on the federal level stems from Brazil's large, multi-member electoral districts, which reduce the incentives and ability of politicians to buy voters. Because each Brazilian state functions as a district, and representatives are elected to represent their own states, the location of votes within a state makes no difference to them, and districts are so large that many areas are remote from the potential "outside" voters that politicians might seek to buy.[55]

Nonetheless, I argue that voter buying did take place in the 1998 federal elections as the result of the introduction of electronic voting. As suggested by Hidalgo and Nichter (2016), electronic voting increased the relative appeal of voter buying by eliminating fraud after voting that had frequently been used in the paper ballot system. Therefore, voter buying became more attractive

[53] Evidence of voter buying has surfaced in the United States, Kenya, Mexico, Bolivia, Botswana, Bulgaria, Ghana, Jordan, Philippines and Swaziland (Hidalgo and Nichter, 2016).

[54] The authors presented three reasons why voter buying is more common in small municipalities: first, individual or small groups of voters are more likely to be pivotal in small districts. Second, political machines in small municipalities can better monitor voter compliance. Third, the returns to scale from political programmatic advertising are larger than returns from clientelism in larger municipalities.

[55] The number of seats in the chamber of deputies vary by state size. The minimum number of seats is eight and the maximum is 70, out of 513 seats.

in electoral districts that had electronic voting across their territories. And, even in electoral districts that had both electronic voting as well as paper ballots, political machines could still find voter buying relatively more attractive for two reasons. First, as Fujiwara (2015) shows, electronic voting raised the probability that a voter would cast a valid vote for representatives (by close to 30%). Consequently, congressional candidates who are willing to buy voters may want to reallocate them to places using electronic voting methods, where voters would be more likely provide valid votes. Second, as electoral districts in Brazil are large, candidates concentrate their effort on a few municipalities within the district that represent their *de facto* constituencies.[56]

I then investigate the impact of electronic voting on the vote share for parties with clientelistic tendencies and voting turnout. As electronic voting eliminated fraud after voting, political machines should lose political support with the introduction to the new voting system. However, as electronic voting also increased voter buying, which is a sub-type of a clientelistic strategy, one could argue that voter buying counterbalances political machines' losses from elimination of fraud after voting. I find that clientelistic parties were hurt by electronic voting, suggesting that fraud after voting was more effective than voter buying. This is especially true because voter buying requires voters' compliance and, as I show in this work, electronic voting decreased voting turnout particularly in areas historically dominated by political machines, providing additional explanation for the decrease in support for clientelistic parties.

[56] Politicians that had electronic voting in their constituencies would therefore be more willing to buy voters. See Ames (2001) for a detailed explanation of *de facto* constituencies in Brazil.

2.2 Literature review and Background

2.2.1 Electronic Voting

Electronic voting involves the use of electronic technologies to cast and/or tabulate the results of electoral competitions, and dates back to the introduction of mechanical lever voting machines in the United States in 1982. In the 1990s and early 2000s, use of these technologies rapidly extended beyond the borders of the United States and took root in many countries around the world, albeit with different trends in different global regions.[57] The entrenchment of electronic voting gave birth to a fierce debate regarding the positive and negative elements of the voting technology. Birch, Cockshott, and Renaud (2014) provide a concise yet informative overview of this debate.[58] Nonetheless, the conversation over electronic voting can be fruitfully extended beyond these areas to consider the effects of voting technologies on political behaviors and, chiefly, on political outcomes.

Scholars have provided compelling evidence that voting technologies can hold important consequences for electoral outcomes. Katz et al. (2011) examine a large-scale voting experiment that randomly assigned different voting machines prototypes to Argentinian voters and find that differences within alternative technologies, such as the way ballot cues are provided to voters, can favor some parties to the detriment of others. In the United States, many authors evaluate how voting technologies affected voting outcomes (Garner and Spolaore 2005, Dee 2007, Card and Moretti 2007, Shue and Luttmer 2009, and Ansolabehere and Stewart 2005). Callen and Long (2015) find that voting technologies reduced fraud in Afghanistan, while Hidalgo and Nichter

[57] See https://www.ndi.org/e-voting-guide/electronic-voting-and-counting-around-the-world

[58] They indicate that the positive aspects of electronic systems include speed, accuracy, and cost, and the negative elements include potential systemic glitches and lack of transparency in the recording, counting, and tabulation of votes (Birch, Cokshott, and Renaud 2014).

(2016) argue that electronic voting impeded fraud *after* voting in Brazil. Fujiwara (2015) shows empirical evidence that the introduction to voting machines in Brazil, by no longer requiring voters to write the names of legislative candidates in the ballot, amplified the ratio of valid votes to turnout for representatives, especially benefitting illiterate voters. To the best of my knowledge, this paper is the first to focus on the impact of voting technology on clientelism.

2.2.2 Introduction to Electronic Voting in Brazil

Electronic voting technology was introduced in Brazil to humper fraud after voting. In 1996, all municipalities with more than 200,000 eligible voters used the new technology. In 1998, the use of the technology expanded to include all municipalities with more than 40,500 eligible voters. In addition, four states used electronic voting for all their municipalities regardless of the number of eligible voters (Rio de Janeiro, Amapá, Alagoas and Roraima). In the 2000 election and for every subsequent election, every Brazilian voter voted electronically.

The new system also enfranchised voters and increased participation in legislative elections. Before electronic voting, many Brazilians experienced difficulties casting valid votes due to low literacy skills; votes were invalidated when candidates' names (or numbers) were not clearly written. Prior to the introduction of the electronic voting, knowing how to read and write as well as understanding the complicated ballot instructions were fundamental for a voter to cast her vote correctly and validly. With the introduction of the new system, the voter had only to indicate the number of his or her preferred candidate (or party in the legislative elections). Following their entry of said candidate's (or party's) number, a photo of the candidate or party would appear on the screen for confirmation. The ratio of the number of valid votes to turnout for federal representatives increased from 54 percent in 1994, when all municipalities used paper ballots, to

90 percent in 2002, when all municipalities used voting machines.

Nonetheless, the introduction of voting technology in Brazil also increased the attractiveness of voter buying. As previously mentioned, while electronic voting eliminated the possibility of some kinds of fraud (such as adding votes to tabulation sheets after actual voting takes place), it generated incentives for alternative methods of electoral malfeasance, such as voter buying (Hidalgo and Nichter 2016). Therefore, electronic voting is expected to impact clientelism. The next section presents my hypotheses on the electronic voting expected effects.

2.3 Consequences of Electronic Voting

Three aspects of the literature motivate my hypotheses' construction: First, electronic voting eliminated fraud after voting and increased the attractiveness of alternative electoral malfeasance (Hidalgo and Nichter 2016). Second, electronic voting made it easier to cast a vote for representatives (Fujiwara 2015). Third, voter buying is a common practice in Brazil (Nichter 2011; Hidalgo and Nichter 2016). Next, I present and discuss the two hypotheses that I empirically test in this work.

Hypothesis One (Attractiveness): Municipalities using voting machines should attract voter buying. Electronic voting eliminated the possibility of fraud after voting, a common practice in Brazil. Thus, alternative methods of electoral malfeasance, such as voter buying, became more attractive, especially for representatives from the four states using electronic voting across their territories. Even in states that had both electronic voting and paper ballot systems, voter buying could be justified. The main goal of voter buying is to acquire an advantage over competitors by guaranteeing that voters *bought* would provide electoral support in exchange for benefits. Therefore, if voter buying takes place, then buyers would want to guarantee that voters *bought* would be able to payback. As electronic voting increased voters' probability of correctly casting a

ballot, places using the new system should receive more voters. Additionally, representatives are tied to their *de facto* constituencies within their district making them more willing to buy voters if their constituencies had electronic voting.

Hypothesis Two (Clientelistic parties): Municipalities using electronic voting should experience a decrease in support for clientelistic parties. Fraud after voting is more effective than voter buying, otherwise it would not be used before electronic voting. Therefore, as electronic voting hampered fraud after voting, political machines should be hurt by its introduction, especially in places with smaller compliance, which I measure using voting turnout.

2.4 Estimation Strategy

2.4.1 Data and Methodology

I collect electoral data to empirically test my hypotheses. I focus on the federal elections of 1994, 1998 and 2002, which elected state and federal representatives, as well as senators, governors and president. Most of the data were collected by the TSE (Superior Electoral Court).[59] The TSE has reliable election results statistics and information about the number of eligible voters and voter turnout. The socioeconomic data I use for my analysis were collected from Ipeadata (Institute of Applied Economic Research).[60]

To test the effect of electronic voting on the share of the vote for parties with clientelistic tendencies, I construct the municipal support for clientelistic parties index.[61] Formally, I take i_P to be the classification for party *P* (ranging from five to 20), where five represents parties that make no clientelistic effort to attract voters, and 20 represents parties that make a major effort to attract

[59] The English version of the Superior Electoral Court can be accessed in the following link: http://english.tse.jus.br/
[60] The English version of the Ipea data can be accessed in the following link: http://www.ipeadata.gov.br/
[61] I use the Democratic Accountability and Linkages Project dataset, organized by the Duke University, to gather information about each Brazilian party's clientelistic tendencies.

voters by offering, in exchange for voter support, benefits such as consumer goods, preferential access to public resources, public employment and preferential access to government contracts.[62] Next, I define $votes_{Pm}$ as the number of votes for party P in municipality m for the federal representatives' election. Using this notation, it is possible to construct the following weighted vote share measure that can classify each municipality m according to index I_m:

$$I_m = \sum_P \left(i_P . \frac{votes_{Pm}}{\sum_P votes_{Pm}} \right)$$

The number of total valid votes in a municipality is represented by $\sum_P votes_{Pm}$, so the fraction that multiplies i_P is the municipal vote share of party P. Hypothetically, one can get I_m to equate any number between the interval $[5,20]$, but it ranges from 11.64 to 18.12.

I use a difference-in-differences (DID) model where municipalities voting electronically comprise the treated group. As previously mentioned, four states used electronic voting in all their municipalities in 1998. As stated by Fujiwara (2015), two reasons guided the selection of these four states. First, Amapá and Roraima (two remote states covered by the Amazon forest) were selected to test the electoral authorities' abilities to use the voting machines in isolated areas. Second, Rio de Janeiro and Alagoas were selected to ensure the coordination of the electronic devices' distribution jointly with the military, which provided security for election officials in these two states. The treated group is composed by municipalities belonging to these four states and all municipalities with more than 40,500 voters (i.e., all the municipalities that used electronic voting in 1998).

[62] This variable is called "b15". It measures the level of parties' clientelistic tendencies and can be accessed in the following link: https://sites.duke.edu/democracylinkage/data/

Table 2.1 shows the differences in political outcomes between both the control and treated groups within three federal elections (1994, 1998 and 2002).[63] Between 1994 and 1998, the treated group had a larger increase in its number of registered voters.[64] However, between 1998 and 2002, both groups experienced a similar change. This pattern should be observed if there is voter buying. The share of the votes for clientelistic parties decreased between 1994 and 1998 for both groups; however, municipalities using electronic voting had a larger decrease. Between 1998 and 2002, support for clientelistic parties once again decreased for both groups, but the control group had a larger reduction this time. Finally, voter turnout decreased for both groups between 1994 and 1998, nonetheless, this reduction was much larger in municipalities using electronic voting. Between 1998 and 2002 both groups had a similar increase in turnout.[65]

2.4.2 Empirical Models

First, I compare electoral outcomes in 1998 to 1994 using municipalities voting electronically in 1998 as treated group. Next, I provide a counterfactual analysis using the same treated group, but comparing electoral outcomes in 1998 to 2002. As all municipalities used electronic voting in 2002, I should observe a "catch up" effect. That is, municipalities using the new voting system for the first time in 2002, should catch up with the municipalities that used it for the first time in 1998. Thus, if the introduction of electronic voting is driving my results, then my second DID analysis should give me coefficients with similar magnitudes to my first DID analysis, but with the opposite

[63] In 1994 all municipalities used paper ballots; in 1998 about 10 percent of all municipalities used electronic voting; and in 2002 all municipalities used the electronic system.

[64] "Registered voters change" measures the change in registered voters from the municipal elections in year t-2 and the federal elections in year t. However, as there are no available data for the 1992 elections, the baseline (1994) is constructed by taking the change in the electorate between 1994 and 1996.

[65] The valid-votes-to-electorate ratio is similar between the two groups, with exception of 1998, where the treated group had a much larger valid-votes-to-electorate ratio. The choice of using the valid votes-to-electorate ratio instead of valid votes-to-turnout ratio is that the latter measurement could be confounding both effects, potentially affecting both any increase in the valid votes and any decrease in voter turnout.

sign. The following equation shows my regression model:

$$y_{mt} = \beta_0 + \beta_1 year + \beta_2 EV + \beta_3 (year * EV) + X_{mt} + \epsilon_{mt} \quad (1)$$

In this equation, y_{mt} is the dependent variable for municipality m at election t, i.e. $t = 1994;\ t = 1998\ or\ t = 2002$.[66] The variable $year$ is a dummy that equals 1 if year=1998 and 1994 is the baseline. Otherwise, $year$ equals 1 if year=2002 and 1998 is the baseline. EV is a dummy that indicates the treated group (municipalities that used electronic voting in 1998) and β_3 captures the effect of the treatment on the treated group and represents the interaction of both dummies for year and electronic voting usage ($year$ x EV). X_{mt} is a variable containing additional controls (such as income and population); β_0 is the constant; and ϵ_{mt} is the white noisy error term.

