

UNIVERSIDADE DE SÃO PAULO
FACULDADE DE ECONOMIA, ADMINISTRAÇÃO E CONTABILIDADE
DEPARTAMENTO DE ECONOMIA
PROGRAMA DE PÓS-GRADUAÇÃO EM ECONOMIA

Essays in Development Economics

Ensaio em Economia do Desenvolvimento

Raphael Guinâncio Bruce

Orientador: Prof. Dr. Marcos Yamada Nakaguma

**São Paulo - Brasil
2020**

Prof. Dr. Vahan Agopyan
Reitor da Universidade de São Paulo

Prof. Dr. Fábio Frezatti
Diretor da Faculdade de Economia, Administração e Contabilidade

Prof. Dr. José Carlos de Souza Santos
Chefe do Departamento de Economia

Prof. Dr. Ariaster Baumgratz Chimeli
Coordenador do Programa de Pós-Graduação em Economia

RAPHAEL GUINÂNCIO BRUCE

Tese apresentada ao Departamento de Economia da Faculdade de Economia, Administração e Contabilidade da Universidade de São Paulo como requisito parcial para a obtenção do título de Doutor em Ciências.

Área de concentração: Teoria Econômica

Orientador: Prof. Dr. Marcos Yamada Nakaguma

Versão Original

**São Paulo - Brasil
2020**

Catálogo na Publicação (CIP)
Ficha Catalográfica com dados inseridos pelo autor

Bruce, Raphael Guinâncio.
Essays in Development Economics / Raphael Guinâncio Bruce. - São Paulo, 2020.
102 p.

Tese (Doutorado) - Universidade de São Paulo, 2020.
Orientador: Marcos Yamada Nakaguma.

1. Economia política – Aspectos psicológicos . 2. Morte – Aspectos sociais .
3. Microeconomia. 4. Econometria . I. Universidade de São Paulo.
Faculdade de Economia, Administração e Contabilidade. II. Título.

AGRADECIMENTOS

Aos meus pais, Claudia e Heitor, por tudo que recebi, desde o começo, e por tudo que sou. Contar com o apoio e carinho de vocês a qualquer hora é um privilégio.

À Graziella, minha esposa, que esteve do meu lado nos momentos mais felizes para celebrar, e também nos momentos mais difíceis para me abraçar e ajudar a seguir em frente. Sem seu amor, nada disso valeria a pena.

Aos demais membros da família: Amadyr, Ricardo, Márcia, Suhiana e Suely, pela torcida e pelos corações sempre abertos.

Aos colegas de doutorado Danilo Souza, Tales Rozenfeld, Bruna Pugialli, Michael França, Rafael Tavares, Tiago Ferraz e Ricardo Sabbadini pelos momentos de leveza. É uma honra ter dividido tantas horas com vocês. Para a decepção da banca, a procrastinação conjunta na salinha custou algumas páginas dessa tese, mas me deu a dose de alegria necessária para seguir em frente e terminá-la. Também a Leonardo Rosa, Bárbara Born, Tássia Cruz, Ana Ribeiro e Beatriz Pousada, pelo carinho e pela Califórnia.

Ao meu orientador Marcos Nakaguma, pelo aprendizado a cada conversa e pela paciência e dedicação com que conduziu esse processo, do primeiro ao último dia. Levarei sua generosidade e compromisso para o resto de minha carreira.

A todos profissionais que abriram portas para mim ao longo desses anos: Rudi Rocha, Rafael Costa Lima, Fernanda Estevan, Ricardo Madeira, Fernando Limongi, Filipe Campante e Ariaster Chimelli. Ninguém chega a lugar algum sozinho e, por isso, tenho uma gratidão imensa por me apoiarem em diferentes etapas da formação.

Aos integrantes do Lemann Center em Stanford, pela hospitalidade, estrutura e experiência acadêmica excepcional que proporcionaram.

Finalmente, ao Conselho Nacional de Pesquisa Científica (CNPq), à Coordenação de Aperfeiçoamento de Pessoal de Nível Superior (CAPES) e à Fundação Lemann que me apoiaram financeiramente durante o doutorado.

Como escreveu Douglas Adams, “*so long and thanks for all the fish.*”

**“The part of economics that is independent of history
and social context is not only small but dull.”**

Robert Solow

RESUMO

Essa tese de doutorado contém três artigos distintos, separados em capítulos. No primeiro capítulo, eu procuro responder a seguinte pergunta: indivíduos procuram se informar quando são induzidos a votar? Mais especificamente, o capítulo apresenta evidência empírica do efeito do voto obrigatório sobre o consumo de noticiários televisivos. No Brasil, cidadão alfabetizados maiores que 18 anos e menores que 70 estão sujeitos a penalidades caso não compareçam em eleições. Isso permite identificar o efeito causal da lei de voto obrigatório sobre aquisição de informação. Eu encontro que a lei possui um efeito positivo sobre a probabilidade de um indivíduo assistir o noticiário televisivo de maior audiência no Brasil. Esse efeito é estatisticamente significativo e substancial em magnitude. Ademais, o efeito é mais forte para indivíduos com avaliações neutras do presidente incumbente. Esses resultados são restritor a eleitores jovens próximos da descontinuidade dos 18 anos de idade e são robustos a diferentes especificações da regressão principal.

No segundo capítulo, eu investigo se há persistência em comparecimento eleitoral no contexto do Brasil. Utilizando um painel de dados inédito contendo todos os eleitores brasileiros para 5 eleições diferentes, eu exploro as descontinuidades geradas pela lei de voto obrigatório para verificar se há persistência. Utilizando um desenho de regressão descontínua *fuzzy* onde a exposição ao voto obrigatório é o instrumento exógeno para comparecimento prévio, eu estimo o efeito de comparecer na eleição anterior sobre comparecimento atual. Com essa estratégia consigo identificar que o comparecimento prévio causa uma probabilidade 46% maior de comparecer no período seguinte. Eu mostro, portanto, que há um grande grau de inércia em comparecimento induzido pelo voto obrigatório.

No terceiro e último capítulo, eu investigo se o aumento do número de horas que os alunos passam em uma escola tem um impacto na taxa de pessoas na faixa de idade afetada por esse aumento. Eu mostro que a política de escolas de turno integral implementada no estado de Pernambuco teve um impacto negativo nas taxas de homicídios entre adolescentes em idade escolar para estar no ensino médio, única faixa etária que recebeu escolas de turno integral. Utilizando um desenho de *event-study*, eu exploro a variação espacial e temporal da introdução do programa para mostrar que essa redução foi focada somente nessa faixa etária. Não é encontrado impacto da política na taxa de homicídios para outras faixas etárias próximas à de alunos de ensino médio. Também não há impacto em taxas de mortalidade por outras causas que não sejam homicídio. Esses resultados sugerem que a introdução da política cumpriu um papel significante na redução de mortes por crimes letais nesse estado.

Palavras-chaves: Voto obrigatório, eleições, tempo integral, homicídios

ABSTRACT

This doctoral dissertation is comprised of three separate research pieces divided by chapters. In the first chapter, I answer the following question: Do people acquire more information when they are encouraged to participate in elections? I present empirical evidence on the effects of compulsory voting laws on the consumption of TV news. In Brazil, literate citizens over the age of eighteen are subject to a number of legal penalties if they fail to turn out on election day. This allows me to identify the causal effect of being under a compulsory voting regime on information acquisition. I find that compulsory voting has a significant and substantial positive impact on the probability of an individual to watch Brazil's main TV newscast. Additionally, the effect is stronger for individuals with neutral evaluations of the incumbent president. My regression discontinuity results are restricted to young voters who just turned eighteen and are robust to different polynomials and bandwidth lengths.

In the second chapter, I investigate if there's persistence in turnout in the context of the world's largest democracy under compulsory voting, Brazil. Using individual panel data for all registered voters in Brazil for five different elections I explore the age cutoffs created by the compulsory voting law in order to provide causal estimates of persistence in turnout. Employing a Fuzzy RD approach, I estimate the effect of previous on current turnout, using the exposure to abstention fines as an instrument. Using this strategy, I am able to identify that voting on an election causes an individual to become 46% more likely to vote on the next election. I show, therefore, that there is a high degree of persistence in turn out due to compulsory voting.

In the third and last chapter, I investigate if longer school hours have an impact on the homicide rates of teenagers. I show how the full-day high school policy in the state of Pernambuco, Brazil, had a negative impact on the homicide rates among youth in that state. Using an event-study design, I exploit the temporal and spatial variation in the introduction of the program to show that this reduction was focused on individuals in the school-age range of high school students. I find no impact of the policy on homicides for similar age ranges. I also don't find any impact on mortality rates by other causes. Overall, these results suggest that the introduction of the policy played a significant role in the reduction of lethal crime rates in that state.

Key-words: Compulsory voting, elections, full-day schooling, homicides

TABLE OF CONTENTS

1	Compulsory Voting and TV News Consumption	15
1.1	Introduction	15
1.2	Theoretical framework	19
1.2.1	Benchmark model	19
1.2.2	Compulsory Voting	22
1.3	Institutional background	23
1.3.1	The compulsory voting law	23
1.3.2	Rede Globo and Jornal Nacional	24
1.4	Data and research design	27
1.4.1	Data	27
1.4.2	Identification strategy	29
1.5	Main results	33
1.6	Robustness	35
1.7	Heterogeneous effects	39
1.8	Concluding remarks	43
1.9	Appendix	44
1.9.1	Theory	44
1.9.1.1	The voting decision	44
1.9.1.2	Proofs	46
1.9.2	Graphs and tables	47
2	Compulsory Voting and Persistence in Turnout	57
2.1	Introduction	57
2.2	Institutional background	61
2.3	Data and research design	62
2.3.1	Data	62
2.3.2	Identification strategy	63
2.4	Results	68
2.4.1	Elderly voters	68
2.4.2	Younger voters	71
2.5	Robustness	74
2.5.1	Bandwidth lengths	74
2.5.2	Placebo cutoffs	76
2.6	Conclusion	78
2.7	Appendix	79
3	Full Day Schooling and Homicides: The Case of Pernambuco	85
3.1	Introduction	85

3.2	Institutional background	87
3.3	Data and summary statistics	90
3.4	Research design and results	92
3.5	Conclusion	94
3.6	Appendix	95

REFERENCES	97
-----------------------------	-----------

1 Compulsory Voting and TV News Consumption

1.1 Introduction

On March 18, 2015, U.S. President Barack Obama publicly endorsed compulsory voting as a way to increase the historically low voter turnout on U.S. elections. He stated that “In Australia, and some other countries, there’s mandatory voting. It would be transformative if everybody voted... it would completely change the political map in this country, because the people who tend not to vote are young; they’re lower income; they’re skewed more heavily towards immigrant groups and minority groups”.^{1.1.1} President Obama’s opinions echo Arend Lijphart’s 1996 Presidential Address to the American Political Science Association, in which he defended compulsion as a way to solve the problem of unequal political participation in elections. He argued that penalizing abstention would increase turnout, political knowledge, and political engagement in the electorate (LIJPHART, 1997). Our interest is related to the last two effects. More specifically, we study how inducing turnout through compulsory voting affects information acquisition.

Compulsory voting (henceforth, “CV”) is an essential feature of 22 different countries. One of the main arguments against CV laws is based on the idea that, even if such rule had a positive impact on turnout, it would have a perverse effect on the outcome of the elections (SAUNDERS, 2012; SINGH, 2016). The logic is that, by increasing the share of uninformed individuals turning out to vote, electoral choices would be worse than the ones made under voluntary voting. Shineman (2016) points that this reasoning was often used during the debates in US Congress regarding the National Voter Registration Act. The author notes that members of the Congress and the media raised concern that lowering the cost of voting would lead to a decrease in voter competence. These arguments are implicitly based on the assumption that voters do not change their consumption of information when the costs and benefits of turning out are altered. In this paper, we challenge this assumption.

In order to illustrate how the introduction of abstention fines can change decisions regarding turnout and information acquisition, we present a framework based on Degan (2006). The model depicts voters facing three decisions: (i) whether or not to acquire information; (ii) whether or not to turn out to vote, and; (iii) whom to vote for. The decision weights the benefit of performing one’s civic duty against the subjective cost of making voting mistakes and the objective cost of acquiring information. The solution

^{1.1.1}For the full transcript see: <https://obamawhitehouse.archives.gov/the-press-office/2015/03/18/remarks-president-city-club-cleveland>

to this decision provides two straightforward predictions: introducing abstention fines increases turnout and also induces information acquisition, but only for individuals who do not hold extreme political preferences.

The current electoral rules in Brazil provide an excellent opportunity to test these predictions. The electoral law determines that every literate citizen above eighteen and younger than seventy years old by the day of the elections is subject to a number of penalties in case of failing to attend an official polling station and casting a ballot.^{1.1.2} This age-based rule determines who's under the CV regime and who's not, enabling us to measure the causal effect of the law on the consumption of TV newscasts. Our empirical strategy employs regression discontinuity design (RDD) on a novel data set about the Brazilian population's media-related habits. Using data from the first two waves of the Brazilian Media Survey (*Pesquisa Brasileira de Midia*), a nationally representative survey planned by the Brazilian government, we create an indicator variable that equals one if the respondent states that Rede Globo's *Jornal Nacional*, Brazil's most trusted and watched newscast, is one of the TV shows he or she watches the most, and zero otherwise. The identification is based on comparing this variable for individuals turning eighteen until the month prior to the 2014 election (subject to CV) with those from individuals turning eighteen after the month of the election (not subject to CV). Results show that individuals who are being exposed to the penalties for the first time are on average 11.3 percentage points more likely to watch *Jornal Nacional*. This effect represents a 57% increase relative to when they were not subject to abstention fines. We also find that only voters with neutral evaluations of the incumbent president significantly increase their consumption of TV news when exposed to the abstention penalties. No impact is found, though, for elderly voters who are transitioning into a voluntary voting regime at the age of seventy or those who have more polarized evaluations of the federal government. Our results are also robust to different polynomial orders and bandwidth lengths.

We also find that this effect is restricted for those becoming eighteen just before October 2014. This is particularly relevant in Brazil because there are a number of possible age-related confounding effects that could affect the decision to acquire information. Eighteen is the age one may be criminally responsible, the age when individuals are allowed to drive and when male citizens are subject to military draft. Moreover, by this age students are expected to have finished high school, and some of them are starting college.^{1.1.3} In order to address these concerns, we show that this impact is only related to

^{1.1.2}Literate citizens are eligible to vote when they become 16 years old, but are only subject to abstention fines (i.e. under compulsory voting) when they reach the age of 18.

^{1.1.3}Officially, schooling age in Brazil is 4 to 17 years. This means that when individuals reach the age of eighteen, they are supposed to be transitioning out of high school. However, as pointed by [Machado e](#)

becoming eighteen before or after the month of the election, and not on becoming eighteen in any month.^{1.1.4} We run placebo tests where the cutoff is changed to months different from the one where the election is taking place and find no significant or substantial results. Another concern is that individuals could be simply watching more television. We find, however, that the result is restricted to *Jornal Nacional*. Compulsory voting does not lead to a change in consumption of other shows such as soap operas, soccer games or other newscasts.

The young voters' preferences towards *Jornal Nacional* over other newscasts highlights the role credibility plays when it comes to information acquisition.^{1.1.5} With an audience share of 70%, the equivalent to 31 million viewers, *Jornal Nacional* is the most viewed and trusted Brazilian television newscast. On air from Monday to Saturday at 8:30 pm, it premiered on September 1, 1969, and since then has become a daily presence in the life of Brazilian families. According to [Porto \(2007a\)](#), almost half of its length focuses on political, economic and social themes, making it a natural source of information for issues that are salient to voters.

The existing evidence regarding compulsory voting and information acquisition is still mixed and relies mostly on cross-country comparisons or laboratory experiments. Using data from the 1989 Euro-Barometer post-election survey, [Gordon e Segura \(1997\)](#) measure the political sophistication of each respondent in the twelve nations of the pre-1995 European Union. They find a positive correlation between the presence of compulsory voting legislation and political sophistication. [Grosser e Seebauer \(2016\)](#) find that participants in a laboratory experiment are more likely to acquire costly information when not given the option to abstain from elections. On the other hand, [Loewen, Milner e Hicks \(2008\)](#) and [Birch \(2009\)](#) find no evidence on the relation between being under compulsory voting and being more informed.

Recently, new research has emerged providing within-country evidence on second-order effects of changes in the calculus of voting. Using a randomized field experiment

[Gonzaga \(2007\)](#), there are often delays for reasons such as late entry in school, grade repetitions, and evasion. This leads the schooling age to be more of a benchmark than a binding rule in Brazil.

^{1.1.4}The military draft and the year of high school graduation cutoffs are based on the year of birth, but not on the month of birth. Citizens can obtain a driver's license the day after turning eighteen. However, if being allowed to drive was generating this result, we would see the same effect for those turning eighteen in any given month, not only on the election cutoff.

^{1.1.5}There's a still small but growing literature on the how sources' credibility shapes the way voters acquire and process information. [Chiang e Knight \(2011\)](#) show how the impact of newspaper endorsements on voter's opinion varies according to the degree and the direction of the endorser's bias. [Alt, Marshall e Lassen \(2016\)](#) find that economic information acquired from the Danish Central Bank causes voters to better update their belief when compared to information received from the government or political parties.

applied during the San Francisco Municipal Elections of 2011, [Shineman \(2016\)](#) finds that voters who were mobilized to vote became more informed about the content of the election. Using data from a survey administered to young voters taking the high-school national exit exam in Brazil, [Holbein e Rangel \(2016\)](#) show that CV has no impact on the search for information on written materials such as newspapers and magazines. The authors emphasize, however, that young voters could be looking for political information on other sources like TV and the internet. Still related to our work are the findings of [Cepaluni e Hidalgo \(2016a\)](#). Using the full voter file from Brazil, they find an average turnout increase of 11.7 p.p. for young individuals subject to CV and a 4.4 p.p. decrease for voters transitioning from 69 to 70. [León \(2017\)](#) combines survey information with a change in the Peruvian voting law in order to gauge how individuals' political behavior is affected by the size of the abstention fine. The author interviewed subjects in the region of Lima just before elections to inform them about changes in the abstention fines. He finds that those who stop voting because they face lower abstention costs do not hold more information than those who face a higher fine.

Our work is most comparable with [Leon e Rizzi \(2014b\)](#) which also takes advantage of the discontinuity generated by the Brazilian CV law on their identification strategy. Their focus is on the impact of compulsory voting on knowledge about political issues, such as candidates' names and political platforms. In their paper, the authors conduct a survey with high school students and college freshmen in the city of São Paulo and find that the increase in turnout induced by CV is not followed by an increase in the consumption of political information or an increase in political knowledge, which is in sharp contrast with our results.

Taking both this result and ours at face value, one would point that those forced to vote are watching more TV news, yet not learning new political information.^{1.1.6} However, the relevance of identifying the increase in TV news consumption is precisely to show that the interest in acquiring information exists. Even though we cannot evaluate if voters actually learn relevant information when induced to vote, we provide nationally representative evidence that, because of CV, they seek broad information on salient topics ranging from the state of the economy, crime, health to educational policy, all of them covered by *Jornal Nacional*. Our results highlight how changes in the calculus of voting can ultimately lead to changes in media habits in one of the world's largest democracies.

^{1.1.6}Another troubling issue lies in defining exactly what the voter regards as relevant information at the time he or she decides to vote. It is perfectly reasonable that voters are more interested in learning about the current state of health, crime, and education when deciding whether to reelect the incumbent or to vote for a contender. Also, the formal political platforms of the main parties in Brazilian presidential elections are quite similar. As a result, the campaign very often focuses on accusations of corruption and mismanagement, and less on meaningful discussions related to political platforms.

This paper also contributes to the growing literature on the relation between the media and political outcomes. Most studies so far explore this link within consolidated democracies such as the ones found in the U.S. or Scandinavian countries.^{1.1.7} As pointed by [Sobbrio \(2014\)](#), there's a need for more work on different institutional settings. Brazil, being the largest democracy in Latin America and the home of Rede Globo, the third largest commercial television network in the world, provides an interesting backdrop for our analysis ([CIOCHETTO, 2013](#)).

A significant amount of research focuses on answering how media consumption affects voter turnout. The evidence is often obtained by taking advantage of exogenous changes in media exposure due to the entry of a new technology or a media channel. For example [Gentzkow \(2006\)](#) shows how the introduction of television in different markets in the United States is partially responsible for the decline in turnout and political knowledge since the 1950s. Other papers show how the exposure to different networks with clear bias or target audience mobilizes specific constituencies ([DELLAVIGNA; KAPLAN, 2007](#); [OBERHOLZER-GEE; WALDFOGEL, 2009](#); [ENIKOLOPOV; PETROVA; ZHURAVSKAYA, 2011](#)). The bulk of the literature remains silent, however, about the converse question: does inducing turnout increase media consumption? Such an answer would allow us to better understand how information is aggregated in large elections ([MARTINELLI, 2006](#)). By assessing what is the impact of the compulsory voting law on information acquisition in the form of TV newscasts, we address this important issue.

The remainder of the paper is organized as follows. Section 1.2 presents a theoretical framework that links compulsory voting to information acquisition. Section 1.3 provides basic background information on the mandatory voting law in Brazil and on *Jornal Nacional*, and Section 1.4 describes the data and our empirical strategy. The results are presented in Section 1.5, followed by robustness checks and placebo tests in Section 1.6 and the analysis of heterogeneous effects in Section 1.7. Section 1.8 concludes the paper.

1.2 Theoretical framework

1.2.1 Benchmark model

Our model follows closely the uncertain-voter model proposed by [Degan \(2006\)](#). It depicts the decision of a voter who is concerned about performing his civic duty, while facing a risk of, without information, voting for the wrong candidate. It studies the

^{1.1.7}See [DellaVigna e Ferrara \(2015\)](#) for a detailed survey.

relationship between policy preferences, turnout and information acquisition. We expand the model to analyze how abstention fines affect voting, turnout decisions and, ultimately, the demand for information.

There are two candidates $c \in \{L, R\}$ in this setting. Each one is running on a platform $y_c \in Y_c \subset Y$, where Y is the left-right policy space. Citizens are continuously distributed across an ideological continuum $Y \in [-1, 1]$. The mass of citizens has measure 1, and each citizen j is identified by his preferred platform $y_j \in Y$. They evaluate candidates according to the following payoff function:

$$u_j^c = -(y_j - y_c)^2. \quad (1.2.1)$$

Citizens are uncertain about the candidates' platforms which, from their perspective, are random variables $(\tilde{y}_L, \tilde{y}_R)$ following a distribution F over $Y_L \times Y_R$. For the sake of simplicity, we assume that $Y_L = [-1, 0]$, $Y_R = [0, 1]$ and that $F(\cdot)$ is symmetric around zero.

Citizen j 's information set is denoted by $\Omega_j \in \Omega = \{F, (y_L, y_R)\}$. She can pay a cost δ to receive a signal that reveals the candidates' true platform with probability $p \in (0, 1]$ (i.e. she learns exact platforms $\Omega_j = (y_L, y_R)$). In case the citizen does not pay the cost, or if the signal is not informative, she remains uninformed and makes her decisions according to the distribution F .

A central feature of this model is that uncertainty about the candidates' platforms generates a disutility from possible voting mistakes. More precisely, the behavioral cost of voting for candidate $L(R)$ is defined as the expected utility loss generated by the possibility of voting for him when candidate $R(L)$ would generate greater utility. That is:

$$C_j(L; y_j, \Omega_j) = \mathbb{E}[\mathbf{1}\{u_j^L < u_j^R\} \cdot (u_j^R - u_j^L) | \Omega_j]. \quad (1.2.2)$$

Where $\mathbf{1}\{\cdot\}$ is an indicator function assuming value one when the condition inside brackets is met and zero otherwise. The voting cost for candidate R is analogous to the expression above. Notice that when the individual is not certain about each candidate's policy position, they incur on a cost $C_j(\cdot) \geq 0$. Those who have successfully acquired information about the candidates' platforms always pick the correct candidate and obtain $C_j(\cdot) = 0$.

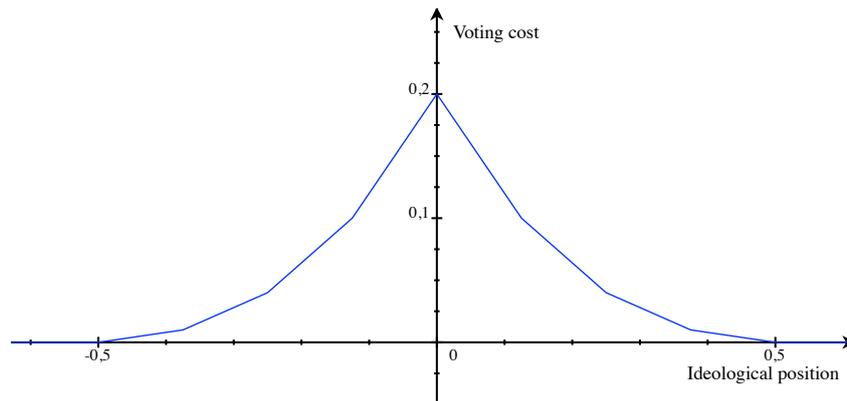
Every citizen also has a civic-duty benefit $d_j \geq 0$ from attending the ballots at the day of the election. As in [Degan \(2006\)](#), we assume d_j is exogenous. It does not depend on information or the chosen candidate, which means this is an expressive voting model.

That is, the voter cares about voting for the right candidate and for fulfilling his civic duty. The actual result of the election has no impact over his preferences regarding the voting decisions. If the voter does not turn out to vote, her payoff is zero. She might, however, still pay the cost of information acquisition if she has chosen to do so.

The citizen's decision-making process is a three-stage problem. In the first stage, she must choose whether or not to acquire information. That is, to choose $i \in \{0, 1\}$ where $i_j = 1$ ($i = 0$) means information was (not) acquired. Next, given her information set, she chooses whether or not to turn out to vote. This choice is expressed by the dummy $t_j \in \{0, 1\}$, where $t = 1$ ($t = 0$) means she does (not) turn out to vote. Lastly, the voter must decide on whom to vote for, represented by the variable $v_j \in \{L, R\}$.

