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Renovating Democracy: The Political Consequences of Election Reforms in Post-War Brazil

By

Fernando Daniel Hidalgo

A dissertation submitted in partial satisfaction of the
requirements for the degree of
Doctor of Philosophy

in

Political Science

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor David Collier, Co-chair
Professor Jasjeet Sekhon, Co-chair
Associate Professor Jason Wittenberg
Assistant Professor Frederico Finan

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Abstract

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Elections in the wake of transitions to democracy are often structured by formal and informal institutions that benefit anti-democratic elites and that reduce the potential of expanded suffrage to affect policy. While most of the writing on counter-majoritarian institutions focuses on formal rules, the political consequences of *informal institutions* that can distort elections' capacity to accurately represent the electorate is less well understood. Drawing upon historical and recent evidence from Brazil, this study analyzes the specific aspects of the mechanics of Brazilian elections that interacted with informal practices to over-represent rural conservative interests, increased the ability of conservative political machines to win elections, and de-facto dis-enfranchised large swaths of the Brazilian electorate. These informal practices had substantial consequences for the quality of representation both during Brazil's first experiment with mass democracy between 1945 and 1964, as well as its most recent experience with widespread suffrage. Furthermore, this analysis considers the conditions under which interventions—such as the provision of information—can improve the extent to which elections can induce accountability and representation.

The first chapter examines a long standing anti-democratic practice in Brazil: the de-facto disenfranchisement of millions of Brazilian voters and widespread voting fraud caused by the interaction of a difficult paper ballot and permissive electoral rules. To provide such evidence, this analysis exploits the phased adoption of electronic voting in Brazil, a reform that increased the effective franchise in legislative elections by about 33 percent and eliminated fraud in the vote counting process. The research design relies on the fact that the reform was initially implemented in municipalities with an electorate over an arbitrary threshold and consequently allows for a regression discontinuity design. The two distinct effects of electronic voting—the enfranchisement of illiterates and other low information voters and the dramatic reduction of fraud—had consequences for the composition of the national legislature. Against the predictions of recent eco-

nomic models of democratization, the data show that the enfranchisement of illiterates and other low information voters caused a small increase in the vote shares of right-wing candidates. More importantly, newly enfranchised voters were dramatically more likely to cast a "party list" or partisan ballot as opposed to a personal or candidate ballot, which benefitted Brazil's more programmatic and ideologically coherent parties. In states with hegemonic conservative parties, the introduction of electronic voting induced a roughly 20 percentage point swing against "political machine" candidates, which is attributable to a substantial reduction in fraud. Overall, the most important consequences of the reform was the strengthening of Brazil's major parties and a weakening of dominant subnational conservative political machines.

The second chapter analyzes the distortion of political representation during Brazil's first experiment with mass democracy between 1949 and 1964 via widespread manipulation of the voter registration process. This study investigates the consequences of an informal practice that functioned to severely over-represent rural interests. Specifically, the delegation of voter registration to local elites in the countryside—-which stemmed from state weakness and the difficulty of effectively monitoring bureaucrats in the countryside—allowed for the fraudulent inflation of the rural electorate and the increased probability of electing rural-biased chief governors and presidents. Drawing upon historical data from the decade after Brazil's first transition to democracy in 1949, the analysis shows that due to the delegation of registration, the voter registry massively over-represented rural areas and that this phenomenon tended to benefit conservative gubernatorial and presidential candidates. This chapter's research design takes advantage of a reform that dramatically decreased the over-representation of the countryside in the voter registry to show how this fraud bolstered the political fortunes of rural-oriented politicians. Paradoxically, this fraud-reducing reform diminished the acceptability of elections to rural elites, thus highlighting the role of *informal* counter-majoritarian institutions in generating support for formal democracy.

The third chapter explores how interventions that increase the amount of information available to the electorate can affect political accountability. An underlying assumption of much of the literature on political corruption is that if voters are provided with information about the performance of politicians by actors in civil society such as the media and non-governmental organizations, then the election of corrupt politicians is less likely. Yet, heterogeneous views about the importance of corruption can determine whether increased information changes electoral outcomes. If partisan cleavages correlate with the importance voters place on corruption, then the consequences of information may vary by candidate, even when voters identify multiple candidates as corrupt. This chapter provides evidence of this mechanism from a field experiment in a mayoral election in São Paulo where a reputable interest group declared both candidates corrupt. Informing voters about the challenger's record reduced turnout by 1.9 percentage points and increased the opponent's vote by 2.6 percentage points. Informing voters about the incumbent's record had no effect on behavior. This divergent finding is attributable to differences in how each candidate's supporters view corruption. Survey data and a sur-

vey experiment show that the challengers' supporters are more willing to punish their candidate for corruption, while the incumbent's supporters lack this inclination.

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Chapter 1

Voting Technology and Enfranchisement: Consequences for Party Competition and the Survival of Regional Oligarchies in Brazil

In Europe, North America, and Latin America, early democratization was typically a gradual process. Rather than emerging all at once, democratic institutions arose incrementally from distinct episodes of reform that introduced new electoral rules (Boix 1999), gradual enlargements of the franchise to include excluded social groups (Przeworski 2009), and the elimination of authoritarian enclaves (Gibson 2005), among other diverse outcomes. Scholars, analyzing a broad array of historical cases, have linked the adoption of new institutions to broader regime dynamics (Collier and Collier 1991), changes in policymaking (Lindert 2004), and the structure of the party system (Przeworski and Sprague 1988). While existing studies have provided substantial insight on the broad effects of institutional change, there is little systematic and comparable evidence on which democratizing reforms were most consequential, as well as precise comparisons of their effects.

One important reform common in many countries' early history of democratization was the removal of barriers preventing or hindering citizens with low socio-economic status from voting (Paxton et al. 2003). Perhaps just as notable were reforms that reduced electoral fraud used by incumbents to maintain power (Lehoucq 2003). Both types of reforms are of great interest and undoubtedly important, but which is most consequential for the kinds of parties that win elections and the mix of policies eventually adopted? This question is fundamental to our understanding of democratization, but it has been difficult to answer with existing evidence and research designs. Previous historical studies of democratic reforms have traditionally used methods that rely on either strong statistical assumptions or deep historical knowledge of a few cases. While these studies have been extremely valuable, they are ill-suited for the task of precisely weighing the relative contributions of different types of democratizing reforms on political change.

In this analysis, I study a reform adopted in Brazil that effectively enfranchised large numbers of mostly illiterate and other low income voters, as well as eliminated most voting fraud used by conservative political machines to maintain their power. The innovation of the paper is that a research design is employed that allows for credible causal inference and precise measurement of the reform's political consequences. Specifically, I examine the consequences of the 1998 adoption of a new voting technology—electronic voting machines— that radically simplified the act of casting a vote for illiterates and other low information voters, and successfully shielded the vote counting process from manipulation. Exploiting this change, one can answer the following question: what happens when the electorate is suddenly and abruptly expanded? Furthermore, what happens when the opportunities for stealing elections with voting fraud are sharply reduced? Which is more consequential and how are their effects different?

Crucially for the design, the voting reform was not adopted across the entire country, but instead implemented in stages over several elections. Just as important, the new voting system was allocated to municipalities (equivalent to a US county) using an arbitrary function of the number of voters as counted in 1996. As a consequence, the outcomes in municipalities immediately above and below a cut-point (known as a regression discontinuity design) can be compared. Under some mild assumptions, the differences

between municipalities in a narrow band around this threshold can be treated as causal. Using this design, I show that the adoption of electronic voting increased the number of counted votes for candidates of the national legislature by about one third. Consistent with the theory that the reform reshaped the socioeconomic profile of the electorate, the increase in valid votes was heavily concentrated in the poorest and most illiterate municipalities. The new voting technology decreased the number of blank and invalid votes across all political offices, but the change was most pronounced in the votes cast for candidates for the the lower chamber of the national legislature, and state deputy, legislators in the unicameral state assemblies. Why did electronic voting have such a large effect on the ability of voters to cast a counted ballot for legislative offices?

The enormous impact of the ballot format could be attributed to how voting technology interacts with the informational demands generated by Brazil's electoral rules. The combination of high district magnitude and candidate centered ballots ensure that both the costs associated with forming a preference over who to vote for and the actual mechanics of voting are higher than those incurred in smaller magnitude or party-centered electoral systems. For a voter to successfully cast a counted ballot for a candidate, first they would have to choose one among the typically hundreds (sometimes thousands) of candidates running in his district to voter for. Once their opinion was formed, to cast a ballot they would have to either write in the candidate's name or unique identification number on the ballot. For an electorate with an illiteracy rate of about a third, the task of remembering a name or number and writing it down quickly without making a mistake was not trivial. As a result, the rate of blank or null[1] ballots reached about 40 percent of all votes cast in 1994.

Electronic voting simplified the electoral process for illiterates and other low information voters. As discussed in more detail below, the user interface of the electronic voting machines eliminated the need to physically write a candidate name or number. Just as importantly, the new machines allowed voters to ensure that their vote choice was correctly registered. After these simple changes were implemented nationwide in 2002, the blank and invalid vote share fell to about 10 percent, the regional average. The reduction in technologically-induced disenfranchisement can be treated as a *de facto* expansion of the electorate, akin to those that occurred with the removal of literacy[2] and property requirements in the 19th and 20th century.

Apart from how it affected the voting experience of voters, the adoption of the new voting system had a second and equally important impact on the conduct of elections in Brazil: electronic tabulation of votes increased the barriers to voting fraud. Under the paper ballot system, votes counting was decentralized and susceptible to penetration

[1]These voters registered as having cast either an intentionally blank vote or no vote at all, known as a "null vote". In the political science literature on voting technology, these votes are often labeled "residual votes". See for example, Ansolabehere and Stewart (2008). Following Brazilian terminology, I retain the label "null" votes.

[2]Notably, in Brazil the literacy requirement was not removed until the adoption of the 1988 constitution.

by organized political machines. While low level fraud was widespread, electoral fraud was particularly acute in several states with dominant ruling parties. For these ruling machines, illegal manipulation of the vote count was an important tool in maintaining the incumbent coalition in power. By centralizing the vote counting process in the hands of an insulated electoral bureaucracy, electronic voting eliminated, by all accounts, fraud in the tabulation of votes.

The two distinct effects of electronic voting—the enfranchisement of illiterates and other low information voters and the elimination of fraud—had consequences for the composition of the national legislature. In contrast to the predictions of recent political economy models of democratization, the enfranchisement of low-income voters marginally benefitted right of center candidates. The sharp increase in the number of low income voters into the electorate did, however, have an important effect on partisan competition in a manner not captured by these spare models. The newly enfranchised elected to cast a "party list ballot" (akin to a party ticket) as opposed to a vote for a particular candidate in much higher numbers than the already enfranchised. Furthermore, this shift towards more partisan voting disproportionately benefited Brazil's major parties with distinct ideological profiles or a record of governing, as opposed to catch-all and other minor parties. The influx of these voters into the electorate acted as a positive shock to the electoral value of these parties' labels. Thus, rather than affecting the ideological composition of the legislature, the increased size of the electorate instead affected the structure of the party system by tilting the electoral playing field towards Brazil's ideological governing parties over their more inchoate and ideologically diffuse rivals.

In states governed by dominant political machines, defined more precisely below, the consequences of electronic voting were particularly potent. The new voting technology induced a sharp decrease in the vote shares of federal and state legislative candidates belonging to the governors' coalition in these states. This effect is attributable to prevalence of extensive fraud under the paper ballot system. Federal and state candidates affiliated with the incumbent machine lost an average of 22 and 30 percentage points, respectively. In neighboring states with similar socio-demographic profiles but with greater pre-existing political competition, electronic voting had no effect on incumbent coalition vote shares.

1.1 The Empirics and Theory of Democratizing Reforms

The reforms examined in this chapter are "democratizing", in that they had similar consequences—expanding the size of the electorate and ending rampant fraud—as many of the reforms implemented in historical cases of democratic transitions. While wholesale transitions from authoritarian to democratic regimes were more common after the second World War, earlier episodes of democratization were often the culmination of a chain of events wherein democratic institutions were created and strengthened over decades, albeit in fits and starts and with notable retrogressions (Capoccia and Ziblatt

2010). Thus, rather than studying the effect democratization as a regime-level variable, as is the norm in cross-national studies,[3] I analyze a more specific set of changes common in historical cases of democratization, which allow for more credible causal inference, precise measurement of how reforms affected political outcomes, and systematic comparisons between different types of democratizing reforms.

An important question is whether the reforms under examination in this study have any bearing on larger theoretical questions about the consequences of democratization. I argue that this reform did in so far as removal of barriers[4] for low income voters and the enactment of safeguards against electoral fraud were important in many episodes of democratization prominent in the literature. In an analysis of 24 early episodes of democratization in Europe and South America, R. B. Collier (1999)[5] finds that expansions of the electorate (greater than 5 percent) occurred in 15 of the cases she considers.[6] Furthermore, the magnitude of the expansion documented in this analysis is larger those that accompanied many historical cases of suffrage extensions. For example, the expansion due to electronic voting was approximately equal to or greater than the increase that occurred in five of the suffrage expansions that coincided with historical democratization episodes in Collier's study.[7] In the dataset collected by Przeworski (2009) covering all suffrage extensions between 1788 and 2000, the average increase in the number of voters as a share of the adult population in legislative elections following extensions was 13.2 percentage points (p. 11), about half the increase documented in this study. Voting fraud was also an important issue in the historical cases documented by Collier: five episodes of democratization included reforms intended to prevent the stealing of elections. In some cases, such as Argentina in 1912, fraud was the central focus of the democratizing reform.

Furthermore, the specific type of enfranchisement barrier—difficult ballot formats—is just one example of a broader phenomenon observed in other countries and historical

[3]Representative examples of the recent literature examining the effects of democracy on economic or political outcomes include Barro (1996), Gerring et al. (2005), Huber et al. (2006), Persson and Tabellini (2007), Przeworski and Limongi (1993), and Przeworski (2000). As discussed by Seawright (2007), a real concern for these studies is that the specification assumption may not be plausible.

[4]Some may object that the barrier under examination—complex ballot format—is a *de facto* as opposed to a *de jure* restriction. There are many examples in the historical record, however, of *de jure* manhood or universal suffrage being coupled with severe *de facto* limitations on the right to vote, such as occurred in the U.S. South (Key 1949) and Imperial Germany (Ziblatt 2009).

[5]Collier formally defines democratization in her study as the adoption of liberal constitutional rule, mass elections, and a legislative assembly. She acknowledges, though, that complications arise in precisely classifying these countries according to these criteria and consequently adopted an "inclusive approach" in cases that were ambiguous.

[6]Suffrage expansions are not equivalent to episodes of democratization as other necessary conditions, such as a functioning legislative assembly, may have not been present at the time of the extension. This was the case in Germany, for example, which had full manhood suffrage long before a parliament with sufficient powers to counterbalance the executive.

[7]These cases are: Great Britain (1884), Great Britain, (1918), Italy (1919), Netherlands (1917), and Sweden (1918-20).

periods. In the post-reconstruction US South, for example, manipulation of ballot designs was one of the portfolio of tools, including fraud, poll taxes, and literacy tests, used by Democratic party politicians to suppress opposition voters, primarily African Americans but also poor whites. When the Australian ballot was introduced, Southern politicians designed the ballot to be difficult for poor and illiterate voters to cast a valid vote (Kousser 1974, p. 53). Party symbols and party lists, for example, were excised in favor of long and complex ballots that advantaged the educated. The president of the Alabama State Senate explained his support for the Australian ballot by arguing that under it, "the ignorant are practically disenfranchised". This strategy was judged as effective by Virginia Governor J. Hoge Tyler, who stated that "thousands of defective or improperly marked ballots have been thrown out in every election since the [secret ballot] law was enacted—in many instances as many as one-third or one-half of the ballots deposited" (p. 54).

From a comparative perspective, widespread voting fraud is a much more commonly observed problem than complex paper ballots.[8] The political history of Latin America (Lehoucq 2003, p. 237-38), the US (Bensel 2004), and Europe (Ziblatt 2009) is rich with examples of political machines manufacturing votes for favored candidates. In Latin America, the paradigmatic case is the rural political machine tied to large landowners that elects conservative politicians through fraud, clientelism, and violence.[9] While some level of voting fraud is unavoidable, it can reach levels that make elections irrelevant. This certainly was the case in Argentina during the 1930s, where conservatives relied on fraud to win control over the national government Alston and Gallo 2009, p. 183. While this level of endemic fraud is less relevant due to urbanization and electoral reforms, significant regional pockets of continued extralegal disenfranchisement persist. Gibson (2005), for example, documents the rise of a political machine that used electoral fraud to win office in the state of Oaxaca, Mexico in the early 2000s. As discussed more fully below, in Brazil voting fraud was considered to be widespread in the early 1990s, particularly in states with low levels of political competition.

Theoretical Framework

Standard models of democratization rely on the logic of the Downsian spatial model to show that the political effects of expanding the franchise are a function of two factors: the extent to which the electorate grows and the difference in the distribution of preferences between the newly-enfranchised and the already enfranchised. If the reform grows the electorate by a small amount, then politicians will have little incentive to cater to new voters even if the new voters were, on average, far to the left or the right of pre-reform electorate. Similarly, even if many new voters enter the electoral arena, their entrance

[8] Alberto Simpser, relying on news reports, finds that 22 percent of 394 of national executive elections between 1990 and 2007 were marred by severe electoral fraud.

[9] For the classic treatment of this phenomenon in the Brazilian case see Leal (1975). For a game theoretic treatment of this phenomenon, see Baland and Robinson (2008).

may have little consequence if their preferences do not diverge from pre-reform voters' preferences.

Under this framework, the nature of the voting restriction determines the political consequences of its removal. The effect of removing a literacy requirement, for example, depends on the degree to which literate and illiterate citizens differ in their preferred political outcome and the number of illiterates in the electorate. Recent prominent scholars of institutional change (Acemoglu and Robinson 2001; Baland and Robinson 2008; Llavador and Oxoby 2005; Naidu 2009) follow Lipset (1960) in treating electoral politics as "democratic class struggle", where voters' preferences are a function of their position in the income distribution. Since many restrictions specifically target poorer voters, in these models suffrage reforms will move the median voter to the left. For Brazil, a country with extremely high levels of economic inequality, the prediction of these models is stark: enfranchising illiterates and other low income voters should benefit left-wing parties.

