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

Redistribution of resources is a key activity of the state, and its extent and form vary substantially worldwide. A government's decision of how much to redistribute, and to whom, is embedded in a political system. This has long been recognized in positive theories where elections determine the allocation of resources, such as Romer (1975), Roberts (1977), and, in particular, Meltzer and Richard (1981).

A key prediction of these models is that increased political participation of poorer voters leads to more redistribution (toward them). Moreover, the conflict between an elite and masses demanding redistribution is the basis of theories of democratization and enfranchisement (Acemoglu and Robinson, 2000, 2006). This insight also has important policy consequences: a disadvantaged group's electoral sway may be a precondition to policies targeting their wellbeing.

The evidence on this, however, is mixed and puzzling. While some estimate positive effects of democratization on government spending (e.g., Aidt and Jensen, 2009a, 2009b, 2013; Acemoglu et al, 2013), others (e.g., Mulligan et al, 2004) argue there are no differences in policymaking in autocracies and democracies. There are also findings indicating that pre-existing factors, such as liberalizations, determine the direction of democratization's effect (Persson and Tabellini, 2006).[1]

This paper provides evidence on how improving the political participation of less educated (poorer) voters can advance policies targeting them and affect their outcomes. It estimates the effects of an electronic voting (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). Hence, while it addresses issues similar to the literature on democratization and *de jure* enfranchisement (discussed below), it does so at an "extensive margin," focusing on improvements in participation within a context where universal suffrage is already established.

While filling a ballot is 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

[1] Acemoglu et al (2013) surveys the evidence on democracy's effect on redistribution and find it "*both voluminous and full of contradictory results*." Putterman (1996) provides an overview on explanations for the variation in redistribution across democracies. See also Alesina and Angeletos (2005) and Benabou and Tirole (2006) for more recent analyses and a discussion of this literature. There is also a large related literature on democratization and income, such as Acemoglu et al (2014) and references therein.

note."[2] Moreover, Brazilian paper ballots required citizens to write down their vote 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 guiding the voting process step by step, facilitated voting and reduced errors.

This paper first estimates the electoral effects of this voting technology exploiting a regression discontinuity design (RDD) 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 then argues that this enfranchisement of the less educated citizenry did indeed affect policy in a manner consistent with political economy theories of redistribution. 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 more likely to use private services (Alves and Timmins, 2003). The less educated have thus relatively stronger preferences for public health services, and a shift in spending toward health care can be interpreted as redistribution to the poor.[3] Consistent with this interpretation, I also find that EV raised the number of pre-natal visits by health professionals and lowered the prevalence of low-weight births (below 2500g) by less educated women, but not for the more educated.[4]

[2] Only 58% of adults (aged 25+) completed 4th grade. Figures are from the 1991 Census.

[3] Health care is also a very salient issue for voters and politicians, as discussed in Section 3.1.

[4] 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, 2013), the focus on birth outcomes is also motivated by issues of data availability and that newborn health can respond rapidly to health care improvements, which is important for an empirical strategy based on the sharp timing of EV roll-out.

The state-level results exploit the fact that the discontinuous assignment in the 1998 election created specific and unusual differences in the timing of 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 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 shares. Intuitively, empirical strategy tests if outcomes of interest track this same pattern.

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 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 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 or mean-reversing shock that (positively) affects health care spending would need to i) be growing faster in states with higher share of voters above the cutoff, and ii) change to growing slower in such states with a timing matching election dates.

Four sets of additional tests provide evidence against these possible confounding effects. First, the timing of effects occur quickly after elections, implying that possible omitted shocks (and their mean reversion) must follow quite specific timing. Second, the share of voters above the cutoff is orthogonal to changes in outcomes in periods when it is not associated with changes in voting technology, addressing the issue of pre-exiting trends. Third, I find negligible (placebo) effects on variables not expected to be affected by EV, such as general economic conditions and birth outcomes for more educated mothers, as well as spending by *municipal* governments, which were exposed to EV under different timing but should also respond to shocks to health care demand. Fourth, the results are robust to controlling for (non-linear) time trends interacted with state characteristics, as well as instrumental variable strategy that focuses on the distribution of municipalities closer to the cutoff.

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 mothers with more than seven prenatal visits by 7 p.p. and lowered the prevalence of low-weight births by 0.5 p.p. (respectively, a 19% and -6.8% change over sample averages).

This paper communicates with three different strands of economic literature. First, it is most closely related to the within-country analysis of enfranchisement's effects, such as Cascio and Washington (2014), Husted and Kenny (1997), and Naidu (2012). These papers focus on legally-mandated franchise extension to racial minorities in the US, while I focus on a *de facto* enfranchisement episode in a developing context. It is also connected with the analysis of nationwide enfranchisement episodes (Acemoglu and Robinson, 2000, 2006; Aidt and Jensen, 2009a, 2009, 2013), as well as female enfranchisement. Miller (2008), in particular, finds that women's suffrage affected child health in the US, and this paper thus corroborates the connection between political participation and health, although for the case of poorer voters as opposed to women.[5]

Second, the introduction of EV in Brazilian elections is also studied by Hidalgo (2013) and Moraes (2012), which exploit the same discontinuous assignment in EV use in the 1998 elections. In particular, Hidalgo (2013) estimates the effect of EV on voting outcomes at the municipal level. Moraes (2012) performs a similar analysis and additionally focuses on how the introduction of EV in state and federal elections affected spending by municipalities above and below the cutoff. Neither paper addresses spending at the state level (the focus of this paper).[6] A growing literature evaluates voting technologies, but focus mostly on election outcomes.[7] Two notable exceptions the exception of Baland and Robinson's (2008) and Anderson and Tollison's (1990) analyses of non-secret ballots in Chile and USA, respectively. The former finds that it helped support patron-client relationships, while the latter that it diminished public spending. Third, a set of pap ers

[5]Kenny and Lott (1999) also studies the impact of women's suffrage in the US. Besley and Kudamatsu (2006) and Kudamatsu (2012) provide cross-country evidence on democracy's effect on health outcomes.

[6]The results in Hidalgo (2013) and Moraes (2012) largely corroborate those in this paper.

[7]Garner and Spolaore (2005), Dee (2007) Card and Moretti (2007), Shue and Luttmer (2009), and Ansolabehere and Stewart (2005) analyze US voting technologies. Crost et al (2013) and Callen and Long (2014) analyse how voting technologies relate to fraud and its consequences in the Philippines and Afghanistan, respectively.

studies interventions to raise political responsiveness to (a group of) voters and its consequences for policymaking within developing countries, and the introduction of EV can be seen as such an intervention.[8]

The remainder of the paper is divided in three sections. Section 2 describes the electoral process and the introduction of EV in Brazil. It also reports RDD 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, and addresses possible confounding factors. 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*).[9] Federal legislation establishes the same election rules for all states. Legislators are elected under an open-list proportional representation with the entire state being 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.[10]

The combination of large districts and voting for individual candidates makes voters face a large number of candidates, making listing names or illustrations 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

[8]Examples of interventions are political reservations for minorities (Pande, 2003; Chattopadhyay and Duflo, 2004), participatory budgeting (Besley et al., 2005), or plebiscites (Olken, 2010).

[9]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.

[10]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.

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 the impossibility of simplified voting under paper ballots, as well as the overall complexity of voting.

Until 1994, Brazilian elections used only paper ballots. Voting for a state legislator involved writing down the chosen candidates' name or electoral number, a five-digit code assigned by the election authority to each candidate. A paper ballot is depicted in Figure 1. There are only written instructions: to write "the name or number of candidate or party" on the box for "state" or "federal" legislator. 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. Figure 1 depicts the initial screen of the device, and the screen just before a vote can be confirmed.

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 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 familiar devices (telephone keypads and ATMs). As noted by electoral authority staff, "*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).[11]

The Supplemental Materials further describes voting procedures under both paper

[11] 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.

and EV technologies and 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.[12]

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 manufacturer and economies of scale in the distribution within large municipalities.[13] In the next election, EV became the sole method of collecting votes.[14]

This threshold-based rule used in the 1998 election creates a standard RDD which can be exploited to estimate the effects of EV. Such interpretation requires agents to have limited control over the forcing variable: registered voters. This is likely the case since the assignment rule (e.g., the value of the cutoff) was announced in May 1998 and the forcing variable is voters registration for municipal elections two years earlier (1996).[15]

Finally, 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

[12]The head designer of the technology describes the new system's association with a reduction in residual votes as "*a surprise*" (Tribunal Superior Eleitoral, 2011).

[13]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.

[14]Municipal elections do not coincide with state races. EV was also used in the 1996 municipal elections (in selected large cities and state capitals), and by the next municipal election (2000) was the first where only EV was used. This paper, however, focuses only on state elections (1994, 1998, and 2002).

[15]Information that EV would be gradually rolled out was available at least since 1995, but the decision on which municipalities would use it in the 1998 election was only made months before the election.

section, but will be used as robustness checks of the state panel estimates on Section 3.

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 5,281) above the 40,500-voter cutoff used EV in 1998.[16] 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 or party and then counted in their vote shares. A vote that is not valid is defined as a residual vote. Hence, turnout 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. The Supplemental Materials discusses how a vote can be residual under paper and EV, as well as results for federal and gubernatorial races.

2.2.2 Estimation Framework

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

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

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

[16] Seven (out of 5,281) 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.

of one using paper ballots. Hence, *TE* identifies the treatment effect for a municipality with 40,500 voters. This highlights that the estimated effects are "local" and apply only to municipalities "at the cutoff." In practice, one should be careful in extrapolating the effects to smaller and larger municipalities. In particular, the nature of elections might be very different in large cities, and the effect of EV different in those cases.

The estimation follows the recommendations in Imbens and Lemieux (2008), Lee and Lemieux (2010), and Imbens and Kalyanaraman (2012). The limits are estimated non-parametrically by local linear regression. This consists of estimating a regression of y_m on 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. Although linear regressions are used, the nature of estimation is non-parametric, and the consistency of the results holds for arbitrary shapes of the relationship between y_m and v_m.[17]

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

$$y_m = \alpha + \beta 1\{v_m > 40,500\} + \gamma v_m + \delta v_m 1\{v_m > 40,500\} + \epsilon_m \tag{2}$$

Where $1\{v{>}\mathit{40{,}500}\}$ is a dummy variable that takes value one if the number of registered voters is above 40,500. The estimate of β is the treatment effect. No covariates are included in equation (2), although the next section considers the inclusion of state fixed effects as a robustness check.

A key decision is h, the kernel bandwidth, and the trade-off between precision and bias. This paper reports estimates based on the Imbens and Kalyanaraman's (2012) optimal bandwidth choice (IKBW), which is itself a function of the data and hence different for each y. In some (but few) cases the computed IKBW is relatively large, and to reinforce the local intuition of RDDs, the largest possible IKBW is capped at 20,000 voters. There are 428 (130) municipalities below (above) the cutoff in the sample with

[17] 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. In other words, the local average treatment effect (LATE) equals the average treatment effect (ATE) at the cutoff. Moreover, dropping the seven non-compliers from the sample or treating them as "treated" lead to no change in any of the results.

a 20,000-voter bandwidth. To check the robustness of results, I also provide estimates based on $h = 10,000$ and $h = 5,000$. These samples include 151 (78) and 75 (41) municipalities above (below) the cutoff, respectively.

2.3 Regression Discontinuity Results

2.3.1 Electronic Voting and Residual Votes

To check if municipalities above and below the cutoff are comparable, Table 1 estimates effects on "pre-determined" covariates calculated from the 1991 Census data. These are monthly income, income inequality, education variables, geographical location, population, and the urbanization rate (share of population in an area officially designated as urban). All effects are close to zero in magnitude and statistically insignificant.

Before reporting the main results, some graphical evidence is provided. Figure 2 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 3 repeats this exercise for turnout (as a share of registered voters) and voter registration (as a share of total population)[18] 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 immediately 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

[18]Total population, and not voting-age population, is used. As the main interest of this analysis is if registration evolves smoothly around the cutoff, the choice of denominator is not particularly relevant.

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.[19]

Panel A of Table 2 presents the local linear 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. Adding state fixed effects has little impact on the results.[20]

Table 2 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. Panel A also presents the estimated treatment effects on turnout (as a share of registered voters) and voter registration (as a share of population). Confirming the graphical analysis, the effects are numerically small and statistically insignificant. Since population is not affected by crossing the cutoff (Table 1), these results imply that the number of valid votes is driving all the results: turnout, measured either as a share of registered voters or population, is not affected.

In addition to the covariate smoothness tests on Table 1, Panel B of Table 2 also provides a more more stringent placebo test. It estimate 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 supports the previous interpretation of the results, since if any unobserved difference in municipalities just above and just below the threshold was driving the result observed in 1998, it would likely have been present in the preceding or subsequent election. Finally, there is no effect of effect of crossing the cutoff (i.e., using EV in 1998) on turnout in the 2002 election: the estimated effect is -0.002 with a s.e.=0.011 when using the IKBW.[21]

[19]Figure 3 and Table 2 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."