The solution to this problem is given by the tuple (v_j^*, t_j^*, i_j^*) and it is found by solving the model backwards. It is also important for the analysis to define the voting cost as a function of the optimal voting decision and the result of information acquisition, that is $C_j = C_j(v_j^*(y_j, \Omega_j = \{F(y_L, y_R)\}))$. This cost function is crucial for the information acquisition and turnout decisions. It is single peaked at zero, and it is zero for individuals with $|y_j| \geq 1/2$ as can be seen in Figure 1.2.1. The intuition is that voters with extreme ideological positions will never make voting mistakes since the candidates' platforms do not overlap. Moderate voters, on the other hand, can be uncertain about their ideal candidate. Finally, the voting cost is zero when information acquisition is successful. A comprehensive presentation of the model's solution can be found in the appendix.

Figura 1.2.1 – Relation between voting cost and ideology



Following [Degan \(2006\)](#), there are four possible outcomes to this decision problem according to the parameters space. When $d_j \geq \delta/p > C_j$, the citizen turns out to vote ($t_j^* = 1$) but does not acquire information ($i^* = 0$). Those are the *strong partisans*, who are confident about their favorite candidate such that their voting cost is so low, it is not worth investing to acquire information. When $d_j \geq C_j \geq \delta/p$, the citizen turns out

to vote ($t_j^* = 1$) and invests to acquire information ($i_j^* = 1$). This group of individuals is referred to as the *weak partisans*, since their voting cost is intermediate: not low enough to induce abstention, but not high enough to find information acquisition unnecessary. When $C_j > d_j \geq \delta/p$ the citizen invests to acquire information ($i_j^* = 1$) but only turns out to vote if the investment is rewarded and she learns the politicians' platforms. They are the *middle of the road* voters. These individuals are within the center of the ideological spectrum. Their risk of making a voting mistake is so great that they only turn out when information acquisition is successful. Finally, if $C_j > \delta/p > d_j$ or $\delta/p > C_j > d_j$, the citizen never tries to acquire information ($i_j^* = 0$) or turns out to vote ($t_j^* = 0$).^{1.2.1}

Our interest is to analyze the effects of compulsory voting laws on the decision to become informed. In order to do so, we must include an abstention penalty in our benchmark model. That is, citizens must pay an additional cost when they fail to turn out.

1.2.2 Compulsory Voting

Under compulsory voting, citizens must now incur in a cost M_j if they choose to abstain. The voter's expected payoff is given by:

$$\text{equation } t_j[d_j - C_j] - (1 - t_j)M_j - \delta \cdot i_j$$

Which can be rewritten as:

$$\text{equation } t_j[\hat{d}_j - C_j] - M_j - \delta \cdot i_j,$$

where $\hat{d}_j = d_j + M_j$ is the induced benefit from attending the ballots and $\hat{d}_j - C_j$ is the opportunity cost of abstention under compulsory voting. The solution to this problem, as in our benchmark set-up, can be found by solving the model backwards. Notice that, after defining \hat{d}_j in equation (1.2.2), the remaining term M_j is a constant on the citizen's preferences. As such, it does not affect any stage of the decision. The induced benefit of attending the ballots \hat{d}_j , on the other hand, changes the decision when compared to the former model with a plain d_j . That said, all the former results hold by simply replacing d_j by \hat{d}_j in the previous subsection. The new results are presented in the following proposition:

Proposition 1. *Let $M_j > 0$ and $\hat{d}_j = d_j + M_j$. Compulsory voting leads to:*

1. *An increase in turnout for citizens j with $C_j \geq d_j$ and either $\hat{d}_j > C_j \geq d_j$ or $C_j > \hat{d}_j > \delta/p > d_j$.*
2. *An increase in information acquisition for those with $\hat{d}_j, C_j > \frac{\delta}{p} \geq d_j$.*

^{1.2.1}For further details on the proofs of these results, see the appendix and [Degan \(2006\)](#).

The first statement of Proposition 2.1 comes from the fact that, for citizens with voting cost superior to the civic duty ($C_j > d_j$), the abstention cost may increase the induced civic duty enough to surpass the voting cost ($\hat{d}_j > C_j > d_j$). The second statement comes from the citizens with civic duty below the effective cost of information acquisition ($\delta/p > d_j$). Their induced civic duty may become greater than the effective cost of information acquisition ($\hat{d}_j > \delta/p > d_j$). All such citizens invest in information acquisition. Some turn out to vote anyway when $\hat{d}_j > C_j$. Others turn out to vote only if information acquisition is successful, in case $C_j > \hat{d}_j$. Notice that CV effects are mostly focused on voters with relatively high voting cost^{1.2.2}. Such voters tend to be those within the center of the ideological distribution, i.e. moderate voters, as can be seen from Figure 1.2.1.^{1.2.3} To sum up, turnout increases due to compulsory voting, as expected and confirmed by empirical evidence previously discussed. Also, information acquisition increases due to CV, a result which is supported by our empirical evidence.

1.3 Institutional background

1.3.1 The compulsory voting law

Brazil's current compulsory voting legislation started with the promulgation of the Brazilian Constitution of 1988. It states that every literate citizen older than 18 and younger than 70 years old must turn out on election day or justify their absence in a special court. Citizens older than 16 and younger than 18 are allowed to register to vote, but do not face any consequence if they don't.^{1.3.1} If an individual fails to justify her absence, she must pay a small fine of R\$ 3.00 (roughly 0.90 dollars), which can be multiplied tenfold according to the decision of the judge handling the case. Those who fail to justify absence three times are also subject to a number of sanctions, such as not being allowed to issue a new ID or passport, being ineligible for cash transfer programs, credit by financial institutions maintained by the government, public jobs and public education.^{1.3.2}

There are also reasons to believe that other non-fiduciary elements may induce turnout. First, citizens are allocated to ballots according to their postal code. This means

^{1.2.2}As in Degan (2006) we avoid describing the distribution of civic duty d_j . That means CV's impact can be different for voters with similar voting cost.

^{1.2.3}It is important to emphasize that citizens' ideology y_j does not change due to CV.

^{1.3.1}According to the Superior Electoral Court of Brazil (TSE), only one-fourth of individuals under eighteen years registered to vote in the 2014 elections.

^{1.3.2}For more information, see <http://www.tse.jus.br/eleitor/servicos/justificativa-eleitoral> (in Portuguese).

that people who live in the same neighborhood will vote near their home in the same place, on the same day. Recent studies stress the fact that individuals care about being seen as participant citizens, and this concern about their social image constitutes a driving force behind voter turnout (GERBER; GREEN; LARIMER, 2008; FUNK, 2010; DELLAVIGNA et al., 2017). In light of these findings, an abstention in the day of an election may be socially costly for citizens who worry about how they will be seen in their local community. Furthermore, the first and second round of the elections are held on the first and last Sunday of October, respectively. Since an individual who failed to justify his absence must pay the fine on a regional electoral court during business hours, it's likely that the opportunity cost of attending the ballots is lower than the one of having to deal with the bureaucracy on weekdays. Also, unlike the ballot, the court is not necessarily located near from where the person who abstained lives.

The abstention rates in Brazil are also low. For example, in the 2012 elections, the average abstention in Brazilian municipalities was 14%, with 9% invalid votes cast for mayor and 5% invalid votes for city council. This leads us to believe that the law is indeed enforced and turnout is high in the presence of compulsory voting. Recent research by Cepaluni e Hidalgo (2016a) using the full Brazilian voter file comprising of 140 million individuals shows that Brazilian compulsory voting law has a sizable causal impact on turnout, in line with the findings of Leon e Rizzi (2014b).

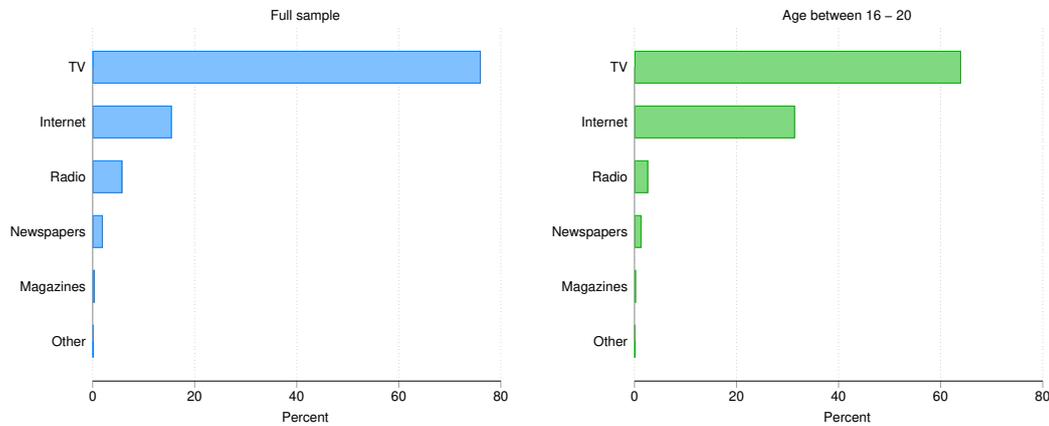
1.3.2 Rede Globo and Jornal Nacional

Television plays a significant role in the life of Brazilian citizens. According to the 2006 National Household Sample Survey (PNAD), 93% of the households owned at least one television set. Fava e Arends-Kuenning (2013) presents evidence that, among poor households, there is a higher probability that a family owns a color TV set than a refrigerator, despite both representing a similar cost. According to the Brazilian Media Survey of 2014, a vast majority of the population has the TV as their primary source of information, as it can be seen in Figure 1.3.1. This is consistent with the findings of Kennedy e Prat (2017) on media consumption patterns around the world. Even though the internet plays a larger role for the younger population, it still fares well behind television as young individuals main source of information.^{1.3.3} Using data from a different survey, Mesquita (2010b) finds that TV is also preferred by 65.4% of the population as their main

^{1.3.3}Even if one accepts that the internet has a growing presence in the life of young adults, it is important to note that consuming information doesn't necessarily mean trusting it. In Figure 1.9.1 and 1.9.2 we show that TV and traditional media are still more a reliable source of information for young adults than online platforms such as websites, blogs, and social media

source of news on political issues.

Figura 1.3.1 – Favorite source of information.



Source: Brazilian Media Survey 2015.

Owned by Globo Organization, a conglomerate founded by media mogul Roberto Marinho, Rede Globo was launched in 1965, amidst Brazil's military regime. Since then it quickly expanded to become the leading TV network in the country. According to Ferrara, Chong e Duryea (2012), Rede Globo's swift growth started during the military dictatorship as a consequence of the central authority's ambition to integrate the country using mass media. The government saw in television the potential to promote a sense of national identity and, thus, encouraged its expansion. This was accomplished by subsidizing credit for set sales and by granting concessions following clientelistic and political criteria, a process from which Globo took great advantage. It has since been regarded as the most powerful television network in Brazil, with 35 affiliated networks and over 3305 retransmission stations. That enables Globo's TV signal to reach 98.56% of municipalities and 99.51% of the population.^{1.3.4} In a country where most people have access to television, Globo's presence in the everyday life of Brazilian citizens cannot be overstated. Kennedy e Prat (2017) call attention to the fact that, along with Dogan Media from Turkey and Atresmedia from Spain, Globo is one of the top media companies in the world in terms of power to swing an election. According to the Brazilian Electoral Study (ESEB), when asked to evaluate nine different institutions, voters placed the Catholic Church in first place and Rede Globo in second (PORTO, 2007b).^{1.3.5}

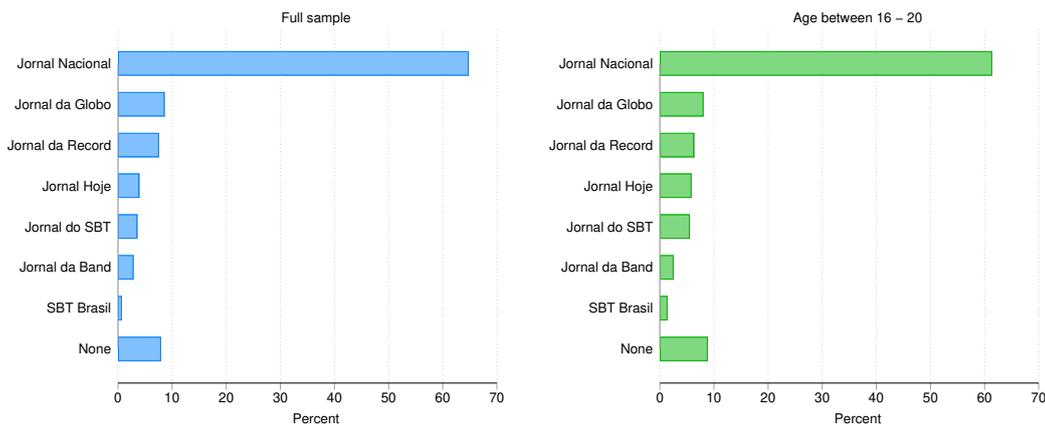
Its flagship newscast is *Jornal Nacional*. On air from Monday to Saturday at 8:30 pm, it holds almost 70% of the TV share in its time slot, the equivalent to 31 million

^{1.3.4}For more information see <http://comercial2.redeglobo.com.br/atlasdecobertura/Paginas/Totalizador.aspx> (in Portuguese).

^{1.3.5}Brazil is the country with the largest number of Catholics in the world.

viewers, an impressive number when compared to similar news programs from Europe and North America (MESQUITA, 2010a).^{1.3.6} According to the Brazilian Media Survey of 2015, it is also the most trusted newscast in Brazilian television, as Figure 1.3.2 shows. Note that this pattern still holds for young voters, our population of interest in this paper.

Figura 1.3.2 – Trust in newscasts from major networks.



Source: Brazilian Media Survey 2015.

The consolidation of Brazilian democracy in the 90's and the many changes that happened in Rede Globo's News Division helped to shape the current format of *Jornal Nacional* (henceforth, "JN"). According to Porto (2007a), during the last two decades, about 40% of the length of the newscast is dedicated to political, social and economic related issues. The author also argues that the newscast has greatly reduced its reliance on official sources, a sign of its departure from its authoritarian roots. Newspapers, often used as a proxy for investment in political information,^{1.3.7} have a low readership which is characterized by a lot overlap with the audience of *Jornal Nacional*: 83% of individuals who read newspapers once a week also watch JN, while just 45% of those who watch *Jornal Nacional* say they also read newspapers (MOISÉS, 2010). Overall, the massive reach of Rede Globo's signal, the salience of *Jornal Nacional*'s content and the trust individuals place on what it broadcasts makes it a relevant source of information for voters.

^{1.3.6}For the sake of comparison, Mesquita (2010a) notes that NBC Nightly News, America's most watched newscast, has an audience share of 15%, BBC's Six O'Clock News has a share of 26% and TF1's 20 Heures from France has 46% of the audience share.

^{1.3.7}For example, Holbein e Rangel (2016) measure the impact of the Brazilian compulsory voting law on political information acquisition using newspaper as their proxy.

1.4 Data and research design

1.4.1 Data

Our data comes from the first two waves of the Brazilian Media Survey (BMS). This is a nationally representative survey planned jointly by the Secretariat of Social Communication of the Brazilian Presidency (SECOM) and the Brazilian Institute of Public Opinion and Statistics (IBOPE). Its first wave, the BMS 2014, was implemented between October 12 and November 6, 2013, while its second wave, the BMS 2015, was implemented between November 5 and November 22, 2014.^{1.4.1} A total of 36624 citizens in 848 municipalities were interviewed face-to-face about their media consumption habits. According to the official release, the main objective of this survey was to map these habits in order to improve the government's advertisement policies. The summary statistics for our main variables of interest can be found in Table 1.4.1.

^{1.4.1}Even though BMS 2016 was released on January, 2017, we do not use this data in our analysis because of two key methodological changes were implemented. First, there is no information on the month and year of birth of respondents. Second, the question used to construct our main dependent variable was removed from the survey.

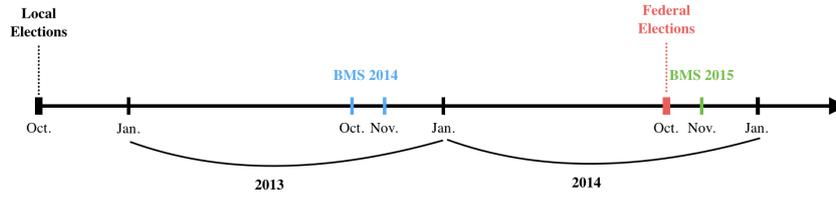
Tabela 1.4.1 – Summary statistics.

	Observations	Mean	Std. Dev.	Min.	Median	Max.
<i>Panel A: TV shows</i>						
Jornal Nacional	28225	0.463	0.499	0.0	0.0	1.0
Other newscasts	27070	0.170	0.376	0.0	0.0	1.0
Soap operas	27807	0.408	0.492	0.0	0.0	1.0
Soccer matches	26575	0.143	0.350	0.0	0.0	1.0
<i>Panel B: Baseline covariates</i>						
Student	33268	0.063	0.242	0.0	0.0	1.0
White	33188	0.376	0.484	0.0	0.0	1.0
Male	33491	0.481	0.500	0.0	0.0	1.0
Lives in state capital	33491	0.304	0.460	0.0	0.0	1.0
Household income	31143	2144.994	2107.692	339.0	1050.0	14000.0
Years of education	33491	9.289	4.116	0.0	10.5	16.0

Notes: On Panel A, the variable *Jornal Nacional* is an indicator which takes value 1 whenever the respondent says that *Jornal Nacional* is one of the three TV shows that he or she watches the most during the week. The variable *Other newscasts* is an indicator which takes value 1 whenever the respondent says that SBT Brasil, *Jornal da Record*, *Jornal da Band*, *Jornal da Globo*, *Jornal da Noite* or *Jornal do SBT* are one of the three TV shows that he or she watches the most. The variable *Soccer games* is an indicator which takes value 1 whenever the respondent says that soccer games broadcasted by *Globo*, *Band* or *Record* are one of the three TV shows that he or she watches the most. The variable *Soap operas* is an indicator which takes value 1 whenever the respondent says that the 7:30 pm or the 9:00 pm soap operas broadcasted by *Rede Globo* are one of the three TV shows that he or she watches the most. Panel B displays indicator variables related to socioeconomic information from the Brazilian Media Survey. They take value 1 if the respondent has a determined characteristic, and 0 otherwise. Household income is calculated based on the mid-points of income ranges provided in the survey. Minimum wages (*mw*) on 2013 and 2014 were R\$ 678.00 and R\$ 700.00 respectively. Income ranges were 0 to 1 *mw*, 1 to 2 *mw*, 2 to 5 *mw*, 5 to 10 *mw*, 10 to 20 *mw*, and 20 or more more *mw*, which we cap at 20 *mw*. Years of education calculated based on the highest completed grade reported on the survey. Incomplete grades are calculated as the mid-point, in years, between two completed grades. Since the compulsory voting law does not apply to illiterate citizens, this group is removed from our sample. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014.

It is also important to consider the timing of the surveys with respect to the elections. The first round of the 2012 Local Elections took place on October 7 of that same year. On this day, citizens from all the 5568 municipalities of Brazil chose their mayors, members of the city council and members of the local legislative assemblies. On 2013, the first wave of the BMS was implemented. On October 2014 the National Elections took place. Citizens attended the ballots to elect their congressmen, senators, state governors and the president. The second wave of the BMS was implemented on November 2014, just one month after the elections. The registration deadline for the 2014 elections was at October 5, 2013. Thus, by the time both surveys were implemented all the candidates running on the national race had been officially announced. The timeline in Figure 1.4.1 helps to understand how the timing of the Brazilian Media Survey relates to the local and federal elections.

Figura 1.4.1 – Timing of the Brazilian Media Survey.



In order to assure that the sample was representative, data from the 2010 National Census and the 2011 National Household Sample Survey were used. In the first stage of the sampling procedure, municipalities were randomly selected using probability proportional to size (PPS) sampling on the population older than 16 years old. Next, within each selected municipality, census tracts were selected using PPS once again. In the last step, individuals were chosen following proportional quotas on gender, race, age, education and activity.

1.4.2 Identification strategy

Our aim is to evaluate the impact of the Brazilian compulsory voting law on the consumption of *Jornal Nacional*, Rede Globo’s main newscast. We exploit the credibly exogenous source of variation of political participation which comes from the compulsory voting law in Brazil in order to provide results which can be interpreted as causal. As previously mentioned, the cost of abstention in Brazilian elections is a deterministic and discontinuous function of the age of the individual, enabling the use of a sharp regression discontinuity design (RDD) approach.

Similar to [Leon e Rizzi \(2014b\)](#) and [Holbein e Rangel \(2016\)](#), we compare individuals whose ages are in the neighborhood of the threshold that determines the change from voluntary voting to compulsory voting. Following [Lee e Lemieux \(2010\)](#) we run an OLS on both sides of the threshold and compare the predicted value of their dependent variable at the discontinuity. We estimate the following equation:

$$Y_i = \alpha + \beta_k M_i + f(A_i - k) + \epsilon_i, \quad (1.4.1)$$

where M_i is a dummy which indicates whether individual i is exposed to compulsory voting ($M_i = 1$) or not ($M_i = 0$). A_i is the age of individual i in months in the elections of October 2014. $k = 216$ is the age cutoff in months set by the law, and $f(\cdot)$ are continuous functions of our running variable centered at the cutoff value. The parameter of interest is β_k , the causal effect on Y_i of being exposed to compulsory voting. We estimate our

equation assuming that $f(\cdot)$ is a flexible polynomial on both sides of the threshold. We also take into account sampling weights in order to make the sample representative of Brazilian population.^{1.4.2} The running variable in our RDD setting is the centered age of the individual, represented by $A_i - k$, which takes negative values for individuals with $M_i = 0$ and positive values for citizens with $M_i = 1$. The population at the 18th birthday cutoff is composed of those who were born at October, 1996. For those who were born in November 1996 and older, voting is mandatory. They have $A_i = -1$ and $M_i = 0$. For those born in September 1996 and younger, voting is voluntary. They have $A_i = 1$ and $M_i = 1$. Although we leave for the supplementary appendix the estimates for those transitioning from compulsory to voluntary voting due to becoming 70 year old, the same reasoning applies. For elderly voters around the 70th birthday cutoff, those born in November 1944 will have $A_i = 1$ and $M_i = 1$ and those born in September 1944 will have $A_i = -1$ and $M_i = 0$. We follow [Gelman e Imbens \(2014\)](#) and estimate only zero, first and second-degree polynomials for different bandwidths, including the optimal bandwidth calculated using the Calonico-Cattaneo-Titiunik (CCT) procedure from [Calonico, Cattaneo e Titiunik \(2014\)](#) when possible.^{1.4.3}

We only have record of the individual's month and the year of birth, so additional care must be taken in order to reduce bias that may arise due to the few mass points of our running variable around the threshold. First, we cluster standard errors on our running variable in order to generate wider confidence intervals that reflect the imperfect fit of the parametric function away from the threshold ([LEE; CARD, 2008](#)). The discreteness of our running variable also means that we have both treated and non-treated individuals at the threshold. In other words, for $A_i = k$, we have individuals that should have either $M_i = 0$ or $M_i = 1$. Assigning our observations to any of these groups may result in biased estimates of our treatment effect and so, we do not include them in our analysis.

To identify β_k as a causal parameter, our design must satisfy two key assumptions. The first is that the selection criteria for compulsory voting is not subject to manipulation. Even though it is highly improbable that the timing of births is affected by the date of the election, we follow the procedure from [Frandsen \(2017\)](#), which tests bunching of the running variable in a similar way to [McCrary \(2008\)](#), but allowing for a discrete running variable. The results in Table 1.4.2 show that we fail to reject the null hypothesis of no

^{1.4.2}Although sampling used a census tract level, the most granular geographical unit by which we can identify each individual is the state they were living in at the time the survey was implemented.

^{1.4.3}Even though optimal bandwidth selection procedures are often used in the literature, our estimates using these procedures must be taken with a grain of salt because of the discreteness of our running variable. All of these selection procedures assume that the conditional expectation is at least two times continuously differentiable, which implies the assumption that the running variable is continuously distributed. For more information, see [Calonico, Cattaneo e Titiunik \(2014\)](#).

manipulation for both cutoffs.^{1.4.4}

Tabela 1.4.2 – RDD density test for discrete running variable.

Cutoffs	p-value
18th birthday	0.824
70th birthday	0.393

Notes: The null hypothesis is that there is no manipulation around the cutoffs. Density tests done following [Frandsen \(2017\)](#). Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014.

The second assumption requires that baseline covariates do not change because of the abstention fine, i.e., there shouldn't be a treatment effect of compulsory voting on predetermined attributes of the individuals. We run our main specification from Equation 1.4.1 using as dependent variable socioeconomic characteristics that we would not expect to change as a consequence of their birthday in October, 2014, such as race, gender and household income. The results can be found in Table 1.4.3. As we should expect for predetermined variables in a valid RD set-up, there is no robust discontinuity around the threshold.^{1.4.5}

^{1.4.4}Furthermore, one can see in the histogram displayed in Figure 1.9.3 at the Appendix that the running variable is smooth around our main threshold.

^{1.4.5}Figure 1.9.4 displays the RD figures for each variable using a 16 months bandwidth linear polynomial fit. We use this same specification on all the remaining RD figures in this paper. Sensitivity of the estimates to different bandwidth lengths can be verified on Figure 1.9.5. Moreover, we also provide balance estimates using the optimal bandwidth from ([CALONICO; CATTANEO; TITIUNIK, 2014](#)) on Table 1.9.1.

Tabela 1.4.3 – Covariate balance around the 18th birthday cutoff.

