The Consequences of Being Forced to Vote: Evidence from Brazil's Dual Voting System*

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Abstract

This paper provides new estimates for the effects of compulsory voting on individuals. The identification relies on the Brazilian dual voting system-voluntary and compulsory-whose exposure is determined by date of birth. Using RD and IV approaches and data from a self-collected survey, we find that the compulsory legislation leads to a significant increase in voter turnout. These changes are followed by a sizable increase in the probability that individuals will express preference for a political party, but not by an increase in political knowledge among the population. Moreover, we find that a first compulsory voting experience permanently affects individuals' preferences.

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1 Introduction

Most countries of the world follow a democratic system of government, in which elections are decided by the population (a strategy that seems consensual). However, from an individual's point of view, the chance of any singular vote to be pivotal is zero. The question of why people vote[1] is as intriguing as how and whether the voting process transforms citizens. Does voting make individuals more politically involved and informed citizens? If so, is this effect permanent or relevant?

The answers to these questions are less clear in the context of a forced democracy, imposed by compulsory voting legislation. This system is in place in 14.5% of the world's countries;[2] however, while it ensures more representativeness of the voting population, it is unclear whether and how citizens react to the compulsory voting legislation, especially those who otherwise would abstain from participating in the political process. A possible downside of the compulsory legislation is that these extra votes could negatively affect election results.

This paper aims to determine how exposure to the controversial legislation of compulsory voting affects citizens and elicits input into the discussion of forced voting. In order to overcome unobservable correlations between voting and preferences, we explore the dual voting system of Brazilian legislation, in which individuals between 16 and 18 years old are entitled to vote but are not required to, while those older than 18 are legally required to vote. [3] This legislation provides an exogenous shift in an individual's likelihood to vote, which is used to identify the causal effects of voting. The data come from a self-collected survey conducted during the week following the 2010 Brazilian Presidential Election. It consists of a comprehensive set of demographic and political preference questions and a political quiz to evaluate respondents' levels of political information.

In the first part of this paper, we estimate the immediate effects of voting—that

[1] See Coate and Conlin (2004), Feddersen (2004).

[2] http://aceproject.org/about-en

[3] The voting enforcement mechanism is explained in Section 2.

is, of being exposed to compulsory legislation—in a regression discontinuity fashion by comparing the political behavior among age groups around the threshold that determines exposure to different electoral institutions. First, we estimate the impact of the compulsory legislation on turnout. These estimates, which are new and interesting on their own, contribute to the vast political science literature.[4] We then estimate the impact of the legal requirement to vote on political preferences and the causal link between voting turnout and information. It is well documented that voters are better informed than non-voters and are citizens who are more likely to vote when provided with information (Lassen 2005; Battaglini, Morton and Palfrey 2008, and Banerjee et al. 2010). We examine the reserve causal effect, that is, whether people acquire information in preparation to vote once forced to vote. The theoretical expectations are controversial. Rational choice theories built on a pivotal voter framework conclude in favor of a "rational ignorance hypothesis," stating that any single vote would make a difference and the cost of acquiring information would always exceed the benefit of voting (Martinelli 2006). However, other models that depart from this framework find that acquiring political information can be consistent with optimal voting behavior (Feddersen and Sandroni 2006; Degan 2006). On the whole, whether people acquire costly information and get involved in the political process when exposed to compulsory elections is an empirical question, and experimental evidence is mixed in this regard.[5]

While some literature has studied the association between forced voting and political behavior, these studies are based on cross-country comparisons[6] or field and lab-experiments.[7] This is the first paper that studies the relationships between com-

[4] See Lijphart (1997) for a literature review.

[5] In a field experiment, Loewen, Milner and Hicks (2008) provided monetary incentives to a randomly assigned group upon the condition that they vote. They found that this group did not become more informed than the control group, whose members were not offered any incentive to vote. Seebauer and Grosser (2006) studied this relationship in the laboratory with a voting game using costly private information. They found that participants were significantly more likely to acquire costly information when they were forced to vote than when they had the opportunity to abstain.

[6] See Engelen and Hooghe (2007) and Czesnik (2007).

[7] Loewen, Milner and Hicks (2008) and Seebauer and Grosser (2006).

pulsory voting, turnout, information acquisition and ideology in a large-scale election under real-world incentives faced by individuals. Comparisons are made among similar individuals who face choices regarding the same politicians and presumably differ only in terms of their date of birth and consequently their voting requirements. Hence, we are able to identify the impact of the requirement to vote using a clear quasi-experimental design through a regression discontinuity (RD) approach. As a complement to this analysis, we conduct instrumental variable regressions (IV) using exposure to the compulsory system as an instrument for voting turnout in order to identify local average treatment effects of the requirement to vote.

More generally, this paper contributes to a large body of literature on the impact of voting turnout on individuals' citizenship.[8] Using US data, previous studies find that voting eligibility polarizes individuals (Meredith 2009).[9] It is plausible to assert that the opportunity to vote affects those who are willing to participate in elections. The estimated effects of democracy reported in this paper are more compelling and unanticipated, as they are based on compulsory voting. This is the first paper that estimates the causal effect of compulsory voting on political preferences and information acquisition of those who abstain from the political process (i.e., non-voters). Finally, our data also provide an opportunity to investigate the heterogeneous effects of voting among individuals from different socio-economic backgrounds. Perhaps the most relevant argument in favor of compulsory elections is distributive, as voters are more likely to come from higher socio-economic statuses than non-voters are (Benabou, 2000). We are able to test whether and how these different groups react to the compulsory voting legislation.

[8] Some of those find a positive relationship between voting and subsequently turnout (Denny and Doyle 2009; Gerber, Green, and Shachar 2003; Plutzer 2002) or establish a causal effect of voting effect on subsequent political preferences (Kaplan and Mukan 2011; Elinder 2011; Meredith 2009; Mullainathan and Washington 2009). These studies are based on evidence from voluntary voting.

[9] Like this paper, he uses an RD approach to determine a voting effect. He compares party registration in California among those who, in the previous election, were "almost eligible to vote" with those that were "just eligible to vote."

We find that the requirement to vote increases turnout by 18 to 27 percentage points (p.p.) among the population, representing an increase of at least 50% in the size of the voting population. Results from RD and IV regressions and RD graphical inspection suggest a modest, if any, impact of compulsory voting on information acquisition. These are driven mostly by individuals whose mother has no college education. They increase their consumption of information through the internet and improve their comprehension about the political spectrum. On the other hand, we find that individuals become significantly more likely to express preference for a political party and to become more ideologically polarized among those with an educated family background.

In the second part of the paper, we test whether this voting effect is permanent and whether it varies with more voting experience by examining how the number of experienced compulsory elections correlates with individual political preferences. To separate the effect of voting experience from aging, we conduct regressions controlling for year-of-birth fixed effects, quarter-of-birth polynomials and alternative specifications using year-of-birth polynomials as control variables. We find that, after exposure to one compulsory election, citizens become approximately 15% more likely to align with a political party. This effect lasts for at least three election cycles (or six years). Further voting experience has no additional impact on individuals' political preferences. These results point to the important role of voting, even when imposed, in increasing individuals' involvement with politics. These results are new and relevant to a full comprehension of how democracy affects individuals and the consequences of compulsory voting.[10]

This paper proceeds in four sections. In Section 2, we explain the Brazilian electoral institutions and describe the data. In Section 3, we present the results relating to the effect of being forced to vote on information and ideology. Next, we estimate the frequency and permanent effects of voting on political preferences. We conclude in

[10] There is extensive literature related to this topic as summarized by Lijphart (1997). He informally discusses the distributive advantages related to the increase in turnout. Other studies discuss welfare implications related to this possible change in election outcomes using a theoretical framework (Krasa and Polborn 2005, Krishna and Morgan 2011).

Section 4.

