Essays on the Political Economy of
Local Brazilian Governments

by

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Abstract

Recent research has stressed the role of political institutions in economic development. This thesis aims at shedding light on this issue by empirically analyzing the political determinants of policymaking and its consequences to living standards in a developing economy setting. Each of its three chapters presents a separate essay.

The first chapter addresses how the political participation of disadvantaged groups can be fostered. It studies the introduction of electronic technology that facilitated voting for the less educated in Brazilian elections. Using a regression discontinuity design embedded in its phase-in, it provides evidence that electronic voting reduced residual (uncounted) votes and generated the de facto enfranchisement of a large fraction of the less educated (poorer) parts of the electorate.

The second chapter tests if this additional political participation of poorer voters shifted public policymaking in a way that benefited them. It finds evidence that electronic voting increased the number of state legislators that are themselves less educated and shifted government spending towards public health care, a government policy that disproportionately benefits the less educated, leading to improved utilization (number of pre-natal visits) and lower prevalence of low-weight births in this group. No effects on health care
utilization by the more educated and on the weight of their newborns are found, suggesting that electronic voting indeed empowered the less educated.

Lastly, the third chapter addresses the empirical relevance of strategic voting, a key issue in theoretical and policy analysis of political institutions. It uses exogenous variation in electoral rules to test the predictions of strategic voting models and the causal validity of Duverger’s Law. Estimations based on a regression discontinuity design in the assignment of single-ballot and dual-ballot electoral systems in Brazilian mayoral races indicate that, in accordance to Duverger’s Law, single-ballot plurality rule causes voters to desert third placed candidates and vote for the two most popular ones. It finds that the effects are stronger in close elections, and that candidates’ characteristics and entry cannot account for the results, suggesting that strategic voting is the driving force behind these findings.
# Table of Contents

- **Abstract** ....................................................... ii
- **Table of Contents** .............................................. iv
- **List of Tables** .................................................. vii
- **List of Figures** ................................................... ix
- **Acknowledgements** ............................................. xi
- **Dedication** ....................................................... xii

1 **Introduction** .................................................. 1

2 **The Impact of Electronic Voting on Political Participation** 6
   2.1 Electoral Rules and Voting Technology in Brazil ............ 8
      2.1.1 Introduction of Electronic Voting and the Design of
            this Study ................................................. 11
   2.2 Data and Estimation Framework ................................ 13
      2.2.1 Municipal Data on Electoral Outcomes ..................... 13
   2.3 Regression Discontinuity Results ............................. 16
      2.3.1 Electronic Voting Increases Valid Votes .................. 16
# Table of Contents

2.3.2 Electronic Voting and the Enfranchisement of Less Educated Voters .............................................. 20

3 Political Participation, Health Care Provision, and Infant Health ................................................................. 28

3.1 Background and Data .................................................. 32

3.1.1 The Brazilian Health Care System .............................. 32

3.1.2 Newborn Health Status ............................................. 36

3.1.3 State Level Panel Data .............................................. 37

3.2 Identification Strategy and Estimation Framework ............... 39

3.3 State-level Results .................................................... 45

3.3.1 Electronic Voting, Legislator Education, and Health Care Spending .................................................. 45

3.3.2 Robustness and Falsification Tests ............................... 48

3.3.3 Discussion of Results: Health Care Funding ................. 51

3.3.4 Effects on Health Care Utilization and Outcomes ........... 53

4 Testing Strategic Voting and Duverger’s Law .......................... 58

4.1 Theoretical Framework ............................................... 63

4.2 Empirical Strategy .................................................. 66

4.2.1 Background and Data ............................................. 66

4.2.2 Identification Strategy ............................................ 69

4.2.3 Estimation Framework ............................................. 72

4.3 Results .................................................................. 74

4.3.1 Main Result: the Causal Validity of Duverger’s Law .... 74

4.3.2 Tests for Quasi-Random Assignment ......................... 78
# Table of Contents

4.3.3 Testing Further Predictions of Strategic Voting ........ 81
4.3.4 A Falsification Test: Municipal Legislature Elections .... 84

5 Conclusion ..................................................... 86

Bibliography ...................................................... 119

Appendices

Appendix A: 
  Summary Statistics ............................................ 130

Appendix B: 
  Additional Results on the Effects of Electronic Voting .. 131

Appendix C: 
  Robustness Checks of RDD Estimates ....................... 134

Appendix D: 
  Additional Effects on Pre-Natal Visits ...................... 138

Appendix E: Treatment Effects of Dual Ballots with Controls 140

Appendix F: TE of Dual Ballots in Elections Predicted to Con-
tested and Uncontested ........................................ 142
### List of Tables

1. Treatment Effects of Electronic Voting ........................................ 100
2. Treatment Effects Interacted with Education ............................... 101
3. Effects of Electronic Voting by List Characteristics ..................... 102
4. Effect of Electorate Share above Cutoff through Time: Election Outcomes and Health Care Funding ............................... 103
5. State-Level Effects of Electronic Voting: Election Outcomes and Health Care Funding ........................................ 104
6. Effect of Electorate Share above Cutoff through Time: Health Care Utilization and Outcomes ............................... 105
7. State Level Effects of Electronic Voting: Health Care Utilization and Outcomes ........................................ 106
8. Treatment Effects of Changing from Single-Ballot to Dual-Ballot on Electoral Outcomes ........................................ 107
10. Treatment Effects on Vote Margins ........................................ 109
11. Treatment Effects in Contested and Uncontested Elections ............ 110
12. Falsification Test: Treatment Effects on Municipal Legislature Election Outcomes ........................................ 111
### List of Tables

<table>
<thead>
<tr>
<th>Table</th>
<th>Description</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>A1</td>
<td>Summary Statistics for Municipal Samples</td>
<td>112</td>
</tr>
<tr>
<td>A3</td>
<td>Descriptive Statistics</td>
<td>114</td>
</tr>
<tr>
<td>A4</td>
<td>Additional Estimated Treatment Effects of Electronic Voting</td>
<td>115</td>
</tr>
<tr>
<td>A5</td>
<td>Effects of Electronic Voting on Number of Pre-Natal Visits</td>
<td>116</td>
</tr>
<tr>
<td>A6</td>
<td>Treatment Effects with Covariates</td>
<td>117</td>
</tr>
<tr>
<td>A7</td>
<td>Treatment Effects in (Predicted) Contested and Uncontested Elections</td>
<td>118</td>
</tr>
</tbody>
</table>
# List of Figures

1. Example of a Paper Ballot ........................................... 88
2. Interface of the Electronic Voting Device ........................... 89
3. Timeline of Elections and Legislative Terms ......................... 90
4. Valid Votes/Turnout - Local Averages and Parametric Fit ....... 91
5. Figure 5: Valid Votes/Turnout - Local Averages and Parametric Fit .......................................................... 91
6. Education and Health Insurance Use .................................. 92
7. Share of Electorate Using Electronic Voting - 1998 Election ... 92
8. Electronic Voting Phase-in and Valid Voting ......................... 93
9. Electronic Voting Phase-In and Share of Low Educated Legislators .......................................................... 93
10. Electronic Voting Phase-In and Health Care Spending ............ 94
11. Electronic Voting Phase-In and Pre-Natal Visits .................... 94
12. Electronic Voting Phase-In and Low Birth-Weight .................. 95
13. Vote Share of Third and Lower Placed Candidates - Local Averages and Parametric Fit ........................................... 96
14. Turnout, Registration and Entry - Local Averages and Parametric Fit .......................................................... 96
### List of Figures

<table>
<thead>
<tr>
<th>Figure</th>
<th>Title</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>15</td>
<td>Sample Distribution - Local Averages and Parametric Fit</td>
<td>97</td>
</tr>
<tr>
<td>16</td>
<td>Vote Share of Third and Lower Placed Candidates, Previous Election</td>
<td>97</td>
</tr>
<tr>
<td></td>
<td>- Local Averages and Parametric Fit</td>
<td></td>
</tr>
<tr>
<td>17</td>
<td>TS and SF ratios- Local Averages and Parametric Fit</td>
<td>98</td>
</tr>
<tr>
<td>18</td>
<td>Outcomes of Legislature Elections - Local Averages and Parametric</td>
<td>98</td>
</tr>
<tr>
<td></td>
<td>Fit</td>
<td></td>
</tr>
<tr>
<td>A1</td>
<td>Testing Manipulation of Forcing Variable</td>
<td>99</td>
</tr>
<tr>
<td>A2</td>
<td>Testing Manipulation of the Threshold Position</td>
<td>99</td>
</tr>
</tbody>
</table>
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Chapter 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 different political and economic outcomes. Aiming to shed light on this issue, this thesis empirically analyses the consequences of different features of the electoral process in local (state and municipal) Brazilian governments.

The first two chapters address 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

¹For 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).
Chapter 1. Introduction

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.

By focusing on a mundane impediment to electoral influence, this thesis departs from a previous literature that 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).

Chapter 2 addresses how the political consequences of electronic voting adoption using data at the municipal level. It argues that electronic technology facilitates voting for the less educated in Brazilian elections. Using a regression discontinuity design embedded in its phase-in, it provides evidence that electronic voting reduced residual (uncounted) votes and generated the *de facto* enfranchisement of a large fraction of the less educated (and poorer) parts of the electorate.

Chapter 3 tests if this additional political participation of poorer voters shifted public policymaking in a way that benefited them, using data of policy choices and outcomes observed at the state level. It finds evidence that electronic voting increased the number of state legislators that
are themselves less educated and shifted government spending towards public health care, a government policy that disproportionately benefits the less educated, leading to improved utilization (number of pre-natal visits) and lower prevalence of low-weight births in this group. I find no effects on health care utilization by the more educated and on the weight of their newborns, suggesting that electronic voting indeed empowered the less educated.

The estimates provided on Chapters 2 and 3 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..

Together, these two chapters communicate 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.\footnote{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).}

Secondly, it contributes to the analysis of \textit{de jure} enfranchisement episodes,
Chapter 1. Introduction

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 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. 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.

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). 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.

Chapter 4 analyses the consequences of another possible intervention on the political system: changes in electoral rules. It addresses its political consequences and how those relate to the empirical relevance of strategic (or tactical) voting - when voters cast a vote for a candidate that is not their preferred one. Understanding how voters express their preferences when

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3For example, Almond et al. (2006, forthcoming) study of the impact of hospital desegregation on birth-weight and child mortality of African-Americans in the US South serve as an example.

4Acemoglu (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.

5See also several other papers and reports associated with the MIT/Caltech Voting Technology Project (website: vote.caltech.edu/drupal/).
casting a vote is a key issue in theoretical and policy analysis of political insituations, as most political economy models featuring 3 or more candidates would have their predictions altered by the choice of assuming that voters act sincerely or strategically.

I use exogenous variation in electoral rules to test the predictions of strategic voting models and the causal validity of Duverger’s Law. Estimations based on a regression discontinuity design in the assignment of single-ballot and dual-ballot electoral systems in Brazilian mayoral races indicate that, in accordance to Duverger’s Law, single-ballot plurality rule causes voters to desert third placed candidates and vote for the two most popular ones. I find that the effects are stronger in close elections, and that candidates’ characteristics and entry cannot account for the results, suggesting that strategic voting is the driving force behind these findings.
Chapter 2

The Impact of Electronic Voting on Political Participation

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. 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 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

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6Figures from the 1991 Census.
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, this chapter estimates 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.
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 (Assembleia Legislativa) and a directly elected governor (Governador). 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. 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.\footnote{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 distribution of the seats.}
2.1. Electoral Rules and Voting Technology in Brazil

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.\textsuperscript{10} 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 his chosen candidate’s name or electoral number, a five-digit code assigned by the election authority.\textsuperscript{11} 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.

\textsuperscript{10} 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.

\textsuperscript{11} 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).
and reduce costs in vote counting.\textsuperscript{12} 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 “\textit{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.}”\textsuperscript{13}

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 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 “\textit{a surprise}.”\textsuperscript{17}

\textsuperscript{12}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.

\textsuperscript{13}Statement from the Brazilian Electoral Authority IT Director (Jorge Freitas), available on video at its official website (www.tse.gov.br).
2.1. Electoral Rules and Voting Technology in Brazil

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.\(^{14}\) 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.\(^{15}\) Figure 3 provides the timeline of the introduction of electronic voting.

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, \textit{ex ante} similar to each other in every possible aspect. In other words, the reason that they are on

\(^{14}\)Election are always held on the first or second sunday of October, with elected officials being inaugurated on January 1st.

\(^{15}\)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 chapter, however, focuses only on state elections.
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\textsuperscript{16} and the forcing variable is the number of voters registered for municipal elections two years earlier in 1996, this is likely the case. 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.\textsuperscript{17} These states are dropped from all samples used in this Chapter.

\textsuperscript{16}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 \textit{Folha de São Paulo}.

\textsuperscript{17}Note, however, that election violence (such as observed in some African countries) is
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 cutoff used electronic voting in 1998. 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

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but will be used as robustness checks of the state panel estimates on Chapter 3.