Dependent variables	(1)	(2)	Mean D.V. for $M_i = 0$	Observations
Student	-0.055 (0.045)	-0.097 (0.064)	0.692	2,263
White	-0.001 (0.051)	0.004 (0.071)	0.415	1,749
Male	0.007 (0.049)	-0.031 (0.077)	0.488	1,767
Capital	-0.038 (0.033)	-0.032 (0.048)	0.242	1,767
Household income	-253.707 (186.060)	202.975 (278.973)	1828	1,598
Schooling	0.006 (0.210)	-0.168 (0.399)	9.630	1,767
Polynomial order	First	Second	-	-

Notes: Each figure in the table is from a separate local regression with a 16 months bandwidth around the cutoff. Household income is calculated based on the mid-points of income ranges provided in the survey. Minimum wages (mw) on 2013 and 2014 were R\$ 678.00 and R\$ 700.00 respectively. Income ranges were 0 to 1 mw , 1 to 2 mw , 2 to 5 mw , 5 to 10 mw , 10 to 20 mw , and 20 or more more mw , which we cap at 20 mw . Years of education calculated based on the highest completed grade reported on the survey. Incomplete grades are calculated as the mid-point, in years, between two completed grades. All other variables are indicators for demographic characteristics. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. All standard errors are clustered on age measured in months and all regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

This confirms that there's no causal effect of the forcing variable on baseline characteristics and, thus, that the predetermined variables are smooth around the threshold. Having satisfied these two assumptions, we have that:

$$\beta_k = \lim_{A_i \rightarrow k^+} E(Y_i | A_i) - \lim_{A_i \rightarrow k^-} E(Y_i | A_i). \quad (1.4.2)$$

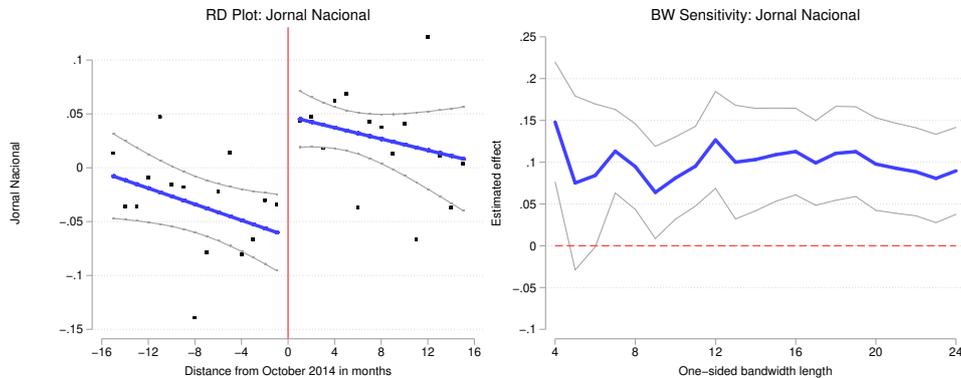
The causal parameter of interest β_k is the average impact of compulsory voting on a random individual at the cutoff. Under both the unconfoundedness and continuity assumption, we can say that any change in outcome Y_i at the threshold is caused by the fact that the individual is subject to the changes that reaching the cutoff age entails. Since not all individuals comply with compulsory voting law, and since we do not have information on actual turnout or voter registration to account for this, this estimate can be interpreted as the intent-to-treat (ITT) estimate for the causal effect of CV on the dependent variable.

1.5 Main results

Our analysis focuses on young citizens who were first exposed to the mandatory voting law when they have reached the age of eighteen. We present our main specification results for seven different bandwidth lengths (including the CCT optimal bandwidth from [Calónico, Cattaneo e Titiunik \(2014\)](#)) and two different polynomials, to ensure robustness of the results. Our largest bandwidth of choice consists of a 24 months open interval. It includes citizens who were born between October 1994 and October 1998, i.e., those who were younger than 20 and older than 16 by the time of the federal elections of October 2014. This ensures that every individual born between September 1994 and September 1996 is facing abstention penalties for the first time. Conversely, everyone with birth date between November, 1996 and September, 1998 has not yet been subject to the compulsory voting legislation.

We use the following question posed to individuals in the two waves of the Brazilian Media Survey: “What is the TV show that you watch the most during the week?” This is an open-ended question which is repeated three times in order to find the respondent’s first, second and third favorite show in both cable and broadcast TV. The phrasing of the question allows us to measure the importance of Rede Globo’s *Jornal Nacional* relative to other TV shows. We transform the answers into an indicator variable coded to take value 1 whenever *Jornal Nacional* is one of the respondent’s three most watched TV shows, and 0 otherwise. This indicator is our dependent variable. The interpretation of the coefficients that come from linear probability models like this one is straightforward: β_k is the percent change on the probability *Jornal Nacional* is reported as one of the individual’s three most watched newscasts.

Figura 1.5.1 – Impact of exposure to abstention penalties on the consumption of Jornal Nacional.



Notes: This figure displays both the treatment effect of exposure to abstention penalties on the consumption of JN and the sensitivity of our estimates to different bandwidth lengths. Running variable is centered at October 2014, our true cutoff. Specification on the RD plot uses a linear polynomial and a 16 months bandwidth. RD plot displays residuals after accounting state and time fixed effects. Effects on the BW sensitivity test are estimated using a linear polynomial. 95% CI are displayed.

Our main result is illustrated in Figure 1.5.1. For our specification of choice, we see a sharp jump around the threshold. This result confirms that exposure to compulsory voting law increases the probability of watching Jornal Nacional. On the same figure we can also see that, for almost all bandwidth lengths treatment coefficients tend to stay around 0.1 and are statistically distinct from zero. Table 1.5.1 presents the estimated effects for different combinations of polynomial and bandwidth length. Column 1 provides RD estimates calculated under optimal bandwidths selected using the procedure from [Calonico, Cattaneo e Titiunik \(2014\)](#) for three different polynomials. The remaining columns present the RD estimates for treatment effects across different bandwidth lengths. All our specifications include year and state fixed effects in order to account for both the timing of the survey and for unobserved characteristics from each state.

Tabela 1.5.1 – Treatment effect of exposure to abstention penalties on the consumption of Jornal Nacional at the 18th birthday cutoff.

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First	0.099 (0.026)***	0.148 (0.037)***	0.095 (0.026)***	0.127 (0.030)***	0.113 (0.026)***	0.098 (0.028)***	0.090 (0.027)***
Second	0.107 (0.031)***	0.146 (0.136)	0.108 (0.053)*	0.032 (0.045)	0.074 (0.031)**	0.113 (0.034)***	0.107 (0.031)***
Mean D.V. for $M_i = 0$	-	0.187	0.191	0.192	0.198	0.205	0.187
Observations	-	374	903	1,377	1,788	2,155	2,444
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the respondent says that Jornal Nacional is one of the three TV shows that he or she watches the most during the week. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. CCT optimal bandwidths are 16.37 (first degree poly.) and 23.15 (second degree poly.). All standard errors are clustered on age measured in months. All regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The estimated coefficient is remarkably stable to different bandwidth lengths and polynomial specifications. Since our dependent variable is a dummy (i.e. our regressions are linear probability models) the interpretation of the coefficients is straightforward. A change from the voluntary voting to the mandatory voting regime increases the probability of an individual to name Jornal Nacional as one of her most watched shows in 11.3 percentage points for the linear local regression in the bandwidth of sixteen months.^{1.5.1} Because the mean of our dependent variable before the cutoff is 0.198 for this bandwidth length, our causal estimate represents a 57% increase in the probability to watch Jornal Nacional. We attribute this impact to the abstention penalties the citizen becomes subject to when she reaches the age of eighteen.^{1.5.2}

1.6 Robustness

Even though most baseline covariates are balanced and there's no reason to believe in manipulation of the forcing variable, we must still address some possible confounding factors that may compromise our results.

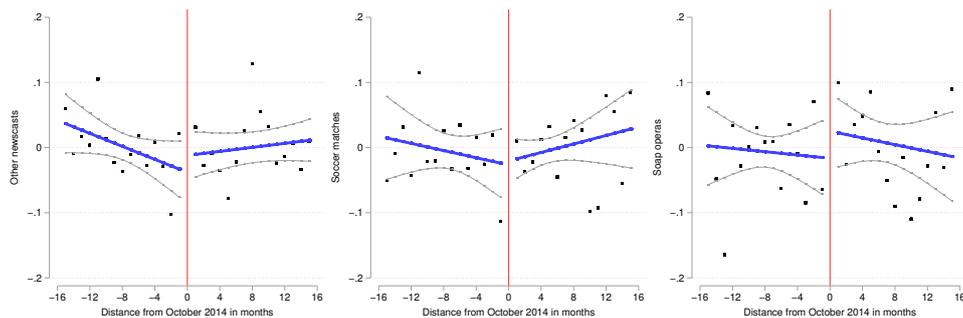
First of all, it is possible that the impact we've found for young voters is not

^{1.5.1}On Figure 1.9.6 we display the RD plots for different bandwidths in order to show how slopes on both sides of the cutoff change as we move away from it.

^{1.5.2}We find very similar effects by running our specifications without any fixed effects, as it can be seen on Figure 1.9.7 and Table 1.9.2 of the appendix. No effect is found for those transitioning from CV to voluntary voting at the 70th birthday cutoff. These results are displayed on Table 1.9.3 of the appendix.

restricted to Jornal Nacional. Individuals may be watching other newscasts or might be just watching more TV. In order to check this possibility we create three different indicators which we use as dependent variables. The first one takes value 1 whenever the respondent says that one of the primetime newscasts broadcasted by the four major TV networks in Brazil, namely Globo, SBT, Band and Record, are one of the three TV shows that he or she watches the most. The second dependent variable takes value 1 whenever the respondent says that soccer games are one of the three three TV shows that she watches the most. Finally, the third dependent variable takes value 1 whenever the respondent says that the 7:30pm or the 9:00pm soap operas broadcasted by Rede Globo are one of the three TV shows that she watches the most. Since Jornal Nacional goes on air from 8:30pm until 9:00pm, it is possible that viewers are merely watching the newscast by inertia, when what they're truly seeking to consume is entertainment.

Figura 1.6.1 – Impact of exposure to abstention penalties on the consumption of other TV shows.



Notes: This figure displays the treatment effect of exposure to abstention penalties on the consumption of other prime-time newscasts, soccer games and soap operas. Running variable is centered at October 2014, our true cutoff. Specification on the RD plots uses a linear polynomial and a 16 months bandwidth. RD plot displays residuals after accounting for state and time fixed effects. 95% CI are displayed.

The results can be found in Figure 1.6.1 and Table 1.6.1. They support the idea that voters, when induced to turn out, are using Jornal Nacional as their main source of information. This points out that voters converge to the greater reputation outlet and do not diversify the news coverage they are exposed to. No changes in the consumption of other primetime newscasts from Globo and other networks is detected. This result is coherent with the idea that JN is the most trusted newscast and, therefore, would be the one where people would try to inform themselves. Moreover, consumption of TV shows focused on entertainment such as soccer games and soap operas is also not affected. Although some significance is found in some bandwidth lengths in Panel C, none of them are robust to polynomial changes. This rules out the idea that individuals are watching JN just because they are waiting for the soap opera or a soccer match to begin. Taken together, these results emphasize that the increase in consumption of JN is not coming

from an increase in general broadcast programming consumption, but for a focused interest on becoming informed from the source that the agents prefer and consider to be more reliable.

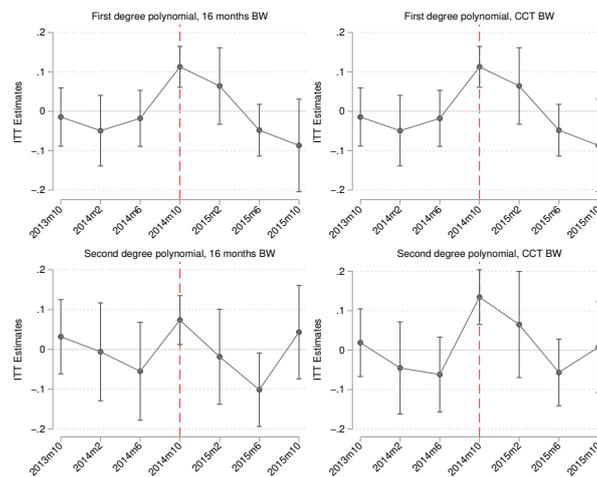
Tabela 1.6.1 – Treatment effect of exposure to abstention penalties on the consumption of other TV shows at the 18th birthday cutoff.

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Other newscasts</i>							
First	0.018 (0.030)	0.027 (0.054)	0.018 (0.052)	0.013 (0.044)	0.027 (0.032)	0.003 (0.030)	-0.006 (0.028)
Second	0.028 (0.048)	-0.112 (0.103)	0.094 (0.071)	-0.020 (0.068)	-0.013 (0.061)	0.021 (0.053)	0.022 (0.044)
Mean D.V. for $M_i = 0$	-	0.105	0.111	0.122	0.121	0.108	0.099
Observations	-	368	893	1,364	1,765	2,128	2,416
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.
<i>Panel B: Soccer games</i>							
First	0.011 (0.033)	0.129 (0.040)**	0.037 (0.053)	0.062 (0.041)	0.006 (0.035)	0.005 (0.030)	0.007 (0.027)
Second	0.031 (0.044)	0.475 (0.093)***	0.132 (0.060)**	-0.001 (0.075)	0.079 (0.053)	0.052 (0.052)	0.031 (0.044)
Mean D.V. for $M_i = 0$	-	0.091	0.113	0.130	0.123	0.121	0.118
Observations	-	360	879	1,344	1,742	2,103	2,387
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.
<i>Panel C: Soap operas</i>							
First	0.058 (0.040)	-0.034 (0.108)	0.079 (0.069)	0.102 (0.053)*	0.042 (0.045)	0.060 (0.037)	0.037 (0.037)
Second	0.108 (0.064)	0.606 (0.113)***	0.032 (0.118)	0.091 (0.089)	0.151 (0.075)*	0.073 (0.066)	0.086 (0.062)
Mean D.V. for $M_i = 0$	-	0.416	0.417	0.425	0.425	0.426	0.419
Observations	-	379	920	1,406	1,827	2,203	2,502
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. Each panel displays the results for different dependent variables. Panel A presents the results for the dependent variable as an indicator which takes value 1 whenever the respondent says that SBT Brasil, Jornal da Record, Jornal da Band, Jornal da Globo, Jornal da Noite or Jornal do SBT are one of the three TV shows that he or she watches the most. Panel B presents the results for the dependent variable as an indicator which takes value 1 whenever the respondent says that soccer games broadcasted by Globo, Band or Record are one of the three TV shows that he or she watches the most. Panel C presents the results for the dependent variable as an indicator which takes value 1 whenever the respondent says that the 7:30 pm or the 9:00 pm soap operas broadcasted by Rede Globo are one of the three TV shows that he or she watches the most. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. For estimates in Panel A, CCT optimal bandwidths are 17.15 (first degree poly.) and 21.63 (second degree poly.). For estimates in Panel B, CCT optimal bandwidths 16.91 (first degree poly.) and 23.10 (second degree poly.). For estimates in Panel C, CCT optimal bandwidths are 18.46 (first degree poly.) and 22.02 (second degree poly.). All standard errors are clustered on age measured in months. All regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Our estimates can also be affected by the broad changes an individual faces when turning eighteen. In Brazil, eighteen is the legal age of criminal responsibility. When reaching this age, citizens are allowed to purchase alcohol, to have a driver's license, and also attend the annual military draft (mandatory for male citizens). The difference from these cases to ours is that our treatment is defined by becoming eighteen at a specific month and year, not simply becoming eighteen (in any month). These other changes take place regardless of the specific date of birthday. If CV indeed has an effect on information acquisition, we should expect no change on how voters acquire information for those turning eighteen in different cutoffs.

Figura 1.6.2 – Treatment effect on consumption of Jornal Nacional for different thresholds.



Notes: This figure displays the treatment effect estimates of exposure to abstention penalties on the consumption of Jornal Nacional for different cutoffs using a linear polynomial and a 16 months bandwidth. 95% CI are displayed.

In order to test for these potential confounding factors, we first estimate the treatment effects at different placebo cutoffs for the election month. As it can be seen on Figure 1.6.2, the magnitude of our treatment estimates tends to increase as we get closer to the true cutoff at October, 2014.^{1.6.1} This suggests that our findings are not related to merely becoming eighteen, but to becoming eighteen before October, 2014, the month of the presidential election. The point estimates can be found in Table 1.6.2. No robust effect is found for both first and second degree polynomials and for different bandwidth lengths.

^{1.6.1}In Figure 1.9.8 we show that this is also true for people turning 18 in October in different years.

Tabela 1.6.2 – Placebo tests of exposure to abstention penalties on the consumption of Jornal Nacional around the 18th birthday cutoff.

False thresholds	(1)	(2)	(3)	(4)
October 2013	-0.006 (0.044)	-0.022 (0.039)	0.016 (0.048)	0.017 (0.053)
February 2014	-0.048 (0.046)	-0.048 (0.046)	-0.065 (0.061)	-0.004 (0.063)
June 2014	-0.015 (0.035)	-0.015 (0.035)	-0.046 (0.049)	-0.041 (0.060)
February 2015	0.038 (0.046)	0.050 (0.049)	0.054 (0.068)	-0.022 (0.061)
June 2015	-0.057 (0.031)*	-0.045 (0.033)	-0.052 (0.044)	-0.093 (0.047)*
October 2015	-0.061 (0.053)	-0.076 (0.060)	0.008 (0.058)	0.044 (0.062)
Pol. Degree Bandwidth	First CCT	First ±16 mos.	Second CCT	Second ±16 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. The dependent variable is an indicator variable which takes value 1 whenever the respondent says that Jornal Nacional is one of the three TV shows that he or she watches the most during the week. Each row presents the results for different months used as placebo cutoffs. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. All standard errors are clustered on age measured in months. All regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

1.7 Heterogeneous effects

We have shown CV induces more information consumption. But is such effect homogeneous or heterogeneous across individuals with different evaluations of the current president? Intuitively, those who already made up their minds and have strong opinions about the *status quo* are less likely to have an interest in a new piece of evidence. They believe they already have all the information necessary to cast their vote. Moderate individuals, who are unsure about the platforms the candidates are running on, will

invest in becoming more informed in order to avoid voting for the “wrong” candidate. This argument is similar to the one made by Zaller (1992). In his seminal work, the author argues that individuals with more polarized attitudes are less likely to change their opinions because they already possess information that can be at odds with new pieces of evidence and can, therefore, neutralize them.

The existing evidence supports the existence heterogeneous effects. Shineman (2017) shows how pre-existing attitudes mediate the effect of changes in the cost of voting. The author finds that increasing the cost of participation deters participation from moderate voters more than extremist ones. León (2017) finds that moderate voters are more responsive to reductions in the cost of voting than extreme ones. Other authors find that CV impacts the level of polarization. For example, Leon e Rizzi (2014a) find that CV increases ideological extremism on a left-right spectrum and individuals’ capacity to identify with political parties.

In this section we investigate if moderate individuals are affected differently by CV compared to extremists when it comes to media consumption. Polarization is measured using one of the questions of the Brazilian Media Survey, which asks the respondent to evaluate the government of the incumbent President. Answers can be “Awful”, “Bad”, “Regular”, “Good”, “Excellent” and “Don’t know”. We split the sample in two groups with roughly the same size based on these answers. Those who answer either “Regular” or “Don’t know” are considered to have neutral evaluations. Those who consider the incumbent government “Awful”, “Bad”, “Good” or “Excellent” are considered polarized.

Going back to the model, it is straightforward to notice that the main difference between individuals is their position on the political spectrum, i.e., their ideology.^{1.7.1} In our set-up, the voting cost function is single peaked at zero, which is the center of the ideological distribution. Also, Proposition 2.1 shows that, in order to acquire more information due to CV, individuals must have a relatively large voting cost, $C_j > \delta/p \geq d_j$. Therefore, we would expect those in the center of the distribution to be more inclined to acquire information.

Ideology, as measured in previous studies, is different from the evaluation of the incumbent government. How could the two be connected? During the time of the two waves the incumbent president was Dilma Rouseff, affiliated to PT, Brazil’s largest left-wing political party.^{1.7.2} It is not unreasonable to assume that individuals with positive opinions about the government were mostly to left of the political spectrum, while individuals with

^{1.7.1}Our arguments here follow from individual having a similar, or randomly distributed d_j .

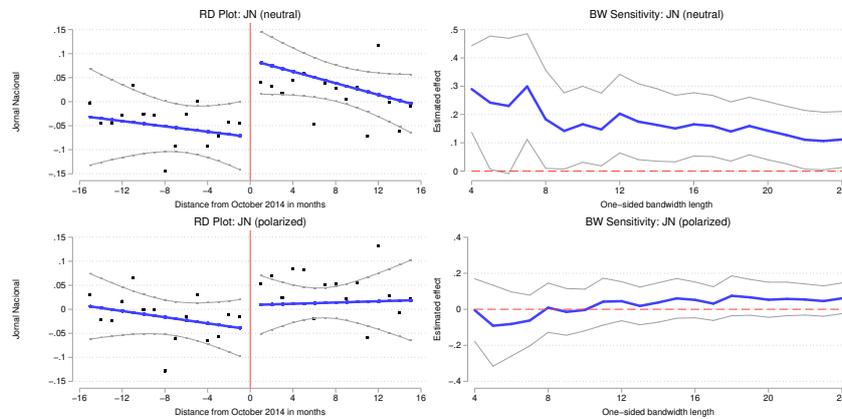
^{1.7.2}Individuals were not only voting for president in the 2014 election. However, the presidential race was the most salient one.

negative opinions were mostly to the right of the political spectrum. Finally, those with moderate opinions should be mostly within the center of the ideological distribution.

Unfortunately, we cannot test this correlation since we do not have any measure of ideology in our database. Nonetheless, previous research on ideology within the Brazilian political context indicates that using government evaluations as a proxy is not inappropriate. [Jr e Lauderdale \(2011\)](#) find that the only relevant division in the Brazilian congress is between government and opposition, not between left and right wing parties. [Mainwaring, Scully et al. \(1995\)](#) points that cleavages such as class, ethnicity, religion, or region never provided a basis for party competition. Current electoral rules also induce the proliferation of different political parties, making it hard for voters to understand where each party stands on different issues, and inducing individualistic behavior from the candidates during their campaigns ([MAINWARING, 1999](#)). These elements induce more personalistic than ideological voting.

We run our main specification, assessing the impact of CV on media consumption, separately for our two subsamples (Neutral and Polarized voters).

Figura 1.7.1 – Evaluation heterogeneous effects.



Notes: This figure displays both the treatment effect of exposure to abstention penalties on consumption of JN and the sensitivity of our estimates to different bandwidth lengths, for those who have neutral or polarized opinions on the incumbent federal government. Neutral individuals are those who don't know how to evaluate the government or classify it as "Regular". Polarized individuals are those who classify the government in either a positive or negative way. Running variable is centered at October 2014, our true cutoff. Specification on the RD plots uses a linear polynomial and a 16 months bandwidth. RD plot displays residuals after accounting for state and time fixed effects. Effects on the BW sensitivity tests are estimated using a linear polynomial. 95% CI are displayed.

In Figure 1.7.1 we see that our theoretical prediction is true for our data. Those who classify the incumbent government as "Regular" or don't know how to evaluate it present a significant increase in the demand for Jornal Nacional, while those who have positive or negative impressions of the administration do not change their behavior. The

results can be seen on Table 1.7.1. On Panel A we display the results for the moderate individuals. For our main specification using a linear polynomial and a sixteen months bandwidth, we find a 0.165 percentage points effect. Since the mean of *Jornal Nacional* consumption for those who are not subject to CV is 0.196 this represents an impact of 84% which can be attributed to exposure to the abstention fines. On Panel B, where the results for more polarized individuals are shown, we find no significant effect of CV on the consumption of *Jornal Nacional*. The magnitude of the coefficients is also substantially smaller when compared to those of Panel A.^{1.7.3}

These results show that those with more moderate opinions have a higher probability to invest in information acquisition than those who are more polarized with respect to the incumbent government. This result is in line with our theoretical prediction that CV would change the consumption of information only of those who are less polarized.^{1.7.4}

^{1.7.3}As a check for the assumption of the model that ideology is fixed, and the fact that one cannot look at heterogeneous effects by endogenous outcomes, we also show on Figure 1.9.9 and Table 1.9.4 that CV has no impact on the evaluation itself.

^{1.7.4}We find no effect for those at the second cutoff who are transitioning back to the voluntary voting regime. For these results see Table 1.9.5

Tabela 1.7.1 – Government evaluation heterogeneous treatment effect of exposure to abstention penalties on the consumption of Jornal Nacional at the 18th birthday cutoff.

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Neutral</i>							
First	0.106 (0.052)**	0.290 (0.078)**	0.183 (0.089)*	0.203 (0.071)***	0.165 (0.057)***	0.143 (0.052)***	0.111 (0.051)**
Second	0.195 (0.063)***	0.591 (0.177)**	0.361 (0.164)**	0.130 (0.125)	0.149 (0.084)*	0.177 (0.069)**	0.184 (0.063)***
Mean D.V. for $M_i = 0$	-	0.154	0.191	0.193	0.196	0.200	0.197
Observations	-	198	472	695	905	1,080	1,217
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.
<i>Panel B: Polarized</i>							
First	0.053 (0.050)	-0.005 (0.089)	0.009 (0.070)	0.045 (0.055)	0.052 (0.051)	0.053 (0.050)	0.061 (0.044)
Second	0.084 (0.066)	-0.206 (0.235)	-0.168 (0.114)	-0.069 (0.102)	-0.017 (0.082)	0.044 (0.076)	0.028 (0.066)
Mean D.V. for $M_i = 0$	-	0.222	0.190	0.192	0.201	0.209	0.219
Observations	-	175	430	681	881	1,071	1,222
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the respondent says that Jornal Nacional is one of the three TV shows that he or she watches the most during the week. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. For estimates in Panel A, CCT optimal bandwidths are 22.05 (first degree poly.) and 24.04 (second degree poly.). For estimates in Panel B, CCT optimal bandwidths are 19.58 (first degree poly.) and 26.48 (second degree poly.). Standard errors are clustered on age measured in months and regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

1.8 Concluding remarks

The interaction between the media and political institutions is a matter of great relevance to the current research agenda in political economy. Although a large literature about how the media affects outcomes such as turnout and civic engagement has been developed in the last decades, little attention has been paid to the opposite relation, i.e., how political institutions change the way citizens consume and relate to the media.

In this paper, we document the effects of compulsory voting on information consumption. According to our model, citizens may acquire information in order to manifest their political preferences in the way that best represents their interests. We find that citizens exposed to abstention penalties for the first time increase their probability to watch Jornal Nacional, Brazil's most trusted newscast, in 11.3 percentage points. We

also find that the impact of the compulsory voting law on the demand for information is stronger for those who have moderate attitudes towards the incumbent government. No impact is found for the consumption of other newscasts or non-journalistic TV shows. Finally, no impact is found at placebo cutoffs.

By employing an internally valid design to assess the effects of compulsory voting on information acquisition, we shed new light on the relation between political institutions and the media. Ultimately, this paper shows that, by increasing turnout, we are also nudging citizens into becoming more informed. Even though we cannot evaluate whether voters are actually learning new information and increasing political knowledge, the results show that voters are interested in acquiring information. The fact that voters are trying to become more informed goes against the rational ignorance hypothesis of [Downs \(1957a\)](#) and in favor of Lijphart's argument that compulsory voting can have consequences beyond simply increasing turnout.