[20]Adding state fixed effects to the specification on column (1) yields an estimate of 0.139 (s.e.=0.013).

[21]Turnout is measured as a share of registered voters. There is also no effect on the registered voters/population ratio (t.e.=-0.002, s.e.=0.012) and population measured in the 2000 Census (the effect is equal 1.62 thousand people from a pre-treatment mean of 69.79 thousand, with s.e.= 3.04 thousand.

2.3.2 Which parties benefited 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 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 and argue that more leftist positions in the index can be associated with more redistribution to poorer voters.[22]

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 outcome, biasing the results toward zero.

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:

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

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).[23]

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 sample municipalities. 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.[24] 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

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

[23] In practice, 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).

[24] 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.

these estimates cannot be driven by entry of parties or candidates.

Panel C of Table 2 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.[25]

The effect on column (1) indicates that the vote share-weighted position of parties moves by 0.22 to the left when EV is used. Adding state fixed effects leads to very similar estimates. This effect is close to a third of a standard deviation in the distribution of the outcome. The effect is robust to using the 10,000-voter bandwidth, but becomes smaller and imprecise when the 5,000-voter bandwidth is used. Given this and the issues in measuring state-level party positions discussed above, the results should be interpreted with care, but they suggest that under EV, parties with relative left-wing inclination benefit disproportionately (i.e., their share of the valid votes increase).

2.3.3 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. 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 3 repeats the estimation presented on the first line of Table 2, 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."[26] The median illiteracy rate in the sample with a 20,000-voter bandwidth equals 25.4%. The

[25]Because of the inclusion of state averages, the standard errors are also clustered at the state level. To address the relative small number of clusters, p-values based on the Cameron et al. (2008) wild bootstrap procedure are also computed: their values for the estimates on columns (1), (2), and (3) are 0.038, 0.002, and 0.568, respectively.

[26]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.

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 15–18 p.p. . The effect on municipalities with below-median illiteracy is in the 9–11 p.p. range. Table 3 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 IKBW is used.

2.3.4 Alternative Explanations

First, it must be noted that the at-large nature of elections implies that any two municipalities in the same state face 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.[27]

Second, manipulation of the forcing variable (voter registration) or of the threshold itself can, in principle, invalidate the causal interpretation of the results. As discussed in Section 2.1, 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. 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, the Supplemental Materials discusses the possibility of electoral fraud as a (non-mutually exclusive) mechanism behind EV's effects, and indicate there is little evidence of fraud.

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

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 less educated citizens. Its consequences on government spending, health service provision and infant health are expounded in the next section.

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.[28] This section uses state-level panel data on election outcomes, legislative representation, fiscal spending, health care utilization, and newborn health to analyze these effects.

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. Municipalities are responsible for 20% and the federal government for the remainder (Andrade and Lisboa, 2002). 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 household survey data, they find

[28] In particular, municipal elections occurred in 1996 (when virtually all municipalities used paper ballots) and 2000 (when only electronic voting was used). There is thus virtually no within-year variation in voting technology use across municipalities, making an analysis based on municipal data unattractive.

that 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*." These differences are also prevalent in nationally representative opinion surveys: 49% of Brazilians list "improving health care services" as a government priority (more than any other area). This number rises to 51% for those with family income below two minimum wages, but drops to 40% for those with income above ten.[29]

When these differing preferences are present, theories of redistributive politics predict that increased political participation (or enfranchisement) of the less educated should raise government spending on health care, as discussed in the introduction. For instance, 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 serve as motivating theory for the present 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-based redistributive politics model (Meltzer and Richard, 1981) 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.[30] Second, this paper analyzes policy outcomes that

[29]This is based on a 2013 survey (CNI, 2014). Given public health care spending has increased since 1998, as discussed in Section 3.3, such numbers were likely larger before EV's introduction. The income categories reported by brackets based on minimum wage value, which was R$ 678/month (USD 300) at the time. Respondents could list up to three priorities. The other highly listed areas were "fighting crime" (31%), and "improving the quality of education" (28%). The income gradient in these responses are reversed in the case of education (31% of the richest strata list it as a priority) or much smaller in the case of fighting crime (27% of the richest strata list it as a priority).

[30]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;

are the product of bargaining between many politicians, making it difficult to assign responsibility for policy changes on specific individuals. The political relevance of health care is corroborated by other studies finding political factors affect spending on it by municipalities: Ferraz and Finan (2009) on the case of legislator wages, and Brollo and Troiano (2014) and Correa and Madeira (2014) in the case of female politicians.[31]

Beyond the overall prominence of health care issues, there are two additional reasons (related to the feasibility of the empirical strategy) to focus on it 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 a common demand of voters and within the 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. The identification strategy, described in the next section, relies on the sharp timing of EV's phase-in.

Nationally representative opinion surveys suggest that increasing the supply of public health services is a likely demand of its users. 57% of Brazilian women state that "wait times / difficulty of being serviced" is the "main problem of the public health care system." The second most listed problem was "lack of doctors" with 10% of responses. Moreover, 60% of women list "increasing the number of doctors" as one of the two main actions the government should take to improve public health services.[32] There has also been rapid growth in the number of pre-natal visits in the 1994-2006 period (described in more detail on Section 3.3). Although this may be partly due to increasing demand, its magnitude and the fact it coincides with growth in health spending suggest that the number of pre-natal visits in the public system was constrained by its supply.

Given the prominence of health care as public policy issues amongst the population, it is not surprising that a large portion of state legislature politics revolves around the issue, and that politicians are particular interested in health issue. For example, 41 out of 94 of São Paulo state legislators list "health care" as an "area of expertise" in the legislature's official webpage.[33] 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 Program (*Programa Saúde da Família*), which allocates a team of health professionals (including a

Pande, 2003).

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

[32] These figures are from a 2011 survey (CNI, 2012). Given the co-existence of the public and private care system, it is possible to "increase the number of doctors" in the public system by having them re-allocate their time from the private one.

[33] The list relates to the 2011-2015 legislature. Other common areas are education (listed by 35 legislators) and public safety (18 legislators). Note that legislators can list multiple categories.

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.[34]

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 (2009) also find that legislators can affect provision of health care quickly in the slightly different setting of Brazilian municipalities.

3.1.2 Newborn Health Status

Low birthweight (below 2,500g) is arguably the most common indicator of poor newborn health, used by numerous 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.[35] It should be noted, however, that whether the EV-induced increases in health spending will lead to adult life outcomes is beyond the scope of this paper, and birthweight which should be interpreted as noisy signal of infant health, which has other (unobservable) dimensions.

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 and avoidance of risk factors. 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.

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

[34] Ames (2001) argues that targeting government expenditures at its bases through budget amendments is the main activity of Brazilian legislators.

[35] Currie and Vogl (2013) survey this evidence from developing countries, and Almond and Currie (2011) focus on the effects of *in utero* health-related interventions on adult outcomes.

sources described in Section 2. Specifically, I use state-level valid votes and turnout data, as well as Power and Zucco's (2009) index to estimate a seat-weighted index of a state assembly's ideological position.[36]

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 Supplemental Materials. The 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.[37]

Prenatal visits and birthweight are computed separately for mothers that completed primary schooling and those that have not. Having these two separate samples allows a test of 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 uneducated mothers are 33% and 7.7%.[38] Finally, state socioeconomic variables (population, GDP, illiteracy rate, area, share of population below the poverty line,[39] and Gini index for the

[36] 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 $\sum_p (i_p * w_{pei})$.

[37] The original data only reports the share of mothers with 0, 1–6, 7+ births for the entire sample. There is also limited data on large birthweights. Additional results based on the former two categories are reported in the Supplemental Materials.

[38] 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 birthweight information, and concludes that these factors cannot account for the results.

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

income distribution) are from the Brazilian Statistical Agency (IBGE). As in Section 2, relevant sample moments are provided in the text or in the tables reporting estimate.

3.2 Estimation Framework

3.2.1 Specification

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 4 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.[40]

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. 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.,

[40]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.

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.

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.[41] 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 joint estimation of equations (6) and (7) under the restriction that $\theta^{98} = -\theta^{02} \equiv \theta$ can be carried out by pooling data from the three electoral terms and estimating:

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

[41] 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.

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 state and time effects are present, θ captures the effect of EV on y. The estimated θ is thus equal to $(\theta^{98} - \theta^{02})/2$, apart from small differences generated by the covariates X_{ie}, since $\beta_1 = \beta_2 = \beta$ is also imposed. All estimations control for a set of region-time dummies, which absorb geographically restricted shocks affecting the outcomes of interest. Unless otherwise noted, no other controls are included.[42]

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

3.2.2 Identification

The main identification assumption behind this empirical approach is that no omitted variables affecting the outcomes follows the particular pattern of electronic voting's introduction.[43] To confound the interpretation of the results as the effect of EV, 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 switch to growing slower in high S_i states with 1998–2002 period. One possible way this can occur is through a mean-reversing shock affecting high-S_i states during the 1998 electoral term. Note, however, that mean reversion in general cannot explain the result: the shock must disproportionately affect high-S_i states with a timing that matches the schedule of elections.

Direct evidence regarding the existence of such confounding omitted variable or mean-reversing shock cannot ultimately be provided, and to certain extent this paper's argument relies on the sign-switch pattern above being "so unusual that no omitted variables

[42]If covariates (X_{ie}) are not included, or if they are fully interacted with time dummies (as the region-time effects), then $\theta = (\theta^{98} - \theta^{02})/2$ holds exactly. Brazilian states are grouped into five official regions based on their natural, demographic and economic characteristics. States within a region are contiguous.

[43]Since EV's introduction is clearly a function of pre-determined voter registration across municipalities, reverse causality (e.g., health spending leading to more EV use) is not an issue.

can follow it." However, Section 3.3 specifically addresses possible threats to the identification strategy. It discusses possible sources of mean-reversing shocks and provides five sets of additional tests regarding the identification assumption.

First, it breaks down the relationship between S_i and the outcomes on a yearly basis, showing that effects occur usually in the year immediately after elections. This implies that possible confounding shocks (and their mean reversion) must follow quite specific, and sharp, timing. Second, it estimates negligible (placebo) effects on variables not expected to be affected by EV, such as as general economic conditions and birth outcomes for educated mothers, as well as spending by *municipal* governments, which were exposed to EV under different timing but should also respond to shocks to health care demand. This highlights how unusual the sign-switch pattern and addresses concerns about specific sources of mean-reversing shocks. Third, it focuses on lagged and lead outcomes and finds that the share of voters above the cutoff is orthogonal to changes in outcomes in the periods when it is not associated with EV changes in voting technology, addressing issues of pre-trends. Fourth, the effects are re-estimated with controls for possible (non-linear) time trends interacted with state characteristics, suggesting the results are not driven by another shock with heterogeneous effect across states. Finally, the Supplemental Materials report effects of electronic voting instrumented by a variable that focuses the distribution of municipalities closer to the cutoff, removing variation driven by large municipalities and addressing concerns that shocks specific to them drive the results.

3.2.3 Inference

The statistical inference aims to account for both possible serial correlation and the relatively small number of cross-sectional units (27 states). Following recommendations in both Bertrand et al (2004) and Donald and Lang (2007), the time dimension of the data is aggregated as much as possible by collapsing it to electoral term (four-year) averages. The estimation of (6) and (7) is done by taking first-differences and not the (equivalent) fixed effects regression, using a single cross-section of first-differences. In this case, the heteroskedasticity-robust standard errors are equivalent to those clustered at the state level. Donald and Lang (2007) discuss why this is an appropriate choice.

The estimation of equation (8) involves three periods and cannot be collapsed into a cross-sectional regression, and hence uses standard errors clustered at the state level. Simulations in Bertrand et al (2004) and Cameron et al (2008) indicate that clustered standard errors lead to relatively appropriate sized inference when there are 27 clusters and small number of periods. However, to further address the small cluster issue, p-

values based on Cameron et al (2008) cluster-robust wild-bootstrap are also provided in all regressions.

The relatively small number of states also raises the possibility of outliers driving some results. This issue is addressed in the Supplemental Materials by showing the robustness to leave-one-state-out estimates and also by providing a graphical representation of the empirical strategy where individual data points can be observed.

3.3 Results

The geographical distribution of the share of voters living in municipality above the 40,500-voter threshold (S_i) is depicted in Figure 5, which also shows the location of municipalities that used electronic voting in the 1998 election (except in the cases of states not following the discontinuity). The variable has a wide range, from 0.147 to 1, with a mean of 0.52 and a standard deviation of 0.26.

3.3.1 Main Results

Columns (1) and (2) in the first row of Panel A on Table 4 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 9.2 p.p. and -11.1 p.p., respectively. This indicates that states with a larger share of their population living above the cutoff experienced 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 $\theta = (\theta^{98} - \theta^{02})/2$ estimated through equation (8). Column (4) presents an estimate of $(\theta^{98} + \theta^{02})/2$, which can also be obtained through a pooled regression analogous to equation (8). This combination of parameters provides a test for $\theta^{98} = -\theta^{02}$. This null hypothesis cannot be rejected at usual levels of significance.