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Note that

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not common in Brazil. Army protection is usually needed to allow elected officials to enter high-crime areas.

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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 section discusses why accounting for this almost negligible deviation from the discontinuity design has no impact on the estimates.
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 any candidate. Such a vote is depicted on Panel C of Figure 2.\footnote{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.}

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.

**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:

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

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) are continuous at the threshold.

I estimate (2.1) 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 $v \in [40,500 - h; 40,500]$. 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 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.

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

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

(2.2)

Where $1\{v > 40,500\}$ is a dummy variable that takes value one if, and only

\footnote{Note that the regression is unweighted (i.e., rectangular kernel)}

\footnote{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.}
2.3. Regression Discontinuity Results

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 $\beta$ 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.\footnote{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.}

2.3 Regression Discontinuity Results

2.3.1 Electronic Voting Increases Valid Votes

Before reporting the estimation results, some graphical evidence is provided. Figure 4 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.
2.3. Regression Discontinuity Results

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 5 repeats the exercise carried out in Figure 4 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
2.3. Regression Discontinuity Results

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.\(^{23}\)

Panel A of Table 1 presents the local polynomial regression estimates of the treatment effects. Irrespective of the bandwidth and specification used, the estimated effect of a switch from paper ballot to 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 4 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 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

\(^{23}\)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”. 

18
2.3. Regression Discontinuity Results

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 \textit{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.\footnote{The figure is from a 2008 survey by the Instituto Nexus. The Economist magazine argued that Brazil “\textit{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.} 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 elec-
2.3. Regression Discontinuity Results

torate, it does not address the issue of the identity of these newly enfran-
chised 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\(^{25}\) and that electronic voting makes voting simpler for less educated voters with
poorer reading and writing skills, and this section provides three pieces of
evidence supporting this hypothesis.

Firstly, the effect of electronic voting is stronger in municipalities with
less educated populations.\(^{26}\) Secondly, parties that are particularly popular
among the less educated observed an increase in their vote shares in mu-
nicipalities 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 thresh-
old (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 par-
ties or candidates.

The second and third results are also useful as they rule out the possi-
bility 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

\(^{25}\) Shue and Luttmer (2009) show that lower education predicts misvoting in a Califor-
nian election.

\(^{26}\) 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.
2.3. Regression Discontinuity Results

Table 1 interacting its explanatory variables with the share of adults (aged 25+) without complete primary education. The near-zero effect of electronic voting by itself and the large size of 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 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

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27Primary 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 Chapter 3. However, I repeated all estimations of this chapter using illiteracy rates and the share of population without 4th grade completion, finding similar results.
2.3. Regression Discontinuity Results

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.\(^{28}\) 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 Brazil-

\(^{28}\)See the description of the proportional representation electoral rule used in state legislature election on Section 2.1.
2.3. Regression Discontinuity Results

ian 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 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:

$$P_{\text{votes}} = \frac{\sum_m [votes_{lm} \cdot (\text{population share without primary education})_m]}{\sum_m votes_{lm}}$$  \hspace{1cm} (2.3)

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. $P_{\text{votes}}$ 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 $P_{\text{votes}}$ 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 $P_{cand}$. 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:

$$\frac{\text{votes}_{lm}}{\text{turnout}_{m}} = \lambda 1\{v_{m} > c\}_m + \psi P_{l}1\{v_{m} > c\}_m + \phi P_{l}^{cand}1\{v_{m} > c\}_m + f_{l}(v_{m}) + \epsilon_{lm}$$

(2.4)

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 $\psi$ and $\phi$. They estimate the impact
2.3. Regression Discontinuity Results

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 (2.4) 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 \( P_{votes} \) distribution (0.63), and 0.70 p.p. for one in the 9th decile (0.81). For the case of \( P_{votes} \), 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 chapter 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 \textit{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 chapter.
Chapter 3

Political Participation, Health Care Provision, and Infant Health

Motivated by the previous results, this chapter provides evidence that the \textit{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.\textsuperscript{29}

\textsuperscript{29}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.
Chapter 3. Political Participation, Health Care Provision, and Infant Health

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.\footnote{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.}

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.\footnote{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.}
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 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.\textsuperscript{32}

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 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

\textsuperscript{32}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.
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.

As before, this chapter provides the relevant background, a description of the data and estimation framework, and the results, in three separate sections.

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.\(^3\)

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

\(^3\)Municipalities are responsible for 20% and the federal government for the remainder (Lisboa and Viegas, 2002).
additional price, which tend to be the more educated (and richer). Figure 6 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. 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.

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,
3.1. Background and Data

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 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.\textsuperscript{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 (\textit{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

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\textsuperscript{36} The survey was carried out in 2009 by the \textit{Movimento Voto Consciente}. 

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\textsuperscript{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).
3.1. Background and Data

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.\(^{38}\)

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.

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\(^{37}\) 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).
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

\(^{39}\)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).
by medical professionals are also known to lead to premature birth and low birth-weight.\textsuperscript{40}

\subsection*{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. 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 Section 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 (\textit{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.\textsuperscript{41}

The measures of health care utilization and outcomes are from the Na-\textsuperscript{40}Kramer (1987) provides a comprehensive survey of the medical literature on birth-weight determinants. \textsuperscript{41}Sanitation is mainly supplied by companies which, when public, are not considered as part of a state’s budget.
3.1. Background and Data

tional 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.

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

\(^{42}\)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 guidelines of the American Congress of Obstetricians and Gynecologists.
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%.\footnote{The average state has about 42 thousand live births by educated mothers and 58 thousand by uneducated ones.}

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 (\textit{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.

\section*{3.2 Identification Strategy and Estimation Framework}

The identification strategy exploits the pattern of electronic voting adoption across states and time. As discussed in the previous chapter, 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,
3.2. Identification Strategy and Estimation Framework

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 3 presents this timeline graphically.

Figure 3 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. 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 3:

- The 2003-2006 legislative term, elected in the 2002 “electronic-only” election.\(^4\)

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, \(S_i\)

\(^4\)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).
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 \( \text{electro}_{ie} \) denote the share of voters using electronic voting in state \( i \) at an election held at year \( e \), then:

\[
\text{electro}_{i1998} - \text{electro}_{i1994} = S_i \quad (3.1)
\]

\[
\text{electro}_{i2002} - \text{electro}_{i1998} = 1 - S_i \quad (3.2)
\]

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 (3.1) and (3.2) that \( (\text{electro}_{i1998} - \text{electro}_{i1994}) = S_i \) and \( (\text{electro}_{i2002} - \text{electro}_{i1998}) = \)
(1 – Si). Hence, in the period where Si 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 Si is inversely proportional to electronic voting adoption, it predicts smaller changes in spending.\footnote{In other words, the share of voters above the cutoff in 1998 (Si) \textbf{positively predicts the change} in spending between the 1995-1998 and 1999-2002 legislative terms when Si 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, Si \textbf{negatively predicts the change} in spending between the 1999-2002 and the 2003-2006 legislative terms, and in this case 1 – Si equals the change in electronic voting adoption between the elections that selected these legislatures (held in 1998 and 2002, respectively).}

Apart from 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 (Si) 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 (Si) 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 Si is not associated with changes voting technology, it is also not associated with changes in outcomes. This 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,
3.2. Identification Strategy and Estimation Framework

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 \( y_{it} \):

\[
y_{it} = \alpha + \theta^{99-02} S_i \cdot Term^{99-02}_t + \gamma_i + \delta_t + \epsilon_{it} \; ; \; t \in [1995, 2002] \tag{3.3}
\]

\[
y_{it} = \alpha + \theta^{03-06} S_i \cdot Term^{03-06}_t + \gamma_i + \delta_t + \epsilon_{it} \; ; \; t \in [1999, 2006] \tag{3.4}
\]

where \( Term^{99-02}_t \) is a dummy indicator for years belonging to legislative term 1999-2002 (elected in the 1998 “discontinuity” election), and \( Term^{03-06}_t \) is a dummy for years belonging to the legislative 2003-2006 (elected in the 2002 “electronic-only” elections). Year (\( \delta_t \)) and state fixed (\( \gamma_i \)) 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 \( \theta^{99-02} \) measures the effect of \( S_i \) on the change in average \( y_{it} \) between the 1995-1998 and 1999-2002 legislative terms. Equation (3.1) indicates that \( S_i \) equals the change in the electronic use in the elections that selected these legislatures (1994 and 1998, respectively). Hence, \( \theta^{99-02} \) is an estimate of the impact of electronic voting on \( y_{it} \).

On the other hand, equation (3.2) implies that \( \theta^{03-06} \) 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 \( \theta^{03-06} \) measures the effect of \( S_i \) on the change between these two
3.2. Identification Strategy and Estimation Framework

legislature, \(-\theta_{03-06}\) is the effect of electronic voting on the outcome \(y_{it}\).

Hence, evidence that \(\theta_{99-02} = -\theta_{03-06} \equiv \theta\) makes the case that \(\theta\) measures the causal effect of electronic voting. Throughout the analysis, I estimate equation (3.3) and (3.4) independently from each other, so that no particular relationship between the parameters \(\theta_{99-02}\) and \(\theta_{03-06}\) is “forced” by the estimation. In the cases where the results imply that \(\theta_{99-02} = -\theta_{03-06}\) holds, equations (3.3) and (3.4) 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 (3.1)-(3.4) 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 (3.3) and (3.4) 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 Section 2.1), comparing the results from specifications that exclude them with that from the full sample creates a robustness check for the estimates.

\footnote{In other words, once fixed effects and time effects are included, equation (3.1) implies that \(S_i \cdot Term^{99-02}_t\) is collinear with electronic voting adoption in equation (3.3), while equation (3.2) implies that \(S_i \cdot Term^{03-06}_t\) is negatively collinear with electronic voting adoption in equation (3.4).}
3.3. State-level Results

Standard errors are clustered at the state level, allowing for arbitrary serial correlation. Note that estimation of (3.3) and (3.4) 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 7, all estimations control for a set of region dummies interacted with time effects, which may absorb geographically restricted shocks affecting the outcomes of interest.\(^{48}\)

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 Chapter 2: the ratio between valid votes and turnout. The graphical representation of empirical strategy can

\(^{48}\)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 Section 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

be found in Figure 8, 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 8 are obtained by estimating equations (3.3) and (3.4) 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 Chapter 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 9, 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 10 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 10, 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 10 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. State-level Results

3.3.2 Robustness and Falsification Tests

The identification strategy is predicated on 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 Chapter 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.\footnote{The enfranchisement of less educated (and hence poorer) voters could increase the size of government through increased redistribution, as Husted and Kenny (1997) found.}
3.3. State-level Results

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.\footnote{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.}

To enhance the precision of the estimated treatment effects, I pool the variation from both equations (3.3) and (3.4) and estimate:

$$y_{it} = \alpha + \theta S_i \cdot Term_{t}^{99-02} + \gamma_i + \delta_t + \beta X_{it} + u_{it} \mid t \in [1995, 2006] \quad (3.5)$$

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).
3.3. State-level Results

where $\text{Term}^{99-02}_t$ is a dummy indicating if year $t$ is part of the 1999-2002 legislative term (1999-2002). Since $S_i \cdot \text{Term}^{99-02}_t$ is perfectly collinear electronic voting adoption when fixed and time effects are present, $\theta$ captures the effect of electronic voting on $y$. Moreover, estimation of (3.5) is equivalent to estimating equations (3.3) and (3.4) 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 $\theta$ 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.
3.3. State-level Results

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 8, 9, and 10), where the estimated slopes are usually similar when the states with the maximum value of $S_i$ are dropped.\textsuperscript{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 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 expe-

\textsuperscript{51}One state - Distrito Federal - is comprised of a single municipality (Brasília, the federal capital) which is above the threshold.
3.3. State-level Results

...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.\footnote{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 enactment of women’s suffrage, with growing effects leading to a 81% increase after 5 years.}

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 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...
3.3. **State-level Results**

to have easier 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
3.3. State-level Results

those that did not. Recall that I refer to these groups as “educated” and “uneducated”, respectively.

Figure 11 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 12 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 11 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
3.3. State-level Results

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 (3.5), 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
3.3. State-level Results

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
3.3. State-level Results

education groups in the same sample and estimates:

\[ y_{eit} = \alpha + \gamma S_i \cdot \text{Term}_{t}^{99-02} \cdot \text{Lowed}_e + \lambda_{it} + \lambda_{et} + \lambda_{ei} + \epsilon_{eit} \]  \hspace{1cm} (3.6)

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 \( S_i \cdot \text{Term}_{t}^{99-02} \) with an indicator for low education group \( \text{Lowed}_e \) 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 \( \gamma \), 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.

