

RESEARCH NOTE

Am I obliged to vote? A regression discontinuity analysis of compulsory voting with ill-informed voters

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Abstract

We study the impact of compulsory voting in Brazil, where voting is mandatory from age 18 to 70 and voluntary for those aged 16, 17 and 70+. Using a survey sample of 8008 respondents, we document voter confusion about how the age criterion applies. Some people falsely believe that what matters is one's age in an election year rather than on Election Day. Next, we perform a regression discontinuity (RD) analysis of compulsory voting among young voters with register-based data from six Brazilian elections (2008–2018). We find that the effect of compulsory voting is seriously underestimated if we focus solely on the discontinuities prescribed by the law. Our findings carry important implications for studies adopting the RD design where knowledge of the cutoff is expected of the units of interest (like those about compulsory voting) and confirm that compulsory voting is a strong institutional arrangement that promotes greater electoral participation.

Keywords: compulsory voting; electoral participation; regression discontinuity design; turnout; voter misinformation

The regression discontinuity (RD) design has been used extensively to evaluate how regulations, laws, and public policies affect behavior. In its “sharp” version, researchers compare units slightly above and slightly below a certain cutoff to estimate the causal effect of a treatment of interest, assuming that possible confounders do not vary at the cutoff (Lee and Lemieux, 2014; Cattaneo et al., 2020). This type of RD analyzes frequently proceed as if the cutoff that establishes treatment assignment is known by everyone so that *all* units assigned to the treatment take it and *none* of those in the control treatment take the treatment. We believe, however, that perfect information about the cutoff is not likely, given what we know about people's low level of knowledge. Specifically, lack of knowledge about the cutoff can induce *some* units from the control treatment to take up the treatment and others assigned to the treatment *not* taking it. This is a case of imperfect compliance, as in the fuzzy RD design. As a consequence, many estimates of the causal effects of public policies may underestimate (local) causal effects and hide heterogeneous effects due to the uneven distribution of knowledge about how the cutoff applies. More specifically, the effects uncovered presumably represent those encountered among the more knowledgeable, that is, among those aware about how exactly the cutoff applies. The less knowledgeable, for their part, are likely to misinterpret the rules and not comply with them as originally intended, conceivably creating additional “informal” cutoff points.¹

¹It is important to distinguish this situation from those where knowledge of the cutoff can induce units to misrepresent their true values to qualify for some public policy, jeopardizing the integrity of such policies. Here, our concern is with designs where units cannot manipulate said values around the cutoff (e.g., date of birth). In these kinds of designs, misinformation about the cutoff represents a problem not a blessing.

One important issue where the RD design has been applied concerns the effect of compulsory voting (CV) on electoral participation. Particular attention has been paid to Brazil and Argentina, two countries where voting is compulsory for some voters but not others. In both countries, CV laws require citizens aged 18–69 to vote but make it voluntary for those aged 16, 17 or 70+. By exploiting the discontinuities at 18 and 70 years of age, researchers have estimated effects of CV on turnout. We argue that such analyzes have ignored decades of research that show that voters are generally ill-informed (e.g., Delli Carpini and Keeter, 1996)—if not misinformed (e.g., Kuklinski et al., 2000)—and that things are not different with respect to knowledge of CV laws (Jaitman, 2013). In this paper, we argue and show that prior estimates may have underestimated causal effects of CV because many (ill-informed) voters not required to vote by law end up voting on Election Day, presumably because they misinterpret how the CV rules apply. This is a situation where some units from the control group take up the treatment, leading to imperfect compliance.

We address the issue of voter incomplete information (and hence compliance) by examining how CV works in Brazil, the largest country in the world where voting is mandatory, combining a survey of some 8000 citizens about knowledge of CV laws and a RD analysis of register-based turnout of millions of voters in six Brazilian elections (2008–2018). We document the amount of confusion that prevails in Brazil about the exact moment when voting becomes mandatory. Next, we examine how CV affects young voters. As expected, we find that those who turned 18 just before Election Day were more likely to vote than those who turned 18 right after. But we also find that those who turned 18 at the very end of an election year falsely believed that they were obliged to vote even if they were 17 on Election Day (and thus free from that obligation). This other effect of CV is best understood as a reflection of voter incorrect information about how CV laws apply and illustrates how the effect of CV laws on turnout at Election Day is underestimated when control treatment units take up the treatment.