As previously discussed, this "sign-switch" pattern is interpreted as evidence of the effect of EV. 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 4) 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 imprecisely estimated. 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. 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.[44]

The first row of Panel B (Table 4) 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 a 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. While θ^{02} is significant at the 5% level under both the standard t-test and the bootstrapped p-values, θ^{98} is significant at the 5% level in the t-test but only marginally significant when using the bootstraps. However, pooling the data (column 3) adds substantial precision to the estimate, yielding p-values below 0.1% in both tests. This substantial gain in precision when the data is pooled is also observed for other outcomes, and highlights that the main focus of columns (1) and (2) is to check whether θ^{98} and $-\theta^{02}$ are similar. The graphical analysis in the Supplemental Materials illustrates why the pooled data yields much more precise estimates.

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. The Supplemental Materials discuss the classification of spending and indicate there are at most seven other categories available for all years of the sample. This implies that a (conservative) Bonferroni correction would involve multiplying the p-value by eight.[45] The t-statistic implied by column (3) is 4.29, which yields a p-value of 0.01% in an independent test (this number is supported by the bootstraps). Hence, the correction indicates that 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 0.8%. Furthermore, the correction gives an upper bound on the family-wise p-value, as it makes no assumption on the dependence between tests.

Note that these estimates should be interpreted as the effect gradually occurring over

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

[45] The correction is based on Boole's inequality, which implies that the probability of at least one test rejecting a true null hypothesis with probability α in a family of n tests is at most $n\alpha$ (regardless of dependence across tests).

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.[46]

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.[47] This indicates that the additional spending in health care occurs at the expense of other categories of spending. The Supplemental Materials provides estimates using outcomes in *reais* per capita (i.e., without logs) and reaches similar conclusions.

The Supplemental Materials 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 mostly 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. This highlights, given the limitations in spending data, that a more detailed welfare analysis of the policy changes induced by EV cannot be provided, and that the welfare gains from health services utilization and outcomes may be offset by losses in other (unobserved) areas.

While the yearly state-level fiscal data does not detail the nature of the additional

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

[47]The enfranchisement of less educated (poorer) voters could boost the size of government through increased direct 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 direct 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 progressive taxation, unlike income taxes (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.

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 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 of prenatal visits and birthweight.[48]

Panel C of Table 4 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 implies that a full switch from paper to EV leads to a 7 p.p. increase in the share of uneducated mothers with more than seven prenatal visits, and a 0.5 p.p. reduction in the probability of a low-weight birth. While the effect on birth weight is significant at the 5% level (using the pooled data), the effect on visits is more imprecisely estimated. It should be noted however, the addition of controls (discussed in the next section) will add precision to the estimates, and the magnitude of the point estimate is substantial, especially compared to the placebo estimate on mothers that completed primary schooling, which is also described in the next section.[49]

[48]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, iii) the share of births for which visits and birthweight data is not missing (for each education group), and iv) the share of male births. 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.

[49]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

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. Another way to summarize the size of effects is to calculate their value as a share of sample averages (provided on Table 4). 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 19%, and low-weight births by 6.8%. 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, such as assuming that EV did not induce other (unobserved, given data constraints) policy changes affecting prenatal care and newborn health.

3.3.2 Assessing the Identification Strategy

As discussed in Section 3.2, to confound the interpretation of the results as the effect of EV, 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 in the 1998–2002 period. This can occur through a mean-reversing shock affecting high-S_i states during the 1998 electoral term. Note, however, that mean reversion in general cannot explain the result: the shock must affect a specific type of states with a timing that matches the schedule of elections. To further exploit the timing issue, the Supplemental Materials breaks down the relationship between S_i and the outcomes on a year-by-year basis, showing that effects occur usually in the year immediately after elections. Hence, an omitted shock would need to occur primarily in 1999 and start its reversion only in 2003 to drive the results.

The existence of such confounding omitted variable or mean-reversing shock following such timing is unlikely *a priori*. The 1998 election and its aftermath was not characterized by substantial changes in policymaking (especially of the temporary kind that could follow the pattern described above). At the federal level it was characterized by the landslide re-election of President Fernando Henrique Cardoso, the maintenance of his coalition's control of congress, and a continuation of most of his policies.[50]

The discussion above notwithstanding, this section addresses the possibility of omitted variables or shocks affecting the results through four sets of additional tests. First, I

at the intensive margin: there is no effect on the share with 0 visits, and a negative one on the 1–6 visit category.

[50] Luis Inácio Lula da Silva, from the Worker's Party, was elected president in 2002, with a platform based on the continuation of existent policies, perhaps with larger focus on combating poverty.

estimate negligible "placebo effects" on variables that are not expected to be affected by EV, such as general economic conditions, health spending by municipal governments, and health outcomes from educated mothers. This not only highlights how unusual the sign-switch pattern is (as other variables do not follow it), but also indicates that if an omitted variable or mean-reversing shock is driving the results, it is also not affecting these variables, making it a less likely concern.

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.

Thirdly, the effects are re-estimated with additional controls, which include possible (non-linear) time trends interacted with state characteristics (i.e., allowing for a sign-switch pattern across other state characteristics). This address concerns of shocks with heterogeneous effects across states (and this heterogeneity being correlated with S_i) driving the results.

Fourth, the Supplemental Materials report effects of electronic voting instrumented by a variable that focuses on the distribution of municipalities closer to the cutoff, removing variation driven by municipalities far from the cutoff, such as large cities. This suggests that the results are driven by the share of population using electronic voting, and not overall population distribution or presence of metropolitan areas.

Panel A of Table 5 presents the first set of (placebo) tests. It follows the same pattern as Table 4, 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.[51] 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 phase-in.

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

[51]The variable is constructed from municipal budgets from the same source used for state spending, and aggregating it to the state level. Municipalities also make sizable spending in health care (of a magnitude roughly two-thirds of state spending), as discussed in Section 3.1.

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 5) 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, the effect of EV being zero cannot be rejected (column 3). 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 suggests possible determinants of health care spending are unlikely to follow it.

To illustrate the relevance of these results, take a possible source of a omitted shocks: the 1999 currency devaluation. To the extent this sizeable macroeconomic effect affected states with high-S_i disproportionately, it could confound the results. If that was the case, one would expected Table 5 to show a sign-switch in general economic conditions. Moreover, if this shock affected state budgets and lead to more health spending, it did so for a reason not affecting municipal budgets in the same manner. Taking these factors into account make it less likely that such macroeconomic shock is affecting the results.

Panel B of Table 5 repeats the exercise from Panel C of Table 4, 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 less than half of the effect for uneducated mothers, and the effect for low-weight births has a different sign (and almost a third of 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 that not only would a confounding 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 second set of placebo tests 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 5 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.[52]

The third set of additional tests involving adding controls to the estimation of equation (8), which is reported without controls on column (3) of Table 4. It focuses on the four main outcomes of the paper (valid votes, share of spending in health, pre-natal visits, and prevalence of low birth-weight). Column (1) adds additional controls (population, GDP, the poverty rate, and the Gini index of income distribution). The point estimates are almost unchanged and become more precise in some cases (being significant at the 5% level in all of them).

More relevant to the issue of omitted variables and shocks, columns (2)-(9) add to the specification a different measure of state heterogeneity, fully interacted with time dummies. This captures any (non-linear) trend, including a "sign-switch", correlated with such variable. For example, if GDP is correlated with S_i, and some temporary shock to richer states is what is actually driving the previous results, then one would expect the results in column (2) to differ substantially from those in column (1). The variables used are (the 1995-1998 average of) the same characteristics analyzed in Table 4 (and added as controls in column 1), as well as the illiteracy rate (measured in the 1991 Census), state area in square kilometers, and number of municipalities per capita, since these are related with distribution of population. Column (9) includes both population and area interacted with time dummies, hence flexibly controlling for different trends by population density. Table 6 also indicates the correlation between the variable interacted with time dummies and S_i.

The general picture from Table 6 is that the results are robust to the inclusion of such trends, the point estimates for all four outcomes are fairly stable. In some cases the precision of some outcomes is affected, especially when the variable interacted with time dummies is strongly correlated with S_i (likely due to issues of multicollinearity).[53] This indicates that even controlling for the possibility of mean-reversing shocks correlated with multiple dimensions of state heterogeneity, the estimated effect of EV remains. This further suggests that such shocks are not confounding the main results.

The fourth set of additional tests is discussed in the Supplemental Materials. It involves estimating equation (8) instrumenting S_i with the ratio between the number of

[52]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.

[53]Even the case that affects the results the most - interaction of trends by state poverty rate - seem to keep most point estimates unaffected and making some less precise (e.g., the effects on visits larger and on birthweight smaller), but the general overall picture of results being similar.

voters in municipalities with electorate in the $[40,500, 40,500+j]$ interval and the number of voters in $[40,500-j, 40,500+j]$ interval, for multiple values of j. This instrument thus focuses differences across states in the distribution of municipality size around the cutoff, and excludes the variation driven by the presence of very large cities. In general, the effects are robust to the use of this instrument. This suggests that shocks affecting states with a high share of their population in large cities are unlikely to drive the results.

Column (10) of Table 8 excludes from the sample the four states that did not participate in the discontinuous assignment in 1998 (and used EV all municipalities). 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 a robustness check. Similar results are found, with point estimates being only marginally affected, and some loss of precision due to the loss of 15% of the sample. The exception is the prenatal visits estimates, which become smaller, although they are imprecise in the full sample too. The robustness to possible outliers is further addressed in the Supplemental Materials by providing leave-one-state-out estimates and also a graphical representation of the empirical strategy where individual data points can be observed. Column (11) adds state-specific trends. All effects remain similar, except for the case of visits that becomes larger.

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 (mainly less educated) voters. Consistent with the predictions of theories of redistributive politics and enfranchisement and democratization, it also increased government spending in a service that particularly benefits less educated voters: health care. It also had effects on its utilization and outcomes, increasing the number of prenatal visits and reducing the number of low-weight births by less educated mothers.

Finally, it should be noted that the estimated effects may be dependent on the particular context. The degree of literacy required to operate the Brazilian paper ballots is higher than in contexts with fewer candidates, where their name and picture can be listed. The other results may also depend on features of the Brazilian political and health care systems. However, this study exemplifies how increasing the political participation of disadvantaged groups can shift policymaking and affect outcomes, and, most importantly, provides evidence supporting the general mechanisms in redistributive politics.

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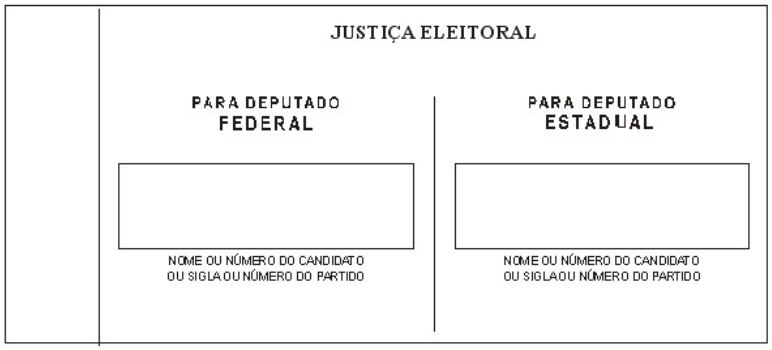
Figure 1: Examples of the Voting Technologies

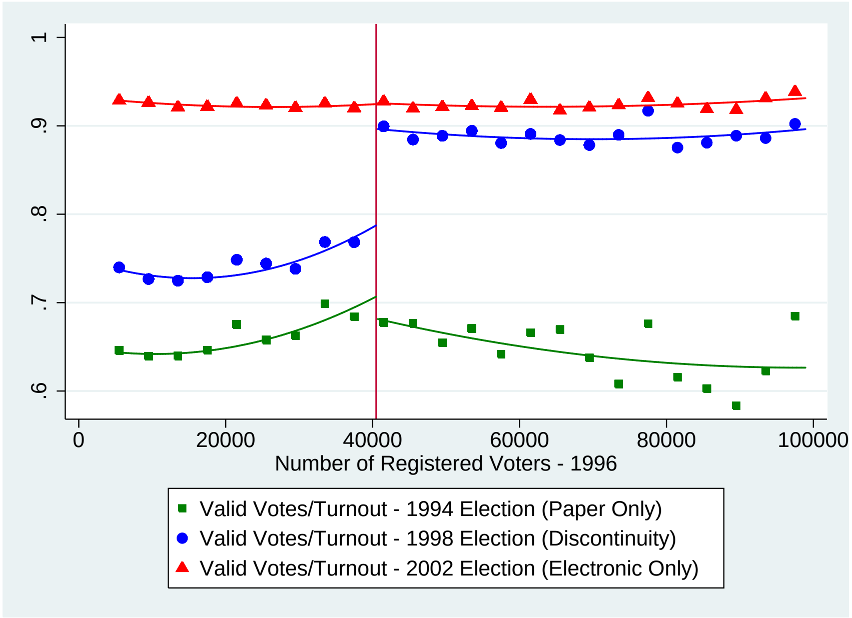
Paper Ballot:



Initial screen of the voting technology:

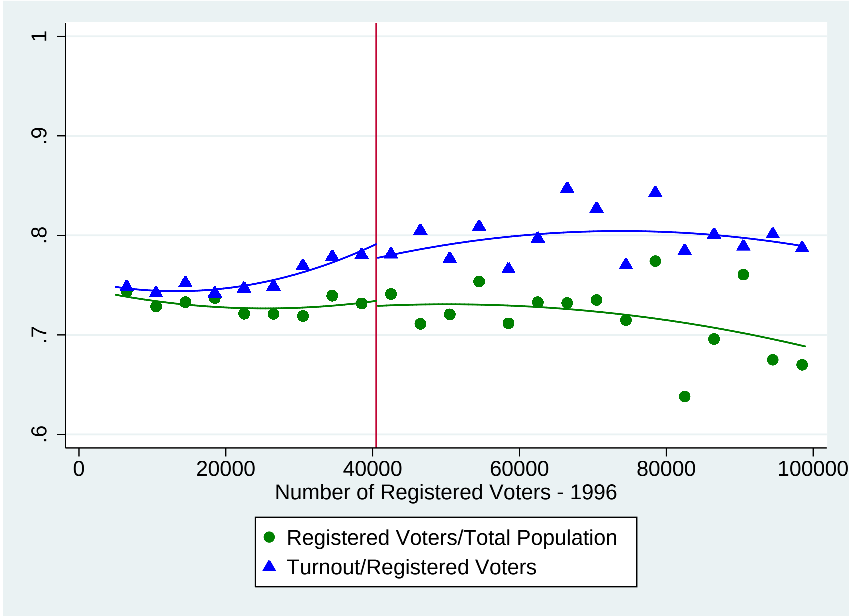
Voting for (fictional) candidate number 92111 (name: Monteiro Lobato, party: PLT)):

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

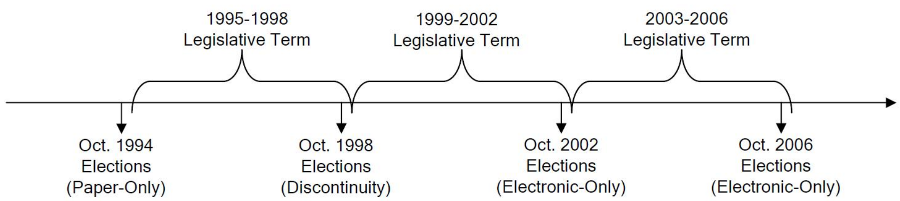
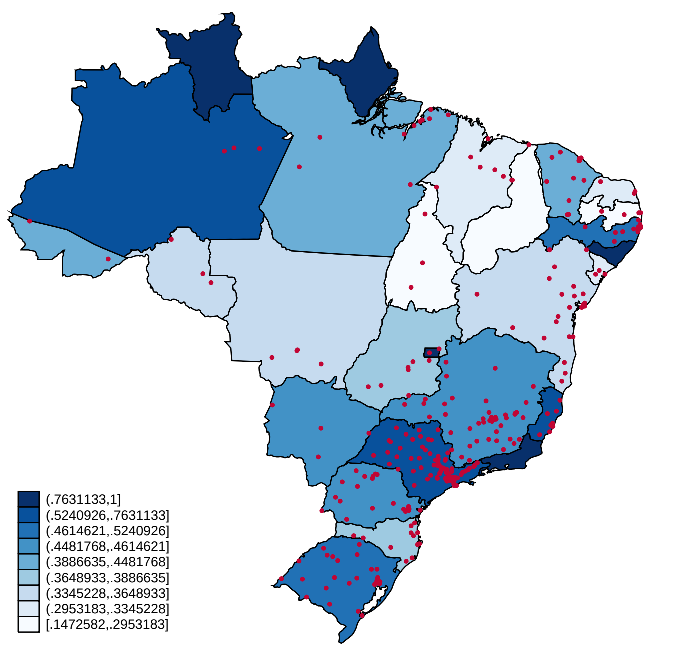


Figure 5: Share of Electorate Using Electronic Voting: 1998 Election



Markers represent the location of the centroid of municipalities using electronic voting in the 1998 election (except in the four states not following the discontinuous rule).

Table 1: Summary Statistics and Covariate Smoothness (1991 Census)

	Full Sample Mean [std. dev.]	Pre-Treat. Mean	IKBW {obs}	(1)	(2)	(3)
Monthly Income	123.13	174.83	20,000	0.908	6.096	14.017
(1991 *reais*)	[73.10]	(8.102)	{558}	(16.292)	(22.097)	(32.863)
Gini Index	0.559	0.575	15,596	0.005	0.002	-0.005
(Income)	[0.058]	(0.007)	{377}	(0.010)	(0.013)	(0.017)
Latitude	-16.53	-16.40	16,547	0.174	0.361	-0.674
(degrees)	[8.23]	(1.078)	{412}	(1.69)	(2.070)	(2.998)
Longitude	46.36	45.18	14,531	0.419	0.550	2.685
(degrees)	[6.319]	(0.850)	{345}	(1.421)	(1.636)	(2.466)
Illiteracy Rate	0.360	0.274	16,068	-0.012	-0.076	-0.041
	[0.183]	(0.020)	{389}	(0.020)	(0.046)	(0.065)
Share w/o 4 years	0.607	0.483	15,415	0.006	-0.026	-0.041
of Schooling	[0.179]	(0.203)	{372}	(0.349)	(0.041)	(0.065)
Share w/o 8 years	0.876	0.788	20,000	-0.009	-0.017	-0.030
of Schooling	[0.077]	(0.008)	{558}	(0.015)	(0.020)	(0.032)
Population - 1991	24.80	58.35	20,000	0.653	1.066	0.962
(thousands)	[153.69]	(0.583)	{558}	(1.456)	(1.716)	(1.880)
Population - 2000	28.73	69.79	17668	1.619	2.639	7.059
(thousands)	[170.91]	(1.257)	{454}	(3.043)	(3.937)	(5.011)
Share of Urban	0.507	0.237	20,000	0.004	-0.015	-0.069
Population	[0.258]	(0.021)	{598}	(0.034)	(0.048)	(0.073)
Bandwidth:	-	-	-	IKBW	10,000	5,000
Observations:	5,281	-	-	-	229	116

Robust standard errors in parenthesis, standard deviations in brackets, number of observations in curly brackets - {}. The unit of observation is a municipality. Each figure in columns (1)-(3) is from a separate local linear regression estimate with the specified bandwidth. 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). The IKBW column provides the Imbens and Kalyanaraman (2012) optimal bandwidth (capped at 20,000) and the associated number of observations. Details on the dependent variables in the text.

Table 2: Treatment Effects of Electronic Voting

	Full Sample Mean	Pre-Treat. Mean	IKBW {obs}	(1)	(2)	(3)
Panel A: Baseline Results						
Valid Votes/Turnout	0.755	0.780	11,871	0.118	0.121	0.124
(1998 Election)	[0.087]	(0.013)	{265}	(0.015)	(0.016)	(0.025)
Turnout/Reg. Voters	0.765	0.786	13,082	-0.006	0.013	0.007
(1998 Election)	[0.091]	(0.011)	{299}	(0.018)	(0.021)	(0.033)
Reg. Voters/Population	0.748	0.737	15,956	-0.004	0.010	0.032
(1998 Election)	[0.141]	(0.010)	{388}	(0.027)	(0.034)	(0.044)
Panel B: Placebo Tests (Election Years Without Discontinuous Assignment)						
Valid Votes/Turnout	0.653	0.697	17,111	-0.013	-0.008	0.006
(1994 Election)	[0.099]	(0.011)	{433}	(0.019)	(0.023)	(0.032)
Valid Votes/Turnout	0.928	0.921	17,204	0.005	0.008	0.009
(2002 Election)	[0.026]	(0.002)	{437}	(0.005)	(0.006)	(0.010)
Panel C: Do Left-wing Parties Benefit Disproportionately from Electronic Voting?						
Vote-weighted Party	5.397	5.162	20,000	-0.222	-0.250	–0.108
Ideology (1998 Elec.)	[0.691]	(0.094)	{558}	(0.100)	(0.081)	(0.170)
Bandwidth:				IKBW	10,000	5,000
Specification:				Linear	Linear	Linear
N	5,281			-	229	116

Robust standard errors in parenthesis, standard deviations in brackets, number of observations in curly brackets - {}. The unit of observation is a municipality. Each figure in columns (1)-(3) is from a separate local linear regression estimate with the specified bandwidth. 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). The IKBW column provides the Imbens and Kalyanaraman (2012) optimal bandwidth (capped at 20,000) and the associated number of observations. Details on the dependent variables in the text.

Table 3: Treatment Effects of Electronic Voting, by Illiteracy Rate

	Pre-Treat Mean	IKBW {obs}	(1)	(2)	(3)	(4)
Panel A: Municipalities with Above-Median Illiteracy						
Valid Votes/Turnout	0.759	11,871	0.147	0.150	0.152	0.176
	(0.017)		(0.019)	(0.015)	(0.020)	(0.031)
N	-	-	116	279	103	49
Panel B: Municipalities with Below-Median Illiteracy						
Valid Votes/Turnout	0.799	11,871	0.092	0.113	0.096	0.089
	(0.018)		(0.020)	(0.016)	(0.022)	(0.032)
N	-	-	149	279	126	67
Test of Equality in TEs (p-value)	-	-	0.049	0.089	0.056	0.053
Bandwidth:	-	-	IKBW	20,000	10,000	5,000

Robust standard errors in parenthesis, standard deviations in brackets, number of observations in curly brackets - {}. The unit of observation is a municipality. Each figure in columns (1)-(4) is from a separate local linear regression estimate with the specified bandwidth. 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). The IKBW column provides the Imbens and Kalyanaraman (2012) optimal bandwidth. Details on the dependent variables in the text. Estimates on Panel A (Panel B) uses only municipalities where the adult illiteracy rate is above (below) 25.43%.

Table 4: Main Outcomes and the Sign-Switch Pattern

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Panel A: Electoral Outcomes					
Valid Votes/Turnout	0.829	0.092	-0.111	0.102	-0.009
	[0.112]	(0.033)	(0.010)	(0.017)	(0.018)
		{0.102}	{0.002}	{0.008}	{0.630}
Seat-Weighted Policy Position	4.623	-0.112	0.299	-0.206	0.094
	[0.601]	(0.641)	(0.167)	(0.350)	(0.302)
		{0.842}	{0.154}	{0.574}	{0.800}
Panel B: Fiscal Outcomes (Health Care Spending)					
log(Total Spending)	-	-0.004	-0.257	0.127	-0.131
		(0.093)	(0.156)	(0.097)	(0.082)
		{0.946}	{0.274}	{0.254}	{0.228}
Share of Spending in Health Care	0.099	0.039	-0.029	0.034	0.005
	[0.037]	(0.017)	(0.013)	(0.008)	(0.013)
		{0.104}	{0.044}	{0.000}	{0.678}
log(Health Spending p.c.)	-	0.428	-0.677	0.552	-0.125
		(0.264)	(0.262)	(0.096)	(0.242)
		{0.200}	{0.034}	{0.000}	{0.628}
Panel C: Birth Outcomes (Mothers Without Primary Schooling)					
Share with 7+ Visits	0.362	0.122	-0.023	0.069	0.047
	[0.123]	(0.065)	(0.033)	(0.040)	(0.039)
		{0.154}	{0.558}	{0.182}	{0.320}
Share with Low-weight Births (×100)	7.721	-0.370	0.528	-0.529	0.201
	[1.110]	(0.304)	(0.269)	(0.246)	(0.236)
		{0.266}	{0.104}	{0.044}	{0.450}
N (state-terms)	-	54	54	-	-
N (states / first-diffs)	-	27	27	-	-

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 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: Placebo Tests

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98} - \theta^{02})/2$	$(\theta^{98} + \theta^{02})/2$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Panel A: Effects on Covariates					
Share of Spending	0.096	-0.011	-0.016	0.003	-0.013
in Health Care -	[0.048]	(0.021)	(0.014)	(0.015)	(0.009)
Municipalities		{0.714}	{0.296}	{0.932}	{0.244}
log(Population)	-	0.064	0.059	0.002	0.062
		(0.032)	(0.026)	(0.015)	(0.025)
		{0.158}	{0.088}	{0.886}	{0.088}
log(GDP)	-	0.007	0.096	-0.051	0.044
		(0.053)	(0.202)	(0.088)	(0.117)
		{0.920}	{0.626}	{0.630}	{0.680}
Poverty Rate	0.397	0.080	0.105	-0.012	0.093
	[0.164]	(0.045)	(0.024)	(0.019)	(0.031)
		{0.144}	{0.014}	{0.676}	{0.032}
Gini Index	0.569	0.034	0.042	-0.004	0.038
	[0.034]	(0.031)	(0.009)	(0.014)	(0.017)
		{0.330}	{0.000}	{0.804}	{0.106}
Panel B: Birth Outcomes for Mothers that Completed Primary Schooling					
Share with 7+ Visits	0.569	0.062	0.009	0.031	0.039
	[0.127]	(0.036)	(0.022)	(0.019)	(0.026)
		{0.152}	{0.742}	{0.134}	{0.182}
Share with Low-weight	6.261	0.391	-0.023	0.196	0.178
Births (×100)	[1.581]	(0.474)	(0.550)	(0.502)	(0.130)
		{0.398}	{0.900}	{0.626}	{0.208}
Panel C: Pre- and Post-trend Analysis: "Effect" in Periods Without Change in Voting Technology					
Parameter:		θ^{94}	θ^{06}	$(\theta^{94} - \theta^{06})/2$	$(\theta^{94} + \theta^{06})/2$
Sample (terms):		1990-1994 (Paper-Paper)	2002-2006 (Electr.-Electr.)		
Share of Spending	-	-0.006	-0.005	-0.0003	-0.006
in Health Care		(0.026)	(0.013)	(0.014)	(0.014)
		{0.884}	{0.756}	-	-
N (state-terms)		54	54	-	-
N (states/first-diffs)		27	27	-	-

See Table 4 notes.