2 Data

2.1 Some Background on the Brazilian Election System

Mandatory voting was introduced in Brazil in 1932, when the country's first Electoral Code was created following the Revolution of 1930.[11] In 1964, a coup d'état initiated a period of 21 years of military rule in the country, during which the regime controlled the electoral process according to its interests through a series of institutional acts, constitutional amendments, laws and decrees. Direct elections for president, governors and mayors of strategic municipalities were suspended, and existing political parties were again extinguished. A new transition to democracy began in 1985, when a constitutional amendment re-established direct elections in the country, reinstating the right to vote (rather than the obligation) for those older than 18 and extending it to illiterates.

In 1988, the current Brazilian Constitution was promulgated, adopting compulsory voting (henceforth, CS) for literate individuals between 18 and 69 years old and voluntary voting (henceforth, VS) for citizens who are illiterate, over 70 years old, or between 16 and 18 years old (TSE).[12]

Democratic elections are currently held every second year in Brazil. All voters must

[11] One of the principles of the Revolution was the moralization of the electoral system. One of the first acts of the provisional government was the creation of a commission to reform the electoral legislation. Advances in the electoral legislation were subsequently included in the Constitution of 1934; in 1937, however, a new constitution was imposed by President Vargas extinguishing the Electoral Justice, dissolving the existent political parties and suspending direct elections. The deposition of President Vargas in 1945 marked the redemocratization of the country, with the reestablishment of the Electoral Justice and the restoration of rights suppressed in 1937. Voting once again became mandatory for all citizens over 18, except for military officers and citizens over 65 years (illiterates were not allowed to register).

[12] www.tse.jus.br/internet/ingles/historia_eleicoes/eleicoes_brasil.htm

register; when individuals who are required to vote fail to do so and fail to provide justification to the electoral authority, they must pay a small fine.[13] Stronger sanctions are applied to those who fail to justify their absence for three consecutive elections; they are not allowed to issue or renew their passports and national identity cards and also become ineligible for public education, public jobs, cash transfer programs and credit by financial institutions maintained by the government. The legal requirement refers to showing up at the polls; all voters have the option of casting an invalid vote (this option is available on the ballot).

One can claim that voting is not in fact compulsory in Brazil, since the option of justifying the absence is available. However, this practice is not commonly used. According to records from the Tribunal Superior Eleitoral (TSE), in the 2006 Presidential Elections, 83% of the total electorate opted to turn up at the polls instead of justifying an absence.[14]

Official records only give information about turnout and only at the aggregate level. An analysis like the one proposed in this study demands survey collection. This took place in the week immediately following the first round of the 2010 Presidential Elections (October 3). At that time, there were three main candidates running for election: Jose Serra, Marina Silva and Dilma Rousseff. Their share of votes accounted for 98.8% of the total valid votes.[15]

[13] In 2011, the fee was between R $1.06 (US $0.66) and R $3.51 (US $2.19), which is equivalent to 0.29% of the average income in the country, according to IBGE, Population Census 2010.

[14] This includes Brazilians living abroad or in cities others than those in which they are registered. Brazilians can only vote in the states in which they are registered, and they can only vote in person. According to TSE, 40.78% of Brazil's residents that justified their absence in the 2006 Election were living in different states from where they were registered.

[15] In the 2010 Election, no candidate received more than 50% of the valid votes in the first round, so there was a runoff between the two leading candidates. In the second round, Dilma Rousseff beat Jose Serra by 12.2 p.p. (56.1% versus 43.9%).

2.2 Survey, Sample and Descriptive

A total of 5,559 students were surveyed in their classrooms from October 4-7, 2010. To exploit the dual system, we conducted the survey among individuals who face compulsory voting and among those who face voluntary voting. The survey sample included students in three types of institutions—public high schools, a preparatory school for college admission and a large university—in 109 classrooms in the city of São Paulo, Brazil.[16]

While conducting the surveys, the same procedure was applied across all institutions: An interviewer entered the classroom about 15 minutes before the end of a class, read an introductory script, and distributed the questionnaires to all students. Teachers also collaborated in this project, soliciting attention and consideration to the survey. Students then had 10 to 12 minutes to individually answer the questions. To limit strategic responses, survey participants were not informed about the precise purpose of the survey, and there was no specific mention about compulsory or voluntary electoral systems. The survey was entitled, "Young Adults Political Behavior," and related to the Universidade de Sao Paulo.[17]

In every classroom, four types of questionnaires containing exactly the same questions but in different orders were randomly distributed to students in order to prevent cheating. The survey consisted of a comprehensive set of questions about demographics, political inclination, vote, media consumption, sentiment toward voting and a political quiz to evaluate the respondents' levels of political knowledge. Most students agreed to answer the survey, and 93% of the respondents declared to have answered it in a serious manner. Respondents had been told they could skip any question, but the vast majority of students answered them. Regarding one sensitive question (whether they had voted and for whom), only 1.26% abstained from answering; 0.27% chose the alternative. In

[16] São Paulo is the largest metropolis in Brazil and among the cities with the highest income per capita in the country.

[17] After returning the completed questionnaire, students received an information sheet containing contact information for the authors.

addition, only 2.36% of them skipped open questions in the political quiz. These high rates of participation might be a response to their teachers' request to collaborate and take the survey seriously.

The sample is composed partly of high school seniors from three public high schools: Escola Estadual Professor Ascendino Reis, Escola Estadual Rui Bloem and Escola Estadual Professor Leopoldo Santana. The second sample is composed of students taking a preparatory course for college admission exams (*cursinho*) at Anglo Vestibulares. These are referred to as Anglo students. They are mostly high school seniors or students who just finished high school but have not yet been admitted to college. While public high school and Anglo students have similar ages, they differ in socioeconomic characteristics, the latter group being more affluent. The last sample consists of freshmen from the Universidade de São Paulo (USP). We surveyed freshmen from the following majors: history, sociology, business administration, economics, physics, architecture, law, mathematics and literature. The students in this sample are older. From the 5,559 surveys collected, 3,703 were completed by Anglo students, 728 by public high-school students and 1,128 by college students. In the Appendix, Table A describes the socioeconomic for these three samples.

Table 1 shows turnout rates (as a fraction of total population) by age group for Brazil and within the sample. Turnout rates are higher among individuals who face compulsory voting. Of note is that, over-reporting of voting turnout is a recognized issue in surveys (Anderson and Silver, 1986). While this is possible among our survey participants, the resemblance between the rates of self-reported turnout by age in our sample and official rates in the country, as shown in Table 1, is much closer than typically reported in international surveys in the US or the UK, for example.[18] In section 3.1, we address this issue in greater detail and provide some evidence that voting over reporting does not affect our estimated effects of turnout.

[18]For example, Swaddle and Heath (1989) finds that reported turnout in the 1987 British General Election Study was 10 p.p. higher than the official rate, and larger discrepancies are detected in the American National Election Study in the US, as reported by Anderson and Silver (1986).

Table 1: Turnout - Brazil

	Turnout %	
Group Age	Brazil	Sample
16	17.7	17.64
17	42.6	39.71
18 to 20	82.6	85.64

Note: Brazil's turnout is from TSE and IBGE

Through this paper, we examine five main set of outcomes related to information and ideology. The first outcome is the respondent's actual knowledge about politics, measured by the performance in the political quiz.[19] The second is based on the performance in one of the quiz questions.[20] The outcome is an indicator of whether individuals were able to correctly distinguish the most right-wing party among two extreme alternatives (distinguish right-wing party). The third set of outcomes are several mechanisms of information acquisition, namely the number of days an individual consumes politics in the media per week via several outlets—TV, newspaper or magazines and the Internet—and whether he discusses politics frequently with parents or friends.

The remaining outcomes are ideology measures. The fourth outcome is based on students' ideological position. We record whether a respondent was self-declared extreme oriented—strongly left- or right-wing—as opposed to moderately left- or right-wing or center oriented. The fifth outcome is a measure of individuals' partisanship. We asked, "Do you have a preference for a political party?" We classified those who answered positively to the question as Partisan. Whereas these are only self-reported measures of ideology, they are highly correlated with candidate voting choices and political engagement. While partisans vote at least 70% of the time for the candidate from the preferred party and only 2.11% declare the intention to invalidate their votes, non-partisans are

[19] The quiz consisted of 14 questions, which are provided in the appendix. Twelve were about the three main candidates running in the presidential elections. More specifically, there were three open-ended questions about the previous political experience of each of the candidates, and four multiple-choice questions about policies previously implemented or supported by the candidates.