Simple "class struggle" models of democracy do not fit the historical record for many countries. As discussed by Lipset and Rokkan (1967) and R. B. Collier (1999, pg. 55), among many others, religion, regional, partisan, and other cleavages can substantially mitigate or even invert the negative correlation between economic resources and left-wing ideology. Depending on how these alternative cleavages are manifested in the party system, newly enfranchised voters may be more or less left-leaning than the already-enfranchised. Furthermore, non-programmatic strategies of voter mobilization, such as clientelistic or particularistic appeals, might be particularly effective among new voters. Comparatively low-income voters are more likely to be amenable to vote buying and other clientelistic exchanges, as the need for economic resources may trump ideological or partisan considerations (Kitschelt 2000, 856-57). Of course, both left-wing and right-wing politicians can employ particularistic strategies, but in Brazil, clientelism is most closely associated with traditional conservative politicians (Hagopian 2007). Thus, new entrants to the political arena may vote disproportionately for conservative candidates due to their privileged access to clientelistic networks, the opposite prediction of "class struggle" models of democratization.

Furthermore, and perhaps more importantly, the party system can mediate the consequences of enfranchisement in ways that are distinct from the simple mapping between candidates' and voters preferences. One dimension of partisan competition not well captured by simple spatial models, for example, is the degree to which party labels carry meaningful information about the policy positions of candidates. As well established in the behavior literature, for voters with low levels of political information, party labels are an important substitute for learning about individual candidates (Popkin 1991). Party labels can be used both for retrospective voting, when the label is associated with an incumbent government, or prospective voting, when the label is informative about the types of policies the party will pursue in office. Yet as is the case in many countries, ideologically inchoate parties can coexist with programmatic parties. Forming a judgement about an entire party in many cases will be much easier than evaluating individual

politicians, but this will depend in part on the clarity of the signal sent by individual parties about their programmatic orientation.[10] Newly enfranchised voters by virtue of their socio-economic status or the fact that they previously were not targets for political mobilization, may especially be information-poor. As a consequence, parties with cohesive brands may benefit simply because voting for them will be less informationally costly. In this fashion, democratization could be consequential for electoral politics by increasing the value of a party label with a strong brand, which in turn could alter the dynamics of the party system.

While suffrage restrictions like literacy and property requirements were common, other barriers to the vote include fraud and violence. These tools have the "virtue" of being more targetable towards voters with specific preferences. If agents of a political machine can penetrate the tabulation process, for example, then their ability to shape political outcomes is greatly enhanced as they subtract votes from the opposition and add votes to allies with relative ease. The systemic consequences of fraud, however, depend on how the means of manipulation are distributed among competing political groups. If fraud is pervasive, but the capacity to manipulate are distributed relatively equally among candidates, then elections may remain highly competitive. In such a context, eliminating fraud could have little or no effect on levels of political competition and the types of candidates elected. Fraud, however, can be costly to sustain, particularly when it must be orchestrated over many locations. Thus, large scale fraud is typically perpetrated by incumbent governments, who can use the apparatus of the state to facilitate vote manipulation. In such cases, the effects of fraud on partisan competition will be very large. While the policy consequences of large scale fraud hinges on the ideological character of the ruling machine, they are likely to be more potent than socio-economic barriers like literacy requirements or overly-difficult ballots. The reason is straightforward: fraud can more precisely mold the electorate than the relatively blunt instrument of socio-economic barriers. In Brazil dominant political machines tend to be conservative, thus in these states, we would expect a reduction in fraud to have large consequences both on the level of political competition and the vote shares of right-wing candidates.

1.2 The Causes of Technological Disenfranchisement

Millions of Brazilian voters were effectively disenfranchised by a combination of paper ballots and a decentralized vote counting process.[11] Paper ballots acted as a *de facto*

[10] In the case of the party of the incumbent prime minister or president, voters may know nothing about its programmatic stance, but still vote in a partisan manner by using simple retrospective voting rules. This is less feasible for non-incumbent parties.

[11] From a comparative perspective, the effect sizes documented are enormous. Most studies of voting technology rely on US data and estimate effects on residual votes on the order of .5 percent to 1.5 percent. For example, Ansolabehere and Stewart (2008, p. 381) find that electronic voting lowered the residual rate

franchise barrier because their format interacted with a complex electoral system to make voting difficult for low information voters. These voters, either due to illiteracy or lack of preparation, could not successfully cast ballots for a candidate or party despite their intention to do so. I label this phenomenon as the "information effect". Because this effect is mediated by the socioeconomic composition of the electorate, it is particularly important in Brazil's poorest and most uneducated regions. The second aspect of Brazil's electoral regulations—the vote counting process—was relevant in parts of Brazil with dominant ruling coalitions, where incumbent machines could penetrate vote counting process and and manufacture votes for themselves. The ability of machine politicians to take advantage of the unwieldy tabulation of paper ballots to inflate their vote totals is what I call the "fraud" effect.

The Interaction Between Electoral Rules and Voting Technology

Brazil's procedures for electing most legislators[12] combine large district magnitudes—the number of members to be elected—with predominantly candidate-centered electoral rules. Legislative districts coincide with state boundaries and district magnitude is approximately related to the size of the electorate in each state. The variation in district magnitude is large: ranging from 8 for the smallest states to 70 for the state of São Paulo. Voters can cast votes for a particular candidate, a particular party, or a "blank" ballot.[13] Party votes[14] only affect how many seats a party or coalition will win, while personal votes affect both the number of seats won by the party and which candidates win office. Voters who show up at the polls but do not register any kind of vote at all are said to have cast "null" votes, to use the Brazilian term. Voting is mandatory, though the sanctions for abstention are weak.

The combination of candidate-centered aggregation rules and the large district magnitudes creates incentives for parties to field a large number of candidates. Unlike in plurality systems, the incentives for voters or parties to coordinate on "viable" candidates are weak at best (Cox 1997). Figure 1.2 depicts the number of candidates for the national legislature and state legislatures in each state. While the number varies from

vote by 1.2 percent in gubernatorial and senate rates when compared to lever machines. Brady et al. (2001, p. 4) estimated that electronic voting technologies cause .5 percent to 1 percent fewer residual votes in presidential elections when compared to punchcard systems. Studies of voting technology outside the US are few, though recent experimental evidence (Calvo, Escolar, and Pomares 2009) from Argentina found that ballot format can have a sizable effect on voters' decisions.

[12]Federal Deputies, state deputies, and city councilors are all elected using using the same open list proportional representation rules. Senators are elected using simple plurality rules, with each district electing three senators.

[13]Seats are distributed to parties or coalitions of parties based on the total number of candidate and party votes of each party or coalition, using the D'Hondt method. After seats are allocated to parties or coalitions, seats are awarded to individual politicians based on the number of personal votes received by each candidate.

[14]There is no option for a voter to vote for one party for all offices, known as "straight-ticket" voting.

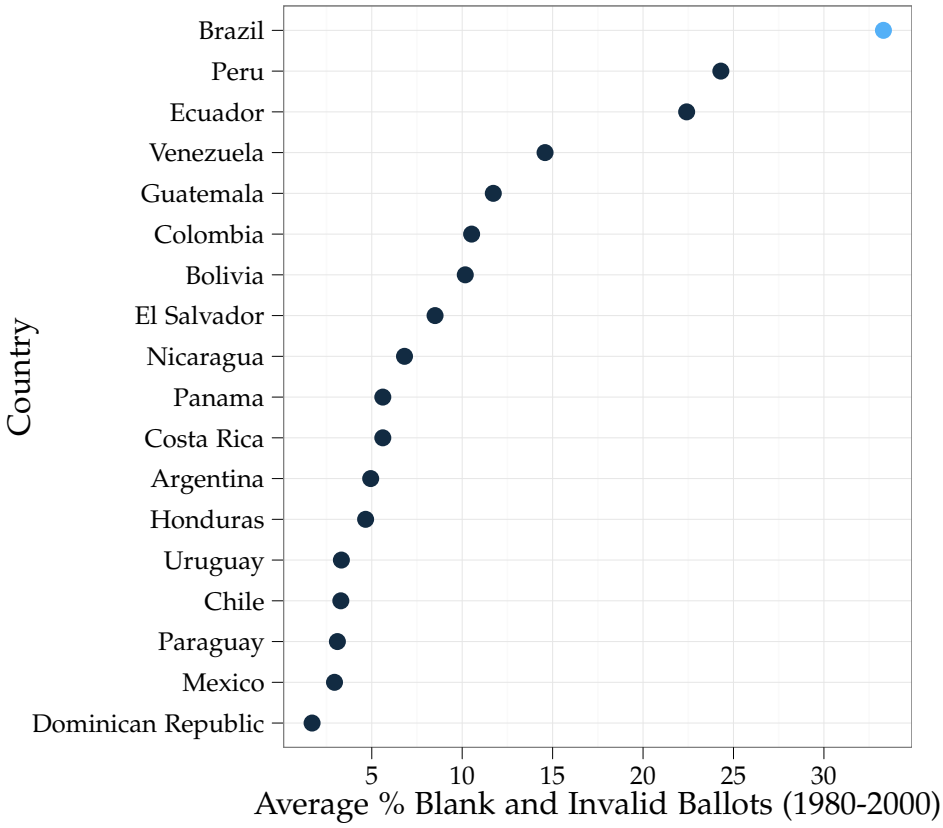


Figure 1.1: Average percent invalid and blank votes in Latin America (1980-2000). Data from Power and Garand (2007).

year to year, elections in medium to large states regularly feature over 200 candidates for the national legislature and over 400 for the state legislature.

The combination of a large number of candidates and the personal vote results in an electoral environment that is more complex than in electoral systems with low district magnitudes or closed lists. Voters cannot rely on simple partisan cues since candidates within a party compete against each other. One indicator of this complexity is the number of null and blank votes cast. While such voters can intentionally cast an invalid vote as a protest vote, existing research suggests that informationally-demanding electoral environments—caused by complicated voting technology (Brady et al. 2001; Kimball and Kropf 2008) or the absence of readily available partisan cues (Bowler, Donovan, and Happ 1992)—is the main contributor to the number of blank and invalid cast in any given election. Figure 1.1 shows the average percent of blank or invalid votes in 18 Latin American countries from 1990 to 2000. Brazil is clearly an outlier on this measure, with average of about 33 percent compared to the regional average (excluding Brazil) of about

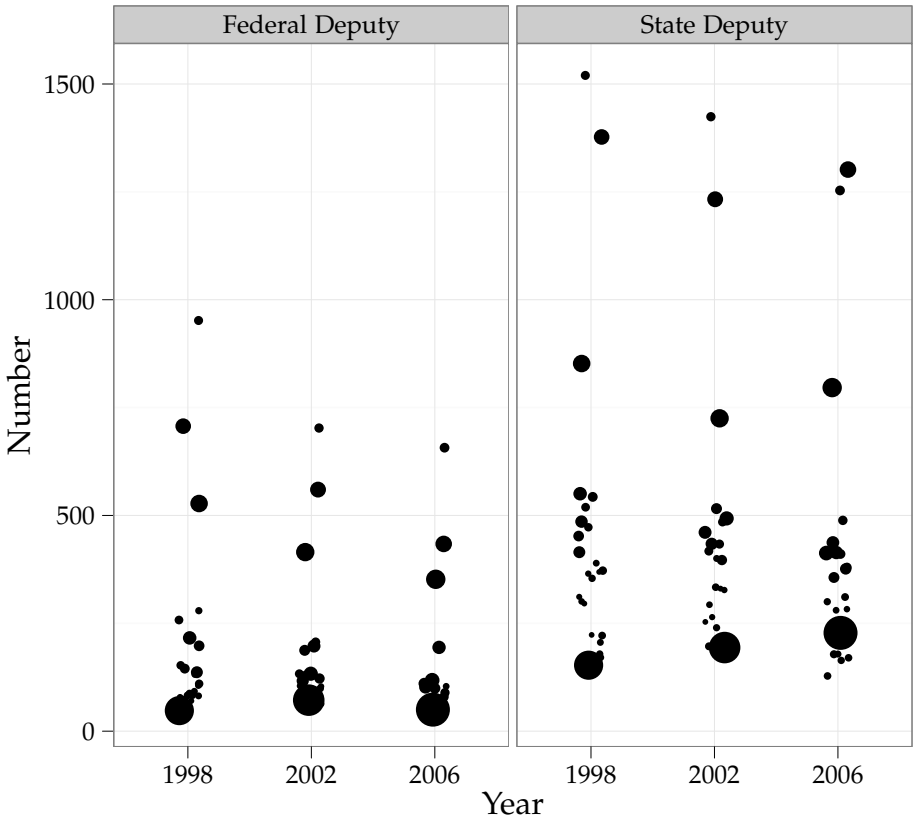


Figure 1.2: Number of candidates for federal deputy and state deputy by state (1998-2006). The size of each dot is proportional to the size of the electorate.

8.5 percent.[15]

One consequence of Brazil's electoral rules is that the actual mechanics of voting and vote counting have a large effect on which voters are able to affect the outcomes of the election (Reynolds and Steenbergen 2006). Voting procedures can typically affect electoral outcomes by influencing how voters choose between candidates and how ballots are counted. Because of the overwhelming number of choices, the ways in which ballots are constructed can powerfully shape voters'—particularly low information voters—ability to register a political preference. Seemingly small changes in the ways in which voters' choices are presented can have large effects on the number of voters unable to cast the vote they intended.[16]

The second important mechanism for how changes in voting procedures affect elec-

[15]In the United States, the average residual vote rate between 1988 and 2000 in US senate elections was 4.2 percent (Ansolabehere and Stewart 2008, p. 375).

[16]The classic example of how ballot layout can impact election outcomes is the Florida "butterfly" ballot in the 2000 presidential election. See Wand et al. (2002) for a detailed examination of this case.

toral outcomes is through how votes are tabulated and assigned to candidates. In systems with low district magnitudes and a small number of candidates, small changes in electoral procedures are unlikely to affect who wins and who loses in most cases. In systems with large multi-member districts, however, winning candidates are typically elected with tiny fractions of each district's electorate and as a consequence, small changes in counting procedures can substantially affect who wins and who loses office. Furthermore, when electoral results depend on a relatively small number of votes, electoral fraud becomes much more potent. Thus, changes that inhibit fraud are much more likely to affect the actual outcome of the election in cases such as Brazil, where the electorate's votes are fragmented among a large numbers of candidates.

From Paper Ballots to Electronic Voting

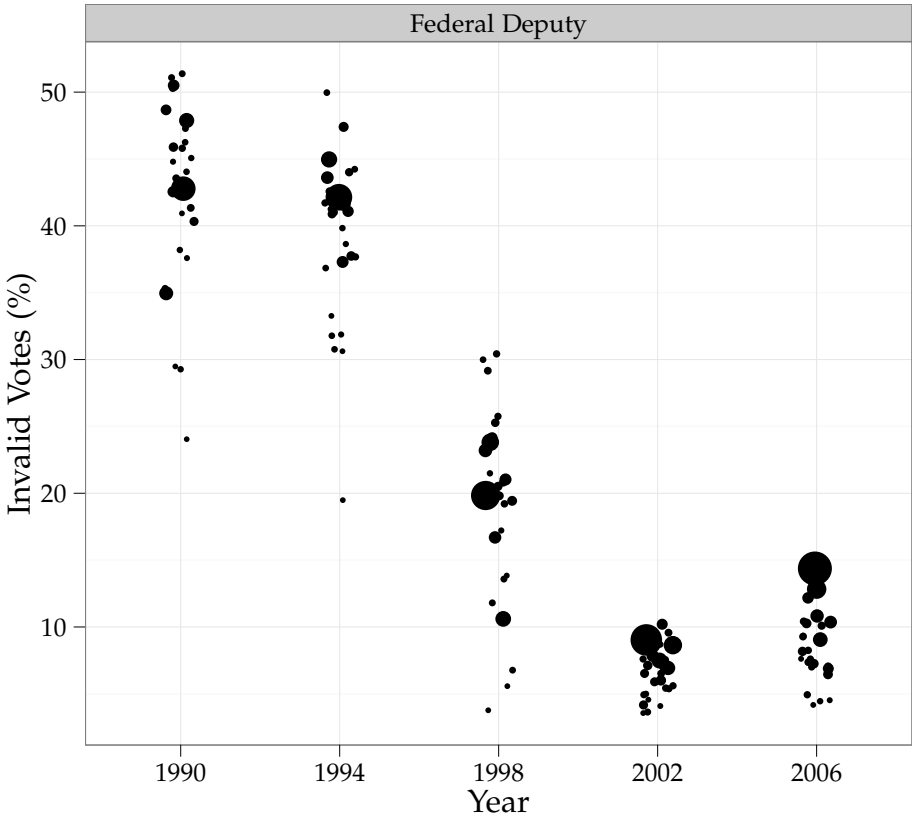
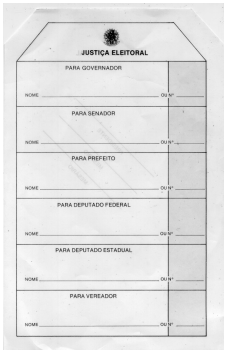


Figure 1.3: Null and blank votes as a percentage of votes cast (1990-2006). Each dot represents a state (district) and their size is proportional to the size of the electorate.

Between 1996 and 2002, increasing numbers of Brazilian voters cast their votes using electronic voting machines, instead of paper ballots. This change in voting procedures



(a) Paper ballot from the 1982 elections.

(b) Electronic voting machine introduced in 1996.

Figure 1.4: Ballots in Brazil

altered the composition of the electorate. Suggestive evidence for this can be seen in figure 1.3. Before 1998, the percentage of voters casting either blank or null votes in legislative elections in each district was generally larger than 20 percent of votes cast and surpassed 40 percent in most cases. In 1998 when approximately 50 percent of the electorate used electronic voting machines, the distribution of invalid votes shifted noticeably downwards and in 2002, when electronic voting was universal, almost all states had below 10 percent invalid or blank votes. In less than ten years, Brazil went from being the regional champion in invalid votes to just average. As shown more systematically below, the introduction of voting technology that simultaneously altered how voters voted and how votes were counted caused this dramatic change.

Paper Ballots

From 1958 until 1996, all Brazilian voters voted with government-provided paper ballots (Nicolau 2002). Unlike in most democracies where the ballot lists all the possible candidates and/or parties, the sheer number of options available to voters made this option impractical in Brazil. As a result, the ballot required voters to write either the candidate's name or the candidate's number, which is five digits for federal deputy candidates and six digits for state deputy candidates. Voters could also simply write a party name or party number to cast a party vote and not indicate a preference for a particular candidate. An example of such a ballot is shown in figure 1.4a, which is the 1982 ballot for governor, senator, mayor, federal deputy, state deputy, and city councilor.[17]

[17]Under the 1988 constitution, the election schedule was altered so that local elections were not held in the same year as national elections.