A potential threat to the voter buying statistical analysis is that during local elections in Brazil, small municipalities experience an artificial rise in the number of registered voters, while large municipalities lose a portion of their registered voters. However, in federal elections this cycle is reversed (Hidalgo and Nichter 2016). As my analysis focuses on federal elections and all large municipalities used electronic voting, my results might be driven by large municipalities that are expected to have a larger increase in the number of registered voters in federal elections. In what follows, I provide evidence showing that this is not likely to be the case and propose a regression discontinuity analysis to mitigate this concern.

First, Figure 1 shows the change in the number of registered voters, across control and treatment groups, within every two elections (e.g., the year 1996 represents the percentage change in the number of registered voters between 1994 and 1996). As the figure shows, both groups follow

[66] As dependent variable I analyze the number of registered voters, share of the vote for parties with clientelistic tendencies, and voting turnout.

similar patterns. The only discrepancy is found in 1998 when the percentage growth in the number of registered voters increases for the treated group, and decreases for the control group.[67]

Second, I mitigate concerns about municipalities' size by constructing a geographical regression discontinuity design, where distance to the boundaries of states using the voting technology across their territory is the forcing variable.[68] For this analysis, I restrict my sample to the municipalities surrounding the geographical boundaries of the four states that used electronic voting in all their territory. Formally, I construct the following regression:

$$EC_{mt} = \alpha + \lambda D + \beta_1(r - c) + \epsilon_{mt}, \, such \, that: (c - h) \leq r \leq (c + h) \quad (2)$$

where EC_{mt} is the number of registered voters' percentage change at municipality m at year t, c represents the cutoff (boundaries of states using electronic voting), r indicates the distance in kilometers to the cutoff, D is a dummy indicating that the boundaries of states using electronic voting were crossed, and h represents the selected bandwidth (in kilometers). λ is my main independent variable, which captures the electronic voting effect. β_1 measures closeness to the cutoff. Finally, ϵ_{mt} contains the error term for each observation.

My last empirical model aims to test whether voting turnout is related to support for clientelistic parties. As voter buying is only effective if voters "bought" turnout to vote, then places with relatively smaller turnout should hurt clientelistic parties the most. I construct a triple differences model to directly measure the impact of voting turnout on the support for clientelistic parties. My triple-differences model builds on equation (1) and adds one more variable to the main term (i.e.,

[67] The smoother change in registered voters beginning in 2008 is likely associated with the 2007 wave of voter audits conducted in Brazil, which was the most comprehensive in decades and eliminated many irregularities in voters' registration (Hidalgo and Nichter, 2016).

[68] For a detailed description of geographical regression discontinuity, see Keele and Titiunik (2015).

the interaction term). I add an interaction between the dummy for year, electronic voting usage and a voting turnout. More specifically, I construct the following model:

$$y_{mt} = \beta_0 + \beta_1 year + \beta_2 EV + \beta_3 turnout + \beta_4(year * EV) + \beta_5(year * turnout) + \beta_6(EV * turnout) + \beta_7(year * EV * turnout) + X_{mt} + \epsilon_{mt} \quad (3)$$

where y_{mt} is the share of the vote for clientelistic parties and β_7 is the variable of interest, which shows the effect of the treatment on the treated group in accordance with the voting turnout.

2.5 Testing Hypotheses

2.5.1 Hypothesis one: Attractiveness

Using the DID model presented above, I empirically test my hypotheses from Section 3. My first hypothesis is that municipalities using electronic voting should experience a greater increase in registered voters. Table 2.2 presents the results. As it shows, places using voting machines for the first time in 1998 (EV*Year=1998), experienced a 4.7 percent larger increase in the number of registered voters. Nonetheless, in 2002 (EV*Year=2002), when all municipalities used electronic voting, places that had used it for the first time in 1998 had a 3.4 percent smaller change in the number of registered voters compared to the control group. This "catch up" effect should be expected because in 2002, both groups were using the new voting system and, therefore, there was no incentive for voter buying in the federal elections.

The sign switch pattern observed in this analysis underscores the strength of the results. The only alternative explanation for this pattern would be a random shock that led to a relatively larger increase in the number of registered voters in locations using electronic voting for the first time in 1998 than in places not using it. In 2002, another random shock would have to occur that disproportionally affected the treated group, but this time in the opposite direction (i.e., the number

of registered voters in areas using electronic voting for the first time in 1998 would have increased relatively less than the control group).

Using equation (2), I show that my DID model results are corroborated using a geographical regression discontinuity design. The main required assumptions to validate the regression discontinuity model is that the conditional expectation of the percentage change in the number of registered voters is continuous when approximated to the cutoff and that electronic voting usage is the only way to justify a discontinuous change around the cutpoint. To validate these assumptions, I show that in 1994 (when all municipalities used paper ballots) as well as in 2002 (when all municipalities voted electronically), the percentage change in the number of registered voters is continuous around the cutoff. The only discontinuity close to the cutoff is observed in 1998, when only the treated group used electronic voting. Figure 2 presents the results. As it indicates, electronic voting caused the number of registered voters between 1996-1998 to increase by 5.1 percent, while in the remaining years, the change in registered voters is continuous around the cutoff. This estimation (5.1 percent) is close to the one estimated in the DID model, which further validates my previous estimation and strengthens my results.

In addition, I show in Table 2.3 that the regression discontinuity estimation is not sensitive to different bandwidth choice and that covariates such as illiteracy rate and percentage of rural areas, do not discontinuously change around the cutoff showing that municipalities above and below the cutoff are comparable. Using the Rdrobust package proposed by Calonico, Cattaneo & Titiunik (2014) and triangular kernel, I find that electronic voting caused the number of registered voters to increase by 4.5 percent (significant at the 0.01 level).

2.5.2 Hypothesis two: Clientelistic parties

My second hypothesis argues that electronic voting caused a reduction in the share of the vote for clientelistic parties. As the voting technology eliminated fraud after voting, a relatively more effective clientelistic strategy, then support for political machines should decrease.

Table 2.4 corroborates this hypothesis. Locations using electronic voting for the first time in 1998 experienced a decrease of 0.14 (about one-sixth of a standard deviation) in the vote share for clientelistic parties. Similarly, locations using electronic voting for the first time in 2002 experienced a decrease of 0.19 in their vote share for clientelistic parties. The observed switch sign pattern once again shows that electronic voting is probably the mechanism explaining the decrease in support for clientelistic parties.

The decrease in political support for clientelistic parties indicates that voter buying is less effective than fraud after voting. Voter buying, differently than fraud after voting, requires voters' compliance, i.e. voter "bought" need to show up to vote. Therefore, voter buying should be especially less effective in areas with smaller voting turnout. In what follows, I test whether areas with lower voting turnout experienced relatively lower support for clientelistic parties.

Before testing this hypothesis, I show in Table 2.5 that electronic voting caused a decrease in voting turnout. More specifically, the new voting system decreased the turnout-to-registered-voters' ratio by close to 4 percentage points (EV*Year=1998). The switch in the sign pattern was observed and statically significant (EV*Year=2002), showing that using electronic voting for the first time in the federal elections caused a decrease in turnout-to-registered voters ratio.

I then establish that areas dominated by political machines had more voter buying as expected, but had also smaller voting turnout and larger reduction in support for clientelistic parties. To do this, I split the sample between municipalities in which clientelism is more and less likely to occur. As

the literature on clientelism in Brazil argues, political machines are especially strong in *small*, *rural* and *poor* municipalities, and clientelism is more concentrated in the *Northeast* (see, for instance, Nichter 2009; Mainwaring, Meneguello and Power 2000; Nichter 2011; Gans-Morse, Mazzuca, and Nichter 2014). Using this information, I create Table 2.6 with four panels, each considering a different characteristic of Brazilian clientelism, and targeting the places where clientelism is more likely to occur.

The first three columns of each panel consider places that are more prone to clientelism according to the characteristics put forward in the literature: Panel A considers ruralism, so the first three columns represent places with above-median percentage of people living in rural areas. Panel B considers income, so the first three columns represent places with below-median income. Panel C considers region, so the first three columns represent places that belong to the Northeast region. Finally, Panel D considers size, so the first three columns represent places with below-median electorate size. In all panels, the last three columns represent the remaining parts of the sample. For instance, in Panel A, the last three columns (4, 5 and 6) represent places with a below-median percentage of people living in rural areas.

I analyze three dependent variables: Columns 1 and 4 measure the vote share for clientelistic parties; columns 2 and 5 measure the turnout-to-electorate ratio, and columns 3 and 6 measure the percentage change in the electorate. Each panel shows, for each dependent variable, two DID analyses, one comparing 1998 to 1994, and another comparing 2002 to 1998.

As Table 2.6 indicates, places where clientelism is more likely to occur demonstrate a much clearer effect in the three variables examined. All estimations for places where clientelism is larger are significant; most of them present a larger magnitude as well; and the switch sign pattern is always observed. Because electronic voting decreased voting turnout most markedly in places dominated

by clientelism, it seems intuitive to conclude that the support for clientelistic parties decreased also because of lower voting turnout. To corroborate this hypothesis, I next present results of a triple differences approach.

Table 2.7 shows equation (3) estimation results indicating that lower voting turnout explains the decrease in support for clientelistic parties. Table 2.7 (column 1) shows that, given electronic voting usage, places with larger voting turnout voted more for clientelistic parties. In other words, the smaller the turnout, the smaller the vote for political machines. The switch sign pattern was observed, and the magnitude of the coefficient was similar for both triple differences estimations. These findings suggest that places where clientelism is stronger used more voter buying, but experienced a disproportionate decrease in voting turnout. As a result, the net effect was a decrease in support for clientelistic parties. The next section discusses these findings and concludes.

2.6 Discussion and concluding remarks

This paper presented empirical evidence showing that electronic voting increases voter buying and decreases support for clientelistic parties.

Why did electronic voting increase voter buying? Politicians "buy voters" when they or their emissaries pay voters from other districts to register in their districts and to vote for them. The use of electronic voting systems created two incentives for politicians to buy voters: first, the system increased the probability of a voter correctly casting a vote; second, it eliminated the means that had been available to politicians to manipulate the outcoming of elections conducted with paper ballots. That is, electronic voting made it more difficult to take other previously used fraudulent steps to increase their votes by, for example, inflating the number of votes to tabulation sheets, and discarding votes for the opposition by claiming that there were illegible. On one hand, politicians whose districts have some locations with paper ballots and others with electronic voting would

want to reallocate voter buying practices to the most-effective locations. In locations where electronic balloting is in effect, "bought voters" would be more likely to correctly cast a vote and to "pay back." At the same time, politicians belonging to districts that rely exclusively on electronic voting throughout their districts, would be incentivized to buy voters as alternative frauds were eliminated. Thus, the fact that electronic voting increased the electorate growth by 5 percentage points, which is confirmed by both DID and regression discontinuity methodologies, indicates that *bought* voters inflated the electorate figures in locations that used electronic voting systems rather than paper ballots.

Finally, I find that electronic voting disproportionately hurt clientelistic parties. This can be intuitively explained by fraud after voting being a more effective way to fraud elections than voter buying, which is especially problematic because places that had more voter buying also experienced smaller voting turnout. Therefore, through the elimination of fraud after voting and the disenfranchisement of clientelistic parties' supporters, electronic voting decreased the share of the vote for parties that are essentially run by political machines.

To conclude, this paper shows that voting technology design matters. It affects electoral outcomes and clientelism. With many democracies on the verge of adopting these technologies (Alvarez and Hall 2008), careful thought should be given to guarantee that the introduction to electronic voting is a Pareto improvement and politically neutral.

2.7 Tables and Figures

Table 2.1 – Political outcomes across groups

Variable	Control	Treated	Difference	t-stat.
	Election year: 1994			
Change in registered voters	0.058	0.056	0.002	0.27
Vote share clientelistic parties	16.58	15.94	0.63	14.09
Turnout/electorate	0.78	0.83	-0.05	-12.11
Valid votes/electorate	0.46	0.48	-0.02	-5.33
Observations	4505	511		
	Election year: 1998			
Change in registered voters	0.022	0.067	-0.045	-5.38
Vote share clientelistic parties	16.28	15.54	0.74	15.06
Turnout/electorate	0.76	0.77	-0.01	-3.54
Valid votes/electorate	0.53	0.69	-0.16	-39.24
Observations	4976	530		
	Election year: 2002			
Change in registered voters	0.031	0.042	-0.011	-3.54
Vote share clientelistic parties	15.89	15.39	0.50	10.97
Turnout/electorate	0.8	0.82	-0.02	-9.12
Valid votes/electorate	0.74	0.77	-0.03	-9.39
Observations	4976	580		

Notes: The treated group is composed by the municipalities with more than 40,500 eligible voters in 1996 and the ones belonging to the following four states: Amapá, Alagoas Roraima and Rio de Janeiro. Therefore, the treated group used electronic voting in 1998. The control group is composed by the remaining municipalities of Brazil, which didn't use voting machines in 1998.