[20] Question 14 in the quiz.

equally likely to vote for any of the three main candidates, and 7.76% plan to invalidate their votes. A preference for a political party may convey conviction for its policy and thought for choosing a preferred alternative.[21]

Table 2 shows the descriptive statistics of respondents' behavior and characteristics for the whole sample (Column 1) and a comparison according to their voting turnout decisions in the 2010 Presidential Elections (Columns 2 and 3). Out of the 5,559 students surveyed, 77.08% declared to have voted in the 2010 Presidential Election. Voting turnout is positively correlated with exposure to the compulsory voting system, which explains at least in part why voters are older than non-voters in the sample.

Consistent with evidence from the US (Degan and Merlo, 2011), voters consume more information and are better informed about politics. They are also more likely to declare a preference for a political party and are more politically polarized (more likely to self-declare as right- or left-wing). In terms of demographic characteristics, voters are slightly older, richer, more likely to be white, and more likely to have a mother with a college degree than non-voters are.

Table 2: here

Differences between voters' and non-voters' preferences and attitudes can cause and/or be caused by voting. In order to overcome this endogeneity issue and estimate the causal effects of voting on behavior, we explore exposure to the compulsory legislation using a regression framework.

[21] The data also show that partisans are more knowledgeable and more positive towards voting than non-partisans. All of these patterns are consistent with the understanding about determinants of alignment with a political party (Brader and Tucker, 2001; Miller and Shanks, 1996).

3 Results

3.1 The Effects of Being Forced to Vote

In this section, we present the results of the immediate impact of exposure to the compulsory voting legislation. We restrict the sample to individuals that could face up to one compulsory election in order to estimate the effect of just being forced to vote. First, we present the results from a regression discontinuity framework to identify the effects of the introduction of compulsory voting legislation in the population's behavior. Then we show graphical evidence suggestive of a causal effect of forced voting on turnout and on other political outcomes. Finally, we conduct IV regressions to quantify voting causal effects.

3.1.1 Regression Discontinuity Results

We use a sharp regression discontinuity framework comparing individuals whose ages are around the threshold that determines the change from the voluntary to the compulsory voting system.[22] We estimate the following equation:

$$y_i = \gamma + m(S) + \beta_1 1(S > 0) + u_i \quad (1)$$

where y_i is the outcome of individual i, $m(S)$ is a continuous function of S, or the distance between the 2010 Election Day and the date the individual had turned or will turn eighteen, $1(S > 0)$ is an indicator equal to one if the respondent was required to vote on 2010 Election Day and u_i is a random error term. We estimate (1) assuming a lower-order polynomial functional form for m() that is flexible on each side

[22] The sharp RDD design is equivalent to the case in which respondents have a perfect knowledge about their voting rights and obligations. In an earlier version of this paper, we estimated the causal effects of voting in the context of "fuzzy RDD." We tested whether political outcomes responded to the perception of the obligation to vote, when this variable was instrumented by exposure to compulsory voting. The results are similar and have the same qualitative implications as the ones presented in this section.

of the cutoff, and clustered standard errors on classrooms. In addition, we estimate the effects controlling for demographic observable variables.

A possible concern is that the results may be sensitive to outcome values for observations far from the cutoff that determines the change in the voting system. For this reason, our estimates use only data within a bandwidth of 15 months from the cutoff, excluding individuals that faced more than one compulsory election or that had not yet had the opportunity to vote.

The identification relies on the orthogonality assumption between age and unobservable factors (such as political preferences that determine individuals' political outcomes) for those close to turning 18 (under VS) or those who have just turned 18 (under CS) by election time. This assumption cannot be entirely verifiable; however, it can easily be rejected. As discussed by Lee and Lemieux (2009), a simple test is to fit regressions for possible confounding variables and test for jumps at S=0. We estimate (1) using several covariates, such as demographic characteristics (X_i) as the endogenous variable. Table 3 shows the results. As discussed in Section 2, participants in different schools differ in terms of demographics and age. For this reason, our analysis is conducted by exploring variation within schools. Table 3 shows these results. Coefficients were not statistically significant for most of the variables, including demographics and family characteristics.[23] One relevant exception is previous voting experience. The 2010 Election was the first opportunity for all respondents in this sample to vote in a presidential election; nonetheless, a small part of the participants older than 18 had the opportunity to vote in the 2008 local elections. The predicted fraction of second-time voters on the right side of the threshold is approximately 5% higher than on its left side. The fact that local elections are not as renowned as presidential elections and that the fraction of second-time voters is small in comparison to the change in turnout (as it will be shown in Table 4, Figure 2) gives some confidence that this is not a relevant confounding.

[23] For the specification in Column (1), we find that females are under-represented on the right-side of the threshold. In spite of that, we do not find evidence that gender explains voting behavior in our sample.

Since the effects are identified at the age of 18, one can claim that, at this particular age, youngsters start feeling more responsible because they reach the legal majority, which could result in a confounding factor. We obviously accept the fact that other opportunities and responsibilities that become available after one turns 18 might change individuals. However, we believe that this happens gradually and not abruptly at the 18th birthday. We tested whether students changed their behavior regarding their propensity to apply for college admission exams or to respond seriously to the survey at 18-year old threshold. In line with our expectations, none of these changed (Table 3).

Table 3: here

Lastly, there is concern about the fact that the survey information is based on self-reported behavior. This could result in non-random sorting across the threshold (i.e., the choice to participate in the survey correlates with participation in the election). In this case, a jump in the number of observations around the threshold would occur. Figures 1A and 1B show a plot with the number of observations by age and the percentage of observations with non-missing reporting values in any of the characteristics controlled in the main regressions, respectively. There are no visible discontinuities around the threshold for both of these variables.

A related concern is that voting over-reporting might be accentuated among those exposed to the compulsory system, if those legally obliged to vote feel more compelled to declare to have voted. One simple test to detect such behavior is to verify differences among voters' level of political knowledge around the 18-year-old threshold. This is a proxy for actual vote participation, given that non-voters are less informed than voters as shown in Table 2. We conducted RD regressions restricting the sample to voters and found no difference among voters on either side of the 18-year old threshold, as

illustrated in Table 3.[24] Graphical evidence is presented in the Appendix. This gives us some confidence that respondents have been sincere about their voting participation and that over-reporting is not biasing our estimates on turnout effects presented next.

Figures 1A and 1B *here*

Before presenting the results, we briefly comment on the regression specification. In this section, we report regression results using first- and second-order polynomials. To determine the choice of polynomial order, we run regressions including a set of bin dummies and conduct an F-test to their joint significance. For most of the outcome variables, only first-order polynomials are relevant in explaining outcomes and specifications, and using higher-order polynomials represents an overfit of the data.[25] In addition, we conduct a cross-validation procedure (Imbens and Lemieux, 2008) to identify the optimal window width, which turned out to be 15 months for most outcome variables.[26]

Turning to the results, we first perform a simple graphical analysis to check for discontinuities at the 18-year threshold. Figure 2 plots turnout (as a share of population) and the proportion of those who perceive to face the voting requirement by age on Election Day. Dots indicate the average values in a month interval, and we include a predicted line based on a second-order polynomial flexible on each side of the cutoff for ease of visualization. The vertical line indicates the 18-year threshold; i.e., those under compulsory voting legislation. While turnout raises progressively with age for individuals younger than 18, this pattern disappears after exposure to the compulsory voting legislation; there is a clear spike in these variables among those at the age of

[24] We conduct this exercise for other outcomes in which voters and non-voters present different behaviour. The regressions do not detect any jump around the 18-year old threshold, for any variable. These results are not reported for the sake of space, but are available under request.

[25] We repeat this procedure for regressions using higher-order polynomials (third and fourth) and find similar results.

[26] We conducted a regression considering smaller windows of width, 9 and 6 months, and find the same qualitative results with similar coefficients' size.