The Challenge of Voting Expressing a preference on a paper ballot that would be ultimately counted was quite difficult for many voters. For the 20 percent of the electorate that was illiterate, writing a five or six digit sequence of numbers was not a trivial task. This is compounded by the fact that in legislative elections, voters vote for multiple offices and would have to fill in a total of 16 to 19 numbers if they were to cast personal votes [18] for all offices. "We don't hold all the names in our head and I didn't know what to write", explained one voter who failed to cast a valid vote in a 1994 news article (Lessa 1994). One electoral official argued that the large number of blank votes could largely be attributed to illiterates, "who did not want to take a long time writing in a name, thus revealing the fact that they could not write."[19] Furthermore, voters had no way to verify that the number they wrote on the ballot actually corresponded to the candidate or party they intended to vote for.[20]. Such errors were in evidence during tests of the 1994 paper ballot format, where voters often wrote the names of the presidential candidates in the ballot line for federal and state deputy.[21]

Voter Fraud Fraud in Brazil, according to press accounts and interviews with former candidates and electoral officials, was widespread in some regions due to the cumbersome tabulation procedures for counting paper ballots. The qualitative evidence suggests that fraud was primarily a phenomenon that occurred after the counting started (which could last up to two months), while the use of violence to engage in ballot stuffing was quite rare.[22] Under the paper ballot system, votes were counted in a decentralized fashion where counting committees ("juntas apuradoras") were responsible for ballots cast in designated geographic areas. These committees were ostensibly composed of appointed citizens of "moral probity" ("notória idoneidade") unconnected to political candidates, but in practice candidates with special access to the administrative machinery of the state or linked to political machines were often able ensure that the committees were stacked in their favor (Viana 1988), particularly in rural areas.[23]

[18] Every other legislative election, voters cast two votes for senators. As a result, the total number of digits they must remember will vary from election to election. In a year where they only vote for one senate candidate, a voter must fill in 2 digits for a presidential candidate, 3 digits for a senate candidate, 5 digits for a federal deputy candidate, and 6 digits for a state deputy candidate.

[19] Interview with TSE minister Sepúlveda Pertence.

[20] Another election official argued similarly: "Once he [an illiterate voter] cast a ballot, the handwriting was so bad, at times so incomprehensible, that his vote wasn't counted." Testimony of Paulo César Bhering Camarão in Câmara de Deputados, June 6, 2005.

[21] Cosa, Rosa. "Votos Viram Salada Partidária", *Estado de São Paulo*, October 1, 1994.

[22] Paper ballots also made vote buying easier because it made vote monitoring easier. A variety of creative methods were used to monitor votes, including a system wherein a voter would enter with a marked ballot, cast the ballot, then exit with a blank ballot for the next voter to mark.

[23] While fraud was reported to particularly prevalent in northeast Brazil and in rural areas, it was not confined to those areas. Rio de Janeiro electoral officials, for example, overturned he results from the 1994 legislative elections for the entire state and called for new elections because of widespread fraud.

Vote counters could commit fraud in a number of ways, but the most common tactic, according to election officials, was to manipulate polling station tabulation sheets by adding votes to machine candidates and then subtracting the amount added from the blank or null vote totals, thus preserving the total number of voters cast.[24] The disenfranchising effect of complex ballots made fraud easier, as described by Federal Deputy Tourinho Dantas:

> If an illiterate voter doesn't know how to read or write, how can he vote? They humiliate themselves at the moment in which they vote. When he goes to the ballot booth and he doesn't know what to do, he casts a blank vote. This vote, in the majority of places, its filled out by those perpetrating fraud. It is by this means that fraudlent votes are cast in so many places.[25]

Another federal deputy from the state of Paraíba discussed his own encounter with this type of fraud:

> In my election, I had to ask for a recount because according to the announced results, I had lost. With the recount, we reversed the fraud that happened in my municipality of Guarabira. More than 800 blank and null votes were given to another candidate.[26]

This tactic was so common that the number of blank and null votes is one of the main criteria used in Brazilian election law[27] to determine whether or not votes should be recounted in a suspect polling station. In several states, abnormally low numbers of the blank and null votes served as prima facie evidence of fraud and consequently triggered recounts (Araujo 1998).

Electronic Voting

Given the alarming share of the vote that was recorded as null or blank, along with evidence of endemic voting fraud, the Superior Election Tribunal or "TSE" [28], Brazil's national electoral authorities, decided to abandon the paper balloting system for a new electronic voting system in the early 1990s. Unlike in the United States where election administration is decentralized and frequently controlled by elected officials, the TSE is part of the judicial branch and major decisions are made by a court comprised of

[24]See Carvalho, Happy. "'Encontrei uma Quadrilha', Afirma Juiz", *Estado de São Paulo*, October 15, 1994. For another expose on such practices see Lessa, Ricardo. "Juízes Comandam Máquinas das Fraudes", *Estado de São Paulo*, October 16, 1994. In Brazilian political slang, this practice was known as "mapismo".}

[25]Tourinho Dantas. *Dîario do Congresso Nacional*, October 27, 1994, p. 13331.

[26]Interview with Federal Deputy Avenzoar Arruda, (11/1/2008).

[27]Article 88, Law 9.404, September 30, 1997.

[28]In Portuguese, *Tribunal Superior Eleitoral*

supreme court justices and judges appointed[29] by the President. The court is well insulated from political pressure and no major political figure disputes its impartiality, a remarkable fact discussed extensively in Fleischer and Barreto (2009).

The court tested a variety of electronic voting systems both for ease of use and for security of the counting process. The TSE settled on an interface reminiscent of banking ATM machines, a technology widespread in Brazil by the early 1990s[30]. The interface is depicted in figure 1.4b. The voter inputs the number of the candidate or party and the candidate or party will appear on the screen for confirmation. Furthermore, there is a dedicated button for a "blank vote". This design was considered an improvement over the paper system because even illiterate voters could typically recognize and enter numbers without error and voters could verify that the number they inputed corresponded to the candidate they intended to vote for. In an article discussing the implementation of a new system, an illiterate voter explained that

> "it's easier because I don't need paper nor pen. Because I don't know how to write, it used to take a long time. With the new system, I see the picture of the candidate and knowing that the number is correct, everything works ok".[31]

While some computer scientists and political activists remain skeptical of the system's security, electronic voting achieved wide legitimacy very quickly. No serious cases of fraud in the tabulation process have emerged since the system was first implemented. Moreover, Brazil exported the technology to other countries in Latin America, including Argentina and Paraguay.

Under the paper ballot system, results would sometimes be released weeks after the election, giving ample opportunity for the vote counting process to be corrupted. After the adoption of the new system, results would be released in a matter of hours. [32] State and federal legislators that interviewed in 2008 and 2009 universally agreed that electronic voting had ended fraud in the tabulation process. News accounts and interviews suggest that some incumbents who previously used fraud substituted in targeted intimidation against opposition party activists after electronic voting was adopted. For example, the campaign of the incumbent governor in the state of Roraima spread rumors that the new electronic voting machines would record each individual's vote, take a pic-

[29] The President is fairly constrained in who he can pick. The supreme court presents a list of six judges to the President and he selects two judges from this list to serve a maximum of four years.

[30] Federal government welfare benefits are distributed through the banking system, so even low income voters were familiar with ATM-style interfaces.

[31] *Folha de São Paulo*, October 2, 1998.

[32] After the polls close, poll workers immediately print out the results from each machine. Representatives of the parties are asked to inspect the results, sign a document indicating their approval of the results, and then they are given a copy of the results. Finally, the results for each machine are posted publicly at the polling station. After the parties indicate their approval, the results are sent to the capital of the state to be tabulated.

ture of the voter, and could even give an electronic shock to the voter.[33] Furthermore, opposition party activists in Bahia argued that the incumbent government had increased harassment of their supporters to compensate for their inability to use fraud.[34] Even without a proper paper trail[35], voters and the political class generally trust the electronic voting machines and many view Brazil's early adoption of the technology with pride. A 2008 poll, albeit one commissioned by the election authorities, showed that 90 percent of voters had a positive opinion of electronic voting.[36] State and federal legislators interviewed in 2008 and 2009 universally agreed that electronic voting had ended fraud in the tabulation process. While critics of the system do exist (e.g. Graaf and Custódio (2002)), by and large the media and elected officials do not question its accuracy.

Consequences of Electronic Voting in Brazil

Research Design and Data

The key feature of my research design for the Brazil case is that electronic voting was only adopted in municipalities—equivalent to a US county—with an electorate over 40,500, as measured two years before the 1998 legislative elections.[37]

Formally, municipality i has a 1996 electorate E_i, which is the "forcing" variable that determines treatment status as follows:

$$T_i = \begin{cases} 1 & \text{if } E_j \geq 40500 \\ 0 & \text{if } E_j < 40500 \end{cases}$$

I wish to estimate the quantity $\tau = \mathbb{E}[Y_i(1) - Y_i(0)]$, where $Y_i(1)$ and $Y_i(0)$ denotes the outcome of interest (e.g. share of null and blank votes) for municipality i with electronic voting machines and paper ballots, respectively. This estimand is unidentified without further assumptions since we only observe $Y_i(1) \mid T_i = 1$ and $Y_i(0) \mid T_i = 0$, but not $Y_i(1) \mid T_i = 0$ and $Y_i(0) \mid T_i = 1$.

The crucial assumption for my study is that the counterfactual outcomes of what would have happened under each treatment condition do not jump abruptly at the discontinuity point. In other words, any abrupt changes in observed outcomes cannot be

[33] See "Terror Electrônico", *Agencia Globo*, Sept. 28, 1998.

[34] Interview with Josias Gomes da Silva, PT official in Bahia.

[35] Models with a paper trail were tested in the 2002 election, but they were abandoned due to logistical difficulties.

[36] "97% dos eleitores aprovam a urna eletrônica, diz pesquisa do TSE", *G1*, January 15, 2009.

[37] The exception to this rule are the states of Rio de Janeiro, Alagoas, Roraima, and Amapa where all municipalities used the new technology. The municipalities in these states comprise 4 percent of the total number of municipalities in Brazil. The rationale for covering all municipalities in Rio de Janeiro and Alagoas was the perception that voting fraud was particularly acute in these states. All municipalities in Roraima and Amapa were covered because it allowed the TSE to test the implementation of the new technology in geographically farflung municipalities. In all analyses presented below, municipalities from these states are dropped.

attributed to any factor other than treatment. Under this smoothness assumption about $Y_i(1)$ and $Y_i(0)$, one can identify a local causal effect at $E_i = 40500$ since on either side of the threshold (with a minimum amount of extrapolation), the outcomes of polling stations in municipalities with electronic voting are valid counterfactuals for the polling stations in municipalities with paper ballots (Imbens and Lemieux 2008, 619). Thus, in this analysis I focus on the following quantity:

$$\tau = \mathbb{E}[Y_i(1) \mid E_i = 40500] - \mathbb{E}[Y_i(0) \mid E_i = 40500]$$

This estimand is a "local" average treatment effect (LATE), since it only represents the effect among voters in municipalities with a specific population. In this study, two categories of outcomes are of central interest: those relating the proportion of voters who are enfranchised by the treatment and those relating to how electoral outcomes are affected by the change in technology. For the former, the outcome variable is defined as some measure of vote validity (number of blank votes, null votes, or valid votes) divided by the total number of voters who turned out. I label these local average treatment effects as τ_D. The other category of treatment effect is for outcome variables that are some measure of voters preferences (total number of votes cast for a candidate or party) divided by the total number of valid votes cast. These treatment effects are labeled as τ_C.

Specification and Inference To calculate local average treatment effects, I use two main specifications. The first specification estimates the conditional expectation function on each side of the discontinuity using the following local linear regression:

$$\min \sum_{i=1}^{N_i} 1\{-h \geq E_i \leq h\} \cdot (Y_i - \alpha - \beta \cdot (E_i - 40500) - \tau_{RD} \cdot T_i - \gamma \cdot (E_i - 40500) \cdot T_i)^2$$

τ_{RD} is our parameter of interest. The variable h is the bandwidth, which specifies how much data in a window around $E_i = 40500$ is retained for estimating τ_{RD}. In this application, h is set to be 15,000.[38] In addition to the local linear regression, I also estimate τ_{RD} by calculating the simple difference-in means using data from a narrow window ("discontinuity sample") around the 40,500 threshold. I use a bandwidth of 5,000 voters, leaving municipalities with a 1996 electorate greater than 35,500 and lower than 45,500, for a total of 114 municipalities.

[38] An alternative is to use automatic bandwidth selection algorithms such as the cross-validation procedure suggested by Ludwig and Miller (2007) and the algorithm proposed by Imbens and Karthik (2009). I tried using both approaches on our data, but in each case, they recommended implausibly large bandwidths that would have involved keeping almost all the data when estimating treatment effects. Given the bias that could be introduced when using data far from the threshold, I chose to use covariate balance as the main criterion for bandwidth selection.

Data Electoral data was obtained from the Supreme Electoral Tribunal at the polling station level and aggregated to the municipal level. Covariates data was obtained from the Institute of Applied Economic Research ("Instituto de Pesquisa Econômica Aplicada" or IPEA).

Balance While the identification assumption that the mean of the outcome on one side of the threshold is a valid counterfactual for the mean outcome on the other side of the discontinuity is fundamentally unverifiable, one can test whether or not implications of the identifying assumption are borne out in the data. Specifically, I check if pre-treatment covariates are *balanced*, i.e. statistically similar across the municipalities adopting electronic voting 1998 and those not. Using the two specifications discussed above—discontinuity sample difference-in-means and a local linear regression—I estimate the difference around the discontinuity in the conditional expectation of 18 covariates at E_j=40,500. These covariates are measured at the municipal level and include a range of political variables and census socio-demographic characteristics. The full list of covariates and the associated balance statistics are presented in figure 1.5.

As expected, many of the covariates are severely imbalanced in the full sample. Variables such as 1991 rates of illiteracy and average income are quite imbalanced, which is unsurprising: control municipalities are substantially smaller than treatment municipalities, and population is negatively correlated with development outcomes. Once municipalities far from the threshold are removed, however, balance improves dramatically. For the local linear estimates, only 1 out of the 18 covariates have a p-value of less than .05, compared to 16 out of 18 in the full sample. Balance for the difference-in-means specification is slightly worse, with 3 out of the 18 covariates showing statistically significant imbalances. Using mean differences scaled by the standard deviation ("standardized difference") as a balance statistics shows a similar pattern of improvement in covariate balance. Overall, these balance statistics show no indications of problems with the research design.

1.3 Political Consequences of Technological Disenfranchisement

Does Electronic Voting Expand the Electorate?

The large effect of the switch in voting technology on the percentage of null and blank votes is clearly evident in Figure 1.6. The distributions of the percent of null and blank on each side of the discontinuity only barely overlap. The thick black vertical lines represent the conditional mean of null and blank votes at each value of the forcing variable estimated using a nonparametric loess regression. Note that the conditional mean is relatively flat on both sides of the discontinuity, except among municipalities with less than a 1996 electorate of about 10,000. The stability of the conditional expectation over

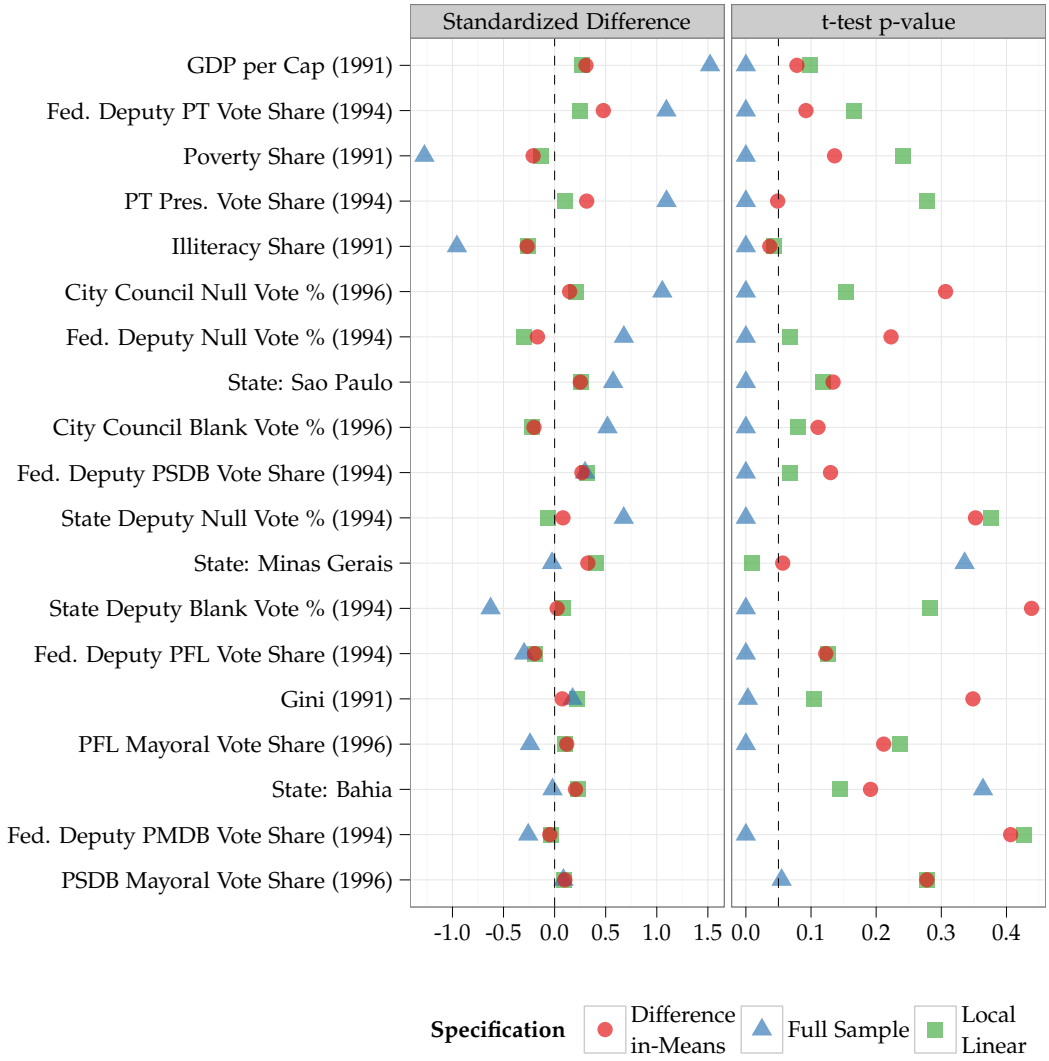


Figure 1.5: Balance under three different specifications. This plot displays balance statistics for three different estimation procedures. "Full Sample" estimates are from the difference-in-means test using the full sample. "Discontinuity Sample" estimates are from difference-in-means test using only municipalities within 5,000 of the 40,500 electorate discontinuity. "Local linear regression" estimates are from a local linear regression with a discontinuity window of 15,000. The left panel shows standardized differences, which is the estimate, divided by the standard deviation in the full sample. The right panel shows p-values from a t-test of equality of means.