\(^{53}\)Without the inclusion of state-year effects, the estimated effect would be the same and the difference between the estimated effect from equation (3.5) reported on columns (1)-(3) in uneducated and educated mothers.
Chapter 4

Testing Strategic Voting and Duverger’s Law

Economists and political scientists have long been interested in the question of whether citizens vote “sincerely” or “strategically.” How voting decisions take place is not only fundamental to the understanding of the democratic process, but also has important implications on how theory is made. Virtually any economic model with voting for three or more candidates requires the assumption that voters act either sincerely or strategically and this choice usually has important implications for the model’s results and conclusions.\textsuperscript{54}

The best-known prediction regarding strategic voting in a multi-candidate setting is Duverger’s Law, named after Duverger’s (1954) prediction that “\textit{simple-majority single-ballot} [plurality rule or first-past-the-post] favors the two party system” while “\textit{simple majority with a second ballot} [dual-ballot, described below] or \textit{proportional representation} favors multipartyism.”\textsuperscript{55}

\textsuperscript{54}A compelling example is the citizen-candidate models of Osborne and Slivinski (1996) and Besley and Coate (1997). The structure of the two independently developed models is similar, but the latter assumes strategic voting while the former assumes sincere voting, which results in different equilibrium policies.

\textsuperscript{55}Riker (1982) describes Duverger’s predictions as “\textit{a true sociological law}.”
In this chapter I empirically test Duverger’s Law, addressing the validity of its causal statement by exploring a regression discontinuity design in the assignment of electoral rules in Brazilian municipal elections. I also investigate the mechanisms driving the association between plurality and two-party dominance, providing evidence that strategic voting is driving the results.

Duverger’s rationale was that plurality rule creates an incentive for voters to engage in a particular pattern of strategic voting, which is best described by an example. A citizen believes that candidates 1 and 2 have the highest probability of winning an election (and that a tie between 1 and 2 is more likely than a tie between any other two candidates). His most preferred choice, however, is candidate 3. To maximize his chances of being the pivotal voter, this citizen strategically chooses to vote for his preferred choice between 1 and 2. As all voters go through a similar logic, candidate 3 is deserted by her supporters, which all vote for candidates 1 and 2.¹⁵⁶

Duverger also argued that this rallying behind only two “serious” candidates would not occur under dual-ballot plurality (also known as “runoff”), a system where voters may vote twice. First, an election is held and if a candidate obtains more than 50% of the votes, she is elected. If not, then a second round of elections is held where only the two most voted candidates in the first round face off.¹⁵⁷

Duverger’s argument has been established in formal game-theoretic mod-

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¹⁵⁶ In this chapter, all voters are male and all candidates female.
¹⁵⁷ Dual-ballot is the most used electoral system for presidential elections in the world, and is common in primaries in the Southern United States and several large American cities, as well as regional elections in France, Italy and Switzerland. In some cases, the threshold for first-round victory differs from 50%.
Chapter 4. Testing Strategic Voting and Duverger’s Law

els of voting and generalized to the “\(m+1\) rule” (Cox, 1997; Myerson, 1999): in an election for \(m\) seats, \(m+1\) candidates should obtain all the votes. In a single-ballot plurality rule election, \(m\) equals one and only two candidates receive a positive number of votes, while in (the first round of) a dual-ballot election \(m\) equals two and the vote share of the third-placed candidate is positive. Hence, an exogenous change from single to dual-ballot emulates the theoretical exercise of a ceteris paribus change in \(m\) and allows a test of the \(m+1\) rule.\(^{58}\)

Such exogenous variation is generated by the Brazilian Constitution, which mandates that municipalities with less than 200,000 registered voters use single-ballot plurality rule to elect their mayors. This regression discontinuity design generates assignment of electoral rules that is “as good as random” and allows causal inference of its effects. Intuitively, municipalities “just below” the threshold should be, on average, similar in all observed and unobserved characteristics to those “just above” it, so that any difference in outcomes between these two groups must be caused by the different electoral rules.

Using detailed data on the outcomes of the universe of mayoral elections in Brazil for the 1996-2004 period, I find evidence that, as predicted by Duverger’s Law, a change from single ballot to dual ballot increases voting for the third and lower placed candidates, and it also decreases the vote margins between second and third (and also first and third) placed candidates, while not affecting the margin between first and second placed candidates. Both these results of are stronger in closely contested races where the incentives

\(^{58}\)Section 4.2 briefly discusses this theoretical literature.
to vote strategically are larger. This combination of results, coupled with the fact that they cannot be explained neither by the number of candidates that enter a race nor a measure of their quality (education) make a case that strategic voting takes place in this context.

The results in this chapter communicate with a large number of theoretical analyses of strategic voter coordination, which are discussed briefly in Section 2. Of particular interest are Martinelli’s (2002) and Bouton’s (2010) models of strategic voting in dual ballot elections, with the latter finding that two kinds of equilibria, one where third placed candidates are deserted or another where they are not, can be observed in both single or dual ballot. This ambiguous prediction emphasizes the need for empirical evidence on the subject.

A previous empirical literature compares single and dual ballot elections to test Duverger’s Law, with mixed results: Wright and Riker (1989) and Golder (2006) find support for it, while Shugart and Taagepera (1994) and Engstrom and Engstrom (2008) do not. Differently from my analysis, the endogeneity of electoral rules is an obstacle to the causal interpretation of the results in these papers. This is particular important since political scientists have suggested “a causality following in the reverse direction, from the number of parties towards electoral rules” (Taagepera, 2003).

A notable exception is Bordignon et al. (2010), a paper written independently from this one that explores a similar regression discontinuity design in Italian municipalities. Their findings are that dual ballot leads

59The argument is that societies with a predisposition to the existence of multiple parties will likely select an electoral system that is capable of fitting them in the political arena.
to larger number of candidates and smaller policy variability than single ballots, conforming to the predictions of an analytical model of party formation. Differently from this chapter, the effects on voter behavior are not analyzed.\textsuperscript{60}

This chapter is also related to the literature measuring the extent of strategic voting, which includes small-scale laboratory experiments (surveyed in Rietz, 2008) and analysis of surveys that directly ask respondents about their preferences and votes (e.g., Alvarez and Nagler, 2000).\textsuperscript{61} While the former approach requires dealing with the difficulties in how to elicit preferences and the measurement issues related to survey questions about previous voting behavior (Wright, 1990, 1992; Mullainathan and Washington, 2009), the latter approach leaves open the question whether strategic voting occurs in elections with electorates thousands of times larger than the ones used in experiments.

The chapter is organized as follows. Section 4.1 discusses the theoretical framework, demonstrating how an exogenous change in electoral rules can be used to test the predictions of strategic voting models. Section 4.2 provides the context, data and empirical strategy, while Section 4.3 presents the empirical results.

\textsuperscript{60}Short after the preparation of the original draft of this manuscript, I became aware of an independently developed paper (Chamon et al., 2009) that explores the same regression discontinuity design, but focuses mainly on the effect of electoral rules on fiscal spending. Another independently developed paper (Gonçalves et al., 2008) explores if dual-ballots increase the number of candidates that enter Brazilian mayoral races, using a difference-in-differences approach instead of the regression discontinuity design.

\textsuperscript{61}Also, Kawai and Watanabe (2010) estimate a structural model of voting using aggregate vote shares. Degan and Merlo (2009) analyse a different kind of strategic voting (“split tickets”).
4.1 Theoretical Framework

The strategic voting rationale behind Duverger’s Law is formalized in game-theoretic models by Palfrey (1989), Myerson and Weber (1993), Cox (1994) and Myerson (2002). The overview of these models provided here is brief and the reader is referred to the surveys by Cox (1997) and Myerson (1999) for both the formal analysis and a detailed discussion of which key assumptions drive the results. A population of rational voters have (strict) preferences over a fixed number of candidates and care only about which candidate is eventually elected (i.e., instrumental voting instead of expressive voting). A voter does not know exactly what are the preferences of his peers, but has a belief of its distribution in the population. Additionally, each voter has an expectation of how many votes each candidate will receive.

If expectations are rational with respect to beliefs and all voters share the same beliefs and expectations (i.e., common priors and publicly formed expectations), then with a large enough electorate (such that the expectations are actually a precise prediction of equilibrium behavior) there will be two classes of equilibria. The first, usually referred as Duvergerian, has the third-placed candidate receiving zero votes, while the second, non-Duvergerian class has the third place candidate receiving the exact same amount of votes as the second placed candidate. This occurs because expectations are self-fulfilling: if two candidates are expected to finish in first and second place, they not only will but also receive all the votes. But if

\[ \text{The expectations are rational with respect to beliefs if an electorate whose preferences were in fact distributed according to the belief and voting optimally given the expectations would actually produce vote shares in accordance with the expectations.} \]
expectations are such that a tie is expected between the second and third (and even forth, fifth), then the tie will be observed in equilibrium, since the expectation does not tell the voters who they should “coordinate on”.

Cox (1994, 1997) extends the models to the case of dual-ballot plurality rule (DB, henceforth) and shows that, in the first round of elections, there are either Duvergerian equilibria with the three most voted candidates concentrating all the votes or non-Duvergerian equilibria where the third and fourth placed candidates tie. Moreover, this class of models is generalized to the “$m+1$” rule (Myerson, 1999): in an election for $m$ seats, there exists an equilibrium where only $m+1$ candidates obtain all the votes.

Leaving the non-Duvergerian equilibrium aside for the moment, what the $m+1$ rule imply for the context of SB and DB is that under the former, a third placed candidate will receive zero votes, while in the first round of the latter the third placed candidate will receive a positive amount of votes.\footnote{In an actual SB election the third and lower placed candidates do not receive zero votes, but this evidence is not informative of whether models of Duvergerian strategic voting capture or not a particular feature that exist in the real world. Myatt (2007) uses a “global games” approach to develop a model of single-ballot plurality rule where there is a unique equilibrium where the third placed does suffer from strategic desertion but still receives a positive amount of votes.}

Under SB, $m$ is equal to one, while in the first round of DB, $m$ is equal to two (as a “seat” is then the right to be on the second ballot). Hence, a comparison between SB and DB is much like a “comparative static” of a change in $m$. Moreover, this comparative static generates predictions that are falsifiable, in the sense that they can be tested against data.

The quasi-random assignment of SB and DB to otherwise similar municipalities generated by the regression discontinuity design guarantees that the
4.1. Theoretical Framework

data satisfy the *ceteris paribus* condition of comparative statics. In other words, the isolated variation in electoral rules is an increase in $m$ that is not accompanied, on average, by a change in any other variable.

In light of the discussion above, three particular predictions can be drawn from these theories:

1. The vote share received by the third and lower placed candidates is lower under SB than in the first round of DB.

2. The difference (or ratio) between the vote share of the third placed candidate and the second (or first) placed one is lower under SB than in the first round of DB.

3. The results in (1) and (2) should be particularly salient in elections where a candidate wins by a small margin, while in elections where the winner obtains a vast majority the incentives to vote strategically are diminished.

The first prediction is just a re-statement of Duverger’s Law. The second and third are more subtle predictions of strategic voting. Prediction (2) captures the feature that under SB strategic voting leads to a desertion of the eventually third placed candidate to the benefit of the first and second placed candidates, while under DB the same incentive is not present.

The third prediction comes from the fact that in elections which one candidate is expected to win for sure, there is no incentive to vote strategically. In the formal models, this phenomenon is represented by the equilibria where expectations are such that the probability of a tie between the first-placed
candidate and any other candidate is exactly zero and hence, the (expected) probability of a voter being pivotal is zero no matter who he votes for.

Notice that the predictions only relate how voting under SB compare to voting under the first ballot of DB. Hence, any reference to voting under DB in this paper should be understood as voting in the first round of DB.

In Sections 4.2 and 4.3, the three predictions listed above are tested using the quasi-random assignment of SB and DB in Brazilian municipal races. In other words, models of strategic voting are tested by checking if the data agrees with its comparative static predictions.

4.2 Empirical Strategy

4.2.1 Background and Data

Brazil is constituted by more than 5,000 municipalities, which are the smallest entities of the federation - similarly to a town or city in the United States. Each municipality has its own government, comprised of a single mayor (Prefeito) and a municipal legislature (Câmara de Vereadores), which are elected every four years. The municipal elections are regulated by the federal government, and all municipalities have their elections and change their governments on the same date. Municipalities are not divided into separate districts, so that elections are “at-large”.

Brazilian legislation requires all Brazilian citizens aged 18-70 to register to vote in the municipality they reside. Moreover, the 1988 Constitution stated that mayoral elections should be run under the single-ballot plurality rule system (SB) in municipalities with less than 200,000 voters (as of the end
4.2. Empirical Strategy

of registration period five months prior to election day). SB is the first-past-the-post system used in the United States and United Kingdom: the most voted candidate in the election is awarded with the seat. Municipalities with 200,000 voters or more must have their elections under dual-ballot plurality rule (DB). ⁶⁴

The data on electoral outcomes used in this chapter was collected by the federal elections authority (Tribunal Superior Eleitoral), a branch of the federal judiciary power that oversees and regulates the electoral process. The sample consists of the electoral outcomes (votes obtained by each candidate) and characteristics (turnout, registration) for all Brazilian municipalities in the 1996, 2000 and 2004 elections. ⁶⁵ The unit of observation is a municipality-election and there are over 16,000 observations in the sample, although most estimates will use substantially smaller samples that include only observations close to the 200,000-voter threshold. A table with descriptive statistics can be found in Appendix A.