18. Figure 2 suggests awareness about the legislation, and the effect on the compulsory legislation in increasing voting turnout. The magnitude of the jump suggests that the legal obligation to vote affects the majority of non-voters.

Figure 2 *here*

The regression results are consistent with Figure 2 (Table 4, Column 1). The estimates for the effect of compulsory voting on turnout vary between 27.2 and 18.6 p.p., depending on the specification. These numbers are higher than previous estimates[27] and represent an increase of at least 50% of the voting population. Since this effect is estimated in a country where most of the adult population votes, it is possibly only a lower bound number for the effect. Those under a voluntary voting system are potentially exposed to some social pressure from the remaining population.[28]

We proceed by looking for changes around the 18-year old threshold for measures of ideology and political information as a possible consequence of this increase in turnout. Table 4 presents the results for two indicators of political information measured by participants' performance in the political quiz. When considering the overall performance in the test (Columns (3)-(4)) and Figure 3, the results do not reveal any discontinuity around the 18-year threshold. The estimated coefficients for any of the tested specifications are not statistically significant, and their size is practically equal to zero. Additionally, we report the results for the probability of being able to distinguish the most right-wing party among two alternatives (Columns 5-6, and Figure 4) and find only weak evidence of an increase in knowledge about the political spectrum at the 18-year threshold.

[27] Using aggregate data in cross-country comparisons, Jackman (1987) and Power (1995) estimate the magnitude of this effect to be between 10 and 15 p.p. Hirczy (1994) finds that the turnout in Carinthia, Austria, increases by 3 p.p. in comparison to other Austrian provinces after the adoption of compulsory voting. He also finds that the abolition of compulsory voting in the Netherlands in 1970 caused a drop of 10 p.p. in turnout.

[28] Previous studies find evidence that people react to the incentive of complying with social norms by becoming more likely to vote in both field (Gerber et al., 2008) and natural (Funk, 2010) experiments.

Table 4: here

Figures 5 and 6 and Columns (7)-(10) in Table 4 present the results for ideological outcomes, namely preference for a political party and identification as extreme oriented. The coefficients related to the 18-year threshold are positive and statistically significant. The magnitude is relevant; it indicates that the exposure to compulsory voting leads to an increase in individuals' propensity to self-identify as extreme oriented in 3.55 p.p. (or by an increase of 72% on the mean of the population exposed to the voluntary voting) and to an increase of approximately 5 p.p. in the probability of having a party preference (or to an increase of 18% on mean of the VV population).

Figures 3-6 *here*

It is important to note that these RD regressions only identify the average impacts for the population. Based on these results, it is unclear which segment of the population is reacting to the compulsory voting legislation or if non-voters are encouraged to engage in politics once they are forced to vote. We test the later hypothesis in the next section, with an instrumental variable approach.

3.2 Instrumental Variable Analysis

In this section, we perform IV regressions to estimate the effect of voting on information acquisition and political preferences using exposure to the compulsory voting system as an instrument for turnout. This variable is highly correlated with turnout but conditional on age; variation on the exposure to the compulsory system should not, by itself, increase political engagement.

In the presence of heterogeneous effects, this method estimates the average treatment effect for individuals who change their treatment status (i.e., become voters),

because they react to the instrument (Imbens and Angrist, 1994; Oreopoulos 2006). We estimate the following equations:

$$\text{First Stage: Vote Turnout}_i = \eta_0 + \gamma_1 1(S_a > 0) + H(age) + \varpi_i$$

$$\text{Secong Stage: y}_i = \nu_0 + \gamma_2 \text{Vote Turnout}_i + \beta X_i \ + H(age) + \epsilon_i$$

Table 5 reports the IV results for the effect of compulsory voting and citizens' political behavior for subgroups according to their mothers' education level and uses alternative age polynomials $H(age)$ as controls. Consistent with the results from Table 4, there is some evidence that individuals become more polarized in response to compulsory voting (Columns 1 to 6). We find heterogeneous effects according to the mothers' level of education. Among the group whose mothers had some college education, once forced to vote, individuals become 35.5% more likely to declare to have a preference for a political party and become 15.4% more likely to self-declare ideological extreme (Columns 2 and 5). For the group whose mothers do not have any college education, the regression results do not detect any change in ideology (Columns 3 and 6).

Table 5: here

In terms of political information, neither group, once forced to vote, shows improvement in performance on the political quiz. We find some weak evidence of an increase in knowledge of the political spectrum in response to compulsory voting. As shown in Column 11, individuals whose mothers do not have any college education become approximately 30% more likely to be able to distinguish the most right-wing party, once forced to vote. This effect is only statistically significant at the 13% level, in regressions using a first-order polynomial of the forcing variable as a control for age.

A natural question is why, once forced to vote, individuals that have more educated mothers become more polarized and take a stand in choosing a political party, while others from less-educated backgrounds do not. Do they exert more effort to acquire information? In Table 6, we present the results from IV regressions in which the related dependent variables are several mechanisms of information acquisition, including discussion with parents and friends and media consumption. Results for the sample with college-educated mothers are reported in Column (2). None of the related coefficients are statistically significant, indicating that neither of these variables explains the change in ideology. On the other hand, among those whose mothers do not have any college education (Column 3), the regression results indicate that, once forced to vote, they increase the consumption of political information by 1.6 days per week on the Internet. That may partly explain how they enhance their comprehension about the political spectrum. Some possible alternative explanations for why individuals from more educated families react by changing their ideological position rely on the fact that they are more knowledgeable about and involved in politics. Based on the results not shown in the paper, they follow politics more often in the media and demonstrate better performance on the political quiz. These are contributing factors for individuals as they make a decision. Alternatively, parents can be information shortcuts, providing simple advice, which, once individuals need to vote, are used to decide on a position.

Table 6: here

The effects estimated in this section reflect the immediate impact of the exposure to compulsory voting. The polarization effect—measured by individuals' decision to align with a political party—may or may not accentuate or dissipate with more voting experience over the individual's life cycle. We address this issue in the next section.

3.3 Frequency and Permanent Effects of Voting on Political Preferences

In order to estimate the frequency effects of compulsory voting and test whether they are persistent, we exploit the variation in the number of compulsory election seasons experienced by individuals. We consider a broader age sample of survey respondents and make cross-age comparisons to identify voting effects. This is a valid exercise, since the whole population has been exposed to the compulsory voting intervention at the age of eighteen. To conduct this exercise, we restricted the sample in two ways. First, we excluded those born before 1988, as they constitute only 7.5% of the sample, and due to the low number of observations, the results regarding the effects of further voting could be inconclusive. In addition, due to the fact that the data come from a survey conducted in classrooms and not from the general population, age can convey information on other possible relevant individuals' unobservable characteristics. For these reasons, we excluded the oldest 15% and the youngest 15% in every school among non-college students and in every major among college students.[29]

We start by investigating the marginal effects of each additional compulsory election faced by individuals. In this analysis, the main confounding factor is age. To circumvent this problem, we conduct regressions controlling for year-of-birth fixed effects (θ_a), quarter of birth, and quarter of birth squared (θ_q), estimating (2). As a result, the effects are identified within the year-of-birth variation in individuals' quarter of birth.[30] For this specification, only frequency effects of voting are identified, as the coefficients capture the effect, in the margin, of having faced an additional election.

[29] The results are robust to the exclusion of longer tail of age distribution, of 20%.

[30] Recently, Buckles and Hungerman (2010) have shown that, for the US case, differences in quarter and month of birth convey differences in children's socio-economic backgrounds. If this relationship holds for the Brazilian data, it can be a potential confounder to identify the effects of voting. We test this hypothesis in our dataset, and the tests do not detect individuals' demographic differences based on quarter or month of birth.

$$y_i = \delta_0 + \delta_1 OneElection_i + \delta_2 TwoElections_i + \delta_3 ThreeElections_i + \theta_a + \theta_q + \beta X_i + \varpi_i \quad (2)$$

The key independent variables are dummies indicating whether the respondent has been exposed to one (*OneElection*), two (*TwoElections*) or three (*ThreeElections*) compulsory elections. Columns (1) to (3) in Table 7A present the results, respectively, for the samples, including respondents born no earlier than 1988 (exposed to up to three compulsory elections), 1990 (exposed to up to two compulsory elections) and 1992 (exposed to up to one compulsory election).