1.9 Appendix

1.9.1 Theory

1.9.1.1 The voting decision

In this subsection of the appendix we present in greater detail the steps involved in the solution of the model. Remember this is a three stage decision model, we begin with the last stage, the voting decision.

Taking into account the citizen decided to turn out to vote, citizen j must choose between L and R . She votes for the candidate that generates the smaller voting cost. That is, according to the following voting rule:

$$v_j^*(y_j, \Omega_j) = \begin{cases} L, & \text{if } C_j(L; y_j, \Omega_j) < C_j(R; y_j, \Omega_j) \\ R, & \text{if } C_j(R; y_j, \Omega_j) < C_j(L; y_j, \Omega_j) \end{cases}. \quad (7)$$

If $C_j(R; y_j, \Omega_j) = C_j(L; y_j, \Omega_j)$, the agent randomizes. The optimal voting decision allow us to define the voting cost as a function of the optimal voting decision $C_j(y_j, \Omega_j) \equiv C_j(v_j^*(y_j, \Omega_j))$

The shape of the voting cost $C_j(v_j^*(y_j, \Omega_j))$, as a function of the ideological position y_j is important for the voter's decision. An example of it is depicted in Figure 1. An informed individual observes the candidates' actual platforms and decides to vote for

L if $y_j < (y_L + y_R)/2$ and votes for R if $y_j > (y_L + y_R)/2$. An uninformed voter can make voting mistakes. Yet, a voter with ideology such that $|y_j| \geq 1/2$ will never be mistaken about whom to vote for, so his voting cost is zero. This is because the candidates platforms are restricted to $[-1, 0]$ and $[0, 1]$. Consequently, the midpoint $(y_L + y_R)/2 \in [-1/2, 1/2]$. The voting cost is only positive for uninformed voters with ideological positions within $(-1/2, 1/2)$. But voters with ideological positions near $-1/2$ (to the right) will have a low probability of making voting mistakes. That chance is greater for voters closer to zero. At zero, the voting cost is the highest since this individual has no ideological bias and has no "guess" on who to vote for. As we move to voters on the right side of the ideological distribution, the voting cost decreases until it reaches zero when $y_j = 1/2$.

Next, we proceed to the turnout decision. Citizen j chooses whether or not to turn out in the day of the election. The benefit of turning out to vote is given by d_j , regardless of chosen candidate. The cost of turning out is given by $C_j(v_j^*(y_j, \Omega_j))$. The citizen decides to turn out to vote according to the following cutoff rule:

$$t^*(y_j, \Omega_j) = \begin{cases} 1, & \text{if } C_j(v_j^*(y_j, \Omega_j)) - d_j < 0, \\ 0 & \text{if } C_j(v_j^*(y_j, \Omega_j)) - d_j \geq 0. \end{cases} \quad (8)$$

If the net benefit of casting a ballot (i.e. the opportunity cost of abstention) is positive, citizen j will turnout, otherwise she stays at home. Notice that an informed citizen always turns out since his cost is zero when voting for the favorite candidate.

Finally, we turn to the first stage. The citizen must decide whether or not to acquire information. She compares the costs and benefits from acquiring information, anticipating the optimal turn out and voting decisions. Let us define $C_j \equiv C_j(v_j^*(y_j, \Omega_j = \{F(y_L, y_R)\}))$ as the voting cost after failing to acquire information. There are two relevant scenarios, one where the civic duty $d_j \geq C_j$, and the other where $d_j < C_j$.

For $d_j < C_j$, the voting cost of an uninformed voter is greater than the civic duty benefit of attending the ballot. Without information, the citizen will not turn out to vote. In such case, she can acquire information in order to reduce her voting cost. She does so whenever the expected benefit of turning out is greater than the cost of acquiring information, that is, whenever $pd_j - \delta \geq 0$. We can also write this inequality as $d_j \geq \delta/p$. In case $d_j < \delta/p$, the citizen does not invest in information acquisition.

If, on the other hand, $d_j \geq C_j$, then the voter always turns out to vote. She may still invest to acquire information, since it reduces the voting cost. The expected payoff she gets from investing in information acquisition is given by $d_j - (1 - p)C_j - \delta$. The payoff from not investing to acquiring information, in turn, is given by $d_j - C_j$. Comparing the two, we find that the citizen acquires information whenever $C_j \geq \delta/p$. Combining these

two cases, gives the following rule for information acquisition:

$$i^*(d_j, \delta, p) = \begin{cases} 1, & \text{if } d_j, C_j \geq \frac{\delta}{p}. \\ 0, & \text{otherwise.} \end{cases} \quad (9)$$

1.9.1.2 Proofs

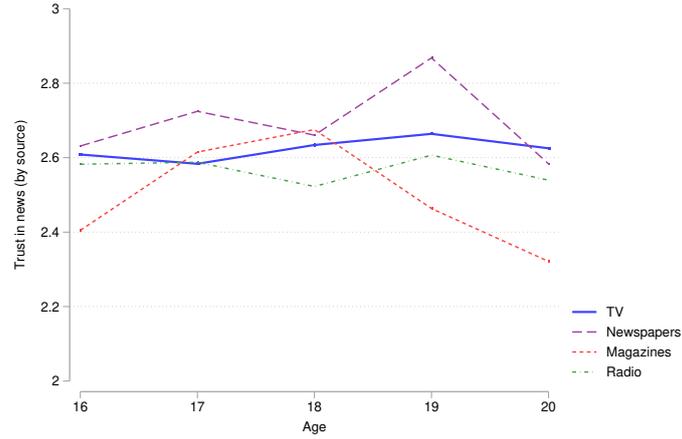
Proof of Proposition 2.1. To prove item 1 we notice that citizens with $d_j \geq C_j$ already turn out to vote. So, compulsory voting is irrelevant for their decision since $\hat{d}_j > d_j > C_j$. Citizens with $C_j > d_j$ either do not vote ($\delta/p > d_j$) or vote only if get information ($d_j \geq \delta/p$). In this last case, the citizen is, without compulsory voting, acquiring information and turning out if this acquisition is successful. The increase in the induced benefit \hat{d}_j may change her decision into turning out irrespective of the result of information acquisition. That is the case whenever $\hat{d}_j \geq C_j > d_j$. Turnout increases in expected terms.

In the second case, citizens do not vote without compulsory voting. As a result of the higher induced benefit from turn out, they may change their decision and vote irrespective of information ($\hat{d}_j \geq C_j > d_j$) or they may gather information and vote if acquisition is successful (when $C_j > \hat{d}_j \geq \delta/p > d_j$). In both cases the range of parameters that lead to turn out ($t_j^* = 1$) increases.

To prove item 2, remember that when $d_j, C_j \geq \delta/p$ the citizens acquire information. In such case their decision is unaffected by compulsion. Since compulsion does not affect the cost of voting, the only relevant situation is of those citizens with $C_j > \delta/p > d_j$ where also $\hat{d}_j \geq \delta/p$. Those are the voters which are induced to acquire information from compulsory voting laws. All other citizens' decision remain unaltered. \square

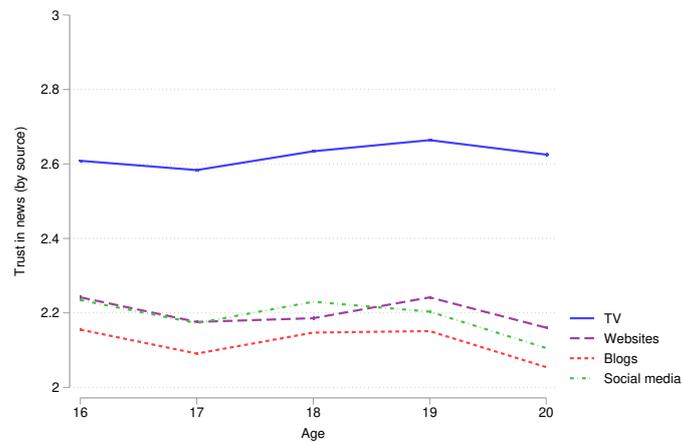
1.9.2 Graphs and tables

Figura 1.9.1 – Trust in different sources of information for young individuals (TV versus traditional media).



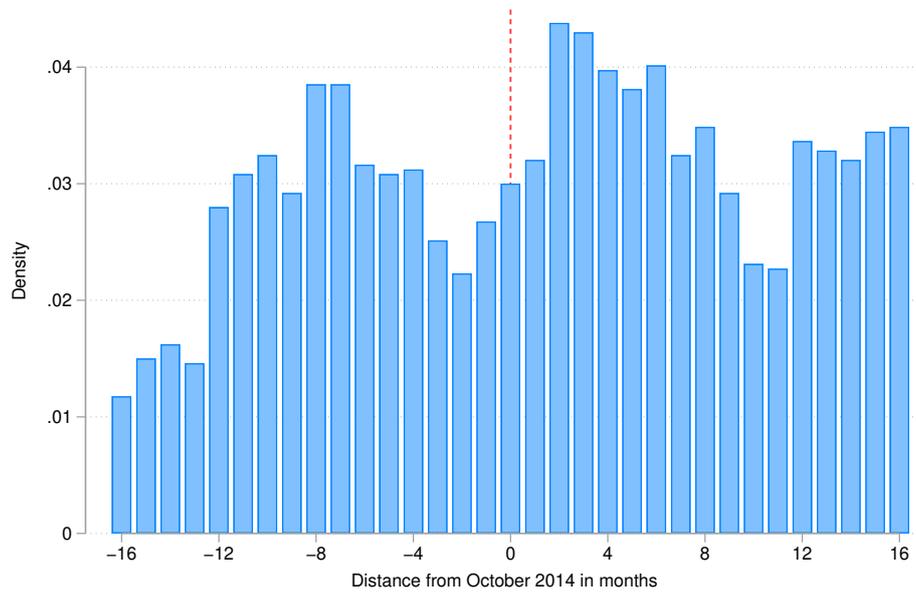
Notes: Trust level taken from the answer to the question “How much do you trust news from a given media outlet?” where answers are coded in the following way: 1 - Never, 2 - Few times, 3 - Many times, 4 - Always. Data from the Brazilian Media Survey 2015.

Figura 1.9.2 – Trust in different sources of information for young individuals (TV versus online media).



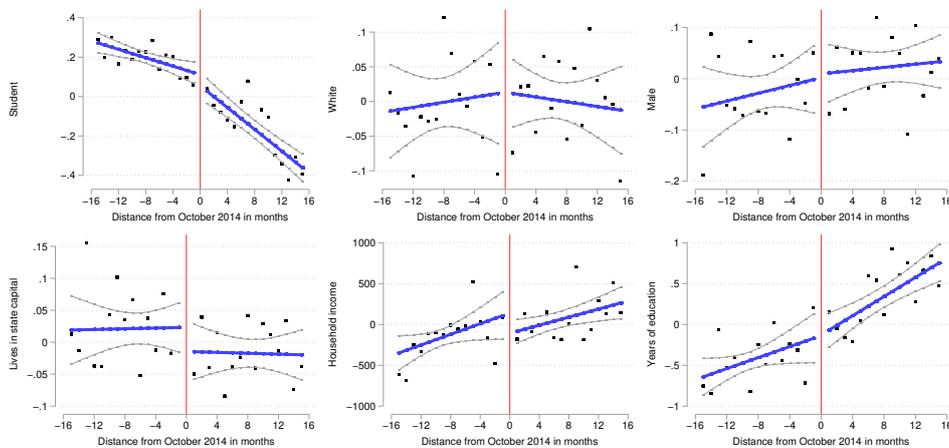
Notes: Trust level taken from the answer to the question “How much do you trust news from a given media outlet?” where answers are coded in the following way: 1 - Never, 2 - Few times, 3 - Many times, 4 - Always. Data from the Brazilian Media Survey 2015.

Figura 1.9.3 – Histogram of the running variable.



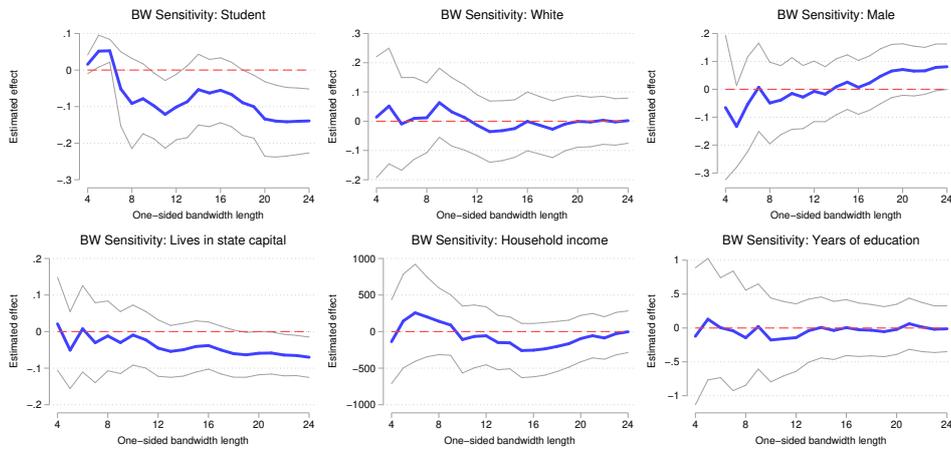
Notes: Running variable is the age of the individuals measured in months centered at October 2014.

Figura 1.9.4 – Impact on baseline covariates: RD plots.



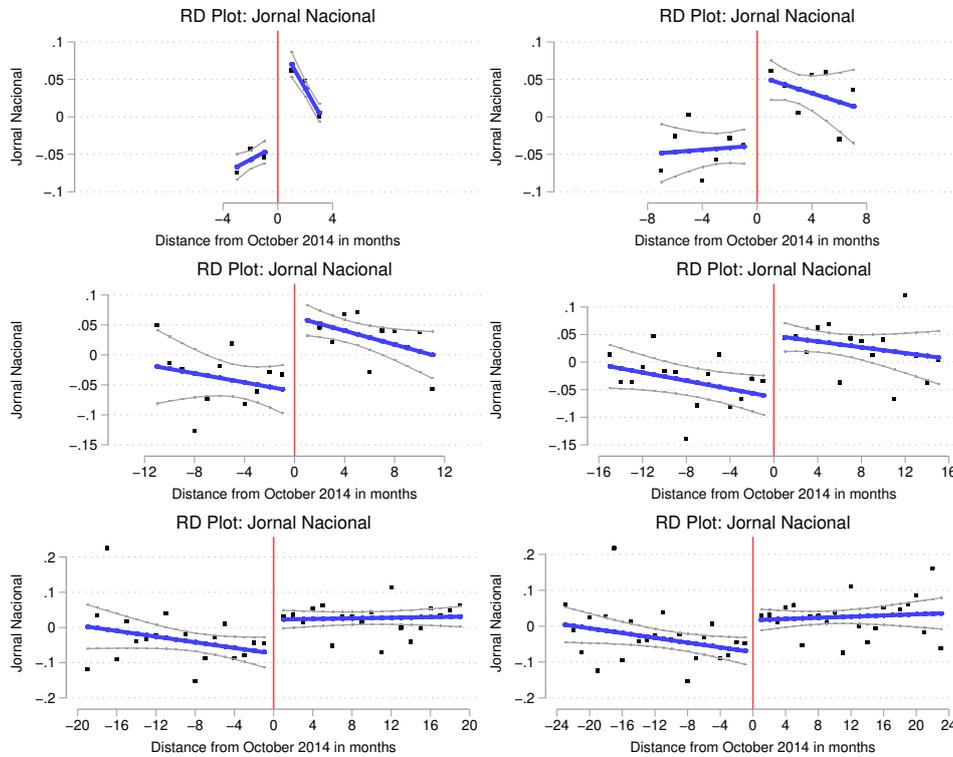
Notes: This figure displays the treatment effect of exposure to abstention penalties on baseline covariates. Running variable is centered at October 2014, our true cutoff. Specification on the RD plots use a linear polynomial and a 16 months bandwidth. RD plots display residuals after accounting for state and time fixed effects. 95% CI are displayed.

Figura 1.9.5 – Impact on baseline covariates: bandwidth sensitivity.



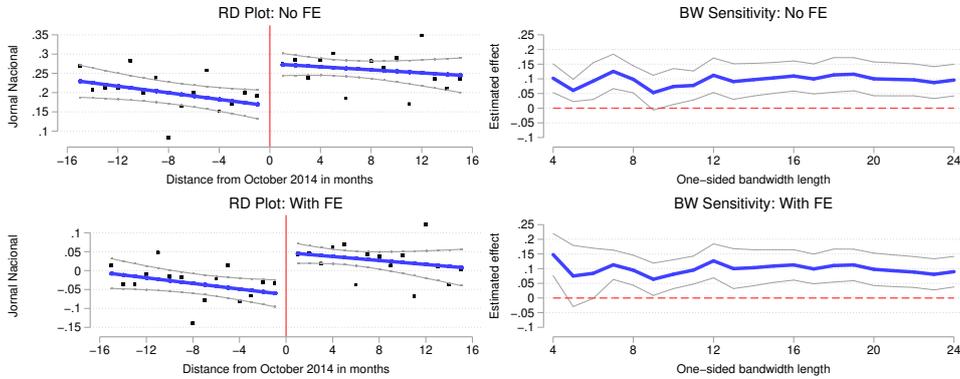
Notes: This figure displays the sensitivity of the treatment effect of exposure to abstention penalties on baseline covariates. Running variable is centered at October 2014, our true cutoff. Estimates are obtained using a linear polynomial. All estimates are obtained after accounting for state and time fixed effects. 95% CI are displayed.

Figura 1.9.6 – RD plots for different bandwidths.



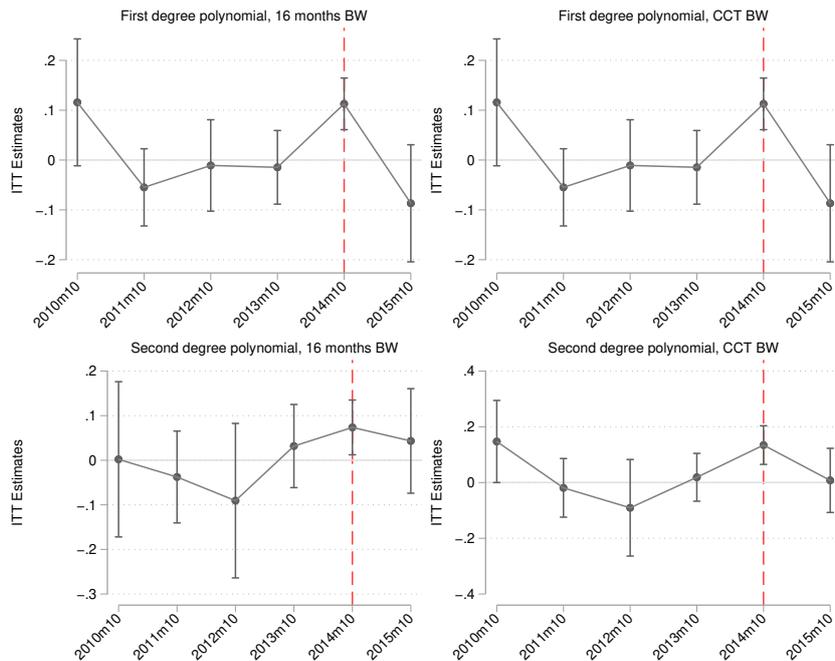
Notes: This figure displays both the treatment effect of exposure to abstention penalties on the consumption of JN and the sensitivity of our estimates to different bandwidth lengths. Running variable is centered at October 2014, our true cutoff. Specification on the RD plot uses a linear polynomial and a 16 months bandwidth. RD plot displays residuals after accounting for state and time fixed effects. Effects on the BW sensitivity test are estimated using a linear polynomial. 95% CI are displayed.

Figura 1.9.7 – Impact of exposure to abstinence penalties on the consumption of Jornal Nacional (without fixed-effects).



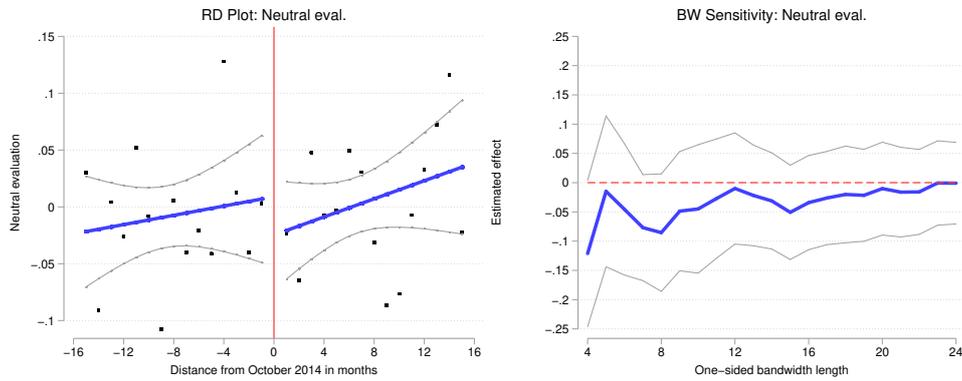
Notes: This figure displays both the treatment effect of exposure to abstinence penalties on the consumption of JN and the sensitivity of our estimates to different bandwidth lengths, without including state or year fixed effects. Running variable is centered at October 2014, our true cutoff. Specification on the RD plot uses a linear polynomial and a 16 months bandwidth. RD plot displays residuals after accounting for state and time fixed effects. Effects on the BW sensitivity test are estimated using a linear polynomial. 95% CI are displayed.

Figura 1.9.8 – Treatment effect on consumption of Jornal Nacional for different thresholds (October on different years).



Notes: This figure displays the treatment effect estimates of exposure to abstinence penalties on the consumption of Jornal Nacional for different cutoffs using a linear polynomial and a 16 months bandwidth. 95% CI are displayed.

Figura 1.9.9 – Impact of exposure to abstention penalties on government evaluation.



Notes: This figure displays both the treatment effect of exposure to abstention penalties on government evaluation and the sensitivity of our estimates to different bandwidth lengths. "Neutral evaluation" is an indicator which takes value 1 whenever the respondent does not know how to evaluate the government or classifies it as "Regular", and 0 whenever the respondent classifies the government as "Awful", "Bad", "Good" or "Excellent". Running variable is centered at October 2014, our true cutoff. Specification on the RD plots uses a linear polynomial and a 16 months bandwidth. RD plot displays residuals after accounting for state and time fixed effects. Effects on the BW sensitivity tests are estimated using a linear polynomial. 95% CI are displayed.

Tabela 1.9.1 – Covariate balance around the 18th birthday cutoff: optimal bandwidth estimates.

Dependent variables	(1)	Optimal BW	Mean D.V. for $M_i = 0$	Observations
<i>Panel A: First degree poly.</i>				
Student	-0.054 (0.049)	13.40	0.694	2,022
White	-0.014 (0.050)	16.83	0.413	2,369
Male	0.009 (0.051)	13.35	0.491	2,036
Capital	-0.045 (0.039)	11.26	0.245	1,767
Household income	-149.730 (189.655)	12.27	1857	1,737
Years of education	0.006 (0.210)	15.15	9.630	2,277
<i>Panel B: Second degree poly.</i>				
Student	0.010 (0.054)	19.94	0.683	2,703
White	-0.003 (0.064)	22.54	0.417	2,978
Male	-0.036 (0.063)	20.94	0.503	2,818
Capital	-0.002 (0.046)	17.35	0.239	2,513
Household income	16.492 (249.386)	17.71	1818	2,273
Years of education	-0.083 (0.315)	21.82	9.571	2,918

Notes: Each figure in the table is from a separate local regression with a CCT optimal bandwidth around the cutoff. Household income is calculated based on the mid-points of income ranges provided in the survey. Minimum wages (mw) on 2013 and 2014 were R\$ 678.00 and R\$ 700.00 respectively. Income ranges were 0 to 1 mw , 1 to 2 mw , 2 to 5 mw , 5 to 10 mw , 10 to 20 mw , and 20 or more mw , which we cap at 20 mw . Years of education calculated based on the highest completed grade reported on the survey. Incomplete grades are calculated as the mid-point, in years, between two completed grades. All other variables are indicators for demographic characteristics. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. All standard errors are clustered on age measured in months and all regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Tabela 1.9.2 – Treatment effect of exposure to abstention penalties on the consumption of Jornal Nacional at the 18th birthday cutoff (without fixed-effects).

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First	0.110 (0.026)***	0.102 (0.025)***	0.098 (0.024)***	0.112 (0.030)***	0.110 (0.026)***	0.100 (0.029)***	0.096 (0.028)***
Second	0.121 (0.032)***	0.063 (0.000)***	0.104 (0.050)*	0.034 (0.048)	0.064 (0.032)*	0.104 (0.034)***	0.103 (0.032)***
Mean D.V. for $M_i = 0$	-	0.187	0.191	0.192	0.198	0.205	0.210
Observations	-	374	903	1,377	1,788	2,155	2,444
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the respondent says that Jornal Nacional is one of the three TV shows that he or she watches the most during the week. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. CCT optimal bandwidths are 15.96 (first degree poly.) and 22.30 (second degree poly.). All standard errors are clustered on age measured in months. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Tabela 1.9.3 – Treatment effect of exposure to abstention penalties on the consumption of Jornal Nacional at the 70th birthday cutoff.

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First	0.156 (0.061)**	0.301 (0.457)	0.197 (0.172)	0.064 (0.141)	0.110 (0.124)	0.070 (0.105)	0.052 (0.095)
Second	0.118 (0.083)	0.538 (0.501)	0.127 (0.330)	0.183 (0.228)	0.102 (0.197)	0.183 (0.158)	0.150 (0.147)
Mean D.V. for $M_i = 0$	-	0.480	0.478	0.445	0.464	0.448	0.424
Observations	-	68	180	283	378	482	603
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the respondent says that Jornal Nacional is one of the three TV shows that he or she watches the most during the week. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. CCT optimal bandwidths are 48.71 (first degree poly.) and 58.25 (second degree poly.). All standard errors are clustered on age measured in months. All regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Tabela 1.9.4 – Treatment effect of exposure to abstention penalties on having a neutral evaluation of the government at the 18th birthday cutoff.