Although the identity and party affiliation of each candidate is available in the data, that information is not used in the analysis. Instead, focus is given to the vote share of the first placed, second placed and third or lower placed candidates. This should be seen as a particular strength of this test of strategic voting: it requires absolutely no assumption on candidates’ (or voters’) positions and ideologies.

Moreover, it is practically impossible to credibly define policy positions

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⁶⁴ The difference between the two systems is outlined in Subsection 2.1.
⁶⁵ Data for previous elections where not available. However, there is good reason not to use data that is closer to the date where the rule was established (1988). Several checks for internal consistency (e.g., vote shares equal to one, turnout is lower than registration, etc.) were made without any inconsistency being found.
of candidates in mayoral elections. First, there is a large number of parties in activity in Brazil. There are 29 parties that were able to elect at least one mayor in the period analyzed. Even in the sample of 121 elections with more than 150,000 and less than 250,000 voters there are 13 parties that successfully elected a mayor.

Although it would be possible to place these parties in some unidimensional spectrum when it comes to their policy choices at the national level, there would be no guarantee that the same positions would be valid at the local level, especially when one takes into account the size of the Brazilian territory and its large geographical differences. Moreover, evidence from the United States show that parties can have different positions at the national level but implement the same policies at the local level (Ferreira and Gyourko, 2010).

The first noteworthy result from the data is that there is full compliance with the assignment rule: no municipality with less than 200,000 voters had a second ballot and all municipalities with more than 200,000 voters where no candidate obtained more than 50% of the votes in the first round of election had a second round of election. Hence, the regression discontinuity design is “sharp”.

Multi-candidate elections seem to be the norm: 95% of the elections with more than 125,000 voters and less than 275,000 voters have at least three candidates receiving a positive number of votes (and 78% of them have four or more candidates). Throughout this chapter, all estimations are carried out using only the sample of elections that had three or more candidates.⁶⁶

⁶⁶All estimations in the chapter were also carried out using samples including two-
4.2.2 Identification Strategy

This subsection discusses the empirical strategy and its possible threats to validity. Focus is given to an intuitive description, leaving the formal aspects to a later section.

In order to obtain causal estimates of Duverger’s Law and successfully emulate the comparative statics of strategic voting models in the data, we need the assignment of SB and DB to be "as good as random".

Intuitively, this is expected to occur because the municipalities falling "just below" and "just above" the threshold should be, on average, \textit{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 electoral rule should be the same in the SB and DB 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 electoral rules.

For this to hold, it is important that the 200,000-voter threshold is somewhat arbitrary and not used to assign anything else to municipalities. To the best of my knowledge, this is the case. Although some other regulations of municipal governments depend on its population (which is different from its number of voters), none of them seem to have a threshold close to 200,000.

The cutoff was established by the federal congress when a new Consti-
4.2. Empirical Strategy

tution was written in 1988 and the reason for it seems to be that, although
dual-ballot was deemed superior to SB, the cost of a possible second round of
elections in the universe of municipalities was prohibitive. Moreover, even if
Congress was aiming to keep a particular group of cities with or without DB
when choosing the value of the threshold, by 1996 (when the first election
in the sample was held) the different rates of population and registration
growth between cities would have dissipated any effect.

Another threat to validity would occur if a change from SB to DB af-
affected the decision to vote or not as well as the decision on who to vote for.
If different groups of voters attend the polls under the different electoral
rules, then the research design may not be able to successfully emulate the
comparative statics of strategic voting models. In other words, the “just
below” and “just above” groups would not have similar voters.

Brazilian law makes registration and voting compulsory for all citizens
aged 18-70.\footnote{Voting is voluntary for citizens aged 16-17 or 70+ and to those who are officially illiterate.} Failing to register or vote in a previous election renders a
citizen ineligible to virtually all public provided services,\footnote{This includes attending public schools, receiving payments from social programs, ob-
taining 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.} 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,\footnote{Figure 14 and Table 8 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”.} it makes
4.2. Empirical Strategy

the issues related to election outcomes (e.g., how close the race is) second-order in the decision to vote or not and hence the difference in turnout under SB and DB is virtually zero, as is the difference between turnout in the first and second round of DB municipalities.\textsuperscript{70}

Another serious issue is the possibility of strategic manipulation of the forcing variable. If, for any reason, some agent (such as a party or the government) had a preference for SB or DB, it could try to manipulate the registration of voters in order to fall on the preferred side of the threshold.\textsuperscript{71} This kind of behavior would likely invalidate the analysis, since some amount of “self-selection” would occur between SB and DB rules. However, if strategic registration does indeed take place, it would likely be reflected in a discontinuity in registration rates or in the number of cities that are above or below the threshold. Both of these features can be tested (and rejected) in the data.

Finally, one should not just accept that a regression discontinuity design generates quasi-random assignment. Just like in randomized controlled experiments it is usual to test for the possible failure to randomize by comparing predetermined variables on treatment and control groups. In this context it is important to look for a treatment effect in outcomes that were determined before the assignment of electoral rules, where it should not exist.

\textsuperscript{70}In the full sample, there are 92 elections that experienced a second round. A regression of the turnout rate in the second round against the turnout rate in the first round (without a constant) yields an estimated parameter of 0.97 (clustered standard error = 0.002) and a R2 of 0.99. This implies that the turnout rates are virtually the same in the first and second round.

\textsuperscript{71}Voters could prefer to register in SB municipalities in order to avoid having to attend the polls twice.
4.2. Empirical Strategy

4.2.3 Estimation Framework

Let \( v \) be the number of registered voters in a municipality. The treatment effect of moving from single-ballot to dual-ballot can on outcome \( y \) can be estimated by:

\[
TE = \lim_{v \downarrow 200,000} E[y|v] - \lim_{v \uparrow 200,000} E[y|v] \tag{4.1}
\]

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 200,000 voters and DB, while the second term converges to the expected outcome of a municipality with 200,000 voters and SB. Hence, \( TE \) identifies the treatment effect of changing from SB to DB for a municipality of 200,000 voters, as long as the distribution of treatment effects is continuous at threshold.

The estimation method used here closely follows the guidelines in Imbens and Lemieux (2008), 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\(^{72}\) of \( y \) on \( v \) using only data on where \( v \in [200,000 - h; 200,000] \). The predicted value at \( v = 200,000 \) is thus an estimate of the limit of \( y \) as \( v \uparrow 200,000 \). Similarly, a regression using only data satisfying \( v \in [200,000; 200,000 + h] \) is used to estimate the limit of \( y \) when \( v \downarrow 200,000 \). The difference between these two estimated limits is the treatment effect. It is important to notice the non-parametric nature of the estimation: although

\(^{72}\) Notice that the regression is unweighted (i.e., rectangular kernel).
4.2. Empirical Strategy

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 $v \in (200,000 - h; 200,000 + h)$.

$$y = \alpha + \gamma 1\{v > 200,000\} + f(v) + u \quad (4.2)$$

Where $1\{v > 200,000\}$ is a dummy variable that takes value one if, and only if, the election is carried under DB, $u$ 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 and cluster-robust) standard error can be obtained in a straightforward manner. $f(\cdot)$ is a linear or quadratic polynomial (fully interacted with $1\{v > 200,000\}$), and $u$ is the error term and the parameters to be estimated are denoted in greek letters.

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 chapter presents the results for three different levels: 25,000; 50,000, and 75,000. Notice that these are relatively small and hence try to reinforce the “local” intuition of regression discontinuity designs: although there are more than 15,000 observations in the data, less than 200 are used to obtain all of the estimates.\textsuperscript{73}

\textsuperscript{73}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 200,000 threshold.
To further test robustness, estimates where the limits are approximated with a quadratic instead of linear relationship, were also used.

4.3 Results

This section presents the empirical results. As is standard practice in the literature using regression discontinuity designs, a mix of graphical evidence and formal estimates are provided.

I start by providing the main result, the causal validity of Duverger’s Law, and evidence against the possible threats to validity in Section 4.3.1. This is followed by evidence in favor of quasi-random assignment of electoral rules (Section 4.3.2). Tests of further predictions of Duvergerian strategic voting models are presented in Section 4.3.3, and Subsection 4.3.4 concludes with a falsification test based on the results of municipality legislature elections.

4.3.1 Main Result: the Causal Validity of Duverger’s Law

Figure 13 presents the main outcome variable, the share of votes that were received by the third and lower placed candidates, against the forcing variable (registered voters). Each point in the figure reflects the average outcome for a bin of municipalities that fall within a 25,000-wide interval of the forcing variable. For example, the first point to the right of 200,000 equals the average vote share of third and lower placed candidates in municipalities with \( v \in [200,000; 225,000] \). To facilitate visualization, a quadratic model is fitted at each side of the 200,000 threshold, so that the point where the lines
are not connected (and a vertical line is plotted) is where the discontinuity in outcomes, if existent, is expected to be visible.\textsuperscript{74}

There is a clear jump at the cutoff value, while the relationship is smooth elsewhere. This implies that votes for the third or lower placed candidates in the election increase by about 10 p.p. as there is a move from SB to DB. This increase is large when taken into account that the vote share is about 15%-20% to the left of the threshold: DB increases the voting for the third (and lower) candidates by more than 50%.

The formal estimates are provided in the first row of Table 8. Columns (1)-(3) presents the results for different bandwidths using the local linear regression, while columns (4) and (5) probe the robustness of the result by using a quadratic specification. Throughout the chapter, the estimate presented is the treatment effect of changing from SB to DB. In the program evaluation jargon, DB is the “treatment” and SB is the “control.” To help evaluate the magnitude of the effects, the “single-ballot mean” - the average for municipalities within a 25,000-voter interval below the 200,000-voter threshold - is presented. All standard errors are clustered at the municipality level and hence are robust to serial correlation of unknown form.

The estimated treatment effects are significant at the 5% level and imply a large positive effect (10-15 p.p.) of DB (as opposed to SB) on the votes of third and lower placed candidates. This is consistent with more than half the voters who would vote for the third placed candidate under DB strategically deserting her and voting for the top two candidates under SB. The fact

\textsuperscript{74}In some graphics, a quadratic relationship is fitted, while in others a linear one is used. The decision on which one to use is made by simply testing one against the other.
that the results remain positive and significant throughout columns (1)-(5) shows that the result is robust to different bandwidths and specifications. Appendix E shows that the estimates above are also robust to the inclusion of several different covariates and also time dummies.

A possible explanation for this result is that DB increases the number of candidates that enter the race, so that the share of votes captured by third and lower placed candidates under this system could be higher by the mechanical reason that there are more candidates to finish third and lower. Previous theoretical research proposed that DB increases the number of candidates entering the race compared to SB (Osborne and Slivinski, 1996, Bordignon et al., 2010), with the latter study - and also Wright and Riker (1989) - finding that there is indeed a larger number of candidates under DB than SB.

However, I find only weak evidence that electoral rules affect the entry of candidates in the Brazilian context. Figure 14 plots the number of candidates\(^\text{75}\) against the number of registered voters. There is no visible jump at the 200,000 voter threshold, but only a clear upward trend,\(^\text{76}\) which would make a naive comparison of municipalities with SB and DB misleading when one does not pay due attention to the “difference at the limits” nature of regression discontinuity designs. The last row of Table 8 presents

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\(^{75}\)This is defined as the number of candidates that received at least one vote in the election. Hence, candidates who entered the race and then withdrew (either voluntarily or through the actions of the elections authority) are not included.

\(^{76}\)This could be explained by the fact that the “payoff” of being a mayor in a larger municipality is larger, or that running for mayor in larger municipalities generates better opportunities for candidates who are looking to increase their visibility for future statewide elections, or simply that larger cities have a larger pool of potential politicians to run for office.
4.3. Results

the estimated treatment effects on the number of candidates and, although all estimates are positive, none are statistically insignificant at acceptable levels.

More importantly, I am able to rule out that the results are driven by candidate entry by re-estimating the treatment effects on the vote share of third and lower placed candidates while adding the number of candidates as a covariate. The results are presented in the second row of Table 8, and they show that controlling for the number of candidates only makes the treatment effects slightly smaller, without affecting the sign and significance of the results.

To present evidence that the results cannot be explained by different quality of candidates under each electoral rule, I estimate models controlling for the share of candidates that have a college/university degree ("ensino superior completo"). Data on candidates’ education, however, is only available for the 2000-2004 races. The results, presented on the third row of Table 8, show that the results cannot be explained by entry of candidates of different levels of education on SB and DB races.