The results in Table 7A, Column 3, show that individuals exposed to one compulsory election are 17.2% more likely to declare a preference for a party than those who never faced a compulsory election. While the size of coefficients does not decrease with the number of experienced compulsory elections, we cannot infer increasing effects of voting. For all relevant regressions (Columns 1-2 in Table 7A) and specifically for the regression specification shown in Column 1, a Wald test does not reject the hypothesis that the size of the coefficient related to the first compulsory election is statistically different from the second compulsory election (p-value=92%) or the third one (p-value=48%).

Tables 7A and 7B *here*

To determine whether and to which extent the effect of the exposure to compulsory elections is a lasting one, we test whether political preference changes permanently according to the number of experienced compulsory elections. We construct three dummies indicating whether the respondent has been exposed to at least one election, at least two elections or at least three elections. To control for the age effect, instead of including year-of-birth fixed effects controls, we use year of birth and year of birth squared as controls in these specifications. The results are reported in Table 7B. Column 1 shows that, after being exposed to at least one compulsory election, individuals become 15.42% more likely to declare a preference for a political party than those who have never experienced a compulsory election, for at least 3 elections cycles (or 6 years).

This result is robust to different sample sizes. As shown by the standard errors of coefficients alfa1 and alfa2 in Columns 2 and 3, we cannot infer that the exposure to more compulsory elections (than the first one) changes individuals' probability to express preference for a political party.

These results are somewhat in line with those of previous studies, based on the notion that voting may change citizens' attitudes, such as their level of partisanship (Finkel 1985). For example, Meredith (2009) finds that past eligibility to vote increases citizens' likelihood of registering with a political party in the subsequent elections by 2.2% when compared to first-time-eligible voters. His findings are consistent with increasing effects of voting, which can be an argument in favor of the compulsory voting system. In contrast, the results presented in Table 7B, Columns 1-3, suggest that exposure to the first compulsory election is the relevant event to change people's preferences. An F-test does not reject the hypothesis that the coefficients associated with exposure to more than one compulsory election are jointly zero at the five-percent level (Column 1).

3.4 Discussion

This paper presents new evidence of the effect of forced voting on political engagement—measured by an expressed preference for a political party. This is observed in the general population in an environment in which a comparison is made between potential voting populations exposed to voluntary or compulsory voting systems. Our results, to some small extent, confirm the conjecture made by Lijphart (1997, p. 10) about one of the benefits of compulsory voting regarding its potential "to serve as an equivalent form of civic education and political stimulation."

The magnitude of this effect is relevant. On average, non-voters become as likely to take a political position by expressing a preference for political party as (voluntary) voters. This is illustrated in Figure 7.

*Figure*7 here

We also find that this effect depends on family background. Only those whose mother has some college education react in changing their preferences. This indicates that the requirement to vote is not by itself a sufficient mechanism of political stimulation. Access to political information and knowledge are important, in conjunction to the obligation to vote, in order for individuals to decide in favor of a specific political party.

As it is widely documented in the US (Degan and Merlo, 2011), our data also show that (voluntary) voters are more informed than non-voters (Table 2). We do not find strong evidence of an effect of compulsory voting on political knowledge. Most of our results are in line with the classical view of rational ignorance and that the obligation to vote by itself would not lead people to engage in the costly process of acquiring information. On one hand, this gives support to the common view about possible negative consequences related to the compulsory voting legislation in terms of increasing in the polls the share of uninformed voters.[31] Addressing this issue is beyond the scope of this paper.[32] On the other hand, we also find some weak evidence of self-educational effects related to the obligation to vote among individuals from low economic background (whose mothers do not have any college education). This suggests that policies that encourage voting might be a way to decrease political apathy among the poor.

4 Conclusion

Voting lies at the heart of democracy. This study investigates the effects of compulsory voting on people's knowledge and political preferences. It circumvents the endogeneity problem and identifies the effects in question by exploring Brazil's dual voting system, which provides an exogenous shift in individuals' likelihood to vote. This paper presents a set of results new and relevant to public policy in terms of un-

[31] "High Turnout Would Be a Disaster" New York Times (11/ 11/2011).

[32] Leon (2012) studies the relationship between voting systems and election outcomes.

derstanding some of the consequences of the adoption of compulsory voting legislation. It also complements the current understanding of the effects of voting on individuals' citizenship.

We find large and significant effects of the legal requirement to vote on turnout (between 16 p.p. and 28 p.p.) These lead to some positive consequences in terms of increasing political involvement among the population, specifically the group of (voluntary) non-voters. We find only weak evidence that individuals react to the obligation to vote by acquiring information, and that holds only for individuals from lower economic backgrounds. On the other hand, our results are consistent with the fact that citizens give more thought to elections and politics when obliged to vote (as measured by a declared preference for a political party). This positive effect seems to be permanent (lasting at least 6 years) and related to one-time voting. In this sense, these results put in question the need for adopting a compulsory system in order to foster political involvement among the population.

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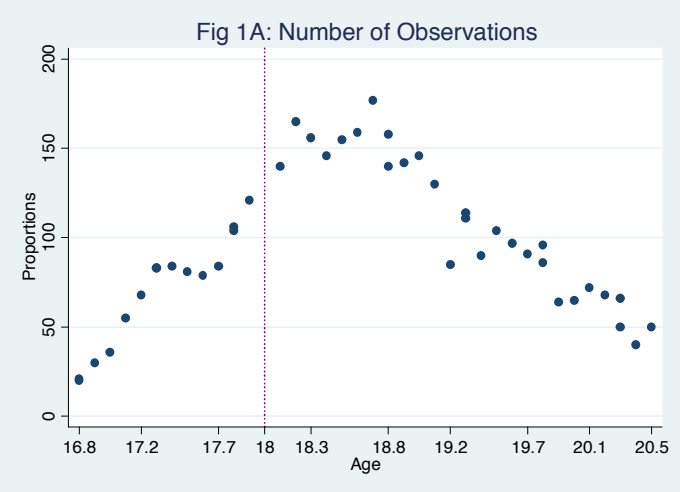
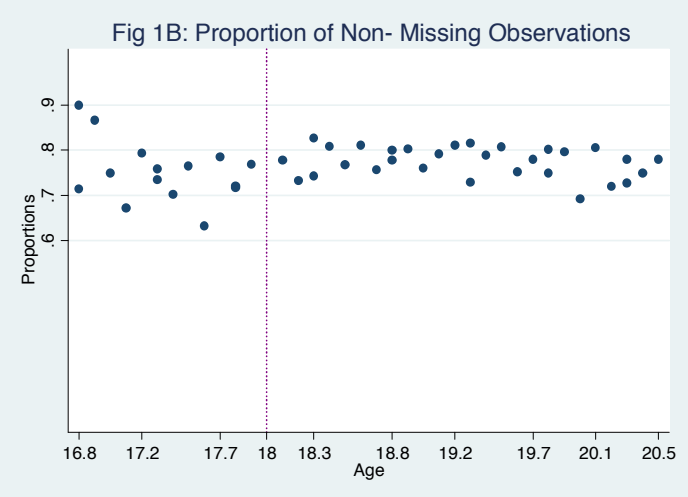
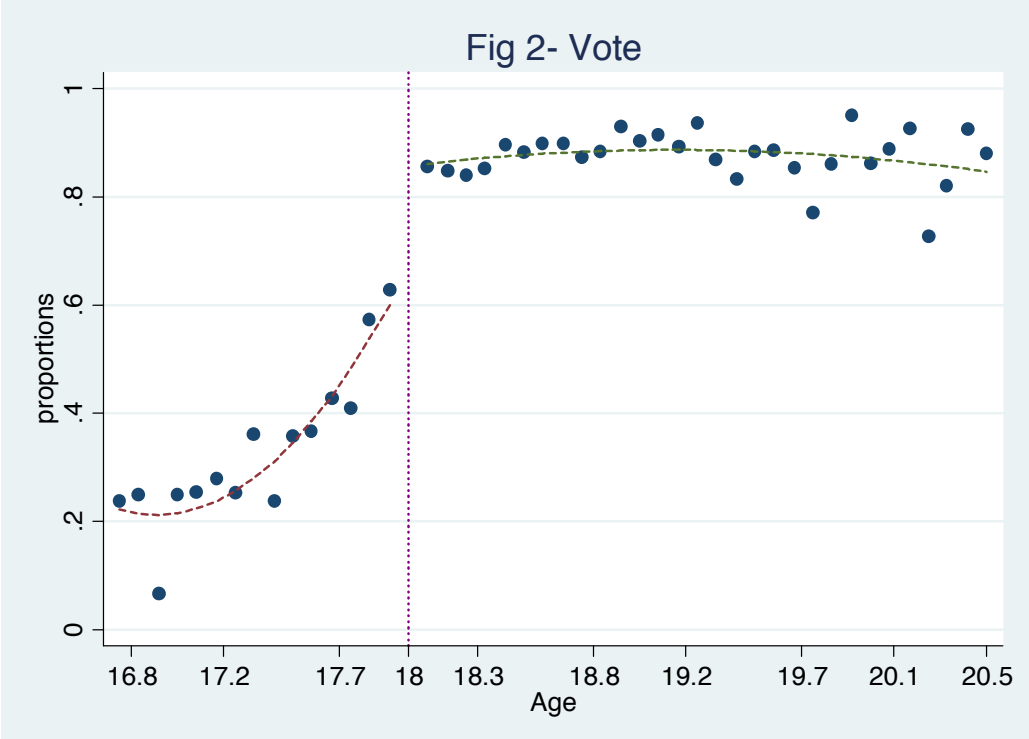
Figures 1