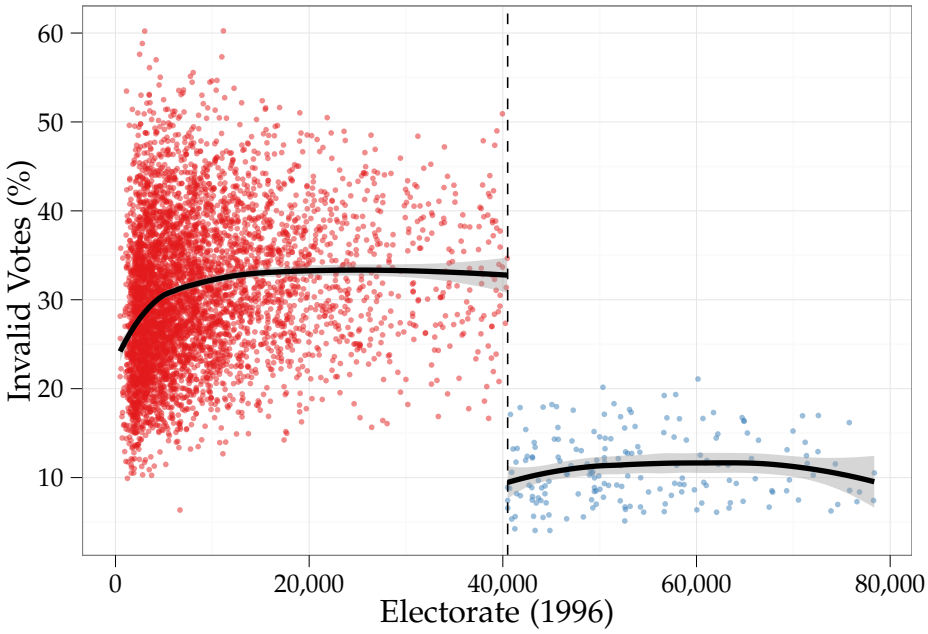


Figure 1.6: The effect of electronic voting on the percent of null and blank votes. Each dot is a polling station. Polling stations to the left of the vertical black line used paper ballots and polling stations to the right used electronic voting. The black horizontal line is the conditional mean of the outcome estimated with a loess regression.

such a large range of the data suggests that the treatment effect at $E_j = 40500$ may apply to municipalities far from the threshold.

Formal treatment effect estimates on federal deputy null and blank votes —separately and together—are reported in the left panel of figure 1.7. Focusing on the local linear regression estimates, the effect of the shift in voting technology lowered null vote rates by an estimated 13.5 percentage points, blank votes by an estimated 10 percentage points, thus increasing the number of votes affecting political outcomes by about 23.4 percentage points. This number amounts to about a 30 percent increase in the size of the electorate casting valid votes. While null votes were somewhat more affected than blank votes, the similarity between the two estimates is surprising. A blank vote, in the Brazilian system, is supposed to be an affirmative choice intended by the voter. A null vote, on the other hand, is a residual category (an "undervote", to use the American parlance) for when the voter fails to register any preference at all. Thus, one might expect that electronic voting would affect null votes much more than blank votes, but these estimates belie that expectation. These estimates suggest that a large percent of blank votes were actually mistakenly cast or counted. For comparison, treatment effect estimates on invalid votes for all other offices are reported in the right panel of figure 1.7. Electronic voting lowers invalid vote rates for all other offices, though estimates are smaller in magni-

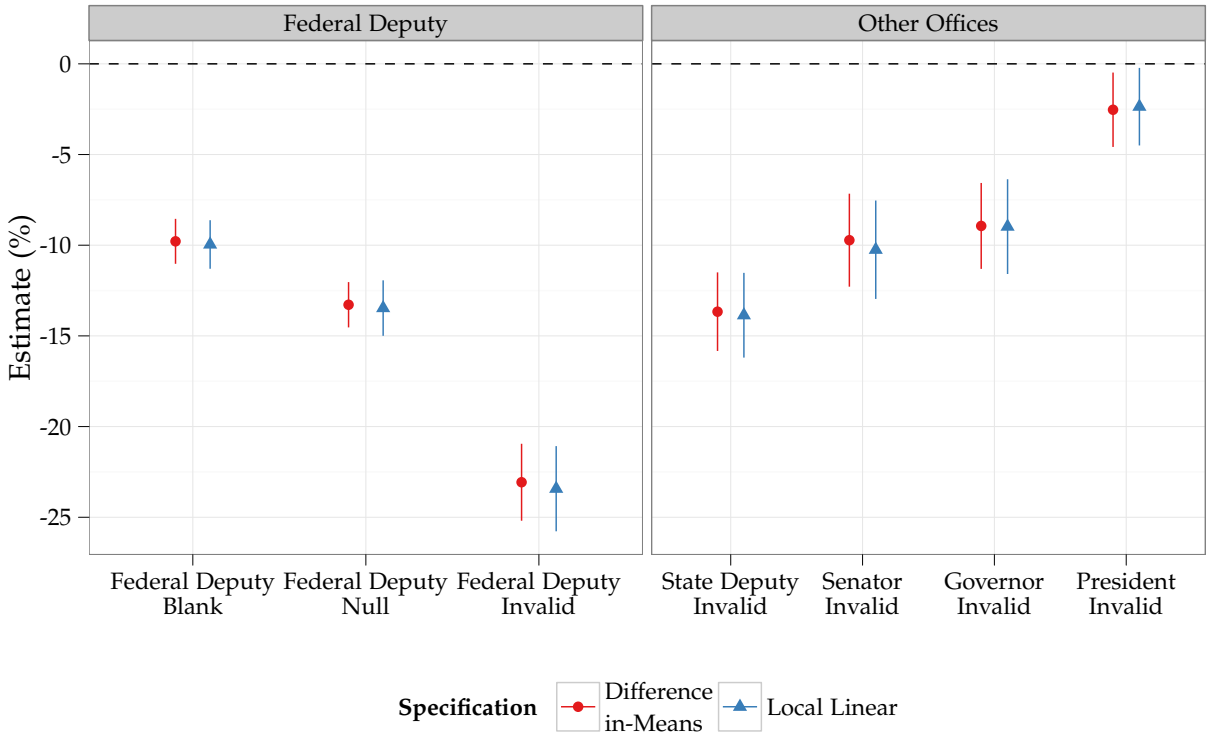


Figure 1.7: The left plot shows the estimated local average treatment effects of electronic voting on blank, null, and invalid (sum of blank and null votes) votes for federal deputy candidates as a percentage of total votes cast. The right plot shows the effect on percent invalid votes (sum of null and blank votes) for state deputy, senatorial, gubernatorial, and presidential candidates. Lines are bootstrapped 95 percent confidence intervals that take into account clustering.

tude. Invalid votes for state deputies, whom are elected using the same electoral rules as federal deputies, lower by about 13.9 percentage points, almost ten points smaller in magnitude than the federal deputy equivalent. Presidents, governors, and senators are elected using majoritarian formulas, resulting in fewer candidates and a less complex political environment. Consistent with the "information effect" mechanism, the results for these offices feature smaller reductions in invalid votes. Furthermore, the ranking of the estimate sizes are consistent with the idea that the information effect is highest for the least visible office. Senate invalid votes decrease by 10.2 percentage points, governor invalid votes decrease by 9 percentage points, president invalid votes decrease by 2.4 percentage points. The heterogeneity in these effects is substantial: the effect estimate for federal deputies is about twelve times the size of the estimate for president invalid votes.

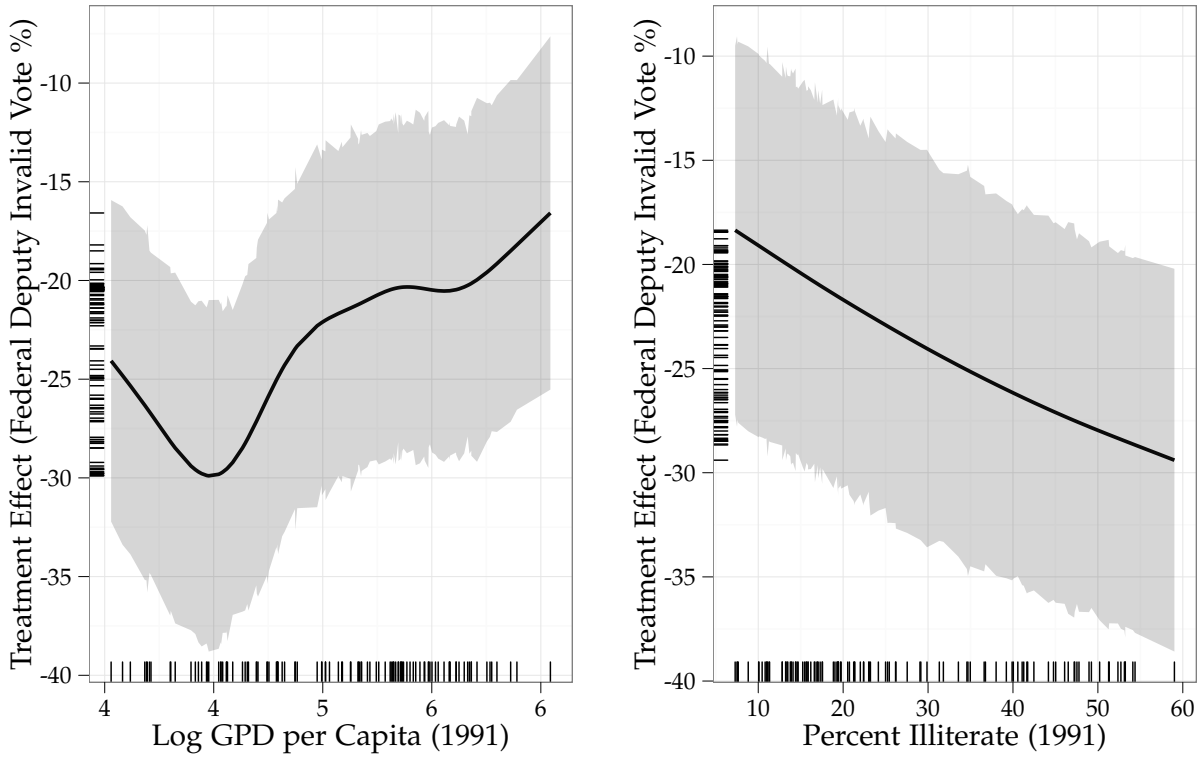


Figure 1.8: Heterogeneous treatment effects. Plots show how the effect of electronic voting on invalid votes for federal deputy varies by income per capita (left panel) and illiteracy (right panel). Solid line represents estimates from loess regression on the interaction between treatment and the covariate within a window of 5000 around $E_j = 40500$. Dashed lines are 95 percent confidence intervals. Dashed lines on margins show marginal distribution of covariate and estimated treatment effects. The treatment effect is larger (more negative) in municipalities with lower GDP per capita and higher illiteracy rates.

While the estimated average local average treatment effect is large, I hypothesize that the information effect would be even greater among poorer and less educated voters. While individual-level data is unavailable to test this hypothesis directly, it is possible to examine how the treatment effect varies by municipalities' sociodemographic characteristics. To that end, I used local weighted polynomial ("loess") regression to estimate how treatment effects vary on monthly income per capita and percent illiterate, as reported by the 1990 census. Specifically, I estimated the relationship between the covariate and the outcome separately in the 5,000 population discontinuity window around 40,500, then computed the difference in the predicted values of the two loess regressions.

The differences in estimated effects, presented in figure 1.8, fit expectations. The

treatment effect of electronic voting on invalid votes monotonically decreases in magnitude with income. For illiteracy, the heterogeneity is not strictly monotonic, but increases in magnitude with percent illiterate for most of the range of the data. The heterogeneity, particularly on illiteracy, is substantial. In the municipalities with the highest rates of illiteracy, treatment effect estimates are about -27 percentage points, while in the most literate municipalities, estimates are around -17 percentage points. In the wealthiest municipalities, τ_D is estimated to be just under -15 percentage points, considerably lower in magnitude than the average estimate. While not dispositive given the problems of making inferences about individual behavior using aggregate data, these heterogenous results do suggest that it was lower income and lower educated voters who were disproportionately affected by the paper ballot system. It is also worth noting that this discontinuity sample excludes both the poorest and wealthiest municipalities in Brazil, since wealth is heavily correlated with population. Thus, the heterogeneity in the treatment effects for the whole population may be even greater.

Partisan and Ideological Effects

The introduction of electronic voting expanded the percent of voters casting valid votes by about a third, but how did it affect the balance of power in the national legislature? In this section, I examine the consequences of the shift in technology on votes received by Brazil's major parties. Brazil has a highly fragmented legislature, with 18 parties or an effective number of 7.1 in 1998. Rather than examine the effect on all 18 parties, I focus on the five parties with the highest number of seats in the lower house of legislature in 1998, representing 79 percent of the total seats: the PFL (*Partido de Frente Liberal* or Liberal Front Party, 105 seats), the PSDB (*Partido da Social Democracia Brasileira* or the Brazilian Social Democratic Brazil, 99 seats), the PMDB (*Partido do Movimento Democrático Brasileiro* or the Brazilian Democratic Movement Party, 83 seats), the PPB (*Partido Progressista Brasileiro* or the Brazilian Progressive Party, 60 seats), and the PT (*Partido dos Trabalhadores* or the Worker's Party, 58 seats). During this period, the PSDB lead a majority coalition, which included the PFL, the PPB, and the PMDB. The PT was in opposition.

How do these parties compare to each other? A simple way of differentiating the five major parties is by their ideology and their internal homogeneity. Figure 1.9 shows the distribution of ideal points estimated from roll call votes cast during the 1994-1998 legislature of the five major parties.[39] The vertical black line is the legislative median.

[39]Ideal points were estimated using the Bayesian simulation ideal-point estimation technique developed by Clinton, Jackman, and Rivers (2004) as implemented by in the R package PSCL. Ideal point estimates are valid under a spatial model of legislative decision-making where legislators with euclidean "quadratic loss" utility functions make voting decisions based on bills arrayed in one dimensional policy space. This statistical approach uses a Markov Chain Monte Carlo (MCMC) procedure that jointly estimates bill locations on the policy space and legislators ideal points. For local identification, ideal points are constrained to have mean 0 a standard deviation of 1.

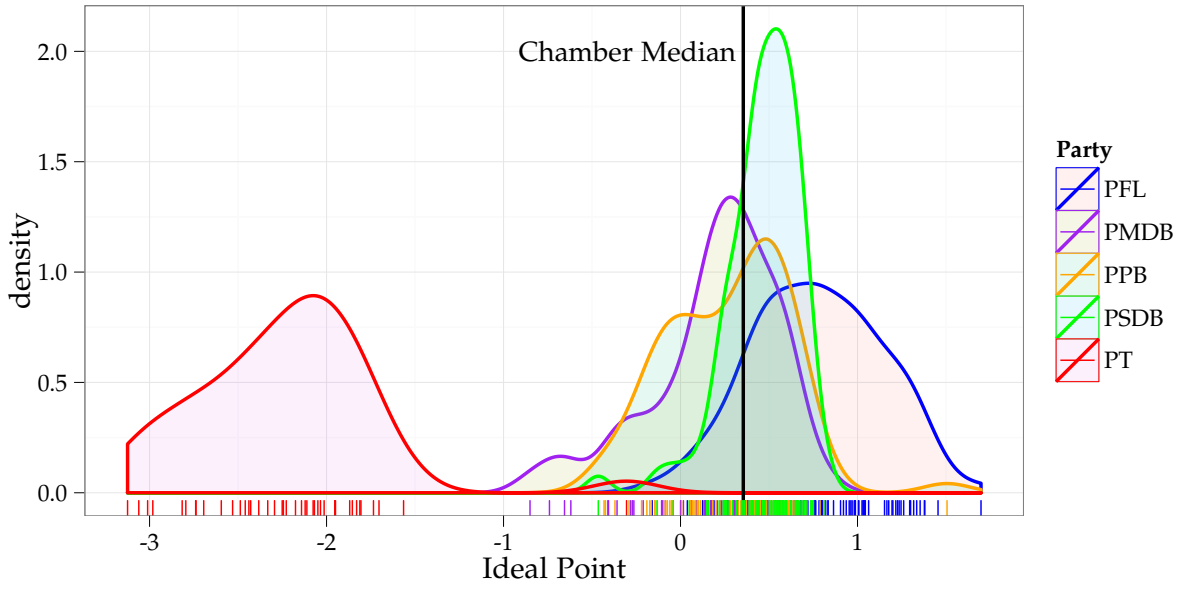


Figure 1.9: The distribution of ideal points for federal deputies of the five largest parties in the 1994-1998 legislature. Black vertical line is the chamber median. Tick marks on the margin show the marginal distribution of the data.

According to these ideal point estimates, the five parties can be ordered by their medians from left to right as follows: PT, PMDB, PPB, PSDB, and PFL.[40] Notably, the distribution for PT legislators shows almost no overlap with the ideal points of the four other parties. For the four parties in the majority coalition, there is clear distinction in the variance of the distributions between the PSDB on the one hand, and the PMDB, PPB, and the PFL on the other. The PSDB is considerably more clustered around its median while the other three parties are more ideologically diffuse. According to the ideal point estimates, the variance of ideal points among legislators from the PFL, the PMDB, and PPB is about three times the variance among PSDB legislators.[41]

These differences across parties have implications for interpreting the consequences of electronic voting. These ideal point estimates suggest that all party labels are not equally informative. For example, the PPB and PMDB party label is a relatively poor predictor of legislative behavior. Thus, if electronic voting were to hurt or help one of these parties in the aggregate, it would be difficult to predict with any precision how the chamber median would shift in future legislatures. Any positive or negative effects

[40]This ranking is broadly consistent with Power and Zucco Jr (2009, 228)'s survey based ranking, although they place the PPB to the right of the PFL.

