Voting Technology, Political Responsiveness, and Infant Health: Evidence from Brazil

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Abstract

This paper studies the introduction of electronic voting technology in Brazilian elections. Estimates exploiting a regression discontinuity design indicate that electronic voting reduced residual (error-ridden and uncounted) votes and promoted a large *de facto* enfranchisement of mainly less educated citizens. Estimates exploiting the unique pattern of the technology's phase-in across states over time suggest that, as predicted by political economy models, it shifted government spending towards health care, which is particularly beneficial to the poor. Positive effects on both the utilization of health services (prenatal visits) and newborn health (low-weight births) are also found for less educated mothers, but not for the more educated.

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1 Introduction

The inadequacy of public services in the developing world is often attributed to the lack of political influence of the neediest citizens. Even in functioning democracies, elected officials ignore the necessities of certain segments of the population. Hence, a common policy recommendation is to intervene in the electoral process and raise disadvantaged groups' influence on public policy decision-making.[1]

However, there is limited evidence on interventions that can strengthen the electorate's voice, and how such a change translates into improved service outcomes. Moreover, research on this topic has focused on relatively drastic changes in electoral rules or the public decision-making process that may not be feasible in some situations, such as mandated political reservations for minorities (Pande, 2003; Chattopadhyay and Duflo, 2004), participatory budgeting (Besley et al., 2005), or plebiscites (Olken, 2010).

This paper provides evidence on how improving political participation can lead to better service outcomes. It estimates the effects of an electronic voting, or EV, technology in reducing a mundane, but nonetheless important, obstacle to political participation: difficulty in operating ballots. The results indicate that EV caused a large *de facto* enfranchisement of less educated voters, which lead to the election of more left-wing state legislators, increased public health care spending, utilization (prenatal visits), and infant health (birthweight).

While filling out a ballot may be a trivial task to educated citizens in developed countries, the same is not true in Brazil, where 23% of adults are "unable to read or write a simple note" and 42% did not complete 4th grade.[2] Moreover, before 1994 Brazilian paper ballots required voters to write a candidate's name or electoral number and involved only written instructions. This resulted in a substantial quantity of error-ridden and blank ballots being cast, generating a large number of *residual* votes (not assigned to a candidate and discarded from the tallying of results).

In the mid-1990s, the Brazilian government developed an EV technology as a substitute for paper ballots. While its introduction aimed at reducing the time and costs of vote counting, other features of the technology, such as the use of candidates' photographs as visual aids, the use of "error" messages for voters about to cast residual votes, and

[1] For example, the World Bank's *World Development Report 2004: Making Services Work for Poor People* cites the fact that "the poor have little clout with politicians" as a cause of under-provision of public services and devotes a whole chapter to citizen influence on politicians, mentioning that "elections, informed voting, and other traditional voice mechanisms should be strengthened [...], helping to produce better service outcomes" (World Bank, 2004, p. 78).

[2] Figures from the 1991 Census. Adults are those aged 25+.

guiding the voting process step by step, facilitated voting and reduced errors.[3]

This paper first estimates the electoral effects of this voting technology exploiting a regression discontinuity design embedded in EV's introduction. Due to a limited supply of devices in the 1998 election, only municipalities with more than 40,500 registered voters used the new technology, while the rest used paper ballots. Estimates indicate that EV reduced residual voting in state legislature elections by a magnitude larger than 10% of total turnout. Such effect implies that millions of citizens who would have their votes go uncounted when using a paper ballot were *de facto* enfranchised. Consistent with the hypothesis that these voters were more likely to be less educated, the effects are larger in municipalities with higher illiteracy rates. Moreover, EV raises the vote shares of left-wing parties. These results are not driven by the (non-existent) effects on turnout or candidate entry (given the at-large nature of state elections).

The paper will go on to argue that this enfranchisement of the less educated citizenry did indeed affect public policy. Since the discontinuous assignment was observed in an election for state officials, I focus on state government spending, in particular on an area that disproportionately affects the less educated: health care. Poorer Brazilians rely mostly on a public-funded system for health care services, while richer voters are substantially more likely to use the co-existing private services (Alves and Timmins, 2003). The less educated have thus relatively stronger preferences for increased public health care provision, and political economy models predict that increasing their political participation leads to higher public spending in this area (Mobarak et al., 2011).

Using data from birth records, I also find that EV raised the number of prenatal visits by women to health professionals and lowered the prevalence of low-weight births (below 2500g), an indicator of newborn health. Moreover, these results hold only for less educated mothers, and I find no effects for the more educated, supporting the interpretation that EV lead to benefits specifically targeted at poorer populations.

Other than its importance for welfare and the growing evidence on the adult-life consequences of early-life health (Almond and Currie, 2011; Currie and Vogl, forthcoming), the focus on health services and birth outcomes is also motivated by three factors. First, health care is a politically salient publicly-provided good that state legislators can affect significantly within a short time frame, especially in the case of primary care that supplies prenatal visits. Second, pregnant women have high demand for health services and may benefit disproportionately from its additional provision. Third, newborn health

[3]Section 2.1 discuses other differences between paper ballots and EV, such as the possibility of election fraud under the different technologies (Hidalgo, 2012), and how they may affect the results and their interpretation.

can respond rapidly to health care improvements. This short time frame between legislation, service provision and health outcomes is an important feature given my empirical strategy, described below.[4]

The state-level results exploit the fact that the discontinuous assignment in the 1998 election created specific and unusual differences in the timing exposure across states. The phase-in of the new technology was carried out over three consecutive elections held in 1994, 1998, and 2002. In 1994, only paper ballots were used. In 1998, there was the discontinuous assignment described above. In 2002, only EV was used. Such a schedule implies that the evolution of EV in a state is entirely determined by a time-invariant cross-sectional variable: the share of voters living in municipalities above the cutoff for its use in 1998. If a state has $S\%$ of its voters living above cutoff, $S\%$ of its voters *changed* from using paper to EV technology between the 1994 and 1998 elections. Moreover, between the 1998 and 2002 elections, the remaining $(1 - S)\%$ of voters switched to EV. Hence, states with relatively higher shares of voters above the 40,500-voter threshold experienced most of the enfranchising effects of EV earlier than the states with a low share. The intuition behind the empirical strategy is essentially testing if outcomes of interest track this same pattern.[5]

The effects of EV on policy outcomes are thus identified only from variation coming from the interaction of a cross-sectional variable (share of voters above the cutoff) with the timing of different elections. In the period (1994–1998) when such a variable positively predicts EV use, it also positively predicts valid voting, health care spending, number of prenatal visits and birthweight. On the other hand, in the period (1998–2002) when the exact same cross-sectional variable negatively predicts EV use, it negatively predicts these outcomes.

Such results are interpreted as evidence of a causal effect of EV since this sharp change in the sign of how the same cross-sectional variable predicts growth in outcomes is unlikely to be driven by omitted variables. In other words, any confounding effect would have to follow a very specific pattern to confound the results. More precisely, an omitted variable that (positively) affects health care spending would need to be i) growing faster in states with higher share of voters above the cutoff, and ii) suddenly change to growing slower in such states exactly after the 1998 election. Neither mean reversion nor omitted state-specific trends fit this description.

[4]Section 3.1 describes the role of health care in Brazilian state politics and how legislators can increase health care provision within a short time frame. In the setting of municipal governments, Ferraz and Finan (2011) show that increasing legislator wages led to substantial improvement in public health provision (number of doctors and clinics) in less than two years.

[5]Section 3.2 formalizes this argument and provides its econometric implementation.

Three falsification tests provide some evidence on the validity of the empirical strategy. First, the share of voters above the cutoff is orthogonal to changes in outcomes in the periods when it is not associated with changes in voting technology: between two pre-1998 elections that only used paper ballots and two post-1998 elections that only used EV. Such tests are analogous to pre-trend analysis and checking for lagged or lead effects of treatment in difference-in-differences estimators. Second, negligible "placebo effects" are estimated on variables that are unlikely to be affected by EV, such as several demographics and birth outcomes for more educated mothers. In particular, I also find no effect of EV use in *state* elections on health care spending by *municipal* governments. Since elections (and exposure to EV) in the municipalities follow a different timing, this "placebo" result addresses two possible confounding effects: the demand for health care services or a municipal health care program that evolves with a similar pattern of EV introduction.

The estimates indicate that the *de facto* enfranchisement of approximately a tenth of Brazilian voters increased the share of states' budgets spent on health care by 3.4 percentage points (p.p.), raising expenditure by 34% in an eight-year period. It also boosted the proportion of uneducated pregnant women with more than seven prenatal visits by 7.3 p.p. and lowered the prevalence of low-weight births by 0.45 p.p. (respectively, a 20.2% and -5.8% change over sample averages).

This paper communicates with four different strands of economic literature. First, it relates most closely to the previously discussed studies of interventions to raise political responsiveness to (a group of) voters and its consequences on policymaking.[6] Second, it contributes to the analysis of *de jure* enfranchisement episodes nationally (Acemoglu and Robinson, 2006) and within the US in the case of racial minorities (Husted and Kenny, 1997; Cascio and Washington, 2012) and women (Kenny and Lott, 1999; Miller, 2008). Third, it addresses the health effects of a large-scale improvement in health care funding. By focusing on politically motivated spending at the state level, the estimates in this paper incorporate political economy and (state-level) general equilibrium considerations that are not present in studies of interventions of a smaller scale (Acemoglu, 2010). Fourth, a growing literature evaluates voting technologies. The focus of these papers is mostly on election outcomes,[7] with the exception of Anderson and Tollison's (1990) analysis of the secret ballots.

The remainder of the paper is divided in three sections. Section 2 describes the elec-

[6] Another related set of papers study the role of information and the media, surveyed in Pande (2011).

[7] Garner and Spolaore (2005), Dee (2007) Card and Moretti (2007), Shue and Luttmer (2009), Ansolabehere and Stewart (2005), Baland and Robinson (2008), and Callen and Long (2012).

toral process and the introduction of EV in Brazil. It also reports regression discontinuity design estimates at the municipal level to argue that EV enfranchised less educated voters. Section 3 first provides the background on the functioning of the public health care system, the politics of its provision, and its consequences for infant health. It then uses state-level panel data to estimate EV's enfranchisement effect on health care funding and health outcomes. Section 4 concludes the paper.

2 The Impact of Electronic Voting on Political Participation

2.1 Electoral Rules and Voting Technology in Brazil

Brazil is a federation of 27 states, which themselves comprise over 5,000 municipalities. Each state has its own legislature (*Assembléia Legislativa*) and a directly elected governor (*Governador*).[8] Federal legislation establishes the same election rules for all states. Legislators are elected under an open-list proportional representation (PR) system where the entire state is a single multi-member district (at-large elections). Under open-list rule, a citizen casts a vote to an individual candidate, not a party list.[9]

The combination of large districts and voting for individual candidates makes voters face a large number of candidates, making listing all their names in a ballot impractical. Moreover, state elections in Brazil are held jointly with federal elections for president, the federal senate and the lower chamber of congress. For example, in the 1998 election a voter in the state of São Paulo had to choose one candidate out of 1,265 for the state legislature; 661 candidates for the lower chamber of federal congress; 10 candidates for state governor; 13 candidates for the federal senate; and 12 presidential candidates. This exemplifies not only the impossibility of listing names on the ballot, but also the complexity of voting.

Until 1994, Brazilian elections used only paper ballots. Voting for a state legislator

[8] In reality there are 26 states and the Federal District which, given the similarity in electoral rule and government form, is referred to as a state in this paper.

[9] More specifically, the electoral rule is the open-list PR with the d'Hondt method. In essence, all votes received by candidates from the same party are added and this total proportionally determines the number of seats awarded to the party. The candidates within a party list are then ranked by the number of votes the individually received, and this rank is followed when assigning candidates to seats within a party. Voters also have the option of instead casting a vote to the party label only, which is then counted as a vote to the party list but has no impact in the within-party ranking of candidates. In this paper's sample only 3.7% of the votes were cast to party labels. Governors are elected under a two-round (runoff) plurality rule.

involved writing down the chosen candidates' name or electoral number, a five-digit code assigned by the election authority to each candidate. Paper ballots only provided written instructions to write "the name or number of candidate or party." In the mid-1990s, the independent branch of the federal judiciary that regulates electoral procedures (*Tribunal Superior Eleitoral*) introduced a new direct-record EV technology in order to reduce the time and costs of vote counting. The interface of the new technology is constituted of a small screen and a set of keys closely resembling a telephone keypad with three additional colored buttons. The voter types the candidate's number into this keypad. The machine responds providing her name, party affiliation, and picture on the screen. The voter can then confirm the vote or choose to start the process again.

There are at least four distinct reasons why less educated voters would find voting under EV easier. First, it provides visual aids (candidates' pictures) which were completely absent in paper ballots. Second, the machine guides the user through the many votes that must be cast (state legislator, governor, federal deputy, senate, president). This minimizes voter confusion over which paper ballot to use for each vote (e.g., casting the vote for federal legislator in the state ballot). Third, the machine provides feedback to users. A voter can check if the number he typed corresponds to his choice of candidate. If the typed number does not correspond to an existing candidate, the machine provides a message stating "wrong number" (and does not provide a picture), alerting the voter that he is about to cast a residual vote. Fourth, it uses a number-based interface that resembles a familiar device (telephone keypads and ATMs). As noted by its head designer, "numbers are the easiest way for people to interact with an interface. People with low schooling, through the use of numbers, can use telephones. The illiterate are able to make phone calls" (Tribunal Superior Eleitoral, 2011).[10]

The Supplemental Materials further describes voting procedures under both paper and EV technologies and provides images exemplifying them. It also discusses why the guidance and feedback can have an effect on an illiterate voter, given the device's graphical interface. The government also conducted a large-scale campaign, using public service announcements on TV and radio to instruct the public on how to use the new machines. While the discussion above indicates that the elections authority took deliberate steps to design an easily operable voting technology, reducing residual votes does not appear to have been the primary aim. The head designer of the technology describes the new

[10] An interesting feature of the electronic technology is that it requires the voter to know the number of his preferred candidate at the time he is casting a vote. This usually occurs through the use of flyers or cheat-sheets that candidates distribute during their campaigns. These (usually pocket-sized) pieces of paper, known as *santinhos*, feature the candidate's face, name and number can be brought by the voter to the polls.

system's association with a reduction in residual votes as "a surprise" (Tribunal Superior Eleitoral, 2011).

2.1.1 Introduction of Electronic Voting and the Design of this Study

Elections for state officials are held in Brazil every four years, and all states have the same election date. In the 1990 and 1994 elections, only paper ballots were used. In the 1998 elections, the electoral authority decided that only municipalities with more than 40,500 registered voters (as of 1996, when municipal elections took place) would use the new electronic technology, while municipalities below this threshold would use paper ballots. The assignment rule was adopted due to the limited production capacity of the technology manufacturer and economies of scale in the distribution within large municipalities. In 2002 and afterwards, the new technology became the sole method of collecting votes in Brazilian elections and referendums.[11]

This threshold-based rule used in the 1998 election creates a standard regression discontinuity design (RDD). Under mild assumptions, this allows us to uncover the causal effect of EV. Lee (2008) provides a formal argument and discusses why RDDs can be interpreted as quasi-random experiments. Such interpretation requires agents to have limited control over the forcing variable: registered voters, in the present study. This is likely the case since the assignment rule was announced in May 1998 and the forcing variable is the number of voters registered for municipal elections two years earlier (1996).