Notes: Dots in Figure 1A indicate the number of respondents with a distance from the cutoff within one month. Dots in Figure 1B indicate the ration between the total number of non-valid answers and the total number of respondents from the cutoff within one month.

Figure 2



Notes: Dots indicate average turnout in a one month interval. The curve is predicted from a second order polynomial flexible on each side of the 18- year threshold.

Figures 3-7

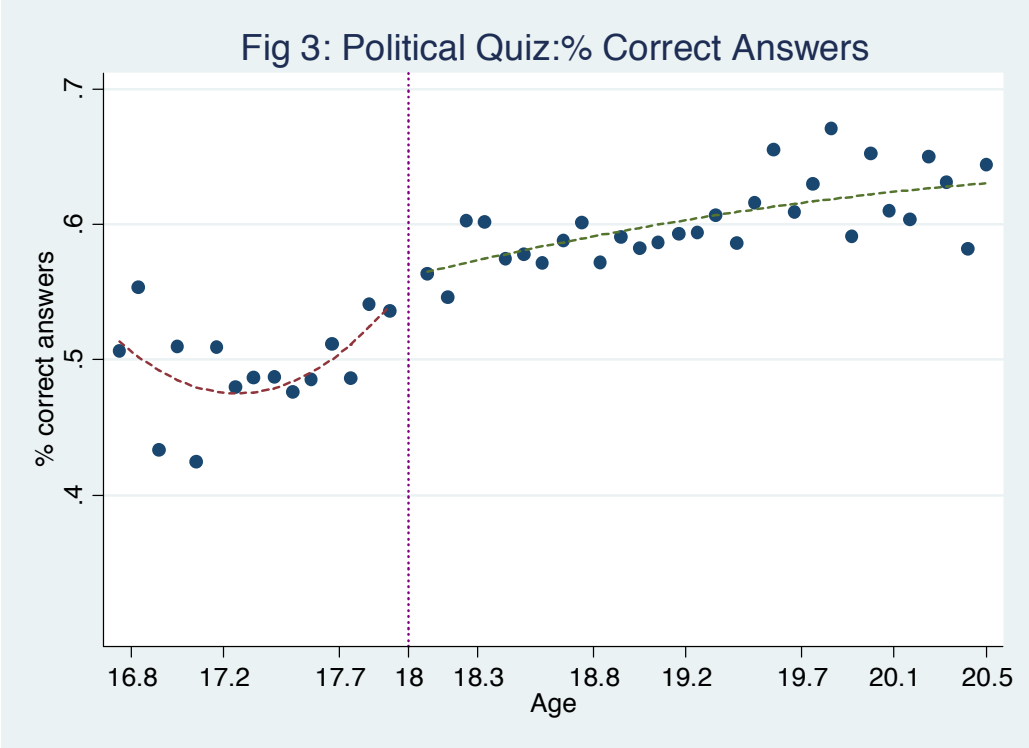


Fig 3: Political Quiz:% Correct Answers
% correct answers
.4
.5
.6
.7
16.8
17.2
17.7
18
18.3
18.8
19.2
19.7
20.1
20.5
Age

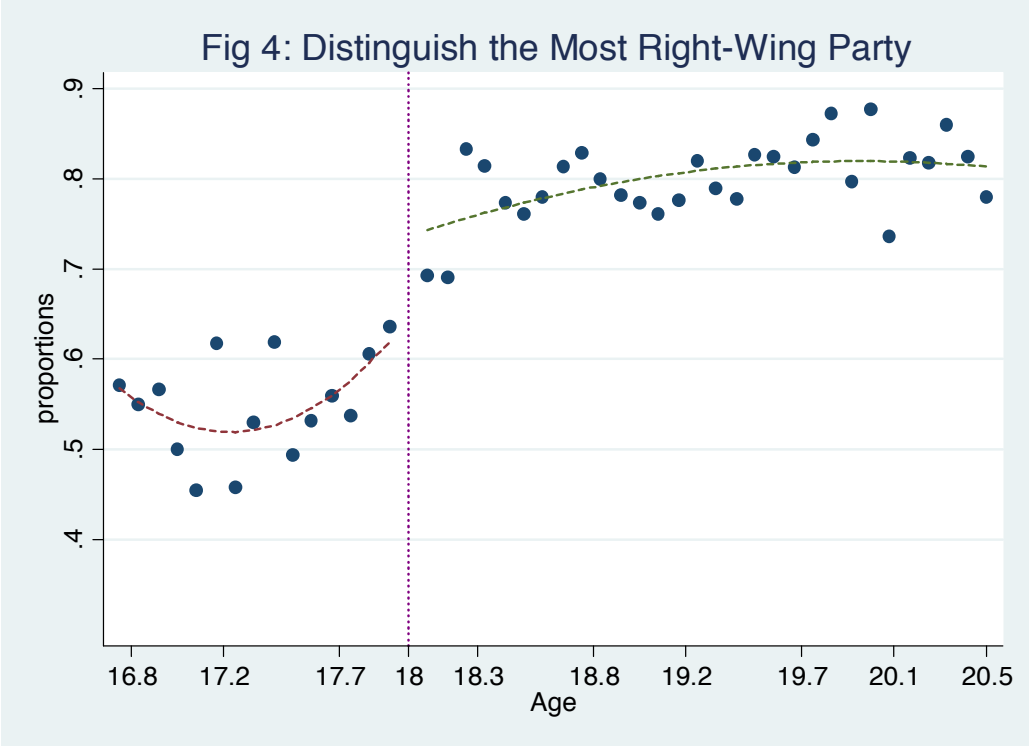


Fig 4: Distinguish the Most Right-Wing Party
proportions
.4
.5
.6
.7
.8
.9
16.8
17.2
17.7
18
18.3
18.8
19.2
19.7
20.1
20.5
Age