[41]This characterization of the parties is supported by the literature on Brazilian parties. For example Power and Zucco Jr (2009, 240) describe the PMDB as "centrist, catchall, decentralized, [with] many feuding owners."

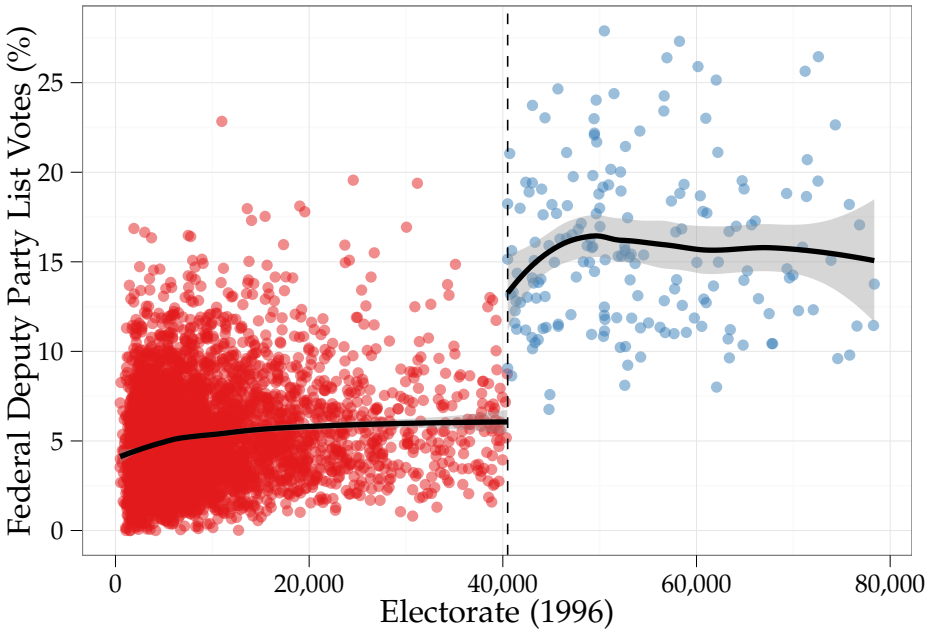


Figure 1.10: The effect of electronic voting on the percent of party list votes cast. Each point is the share of valid votes cast using the party list option, as opposed to the individual candidate option.. Municipalities to the right of the vertical black line used paper ballots and municipalities to the left used electronic voting. The black horizontal line is the conditional mean of the outcome estimated with a loess regression.

on the PT, the PSDB, and PFL would be more meaningful. PT gains due to electronic voting would clearly move policy making to the left, while PSDB and PFL gains would likely move the median legislator somewhat to the right. Furthermore, these ideal point estimates suggest that the party brands of the PT, the PSDB, and the PFL are more informative for voters. Particularly for voters with low levels of political information, precisely those most affected by voting technology, casting a ballot for a party with a more cohesive brand has more predictable consequences on policy making than for one without.

Before discussing the kinds of candidates for legislative office that newly enfranchised voters chose, it is worth studying the effect of electronic voting on "party list" votes because of the light this might shed on how these new voters respond to Brazil's complex electoral environment. As discussed earlier, voters have the option for casting a ballot for the party list as opposed to an individual candidate. This option is sometimes ignored in the literature on Brazilian voting behavior (though see D. Samuels (1999)), because of the consensus that partisanship is weak and voter mobilization is largely per-

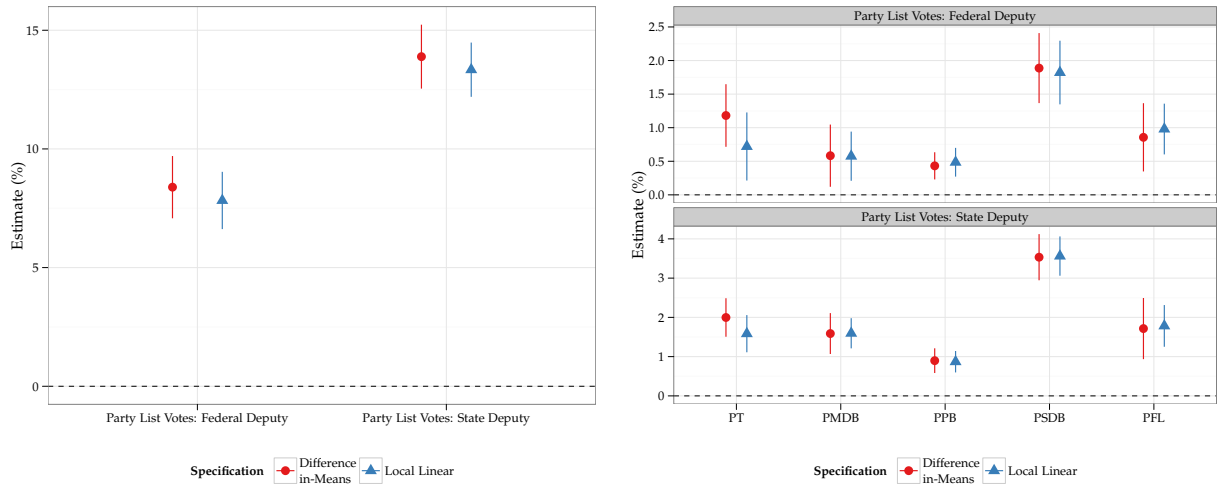


Figure 1.11: The left plot shows the estimated local average treatment effects of electronic voting on party list votes for federal deputy of the five largest parties in Brazil, as well as the total number of party list votes. The right side shows the estimated δ, the estimated difference in vote shares between newly enfranchised and already enfranchised, for each variable. Lines are bootstrapped 95 percent confidence intervals that take into account clustering.

sonalistic.[42] One might think that the type of voter most affected by the complexity of the paper ballot—illiterate and uninformed—would be less likely to have partisan leanings and thus less likely to use the party vote option. Indeed, the consensus in the literature is that Brazilian partisans are more likely to come from the middle class, at least in the late 1990s (Samuels and Zucco 2010, 16). But a countervailing hypothesis, one informed by the literature that conceives of party labels as information shortcuts, would predict just the opposite. This literature suggests that the least informed would be the precisely the type of voters most likely to rely on the party cue since they are unlikely to have formed an opinion about individual candidates.

Figure 1.10 displays strong evidence that electronic voting sharply increased the percentage of votes cast for party lists. This figure displays the total number of federal deputy party list votes cast for any party as a percentage of valid votes cast in each municipality in the sample. Notably, the distributions on each side of the discontinuity show little overlap. Point estimates and their associated 95 percent confidence intervals are presented in the top panels of figure 1.11. Formal estimates for both federal deputy and state deputy list votes are presented in the left side of figure 1.11. The late estimate τ_c for the federal deputy vote using the local linear specification is about 7.8 percentage

[42]Under Brazil's electoral rules, party list votes affect the distribution of seats between parties or coalitions, but they do not affect the intra-party or intra-coalition allocation of seats.

points (standard error of 0.6). The effect on state deputy party list votes is a substantially higher 13.3 percentage points (standard error of 0.6). These point estimate suggest that the rate at which the previously disenfranchised voters use the party list option is substantially higher than those who could vote using the old paper ballot system.

One probably should not interpret this finding as revealing that the Brazilian poor are in fact partisans, a finding that would conflict with the existing literature (Mainwaring 1999; D. Samuels 2006). A more likely mechanism is that since excluded voters are disproportionately uninformed and less politicized voters, the party vote acts as a device that relieves them of the need to choose one out of hundreds of candidates to vote for. Instead, these uninformed voters can decide among one of about a dozen party labels, a considerably simpler task.

Which parties benefited most from this jump in party list votes? In figure 1.11, on the right side estimated treatment effect (τ_C) on the federal and state deputy party list votes of the five largest parties in Brazil are displayed. Notably, τ_c for all five parties are positive and statistically significant for both offices. Using the difference-in-means estimates, ranking the federal deputy estimates by magnitude give the following order: PSDB ($\hat{\tau}_c = 1.9$), PT ($\hat{\tau}_c = 1.2$), PFL ($\hat{\tau}_c = 0.9$), PMDB ($\hat{\tau}_c = 0.6$), and PPB ($\hat{\tau}_c = 0.4$). The rank ordering for state deputy list votes is precisely the same as those for federal deputy list votes. The ordering of these effects correlates perfectly with the distinctiveness of each party label's brand. As discussed earlier, the PMDB and the PPB are the most ideologically incoherent major parties and their federal deputy party list point estimates are about half of those found for the more distinctive PT and PFL. Presumably, this incoherence makes their labels less useful for the information-scarce voters enfranchised by electronic voting and as a result, they benefit the least from the influx of new voters. The ruling party, the PSDB, gained the most. It is likely that the PSDB benefitted from the concurrent presidential campaign, where President Henrique Cardoso of the PSDB was handily re-elected with 53 percent of the vote in the first round. Voters who approved of President Cardoso could easily reward his party's legislative co-partisans by casting a party vote without the need to develop an opinion about a particular candidate.

While the party list estimates are of theoretical interest given what they reveal about the informational value of a party label, the aggregate impact of electronic voting must take into account the more common candidate vote. To do so, I estimated the effect of the change in voting technology on the candidate vote for the five major parties as a share of (non-partylist) valid votes. These estimates are shown in Figure 1.12. Unlike the party list estimates, these effects are less precisely estimated and most are statistically indistinguishable from 0 in at least one of the specifications. Among federal deputy candidates, the estimates suggest that the PT ($\hat{\tau}_c = 1.3$) and the PPB ($\hat{\tau}_c = 0.5$) most benefitted, though these estimates are rather imprecise. PT state deputy candidates, in contrast to state deputy candidates from other parties, clearly benefitted from the adoption of electronic voting as their aggregate vote share increased by an estimated (local linear specification) 8.5 percentage points (standard error of 8.5). In contrast to their federal deputy counterparts, PPB state deputy candidates lost votes as a result of

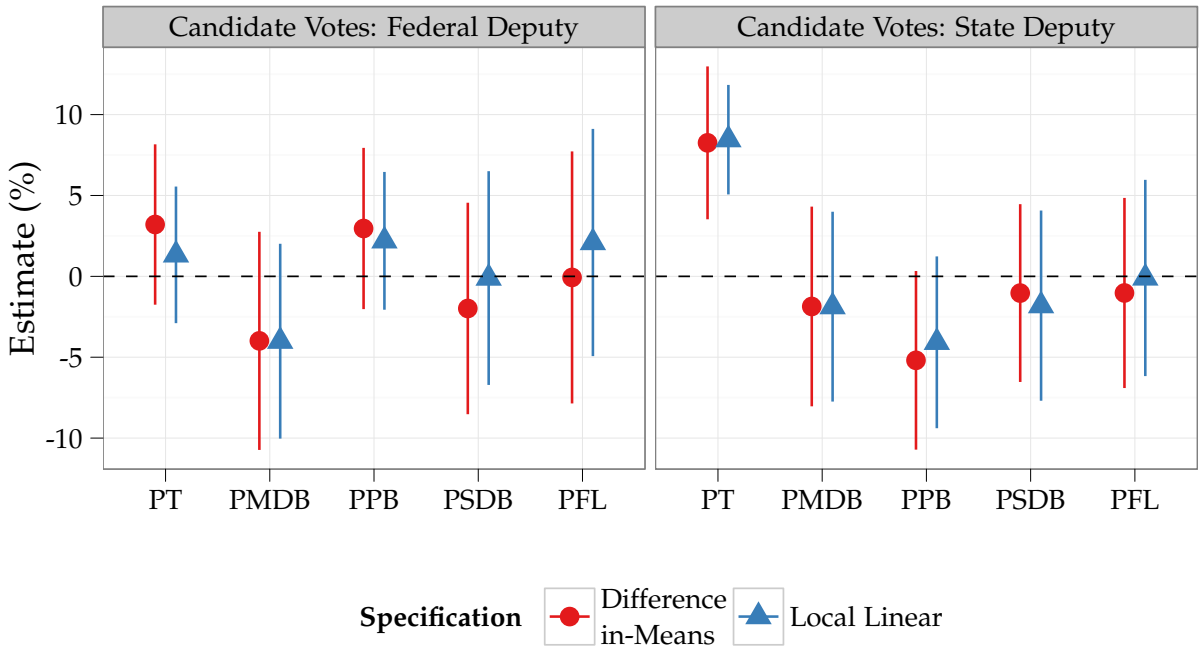


Figure 1.12: The left plot shows the estimated local average treatment effects of electronic voting on candidate votes for federal deputy of the five largest parties in Brazil. The right plot shows esimates for state deputy candidates. Parties are ordered by estimated median ideal point.

electronic voting ($\hat{\tau}_c$ = -4.1, standard error of 2.7).

Which Candidates Benefit?

I estimated the local average treatment effect in each ideological bin separately and the estimates are presented in figure 1.13. Among left, center-left, and center candidates, there is no statistically detectable effect. On the right side of the ideological spectrum, however, a contrasting pattern emerges wherein center-right candidates are hurt and right-wing candidates benefit. This pattern of treatment effects suggests that—on aggregate—those enfranchised by the new voting technology are somewhat more right-leaning than those who were previously enfranchised. When states with dominant party machines (discussed below) are excluded, this pattern is even more pronounced.

Discussion

The consequences of expanding the effective size of the electorate in legislative elections by about 34 percent are not easily characterized in partisan or ideological terms. Most

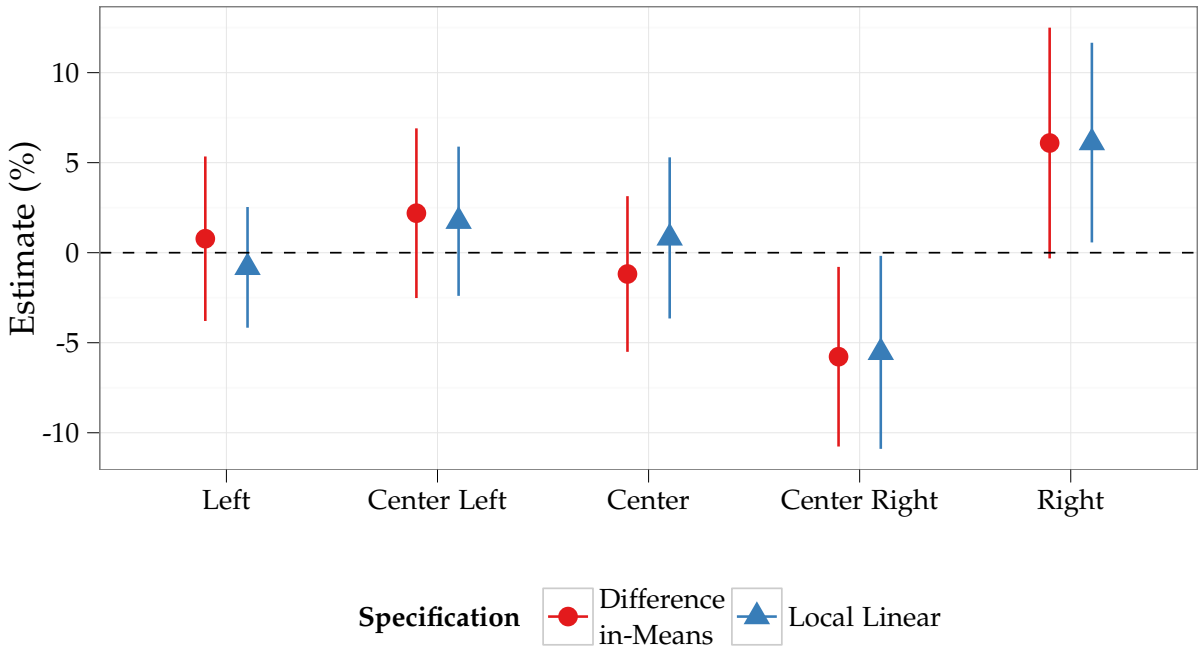


Figure 1.13: Local average treatment effects of electronic voting on the vote share of five categories of incumbent federal deputy candidates. The bins were formed using ideal points estimated from roll call votes cast from 1995-1998.

parties' candidates, on average, were unaffected. Ideologically, it appears that newly enfranchised voters were somewhat more right-wing than the already enfranchised as center-right federal deputy candidates lost ground to right wing federal deputy candidates. At the very least, these findings contradict the prediction of simple "democratic class struggle" models of electoral politics wherein left-wing ideology is negatively correlated with socio-economic status. As the effective electorate became comparatively poorer due to electronic voting, the vote shares of the most right-wing federal deputy candidates increased and the vote share of left-wing candidates remained unchanged. With the exception of the moderate right-wing shift, however, the candidate vote—when examined in aggregate across the entire country—was not significantly reshaped by the sharp increase in voters casting valid ballots.

Because of its implications for the party system, perhaps the most important change induced by electronic voting was in the type of ballots cast, rather than in the partisan or ideological cast of the candidates elected. Municipalities with electronic voting experienced an 8 percentage point increase in the share of party list ballots cast, which amounts to a 160 percent increase over baseline. While candidate votes are still far more numerous than party list votes, this shift may represent an increase in the value of the party

label relative to the value of a candidate's personal reputation. Furthermore, these party list ballots were disproportionately cast for the legislature's more ideologically cohesive parties like the PT and the PSDB, as opposed to the more diffuse catch-all parties like the PMDB and the PPB. Indeed, the reshaping of the Brazilian electorate induced by the electronic ballot may have functioned as an exogenous shock to the value of being a member of one of Brazil's governing parties. If this is the case, as these estimates indicate, then the expansion of the electorate may be one of the heretofore unnoticed contributors to Brazil's trend towards programmatic governance documented by Hagopian, Gervasoni, and Moraes (2009).

1.4 (Electronic) Machines versus (Political) Machines

The expansion of the electorate by about one third did not, on the aggregate, have a large effect on the partisan distribution of power in the national legislature. Yet as discussed above, an additional motivation for the adoption of the new technology was to combat tabulation fraud. While officials claimed that low level fraud was pervasive throughout the country, it was in states with dominant political machines that distortion of votes was considered to be most consequential. Did removal of the ability to directly alter vote totals affect the political equilibrium in these states?

In this section, I show that in three states with dominant political machines—Bahia, Maranhão, and Paraíba—electronic voting had an enormous effect on the incumbent coalition's vote share. In contrast, in a comparison group of states from the same region with more competitive party systems, the new technology had no effect on the incumbent coalitions' ability to stay in office. Indeed, in two of the political machine states, ruling parties faced their first serious opposition challenge in the elections following full adoption of electronic voting.