Table 2.2 – Measuring the electronic voting effect on the number of registered voters

VARIABLES	(1) Change in registered voters	(2) Change in registered voters
$year$ **x** EV **- 1998**	0.047***	0.071***
	(0.007)	(0.011)
Municipalities fixed effects	NO	YES
Controls	NO	YES
Observations	10,492	10,445
R-squared	0.014	0.767
$year$ **x** EV **- 2002**	-0.034***	-0.033***
	(0.006)	(0.011)
Municipalities fixed effects	NO	YES
Controls	NO	YES
Observations	10,994	10,994
R-squared	0.005	0.518

Robust standard errors clustered at the municipality level are reported in parenthesis. Control variables are composed by population and income. EV*Year=1998 represents the interaction between dummy for treated and a dummy for 1998 election year. EV*Year=2002 represents the interaction between a dummy for treated and a dummy for 2002 election year. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table 2.3 – RDD measuring the electronic voting effect on the number of registered voters and covariates

VARIABLES	(1) 100 km	(2) 50 km	(3) 25 km
Change in registered voters	0.048*** (0.010)	0.052*** (0.014)	0.050*** (0.017)
Share of rural population	0.036 (0.033)	0.026 (0.044)	-0.053 (0.055)
Human Development Index	-0.017 (0.013)	-0.004 (0.018)	0.035 (0.022)
GDP per capita	-9.077 (13.413)	6.685 (16.354)	37.429* (19.361)
Population in thousand	-30.957 (33.332)	14.293 (9.182)	32.168*** (10.656)
Illiteracy rate	4.996** (2.236)	2.848 (2.953)	-3.556 (3.639)
Observations	586	330	185

Robust standard errors clustered at the municipality level are reported in parenthesis. Each row shows the electronic voting effect on one specific variable. The socioeconomic variables were collected at the 2000 Brazilian Census. Each column represents a regression discontinuity with a specified bandwidth. Columns 1, 2 and 3 contains, respectively, a bandwidth of 100, 50 and 25 kilometers. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table 2.4 – Measuring the electronic voting (EV) effect on the share of the vote for clientelistic parties

VARIABLES	(1) Clientelism	(2) Clientelism
$year$ **x** EV **- 1998**	-0.111**	-0.144*
	(0.054)	(0.084)
Municipalities fixed effects	NO	YES
Controls	NO	YES
Observations	10,522	10,475
R-squared	0.058	0.778
$year$ **x** EV **- 2002**	0.245***	0.190***
	(0.047)	(0.070)
Municipalities fixed effects	NO	YES
Controls	NO	YES
Observations	11,065	11,065
R-squared	0.059	0.781

Robust standard errors clustered at the municipality level are reported in parenthesis. Control variables are composed by population and income. EV*Year=1998 represents the interaction between dummy for treated and a dummy for 1998 election year. EV*Year=2002 represents the interaction between a dummy for treated and a dummy for 2002 election year. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table 2.5 – Measuring the electronic voting (EV) effect on turnout-to-registered voters' ratio

VARIABLES	(1) Turnout/Voters	(2) Turnout/Voters
$year$ **x** EV **- 1998**	-0.037***	-0.039***
	(0.003)	(0.005)
Municipalities fixed effects	NO	YES
Controls	NO	YES
Observations	10,522	10,475
R-squared	0.031	0.902
$year$ **x** EV - 2002	0.013***	0.018***
	(0.004)	(0.005)
Municipalities fixed effects	NO	YES
Controls	NO	YES
Observations	11,062	11,062
R-squared	0.063	0.841

Robust standard errors clustered at the municipality level are reported in parenthesis. Control variables are composed by population and income. EV*Year=1998 represents the interaction between dummy for treated and a dummy for 1998 election year. EV*Year=2002 represents the interaction between a dummy for treated and a dummy for 2002 election year. The 1%, 5% and 10% level of significance are represented by ***, ** and * respectively.

Table 2.6 – Measuring the electronic voting (EV) effect on the share of the vote for clientelistic parties, turnout and electorate

Panel A – Tracing clientelism through rural areas

VARIABLES	(1) Clientelism	(2) Turnout	(3) Electorate	(4) Clientelism	(5) Turnout	(6) Electorate
year **x** *EV* **1998**	-0.587***	-0.1***	0.078**	-0.033	-0.02***	0.051***
	(0.224)	(0.012)	(0.034)	(0.089)	(0.005)	(0.011)
Observations	5,049	5,049	5,048	5,426	5,426	5,412
R-squared	0.792	0.901	0.843	0.761	0.898	0.622
year **x** *EV* **2002**	0.611***	0.040***	-0.071***	0.201***	0.018***	-0.023**
	(0.162)	(0.010)	(0.027)	(0.075)	(0.006)	(0.010)
Observations	5,557	5,555	5,503	5,508	5,507	5,494
R-squared	0.782	0.850	0.514	0.781	0.821	0.548

Notes: All regressions use municipalities fixed effects. Standard errors, clustered by municipalities, are presented in parenthesis. All regressions are controlled for income and population. Regressions (1), (2) and (3) consider only the places with above-median percentage of people living in rural areas. Regressions, (4), (5), and (6) consider only the places with below-median percentage of people living in rural areas. EV*Year=1998 shows a DID comparing 1998 to 1994. EV*Year=2002 shows a DID comparing 2002 to 1998. *** p<0,01, ** p<0,05, * p<0,1.

Panel B - Tracing clientelism through poor areas

VARIABLES	(1) Clientelism	(2) Turnout	(3) Electorate	(4) Clientelism	(5) Turnout	(6) Electorate
year **x** *EV* **1998**	-0.796***	-0.09***	0.066***	0.189**	-0.01***	0.047***
	(0.175)	(0.011)	(0.022)	(0.084)	(0.004)	(0.013)
Observations	5,094	5,094	5,094	5,381	5,381	5,366
R-squared	0.802	0.842	0.837	0.744	0.936	0.652
year **x** *EV* **2002**	0.707***	0.04***	-0.085***	0.019	0.014**	-0.002
	(0.134)	(0.008)	(0.022)	(0.071)	(0.006)	(0.010)
Observations	5,506	5,504	5,506	5,559	5,558	5,491
R-squared	0.794	0.831	0.510	0.768	0.804	0.598

Notes: All regressions use municipalities fixed effects. Standard errors, clustered by municipalities, are presented in parenthesis. All regressions are controlled for income and population. Regressions (1), (2) and (3) consider only the places with below-median income. Regressions, (4), (5), and (6) consider only the places with above-median income. EV*Year=1998 shows a DID comparing 1998 to 1994. EV*Year=2002 shows a DID comparing 2002 to 1998. *** p<0,01, ** p<0,05, * p<0,1.

Table 2.6 - (Cont.)

Panel C - Tracing clientelism through Northeast region

VARIABLES	(1) Clientelism	(2) Turnout	(3) Electorate	(4) Clientelism	(5) Turnout	(6) Electorate
year **x** *EV* **1998**	-0.787***	-0.08***	0.060***	0.179**	-0.01**	0.075***
	(0.162)	(0.007)	(0.017)	(0.086)	(0.005)	(0.015)
Observations	3,341	3,341	3,334	7,134	7,134	7,126
R-squared	0.822	0.825	0.873	0.753	0.927	0.578
year **x** *EV* **2002**	0.608***	0.037***	-0.081***	-0.025	0.008	-0.010
	(0.133)	(0.007)	(0.022)	(0.070)	(0.007)	(0.011)
Observations	3,577	3,575	3,565	7,488	7,487	7,432
R-squared	0.805	0.829	0.506	0.768	0.838	0.575

Notes: All regressions use municipalities fixed effects. Standard errors, clustered by municipalities, are presented in parenthesis. All regressions are controlled for income and population. Regressions (1), (2) and (3) consider only the places belonging to the Northeast region. Regressions, (4), (5), and (6) consider all regions, except the Northeast. EV*Year=1998 shows a DID comparing 1998 to 1994. EV*Year=2002 shows a DID comparing 2002 to 1998. *** p<0,01, ** p<0,05, * p<0,1.

Panel D - Tracing clientelism through small areas

VARIABLES	(1) Clientelism	(2) Turnout	(3) Electorate	(4) Clientelism	(5) Turnout	(6) Electorate
year **x** *EV* **1998**	-0.949***	-0.05**	0.134***	-0.058	-0.03***	0.043***
	(0.255)	(0.025)	(0.046)	(0.086)	(0.004)	(0.009)
Observations	4,993	4,993	4,993	5,480	5,480	5,465
R-squared	0.774	0.877	0.835	0.767	0.927	0.652
year **x** *EV* **2002**	0.815***	0.050***	-0.107***	0.011	0.010*	-0.021**
	(0.207)	(0.013)	(0.037)	(0.072)	(0.006)	(0.008)
Observations	5,490	5,487	5,490	5,573	5,573	5,505
R-squared	0.755	0.822	0.530	0.799	0.856	0.529

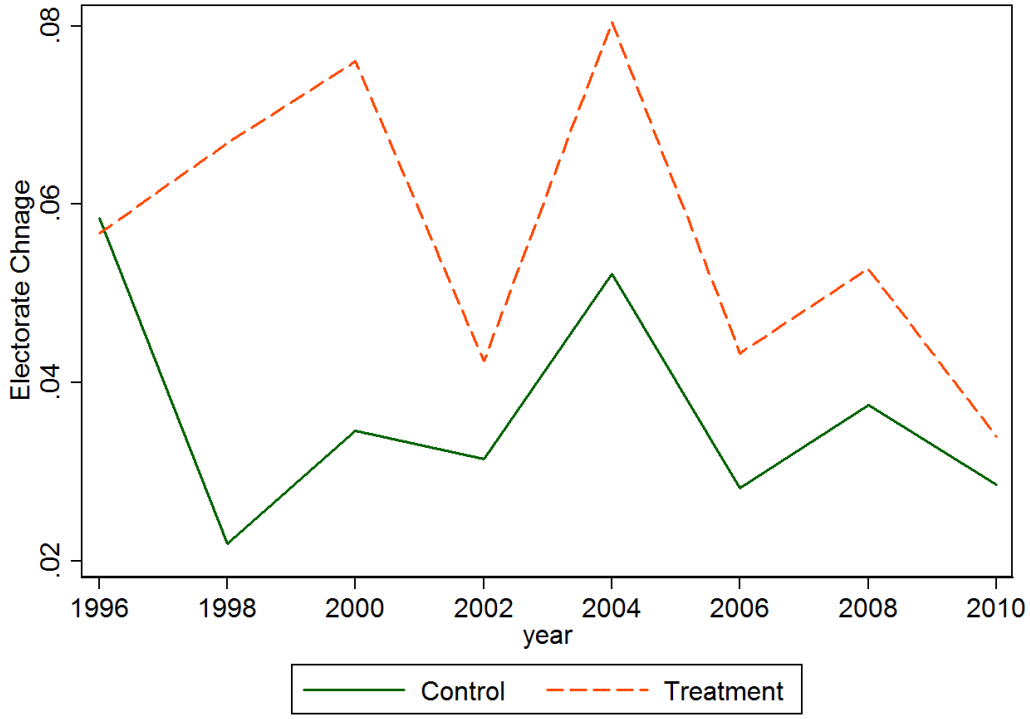
Notes: All regressions use municipalities fixed effects. Standard errors, clustered by municipalities, are presented in parenthesis. All regressions are controlled for income and population. Regressions (1), (2) and (3) consider only the places that have a below-median electorate size. Regressions, (4), (5), and (6) consider only the places that have an above-median electorate size. EV*Year=1998 shows a DID comparing 1998 to 1994. EV*Year=2002 shows a DID comparing 2002 to 1998. *** p<0,01, ** p<0,05, * p<0,1.

