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

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Abstract

This paper studies the effects of an electronic voting technology that introduced visual aids in Brazilian elections and facilitated voting for the less educated. Estimates exploiting a regression discontinuity design embedded in its phase-in through time indicate that electronic voting reduced residual (error-ridden and uncounted) votes and generated the *de facto* enfranchisement of a large fraction (11%) of the electorate.

This enhanced political participation of less educated (poorer) voters is then shown to have: (1) increased the number of state legislators that are themselves less educated; and (2) shifted government spending towards public health care, a policy that is particularly beneficial to the poor; leading to (3) improved health services utilization (pre-natal visits) by less educated mothers and (4) reduced occurrence of low-weight births in this group. No effects on health care utilization by more educated mothers and on the weight of their newborns are found. The results are consistent with the predictions of political economy models and demonstrate that electronic voting can promote the political empowerment of the poor and raise their living standards.

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1 Introduction

The inadequacy of public services in the developing world is often explained by the lack of influence of its neediest citizens on the political process. Even within free, functioning, democracies, elected officials are blamed for ignoring the necessity for government action from substantial parts of the population. A common policy recommendation, thus, is to intervene in the electoral process and raise disadvantaged groups' influence on public decision-making.[1]

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

This paper provides quasi-experimental, regression discontinuity-based, evidence on how improved political participation can lead to better service outcomes. It estimates the effects of a new electronic voting technology that introduced visual aids (candidates' photographs) in Brazilian elections and reduced a mundane, but nonetheless important, obstacle to the political participation of its less educated voters: difficulty in operating writing-intensive paper ballots. It quantifies a large *de facto* enfranchisement caused by electronic voting and estimates its impact on policymaking and its outcomes. Specifically, it finds that increased political participation of the poor lead to increased public health care spending, utilization (pre-natal visits), and infant health, measured by birth-weight.

Filling a ballot may be a trivial task to the educated citizens of developed countries, but the same is not true in Brazil, where 23% of adults (age 25+) are "unable to read and write a simple message" and 42% did not complete 4th grade education.[2] Moreover, Brazilian paper ballots required voters to write the candidate's name or electoral number and provided no visual aids. The combination of these two facts resulted in a substantial

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

[2] Figures from the 1991 Census.

quantity of error-ridden and blank ballots being cast in its elections. Hence, a large number of votes were **residual**, a term describing the votes that cannot be assigned to a candidate and are discarded from the tallying of results.

In the late-1990s, the Brazilian federal electoral authority developed an electronic voting technology as a substitute for the 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, resulted in the surprising unintended effect of facilitating voting and reducing errors.

Exploiting a regression discontinuity design in the assignment of voting technology in the 1998 election, I estimate that electronic voting reduced residual voting in state legislature elections by a magnitude of 11% 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. The discontinuity design is generated by the fact that, due to limited supply of devices, only municipalities with more than 40,500 registered voters used electronic technology, while the remainder used paper ballots. Comparison of municipalities "just above" and "just below" the threshold estimates causal effects, since assignment of electronic voting is "almost random" for municipalities near the cutoff.

Moreover, three pieces of evidence indicate that the bulk of these *de facto* enfranchised voters had low levels of education. Firstly, the reduction of residual votes caused by electronic voting was stronger in municipalities with less educated populations. Secondly, the parties that benefited from the larger number of valid (non-residual) votes were popular in less educated municipalities and, thirdly, had less educated candidates. Since voting is compulsory in Brazil and elections are at-large, these three results are not driven by the (non-existent) differences in turnout or candidate entry across municipalities above and below the threshold.

Motivated by the results above, this paper also provides evidence that this *de facto* enfranchisement of the less educated shifted public policymaking in a way that benefited them. Since the discontinuous assignment was observed in an election for state government officials, I test for such effects in policy decisions made at the state level, focusing on a particular area of government spending that disproportionately affects the less educated:

expenditures on public health care. While poorer Brazilian rely solely on the public system for health care services, richer voters are substantially more likely to use private services. The less educated have thus relatively stronger preferences for increased public health care provision, and political economy models predict that their enfranchisement should lead to higher public spending in this area.[3]

This paper also estimates the effects of the additional health care spending induced by electronic voting on health services utilization and outcomes. Specifically, I use vital statistics data covering the universe of Brazilian birth records to find that additional public spending raised the number of pregnant women visiting health professionals and lowered the prevalence of low-weight births, a common indicator of infant health. Moreover, these results hold only for less educated mothers, with no effects being found on more educated ones, supporting the interpretation that electronic voting lead to the political empowerment of the poor.[4]

Other than its importance for welfare, the focus on health care spending, pre-natal visits, and birth-weight is motivated by three factors. Firstly, public health care is a politically salient policy that state legislators can affect significantly within a short time frame, especially in the case of primary care that supplies pre-natal visits. Secondly, pregnant women have high demand for health services and benefit disproportionately from its additional provision. Thirdly, newborn health can respond strongly and rapidly to health care improvements (as opposed to adult outcomes, in which the effects are lagged through time). The short time frame between legislation, service provision and health outcomes is an important feature given the identification strategy used in this paper, which is described below.[5]

The identification strategy for the results described above uses the fact that the discontinuous assignment in the 1998 election generated specific and unusual differences in the timing

[3]Section 3.1 discusses the differential usage of the Brazilian health care system across income and education groups and how the results relate to political economy models in more detail.

[4]Low birth-weight is defined as less than 2500 grams by the World Health Organization. It is the leading indicator of infant health, and is known to predict adult outcomes, as discussed in Section 3.1.

[5]Section 3.1 describes the role of health care in Brazilian state politics and how legislators can increase health care provision within a short time frame. In the setting of municipal governments, Ferraz and Finan (2010) show that increasing legislator wages leads to substantial improvement in public health provision (number of doctors and clinics) in less than two years. Section 3.1 also discusses the channels through which pre-natal visiting affect birth-weight.

in the adoption of electronic technology across states. The phase-in of the new technology was carried out within three consecutive elections held in 1994, 1998, and 2002. In 1994, only paper ballots were used. In 1998, there was the (previously described) discontinuous assignment. In 2002, only electronic voting was used.

Such schedule implies that the differential timing of electronic voting adoption in a state is determined by a time-invariant cross-sectional variable: the share of voters living in municipalities above the cutoff for electronic voting use in 1998. Take, for example, a state where 75% of its voters lived above such cutoff. Between the 1994 and 1998 elections, 75% of its voters **switched** from using paper to electronic voting technology, and between the 1998 and 2002 elections, the remaining 25% of voters switched to electronic voting. Compare that to a state with a lower share of voters residing above the cutoff (30%, for example). Such state observed only 30% of its voters switching to electronic voting between the 1994 and 1998 elections, with the remaining 70% changing from paper to electronic voting between the 1998 and 2002 elections.

Hence, states with a high share of voters above the 40,500-voter threshold experienced most of the enfranchising effects of electronic voting (four years) earlier than the states with a low share of voters above it. The intuition behind the identification strategy relies on testing if outcomes of interest track this same pattern. For example, finding that states with higher shares of voters above the cutoff also observed increases in health spending earlier indicates that electronic voting is causing increased health spending.[6]

The effects of electronic voting on policy outcomes are thus identified only from variation coming from the interaction of a cross-sectional variable (share of voters above the cutoff) with the timing of different elections. In the period where such variable positively predicts

[6] Section 3.2 formalizes this argument and provides the actual econometric implementation. A brief description (which, for concreteness, focuses on the specific outcome of health spending) is the following. Let S_i denote the share of voters residing above the cutoff for electronic voting use in 1998 in state i. Denote $electro_{ie}$ as the share of voters using electronic voting in state i at election e, and define H_{ie} as the health spending by the legislature selected by the election held at year e (e.g., H_{i1998} is the spending in the four-year legislative term starting after the 1998 election). By design, $(electro_{i1998} - electro_{i1994}) = S_i$ and $(electro_{i2002} - electro_{i1998}) = 1 - S_i$. Hence S_i is directly proportional to electronic voting use in one period, but inversely proportional to it in the next. If regression results indicate that S_i and $(H_{i1998} - H_{i1994})$ are *positively* correlated, while S_i and $(H_{i2002} - H_{i1998})$ are *negatively* correlated, a causal effect of electronic voting on health spending can be inferred. The logic behind this is that this sharp sign change in how the same cross-sectional variable predicts increases in health care is unlikely to be driven by omitted variables.

electronic voting adoption, it also positively predicts valid voting, number of uneducated legislators, health care spending, number of pre-natal visits and birth-weight. On the other hand, in the period where the same cross-sectional variable negatively predicts electronic voting adoption, it negatively predicts the same outcomes. This identifies the causal effect of electronic voting, since omitted variables are unlikely to follow this pattern. Note also that neither mean reversion or omitted state-specific trends can explain the results.

Moreover, two falsification tests check the validity of the identification strategy. The first is the finding that the share of voters above the cutoff is orthogonal to changes in all the relevant outcomes in the periods when it is not associated with changes in voting technology (e.g., between two elections that only used paper ballots). The second one consists of the placebo test of estimating effects of electronic voting on variables that are not expected to be affected by it (e.g., population, income, pre-natal visits and birth-weight of **more educated** mothers). The finding of zero effects in these cases supports the identification strategy.

The estimates indicate that the de facto enfranchisement of (mostly less educated) 11% of the Brazilian electorate: i) increased the share of states' budgets spent on health care by 3.7 percentage points (p.p.), raising expenditure by more than 50% in a 8-year period; ii) raised the proportion of uneducated pregnant women with adequate (7+) number of pre-natal visits by 7 p.p. and iii) lowered the prevalence of low-weight births by 0.4 p.p..

This paper communicates with four different strands of economic literature. Firstly, as mentioned before, it is most closely related to studies of interventions to raise political responsiveness to (a group of) voters and its consequences on policymaking (Pande, 2003; Chattopadhyay and Duflo, 2004; Besley et al., 2005; Olken, 2010). It also speaks to the role of political mechanisms in improving public good provision, such as Ferraz and Finan's (2010) analysis of Brazilian municipal legislators' remuneration.[7]

Secondly, it contributes to the analysis of *de jure* enfranchisement episodes, such as the historical case of poor and racial minorities (Husted and Kenny, 1997) and women (Kenny and Lott, 1999; Miller, 2008) in the United States. Thirdly, the estimates in this paper

[7]See also Beaman et al. (2009) on the effects of gender quotas on political outcomes. Another related set of papers are those related to the role of information and media, such as Besley and Burgess (2002), Strömberg (2003), and Ferraz and Finan (2008), and also the literature on citizens monitoring public health care services directly (i.e., not through the political process), such as the field experiments by Banerjee et al. (2008) and Björkman and Svensson (2009).

quantify the health effects of a large-scale improvement in health care funding. While the cost-benefit analysis of specific treatments is usually found in clinical studies, estimating the effect of larger interventions is more common in economics.[8] Moreover, by focusing on politically motivated spending at the state level, the estimates in this paper incorporate political economy and general equilibrium considerations that are not present in small-scale studies.[9]

Finally, a growing literature evaluates voting technologies both in economics (Garner and Spolaore, 2004; Dee, 2007; Card and Moretti, 2007; Shue and Luttmer, 2009) and political science, as exemplified by Ansolabehere and Stewart III (2005).[10] The focus of these papers is mostly on technical outcomes (e.g., residual and miscast votes), with the exception of Anderson and Tollison's (1990) historical analysis of secret voting in the United States.

The remainder of the paper is divided in three sections. Section 2 describes the electoral process and the introduction of electronic voting in Brazil, using regression discontinuity design estimates at the municipal level to argue that it 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 electronic voting's enfranchisement effect on health care funding and health outcomes. Section 4 concludes the paper.

2 The Impact of Electronic Voting on Political Participation

2.1 Electoral Rules and Voting Technology in Brazil

Brazil is a federation of 27 states, which are themselves comprised of over 5,000 municipalities. Each state has its own legislature (*Assembléia Legislativa*) and a directly elected gov-

[8]For example, Almond et al. (2006, forthcoming) study of the impact of hospital desegregation on birthweight and child mortality of African-Americans in the US South serve as an example.

[9]Acemoglu (2010) discusses the importance of political economy and general equilibrium considerations in development economics. Examples of general equilibrium effects in increased health care funding are feedback effects in the labor market of health professionals, or the presence of treatment externalities.

[10]See also several other papers and reports associated with the MIT/Caltech Voting Technology Project (website: vote.caltech.edu/drupal/).

ernor (*Governador*).[11] Federal legislation establishes the same election (and inauguration) dates and rules for all states. Governors are elected under a dual-ballot (runoff) plurality rule, while legislators are elected under a **open-list proportional representation** system, where the whole state is a single multi-member district (i.e., elections are at large).

Proportional representation (PR) systems allocate legislative seats in proportion to the percentage of the total votes that a party receives, so that a party that receives $x\%$ of the votes should hold $x\%$ of the seats.[12] Brazil uses an open-list PR system, where a citizen casts a vote to an individual candidate, not a party list. After the election all votes won by candidates of each party are added together, and this total determines how many seats the party is entitled to. The candidates within a party list are then ranked by the number of votes each received, and this rank is followed in assigning candidates to seats.[13]

When it comes to the design of voting technology, the main consequence of open-list PR is that votes are cast to individual candidates, and in the case where there is a large number of them, a full list with visual aids is impractical. Moreover, state elections in Brazil are held jointly with federal elections for president, the federal senate and the lower chamber of congress.[14] For example, in the 1998 election a voter in the state of São Paulo had to choose one candidate out of 1.265 for the state legislature; 661 candidates for the lower chamber of federal congress; 10 candidates for state governor; 13 candidates for the federal senate; and 12 presidential candidates. This exemplifies both the impossibility of listing names (not to mention visual aids) and also the relative complexity of the task of voting.

Until 1994, voting in Brazil was done entirely in paper ballots such as the one exemplified in Figure 1. In the cases where open-list PR was used, the voter was required to write down

[11] There are actually 26 states and a "Federal District" which, given the similarity of electoral rule and government form, I refer to as a state.

[12] Subject to integer constraints. In the Brazilian case, the d'Hondt formula is used. A detailed description of electoral electoral rules can be found on Cox (1997).

[13] For example, suppose that Party A has a list of 10 candidates running for a 50-seat legislature. If the total amount of votes received by the 10 candidates equals 10% of all votes, the party is then entitled to $0.1 \times 50 = 5$ seats. These 5 seats are then allocated to the 5 most voted candidates of Party A. Voters also have the option of casting a vote to the party only, which is then counted as a vote to the list but has no impact in the ranking of candidates within it. In the sample used in most of the analysis only 3,7% of the votes were cast to party labels.

[14] Members of the federal senate are elected by plurality rule, while the president (like governors) are elected through a dual-ballot plurality rules (runoff system). Elections for the lower chamber of congress use the same open-list PR system used for state legislatures.

his chosen candidate's name or electoral number, a five-digit code assigned by the election authority.[15] Given the absence of visual aids on the ballot, this operation required some degree of literacy.

In the mid-1990s, the independent branch of the federal judiciary that regulates electoral procedures (*Tribunal Superior Eleitoral*) introduced a new direct-record electronic voting technology in order to increase speed and reduce costs in vote counting.[16] The interface of the new technology is constituted of a small screen and a set of keys closely resembling a touch-tone phone (Figure 2) with three colored buttons added. In this machine a voter types the candidate's number and after her name, party affiliation, and picture appears on the screen, confirms the vote (or chooses to start the process again).

