

# Can Voting Technology Empower the Poor? Regression Discontinuity Evidence from Brazil

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## Abstract

This paper estimates the impacts of a new electronic voting technology that introduced visual aids in Brazilian elections. Exploring a regression discontinuity design embedded in its phase-in, I estimate that the new voting technology increased the share of valid votes (i.e., votes that can be counted for a candidate) by more than 10%, generating the *de facto* enfranchisement of millions of illiterate Brazilian citizens. I then show that, consistent with theories of enfranchisement, the increased political participation of the low educated population shifted fiscal policy towards their needs, generating a large increase in public health care and sanitation expenditures.

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# 1 Introduction

Interventions to foster political participation, by increasing turnout in elections or enfranchising citizens without the right to vote, are commonly discussed in both developed and developing countries policy arenas. However, there are few studies that assess their economic and welfare impacts, and the existing ones usually lack a convincing identification strategy.

In this paper I study the impacts of a new electronic voting technology that introduced visual aids (candidates' pictures) in Brazilian elections, facilitating voting for illiterate citizens. Exploring the clean identification generated by a regression discontinuity design embedded in the phase-in of the new technology, I estimate that it substantially increased the number of valid votes (i.e., votes that can be assigned to a candidate).

The results are large (equivalent to more than 10% of the Brazilian electorate) and consistent with the fact that low educated citizens that were unable to cast a vote using a paper ballot were able to do so under the new electronic system. Hence, I find that the new technology promoted *de facto* enfranchisement of millions of less educated Brazilians.<sup>1</sup>

I then show that this increased political participation of the illiterate population shifted fiscal policy towards their needs, generating a large increase in public health care expenditures. Hence, this paper can also be seen as an opportunity to test the economic impacts of enfranchisement (Husted and Kenny, 1997; Kenny and Lott, 1999; Miller, 2008) with a credible identification strategy.

The remainder of the paper is divided in five sections. Section 2 discusses the previous literature on electronic voting and enfranchisement, while Section 3 provides the background on the introduction of the new technology in Brazil. Section 4 presents the estimates of its impact on voting outcomes, demonstrating its enfranchisement effect. Section 5 addresses how fiscal policy shifted, while Section 6 concludes the paper.

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<sup>1</sup>While most educated citizens in the developed world see the act of casting a vote in itself - simply filling a ballot - as a trivial task, the same may not be true in most developing countries where a significant share of the electorate is illiterate. In these cases the design of a voting technology that is easy to operate by a low-educated citizen and maintains the secrecy of the ballot can be a challenge.

## 2 Previous Literature

### 2.1 Electronic Voting

The use of electronic voting systems has substantial debate recently. Difficulties in vote counting in developed countries (such as those observed in the 2000 U.S. presidential election) and the longstanding issues of election fraud throughout the developing world have made some point to electronic voting machines as a solution to the difficulties associated to with the usual paper-based voting. On the other hand, there is substantial concern regarding the possibility of fraud in electronic voting. This distrust goes well beyond “conspiracy theories” and can be found in the mainstream media<sup>2</sup> and peer-reviewed research (such as several technical studies on the possibility of hacking and fraud of the existing systems).

Economists also have been increasingly interested in the issue of voting technology. Dee (2007) uses a Californian recall election as a natural experiment to test which voting technologies generate higher residual voting and mistakes when filling ballots, while Card and Moretti (2007) explore the association between touch-screen voting machines and the outcomes of the 2004 U.S. presidential election.<sup>3</sup>

A serious limitation with all the above studies is that they use data on the U.S., where counties choose their voting technologies, and selection bias likely affect the estimates. For example, Card and Moretti (2007) find that the use of touch-screen machines and voting for George W. Bush are associated in a fixed effect estimation using a panel of counties. However, a causal interpretation is difficult given that any omitted variable that is related both to preference for Bush and adoption of electronic voting can be driving the result.

Following Dee’s (2007) footsteps, this paper assesses the effect of electronic technology on residual voting. Residual votes (i.e., votes that are either blank or void and are hence uncounted) are usually regarded as measure of a voting technology’s quality (Alvarez et al., 2001; Brady, 2001), and proposals of abolishing voting technologies based on their tendency to generate residual votes occur in the U.S. Congress and other countries. As put by Dee (2007), “*Any voting system that systematically increased the preva-*

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<sup>2</sup>See, for example, the New York Times’ editorial of March 10, 2005.

<sup>3</sup>See also Garner and Spolaore’s (2004) analysis of the determinants of voting technology adoption

*lence of uncounted votes should unquestionably be viewed as an affront to the important and universally shared norms of electoral fairness.”* This is especially true in a developing country context, where a substantial share of the population is illiterate and a “complicated” voting technology may promote their exclusion from the democratic process.