Finally, it must be noted that four states used EV in all its municipalities during the 1998 election. Two remote states largely covered by the Amazon forest (Amapá and Roraima) were chosen to check the electoral authority's ability to distribute EV in isolated areas, while the states of Rio de Janeiro and Alagoas had areas where the army provided security to election officials, allowing an opportunity to check the logistics of distributing the electronic devices jointly with the military. These states are dropped from all samples used in this section, but will be used as robustness checks of the state panel estimates on Section 3.

[11] Specifically, the 40,500-voter threshold was determined by assigning the fixed supply of devices available at the time from largest to smallest municipality and establishing where it would end. The distribution economies of scale are due to the fact that distributing a large number of devices across few large municipalities is less costly than distributing few devices across several small municipalities. EV was also used in the 1996 municipal elections (in the 57 largest cities and state capitals). This paper, however, focuses only on state elections.

2.2 Data and Estimation Framework

2.2.1 Municipal Data and Electoral Outcomes

Information on voter registration, turnout, and election results, at the municipality level for several years was obtained from the federal electoral authority. The institution also published reports listing the municipalities that used EV, showing an almost perfect compliance with the discontinuous assignment rule. All the 307 municipalities (out of 5,282) above the 40,500-voter cutoff used EV in 1998.[12] Additional data on municipal characteristics are from tabulations of the 1991 Brazilian Census.

The main outcome of interest is the number of votes that are valid (i.e., non-residual). A vote is considered valid if, and only if, it can be assigned to a particular candidate and then counted in the party vote shares. A vote that is not valid is defined as a residual vote. Hence, turnout must equals the sum of valid and residual votes. A vote cast in a paper ballot is deemed residual if it is left blank or if the name or number written on the ballot does not correspond to a candidate. Under EV, a residual vote may be cast by pressing the "blank" button or by typing and confirming a number that does not correspond to any candidate. This section focuses on the election outcomes for state legislature elections. The relevant sample moments are provided in the discussion and in the tables reporting estimates, so that the summary statistics are deferred to the Supplemental Materials, which also provides a discussion of how a vote can be residual under paper and EV, and provides results for federal and gubernatorial elections.

2.2.2 Estimation Framework

Let v_m be the number of registered voters in municipality m. The treatment effect of moving from paper ballots to EV on outcome y_m is given by:

$$TE = \lim_{v_m \downarrow 40{,}500} \mathrm{E}[y_m|v_m] - \lim_{v_m \uparrow 40{,}500} \mathrm{E}[y_m|v_m] \tag{1}$$

Under the assumption that the conditional expectation of y_m on v_m is continuous, the first term on the right-hand side converges to the expected outcome of a municipality with 40,500 voters using EV, while the second term converges to the expected outcome of a municipality with 40,500 voters using paper ballots. Hence, TE identifies the treatment

[12] Seven (out of 5,282) municipalities below the threshold deviated from the rule, by having their (formal) requests to use EV accepted by the electoral authority. The transcripts of the requests and decisions indicate mainly vague idiosyncratic reasons (e.g., no municipality would be using EV in a particular sub-region of a state). The next subsection discusses why accounting for this almost negligible deviation from the discontinuity design has no impact on the estimates.

effect for a municipality with 40,500 voters, as long as the conditional expectations (and distribution of treatment effects) are continuous at the threshold.

The limits on the right-hand side are estimated non-parametrically using local polynomial regression. This consists of estimating a regression of y_m on (a polynomial of) v_m using only data satisfying $v_m \in [40,500-h; 40,500]$. The predicted value at $v_m = 40,500$ is thus an estimate of the limit of y_m as $v_m \uparrow 40,500$. Similarly, a regression using only data satisfying $v_m \in [40,500; 40,500+h]$ is used to estimate the limit of y when $v_m \downarrow 40,500$. The difference between these two estimated limits is the treatment effect. It is important to note the non-parametric nature of the estimation: although linear or quadratic regressions are used, the consistency of the results holds for any arbitrary and unknown shape of the relationship between y_m and v_m. Notice also that the limit approaching one side of the threshold is estimated using only data on that particular side.[13]

The local polynomial regression estimate is equivalent to the OLS estimation of the following equation using only observations that satisfy $v_m \in (40,500-h; 40,500+h)$.

$$y_m = \alpha + \beta 1\{v_m > 40,500\} + f(v_m) + \epsilon_m \tag{2}$$

Where $1\{v>40,500\}$ is a dummy variable that takes value one if the number of registered voters is above 40,500 and $f(\cdot)$ is a polynomial fully interacted with $1\{v>40,500\}$. The estimate of β is the treatment effect and its (heteroskedasticity-robust) standard error can be obtained in a straightforward manner.

A key decision is h, the kernel bandwidth, and the trade-off between precision and bias. To show the robustness of the results to different choices of h, this paper presents the results for three different levels: 5,000; 10,000, and 20,000 voters. Note that these are relatively narrow (given the overall range in the size distribution of municipalities) and hence reinforce the local intuition of regression discontinuity designs. Although there are over 5,000 observations, only 558 are included in the largest chosen bandwidth, of which 130 are above the cutoff.[14]

[13]The estimation in this paper follows the recommendations in Lee and Lemieux (2010), which in turn rely on the results provided by Hahn et al. (2001). Regressions are unweighted (rectangular kernel). This approach is appropriate for "sharp" discontinuity designs, where the probability of treatment is zero below the threshold and one above it (perfect compliance). Even though there is (an almost negligible) lack of compliance since seven municipalities below the threshold used EV, the estimated effect of crossing the threshold on the probability of using EV is a precisely estimated one, implying that approaching the estimation as a fuzzy design would lead to nearly identical results. Moreover, dropping the seven non-compliers from the sample or treating them as "treated" lead to no change in any of the results.

[14]Methods to select the optimal bandwidth (Ludwig and Miller, 2005; Imbens and Kalyanaraman, 2009) lead to very large bandwidths (almost all municipalities). This occurs since the conditional expec-

2.3 Regression Discontinuity Results

2.3.1 Electronic Voting and Residual Votes

Before reporting the estimation results, some graphical evidence is provided. Figure 1 plots the main outcome of interest (valid votes in state legislature elections, as a share of turnout) against the forcing variable (registered voters in 1996) for three different elections. Each marker represents a local average: the mean of the outcome in a bin of municipalities within a 4,000-wide interval of the forcing variable. A quadratic polynomial is fitted on the original (i.e., "unbinned") data at each side of the vertical threshold, so that the point where the lines are not connected is where the discontinuity in outcomes, if existent, is expected to be visible.

A clear jump is visible in the 1998 election (in circles). A little over 75% of the votes are valid on the municipalities below the cutoff, and this figure rises to close to 90% as the cutoff is crossed and EV is introduced. The fact that no discontinuity is visible for the elections held in 1994 (when all municipalities used paper ballots) and 2002 (when EV was completely phased in) provides a falsification test indicating that municipalities "just above" and "just below" the cutoff are indeed valid treatment and control groups.

Figure 2 repeats this exercise for turnout (as a share of registered voters) and voter registration (as a share of total population)[15] in the 1998 election. There are no visible discontinuities, implying the turnout and registration behavior are the same in both treatment and control municipalities. There are at least three complementary factors explaining why voters did not respond to a technology that increased their likelihood of casting a valid vote. First, it is possible that voters did not anticipate this effect (as those above the cutoff are using it for the first time). Second, the main driver of voters to the polls may be the presidential election, for which EV had relatively small impacts (Supplemental Materials). Third, turnout rates are relatively high in Brazil. Federal law makes registration and voting compulsory for all citizens aged 18–70. Failing to register or vote renders a citizen ineligible to several public services until the payment of a fine. Although these features do not guarantee a turnout close to 100%, it is possible that it makes voting technology a second-order issue in the decision to register and vote.[16]

tation between v and the outcome is usually flat and linear (Figure 1), so that adding more observations lead to additional precision with little changes to the point estimates.

[15] Total population, and not voting-age population, is used since it is reported for multiple years. As the main point of interest in this analysis is if registration evolves smoothly around the cutoff, the choice of denominator is not particularly relevant.

[16] Figure 2 and Table 1 show that turnout is in the order of 85% of registered voters. Citizens who are not in their city of residence on election day can be waived from sanctions by attending a poll in any other municipality and submitting a "waiver form."

Panel A of Table 1 presents the local polynomial regression estimates of the treatment effects. Irrespective of the bandwidth and specification used, the estimated effect of a switch from paper ballot to EV is an increase in the valid vote-turnout ratio in the order of 12 p.p. and is statistically significant at the 1% level.

Table 1 and Figure 1 both demonstrate the inadequacy of the paper ballot technology, with a quarter or more of the electorate casting residual votes when using it. While some residual votes may be deliberate abstentions and protest votes, such large shares probably reflect error. Moreover, there is no clear reason why the incentives to abstain or protest would be affected by voting technology. Panel A also presents the estimated treatment effects on turnout and voter registration. In conformity with the graphical analysis, the treatment effects are numerically small and not statistically different from zero.

It is usual in RDD analysis to test for effect on pre-determined covariates as a check to its "quasi-random" nature. The RDD being analyzed here allows for an even more stringent test. Panel B of Table 1 provides a placebo test by estimating treatment effects in the election years without a discontinuity present: when all municipalities used paper ballots (1994) or EV (2002). The results indicate zero impact across all specifications. This provides supportive evidence on the "quasi-random" interpretation of the results, since if any unobserved difference in municipalities just above and just below the threshold were driving the result observed in 1998, they would likely have been present in the preceding or subsequent election. I also test for effects on pre-determined covariates (income, education, inequality, geographical location) using data from the 1991 Census, and find no effects of crossing the threshold on them. These results are somewhat redundant, given the ones based on the 1994 and 2002 elections, so they are only reported at the Supplemental Materials.

2.3.2 Electronic Voting and the *de facto* Enfranchisement of the Less Educated

While the results above indicate that EV generated the effective political participation of a large share of the electorate, it does not address the issue of the identity of these newly enfranchised voters and the recipients of their votes. Section 2.1 discussed why it is presumable that the less educated are more likely to cast residual votes and, hence, EV should make voting simpler for those with poorer reading and writing skills. Supporting this hypothesis, the effect of EV is stronger in municipalities with less educated populations.

Table 2 repeats the estimation presented on the first line of Table 1, but dividing

the sample into two categories: municipalities with illiteracy rates below and above the sample median. This information is from the 1991 Census and measures the share of adults (aged 25+) that report "not being able to read nor write a simple note."[17] The median illiteracy rate in the sample with a 20,000-voter bandwidth equals 25.4%. The average illiteracy rates (and standard deviations) in the above- and below-median subsample are 45.8% (s.d.=11.4) and 16.1% (s.d.=4.5), respectively.

Panel A and Panel B provide the results for the above-median and below-median illiteracy samples, respectively. The comparison of pre-treatment means already indicates that high-illiteracy municipalities have more residual voting under paper ballots than low-illiteracy ones. This is consistent with the less educated having difficulty voting on paper. The effect on the high-illiteracy sample is in the 15–19 p.p. range. The effect on municipalities with below-median illiteracy is in the 8–11 p.p. range. Table 2 also presents the p-values from testing the null hypothesis that the estimates in each sample are equal. This can be rejected at the 10% level in all cases, and at the 5% level when the quadratic specification is used.

2.3.3 Did left-wing parties benefit from electronic voting?

Brazilian politics is characterized by a multi-party system. In the 1998 election, 30 separate parties had candidates running for state legislature. To systematically assess which parties gained from the introduction of EV, I rely on a measure of party position in a uni-dimensional left-to-right scale from Power and Zucco (2009) to define party ideological positions. The index is constructed from answers of a survey of federal legislators elected in 1998, and ranks parties from zero to ten, with larger numbers being associated with right-wing ideologies. Power and Zucco (2009) describe the methodology further and argue that left (right) positions in the index can be associated with more (less) redistribution to poorer voters.[18]

It must be noted that the measure captures party positions at the federal congress. A common perception of Brazilian politics is that party positions at the federal level does not correlate with their actions at the state level (Ames, 2001). In the context of this paper, this is likely to introduce noise in the measured outcome, biasing the results toward zero.

[17] The original Portuguese word for "note" is "*bilhete*", which means a very short note (a written message shorter than a letter). The correlation between illiteracy rate an the share of adult population without completed 4th or 8th grade education is 0.96 and 0.85, respectively, and using these variables instead of illiteracy rates lead to similar results.

[18] Power and Zucco (2009) also discuss how their measure matches well common perceptions, stated party goals, historical legacies, and previous attempts at party classifications.

Let i_p denote the index for party p, and $votes_{pm}$ the number of votes for party p in municipality m. I construct the following vote share weighted measure of the parties receiving votes from a municipality m:

$$I_m = \sum_p (i_p \cdot \frac{votes_{pm}}{\sum_p votes_{pm}}) = \frac{\sum_p i_p \cdot votes_{pm}}{\text{valid votes}_m} \tag{3}$$

Note that $\sum_p votes_{pm}$ equals the total number of (valid) votes in a municipality, so that the fraction multiplying i_p inside the parentheses is the municipal vote share of party p. Hypothetically, I_m goes from zero to ten (the cases where an extreme left- or right-wing party takes all the votes). In practice, the index takes an average of 5.13 (s.d.=0.77) in the sample defined by the bandwidth of 20,000 voters.[19]

Note that not all parties that received votes in state legislature elections are included in Power and Zucco's (2009) analysis. However, the 11 parties for which the index is available account, on average, for 90% of the valid votes in a sample municipality. More importantly, the ratio between votes of parties for which the index is available and the total number of valid votes is not affected by EV, so that changes in the vote shares of parties for which position is not observed cannot account for the results.[20] As noted before, the at-large nature of state elections imply that voters residing in municipalities above and below the threshold (but within the same state) face the exact same choice of candidates, so that these estimates cannot be driven by entry of parties or candidates.

Panel C of Table 1 reports the estimated treatment effect of EV on I_m. The only difference in the estimation from the other panels is the inclusion of two controls: the state average of I_m and the ratio between votes for parties for which the index is available and total valid votes. For the reasons described above, these two variables evolve smoothly around the cutoff, and are included only to increase the precision of the estimates. Their inclusion has little effect on the point estimates.[21]

The results indicate that the vote share-weighted position of parties moves by about 0.25 to the left when EV is used. This effect is close to a third of a standard deviation in the distribution of the dependent variable, implying a sizable effect. Comparison of columns (1)-(5) show that size of the effect is robust across bandwidths and specifications,

[19]The maximum and minimum values are 6.91 and 2.27 (the maximum and minimum values in i_p are 7.14 and 1.68).

[20]In other words, the estimated treatment effect of EV on the ratio between votes for parties covered by Power and Zucco (2009) and total valid votes is numerically close to zero and statistically insignificant.

[21]Because of the inclusion of state averages, the standard errors are also clustered at the state level. Given the relative small number of clusters (22), p-values based on the Cameron et al. (2008) wild bootstrap procedure are also reported (in curly brackets). In most cases, p-values computed using this method differ only slightly from the ones based on cluster-robust standard errors.

and significant in three out of five cases. The estimated effect indicates that under EV, parties with a relative left-wing inclination benefit disproportionately (i.e., their share of the valid votes increase).