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First	-0.026 (0.041)	-0.121 (0.064)	-0.085 (0.051)	-0.010 (0.049)	-0.034 (0.041)	-0.010 (0.041)	-0.001 (0.036)
Second	-0.057 (0.055)	-0.048 (0.084)	-0.023 (0.082)	-0.113 (0.058)*	-0.039 (0.057)	-0.057 (0.055)	-0.049 (0.051)
Mean D.V. for $M_i = 0$	-	0.490	0.497	0.497	0.494	0.491	0.493
Observations		475	1,170	1,764	2,272	2,713	3,071
Bandwidth	CCT	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the respondent does not know how to evaluate the government or classifies it as "Regular", and 0 whenever the respondent classifies the government as "Awful", "Bad", "Good" or "Excellent". Sample consists of literate citizens who responded the Brazilian Media Survey at the years 2013 and 2014. CCT optimal bandwidths are 16.43 (first degree poly.) and 19.14 (second degree poly.). All standard errors are clustered on age measured in months. All regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Tabela 1.9.5 – Government evaluation heterogeneous treatment effect of exposure to abstention penalties on the consumption of Jornal Nacional at the 70th birthday cutoff.

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Neutral</i>						
First	-0.737 (0.378)	0.031 (0.340)	0.130 (0.245)	0.165 (0.197)	0.207 (0.175)	0.159 (0.145)
Second	-1.534 (2.040)	-0.784 (0.602)	-0.153 (0.453)	0.045 (0.350)	0.089 (0.322)	0.228 (0.284)
Mean D.V. for $M_i = 0$	0.529	0.458	0.438	0.431	0.431	0.431
Observations	31	81	128	166	218	271
Bandwidth	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.
<i>Panel B: Polarized</i>						
First	0.916 (0.525)	0.093 (0.258)	-0.017 (0.168)	0.002 (0.149)	-0.054 (0.131)	-0.035 (0.116)
Second	2.118 (1.218)	0.544 (0.455)	0.317 (0.225)	0.084 (0.176)	0.219 (0.194)	0.066 (0.179)
Mean D.V. for $M_i = 0$	0.447	0.501	0.470	0.514	0.480	0.426
Observations	37	97	152	207	258	326
Bandwidth	±4 mos.	±8 mos.	±12 mos.	±16 mos.	±20 mos.	±24 mos.

Notes: Each figure in the table is from a separate local regression with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the respondent says that Jornal Nacional is one of the three TV shows that he or she watches the most during the week. Sample consists of literate citizens who responded the Brazilian Media Survey at the years of 2013 and 2014. All standard errors are clustered on age measured in months. All regressions include state and survey year fixed-effects. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2 Compulsory Voting and Persistence in Turnout

2.1 Introduction

Electoral participation is considered a measure of the robustness of democratic institutions and one of the pillars of political equality (LIJPHART, 1997). The current levels of turnout, however, vary greatly between different countries. In New Zealand, Portugal and Albania turnout rates are as high as 85%, while in the United States, Ghana and Colombia it plateaus at 50%.^{2.1.1} Understanding what drives individuals to vote is crucial to explain and design policies that increase turnout as a way to increase the legitimacy of election results.^{2.1.2} This leads us to one of the main questions in political science research: why people vote? Since the seminal work of Downs (1957b), applied social scientists have been trying to find an answer to a puzzle that became known as the “paradox of voting”, i.e., what drives the large turnout rates we see, even though the probability of an individual being pivotal to the elections is very small. The usual hypotheses on voter rationality imply that turnout converges to zero as the electorate grows (PALFREY; ROSENTHAL, 1985; MYERSON, 1988). In the absence of some collateral benefit related to voting, it is not possible to explain the turnout rates we see in large democracies.

One of the most well succeeded attempts to explain this phenomenon is the “calculus of voting” model, developed by Riker e Ordeshook (1968). According to this model, individuals obtain a positive utility when they fulfill their civic duty of voting. This warm-glow benefit is compared to the cost associated to turning out (e.g. transportation or the opportunity cost). If benefit minus cost is positive, the individual turns out, otherwise, she abstains. Despite explaining in a very simple way the patterns of voter turnout we observe in large elections, this model does not provide an explanation for dynamic individual patterns of variation in turnout in different countries, as documented by Franklin (2004). Recent developments in the literature try to overcome this limitation through behavioral models, as in Bendor, Diermeier e Ting (2003) and Fowler (2006). In these models, the individual decides whether or not to vote based on their previous turnout decisions and the results from these elections. As a consequence, voting becomes habit forming in order to explain the observed patterns of persistence in turnout. Understanding the elements behind this process is, therefore, crucial in order to design optimal policies that intend to increase turnout in large democracies.

^{2.1.1}See Pintor, Gratschew e Sullivan (2012).

^{2.1.2}See Jr (1986).

The empirical challenge in this case consist in identifying whether the decision to vote in election $t - 1$ affects turnout in election t . Identifying turnout in this case becomes problematic because we need to separate what is habit formation from what is mere autocorrelation between both decisions. Voter with a high level o civic duty will vote with a larger probability than those with low civic duty, for example. The ideal experiment in this case would be to randomize shocks in their calculus of voting (i.e. to exogenously affect their costs or benefits of turning out) in order to measure the impact of these changes on their future turnout.

In this paper we analyze how individuals adjust their current decisions when they receive shocks in their calculus of voting in the past. The current electoral rules in Brazil provide an excellent opportunity to test this. The electoral law determines that every literate citizen above eighteen and younger than seventy years old by the day of the elections is subject to a number of penalties in case of failing to attend an official polling station and casting a ballot.^{2.1.3} This age-based rule determines who's under the compulsory voting (henceforth CV) regime and who's not, enabling us to measure the causal effect of exposure to the law in $t - 1$ on turnout in t . Our empirical strategy employs regression discontinuity design (RDD) on a Brazil's full voter file from 2010 until 2018. Covering each round of 5 elections that took place during this period, our data allow us to follow each voter turnout pattern over time. So far, this is the most complete voter behavior dataset to be used for Brazil in the literature.

We find three main results. First, there is a strong first-stage effect, i.e., the impact of contemporaneous exposure to abstention penalties on turnout. This confirms the previous findings of [Cepaluni e Hidalgo \(2016b\)](#) and proves that, indeed, compulsory voting plays a significant role in increasing turnout. This finding holds for both older and younger voters, showing that exposure to the abstention fines is a salient element in voters decision regardless of whether individuals are transitioning from voluntary to compulsory or from compulsory to voluntary voting. Second, we find a sizeable persistence effect of exposure to these penalties in the previous election on turnout in the current election. This reduced-form result is valid only for older voters, something that suggests the existence of heterogeneity in the dynamics of turnout between the two transitions. Finally, in the third and most important result, we find that voting in the past election causes voting in the present election. Once again, this result holds only for older voters. All of these results are robust to different combinations of polynomial degrees and bandwidth lengths.

Understanding how habit is formed in the context of elections is crucial in order to

^{2.1.3}Literate citizens are eligible to vote when they become 16 years old, but are only subject to abstention fines (i.e. under compulsory voting) when they reach the age of 18.

understand not only the current levels of turnout, but also their pattern of persistence over time. Franklin (2004) argues that this stability over time comes from voters' experiences in their first elections, in the beginning of their adult lives. According to the author, those who cannot find any reason to vote in their first elections tend not to turn out in the following years. This argument is empirically corroborated by Putnam (2000) and (PLUTZER, 2002) for the United States, and by Blais, Gidengil e Nevitte (2004) for Canada. It follows from this that compulsory voting laws can start a virtuous cycle of turnout. Once exposed to an election due to an exogenous change in their calculus of voting, the individual has a greater probability to turnout in their next election. The idea that adherence to norms is persistence is hardly novel. Max Weber understood that, once agents internalize a norm, they would follow it *“as the result of unreflective habituation to a regularity of life that has engraved itself as a custom.”* (WEBER, 1978)

The idea of making voting compulsory came to prominence in the last decades, as a solution to the problem of unequal participation in modern democracies (LIJPHART, 1997). On March 2015, the US president Barack Obama publicly endorsed this policy: *“In Australia, and some other countries, there’s mandatory voting. It would be transformative if everybody voted... it would completely change the political map in this country, because the people who tend not to vote are young; they’re lower income; they’re skewed more heavily towards immigrant groups and minority groups.”* Exposing these groups to abstention penalties could not only change the results of the elections by shifting the chosen candidate to one closer to the median voter of the population, but could also bring more legitimacy to the result. Having this in mind, Lodge e Birch (2012) suggests an alternative to the traditional CV framework. According to the authors, the norm could be internalized by making voting mandatory only in the first election, thus, inducing turnout in subsequent periods. In order to design an optimal compulsory voting policy, it is necessary to understand how habit formation changes the calculus of voting over time.

The first direct evidence on habit formation in voter turnout comes from Gerber, Green e Shachar (2003). In this paper, the authors apply a randomized controlled trial on 25000 voters before the 1998 US elections. Individuals in the treatment group were induced to vote through in-person interactions or mail messages, while those in the control group didn't. The authors find that the turnout in the 1998 elections increased significantly and that such effect persisted on the local elections of 1999. Meredith (2009) takes advantage of discontinuities on age restrictions to vote in the US as an exogenous source of variation on young voters' first electoral experience. The author finds a positive effect of the probability to vote on the current election on the probability to vote on the next election. This effect persists for many subsequent elections, bringing additional evidence of the persistence of

habit formation on voting.

In Switzerland, [Bechtel, Hangartner e Schmid \(2015\)](#) find that CV has persistent effects on turnout in the long-run. Between 1925 and 1948, in the *canton* of Vaud, people who failed to turnout in federal referendums were subject to abstention penalties. The enforcement was applied by the local police, who would go to the residence of the absentee voter and charge the fine in person, adding a social “shaming cost” on top of the pecuniary one. Because the law applied only to one kind of voting, the authors are able to measure its effect not only on federal referendums, but its spillover effect on other kind of elections. The temporal variation and the particular design of the law allow the researchers to test if the norm is internalized by the citizens. The authors find that an increase in 30% on federal referendums, that fades out after the law ends. Little spillover effects on other forms of political participation are found. Their evidence suggests that habit formation due to compulsory voting is unlikely to happen.

In [Fujiwara, Meng e Vogl \(2016\)](#), the authors use transitory shocks on the cost of turning out in the form of rainfall on election day in order to verify if there is persistence in political participation. Using county-level data on presidential elections from 1952 to 2012, they find evidence on the causal effect of rainfall on election on both current and future turnout, four years later. Using an expressive utility model to understand the mechanisms behind this, they argue that this impact is due a change in the consumption value of voting, not a change in the beliefs or the cost of turning out, as suggested by [Pollak \(1970\)](#) and [Becker e Murphy \(1988\)](#). On a related work, [Coppock e Green \(2016\)](#) show the long-term consequences of random and quasi-random nudges to vote. They confirm that there is persistence of voter turnout and show that the degree of persistence varies by electoral context and by the individual characteristics of those who comply with an initial incentive to vote.

Our research expands previous analyses on the determinants of habit formation in politics. First, we provide the first comprehensive evidence of such effect as a consequence of compulsory voting in the context of a middle-income country. Second, the extent of our dataset and the way the compulsory voting law is designed in Brazil allows us to verify whether there is heterogeneity in the downstream effects of compulsory voting. We find that turnout at the individual level is path-dependent due to the existence of abstention penalties. Moreover, we find that this effect is asymmetric for younger and older voters. Transitioning from compulsory to voluntary voting after many years of exposure to the abstention penalties leads to a sizeable persistence in turnout. We do not find the same result for younger voters.

The remainder of the paper is organized as follows. Section 2.2 provides basic

background information on the compulsory voting law in Brazil. Section 2.3 describes the data and our empirical strategy. The results are presented in Section 2.4, followed by robustness checks and placebo tests in Section 2.5. Section 2.6 concludes the paper.

2.2 Institutional background

Brazil's current compulsory voting legislation started with the promulgation of the Brazilian Constitution of 1988. It states that every literate citizen older than 18 and younger than 70 years old must turn out on election day or justify their absence in a special court. Citizens older than 16 and younger than 18 are allowed to register to vote, but do not face any consequence if they don't.^{2.2.1} If an individual fails to justify her absence, she must pay a small fine of R\$ 3.00 (roughly 0.70 dollars), which can be multiplied tenfold according to the decision of the judge handling the case. Those who fail to justify absence three times are also subject to a number of sanctions, such as not being allowed to issue a new ID or passport, being ineligible for cash transfer programs, credit by financial institutions maintained by the government, public jobs and public education.^{2.2.2}

There are also reasons to believe that other non-fiduciary elements may induce turnout. First, citizens are allocated to ballots according to their postal code. This means that people who live in the same neighborhood will vote near their home in the same place, on the same day. Recent studies stress the fact that individuals care about being seen as participant citizens, and this concern about their social image constitutes a driving force behind voter turnout (GERBER; GREEN; LARIMER, 2008; FUNK, 2010; DELLAVIGNA et al., 2016). In light of these findings, an abstention in the day of an election may be socially costly for citizens who worry about how they will be seen in their local community. Furthermore, the first and second round of the elections are held on the first and last Sunday of October, respectively. Since an individual who failed to justify his absence must pay the fine on a regional electoral court during business hours, it's likely that the opportunity cost of attending the ballots is lower than the one of having to deal with the bureaucracy on weekdays. Also, unlike the ballot, the court is not necessarily located near from where the person who abstained lives.

The abstention rates in Brazil are also low. For example, in the 2012 elections, the

^{2.2.1}According to the Superior Electoral Court of Brazil (TSE), only one-fourth of individuals under eighteen years registered to vote in the 2014 elections.

^{2.2.2}For more information, see <http://www.tse.jus.br/eleitor/servicos/justificativa-eleitoral> (in Portuguese).

average abstention in Brazilian municipalities was 14%, with 9% invalid votes cast for mayor and 5% invalid votes for city council. This leads us to believe that the law is indeed enforced and turnout is high in the presence of compulsory voting. Recent research by [Cepaluni e Hidalgo \(2016b\)](#) using the full Brazilian voter file comprising of 140 million individuals shows that Brazilian compulsory voting law has a sizable causal impact on turnout, in line with the findings of [Leon e Rizzi \(2014b\)](#).

2.3 Data and research design

2.3.1 Data

Our data consists of the file of every Brazilian citizen with a voter registration. Different from [Cepaluni e Hidalgo \(2016b\)](#), that uses this same data but for one municipal election, we use information from the five elections that took place between 2010 and 2018. For every voter in every municipality, we have information on gender, date of birth, level of education, marital status and whether the citizen turned out or not to vote in each round of the elections. Voters in the dataset were given an anonymous ID by Brazil’s Supreme Electoral Court (TSE), allowing us to follow every individual over time. The voter file for each election has more than 140 million observations, giving us a total of approximately 700 million observations. This is, to date, the largest dataset regarding voter participation in Brazil to be used. Due to computational concerns, in this version we restrict our analysis to a random sample of 10% of the data.

Tabela 2.3.1 – Summary statistics.

	Obs	Mean	Std. Dev.	Min	Median	Max
Turnout	72419481	0.80	0.40	0	1	1
Primary edu.	72348499	0.53	0.50	0	1	1
Secondary edu.	72348499	0.22	0.41	0	0	1
Male	72419481	0.48	0.50	0	0	1
Married	72419481	0.27	0.45	0	0	1

Notes: *Turnout* is an indicator which takes value 1 whenever the individual turns out to vote in the first round of an election. The variable *Primary education* is an indicator which takes values 1 if the individual has completed the primary education. The variable *Secondary education* is an indicator which takes values 1 if the individual has completed the secondary education. *Male* and *Married* are indicators which take value 1 whenever the individual is either male or married. Since the compulsory voting law does not apply to illiterate citizens, this group is removed from our sample. Sample consists of a random 10% draw of the complete Brazilian voter file for the years of 2010, 2012, 2014, 2016 and 2018.

As we can see from Table A1, the average turnout in Brazil is high, specially in comparison to other countries as mentioned in the introduction. The educational pattern

among registered voters shows that just about half of the electorate has completed the primary education, and only 28% secondary education. About half of the population is male and 32% is married. We stress that both the education and marital status indicators must be taken with a grain of salt, since voters do not have to update them regularly. Voter registrations do not have an expiration date and, therefore, this information can stay obsolete for a long period of time.

2.3.2 Identification strategy

Our aim is to verify if there's persistence in turnout under compulsory voting. Similar to [Cepaluni e Hidalgo \(2016b\)](#), we compare individuals whose ages are in the neighborhood of the threshold that determines a change in their calculus of voting. In our case, this change is the exposure to abstention fines. Using a sharp regression discontinuity design, we first estimate the following equations:

$$\text{First stage: } V_{i,t-1} = \eta_{t-1}^{FS} + \theta_{t-1}^{FS} C_{i,t-1} + f(A_{i,t-1} - k) + \epsilon_{i,t-1}^{FS}, \quad (2.3.1)$$

$$\text{Reduced form: } V_{i,t} = \eta_{t-1}^{RF} + \theta_{t-1}^{RF} C_{i,t-1} + f(A_{i,t-1} - k) + \epsilon_{i,t-1}^{RF}, \quad (2.3.2)$$

where $C_{i,t-1}$ is a dummy which indicates whether individual i is exposed to compulsory voting in the previous elections ($C_{i,t-1} = 1$) or not ($C_{i,t-1} = 0$). $A_{i,t-1}$ is the age of individual i in days in election $t - 1$. According to the electoral law in Brazil, since those between 18 and 70 years old before the day of the election are under CV, we define $k = 70$ as the age cutoff in days for those who become 70 in $t - 1$. It follows that $C_{i,t-1} = 1(A_{i,t-1} < k)$. Moreover, $f(\cdot)$ is a continuous function of our running variable centered at the cutoff value. The parameters θ^{FS} and θ^{RF} are the causal effects of $C_{i,t-1}$ on $V_{i,t-1}$ and $V_{i,t}$, respectively. We estimate our equation assuming that $f(\cdot)$ is a flexible polynomial on both sides of the threshold. Following [Gelman e Imbens \(2014\)](#) we estimate only first and second-degree polynomials for different multiples of the optimal bandwidth calculated using the Calonico-Cattaneo-Titiunik (CCT) procedure from [Calonico, Cattaneo e Titiunik \(2014\)](#).

The causal estimate from Equation 2.3.1, θ^{FS} , provides the "first stage" impact of compulsory voting on turnout. In our set-up, this amounts to the contemporaneous impact of exposure to CV in $t - 1$ on turnout in $t - 1$. Ultimately, this estimate provides the answer to the following question: "Does compulsory voting leads to an increase in voter turnout in the current election?" This is different from the "reduced form" impact provided by θ^{RF} in Equation 2.3.2, which gives us the impact of exposure to CV in $t - 1$ on turnout

in t , i.e., the answer to the question "Does compulsory voting leads to an increase in voter turnout in the next election?"

Nevertheless, our data allows us to go one step further, and answer the following question "Does voting in this election causes voting in the next one?" In this formulation, we are trying to measure the effect of previous on current turnout. In order to do so, we perform an instrumental variables regression using the age discontinuity as an instrument for turnout. In other words, the institutional design enables us to use employ a fuzzy regression discontinuity design, as can be seen in the following equation:

$$\mathbf{IV\ regression:} \quad V_{i,t} = \alpha_t + \tau_t \tilde{V}_{i,t-1} + f(A_{i,t-1} - k) + \xi_{i,t}, \quad (2.3.3)$$

where $\tilde{V}_{i,t-1}$ represents the fitted values from Equation 2.3.1. The coefficient τ_t in this specification measures whether or not the act of turning out is persistent over time.

In order to interpret our parameters of interest as causal, we must first make sure that we satisfy two key assumptions for identification in RD setups: smoothness of the forcing variable around the threshold and smoothness of baseline covariates. As pointed by [Nyhan, Skovron e Titiunik \(2017\)](#), studies which use voter files suffer from a potential bias from missing information about the whole eligible population, not only for registered voters. This issue is specially salient in our case for the younger voters, since they are only obliged to register when they reach the age of 18. From 16 to 18 years old, we can only observe individuals who voluntarily registered as a voter, a group that, on average, is more politically engaged than those that only registered when subject to compulsory voting. In order to deal with this and approximate our dataset to the whole population, not only to registered voters, we generate observations for voters who were eligible to vote on years prior to the one they have registered and mark them as absentees in these elections. This is the data we use in our main analyses throughout this paper.^{2.3.1} We verify whether there is any discontinuity in the density of individuals around the threshold in Figure 2.3.1, below:

^{2.3.1}We also provide results using the original dataset in the appendix.

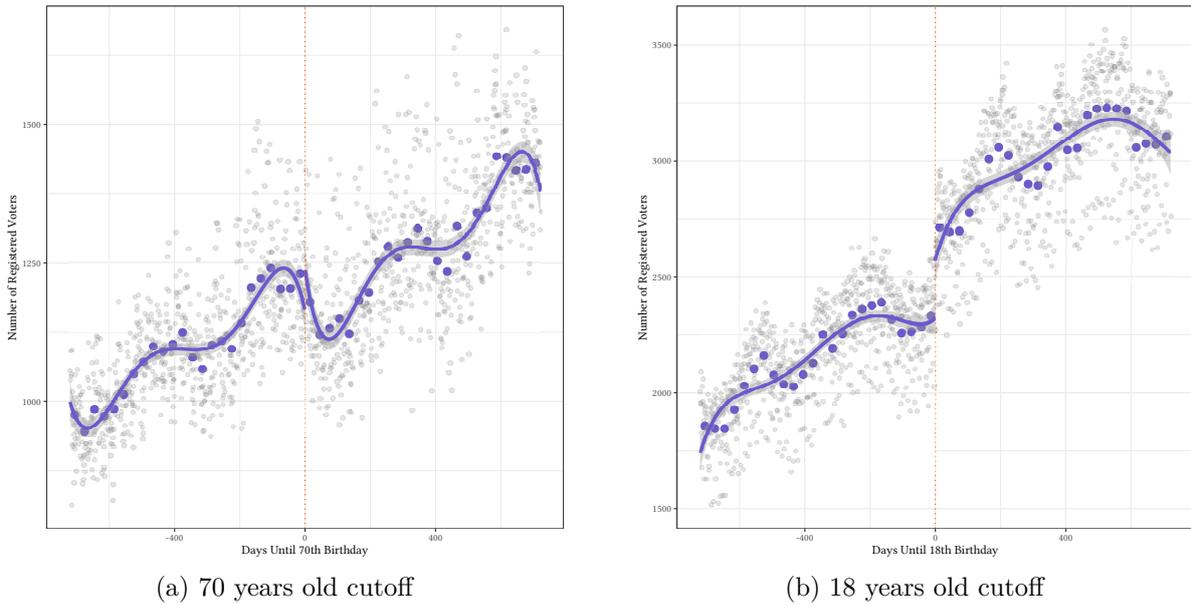


Figure 2.3.1 – Smoothness of running variable around cutoffs

Although there is no jump in the density for the elderly, there is still a sizeable and significant bunching for younger individuals. Since there is no reason to expect that voters would strategically lie about their date of birth, this discontinuity is probably due to the causal effect of becoming 18 years old on voter registration. Note that this takes place despite of the adjustment we previously described.^{2.3.2}

A second necessary condition that must be satisfied in order to interpret both θ_{t-1} and τ_{t-1} as causal parameters is that baseline covariates must be balanced around the cutoff. This means that there should not be a treatment effect of compulsory voting on predetermined attributes of the voters. We run our specification from Equation 2.3.1 using as dependent variable socioeconomic characteristics that we would not expect to change as a consequence of their the transition between compulsory and voluntary voting, such as education, gender and marital status. The results for individuals at the 70 years old cutoff can be found in Table 2.3.2.

^{2.3.2}The histogram for the original dataset, without the adjustment, can be seen in Figure A1.

Tabela 2.3.2 – Baseline covariate balance for elderly voters.

	Optimal BW	N	RD Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: Primary education					
Linear	81.676	162952	-0.008 (0.006)	-0.008 (0.004)*	-0.005 (0.003)
Quadratic	122.225	245804	-0.005 (0.007)	-0.006 (0.005)	-0.009 (0.004)**
Panel B: Secondary education					
Linear	55.226	111843	0.001 (0.007)	0.002 (0.005)	0.000 (0.003)
Quadratic	84.77	168991	0.003 (0.008)	0.005 (0.005)	-0.001 (0.004)
Panel C: Male					
Linear	61.325	124236	0.001 (0.008)	-0.001 (0.006)	-0.006 (0.004)
Quadratic	99.822	199713	-0.006 (0.01)	0.002 (0.007)	-0.006 (0.005)
Panel D: Married					
Linear	88.316	177482	-0.006 (0.006)	-0.006 (0.004)	-0.003 (0.003)
Quadratic	153.059	309201	-0.007 (0.007)	-0.006 (0.005)	-0.002 (0.003)

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The mean value of the dependent variable "Primary education" at the discontinuity is 0.293. The mean value of the dependent variable "Secondary education" at the discontinuity is 0.179. The mean value of the dependent variable "Male" at the discontinuity is 0.454. The mean value of the dependent variable "Married" at the discontinuity is 0.717. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

Overall, the baseline covariates at this cutoff do not display any robust discontinuity. This is not the case for younger voters, as it can be seen on Table 2.3.3. There are systematic discontinuities for different levels of education and for marital status. In order to tackle this issue, we control for these covariates in all our main regressions.

Tabela 2.3.3 – Baseline covariate balance for young voters.

	Optimal BW	N	RD Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: Primary education					
Linear	81.676	536654	-0.02 (0.003)***	-0.023 (0.002)***	-0.029 (0.002)***
Quadratic	122.225	806441	-0.016 (0.004)***	-0.02 (0.003)***	-0.027 (0.002)***
Panel B: Secondary education					
Linear	55.226	366657	-0.026 (0.004)***	-0.036 (0.003)***	-0.052 (0.002)***
Quadratic	84.77	555725	-0.022 (0.005)***	-0.029 (0.003)***	-0.05 (0.002)***
Panel C: Male					
Linear	61.325	406218	-0.005 (0.005)	-0.002 (0.003)	-0.004 (0.002)*
Quadratic	99.822	654244	-0.004 (0.005)	-0.002 (0.004)	-0.003 (0.003)
Panel D: Married					
Linear	88.316	582043	-0.003 (0.001)***	-0.003 (0.001)***	-0.003 (0.000)***
Quadratic	153.059	1016473	-0.003 (0.001)***	-0.003 (0.001)***	-0.003 (0.001)***

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The mean value of the dependent variable "Primary education" at the discontinuity is 0.763. The mean value of the dependent variable "Secondary education" at the discontinuity is 0.200. The mean value of the dependent variable "Male" at the discontinuity is 0.492. The mean value of the dependent variable "Married" at the discontinuity is 0.013. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

These results show that, although there are problems for the causal interpretation of our results for younger voters, both density of individuals and baseline covariates are smooth around the 70 years old cutoff. In the next section we display the results for both young and older individuals in order to provide a more complete view of the dynamic effects of CV on turnout for both those transitioning into compulsory voting (younger voters) and out of it (older voters).