Table 2.7 – Triple differences model

Dependent variable: Share of the vote for clientelistic parties	
VARIABLES	(1) Turnout-to-Registered Voters' Ratio
$year$ **x** EV x $Turnout$ - **1998**	2.887**
	(1.299)
Observations	10,475
R-squared	0.778
$year$ **x** EV x $Turnout$ - **2002**	-3.064***
	(0.903)
Observations	11,062
R-squared	0.785

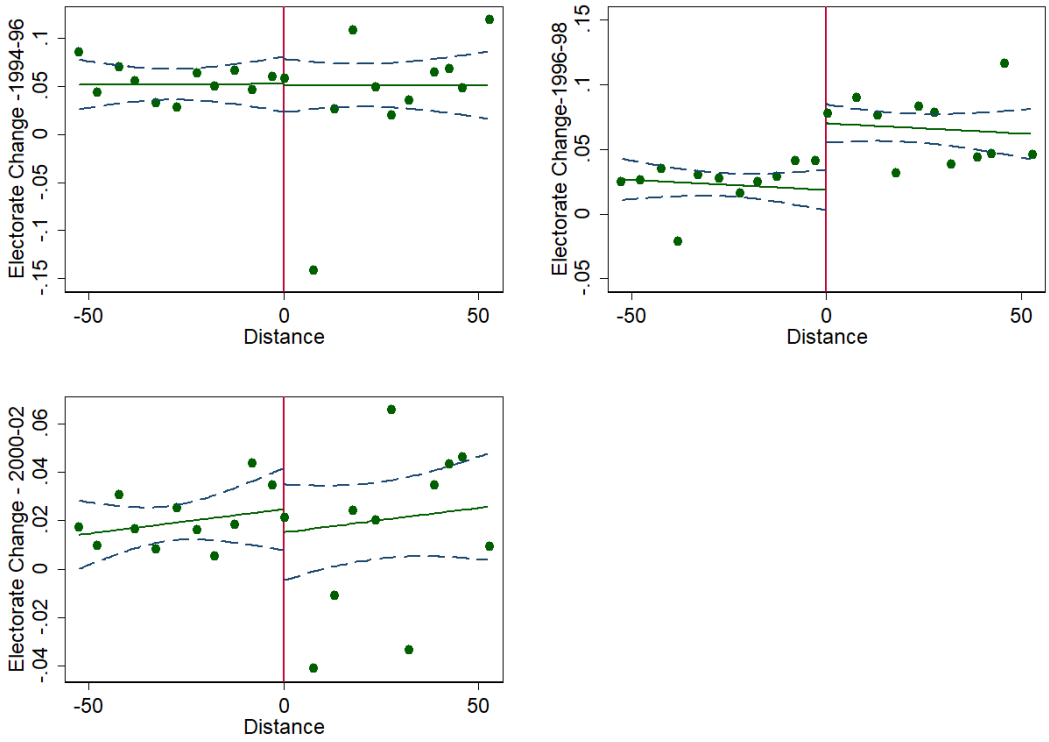
Notes: All regressions use municipalities fixed effects. Standard errors, clustered by municipalities, are presented in parenthesis. All regressions are controlled for population. EV*Year=1998*Turnout shows a triple differences model comparing 1998 to 1994. EV*Year=2002*Turnout shows a triple differences model comparing 2002 to 1998. *** p<0,01, ** p<0,05, * p<0,1.

Figure 2.1 – Percentage change in the number of registered voters across groups



Notes: The dashed and solid lines compose, respectively, the control group and the group receiving the treatment. Each year shows the average percentage change in the number of registered voters between year t-2 and year t (e.g. the year 1998 shows the percentage change in electorate between 1996 and 1998).

Figure 2.2 – RDD showing the effect of electronic voting (EV) on the number of registered voters



Notes: Figure 2 shows three time-varying functions using a 52.529 kilometers' bandwidth (selected by the robust bias-corrected confidence intervals proposed by Calonico, Cattaneo & Titiunik, 2014) and a vertical red line representing the cutoff point. 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 5 kilometers' bins averages. The first and third graphs show, respectively, placebo estimations using the change in the number of registered voters between 1994-1996 and 2000-2002. The second graph shows the change in the number of registered voters between 1996-1998 and captures the "true" EV effect.

Chapter 3: Electronic Voting and Social Spending: The Impact of Enfranchisement on Municipal Public Spending in Brazil[69]

3.1 Introduction

The discussion associating public spending to voters' income has an early tradition. Alexis de Tocqueville in his seminal book "Democracy in America" argued that allowing those who do not own property to vote would increase the proportion of voters who are in favor of income redistribution. Theoretical models also predict that an increase in voting participation of poorer voters increases social spending (Meltzer and Richards, 1981). Many empirical works attempt to confirm this prediction; however, it has remained a challenge to solve the endogeneity problem of an overall increase in voting turnout in response to public spending.[70]

This work uses the electronic voting (EV) gradual introduction in Brazil – which increased voters' probability of casting a valid vote without directly affecting social spending – as an instrument to solve the discussed endogeneity. We show that voting enfranchisement biased toward poorer voters increase social spending. Our main contributions to the literature are: present a theoretical model; estimate the magnitude of an increase in voting turnout on public spending; and expand Fujiwara's (2015) analysis to municipalities and to other social spending outcomes.[71]

Brazil has a turnout-to-electorate level of 80% on average.[72] However, before electronic voting (EV), only 58% of votes for Representatives were valid.[73] Therefore, congressmen could ignore the voice of voters that were not able to cast a vote, which, as argued in this work were mostly

[69] This work is co-authored with Diloá Athias, University of Illinois and Mauricio Bugarin, University of Brasilia.
[70] See, for instance, Meltzer and Richards (1983); Lindert (2004) and Mueller and Stratmann (2003).
[71] Fujiwara (2015) focused his analysis to Brazilian states and measured the impact of poor citizens' enfranchisement on health spending and health outcomes.
[72] In the United States, for instance, this number is close to 60%.
[73] In the 1994 elections, before EV, the valid vote to turnout ratio for Federal Representatives was only 58%. While in 2002, when EV was used in all polling stations in the country this number increased to 92% according to the Supreme Electoral Court (*Tribunal Superior Eleitoral*, TSE).

poor.[74] After EV, the valid vote to turnout ratio increased to 92% and the main purpose of this work is to associate this *de facto* enfranchisement to municipal level public spending, which we find to increase.

First, we build a theoretical model allowing voters to cast, not purposefully, an *invalid* vote, and show that when poorer voters' likelihood of casting a *valid* vote increase, social spending increases as well. Next, we propose a two stage least square regression (2SLS), using EV as instrument, to directly test and confirm our model prediction. Finally, we present a difference-in-differences (DID) model, where municipalities using EV is our treatment group, to expand our sample analysis and bring external validity to our 2SLS estimation.

Our two empirical models find similar estimates on the impact of EV on social spending. To preview our findings, our 2SLS estimation shows that an increase of 1 percentage points in the valid votes to turnout ratio for state representatives increases health spending by 1.65%; public employment by 0.71%; total spending by 1%; total revenue by 0.95% and intergovernmental transfers by 1.5%. Our DID model estimates shows that an increase of 1 percentage points in the valid vote to turnout ratio for state representatives increases health spending by 1.42%; public employment by 1.28%; total spending by 1.26%; total revenue by 1.07% and intergovernmental transfer by 1.11%. Both methodologies present close results, even though the sample we use is different, which brings external validity to our 2SLS estimation and increase the robustness of our results.

[74] The valid vote for executive elections were almost not affected by EV.

3.2 Background

3.2.1 Voters' enfranchisement and public spending

Meltzer and Richards (1981) show that voting enfranchisement biased toward poor voters increases public spending. Using a model of electoral competition, they argue that the median voter is the one imposing her preferences on public spending. Therefore, the poorer the median voter is, the larger will be her public spending optimum provision demanded. This result derives from the assumption that all citizens enjoy public goods the same way, however, poorer citizens will contribute less (lower tax) to finance the public provision.

Many authors attempted to show empirical evidence of Meltzer and Richards' theoretical prediction. Lindert (2004), using decennial data from OECD countries between 1880 and 1930, find a positive relationship between government size and vote participation. In Latin America, Brown and Hunter (1999) find that democracies spend more on social programs than dictatorships. Husted and Kenny (1997) analyze 46 U.S. states between 1950 and 1988 and find that a reduction of 0.2 on the median voter income to the total population income ratio caused an increase of 5 to 12% on public social spending.

Nonetheless, corroborating Meltzer and Richard's (1981) argument is not trivial. As Alesina and Giuliano (2009) argue, empirical studies attempting to achieve this goal may suffer from endogeneity as public spending may cause voting turnout. Additionally, other aspects such as the median voter's perspective on social mobility and strength of lobbying groups could reinforce the limits imposed on government intervention in the economy. Another difficulty to establish causal relationship between the median's voter income and public spending is given by the fact that the median income of the population may not be the same as the median income of those who show

up to vote and cast valid votes. That is, even if democracy allows all eligible voters to cast their votes, those who do not vote may have their preferences completely ignored by politicians.

As Bugarin and Portugal (2015) argue, lower political participation concentrated on the poor makes the median voter income to be larger than the median citizen income reducing the preferences for public goods. A solution pointed by these authors is to use mandatory voting. Jackman (2001) uses the Australia elections to show how mandatory voting increase voting participation (the turnout has increased from 65% to 95% after mandatory voting was imposed in this country). However, mandatory voting by itself cannot guarantee voting participation. As aforementioned, Brazil's case is illustrative. Although the constitution makes it mandatory for all literate citizens between 18 and 70 years old to vote,[75] in 1994 for instance, less than 60% of those who showed up to vote (turnout close to 80%) cast a valid vote for a candidate or party to the legislative seats.

3.2.2 Electronic voting and political participation in Brazil

In Brazil, there are elections every other year as Figure 3.1 shows. For instance, in 1994, the federal elections elected the Federal and State Representatives; senators; governors and president. Two years later, the municipal elections elected the municipal representatives and mayors.[76] EV was first implemented in the 1996 municipal level elections. All municipalities with more than 200,000 eligible voters and the states' capitals used the new technology.[77] In the 1998 federal elections all Brazilian municipalities with more than 40,500 eligible voters[78] used

[75] All Brazilian citizens, age 16 and older have the right to vote. Illiterate citizens are not mandated to vote.
[76] Both municipal and federal elections grant a four years term to the ones elected (except senator that get an 8 year term). In addition, a two years distance separates these two elections.
[77] Only 57 municipalities used EV in 1996.
[78] Four states used EV in all their territories independent of the number of eligible voters (Rio de Janeiro, Amapá, Alagoas and Roraima).

the electronic voting system. Finally, in the municipal elections of 2000 and all the following ones every Brazilian voter could electronically vote.

As showed in the literature (Fujiwara, 2015), EV usage is responsible for an increase of 20 percentage points and 14 percentage points in the valid votes (correctly cast votes) to turnout ratio for federal and state representatives respectively. To cast a vote for Representative before EV, one should clearly write the name or number of the candidate in the ballot. Therefore, it was essential to know how to read to understand the ballot instructions and vote in Brazil.

The EV introduction made it easier for voters to cast a ballot. In the new voting system, voters need to press the candidate's number on a numerical keyboard[79] and after verifying the picture of the candidate, press a green button to confirm their vote.[80] The only way to cast an *invalid* vote, accidentally, is to type a candidate's number with no correspondence and press the green button after seeing the screen warning "this number is wrong". [81] As Hidalgo (2010) points out, the EV was considered a democratic progress since even illiterates could press a number followed by the green button after seeing their preferred candidates' face on the screen.

The main two works on EV in Brazil are Hidalgo (2010) and Fujiwara (2015). Both show that EV increased political participation.[82] Focusing on public health spending at the state level in Brazil, Fujiwara (2015) finds that there is a positive relationship between the percentage of voters using EV within a state and the amount of public spending in health at the state level. Also, he finds that

[79] Similar to a regular phone keypad commonly used in Brazil at the time according to Hidalgo (2010). Note that Brazilians are intensive users of cellular phones; by the end of November 2016 there were 248.4 million active cellular lines in Brazil, which corresponds to 1.2 cellular phone line per citizen in Brazil (http://www.teleco.com.br/ncel.asp).

[80] Fujiwara (2015) shows illustrations of the old ballot comparing it to the electronic one. It is also important to notice that the government had made TV advertisement teaching how to vote in the new system and trained people to help voters if something went wrong during the voting process in the Election Day.

[81] Voters could cast a blank vote by pressing a white button followed by the green one to confirm it.

[82] By increasing the valid votes to turnout ratio for federal and state representatives by close to 22% and 15% respectively.

places with larger share of voters using EV had their health outcomes improved. Recently, Schneider (2017) showed that places where clientelism have stronger ties in Brazil (less developed areas) had a decrease in voting turnout due to EV. We consider this decrease in voting turnout, especially in less developed areas, in our work and our findings suggest that, had EV not affected voting turnout, the increase in local public spending in municipalities using EV would have been even larger.

This article brings new results and intuition on how enfranchisement affects public spending. Differently from Fujiwara (2015), our work relies on municipal level data and shows that not only health, but also public employment, education and the overall municipalities' public expenditures increase due to EV usage.[83] Also, this work shows that municipal revenue, mostly composed by federal and state transfers, in places using EV in 1998 disproportionately increased explaining how these municipalities could spend more on public goods.

3.3 The effect of electronic vote on the electoral outcome: A political economy model

3.3.1 Foundations

Section 3.3 builds a voting model aimed at better understanding the effect of EV on the electoral equilibrium. The model distinguishes to different stages of voters' decision; first, a voter decides whether to vote. Next, if the voter decides to vote, then she will decide to which party to vote for. A voter's decision to vote is one of the most discussed issues both in Political Sciences and in Economics as well. Indeed, considering that there is a cost associated to voting, a rational agent will choose to vote only if she believes it is reasonably likely that her vote will change the electoral outcome. However, actual electoral data show a much higher level of electoral participation, even in countries where voting is not mandatory. For instance, the 2012 US presidential elections

[83] Fujiwara (2015) analyses state level data (27 observations).

showed a record low participation level of 57.4%, which is much higher than social choice theories would predict.