2.3.4 Alternative Explanations

First, it must be noted that the at-large nature of elections implies that two municipalities in the same state have the exact same choice of candidates. Hence, on average, the same choice of candidates are available in municipalities above and below the threshold, and candidate entry cannot account for the effects.[22] There are, however, two other possible factors that could invalidate the interpretation of results as the causal effect of EV.

The first one is manipulation of the forcing variable (voter registration) or of the threshold itself. As discussed above, this is unlikely since the number of registered voters was measured before (1996) the announcement of the cutoff (1998). Second, there could be other treatments assigned by the same discontinuity. To the best of my knowledge, this is not the case. Recall the threshold was chosen based on the available supply of machines. Moreover, the zero effects found in the 1994 and 2002 elections require an unknown confounding discontinuity to be present only in 1998. The Supplemental Materials address both these issues in more detail, providing evidence of no manipulation of the forcing variable (and of the choice of cutoff), listing all known discontinuous population- or electorate-based assignments across Brazilian municipalities, and discussing why they cannot confound the results.

Finally, while this paper suggests that EV being easier to use (especially for the less educated) is the main mechanism behind the causal effect of the technology on valid votes, this argument is not essential to the paper's main argument: EV-induced enfranchisement affected public policy in a manner consistent with political economy models. However, it may be of interest to discuss the possibility of electoral fraud as a (non-mutually exclusive) mechanism behind EV's effects.[23]

First, there is virtually no evidence of electoral fraud under the electronic system, which is perceived as honest by 98% of Brazilians and usually presented as a international case study of trustworthy government technology.[24] While there is evidence of electoral

[22]The Supplemental Materials report that municipalities above and below the cutoff are not in systematically different states.

[23]Note that there is little reason to suspect that voting technology would affect any illegal activity occurring before votes are cast (e.g., vote-buying), especially given both technologies keep the vote secret.

[24]This figure from 2008 is cited in Avgerou et al. (2009), which also presents the electronic voting system and the Brazilian electoral authority as a success story in fostering citizens' trust in information technology and government institutions.

fraud during the military rule (1964–1985) and the democratic transition period, these are much less likely to be prevalent during the 1998 election analyzed here. Hidalgo (2012) argues that tabulation fraud occurs under paper ballots but not under EV. However, it does not challenge the interpretation that the increase in valid votes is initially caused by EV's ease of operation by less educated voters (its main argument is that a large number of residual votes under the paper system creates the possibility of fraud at the vote tabulation stage). Given that the main objective of my analysis is to study the effects of more valid votes coming from less educated voters (the *de facto* enfranchisement), the interpretation of results is not substantially affected by Hidalgo's (2012) argument.

Moreover, it must be noted that to explain the *entirety* of effects (i.e., all additional valid votes), fraud under paper ballots would require tampering with more than 10% of all ballots cast (and up to 15%–20% in high-illiteracy municipalities) which would add to millions of pieces of paper nationwide and make it unlikely to go undetected. Moreover, "ballot-stuffing" is also unlikely to explain the entirety of results, since it would generate differences in official turnout between municipalities below and above the cutoff.

Finally, one possible way to assess the possibility of fraud would be to estimate EV's effect on the vote shares of parties that one could presume are in a position to engage in fraud. However, these are difficult to define in a non-*ad-hoc* manner. While focusing on parties controlling national, state or local governments would be *a priori* reasonable, there is little variation to be exploited in such exercise. The main center-right party alliance that controlled the federal congress was also in power in the vast majority of municipalities around the cutoff. Hence, estimating effects on the vote share of parties in power would be primarily to capture these party-specific effects and would reveal little about the possibility of fraud.[25]

In summary, this section provides evidence that EV promoted large increases in valid votes in municipalities with a large number of low educated citizens, and that these additional votes were disproportionately cast to left-wing parties. This combination of results suggests that the introduction of EV promoted the *de facto* enfranchisement of mostly low educated citizens. Its consequences on government spending, health service provision and infant health are expounded in the next section.

[25]For example, approximately 80% of municipalities in the 10,000 or 5,000 bandwidth sample are in a state with a governor (who has influence over state-level counting officials) from the PSDB, PFL, or the PMDB. These are the three center-right parties that supported the Cardoso presidency (and his successful reelection in 1998). Hidalgo (2012) follows a similar empirical strategy, and finds that state legislators "aligned" with the governor are negatively affected by EV in four (out of 22) Brazilian states.

3 Political Participation, Health Care Provision, and Infant Health

The 1998 election which provided the RDD in voting technology assignment between municipalities only involved the election of state (and federal) officials. Hence, the state becomes the relevant unit of observation to study the effects of EV on policy outcomes. This section uses state-level panel data on election outcomes, legislative representation, fiscal spending, health care utilization and newborn health to analyze these effects. As in Section 2, the first subsection provides the relevant background, a description of the data and estimation framework, and the following three separate subsections explain the results.

3.1 Background and Data

3.1.1 The Political Economy of Health Care in Brazil

Federal legislation mandates the provision of free-of-charge health care to all Brazilian citizens. This is achieved by a nationally coordinated public system named SUS (*Sistema Único de Saúde*), which is almost entirely funded by government tax revenues. Citizens are not required to pay health insurance premiums and do not incur out-of-pocket expenses when using the public system. The administration of public health care is decentralized: state governments are responsible for almost 30% of all expenditures on the system, they exert significant control over the allocation of federal funds and are able to make "earmarked" health-related transfers to municipalities.[26] A coexisting private system of health insurance and care services requires users to pay either per-service fees or private insurance premiums. Alves and Timmins (2003) describe the functioning of this dual system and provide evidence on the "*social exclusion*" from better-quality private services. Using 1998 survey data, they find that only 25% of the population, and mostly those with higher income and schooling, have access to private care.

The political consequences of this arrangement are aptly discussed in Mobarak et al. (2011), which notices that the wealthier insured population "generally make little use of the SUS system" and that "the uninsured value SUS health services relatively more than the insured do." When these differing preferences are present, theories of voter influence on policymaking predict that increased political participation (or enfranchisement) of

[26]Municipalities are responsible for 20% and the federal government for the remainder (Andrade and Lisboa, 2002).

the less educated should raise government spending on health care. For instance, the Mobarak et al. (2011) analysis of health care provision in Brazilian municipalities is informed by a probabilistic voting model which "predicts that an increase in the voting rate of the poor will increase public health care provision." The results in this section test this prediction, with respect to the *de facto* enfranchisement promoted by EV.

While Mobarak et al.'s (2011) model could also serve as motivating theory for my results, this paper takes a more agnostic position on the political mechanism driving the results, especially on the issue of voters "affecting" or "electing" policies (Lee et al., 2004). The canonical median-voter model (Downs, 1957) would yield the tested prediction by having candidates committing to policy changes catered to the newly enfranchised. Citizen-candidate approaches that are based on politicians without credible commitment (Orsbone and Slivinski, 1996; Besley and Coate, 1997) would also generate this prediction through the election of additional officials with preferences closer to the newly enfranchised. These two channels are complementary and quantifying their distinct contributions is outside the scope of this paper for two reasons. First, it analyzes an event that changed both who votes and who gets elected, making it difficult to separate the effects of new politicians from incumbents changing their behavior.[27] Second, this paper analyzes policy outcomes that are the product of bargaining between many politicians, making it difficult to pin down the responsibility for policy changes on specific individuals.[28]

There are three additional reasons to focus on health care provision, instead of other policies that this enfranchisement episode could presumably affect. First, impacts on health care funding are feasible, since the provision of health services is within reach of individual legislators. Second, health care funding can be affected in a short span of time, and third, so can its effects on newborn health. This is particularly useful given that the identification strategy, described in the next section, relies on the timing of EV.

A large portion of state legislature politics revolves around health care. A survey of the São Paulo state legislature, for example, shows that 34% of its members cite health care as their "area of expertise," the modal answer.[29] State legislators can generate additional health care funding by either amending the budget or making official requests to the executive. A common objective of amendments and requests deals with the Family Health

[27]This contrasts with papers that focus on changes in politician behavior when the electorate remains unchanged, which occurs in cases of political quotas, for example (Chattopadhyay and Duflo, 2004; Pande, 2003).

[28]This contrasts with papers that analyze the behavior individual politicians, such as roll call voting (Lee et al., 2004; Mian et al., 2010).

[29]The survey was carried out in 2009 by the *Movimento Voto Consciente*.

Program (*Programa Saúde da Família*), which allocates a team of health professionals (including a family doctor) to provide general practice services for small communities. The relatively low per-team cost of the program makes it easy for state legislators to amend the budget and target it geographically to its voter bases. As an example, the 94-member São Paulo state legislature recorded 107 separate legislator-proposed budget amendments citing the Family Health Program in the 2002 budget process.[30]

Another source of additional health care funding can be direct requests, which can increase health care funding within a short time frame. Anecdotal evidence suggests that a request by a legislator can lead to a clinic providing services in less than a year. Ferraz and Finan (2011) also find that legislators can affect provision of health care quickly in the slightly different setting of Brazilian municipalities. They find that an increase in legislator wages that took office in (January of) 2005 leads to more health clinics and doctors by 2006.

3.1.2 Newborn Health Status

Low birthweight (below 2,500g) is arguably the most common indicator of poor newborn health, used by literally thousands of studies in clinical, epidemiological and economic research. While infant health is an important public policy goal and research interest in itself, there is also considerable evidence that birth outcomes affect a wide range of adult health, human capital accumulation and labor market productivity. Currie and Vogl (2012) survey this evidence from developing countries, and Almond and Currie (2011) focus on the effects of *in utero* health-related interventions on adult outcomes.

The medical literature indicates that prenatal visits (i.e., the interaction between a gestating woman with a doctor or other qualified health professional) is an important determinant of birthweight, mainly by positively affecting maternal behavior such as nutrition (e.g., monitoring if fetal growth and maternal weight gain are appropriate) and avoidance of risk factors (e.g., informing mothers on the risks of alcohol, caffeine, and nicotine intake, and suggesting bed rest when appropriate). Common infections that are easily treatable by medical professionals are also known to lead to premature birth and low birthweight. Kramer (1987) provides a comprehensive survey of the medical literature on birthweight determinants, while Currie and Gruber (1996a, 1996b) find that Medicaid expansions increase medical care utilization and birthweight in the USA.

[30] See also Ames (2001) for the argument that targeting government expenditures at its bases through budget amendments is the main activity of Brazilian legislators.

3.1.3 State Level Panel Data

The following sections use yearly data covering all Brazilian states during the 1994-2006 period that was constructed from several sources. Electoral data comes from the same sources described in Section 2. Specifically, I use state-level valid votes and turnout data, and use the Power and Zucco (2009) to estimate a seat-weighted index of a state assembly's ideological position.[31]

State governments' expenditures on health care and other categories of spending were obtained from the National Secretary of Treasury's FINBRA database, which collects comparable accounting information for the states' budget execution. This data source is discussed further in the Appendix. Our main variable of interest is the amount spent in a legislative term (four-year period) by a state that is categorized as "health care and sanitation." I will henceforth refer to this as "health care spending," since sanitation is a negligible fraction of expenditures. The measures of health care utilization and outcomes come from the National System of Information on Live Births, which contains birth records collected from medical and official registries, such as birth certificates. A positive aspect of this data is its near universal coverage of Brazilian births.

I use three particular variables from the birth records to generate outcomes of interest: mother's education, number of prenatal visits, and the birthweight of the newborn child. Information is coded categorically in the original data, and the specific variables computed for this study are the share of mothers that had seven or more prenatal visits and the share of births that are low-weight.[32]

Prenatal visits and birthweight are computed separately for mothers that completed primary schooling and those that have not. Having these two separate samples allows us to test differential effects by level of education. Primary education is the lowest level of schooling detailed in the data, so separately testing for effects on illiterate mothers, for example, is not possible. To facilitate referencing, I henceforth refer to these two groups as "educated" and "uneducated." The sample means show the difference in health care access and outcomes: the average state has 53% of educated mothers reporting 7+ prenatal visits, and 6.3% of their births are low-weight. The respective figures for

[31] Formally, let i_p be the ideology index for party p and w_{pei} be the seat share of party p on the state assembly elected after election e in state i. The computed variable is $\sum_p (i_p * w_{pei})$.

[32] The original data only reports the share of mothers with 0, 1–6, 7+ births for the entire sample. Additional results based on the former two categories are reported in the Supplemental Materials. Seven is the minimal amount of visits for a gestating woman following the recommended schedule from the Brazilian Medical Association and Federal Council of Medicine, which in turn follow recommendations from the *Williams Obstetrics* textbook.

uneducated mothers are 33% and 7.7%.[33]

Finally, four variables from the Brazilian Statistical Agency (IBGE) serve as covariates: population, GDP, share of population below the poverty line,[34] and Gini index for the income distribution. As in Section 2, relevant sample moments are provided in the text or in the tables reporting estimate, and summary statistics are available in Supplemental Materials.

3.2 Estimation Framework

The empirical strategy exploits the pattern of EV across states and time. As discussed in Subsection 2.2, in the 1994 election only paper ballots were used, while in the 1998 election only municipalities with more than 40,500 registered voters used EV. In 2002 only the new technology was used. To facilitate referencing, I denote the 1994, 1998 and 2002 elections as the "paper-only," "discontinuity" and "electronic-only" elections, respectively. Figure 3 presents this timeline graphically, as well as the timing of different legislative terms, which are the four-year periods during which a state legislature holds office. Note that elections are always held in October, with the elected legislature taking office on the first day of the following year.[35]

Let S_i denote the share of voters in state i that reside in municipalities above the cutoff in the 1998 election. Since 1994 was a paper-only election, S_i equals the share of voters in state i that changed from using paper ballots to EV between the 1994 and 1998 elections. Since the 2002 election was electronic-only, the change in the share of voters using EV between 1998 and 2002 elections equals $1 - S_i$. Formally, let $electro_{ie}$ denote the share of voters using EV in state i at an election held at year e. Hence:

$$electro_{i1998} - electro_{i1994} = S_i \tag{4}$$

$$electro_{i2002} - electro_{i1998} = 1 - S_i \tag{5}$$

This implies that a time-invariant cross-sectional variable, S_i, is *positively* related to *changes* in EV use in a period and *negatively* related to it in the following period.

[33]Section 3.3 discusses if EV is associated with the number of births (in total and by mother's education) and with the share of births that have valid prenatal visits and birthweighted information, and concludes that these factors cannot account for the results.

[34]This variable uses the official poverty line defined by the Brazilian government, which is substantially higher than the one USD per day benchmark.

[35]For example, the election in October 1994 selected all the legislators that held office from January 1st, 1995 to December 31st, 1998. The dates are the same for all states, and all seats of a legislature are elected and inaugurated simultaneously.

Hence, if a particular outcome of interest (such as health care funding, utilization and outcomes) follows this same pattern, this is interpreted as evidence of a likely causal effect of EV. The essence of the argument is that it is unlikely that an omitted variable (or measurement error) would follow this same pattern.

Take expenditures on health care as an example of an outcome of interest. Denote H_{ie} as health care spending by the legislature selected at election year e at state i (e.g., H_{i1998} is the spending for the 1999–2002 legislative term). The next section shows that S_i and $(H_{i1998} - H_{i1994})$ are *positively* correlated, and S_i and $(H_{i2002} - H_{i1998})$ are *negatively* correlated. Given equations (4) and (5), in the first period where S_i is proportional to larger changes in the use of EV, it predicts larger changes in health care spending. In the period where the same cross-sectional variable S_i is inversely proportional to changes in EV usage, it predicts smaller changes in spending.