In order to assess the threats to validity, Figure 14 repeats the exercise of Figure 13 for the turnout rate (total turnout divided by the number of registered voters) and the registration rate (ratio of registered voters to the total population in the municipality). The relationship between these variables and the number of voters is smooth and does not present a jump at the threshold. Hence, the increase in votes for third and lower placed candidates

\[\text{In the 2000 races, it is also possible to determine the education of the third placed candidates. Adding dummies of third-placed candidate education levels does not affect the results significantly either.}\]
4.3. Results

is not likely driven by differences in turnout in SB and DB municipalities, just as there is no evidence that strategic manipulation of the number of registered voters has taken place. The formal counterpart is provided in the second and third row of Table 8, where one can see that estimated treatment effects on turnout and registration are numerically small and statistically insignificant.

To further probe the possibility of strategic manipulation, Figure 15 implements an exercise suggested by McCrary (2008) and plots the number of observations contained in each bin of the previous figures. If strategic manipulation has taken place, it would likely reflect in a jump close to the threshold. If, for example, governments in municipalities just below the threshold tried to deter registration in order to avoid switching to DB in the near future, then the number of municipalities just below the threshold would probably be unusually large compared to the number of municipalities just above. As Figure 15 shows, no such jump is observed and no evidence of strategic manipulation is found.

4.3.2 Tests for Quasi-Random Assignment

The intuition of the identification strategy is that SB and DB systems in elections close to the threshold are assigned “almost randomly”, so that municipalities “just below” and “just above” the threshold are similar in all observed and unobserved predetermined characteristics.

Although there is good reason to believe that this is indeed the case, it is important to provide evidence that this intuition holds. Hence, I check if the values of predetermined variables (i.e., variables measured before the
4.3. Results

electoral rules were assigned) are the same on each side of the 200,000-voter threshold. In other words, I estimate treatment effects where they are expected to be zero.

This exercise is analogous to the common practice of testing for randomization in controlled experiments by comparing averages of predetermined variables in the treatment and control group.

First, I look for the effects of current electoral rules on previous election outcomes. Notice that only 24 municipalities switched from SB to DB between the 1996 and 2004 elections (10 in the 1996-2000 period and 14 in the 2000-2004 period). This makes current and previous electoral rules very correlated and thus generates a very stringent test of quasi-random assignment.\textsuperscript{78}

Figure 19 plots the vote share of the third and lower placed candidates against the number of registered voters in the next election (four years later). For example, the point above the 200,000 mark represents the average vote share of elections in municipalities that will have between 200,000 and 225,000 voters in the next election. Consistent with quasi-random assignment, no clear discontinuity is visible.

The formal counterpart is provided in the first row of Table 9, where we test for the effect of switching from SB to DB on the (pre-determined) results or the previous election. As expected, no statistically significant result is found. Obviously, estimating the effect of current electoral rule on previous election result requires dropping outcome observations for the

\textsuperscript{78}This is the reason why it is not possible to have estimations controlling for municipalities fixed effects.
4.3. Results

latest election in the sample (2004). In order to show that the insignificant results are not driven by the smaller sample size, the second row of Table 9 repeats the estimate of the first row (i.e., estimates treatment effect on vote share of the third and lower candidates) using the same sample (1996 and 2000 elections), and finds positive and statistically significant results.

Table 9 also presents the estimated treatment effects for a host of geographic and economic variables: the municipalities’ longitude and latitude (measured in degrees), per capita monthly income (in 2000 reais), income inequality (measured by the Gini index), education (average years of schooling in the population aged 25 or older) and the population share living in a rural area. The source of all these variables is the Brazilian statistical agency (*Instituto Brasileiro de Geografia e Estatística*).

These variables are available only for the year 2000 (when a Census was carried out).\textsuperscript{79} I assume that the value of these variables have not changed between 2000 and 2004 and estimate the treatment effect using a sample that includes the elections in the two years,\textsuperscript{80} using the exact same procedure of Section 4.1.

The estimated treatment effects are always insignificant at the 10\% level, independently of the bandwidth or specification (linear and quadratic) used in the estimation. This evidence implies that SB and DB municipalities are similar in several dimensions and strongly supports a valid regression

\textsuperscript{79}The previous census occurred in 1991. However several changes in the boundaries of municipalities that took place in the 1991-1994 period make it difficult to match the data from 1991 for municipalities in the sample (that includes elections that happened in 1996, 2000 and 2004).

\textsuperscript{80}I also experimented with inputting the 2000 values for the 1996 elections and performing the regressions using the whole sample, but none of the results changed in a significant way.
discontinuity design.

4.3.3 Testing Further Predictions of Strategic Voting

The results in Section 4.3.1 make a strong case that there is indeed a causal validity to Duverger's Law in the context analyzed here - SB causes lower voting for third and lower placed candidates when compared to DB. However, it conveys limited information about what are the mechanisms that drives the result. More specifically, it is not clear that it is the strategic voting pattern of avoiding wasted votes that lowers the voting of third placed candidates under DB.

This section tests other predictions of strategic voting - namely, predictions (2) and (3) stated in Section 4.1. Prediction 2 states that switching from SB to DB generates increased voting for the third-placed and decreased voting for the second and first placed candidate, so that the ratio between votes for the third placed candidate and the second placed one (hence the TS - as in “third to second” - ratio) - and also the ratio between third and first placed candidates (the TF ratio) - should be larger under DB. On the other hand, there is no clear prediction of how the distribution of votes between the second and first most voted candidates should differ under the different electoral systems, so that there are no particular priors on what the particular treatment effect on the ratio between their votes (the SF ratio) should be.

As discussed in Section 4.2, virtually all the elections in the samples used in estimations have at least three candidates. However, almost a quarter of the races do not have a fourth candidates. Hence, any comparison based
4.3. Results

on the votes received by fourth or lower placed candidates would be hard to interpret given the sample selection issues, and for this reason are not carried out here.

Figure 17 presents the TS ratio and the SF ratio by different bins of the forcing variable. As expected there is a clear jump in the TS ratio at the 200,000 voter threshold, but not for the SF ratio. This is formally presented in Table 10: a positive and significant effect of changing from SB to DB is found on the TS (and also the TF) ratios, but not on the SF ratio. Again, the results are robust to different bandwidths and specifications.

This result is strongly consistent with the pattern of strategic voting behind Duverger’s Law. Under SB the third placed candidate is deserted, to the gain of the first and second placed candidates. The fact that the effect on the margin of votes between the second and first (the SF ratio) is close to zero and insignificant can be seen as a “falsification test”: the concentration of votes caused by SB takes only the particular pattern predicted by theory.

Prediction 3, in its turn, indicates that in elections where one candidate is expected to obtain a large majority of the votes, there is little point for a voter to engage in strategic voting. To capture this idea, the sample is split into a “contested” and “uncontested” elections subsamples. The former are those where the winner obtained less than 50% of the votes (in the SB election or on the first round of the DB election), while the latter includes those where the winner obtained a majority.

The 50% mark captures two important features. First, in ‘uncontested” elections even if all voters that did not vote for the winner coordinated perfectly and voted for some other candidate, the results of the election
4.3. Results

would remain unchanged. Second, the “uncontested” election are those were there is no second round (under DB), so that in some sense a change from SB to DB has no “bite” in this group of races. Moreover, the median of the vote share of the most voted candidate is very close to 50%, so that the samples are divided close to evenly.

Of course, the share of votes obtained by the most voted candidate and the outcome variables (e.g., votes for the third and lower placed candidates) are almost mechanically correlated, which could generate possible sample selection biases. In order to show the robustness of the exercise to the issue, Appendix C replicates the estimations dividing the sample by the vote share that is predicted using data from municipal legislature elections occurring simultaneously to the mayoral races. All the qualitative results are the same.

As predicted by strategic voting, Panel A in Table 11 shows that the estimated effect of switching from SB to DB on voting for the third and lower placed candidates and the TS and TF ratios are always positive and generally statistically significant in the “contested” sample (the 25,000 bandwidth sample is likely too small to generate significant results). On the other hand, the estimated effects are close to zero and always insignificant in the “uncontested” elections (Panel B). Moreover, the magnitude of the estimates in the “contested” sample is larger than in the full sample and close to zero in the “uncontested” sample implies that it is the closer races that drive the results of Section 4.3.1.

\footnote{However, it is not clear how this sample bias would operate. Notice also that the effect of a change from SB to DB in the probability of having an election labeled as “contested” was estimated to be insignificantly different from zero.}
4.3. Results

The importance of the evidence supporting Predictions 2 and 3 cannot be overemphasized. It rules out almost any other alternative explanation for the results and substantially increase confidence that it is indeed strategic voting that drives the Duverger’s Law results of Section 4.3.1. For example, one could argue that under DB candidates may adopt different positions than under SB, affecting the distribution of votes. However, it would be extremely hard for such argument to account for all the particular results found above.

4.3.4 A Falsification Test: Municipal Legislature Elections

The mayoral elections are run simultaneously with the elections for the legislative body of the municipalities (Câmara dos Vereadores) - a voter casts his vote for the municipal legislature at the same time and place that he votes for mayor (in the case of DB municipalities, at the same time of the first round).

Elections for municipal legislature are run under a proportional representation system.\textsuperscript{82} As in mayoral elections, a municipality is considered a single district and most importantly, the electoral rules are exactly the same for cities below and above the 200,000 voter threshold. This allows for a powerful falsification test: given that electoral rules are the same on both sides of the threshold, one should not expect a difference in electoral

\textsuperscript{82}Specifically, the system used is open-list proportional representation with seats awarded by the d’Hondt formula. This is the proportional representation system where a voter can cast a vote to individual candidates or party lists. The number of seats awarded to a party is proportional to votes that the party list or party candidates received, but the votes for which candidate within a party list define which individual will get the seat. For a discussion of different variations of proportional representation, see Cox (1997).
4.3. Results

I estimate the treatment effects on four different electoral outcomes: the share of seats\textsuperscript{83} that is awarded to the party of the elected mayor and mayoral elections, the share of seats that are awarded to the most voted (and also the two most voted) parties in the legislature election and the Hirschman-Herfindahl Index (HHI)\textsuperscript{84} of concentration in the elected legislature.

The results are presented graphically on Figure 18. For the four outcomes, there seems to be a smooth relationship with no clear jumps at the 200,000-voter threshold. The formal counterpart can be seen on Table 12, where the results are mostly close to zero and generally insignificant.\textsuperscript{85}

These results not only increase confidence in the causal validity and quasi-random nature of the previous evidence, but it also provides the interesting evidence that there is very limited spillovers from voting behavior in the mayoral election to the legislature election, even though they occur simultaneously.

\textsuperscript{83}Given the proportional representation nature of the election, seat shares and vote shares by party are virtually the same.

\textsuperscript{84}The index equals the sum of the squares of the seat shares of each party. Hence it goes from zero (infinite amount of parties, one with each seat) to one (one party has all the seats). The inverse of this measure is commonly used in the political science literature and is referred to as the “effective number of parties”.

\textsuperscript{85}The significant results appear only in the 25,000 bandwidth sample with linear specification and 50,000 bandwidth sample with quadratic specification, which likely implies that an outlier close to the threshold is driving the result.
Chapter 5

Conclusion

Chapters 2 and 3 quantified 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 4.3 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 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.

Chapter 4 argued that, at least in the context of Brazilian mayoral elections, there is strong evidence that voters act strategically. Single-ballot plurality rule, when compared to dual-ballot, generates an incentive for voters to desert the candidates that are expected to finish third and lower and switch their vote to the top two contenders.

Although the patterns found in the data make a strong case that it is strategic voting, rather than strategic entry or positioning of candidates that drives the results, it says very little about the mechanisms that generate the (perhaps self-fulfilling) expectations of which candidates will finish first, second and third, and how these expectations allow coordination between voters.

In a representative election of the sample used in estimations, over 160,000 thousand citizens vote. How do such a large number of people coordinate? An useful direction of future research would be to address this question by investigating if it is polls, media coverage, campaign contributions or some other factor that allow coordination to arise.
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: Timeline of Elections and Legislative Terms
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 6: Education and Health Insurance Use

Q: How do you Cover Your Health Expenses?
Answer by Highest Grade Completed

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

Figure 7: Share of Electorate Using Electronic Voting - 1998 Election

Darker Shading is Proportional to $S_i$, as Shown in Scale
Figure 8: Electronic Voting Phase-in and Valid Voting

Figure 9: Electronic Voting Phase-In and Share of Low Educated Legislators
Figure 10: Electronic Voting Phase-In and Health Care Spending

Figure 11: Electronic Voting Phase-In and Pre-Natal Visits
Figure 12: Electronic Voting Phase-In and Low Birth-Weight
Figure 13: Vote Share of Third and Lower Placed Candidates - Local Averages and Parametric Fit

The right Y-axis presents the scale of turnout and registration rates, while the left Y-axis presents the scale of the number of candidates.