In the present paper we use the concept of "willingness to vote" as a proxy for all the motives for voting described above. In our model each citizen i has a willingness to vote $\nu_i \in V \subset \mathbb{R}_+$. The willingness $\nu_i \geq 0$ represents the utility gain agent i receives when she votes, regardless of the final result of the election. Note that, since the citizen understands that her vote is insignificant, her decision on whether or not to vote depends on the comparison between the cost of voting and her willingness to vote. If the cost is lower than the willingness to vote, the agent will then decide to participate and will vote sincerely, for the party that better represents her preferences.

Hence, our electoral analysis will be divided in two steps. In the first step, each citizen decides whether to vote, based on her cost to vote and on her willingness to vote. In the second step, those who decided to vote cast their ballots.

3.3.2 First step: The decision to vote

Primitives of the model

There is a continuum of agents of mass 1, $W = [0,1]$. Each agent $i \in W$ has a type $\nu_i \in V \subset \mathbb{R}_+$ – her willingness to vote. In particular, if $\nu_i = 0$, then agent i sees no value in voting. The willingness to vote ν_i is a continuous random variable distributed in a non-negative set V according to the distribution $F(\nu_i)$.

If she decides to vote, citizen i will incur a cost $\kappa_i \in \mathbb{R}_+$. The cost reflects a number of components. Directly, it reflects the displacement costs, the opportunity cost of time, etc. Most importantly, it reflects the cost of gathering the information she needs in order to decide who to vote for, as well as preparing for filling properly the complex voting cell. This is the component

that will matter in the present model as it may change according to the voting technology (discussed previously).

General electoral participation

An agent of type ν_i and cost κ_i will decide to vote if and only if:

$$\nu_i - \kappa_i \geq 0. \qquad (1)$$

Let $E = \{i \in W | \, \nu_i - \kappa_i \geq 0\}$ be the set of voting citizens. Then the cardinality of E, $|E|$, corresponds to the proportion of voting citizens. Note that the higher the expected value of the willingness to vote, the higher the overall electoral participation, ceteris paribus. More importantly for the present study, the lower the voting costs, the higher the proportion of voting citizens, ceteris paribus.

An illustration of the voting costs associated to legal requirements can be found in Brazilian institutions. Before the 1988 Brazilian Constitution voters were required to be literate in order to vote; therefore, an illiterate citizen would have to first learn how to read and write in order to have access to voting. Similarly, before the 1960s several American States required citizens to pass literacy tests in order to vote; that, in practice, reduced the vote of the black citizens for whom these tests were typically difficult (Husted and Kenny, 1997).

These examples suggest that poorer citizens tend to have lower electoral participation. In what follows we include such a friction in the original model.

Different electoral participation by social classes

Suppose now that society is divided in three income classes. The low-income class *P* is formed of poorer citizens with income y^P. The middle-income class *M* congregates the middle class with income y^M and the high-income class R is composed of richer citizens with income y^R, where $y^P < y^M < y^R$. A class $J = P, M, R$ has mas $\alpha^J \in [0,1]$ where $\alpha^P + \alpha^M + \alpha^R = 1$.

Suppose now that there is total orthogonality between income and willingness to vote, so that the willingness to vote is distributed in each class according to the same distribution function $F(\nu_i)$. Furthermore, suppose for simplicity that all citizens sharing the same income class share the same voting cost, i.e., $\kappa_i = \kappa^J$ for every citizen i class $J, J = P, M, R$. Finally, as discussed before, suppose that the cost of voting is higher for the low-income class, i.e., $\kappa^P > \kappa^M, \kappa^R$.

Therefore, $F(\kappa^J)$ corresponds to the percentage of citizens from class $J = P, M, R$ that gives up voting. Hence, $\alpha'^J = [1 - F(\kappa^J)]\alpha^J$ is the percentage of citizens that belong to class J and vote, $\eta^J = F(\kappa^J)\alpha^J$ is the percentage of citizens that belong to class J and do not vote, and $\alpha^J = \alpha'^J + \eta^J$.

The effect of the electronic vote on each class' electoral participation

Our model allows us to investigate the effect of EV on each income class. Suppose that class *P*, besides being the poorer class, is also the class with lowest literacy levels, so that, it is also the class with highest voting costs with the older voting technology, because it requires memorizing and writing down the candidates' names, as discussed earlier. Then, the percentage of electoral participation will be lower in class P $\left(\kappa^P > \kappa^M, \kappa^R \rightarrow 1 - F(\kappa^P) < 1 - F(\kappa^M), 1 - F(\kappa^R)\right)$.

What would be the effect of implementing EV? We expect that the EV will create the highest changes precisely in class *P* that has the highest rate of illiteracy. In that class, the easier voting technology will reduce voting costs, from κ^P to $\tilde{\kappa}^P < \kappa^P$. As for the other classes, including citizens better able to read and write and with higher education levels, the effect of EV will be less significant. Hence, for simplicity we assume that EV does not affect the voting costs for the other two classes. Therefore, EV will allow higher participation rates for the poor class without significantly changing the participation rates in the remaining classes.

3.3.3 Second step: Electoral equilibrium with heterogeneous participation

The basic ideas of the model

The electoral competition model presented here follows Bugarin and Portugal (2015). Two parties simultaneously announce political platforms. A platform consists of a provision of a public good that will be produced if the party wins the election. Production of the public good is totally funded by taxes to be collected from every citizen according to a single tax rate. Since society is composed of three income classes, all citizens from the same class will have the same preferences for public good provision. Furthermore, since all citizens benefit the same way from public good consumption but the poorer ones pay fewer taxes for its production, typically the poorer classes prefer more public goods than the rich ones.

A percentage of citizens in each class does not vote. Those who vote will vote sincerely, for the party that better represents his preferences. Citizens' preferences take into consideration parties' platforms but are also influenced by unpredicted stochastic factors that are orthogonal to the announced platforms. Examples of such factors are sexual scandals or a terrorist attack, among others.

Elections are held in one national electoral district in which each voter has one vote. After the elections, each party is assigned a quantity of seats in the Legislature that corresponds to the percentage of votes it received. After the new Legislature is formed, the party that has a majority of seats (we assume an odd number of seats) implements its campaign platform: taxes are collected and the public good is provided.

The electoral completion game with heterogeneous participation

Society is composed of three income classes, as previously described. Two parties P=A, B announce simultaneously a per capita level of provision of a public good, $g_J, J = A, B$, to be

implemented by the winning party. Public good production is financed by an income tax collected according to the tax rate τ, common to all agents. All tax-collected resources are used for the public good's provision. Then the government budget constraint is given by the equation below, where $\sum_J(\eta^J + \alpha'^J)\, y^J = \sum_J \alpha^J\, y^J = y$ represents the average income of all citizens.

$$\tau \sum_J(\eta^J + \alpha'^J)\, y^J = \tau y = g. \qquad (2)$$

A voter's utility has two components: a pragmatic component and an ideological one. The pragmatic or *economic* part of the utility represents the voter's decisions as a *homo œconomicus* and depends on the consumption of a private good, as well as the consumption of the public good. Thus, if a citizen of class J has private consumption c^J and public good consumption g, its utility is $c^J + H(g)$ where H is a twice differentiable, strictly increasing, and strictly concave function. In the present model public good provision and the corresponding income tax rate are the result of the electoral process; therefore, the *homo œconomicus* will choose the highest possible private consumption, i.e., $c^J = (1-\tau)y^J$, and the resulting pragmatic component of his utility is:

$$(1-\tau)y^J + H(g). \qquad (3)$$

Hence, we can write that agent's pragmatic utility as $W^J(g) = (y-g)\frac{y^J}{y} + H(g)$. Therefore, her preferred public policy is:

$$g^{*J} = (H')^{-1}\left(\frac{y^J}{y}\right),\ J = P, M, R. \qquad (4)$$

Note that $g^{*P} > g^{*M} > g^{*R}$, i.e., the poorer a citizen is, the more favorable she is to public expenditure, as discussed before. This result is well known in the literature and has been carefully formalized in Meltzer and Richard (1981). It explains the increase in the size of governments

throughout the 19th and 20th centuries as a consequence of the expansion of suffrage in the consolidating western democracies.

The ideological component of a voter's utility function reflects her concerns as a *homo politicus* and depends on two random variables corresponding to the voter's bias towards party *B*, or equivalently, party *B*'s popularity at the time the election is held.[84] The first random variable is common to all voters and relates to the realization of a state of nature that affects the entire population. A war, an abrupt change in international oil prices and a countrywide energy crisis are examples of such phenomena. A clear example is the popularity of the U.S. president after the terrorist attack on September 11th, 2001, which increased from 57% in February to 90% in September.[85] We model that process with a random variable δ uniformly distributed on $\left[-\frac{1}{2\psi}, \frac{1}{2\psi}\right]$. The parameter $\psi > 0$ measures the level of society's sensibility to these shocks: the lower the value of ψ, the more those shocks may affect society. To illustrate, price changes in oil may strongly affect the political equilibrium in a country that depends strongly on that product, such as Venezuela, and have much less effect in countries that produce near their internal demand levels, such as Brazil.

The second random variable is particular to each voter *i* in group *J* and reflects her personal bias towards party B. It relates to information about relevant politicians on issues that are not consensual in society, such as information that a candidate used drugs in his youth; some voters may believe that this fact makes the candidate unsuitable to a political leadership career, others may find no relation whatsoever with political career, others may even sympathize with the

[84] Analogous results would obtain if we had set the bias with respect to party *A* due to the symmetry of the bias.
[85] See "Poll Analyses", Section "Gallup Poll News Service", The Gallup Organization, http:/www.gallup.com, 09/24/2001.

candidate. We model that bias as a random variable σ^{iJ} uniformly distributed on $\left[-\frac{1}{2\phi}, \frac{1}{2\phi}\right]$. Hence, the greater the parameter ϕ, the more homogeneous class J is.

Therefore, if party *B* wins a majority of seats in the Legislature with platform g^B, voter *i* in the social class *J* derives utility:

$$W^J(g^B) + \sigma^{iJ} + \delta. \qquad (5)$$

Note that it may be the case that the realization of δ is positive, whereas the realized value of σ^{iJ} is negative. Suppose, for example, that the GDP of a country increases above expectations, which brings about overall support for the incumbent president's party, but the media releases the news of a sexual scandal in the presidential office, which may affect different voters in different ways.

The solution to the electoral competition game

We solve the game by backwards induction. Suppose party *P* announces policy g^P, *P* = *A*, *B*. Then, voter i in class J prefers party *A* to party *B* if and only if:

$$W^J(g^A) > W^J(g^B) + \sigma^{iJ} + \delta. \quad (6)$$

Then, the voter that is exactly indifferent between the two parties in class J corresponds to the realization σ^J of the random variable σ^{iJ} given by the following equation $\sigma^J = W^J(g^A) - W^J(g^B) - \delta$.

Since citizens vote sincerely, the number of votes party *A* receives is:

$$\pi^A = \sum_J \alpha'^J \cdot \text{Prob}\left[\sigma^{iJ} \leq \sigma^J\right] = \sum_J \alpha'^J \left[\sigma^J + \frac{1}{2\phi}\right]\phi = \sum_J \alpha'^J \sigma^J \phi + \frac{\alpha'}{2}. \quad (7)$$

Define $W'(g^A) = \sum_J \alpha'^J W^J(g^A)$ and $W'(g^B) = \sum_J \alpha'^J W^J(g^B)$. Then the probability of victory of party *A* is:

$$p^A = \text{Prob}\left[\pi^A \geq \frac{\alpha'}{2}\right] = \text{Prob}\left[\delta \leq \frac{1}{\alpha'}[W'(g^A) - W'(g^B)]\right]. \quad (8)$$

The above expression can be rewritten as:

$$p^A = \frac{1}{2} + \frac{\psi}{\alpha'}[W'(g^A) - W'(g^B)]. \quad (9)$$

By symmetry, the probability of victory of party *B* is:

$$p^B = \frac{1}{2} - \frac{\psi}{\alpha'}[W'(g^A) - W'(g^B)]. \quad (10)$$

Parties choose their announced platforms in order to maximize their probability of winning the election given by (9) and (10). Therefore, party *A* solves the following problem:

$$\max_{g^A} p^A(g^A, g^B) = \frac{1}{2} + \frac{\psi}{\alpha'}[W'(g^A) - W'(g^B)] \quad (11)$$

$$\text{Subject to: } 0 \leq g^A \leq y.$$

Moreover, party *B* solves a completely similar problem. The solution to this platform announcement simultaneous game yields the same dominant strategy to both parties, given below, where $y' = \frac{\sum_J \alpha'^J y^J}{\sum_J \alpha'^J} = \frac{\sum_J \alpha'^J y^J}{\alpha'}$.

$$g^A = g^B = g^E = (H')^{-1}\left(\frac{y'}{y}\right). \quad (12)$$

Note that income $y' = \frac{\sum_J \alpha'^J y^J}{\sum_J \alpha'^J} = \frac{\sum_J \alpha'^J y^J}{\alpha'}$ is a convex combination of each income class' income, in which the weights are the percentage of citizens in each class that really vote. Therefore, the

higher the political participation in one class, the higher the weight parties give to that class' income and, thereby, the closer the equilibrium policy will be to that class' preferred policy.