To confound this result, an omitted variable which (without loss of generality) positively affects the outcome needs to grow faster in states with high S_i in the 1994–1998 period and sharply switch to growing slower in high S_i states with 1998–2002 period. Moreover, note also that mean reversion (or states catching up with the others) also cannot explain this pattern. First, mean reversion cannot explain why a cross-sectional variable (S_i) has predictive power over the changes. Second, the fact that the schedule of elections matches the timing of the sign change in the correlation between the share of voters above the cutoff (S_i) and spending growth would not be implied by mean reversion.

While the existence of a confounding omitted variable is unlikely *a priori*, I further test this key part of the identification strategy through two sets of falsification tests. First, in periods where S_i is not associated with changes in voting technology, it is also not associated with changes in outcomes. Such tests are analogous to pre-trend analysis and checking for lagged or lead effects of treatment in difference-in-differences estimators. They also address the issue of mean reversion. Second, negligible "placebo effects" are estimated on variables that are not expected to be affected by EV. In particular, the sign-switch pattern is not found on health care spending by *municipal* governments. Since elections (and exposure to EV) in the municipalities follow a different timing, this "placebo" result addresses two possible confounding effects: the demand for health care services or a municipal health care program that evolves with a similar pattern of EV use. I also find no effects on prenatal visits and birth outcomes by educated mothers, which presumably should be affected to a lesser extent by public health care spending. Hence, an omitted variable that confounds the birth outcome results must affect specifically the less educated population.

Although the original data is at the yearly level, all the estimations are carried out using legislative term (four-year) averages. Let y_{ie} denote an outcome for state i at the legislative term elected at year e (e.g., y_{i1998} is the outcome observed during the 1999–2002 legislative term). The estimated equations are:

$$\Delta y_{i98} = \alpha^{98} + \theta^{98} S_i + \beta^{98} X_{i02} + \epsilon_{i98} \tag{6}$$

$$\Delta y_{i02} = \alpha^{02} + \theta^{02} S_i + \beta^{02} X_{i02} + \epsilon_{i02} \tag{7}$$

where Δ is the lag operator defined at the term (not year) level, so that $\Delta y_{i98} = y_{i98} - y_{i94}$, for example. X_{ie} is a vector of possible controls. This specification is equivalent to an equation in levels including electoral term and state fixed effects that captures the effect of any unobserved state-specific, time-invariant variable and of national-level factors that vary through time. Heteroskedasticity-robust standard errors are provided.[36]

The parameter θ^{98} measures the effect of S_i on the change in average y_{ie} between the 1995–1998 and 1999–2002 legislative terms. Equation (4) indicates that S_i equals the change in EV use in the elections that selected these legislatures (1994 and 1998, respectively). Hence, θ^{98} is an estimate of the impact of EV on y_{ie}. Similarly, equation (5) implies that θ^{02} measures the effect of the *opposite* of EV use on y_{ie}. The change in EV use between the elections that selected the 1999–2002 and the 2003–2006 legislatures (1998 and 2002, respectively) equals to $(1 - S_i)$. Since θ^{02} captures the effect of S_i on the change between these two legislatures, $-\theta^{02}$ is the effect of EV on the outcome y_{ie}. I take evidence that $\theta^{98} = -\theta^{02} \equiv \theta$ as an indication that θ measures the causal effect of EV. Throughout the analysis, equations (6) and (7) are estimated separately, so that no particular relationship between the parameters θ^{98} and θ^{02} (or any other) is imposed by the estimation. Finally, note that an omitted variable that positively affect y and grows faster in high-S_i states in the entire period would upward bias θ^{98} and downward bias θ^{02}.

The discussion so far applies to the states that followed the discontinuous assignment rule in the 1998 election. As discussed in Section 2.1, four states implemented EV in all its municipalities in that year. They are included in the sample as having $S_i = 1$, so that the equivalence between changes in EV and S_i on equations (4)–(5) remains. Henceforth, I refer to "share of electorate above the threshold" and S_i interchangeably. Hence, estimation of (6) and (7) pools both variation from the unusual pattern generated by the discontinuous assignment and also from what would amount to a standard

[36] Note that each equation is based on a single cross-section of first-differences, so that there is no need to use a standard error that account for serial correlation (e.g, state clustering).

differences-in-differences approach. Since the four states did not comply with the rule for distinct reasons (discussed on Subsection 2.1), comparing the results from specifications that exclude them with those from the full sample can be seen as a robustness check for the estimates.

The geographical distribution of the share of voters living in municipality above the 40,500-voter threshold (S_i) is depicted in Figure 4. The variable has a wide range, from 0.147 to 1, with a mean of 0.52 and a standard deviation of 0.26. All estimations control for a set of region dummies, which absorb geographically restricted shocks affecting the outcomes of interest.[37] Unless otherwise noted, no other controls are included. The Supplemental Materials provide a graphical representation of this empirical strategy.

3.3 Results

3.3.1 Main Results

The first row of Panel A on Table 3 presents the estimates from equations (6) and (7) using the valid votes (as a share of turnout) as the dependent variable. This variable was the main outcome of interest in the RDD studied on Section 2, allowing a comparison between the two empirical strategies. The estimated θ^{98} and θ^{02} are 0.092 and -0.111, respectively. This indicates that states with a larger share of their population living above the cutoff experience faster growth in valid votes in the 1994 and 1998 elections, but slower growth between the 1998 and 2002 elections. Column (3) presents an estimate of the average implied effect of EV by providing $(\theta^{98} - \theta^{02})/2$ and its standard error. Column (4) presents a combination of parameters that serves as a test for the hypothesis that $\theta^{98} = -\theta^{02}$: the value and standard error of $(\theta^{98} + \theta^{02})$. This null hypothesis cannot be rejected at the usual levels of significance.

As previously discussed, this "sign-switch" pattern is interpreted as evidence of the causal effect of EV, as that would require unusually specific behavior from a time-varying omitted variable. The estimates indicate that the effect of a change from having the entire electorate of a state using paper ballots switch to EV is approximately 10 p.p., which is remarkably close to the RDD estimates.

The second row of Panel A (Table 3) shows that the positions of elected parties to state legislatures also follow the sign-switch pattern, which is consistent with EV leading to the election of left-wing parties. As in the case of valid votes, the implied effect is remarkably close to those from the RDD. However, the estimated effect is not statistically significant

[37] Brazilian states are grouped into five official regions based on their natural, demographic and economic characteristics. States within a region are contiguous.

at the 10% level. As discussed in Section 2, party positions are based on behavior at the federal congress, and hence are a noisy proxy for their positions at the state level. This is likely to bias the results towards zero and reduce their precision. The Supplemental Materials repeats this estimation for each of the main parties in Brazil, and finds that two parties that benefited the most from EV have left-wing positions.[38]

The first row of Panel B (Table 3) indicate that the same sign-switch pattern is observed for the share of state budgets spent on health care. The magnitudes of the estimated θ^{98} and θ^{02} are close (0.39 and -0.29), and one cannot reject the hypothesis that $\theta^{98} = -\theta^{02}$ (column 4). The average effect (column 3) implies that a full switch from using only paper ballots to the complete use of EV generates an increase of about 3.4 p.p. on the share of state expenditure destined to health care. This is also a 34% increase with respect to the sample mean. Both parameters are significant at the 5% level, and the implied average effect is also significant at levels well below 1%.

Although health care spending is the most likely (and perhaps only) policy expected to be affected by EV, it is still of interest to report a "family-wise" p-value which accounts for the existence of other categories of spending for which effects can be estimated. As discussed in the Appendix, there are at most seven other categories of spending available for all years of the sample. Applying the (conservative) Bonferroni correction to the average implied effect (column 3) would thus yield a p-value of 3.1%. In other words, under the null that EV has no effects on any category of spending, an effect of the magnitude found for health care spending would be found by chance for one category when testing amongst the eight variables with a probability below 3.1%.

Note that these estimates should be interpreted as the effect gradually occurring over an eight-year period, and not as an immediate effect. The average state has half of its electorate residing in municipalities above the cutoff (mean S_i is 0.52). So the estimates imply that it redirects 1.7% of its budget to health care in a four-year legislative term, and then another 1.7% in the next four-year period. While this increase is relatively large, it must be put into the perspective of the relatively low levels of health care expenditures, accounting for only 7.9% of state government budgets in the 1995–1998 legislative term. This figure grew to 12.3% in the 2003–2006 legislative term. Hence, the implied effect of EV is equivalent to approximately three quarters of the increase in health care budget share growth in the period.[39]

[38] Specifically, the Workers' Party (local acronym PT) and the Democratic Labor Party (PDT).

[39] Large political responsiveness of health care spending has been found for the US during the 1900–1930 period. Miller (2008) reports that US states observed a 36% increase in health-related spending in the year immediately following the enaction of women's suffrage, with growing effects leading to an 81% increase after five years.

Panel B also indicates a similar sign-switch pattern is found when using (log) spending per capita in health care. Total log per capita state spending, however, does not follow the sign-switch pattern, suggesting no significant effects of EV on government size.[40] This indicates that the additional spending in health care occurs at the expense of other categories of spending. The Appendix estimates equations (6) and (7) using all available categories of spending. The only detectable effect of EV on these categories is a reduction on the "administration and planning" spending, which is large enough to counteract the increased health care spending. This category consists basically of the overhead of government operation (and accounts for 18% of budgets). Given this definition, a reduction in this category would be consistent with governments becoming more efficient, but it may also mask reductions in government projects or programs that are not assigned to other categories.

While the yearly state-level fiscal data does not detail the nature of the additional health care spending, other sources may shed some light on this issue. As discussed in Section 3.1, a natural candidate would be the Family Health Program. Its coverage grew quickly in the period following EV's introduction. One of the stated goals of the family health program is facilitating access to basic services that would otherwise only be available at hospital and clinics. One such service is prenatal visits, that can be done at the community health outpost or with house visits. As registration in the program grew, so did the use of these services: the number of visits by pregnant women to family health program professionals grew by a factor of 4.5 in the eight years following the first use of EV in state elections. Moreover, the improved prenatal care provided by the Family Health Program has been associated with reduced child mortality (Rocha and Soares, 2010).

While the narrative above suggests that EV may have played a role in the expansion of the Family Health Program and its improved access to health services and outcomes, yearly state-level data on the program is not comprehensive enough to allow for an econometric quantification. However, it is possible to estimate the effects on the number

[40] The enfranchisement of less educated (and hence poorer) voters could increase the size of government through increased redistribution. Husted and Kenny (1997) find evidence along these lines in the case of US states. Brazilian state governments however, have less ability to engage in redistribution, since most income transfers programs such as social security and conditional cash transfers are determined at the federal level (with their operation often decentralized to the municipal level). Moreover, states' main source of tax revenue is a value-added tax on goods and services, which does not lend itself as easily to redistribution, unlike income tax (which is set and collected by the federal government). Finally, the empirical strategy relies on relatively sharp timing, and would not capture an effect on government size that takes more than four years to occur.

of prenatal visits and birthweight.[41]

Panel C of Table 3 provides the results for birth outcomes of uneducated mothers. Its first row reports results for the share of mothers with more than seven prenatal visits while the second deals with low-weight births. For both outcomes, the signs of θ^{98} and θ^{02} are the opposite. Their magnitudes in the case of prenatal visits are not too similar, but one cannot reject the hypothesis of $\theta^{98} = -\theta^{02}$ in both cases. The implied average effect (statistically significant at the 5% level in both cases) implies that a full switch from paper to EV leads to 7.3 p.p. increase in the share of uneducated mothers with more than seven prenatal visits, and 0.45 p.p. reduction in the probability of a low-weight birth, with both effects being significant at the 5% level.[42]

Both these effects are large in magnitude, and roughly correspond to a third of the gap between average outcomes of educated and uneducated mothers. As the previous estimates, it should be noted that these effects probably occur gradually over the course of eight years. The next subsection repeats the same exercise using the outcomes for educated mothers and finds negligible effects of EV on visits and birthweight. This result indicates that the *de facto* enfranchisement of less educated voters leads to improvements in health services and newborn health only by less educated mothers, providing further evidence consistent with the political economy models discussed in the previous subsection.

Another way to summarize the size of effects is to calculate them effects as a share of sample averages (provided on Table 3). The switch from paper to EV increases the share of valid votes by 12.3%, spending in health care by 34.3%, prenatal visits by 20.2%, and low-weight births by 5.8%. While the overall pattern of results might suggest a causal chain linking these effects, one must be careful in using them to compare implied elasticities. This would require imposing strict exclusion restrictions that may not be warranted in some cases.[43]

[41] To assess the possibility that the composition of births is confounding the results, possible effects of EV were estimated on i) the total number of births (including those where mother's education is missing), ii) the share of births by educated and uneducated mothers, and iii) the share of births for which visits and birthweight data is not missing (for each education group). The effects are negligible and statistically insignificant, indicating that the composition of births is unlikely to affect the results. To further account for this possibility and increase the estimates' precision, the reported results on prenatal visits and birthweight include the respective variable (iii) as a control (e.g., the effects on birthweight by uneducated mothers include the share of births by uneducated mothers for which weight data is not missing as a control), not including these controls lead to similar estimates.

[42] The Supplemental Materials report the analogous estimates for the share of uneducated mothers with 0, and 1–6 prenatal visits (the categories in the original data). The results suggest the effects occur at the intensive margin: there is no effect on the share with 0 visits, and a negative one on the 1–6 visit category.

[43] To illustrate this argument, one could be tempted to infer that the health-spending elasticity of low

The Supplemental Materials provide a graphical representation of the estimation of these effects. It allows us to assess that outliers are not driving the results, and illustrates why the implied average effect $(\theta^{98} - \theta^{02})/2$ is more precisely estimated than θ^{98} and θ^{02} individually.

3.3.2 Falsification Tests

The empirical strategy is based on the notion that the pattern of EV introduction is so specific that it is unlikely that an omitted variable would follow it. While it is not possible to directly test this in the case of unobservable factors, this section provides falsification tests based on estimating (negligible) placebo effects on variables that are unlikely to be affected by EV. This also includes lagged or lead outcomes, indicating that preexisting trends are not driving the results.[44]

Two sets of falsification tests provide some evidence on the validity of the empirical strategy. First, I estimate negligible "placebo effects" on variables that are not expected to be affected by EV. Second, I focus on lagged and lead outcomes and find that the share of voters above the cutoff is orthogonal to changes in outcomes in the periods when it is not associated with changes in voting technology. This is analogous to pre- (and post-) trend analysis in difference-in-differences estimators.

Table 4 presents these placebo tests. It follows the same pattern as Table 3, focusing on the possibility of an outcome following the sign-switch pattern. The first row of Panel A reports results for the budget share spent on health care by *municipalities* in each state. Municipal elections are staggered with state elections, occurring in even years when the latter do not take place (e.g., 1996, 2000, and 2004). Hence, there is no reason why municipal budgets should track the pattern of EV use in state elections.[45] Consistent with this notion, the variable does not follow the sign-switch pattern. The estimated θ^{98} and θ^{02} have the same sign and magnitude and furthermore are small and statistically indistinct from zero, indicating that municipal health spending is uncorrelated with EV.