Competitive and Non-Competitive Party Systems In Brazil, national partisan competition is strongly structured by state-level politics, with the degree of competition varying widely across states (Borges 2010). Many states' party systems are quite competitive and in these states, different governing coalitions regularly alternate in and out of power. On the other end of the spectrum are states where—at least through the early 2000s—a single ruling party controlled both the governorship and the state legislature over many elections. These states are easily identified: out of the 27 states, five between 1990 and 2006 had one or fewer transfers of power between governors of different parties. Notably, all of partisan turnovers occurred after 1998, when electronic voting was introduced to most municipalities. Out of these five, four of the ruling parties had a high share of seats in the state legislature and all of them were in the socioeconomically backward Northeast. The four states with strong indications of being governed by a hegemonic

ruling group or "political machine", are Bahia, Ceará, Maranhão, and Paraíba.[43]

Out of the four states with the fewest partisan turnovers and high levels of legislative dominance, Bahia, Maranhão, and Paraíba were governed in the 1990s by conservative political machines.[44] Bahian state and municipal governments were dominated by the political machine of Antonio Carlos Magalhães (ACM), a three term governor that rose to prominence during the military dictatorship and successfully enhanced his power after the transition to democracy via his position as minister of communications in the federal government (C. M. d. Souza 1997). A perhaps even more dominant electoral group than Magalhães's in Bahia was José Sarney's in Maranhão, whose machine, with one brief exception, has governed the state since 1965 (Costa 2006). In addition to retaining his grip on Maranhão politics, Sarney has maintained a high profile position in national politics, serving as president between 1985 and 1990 and president of the senate since 2009. In Paraíba, state politics are dominated by the group led by Ronaldo José da Cunha Lima, a PMDB (now PSDB) politician and governor between 1991 and 1994. After his term, Cunha Lima served in the senate and was succeeded by close allies in the governor's seat, including his son in 2006.

The dominant coalitions in Bahia, Maranhão, and Paraíba are typically classified as right-wing and this characterization is borne out in the voting patterns of the machines' congressional delegations. One of way showing this is to rank all federal deputies left to right from -219 to to 219 using the ideal points estimated above, with 0 signifying the median legislator. The median machine candidate from those three states occupies position 152, far to the right of the chamber median. This right-leaning tendency persists even when examining intra-party differences. For example, the median rank of PFL legislators not coming from machine states is 150, while PFL federal deputies belonging to the machine's coalitions have a median rank of 189. While all three delegations are right of the chamber median, Bahian machine legislators are the most extreme, with a

[43]The median number of transfers of power between governors of different parties in all states was 3, out of a total possible of 5. The mean percentage of the state assembly controlled by the party of the governor for all states was 18, while the mean among the four machine states was 27.3 percent. While this percentage of seats controlled by the governor's party may seem low compared to those found in two party systems, in practice, governing parties are typically part of large multi-party coalitions. Epstein (2009), in a study of state-level party systems, categorizes all four states as "hegemonic" which he defines as a system wherein "one party wins the lion's share of the seats in every election, including majoritarian polls, eliminating true electoral choice for rational voters through unfair, if not illegal means" (343). The one other state that Epstein categorizes as hegemonic is Pernambuco, but that state had 3 episodes partisan turnover in gubernatorial elections, as well as about an average share of state legislative seats controlled by the governor's party.

[44]The fourth case–Ceará—has a ruling group based in the PSDB that is ideologically distinct from the machines in the other three states. Unlike in Bahia, Maranhão, and Paraíba, a modernizing group lead by businessman Tasso Jereissati defeated the oligarchic machines that ruled during the military dictatorship and succeeded in completely marginalizing the previous ruling families (Ames 2002, pgs. 126-127). The PSDB governed the state through 2006 and modernized governance of the state in a progressive direction, as discussed by Tendler (1997). Ceará is omitted from the following analyses given its ruling party's ideological distinctiveness, but the overall conclusions are robust to its inclusion.

median ranking of 194. Given machine deputies' ideological positions, any loss in the ability of machines to elect candidates would most certainly move the chamber median to the left.

I compare the consequences of electronic voting in the three machine states to competitive non-machine states in the same region: Piauí, Pernambuco, Rio Grande do Norte, and Sergipe. Unlike Bahia, Maranhão, and Paraiba, these four states had competitive elections with incumbent coalitions facing real chance of defeat and furthermore, the governor's party controlled an average or below average share of the seats in the state legislature.[45] These states had no hegemonic political group controlling the state apparatus for extended periods of time and left-of-center and right-of-center political coalitions alternated in and out of power. The incumbent governments were basically centrist: the median legislator from these states' incumbent coalitions was 63 (with 0 being the chamber median), which was far to the left of median incumbent legislator from political machine states.

Evidence of Fraud There is substantial circumstantial evidence that these machines used fraud to help maintain their dominant position. While direct indications of fraud are unavailable, two main pieces of evidence suggest that manipulation of the vote count was particularly acute in these states, especially in Bahia and Maranhão. One indicator is legal cases involving fraud in the vote count process, which were adjudicated at substantially higher rates than the national average in the state electoral tribunals of Bahia and Maranhão. Between 1980 and 1998, 603 cases involving accusations of fraud were heard in Maranhão's regional electoral tribunal, about six times the national average rate of 108 cases (median of 58).[46] In Bahia, the number of fraud cases were comparatively fewer at 265, though still over two times the national average.[47]

A second piece of evidence indicative of extensive fraud in Bahia, Maranhão, and Paraíba is the existence of patterns present in aggregate electoral results consistent with qualitative accounts of how fraud operates. As discussed above, political machine candidates would typically take advantage of huge numbers of invalid and blank votes arising from the difficulty of voting with paper ballots, and transfer those votes to themselves. The implication of these accounts is that in municipalities using paper ballots, a negative correlation should exist between the share of the valid vote received by the incumbent candidates and the number invalid votes across polling stations. The logic of these contrasting patterns being that candidates with access to state machinery would be able to

[45]Between 1990 and 2006, Piauí, Sergipe , and Rio Grande do Norte had 2 transfers of power between governors of different parties. Pernambuco had 3. In 1994, governor's party controlled 16.7 percent, 4.2 percent, 33.3 percent, and 32.7 percent of the state legislature in Minas Gerais, Piauí, Sergipe , Rio Grande do Norte, and Pernambuco, respectively.

[46]These figures were obtained at the TSE website: `http://www.tse.gov.br`. To calculate these figures, I searched in the online database of cases using the keywords of "apuracão" (tabulation) and "fraude" (fraud). Experimenting with other key words gave similar results.

[47]When normalized by state population, both states, particularly Maranhão, are still outliers.

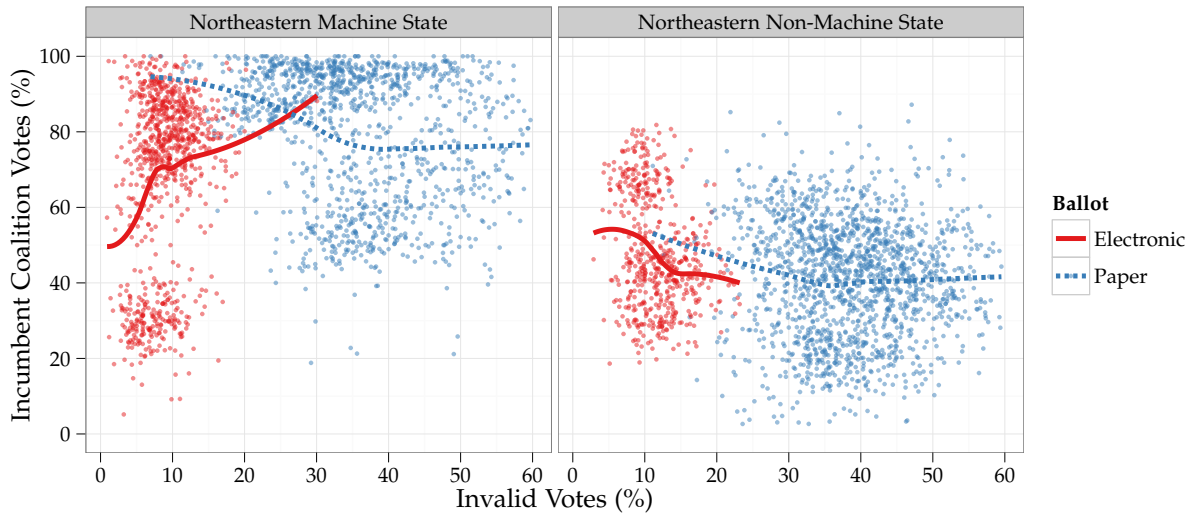


Figure 1.14: The relationship between the vote share of candidates belonging to the incumbent coalition and the share of invalid votes. The right panel presents polling station level data within a small window (bandwidth of 5000) around the discontinuity cut point from Northeastern states with competitive party systems and the right panel shows data from Northeastern states with dominant political machines. The red points are data from municipalities with electronic voting and the blue dots are data from municipalities with paper ballots. The thick lines are smoothed loess regressions estimated separately in electronic and paper ballot municipalities. In competitive states, the correlation between invalid votes and incumbent coalition votes is mostly negative regardless of voting technology. In political machine states, electronic voting causes the relationship to switch from a negative to a positive one.

penetrate the vote counting process in paper ballot municipalities and switch invalid votes to themselves. In polling stations with electronic voting, this correlation should change. Similarly, one would expect that voting technology would not affect the relationship between invalid votes and incumbent vote share in competitive municipalities.

Figure 1.14 shows the bivariate relationship between the share of invalid votes and incumbent coalition candidate vote shares in both competitive and political machine states. The data shown is within a narrow window around the discontinuity point (bandwidth of 5000), so differences between electronic voting and paper ballot municipalities—if my identification assumptions are valid—are causal. The nonparametric loess estimate is separately estimated in paper ballot and electronic voting municipalities. In the three competitive states, the correlation between invalid votes and incumbent coalition vote share is negative, regardless of the voting technology used. In political machine states, the relationship is positive in electronic voting municipalities. The observed relationship, however, is substantially different in paper ballot municipalities in political ma-

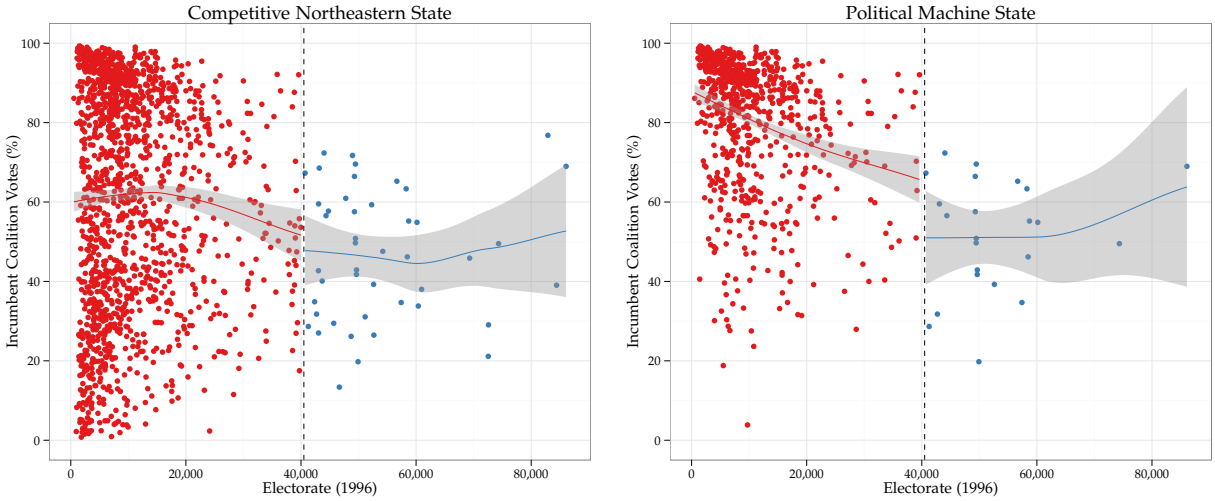


Figure 1.15: The effect of electronic voting on the vote share received by federal deputies from the state's incumbent coalition. The left panel shows data from states with competitive party systems and the right panel shows data from states with hegemonic ruling parties. Each dot is a municipality. Municipalities to the left of the vertical black line used paper ballots and municipalities to the right used electronic voting.

chine states, one that is consistent with large scale voting fraud. In these states, there is a *negative* relationship between invalid votes and incumbent coalition vote share. In paper ballot polling stations with fewer than 10 percent blank and invalid votes, candidates belonging to the incumbent coalition in political machine states received on average over 90 percent of all valid votes. Though not dispositive, the negative relationship is consistent with interview and newspaper accounts of political machines transferring invalid votes to their favored candidates, thus inducing a negative correlation only in paper ballot municipalities where stealing votes was relatively easy.

Given the considerable evidence of widespread fraud in "political machine" states, what are the consequences of introducing voting technology that largely eliminates it? Figure 1.15 visually depicts the basic results. In the left panel, in states with little of evidence of widespread fraud used by the incumbent to preserve their hold on office, the effect of electronic voting appears to be negligible. At the discontinuity point, there is no shift at all in the distribution of support for candidates from the incumbent coalition. In states with dominant incumbent governments (right panel), the effect of changing voting systems is quite different: the support for incumbent candidates drops substantially at the discontinuity.

Point estimates and their associated confidence intervals are reported in 1.16 for the effect of electronic voting on the vote share of candidates belonging to the incumbent coalition in both competitive (left panel) and political machine states (right panel). The effect in uncompetitive states is consistently large and negative for both federal and

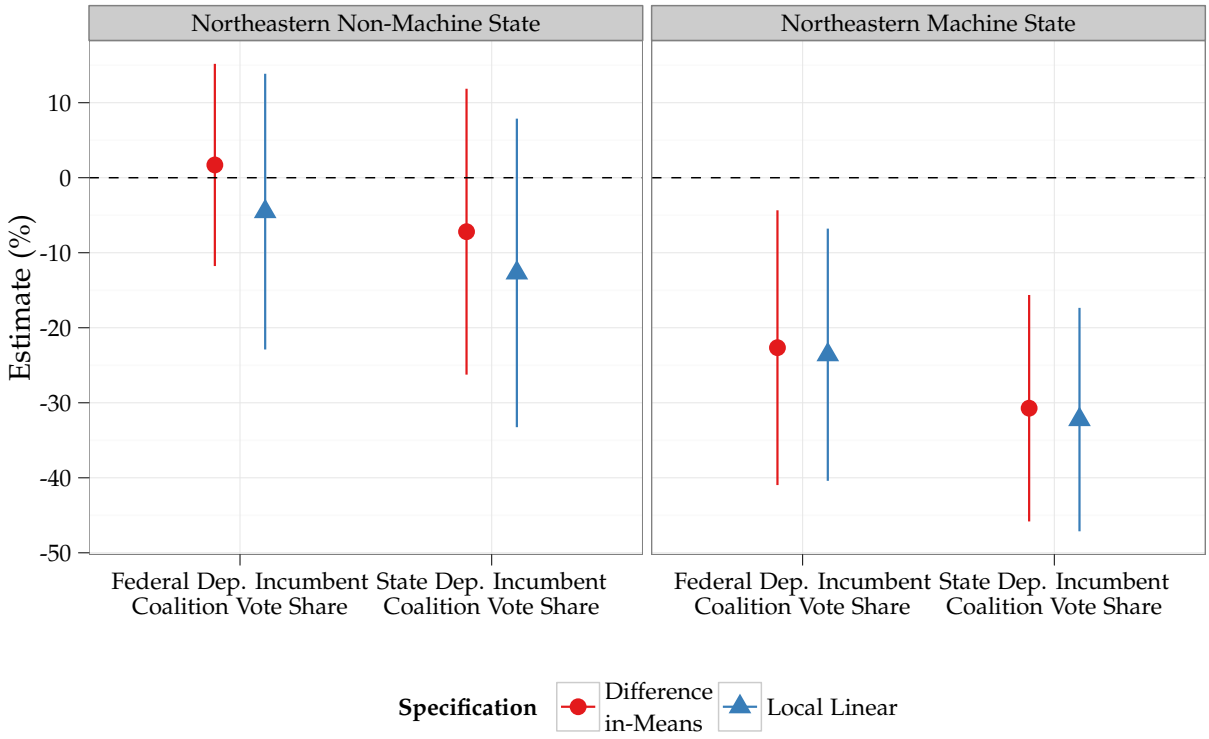


Figure 1.16: The estimated local average treatment effects of electronic voting on the vote share received by federal deputy and state deputy candidates belonging to the coalition of their states' governor. The estimated effects in competitive Northeastern states are shown in the left panel, while the effect in political machine states are shown in the right panel. .

state deputies. The estimates are about -22 percentage points for federal deputies and -30 percentage points for state deputies. The baseline percent of valid votes received by incumbent machine federal deputy candidates is about 75 percent, which drops to roughly 53 percent when electronic voting is introduced. While 53 percent incumbent vote share is still substantially higher than the 38 percent incumbent average vote share observed in the competitive states, the effect of electronic voting does amount to a large and sudden increase in the competitiveness of legislative elections in these states with dominant ruling parties. In the comparison group of states with more competitive party systems, the effect on incumbent candidates is much smaller and insignificant.

Discussion

The estimates above suggest that the elimination of vote count fraud had important implications for the trajectory of partisan competition in states governed by hegemonic political machines. If these findings are generalizable to other municipalities in the machine states—not just those immediately above and below the discontinuity threshold—then electronic voting may have significantly weakened the capacity of conservative machines to maintain their grip on political office. Indeed, the post-1998 history of these states is suggestive of the importance of fraud. In Bahia, the political organization of Antonio Carlos Magalhães, after suffering substantial electoral losses in the 2002 elections, finally lost control of the state government in 2006 to a left of center PT candidate after almost 25 years of uninterrupted rule. The Sarney clan in Maranhão suffered a similar reversal in 2006, when a left-wing candidate defeated the machine candidate for governor after more than 50 years of dominance.[48] Only in Paraíba has the conservative political machine not faced serious competition from opposition parties. As the conservative political machines weakened in Bahia and Maranhão, the legislative delegations, previously ideological outliers, began to move towards the center of the Brazilian ideological spectrum. The collapse of conservative hegemony in these states may have then contributed to the subsequent leftward shift and decline of ideological polarization in the national legislature, documented in Power and Zucco Jr (2009).