For the sake of illustration, suppose that $\alpha'^P = \alpha'^M = 0$ and $\alpha'^R > 0$, i.e., only the rich citizens vote. Then, $\alpha' = \alpha'^R$, $y' = y^R$ and $g^E = (H')^{-1}\left(\frac{y^R}{y}\right) = g^{*R}$, so that the platform announced by each party is precisely the one preferred by the rich citizens. This explains again why there was so little redistribution in the past when voting rights were restricted to land owners.

3.3.4 The effect of electronic voting on the electoral equilibrium

Consider first the electoral equilibrium prior to EV. Recall that $\alpha'^J = [1 - F(\kappa^J)]\alpha^J$, $J = P, M, R$ and $\kappa^P > \kappa^M, \kappa^R$. Then we can write (with the subscript *b* for "before") as:

$$y'_b = \frac{\sum_J \alpha'^J y^J}{\alpha'} = \frac{\sum_J [1-F(\kappa^J)]\alpha^J y^J}{\alpha'} > \sum_J \alpha^J y^J = y. \quad (13)$$

Since $\alpha'^P < \alpha'^M, \alpha'^R$, then it follows that $g_a^E = (H')^{-1}\left(\frac{y'_a}{y}\right) < (H')^{-1}(1)$, i.e., public goods provision before EV is below what it would be if all citizens were voting. This is a direct consequence of the fact that precisely the poor citizens, who prefer more public goods provision, are the ones to present the lowest electoral participation.

Consider now the situation posterior to the introduction of EV. According to our model's assumption, κ^M and κ^R remain unchanged, whereas the cost parameter κ^P decreases to $\tilde{\kappa}^P < \kappa^P$. Then, using the subscript *a* for "after", we can write:

$$y'_a = \frac{[1-F(\tilde{\kappa}^P)]\alpha^P y^P + [1-F(\kappa^M)]\alpha^M y^M + [1-F(\kappa^R)]\alpha^R y^R}{[1-F(\tilde{\kappa}^P)]\alpha^P + [1-F(\kappa^M)]\alpha^M + [1-F(\kappa^R)]\alpha^R} <$$

$$\frac{[1-F(\kappa^P)]\alpha^P y^P + [1-F(\kappa^M)]\alpha^M y^M + [1-F(\kappa^R)]\alpha^R y^R}{[1-F(\kappa^P)]\alpha^P + [1-F(\kappa^M)]\alpha^M + [1-F(\kappa^R)]\alpha^R} = y'_b. \quad (14)$$

But then: $g_a^E = (H')^{-1}\left(\frac{y'_a}{y}\right) > (H')^{-1}\left(\frac{y'_b}{y}\right) = g_b^E$.

In other words, the new voting technology brings about a reduction in the cost of voting to the poor, which increases their participation and, thereby, increases the weight of their preferences in parties' calculations, thereby increasing the equilibrium provision of public goods.

This is the main conclusion of the present theoretic model. The main theoretic insight is that increasing *de jure* access to voting, by legally extending the suffrage to poorer citizens, is not enough to ensure that the political parties will take these citizens' preferences into account. It is necessary that, in addition to having the right to vote, these citizens really exert that right. Only in the case where poorer citizens do participate strongly in the political arena by voting, will public policy reflect their preferences.

The main point of the present work is that, due to the high cost of voting to poorer, illiterate citizens in Brazil, their preferences were not fully considered until EV technology strongly increased their participation, changing the electoral equilibrium.

The empirical implication of the model and its testable hypotheses are straightforward: if the model does rightfully reflect the real situation, then, we should have observed a significant increase in the provision of public goods in Brazil after the implementation of EV. More specifically, since poorer citizens care more about social policy (health, education, cash transfers, etc.) we should have observed a clear increase in public spending in these areas.

The following sections test these hypotheses confirming that there was indeed a robust increase in social expenditure in Brazil after the advent of EV and that this increase is particularly strong in municipalities with higher numbers of illiterate citizens.

3.4 Data Collection

We use publicly available data on local government spending focusing social expenses related to health and education services, and public employment.[86] We also look at total budget, total expenses and receipt of intergovernmental transfers. Total budget and expenses shows the overall increase in social spending in response to enfranchisement. Intergovernmental transfers are selected because Federal and State Representatives have discretionary power over these transfers and use them to improve their electoral success (see Ferreira and Bugarin, 2007 and Brollo and Nannicini, 2012 for a discussion on politically motivated transfers in Brazil). In addition, as discussed in Novaes (2017), mayors act as brokers for Representatives campaigning for them in exchange for financial support. Therefore, Representatives would be interested in transferring money to the municipalities with more valid votes to turnout ratio (positively related to EV usage), since the mayor will be able to deliver a larger number of votes in exchange for these transfers.

EV was first implemented in a federal election, however, we examine data at the municipal level. Therefore, we use the previous literature (Brollo and Nannicini 2012; Novaes 2017) to support our assumption that municipal spending is influenced by federal elections' outcomes. To capture the response of municipal spending to federal elections, we use the average spending in the two years following the federal elections, which are also the two years preceding the municipal ones. For instance, the 1998 federal elections' impact is measured by the average of the municipal public spending between 1999 and 2000. Places that used EV in 1998, and that therefore have extra political participation biased toward the poor, are expected to spend more on public goods provision between 1999 and 2000.

[86] The Brazilian National Treasury publishes detailed annual municipal expenditures. All variables on spending are in per capita values and have been deflated using the IGPM index (1994 is the base year).

3.5 Two Stage Least Square Regression

3.5.1 Estimation Strategy

The natural regression to test the theoretical model presented in Section 3, would be the following one:

$$ln\, Y_m = \alpha + \beta_1 V_m + \beta_2 X_m + \epsilon_m \qquad (15)$$

where $ln\, Y_m$ is the logarithm of the average social spending between 1999 and 2000 in municipality m, V_m is the valid votes to turnout ratio for State Representatives in 1998, X_m contains the control variables and ϵ_m is the error term.

Two problems may arise with this model. First, the social spending between 1999 and 2000 may be correlated to past social spending which in turn increased V_m. For instance, suppose the spending in education between 1999 and 2000 is correlated to the spending in education in the past 10 years. If this is true, then previous spending on education would benefit the poor by giving them access to schooling and help them to be enfranchised as they could cast a vote. The estimated return to enfranchisement would then be biased due to this reverse causality – bringing an overestimated β_1. Secondly, omitted variables such as the measurement of the median voter income may also bias the results. Valid votes by itself may not show poor voter enfranchisement. It could be the case that the municipality has a large valid vote to turnout ratio because most citizens are rich and can therefore cast a vote. This could underestimate our results since large number of valid votes would show smaller preferences for redistribution.

To solve these problems, we estimate the following 2SLS model:

$$V_m = \mu + \pi_1 D_m + \pi_2 X_m + u_m \qquad (16)$$

$$ln\, Y_m = \delta + \lambda V_m + \Lambda X_m + \varepsilon_m \qquad (17)$$

where D_m is a dummy variable indicating if municipality m used EV. The difference between equations (15) and (17) is that λ measures the impact of the estimated valid votes to turnout ratio captured by equation (16). Therefore, the instrumented valid votes to turnout ratio in equation (17) impacts social spending only through the enfranchisement brought by EV that is biased toward poor voters. Since the number of eligible voters is related to EV usage, there are no controls for number of voters. To compensate for this fact, the regressions are restrained to a small interval close to the cutoff for EV usage (40,500 voters) so municipalities are comparable.[87] Results are presented next.

3.5.2 Results

Table 3.1 shows the estimations for a closed interval of municipalities containing between 35,500 and 45,500 voters.[88] An increase of 1 percentage points in the valid votes to turnout ratio increases health spending by 1.65%; public employment by 0.71%; total spending by 1%; total revenue by 0.95% and intergovernmental transfer by 1.5%. Although the municipalities are likely to be similar, one can still argue that the results are driven by the lack of control for population. However, we provide in the appendix (Table B1) a robustness check that shows how replicating our 2SLS analysis, but using the municipal social spending variables after the 1994 (no EV) and 2002 (only EV) elections as dependent variables shows no significant results.

The 2SLS estimates presented above empirically confirms the prediction of the model presented in section 3.3. However, it has some limitations. First, our model contains a small sample. Second, although the difference between the number of eligible voters is small across municipalities close

[87] Note that municipalities belonging to the four states mentioned earlier (Rio de Janeiro, Amapá, Roraima and Alagoas), used EV even if they had less 40,500 voters.
[88] Increasing the interval to a bandwidth of 15,000 voters increases the significances of the results.

to the cutoff, regressions do not control for it due to the high correlation between the number of eligible voters and the instrumental variable (correlation close to .70 for the 5,000-bandwidth considered). To overcome these concerns, we propose a difference-in-differences methodology that we present in the next section.

3.6 Difference-in-Differences

3.6.1 Estimation Strategy

An alternative way to test our hypothesis is to use the difference-in-differences (DID) methodology. As mentioned before, this method compares municipalities that used EV (treatment group) to the ones that did not (control group). It then presents the differences in public spending between two periods, before and after EV usage, within these two groups as the following regression shows:

$$ln(y_{it}) = \beta_0 X_{it} + \beta_1 (Year_t * EV_i) + \beta_3 \text{Year} + \beta_4 EV_i + \epsilon_{it}, \qquad (18)$$

where $Year_t$ is a dummy variable equal to 1 when $t = 1998$ and EV_i is a dummy variable equal to 1 for municipalities that used EV that year. For this estimation, we restrict the EV usage to municipalities with less than 40,500 eligible voters to avoid heterogeneity.[89] The vector X_{it} includes all control variables that vary across time and municipalities (such as average income and number of eligible voters). The dependent variable y_{it} corresponds to the *per capita* social public spending (such as health and education) and ϵ_{it} represents the error term. The coefficient β_1 is the parameter of interest that captures the effect of EV on municipal public spending.

[89] If we were to consider all municipalities that used EV in 1998, our treatment group would have municipalities where the number of eligible voters would vary from 947 to 7,131,342. On the other hand, the control group would at most have 40,499 eligible voters.

Our estimations rely on the following assumptions. First, as EV was implemented in the 1998 federal elections, we assume that the local administrations observe and react to the change in political participation in these elections. Second, we assume that non-observed variables that vary across time are orthogonal to the variable of interest.[90] Third, the selection of the four states which used EV in all their territories was not driven by political interests. As Fujiwara (2015) argue, there are no political motivation behind the EV usage selection.[91] Fourth, the control and treatment group do not present significant differences due to the EV usage on variables that are not likely to be affected by it. Table 3.2 presents a balance check to support the argument.

Table 3.2 shows that EV impacted valid votes and voting turnout. EV increased valid votes to turnout ratio for federal and state representatives by 22 and 14 percentage points respectively. This result is close to the ones reported in the literature (Fujiwara 2015, Hidalgo 2010). EV did also decrease the turnout to electorate ratio by 6.8 percentage points.[92] Before testing whether EV usage affects public spending, Figure 3.2 motivates the DID methodology we use.

Figure 3.2 shows that the average social spending (all variables considered) between 1999 and 2000 (year of the municipal election) disproportionately increased on municipalities using EV in 1998. It is noteworthy that the social spending before 1998 (for most variables with exception of health and public employment) was disproportionately larger on municipalities that did not use EV making stronger the argument that EV changed the municipal social spending. In 2004, there

[90] See Angrist and Krueger (1999) for a complete discussion on the DID methodology.
[91] Fujiwara (2015) explain the four states selection as follows: "Two remote states largely covered by the Amazon forest (Amapá and Roraima) were chosen to check the electoral authority's ability to distribute EV in isolated areas, while the states of Rio de Janeiro and Alagoas had areas where the army provided security to election officials, allowing an opportunity to check the logistics of distributing the electronic devices jointly with the military" (p.431).
[92] This should be a concern to our measurement because the turnout was reduced especially in places where clientelism is stronger (Schneider 2017), which are poorer municipalities. Therefore, our estimations could be underestimated as the impact of EV on social spending would be even larger had those poorer voters participated in the election. This will be further discussed in this section.

is a clear pattern in public spending that is similar for both groups, which can be explained by the leveling on political participation in all municipalities brought by the general usage of EV since 2000.

3.6.2 Results

The DID estimation results are presented in Table 3.3. Columns (1), (2) and (3) show respectively, the results obtained for social spending on health, education and public employment. Columns (4) and (5) consider, respectively, municipal total spending and budget. Finally, Column (6) shows the intergovernmental transfers, both national and subnational (state), received by municipalities. EV usage increases total spending on health, education and public employment by 21, 14.5 and 18.5 percent respectively.[93] EV caused total spending, revenue and intergovernmental transfers to increase by 17.7, 15 and 15.5 percent respectively.