This result addresses the possibility that the sign-switch pattern in *state* health spending was driven by the demand for public health care or by a nationwide health program that was rolled out from larger to smaller municipalities (with a timing that matched

birthweight is -5.8/34.3=-0.17. Such an argument would require assuming that EV did not induce any other (unobserved, given data constraints) policy changes that affect infant health.

[44]This exercise is similar in spirit to reporting insignificant differences in covariates between control and treatment groups in randomized trials or that covariates evolve smoothly around the RDD cutoff.

[45]The variable is constructed from municipal budgets from the same source used for state spending, and aggregating it to the state level. EV was used in state capitals and some other large municipalities in the 1996 election. From 2000 onwards, all municipal elections were done entirely under EV.

elections and EV's introduction). If either were the case, municipal health care spending should also track the same sign-switch pattern. This is consistent with the fact that, to the best of my knowledge, there is no shock to health care demand or program roll-out that fits the pattern across time and areas of EV's introduction.

The remaining rows of Panel A (Table 4) report that state-level (log) GDP, (log) population, inequality, and the poverty rate do not follow the sign-switch pattern. The point estimates that all these variables seem to grow relatively faster in states with high shares of population above the cutoff in both periods (i.e., no sign switch). In all cases, I cannot reject that the implied effect of EV is zero (column 3). Moreover, in all cases (except the imprecisely estimated effects on GDP) the hypothesis that $\theta^{98} = -\theta^{02}$ can be rejected at the 5% level of significance. These results not only indicate that the results of the previous section cannot be explained by these covariates, but also exemplify that the sign-switch pattern is unusual and that possible determinants of health care spending are unlikely to follow it.

Panel B of Table 4 repeats the exercise from Panel C of Table 3, but focusing on prenatal visits and low-weight births of educated mothers (while the latter focused only on uneducated ones). The sign-switch pattern is not present. The implied average effect (column 3) for prenatal visits is approximately one third of the effect for uneducated mothers, and the effect for low-weight births has a different sign (and half the magnitude). Neither of these effects is significantly different from zero at the usual levels. These results indicate that EV affected service utilization and infant health only for uneducated mothers, and thereby increase our confidence in the causal interpretation of the results. (Not only would an omitted variable need to follow an unusual pattern, but it would need to be specific to uneducated mothers.) The results also fit the previous discussion on the political economy of Brazilian health care.

The final placebo test involves assessing if the share of voters above the cutoff for EV use in 1998 (S_i) predicts changes in health care spending in periods where the voting technology does not change, so that the previous results are not driven by preexisting trends that are correlated with S_i. Panel C of Table 4 presents the effect of S_i on changes between the 1990 and 1994 electoral terms (both only used paper ballots) and the 2002 and 2006 election (both only used EV). The estimated effect on both cases is close to zero (and statistically insignificant). This indicates that, apart from the periods where it predicts EV use, the share of voters living above the cutoff is orthogonal to growth in health care spending.[46]

[46] Birth records data is not available for the 1991–1994 electoral term or the entire 2007–2010 electoral term. Hence, it is not possible to perform this test on prenatal visits and birthweight outcomes.

3.3.3 Robustness Checks

To enhance the precision of the estimated effects, variation from both equations (6) and (7) can be pooled and used to estimate:

$$y_{ie} = \alpha_e + \theta S_i \cdot Term_e^{98} + \gamma_i + \beta X_{ie} + \epsilon_{ie} \tag{8}$$

where $Term_e^{98}$ is a dummy indicating the legislative term elected in 1998. Since $S_i \cdot Term_e^{98}$ is collinear with EV use ($electro_{ie}$) when fixed and time effects are present, θ captures the effect of EV on y. Estimation of (8) is also equivalent to estimating equations (6) and (7) jointly under the restriction that $\theta^{98} = -\theta^{02}$.

The results are presented from estimating (8) on the main outcomes are reported on Table 5. Column (1) shows the estimate of θ for different outcomes. Since (8) can only be estimated as a panel, standard errors are clustered at the state level. I also report p-values based on a cluster-robust wild-bootstrap procedure suggested by Cameron et al. (2008), which provides inference with correct size in cases with relatively small number of clusters.

As expected, the effects are of similar magnitude to the average implied effects reported on Table 3.[47] Columns (2) probes the robustness of the results to the inclusion of additional controls (population, GDP, the poverty rate, and the Gini index of income distribution). Column (3) adds a set of state-specific trends, capturing unobserved state differences that evolve constantly through time. Their inclusion leads to negligible differences in the point estimates and usually increase precision of the estimates. For example, all estimated effects on column (3) are significant at the 1% level (in both the usual t-test and the bootstrapped p-values).

A final robustness check is provided by columns (4) which excludes from the sample the four states that did not participate in the discontinuous assignment in 1998 (and used EV all municipalities). Column (5) then adds controls to this restricted sample. As discussed in Section 3.2, previous estimation pooled variation from the unusual pattern of EV's phase-in with one akin to a standard difference-in-differences approach. Removing the states fitting the latter description hence provides an useful robustness check. Similar results are found, with point estimates being only marginally affected. As expected from a reduction in sample size, standard errors become larger, negatively affecting the

[47] In the case where there is no (fully interacted with time effects) controls, θ from equation 8 equals $(\theta^{98} - \theta^{02})/2$ from equations (6) and (7). This explains why the point estimates in column (1) of Table 5 perfectly match those of column (3) in Panel B, Table 3, while those from Panel C, Table 3 differ slightly (since they include controls for the coverage of birth records).

estimates' precision in some cases. The only exception is the number of prenatal visits. The point estimate of the effect is smaller in the restricted sample. The lack of precision is such that it is not possible to rule out a zero effect or an effect as large as those in the unrestricted. This implies that while the evidence on the effects of prenatal visits is hence not as strong as for the other outcomes, it is also not possible to dismiss an effect based on this one exercise.

4 Conclusion

This paper estimates the effects of electronic voting technology that facilitated ballot operation in Brazilian state elections. Results indicate that it promoted the *de facto* enfranchisement of a significant number of (mainly less educated) voters. Consistent with the predictions of political economy models, this *de facto* enfranchisement increased government spending in an area that particularly benefits less educated voters: health care services. It also had effects on their utilization and outcomes, increasing the number of prenatal visits and reducing the occurrence of low-weight births by less educated mothers (while the more educated were not affected).

While the estimated effects may be dependent on the Brazilian context and not carry over to other countries, this study exemplifies how removing a mundane obstacle to voting can have substantial effects on political participation, which in turn can have substantial effects on public service provision and tangible outcomes. This points to the value of fostering of political participation, through such methods as campaigns to increase turnout or reductions in the costs of registration and voting, especially among disadvantaged groups in the developing world.

Appendix: Classification of State Public Spending

The FINBRA database collects budget information that state governments report to the federal Ministry of Finances. This is the data source used to construct state-level spending on health care used throughout this paper. The reports break down state spending across multiple categories. Throughout the 1991–2010 period analyzed in this paper, it is only possible to construct at most eight consistently defined categories of spending.[48] Additionally, a residual category name "other spending" also has to be

[48] While in later years in the sample spending is further specified (e.g., primary vs. secondary education), in earlier years only wider categories (e.g., total spending on education) is provided.

constructed in order to i) account for years in which there is spending classified simply as "other" and ii) incorporate categories that appear only in certain years of the sample (e.g., "foreign relations"). The categories used here match well with another attempt to build comparable state spending by category.[49] While the results on share of budget spent on health care are provided on Table 3, Appendix Table 1 provides the results for the other categories of spending, using the same pattern of Table 3. The sample averages also provide a breakdown of the average state budget. The largest category, and the only one that tracks the sign-switch pattern of EV's introduction (with an implied average effects that is statistically significant) is "administration and planning." This is essentially the "overhead" of government operations, making an exact interpretation of this effect difficult. States also spend a large share of budget on education. This is mostly at the secondary level (and at the post-secondary in some states), given municipalities are the main providers of primary education in the period, which may explain why the enfranchisement of less educated voters does not lead to more state spending on education. Public safety is also a sizable category, since most police forces in Brazil are funded by state governments. Finally, it must be noted that "social ssistance" may be inflated by including government employees' pensions.

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[49]Data provided the Instituto de Pesquisa Economica Aplicada on www.ipeadata.gov.br

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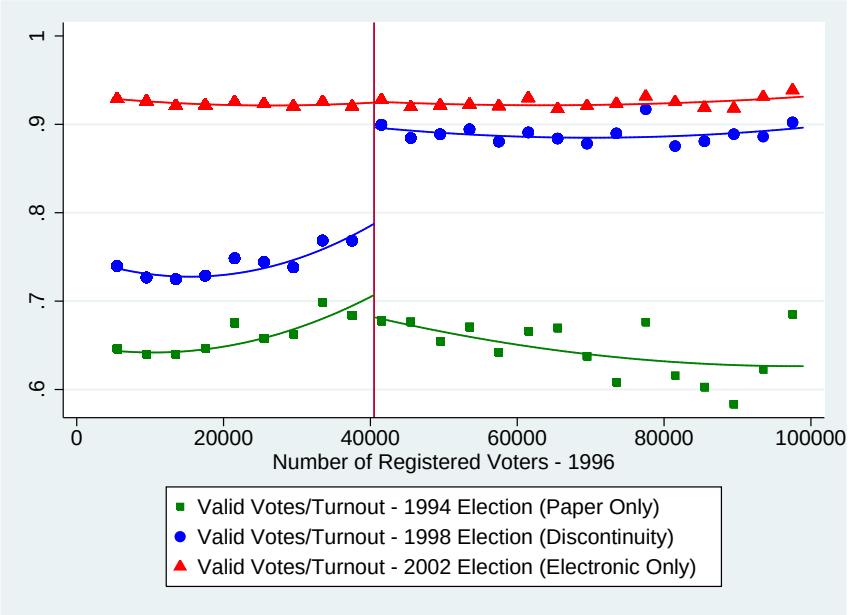
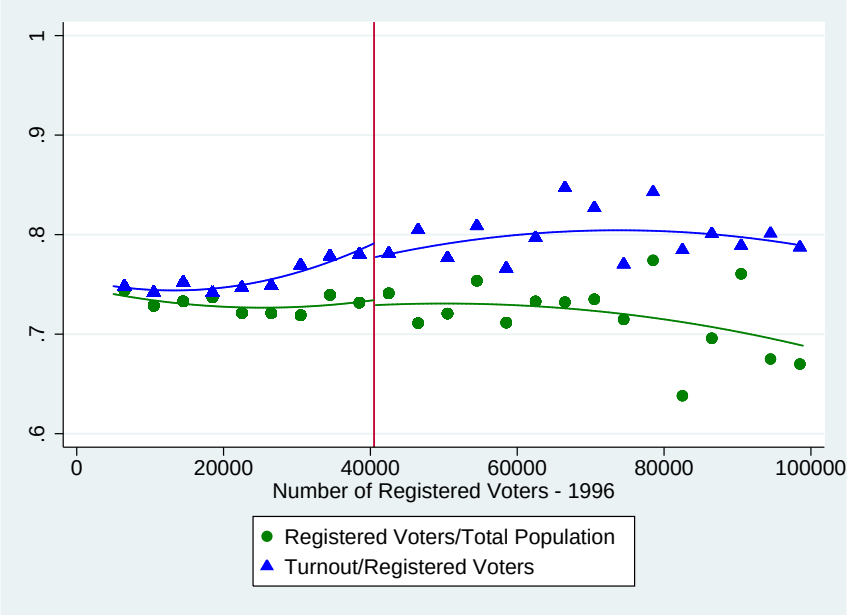
Figure 1: Valid Votes/Turnout - Local Averages and Parametric Fit



Each marker represents the average value of the variable in a 4,000-voter bin. The continuous lines are from a from a quadratic fit over the original ("unbinned") data. The vertical line marks the 40,500-voter threshold.

Figure 2: Valid Votes/Turnout - Local Averages and Parametric Fit



Each marker represents the average value of the variable in a 4,000-voter bin. The continuous lines are from a from a quadratic fit over the original ("unbinned") data. The vertical line marks the 40,500-voter threshold.

Figure 3: Share of Electorate Using Electronic Voting: 1998 Election

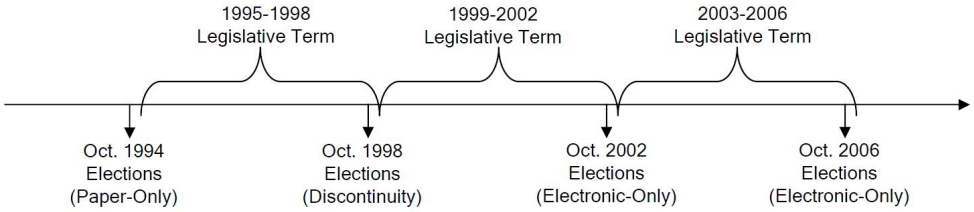


Figure 4: Share of Electorate Using Electronic Voting: 1998 Election

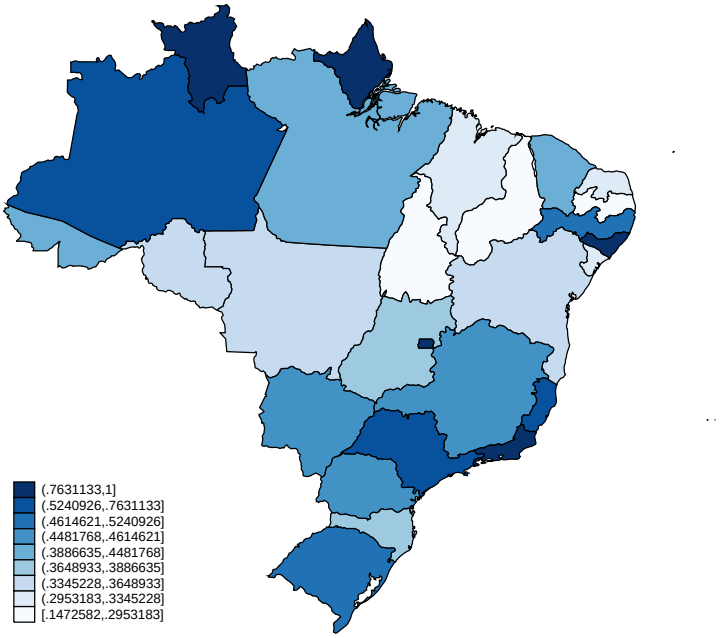


Table 1: Treatment Effects of Electronic Voting

	Pre-Treat. Mean	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline Results						
Valid Votes/Turnout (1998 Election)	0.769	0.128	0.121	0.124	0.116	0.118
		(0.011)	(0.016)	(0.025)	(0.017)	(0.026)
Turnout/Reg. Voters (1998 Election)	0.789	0.001	0.013	0.007	-0.006	0.010
		(0.017)	(0.021)	(0.033)	(0.023)	(0.034)
Reg. Voters/Population (1998 Election)	0.732	0.001	0.010	0.032	0.004	0.047
		(0.024)	(0.034)	(0.044)	(0.036)	(0.045)
Panel B: Placebo Tests (Election Years Without Discontinuous Assignment)						
Valid Votes/Turnout (1994 Election)	0.688	-0.006	-0.008	0.006	-0.013	0.007
		(0.017)	(0.023)	(0.032)	(0.025)	(0.033)
Valid Votes/Turnout (2002 Election)	0.921	0.005	0.008	0.008	0.007	0.010
		(0.005)	(0.006)	(0.010)	(0.007)	(0.010)
Panel C: Do Left-wing Parties Benefit Disproportionately from Electronic Voting?						
Vote-weighted Party Ideology (1998 Elec.)	5.161	-0.222	-0.250	–0.108	-0.269	-0.243
		(0.100)	(0.081)	(0.170)	(0.123)	(0.191)
		{0.038}	{0.002}	{0.568}	{0.070}	{0.250}
Bandwidth:	-	20,000	10,000	5,000	20,000	10,000
Specification:	-	Linear	Linear	Linear	Quadratic	Quadratic
N	-	558	229	116	558	229

Robust standard errors in parenthesis. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets - { }. The unit of observation is a municipality. Each figure is from a separate local polynomial regression estimate with the specified bandwidth and specification (quadratic or linear). Separate polynomials are fitted on each site of the threshold. The pre-treatment mean is the estimated value of the dependent variable for a municipality with 40,500 registered voters that uses paper ballot, based on the specification on column (1). Details on the dependent variables in the text.