Our research makes two important contributions, one methodological and one empirical. First, we show that because of widespread misinformation about when exactly one becomes obliged to vote, it is crucial to look at not only the discontinuity created by the CV law, but also at other discontinuities associated with voters' incorrect information. More specifically, in the case at hand, there are two discontinuities: one discontinuity on the day of the election—in line with the law—and another at the end of the year—as many people mistakenly believe that they are obliged to vote if they turn 18 after the election but in the same calendar year. This finding is important because it illustrates how imperfect compliance in RD designs can affect causal estimates of treatments. Specifically, considering only the discontinuity on Election Day produces an underestimation of CV on turnout because many (ill-informed) voters *not* required to vote ended up voting in the election. Second, we show that citizens' misinformation promotes greater electoral participation. More precisely, our results suggest that turnout would be lower if people were perfectly informed about the CV law. We thus have an interesting case where misinformation contributes to the public good (assuming that a high turnout is a public good), although we leave aside the “quality” of the votes attributed to this increased turnout (Freire and Turgeon, 2020).

Design and Data

Today, as many as 13 countries from the democratic world have some kind of CV. We turn our attention to Brazil, the largest CV country in the world with an electorate of more than 140 million. Brazilian CV laws require citizens aged 18–69 to vote but make it voluntary for those aged 16, 17, 70 or more. This institutional arrangement constitutes a natural experiment; the criterion of age “serendipitously” assigns voters to one condition (voluntary voting) or another (CV). In Brazil, the date of interest is the Election Day. On that day, voters with a specified age are required to vote by law. There are reasons to believe, however, that many voters hold incorrect information about the law.

If voters are unsure as to how the age criterion applies exactly, the effect of CV can be decomposed into two parts. The first, that on Election Day, is attributed to the effect of CV in a world of complete and perfect information. In that world, voters know exactly who is obliged to vote by law on Election Day. The second part is the consequence of incorrect information. Some voters, for example, may falsely believe that those who are 17 on Election Day but turn 18 later in the same year are obliged to vote. If so, the effect of CV on turnout can manifest itself in at least one other critical moment: at the end of the election year. More specifically, among those who are 17 on Election Day, those who turn 18 at the end of the same year will be more prone to vote than those who turn 18 at the beginning of the following year.

To estimate the impact of CV on participation among young voters in Brazil, we use electoral participation data collected by the *Tribunal Superior Eleitoral* (TSE) for the first round of the 2010, 2014 and 2018 presidential elections and the 2008, 2012 and 2016 municipal elections (six elections in total). First round municipal and presidential elections in Brazil since 2000 and 1994, respectively, have occurred on the first Sunday of October, that is, about three months before the end of the year. The participation data include information about the voters' date of birth, allowing the comparison of voters who barely turned 18 with those who are just about to turn 18 on Election Day. These voters are nearly identical but in one respect: the obligation or not to vote. We use that same strategy to explore discontinuities at the end of the election year to evaluate a possible voter misinformation effect of CV. From these analyzes, we obtain local estimates of the increase in the number of voters that can be attributed to CV at the two critical moments of interest.

We first present evidence about voters' knowledge about CV laws from survey data collected in September and October of 2018, just before the first round of the Brazilian presidential elections. The survey was conducted online among a sample of 8008 citizens. Although respondents were not selected randomly, the sample used quotas to match the population's age, gender, region, and social class.²

Findings

Knowledge about CV laws: survey evidence from Brazil

The survey respondents were randomly assigned to receive one of three sets of questions about knowledge of CV laws in Brazil. Our interest lies with the first two sets of questions because they contain the items about how CV applies to young voters. The first set of questions asked respondents if the following statement is TRUE or FALSE: *All voters who are 18 on Election Day are obliged to vote*. Nearly all respondents correctly identified this statement as true, with 93.2 percent stating that voters aged 18 are obliged to vote. To be sure, most Brazilian voters are aware that voting becomes compulsory by age 18. So far, so good. But when we presented a second group of respondents with the following statement: *All voters who turn 18 this year, including those who turn 18 after Election Day, are obliged to vote*, only 30.9 percent correctly identified this statement as false. Responses to these two questions suggest that there is substantial confusion about how the criterion of age applies.