Figure 14: Turnout, Registration and Entry - Local Averages and Parametric Fit

The right Y-axis presents the scale of turnout and registration rates, while the left Y-axis presents the scale of the number of candidates.
Figure 15: Sample Distribution - Local Averages and Parametric Fit

Figure 16: Vote Share of Third and Lower Placed Candidates, Previous Election - Local Averages and Parametric Fit
Figure 17: TS and SF ratios- Local Averages and Parametric Fit

![Figure 17: TS and SF ratios- Local Averages and Parametric Fit](image)

Figure 18: Outcomes of Legislature Elections - Local Averages and Parametric Fit

![Figure 18: Outcomes of Legislature Elections - Local Averages and Parametric Fit](image)
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 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.
Table 1: Treatment Effects of Electronic Voting

<table>
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<th>Pre-Treat. Mean</th>
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<td></td>
</tr>
<tr>
<td><strong>Panel A: Effects on Outcomes (1998 Election)</strong></td>
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<td></td>
<td></td>
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<tr>
<td>Valid Votes/Turnout</td>
<td>0.769</td>
<td>0.128</td>
<td>0.122</td>
<td>0.124</td>
<td>0.116</td>
<td>0.118</td>
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<td></td>
<td></td>
<td>(0.011)***</td>
<td>(0.016)***</td>
<td>(0.025)***</td>
<td>(0.017)***</td>
<td>(0.026)***</td>
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<td>Turnout/Reg. Voters</td>
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<td>-0.001</td>
<td>0.013</td>
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<td>(0.021)</td>
<td>(0.033)</td>
<td>(0.023)</td>
<td>(0.034)</td>
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<td>0.012</td>
<td>0.007</td>
<td>-0.006</td>
<td>0.010</td>
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<td></td>
<td></td>
<td>(0.024)</td>
<td>(0.021)</td>
<td>(0.032)</td>
<td>(0.024)</td>
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<td><strong>Panel B: Placebo Tests (Election Years Without Discontinuous Assignment)</strong></td>
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<td>Valid Votes/Turnout</td>
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<td>-0.008</td>
<td>0.006</td>
<td>-0.013</td>
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<tr>
<td>(1994 Election)</td>
<td></td>
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<td>(0.023)</td>
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<td>0.008</td>
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<td>(2002 Election)</td>
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<td>(0.005)</td>
<td>(0.006)</td>
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<td><strong>Panel C: Tests of Quasi-Random Assignment (Effects on Pre-Determined Covariates)</strong></td>
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<td>Share w/o Primary Schooling</td>
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<td>-0.014</td>
<td>0.001</td>
<td>0.004</td>
<td>0.004</td>
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<td></td>
<td></td>
<td>(0.016)</td>
<td>(0.020)</td>
<td>(0.029)</td>
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<td>Monthly Income</td>
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<td>6.096</td>
<td>14.017</td>
<td>10.759</td>
<td>38.957</td>
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<td>(1.636)</td>
<td>(2.466)</td>
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</table>

***,**,* 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>
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<tr>
<th>Explanatory Variable</th>
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<td>-0.051</td>
<td>-0.053</td>
<td>-0.079</td>
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<td></td>
<td>(0.114)</td>
<td>(0.163)</td>
<td>(0.260)</td>
<td>(0.171)</td>
<td>(0.262)</td>
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<td>Electronic Voting</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>x(% pop. w/o Primary School)</td>
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<td>0.221</td>
<td>0.227</td>
<td>0.251</td>
<td>0.315</td>
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<td>(0.111)**</td>
<td>(0.162)*</td>
<td>(0.319)</td>
<td>(0.112)**</td>
<td>(0.321)</td>
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</table>

**Implied Effect on a municipality:**

- with 71% w/o Primary School (1st decile)
  - 0.110
- with 92% w/o Primary School (9th decile)
  - 0.160

| 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

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<td>(0.044)**</td>
<td>(0.100)</td>
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<td></td>
<td></td>
<td>(0.156)**</td>
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<td>(0.221)**</td>
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<td></td>
<td>0.903</td>
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<td></td>
<td>(0.301)**</td>
</tr>
<tr>
<td>List-Specific Effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>List-Specific Slopes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Bandwidth:</td>
<td>20,000</td>
<td>20,000</td>
<td>20,000</td>
<td>20,000</td>
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</tr>
<tr>
<td>Observations::</td>
<td>11,745</td>
<td>11,745</td>
<td>11,745</td>
<td>11,745</td>
<td></td>
</tr>
</tbody>
</table>

***,**,* 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

|------------|-------------------------------------------------------------------------------------------------|---------------|

**Panel A: Estimated Effect of Electronic Voting and Placebo Tests**

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Valid Votes/Turnout</td>
<td>0.108</td>
<td>-0.112</td>
<td>-</td>
<td>-0.010</td>
</tr>
<tr>
<td>(0.037)**</td>
<td>(0.010)**</td>
<td>(0.011)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Legislators w/o Primary Schooling</td>
<td>0.046</td>
<td>-0.047</td>
<td>-</td>
<td>0.012</td>
</tr>
<tr>
<td>(0.020)**</td>
<td>(0.029)</td>
<td>(0.058)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>log(Health Care Spending per capita)</td>
<td>0.512</td>
<td>-0.483</td>
<td>-0.134</td>
<td>-</td>
</tr>
<tr>
<td>(0.319)*</td>
<td>(0.210)**</td>
<td>(0.349)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Panel B: Falsification Tests**

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>log(Total Spending per capita)</td>
<td>-0.021</td>
<td>-0.203</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>(0.113)</td>
<td>(0.177)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>log(GDP per capita)</td>
<td>-0.042</td>
<td>-0.141</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>(0.090)</td>
<td>(0.140)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>log(Population)</td>
<td>0.066</td>
<td>0.038</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>(0.043)</td>
<td>(0.028)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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

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

***, **, * 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

<table>
<thead>
<tr>
<th>Parameter</th>
<th>( \theta^{99-93} )</th>
<th>( \theta^{03-06} )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(Paper-Discont.)</td>
<td>(Discont.-Electr.)</td>
</tr>
</tbody>
</table>

**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 (\( \times 100 \)) | \(-0.431\) | \(0.429\) |
|                                 | \((0.331)\) | \((0.238)\)* |

**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 (\( \times 100 \)) | \(0.142\) | \(0.266\) |
|                                 | \((0.475)\) | \((0.453)\) |
| 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

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Sample Average</th>
<th>Baseline Specification</th>
<th>“Triple Differences” Specification</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Panel A: Uneducated Sample (Mothers without primary schooling)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share with 7+ Prenatal Visits</td>
<td>0.333</td>
<td>0.070</td>
<td>0.062</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Low-weight Births (×100)</td>
<td>7.721</td>
<td>-0.430</td>
<td>-0.414</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel B: Educated Sample (Mothers with primary schooling)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share with 7+ Prenatal Visits</td>
<td>0.527</td>
<td>0.009</td>
<td>0.017</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Low-weight Births (×100)</td>
<td>6.261</td>
<td>-0.062</td>
<td>-0.043</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Additional Controls (GDP and Pop.)</td>
<td>-</td>
<td>-</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Specific Trends</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>State-Year Effects</td>
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<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Restricted Sample</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Observations</td>
<td>-</td>
<td>324</td>
<td>324</td>
</tr>
</tbody>
</table>

***, **, * 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 8: Treatment Effects of Changing from Single-Ballot to Dual-Ballot on Electoral Outcomes

<table>
<thead>
<tr>
<th>Specification/Bandwidth</th>
<th>Single-Ballot Linear Mean 50 000</th>
<th>Linear 25 000</th>
<th>Linear 75 000</th>
<th>Quad. 50 000</th>
<th>Quad. 75 000</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Vote Share (3rd and Lower)</td>
<td>0.17</td>
<td>0.10</td>
<td>0.13</td>
<td>0.06</td>
<td>0.15</td>
</tr>
<tr>
<td></td>
<td>(0.05)**</td>
<td>(0.05)*****</td>
<td>(0.04)*</td>
<td>(0.06)*****</td>
<td>(0.05)*****</td>
</tr>
<tr>
<td>Vote Share (3rd and Lower) (control: num. of cand. ◊)</td>
<td>0.17</td>
<td>0.07</td>
<td>0.10</td>
<td>0.04</td>
<td>0.10</td>
</tr>
<tr>
<td></td>
<td>(0.03)**</td>
<td>(0.06)**</td>
<td>(0.03)</td>
<td>(0.05)**</td>
<td>(0.04)**</td>
</tr>
<tr>
<td>Vote Share (3rd and Lower) (control: cand. quality△)</td>
<td>0.14</td>
<td>0.11</td>
<td>0.15</td>
<td>0.08</td>
<td>0.16</td>
</tr>
<tr>
<td></td>
<td>(0.05)**</td>
<td>(0.06)**</td>
<td>(0.05)*</td>
<td>(0.06)**</td>
<td>(0.06)**</td>
</tr>
<tr>
<td>Registration Rate</td>
<td>0.63</td>
<td>0.01</td>
<td>0.02</td>
<td>0.03</td>
<td>0.02</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.02)</td>
<td>(0.04)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Turnout Rate</td>
<td>0.85</td>
<td>0.0004</td>
<td>-0.004</td>
<td>0.001</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.008)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Number of Candidates</td>
<td>5.17</td>
<td>0.63</td>
<td>1.02</td>
<td>0.47</td>
<td>1.09</td>
</tr>
<tr>
<td></td>
<td>(0.56)</td>
<td>(0.74)</td>
<td>(0.49)</td>
<td>(0.77)</td>
<td>(0.68)</td>
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<tr>
<td>Observations</td>
<td>-</td>
<td>114</td>
<td>56</td>
<td>183</td>
<td>114</td>
</tr>
</tbody>
</table>

***-Significant (1% level); **-Significant (5% level); *-Significant (10% level).

◊Includes the total number of candidates that received positive votes in the race as a covariate.

△Includes the candidates with college education as a covariate. Data are for the 2000-2004 races only (the number of observations are 70, 36, and 103 for the 50 000, 25 000 and 75 000 bandwidth samples, respectively).

Robust standard errors clustered at the municipality level in parenthesis. Each entry in the table is from a separate local linear/quadratic regression using the specified bandwidth. The level of observation is a municipality-election. The estimated treatment effect is of a change from SB to DB. An estimate using bandwidth k uses a sample of elections in municipalities with more than 200 000-k and less than 200 000+k registered voters. Details on the dependent variables are in the text. Single-ballot mean refer to the average value in municipalities with more than 175,000 and less than 200 000 voters.
Table 9: Tests of Quasi-Random Assignment

<table>
<thead>
<tr>
<th>Specification/Bandwidth</th>
<th>Single-Ballot Mean</th>
<th>Linear 50 000</th>
<th>Linear 25 000</th>
<th>Linear 75 000</th>
<th>Quad. 50 000</th>
<th>Quad. 75 000</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td></td>
</tr>
<tr>
<td>Vote Share, 3rd and Lower, t-1</td>
<td>0.16 (0.06)</td>
<td>0.10 (0.08)</td>
<td>0.12 (0.05)</td>
<td>0.04 (0.05)</td>
<td>0.12 (0.07)**</td>
<td>0.07 (0.05)</td>
</tr>
<tr>
<td>Vote Share, 3rd and Lower, t</td>
<td>0.16 (0.06)**</td>
<td>0.13 (0.06)**</td>
<td>0.21 (0.05)**</td>
<td>0.09 (0.07)**</td>
<td>0.26 (0.07)**</td>
<td>0.18 (0.06)**</td>
</tr>
<tr>
<td>Longitude (in degrees)</td>
<td>47.54 (1.57)</td>
<td>-0.30 (2.14)</td>
<td>0.30 (1.7)</td>
<td>-1.17 (2.46)</td>
<td>-1.38 (1.97)</td>
<td>-0.47 (1.97)</td>
</tr>
<tr>
<td>Latitude (in degrees)</td>
<td>-19.58 (2.60)</td>
<td>-3.56 (3.70)</td>
<td>-4.34 (2.27)</td>
<td>-2.55 (3.82)</td>
<td>-4.17 (3.00)</td>
<td>-3.55 (3.00)</td>
</tr>
<tr>
<td>Per Capita Income (R$)</td>
<td>318.92 (41.43)</td>
<td>15.77 (55.93)</td>
<td>10.80 (39.07)</td>
<td>34.17 (53.29)</td>
<td>-13.35 (1.97)</td>
<td>-0.47 (1.97)</td>
</tr>
<tr>
<td>Gini Index (Income)</td>
<td>0.54 (0.02)</td>
<td>0.004 (0.02)</td>
<td>0.02 (0.01)</td>
<td>-0.001 (0.02)</td>
<td>0.009 (0.02)</td>
<td>0.005 (0.02)</td>
</tr>
<tr>
<td>Years of Schooling</td>
<td>6.34 (0.28)</td>
<td>0.008 (0.35)</td>
<td>0.02 (0.27)</td>
<td>0.13 (0.36)</td>
<td>-0.20 (0.32)</td>
<td>-0.05 (0.32)</td>
</tr>
<tr>
<td>Pop. Share in Rural Areas</td>
<td>0.04 (0.01)</td>
<td>-0.001 (0.02)</td>
<td>-0.0005 (0.01)</td>
<td>-0.01 (0.02)</td>
<td>-0.01 (0.02)</td>
<td>-0.005 (0.02)</td>
</tr>
</tbody>
</table>

Observations$^a$ - 82 39 133 82 133

*** -Significant (1% level); ** -Significant (5% level); * -Significant (10% level).

$^a$ The observations for the vote share of third and lower placed candidates (first and second rows) are 70, 36, and 103 for the 50 000, 25 000 and 75 000 bandwidth samples, respectively.