Table 2: Treatment Effects of Electronic Voting, by Illiteracy Rate

	Pre-Treat. Mean	(1)	(2)	(3)	(4)	(5)
Panel A: Municipalities with Above-Median Illiteracy						
Valid Votes/Turnout	0.747	0.150	0.152	0.176	0.150	0.195
		(0.015)	(0.020)	(0.031)	(0.022)	(0.033)
N	-	279	103	49	279	103
Panel B: Municipalities with Below-Median Illiteracy						
Valid Votes/Turnout	0.799	0.113	0.096	0.089	0.085	0.074
		(0.016)	(0.022)	(0.032)	(0.023)	(0.032)
N	-	279	126	67	279	126
Test of Equality in TEs (p-value)	-	0.089	0.056	0.053	0.041	0.008
Bandwidth:	-	20,000	10,000	5,000	20,000	10,000
Specification:	-	Linear	Linear	Linear	Quadratic	Quadratic

Robust standard errors in parenthesis. The unit of observation is a municipality. Each figure is from a separate local polynomial regression estimate with the specified bandwidth and specification (quadratic or linear). Estimates on Panel A (Panel B) uses only municipalities where the adult illiteracy rate is above (below) 25.43%. The pre-treatment mean is the estimated value of the dependent variable for a municipality with 40,500 registered voters that uses paper ballot, based on the specification on column (1).

Table 3: Main Outcomes and the Sign-Switch Pattern

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})$
Sample (terms):		1994-1998	1998-2002		
		(Paper-Disc.)	(Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Panel A: Electoral Outcomes					
Valid Votes/Turnout	0.829	0.092	-0.111	0.102	-0.019
	[0.112]	(0.033)	(0.010)	(0.017)	(0.034)
Seat-Weighted	4.623	-0.112	0.299	-0.206	0.187
Policy Position	[0.601]	(0.623)	(0.162)	(0.322)	(0.643)
Panel B: Fiscal Outcomes (Health Care Spending)					
Share of Spending	0.099	0.039	-0.029	0.034	0.010
in Health Care	[0.037]	(0.017)	(0.013)	(0.011)	(0.021)
log(Health Spending)	-	0.428	-0.677	0.552	-0.250
		(0.256)	(0.255)	(0.181)	(0.362)
log(Total Spending)	-	-0.004	-0.257	0.127	-0.262
		(0.090)	(0.152)	(0.088)	(0.177)
Panel C: Birth Outcomes (Mothers Without Primary Schooling)					
Share with 7+ Visits	0.362	0.122	-0.023	0.073	0.099
	[0.123]	(0.064)	(0.032)	(0.036)	(0.071)
Share with Low-weight	7.721	-0.370	0.528	-0.449	0.157
Births ($\times$100)	[1.110]	(0.295)	(0.261)	(0.197)	(0.394)
N (state-terms)	-	54	54	-	-
N (states / first-diffs)	-	27	27	-	-

Robust standard errors in parenthesis. Standard deviations in brackets. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}) . Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

Table 4: Placebo Tests

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Panel A: Effects on Covariates					
Share of Spending in Health Care - Municipalities	0.096 [0.048]	-0.011 (0.021)	-0.016 (0.014)	0.003 (0.012)	-0.028 (0.025)
log(Population)	-	0.064 (0.032)	0.059 (0.026)	0.002 (0.020)	0.124 (0.041)
log(GDP)	-	0.007 (0.052)	0.096 (0.196)	-0.051 (0.102)	0.088 (0.203)
Poverty Rate	0.397 [0.164]	0.080 (0.044)	0.105 (0.024)	-0.012 (0.025)	0.185 (0.088)
Gini Index	0.569 [0.034]	0.034 (0.030)	0.042 (0.009)	-0.004 (0.016)	0.076 (0.031)
Panel B: Birth Outcomes for Mothers that Completed Primary Schooling					
Share with 7+ Visits	0.569 [0.127]	0.062 (0.035)	0.009 (0.021)	0.027 (0.021)	0.071 (0.041)
Share with Low-weight Births (×100)	6.261 [1.581]	0.391 (0.461)	-0.023 (0.535)	0.207 (0.353)	0.368 (0.706)
Panel C: Pre- and Post-trend Analysis: "Effect" in Periods Without Change in Voting Technology					
Parameter:		θ^{94}	θ^{06}	$(\theta^{94}-\theta^{06})/2$	$(\theta^{94}+\theta^{06})$
Sample (terms):		1990-1994 (Paper-Paper)	2002-2006 (Electr.-Electr.)		
Share of Spending in Health Care	-	-0.006 (0.026)	-0.005 (0.013)	-0.0003 (0.014)	-0.011 (0.029)
N (state-ternns)		54	54	-	-
N (states/first-diffs)		27	27	-	-

Robust standard errors in parenthesis. Standard deviations in brackets. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}) . Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

Table 5: Robustness Checks

Dependent Variable	Sample Average	(1)	(2)	(3)	(4)	(5)
Panel A: Valid Votes and Health Care Spending						
Valid Votes/Turnout	0.829	0.102	0.103	0.105	0.123	0.133
	[0.112]	(0.017)	(0.016)	(0.013)	(0.014)	(0.014)
		{0.008}	{0.000}	{0.000}	{0.000}	{0.000}
Share of Spending	0.099	0.034	0.031	0.034	0.028	0.041
in Health Care	[0.037]	(0.008)	(0.011)	(0.010)	(0.013)	(0.017)
		{0.000}	{0.026}	{0.004}	{0.004}	{0.000}
Panel B: Birth Outcomes (Mothers Without Primary Schooling)						
Share with 7+ Prenatal Visits	0.362	0.069	0.069	0.062	0.033	0.026
	[0.123]	(0.040)	(0.017)	(0.015)	(0.042)	(0.053)
		{0.182}	{0.000}	{0.000}	{0.490}	{0.670}
Share with Low-Weight Births ($\times$100)	7.721	-0.529	-0.546	-0.757	-0.504	-0.460
	[1.110]	(0.244)	(0.210)	(0.286)	(0.479)	(0.492)
		{0.044}	{0.004}	{0.006}	{0.332}	{0.384]
Additional Controls	-	-	Yes	Yes	-	Yes
State-Specific Trends	-	-	-	Yes	-	-
Restricted Sample	-	-	-	-	Yes	Yes
N	-	81	81	81	69	69

Standard errors clustered at the state level in parenthesis. Standard deviations in brackets - [ ]. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets - { }. The unit of observation is a state-electoral term. Each row reports the estimation of equation (8) using the specified dependent variable. Each figure is from a separate regression using a sample covering three electoral terms, reporting the coefficient from the interaction between the share of voters living in municipalities above the 40,500 cutoff (as of 1996) and dummy indicator for the 1998 electoral term (1999-2002). State fixed effects, time effects and region-time effects are included in all regressions. Additional controls are (log) population, (log) GDP, poverty rate, and Gini index of income distribution. The restricted sample excludes states that did not follow the discontinuous assignment rule.

Appendix Table 1: Effects by Categories of State Spending

Parameter:		θ^{98}	θ^{02}	Linear Combinations $(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Administration and Planning	0.181	-0.072	0.121	-0.097	0.050
	[0.097]	(0.043)	(0.083)	(0.047)	(0.094)
Social Assistance	0.108	-0.016	-0.053	0.018	-0.069
	[0.054]	(0.016)	(0.024)	(0.015)	(0.029)
Education	0.176	-0.005	0.016	-0.010	0.010
	[0.037]	(0.013)	(0.015)	(0.010)	(0.020)
Judiciary	0.063	0.008	-0.024	0.016	-0.016
	[0.023]	(0.019)	(0.011)	(0.011)	(0.022)
Legislative	0.036	-0.002	0.015	-0.009	0.014
	[0.016]	(0.008)	(0.011)	(0.007)	(0.013)
Public Safety	0.074	0.002	-0.017	0.009	-0.015
	[0.026]	(0.015)	(0.024)	(0.014)	(0.028)
Transportation	0.053	0.010	-0.004	0.007	0.006
	[0.049]	(0.017)	(0.036)	(0.020)	(0.039)
Other Categories	0.210	0.036	-0.025	0.030	0.011
	[0.086]	(0.031)	(0.036)	(0.024)	(0.048)
N (state-ternns)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

Robust standard errors in parenthesis. Standard deviations in brackets. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}) . Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

Supplemental Materials (Not for Publication)

A. Summary Statistics

Tables A1 and A2 provides the summary statistics for the municipal and state-level sample, respectively.

B. Description of Voting Technologies

Figure A1 depicts the paper ballots used to cast a vote for state and federal legislators. A vote is cast by writing the number or name of a candidate in the specified boxed space. There are only (minimal) written instructions.

The interface of the EV technology is a small screen and a set of keys closely resembling a touch-tone phone with the addition of three colored buttons, as Figure A2 illustrates. Panel A depicts the initial screen a voter faces. It provides an instruction to vote for a state legislator (*deputado estadual*). Once the voter types the number of his choice of candidate, her information (photo, name, and party) appears on the screen, as depicted on Panel B. A voter can then "confirm" his vote by pressing the green button or "correct" his vote using the orange button. The former option casts a vote and the latter re-starts the process and returns to the initial screen. There are two ways a residual vote can be cast with EV. One is by pressing the "blank" (white) button. Another one is by typing a number that does not correspond to a candidate. Doing so will lead to the screen depicted on Panel C. Pressing the "confirm" button at this point will lead to a residual vote.

There are at least three features of the electronic technology that can reduce residual votes, especially from less educated voters. First, there is the introduction of visual aids (candidate photographs). Second, the machine provides instant feedback to the voter. When he types a wrong number, the machine informs him (Panel C). Note that even a completely illiterate voter that could not read the "wrong number" message on the screen would notice that a picture has not appeared on the screen. Obviously, a piece of paper cannot provide this type of feedback.

Third, the technology guides the user through the many votes (state legislator, federal legislator, senator, governor, president) he has to cast. It also informs the voter when

all votes are cast, making it less likely that he forgets to vote for one office. Moreover, with paper ballots, a voter may misvote by, for example, writing the number of a state legislator in the federal legislator box. The electronic device also makes salient how many digits are needed for a vote so that even a completely illiterate voter can take this as a cue for which vote he hast to cast. For example, Panel A asks the user for a 5-digit number, which has to be the case of a state legislator (as federal legislators have 4-digit and other offices use 2-digit numbers).

C. Additional RDD Results

This appendix provides two sets of additional results regarding the effect of electronic technology on valid votes. First, the effect of EV on residual (invalid) votes is decomposed by its effect on "blank" and "null" voting. Second, the estimated effects on valid voting in elections for other offices are presented (lower chamber of federal congress, federal senate, governor, and president). The evidence provided in this appendix supports the interpretation that EV increases valid voting by facilitating ballot operation and diminishing errors in cast votes.

A residual (i.e., not valid) vote is classified as either blank (*"branco"*) or null ("*nulo*"). When a paper ballot is in use, a vote is considered blank when nothing is written on the ballot, and it is considered as null when the number or name on the ballot cannot be assigned to any candidate. With electronic technology, a null vote is cast when the voter confirms a vote for a number that does not correspond to a candidate, and a blank vote is cast by pressing the "blank" button on the machine.[50]

Panel A of Table A3 presents the estimated treatment effects of EV on blank and null votes separately, with each measure as a share of the total turnout (which equals the sum of valid and invalid votes). The results are estimated with the exact same procedure of those reported on Table 1 and described on Section 2. The estimates indicated that EV reduces null voting by an amount equivalent to 8 p.p. of total turnout, with some minor variation depending on specification and bandwidth used. Blank votes, on the other hand, are reduced by about 4 p.p. of total turnout. Part of the larger effect on null votes can be explained by its higher prevalence under paper ballots (13.8% of voters left their paper ballots blank and 9.3% cast null votes). The results above are consistent with the interpretation that some votes were unable to properly fill the paper ballots (since a

[50]This description applies to the case of election under proportional representation rules, such as the ones for state legislature. In the case of plurality elections where a paper ballot requires checking a box, a blank vote occurs when no candidate is checked, and a null vote when multiple candidates are checked.

possible reaction for someone that is challenged by the task is to leave it blank).

Panel B of Table A3 reports the estimated effects of EV on the ratio between valid votes and turnout. The first line reports the same estimates provided on Table 1 for the state legislature, and the following lines present the similar estimated effects for elections for federal congress (lower chamber and senate), state governor, and federal president. Recall that these five separate races occurred simultaneously, and hence the turnout in all races is the exact same.

The main inference to be drawn from Panel B is that the effect of EV is larger in the proportional representation races where a paper ballot requires writing down the name or number of the candidate (lower chamber of congress and state legislature) than in the plurality races where a paper ballot involves checking a box (senate, governor, and president). Since writing a name/number is presumably more difficult than checking a box, especially for the less educated, the results support the interpretation that the electronic technology facilitates the task of voting. The effects on valid voting for the lower chamber of federal congress are larger than the ones for the state legislature. Comparison of the "pre-treatment means" on Table A3 indicate that this difference could be due to the fact that valid voting in federal elections is smaller than in state elections. The effects are particularly small (and statistically insignificant) for presidential elections. A possible explanation would be that since the presidential race dominates popular and media attention, voters are particularly better prepared to cast their votes for president, even when using the more error-prone paper ballot.

D. Robustness Checks on RDD Estimates

This section provides further results regarding the validity of the regression discontinuity estimates of the effects of EV presented on Section 2. Firstly, it shows that several covariates evolve smoothly around the cutoff. Secondly, it provides evidence against the manipulation of the position of the 40,500-voter threshold and also of the forcing variable (number of registered voters). Thirdly, it briefly discusses the presence of other discontinuous assignment rules across Brazilian municipalities, and why they are unlikely to affect the results.

D.1 Effects on Pre-Determined Covariates

Table A4 provides a standard robustness check in the RDD literature: testing for effects on pre-determined covariates. In the same spirit that insignificant differences between

covariate means in treatment and control groups are interpreted as evidence of successful randomization in controlled trials, the robust finding of zero effects on demographics (income and education) and geographical location (latitude and longitude) reported on Table A4 can be seen as evidence of the "quasi-random" nature of our RDD.

D.2 Manipulation of the Forcing Variable and Threshold Position

To test the possibility of strategic manipulation, Figure A3 plots the distribution of number of voters as of 1996 (the forcing variable in the RDD) across municipalities. Each marker represents the number of municipalities that fall into a 4,000-voter interval. For example, the circle to the immediate right of the vertical line represents the number of municipalities that have between 40,500 and 44,500 registered voters.