Not only the use of pictures as visual aids facilitates voting for those unable to read, but the use of a number-based interfaced resembling using a phone dial was likely instrumental. One of it's designers indeed notes that "*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.*"[17]

The federal government also promoted a large scale campaign instructing the public on how to use the new machines, which included public service announcements on television and radio stations and also allowing voters to test the new technology before the election, by voting on imaginary candidates on sample devices at election authority offices.

While the discussion above indicates that the elections authority took deliberate steps to design an easily operable voting technology, it does not to seem to be the case that the authority was aiming in reducing residual voting. For example, the chief designer of the technology describes its association with reduced in residual votes as "*a surprise.*"[17]

[15] Voting for president, governor and senator are done by checking a box by the name of the candidate, while votes for federal congress used a 4-digit number (see Figure 1).

[16] This objective was indeed achieved. The Brazilian electoral authority is able to fully count and process over 100 million votes cast across a large territory in three to five hours, a feat that is largely advertised by the authority.

[17] Statement from the Brazilian Electoral Authority IT Director (Jorge Freitas), available on video at its official website (www.tse.gov.br).

2.1.1 Introduction of Electronic Voting and the Design of this Study

Since Brazil's re-democratization in 1988, elections for state governments were held on 1990, 1994, 1998, 2002 and 2006.[18] In the 1990 and 1994 elections, only paper ballots such as the one exemplified in Figure 1 were used. For 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 used standard paper ballots. The assignment rule was adopted due to the limited production capacity of the manufacturer and economies of scale in its distribution, hence it is unlikely that the threshold was manipulated. In 2002 and afterwards the new technology became the sole method of collecting votes in Brazilian elections and referendums.[19]

This threshold-based rule used in the 1998 election creates a standard regression discontinuity design. Under mild assumptions, it generates assignment "as good as random" since municipalities falling "just below" and "just above" the threshold should be, on average, *ex ante* similar to each other in every possible aspect. In other words, the reason that they are on a particular side of the threshold is due to random uncontrollable events that should not be related to the outcome of interest. This argument is formalized by Lee (2008).

Other than voting technology, any observed or unobserved variable that could affect voting should be the same for all municipalities that are sufficiently close to the threshold. This guarantees that any difference in outcomes between these two groups is a causal consequence of the different voting technologies.

In practice, the "quasi-random" interpretation of the estimates require that agents have limited or no control over the forcing variable (number of registered voters). Since the assignment rule was announced in May of 1998[20] and the forcing variable is the number of voters registered for municipal elections two years earlier in 1996, this is likely the case.

[18]Election are always held on the first or second sunday of October, with elected officials being inaugurated on January 1st.

[19]Specifically, the 40,500-voter threshold was pinned down by assigning the fixed supply of devices available at the time (168,000 of them) 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 municipalities is less costly than distributing few devices across several municipalities. Electronic voting was also used in the 1996 municipal elections (in the 57 largest cities and state capitals). This paper, however, focuses only on state elections.

[20]This is the date that first appears on official records of the electoral authority, and also when it was first mentioned on Brazil's largest newspaper, the *Folha de São Paulo*.

Additionally, evidence against the of manipulation of registration and the position of the threshold is presented in Appendix C.

Finally, it must be noted that four (out of 27) states used electronic voting 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 electronic voting to isolated areas, while the states of Rio de Janeiro and Alagoas have areas where the army provides security to election officials, allowing an opportunity to check the logistics of distributing the electronic devices jointly with the army.[21] These states are dropped from all samples used in this Section, but will be used as robustness checks of the state panel estimates on Section 3.

2.2 Data and Estimation Framework

2.2.1 Municipal Data on Electoral Outcomes

Information on voter registration, turnout, election results, and candidate characteristics at the municipality level for several years were obtained from the federal electoral authority. The institution also published reports listing the municipalities that used electronic voting which shows an almost perfect compliance with the discontinuous assignment rule. All the 307 municipalities (out of 5,282) above the 40,500-voter cutof used electronic voting in 1998.[22] Additional data on municipal level demographics (population and educational attainments) are from tabulations of the 1991 Brazilian Census.

The main outcome of interest is the number of votes that are **valid** (**i.e., non-residual**). A vote is considered valid if, and only if, it can be assigned to a particular candidate and then counted in the party vote shares. A vote that is not valid is commonly called a **residual vote** so that total turnout must equals the sum of valid and residual votes. A vote cast in a paper ballot is deemed residual if it is left blank or if the name or number written on the ballot does not correspond to an actual candidate. With electronic voting, a residual vote

[21] Note, however, that election violence (such as observed in some African countries) is not common in Brazil. Army protection is usually needed to allow elected officials to enter high-crime areas.

[22] Seven (out of 5,282) municipalities below the threshold deviated from the rule, by having their (formal) requests to use electronic voting accepted by the electoral authority. The transcripts of the requests and decisions indicate mainly vague idiosyncratic reasons (e.g., no municipality would be using electronic voting 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.

may still be cast, as there is a button that casts a blank vote and the voter may type in a number that does not correspond to any candidate. Such a vote is depicted on Panel C of Figure 2.[23]

This section focuses solely on the election outcomes for state legislature elections. The results for elections for other offices are presented in Appendix B. The relevant sample moments are provided in the discussion and in the tables reporting estimates, so that the summary statistics are deferred to the appendix.

2.2.2 Estimation Framework of RDDs

Let v be the number of registered voters in a municipality. The treatment effect of moving from paper ballots to electronic voting on outcome y is given by:

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

Under the assumption that the conditional expectation of y on v is continuous, the first term on the right hand side converges to the expected outcome of a municipality with 40,500 voters using paper ballot, while the second term converges to the expected outcome of a municipality with 40,500 voters using electronic voting. Hence, TE identifies the treatment effect for a municipality of 40,500 voters, as long as the conditional expectations (and distribution of treatment effects) are continuous at the threshold.

I estimate (1) following the guidelines in Imbens and Lemieux (2008) and Lee and Lemieux (2010), which in turn rely on the results provided by Hahn et al. (2001). The limits on the right hand side are estimated non-parametrically using local polynomial regression. This consists of estimating a regression of y on (a polynomial of) v using only data satisfying $v \in [40,500 - h; 40,500]$.[24] The predicted value at $v = 40,500$ is thus an estimate of the limit of y as $v \uparrow 40,500$. Similarly, a regression using only data satisfying $v \in [40,500; 40,500 + h]$ is used to estimate the limit of y when $v \downarrow 40,500$. The difference between these two estimated limits is the treatment effect. It is important to note the non-parametric nature of the estimation: although linear or quadratic regressions are used, the

[23] Note that, in this case, the voter would still need to confirm his vote even after the machine informed him that the typed number does not correspond to any candidate/party.

[24] Note that the regression is unweighted (i.e., rectangular kernel)

consistency of the results holds for any arbitrary and unknown shape of the relationship between y and v. Notice also that the limit approaching one side of the threshold is estimated using only data on that particular side.[25]

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

$$y = \alpha + \beta 1\{v > 40,500\} + f(v) + u \tag{2}$$

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

A key decision is h, the kernel bandwidth. Higher (lower) values will generate more (less) precision but create larger (smaller) bias. To show the robustness of the results to different choices of h this paper presents the results for three different levels: 5,000; 10,000, and 20,000 voters. Note that these are relatively narrow (given the overall range in the size distribution of municipalities) and hence reinforce the "local" intuition of regression discontinuity designs. Although there are more than 5,282 observations in the data, only 558 are included in the largest chosen bandwidth, of which 130 are above the cutoff.[26]

2.3 Regression Discontinuity Results

2.3.1 Electronic Voting Increases Valid Votes

Before reporting the estimation results, some graphical evidence is provided. Figure 3 plots the main outcome of interest (valid votes in the state legislature elections, as a share of turnout) against the forcing variable (registered voters in 1996) for three different elections.

[25] This approach is appropriate for "sharp" discontinuity designs, where the probability of treatment (in this case, electronic voting use) is zero below the threshold and one above it (perfect compliance). Even though there is (a very small) lack of compliance since seven municipalities below the threshold used electronic voting, the estimated effect of crossing the threshold on the probability of using electronic voting is a precisely estimate one, implying that approaching the estimation as a fuzzy design would lead to nearly identical results. Moreover, dropping the seven non-compliers from the sample or treating them as "treated" lead to no change in any of the results.

[26] As the bandwidth increase, the number of smaller municipalities that are included at the extreme of the left interval increase rapidly (see Figure A1, in Appendix C). Hence, estimates with large bandwidths will likely put too much weight on fitting the relationship away from the neighborhood of the 40,500 threshold.

Each marker in the figure reflects the average outcome in a bin of municipalities that fall within a 4,000-wide interval of the forcing variable (e.g., the square just to the right of the 40,500 contains the average valid vote ratio for municipalities with $S \in [40,500; 44,500]$ in the 1994 election). To facilitate visualization, 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 upward jump is visible in the 1998 election (in circles). A little over 75% of the votes are valid on the (paper ballot using) municipalities below the cutoff, and this figure suddenly changes to close to 90% as the cutoff is crossed and electronic voting is introduced. The fact that no discontinuity is visible for the elections held in 1994 (when all municipalities used paper ballots) and 2002 (when electronic voting was completely phased-in) provides a falsification test and reassure that municipalities "just above" and "just below" the cutoff are indeed valid treatment/control groups.

Figure 4 repeats the exercise carried out in Figure 3 for turnout (as a share of registered voters) and voter registration (as a share of total population) in the 1998 election. There are no visible discontinuities, implying the turnout and registration behavior are the same in both treatment and control municipalities. These results are not surprising given that Brazilian 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. Moreover, elections are always held on a Sunday and voters are allocated to polls close to their residence in order to foster turnout. Although these features do not guarantee a turnout close to 100%, it likely makes voting technology a second-order issue in the decision to register and vote.[27]

Panel A of Table 1 presents the local polynomial regression estimates of the treatment effects. Irrespective of the bandwidth and specification used, the estimated effect of a switch

[27] Voting is voluntary for citizens aged 16-17 or 70+. Compliance with electoral duties is required to attend public schools, receive payments from social programs, obtain government sponsored credit, work for the public sector and renew documents (passports, driver licences, social security cards, etc.). Figure 3 and Table 1 show that turnout is in the order of 85% of registered voters. Citizens who are not in their city of residence on election day can be waived from the punishment by attending a poll in any other municipality and submitting a "waiver form".

from paper ballot to electronic voting is an increase in the valid vote-turnout ratio in the order of 12 p.p.. Moreover, the estimates are precisely estimated and significant at the 1% level.

Table 1 and Figure 3 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 "protest votes", such large shares likely reflect error, especially since there is no reason to expect protesting to be affected by voting technology.

Panel A also presents the estimated treatment effects on turnout (as a share of registered voters) and voter registration (as a a share of total population). In conformity with the graphical analysis, the treatment effects are numerically small and not statistically different from zero at any reasonable level of significance.

Panel B provides a placebo test by estimating treatment effects in the election years without a discontinuity present: when all municipalities used paper ballots (1994) or electronic voting (2002). The results indicate zero impact across all specifications. This provides supportive evidence on the "quasi-random" interpretation of the results, if any unobserved difference in municipalities just above and just below the threshold were driving the result observed in 1998, they would likely have been present in the preceding or subsequent election.

Panel C provides a standard robustness check in the RDD literature: testing for effects on pre-determined covariates. In the same spirit that insignificant differences between covariate means in treatment and control groups are interpreted as evidence of successful randomization in controlled trials, the robust finding of zero effects on demographics (income and education) and geographical location (latitude and longitude) reported on Panel C can be seen as evidence of the "quasi-random" nature of our RDD.

Recall that the at-large nature of state elections imply that, on average, the same choice of candidates are available in municipalities above and below the threshold, and hence candidate entry cannot be a confounding issue for the treatment effects. There are, however, three other possible threats to validity that could invalidate the estimated treatment effects. The first one is manipulation of the forcing variable (voter registration) or of the threshold itself. Previous discussion highlighted that since the number of registered voters was measured before the announcement of the cutoff, this is unlikely the case. Evidence rejecting manipulation is provided in Appendix C. Secondly, there could be other treatments assigned by the same discontinuity. To the best of my knowledge, this is **not** the case, as would be expected since the threshold was chosen based on the available supply of machines. Moreover, the zero effects found in the 1994 and 2002 elections require such unknown discontinuity to be present only in 1998. Appendix C lists known discontinuous assignments in Brazil and discuss why they do not confound the results.

Thirdly, the issue of electoral fraud (either under paper or electronic voting) could, in principle, play a role in the results. This is unlikely given both *a priori* grounds and formal testing. There is no known evidence of electoral fraud at the vote counting level in Brazil, and the electoral authority and electronic voting system are trusted and perceived as honest by 98% of Brazilians.[28] To further probe the possibility of fraud, I estimated the treatment effect of electronic technology on the vote shares of parties holding the national and state executives (i.e., the presidency and governorship). If fraud was confounding the results, it would be expected an (either positive or negative) effect of electronic voting on their vote shares, as they are the ones with better opportunities to influence the electronic system design and the counting of paper ballots. The estimates, which are not reported due to space considerations, are close to zero and statistically insignificant, hence not supporting the occurrence of fraud.

2.3.2 Electronic Voting and the Enfranchisement of Less Educated Voters

While the results in the previous subsection indicate that electronic voting generated the effective political participation of a large share of the electorate, it does not address the issue of the identity of these newly enfranchised voters and the recipients of their votes. Section 2.1 discussed why it is presumable that the less educated are more likely to cast residual votes[29] and that electronic voting makes voting simpler for less educated voters with poorer reading and writing skills, and this subsection provide three pieces of evidence supporting this hypothesis.

[28]The figure is from a 2008 survey by the Instituto Nexus. The Economist magazine argued that Brazil "*sets the example on how to run clean and fair elections in developing countries.*" Avgerou et al. (2007) present the electronic voting system and the Brazilian electoral authority as a success case in citizens' trust in information technology and government institutions.

[29]Shue and Luttmer (2009) show that lower education predicts misvoting in a Californian election.

Firstly, the effect of electronic voting is stronger in municipalities with less educated populations.[30] Secondly, parties that are particularly popular among the less educated observed an increase in their vote shares in municipalities with electronic voting. Thirdly, the same occurred to parties that have less educated candidates. Notice that, given the at-large nature of state elections, voters residing in municipalities above and below the threshold (but within the same state) face the same option of candidates, so that these estimates are driven by voters' choices and not strategic entry of parties or candidates.

The second and third results are also useful as they rule out the possibility that electronic voting generates valid votes by substituting mistakes leading to residual votes for mistaken votes that are assigned to "random candidates". In other words, they not only demonstrate that more votes from less educated citizens became valid, but also show that these votes represent their preferences.

Panel A of Table 2 repeats the estimation presented on the first line of Table 1 interacting its explanatory variables with the share of adults (aged 25+) without complete primary education.[31] The near-zero effect of electronic voting by itself and the large size of the its interaction with education imply that treatment effects are higher where schooling levels are lower. The specification on Column (1), for example, implies that a (hypothetical) municipality where no citizen completed primary schooling would observe a 22 p.p. increase in the valid vote to turnout, while a (also hypothetical) municipality where every adult completed primary schooling would have a near zero impact. In practice, the distribution of primary schooling attainment across municipalities in the sample does not contain zero or one. Table 2 reports that the implied effect on valid voting for a municipality that had 71% of its adult population without primary schooling (the first decile of distribution) is 11 p.p. of total turnout. The similarly implied effect for municipality in the ninth decile of the education distribution, which has 92% of its adults with completed primary schooling, is 16

[30] Testing if the effect of electronic voting varies with education at the individual level would require observation of individual votes, which are not available given vote secrecy.