Table 6: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Valid Votes and Health Care Spending											
Valid Votes/Turnout	0.103	0.103	0.111	0.102	0.111	0.109	0.102	0.099	0.105	0.123	0.102
	(0.016)	(0.016)	(0.013)	(0.020)	(0.017)	(0.015)	(0.017)	(0.022)	(0.014)	(0.014)	(0.017)
	{0.000}	{0.004}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}	{0.048}	{0.000}	{0.000}	{0.008}
Share of Spending in Health Care	0.031	0.033	0.037	0.045	0.030	0.031	0.030	0.039	0.030	0.028	0.034
	(0.011)	(0.009)	(0.007)	(0.006)	(0.012)	(0.009)	(0.010)	(0.009)	(0.010)	{0.013}	(0.008)
	{0.026}	{0.000}	{0.000}	{0.000}	{0.064}	{0.008}	{0.060}	{0.000}	{0.062}	{0.004}	{0.000}
Panel B: Birth Outcomes (Mothers Without Primary Schooling)											
Share with 7+ Prenatal Visits	0.069	0.068	0.089	0.069	0.095	0.065	0.060	0.095	0.063	0.033	0.078
	(0.017)	(0.040)	(0.032)	(0.039)	(0.037)	(0.037)	(0.036)	(0.048)	(0.033)	(0.042)	(0.043)
	{0.000}	{0.172}	{0.000}	{0.098}	{0.000}	{0.128}	{0.160}	{0.054}	{0.104}	{0.490}	{0.182}
Share with Low-Weight Births (x100)	-0.546	-0.545	-0.489	-0.401	-0.263	-0.495	-0.394	-0.605	-0.406	-0.504	-0.471
	(0.210)	(0.251)	(0.238)	(0.226)	(0.208)	(0.245)	(0.238)	(0.338)	(0.236)	(0.479)	(0.236)
	{0.004}	{0.040}	{0.052}	{0.062}	{0.174}	{0.070}	{0.126}	{0.108}	{0.112}	{0.332}	{0.068}
Controls:											
GDP, Gini, Poverty, Pop.	Yes	-	-	-	-	-	-	-	-	-	-
Time-dummies interacted w/	-	GDP	Gini	Illiteracy	Poverty	Pop.	Area	#Munic.	Pop.	-	-
[Correlation of var. w/ S_i]		[0.049]	[-0.159]	[-0.402]	[-0.334]	[-0.129]	[-0.196]	[-0.616]	& Area		
Restricted Sample	-	-	-	-	-	-	-	-	-	Yes	-
State-specific trends	-	-	-	-	-	-	-	-	-	-	Yes
N	81	81	81	81	81	81	81	81	81	69	81

Standard errors clustered at the state level in parenthesis. 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 a dummy indicator for the 1998 electoral term (1999-2002). State fixed effects, time effects and region-time effects are included in all regressions. The restricted sample excludes states that did not follow the discontinuous assignment rule.

Supplemental Materials (Not for Publication)

A Description of Voting Technologies

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 1 illustrates. Panel B 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 C. 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 Figure A1. 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 (Figure A1). 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, Figure 1 depicts the machine requesting 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).

In principle, another possible explanation for the reduction in residual votes is due to voting technology affecting electoral fraud at the vote-counting stage. However, this is unlikely to be the case. There is virtually no evidence of electoral fraud related to the electronic system, which is perceived as honest by 98% of Brazilians and usually presented as a international case study of trustworthy government technology.[54] While there is evidence of vote-tallying 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. 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.

B 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

[54]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.

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.[55]

Panel A of Table A1 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 2 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 possible reaction for someone that is challenged by the task is to leave it blank).

Panel B of Table A1 reports the estimated effects of EV on the ratio between valid votes and turnout. The first line reports the same estimates provided on Table 2 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.

Panel B indicates 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 A1 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

[55]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.

are particularly better prepared to cast their votes for president, even when using the more error-prone paper ballot.

C Robustness Checks on RDD Estimates

This section provides further results regarding the validity of the RDD estimates of the effects of EV presented on Section 2. Firstly, 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). Secondly, it briefly discusses the presence of other discontinuous assignment rules across Brazilian municipalities, and why they are unlikely to affect the results.

C.1 Manipulation of the Forcing Variable and Threshold Position

To test the possibility of strategic manipulation, Figure A2 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 A2, 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 it is 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. First, 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). Second, I test if the relationship between these predicted values and the forcing variable (number of voters) is smooth around the threshold. Figure A3 provides the graphical evidence on this second step: the predicted value evolves continuously around the threshold.

The evidence in Figure A3 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 A3 shows that this is the case.[56]

C.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.[57]

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.[58] The exception is a multi-threshold rule regarding the distribution of federal funds to municipal governments. Since

[56]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.

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

[58]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 (2009) exploit the RDD regarding legislator wages.

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.[59]

D Effects on Party Seat Shares

Table A2 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. Table A2 follows the same pattern used for Table 4. 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 traditionally left-wing parties.

E Effects on Levels of Spending

Table A3 repeats the estimation provided on Table 4 using levels of spending (instead of budget shares or logarithms). The unit of measurement is a 2000 *real* per capita. It provides estimates for health care spending, which largely corroborate those on Table 4 (although standard errors are somewhat larger). It also provides results on total spending (including transfers and interest payments and debt amortization) and total revenue (including taxes and transfers). Since yearly data on debt positions is not available for the whole period, Table A3 provide estimates on net interest payments to address this issue. The sign-switch pattern is not observed in total spending and revenue, and the estimated coefficients are not significant, although their magnitude is large compared to health spending.

F 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 spend-

[59]Litschig and Morrison (2013), and Brollo et al. (2013) exploit this discontinuity to estimate effects of government grants.

ing 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.[60] Additionally, a residual category name "other spending" also has to be 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"). Interest payments and debt amortization are fall in the "other spending" category, which likely explains its sizable budget share. The categories used here match well with another attempt to build comparable state spending by category.[61] While the results on share of budget spent on health care are provided on Table 4, Table A4 provides the results for the other categories of spending, using the same pattern of Table 4. 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 assistance" may be inflated by including government employees' pensions.

G 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 C of Table 4 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.

[60] 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.

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

H Assessing the Role of Outliers I: Graphical Analysis of State-Level Results

Figures A4 and A5 present a graphical representation of the main results provided on Table 4. 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 4.

The top left graph of Figure A4 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 4. 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 A4 repeats the exercise for the change in health care spending. Figure A5 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.

I Assessing Outliers II: Leave-One-State-Out Estimates

To assess the role of outliers in driving the results on, Figures A6 depicts the estimated effect of EV on the valid votes to turnout ratio, sequentially dropping each state of the sample. Each marker represents the estimated θ from equation (8), dropping the designated state (the horizontal axis uses states' official acronyms). The bars represent 95% confidence intervals based on Cameron et al (2008) cluster-robust wild-bootstrap. The vertical line represents the effect using the whole sample (reported on Table 4).

Figures A7, A8, and A9 repeat the exercise for health care spending, prenatal visits, and low birthweight prevalence. The results are, in general, robust to dropping individual observations, the only case where there is some sensitivity in the results to some states is the number of prenatal visits: dropping Amapá reduces the estimated effect (although the original estimate is still in its confidence interval), while dropping Roraima increases the effect. Both states are quite similar, being small, relatively unpopulated, and situated in the Amazon ecosystem. Most importantly, both did not follow the discontinuous rule (and hence are dropped from the estimation in column 10 of Table 6): given the main text already discuss dropping these states and highlights that the effects on visits are noisier than on other outcomes, the main conclusions in the main text remain.

J Timing of Effects

To maintain the statistical inference as conservative as possible in the presence of a relatively small number of states, the effects reported on the text aggregate yearly data into electoral-term aggregates. In order to assess more precisely the dynamics of the effects, I estimate the following regression using yearly data for state i at year t:

$$y_{it} = \alpha_t + \sum_{k=1996}^{2006} \pi_k S_i 1\{t = k\} + \gamma_t + \beta X_{it} + \epsilon_{ie} \quad (9)$$

The set of π_k hence maps the relationship between voters above the cutoff (S_i) and the outcome of interest y_{it} over time. This equation is a generalization of equation (8) for yearly data: it also includes state and time effects, and uses only region-time effects as controls (X_{it}). The estimation uses the 1995-2006 years like equation (8), with π_{1995} omitted to avoid collinearity (recall the 1995-1998 legislature is elected in 1994, etc). If equation (9) is aggregated to electoral term (four-year) level data, the differences in π_k map into θ^{98} and θ^{02}.

The results for share of health care spending are provided on Figure A10. The markers represent the estimated π_k for the designated year, the estimates a re-scaled (i.e., a constant is added) in order to make the 1995-1998 πs average equal to zero. The solid line without markers repeats the analysis using data at the bi-yearly level (e.g., 1995-1996 averages, 1997-1998 averages, etc), and the dashed lines the one using four year averages. Hence the first jump in the dashed lines represent θ^{98}, while the second one represents θ^{02} (both reported on Table 4). The vertical lines represent the 1998 and 2002 elections.

Figure A10 indicate a stability in the relationship between S_i and health spending, but that jumps immediately after the election, and drops after the 2002 election. The timing is fairly sharp: the switching occurs immediately after elections. This highlights how unlikely it is for a mean-reversing to shock or omitted variable to drive the results: it has to affect a certain type of states exactly in 1999, and switch its behavior exactly in 2003.

The solid and dashed lines essentially smooth out the effects, and highlight the degree of noise in yearly data that drives jumps across within-election years. Hence, I avoid assigning specific interpretation to, say, the jump between 2000 and 2001.

Figure A11 repeats the exercise for prenatal visits. The jump between the 1995-1998 and 1999-2002 periods are clear and stable (e.g., no pre-EV trends), while the reduction in 2003-2006 is less pronounced. This can be seen in the estimated θ^{98} and θ^{02} and discussed in the text. Figure A12 repeats the exercise for birthweight. There is a little more yearly noise in these estimates, which get smoothed out once one focus on the two-year averages, especially when comparing 1999-2002 and 2003-2006 periods.

Finally, I reiterate the noisy nature of the data: most π_k is not statistically different from the previous and subsequent π_k (but they are when aggregated to four-years an tested in a manner to simulate the results in Table 4). Hence, I avoid assigning an interpretation to jumps in specific years.

K Instrumental Variable Estimates

To assess the extent to which the results are driven by variation in S_i driven the presence of larger cities or very small municipalities, this section provide results based on the following 2SLS procedure. The second-stage regression is similar to equation (8):

$$y_{ie} = \alpha_e + \theta_{IV} S_i \cdot Term_e^{98} + \gamma_i + \beta X_{ie} + \epsilon_{ie} \tag{10}$$

with $(S_i \cdot Term_e^{98})$ instrumented according to the following first-stage:

$$S_i \cdot Term_e^{98} = \alpha_e^{1st} + \lambda Z_i^j \cdot Term_e^{98} + \gamma_i^{1st} + \beta^{1st} X_{ie} + \epsilon_{ie}^{1st} \tag{11}$$

where $Z_i^j = \frac{\text{Number of voters in municipalities with electorate between 40,500 and 40,500+}j\text{ in state }i}{\text{Number of voters in municipalities with electorate between 40,500-}j\text{ and 40,500+}j\text{ in state }i}$.