If strategic manipulation has taken place, it would likely reflect in a jump around the threshold. For example, voter registration was encouraged in municipalities that would be close to the left of the threshold, then the number of municipalities just above the threshold would probably be unusually large compared to the number of municipalities just below it, creating a discontinuity in the distribution. The same logic would apply to deterrence of registration. Figure A3, however, shows that the distribution of municipalities is smooth around the threshold, providing no evidence of manipulation. This is, as discussed in Section 2, expected since the forcing variable (number of voters in 1996) was measured and determined before the announcement of the threshold (in 1998).

Another potential threat to validity would occur if the position of the threshold was manipulated. For example, if the federal electoral authority could have chosen the 40,500-voter cutoff in order to extend EV in a state that had a large number of municipalities just above this cutoff. To test if the threshold was manipulated based on state characteristics, I check if its possible to predict the voting technology used in a municipality (i.e., on which side of threshold it is) with the information on which state it belongs.

The test is performed in two steps. Firstly, I compute the predicted values from a regression of a dummy indicator for electronic vote use (i.e., being to the right of the threshold) against a full set of state of state dummies (using municipal-level data for 1998). Secondly, I test if the relationship between these predicted values and the forcing variable (number of voters) is smooth around the threshold. Figure A4 provides the graphical evidence on this second step: the predicted value evolves continuously around the threshold.

The evidence in Figure A4 is supportive of non-manipulation of the threshold's position. If the cutoff was chosen in order to target a particular state, then information on

which state a municipality is located in (captured by the state dummies) would predict EV and leads to a "jump" around the threshold. In other words, non-manipulation implies that the probability of a municipality "just below" and "just above" the threshold being in a particular state should be, on average, the same. Figure A4 shows that this is the case.[51]

D.2 Other Discontinuous Assignments

To the best of my knowledge, there is only one discontinuous assignment across Brazilian municipalities that, like the one used in this paper, is based on the number of registered voters. It occurs in municipal elections that do not occur simultaneously with the state legislature elections analyzed in this paper. Municipalities with more than 200,000 voters should use runoff rules instead of plurality to elect its mayors. Given that 200,000 is far from the the 40,500-voter threshold in this paper and that municipal and state elections are never held in the same year, there is no reason to believe that it can confound the results.[52]

There are other discontinuous assignments based on a municipality's population (and not number of voters, as the one used in this paper). All but one were created *after* the 1998 election, and hence cannot account for the results.[53] The exception is a multi-threshold rule regarding the distribution of federal funds to municipal governments. Since this rule has been present throughout the whole period analyzed in this paper, any confounding effects it could have on the results should also be seen in the placebo tests using the 1994 and 2002 elections. Since the behavior of valid voting is smooth around the 40,500-voter threshold in those two elections, it is unlikely that the assignment of federal funds confounded the results.[54]

[51]Note also that by using predicted values from all state dummies, this test has more power than the alternative of testing if every individual state has a continuous distribution of municipalities around the threshold. The rationale behind this procedure is similar to that of, in a randomized trial context, testing if all available covariates can predict treatment status, as opposed to checking for treatment effects on individual covariates.

[52]Fujiwara (2011), Chamon et al. (2009) and Goncalves et al. (2008) exploit this regression discontinuity design to estimate the effects of runoff systems.

[53]Population-based discontinuous assignments created in the 2000s include regulations for the size of municipal legislatures and the wages of its members, and also restrictions on the use of firearms by municipal police forces. Ferraz and Finan (2011) exploit the RDD regarding legislator wages.

[54]Litschig and Morrison (forthcoming), and Brollo et al. (forthcoming) exploit this discontinuity to estimate effects of government grants.

E. Graphical Analysis of State-Level Results

Figures A5 and A6 present a graphical representation of the main results provided on Table 3. Since the estimations include a set of region-time dummies, the graphical analysis is based on residuals of a regression of the (change in) variables against the region dummies. Hence the slopes in the fitted lines match the point estimates presented on Table 3.

The top left graph of Figure A5 plots the (residual of) share of voters living in municipalities above the cutoff on the x-axis and the *differences* in the (residual of) valid vote ratio observed in 1994 and 1998 (the paper-only and discontinuity elections) on the y-axis. A clear positive relationship is observed, as expected since the variable in the horizontal axis has a *positive* association with EV in this period. The middle graph on the top row plots the relationship between the *exact same variable on the x-axis* and the 1998-2002 *differences* in valid voting (i.e., between the discontinuity and electronic elections). Since in this case the share of voters living in municipalities above the cutoff is *negatively* associated with EV, the relationship changes sign, as expected.

As discussed in the paper, this sign switch in the slopes of the fitted curves is interpreted as the causal effect of EV on valid votes. The top right plot on the then pools the data from both previous graphs. It plots the first differences for both the 1994-1998 and 1998-2002 against the changes in the share of electorate using EV. Since change in EV use equals S_i in 1994-1998 period and $1 - S_i$ in the 1998-2002 period, the bottom graph is the same of overlapping the "mirror image" of the middle graph (i.e., the same plot with $1 - S_i$ instead of S_i in the x-axis) over the left graph. This exercise allows for a graphical evaluation of the similarity of the slopes in the previous graphs. In other words, the left, middle, and right plots are the graphical counterparts of columns (1), (2), and (3) of Table 3. The "tighter" fit on the left graph exemplifies why the pooled estimates (column 3) are usually more precisely estimated than the ones on columns (1) and (2).

The second row of Figure A5 repeats the exercise for the change in health care spending. Figure A6 does the same for prenatal visits and low-birth weight. In all cases, a graphical sign switch and a association between EV and the outcomes can be observed (especially in the plots to the right, which pool the data and allow a more precise estimation).

F. Additional Results on Prenatal Visits

Table A5 presents results on the share of (uneducated) mothers with 0 or 1-6 prenatal visits. It follows the same format as Panel B of Table 3 in the text, which presents results for mothers with 7+ prenatal visits. The results indicate that there is no association between EV use and the share of mothers with 0 prenatal visits, with small (and insignificant) estimates. On the other hand, there is a clear sign-switch and a negative impact of EV on the number of mothers with 1-6 visits. The size of this negative impact is comparable to the positive impact on the share of 7+ visits, suggesting that EV increased prenatal visits mainly at the intensive margin.

G. Effects on Party Seat Shares

Table A6 provides the effects of EV on a party's representation in the state's legislative assembly. Effects are provided for the ten most represented parties, and the smaller parties are aggregated into the "other parties" categories. Parties are referred to by their local acronyms (which is how they are known to most Brazilian voters and how they appear in ballots). Table A6 follows the same pattern used for Table 3. The only parties that follow the sign-switch pattern and have a significant implied average effect (column 3) are the PT - Partido dos Trabalhadores (Workers' Party) and the PDT - Partido Democrático Trabalhista (Democratic Labor Party), two traditional left-wing parties.

Additional References for Supplemental Materials

Brollo, Fernanda, Tomasso Nannicini, Roberto Perotti and Guido Tabellini (forthcoming). "The Political Resource Curse." *American Economic Review.*

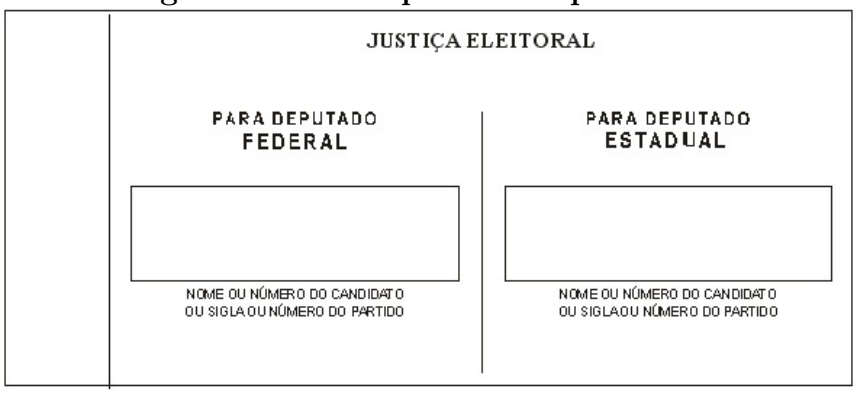
Chamon, Marcos, Joao M. P. de Mello and Sergio Firpo (2009). "Electoral Rules, Political Competition and Fiscal Spending: Regression Discontinuity Evidence from Brazilian Municipalities" *IZA Discussion Paper n. 4658.*

Fujiwara, Thomas (2011). "A Regression Discontinuity Test of Strategic Voting and Duverger's Law." *Quarterly Journal of Political Science,* 6(3-4): 197-233.

Gonçalves, Carlos E. S., Ricardo A. Madeira and Mauro Rodrigues (2008). *Two-ballot vs. Plurality Rule: An Empirical Investigation on the Number of Candidates.* Mimeo.

Litschig, Stephan and Kevin Morrison (forthcoming). "The Impact of Intergovernmental Transfers on Education Outcomes and Poverty Reduction." *American Economic Journal: Applied Economics.*

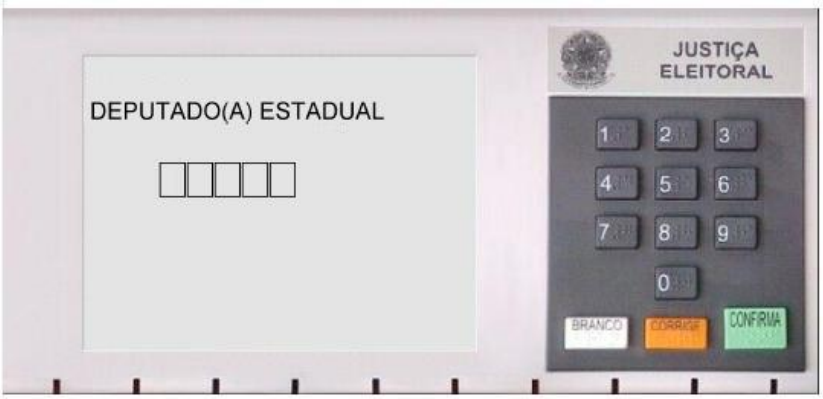
Figure A1: Example of a Paper Ballot



The picture above depicts a paper ballot used to elect federal and state legislators. The sentence above the box where voters write their vote reads "for federal legislator" and "for state legislator" in the left and right columns, respectively. The sentence below the box reads "name and number of candidate or party acronym or number"

Figure A2: Interface of the Electronic Voting Device

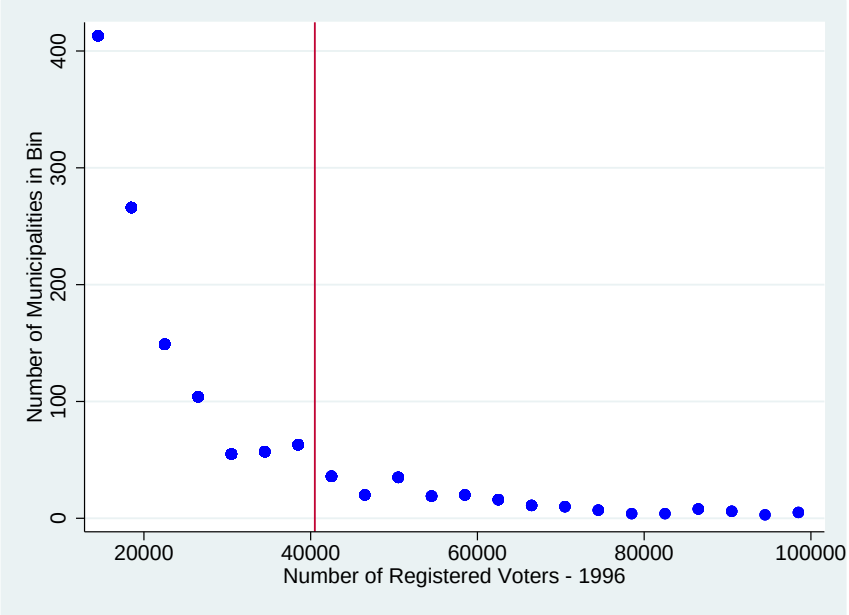
Panel A: Initial Screen of Voting Interface



Panel B: A Vote for (fictional) Candidate
(Number 92111, Name: Monteiro Lobato, Party: PLT)

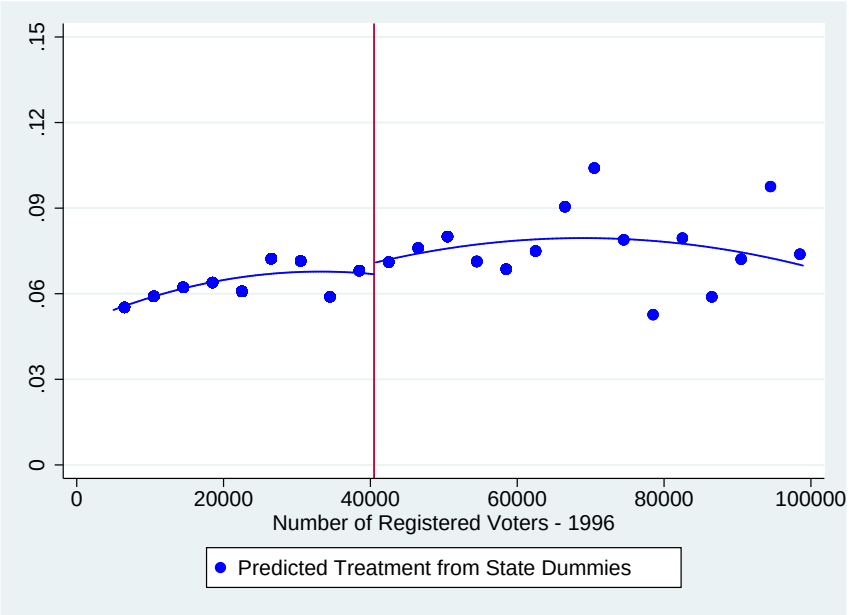
Panel C: A Residual Vote (There is no Candidate with Number 88888)

Figure A3: Testing Manipulation of Forcing Variable



Each marker represents the number of municipalities in a 4,000-voter bin. The vertical line marks the 40,500-voter threshold.

Figure A4: Testing Manipulation of the Threshold Position



Each marker represents the average value of the variable in 4,000-voter bin. The continuous lines are from a from a quadratic fit over the original ("unbinned") data. The vertical line marks the 40,500-voter threshold.