As shown in [Table 1](#), the results are similar when we only consider young voters. Large majorities of young voters know that voting is compulsory between 18 and 70 (line 1), but most are unsure about how the criterion of age applies (line 2). More educated voters, however, show systematically greater knowledge (although substantially still low), as compared to less educated voters. In what follows, we demonstrate how it is crucial to take into account voter confusion when we estimate the impact of CV on participation.

²Section S1 of the online Appendix compares the sociodemographic characteristics of our online sample survey with those from the Brazilian population.

Table 1. Knowledge of CV laws

	All	Age 18–24	HS degree	Less than HS
1. All voters who are 18 on Election Day are obliged to vote	93.2	93.5	93.5	91.7
2. All voters who turn 18 this year, including those who turn 18 after Election Day, are obliged to vote	30.9	34.1	32.9	21.8 [†]

Note: All entries are percentages of correct answers.

[†] Indicates that the difference by education is significant at 0.05 (two-tailed), adopting a binomial proportion test (Cattaneo et al., 2017).

The impact of CV on participation in six Brazilian elections: discontinuity analyzes

RD analyzes frequently start with a visual representation of the discontinuities of interest. In [Figure 1](#), we show the number of young (16–20) voters who participated in the first round of the 2010 presidential (panel A) and 2012 municipal (panel B) elections in Brazil, binned by date of birth. We center the data on voters who reached the required age to vote by law (18) on Election Day (ED). We do the same by drawing a vertical line at the other critical moment of interest: end-of-year (EY). The gray shaded area identifies voters who were required to vote by law.

In panels A and B of [Figure 1](#), we see a general upward trend among young voters as they approach 18 on Election Day. The upward trend is accentuated among voters who reached 18 before the end of the election year (EY) but who were *not* required to vote by law on Election Day (ED) (because they were not 18 yet on that date). This accentuation is an indication of a possible voter confusion effect. Indeed, voters who turned 18 between EY and ED were more likely to vote in both the 2010 and 2012 elections than those who turned 18 after EY, presumably because they wrongly believed that they were obliged to vote. The jump at Election Day (ED) is particularly sharp, indicating a very strong effect for CV among young voters who were fully aware of the obligation to vote on that day. Interestingly, no observable trend is discernible among voters who turned 19 on Election Day. Next, we take a closer look at the two critical moments of interest, by adopting a RD design.

[Figure 2](#) presents local estimates for young (18) voters around Election Day and at the end of the year, binned by date of birth, for six Brazilian elections from 2008 to 2018 (three presidential and three municipal elections). The estimates in [Figure 2](#) show the percentage increase in voter participation with 95 percent confidence intervals and were obtained by the local randomization approach because the running variable (date of birth) is a discrete variable (Cattaneo et al., 2015).³ This approach, contrary to the traditional local polynomial methods for continuous scores, does not require any specific window selection procedure, as the smallest window is deemed appropriate. We proceed with a conservative 7-day window on each side of the discontinuities. Such a window offers the advantage of being very short (thus avoiding temporal trends in births) and having the same distribution of weekdays on each side. Moreover, given the low number of days, the local randomization approach provides a rigorous test of the discontinuities. For Election Day, the cutoff separates young voters who turned 18 on Election Day or a few days earlier (treatment)—who were obliged to vote—from those who turned 18 after the election (control)—who were not obliged to vote. The first round of the 2010 presidential election, for example, occurred on 3 October. To estimate the effect of CV at that cutoff, we considered all voters who were born between 27 September and 3 October in 1992 as receiving the treatment (because they turned 18 on Election Day or shortly before) and those born between 4 and 10 October as control (as they were not required to vote by law). The same procedure applies for the end-of-year cutoff, which separates those who turned 18 in the last week of the election

³The estimates from [Figure 2](#) were calculated as $((\bar{V}_t - \bar{V}_c)/\bar{V}_c) \times 100$ where \bar{V}_c and \bar{V}_t are the average number of voters from the control and treatment groups, respectively.