The results suggest that electronic voting substantially reduces the number of residual votes. The large size of the estimates suggest that perhaps millions of people would not have their votes counted in the absence of electronic voting, so that this may be a powerful instrument for including people in the political process. No effect of electronic voting adoption on the registration and turnout of voters is found, indicating that the lower number of residual votes is caused by the DRE machines themselves and not by a change in the composition of voters.

## **2.2 Theories of Enfranchisement**

[in progress]

# **3 Elections and Voting Technology in Brazil**

Brazil is a federation of 26 states (and a federal district), which are themselves comprised of over 5,000 municipalities. Every four years elections are held to elect the federal and state executive and legislative powers. The electoral rules (including the election date) are the same throughout the country. The districts are the states (in other words, elections for state level governments are at-large), which implies that any two voters in the same state face the exact same choice of candidates. The federal congress (lower house) and state legislature are elected under an open-list proportional representation system and hence votes are cast to individual candidates, and not party lists.<sup>4</sup>

This combination of rules makes voting in Brazil a relative complex task. In the 1998 election, for example, a voter residing in the state of São Paulo had to choose from: one out of 1.265 candidates for the state legislature; one out of 661 candidates for the federal congress (lower house); one out of 10

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<sup>4</sup>Members of the federal senate are elected by simple plurality rule, while the president and state governors are elected through a dual-ballot plurality rules (runoff system). The terminology used to describe the electoral system follows closely the one used in Cox (1997), which the reader is referred to for a more detailed explanation.

candidates for state governor; two out of 13 candidates for the federal senate (upper house); and one out of 12 candidates for president.

Before 1994, voting in Brazil was done in paper ballots such as the one exemplified in Figure 1. In the case of the lower house of federal congress and state legislatures, the voter was required to write down the candidate's name or number, a 4- or 5-digit code assigned by the election authority.<sup>5</sup> Notice that there are no visual aids in the ballot.

In the mid-1990s, the independent branch of the judiciary (the *Tribunal Superior Eleitoral*) that regulates elections procedures introduced a new direct-record electronic voting technology in order to increase efficiency and reduce costs in vote counting.<sup>6</sup>

The interface of the new "electronic ballots" 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 candidate would type the candidate's number, wait for her name, party affiliation and picture to appear on the screen, and then confirm the vote (or choose to start the process again).

This particular design was done in a way to facilitate voting by low educated people and those not familiar with computers. Not only the use of pictures obviously help those unable to read, but a interface based on numbers using a phone-like dial was probably also instrumental.<sup>7</sup>

The government also promoted a large scale campaign instructing the public on how to use the new machines, which included repeated official "advertisements" on national TV and radio stations and also the possibility of voters to test them before the election, by voting on imaginary candidates using sample machines at election authority offices.

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<sup>5</sup>Voting for president, governor and senator are done by checking a box by the name of the candidate (see Figure 1).

<sup>6</sup>This objective was indeed achieved. The Brazilian elections authority is able to fully count and process over 100 million votes cast across a large country in three to five hours.

<sup>7</sup>One of the machine's designers indeed notices that "*illiterate people know how to operate number and can use a phone*" (Tribunal Superior Eleitoral, 2006).

## 4 The Effect of Electronic Voting on Political Participation

### 4.1 Identification Strategy

Of particular interest for this paper is how the new technology was phased in. In the 1998 elections, only municipalities with more than 40,500 registered voters (as of 1996, when municipal elections took place) used the electronic ballots, while municipalities below this threshold used the standard paper ballot, which was used in the previous (1994) election in all municipalities.<sup>8</sup> Since 2002, this new technology is the sole method of collecting votes in Brazilian elections and referendums.

This threshold-based rule used to assign voting technologies in the 1998 election creates a standard regression discontinuity design. Under mild assumptions, generates “assignment as good as random”. Intuitively, municipalities falling “just below” and “just above” the threshold should be, on average, *ex ante* similar to each other in every possible aspect. The reason that they are on a particular side of the threshold is due to random uncontrollable events that are not related to how people vote in any way.

Hence, any variable, observed or unobserved, that could affect voting independently of the voting technology 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 of cities is, in fact, a causal consequence of the different voting technologies.

In practice, there are two important factors that ensure that “almost random” interpretation of the estimates. First, it is important that 40,500-voter threshold is somewhat arbitrary and not used to assign anything else to municipalities. To the best of my knowledge, this is indeed the case, as the assignment rule was adopted due to restrictions on the production capacity of the “electronic ballots” manufacturer and the logistics of distributing them on election day. There is also no other administrative rule based on the same threshold.