[31] Primary schooling (the *Ensino Fundamental* or *Ensino de Primeiro Grau*) are the first eight grades of formal Brazilian education, being roughly equivalent to elementary schooling in the United States. The choice of this particular level of education is guided by the fact that it is the one compatible with the data on legislator's education and mothers' education that will be used on Section 3. However, I repeated all estimations of this section using illiteracy rates and the share of population without 4th grade completion, finding similar results.

p.p.. As the subsequent columns show, changes in the bandwidth and specification lead to similar results.

Testing if electronic voting increased the vote share of parties popular amongst the less educated share of the population requires some objective definition of party popularity across educational groups, which is not a trivial task. The large number of parties and the nature of partisan politics in Brazil contribute make a definition based on *ex ante* grounds nearly impossible.

A brief but precise characterization of Brazilian parties is Ames' (2001) statement that "*parties exist only at the state level, [...] and can be surrogates for traditional factional disputes.*" The same (nationally identified) party can represent different ideological standpoints and cater to opposing special interests in different states. For example, Party A can be associated with factory unions in one state but large land-owners in another. Moreover, these different "*factions*" can change the parties used as "*surrogates*" from one election to the other, making not only comparisons across states but also time non-informative.

Hence I follow a systematic approach that uses the geographical distribution of votes to different parties to measure their popularity amongst the less educated. To address that parties with the same name may be, in practice, different entities in different states, the analysis is calculated at the level of a *list*, which is the listing of candidates that a party uses when running in for the legislature in a state.[32] An example can clarify the distinction between *party* and *list*: the Progressive Party's list of candidates for the São Paulo state legislature is a separate observation from the list of candidates that the Progressive Party ran for the Minas Gerais state legislature. Treating these two lists separately is important since the Progressive Party, using Ames' (2001) terms, may be a "surrogate" for a "faction" with interests close to the less educated voters' in one state, but not in the other.

The sample includes election outcomes at the list level for all Brazilian municipalities, allowing the construction of a paired municipality-list dataset, in which the unit of observation is a dyad representing a municipality and list jointly. There are 83,491 of such pairs in the 1998 election, in which Brazil had 30 political parties in activity, and saw 509 state

[32] See the description of the proportional representation electoral rule used in state legislature election on Section 2.1.

legislature candidate lists in the constructed 22-state sample (average of 23.1 lists per state).

Let $votes_{lm}$ denote the number of votes that party list l obtained in municipality m, and party list's l's popularity amongst the less educated is given by:

$$P_l^{votes} = \frac{\sum_m [votes_{lm} \cdot (\text{population share without primary education})_m]}{\sum_m votes_{lm}} \tag{3}$$

This formula is a coarse approximation to an ideal measure that would be the share of votes that a list would actually obtain from less educated voters. The coarseness is due to the fact that only votes aggregated to the municipal level, and not individual votes, are observed. P_l^{votes} is zero for a list that obtains all its votes from municipalities where all adults completed primary schooling, and equal to one for a list that obtained all its votes from municipalities where no citizen completed it.

Finding that electronic voting raises the vote shares of parties with higher P_l^{votes} is a distinct exercise from the previously discussed result that the effects on valid voting are higher in less educated municipalities. Firstly, increased valid voting from the less educated does not necessarily imply higher vote shares for parties popular amongst them. Additionally, the at-large nature of the elections imply that, on average, the choice of lists (and their popularity measure) are the same for municipalities just above and below the threshold. Hence, there is no issue of "mechanically" driven results, even though the popularity measure is calculated using voting outcomes from the election where the discontinuity is present.

Using data from the official registration of candidates to the electoral authority, I also computed the share of candidates in a list that did not complete primary schooling, denoting it by P_l^{cand}. This is, hence, a measure capturing similar identity between voters and candidates.

To measure how the effects of electronic voting differ across lists with different popularity measures, I estimate the following regression, where a observation is a municipality and list *pair*:

$$\frac{votes_{lm}}{turnout_m} = \lambda 1\{v_m > c\}_m + \psi P_l \cdot 1\{v_m > c\}_m + \phi P_l^{cand} \cdot 1\{v_m > c\}_m + f_l(v_m) + \epsilon_{lm} \tag{4}$$

As before, $1\{v_m > c\}$ indicates if municipality m is above the cutoff ($c \equiv 40,500$) and used electronic voting. The specification includes a list-specific polynomial $f_l(\cdot)$ which is interacted with the indicator for municipalities above the threshold (i.e., it fits a separate polynomial on each side of the threshold for every individual list). Hence $f_l(\cdot)$ includes a list-specific set of constants, which can be interpreted as list fixed effects that absorb the effect all list-specific variables (e.g., ideology) that are common among municipalities.

The main parameters of interest are ψ and ϕ. They estimate the impact of the two measures of popularity on electronic voting's treatment effect: positive values imply that the effect is larger for party lists that obtain higher shares of votes from less educated municipalities or have more candidates without primary schooling.

The results from the estimation of equation (4) using a linear specification and 20,000-voter bandwidth are reported on Table 3. Similar results were obtained with a 10,000 bandwidth. Column (1) first shows the results without the interaction between electronic voting and the popularity measures. It shows that electronic voting increased the vote share of a list by an average of 0.6 p.p. of total turnout. Since the average list obtains votes equalling 3.6% of total turnout in municipalities using the paper ballot, electronic voting generates a 16% increase in vote shares (a similar number is implied by Table 1 estimates).

Column (2) and (3) add, sequentially, the interactions. The (statistically significant) positive coefficient on both interactions indicate that a list that receive a large share of its votes from less educated municipalities and has less educated candidates receives relatively higher number of votes due to electronic voting. As a robustness check, column (4) included both variables interactions simultaneously and obtains similar results.

Estimates of column (2) imply that the effect of electronic voting on vote shares is 0.55 p.p. for a list in the 1st decile of the P_l^{votes} distribution (0.63), and 0.70 p.p. for one in the 9th decile (0.81). For the case of P_l^{votes}, which has a distribution with 1st (9th) decile at zero (0.21), the respective impacts implied by column (3) are 0.57 p.p. and 0.73 p.p..

When municipality specific indicators could also be added, the results remain the same. This is somewhat expected because these dummies would control for municipality-specific characteristics which are on average the same on municipalities on both sides of the threshold, given the regression discontinuity design.

In summary, this section provides evidence that electronic voting 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 parties that are popular amongst the less educated and have less educated candidates. This combination of results makes the case that the introduction of electronic voting promoted the *de facto* enfranchisement of mostly low educated citizens.

The consequences of this enfranchisement on public policy, services provision and infant health outcomes is studied in the next section.

3 Political Participation, Health Care Provision, and Infant Health

While the 1998 election presented a regression discontinuity design at the municipal level (municipalities above and below the cutoff used different voting technologies), only officials at state (and federal) governments were selected through it. Hence, the state becomes the relevant unit of observation to study the effects of electronic voting on policy outcomes.

This section uses state-level panel data on election outcomes, legislative representation, fiscal spending, health care utilization and newborn health to analyze the effects of electronic voting adoption. As in Section 2, this section provides the relevant background, a description of the data and estimation framework, and the results, in three separate subsections.

3.1 Background and Data

3.1.1 The Brazilian Health Care System

The Brazilian Constitution mandates the provision of universal health care, which is achieved by a nationally coordinated public system (the *Sistema Único de Saúde*) almost entirely funded by government tax revenues. Citizens are not required to pay health insurance premiums and have out-of-pocket expenses when using the public system. The administration of public health care is fairly decentralized, with state governments being responsible for almost 30% of all expenditures on the system and having significant control over the allocation of federal funds.[33]

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

There is also a parallel private system of health insurance, hospitals and clinics that requires users to either pay per-service fees or private insurance premiums. Given the zero cost of using the public system, private health care exists by providing higher quality of services to those willing to pay its additional price, which tend to be the more educated (and richer). Figure 5 shows the proportion of Brazilians that use private health care system by education group: while only 17% of those with less than 4 years of schooling use private health care, the vast majority of college graduates do it, and the share rises monotonically between those groups.[34] Since users of the private system have less at stake regarding the quality of the public system, differences in the preferences for government spending should arise between educational groups: the less educated should value expenditures much more than those with more schooling.

When these differing preferences are present, economic theories of voter influence on policymaking predict that increased political participation (or enfranchisement) of the less educated, such as the one generated by electronic voting, should raise government spending on health care. The evidence provided in this section tests, and confirms, this prediction. To shed some light on the political mechanisms driving this results, it also demonstrates that the enfranchisement of less educated voters leads to higher numbers of state legislators that are themselves less educated. While this change in legislator characteristics can explain the effects of electronic voting on health care spending, it must be noted that other complementary channels, such as changes in behavior by educated and/or incumbent legislators, may also play a role.[35]

There are three additional reasons to focus on health care provision, instead of other policies that this enfranchisement episode could presumably affect. Firstly, impacts on health

[34] The data is from the nationally representative Latinobarometro survey (1998 Wave), which asked respondents "*How do you cover your health care costs?*" and allowed two possible answers ("*With Public Insurance*" and with "*With Private Insurance*").

[35] The canonical median-voter model (Downs, 1957) and its multiparty variants (Shepsle, 1991) 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 close to those newly enfranchised. These approaches are complementary and quantifying their distinct contributions is outside the scope of a paper analyzing final policy outcomes such as this one (as opposed to work on individual politician behavior such as Lee et al., 2003; and Mian et al., forthcoming).

care funding are feasible, since the provision of health services is within reach of individual legislators. Secondly, health care funding can be affected in a short span of time, and thirdly, so can its effects on newborn health. This is particularly useful given that the identification strategy, described in the next section, relies on the timing of electronic voting adoption.

A large portion of state legislature politics revolves around health care. A survey of the São Paulo state legislature, for example, shows that 34% of its members cite health care as their "area of expertise", the modal answer.[36] State legislators can generate additional health care funding by either amending the budget or making official requests to the executive (the governor).

It is common for budget amendments to deal with the Family Health Program (*Programa Saúde da Família*), which allocates a team of at least six health professionals (lead by a doctor) to provide general practice services for a community of no more than a 1,000 families (3,000-4,500 people). The relatively low per-team cost (less than yearly US$ 50 per covered individual) 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.[37]

Another source of additional health care funding can be direct requests which can increase health care funding within a short time frame. Take the case of a request made by Edson Pimenta, a member of the Bahia state legislature. Mr. Pimenta filed an official request to allocate two Family Health Program teams (and provide a small clinic for them) in the municipality of Cansanção on February 2004. By July of the same year the request was approved by both the state legislature and and its governor, with the order being sent to "execution" on that month. The clinic started providing services on November 2004, less than 10 months after the initial request. Mr. Pimenta's case also exemplifies the other results in this paper, as he does not have a primary school degree and was first elected in 2002, the year that more than half of Bahia's electorate used electronic voting in state elections.[38]

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

[37]Ames (2001) argues that targeting government expenditures at its voter bases through budget amendments is the main activity of Brazilian legislators, and provides further details on state legislative process.

[38]This request is the *Indicação 13.872/2004*, which can be tracked in the Bahia state legislature official website (www.al.ba.gov.br).

Similarly, Ferraz and Finan (2010) also find that legislators can affect the provision of health care quickly, albeit in the case of Brazilian municipal (instead of state) legislatures. Exploiting another regression discontinuity design, they find that an increase in legislator wages that took office in (January of) 2005 leads to more health clinics and doctors by 2006.

3.1.2 Newborn Health Status

Low birth-weight (defined by the World Health Organization as below 2500g) is arguably the most common indicator of (poor) newborn health, being used by literally thousands of studies in clinical, epidemiological and economic research. It is also considered the most important factor influencing neonatal mortality and early-life health.

While infant health is an important goal of public policy and research interest in itself, there is also considerable evidence that birth outcomes affect a wide range of adult outcomes such as mortality, disability, human capital accumulation and labor market productivity. Birth-weight was the variable used by Barker (1995) in his formulation of the fetal origins hypothesis, which has been frequently studied (and corroborated) by economic research, and within-twin pair comparisons find that low birth weight predicts negative adult outcomes (Behrman and Rosenzweig, 2004; Black et al., 2007; Oreopoulos et al., 2008).[39]

The medical literature indicates that pre-natal visits (i.e., a pregnant woman interaction with a doctor or other qualified health professional) is an important determinant of birth weight, mainly by positively affecting maternal behavior such as nutrition (e.g., monitoring if fetal growth and maternal weight gain are appropriate) and avoidance of risk factors (e.g., informing mothers on the risks of alcohol, caffeine, and nicotine intake, suggesting bed rest when appropriate). Common infections that are easily treatable by medical professionals are also known to lead to premature birth and low birth-weight.[40]

3.1.3 State Level Panel Data

The estimation reported in the following sections use a yearly panel dataset covering all Brazilian during the 1994-2006 period that I constructed using data from several sources.

[39]The economics literature on the consequences of infant (and *in utero*) health on adult outcomes includes Almond (2006), Case, Fertig and Paxson (2005); Currie and Moretti (2007); Duflo et al. (forthcoming) and Meng and Qian (2006).

[40]Kramer (1987) provides a comprehensive survey of the medical literature on birth-weight determinants.

Electoral outcomes and individual characteristics of state legislators elected in the 1994-2006 elections are from the electoral authority, the same data source described on Subsection 2.2. The two variables used in the estimations are the valid vote to turnout ratio and the share of elected legislators that did not complete primary education, which are referred as "uneducated" legislators.

State governments' expenditures on health care (and other fiscal data) were obtained from the National Secretary of Treasury, a branch of the federal Ministry of Finance (*Secretaria do Tesouro Nacional - Ministério da Fazenda*) that collects comparable accounting information for the states' budget execution. Our main variable of interest is the monetary amount spent (not just budgeted) in a fiscal year by a state that is categorized as "health care and sanitation." Given that sanitation is a relatively small fraction of expenditures, I refer to them simply as "health care spending" hereafter.[41]

The measures of health care utilization and outcomes are from the National System of Information on Live Births (*Sistema Nacional de Informações de Nascidos Vivos*), a vital statistics database kept by the federal Ministry of Health containing data from birth records collected from medical and official registries (such as birth certificates). A positive aspect of this dataset is its near universal coverage of Brazilian births.[42]

I use three particular variables from the birth records to generate outcomes of interest: mother's education, number of pre-natal visits, and the birth-weight 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 pre-natal visits and the share of births that are low-weight (below 2500g). Seven is the minimal amount of doctor visits that a gestating women following the recommended schedule from the Brazilian Society of Gynecology and Obstetrics[43] would have, and is a common indicator for adequate pre-natal care. The data also allows to compute the number of mothers with zero and less than six pre-natal visits, but these variables are used for additional results reported on Appendix D.

[41] Sanitation is mainly supplied by companies which, when public, are not considered as part of a state's budget.