2.4 Results

2.4.1 Elderly voters

This part of the analysis focuses on older citizens who transitioned from compulsory to voluntary voting when they reached the age of 70. First, we present the first stage and reduced form results for three different bandwidth lengths: half, standard and double CCT, where CCT stands for the Calonico-Cattaneo-Titiunik optimal bandwidth obtained from the procedure described in [Calonico, Cattaneo e Titiunik \(2014\)](#). We also check the robustness of our result for first and second degree polynomials in order to rule out the suspicion that any effect we capture is simply due to the functional form we use.

In this first set of specifications, since the dependent variables $V_{i,t-1}$ and $V_{i,t}$ are indicators (i.e. they can be either 0 or 1), the interpretation of the coefficients is straightforward: they are the point percent change on the probability of an individual turn out to vote in year $t - 1$ or t due to the direct effect of $C_{i,t-1}$. The results can be found below in Figure 2.4.1.

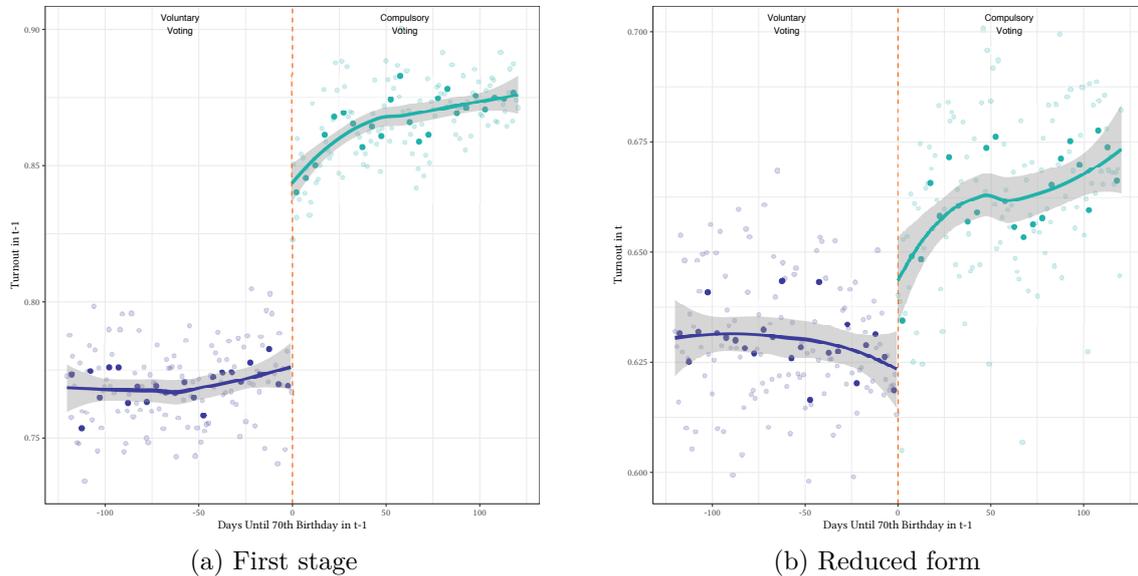


Figure 2.4.1 – Impact of exposure to CV in $t - 1$ on turnout in $t - 1$ and t for elderly voters

Both subfigures show that there is indeed a discontinuity on first stage and reduced form specifications. In fact, the first stage specification confirms the findings of [Cepaluni e Hidalgo \(2016b\)](#) that CV has a contemporaneous effect on voter turnout. The reduced

form specification shows a smaller but still significant effect of previous exposure to CV on turnout on the following election.

The robustness of this result to different polynomials and bandwidth lengths can be assessed below, on Table 2.4.1. Panel A displays the treatment coefficients for the first stage regressions, while Panel B displays this same coefficient for the reduced form specifications. The results are remarkably stable to different choices of polynomial degree and variations on the optimal bandwidth. The coefficients in Panel A range from 0.070 to 0.083, while the coefficients in Panel B range from 0.016 to 0.025. This means that CV has a contemporaneous effect of around 7.5 percentage points (or 9.1%) and a forward effect of 2 percentage points (or 3.1%).

More specifically, results in Panel B show that indeed there is some long term effect of exposure to compulsory voting penalties on turnout, albeit small when compared to the contemporaneous effect. This effect highlights that compulsory voting induces turnout even in the immediate absence of costs to citizens who fail to comply with the law. Two years after being subject to these costs for the last time, elderly voters are still being affected by it. Ultimately, this means that there is a dimension to compulsory voting that is unrelated to contemporaneous exposure to penalties.

Tabela 2.4.1 – RD Estimates for the impact of exposure to CV in $t - 1$ on turnout in $t - 1$ (first stage) and t (reduced form) for elderly voters.

	Optimal BW	N	RD Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: First stage					
Linear	99.617	167500	0.07 (0.006)***	0.079 (0.004)***	0.083 (0.003)***
Quadratic	115.851	195305	0.071 (0.008)***	0.072 (0.005)***	0.078 (0.004)***
Panel B: Reduced form					
Linear	100.388	201138	0.016 (0.006)***	0.025 (0.004)***	0.025 (0.003)***
Quadratic	108.343	217355	0.017 (0.009)*	0.022 (0.006)***	0.024 (0.004)***

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable for the first stage and reduced form regression is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t or $t-1$ respectively. The mean value of the dependent variable at the discontinuity for the first stage specification is 0.823. The mean value of the dependent variable at the discontinuity for the reduced form specification is 0.640. All regressions include state and year fixed effects and education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

This still leaves our main question open: what is the effect of previous on current turnout as a result of compulsory voting? In order to evaluate this, we instrument voting in $t - 1$ with exposure to compulsory voting in this same period. This allows us to recover the causal impact of previous of current voting, as described in Equation 2.3.3. The results can be found below, on Table 2.4.2.

Tabela 2.4.2 – IV Estimates for the impact of turnout in $t - 1$ on turnout in t for elderly voters.

	Optimal BW	N	IV Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Linear	102.159	172781	0.153 (0.095)	0.306 (0.058)***	0.279 (0.039)***
Quadratic	111.262	188429	0.149 (0.135)	0.281 (0.092)***	0.292 (0.059)***

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t . The mean value of the dependent variable at the discontinuity is 0.640. All regressions include education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

Once again, we display the results for different polynomials and bandwidth lengths in order to evaluate the robustness of the coefficients. As it can be seen there is a substantial and statistically significant causal effect of previous on current turnout. For the main CCT bandwidth, both linear and quadratic specifications reveal an increase of around 30 percentage points in the probability of turning out in election t . Since the mean value of turnout at the discontinuity is 0.640, our result indicates an increase in 46% in this probability.

2.4.2 Younger voters

We now turn to the results for individuals transitioning from voluntary to compulsory voting around the 18 years old cutoff. On Figure 2.4.2 below we can see the results for both first stage and reduced form specifications. Once again, we check for robustness for variations on the optimal bandwidth and functional form of the polynomial. As in the case for elderly voters, we find a strong first stage effect and a smaller, but still significant, reduced form impact.

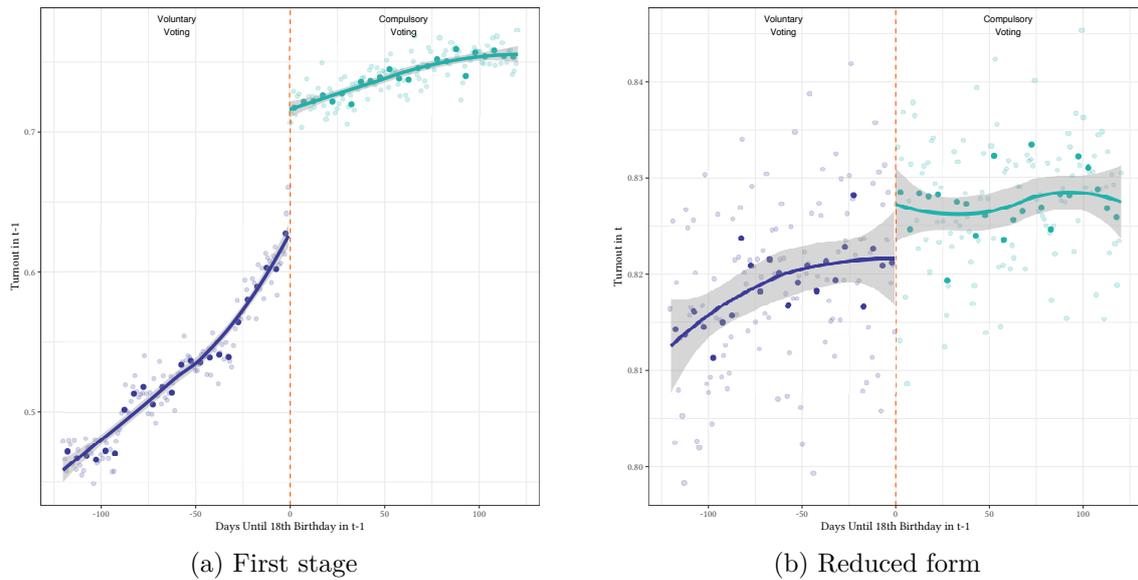


Figure 2.4.2 – Impact of exposure to CV in $t - 1$ on turnout in $t - 1$ and t for young voters

These results can be confirmed on Table 2.4.3. First stage estimates range from 7.7 to 8.8 percentage points and reduced form estimates range from 0.4 to 0.9 percentage points. Since the averages of the dependent variable at the cutoff are, respectively, 70% and 82.7%, it follows that the contemporaneous impact of CV is about 11.7% and the forward impact is close to 0.78%. Despite the fact that the first stage estimates for younger voters are similar to those obtained for the elderly, there is a sizeable difference in the reduced form impact on future turnout.

Tabela 2.4.3 – RD Estimates for the impact of exposure to CV in $t - 1$ on turnout in $t - 1$ (first stage) and t (reduced form) for younger voters.

	Optimal BW	N	RD Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: First stage					
Linear	28.661	137231	0.082 (0.007)***	0.088 (0.005)***	0.087 (0.003)***
Quadratic	68.547	325627	0.085 (0.007)***	0.077 (0.005)***	0.087 (0.003)***
Panel B: Reduced form					
Linear	107.923	706822	0.006 (0.003)**	0.005 (0.002)***	0.007 (0.001)***
Quadratic	101.159	667615	0.009 (0.004)**	0.009 (0.003)***	0.004 (0.002)**

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable for the first stage and reduced form regression is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t or $t-1$ respectively. The mean value of the dependent variable at the discontinuity for the first stage specification is 0.707. The mean value of the dependent variable at the discontinuity for the reduced form specification is 0.827. All regressions include education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Since the forward impact at the 18 years old cutoff is substantially smaller, it is not surprising that the IV estimates at Table 2.4.4 below are also reduced when compared to the same estimates for elderly voters. Coefficients are highly unstable to different variations on the CCT bandwidth, to the point of becoming negative when close to the cutoff. They are also much smaller when compared to the same estimates for elderly voters. This suggests that the degree of persistence in voting for younger voters who were recently exposed to CV is indistinguishable from zero.

Tabela 2.4.4 – IV Estimates for the impact of turnout in $t - 1$ on turnout in t for younger voters.

	Optimal BW	N	IV Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Linear	36.038	174823	-0.046 (0.065)	0.064 (0.045)	0.058 (0.028)**
Quadratic	59.597	283242	-0.034 (0.075)	0.087 (0.058)	0.067 (0.035)*

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t . The mean value of the dependent variable at the discontinuity is 0.827. All regressions include education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

2.5 Robustness

2.5.1 Bandwidth lengths

In this section, we test the robustness of our results to a larger number of bandwidth lengths within a year around the cutoff. We also verify if there's any causal effect on placebo cutoffs, in order to rule out any suspicion that the effects we capture are not related to becoming 18 or 70 close to an election day.

Although we have done bandwidth robustness tests in the previous section, those were limited to variations on the optimal bandwidth. We expand this analysis to bandwidths from 15 to 180 days (6 months) away from the cutoff in order to have a clearer idea of how stable are these estimates. First, we focus on the results at the 70 year old cutoff. The results for first stage and reduced form specifications can be seen below, on Figure 2.5.1.

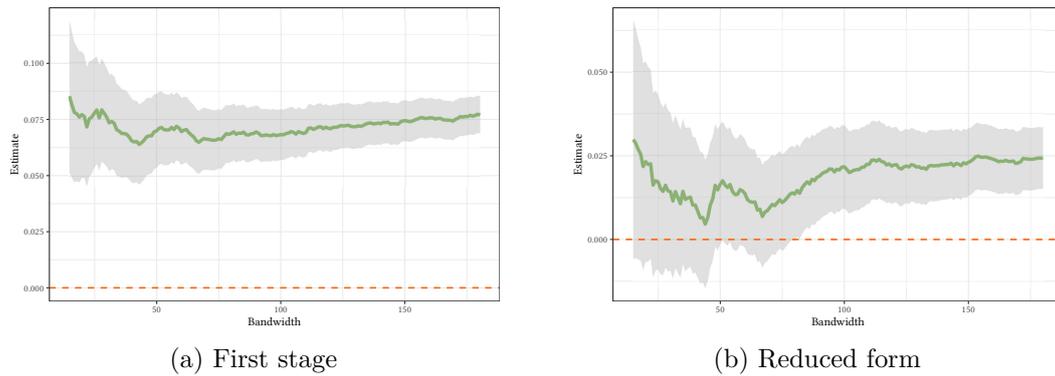


Figure 2.5.1 – Bandwidth robustness test for elderly voters (reduced form and first stage)

Unsurprisingly, the subfigure on the left confirms the strong and stable contemporaneous impact of CV on turnout. Reduced form estimates, however, show a small instability when close to the cutoff which rapidly stabilize close to 60 days, confirming the findings on Table 2.4.1. Since the first stage estimates are basically flat over the bandwidth interval we analyze, and τ can be obtained from the ratio θ^{RF}/θ^{FS} , it follows that the IV estimates on Figure 2.5.2 mimic the pattern from reduced form coefficients.

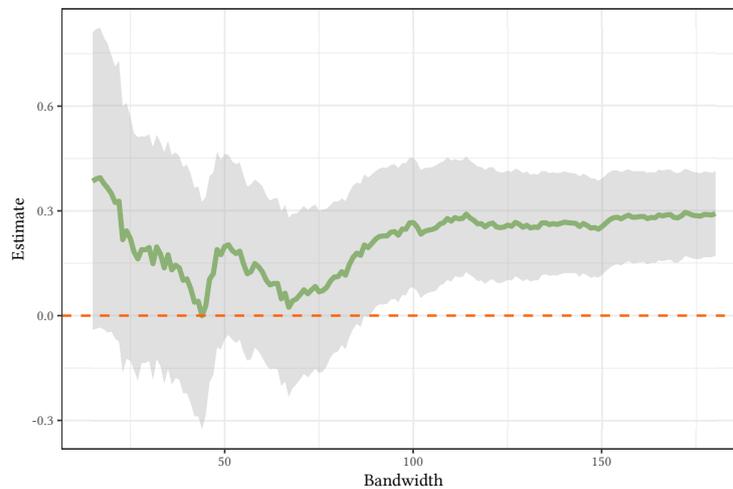


Figure 2.5.2 – Bandwidth robustness test for elderly voters (instrumental variable)

On Figure 2.5.3 we provide the robustness tests for reduced form and first stage coefficients for young voters. Once again, first stage estimates are stable while reduced form estimates are smaller and much closer to zero. In this case, as stressed in the previous section, the reduced form estimates are much closer to zero than the ones obtained for elderly voters. This leads to lower IV estimates, as shown on Figure 2.5.4. Despite being stable over the interval, we fail to reject the null at many different bandwidth values.

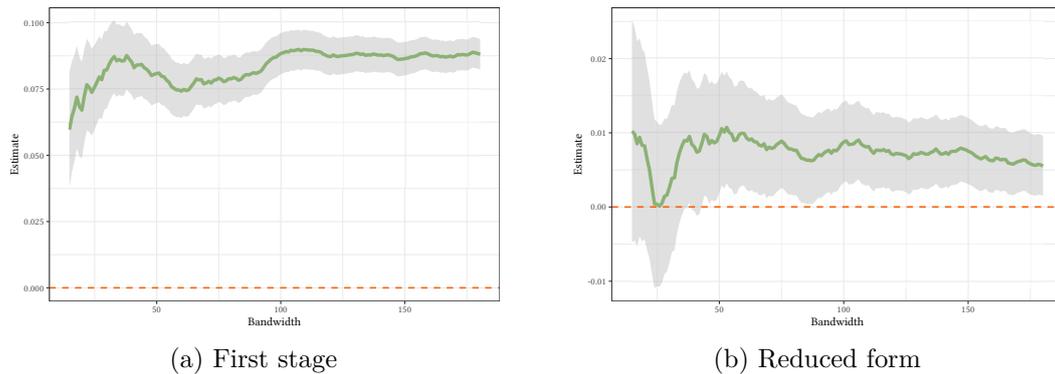


Figure 2.5.3 – Bandwidth robustness test for young voters (reduced form and first stage)

Our robustness tests confirm the main results: when transitioning from compulsory to voluntary voting, elderly voters present a significant and substantial amount of persistence in turnout due to exposure to the compulsory voting law. We cannot confirm that this happens for younger voters, however, even though we do find a strong contemporaneous effect of the law on turnout and a statistically significant (albeit small) forward effect of CV for this specific age range.

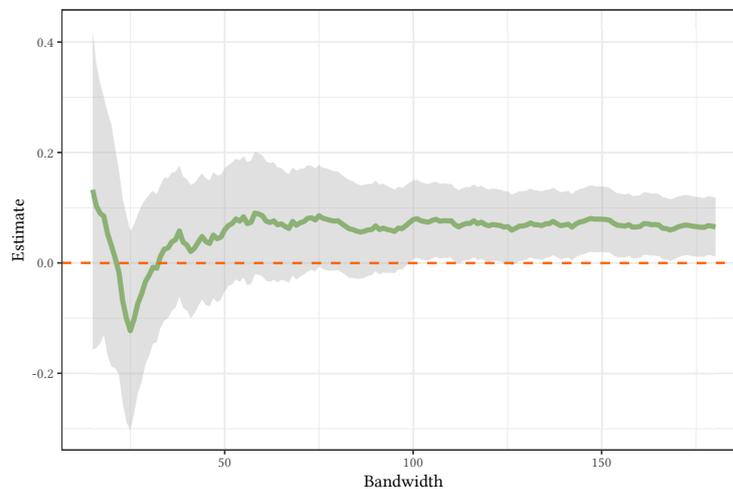


Figure 2.5.4 – Bandwidth robustness test for young voters (instrumental variable)

2.5.2 Placebo cutoffs

We now investigate if there is any effect on becoming 18 or 70 relative to days that are not the ones when an election took place. This is necessary in order to rule out any suspicion that the impact we are capturing is not related to exposure to compulsory voting, but simply with becoming older. The results in Figure 2.5.5 below show the results

for the elderly. As expected from a valid RD, the coefficients peak at the true cutoff and decrease as we move away from it.

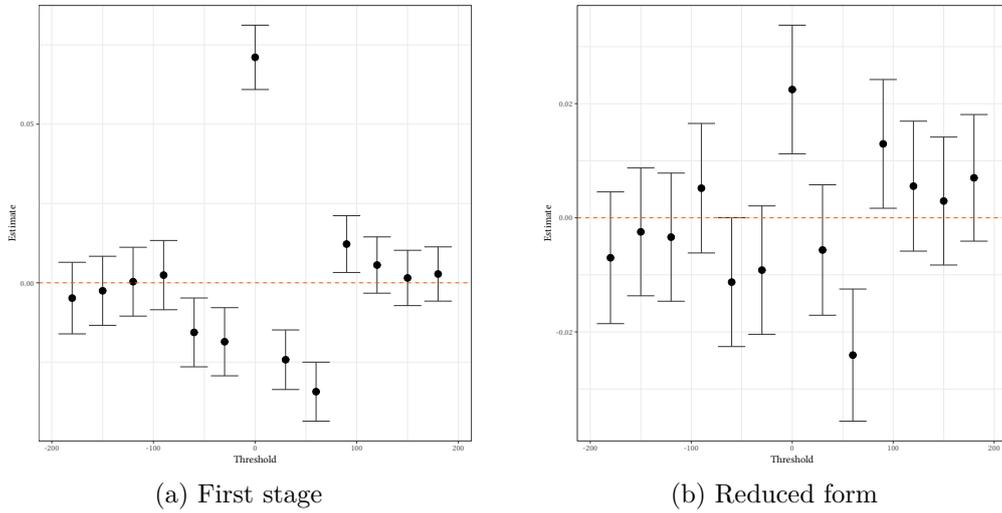


Figure 2.5.5 – Placebo tests around the 70th birthday

The same takes place for younger voters. In Figure 2.5.6, it's also possible to observe the same pattern: the estimates θ^{FS} and θ^{RF} peak in the true cutoff. Most of the coefficients away from this cutoff are also not statistically significant. This strengthens the case that the effects we capture are indeed related to being exposed to CV, not just becoming 18 or 70.

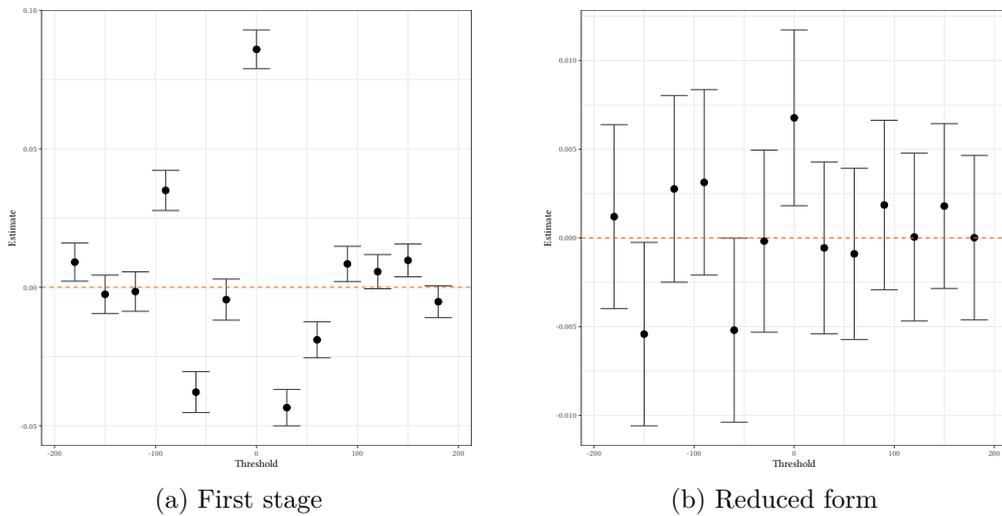


Figure 2.5.6 – Placebo tests around the 18th birthday

2.6 Conclusion

In this paper we analyze how individuals adjust their current decisions when they receive shocks in their calculus of voting in the past. We take advantage of the age-based rule determines who's under the compulsory voting and who's not, enabling us to measure the causal effect of exposure to the law in $t - 1$ on turnout in t . Our empirical strategy employs a regression discontinuity design (RDD) on a Brazil's full voter file from 2010 until 2018. Covering each round of 5 elections that took place during this period, our data allow us to follow each voter turnout pattern over time.

We find three main results. First, there is a strong impact of contemporaneous exposure to abstention penalties on turnout. This finding holds for both older and younger voters, showing that exposure to the abstention fines is a salient element in voters decision regardless of whether individuals are transitioning from voluntary to compulsory or from compulsory to voluntary voting. Second, we find a sizeable persistence effect of exposure to these penalties in the previous election on turnout in the current election. This result is strong for older voters and much smaller for younger individuals, which suggests the existence of heterogeneity in the dynamics of turnout between the two transitions. Finally, in the third and most important result, we find that voting in the past election causes voting in the present election. This result holds only for older voters. All of these results are robust to different combinations of polynomial degrees and bandwidth lengths.

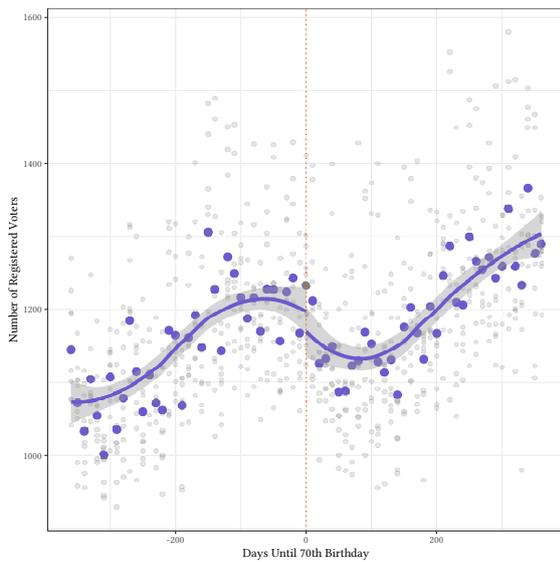
Our research expands previous analyses on the determinants of habit formation in politics. First, we provide the first comprehensive evidence of such effect as a consequence of compulsory voting in the context of a middle-income country. Second, the extent of our dataset and the way the compulsory voting law is designed in Brazil allows us to verify whether there is heterogeneity in the downstream effects of compulsory voting. We find that turnout at the individual level is path-dependent due to the existence of abstention penalties.