Second, it is important that municipalities had only a limited control over

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<sup>8</sup>Also, four states were chosen to use the new in all of their municipalities, in order to test the logistics of distributing the machines on unsafe areas were election official requirer military escort (Alagoas and Rio de Janeiro) or regions covered by the dense Amazon Forest (Amapá and Roraima). The data from these states are not used in any of the analyses reported in this paper.

the forcing variable. Since the assignment rule was announced in May of 1998 and the forcing variable is the number of voters who registered for municipal elections *two years earlier* in 1996, that is again clearly the case.

Fortunately (for the paper’s research design), Brazilian law makes registration and voting compulsory for all citizens aged 18-70.<sup>9</sup> Failing to register or vote in a previous election renders a citizen ineligible to several public provided services,<sup>10</sup> until a fine is paid. Moreover, elections are held on a Sunday and a 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% in the elections,<sup>11</sup> it makes the issues related to voting technology second-order in the decision to vote or not and hence the difference in turnout between cities above and below the threshold is virtually zero.

## 4.2 Data

The federal elections authority (*Tribunal Superior Eleitoral* ) publishes data on the number of registered voters and election outcomes disaggregated by municipality for several election years. It also published reports with a list of municipalities that used electronic voting in the 1998 election.

Out of the 5,282 municipalities in the data, *all* of the 307 that were above the threshold used electronic voting in 1998. There are only seven municipalities with less than 40,500 registered voters that, due to idiosyncratic reasons, adopted electronic voting. Since a number of those have well below 40,500 registered voters, these are dropped from all of the data analysis (including them has a negligible effect in all the estimations).

Of the possible election outcomes, the data collected include the turnout and also the number of *valid* votes. A vote is deemed valid if, and only if, it can be counted and assigned to a particular candidate/party. A vote that is not valid is referred to as residual (hence, the sum of valid and residual votes

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<sup>9</sup>Voting is voluntary for citizens aged 16-17 or 70+ and to those who are officially illiterate.

<sup>10</sup>This includes attending public schools, receiving payments from social programs, obtaining government sponsored credit, working for the public sector and renewing several documents (passports, driver licences, social security cards, etc.) that are necessary to the everyday activity of citizens.

<sup>11</sup>Figure 2 and Table 1 show that turnout is in the order of 85% of registered voters. This occurs because 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”.

equals turnout). 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 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 a candidate.<sup>12</sup>

Throughout this paper, we will refer to the ratio between valid votes and total turnout as the valid vote ratio. Data on municipal-level demographics (such as average schooling and income) are computed from the 1991 Census.

### 4.3 Estimation Framework

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

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

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) is continuous at threshold.

The estimation method used here closely follows the guidelines in Imbens and Lemieux (2008) and Lee and Lemieux (2009), which in turn rely on the results provided by Hahn et al. (2001). The reader is referred to these papers since only a brief overview is provided here. The limits on the right hand side are estimated non-parametrically using local linear regression. This consists of estimating a linear regression<sup>13</sup> of  $y$  on  $v$  using only data on where  $v \in [40,500 - h; 40,500]$ . 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

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<sup>12</sup>Notice 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.

<sup>13</sup>Notice that the regression is unweighted (i.e., rectangular kernel)

the treatment effect. It is important to notice the non-parametric nature of the estimation: although linear regressions are used, the assumption that the relationship between  $y$  and  $v$  is linear is not required. The limit approaching one side of the threshold is estimated using only data on that particular side.

The local linear regression estimate can be implemented in a single OLS estimation of the following equation using only observations that satisfy  $v \in (40,500 - h; 40,500 + h)$ .

$$y = \alpha + \beta v + \gamma \cdot 1\{v > 40,500\} + \delta v \cdot 1\{v > 40,500\} + u$$

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,  $u$  is the error term and the parameters to be estimated are denoted in greek letters. The estimate of  $\gamma$  is the treatment effect and its (heteroskedasticity-robust) standard error can be obtained in a straightforward manner. The estimation using a quadratic specification is done by just adding two more variables: the square value of  $v$  and its interaction with  $1\{v > 40,500\}$ .

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. Notice that these are relatively small and hence try to reinforce the “local” intuition of regression discontinuity designs: although there are more than 5,000 observations in the data, less than 600 are used to obtain all of the estimates.<sup>14</sup>

## 4.4 Results

Before reporting the estimation results, some graphical evidence is provided. Figure 3 presents a scatter of the main outcome of interest (valid vote ratio in the state legislature elections) against the forcing variable (registered voters in 1996) for three different elections.