The intergovernmental transfers variable helps to explain how municipalities can get more revenue to spend on social expenditures. As Brollo and Nannicini (2012) argue, these transfers are extremely relevant since it accounts, on average, for 65% of the municipal budget. However, parts of these transfers are constitutional automatic transfers such as the Fundo de Participação dos Municípios (FPM), main source of revenue for small municipalities.[94] Therefore, we control for the FPM in our Table 3.3 estimates. Using FPM as control made our estimates larger in magnitudes, showing evidence that not taking this transfer into account downward bias the results.[95]

Table 3.3 results are close to the ones presented on Table 1. To see this, take Table 3.2 estimation that EV increased the valid votes to turnout ratio for State Representatives by 14 percentage points

[93] A 21% increase on health spending, for instance, would be equivalent to an increase of 24.75R$ (or 12$) per capita.
[94] According to IBGE (the Brazilian institute of geography and statistics), municipalities with less than 5,000 citizens, between 1998 and 2000, got on average 57.3% of their revenue from FPM.
[95] Table B2 in the appendix presents a falsification test for this DID analysis.

and connect this value to Table 1 estimated coefficients to find that an increase of 14 percentage points in the valid votes to turnout ratio increases health spending by 23.1%; public employment by 10%; total spending by 13.9%; total revenue by 13.3% and intergovernmental transfer by 20.3%. This exercise shows that both Tables 1 and 3 present a similar estimation of the response of public spending to political participation, even though they use different data. This provides external validity to our 2SLS estimations, increasing the robustness of our results.

Finally, we address the problem of lower turnout caused by EV as presented in Table 3.2. Schneider (2017) argues that EV caused lower turnout specially on places where clientelism is stronger, which are largely composed by poor municipalities. To test if this is also the case for our restricted sample, we measure whether turnout had a larger decrease in places with below median income.

Table 3.4 indicates that EV only affected voting turnout of poorer municipalities. As column 1 shows, there was no change in voting turnout in municipalities with above-median income, while these places had a large increase in enfranchisement (close to 20 percentage points increase in valid votes to turnout ratio). Places with below median income had a large decrease in voting turnout caused by EV (close to 12 percentage points), together with an increase in valid votes to turnout ratio (25 percentage points). These findings allow us to isolate the impact of enfranchisement on social spending if we consider only the above median income municipalities, which had no change in turnout. By splitting the sample between above- and below-median income we show, in Table 3.5, that the EV impact on social spending is driven by above-median income (Panel A) municipalities indicating that our previous estimations (on Table 3.3) are underestimated because, as our model predicts, lower political participation biased toward poor places decreases

social spending. Therefore, had the voting turnout in poorer municipalities not decreased due to EV, the increase in social spending would likely be larger.

Thus, section 3.6 confirms the results presented in section 3.5 and reinforces our model prediction that enfranchisement of the poor increases social spending. In addition, we provide evidence that the decrease in turnout caused by EV, if anything, downward bias our estimations. Finally, in the appendix we present two additional robustness checks (Tables B2 and B3) to give more confidence in our results.

3.7 Conclusion

The connection between democracy and representation lies at the foundation of political participation and liberties. If participation is widespread and voters are free to express their choices, then elected policy makers will act in the best interest of the people. Consequently, institutions and rules are often devised to encourage civic involvement in politics and to promote freely contested elections. For instance, democracies promote participation by holding elections on holidays or weekends, permitting absentee and early voting, and creating initiatives such as the prominent Get-Out-The-Vote (GOTV) campaign (see Berinsky, Burns and Traugott 2001, Berinsky, 2005).

Nonetheless, campaigns to increase voting participation as absentee and early voting as well as GOTV, mostly increases the participation of the rich and the impact of enfranchising poor voters is still unknown.[96] This paper shows that voters' enfranchisement in Brazil, concentrated on the poor and illiterates that were no longer required to write on the ballot after the electronic voting (EV) introduction, increased social spending. Our results indicate that public spending on health, education and public employment increased by 21, 14.5 and 18.5% respectively. In addition,

[96] Berinsky, Burns and Traugott 2001, Berinsky, 2005.

municipalities total spending, total revenue and total intergovernmental transfers also disproportionately increased in municipalities using EV by 17.7, 15 and 15.5% respectively. This empirical result corroborates our model prediction suggesting larger public provision in municipalities using EV.

This paper also considered that EV reduced turnout in areas where clientelism is stronger (poorer places). Our investigation shows that lower turnout in poorer municipalities made our findings, if anything, underestimated. Therefore, had EV not changed voting turnout, the impact of EV on social spending would be even larger than the number we find. The main contribution of the present work is, therefore, to show the consequences of *de facto* enfranchisement on public spending and shed a light on the impact of larger turnout in democracies where vote is not mandatory. When electoral participation in a country is low, the level of public spending might not represent the choice of the majority diminishing the strength of the democracy.

3.8 Tables and Figures

Table 3.1 – Enfranchisement of the poor and local government finances, 2SLS estimates[97]

VARIABLES	(1) Health	(2) Education	(3) Public Employment	(4) Total Spending	(5) Total Budget	(6) Intergov. Transfers
V_m	1.657**	0.319	0.714*	0.991**	0.951**	1.449***
	(0.672)	(0.519)	(0.392)	(0.437)	(0.426)	(0.375)
Observations	116	116	115	116	116	116
R-squared	0.557	0.572	0.652	0.612	0.583	0.495

Notes: Robust standard errors clustered at the Brazilian state level are reported in parenthesis. All regressions control for average household monthly income per capita and use state fixed effects. Regression (1), (2), (3), (4), (5), and (6) consider the logarithm of per capita municipal spending on health; education; public employment, total spending, total revenue and total intergovernmental current transfers. All regressions use a bandwidth of 5,000 voters. *** p<0,01, ** p<0,05, * p<0,1.

[97] The 2SLS first stage shows that EV increases valid votes to turnout ratio for state representatives. This relationship is strong (t-statistic = 12.18) and large in magnitude (14.8 percentage points).

Table 3.2 – DID estimation showing that the treatment and control group have not changed across periods

VARIABLES	(1) Valid Votes Fed.	(2) Valid Votes St.	(3) Rural	(4) Income	(5) Voters	(6) HDI	(7) Illiterates	(8) Turnout
EV*Year=1998	0.23***	0.14***	0.026	-14.807	368.82	0.005	-1.718	-0.06**
	(0.037)	(0.029)	(0.03)	(16.832)	(415)	(0.009)	(1.622)	(0.033)
Observations	9,760	9,760	10,222	10,222	9,761	10,222	10,222	9,761
R-squared	0.885	0.888	0.940	0.936	0.987	0.983	0.969	0.900

Notes: All regressions use municipalities fixed effects. Standard errors, clustered by mesoregions, are presented in parenthesis. All regressions are controlled for a dummy identifying EV usage and a dummy identifying the year of EV usage. Columns (1), (2), (3), (4), (5), (6), (7) and (8) consider the dependent variable to be respectively: valid votes to turnout ratio for federal representatives; state representatives; percentage of people in the municipality living on rural areas; average income; number of voters; human development index; percentage of illiterate adults; turnout to electorate ratio. The sample considers municipalities with more than 1245 and less than 40500 voters. *** p<0,01, ** p<0,05, * p<0,1.

Table 3.3 - Estimating the impact of EV usage on public spending

VARIABLES	(1) Health	(2) Education	(3) Public Employment	(4) Total spending	(5) Total revenue	(6) Intergov. transfers
EV*Year=1998	0.209**	0.145***	0.185*	0.177***	0.150**	0.155***
	(0.099)	(0.053)	(0.096)	(0.054)	(0.061)	(0.036)
Observations	8,102	8,124	9,386	9,051	9,053	9,393
R-squared	0.806	0.860	0.895	0.917	0.941	0.951

Notes: All regressions use municipalities fixed effects. Standard errors, clustered by mesoregions, are presented in parenthesis. All regressions are controlled for income, population and FPM transfers. Regression (1), (2), (3), (4), (5), and (6) consider the logarithm of per capita municipal spending on health; education; public employment, total spending, total revenue and total intergovernmental current transfers. The DID regressions comparing social spending between 1995-1996 and 1999-2000. The sample considers municipalities with more than 1245 and less than 40500 voters. *** p<0,01, ** p<0,05, * p<0,1.

Table 3.4 - Estimating the impact of EV usage on turnout and valid votes to turnout ratio

	Turnout		Valid votes to turnout ratio	
VARIABLES	Above-median Income	Below-median Income	Above-median Income	Below-median Income
EV*Year=1998	-0.001	-0.118***	0.204***	0.255***
	(0.026)	(0.018)	(0.024)	(0.053)
Observations	4,982	4,732	4,982	4,731
R-squared	0.932	0.845	0.897	0.878

Notes: All regressions use municipalities fixed effects. Standard errors, clustered by mesoregion, are presented in parenthesis. All regressions are controlled for income and population. Columns (1) and (2) consider the different impact of EV on turnout between above and below-median income. Columns (3) and (4) consider the different impact of EV on valid votes to turnout ratio between above and below-median income. The sample considers municipalities with more than 1245 and less than 40500 voters. *** p<0,01, ** p<0,05, * p<0,1.

Table 3.5 - Estimating the impact of EV usage on public spending (splitting the sample between above and below-median income)

VARIABLES	(1) Health	(2) Education	(3) Public employment	(4) Total spending	(5) Total revenue	(6) Intergovernmental transfers
Panel A: Above-median income						
EV*Year=1998	0.268*	0.176*	0.119	0.272***	0.275***	0.209***
	(0.157)	(0.089)	(0.114)	(0.089)	(0.085)	(0.065)
Observations	4,080	4,090	4,880	4,578	4,579	4,883
R-squared	0.826	0.887	0.882	0.915	0.944	0.947
Panel B: Below-median income						
EV*Year=1998	0.118	0.080	0.112	0.064	0.015	0.066*
	(0.107)	(0.073)	(0.120)	(0.058)	(0.057)	(0.039)
Observations	4,022	4,034	4,506	4,473	4,474	4,510
R-squared	0.765	0.823	0.882	0.903	0.939	0.961

Notes: All regressions use municipalities fixed effects. Standard errors, clustered by mesoregions, are presented in parenthesis. All regressions are controlled for income, population and FPM transfers. Regression (1), (2), (3), (4), (5), and (6) consider the logarithm of per capita municipal spending on health; education; public employment, total spending, total revenue and total intergovernmental current transfers. The sample considers municipalities with more than 1245 and less than 40500 voters. *** p<0,01, ** p<0,05, * p<0,1.

Figure 3.1 - Brazilian elections' timeline

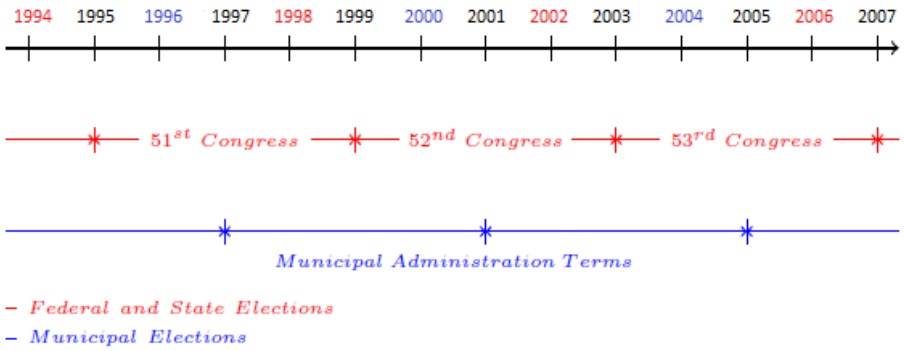
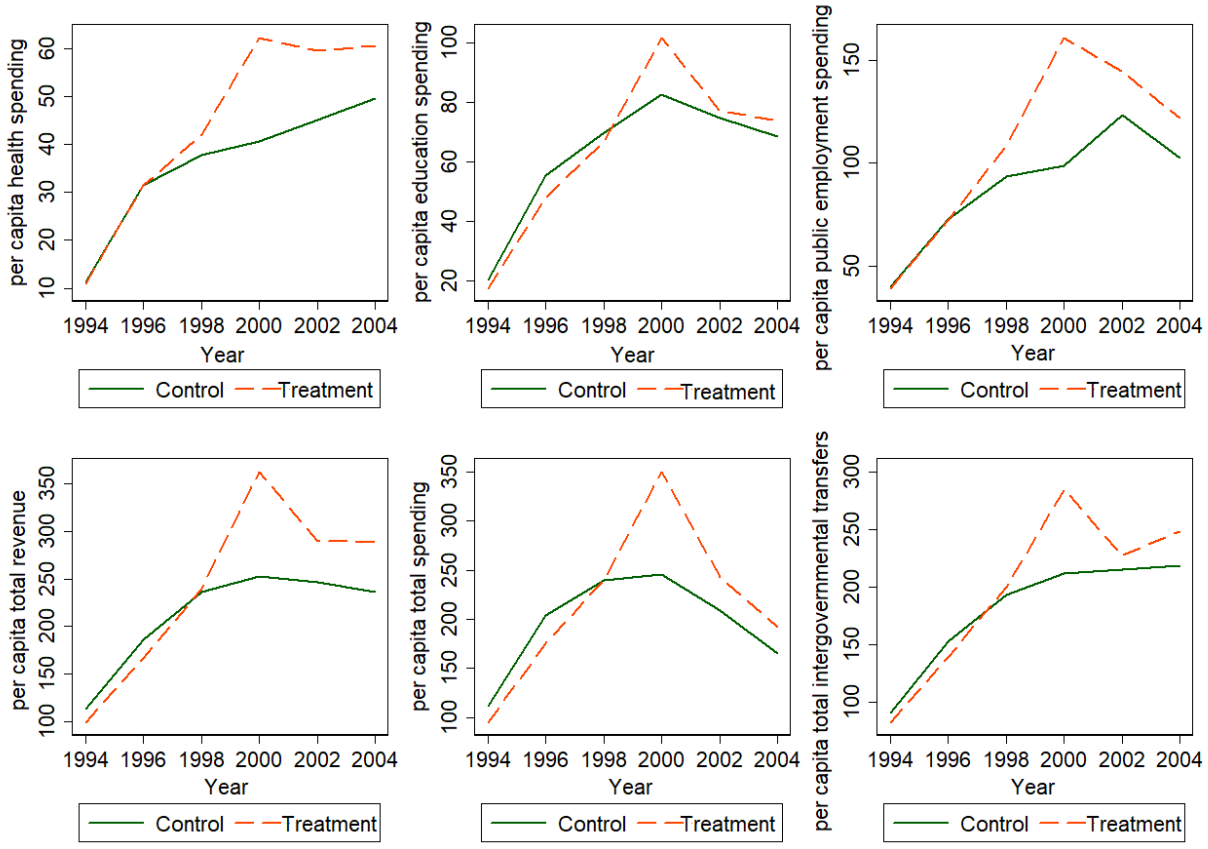