2.7 Appendix

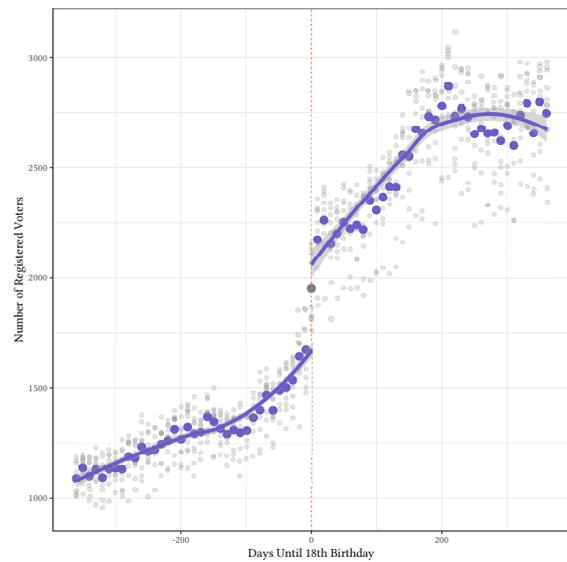
Tabela A1 – Summary statistics.

	Obs	Mean	Std. Dev.	Min	Median	Max
Turnout	69081011	0.84	0.36	0	1	1
Primary education	69023366	0.55	0.50	0	1	1
Secondary education	69023366	0.280	0.45	0	0	1
Male	69081011	0.47	0.50	0	0	1
Married	69081011	0.32	0.47	0	0	1

Notes: *Turnout* is an indicator which takes value 1 whenever the individual turns out to vote in the first round of an election. The variable *Primary education* is an indicator which takes values 1 if the individual has completed the primary education. The variable *Secondary education* is an indicator which takes values 1 if the individual has completed the secondary education. *Male* and *Married* are indicators which take value 1 whenever the individual is either male or married. Since the compulsory voting law does not apply to illiterate citizens, this group is removed from our sample. Sample consists of a random 10% draw of the complete Brazilian voter file for the years of 2010, 2012, 2014, 2016 and 2018.



(a) 70 years old cutoff



(b) 18 years old cutoff

Figure A1 – Smoothness of running variable around cutoffs

Tabela A2 – Baseline covariate balance for elderly voters.

	Optimal BW	N	RD Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: Primary education					
Linear	69.275	139258	-0.025 (0.007)***	-0.017 (0.005)***	-0.01 (0.003)***
Quadratic	85.95	170418	-0.026 (0.009)***	-0.023 (0.007)***	-0.016 (0.005)***
Panel B: Secondary education					
Linear	55.562	111534	-0.008 (0.007)	-0.002 (0.005)	-0.002 (0.003)
Quadratic	98.361	196563	-0.002 (0.007)	-0.001 (0.005)	-0.008 (0.004)**
Panel C: Male					
Linear	65.842	158809	-0.001 (0.007)	-0.002 (0.005)	-0.006 (0.004)
Quadratic	95.858	231341	-0.004 (0.009)	0.002 (0.006)	-0.004 (0.004)
Panel D: Married					
Linear	70.448	170822	-0.012 (0.006)*	-0.003 (0.005)	-0.005 (0.003)
Quadratic	115.239	281174	-0.01 (0.007)	-0.004 (0.005)	-0.006 (0.004)

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The mean value of the dependent variables at the discontinuity for the first stage specification is 5.344. The mean value of the dependent variables at the discontinuity for the first stage specification is 0.436. The mean value of the dependent variables at the discontinuity for the first stage specification is 0.636. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

Tabela A3 – Baseline covariate balance for elderly voters.

	Optimal BW	N	RD Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: Primary education					
Linear	69.275	139258	-0.025 (0.007)***	-0.017 (0.005)***	-0.01 (0.003)***
Quadratic	85.95	170418	-0.026 (0.009)***	-0.023 (0.007)***	-0.016 (0.005)***
Panel B: Secondary education					
Linear	55.562	111534	-0.008 (0.007)	-0.002 (0.005)	-0.002 (0.003)
Quadratic	98.361	196563	-0.002 (0.007)	-0.001 (0.005)	-0.008 (0.004)**
Panel C: Male					
Linear	65.842	158809	-0.001 (0.007)	-0.002 (0.005)	-0.006 (0.004)
Quadratic	95.858	231341	-0.004 (0.009)	0.002 (0.006)	-0.004 (0.004)
Panel D: Married					
Linear	70.448	170822	-0.012 (0.006)*	-0.003 (0.005)	-0.005 (0.003)
Quadratic	115.239	281174	-0.01 (0.007)	-0.004 (0.005)	-0.006 (0.004)

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The mean value of the dependent variables at the discontinuity for the first stage specification is 5.344. The mean value of the dependent variables at the discontinuity for the first stage specification is 0.436. The mean value of the dependent variables at the discontinuity for the first stage specification is 0.636. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

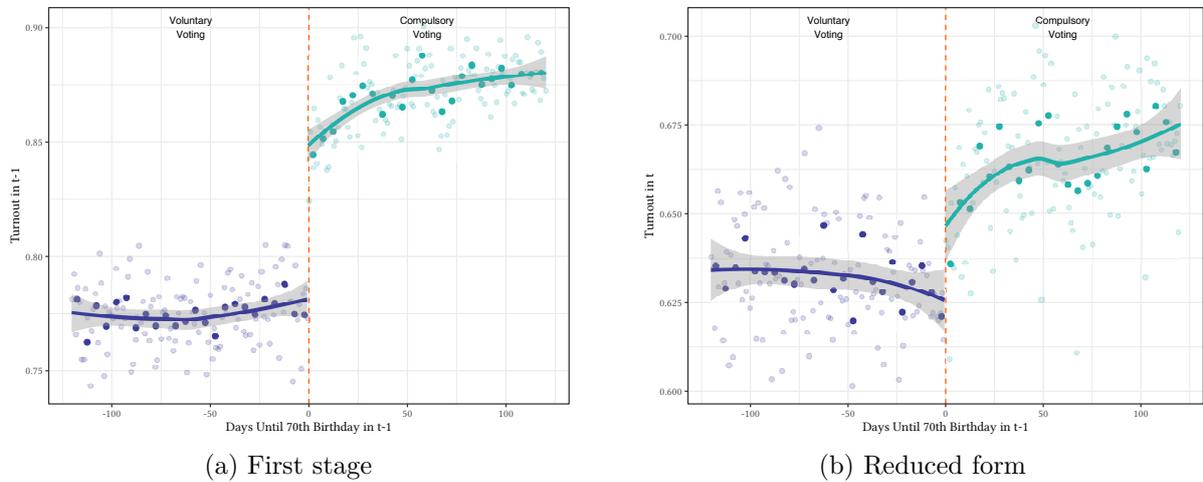
Figure A2 – Impact of exposure to CV in $t - 1$ on turnout in $t - 1$ and t

Tabela A4 – RD Estimates for the impact of exposure to CV in $t - 1$ on turnout in $t - 1$ (first stage) and t (reduced form) for elderly voters.

	Optimal BW	N	RD Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: First stage					
Linear	65.658	110429	0.065 (0.007)***	0.072 (0.005)***	0.081 (0.003)***
Quadratic	107.919	180976	0.069 (0.008)***	0.07 (0.005)***	0.078 (0.004)***
Panel B: Reduced form					
Linear	109.782	218973	0.021 (0.006)***	0.027 (0.004)***	0.025 (0.003)***
Quadratic	108.543	216711	0.02 (0.009)**	0.025 (0.006)***	0.029 (0.004)***

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable for the first stage and reduced form regression is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t or $t-1$ respectively. The mean value of the dependent variable at the discontinuity for the first stage specification is 0.754. The mean value of the dependent variable at the discontinuity for the reduced form specification is 0.581. All regressions include education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Tabela A5 – IV Estimates for the impact of turnout in $t - 1$ on turnout in t for elderly voters.

	Optimal BW	N	IV Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Linear	84.195	141973	0.293 (0.09)***	0.356 (0.061)***	0.329 (0.04)***
Quadratic	155.205	262794	0.185 (0.112)*	0.323 (0.068)***	0.347 (0.045)***

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t . The mean value of the dependent variable at the discontinuity is 0.581. All regressions include education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

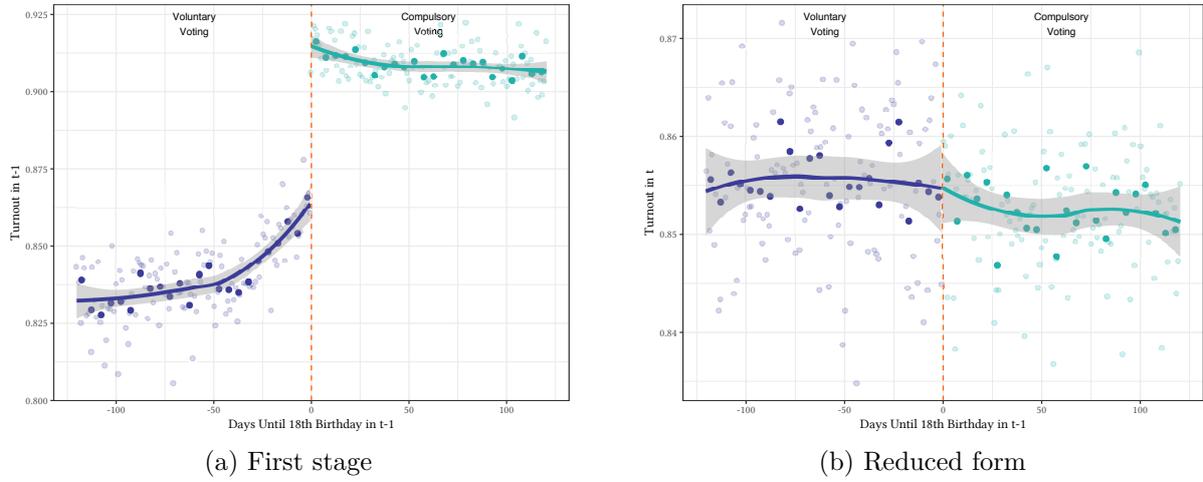


Figura A3 – Impact of exposure to CV in $t - 1$ on turnout in $t - 1$ and t for young voters

Tabela A6 – RD Estimates for the impact of exposure to CV in $t - 1$ on turnout in $t - 1$ (first stage) and t (reduced form) for younger voters.

		RD Estimates			
	Optimal BW	N	Half CCT (1)	CCT (2)	Twice CCT (3)
Panel A: First stage					
Linear	51.812	182209	0.051 (0.004)***	0.053 (0.003)***	0.06 (0.002)***
Quadratic	109.025	380595	0.049 (0.004)***	0.054 (0.003)***	0.058 (0.002)***
Panel B: Reduced form					
Linear	93.768	592881	0.002 (0.003)	0.001 (0.002)	0.002 (0.001)
Quadratic	98.944	625095	0.003 (0.004)	0.002 (0.003)	0 (0.002)

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable for the first stage and reduced form regression is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t or $t-1$ respectively. The mean value of the dependent variable at the discontinuity for the first stage specification is 0.906. The mean value of the dependent variable at the discontinuity for the reduced form specification is 0.859. All regressions include education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Tabela A7 – IV Estimates for the impact of turnout in $t - 1$ on turnout in t for younger voters.

	Optimal BW	N	IV Estimates		
			Half CCT (1)	CCT (2)	Twice CCT (3)
Linear	55.347	196145	-0.012 (0.086)	-0.028 (0.058)	-0.01 (0.038)
Quadratic	110.996	384009	-0.04 (0.098)	-0.015 (0.062)	-0.016 (0.041)

Notes: Columns Optimal BW and N display, respectively, the CCT optimal bandwidths for each polynomial degree and the number of observations from both sides of the within this bandwidth. Columns (1), (2) and (3) are estimates from separate local regressions with the specified bandwidth and polynomial. The dependent variable is an indicator which takes value 1 whenever the individual turns out to vote on and election in year t . The mean value of the dependent variable at the discontinuity is 0.581. All regressions include education controls. Sample consists of a random 10% draw of the total population of voters in Brazil from 2010 until 2018.

3 Full Day Schooling and Homicides: The Case of Pernambuco

3.1 Introduction

Can longer school hours keep teenagers safe from violence? There are two ways to arrive at an affirmative answer to this question. First, more time in school can mean more time studying and acquiring knowledge. This positive shock in human capital leads to potentially better job opportunities with higher wages in the future, making crime, a highly dangerous activity, less attractive. It also leads to more opportunities to live out of a dangerous neighborhood, reducing vulnerability. It follows that more time in school can raise the costs of future punishment, making crime less attractive in the present and keeping the teenager safe.^{3.1.1}

The second and most straightforward way to keep teenagers safe is simply by keeping them inside school. Assuming that schools are safer than the streets, this solution leads to a contemporaneous reduction in victimization by what the literature calls an incapacitating effect (LOCHNER, 2010). Staying longer inside school can be particularly effective for teenagers, since this is a period in life where individuals are more prone to engage in criminal activities and, therefore, put themselves in life endangering situations (LEVITT; LOCHNER, 2001; DEMING, 2011).^{3.1.2}

Our interested in this paper lies on the latter mechanism. We show how the full-day high school policy in the state of Pernambuco, Brazil, had a negative impact on the homicide rates among youth in that state. Using an event-study design, we exploit the temporal and spatial variation in the introduction of the program to show that this reduction was focused on individuals in the school-age range of high school students. We find no impact of the policy on homicides for similar age ranges. We also don't find any impact on other crimes. Overall, these results suggest that the introduction of the policy played a significant role in the reduction of lethal crime rates in that state.

Pernambuco constantly ranks in official reports as one of Brazil's most violent states in terms of homicide rates (CERQUEIRA et al., 2019). Nevertheless, from 2007 and 2011, the state experienced a 26% reduction in homicide rates, with an average of 5.25% decrease per year. In order to understand this major change, the authors in Ratton, Galvão e Fernandez (2014) interview several public managers from the state bureaucracy.

^{3.1.1}See, for example, Lochner e Moretti (2004), Machin, Marie e Vujić (2011) and Hjalmarsson, Holmlund e Lindquist (2015) for supporting evidence on this mechanism.

^{3.1.2}For evidence for Brazil, see Carvalho e Soares (2016).

They argue that, during this period, the state made a coordinated effort in many different areas that may have played a role in this reduction, most of them related to police activity. One of the interviewed managers, however, stresses that the main crime prevention policy proposed by then-governor Eduardo Campos was in fact the full-day school program. In our paper, we show that this educational component of the coordinated effort was indeed successful, playing a substantial role in the reduction of homicide rates of teenagers in the state of Pernambuco.

The full-day school policy in Pernambuco provides an interesting setting to examine this kind of policy. Its success on education outcomes was measured by [Rosa et al. \(2019\)](#). The authors find that the policy generated increases in grades of high-school students in the order of 50% in math and 35% in language. In [Rosa \(2019\)](#), the author examines if the same policy had an impact on local educational markets. He finds that full-day schools attract students from private schools, increasing the likelihood of private schools closing. The author also shows that the program does not have any significant effect on the dropout rate of low socioeconomic students.

Several papers in the literature have addressed whether longer school hours have an impact on violence. [Jacob e Lefgren \(2003\)](#) examine the short-term effects of school attendance affects juvenile crime. The authors use as source of exogenous variation days where teaching staff are required to attend work and students are not required to be at school. The authors find a decrease in property crime and an increase in violent crime in such days. The authors argue that the latter effect can be explained due to extended social interactions among crime-prone students.^{3.1.3} In [Luallen \(2006\)](#), the author uses teacher strikes as a source of exogenous variation in order to measure the incapacitation effect of schools on crime. His findings reinforce the previous results: the incapacitation effect of the school reduces property crime and at the same time increases violent crime. The author also shows that these effects are more pronounced in highly urbanized areas and is stronger for repeat offenders. More recently, [Fischer e Argyle \(2018\)](#) examines how the adoption of a four-day school week in the state of Colorado. This experiment is particularly interesting since the number of weekly school hours was kept constant between schools who adopted the four-day school week and schools who did not. The authors find that places with schools which adopted the four-day week had a 20% increase in juvenile criminal offenses. Moreover, the effect they find is stronger for property crimes.

In the context of Chile, [Berthelon e Kruger \(2011\)](#) examine whether nationwide school reform which extended the length of the school day from half to full-day had any

^{3.1.3}For a thorough explanation on the social nature of crime behavior see [Glaeser, Sacerdote e Scheinkman \(1996\)](#).

impact on risky behaviors among youth. The authors find that the reform decreased both property and violent crime among youth. They also find that the reform decreased the probability of a female adolescent becoming a mother. [Carvalho \(2019\)](#) provides similar evidence for Brazil. Using crime data from the state of São Paulo, the author investigates if the *Mais Educação* program, which aimed to increase school time in public schools, had an impact on crime rates near treated schools. Since the probability of receiving the program changes discontinuously as a function of a standardized test score, the author is able to recover the causal effect of the policy using a regression discontinuity design. The author finds a negative impact on less offensive crimes, which is stronger evidence for drug-related offenses and for schools with poorer students.

In this paper we add further evidence to this literature. By taking advantage of the rapid expansion of the full-day school program in Pernambuco, we evaluate if longer hours in school have an impact on exposure to lethal violence. Using an event-study design we investigate if there were changes in the homicide rates of municipalities in the years after they received their first full-day school. Focusing on the age group that was most affected by the policy (i.e. high-schoolers), we find a sizeable effect of the program on the reduction of homicides. We also provide placebo effect results for different age groups and different causes of death, and find no results.

The remainder of the paper is organized as follows. Section 3.2 provides basic background information on the full-day school program in Pernambuco. Section 3.3 describes the data and provides summary statistics. The research design and results are presented in Section 3.4. Section 3.5 concludes the paper.

3.2 Institutional background

Pernambuco is located in the northeastern region of Brazil and is one of the poorest states in the country, with a GDP per capita close to 6,000 USD.^{3.2.1} It has a population of approximately nine million citizens, with about 35% of its population in school age.^{3.2.2} This study focuses on the decisions of high school students, who are generally between 15 and 17 years old. Federal law in Brazil mandates school attendance until an individual is 17 years old, which is the expected high school graduation age. Although attending school is mandatory, there is imperfect enforcement of this rule by the government. For example, in 2015, about 20% of the population between 15 and 17 in the state was not enrolled in any educational institution. Among those who attended school in that age

^{3.2.1}National GDP per capita in Brazil is close to 9,800 USD, 63% larger than Pernambuco's.

^{3.2.2}School age in Brazil ranges from 4 to 19 years old.

range, only 56% of were enrolled in high school. Rates of conclusion are also precarious. Among 19-year-olds, only 54% graduated from high school. It is important to stress that outside options incentives may play a role in these statistics. High school students can join the formal labor market when they turn 16, which can incentivize early school dropout.

The roll-out of the full-day high school program started in 2004. Before the implementation, students attended regular high-schools in which a school day was comprised of day is five hours, and students attend only one shift (morning, afternoon, or evening). In 2004, the government of Pernambuco launched a pilot program aiming to convert regular schools into full-day high schools. The program doubled the number of hours students spent at high school from five to ten hours per day. Also, buildings from full-day schools, once converted, could only be used for high-school grade classes and activities.^{3.2.3}

Based on interviews with local authorities, the expansion of the program across the state was based on the goal of having at least one full-day high school per municipality. In order to achieve this, the government added full-day schools in different municipalities over time instead of concentrating geographically the implementation of the policy. Figure A1 below shows how the policy was expanded on the state over time.

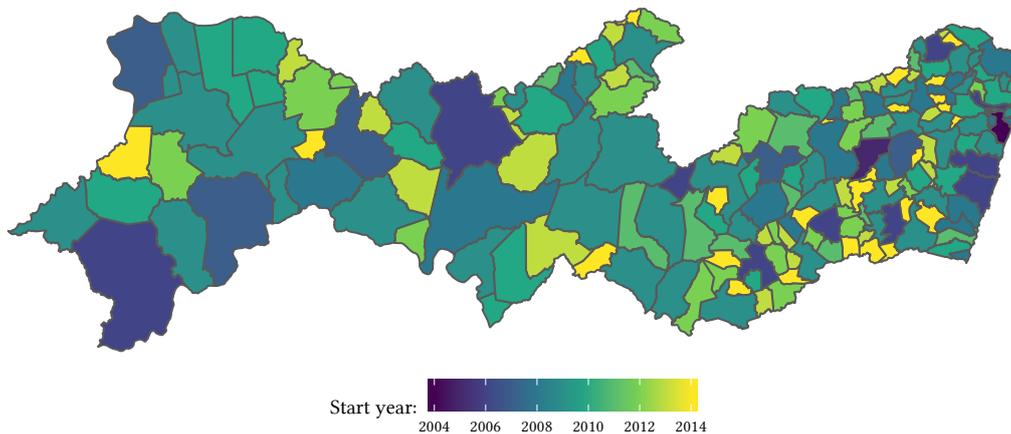


Figura A1 – Roll-out of the full-day schooling policy in the state of Pernambuco

^{3.2.3}It's important to note that the government implemented two different full-day school programs. While in some high schools the government implemented the full-day program five days per week, in other schools extra hours occurred only three days per week. In the current version of this paper, we will treat both as same.

Full-day implementation within schools also had a phase-in period. At the end of each academic year the state government released a list with the name of the schools which would be converted into full-day high schools in the following year. When a school joined the program, only students starting high school (10th graders) attended the full-day program. Students in 11th and 12th grades continued on the regular schedule. In the following year, 10th and 11th graders were under full-day regime. Finally, in the year after that, the phase-in was over and all high-school students were under the full-day program. This expansion was fast, and after a decade, almost 40% of enrolled high-schoolers in the state were in the program, as it can be seen in Figure A2.

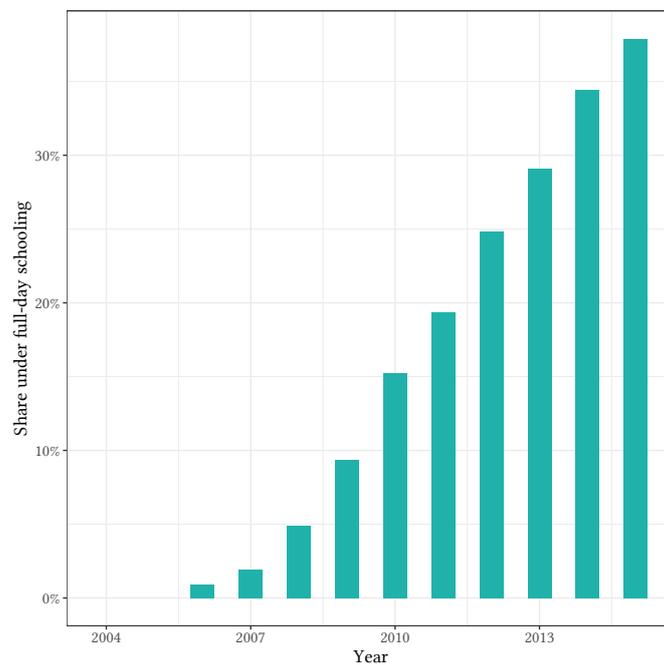


Figure A2 – Coverage expansion among high-school students

The main objective of the program is to increase the number of hours high-school students spend in learning activities. This implies that schools need to obtain more resources, either by recruiting more staff or by increasing inputs (e.g. desks). Schools under the full-day regime had a sharp increase in the number of hours dedicated to core subjects such as maths and language. Additional activities intended to complement what was taught in class were also created, such as lab exercises and remedial classes in order to improve student achievement. Finally, students in full-day high schools also had the opportunity to take classes typically offered by after-school programs in private education, such as foreign language.

Local authorities also indicated that, when making investments in physical facilities, they prioritized schools with better infrastructure *ex-ante*, such as the presence of computer

labs and/or one indoor sports court. They affirmed that financial resources were allocated to reform or equip these spaces since students would spend part of their extra hours in them.

3.3 Data and summary statistics

This study combines multiple data sources to test whether the full-day high school program had an impact on homicides in the state of the Pernambuco. The primary data source is the Censo Escolar (School Census) from the Brazilian Educational Research Institute (INEP, 2019). This dataset covers the school years 2001 to 2016. It also covers all grades and schools in the state. Besides the enrollment per grade, it also includes unique identifiers of schools, their municipalities, and indicates if the school is public or private. We also use data from Pernambuco’s department of education regarding the roll-out of the policy. This data provides information on the conversion of former regular public schools to full-day public high schools. It indicates the year a regular school was converted into a full-day school. We also supplement our dataset with information on annual rate of formal employment obtained from RAIS and the annual municipal GDP from IBGE, in order to account for the economics and labor market environment in the municipality in our analysis.

Finally, we compute homicide rates using information from DATASUS (*Departamento de Informática do Sistema Único de Saúde*), an administrative dataset from the Ministry of Health. DATASUS contains information on deaths by external causes classified according to the International Classification of Diseases and Related Health Problems (ICD-10).^{3.3.1} We use annual homicide rates aggregated to the municipality level, from 2001 to 2016. Figure A1 below shows how the homicide rate evolved over time in Pernambuco for two different age groups: those between 15 and 19 years old (i.e. in high-school age) and those between 12-14 and 20-24 (i.e. those slightly younger and older than the high-school group). This first examination suggests that both groups follow the same pattern of mortality due to homicides, with a sharp dip in the rates from 2007 to 2013.

^{3.3.1}We use the same ICD-10 codes from Costa et al. (2018), which account for misreporting and underreporting as suggested by Cerqueira (2013).

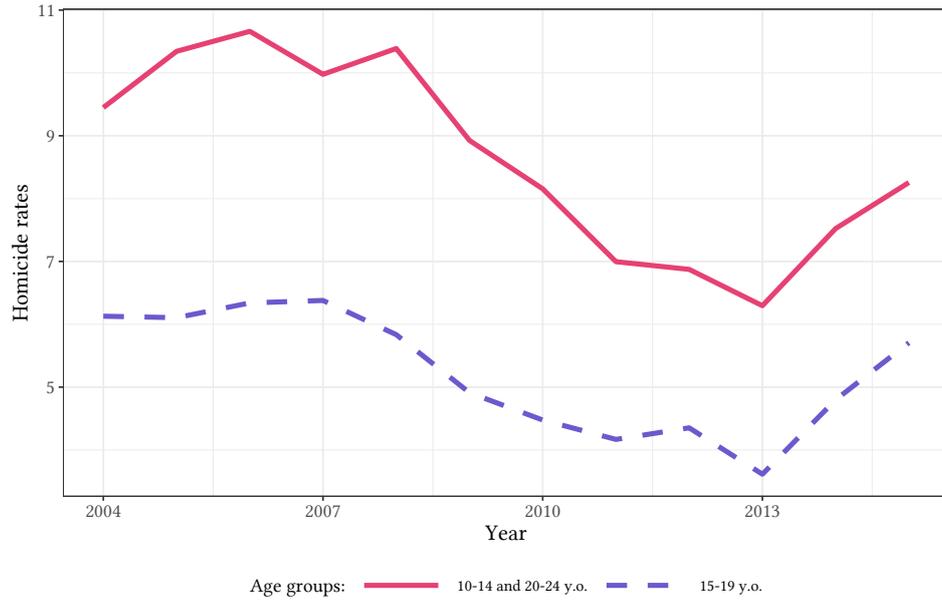


Figura A1 – Homicide rates in Pernambuco by different age groups

The use of homicides as a way to measure urban violence can be problematic, however. It is an extreme form of violence, less prevalent than others such as robbery and vehicle theft. Unfortunately, as of this version of the paper, we do not have access to administrative data at the municipal level from the state police of Pernambuco. It is considered a reliable crime statistics for developing countries, nevertheless. In Soares (2004), the author argues that in such contexts where less serious offenses can be widespread and non-randomly underreported, homicides can be used as a proxy measure for violent crimes.