Each “dot” in the figure reflects the average outcome in a bin of municipalities that fall within an 4,000-wide interval of the forcing variable (e.g., the green dot just above 40,500 contains the average valid vote ratio for

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<sup>14</sup>As the bandwidth increase, the number of smaller municipalities that are included at the extreme of the left interval increase rapidly (see Figure 3). Hence, estimates with large bandwidths will likely put too much weight on fitting the relationship away from the neighborhood of the 40,500 threshold.

municipalities with  $S \in [40, 500; 44, 500]$  in the 1994 election). To facilitate visualization, a quadratic polynomial is fitted on the original data at each side of the 40,500 threshold, so that the point where the lines are not connected is where we expect the discontinuity in outcomes, if existent, to be visible.

A clear upward jump in the valid vote ratio is visible in the 1998 election (in blue). Only about 75% of the votes are valid on municipalities below the 40,500 voter threshold, 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 election (when all municipalities used paper ballots) and 2002 (when electronic voting was completely phased-in) provide powerful falsification tests 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 two important variables: the turnout (as a share of registered voters) and voter registration (as a share of total population) for the 1998 election. Interestingly, there is no visible discontinuity, implying the turnout and registration behavior are the same in both treatment and control municipalities.

Table 1 presents the local linear regression estimates of the treatment effects. Irrespective of the bandwidth used, the estimated effect of a switch from paper to electronic ballot is an increase in the valid vote ratio in the order of 13-15%. Moreover, the estimates are precisely estimated and significant at the 1% level.

As robustness checks, Table 1 also presents the estimated treatment effects on the turnout (as a share of registered voters) and the valid vote ratio in the 2002 election. In conformity with the graphical analysis, the treatment effects are numerically small and not statistically different from zero at any reasonable level of significance.

Two more pieces of evidence are provided to make the case that the above result is driven by the fact that less educated (and hence poorer) citizens found that voting easier with the use of electronic voting machines.

First, Table 2 repeats the estimation presented on Table 1 with the addition of an interaction between the dummy indicating electronic voting adoption and two measures of the socioeconomic status of the municipalities: the share of adult (age 25+) with less than 4 years of education and the per capita income (measured in 1991 *reais*). The statistically significant interactions show that, as the education (and income) levels increases, the size of

the treatment effects is smaller. For example, the specification in the second column shows that in a (hypothetical) city where all citizens have at least 4 years of education, the treatment effect would be in the order of 10%, while in a (also hypothetical) city where no one has completed four years of education, the impact of introducing electronic voting on the valid vote ratio would be above 15%.

Second, Table 3 provides estimated treatment effects on votes gathered by different political parties (divided by the total turnout). For most parties, we find a positive, although insignificant. The only (notable) exception is the large and positive effect on votes for the Worker’s Party, which is particularly popular with the less educated (and poorer) electorate (Ames, 2001).

Hence, I find that the introduction of electronic voting not only promotes larger increases in valid votes in municipalities with a large number of low educated citizens, but it also is the case that close to half of this additional valid votes are cast to a party that has strong identification with this group. This combination of results make a strong case that the introduction of electronic voting indeed promoted the *de facto* enfranchisement of mostly low educated citizens.

## 5 Can Voting Technology Empower the Poor?

In this section I test if the *de facto* enfranchisement of more than 10% of the Brazilian electorate had an effect on fiscal state-level spending. Using data reported by the state governments to the federal Ministry of Finance, I estimate the following equation for an outcome  $y$  of state  $i$  at electoral-cycle (4-year period)  $t$ :

$$y_{it} = \alpha + \theta_t s_i + \beta X_{it} + \gamma_i + \delta_t + u_{it}$$

Where  $s_i$  is the share of voters residing in municipalities with more than 40,500 registered voters (as of the 1996) and  $X_{it}$  is a set of control variables. Notice that, although  $s_i$  does not vary through time, the parameter measuring its effect -  $\theta_t$  - does.

Hence, the identification comes from the same cross-sectional variable having different effects through time, even after we control for time and fixed effects. Specifically, I explore the fact that a state with a relatively large share of voters living in municipalities above the cutoff would have substantial

introduction of electronic voting between the 1994 and 1998 election, followed by a smaller phase-in (for the remaining municipalities) between the 1998 and 2002 elections.<sup>15</sup> On the other hand, a state where the minority of the voters lived above the cutoff would only observe a large introduction of electronic voting between the 1998 and 2002 elections.