[42] The Ministry of Health cross-checks the number of recorded births with nationally representative surveys that elicit the fertility history of respondents, validating its coverage. Following international standard definitions, a live birth occurs when an infant shows any life sign (e.g., breathing, heartbeats) at the time of full extraction from its mother's body.

[43] The same is the case for the guideleines of the American Congress of Obstetricians and Gynecologists.

Pre-natal visiting and low birth-weight are computed separately for mothers that completed, or did not, primary schooling, which is the lowest level of education discriminated in the data. Having these two separate samples allows a placebo test based on the finding that electronic voting only impacted pre-natal visits and birth weight for mothers without primary schooling. 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+ pre-natal visits, and 6.2% of their births are low-weight. The similar figure for uneducated mothers are 33% and 7.7%.[44]

For both levels of education, a negligible share (less than 1%) of births do not record the newborn's weight, making under-reporting a non-issue. The number of pre-natal visits is ignored in 7.8% of births (the figure is virtually the same for both education groups), and Appendix D discusses how the results are robust to accounting for ignored pre-natal visiting.

Finally, I obtain two additional covariates, real GDP and population, from the Brazilian Statistics Agency (*Instituto Brasileiro de Geografia e Estatística*). The relevant sample moments are provided in the discussion and on the tables reporting estimates, deferring the summary statistics to Appendix A.

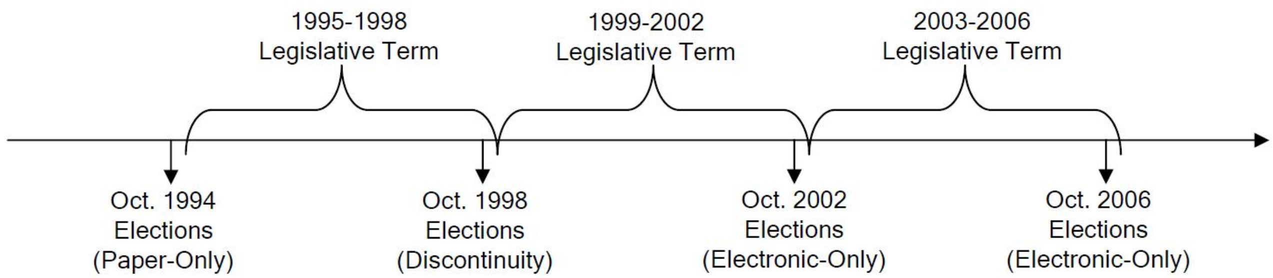
3.2 Identification Strategy and Estimation Framework

The identification strategy exploits the pattern of electronic voting adoption across states and time. As discussed in Subsection 2.2, in the 1994 elections only paper ballots were used, while in the 1998 election only municipalities with more than 40,500 registered voters used electronic voting, and in 2002 only the new technology was used. To simplify referencing, denote the 1994, 1998 and 2002 elections as the "paper-only", "discontinuity" and "electronic-only" elections, respectively. Figure 5 presents this timeline graphically.

Figure 5 also depicts the timing of different **legislative terms**, which are defined as the four-year periods where 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.

[44] The average state has about 42 thousand live births by educated mothers and 58 thousand by uneducated ones.

Figure 5: Timeline of Elections and Legislative Terms



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.

The main estimates involve the three quadrennial legislative terms on Figure 5:

- The 1995-1998 legislative term, elected in the 1994 "paper-only" election.
- The 1999-2002 legislative term, elected in the 1998 "discontinuity" election.
- The 2003-2006 legislative term, elected in the 2002 "paper-only" election.[45]

Let S_i denote the share of voters in state i that reside in municipalities above the cutoff for voting technology use in the 1998 election. Note that S_i does **not** vary through time. Since 1994 was a "paper-only" election, S_i equals the share of voters in state i that changed from using paper ballots to electronic voting between the 1994 and 1998 elections. On the other hand, since the 2002 election was "electronic-only", it is also the case that the change in the share of voters using electronic voting between 1998 and 2002 elections equals $1 - S_i$.

Formally, let $electro_{ie}$ denote the share of voters using electronic voting in state i at an election held at year e, then:

$$electro_{i1998} - electro_{i1994} = S_i \tag{5}$$

$$electro_{i2002} - electro_{i1998} = 1 - S_i \tag{6}$$

[45]Note that all legislative terms start on January 1st of its first year and end on December 31st of its last year (e.g., the 1995-1998 term covers Jan 1st, 1995 - Dec. 31st, 1998).

This implies that a time-invariant cross-sectional variable, S_i, is **positively** related to **changes** in electronic voting 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, electronic voting is the likely driving cause behind it, since there is little reason to believe that an omitted variable would follow this same pattern, with the same argument applying to measurement error.

To illustrate the argument, take the example of a specific outcome of interest: expenditures on health care. Denote H_{ie} as health care spending by the legislature selected at election year e at state i (e.g., H_{i1998} is the spending for the 1999-2002 legislative terms). 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. Recall from equations (5) and (6) that $(electro_{i1998} - electro_{i1994}) = S_i$ and $(electro_{i2002} - electro_{i1998}) = (1 - S_i)$. Hence, in the period where S_i is proportional to larger changes in electronic adoption, it predicts larger changes in health care spending. In the period where the same cross-sectional variable S_i is inversely proportional to electronic voting adoption, it predicts smaller changes in spending.[46]

Apart from the the causal effect of electronic voting adoption, it is difficult to see any reason (e.g., an omitted variable) why health care spending could behave the pattern described above. Note also that mean reversion (or states "catching up" with the others) also cannot explain this pattern. Firstly, mean reversion cannot explain why a cross-sectional variable (S_i) has predictive power over the changes. Secondly, the fact that the schedule of elections matches the timing of the sign change in the correlation between the share of voters above the cutoff (S_i) and spending growth wouldn't be implied by mean reversion.

To probe the identification strategy described above, the following sections provide two sets of falsification tests. The first one finds that, in periods where S_i is not associated with changes voting technology, it is also not associated with changes in outcomes. This

[46]In other words, the share of voters above the cutoff in 1998 (S_i) **positively predicts the change** in spending between the 1995-1998 and 1999-2002 legislative terms when S_i equals the change in electronic voting adoption between the elections that selected these two groups of legislatures (i.e., the 1994 and 1998 elections). On the other hand, S_i **negatively predicts the change** in spending between the 1999-2002 and the 2003-2006 legislative terms, and in this case $1 - S_i$ equals the change in electronic voting adoption between the elections that selected these legislatures (held in 1998 and 2002, respectively).

rules out the role of pre-existing trends and reinforces the distinction between the results and mean reversion. The second one involves demonstrating that observable covariates (e.g., population, income) do not follow the pattern found in outcomes.

While the discussion above was based on quadrennial legislative terms, the estimations are carried out using state-level data with a yearly frequency. Let y_{it} denote an outcome for state i at year t. The econometric implementation of the concept described above is the following fixed effects estimation for outcome y_{it}:

$$y_{it} = \alpha + \theta^{99-02} S_i \cdot Term_t^{99-02} + \gamma_i + \delta_t + \epsilon_{it} \ ; \ t \in [1995,\ 2002] \tag{7}$$

$$y_{it} = \alpha + \theta^{03-06} S_i \cdot Term_t^{03-06} + \gamma_i + \delta_t + \epsilon_{it} \ ; \ t \in [1999,\ 2006] \tag{8}$$

where $Term_t^{99-02}$ is a dummy indicator for years belonging to legislative term 1999-2002 (elected in the 1998 "discontinuity" election), and $Term_t^{03-06}$ is a dummy for years belonging to the legislative 2003-2006 (elected in the 2002 "electronic-only" elections). Year (δ_t) and state fixed (γ_i) effects are included, capturing the effect of any unobserved state-specific time-invariant variable and of national-level factors that vary through time.

The parameter θ^{99-02} measures the effect of S_i on the change in average y_{it} between the 1995-1998 and 1999-2002 legislative terms. Equation (5) indicates that S_i equals the change in the electronic use in the elections that selected these legislatures (1994 and 1998, respectively). Hence, θ^{99-02} is an estimate of the impact of electronic voting on y_{it}.

On the other hand, equation (6) implies that θ^{03-06} measures the effect of the **opposite** of electronic voting adoption on y_{it}. The change in electronic voting adoption between the elections that selected the 1999-2002 and the 2003-2006 legislatures (1998 and 2002, respectively) equals to $(1 - S_i)$. Since θ^{03-06} measures the effect of S_i on the change between these two legislature, $-\theta^{03-06}$ is the effect of electronic voting on the outcome y_{it}.[47]

Hence, evidence that $\theta^{99-02} = -\theta^{03-06} \equiv \theta$ makes the case that θ measures the causal effect of electronic voting. Throughout the analysis, I estimate equation (7) and (8) independently from each other, so that no particular relationship between the parameters θ^{99-02} and θ^{03-06} is "forced" by the estimation. In the cases where the results imply that

[47] In other words, once fixed effects and time effects are included, equation (5) implies that $S_i \cdot Term_t^{99-02}$ is collinear with electronic voting adoption in equation (7), while equation (6) implies that $S_i \cdot Term_t^{03-06}$ is negatively collinear with electronic voting adoption in equation (8).

$\theta^{99-02} = -\theta^{03-06}$ holds, equations (7) and (8) are estimated jointly with this restriction imposed on the parameters.

The discussion so far applies to the 23 states that followed the discontinuous assignment rule in the 1998 election. As discussed in Section 2.1, four other states implemented electronic voting in all its municipalities in that year. I include them in the sample as having $S_i = 1$, so that the equivalence between changes in electronic voting adoption and S_i on equations (5)-(8) remains. Henceforth, I refer to "share of electorate above the threshold" and S_i interchangeably, with both indicating a variable that equals one for these four states.

Hence estimation of (7) and (8) 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. Given that the four states did not comply to the rule for distinct reasons (discussed on Subsection 2.1), comparing the results from specifications that exclude them with that from the full sample creates a robustness check for the estimates.

Standard errors are clustered at the state level, allowing for arbitrary serial correlation. Note that estimation of (7) and (8) yield mathematically identical point estimates to what would be obtained by collapsing the data into quadrennial legislative term averages (as long as controls with yearly variation are not added), and clustering at the state level implies the same holds for standard errors.

The geographical distribution of the share of voters living in municipality above the 40,500-voter threshold (S_i) is depicted on the map in Figure 6. The variable has a wide range, from 0.147 to 1, with a mean of 0.52 and a standard deviation of 0.26. Since some degree of geographical correlation may be inferred from Figure 6, all estimations control for a set of region dummies interacted with time effects, which may absorb geographically restricted shocks affecting the outcomes of interest.[48]

[48] Brazilian states are grouped into 5 regions (South, Southeast, Northeast, North and Center-West), based on their natural, demographic and economic characteristics. States within a region are contiguous. The graphical plots throughout Subsection 3.3 are based on the raw data and hence do not control for regional dummies - the usually negligible differences between the graphic slopes and its estimated counterparts indicate the robustness of results to the inclusion of regional controls.

3.3 State-level Results

3.3.1 Electronic Voting, Legislator Education, and Health Care Spending

First, the empirical strategy is exemplified on an outcome that was found to be affected by electronic voting on Section 2: the ratio between valid votes and turnout. The graphical representation of empirical strategy can be found in Figure 7, where the panel on the left plots the share of voters living in municipalities above the cutoff on the x-axis and the **differences** in the 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 electronic voting adoption in this period. On the right panel, 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-only elections). Since in this case the share of voters living in municipalities above the cutoff is **negatively** associated with electronic voting adoption, the relationship changes sign, as expected.

The estimated counterparts of slopes of Figure 7 are obtained by estimating equations (7) and (8) with the valid vote ratio as the dependent variable. Table 4 reports then as a statistically significant 0.108 and -0.112, respectively. They imply that the effect from having all of a state's population using paper ballots switch to using electronic voting is of 11 p.p., which is remarkably close to the RDD estimates of Section 2.

It is very difficult to explain this "see-saw" pattern with a channel other than the causal effect of electronic voting. That would require a time varying omitted variable which (without loss of generality) positively affects the outcome to grow faster in states with high S_i in the 1994-1998 period and sharply changes its time pattern to growing slower in high-S_i states with 1998-2002 period. Such omitted variable is not only unlikely *a priori*, but the next subsection provides some evidence against its existence.

The same graphical exercise is repeated for the share of elected legislators with incomplete primary schooling on Figure 8, which again shows a see-saw pattern with slopes estimated to be 0.046 and -0.047 (Table 4). Following the same argument used for the valid vote ratio, this provides evidence that a full switch to electronic voting caused 4.6 p.p. of state legislature seats to be held by uneducated legislators.

Figure 9 provides the same exercise for the logarithm of inflation-adjusted spending in health care per capita. The variable is observed at an yearly frequency, so Figure 9, Panel A, plots in its y-axis the difference between the average yearly log-spending in the 1995-1998 and 1999-2002 legislative terms (i.e., differences in spending by governments elected in the "paper-only" and the "discontinuity" elections). As the preceding figures, Panel B repeats the same exercise for the 1999-2002 and 2003-2006 legislative terms, elected by the "discontinuity" and "electronic-only" elections.

Figure 9 shows that the same "see-saw" pattern of electronic voting adoption is also observed by health care spending, suggesting a causal link between the two variables and supporting the hypothesis that electronic voting and its "enfranchisement" of less educated voters shifted government spending towards a policy that disproportionately benefits this population. The estimated counterparts of the slopes are reported on Table 4, indicating that a full switch from using only paper ballots to complete electronic voting generates an increase of about 50% in health care spending over the course of two legislative terms (8 years). The next section first checks the robustness and precision of this estimate, and then discusses its magnitude and interpretation.

3.3.2 Robustness and Falsification Tests

The identification strategy is predicated on the the notion that the time and space pattern of electronic adoption is so unusual that no omitted variable would follow it. While it is not possible to directly test if this is the case for unobservable factors, it is possible to show that some observable ones do not follow the pattern, hence increasing confidence that the same holds for unobservable ones. This exercise has a similar nature to that of reporting insignificant differences in average covariates between control and treatment groups in randomized controlled trials, or that covariates evolve smoothly around the cutoff of a regression discontinuity design (as performed in Section 2).

I show that two variables that are not expected to be influenced by electronic voting, state-level GDP per capita and population, do not follow the "see-saw" pattern found in the outcomes. Panel B of Table 4 reports that income growth is negatively correlated with

S_i in both periods, while population growth is always positively related to it. Moreover, the results are statistically insignificant, so the possibility that income and population are unrelated to S_i cannot be rejected. These results not only imply that the results of Panel A cannot be explained by income and population growth, but also serve as an example that possible determinants of health care spending do not follow its pattern through time. Results are similar for the case of total fiscal spending, implying that electronic voting does not affect the absolute size of government.[49]

Another falsification test involves testing if S_i predicts changes in outcomes during periods where the voting technology does not change. If those are found to be negligible, it increases confidence that the case the estimates in columns (1) or (2) of Table 4 are not capturing some omitted trend that is correlated with S_i.

Column (4) of Table 4, Panel A, presents the effect of S_i on changes between the 2002 and 2006 election, in which only electronic voting was used. The estimated effect on valid votes and the share of legislators is virtually zero, indicating that no spurious trend, other than changes in voting technology, is driving the results in columns (1) and (2).