Figure A5: Electronic Voting Phase-in, Valid Votes, and Health Spending

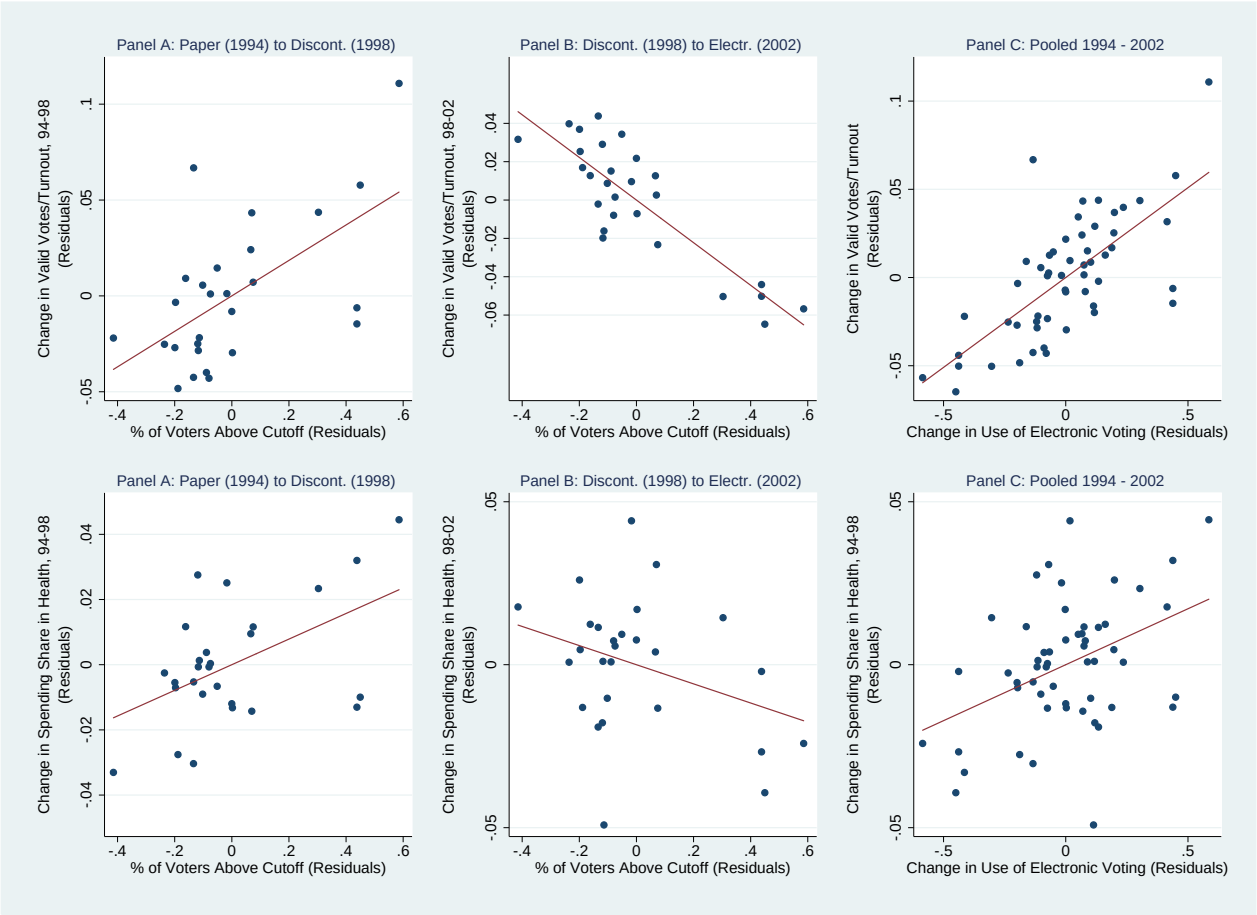


Figure A6: Electronic Voting Phase-in, Prenatal Visits, and Birthweight

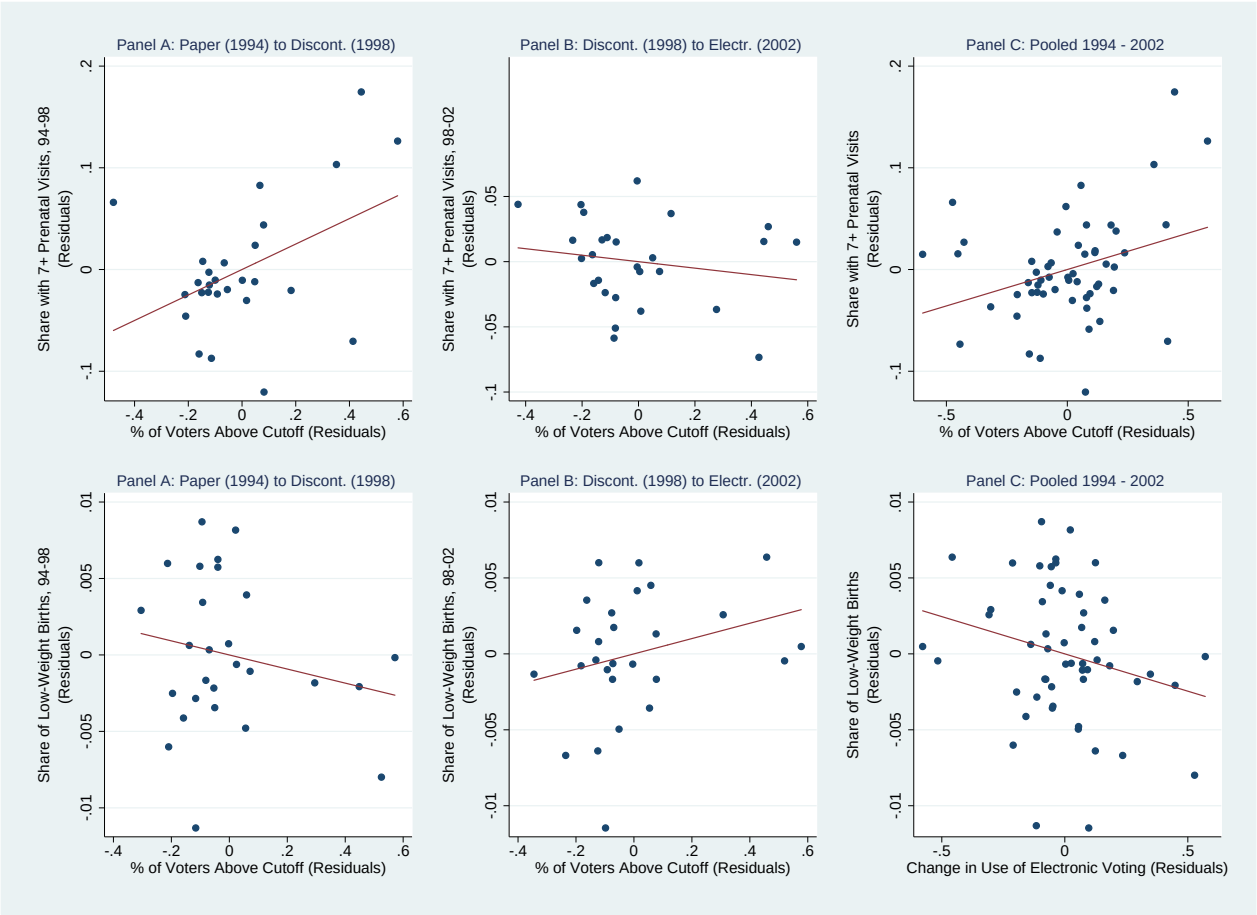


Table A1: Summary Statistics for Municipal Sample

Variable	Year of Observation	Number of Observations	Mean	Standard Deviation
Panel A: Municipalities With an Electorate Size Between 20,500 and 60,500				
Valid Votes/Turnout	1994	557	0.672	(0.095)
Valid Votes/Turnout	1998	558	0.784	(0.094)
Valid Votes/Turnout	2002	558	0.923	(0.024)
Turnout/Registered Voters	1998	558	0.765	(0.089)
Registered Voters/Population	1998	558	0.726	(0.094)
Illiteracy Rate (Adults 25+)	1991	558	0.309	(0.172)
Monthly Income (*reais* per capita)	1991	558	160.61	(86.01)
Latitude (degrees)	-	558	-15.71	(8.50)
Longitude (degrees)	-	558	45.53	(6.26)
Panel B: All Municipalities				
Valid Votes/Turnout	1994	4810	0.653	(0.099)
Valid Votes/Turnout	1998	5282	0.755	(0.087)
Valid Votes/Turnout	2002	5282	0.928	(0.026)
Turnout/Registered Voters	1998	5282	0.765	(0.092)
Registered Voters/Population	1998	5282	0.748	(0.141)
Illiteracy Rate (Adults 25+)	1991	5282	0.360	(0.183)
Monthly Income (*reais* per capita)	1991	5282	123.11	(73.09)
Latitude (degrees)	-	5282	-16.53	(8.23)
Longitude (degrees)	-	5282	46.36	(6.32)

Table A2: Summary Statistics for State Sample: 1995-2006 (3 Legislative Terms)

Variable	Number of Observations	Mean	Standard Deviation
Share of Voters Above EV Cutoff	27	0.52	(0.26)
Valid Votes/Turnout - State Legislature Elections	81	0.83	(0.11)
Average Legislators Party Position	81	4.62	(0.60)
State Expenditures on Health Care (2000 *reais* per capita)	81	99.66	(82.95)
Share of State Budgets Spent on Health Care	81	0.10	(0.04)
Share of Uneducated Mothers with 7+ Pre-Natal Visits	81	0.36	(0.12)
Share of Educated Mothers with 7+ Pre-Natal Visits	81	0.57	(0.13)
Share of Uneducated Mothers with Low-Weight Births(x100)	81	7.27	(1.10)
Share of Educated Mothers with Low-Weight Births(x100)	81	6.261	(1.58)
Population (thousands)	81	6294	(7572)
GDP (millions of 2000 *reais*)	81	39.31	(72.46)
Gini Index - Income	81	0.56	(0.034)
Poverty Rate	81	0.40	(0.16)

Table A3: Additional Estimated Treatment Effects of Electronic Voting

	Pre-Treat. Mean	(1)	(2)	(3)	(4)	(5)
Panel A: Effects on Null and Blank Votes, State Legislature Election						
Null Votes/Turnout	0.138	-0.093	-0.086	-0.084	-0.081	-0.076
		(0.007)	(0.010)	(0.015)	(0.010)	(0.015)
Blank Votes/Turnout	0.093	-0.035	-0.036	-0.040	-0.035	-0.042
		(0.006)	(0.008)	(0.012)	(0.009)	(0.013)
Panel B: Effects on Valid Votes/Turnout, by Office						
State Legislature	0.769	0.128	0.122	0.124	0.116	0.118
		(0.011)	(0.016)	(0.025)	(0.017)	(0.026)
Federal Congress (Lower Chamber)	0.681	0.218	0.210	0.205	0.212	0.207
		(0.012)	(0.018)	(0.030)	(0.019)	(0.031)
Federal Congress (Senate)	0.705	0.081	0.087	0.100	0.079	0.108
		(0.014)	(0.020)	(0.030)	(0.020)	(0.031)
State Governor	0.748	0.078	0.078	0.082	0.071	0.093
		(0.012)	(0.017)	(0.026)	(0.019)	(0.028)
President	0.799	0.018	0.021	0.031	0.011	0.034
		(0.011)	(0.013)	(0.021)	(0.015)	(0.022)
Bandwidth:	-	20,000	10,000	5,000	20,000	10,000
Specification:	-	Linear	Linear	Linear	Quadratic	Quadratic
Observations:	-	558	229	116	558	229

***,**,* Significant at the 1%, 5%, 10% level. Each figure is from a separate local polynomial regression estimate with the specified bandwidth and specification (quadratic or linear). Separate polynomials are fitted on each side of the threshold. The pre-treatment mean is the estimated value of the dependent variable for a municipality with 40,500 registered voters that uses paper ballots (based on the specification on column 1). Heteroskedasticity-robust standard errors in parenthesis.

Table A4: Covariate Smoothness around Cutoff

	Pre-Treat. Mean	(1)	(2)	(3)	(4)	(5)
Monthly Income (1991 *reais*)	174.83	0.908	6.096	14.017	10.759	38.957
		(16.292)	(22.097)	(32.863)	(24.085)	(34.899)
Gini Index (Income)	0.575	-0.002	0.002	-0.005	0.008	-0.004
		(0.010)	(0.013)	(0.017)	(0.013)	(0.017)
Latitude (degrees)	-16.40	-0.744	0.361	-0.674	0.580	-2.713
		(1.565)	(2.070)	(2.998)	(2.256)	(3.119)
Longitude (degrees)	45.51	0.322	0.550	2.685	0.597	2.812
		(1.248)	(1.636)	(2.466)	(1.797)	(2.511)
Illiteracy Rate	0.315	-0.043	-0.076	-0.041	-0.023	-0.089
		(0.038)	(0.046)	(0.065)	(0.041)	(0.059)
Share w/o 4 years of Schooling	0.483	-0.018	-0.026	-0.041	-0.001	-0.059
		(0.031)	(0.041)	(0.065)	(0.045)	(0.069)
Share w/o 8 years of Schooling	0.789	-0.009	-0.017	-0.030	-0.008	-0.038
		(0.015)	(0.020)	(0.032)	(0.022)	(0.034)
Bandwidth:	-	20,000	10,000	5,000	20,000	10,000
Specification:	-	Linear	Linear	Linear	Quadratic	Quadratic
Observations:	-	558	229	116	558	229

Robust standard errors in parenthesis. The unit of observation is a municipality. Each figure is from a separate local polynomial regression estimate with the specified bandwidth and specification (quadratic or linear). Separate polynomials are fitted on each site of the threshold. The pre-treatment mean is the estimated value of the dependent variable for a municipality with 40,500 registered voters that uses paper ballot (based on the specification on column 1). Details on the dependent variables in the text.

Table A5: Effect of Pre-Natal Visits (Mothers with less than 8 years of schooling)

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98} - \theta^{02})/2$	$(\theta^{98} + \theta^{02})$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Share with 0 Visits	0.099	-0.040	-0.088	0.024	-0.128
	[0.092]	(0.073)	(0.053)	(0.045)	(0.090)
Share with 1 to 6 Visits	0.537	-0.082	0.111	-0.097	0.029
	[0.123]	(0.053)	(0.034)	(0.031)	(0.063)
N (state-terms)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

Robust standard errors in parenthesis. Standard deviations in brackets. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}) . Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

Table A6: Effect on Party Seat Shares

Parameter:		θ^{98}	θ^{02}	Linear Combinations $(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
PCdoB	0.010	-0.002	0.019	-0.011	0.017
	[0.016]	(0.003)	(0.013)	(0.007)	(0.014)
PDT	0.066	0.089	-0.066	0.077	0.022
	[0.054]	(0.049)	(0.041)	(0.032)	(0.063)
PFL	0.150	-0.023	-0.025	0.001	-0.048
	[0.106]	(0.086)	(0.031)	(0.046)	(0.091)
PL	0.052	-0.025	0.030	-0.028	0.005
	[0.048]	(0.044)	(0.032)	(0.027)	(0.054)
PMDB	0.170	0.038	0.078	-0.020	0.115
	[0.096]	(0.082)	(0.041)	(0.046)	(0.091)
PPS	0.022	-0.028	0.014	-0.021	-0.014
	[0.030]	(0.016)	(0.021)	(0.013)	(0.026)
PSB	0.042	0.010	0.052	-0.021	0.062
	[0.056]	(0.021)	(0.027)	(0.017)	(0.034)
PSDB	0.112	-0.054	-0.099	0.023	-0.153
	[0.096]	(0.063)	(0.045)	(0.039)	(0.077)
PT	0.094	0.030	-0.087	0.058	-0.058
	[0.062]	(0.027)	(0.022)	(0.017)	(0.035)
PTB	0.070	0.003	-0.025	0.014	-0.022
	[0.064]	(0.098)	(0.020)	(0.050)	(0.100)
Other Parties	0.214	-0.038	0.110	-0.074	0.072
	[0.116]	(0.103)	(0.067)	(0.061)	(0.123)
N (state-terms)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

Robust standard errors in parenthesis. Standard deviations in brackets. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}) . Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.