Figure 5 exemplifies this graphically. The panel on the left plots the share of voters living in municipalities above the cutoff against the change in the valid vote ratio between observed between 1994 and 1998. A clear positive relationship is observed, given that the variable in the horizontal axis is positively correlated with electronic voting adoption. On the right panel, the relationship between the same variable and the change in valid vote ratios between 1998 and 2002 is plotted. Since in this case the share of voters living in municipalities above the cutoff is negatively correlated with electronic voting adoption, the relationship changes sign.

The regression counterparts of Figure 5 are provided on Table 4. The estimates suggest that the effect of introducing electronic voting on the valid vote ratio is of 10%-15%, remarkably close to the estimates obtained under a different approach on section 4.

Table 5 reports the estimated effect of electronic voting adoption on different types of government spending. All dependent variables are the log of real spending in a particular category - health care, education, public safety and total expenditures.

Of these expenditure categories, health care is expected to be particularly responsive to the enfranchisement of less educated citizens. The provision of health care in Brazil is characterized by a dual system: the existence of a private system in parallel with the public one would make only those who cannot afford to use the private system to be interested in larger public expenditures in health care. In other words, there is a clear rich/poor divide in the desire to have government spending on health care.<sup>16</sup> [*include more motivation and evidence*]

Confirming this expectation, the only case where statistically significant

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<sup>15</sup>As an example, consider a state where 70% of the electorate resides in a municipality above the cutoff. Then it is the case that 70% percent of its voters switched from paper ballots to electronic voting between the 1994 and 1998 elections. The remaining 30% would then switch from paper to machine only in the 2002 elections.

<sup>16</sup>One would also expect education to be one of the demands of the less educated population. However, state governments provide mostly secondary and post-secondary education (while elementary education is provided at the municipal level).

results are found is for health care spending. The results indicate that a larger share of voters in municipalities above the 40,500-voter cutoff is associated with increasing spending in health care following the 1998 elections and with smaller changes in spending following the 2002. Figure 6 presents this result graphically, demonstrating how changes in health care spending track the changes in valid vote ratios promoted by the introduction of electronic voting (Figure 5).

This “sign change” in the relationship between the share of voters living in municipalities above the 40,500-voter cutoff makes a very strong case for the causal interpretation of the estimates. It is difficult to imagine any omitted variable that would generate this result, as it is clear that state specific trends cannot also account for the results.

The size of the impacts is very large, a full switch from paper ballots to electronic voting is associated with an 80% increase in health care spending. However, it is the case that only about 10% of the state budgets are spent on health care.

## 6 Conclusions

This paper demonstrates how an usually overlooked issue (the design of the voting technology) can be an extremely useful instrument in promoting increased political participation and increasing the responsiveness of governments to particular sets of the electorate.

Exploring a regression discontinuity design embedded in the phase-in of the new technology, I identify its effect on the share of valid votes (i.e., votes that can be counted for a candidate) to be in the order of 10-15%, promoting the de facto enfranchisement of millions of low educated Brazilians. I then show that, consistent with theories of enfranchisement, the increased political participation of low educated citizens shifted fiscal policy towards their needs, generating a large increase in public health care and sanitation expenditures.

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**JUSTIÇA ELEITORAL**

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<input type="checkbox"/>	13 - FÁTIMA BEZERRA      PT	