Similarly, estimating the correlation between S_i and health care spending growth between two terms elected under paper ballots yields a substantially smaller estimate (a statistically insignificant -0.13) than that found in the periods where voting technology changes.[50]

To enhance the precision of the estimated treatment effects, I pool the variation from both equations (7) and (8) and estimate:

$$y_{it} = \alpha + \theta S_i \cdot Term_t^{99-02} + \gamma_i + \delta_t + \beta X_{it} + u_{it} \; ; t \in [1995, 2006] \tag{9}$$

where $Term_t^{99-02}$ is a dummy indicating if year t is part of the 1999-2002 legislative term (1999-2002). Since $S_i \cdot Term_t^{99-02}$ is perfectly collinear electronic voting adoption when

[49] The enfranchisement of less educated (and hence poorer) voters could increase the size of government through increased redistribution, as Husted and Kenny (1997) found in the case of US states. Brazilian state governments however, have less ability to engage in redistribution, since most income transfers programs such as social security and conditional cash transfers are determined at the federal level (with their operation often decentralized to the municipal level). Moreover, states' main source of tax revenue is a value-added tax on goods and services, which does not lend itself as easily to redistribution, unlike income tax (which is set and collected by the federal government).

[50] Detailed election results (such as legislator education) are not available for the 1990 election, and (at the time this manuscript was written) data on the expenditures of 2007-2010 legislative term were not available. Hence the use of separate periods to perform the placebo tests. The birth records data is available only for the the 1994-2002 legislative terms, and also do not allow this falsification test.

fixed and time effects are present, θ captures the effect of electronic voting on y. Moreover, estimation of (9) is equivalent to estimating equations (7) and (8) jointly under the restriction that $\theta^{99-03} = -\theta^{03-06}$. A set of additional controls (X_{it}) is also added.

The results are presented on Table 5, in which column (1) shows the estimate of θ for different outcomes. Similarly to what was found on Section 2.3, it indicates that a state that changes from using only paper ballots to complete adoption of electronic voting experiences 11 p.p. additional valid votes (as ratio of turnout), 4.4 p.p increase in the share of state legislators without primary schooling, and 55% more health care spending. It also presents results when health care spending is measured as a share of the total government spending, with results indicating that it leads to 3.7 p.p. of the budget being redirected to it. Pooling the variation from all periods lead to higher precision (smaller standard errors), with most estimates being significant at the 1% level.

Columns (2) and (3) check the robustness of the results to the inclusion of additional controls such as population and GDP, and also set of state-specific trends, capturing unobserved state differences that evolve constantly through time. Their inclusion leads to negligible differences in the estimates, for all outcomes of interest.

A final robustness check is provided by column (4), which excludes from the sample the four states that did not participate in the discontinuous assignment in 1998, using electronic voting in all its municipalities in that year. The same qualitative results are found, indicating that different sources of variation in voting technology adoption leads to similar results. This result can also be seen in the graphical analyses (Figures 7, 8, and 9), where the estimated slopes are usually similar when the states with the maximum value of S_i are dropped.[51]

3.3.3 Discussion of Results: Health Care Funding

The results above shows that the adoption of electronic voting in a state generates 11 p.p. more valid votes (as a ratio of turnout), 4.3 p.p. higher share of elected legislators without primary schooling and a redirection of 3.7 p.p. of its budget towards health care spending, the equivalent of a 55% percent increase.

This picture is consistent with the notion that electronic voting enfranchised less educated

[51] One state - Distrito Federal - is comprised of a single municipality (Brasília, the federal capital) which is above the threshold.

voters which were casting residual votes with paper ballots, and their enhanced political participation lead to more legislators with similar characteristics, and also increased funding for a public service disproportionately used by them. It does not necessarily imply, however, that the additional 4.3 p.p. of uneducated legislator is the sole cause behind the redirection of 3.7 p.p. of states' budget to health care. The state-level data does not allow to separately quantify the role of educated (and possibly incumbent) legislators changing their behavior in response to the political changes generated by electronic voting.

The average state in the sample has an S_i of about 0.5, so that it experiences half of its electorate changing from paper ballot to electronic voting in two consecutive legislative terms. Hence, the estimates should be interpreted as the effect accruing over an 8-year period, and not as a one-shot immediate effect: the average state redirects 1.8% of its budget to health care in a legislative term, and then another 1.8% in the next 4-year period.

While, at first glance, this increase in health care spending might seem large, it must be put into the perspective of a developing country with low levels of health expenditures, which accounted for only 7.5% of state government budgets in 1998, the last year before the first legislature elected under some electronic voting took office. This figure grew to 13.2% in 2006, the last year of government by the legislature elected in the first all-electronic election. The estimated 3.7 p.p. increase in share of spending on health care can thus account for 64% of health care budget share growth in the period.[52]

While the yearly state-level fiscal data does not detail the nature of the additional health care spending, some light on this issue may be shed with more aggregate variables. As discussed in Section 3.1, a natural candidate would be the Family Health Program. Coverage of the Family Health Program grew indeed very quickly in the period after the introduction of electronic voting. It's number of of registered users more than doubled between 1999 and 2002, going from 39 to 86 million users. By 2006, it reached 108 million people, or 56% of the Brazilian population.

One of the stated goals of the family health program is allowing its users to have easier

[52]Large political responsiveness of health care spending has been found in a country at a similar level of development of current Brazil: 1900-1930 United States. Miller (2008) reports that US states observed a 36% increase in health-related spending on the immediate year following the enaction of women's suffrage, with growing effects leading to a 81% increase after 5 years.

access to basic services that would otherwise only be available at hospital and clinics. One such service is doctor visits, that can be done at the community health outpost or with house calls. 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 8 years following the first use of electronic voting in state elections. Moreover, the improved pre-natal care provided by the Family Health Program has been associated with reduced mortality (Rocha and Soares, 2010).

While the narrative above suggests that electronic voting 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 too incomplete to allow for an econometric quantification. However, the political salience, large size, and fast growth of a program known to improve infant health imply that the effects of this additional funding could be found in population-level measures of health care use and outcomes. The next section reports such findings, using birth records data on pre-natal care and birth-weight.

3.3.4 Effects on Health Care Utilization and Outcomes

I estimate the effect of electronic voting on health care utilization (share of pregnant women with more than seven pre-natal visits) and health outcomes (low birth-weight) with a similar strategy used for health care spending. The main difference is that the vital statistics data allows observation of these two variables separately for mothers that completed primary schooling and those that did not. Recall that I refer to these groups as "educated" and "uneducated", respectively.

Figure 10 shows that, in the case of uneducated mothers, health care utilization tracks the same "see-saw" pattern followed by electronic voting adoption and health care spending. States with higher share of voters living above the 40,500-voter cutoff (S_i) is associated with larger increases in the share of mothers that had seven or more pre-natal visits between the 1995-1998 legislative term and the 1999-2002 legislative term. On the other hand, S_i is negatively associated with the differences between the 1999-2002 and 2003-2006 legislative terms.

Figure 11 shows that the growth in share of newborns from uneducated mothers that present low birth-weight follow an "inverse see-saw" pattern, being first negatively correlated with S_i between the 1995-1998 and 1999-2002 legislatives, but positively correlated in the following period.

The estimated counterparts of Figure 10 are presented on the first line of Panel A on Table 6. S_i is positively associated with difference in pre-natal visits by uneducated mothers between the 1995-1999 and 1999-2002 legislative terms, but negatively associated with changes between the 1999-2002 and 2003-2006 terms, although with a slightly smaller magnitude. The "inverse see-saw" pattern also holds for the share of low birth-weights by uneducated.

Following the argument made for residual voting, election of uneducated legislators and health care spending, Panel A of Table 6 indicates a causal link between voting technology and pre-natal visits and birth outcomes. To invalidate this interpretation, an omitted variable that varies through time in the same unusual pattern of electronic voting adoption would be required.

To provide both a placebo test and further the interpretation that this causal link is the result of increased policy influence by uneducated voters, I test for the presence of a see-saw pattern in the outcomes of educated mothers, and find no evidence supporting it. Panel B of Table 2 shows that the "see-saw" in the number of visits by educated mothers move in the opposite direction, and with a much smaller magnitude than that observed for educated mothers. Low birth-weight presents no change in the correlation of its growth and S_i between periods.

The estimates on Table 6 are relatively imprecisely estimated, with large standard errors leading to low statistical significance. However, the clear "see-saw" pattern for the case of uneducated mothers indicates the appropriateness of estimating the effect of electronic voting with equation (9), which pools data from both periods and generates more precise and statistically significant results, reported on Table 7.

The estimates on column (1) of Panel A imply that a change from using only paper ballots to complete adoption of electronic voting leads to 7 p.p. higher share of uneducated mothers having seven or more pre-natal visits, and a 0.43 p.p. reduction on the probability of a low-weight birth in the same group, with both estimates being significant at the 5% level.

In comparison, column (1) of Panel B indicates that for the case of educated mothers, the effects are 7 times smaller than the counterpart for uneducated ones, being close to zero both in magnitude and in statistical significance. This difference in effects indicates that only uneducated mothers benefited from electronic voting. Coupled with the results that the new technology also *de facto* enfranchised mostly uneducated voters and generated votes to parties popular amongst them (Section 2), this result reinforces the interpretation that electronic voting increased the policy influence of the uneducated.

The results for uneducated mother imply that a full switch to electronic voting generates a 20% increase over average sample values in pre-natal visiting, and a 5.6% reduction in the prevalence of birth-weight. The implied elasticities (at sample mean levels) of low birth-weight share with respect health care spending and pre-natal visiting are thus -0.10 and -0.28. Appendix D shows that the increased number of pre-natal visits happened mostly at the intensive margin: all the effect in the share of women reporting 7+ pre-natal visits is accounted by a reduction in the share having between one and six visits, while the effects on those with no visits are zero. This is consistent with additional health care expenditures reducing the cost of visits to those that already had some access to health care, which is one of the stated goals of the Family Health Program (e.g., having family doctors provide services that would require a visit to a hospital).

Columns (2) and (3) of Table 7 provide robustness checks for the estimates of column (1) by adding additional controls (population and income) and state-specific linear trends to the estimation. The result of economic and statistically significant impacts for uneducated mothers, and virtually no effects for educated mothers, remain.

Another robustness check is provided by column (4), which pools both education groups in the same sample and estimates:

$$y_{eit} = \alpha + \gamma S_i \cdot Term_t^{99-02} \cdot \ Lowed_e + \lambda_{it} + \lambda_{et} + \lambda_{ei} + \epsilon_{eit} \tag{10}$$

where y_{eit} is the outcome of interest in mothers of education group e on state i at year t. The variation across these three dimensions allows to control for education-state, education-year, and state-year fixed effects. Hence the interaction of $S_i \cdot Term_t^{99-02}$ with an indicator for low education group $Lowed_e$ captures the difference between the effect of electronic voting on uneducated and educated mothers, controlling for any omitted variable that varies arbitrarily across states and years, but that has similar effects on mothers of different schooling.[53] Column (4) of Table 7 and reports the estimated γ, and find that qualitative results remain. The magnitude of the effect on visits become slightly smaller, and on birth-weight slightly larger.

As a final check, column (5) reports estimates excluding the four states that did not follow the discontinuous assignment rule from the sample. The changes in estimated coefficients and significance are small, suggesting that, like in the case of other variables studied on Subsection 3.3.2, different sources of variation in electronic voting adoption lead to similar estimates.

4 Conclusion

This paper quantifies the effects of voting technology in Brazilian state legislature elections, demonstrating that, by facilitating ballot operation, it *de facto* enfranchised a significant number of (mostly less educated) voters. This enfranchisement is then shown to have raised the number of elected legislators that are themselves less educated and increased fiscal spending in an area that particularly benefits less educated voters: public health care services. Such additional expenditures had measurable effects on utilization and outcomes, increasing the number of pre-natal visits and reducing the occurrence of low-weight births.

The estimated magnitudes of the effects is that a full switch from paper ballots to electronic voting in Brazil over a 12 year period *caused* i) an increase of 11 p.p. in the share of the electorate casting valid votes; ii) the share of less educated state legislators to grow by 4.4 p.p.; iii) 3.7 p.p. of the state budget to be relocated to health care spending (increasing it by more than 50%); iv) the extension of adequate pre-natal visiting to 7% of less educated mothers, with (v) a 0.43 p.p. reduction in their probability of giving birth to a low-weight infant.

While the estimated effects may be dependent to the Brazilian context and not carry over

[53]Without the inclusion of state-year effects, the estimated effect would be the same and the difference between the estimated effect from equation (11) reported on columns (1)-(3) in uneducated and educated mothers.

to other countries, the more general lesson is that isolating and removing a mundane obstacle to voting can have substantial effects on public service provision and outcomes. This suggests that analyses of the consequences of "bottom-up" fostering of political participation, such as reducing the cost of registration or "get-out-the-vote" campaigns, could be a fruitful avenue for future research, which would well complement the existent studies of more "top-down" interventions such as mandated representation (quotas) for disadvantaged groups.

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Appendix A: Summary Statistics

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

Appendix B: Additional Results on the Effects of Electronic Voting

This appendix provides two sets of additional results regarding the effect of electronic technology on valid voting in state legislature elections. Firstly, the causal effect of electronic voting on residual (invalid) votes is decomposed by its effect on "blank" and "null" voting. Secondly, 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 electronic voting increases valid voting by facilitating ballot operation and diminishing errors in cast votes. The results are hard to conciliate with other interpretation, such as that inattentive citizens are less likely to forget to cast a vote under electronic voting.

A residual (i.e., not valid) vote is classified as either blank (*branco*) or null (*nulo*). When a paper ballot is in use, a vote is considered blank when nothing is written on the ballot, and it is considered as null when the number or name on the ballot cannot be assigned to any candidate. With electronic technology, a null vote is cast when the voter confirms a vote for a number that does not correspond to a candidate, and a blank vote is cast by pressing the "blank" button on the machine.[54]

Panel A of Table A3 presents the estimated treatment effects of electronic voting on blank and null votes separately, with each measure as a share of the total turnout (which equals the sum of valid and invalid votes). The results are estimated with the exact same procedure of those reported on Table 1, as described on Section 2. The estimates indicated that electronic voting reduces null voting by an amount equivalent to 8 p.p. total turnout, with some minor variation depending on specification and bandwidth used. Blank votes, on

[54]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.

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 (notice that a possible reaction of someone that is challenged by the task of filling a ballot may be to leave it blank).

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

The main inference to be drawn from Panel B is that the effect of electronic voting is larger in the proportional representation races where a paper ballot requires writing down the name or number of the candidate (lower chamber of congress and state legislature) than in the plurality races where a paper ballot involves checking a box (senate, governor, and president). Since writing a name/number is presumably more difficult than checking a box, especially for the less educated, the results support the interpretation that the electronic technology facilitates the task of voting.

The effects on valid voting for the lower chamber of federal congress are larger than the ones for the state legislature. Comparison of the "pre-treatment means" on Table A3 indicate that this difference is due to the fact that valid voting in federal elections is smaller than in state elections.[55]

[55] An interesting result is that the results are particularly small (and statistically insignificant) for presidential elections. A possible explanation would be that since the presidential race dominates popular and media attention, voters are particularly better prepared to cast their votes for president, even when using the more error-prone paper ballot.

Appendix C:
Robustness Checks of RDD Estimates

This appendix discusses two possible threats to the validity of the regression discontinuity estimates of the effects of electronic voting 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 play no role in explaining the results.