Fig 5: Prefers a political party

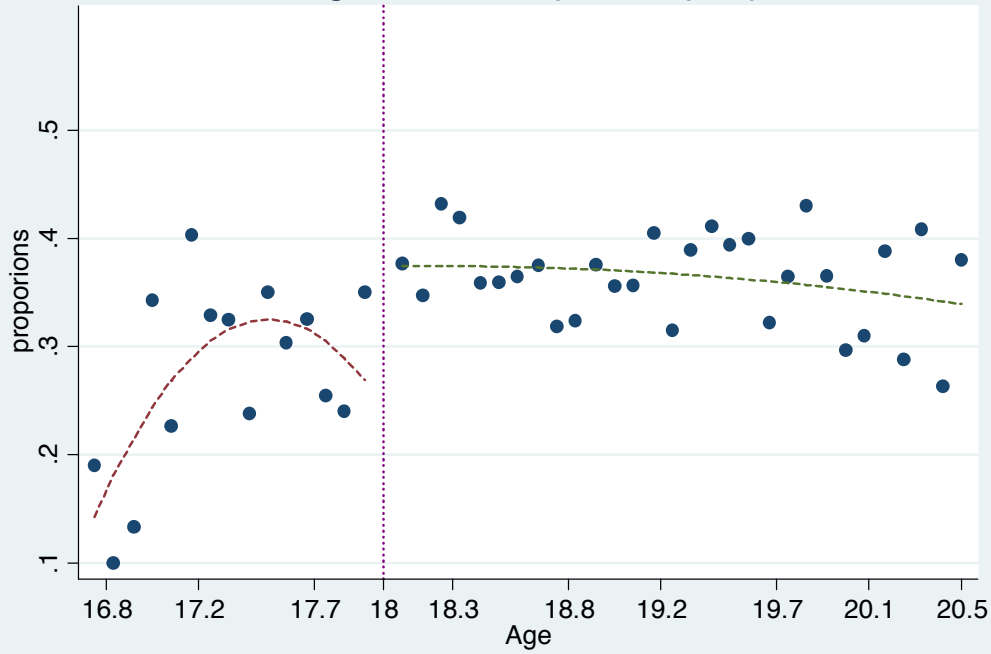
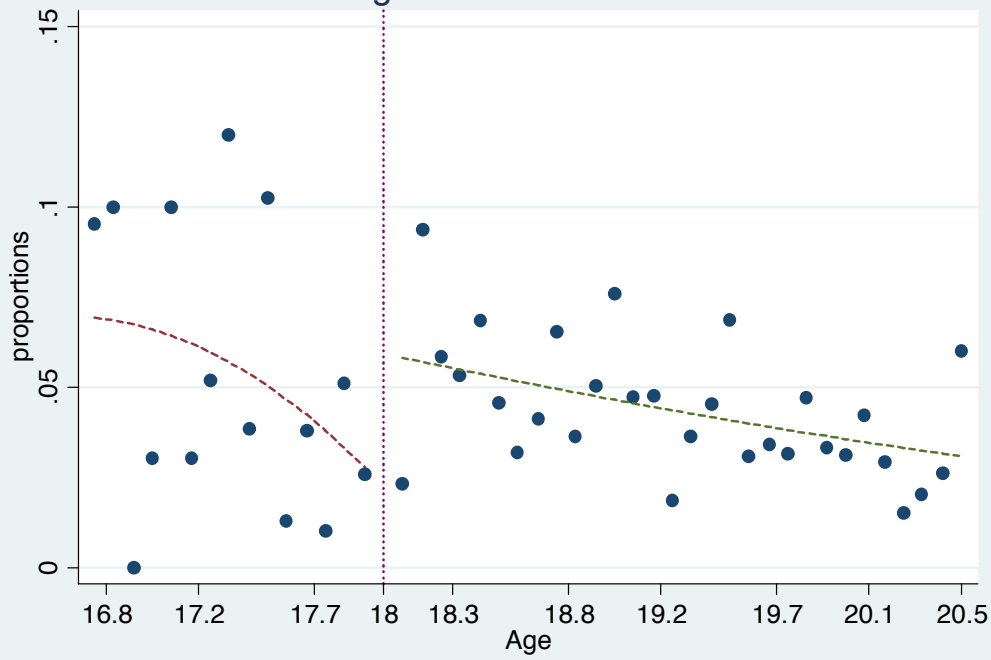
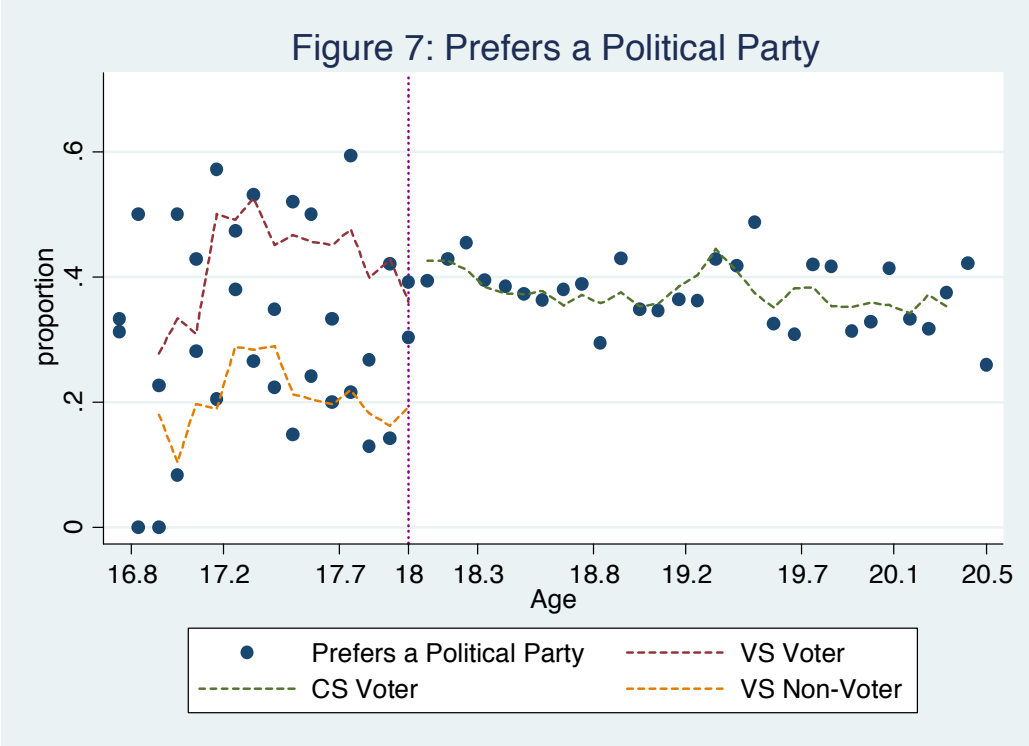


Fig 6: Extreme oriented





Notes: Dots indicate average turnout in a one-month interval. The curve is predicted from a 3-month moving average calculated separately for the each of the three categories (VS voters, VS non-voters and CV voters).

Table 2 - Outcomes and Characteristics of Individuals by Turnout Decision

Outcomes	Total (1)	Voter (2)	Non-Voter (3)	Difference (2) - (3)	
Political Information (in %)					
Political Quiz (% correct answers)	58.38	60.63	50.94	9.69	**
Able to distinguish which party is more right oriented among two choices (%)	75.23	79.13	62.34	16.79	**
Number of days a week learns about politics from:					
TV news	3.47	3.50	3.39	0.10	
Newspapers and magazines	2.45	2.55	2.14	0.41	**
Internet	3.44	3.61	2.87	0.74	**
Talk to parents frequently about politics	43.14	45.89	33.86	12.03	**
Talk to friends frequently about politics	46.2	46.56	44.89	1.67	
Political Inclination and Voting (in %)					
Has a political party preference	35.62	38.99	23.96	15.03	**
Extreme Left-wing	3.42	3.49	2.57	0.92	
Moderately Left-wing	23.01	28.05	20.15	7.90	**
Center	48.65	46.06	58.21	-12.15	**
Moderately Right-wing	23.27	25.88	21.64	4.24	**
Extreme Right-wing	1.65	1.59	1.82	-0.23	
Voted in the 2010 Election	77.07				
Voted before the 2010 Election	33.91	37.98	19.79	18.19	**
Characteristics					
Age	19.16	19.43	18.19	1.24	**
Female (in %)	57.09	56.66	59.16	-2.50	
White (in %)	76.32	78.00	70.93	7.07	**
Mother education : college or more	67.62	70.42	58.83	11.59	**
Has the Requirement to Vote (%)	80.09	90.17	45.89	44.28	**
Number of Observations	5,514	4,250	1,264		

Note: **Significant at the 5 percent level.