Figure 1: Example of a Brazilian Paper Ballot



Figure 2: Interface of the Electronic Voting Machine

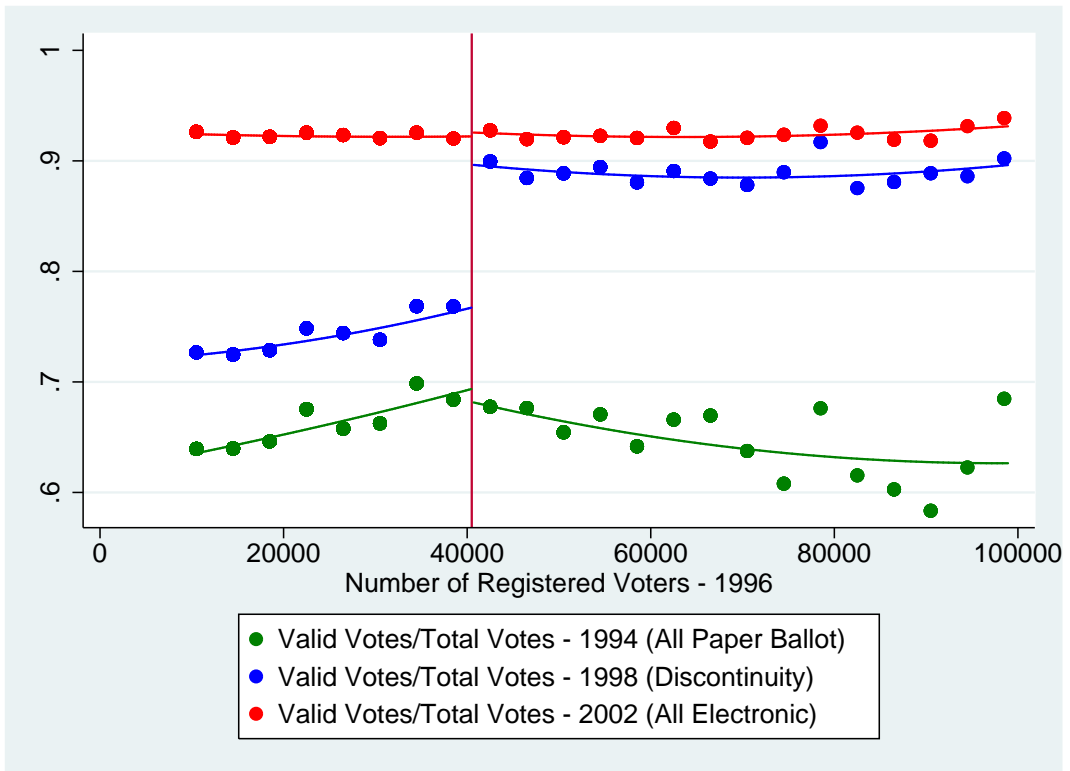


Figure 3: Valid Vote Ratio - Local Averages and Parametric Fit

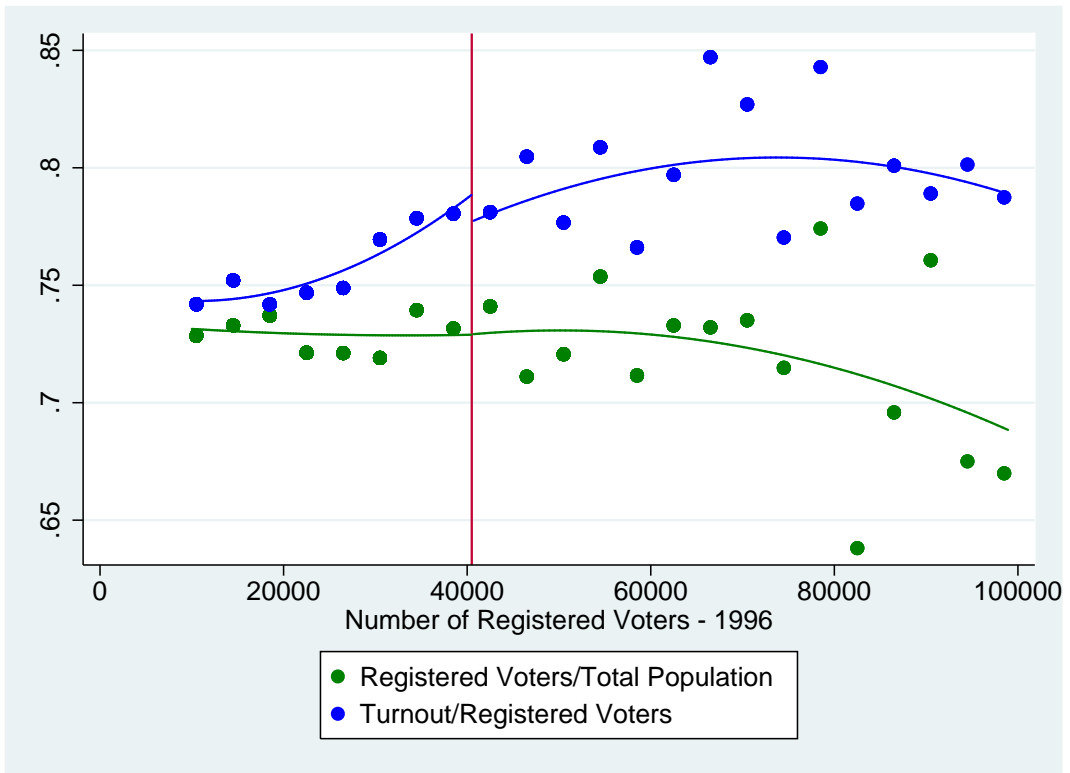


Figure 4: Registration and Turnout - Local Averages and Parametric Fit

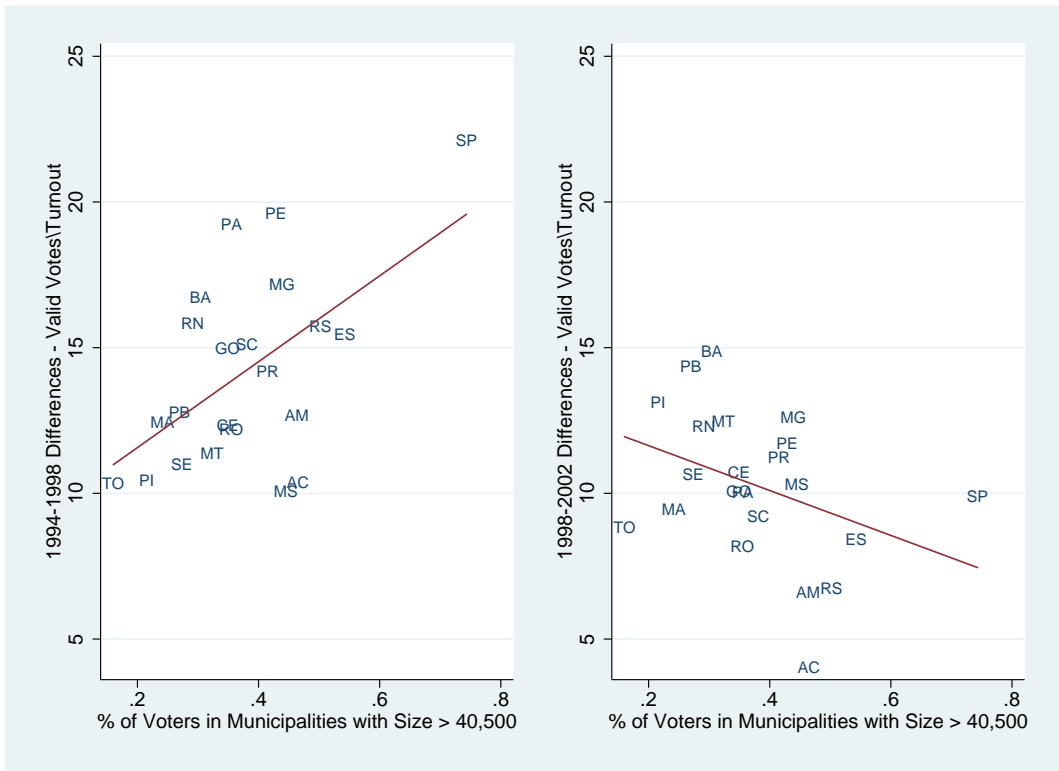


Figure 5: Electronic Voting Phase-in and Valid Vote Ratios

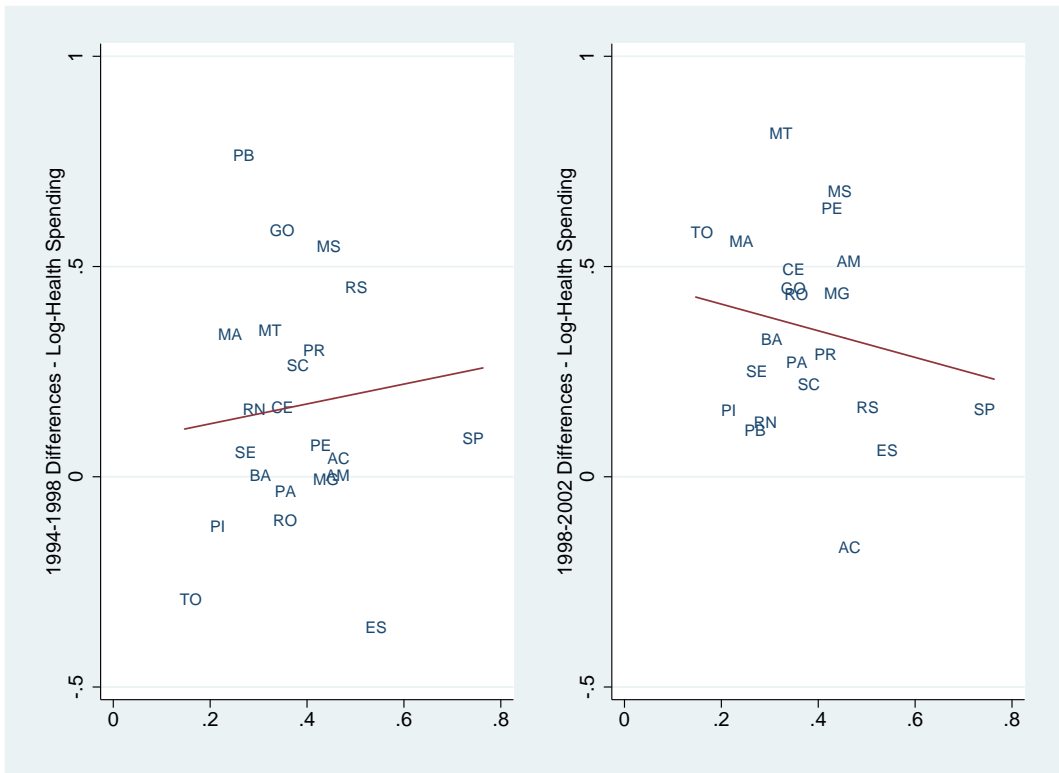


Figure 6: Electronic Voting Phase-In and Health Care Spending

Table 1: Estimated Treatment Effects (in p.p.) - 1998 Election

Bandwidth	Share of Valid Votes	Turnout	Share of Valid Votes - 2002
20.000	13.7 (1.1)***	0.6 (1.2)	0.6 (0.5)
Observations	552	552	552
10.000	13.5 (1.3)***	1.4 (2.1)	0.9 (6.6)
Observations	224	224	224
5.000	14.9 (1.5)***	1.5 (3.3)	1.1 (1.0)
Observations	112	112	112

\* - Significant at the 10% level. \*\* - Significant at the 5% level. \*\*\* - Significant at the 1% level.

Robust standard errors are in parenthesis. Each figure on the table comes from a different local linear regression estimate using a rectangular kernel with the specified bandwidth.

Table 2: Estimated Treatment Effects Interacted with Education and Income

Indep. Variable:	(1)	(2)	(3)	(4)