C.1 Manipulation of the Forcing Variable and Threshold Position

To test the possibility of strategic manipulation, Figure A1 implements an exercise suggested by McCrary (2008) and plots the distribution of number of voters in 1998 (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 A1, 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). It is also expected since the results in Panel C of Table 1 show that municipalities above and below the threshold are similar in several dimensions.

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 electronic voting in a state that had a large number of municipalities just above this cutoff.

This section tests if the threshold was manipulated based on state characteristics. More specifically, it checks if its possible to predict the voting technology used in a municipality (i.e., on which side of threshold it is) with the information on which state it belongs.

The test is performed in two steps. Firstly, I compute the predicted values from a regression of a dummy indicator for electronic vote use (i.e., being to the right of the threshold) against a full set of state of state dummies (using municipal-level data for 1998). Secondly, I test if the relationship between these predicted values and the forcing variable (number of voters) is smooth around the threshold. Figure A2 provides the graphical evidence on this second step: the predicted value evolves continuously around the threshold.

The evidence in Figure A2 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 electronic voting adoption 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 A2 shows that this is the case.[56]

C.2 Other Discontinuity Designs

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 regards municipal elections, while this paper deals with state legislature elections analyzed in this paper. Specifically, it rules that 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

[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.

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 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]

Appendix D: Additional Effects on Pre-Natal Visits

This appendix reports the effects of electronic voting on additional variables measuring the number of pre-natal visits. The results show that all the increase in the share of mothers with seven or more visits reported on Section 3.3 can be accounted by the intensive margin. Moreover, it shows the robustness of the results to issue related to non-reporting of the number of visits.

The birth-records data report (by state and year) the number of births by mothers that had zero, 1-6, and 7+ pre-natal visits. For some births, this number of visits is ignored. Table A2 reports effect of electronic voting, estimated by the procedure described on Section 3.2, on these four variables. Only results for the sample of less educated mothers (without primary schooling) are reported. The dependent variables is always measured as a share of total births in the state year.

The first line of panel Panel A presents the effects on the share of mothers without

[57]Fujiwara (2009) and Chamon et al. (2009) exploit this regression discontinuity design to estimate effects on voting and fiscal outcomes, respectively. Goncalves et al. (2008) also analyses this rule in a non-RDD framework.

[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 (2010) exploit the RDD regarding legislator wages.

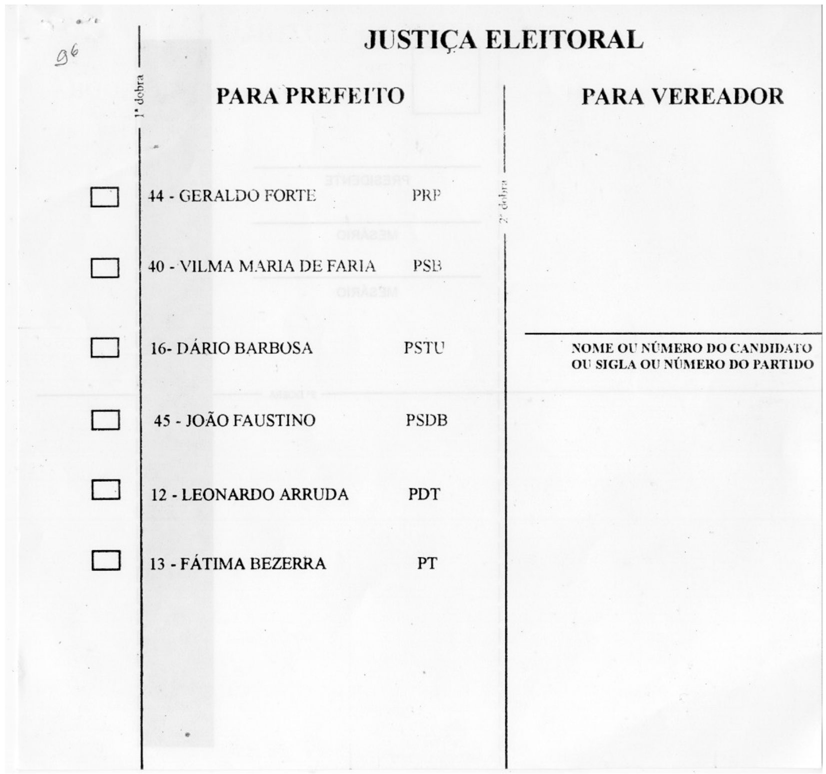
[59]Litschig (2010), Litschig and Morrison (2010), and Brollo et al. (2010) exploit this discontinuity to estimate the effects of government grants.

primary schooling. The point estimates are small and statistically insignificant across specifications. The share of mother with one-to-six pre-natal visits, however, is affected negatively by electronic voting adoption. The point estimates are about 9 p.p. and significantly at the 1% level. The third line repeats the estimates found on Table 5: electronic voting increases the share of mothers with seven or more visits. In the case of the share of mothers that did unreported number of visits, the point estimates are relatively small and positive, but never statistically distinct from zero.

These results imply that all the increase in the share of mother with 7+ pre-natal visits reported on Section 3.3. can be explained by the intensive margin: mother that would have would have at least one pre-natal visits increased the health services utilization. The result is somewhat expected given the small share of women with no pre-natal care (8.6%).

There is a slight difference in the point estimates on the share with 1-6 visits (-9 p.p.) and 7+ (7 p.p.), which can be accounted by the (statistically insignificant) positive effects on the share with zero and ignored number of visits. To check if such statistically insignificant difference is caused mostly by noise in the data, Panel B repeats the estimates of Panel A but adds the share of mothers whose number of visits is ignored as control. The results show that electronic voting is associated with a 7 p.p. increase in the share of mothers with 7+ visits, and equally sized reduction on the share with 1-6 visits. The results on the share of mothers with zero visits is numerically and statistically close to zero.

Figure 1: Example of a Paper Ballot

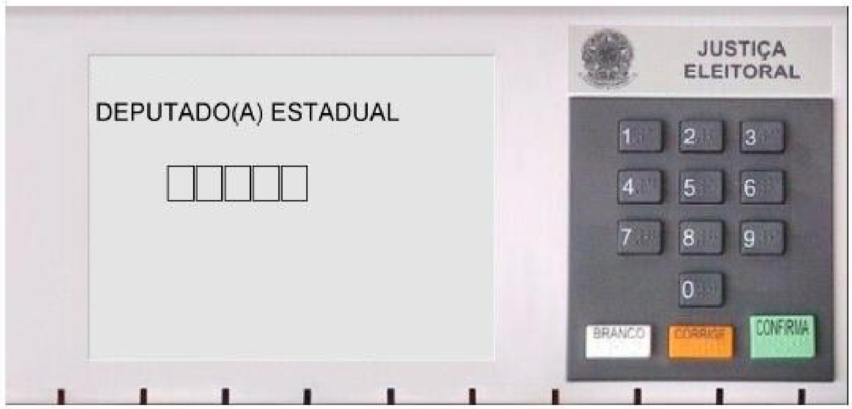


The picture above depicts a paper ballot used in a municipal election. The column on the right is for a vote to the mayor (*Prefeito*) and is based on checking a box to the left of the candidates electoral number, name, and party acronym. Other plurality elections (gubernatorial, senatorial, and presidential) use a similar pattern.

The column on the left is for a vote to the municipal legislator (*Vereador*). It reads "Name or Number of Candidate or Acronym or Number of Party" under the line where the voter should write his vote. Other proportional representation elections (state legislatures, federal congress) use a similar pattern.

Figure 2: Interface of the Electronic Voting Device

Panel A: Initial Screen of Voting Interface



Panel B: A Vote for Candidate Monteiro Lobato (Electoral Number 92111) of Party PLT

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

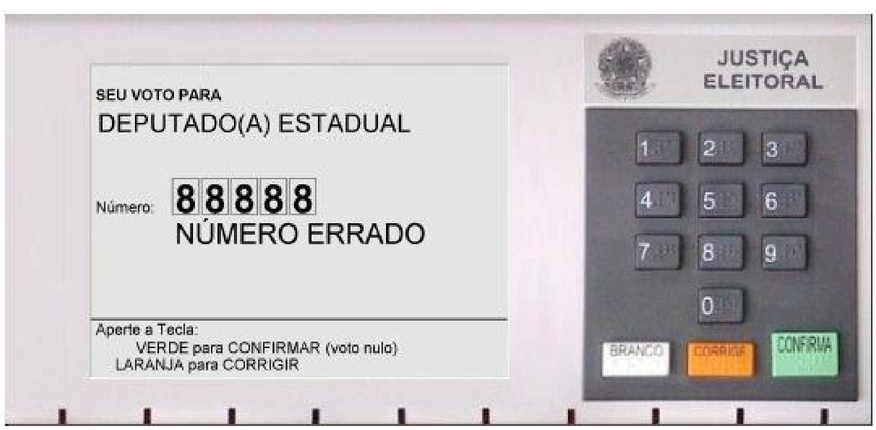
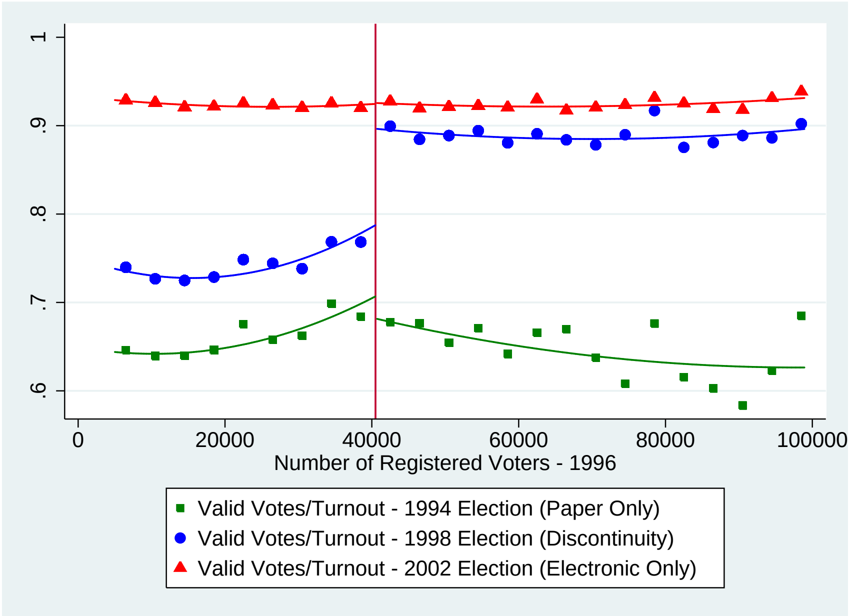
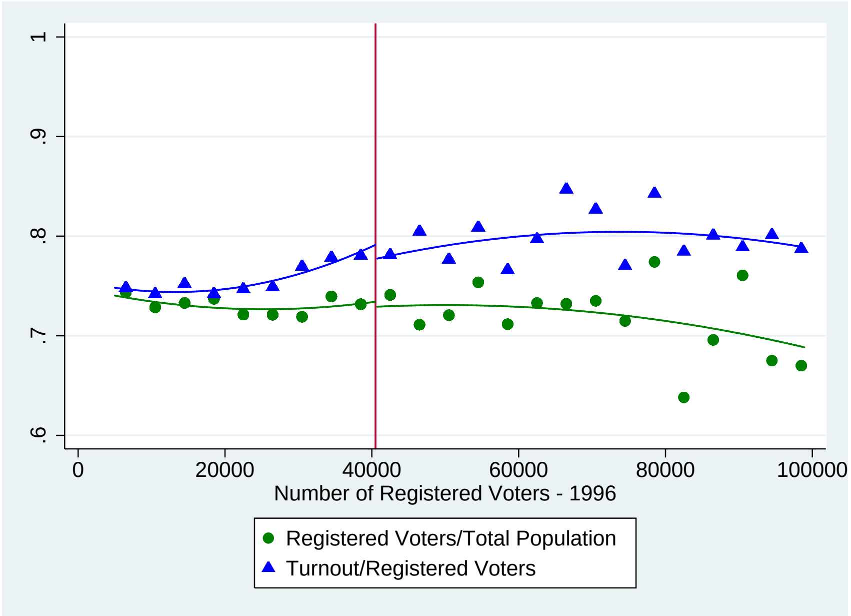


Figure 3: Valid Votes/Turnout - Local Averages and Parametric Fit



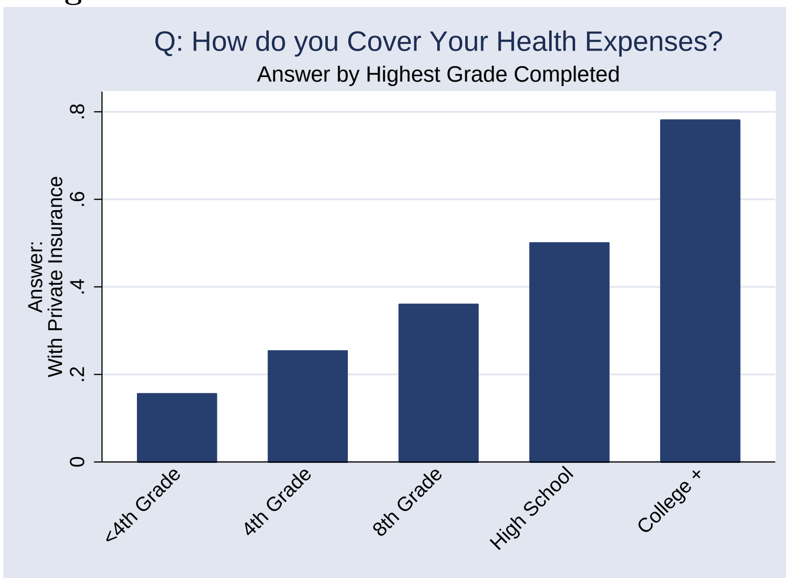
Each marker represents the average value of the variable in a 4,000-voter bin. The continuous lines are from a quadratic fit over the original ("unbinned") data. The vertical line marks the 40,500-voter threshold.

Figure 4: Valid Votes/Turnout - Local Averages and Parametric Fit



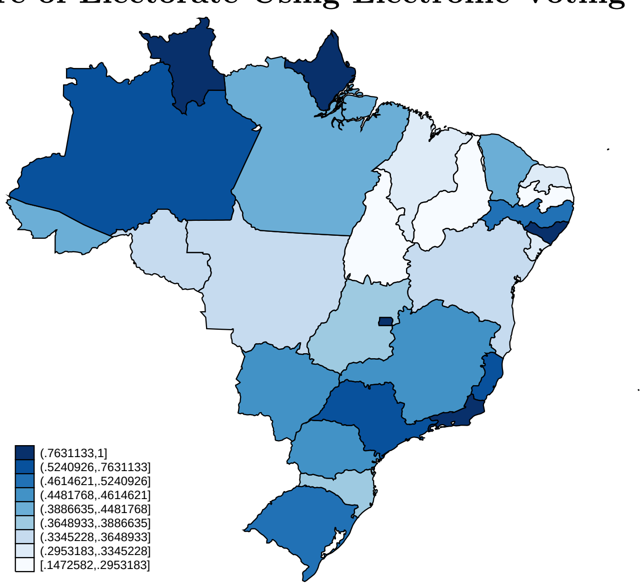
Each marker represents the average value of the variable in a 4,000-voter bin. The continuous lines are from a quadratic fit over the original ("unbinned") data. The vertical line marks the 40,500-voter threshold.