Figure 3.2 – Per capita public social spending between 1993 and 2004



Notes: All graphs show the public spending average of municipalities that used EV in 1998 (treatment) and the ones that did not (control). All data points represent a two-year average of total spending (e.g. the year 2000 contains the average spending between 1999 and 2000) and all values are deflated. The smallest 10% municipalities were dropped to attenuate the per capita spending on the smallest municipalities. The analysis is restricted to municipalities with less than 40,500 voters. 4577 municipalities (82% of the total Brazilian municipalities) are covered in this representation.

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Appendix A: Robustness Checks to Chapter 1

Model specifications and restricting the bandwidth

I first show that the concealed carry prohibition effects on gun-related homicides are not sensitive to varying the model, presented on Table 1.2, specifications. Table A.1 shows that adding fixed effects or Fourier terms do not substantially change the results. When I use a Poisson regression I find that gun-related homicides were reduced by 8.5%.[98] Finally, restricting the sample to municipalities with more than 50,000 and 100,000 people slightly change the concealed carry prohibition impact on gun-related homicides. In the former case, concealed carry prohibition reduces gun-related homicides by 4,073 in 2004 and in the latter, it reduces gun-related homicides by 4,516 in 2004. Both numbers are close to the baseline estimation of 3,900.

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

[98] As the coefficient of interest is a dummy variable, the interpretation of the Poisson estimation is intuitive. The percentage change in gun-related homicides is equal to $e^{\widehat{\beta}}$-1. The 8.5% reduction in gun-related homicides is close to the reduction estimated using the baseline model (10.8%). This result is not sensitive to municipalities' threshold selection. For instance, when I restrict my sample to municipalities with more than 50 and 100 thousand inhabitants, I find respectively that gun-related homicides were reduced by 7.3% and 7.5% (all significant at the 0.01 level).

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 A.2 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.[99] 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.

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 for the whole country, I propose a variable that measures gun related homicides' deviation from the historical average.[100] This

[99] 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).

[100] Formally, this variable is constructed as follows: $Std.Death_i = \frac{(\sum_{m=1}^{12} deaths_{mi}) - Yearly\ Historical\ Average_i}{Standard\ Deviation_i}$, where $deaths_{mi}$ indicates the number of gun related deaths at municipality i, on month m. More specifically, $deaths_{12i}$ represents the number of gun related deaths, at municipality i, on the month in which the referendum

variable is constructed to measure the impact of an increase in gun related homicides, within one year of the referendum, on its outcome. Table A.3 presents the estimated coefficient and shows that one deviation from the mean increases the support for gun prohibition by .62 percentage points Table A.4 shows that this effect vanishes as the gun related homicides' deviation from the historical average happens 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).

happened (12). The Yearly Historical Average and standard deviation takes into account the period between 1996 and 2005. The monthly data on gun related death was collected at DATASUS.

Table A.1 – RDD estimating the ED effect on total homicides and Gun and Non-Gun related homicides

VARIABLES	(1) Total Homicides	(2) Non-gun Related Homicides	(3) Gun Related Homicides
ED – Baseline	-0.227***	-0.036	-0.191***
	(0.070)	(0.046)	(0.053)
ED -fixed effects	-0.162**	-0.011	-0.151***
	(0.071)	(0.047)	(0.053)
ED - sine, cosine	-0.235**	-0.032	-0.202***
	(0.093)	(0.060)	(0.070)
ED - Poisson	-0.046**	0.055	-0.089***
	(0.023)	(0.042)	(0.027)
ED - 50,000	-0.372***	-0.073	-0.298***
	(0.103)	(0.056)	(0.084)
ED - 100,000	-0.439***	-0.031	-0.408***
	(0.127)	(0.067)	(0.105)

Robust standard errors clustered at the municipality level are in parenthesis. Bandwidth is equal to 12 months. All regression control for calendar months, rain and temperatures. Rows 1 – 3 consider municipalities with more than 10,000 people and contains 71,420 observations. Row 1 uses the baseline estimation presented on Table I. Row 2 adds fixed effects. Row 3 adds sine and cosine functions and their interaction. Row 4 uses the Poisson regression model with municipality fixed effects and uses homicides counts instead of homicides rates as dependent variable. This model drops municipalities that contains all zero outcomes, therefore, the number of observations for columns 1, 2 and 3 are respectively 63,406; 56,558 and 54,131. Row 5 and 6 use the baseline estimation, but restrict the sample to municipalities with respectively more than 50,000 and 100,000 people. Row 5 and 6 contain respectively 13,738 and 6,059 observations. *** p<0.01, ** p<0.05, * p<0.1

Table A.2 – 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 A.3 - OLS regression showing the relationship between voting in favor of gun prohibition (dependent variable) and gun-related homicides' deviation from the historical average

VARIABLES	Vote in favor of the prohibition
gun-related homicides std.	0.622**
	(0.262)
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 women to men ratio, CCT spending per capita, ideology distance to capital, income per capita, number of cattle per rural worker, population, rural population, vulnerability index, drought, land protest, public distribution of agricultural land. *** p<0.01, ** p<0.05, * p<0.1

Table A.4 – OLS regression simulating different dates in which the referendum took place

VARIABLES	(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.	0.62**	0.60**	0.51*	0.438	0.46*	0.310	0.346	0.072	0.089	0.181
	(0.263)	(0.276)	(0.28)	(0.28)	(0.27)	(0.27)	(0.26)	(0.26)	(0.27)	(0.28)

The regressions contain 5,505 observations each and use microregion fixed effects and robust standard errors are adjusted for clusters at the microregion (557 microregions) level. It is additionally controlled for women to men ratio, conditional cash transfer spending per capita, ideology distance to capital, income per capita, number of cattle per rural worker, population, rural population, vulnerability index, drought, land protest, public distribution of agricultural land. *** p<0.01, ** p<0.05, * p<0.1

Appendix B: Robustness Checks to Chapter 3

Falsification tests for the 2SLS analysis

The robustness check for the 2SLS analysis comes from a falsification test that does a series of similar regressions as the ones presented in section 3.5, but using the municipal social spending variables after the 1994 and 2002 elections as dependent variables. As presented in Table B.1 (in Panels A and B), these two sets of regressions show no significant effect on social spending due to the EV usage, except for public employment and intergovernmental transfers in 2002. This is expected given that there were no differences on voting systems adopted between the municipalities in the considered years (either no one used EV or every municipality used it).

Were municipal representatives affected by EV?

Next, we show that the municipal representatives were also affected by EV. This is pertinent because it brings support to another mechanism on how the mayors increased social spending. Municipal representatives, interested on poor voters that are now enfranchised, would support the mayor's decision of increasing social spending. Since there is no study showing that the vote for municipal representatives were also impacted by the EV usage, this work used a regression discontinuity design (RDD), as in the previous literature (Fujiwara 2015), to show that the valid vote to turnout ratio for municipal representatives also increased due to EV.

The sample selected considers the 1996 elections where State Capitals and municipalities that had more than 200,000 voters were able to use EV. As most states, 17 out of 26, used EV only in one municipality and there were State Capitals with less than 200,000 voters (e.g. Palmas-TO with only 42,313 voters), this work selected the São Paulo state to do the RDD analysis. Almost 23% (13 out of 57) of the municipalities that used EV in 1996 belonged to São Paulo state and there

were enough number of municipalities close to the cutoff to be considered[101] (not the case for the remaining states). Table B.2. shows that there was an increase close to 10 percentage points in the number of valid votes to turnout ratio for municipal representatives due to the EV usage. Therefore, municipal representatives would also be inclined to help the mayors to increase social spending as the enfranchisement biased toward poor voters potentially affected their reelection's chances.

Is timing selection driving the results?

Our goal in this subsection is to show that the previous findings are robust to different timing selection. Table B.3 shows that the timing chosen for our estimations does not change the significance or sign of our results. This table presents a DID that compares the average of municipalities social spending between 1999 and 2000 (after federal elections) and compare it to the average on social spending between 1997 and 1998 to guarantee that the mayor is the same between these two periods. As one can notice, although the coefficients present some changes when compared to table 3.3 (had their magnitude decreased), they remain being positive and significant.

[101] The balance check considering number of voters and average income shows that the cutoff by itself does not bring differences between the municipalities that used EV and the remaining ones.

Table B.1 – Falsification tests[102]

VARIABLES	(1) Health	(2) Education	(3) Public Employment	(4) Total Spending	(5) Total Budget	(6) Intergov. Transfers
Panel (A): 1994 elections and municipal spending between 1995 and 1996						
V_m	25.253	18.246	9.377	13.291	14.158	18.879
	(33.427)	(22.508)	(13.019)	(15.820)	(16.503)	(26.403)
Observations	116	117	116	116	116	115
Panel (B): 2002 elections and municipal spending between 1995 and 1996						
V_m	12.314	9.938	14.843*	11.890	10.021	14.093*
	(10.378)	(8.548)	(8.470)	(7.995)	(7.587)	(7.782)
Observations	117	117	117	117	117	117

Notes: Robust standard errors clustered at the Brazilian state level are reported in parenthesis. Regressions for the Placebo 1994 control for average household monthly income per capita (for the year of 1991) and regressions for the Placebo 2002 control for the 2002 GDP per capita. All regressions use state fixed effects. Regression (1), (2), (3), (4), (5), and (6) consider the logarithm of per capita municipal spending on health; education; public employment, total spending, total revenue and total intergovernmental current transfers. Wald Chi-Square test for all Placebo 1994 regressions do not allow one to reject the hypothesis that at least one of the predictors' regression coefficient is not equal to zero. All regressions use a bandwidth of 5,000 voters. *** p<0,01, ** p<0,05, * p<0,1.

[102] The 2SLS first stage in Panel A shows no EV effects on valid votes to turnout ratio for state representatives. This should be expected as there was only paper ballot in Brazil at the time. In Panel B, the 2SLS first stage shows that EV increases valid votes to turnout ratio for state representatives. However, the magnitude of this relationship is insignificant (0.8 percentage points).

Table B.2 - Estimating the impact of EV usage on valid votes to turnout ratio for municipal representatives in 1996

	Valid Votes to turnout ratio	Valid Votes to turnout ratio	Valid Votes to turnout ratio
	(1)	(2)	(3)
VARIABLES			
EV	0.090***	0.101***	0.095***
Observations	24	22	20
R-squared	0.65	0.78	0.78

Notes: Robust standard errors presented in parenthesis. All regressions are controlled for income, number of voters, number of voters minus the cutoff and an interaction between the former variable and EV usage. Regression (1), (2) and (3) consider respectively municipalities with more than 120,000; 130,000 and 140,000 voters. *** p<0,01, ** p<0,05, * p<0,1.

Table B.3 - Estimating the impact of EV usage on public spending (changing the time framing)

VARIABLES	(1) Health	(2) Education	(3) Public employment	(4) Total spending	(5) Total revenue	(6) Intergovernmental transfers
EV*Year 2000	0.220**	0.111**	0.120**	0.135***	0.118***	0.088***
	(0.093)	(0.046)	(0.048)	(0.024)	(0.026)	(0.028)
Observations	9,829	9,853	9,878	9,884	9,886	9,886
R-squared	0.45	0.60	0.67	0.73	0.75	0.78

Notes: All regressions use state fixed effects. Standard errors, clustered by mesoregion, are presented in parenthesis. All regressions are controlled for income, number of voters, FPM transfers and a dummy identifying EV usage (collinear to the state fixed effects). Regression (1), (2), (3), (4), (5), and (6) consider the logarithm of per capita municipal spending on health; education; public employment, total spending, total revenue and total intergovernmental current transfers. The sample considers municipalities with more than 1245 and less than 40500 voters. *** p<0,01, ** p<0,05, * p<0,1