Valid Vote Ratio				
Treat. Dummy	13.5 (1.5)***	27.4 (4.2)***	10.9 (2.0)***	42.5 (12.1)***
Treat. Dummy*log(income)	-	-2.6 (0.8)***	-	-4.9 (1.9)***
Treat. Dummy*(share of pop. w/ 4 years of education)	-	-	5.6 (2.5)**	-8.1 (5.4)
Observations:	224	224	224	224

\*\*\* - Significant at the 1% level. \*\* - Significant at the 5% level. Robust standard errors are in parenthesis. Each figure on the table comes from a different local linear regression estimate using a rectangular kernel with a bandwidth of 10,000 voters.

Table 3: Treatment Effects on Party Vote Shares

Dependent Variable - Vote Share of:	Pre-Treatment Mean	Treatment Effect	Observations
Worker's Party	4.6	6.3 (1.8)***	224
Democratic Movement	12.9	-1.2 (3.3)	224
Social Democrats	10.5	2.5 (3.2)	224
Liberal Front	8.3	2.6 (3.2)	224
Democratic Labour	5.7	2.3 (3.3)	224

\*\*\* - Significant at the 1% level. Robust standard errors are in parenthesis. Each figure on the table comes from a different local linear regression estimate using a rectangular kernel with a bandwidth of 10,000 voters.

Table 4: State-Level Regressions of Enfranchisement Effect

Dep. Variable: Valid Votes \ Total Votes

Indep. Variable: Share of voters residing in municipalities with size>40,500

	Falsification Test			