Figure 5: Education and Health Insurance Use



Computed from the Latinobarometro Survey (1998 Wave). The nationally representative sample consists of 1,100 adults aged 18 or older.

Figure 6: Share of Electorate Using Electronic Voting - 1998 Election



Darker Shading is Proportional to S_i, as Shown in Scale

Figure 7: Electronic Voting Phase-in and Valid Voting

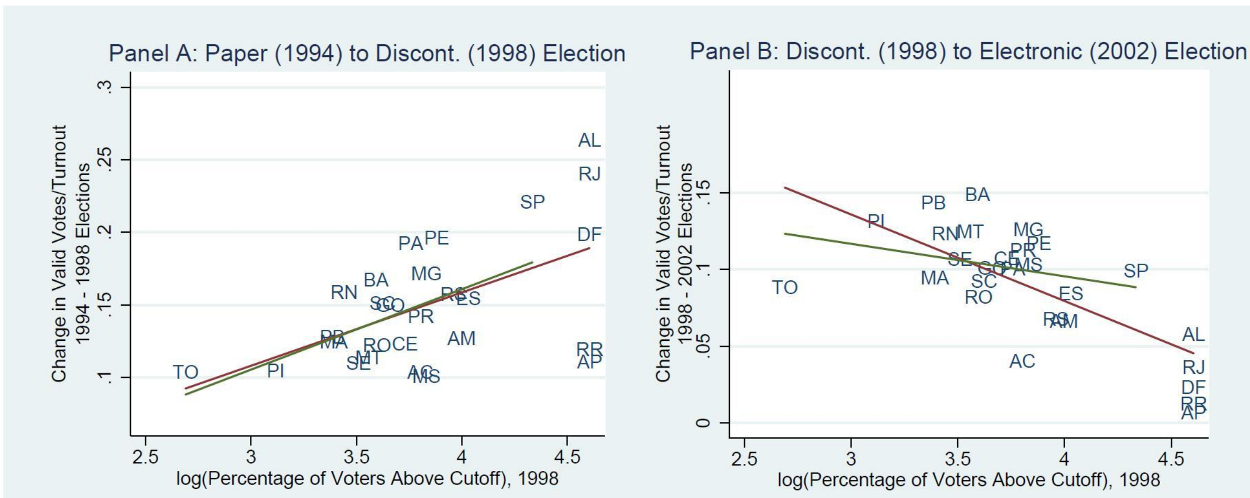


Figure 8: Electronic Voting Phase-In and Share of Low Educated Legislators

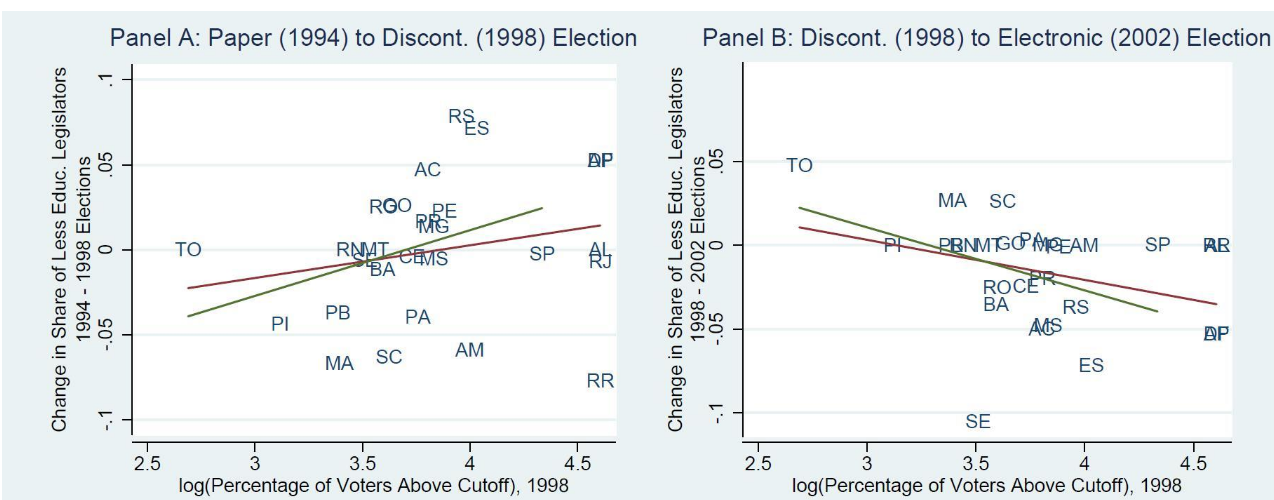


Figure 9: Electronic Voting Phase-In and Health Care Spending

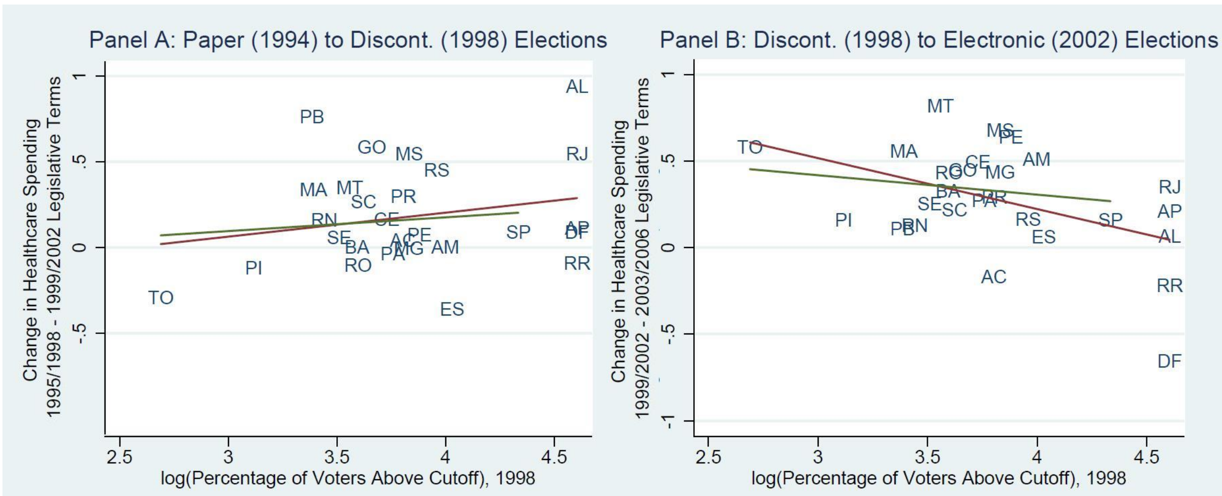


Figure 10: Electronic Voting Phase-In and Pre-Natal Visits

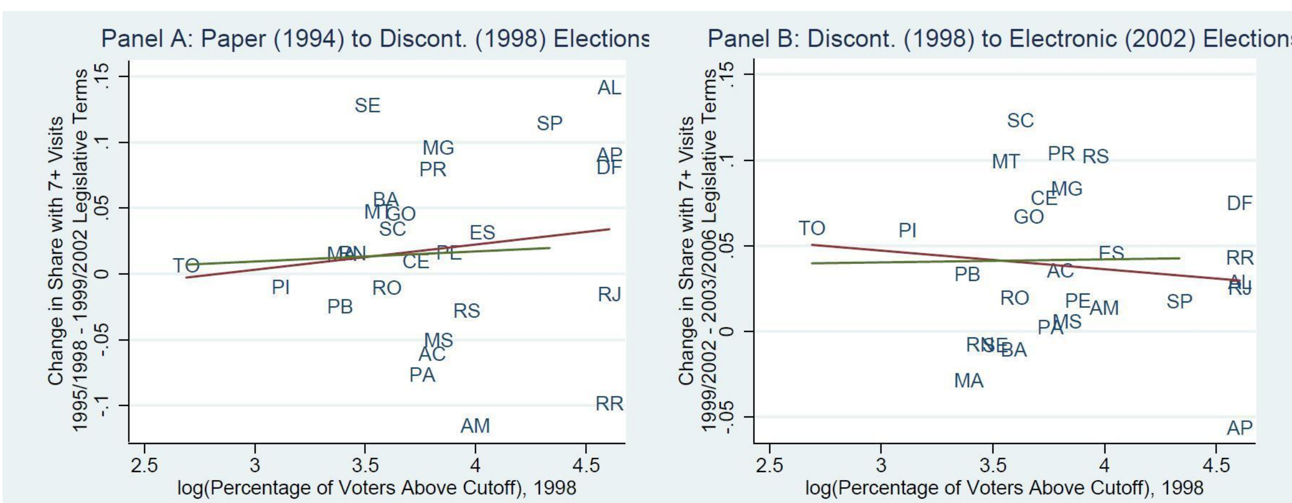


Figure 11: Electronic Voting Phase-In and Low Birth-Weight

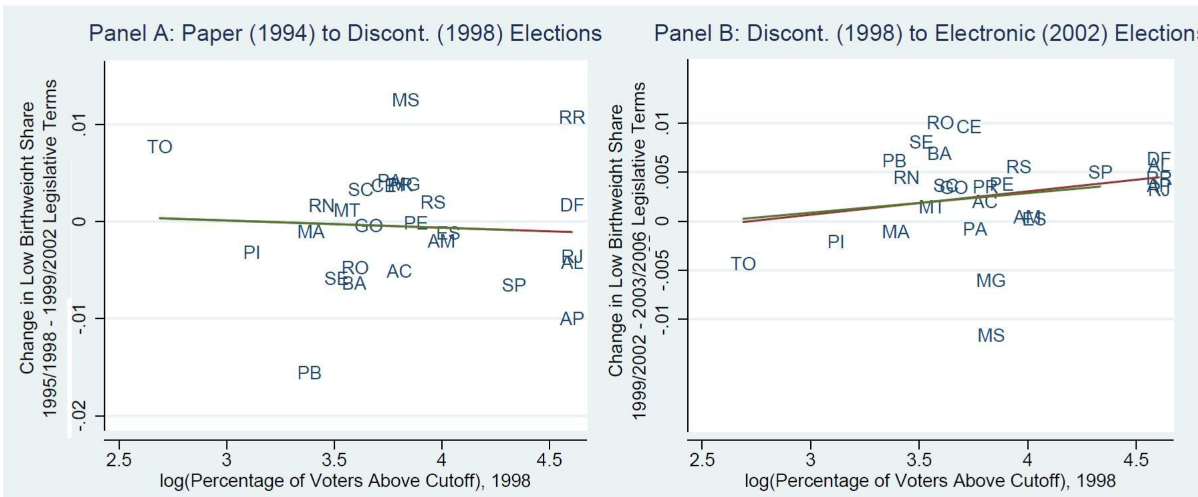
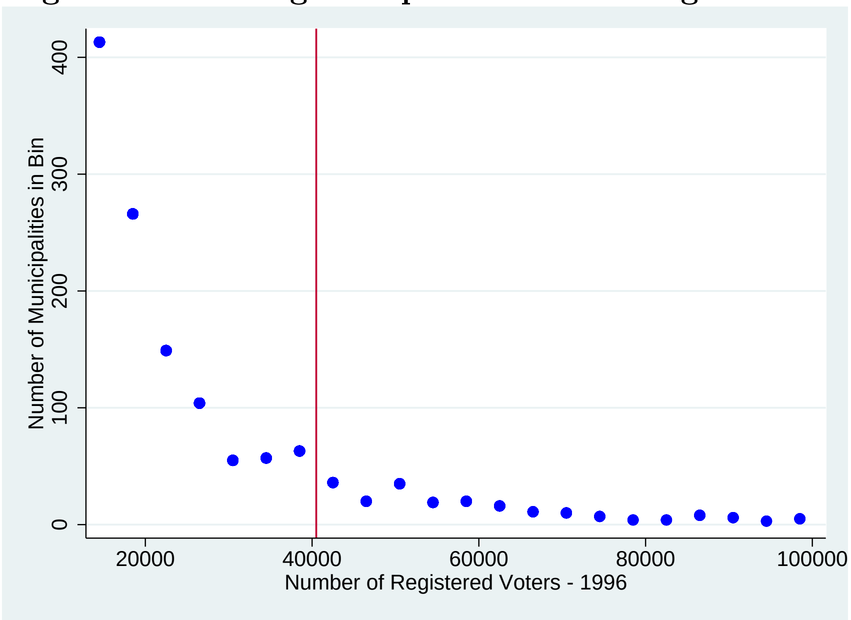
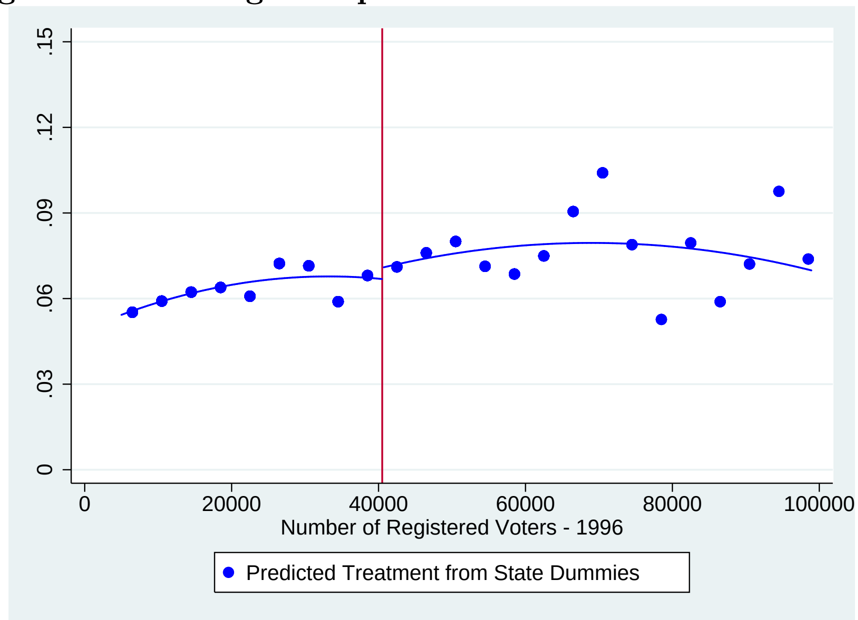


Figure A1: Testing Manipulation of Forcing Variable



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

Figure A2: 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 quadratic fit over the original ("unbinned") data. The vertical line marks the 40,500-voter threshold.