1.5 Conclusion

In this study, I exploit a natural experiment in Brazil to document the consequences of a technological change that simultaneously reduced voting barriers and dampened fraud. First, I show that—due to Brazil's highly complex political environment created by open list electoral rules—voting by paper ballot was difficult for a large fraction of Brazilian voters. The introduction of electronic voting dramatically reduced these difficulties, and consequently functioned as a *de facto* expansion of the suffrage. These new voters, while somewhat more right-wing, differed most from already enfranchised voters in their increased propensity to cast a partisan—as opposed to a personal—ballot. Given the fact that Brazil's party system is often characterized as highly personalistic, this change is potentially consequential for the long term dynamics of partisan competition in Brazil. The effects of reducing fraud were particularly acute in a few states ruled by dominant conservative parties. Without the ability to directly manipulate vote totals, candidates belonging to the incumbent political machines suffered large losses. These electoral setbacks, concentrated among right-of-center candidates, suggest that fraud may have played a key role in sustaining conservative rule.

[48]This candidate was later removed from office by the judiciary and the Sarney group returned to power.

A key puzzle presented by this study is why a large expansion of electorate had little effect on the types of candidates elected. While right-of-center candidates generally benefitted to a limited extent (excluding states with oligarchic political machines), the distribution of preferences among the previously disenfranchised were broadly similar to those already enfranchised. Because this enfranchisement was largely unexpected, it is possible that these new voters were largely unmobilized in the 1998 election, and consequently left little imprint on the kinds of legislators elected. This leaves open the question of whether the entrance of these largely poorer and less educated voters represented a new constituency to be mobilized in future elections. If candidates and parties began to adapt their platforms and policymaking to these new voters in future elections, then the long-term effect of their enfranchisement would not be apparent in the data used in this study.

The findings on the effect of electoral reform on the degree of political competition in the Northeast, provide insight on the empirical phenomenon of considerable interest to scholars of politics in developing democracies: the combination of state weakness and national electoral competition. As pointed out by O'Donnell (1993) and others, robust national political competition does not preclude the persistence of authoritarian enclaves where local elites resist the authority of the central state. Cosmopolitan urban centers where the government can provide public goods in a manner more or less consistent with the Weberian notion of an effective state coexist with vast hinterlands where the state's reach is limited or easily corroded. The fact that political machines in several northeastern Brazilian states could penetrate the electoral bureaucracy to commit fraud is one example of this more general phenomenon. While the amount of fraud has declined in most of Brazil since the 1940s, the strength of right-wing machines in Bahia, Maranhão, and Paraíba show that the capacity of the state to regulate elections was still highly heterogenous by the mid-1990s. The adoption of electronic voting, however, could be viewed as a sign of the further centralization and expansion of the authority of the central state vis-a-vis local political organizations, at least in the realm of elections. An insulated federal bureaucracy, through technological innovation and impressive organizational ability, significantly reduced the heterogeneity in the exercise of state authority and consequently brought genuine political competition to millions of Brazilians living in these oligarchic states. The adoption of electronic voting is thus a vivid example of how the rule of law can emerge through the extension of state authority by a centralized and insulated bureaucracy. Whether this is an exceptional case or an example of a more general phenomenon remains an area of future research.

While the downstream political effects of electronic voting and the survival of political machines are of theoretical interest, it is worth highlighting the policy implications of my findings. The voting technology literature has overwhelmingly focused on the US, where effects of voting technology on valid vote rates are typically less than 5 percent. While normatively problematic, in most cases—the famous Florida "butterfly" ballot notwithstanding—invalid vote rates of that size are unlikely to change electoral outcomes in most cases. My findings show that the potential disenfranchising effects

of voting technology can be much larger in electoral systems that require high levels of information to successfully vote, such as when candidate-centered voting is combined with large district magnitudes. Countries with these systems are not rare, as candidate centered and high district magnitude systems are used countries as diverse as Iraq, Colombia, and Finland. In these electoral environments, small changes to the ballot might have enormous effects and thus particular care must be exercised when designing these ballots in those contexts. The high informational requirements are particularly relevant in new democracies such as Iraq, where readily available partisan or incumbency cues are likely to be weaker.

Another implication of these findings is that the literature on *electronic* voting needs to be sensitive to the locus of fraudulent vote manipulation. Computer scientists have heavily criticized electronic voting systems due to their perceived susceptibility to fraud. One commonly invoked danger is that security vulnerabilities could allow incumbents or other actors to easily change vote totals in a centralized computer responsible for vote tabulation. A suspected case of this kind of event is the 1988 Mexican presidential elections, where the central vote tabulation computer crashed after the initial results showed high rates of opposition support. This case, however, was one in which the administration of elections were politicized and easily manipulated by the governing party. The Brazilian case is an example where the conditions are very different and the introduction of computers into the voting process was, on balance, fraud reducing, at least in the short term. The comparison between Brazil and Mexico suggests an important area of future research: what are the conditions under which electronic voting enhances political competition and what are the conditions in which new voting technology undermines it?

Chapter 2

Citizen Registration and Rural Over-Representation in Post-war Brazil

The institutional changes that accompany transitions to democracy often include the creation of rules designed to moderate the effect of widespread suffrage and safeguard the interests of the outgoing elite. Among the most frequent goals of countermajoritarian reforms is the over-representation of rural voters and elites through the use of institutional mechanisms like legislative malapportionment (Samuels and Snyder 2001) and the gerrymandered drawing of districts. These countermajoritarian institutions are thought to make democracy more acceptable to the nondemocratic elite (Alberts, Warshaw, and Weingast 2011) by ensuring that their interests will be protected even as urbanization and other demographic changes weakens the economic sources of their political power. These countermajoritarian rules, however, typically only apply to legislatures[1], while leaving popularly elected chief executives in presidential democracies more responsive to the broader electorate. Presidents—often the most powerful policymaker, especially when granted decree and agenda setting powers—are not directly constrained by common anti-majoritarian formal rules and thus may constitute a continual risk to the anti-democratic elite. If countermajoritarian safeguards are required for "losers" in democratic transitions to accept popularly elected legislatures, then non-democratic elite may also require that chief executives face similar anti-democratic political incentives. If so, this raises the question: in new democracies with powerful presidents, what makes the institution of a popularly elected chief executive acceptable to the non-democratic elite?

In this chapter, I identify and study the effects of an *informal* institution that can bias chief executives and other non-districted offices towards rural interests that does not involve formal rules like malapportionment. Specifically, I study how the delegation of voter registration to local elites, combined with the absence of the state in the countryside, allows for the fraudulent inflation of the rural electorate and the increased probability of electing rural-biased chief executives. Empirically, I draw upon historical data from the decade after Brazil's first transition to democracy in 1949 to show that due to the delegation of registration, the voter registry massively over-represented rural areas and that this phenomenon tended to benefit conservative gubernatorial and presidential candidates. I furthermore show how reforms that substantially reduced this rural bias in the voter registry resulted in the election of urban-oriented politicians. The rise of urban-based populist politicians, however, provoked increased redistributive conflict and consequently diminished support of democracy among the rural elite, thus highlighting the role of informal countermajoritarian institutions in generating support for democracy. These fraud reducing reforms, by diminishing the electoral influence of conservative elites, may have ultimately contributed to the demise of Brazil's democracy in 1964.

While the precise motivations behind weighting votes differentially through malapportionment can vary, institutional designers generally use legislative over-representation to compensate those who will be relative losers from the advent of popular sovereighty

[1]The electoral college in the United States, which over-represents rural states, is a prominent exception. See Shugart (2004) for a discussion of electoral rules governing the selection of chief executives.

by overvaluing their vote.[2] In presidential systems, this over-representation can induce an asymmetry between the political incentives of the popularly elected chief executive and legislators elected from disproportionately rural districts, a phenomenon that Samuels and Snyder (2004, 152-153) term the "estrangement of the legislative and executive branches". This l compromise, however, is less valuable to the non-democratic elite where presidents and governors have substantial unilateral policy-making authority. The greater the degree to which the chief executive can rule without the support of the legislature through decree powers and other formal mechanisms, the less relevant legislative countermajoritarian institutions are for placating pre-democratic elites. In Brazil, while the president was constrained by the malapportioned legislature in some domains of policy making, he retained considerable powers that allowed for unilateral policymaking.

Legislative malapportionment is also less relevant in polities with electoral systems that use un-districted proportional representation, i.e. where candidates are elected in a single polity-wide district. An apt example of this type of system are the Brazilian state goverments. Not only are governors very powerful institutionally relative to the state legislatures (Abrucio 1998), but state legislators are elected from state-wide at-large districts. This institutional setup, mandated by the constitution, precludes any form of malapportionment via biased districting. For rural elites operating in these settings, other mechanisms are necessary to preserve their formal political power when solutions such as malapportionment are unavailable.

In the absence of formal safeguards of anti-democratic elite interests, informal arrangements might act as close substitutes. The informal, but consequential, practice I identify in this chapter occurs at the the intersection between state weakness and the voter registration process. Important for my argument is the assumption that in countries with low capacity states, state weakness is correlated with geography and that in particular, the ability of political elties to subvert bureacratic procedures and manipulate voter registration will be higher in the countryside than in the city. While this assumption is not necessarily true in all cases, there are structural factors which I discuss below that tend to make rural environments more hospitable to registration fraud than cities. Rural politicians, more so than urban politicians, can take advantage of this assymetry in state capacity between city and country to fraudulently expand the electorate in their areas of influence. Rural bias in the capacity to engage in fraud has similar consequences of formal malapportionment: the empowerment of the countryside relative to the city.

The relative ease at which rural elites can fraudulently inflate the electorate is partic-

[2]US history is replete with examples of malapportionment, even for lower houses. For example, in California before the 1962 *Baker v. Carr* decision, the smallest, rural, counties had as much as 400 times as much representation in the state legislature as urban Los Angeles, the largest county in the state (Ansolabehere, Gerber, and Snyder 2003, 767). Another example occurred in the US House in 1920, which skipped a constitutionally-mandated decennial round of redistricting because of rural representatives concern over the growth in the urban electorate (Okrent 2010, 237-238). As one representative baldly put it, "It is the city versus the country" (241).

ulary important when combined with another trend that often coincides with democratization: urbanization. As was the case in post-war Latin America, rapid urbanization led to sharp growth in the urban population. The draining of the countryside due to outmigration and increased fertility in the cities posed a long-term threat to the power of anti-democratic elites. While the population remained predominantly rural, rural landlords could use their coercive control over rural voters to ensure that elected politicians were responsive to their interests. As cities grew in size, however, domination of the rural electorate became a less potent political resource. As I document in this chapter via a discussion of the historiography of the period and newly collected quantitative measures, one strategy pursued by rural political elites to mitigate the political problems of an increasingly urban electorate in post-war Brazil was to take advantage of the weakness of the state in the countryside to induce a substantial gap between true distribution of the voting eligible population and the rural-ruban distribution in the electoral registry. Rapid, albeit fraudulent, growth in the rural electorate counteracted growth in the urban electorate.

To study the political consequences of rural fraud in post-war Brazil, I examine the effects of an electoral reform that re-registered the electorate of the entire country and increased the barriers to fraud. The most immediate consequence of the reform was a sudden and sharp decrease in the size of the electorate across most of Brazil. Despite a rapidly growing population, after the 1958 reform, Brazil's total electorate *fell* by 22 percent from one year to the next. I use newly collected historical data to show that the effects of the reform varied substantially across Brazil, with regions well known for the strength of the conservative elite showing particularly large declines in the size of the electorate. In the Brazilian Northeast, for example, the electorate in rural municipalities shrank by over 30 percent as a result of the re-registration of the voting age population. In contrast in the urban Southeast, home to a more populist style of politics, the urban electorate grew 1 percent in the aftermath of the reform. The disaggregated figures suggest that the reform reconfigured the electoral geography of post-war Brazil in a more urban direction. This reconfiguration, I argue, had weighty implications for the polarized politics of late 1950s and early 1960s Brazil that preceded the 1964 military coup.

To more precisely quantify the effects of this reform, I use panel data to study how differences in the degree to which the electorate declined in the aftermath of the reform affected the geographic distribution of turnout, as well as the political fortunes of populist and conservative political parties. I focus on two samples of municipal-level data. The first sample contains data on municipal level results of the 1955 (before the reform) and 1960 (after the reform) presidential elections in the Northeastern region of Brazil. This larger sample of 602 municipalities allows me to estimate the effects of the reform on the geographic distribution of turnout over a relatively broad cross-section of Brazilian municipalities. The second sample consists of archival data from municipalities in the Northeastern state of Pernambuco. This sample contains data from five different gubernatorial elections and a much richer set of socio-economic covariates than in the

presidential sample. While the Pernambuco sample is considerably smaller than the national sample with only 86 municipalities, the focus on a single state allows me to more precisely characterize the consequences of the reform on partisan competition, as well as take advantage of the rich historiography on the politics of the state during the period to properly contextualize the estimates.

Estimates from difference-in-differences models reveal a strong correlation between the decline in the electorate and changes in turnout in both the Northeastern sample and the Pernambuco sample. The estimate using the broader Northeastern sample implies that going from the 10th percentile in the treatment variable, defined as the percent change in the size of the electorate as a result of the reform, to the 90th percentile resulted in a 5 percentage point decline in turnout (as a fraction of adult population). This point estimate is very large, considering that the median municipality had a turnout of only 14 percent. Estimates of similar magnitude were found using the Pernambuco sample. I interpret these sudden and sharp declines in turnout as a result of the 1958 re-registration purging fraudulent "ghost" voters from the voter rolls, thus diminishing widespread fraud in the Brazilian countryside.

To show that these purged "ghost" votes mostly benefited conservative, rural-oriented, candidates and that the reform had consequential effects, I rely on the Pernambuco sample. Using this sample, I document that the declines in turnout primarily affected conservative gubernatorial candidates, as opposed to the candidates belonging to more populist, urban-oriented, coalitions. My results suggest, via a preliminary simulation, that the impact of the reform politics was decisive for the trajectory of Pernambucan politics, as the 1958 reform allowed a populist candidate to capture the state government for the first time since democratization. The governors elected subsequent to the reform did not rely on countryside for the bulk of their votes and as a consequence enacted policies that weakened the political and economic power of the traditional elite. In particular, the post-reform governments removed control of the police from municipal governments, which relaxed one of the chief constraints to peasant and labor collective action. The subsequent heightening of redistributive conflict in the countryside in Pernambuco became a issue of national importance and contributed to the polarized political atmosphere which fostered the 1964 military coup.

2.1 Voter Registration and State Capacity

Academic and policy discussions of voter registration procedures have generally focused on either the administrative challenges involved in counting and categorizing the electorate—the "state capacity" dimension—or the political logic behind the changes—the "partisan" dimension. Administratively, registration is difficult because it requires a state capable of continuously screening and monitoring the populace throughout the national territory for eligible voters. The government must create a bureaucratic apparatus with the technical capacity to count voters in even the most far flung areas of the

national territory. Electoral registration is also the focus of political contention because the voter registry determines who is and who is not eligible to affect the identity of policy-makers through their vote. Historians (Keyssar 2009, 123-131) and political scientists (Piven 2000), mostly drawing on the US experience, have richly documented how registration laws are used for political advantage, as incumbents seek to include favored constituencies and exclude opposition voters.[3] The coverage and quality of the voter registry thus depends on the capacity of the state, as well as the decisions made by often self-interested lawmakers about registration procedures.

While most political science research on the politics of voter registration laws have focused on partisan conflict over eligibility rules, in much of the world, whether or not the state is actually capable of counting the electorate is just as politically relevant. After the political decision demarcating the boundaries of the *demos* via age, literacy, or gender qualifications is made, the burden of actually registering voters falls upon the state. To adapt Scott (2009)'s formulation[4], an effective election system requires that the object of an election (the voter) must be made *legible*. In other words, prior to an election, the state faces the formidable challenge of solving the logistical and informational difficulties of "mapping" the populace and accurately adjudicating the political status of its members. The mapping and categorization of the citizens is potentially a politically charged process, as being made visible to the central state can be quite costly. Scott (2009), for example, amply documents the many costs associated with detection by the state in pre-colonial Southeast Asia, such as taxation and conscription, that can outweigh any benefits that come with being recorded on census rolls or other registries.

An electoral registry is distinct from a census or civil registry, however, in that its composition is directly linked to the distribution of *de jure* political power. The outcomes of elections are in part a function of who is officially deemed part of the electorate. If the electoral registry faithfully reflects the "true" underlying eligible population, then election outcomes will purely be a function of the decisions of qualified voters over whether to turnout and who to vote for. When the voting registry, however, is not equivalent to the true population of qualified voters, then election outcomes are also a function of the process that determines the makeup of the voter rolls. As a result, the registration process and consequently the composition of the electorate is itself likely to be an object of political contention.

The Geographic Dimensions of State Authority The political implications of the procedures and rules governing voter registration is particularly relevant when the administrative capacity of the national government is geographically uneven. Elites in areas

[3]For example, Keyssar (2009, 127) discusses a case where Republicans in the Michigan state legislature sought to prevent the re-election of an opposition mayor of Detroit by removing foreign born voters from the registry. The Republican sponsor of a legislative proposal to enact this purge argued that "[i]t will take off the books just about enough Pingree [the mayor] votes to prevent his ever becoming mayor again."

[4]With the original quote being "[a]n efficient system of taxation requires, first and foremost, that the objects of taxation (people, land, trade) be made legible"(Scott 2009, 91).

where monitoring and categorization of the electorate is difficult can use state weakness to their advantage by manipulating the registration process to inflate the number of voters in their area of influence or otherwise interfere with the electoral process. This is possible because the typical solution to the challenge of counting the electorate by the central state is the delegation of citizen registration to outside actors such as political parties, civil society groups, or subnational governments. The dilemma of this strategy is, of course, that these outside actors have their own political ambitions and can use their control over the registration process to further them. Elites interested in implementing clean elections then face the potential dilemma of delegating administration of a crucial aspect of elections to precisely those actors who have no interest in accurately counting and categorizing voters.[5] When the state is weak and its reach is uneven, however, delegation may be the only option.