Hence, the share of voters living above the cutoff (S_i) is instrumented with the share of voters living above the cutoff in a window (of size j) around the cutoff. Note that if j is large enough, Z_i becomes S_i. Hence, the instrument focus on the shape of (within-state) distribution of municipality size around the cutoff. Intuitively, it "throws away" the variation related to presence of very large or very small municipalities. For example, if a shock to very large municipalities is driving the results, the 2SLS estimates should differ from those from the main text.

There are two important choices to be made in constructing the Z_i^j. The first is the choice of j. While intuitively very small j are desirable, they will yield less predictive instruments, and may indeed lead to biases due to weak instruments. I address this issue by estimating effects with multiple sizes of j, from 5,000 to 500,000. The second issue is how to assign an instrument to the states that did not follow the discontinuity, which are assigned $S_i = 1$ in the text.[62] One possibility, and that keeps the IV estimate closer to the analysis in the text, is to assign them a $Z_i^j = 1$. The estimates provided on Table A6 are based on this choice.

Each row of Table A6 provides the estimated λ and θ_{IV} for the four main outcomes (those reported on Table 6) and the designated j. The results are very similar to those in Table 4, regardless of j's size, except that the effect on prenatal visits is usually larger. The first-stage effect is strong for all windows, although it should be highlighted that in small j cases, that is likely driven by assigning $Z_i^j = 1$ for the four states not following the discontinuity. This is an important caveat in interpreting this result: the small j are likely driven by the variation from those states.

Another choice for dealing with states that did not follow the discontinuity is to drop them from the estimation. Table A7 repeats the analysis from Table A6 doing so. However, in the cases of smaller j, the first-stage is weak, to an extent that the weak instruments became an issue interpreting the results. Using the "rule of thumb" of a excluded instrument F-stat larger than ten to rule out sizable weak instrument bias, only the cases with j of 40,000 and higher are valid. In those, the estimates from the main text are mostly corroborated (if anything, the effects on visits and birthweight

[62]Note that the main text also uses dropping these states as a robustness check.

are larger).[63] More importantly, the cases where j is small (below 30,000) are clearly uninformative about the results.

A third possibility would be to include the four states not following the discontinuity with their actual Z_i^j (not setting it to one or dropping them). In this case, the instrument is always weak, since most of these states have small Z_i^j. Hence I avoid estimating the second stage. Finally, I highlight that the main issue here is not the presence of four states not following the rule, but the fact that when j is "too small", it becomes unpredictive of S_i. More intuitively, the correlation between the distribution of municipalities above/below the cutoff in a "small" window around it is not a strong enough predictor of the distribution above/below the cutoff in general, and hence the roll-out of EV, to allow its use as an instrument.

Additional References for Supplemental Materials

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Brollo, Fernanda, Tomasso Nannicini, Roberto Perotti and Guido Tabellini (2013). "The Political Resource Curse." *American Economic Review*, 103(5): 1759-1896.

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

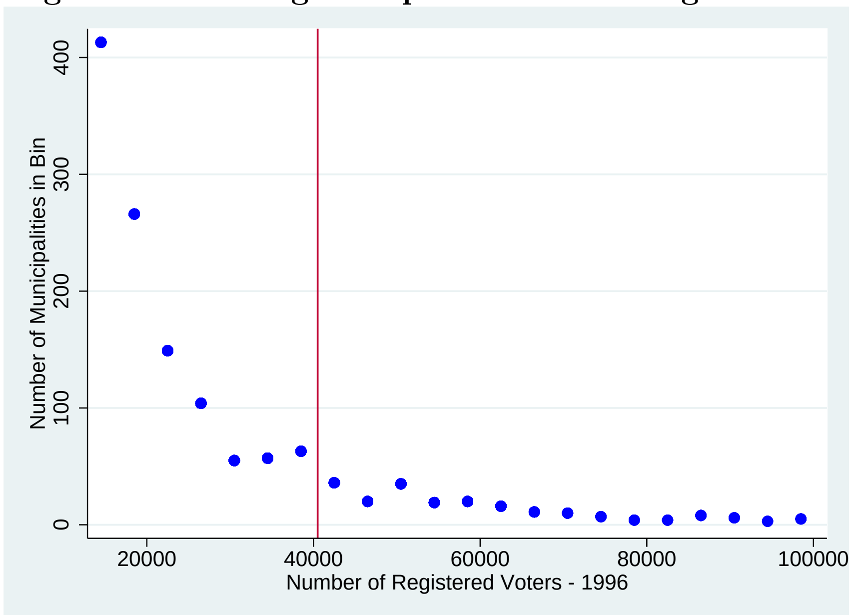
Gonçalves, Carlos E. S., Ricardo A. Madeira and Mauro Rodrigues (2008). *Two-ballot vs. Plurality Rule: An Empirical Investigation on the Number of Candidates.* Mimeo.

Litschig, Stephan and Kevin Morrison (2013). "The Impact of Intergovernmental Transfers on Education Outcomes and Poverty Reduction." *American Economic Journal: Applied Economics*, 5(4): 206-240.

[63] Note that, in the case of single instrument, the excluded instrument F-stat is the square of is t-stat, so a ratio between reported coefficient and standard error of below 3.16 would fail the rule of thumb.

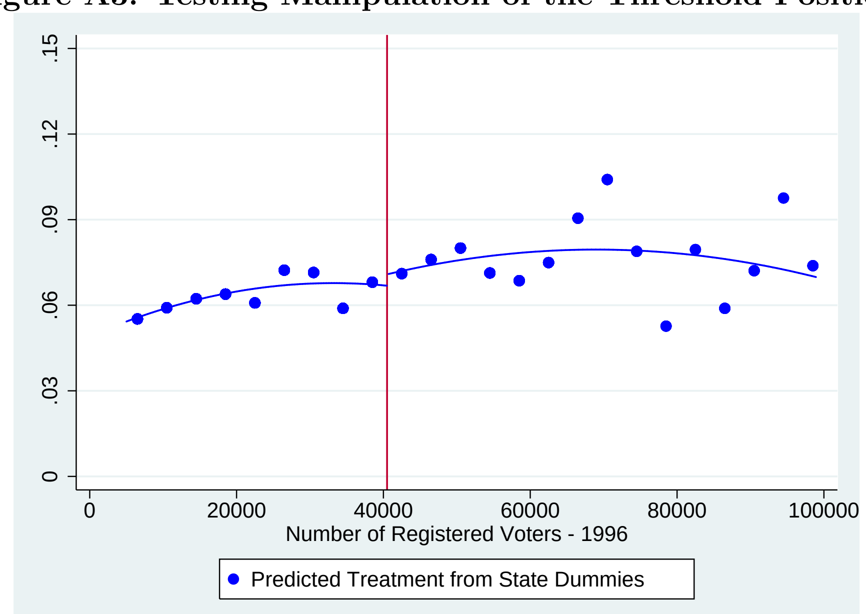
Figure A1: Residual vote under the electronic technology

Figure A2: 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 A3: 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 A4: Electronic Voting Phase-in, Valid Votes, and Health Spending