Table3 - Estimated Discontinuities in Pre-determined Characteristics

	(1)	(2)
Sample: All		
Dependent variable:		
White	0.0046	0.0142
	[0.0300]	[0.0422]
Female	-0.0816	-0.0589
	[0.0355]**	[0.0485]
Mother has some college education	0.0362	0.0447
	[0.0293]	[0.0400]
Mother has a political party preference	-0.0238	0.0281
	[0.0399]	[0.0549]
Live with a parent	-0.0205	0.0317
	[0.0224]	[0.0351]
Attend church frequently	-0.0659	-0.0544
	[0.0439]	[0.0571]
Plan to apply to College	-0.0047	0.0123
	[0.0226]	[0.0322]
Responded seriously to the survey	0.0161	0.0056
	[0.0183]	[0.0244]
Voted before the 2010 Election	0.0552	0.0406
	[0.0134]**	[0.0181]**
Sample: Voters		
Political Quiz (% correct answers)	-0.0117	-0.0098
	[0.0158]	[0.0208]
Age polynomial controls	linear	quadratic

Notes: 1) Explanations about the samples are in the text. 2) Standard errors robust to heteroskedasticity are in brackets. Entries are estimated regression discontinuities at S=0, from models that include age polynomial controls for S fully interacted with a dummy for age 18 or older. Other controls include school fixed effects. 3) **Significant at the 5 percent level.

Table 4 - Effects of the Compulsory Voting Legislation on Turnout, Political Information and Ideology- RD Results

Coefficient on Turning 18 (Required to Vote)										
Outcomes:	**Voting Turnout**		**Political Quiz**		**Distinguish most right wing party**		**Prefers a Political Party**		**Ideologically Extreme**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean Outcome - VS population	0.3728		0.4975		0.553		0.2937		0.049	
Linear regression on both sides of discontinuity	0.2665 [0.0315]*	0.2719 [0.0310]**	-0.0002 [0.0129]	0.00741 [0.0128]	0.0496 [0.0326]	0.0576 [0.0317]*	0.0479 [0.0342]	0.0554 [0.0333]*	0.0276 [0.0144]*	0.0232 [0.0144]
Second order polynomial on both sides of the discontinuity	0.1864 [0.0427]**	0.1965 [0.0421]**	0.0009 [0.0177]	0.0029 [0.0176]	0.0294 [0.0452]	0.0374 [0.0438]	0.0864 [0.0475]*	0.0895 [0.0464]**	0.0355 [0.0195]*	0.0297 [0.0195]
Joint significance of polynomial dummies (p-value of F-test)	0.7938	0.6529	0.3921	0.4405	0.2002	0.1253	0.1523	0.1498	0.0462	0.0482
N	3053	3242	3059	3251	3059	3251	3037	3227	2978	3163
Demographics controls	yes	no	yes	no	yes	no	yes	no	yes	no

Notes: 1) Same from Table 3. 2) All regressions include school-fixed effects and an indicator for whether an individual has voted before. Demographic controls include dummies for gender, race and mothers' education.2) *Significant at the 10 percent level, **Significant at the 5 percent level.

Table 5 - IV Results for Compulsory Voting on Ideology and Information

Outcomes:	**Prefers a Political Party**			**Extreme (Strongly Lelf- or Right-wing)**		
Samples:	All	Mother with some college	Mother with no college	All	Mother with some college	Mother with no college
	(1)	(2)	(3)	(4)	(5)	(6)
Year of birth fixed-effect, quarter of birth and quarter of birth squared	0.2572 [0.1450]*	0.4649 [0.1872]**	0.0000 [0.2056]	0.1484 [0.0660]**	0.1878 [0.0908]**	0.0390 [0.0890]
Linear regression on both sides of discontinuity	0.1852 [0.1269]	0.3554 [0.1698]**	0.0054 [0.1716]	0.1054 [0.0565]*	0.1548 [0.0807]*	0.0084 [0.0715]
N	3024	2025	999	2964	2003	961
Outcomes:	**Political Quiz (% correct answers)**			**Distinguish most right wing party**		
Samples:	All	Mother with some college	Mother with no college	All	Mother with some college	Mother with no college
	(7)	(8)	(9)	(10)	(11)	(12)
Year of birth fixed-effect, quarter of birth and quarter of birth squared	0.0020 [0.0552]	0.0072 [0.0718]	0.0422 [0.0795]	0.2287 [0.1392]*	0.0898 [0.1737]	0.3889 [0.2092]*
Linear regression on both sides of discontinuity	-0.0018 [0.0483]	0.005 [0.0670]	0.027 [0.0659]	0.1839 [0.1231]	0.1314 [0.1635]	0.2659 [0.1752]
N	3045	2034	1011	3045	2034	1011

Note: Same from note 2 in Table 4.

Table 6 - IV Results for Effects of Compulsory Voting - Mechanisms of Information Acquisition

Samples:	All	Mother with some college	Mother without any college education
Watched 2010 TV political advertisement	0.0019	0.0857	-0.0159
	[0.1065]	[0.1274]	[0.0958]
	3045	*2034*	*1011*
Talk to parents frequently about politics	-0.1796	-0.1974	-0.0871
	[0.1391]	[0.1994]	[0.1756]
	3032	*2028*	*1004*
Talk to friends frequently about politics	-0.0714	-0.1069	0.0111
	[0.1370]	[0.1926]	[0.1806]
	3044	*2033*	*1011*
Number of days you follow politics per week on the media outlets			
TV news	-0.5505	0.04901	-1.168
	[0.5763]	[0.8163]	[0.7560]
	3016	*2011*	*1005*
Newspapers/Magazines	-0.2702	-0.5981	0.526
	[0.5409]	[0.7798]	[0.6926]
	2998	*2013*	*985*
Internet	0.7775	0.3978	1.603
	[0.6215]	[0.8691]	[0.806]**
	2993	*2016*	*977*

Notes: 1) Standard errors are in brackets. All regressions include a first-order age polynomial, school-fixed effects, an indicator for whether an individual has voted before and indicators for gender, race and mothers' education.
2) Number of observations for each regression are in italic. 3) *Significant at the 10 percent level, **Significant at the 5 percent level.

Table 7A - Effects of Compulsory Voting on Preference for a Party - Frequency Effects

	(1)	(2)	(3)
Number of Experienced Compulsory Elections			
Zero (omitted)			
One (α1)	0.1481	0.1535	0.1717
	[0.0605]**	[0.0600]**	[0.0608]**
Two (α2)	0.1713	0.1691	
	[0.0887]**	[0.0896]*	
Three (α3)	0.2881		
	[0.2366]		
Year of Birth- fixed effects	yes	yes	yes
Quarter of Birth and Quarter of Birth squared	yes	yes	yes
R2	0.0113	0.109	0.0181

Table 7B - Effects of Compulsory Voting on Preference for a Party - Permanent Effects

	(1)	(2)	(3)
Number of Experienced Compulsory Elections			
Zero (omitted)			
At least one (α1)	0.1542	0.1536	0.1703
	[0.0528]**	[0.0530]**	[0.0566]**
At least two (α2)	-0.0291	-0.0217	
	[0.0364]	[0.0392]	
At least three (α3)	0.0657		
	[0.0162]		
At least four (α4)			
P-value(F-stat: αi=0, i≠1)	0.6115		
Year of Birth and Year of Birth Squared	yes	yes	yes
Quarter of Birth and Quarter of Birth squared	yes	yes	yes
Sample by year of birth' range:			
1994-1988	√		
1994-1990		√	
1994-1992			√
R2	0.011	0.0108	0.0169
Number of Observations	3,337	3,266	1,555

Notes: 1) Standard errors robust to heteroskedasticity are in brackets. All regressions include school-fixed and major fixed-effects, indicators for gender, race and mothers' education. 2) **Significant at the 5 percent level.