Robust standard errors clustered at the municipality level in parenthesis. Each entry in the table is from a separate local linear/quadratic regression using the specified bandwidth. The level of observation is a municipality-election. The estimated treatment effect is of a change from SB to DB. An estimate using bandwidth k uses a sample of elections in municipalities with more than 200 000-k and less than 200,000+k registered voters. Details on the dependent variables are in the text. Single-ballot mean refer to the average value in municipalities with more than 175 000 and less than 200 000 voters.
Table 10: Treatment Effects on Vote Margins

<table>
<thead>
<tr>
<th>Specification/ Bandwidth</th>
<th>Single-Ballot Mean</th>
<th>Linear 50 000</th>
<th>Linear 25 000</th>
<th>Linear 75 000</th>
<th>Quad. 50 000</th>
<th>Quad. 75 000</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td></td>
</tr>
<tr>
<td>Third/First. Vote Ratio</td>
<td>0.27</td>
<td>0.15</td>
<td>0.22</td>
<td>0.11</td>
<td>0.24</td>
<td>0.24</td>
</tr>
<tr>
<td></td>
<td>(0.09)*</td>
<td>(0.11)**</td>
<td>(0.07)</td>
<td>(0.12)**</td>
<td>(0.11)**</td>
<td></td>
</tr>
<tr>
<td>Third/Second Vote Ratio</td>
<td>0.41</td>
<td>0.19</td>
<td>0.28</td>
<td>0.14</td>
<td>0.30</td>
<td>0.28</td>
</tr>
<tr>
<td></td>
<td>(0.09)**</td>
<td>(0.13)**</td>
<td>(0.08)*</td>
<td>(0.13)**</td>
<td>(0.12)**</td>
<td></td>
</tr>
<tr>
<td>Second/First Vote Ratio</td>
<td>0.66</td>
<td>0.03</td>
<td>0.10</td>
<td>0.01</td>
<td>0.11</td>
<td>0.06</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.09)</td>
<td>(0.06)</td>
<td>(0.10)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>-</td>
<td>114</td>
<td>56</td>
<td>183</td>
<td>114</td>
<td>183</td>
</tr>
</tbody>
</table>

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).

Robust standard errors clustered at the municipality level in parenthesis. Each entry in the table is from a separate local linear/quadratic regression using the specified bandwidth. The level of observation is a municipality-election. The estimated treatment effect is of a change from SB to DB. An estimate using bandwidth k uses a sample of elections in municipalities with more than 200 000-k and less than 200 000+k registered voters. Details on the dependent variables are in the text. Single-ballot mean refer to the average value in municipalities with more than 175 000 and less than 200 000 voters.
Table 11: Treatment Effects in Contested and Uncontested Elections

<table>
<thead>
<tr>
<th>Specification / Bandwidth</th>
<th>SB Mean 50 000</th>
<th>Linear 25 000</th>
<th>Linear 75 000</th>
<th>Linear 50 000</th>
<th>Quad. 75 000</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Contested Elections (Vote Share of Winner &lt; 0.5)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Vote Share - 3rd and Lower</td>
<td>0.11</td>
<td>0.11</td>
<td>0.07</td>
<td>0.11</td>
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<td>(0.05)**</td>
<td>(0.06)</td>
<td>(0.04)**</td>
<td>(0.06)*</td>
<td>(0.05)**</td>
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<tr>
<td>Third/Second Vote Ratio</td>
<td>0.48</td>
<td>0.21</td>
<td>0.16</td>
<td>0.21</td>
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<td></td>
<td>(0.1)**</td>
<td>(0.16)</td>
<td>(0.08)**</td>
<td>(0.15)</td>
<td>(0.12)*</td>
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<td>Third/First Vote Ratio</td>
<td>0.38</td>
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<td>0.12</td>
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<tr>
<td></td>
<td>(0.09)*</td>
<td>(0.14)</td>
<td>(0.08)**</td>
<td>(0.13)</td>
<td>(0.11)</td>
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<tr>
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<td>-</td>
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<td>32</td>
<td>113</td>
<td>71</td>
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<td></td>
<td>70</td>
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<td>24</td>
<td>70</td>
<td>43</td>
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<tr>
<td></td>
<td>70</td>
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</tr>
</tbody>
</table>

Panel B: Uncontested Elections (Vote Share of Winner > 0.5)

| Vote Share - 3rd and Lower | 0.10       | 0.03       | 0.07       | -0.003      | 0.06       |
|                           |           | (0.05)     | (0.07)     | (0.04)      | (0.08)     |
|                           |           | 0.04       | 0.10       | -0.008      | 0.09       |
|                           |           | (0.06)     | (0.09)     | (.05)       | (.11)      |
| Observations              | -         | 43         | 24         | 70          | 43         |
|                           | 70        | 43         | 70         |             |             |

*** - Significant (1% level); ** - Significant (5% level); * - Significant (10% level).

Robust standard errors clustered at the municipality level in parenthesis. Each entry in the table is from a separate local linear/quadratic regression using the specified bandwidth. The level of observation is a municipality-election. The estimated treatment effect is of a change from SB to DB. An estimate using bandwidth k uses a sample of elections in municipalities with more than 200 000-k and less than 200 000+k registered voters. Details on the dependent variables are in the text. Single-ballot mean refer to the average value in municipalities with more than 175 000 and less than 200 000 voters.
Table 12: Falsification Test: Treatment Effects on Municipal Legislature Election Outcomes

<table>
<thead>
<tr>
<th>Specification/ Bandwidth</th>
<th>Single-Ballot Mean</th>
<th>Linear 50 000</th>
<th>Linear 25 000</th>
<th>Linear 75 000</th>
<th>Linear 50 000</th>
<th>Linear 75 000</th>
<th>Quad. 50 000</th>
<th>Quad. 75 000</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Seat Share - Mayor’s Party</td>
<td>0.18 (0.03)</td>
<td>0.0002 (0.04)</td>
<td>-0.06 (0.02)</td>
<td>0.02 (0.04)</td>
<td>-0.05 (0.03)</td>
<td>-0.02 (0.03)</td>
<td></td>
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</tr>
<tr>
<td>Seat Share - Most Voted Party</td>
<td>0.23 (0.02)</td>
<td>-0.008 (0.03)**</td>
<td>-0.06 (0.02)</td>
<td>0.01 (0.03)*</td>
<td>-0.05 (0.03)*</td>
<td>-0.02 (0.02)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Seat Share - 2 Most Voted Parties</td>
<td>0.40 (0.03)</td>
<td>-0.02 (0.04)*</td>
<td>-0.08 (0.03)</td>
<td>-0.002 (0.04)*</td>
<td>-0.08 (0.04)*</td>
<td>-0.04 (0.04)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>HHI</td>
<td>0.15 (0.01)</td>
<td>-0.003 (0.01)*</td>
<td>-0.03 (0.01)</td>
<td>0.006 (0.01)</td>
<td>-0.03 (0.01)</td>
<td>-0.01 (0.01)</td>
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<tr>
<td>Observations</td>
<td>-</td>
<td>121</td>
<td>59</td>
<td>192</td>
<td>121</td>
<td>192</td>
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</tr>
</tbody>
</table>

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).
Robust standard errors clustered at the municipality level in parenthesis. Each entry in the table is from a separate local linear/quadratic regression using the specified bandwidth. The level of observation is a municipality-election. The estimated treatment effect is of a change from SB to DB. An estimate using bandwidth k uses a sample of elections in municipalities with more than 200 000-k and less than 200 000+k registered voters. Details on the dependent variables are in the text. Single-ballot mean refer to the average value in municipalities with more than 175 000 and less than 200 000 voters.
Table A1: Summary Statistics for Municipal Sample

<table>
<thead>
<tr>
<th>Variable</th>
<th>Year of Observation</th>
<th>Number of Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Valid Votes/Turnout</td>
<td>1994</td>
<td>557</td>
<td>0.672</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Valid Votes/Turnout</td>
<td>1998</td>
<td>558</td>
<td>0.784</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Valid Votes/Turnout</td>
<td>2002</td>
<td>558</td>
<td>0.923</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Turnout/Registered Voters</td>
<td>1998</td>
<td>558</td>
<td>0.765</td>
<td>(0.089)</td>
</tr>
<tr>
<td>Registered Voters/Population</td>
<td>1998</td>
<td>558</td>
<td>0.726</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Share of Adults Without Primary Schooling</td>
<td>1991</td>
<td>558</td>
<td>0.813</td>
<td>(0.080)</td>
</tr>
<tr>
<td>Monthly Income (<em>reais</em> per capita)</td>
<td>1991</td>
<td>558</td>
<td>160.61</td>
<td>(86.01)</td>
</tr>
<tr>
<td>Latitude (degrees)</td>
<td></td>
<td>558</td>
<td>-15.71</td>
<td>(8.50)</td>
</tr>
<tr>
<td>Longitude (degrees)</td>
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<td>558</td>
<td>45.53</td>
<td>(6.26)</td>
</tr>
</tbody>
</table>

Panel A: Municipalities With an Electorate Size Between 20,500 and 60,500

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>
<thead>
<tr>
<th>Variable</th>
<th>Number of Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share of Voters Using Electronic Voting</td>
<td>324</td>
<td>0.51</td>
<td>(0.43)</td>
</tr>
<tr>
<td>Valid Votes/Turnout - State Legislature Elections</td>
<td>324</td>
<td>0.83</td>
<td>(0.11)</td>
</tr>
<tr>
<td>Share of State Legislators Without Primary Schooling</td>
<td>324</td>
<td>0.08</td>
<td>(0.03)</td>
</tr>
<tr>
<td>State Expenditures on Health Care (2000 reais per capita)</td>
<td>324</td>
<td>99.65</td>
<td>(86.34)</td>
</tr>
<tr>
<td>Share of State Budgets Spent on Health Care</td>
<td>324</td>
<td>9.90</td>
<td>(4.16)</td>
</tr>
<tr>
<td>Share of Uneducated Mothers with 7+ Pre-Natal Visits</td>
<td>324</td>
<td>0.33</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Share of Educated Mothers with 7+ Pre-Natal Visits</td>
<td>324</td>
<td>0.53</td>
<td>(0.15)</td>
</tr>
<tr>
<td>Share of Uneducated Mothers with Low-Weight Births</td>
<td>324</td>
<td>0.08</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Share of Educated Mothers with Low-Weight Births</td>
<td>324</td>
<td>0.06</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Population (in thousands)</td>
<td>324</td>
<td>6294</td>
<td>(7540)</td>
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<tr>
<td>GDP (in thousands of 2000 reais per capita)</td>
<td>324</td>
<td>5.36</td>
<td>(3.29)</td>
</tr>
<tr>
<td>Variable</td>
<td>Panel A: Elections with Less Than 200 000 Voters (Single Ballot Elections - 15 551 Observations)</td>
<td>Panel B: Elections with More Than 200 000 Voters (Dual Ballot Elections - 159 Observations)</td>
<td>Panel C: Elections with More than 150 000 but Less than 200 000 Voters (Single Ballot Elections - 80 Observations)</td>
</tr>
<tr>
<td>---------------------------------------------------</td>
<td>-----------------------------------------------------------------------------------------------</td>
<td>------------------------------------------------------------------------------------------------</td>
<td>------------------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>Vote Share - 1st Placed Candidate</td>
<td>Mean: 0.538, Std. Dev.: 0.096, Minimum: 0.227, Maximum: 0.999</td>
<td>Mean: 0.483, Std. Dev.: 0.123, Minimum: 0.255, Maximum: 0.827</td>
<td>Mean: 0.511, Std. Dev.: 0.127, Minimum: 0.312, Maximum: 0.930</td>
</tr>
<tr>
<td>Vote Share - 2nd Placed Candidate</td>
<td>Mean: 0.388, Std. Dev.: 0.081, Minimum: 0.0008, Maximum: 0.500</td>
<td>Mean: 0.289, Std. Dev.: 0.073, Minimum: 0.072, Maximum: 0.475</td>
<td>Mean: 0.317, Std. Dev.: 0.091, Minimum: 0.070, Maximum: 0.498</td>
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<tr>
<td>Vote Share - 3rd and Lower Placed</td>
<td>Mean: 0.073, Std. Dev.: 0.110, Minimum: 0.000, Maximum: 0.571</td>
<td>Mean: 0.228, Std. Dev.: 0.121, Minimum: 0.000, Maximum: 0.511</td>
<td>Mean: 0.172, Std. Dev.: 0.119, Minimum: 0.000, Maximum: 0.423</td>
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<tr>
<td>Number of Candidates</td>
<td>Mean: 2.78, Std. Dev.: 1.01, Minimum: 2.00, Maximum: 10.00</td>
<td>Mean: 6.36, Std. Dev.: 2.32, Minimum: 2.00, Maximum: 15.00</td>
<td>Mean: 4.61, Std. Dev.: 1.61, Minimum: 2.00, Maximum: 9.00</td>
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Table A4: Additional Estimated Treatment Effects of Electronic Voting