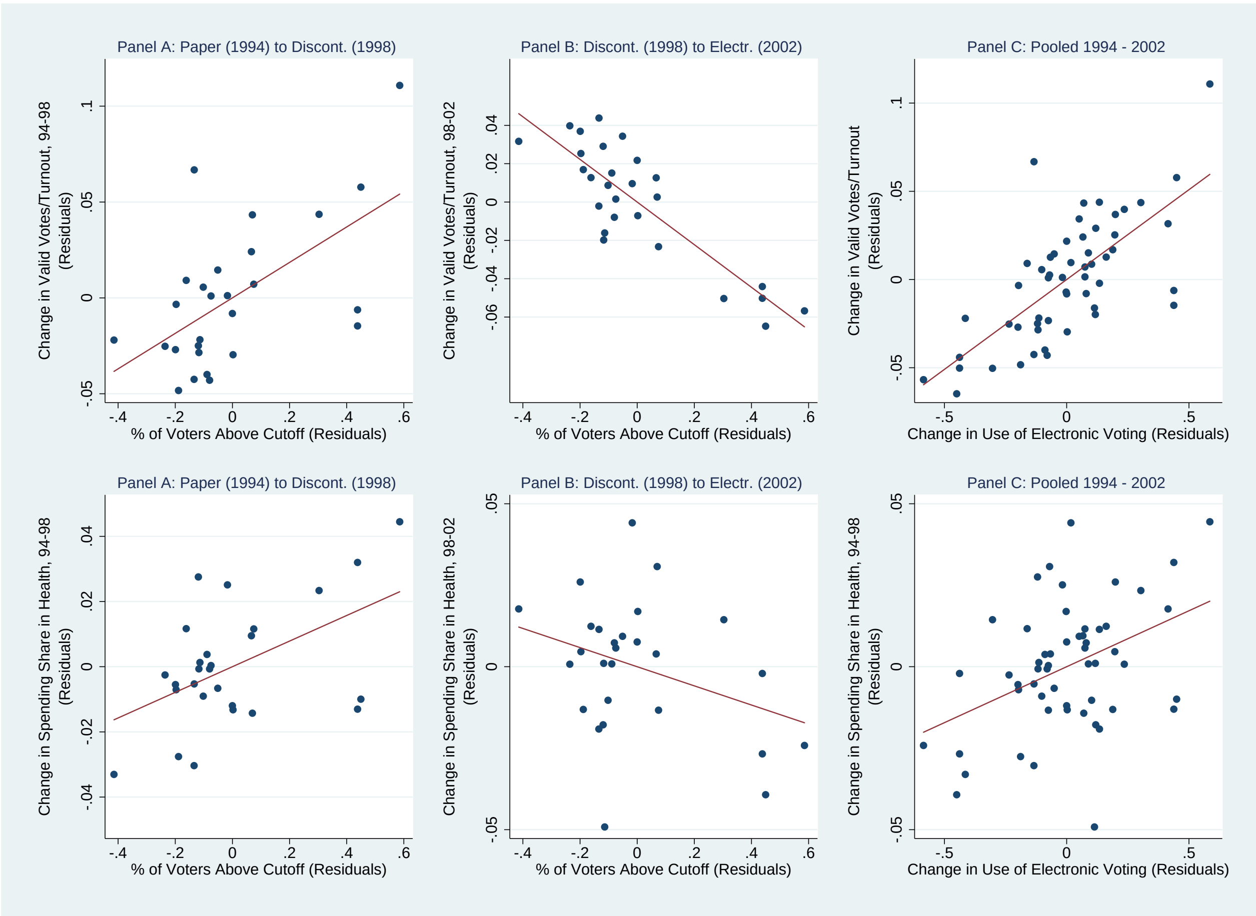


Figure A5: Electronic Voting Phase-in, Prenatal Visits, and Birthweight

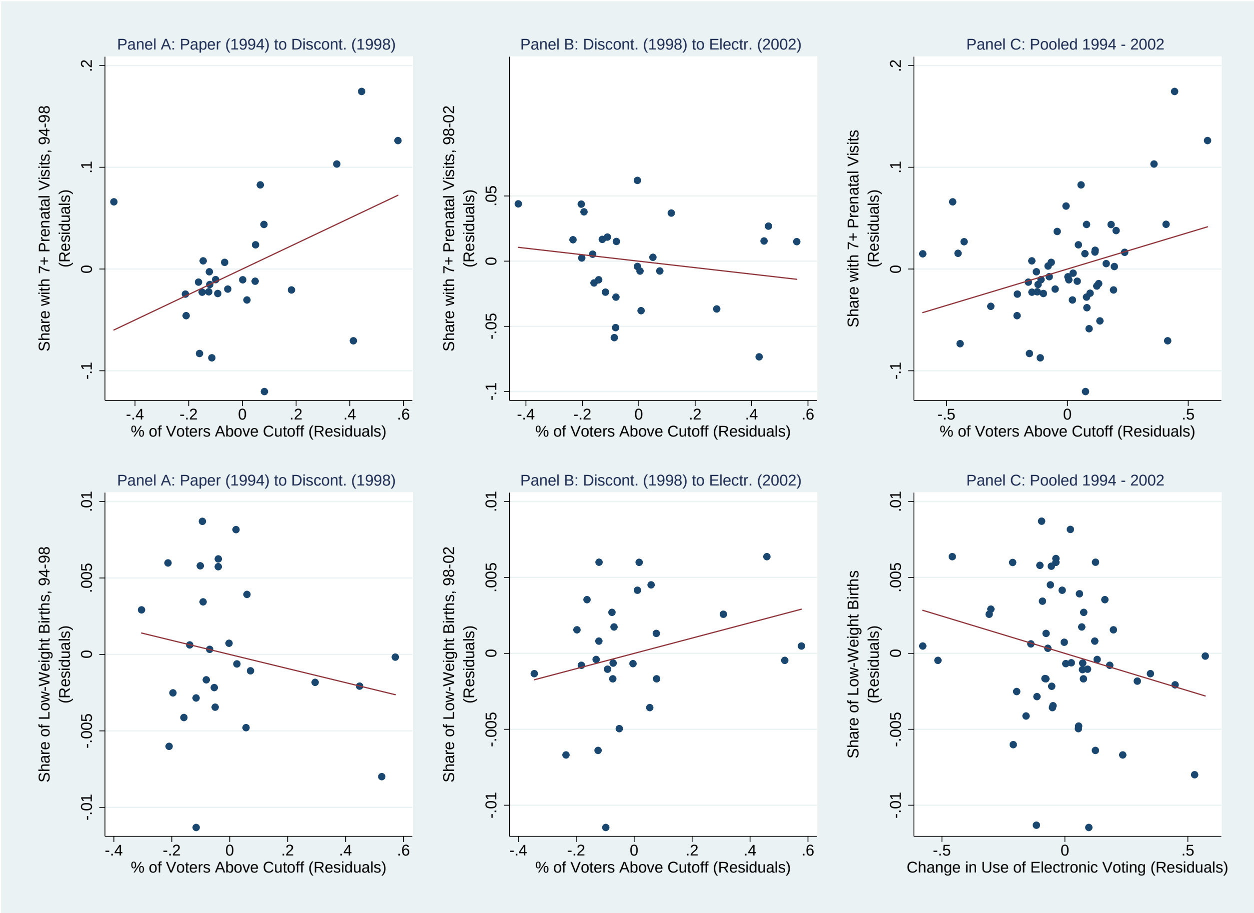
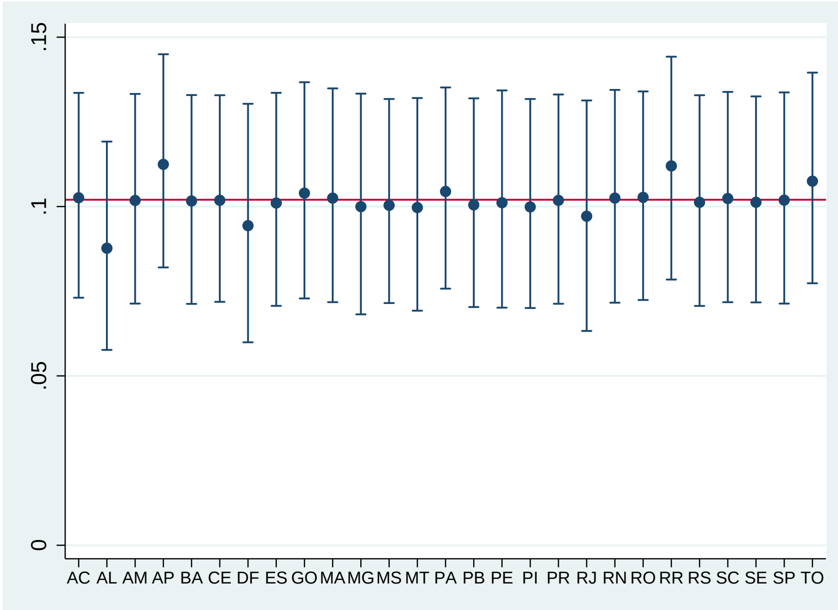
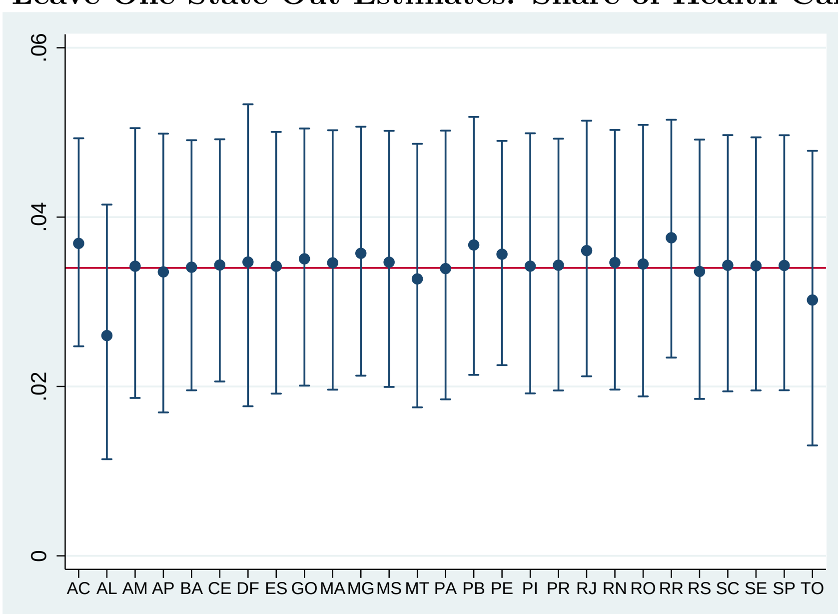


Figure A6: Leave-One-State-Out Estimates: Valid Votes/Turnout



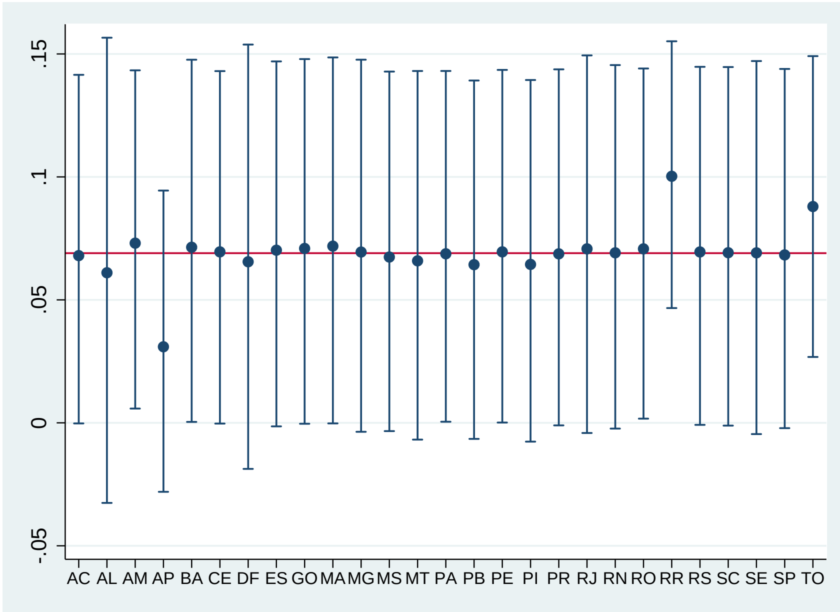
Each marker represents the estimated effect of electronic voting using the state sample (equation 8), dropping the specified state from the sample. Confidence intervals based on Cameron et al. (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

Figure A7: Leave-One-State-Out Estimates: Share of Health Care Spending



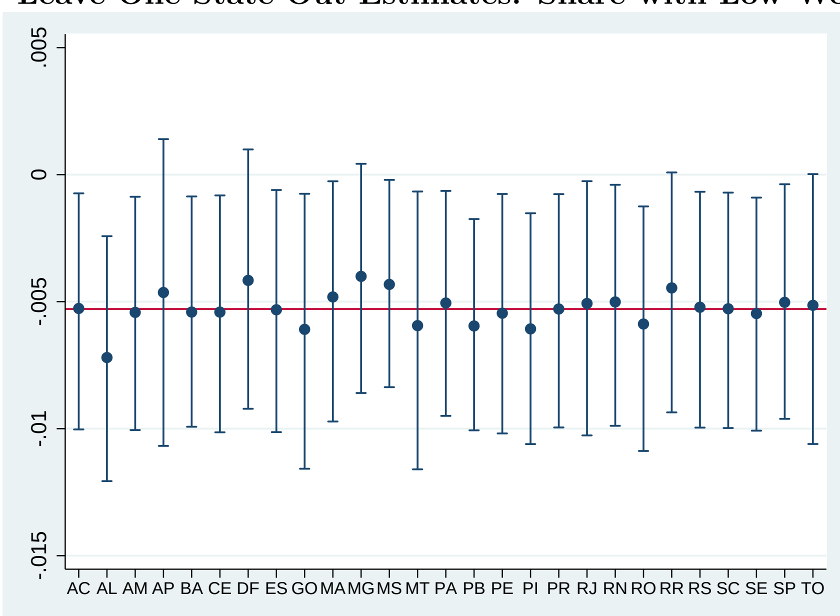
Each marker represents the estimated effect of electronic voting using the state sample (equation 8), dropping the specified state from the sample. Confidence intervals based on Cameron et al. (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

Figure A8: Leave-One-State-Out Estimates: Share with 7+ Prenatal Visits



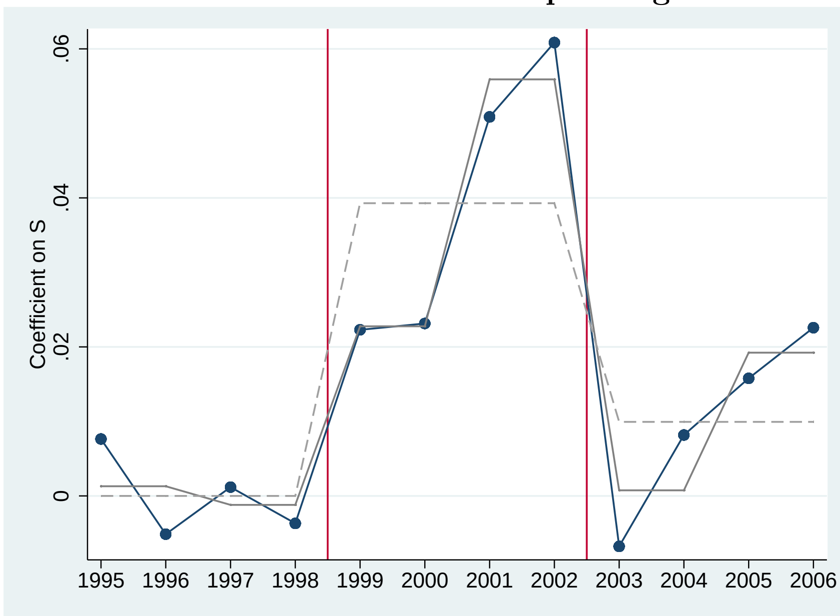
Each marker represents the estimated effect of electronic voting using the state sample (equation 8), dropping the specified state from the sample. Confidence intervals based on Cameron et al. (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

Figure A9: Leave-One-State-Out Estimates: Share with Low-Weight Births



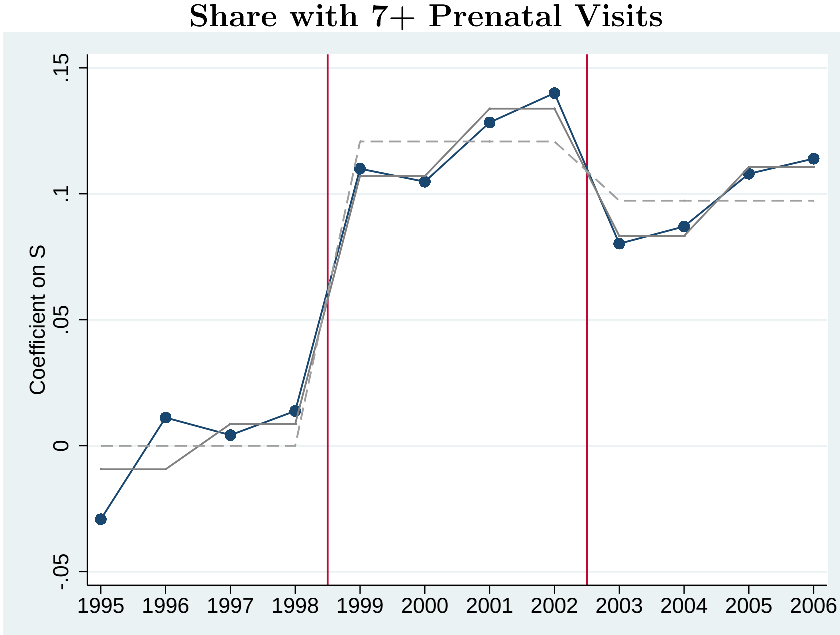
Each marker represents the estimated effect of electronic voting using the state sample (equation 8), dropping the specified state from the sample. Confidence intervals based on Cameron et al. (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

Figure A10: Coefficient of Share Above Cutoff (S), by Year: Share of Health Care Spending



Each marker represents the coefficient of the share of voters residing above cutoff (S) interacted with the specified year (equation 9). Solid (dashed) lines plot two-(four-)year averages. See text for more details.

Figure A11: Coefficient of Share Above Cutoff (S), by Year: Share with 7+ Prenatal Visits



Each marker represents the coefficient of the share of voters residing above cutoff (S) interacted with the specified year (equation 9). Solid (dashed) lines plot two-(four-)year averages. See text for more details.