Tabela A1 – Summary statistics.

	Obs	Mean	Std. Dev.	Min	Median	Max
Homicide rate (HS age)	2575	3.18	4.50	0.00	0.00	35.22
Homicide rate (non-HS age)	2575	5.69	6.41	0.00	4.45	44.04
Share in converted schools	2542	0.63	0.33	0.00	0.61	1.00
Population	2575	39057.50	67772.72	2128.00	21171.00	687688.00
GDP pc.	2391	5.44	5.34	1.21	4.34	89.79

Notes:

In Table A1 above we provide the summary statistics for our main variables, where the unit of observation is a municipality in a year. Homicide rates are the ratio between the number of homicides and the population on a given year, multiplied by 100,000. The share of converted schools is given by the ratio between the students enrolled in schools which were converted into full-day schools and the number of students in the municipality.

GDP per capita is in thousands of BRL and employment rate is the ratio between the number of formally employed individuals in the city and the population.

3.4 Research design and results

Our aim in this paper is to verify if the full-day school policy had an effect on homicide rates in the state of Pernambuco. In order to do this, we estimate the following specification:

$$y_{mt} = \alpha_m + \phi_t + \mathbf{x}'_{mt}\theta + \sum_{r=k_{min}}^{k_{max}} \mathbb{1}(t = t_m^* + r)\delta_r + \varepsilon_{mt}, \quad (3.4.1)$$

where y_{mt} is the outcome of interest y in municipality m in year t . The vector \mathbf{x}_{mt} has as its elements baseline covariates by which we control for, namely the GDP per capita, the share of formally employed individuals in the municipality, the share of individuals enrolled in school in the population, the ratio between the students enrolled in schools which were converted into full-day schools and the total population of students in the municipality and, finally, the population in m . The terms α_m and ϕ_t are, respectively, municipality and time fixed-effects. Since municipalities receive the program in different years, we defined t_m^* as the calendar year this municipality got its first full-day school. It follows that the coefficient of interest, δ_r , represents the effect of the program on the outcome variable r years later (or, if $r < 0$, r years previously).

Our main results can be seen below, on Figure A1. Each point represents a different value for δ_r , where for $r < 0$ they correspond to pre-trends, and for $r \geq 0$ to dynamic effects r periods relative to the introduction of the policy. The figure shows that there is, indeed, a negative impact of full-day schooling on the homicide-rates. This effect presents some monotonicity over time, which may be caused by the phase-in within each school for the first three years when the number of affected students grows over time. One hint that this might be the case is the fact that coefficients stabilize for $r \geq 4$, with similar point-estimates and major overlapping of confidence intervals.

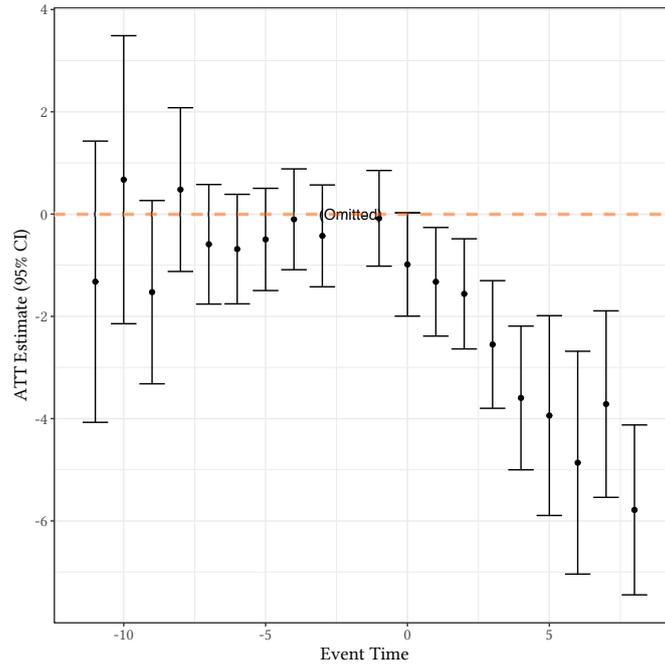


Figura A1 – Pooled ATT effects of full-day schooling on the homicide rate of individuals 15-19 age range.

In order to minimize any suspicion that this result might not be the consequence of the full-day school program, we make three different placebo tests, by evaluating if the program has any effect on: (i) homicides on individuals in age ranges close to the HS school age (i.e. 12-14 and 20-24); (ii) other death causes for individuals in the HS school age, and; (iii) other death causes for individuals in age ranges close to the HS school age. The results can be found below on Figure A2. All subfigures show no robust dynamic effect after $r = 0$. This provides further evidence that the decrease in homicides in the age range of high-schoolers was the consequence of full-day schooling.

Had any similar effect to the one in Figure A1 been captured, one could argue that the decrease in homicides for $r \geq 0$ is spurious, or correlated to some other confounding unobserved factor. This is not our case. The impact is focused on homicides for individuals on HS age.^{3.4.1} However, we cannot untangle whether the incapacitation effect of full-day schools is reducing the number of potential criminal youth on the streets, reducing the number of vulnerable teenagers, or both. Nevertheless, the sizeable bundle effect we capture remains relevant for policy purposes: full-day school policies in Pernambuco decreased fatal victimization among youth.

^{3.4.1}The robustness of our results can also be assessed on Table A1 in the Appendix.

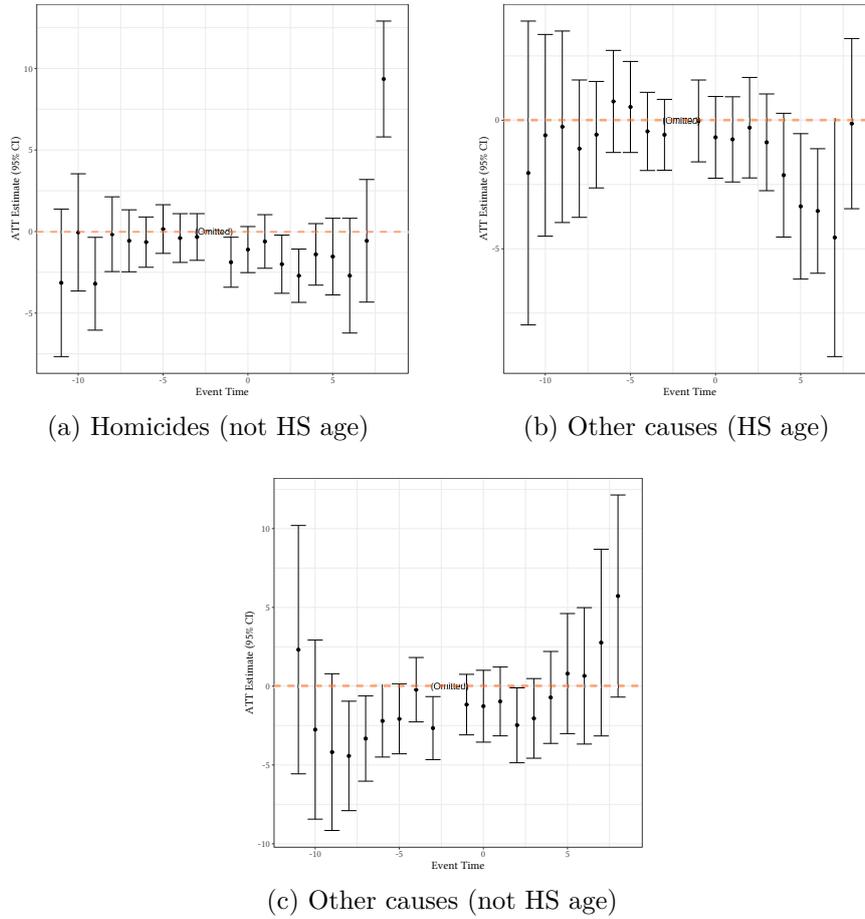


Figura A2 – ATT effects of full-day schooling on placebo outcomes and with different samples

3.5 Conclusion

In this paper we show how the full-day high school policy in the state of Pernambuco, Brazil had a negative impact on the homicide rates among teenagers. We find no impact of the policy on homicides for similar age ranges. In order to investigate if the channel of this reduction was indeed the program, we test if there is any impact on other causes of mortality and on other age groups. We find no impact in these tests, strengthening our argument that full-day schooling had an impact on the reduction in homicides in the state of Pernambuco during the last decade.

All things considered, these results suggest that the introduction of the policy played a significant role in the reduction of lethal crime rates in that state. Our research expands a large literature in the field and provide the first policy evaluation of the impact of this program on outcomes not directly related to education.

3.6 Appendix

Tabela A1 – ATT estimates on the impact of full-day schooling in death rates.

Dependent variables:	<i>High-school age</i>			<i>Not high-school age</i>	
	Homicides		Other causes	Homicides	Other causes
	(1)	(2)	(3)	(4)	(5)
After full-day program	-0.526* (0.285)	-0.632** (0.292)	-0.353 (0.487)	-0.462 (0.436)	0.310 (0.752)
Controls	No	Yes	Yes	Yes	Yes
Observations	2,575	2,360	2,360	2,360	2,360

Notes: The first three columns focus on mortality rates for individuals between the ages of 15 and 19, which we label *High school age*. The last three columns focus on mortality rates for individuals between the ages of 12 and 14 and individuals between the ages of 20 and 24, which we label *Not high school age*. Columns (1), (2) and (4) present results using the homicide rate among the indicated age group. Columns (3) and (5) present results using the death rate for causes different from homicides among the indicated age group. Death rates are calculated as the number of deaths over 100,000 inhabitants in each municipality. Significance: *** p<0.01, ** p<0.05, * p<0.1.

REFERENCES

- ALT, J. E.; MARSHALL, J.; LASSEN, D. D. Credible sources and sophisticated voters: When does new information induce economic voting? *The Journal of Politics*, v. 78, n. 2, p. 327–342, 2016.
- BECHTEL, M. M.; HANGARTNER, D.; SCHMID, L. Compulsory voting, habit formation, and political participation. *Working Paper*, 2015.
- BECKER, G. S.; MURPHY, K. M. A theory of rational addiction. *Journal of Political Economy*, JSTOR, p. 675–700, 1988.
- BENDOR, J.; DIERMEIER, D.; TING, M. A behavioral model of turnout. *American Political Science Review*, Cambridge Univ Press, v. 97, n. 2, p. 261–280, 2003.
- BERTHELON, M. E.; KRUGER, D. I. Risky behavior among youth: Incapacitation effects of school on adolescent motherhood and crime in chile. *Journal of Public Economics*, Elsevier, v. 95, n. 1-2, p. 41–53, 2011.
- BIRCH, S. *Full participation: A comparative study of compulsory voting*. [S.l.]: Manchester University Press, 2009.
- BLAIS, A.; GIDENGIL, E.; NEVITTE, N. Where does turnout decline come from? *European Journal of Political Research*, Wiley Online Library, v. 43, n. 2, p. 221–236, 2004.
- CALONICO, S.; CATTANEO, M. D.; TITIUNIK, R. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, v. 82, n. 6, p. 2295–2326, 2014.
- CARVALHO, E. F. d. *School Time and Crime: Incapacitation Effects in Brazil*. Tese (Doutorado) — PUC-Rio, 2019.
- CARVALHO, L. S.; SOARES, R. R. Living on the edge: Youth entry, career and exit in drug-selling gangs. *Journal of Economic Behavior & Organization*, Elsevier, v. 121, p. 77–98, 2016.
- CEPALUNI, G.; HIDALGO, F. D. Compulsory voting can increase political inequality: Evidence from brazil. *Political Analysis*, v. 24, n. 2, p. 273–280, 2016.
- CEPALUNI, G.; HIDALGO, F. D. Compulsory voting can increase political inequality: Evidence from brazil. *Political Analysis*, SPM-PMSAPSA, p. mpw004, 2016.
- CERQUEIRA, D. *Mapa dos homicídios ocultos no Brasil*. [S.l.], 2013.
- CERQUEIRA, D. et al. Atlas da violência 2019. Instituto de Pesquisa Econômica Aplicada (Ipea), 2019.
- CHIANG, C.-F.; KNIGHT, B. Media bias and influence: Evidence from newspaper endorsements. *The Review of Economic Studies*, p. 1–26, 2011.

- CIOCHETTO, L. *Globalisation and Advertising in Emerging Economies: Brazil, Russia, India and China*. [S.l.]: Routledge, 2013. v. 51.
- COPPOCK, A.; GREEN, D. P. Is voting habit forming? new evidence from experiments and regression discontinuities. *American Journal of Political Science*, Wiley Online Library, v. 60, n. 4, p. 1044–1062, 2016.
- COSTA, F. et al. Homicides and the age of criminal responsibility in brazil: A density discontinuity approach. *Economía, the journal of LACEA*, 2018.
- DEGAN, A. Policy positions, information acquisition and turnout. *The Scandinavian Journal of Economics*, v. 108, n. 4, p. 669–682, 2006.
- DELLAVIGNA, S.; FERRARA, E. L. *Economic and social impacts of the media*. [S.l.], 2015.
- DELLAVIGNA, S.; KAPLAN, E. D. The fox news effect: Media bias and voting. *The Quarterly Journal of Economics*, MIT Press, v. 122, n. 3, p. 1187–1234, 2007.
- DELLAVIGNA, S. et al. Voting to tell others. *The Review of Economic Studies*, Review of Economic Studies Ltd, v. 84, n. 1, p. 143–181, 2016.
- DELLAVIGNA, S. et al. Voting to tell others. *The Review of Economic Studies*, v. 84, n. 1, p. 143–181, 2017.
- DEMING, D. J. Better schools, less crime? *The Quarterly Journal of Economics*, MIT Press, v. 126, n. 4, p. 2063–2115, 2011.
- DOWNS, A. An economic theory of political action in a democracy. *The journal of political economy*, p. 135–150, 1957.
- DOWNS, A. An economic theory of political action in a democracy. *Journal of Political Economy*, p. 135–150, 1957.
- ENIKOLOPOV, R.; PETROVA, M.; ZHURAVSKAYA, E. Media and political persuasion: Evidence from russia. *The American Economic Review*, p. 3253–3285, 2011.
- FAVA, A. C. P. e.; ARENDS-KUENNING, M. P. *Intrahousehold bargaining and the demand for consumer durables in Brazil*. [S.l.], 2013.
- FERRARA, E. L.; CHONG, A.; DURYEA, S. Soap operas and fertility: Evidence from brazil. *American Economic Journal: Applied Economics*, p. 1–31, 2012.
- FISCHER, S.; ARGYLE, D. Juvenile crime and the four-day school week. *Economics of Education Review*, Elsevier, v. 64, p. 31–39, 2018.
- FOWLER, J. H. Habitual voting and behavioral turnout. *Journal of Politics*, Wiley Online Library, v. 68, n. 2, p. 335–344, 2006.
- FRANSEN, B. R. Party bias in union representation elections: Testing for manipulation in the regression discontinuity design when the running variable is discrete. In: *Regression Discontinuity Designs: Theory and Applications*. [S.l.: s.n.], 2017. p. 281–315.

FRANKLIN, M. N. *Voter turnout and the dynamics of electoral competition in established democracies since 1945*. [S.l.]: Cambridge University Press, 2004.

FUJIWARA, T.; MENG, K.; VOGL, T. Habit formation in voting: Evidence from rainy elections. *American Economic Journal: Applied Economics*, v. 8, n. 4, p. 160–88, 2016.

FUNK, P. Social incentives and voter turnout: evidence from the swiss mail ballot system. *Journal of the European Economic Association*, Wiley Online Library, v. 8, n. 5, p. 1077–1103, 2010.

GELMAN, A.; IMBENS, G. *Why high-order polynomials should not be used in regression discontinuity designs*. [S.l.], 2014.

GENTZKOW, M. Television and voter turnout. *The Quarterly Journal of Economics*, p. 931–972, 2006.

GERBER, A. S.; GREEN, D. P.; LARIMER, C. W. Social pressure and voter turnout: Evidence from a large-scale field experiment. *American Political Science Review*, Cambridge Univ Press, v. 102, n. 01, p. 33–48, 2008.

GERBER, A. S.; GREEN, D. P.; SHACHAR, R. Voting may be habit-forming: evidence from a randomized field experiment. *American Journal of Political Science*, Wiley Online Library, v. 47, n. 3, p. 540–550, 2003.

GLAESER, E. L.; SACERDOTE, B.; SCHEINKMAN, J. A. Crime and social interactions. *The Quarterly Journal of Economics*, MIT Press, v. 111, n. 2, p. 507–548, 1996.

GORDON, S. B.; SEGURA, G. M. Cross-national variation in the political sophistication of individuals: Capability or choice? *The Journal of Politics*, Cambridge Univ Press, v. 59, n. 01, p. 126–147, 1997.

GROSSER, J.; SEEBAUER, M. The curse of uninformed voting: An experimental study. *Games and Economic Behavior*, Elsevier, v. 97, p. 205–226, 2016.

HJALMARSSON, R.; HOLMLUND, H.; LINDQUIST, M. J. The effect of education on criminal convictions and incarceration: causal evidence from micro-data. *The Economic Journal*, Oxford University Press Oxford, UK, v. 125, n. 587, p. 1290–1326, 2015.

HOLBEIN, J. B.; RANGEL, M. A. Does voting have upstream and downstream consequences? evidence from compulsory voting in brazil. 2016.

INEP. *Censo Escolar 2019*. [S.l.]: Brasília, 2019.

JACOB, B. A.; LEFGREN, L. Are idle hands the devil's workshop? incapacitation, concentration, and juvenile crime. *American Economic Review*, v. 93, n. 5, p. 1560–1577, 2003.

JR, C. Z.; LAUDERDALE, B. E. Distinguishing between influences on brazilian legislative behavior. *Legislative Studies Quarterly*, Wiley Online Library, v. 36, n. 3, p. 363–396, 2011.

- JR, G. B. P. American voter turnout in comparative perspective. *The American Political Science Review*, JSTOR, p. 17–43, 1986.
- KENNEDY, P.; PRAT, A. Where do people get their news? *Mimeo, Columbia University*, 2017.
- LEE, D. S.; CARD, D. Regression discontinuity inference with specification error. *Journal of Econometrics*, v. 142, n. 2, p. 655–674, 2008.
- LEE, D. S.; LEMIEUX, T. Regression discontinuity designs in economics. *Journal of Economic Literature*, v. 48, p. 281–355, 2010.
- LEON, F. de; RIZZI, R. *Does Forced Voting Result in Political Polarization?* [S.l.], 2014.
- LEON, F. L. L. de; RIZZI, R. A test for the rational ignorance hypothesis: Evidence from a natural experiment in brazil. *American Economic Journal: Economic Policy*, v. 6, n. 4, p. 380–398, 2014.
- LEÓN, G. Turnout, political preferences and information: Experimental evidence from Perú. *Journal of Development Economics*, v. 127, p. 56–71, 2017.
- LEVITT, S. D.; LOCHNER, L. The determinants of juvenile crime. In: *Risky behavior among youths: An economic analysis*. [S.l.]: University of Chicago Press, 2001. p. 327–374.
- LIJPHART, A. Unequal participation: Democracy's unresolved dilemma presidential address, american political science association, 1996. *American Political Science Review*, Cambridge Univ Press, v. 91, n. 01, p. 1–14, 1997.
- LOCHNER, L. Education policy and crime. In: *Controlling crime: strategies and tradeoffs*. [S.l.]: University of Chicago Press, 2010. p. 465–515.
- LOCHNER, L.; MORETTI, E. The effect of education on crime: Evidence from prison inmates, arrests, and self-reports. *American economic review*, v. 94, n. 1, p. 155–189, 2004.
- LODGE, G.; BIRCH, S. The case for compulsory voting. *New Statesman*, 2012.
- LOEWEN, P. J.; MILNER, H.; HICKS, B. M. Does compulsory voting lead to more informed and engaged citizens? an experimental test. *Canadian Journal of Political Science*, Cambridge Univ Press, v. 41, n. 03, p. 655–672, 2008.
- LUALLEN, J. School's out... forever: A study of juvenile crime, at-risk youths and teacher strikes. *Journal of urban economics*, Elsevier, v. 59, n. 1, p. 75–103, 2006.
- MACHADO, D. C.; GONZAGA, G. O impacto dos fatores familiares sobre a defasagem idade-série de crianças no Brasil. *Revista Brasileira de Economia*, v. 61, p. 449 – 476, 12 2007.
- MACHIN, S.; MARIE, O.; VUJIĆ, S. The crime reducing effect of education. *The Economic Journal*, Oxford University Press Oxford, UK, v. 121, n. 552, p. 463–484, 2011.

- MAINWARING, S. *Rethinking party systems in the third wave of democratization: the case of Brazil*. [S.l.]: Stanford University Press, 1999.
- MAINWARING, S.; SCULLY, T. et al. *Building democratic institutions: Party systems in Latin America*. [S.l.]: Stanford University Press Stanford, 1995.
- MARTINELLI, C. Would rational voters acquire costly information? *Journal of Economic Theory*, Elsevier, v. 129, n. 1, p. 225–251, 2006.
- MCCRARY, J. Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, v. 142, n. 2, p. 698–714, 2008.
- MEREDITH, M. Persistence in political participation. *Quarterly Journal of Political Science*, v. 4, n. 3, p. 187–209, 2009.
- MESQUITA, N. Jornal nacional, democracia e confiança nas instituições democráticas. *Democracia e Confiança: por que os cidadãos desconfiam das instituições públicas*, p. 185–216, 2010.
- MESQUITA, N. C. Mídia noticiosa e adesão democrática: O papel de fontes secundárias de notícias. *Cadernos do NUPPs*, v. 2, 2010.
- MOISÉS, J. Á. Democracia e confiança: por que os cidadãos desconfiam das instituições públicas. *São Paulo: Edusp*, 2010.
- MYERSON, R. Population uncertainty and poisson voting games. *International Journal of Game Theory*, v. 27, p. 375, 1988.
- NYHAN, B.; SKOVRON, C.; TITIUNIK, R. Differential registration bias in voter file data: A sensitivity analysis approach. *American Journal of Political Science*, Wiley Online Library, v. 61, n. 3, p. 744–760, 2017.
- OBERHOLZER-GEE, F.; WALDFOGEL, J. Media markets and localism: Does local news en español boost hispanic voter turnout? *American Economic Review*, v. 99, n. 5, p. 2120–28, 2009.
- PALFREY, T. R.; ROSENTHAL, H. Voter participation and strategic uncertainty. *American Political Science Review*, Cambridge Univ Press, v. 79, n. 01, p. 62–78, 1985.
- PINTOR, R. L.; GRATSCHEW, M.; SULLIVAN, K. Voter turnout rates from a comparative perspective. *RL Pintor, & M. Gratschew, Voter turnout since*, p. 75–94, 2012.
- PLUTZER, E. Becoming a habitual voter: Inertia, resources, and growth in young adulthood. *American Political Science Review*, Cambridge Univ Press, v. 96, n. 01, p. 41–56, 2002.
- POLLAK, R. A. Habit formation and dynamic demand functions. *Journal of Political Economy*, JSTOR, v. 78, n. 4, p. 745–763, 1970.
- PORTO, M. Tv news and political change in brazil the impact of democratization on tv globo's journalism. *Journalism*, SAGE Publications, v. 8, n. 4, p. 363–384, 2007.

- PORTO, M. P. Framing controversies: television and the 2002 presidential election in Brazil. *Political Communication*, Taylor & Francis, v. 24, n. 1, p. 19–36, 2007.
- PUTNAM, R. D. Bowling alone: America's declining social capital. In: *Culture and Politics*. [S.l.]: Springer, 2000. p. 223–234.
- RATTON, J.; GALVÃO, C.; FERNANDEZ, M. Pact for life and the reduction of homicides in the state of Pernambuco. *Stability: International Journal of Security and Development*, Centre for Security Governance, v. 3, n. 1, 2014.
- RIKER, W. H.; ORDESHOOK, P. C. A theory of the calculus of voting. *American Political Science Review*, Cambridge Univ Press, v. 62, n. 01, p. 25–42, 1968.
- ROSA, L. Subsidies for public high schools and students' educational decisions. 2019.
- ROSA, L. et al. The effects of public high school subsidies on student test scores. 2019.
- SAUNDERS, B. The democratic turnout 'problem'. *Political Studies*, SAGE Publications, v. 60, n. 2, p. 306–320, 2012.
- SHINEMAN, V. A. If you mobilize them, they will become informed: experimental evidence that information acquisition is endogenous to costs and incentives to participate. *British Journal of Political Science*, Cambridge University Press, p. 1–23, 2016.
- SHINEMAN, V. A. Costly extremism: Increasing the cost of voting deters participation among moderates, and generates a voting population dominated by extremists. 2017.
- SINGH, S. P. Elections as poorer reflections of preferences under compulsory voting. *Electoral Studies*, Elsevier, v. 44, p. 56–65, 2016.
- SOARES, R. R. Crime reporting as a measure of institutional development. *Economic Development and Cultural Change*, The University of Chicago Press, v. 52, n. 4, p. 851–871, 2004.
- SOBBRIO, F. The political economy of news media: theory, evidence and open issues. *A Handbook of Alternative Theories of Public Economics*, Edward Elgar Publishing, p. 278, 2014.
- WEBER, M. *Economy and society: An outline of interpretive sociology*. [S.l.]: Univ of California Press, 1978.
- ZALLER, J. *The nature and origins of mass opinion*. [S.l.]: Cambridge university press, 1992.