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<th>Pre-Treat. Mean</th>
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<th>(5)</th>
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<td><strong>Panel A: Effects on Null and Blank Votes, State Legislature Election</strong></td>
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<td></td>
<td></td>
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<tr>
<td>Null Votes/Turnout</td>
<td>0.138</td>
<td>-0.093</td>
<td>-0.086</td>
<td>-0.084</td>
<td>-0.081</td>
<td>-0.076</td>
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<td>(0.007)***</td>
<td>(0.010)***</td>
<td>(0.015)***</td>
<td>(0.010)***</td>
<td>(0.015)***</td>
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<tr>
<td>Blank Votes/Turnout</td>
<td>0.093</td>
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<td>-0.034</td>
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<td>(0.008)***</td>
<td>(0.012)***</td>
<td>(0.009)***</td>
<td>(0.013)***</td>
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<td><strong>Panel B: Effects on Valid Votes/Turnout, by Office</strong></td>
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<td>State Legislature</td>
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<td>0.124</td>
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<td>(0.011)***</td>
<td>(0.016)***</td>
<td>(0.025)***</td>
<td>(0.017)***</td>
<td>(0.026)***</td>
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<td>Federal Congress</td>
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<td>0.210</td>
<td>0.205</td>
<td>0.212</td>
<td>0.207</td>
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<td>7 (Lower Chamber)</td>
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<td>(0.012)***</td>
<td>(0.018)***</td>
<td>(0.030)***</td>
<td>(0.019)***</td>
<td>(0.031)***</td>
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<tr>
<td>Federal Congress</td>
<td>0.705</td>
<td>0.081</td>
<td>0.087</td>
<td>0.100</td>
<td>0.079</td>
<td>0.108</td>
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<td>(Senate)</td>
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<td>(0.014)***</td>
<td>(0.020)***</td>
<td>(0.030)***</td>
<td>(0.020)***</td>
<td>(0.031)***</td>
</tr>
<tr>
<td>State Governor</td>
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<td>0.078</td>
<td>0.078</td>
<td>0.082</td>
<td>0.071</td>
<td>0.093</td>
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<td>(0.012)***</td>
<td>(0.017)***</td>
<td>(0.026)***</td>
<td>(0.019)***</td>
<td>(0.028)***</td>
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<tr>
<td>President</td>
<td>0.799</td>
<td>0.018</td>
<td>0.021</td>
<td>0.031</td>
<td>0.011</td>
<td>0.034</td>
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<td></td>
<td>(0.011)*</td>
<td>(0.013)</td>
<td>(0.021)</td>
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<td>Observations:</td>
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<td>558</td>
<td>229</td>
<td>116</td>
<td>558</td>
<td>229</td>
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***,**,* 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 A5: Effects of Electronic Voting on Number of Pre-Natal Visits

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

***, **, * 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.
<table>
<thead>
<tr>
<th>Specification/ Bandwidth</th>
<th>Single-Ballot Mean</th>
<th>Linear 50 000</th>
<th>Linear 25 000</th>
<th>Linear 75 000</th>
<th>Quad. 50 000</th>
<th>Quad. 75 000</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vote Share (3rd and Lower) (No Covariates)</td>
<td>0.17</td>
<td>0.11</td>
<td>0.14</td>
<td>0.08</td>
<td>0.16</td>
<td>0.14</td>
</tr>
<tr>
<td>Vote Share (3rd and Lower) (Electoral Covariates)</td>
<td>0.17</td>
<td>0.07</td>
<td>0.10</td>
<td>0.05</td>
<td>0.11</td>
<td>0.10</td>
</tr>
<tr>
<td>Vote Share (3rd and Lower) (Economic Covariates)</td>
<td>0.17</td>
<td>0.11</td>
<td>0.13</td>
<td>0.07</td>
<td>0.16</td>
<td>0.14</td>
</tr>
<tr>
<td>Vote Share (3rd and Lower) (Geographic Covariates)</td>
<td>0.17</td>
<td>0.09</td>
<td>0.10</td>
<td>0.07</td>
<td>0.14</td>
<td>0.13</td>
</tr>
<tr>
<td>Year Dummies</td>
<td>-</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>-</td>
<td>114</td>
<td>56</td>
<td>183</td>
<td>114</td>
<td>183</td>
</tr>
</tbody>
</table>

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).

Robust standard errors clustered at the municipality level in parenthesis. Each entry in the table is from a separate local linear/quadratic regression using the specified bandwidth and including a full set of year dummies. The level of observation is a municipality-election. The estimated treatment effect is of a change from SB to DB. An estimate using bandwidth k uses a sample of elections in municipalities with more than 200,000-k and less than 200,000+k registered voters. The dependent variable is always the vote share obtained by the third and lower placed candidate. The different sets of covariates are described in Appendix B. Single-ballot mean refer to the average value in municipalities with more than 175,000 and less than 200,000 voters.
Table A7: Treatment Effects in (Predicted) Contested and Uncontested Elections

<table>
<thead>
<tr>
<th>Specification / Bandwidth</th>
<th>SB 50 000</th>
<th>Linear 25 000</th>
<th>Linear 75 000</th>
<th>Linear 50 000</th>
<th>Quad. 75 000</th>
<th>Quad. 75 000</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Contested Elections (Predicted Vote Share of Winner &lt; 0.5)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Vote Share 3rd and Lower</td>
<td>0.15 (0.05)***</td>
<td>0.14 (0.07)**</td>
<td>0.13 (0.05)**</td>
<td>0.17 (0.07)**</td>
<td>0.17 (0.06)***</td>
<td></td>
</tr>
<tr>
<td>Third/Second Vote Ratio</td>
<td>0.35 (0.10)***</td>
<td>0.28 (0.16)</td>
<td>0.25 (0.1)**</td>
<td>0.31 (0.16)**</td>
<td>0.30 (0.12)**</td>
<td></td>
</tr>
<tr>
<td>Third/First Vote Ratio</td>
<td>0.21 (0.09)***</td>
<td>0.25 (0.12)</td>
<td>0.23 (0.08)***</td>
<td>0.22 (0.13)*</td>
<td>0.24 (0.1)**</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>-</td>
<td>78</td>
<td>38</td>
<td>117</td>
<td>78</td>
<td>117</td>
</tr>
<tr>
<td>Panel B: Uncontested Elections (Predicted Vote Share of Winner &gt; 0.5)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Vote Share 3rd and Lower</td>
<td>0.22 (0.08)</td>
<td>-0.06 (0.08)</td>
<td>0.09 (0.05)</td>
<td>-0.06 (0.08)</td>
<td>0.06 (0.09)</td>
<td></td>
</tr>
<tr>
<td>Third/Second Vote Ratio</td>
<td>0.52 (0.24)</td>
<td>-0.007 (0.23)</td>
<td>0.33 (0.13)</td>
<td>-0.08 (0.22)</td>
<td>0.24 (0.28)</td>
<td></td>
</tr>
<tr>
<td>Third/First Vote Ratio</td>
<td>0.39 (0.23)</td>
<td>-0.12 (0.21)</td>
<td>0.28 (0.12)</td>
<td>-0.14 (0.18)</td>
<td>0.25 (0.26)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>-</td>
<td>36</td>
<td>18</td>
<td>66</td>
<td>36</td>
<td>66</td>
</tr>
</tbody>
</table>

*** -Significant (1% level); **-Significant (5% level); *-Significant (10% level).

Robust standard errors clustered at the municipality level in parenthesis. Each entry in the table is from a separate local linear/quadratic regression using the specified bandwidth. The level of observation is a municipality-election. The estimated treatment effect is of a change from SB to DB. An estimate using bandwidth k uses a sample of elections in municipalities with more than 200 000-k and less than 200 000+k registered voters. Details on the dependent variables are in the text. Single-ballot mean refer to the average value in municipalities with more than 175 000 and less than 200 000 voters.
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Bibliography


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Appendix A:

Summary Statistics

Tables A1, A2, and A3 provide the summary statistics for the data used on Chapters 1, 2 and 3, 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.\footnote{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.}

Panel A of Table A4 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 Chapter 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 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 A4 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
Appendix B: Additional Results on the Effects of Electronic Voting

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 A4 indicate that this difference is due to the fact that valid voting in federal elections is smaller than in state elections.\(^{87}\)

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\(^{87}\)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
Appendix C: Robustness Checks of RDD Estimates

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.\textsuperscript{88}

\textbf{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

\textsuperscript{88}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.
Appendix C: Robustness Checks of RDD Estimates

runoff rules instead of plurality to elect its mayors. Given that 200,000 is far from the 40,500-voter threshold in this paper and that municipal and state elections are never held in the same year, there is no reason to believe that it can confound the results.\textsuperscript{89}

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 \textit{after} the 1998 election, and hence cannot account for the results.\textsuperscript{90} 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.\textsuperscript{91}

\textsuperscript{89}Chapter 3 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.

\textsuperscript{90}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.

\textsuperscript{91}Litschig (2010), Litschig and Morrison (2010), and Brollo et al. (2010) exploit this discontinuity to estimate the effects of government grants.
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 issues 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 A5 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 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.
Appendix E: Treatment Effects of Dual Ballots with Controls

As in a randomized experiment, with a regression discontinuity design consistent estimates of the treatment effects can be obtained without including covariates in the estimations. However, it is common practice to do so for two reasons. First, covariates that are known not to be affected by treatment/control status but are correlated to the outcome variable may increase the precision of the estimates. Second, it provides a robustness check, since the inclusion of the covariates should not affect the size of the estimated treatment effects.

In this section I repeat the main estimation of the paper (presented in the first row of Table 8) using different covariates as controls. First, a full set of time dummies is included in all the regressions presented in this appendix. I also add three separate sets of controls. The first one is the “electoral covariates” set, which include three variables - the number of candidates, the registration rate and the turnout rate - that are describe in Subsection 4.3.1.
The second set is named “economic covariates” and includes the per capita income, average years of schooling, share of population living in a rural area and a measure of income inequality (Gini index) in the municipality - these variables and their sources are described in Subsection 4.3.2. Finally, there is a “geographical covariates” set that includes the municipality’s longitude and latitude (see Subsection 4.3.2 for details).

The results are presented in Table A6. A comparison with the first row of Table 8 shows that the estimates’ magnitude and significance are robust to a number of different covariates. The size of the standard errors show, however, that there is not much gain in precision by adding additional controls. I also add three separate sets of controls. The first one is the electoral covariates set, which include three variables - the number of candidates, the registration rate and the turnout rate - that are describe in Subsection 4.3.1. The second set is named economic covariates and includes the per capita income, average years of schooling, share of population living in a rural area and a measure of income inequality (Gini index) in the municipality - these variables and their sources are described in Subsection 4.3.2. Finally, there is a geographical covariates set that includes the municipality’s longitude and latitude (see Subsection 4.3.2 for details). The results are presented in Table A6. A comparison with the first row of Table 8 shows that the estimates magnitude and significance are robust to a number of different covariates. The size of the standard errors show, however, that there is not much gain in precision by adding additional controls.
Appendix F: TE of Dual Ballots in Elections

Predicted to Contested and Uncontested

This appendix repeats the exercise described in Subsection 4.3.3 and presented in Table 11. However, instead of coding elections as “contested” and “uncontested” according to the actual vote share of the most voted candidate, I use vote shares predicted by the outcome of the municipal legislature elections (see Subsection 4.4 for details).

First, I run a regression of the vote share of the first placed mayoral candidate against the vote share of the most voted party in the municipal legislature election. The latter is a strong predictor of the former given that it captures the presence of a popular party in the election. The predicted first placed mayoral candidate vote shares obtained from this regression is then used to generate a “contested elections” (and “uncontested elections”) sample, with predicted vote share lower (above) than 50 percent.
Appendix F: TE of Dual Ballots in Elections Predicted to Contested and Uncontested

By using predicted vote shares instead of the actual ones, I avoid sample selection issues created by the fact that the first placed candidate vote share is mechanically correlated with the dependent variables of interest. The intuition is that the criterion to separate the samples relies solely on variation in the outcomes of the municipal legislature elections, which are shown in Subsection 4.3.1 not to be affected by a change from SB to DB.

The results are presented in Table A7. Comparison with Table 11 shows that they are very similar. There are positive and significant results for the contested elections subsample but not for the uncontested elections subsample. Notice that, although the sample sizes in the former are relatively small, this result is mostly driven by the small (or even negative) size of the estimates and not by the larger standard errors.

In conclusion, this exercise increases confidence in the evidence presented in Subsection 4.3.3.