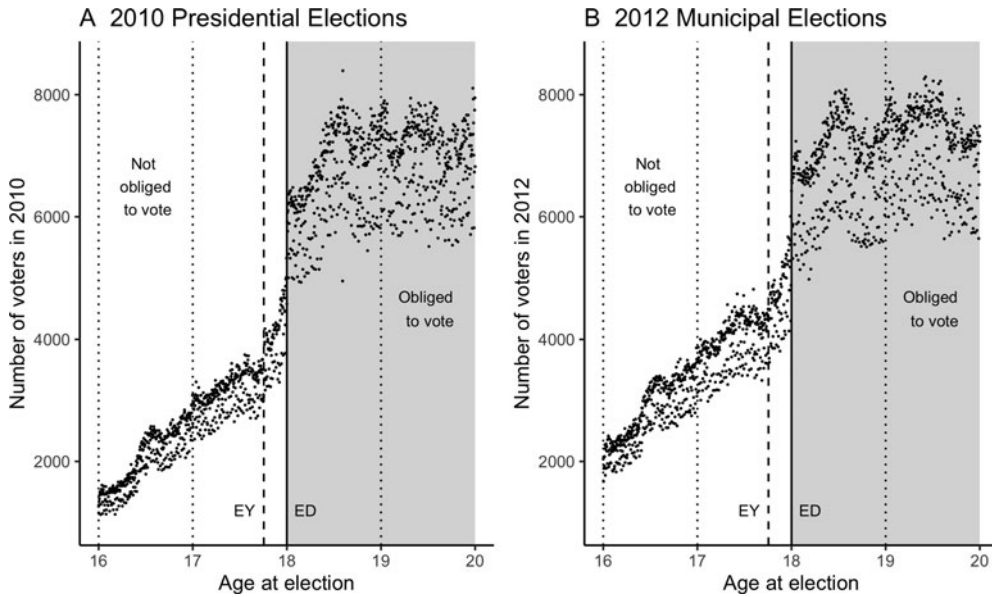


Figure 1. Number of voters who voted in the first round of the 2010 and 2012 elections, by age at election date.

year up to 31 December, who were not obliged to vote but might mistakenly believe that they were, and those who turned 18 in the first week of the following year, who presumably knew that they were not obliged to vote. At the end, this leaves us with at most 10 days in total, 5 on each side of the cutoff, for each RD estimation (Election Day and end-of-year), given that weekend days and holidays are excluded.⁴ The values reported in [Figure 2](#) were calculated by with the `rdlocrand` R package from Cattaneo et al. (2018) (see also Cattaneo et al., 2016). Details about the estimates are presented in Section S3 of the online Appendix.⁵

The estimates presented in [Figure 2](#) indicate a very strong effect for CV on participation among young voters. Specifically, we find that CV increases electoral participation, on average, by 17.8 percent on Election Day with the strongest effect found in 2014 (28.1 percent) and the weakest in 2008 (11.2 percent). All Election Day effects reach statistical significance. More importantly, we also find a strong effect for CV at the end of the year. The average effect is 9.7 percent with the strongest effect found in 2014 (14.2 percent) and the weakest in 2012 (4.5 percent). Effects at the end of the year are smaller than those at Election Day (as they should), but they are still substantively large. All effects are statistically significant except for the one in 2016 which fails only shortly. The effects uncovered at the end of the year clearly reflect the impact of citizens' incorrect information. More precisely, turnout is higher among those who turned 18 in the last week of the election year than among those who turned 18 one week later—even though *both* groups were not obliged to vote according to the law—presumably because many in the first group mistakenly believed that they were obliged to vote since they turned 18 on election year (see [Table 1](#)).

Specification checks

Specification checks are needed to establish that the discontinuities are valid (Lee and Lemieux, 2014). One common threat to RD designs is the ability of individuals to manipulate the running variable. Such possibility is easily discarded because voters were not able to choose their date of

⁴See Section S2 of the online Appendix.

⁵Analyses were also conducted adopting the continuity-based approach. See Section S4 of the online Appendix.