Figure A12: Coefficient of Share Above Cutoff (S), by Year: Share with Low-Weight Births

Each marker represents the coefficient of the share of voters residing above cutoff (S) interacted with the specified year (equation 9). Solid (dashed) lines plot two-(four-)year averages. See text for more details.

Table A1: Additional Treatment Effects of Electronic Voting

	Full Sample Mean	Pre-Treat. Mean	IKBW {obs}	(1)	(2)	(3)
Panel A: Effects on Null and Blank Votes, State Legislature Election						
Null Votes/Turnout	0.136	0.134	12,605	-0.087	-0.086	-0.084
	[0.056]	(0.008)	{286}	(0.009)	(0.010)	(0.015)
Blank Votes/Turnout	0.109	0.093	12,659	-0.037	-0.036	-0.040
	[0.041]	(0.005)	{287}	(0.007)	(0.008)	(0.012)
Panel B: Effects on Valid Votes/Turnout, by Office						
State Legislators	0.755	0.780	11,871	0.118	0.121	0.124
	[0.087]	(0.013)	{265}	(0.015)	(0.016)	(0.025)
Federal Congress	0.704	0.689	11,546	0.212	0.210	0.205
(Lower Chamber)	[0.092]	(0.015)	{259}	(0.017)	(0.030)	(0.019)
Federal Congress	0.666	0.704	14,287	0.074	0.087	0.100
(Senate)	[0.106]	(0.012)	{342}	(0.016)	(0.020)	(0.030)
State Governor	0.721	0.749	14,795	0.069	0.078	0.082
	[0.097]	(0.011)	{352}	(0.014)	(0.017)	(0.026)
President	0.767	0.796	17,386	0.016	0.021	0.031
	[0.068]	(0.007)	{446}	(0.011)	(0.013)	(0.021)
Bandwidth:	-			IKBW	10,000	5,000
Observations:	5281			-	229	116

Robust standard errors in parenthesis, standard deviations in brackets, number of observations in curly brackets - {}. The unit of observation is a municipality. Each figure in columns (1)-(3) is from a separate local linear regression estimate with the specified bandwidth. 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). The IKBW column provides the Imbens and Kalyanaraman (2012) optimal bandwidth (capped at 20,000) and the associated number of observations. Details on the dependent variables in the text.

Table A2: Effect on Party Seat Shares

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
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	0.008
	[0.016]	(0.003)	(0.013)	(0.007)	(0.007)
		{0.564}	{0.232}	{0.170}	{0.340}
PDT	0.066	0.089	-0.066	0.077	0.011
	[0.054]	(0.049)	(0.041)	(0.042)	(0.018)
		{0.064}	{0.300}	{0.084}	{0.528}
PFL	0.150	-0.023	-0.025	0.001	-0.024
	[0.106]	(0.086)	(0.031)	(0.046)	(0.046)
		{0.814}	{0.466}	{0.994}	{0.654}
PL	0.052	-0.025	0.030	-0.028	0.003
	[0.048]	(0.044)	(0.032)	(0.024)	(0.030)
		{0.588}	{0.508}	{0.274}	{0.954}
PMDB	0.170	0.038	0.078	-0.020	0.057
	[0.096]	(0.082)	(0.041)	(0.046)	(0.049)
		{0.696}	{0.106}	{0.686}	{0.362}
PPS	0.022	-0.028	0.014	-0.021	-0.007
	[0.030]	(0.016)	(0.021)	(0.015)	(0.012)
		{0.082}	{0.526}	{0.190}	{0.612}
				continues next page...	
N (state-terms)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

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 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 A2: Effect on Party Seat Shares (*continued*)

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
PSB	0.042	0.010	0.052	-0.021	0.031
	[0.056]	(0.022)	(0.027)	(0.020)	(0.013)
		{0.714}	{0.060}	{0.348}	{0.054}
PSDB	0.112	-0.054	-0.099	0.023	-0.077
	[0.096]	(0.063)	(0.045)	(0.047)	(0.030)
		{0.466}	{0.054}	{0.654}	{0.068}
PT	0.094	0.030	-0.087	0.058	-0.029
	[0.062]	(0.027)	(0.022)	(0.022)	(0.012)
		{0.290}	{0.004}	{0.026}	{0.010}
PTB	0.070	0.003	-0.025	0.014	-0.011
	[0.064]	(0.098)	(0.020)	(0.049)	(0.052)
		{0.982}	{0.224}	{0.746}	{0.814}
Other Parties	0.214	-0.038	0.110	-0.074	0.036
	[0.116]	(0.103)	(0.067)	(0.039)	(0.079)
		{0.680}	{0.214}	{0.080}	{0.658}
N (state-terms)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

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 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 A3: Effect on Levels of Spending

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
Sample (terms):		1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Health Care Spending	99.66	40.705	-105.670	73.187	-32.483
(2000 *reais* per capita)	[82.95]	(14.650)	(63.533)	(32.932)	(31.385)
		{0.000}	{0.194}	{0.054}	{0.366}
Total Spending	963.33	-64.783	-545.042	240.130	-304.913
(2000 *reais* per capita)	[475.223]	(148.472)	(304.307)	(190.841)	(139.267)
		{0.710}	{0.212}	{0.276}	{0.136}
Total Revenue	955.15	-102.113	-534.818	216.353	-318.466
(2000 *reais* per capita)	[476.41]	(162.058)	(305.174)	(19.943)	(13.548)
		{0.572}	{0.228}	{0.352}	{0.092}
Net Interest Payments	4.79	10.935	-18.459	14.697	-3.762
(2000 *reais* per capita)	[5.75]	(35.880)	(18.161)	(21.320)	(18.243)
		{0.772}	{0.380}	{0.554}	{0.848}
N (state-terms)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

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 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 A4: Effects by Categories of State Spending

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
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	0.025
	[0.097]	(0.044)	(0.083)	(0.043)	(0.052)
		{0.126}	{0.192}	{0.084}	{0.686}
Social Assistance	0.108	-0.016	-0.053	0.018	-0.035
	[0.054]	(0.017)	(0.024)	(0.015)	(0.015)
		{0.316}	{0.074}	{0.274}	{0.034}
Education	0.176	-0.005	0.016	-0.010	0.005
	[0.037]	(0.014)	(0.015)	(0.011)	(0.009)
		{0.708}	{0.324}	{0.626}	{0.554}
Judiciary	0.063	0.008	-0.024	0.016	-0.008
	[0.023]	(0.020)	(0.011)	(0.012)	(0.010)
		{0.726}	{0.048}	{0.326}	{0.540}
Legislative	0.036	-0.002	0.015	-0.009	0.007
	[0.016]	(0.008)	(0.011)	(0.006)	(0.007)
		{0.878}	{0.318}	{0.266}	{0.524}
Public Safety	0.074	0.002	-0.017	0.009	-0.008
	[0.026]	(0.015)	(0.024)	(0.010)	(0.018)
		{0.922}	{0.564}	{0.348}	{0.752}
Transportation	0.053	0.010	-0.004	0.007	0.003
	[0.049]	(0.017)	(0.036)	(0.017)	(0.023)
		{0.606}	{0.910}	{0.658}	{0.2968}
Other Categories	0.210	0.036	-0.025	0.030	0.005
	[0.086]	(0.031)	(0.036)	(0.031)	(0.013)
		{0.316}	{0.536}	{0.426}	{0.654}
N (state-terms)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

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

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
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	0.021	-0.065
	[0.092]	(0.075)	(0.054)	(0.056)	(0.032)
		{0.682}	{0.226}	{0.678}	{0.146}
Share with 1 to 6 Visits	0.537	-0.082	0.111	-0.090	0.017
	[0.123]	(0.054)	(0.035)	(0.031)	(0.034)
		{0.178}	{0.004}	{0.004}	{0.596}
N (state-terms)	-	54	54	-	-
N (states/first-diffs)	-	27	27	-	-

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 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: IV Estimates of the Effect of Electronic Voting

Outcome: Window	**First-Stage**	Valid Votes/ Turnout	Share of Health Care Spending	Birth Outcomes (Mothers w/o Primary Schooling): Share w. 7+ Prenatal Visits	Birth Outcomes (Mothers w/o Primary Schooling): Share w. Low-Weight Births (x100)
500,000	0.822	0.100	0.041	0.089	-0.640
	(0.078)	(0.019)	(0.007)	(0.047)	(0.265)
	{0.000}	{0.026}	{0.000}	{0.053}	{0.016}
200,000	0.772	0.106	0.039	0.086	-0.696
	(0.061)	(0.018)	(0.007)	(0.042)	(0.291)
	{0.000}	{0.004}	{0.000}	{0.032}	{0.010}
100,000	0.740	0.105	0.038	0.084	-0.575
	(0.050)	(0.018)	(0.007)	(0.042)	(0.268)
	{0.000}	{0.004}	{0.000}	{0.050}	{0.038}
50,000	0.680	0.103	0.032	0.087	-0.612
	(0.041)	(0.018)	(0.010)	(0.043)	(0.266)
	{0.000}	{0.006}	{0.002}	{0.056}	{0.018}
40,000	0.673	0.104	0.033	0.088	-0.602
	(0.040)	(0.018)	(0.009)	(0.043)	(0.258)
	{0.000}	{0.006}	{0.000}	{0.042}	{0.018}
30,000	0.679	0.103	0.033	0.095	-0.615
	(0.048)	(0.019)	(0.011)	(0.043)	(0.264)
	{0.000}	{0.012}	{0.019}	{0.008}	{0.034}
20,000	0.677	0.101	0.032	0.105	-0.397
	(0.079)	(0.020)	(0.012)	(0.046)	(0.279)
	{0.000}	{0.020}	{0.076}	{0.020}	{0.168}
10,000	0.658	0.102	0.029	0.107	-0.332
	(0.105)	(0.020)	(0.014)	(0.046)	(0.268)
	{0.000}	{0.014}	{0.190}	{0.080}	{0.268}
5,000	0.559	0.111	0.026	0.111	-0.631
	(0.083)	(0.019)	(0.014)	(0.043)	(0.393)
	{0.000}	{0.006}	{0.212}	{0.012}	{0.112}
N	81	81	81	81	81

Standard errors clustered at the state level in parenthesis. 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 column reports the estimation of equation (10) using the specified dependent variable. Each figure is from a separate 2SLS regression using a sample covering three electoral terms, with the instrument with the window (j) specified in the row. See text for further information on the construction of instruments.

Table A7: IV Estimates of the Effects of Electronic Voting (Restricted Sample)

Outcome:				**Birth Outcomes (Mothers w/o Primary Schooling)**	
			Share of	Share w. 7+	Share w.
		Valid Votes/	Health Care	Prenatal	Low-Weight
Window	**First-Stage**	Turnout	Spending	Visits	Births (x100)
500,000	0.715	0.134	0.055	0.103	-0.933
	(0.164)	(0.020)	(0.024)	(0.050)	(0.603)
	{0.000}	{0.000}	{0.032}	{0.000}	{0.148}
200,000	0.678	0.141	0.043	0.085	-1.163
	(0.111)	(0.016)	(0.018)	(0.033)	(0.627)
	{0.000}	{0.000}	{0.026}	{0.000}	{0.002}
100,000	0.709	0.139	0.041	0.083	-0.734
	(0.085)	(0.015)	(0.019)	(0.028)	(0.564)
	{0.000}	{0.000}	{0.000}	{0.000}	{0.164}
50,000	0.670	0.138	0.018	0.102	-0.984
	(0.048)	(0.015)	(0.015)	(0.028)	(0.606)
	{0.000}	{0.000}	{0.386}	{0.000}	{0.002}
40,000	0.655	0.140	0.022	0.106	-0.932
	(0.040)	(0.014)	(0.014)	(0.031)	(0.566)
	{0.000}	{0.000}	{0.234}	{0.000}	{0.002}
30,000	0.525	0.142	0.018	0.145	-1.131
	(0.177)	(0.019)	(0.026)	(0.069)	(0.672)
	{0.000}	{0.000}	{0.608}	{0.000}	{0.002}
20,000	0.363	0.143	0.011	0.208	-0.238
	(0.278)	(0.031)	(0.048)	(0.166)	(0.891)
	{0.424}	{0.346}	{0.878}	{0.000}	{0.854}
10,000	0.311	0.145	-0.002	0.240	0.082
	(0.219)	(0.029)	(0.053)	(0.157)	(1.101)
	{0.242}	{0.176}	{0.898}	{0.001}	{0.944}
5,000	0.277	0.161	-0.005	0.206	-1.097
	(0.148)	(0.030)	(0.046)	(0.114)	(1.025)
	{0.178}	{0.094}	{0.896}	{0.000}	{0.296}
N	69	69	69	69	69

Standard errors clustered at the state level in parenthesis. 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 column reports the estimation of equation (10) using the specified dependent variable. Each figure is from a separate 2SLS regression using a sample covering three electoral terms, with the instrument with the window (j) specified in the row. See text for further information on the construction of instruments. The sample excludes states that did not follow the discontinuous assignment rule.