Table 1: Treatment Effects of Electronic Voting

	Pre-Treat. Mean	(1)	(2)	(3)	(4)	(5)
Panel A: Effects on Outcomes (1998 Election)						
Valid Votes/Turnout	0.769	0.128	0.122	0.124	0.116	0.118
		(0.011)***	(0.016)***	(0.025)***	(0.017)***	(0.026)***
Turnout/Reg. Voters	0.789	-0.001	0.013	0.007	-0.006	0.010
		(0.017)	(0.021)	(0.033)	(0.023)	(0.034)
Reg. Voters/Population	0.732	0.001	0.012	0.007	-0.006	0.010
		(0.024)	(0.021)	(0.032)	(0.024)	(0.034)
Panel B: Placebo Tests (Election Years Without Discontinuous Assignment)						
Valid Votes/Turnout (1994 Election)	0.688	-0.006	-0.008	0.006	-0.013	0.007
		(0.017)	(0.023)	(0.032)	(0.025)	(0.033)
Valid Votes/Turnout (2002 Election)	0.921	0.005	0.008	0.008	0.007	0.010
		(0.005)	(0.006)	(0.010)	(0.007)	(0.010)
Panel C: Tests of Quasi-Random Assignment (Effects on Pre-Determined Covariates)						
Share w/o Primary Schooling	0.789	-0.014	0.001	0.004	0.004	0.004
		(0.016)	(0.020)	(0.029)	(0.020)	(0.029)
Monthly Income (1991 *reais*)	174.83	0.908	6.096	14.017	10.759	38.957
		(16.292)	(22.097)	(32.863)	(24.085)	(34.899)
Latitude (degrees)	-16.40	-0.744	0.361	-0.674	0.580	-2.713
		(1.565)	(2.070)	(2.998)	(2.256)	(3.119)
Longitude (degrees)	45.51	0.322	0.550	2.685	0.597	2.812
		(1.248)	(1.636)	(2.466)	(1.797)	(2.511)
Bandwidth:	-	20,000	10,000	5,000	20,000	10,000
Specification:	-	Linear	Linear	Linear	Quadratic	Quadratic
Observations:	-	558	229	116	558	229

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

Table 2: Treatment Effects Interacted with Education

Dependent Variable: Valid Votes/Turnout					
Explanatory Variable	(1)	(2)	(3)	(4)	(5)
Electronic Voting	-0.061	-0.051	-0.053	-0.079	-0.127
	(0.114)	(0.163)	(0.260)	(0.171)	(0.262)
Electronic Voting x(% pop. w/o Primary School)	0.239	0.221	0.227	0.251	0.315
	(0.111)**	(0.162)*	(0.319)	(0.112)**	(0.321)
Implied Effect on a municipality:					
with 71% w/o Primary School (1st decile)	0.110	0.107	0.108	0.100	0.097
	(0.017)***	(0.024)***	(0.039)***	(0.026)***	(0.040)**
with 92% w/o Primary School (9th decile)	0.160	0.153	0.156	0.152	0.163
	(0.020)***	(0.028)***	(0.043)***	(0.030)***	(0.043)***
Bandwidth	20,000	10,000	5,000	20,000	10,000
Specification	Linear	Linear	Linear	Quadratic	Quadratic
Observations	558	229	116	558	229

***,**,* Significant at the 1%, 5%, 10% level. Each column reports a separate local polynomial regression estimate with the specified bandwidth and specification. All specifications are fully interacted with the "% w/o Primary School" variable. Robust standard errors in parenthesis.

Table 3: Effects of Electronic Voting by List Characteristics

Dependent Variable: $(votes_{lm} \backslash turnout_m) \times 100$					
Explanatory Variable	Parameter	(1)	(2)	(3)	(4)
Electronic Voting	λ	0.613	0.055	0.521	-0.092
		(0.044)***	(0.100)	(0.150)***	(0.063)
Electronic Voting $\times$ P_l^{votes}	ψ	-	0.794	-	0.603
			(0.156)***		(0.221)**
Electronic Voting $\times$ P_l^{cand}	ϕ	-	-	0.873	0.903
				(0.301)**	(0.558)*
List-Specific Effects		Yes	Yes	Yes	Yes
List-Specific Slopes		Yes	Yes	Yes	Yes
Bandwidth:		20,000	20,000	20,000	20,000
Observations::		11,745	11,745	11,745	11,745

***,**,* Significant at the 1%, 5%, 10% level. Standard errors clustered at the party list level in parenthesis. The level of observations is a list-municipality pair. Each column reports a separate regression estimate, including a linear spline of the number of voters fully interacted with list indicators. The pre-treatment mean of the dependent variable is 3.63.

Table 4: Effect of Electorate Share above Cutoff through Time: Election Outcomes and Health Care Funding

	(1)	(2)	(3)	(4)
			Placebo Tests	
Parameter:	θ^{99-02}	θ^{03-06}	θ^{91-94}	θ^{07-10}
Sample (legislative terms included):	1995-1998 and 1999-2002 (Paper-Discont.)	1999-2002 and 2003-2006 (Discont.-Electr.)	1991-1994 and 1995-1998 (Paper-Paper)	2003-2006 and 2007-2010 (Electr-Electr.)
Panel A: Estimated Effect of Electronic Voting and Placebo Tests				
Valid Votes/Turnout	0.108 (0.037)***	-0.112 (0.010)***	-	-0.010 (0.011)
Share of Legislators w/o Primary Schooling	0.046 (0.020)**	-0.047 (0.029)	-	0.012 (0.058)
log(Health Care Spending per capita)	0.512 (0.319)*	-0.483 (0.210)**	-0.134 (0.349)	-
Panel B: Falsification Tests				
log(Total Spending per capita)	-0.021 (0.113)	-0.203 (0.177)	-	-
log(GDP per capita)	-0.042 (0.090)	-0.141 (0.140)	-	-
log(Population)	0.066 (0.043)	0.038 (0.028)	-	-
Observations	216	216	216	216

***,**,* Significant at the 1%, 5%, 10% level. Standard errors clustered at the state level in parenthesis. Each figure is from a separate regression using a sample covering two legislative terms (8 years), reporting the coefficient from the interaction between the share of voters living in municipalities above the 40,500-voter cutoff (in 1998) and a dummy indicator for the second legislative term in the sample. State fixed effects, time effects and region-time effects are included in all regressions.

Table 5: State-Level Effects of Electronic Voting: Election Outcomes and Health Care Funding

Dependent Variable	Sample Average	(1)	(2)	(3)	(4)
Panel A: Political Outcomes					
Valid Votes/Turnout	0.833	0.110	0.112	0.110	0.117
		(0.019)***	(0.018)***	(0.020)***	(0.013)***
Share of Legislators w/o Primary Schooling	0.082	0.044	0.043	0.049	0.073
		(0.022)**	(0.022)*	(0.022)**	(0.029)**
Panel B: Health Care Spending					
log(Health Care Expenditures per capita)	-	0.550	0.526	0.515	0.510
		(0.111)***	(0.131)***	(0.135)***	(0.181)***
Share of Total Budget Spent on Health Care	0.099	0.037	0.037	0.033	0.025
		(0.008)***	(0.009)***	(0.012)***	(0.015)*
Additional Controls (GDP and Population)	-	-	Yes	Yes	Yes
State-Specific Trends	-	-	-	Yes	Yes
Restricted Sample	-	-	-	-	Yes
Observations	-	324	324	324	276

***,**,* Significant at the 1%, 5%, 10% level. Standard errors clustered at the state level in parenthesis. Each figure is from a separate regression using a sample covering three legislative terms (12 years), reporting the coefficient from the interaction between the share of voters living in municipalities above the 40,500-voter cutoff (in 1998) and a dummy indicator for the 1999-2002 legislative term. 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.

Table 6: Effect of Electorate Share above Cutoff through Time: Health Care Utilization and Outcomes

Parameter	θ^{99-93}	θ^{03-06}
Sample (legislative terms included)	1995-1998 and 1999-2002 (Paper-Discont.)	1999-2002 and 2003-2006 (Discont.-Electr.)
Panel A: Uneducated Sample (Mothers without primary schooling)		
Share with 7+ Prenatal Visits	0.088	-0.052
	(0.055)	(0.032)*
Share of Low-weight Births (×100)	-0.431	0.429
	(0.331)	(0.238)*
Panel B: Educated Sample (Mothers with primary schooling)		
Share with 7+ Prenatal Visits	-0.015	0.032
	(0.049)	(0.025)
Share of Low-weight Births (×100)	0.142	0.266
	(0.475)	(0.453)
Observations (elections)	216	216

***,**,* Significant at the 1%, 5%, 10% level. Standard errors clustered at the state level in parenthesis. Each figure is from a separate regression using a sample covering two legislative terms (8 years), reporting the coefficient from the interaction between the share of voters living in municipalities above the 40,500-voter cutoff (in 1998) and a dummy indicator for the second legislative term in the sample. State fixed effects, time effects and region-time effects are included in all regressions.

Table 7: State Level Effects of Electronic Voting: Health Care Utilization and Outcomes

		Baseline			"Triple Differences"	
	Sample		Specification		Specification	
Dependent Variable	Average	(1)	(2)	(3)	(4)	(5)
Panel A: Uneducated Sample (Mothers without primary schooling)						
Share with 7+ Prenatal Visits	0.333	0.070	0.062	0.070	0.058	0.062
		(0.029)**	(0.029)**	(0.034)**	(0.048)	(0.035)*
Share of Low-weight Births ($\times$100)	7.721	-0.430	-0.414	-0.426	-0.601	-0.707
		(0.219)**	(0.214)*	(0.229)*	(0.265)**	(0.297)**
Panel B: Educated Sample (Mothers with primary schooling)						
Share with 7+ Prenatal Visits	0.527	0.009	0.017	-0.004	-	-
		(0.028)	(0.032)	(0.029)		
Share of Low-weight Births ($\times$100)	6.261	-0.062	-0.043	-0.136	-	-
		(0.441)	(0.435)	(0.434)		
Additional Controls (GDP and Pop.)	-	-	Yes	Yes	-	-
State-Specific Trends	-	-	-	Yes	-	-
State-Year Effects	-	-	-	-	Yes	Yes
Restricted Sample	-	-	-	-	-	Yes
Observations	-	324	324	324	628	552

***,**,* Significant at the 1%, 5%, 10% level. Standard errors clustered at the state level in parenthesis. Each figure is from a separate regression using a sample covering three legislative terms (12 years), reporting the coefficient from the interaction between the share of voters living in municipalities above the 40,500-voter cutoff (in 1998) and a dummy indicator for the 1999-2002 legislative term. 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.

Table A1: Summary Statistics for Municipal Sample

Variable	Year of Observation	Number of Observations	Mean	Standard Deviation
Panel A: Municipalities With an Electorate Size Beteween 20,500 and 60,500				
Valid Votes/Turnout	1994	557	0.672	(0.094)
Valid Votes/Turnout	1998	558	0.784	(0.094)
Valid Votes/Turnout	2002	558	0.923	(0.024)
Turnout/Registered Voters	1998	558	0.765	(0.089)
Registered Voters/Population	1998	558	0.726	(0.094)
Share of Adults Without Primary Schooling	1991	558	0.813	(0.080)
Monthly Income (*reais* per capita)	1991	558	160.61	(86.01)
Latitude (degrees)	-	558	-15.71	(8.50)
Longitude (degrees)	-	558	45.53	(6.26)
Panel B: All Municipalities				
Valid Votes/Turnout	1994	4810	0.653	(0.099)
Valid Votes/Turnout	1998	5282	0.755	(0.087)
Valid Votes/Turnout	2002	5282	0.928	(0.026)
Turnout/Registered Voters	1998	5282	0.765	(0.092)
Registered Voters/Population	1998	5282	0.748	(0.141)
Share of Adults Without Primary Schooling	1991	5282	0.876	(0.077)
Monthly Income (*reais* per capita)	1991	5282	123.11	(73.09)
Latitude (degrees)	-	5282	-16.53	(8.23)
Longitude (degrees)	-	5282	46.36	(6.32)

Table A2: Summary Statistics for State Sample (1995-2002)

Variable	Number of Observations	Mean	Standard Deviation
Share of Voters Using Electronic Voting	324	0.51	(0.43)
Valid Votes/Turnout - State Legislature Elections	324	0.83	(0.11)
Share of State Legislators Without Primary Schooling	324	0.08	(0.03)
State Expenditures on Health Care (2000 *reais* per capita)	324	99.65	(86.34)
Share of State Budgets Spent on Health Care	324	9.90	(4.16)
Share of Uneducated Mothers with 7+ Pre-Natal Visits	324	0.33	(0.13)
Share of Educated Mothers with 7+ Pre-Natal Visits	324	0.53	(0.15)
Share of Uneducated Mothers with Low-Weight Births	324	0.08	(0.01)
Share of Educated Mothers with Low-Weight Births	324	0.06	(0.02)
Population (in thousands)	324	6294	(7540)
GDP (in thousands of 2000 *reais* per capita)	324	5.36	(3.29)

Table A3: Additional Estimated Treatment Effects of Electronic Voting

	Pre-Treat. Mean	(1)	(2)	(3)	(4)	(5)
Panel A: Effects on Null and Blank Votes, State Legislature Election						
Null Votes/Turnout	0.138	-0.093	-0.086	-0.084	-0.081	-0.076
		(0.007)***	(0.010)***	(0.015)***	(0.010)***	(0.015)***
Blank Votes/Turnout	0.093	-0.035	-0.036	-0.040	-0.034	-0.042
		(0.006)***	(0.008)***	(0.012)***	(0.009)***	(0.013)***
Panel B: Effects on Valid Votes/Turnout, by Office						
State Legislature	0.769	0.128	0.122	0.124	0.116	0.118
		(0.011)***	(0.016)***	(0.025)***	(0.017)***	(0.026)***
Federal Congress 7 (Lower Chamber)	0.681	0.218	0.210	0.205	0.212	0.207
		(0.012)***	(0.018)***	(0.030)***	(0.019)***	(0.031)***
Federal Congress (Senate)	0.705	0.081	0.087	0.100	0.079	0.108
		(0.014)***	(0.020)***	(0.030)***	(0.020)***	(0.031)***
State Governor	0.748	0.078	0.078	0.082	0.071	0.093
		(0.012)***	(0.017)***	(0.026)***	(0.019)***	(0.028)***
President	0.799	0.018	0.021	0.031	0.011	0.034
		(0.011)*	(0.013)	(0.021)	(0.015)	(0.022)
Bandwidth:	-	20,000	10,000	5,000	20,000	10,000
Specification:	-	Linear	Linear	Linear	Quadratic	Quadratic
Observations:	-	558	229	116	558	229

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

Table A4: Effects of Electronic Voting on Number of Pre-Natal Visits

Dependent Variable	Sample Average	(1)	(2)	(3)
Panel A: Baseline Estimates				
Share with Zero Prenatal Visits	0.086	-0.017	-0.010	-0.007
		(0.015)	(0.013)	(0.013)
Share with 1-6 Prenatal Visits	0.502	-0.093	-0.101	-0.093
		(0.023)***	(0.027)***	(0.021)***
Share with 7+ Prenatal Visits	0.333	0.070	0.062	0.070
		(0.029)**	(0.029)**	(0.034)**
Share with Ignored Number of Prenatal Visits	0.079	0.040	0.049	0.031
		(0.030)	(0.032)	(0.028)
Panel B: Estimates Controlling for Share with Ignored Visits				
Share with Zero Prenatal Visits	0.086	-0.011	-0.003	-0.001
		(0.014)	(0.012)	(0.013)
Share with 1-6 Prenatal Visits	0.502	-0.073	-0.077	-0.081
		(0.019)***	(0.022)***	(0.023)***
Share with 7+ Prenatal Visits	0.333	0.085	0.080	0.082
		(0.025)***	(0.026)***	(0.027)***
Additional Controls (GDP and Pop.)	-	-	Yes	Yes
State-Specific Trends	-	-	-	Yes
State-Year Effects	-	-	-	-
Restricted Sample	-	-	-	-
Observations	-	324	324	324

***,**,* Significant at the 1%, 5%, 10% level. Standard errors clustered at the state level in parenthesis. The sample includes only mothers without primary schooling. Each figure is from a separate regression using a sample covering three legislative terms (12 years), reporting the coefficient from the interaction between the share of voters living in municipalities above the 40,500 cutoff (in 1998) and dummy indicator for the 1999-2002 legislative term. 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.