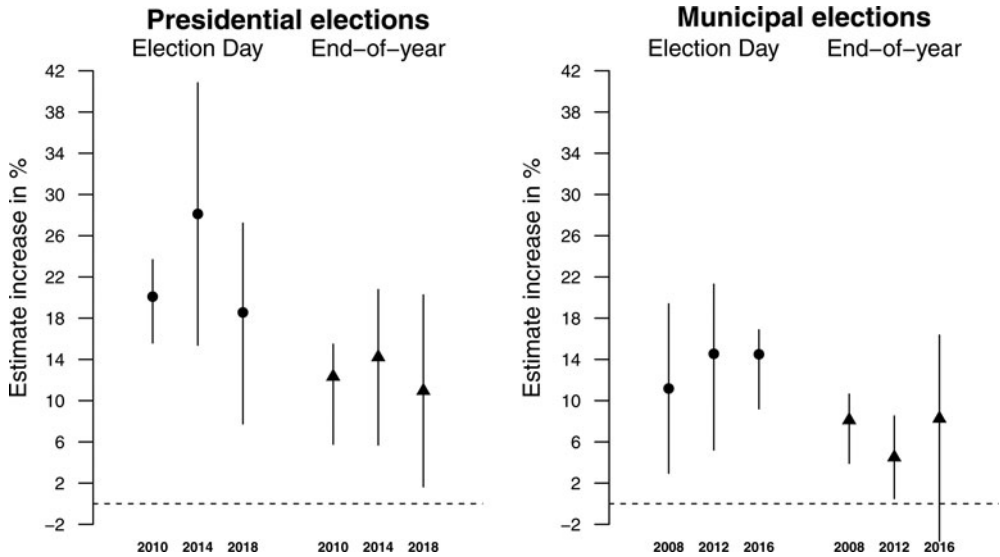


Figure 2. Effect of CV on participation at Election Day and the end of the year, by election type (2008–2018).

birth or influence the choosing of the cutoffs. Another threat is the possibility that the treatment at the cutoff is confounded with some other factor. At 18, Brazilians gain the right to drive and drink but it is very unlikely that the effects uncovered are to be attributed to these two factors. The effect for CV at the end of the year, however, could possibly be attributed to peer influence, where the behavior of some voters not compelled to vote can be affected by the behavior of those who have the obligation to vote. Young voters share many commonalities like being in the same school year. In Brazil, students turning 18 at the end of the year and those turning 18 just a little after, in the following year, are generally in the same school year (because January 1st does not serve as the cut date for school enrollment) and should consequently be equally affected by their peers. Therefore, we have strong reasons to believe that the effects found on participation at the end of the year are attributable to voter incorrect information, not peer influence.

In Section S5 of the online Appendix, we present a series of other specification checks, including a covariate balance analysis, placebo tests of jumps at other points including between Election Day and the end of the year, and sensitivity to different bandwidth sizes using the 2010 election as an example. These additional analyzes show that the effects of CV on electoral participation, on both Election Day and at the end of the year, are robust.

Conclusion

We have estimated the impact of CV in the largest CV country in the world, Brazil, with data about the date of birth of millions of voters who voted in the first round of the last six elections (2008–2018). By adopting a RD design and allowing for voter incomplete and incorrect information about CV laws (which we documented with survey data), we have shown that the local effects of CV are not confined to the official cutoff prescribed by the law, which specifies that voting is compulsory for those who are aged 18 or more on the day of the election. We have shown that there is a second cutoff associated with voter misinformation, that is, many of those who turn 18 after the day of the election (but in the same calendar year) mistakenly believing that they are obliged to vote and do so. Our study is a reminder that citizens are poorly informed, and consequently, that RD analyzes of cutoffs where there is room for misinformation about the exact nature of the cutoff, like in the case of CV, may hide other “informal” cutoffs as a result of

imperfect compliance. Such designs are better interpreted as fuzzy RD designs because some control treatment units end up taking the treatment, resulting in an underestimation of the treatment at the cutoff. In our study, we were able to retrieve some of that underestimation by identifying another cutoff wrongly believed by many ill-informed voters to represent the “official” cutoff.

Incorporating the fact that many voters hold incorrect information about how CV laws apply to one’s research design leads to a more accurate appreciation of the impact of CV on electoral participation. We have shown that the effect of CV is much stronger than initially believed as many voters, misinformed but predisposed to follow the law, decide to vote on Election Day although their participation is not mandatory. Interestingly, turnout would have been *lower* if those not required to vote on Election Day had known their participation was not compulsory.

Our results add to research that shows that CV is a strong institutional arrangement that promotes greater electoral participation (Birch, 2013), even in a country like Brazil that applies moderate penalties and has weak enforcement (Singh, 2011). Our research shows, however, that in order to better understand how and why CV increases turnout we need to take into account what people do and do not know about CV laws and regulations.

Supplementary material. The supplementary material for this article can be found at <https://doi.org/10.1017/psrm.2021.18>.

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