Sample:	1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)	Pooled w/ Restriction	2006-2002 (Electr.-Electr.)
	14.6 (5.2)***	-9.9 (3.4)***	12.2 (2.3)***	-1.84 (2.3)
R2	0.97	0.97	0.98	0.95
Obs.	44	44	66	44

\*\* - Significant at the 5% level. Robust standard errors clustered at the state level in parenthesis. Each figure on the table is an estimate from a different regression controlling for fixed effects, time effects and region-time interactions.

Table 5: State Level Regressions: Effects on Fiscal Policy  
 Indep. Variable: Share of voters residing in municipalities with size >40,500

Sample:	Falsification Test			
	1994-1998 (Paper-Disc.)	1998-2002 (Disc.-Electr.)	Pooled w/ Restriction	1990-1994 (Paper-Paper)
Health Care and Sanitation	0.84 (0.37)**	-0.65 (0.33)*	0.83 (0.35)**	-0.31 (0.6)
Education	-0.23 (0.28)	0.14 (0.17)	-0.32 (0.31)	-0.24 (0.37)
Public Safety	-0.99 (1.18)	0.81 (0.67)	0.83 (0.95)	0.74 (0.65)
Total Spending	-0.28 (0.21)	-0.18 (0.10)	-0.14 (0.21)	0.03 (0.19)
Obs.	44	44	66	44

\*\* (\*) - Significant at the 5% (10%) level. Robust standard errors clustered at the state level in parenthesis. Each figure on the table is an estimate from a different regression controlling for fixed effects, time effects and region-time interactions.