The primary geographic variable correlated with the reach of the state highlighted in studies of state formation is the urban-rural divide (Soifer 2008, 242). Social actors capable of ignoring, hiding from, or even challenging the state are overwhelmingly found in the rural hinterlands, far from the urban centers where the writ of the central government is more likely to bind (Migdal 1988). Indeed, the literature on state formation closely ties urbanization to the rise of the modern nation states. Tilly (1992, 51), for example, conceptualizes cities as "containers and distribution points for capital" that allow urban ruling classes to "extend their influence throughout the urban hinterland and across far-flung trading networks". Where transportation networks are sparse and bureaucrats are few, peripheral elites are more able to escape monitoring and subvert voter registration procedures for their own ends. In cities, however, population density, increased state presence, and links to transportation networks facilitate state capacity and consequently a more accurate voter registry.[6] More competitive electoral politics and the presence of the media typically found in cities also contributes to reduced fraud. Incumbents manipulation of the voter rolls is more likely to be caught and corrected when a viable opposition party or independent media exists to uncover it, both of which

[5]The US experience with disenfranchisement of African Americans in the Southern states is a case in point. One of the principal mechanisms used to exclude African-Americans from the electorate was local control of voter registrars. That registration procedures could be used to disenfranchise large swaths of the population was evident early on, as subnational autonomy was subject to heated debate in the aftermath of the Civil War. For example, when Republicans sought to pass a Federal Elections Bill (also known as the "force bill") in 1891 that would have empowered federal supervisors to regulate voter registration, Democrats opposed it because of its implications for federal power (Keyssar 2009, 87). Given that Southern Democrats' electoral hegemony rested, in part, on the continued absence of federal control of elections, Southern democrats would be suspicious of any expansion of national power in the subsequent decades. Of course, the 1965 Voting Rights Act implemented some of the proposals in the 1891 bill nearly a hundred years later by empowering federal examiners to enroll voters and observe registration practices.

[6]That is not to say that urban voter registration fraud is rare. As histories of New York City politics vividly recount, political machines would often resort to registration fraud to win elections. Yet, precisely because the fraud occurred in the city, it could be detected with relative ease by opponents of the machine. Visible fraud became the target of electoral reforms promoted by progressive movements and was significantly reduced as a result of these reforms, at least in late nineteenth century New York.

are made more possible by the economic vitality and population density of cities.

The asymmetry between city and country in the capacity of the state to accurately construct a voter registry creates the potential for an informal form of malapportionment that privileges rural-based interests. If the central state delegates its authority to parties or local governments, then seeking to forestall the rise of the cities, rural interests will use their control over citizen registration to "augment" the size of the rural electorate. Furthermore these interests can use the lack of state oversight, to ensure that these "ghost" voters show up on election day. If candidates are elected by the "official" electorate as measured by the voter rolls, then governments that win elections will be more responsive than they otherwise would have been to the rural bosses and interests that skew the electorate in their favor.

This argument suggests that urbanization may not inexorably lead to a decline in the political power of rural actors like landed elites. If elections are simply a matter of numbers, then the urban electorate should eventually outweigh the rural vote, leading to a national government more responsive to urban interests. James Kent, a conservative politician in early nineteenth century New York, expressed such a fear when he argued

> What has been the progress of the city of New York? In 1773, it contained only 21,000 inhabitants; in 1821, 123,000 souls. It is evidently destined to become the London of America... And can gentlemen seriously and honestly say, that no danger is to be apprehended from those combustible materials which such a city must ever enclose? (Keyssar 2009)

This argument, however, presupposes that elections, a state function, will accurately reflect the shifting demography of the electorate. There is, however, an overlooked transition problem: carrying out free and fair elections throughout the national territory that reflects the new demographic balance of power requires an electoral administrative apparatus capable of accomplishing such a feat. Once the decision to hold elections is made, the formidable task of constructing the infrastructure for actually administering democracy throughout the national territory remains.

A policy priority of rural elites will be the preservation of the institutional environment that underpins their authority: the devolution of authority to local authoritarian enclaves or what Hutchcroft (2001, 33) calls "autocratic decentralization". Governments elected with the aid of manufactured rural voters will be resistant to measures that extend the coercive capacity of the central state to areas traditional controlled by rural bosses. As a result of this dynamic, pre-existing state weakness hinders the emergence of governments willing to project state authority into rural areas, even when the urban electorate is growing rapidly.

2.2 Registration Fraud and Rural Bias in Brazil

Prior to the 1937 populist coup led by Getúlio Vargas, control of the national state alternated between different factions of rural based oligarchic elites. Elections were riddled with fraud and consequently were not mechanisms to transmit preferences of the populace but rather means to resolve intra-elite conflicts. The end of the "Old Republic" with the revolution of 1930 and the subsequent centralization of power by more urban-oriented elites marked a decisive defeat for traditional power holders (Hagopian 2007, 52). This defeat of the traditional state-based machines was exemplified by Getúlio Vargas's regular practice of imposing direct control over state and municipal governments using "interventors", rather than accommodating or negotiating with dominant state machines. After almost two decades of centralization of authority and a raft of new social and economic policies, the onset of elections after Getúlio Vargas left power in 1945 provided a new opportunity for traditional elites to once again use manipulation of the electoral process to check the influence of emerging, urban-based, social groups.

The potential of elections to return rural elites to complete control of the state, however, was limited by institutional architecture put in place by the Vargas government. Despite his authoritarian tendencies, Getúlio Vargas's government created a framework for future elections that would represent a sharp break from past practices. Clean elections was one of the key demands of the reformers (Sadek 1994, 30) who backed Getúlio Vargas's insurgency and the institutional designers ensured that the administration of elections would be insulated from the old elite. This insulation was embodied in the creation in 1932 of the Supreme Electoral Tribunal (TSE, *Tribunal Regional Eleitoral*), a judicial body responsible for regulating and implementing elections. Originally created via a presidential decree but later enshrined in the 1946 constitution, the TSE was headed by a court consisting of six judges that were appointed via procedures that shielded them from the influence of rural machines.[7] The TSE took over responsibility for creating the specific regulations governing voter registration and vote counting, matters that prior to 1937 had been under local control and consequently easily manipulated. While implementation of these rules in a uniform manner over Brazil's vast territory would be impossible, the creation of an insulated, national bureaucracy interested in carrying out fair elections was a formidable obstacle to continued rural dominance of electoral politics.[8]

[7]The TSE consisted of two judges from the Supreme Court (*Supremo Tribunal Federal*), two judges from the Federal Court of Resources (*Tribunal Federal de Recursos*), a judge from Federal Court in the capital (*Tribunal de Justica do Distrito Federal*), and two judges picked by the president from a list of six citizens provided by the supreme court. Judges are placed on their original courts through nomination by the president and confirmation by a majority of the senate.

[8]The new electoral laws also favored the urban electorate through the use of "ex-oficio" voter registration, wherein national government agencies and large employers could register their employees directly (M. d. C. C. d. Souza 2006, 23). Furthermore, the electoral law restricted voting to the literate, which were disproportionately located in the cities. Too much can be made of this restriction, however, as many qual-

Despite the rules put in place designed to prevent a return of the rural-dominated politics of the past, in the years immediately following the transition, previously dominant regional elites won overwhelmingly in the new elections. The newly formed conservative parties—the National Democratic Union (*União Democrática Nacional* or the UDN) and, above all, the Social Democratic Party (*Partido Social Democrático* or PSD)—had substantial autonomy in each state to form alliances with local actors. Unsurprisingly, the local elites with substantial control over rural voters[9] represented a tempting alliance partner for the new parties and in state after state, the old rural elites provided a crucial electoral asset to the winning coalitions. State-level governing coalitions in much of Brazil tended to be composed of alliances between rural elites and centrally-appointed interventors and other bureaucrats that chose to seek office via elections after the end of the authoritarian regime (M. d. C. C. d. Souza 2006, 23; Hagopian 2007, 62). The electoral assets of the rural elite was undeniable: after the first national election, 85 percent of elected legislators in the lower house belonged to predominantly rural and conservative parties (G. Soares 2001, 81).

The Threat of Urbanization

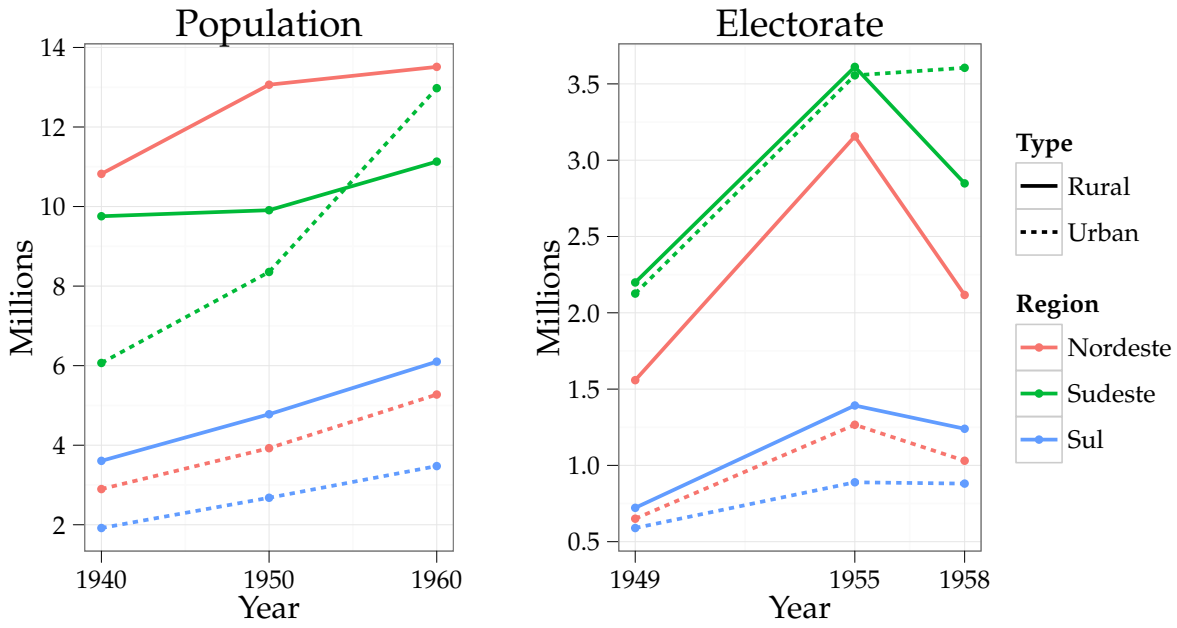
As in much of the world, industrialization and urbanization bred a new kind of politics (Rodden 2010, 324), which in Latin America and in Brazil, in particular, tended to be associated with the rise of "populist" urban elites leading multi-class alliances, with unionized workers playing an important role in these new political coalitions (R. Collier 2001). In addition to the more populist and leftwing ideological currents that prospered, politics in the cities tended to be substantially more competitive than in the rural hinterlands. Data from the 1954 and 1955 city council elections supports this: the median effective number of parties in urban municipalities was 3.7, while in the countryside it was 2.2. This difference in levels of competition reflect both the higher degree of social heterogeneity among urban residents, but also the ability of local rural elites to use economic and resources to insulate themselves from political challengers.

itative accounts of elections in the period note that in the countryside, the literacy requirement was often only honored in the breach. Local politicians with influence over the local registrar could ensure that the illiterate workers under their control would be registered. Ironically, it was probably more difficult for the illiterate to vote in the cities than it was in the countryside.

[9]Control of rural voters, while weakening during this period because of advent of mass media and increased urbanization, flowed from large landowners' economic dominance over the lives of the peasantry. Wilkie (1968), an anthropologist who spent the early 1960's in rural Pernambuco, described this relationship quite vividly:

> The isolation of the workers in the sugar growing area of Pernambuco is striking. Groups of twenty to forty families live in long houses marooned in the sugar. The provvision of schools, medical attention, and recreational facilities depend on the good will of the owner... (5).

For a theoretical treatment of this employment relationship and its effect on electoral politics, see Robinson and Baland (2008).



(a) Change in Rural and Urban Population (1940-1960)

(b) Change in Rural and Urban Electorate (1949-1958)

Figure 2.1: **Population and Electorate Change in Brazil (1940-1960).** The figure on left shows population trends in rural and urban Brazil by region. The figure on the right shows trends in the number of registered voters. While population increased in all regions throughout the period, the electorate fell substantially in most regions after 1955.

The populist threat of the cities, however, could be contained so long as the electorate remained predominantly rural. After the first post-democratization 1945 elections this remained the case as the political institutions of the Brazilian government, particularly the legislature, were back in the hands of the conservative elite. The traditional elites, however, faced a demographic challenge that threatened their continued electoral dominance: rapid urbanization. Figure 2.1a shows the population trends in Brazil's three largest regions. In the 1940s and 1950s in particular, population growth in the cities outpaces growth in the countryside. The urban growth experienced during this period of democratic opening reflected the sharp increase in the rate of migration from rural areas to industrial centers like São Paulo, as well as improved health conditions. These divergent trends were particularly noticeable in the more industrialized Southeast. Temporarily, however, the ability of the rural elite to translate their economic resources into votes could counteract the more competitive and populist electoral politics in the cities. Eventually, however, the demographic balance of power would shift sufficiently

to deprive rural politicians a sufficient number of voters to ensure that governments less friendly to their interests would not take power.

The Strategic Use of Voter Registration Fraud

One strategy to prevent the more competitive populist politics of the cities from dominating national and state politics was to place a wedge between the true voting eligible population and voter registry. As long as the rural "electorate" overwhelmed the urban vote, popular elections would be less likely to deliver results that displeased rural elites. In fact, anomalous growth in the rural voter registration rolls is easily visible in the available historical data, particularly before the 1955 reforms. Overall, between 1945 and 1964, the electorate expanded at a tremendous rate, starting at 7.5 million voters in 1945[10] and growing to 18.5 million in 1962; a threefold increase. Yet the demographic and regional composition of this growth was quite different in the pre-1955 reform period of democratic competition than in the later (post-1955 reform) period. Figure 2.1b depicts the growth in the electorate between 1949 and 1962. While the electorate grew in urban and rural municipalities alike prior to 1955, growth in the countryside occurred as fast or faster than electorate growth in the cities. Pre-1955 growth in the countryside was particularly rapid in the Northeast, precisely where the traditional elite were most dominant.

The large gap between the true voting eligible population and the number of registered voters was possible because of flaws in the prevailing electoral law, as well as the inability of the election authorities to adequately vet the voter rolls. The largest problems in the pre-1955 electoral laws[11] were third party registration of voters and the possibility of multiple voting due to voters being able to cast ballots in polling stations to which they had not been originally assigned. Politicians abused their ability to register on behalf of voters by registering their supporters multiple times in different jurisdictions, in addition to manufacturing fake voters out of whole cloth (so called "ghost voters"). Case studies of elections during the period, as well as legislative debates over electoral reforms, are rife with anecdotes of these "ghost voters" or voters registered in multiple jurisdictions voting many times. The qualitative evidence aligns with the quantitative picture: registration fraud was particularly acute in the countryside, where political machines allied with local economic elites could act with relative impunity.

Because of the absence of a bureaucracy capable of registering the country's voters, particularly in the countryside, lawmakers in the first years of Brazil's democratic pe-

[10]The elections in 1945 were the first in Brazilian history with relatively widespread participation, while the 1962 elections were the last elections before the 1964 military coup that initiated 21 years of military rule.

[11]The most important laws governing election during this period were Law 7.586 (1945), and Law 1.164 (1950). The 1945 law allowed large employers to register voters en-masse, while the 1950 law empowered parties to do so.

riod gave third parties, such as employers[12] and political parties, an important role in registering voters. Party officials and employers were empowered to find voters, fill out registration forms, take their picture for the registration document, and then deliver these documents to election officials for certification. Once the documents were delivered to registrars employed by the state election tribunals, voters would receive a voting card ("titulo") that would allow them to cast a ballot on election day. While election registrars were legally required to verify the accuracy of the registration paperwork, in practice, election officials did not, either due to logistical difficulties or because of collusion with politicians. Such collusion was described by Marcos Palmerio, a novelist and congressman, thusly:

> The process was simple. In the last few days of the registration period, the party gathered all the identification documents provided by the corrupt registrar and gave them to party operatives. ....Each voter was provided with various voting cards, allowing them to vote and sign voting documents; there was no way of discovering the fraud (Palmério 1978, 235).

José Sarney, former president and political boss in the state of Maranhão recalled the status quo prior to later reforms: "In that time, voter registration was totally anarchic because politicians carried it out and not electoral officials. It was the politicians who took the initiative to register voters and they would register voters in many places, creating real chaos" (TSE 2005, 56).

Registration fraud typically occurred via links between local politicians and the bureaucrats in the election administration charged with the distribution of voting cards. One such case, as documented by Maria de Lima (2010, 234), involved Theodorico Bezerra, a prominent landlord and PSD party boss in Rio Grande do Norte. Bezerra succinctly described the relationship between voter registration and political power in a speech given to his workers, which was recorded:

> You know that here on my property, all workers are required to be registered voters. When you get your voting card, I myself want to take your photo. Why? So you can see me and I can see you. Your registration card will be given to me. Because I only want you to live on this land if you are a voter. Those who aren't voters can't live here. As I've always said, the only thing I want from you is your vote. You don't have a car to lend me, money to lend me, a cow to give me milk, or a horse for me to ride on. But vote, vote you do have. And if you don't want to give me your vote, why should I waste time with you? (237)

Bezerra exploited his economic dominance vis-a-vis his workers to great advantage to increase his influence in the state by registering voters en masse in municipalities where

[12]Large employers were initially permitted to register their employees through a procedure labeled "ex-officio" registration, but this practice was no longer permitted after 1950.

they did not live. For example in 1952, in São Tomé, a municipality close to his landholdings, Bezerra enrolled 1,065 voters; an expansion amounting to approximately 20 percent of the electorate (Maria de Lima 2010, 234). This sharp and sudden growth in the number of registered voters, it was later revealed, was possible because of a pact between a local registrar and political operatives that allowed for the mass registration of voters from municipalities under Bezerra's control. On election day, these voters would be transported to São Tomé by Bezerra to vote again after casting ballots in their own municipality.[13]

In parliamentary discussions over electoral reforms, the theme of registration fraud emerged repeatedly. Federal Deputy Ernâni Sàtiro, for example, discussed a case where a Rio de Janeiro state legislator was found with 3000 registration cards that had been signed by a local registrar (Brasil 1954, 313). Satiro also complained of a political operative who brazenly proclaimed that "I don't worry if I have voters who support me or not. The registration cards I have are what matters". Testimony by electoral officials (499) underlined the point that the election bureaucracy was incapable of adequately vetting the voter list due to inadequate staffing and administrative controls, as well as pervasive corruption. These officials presented exhaustively detailed reports of voters who succeeded in voting multiple times by using different voting cards in different locations.

Recounting the Electorate: The 1955 Reform

Given the deficiencies in the existing electoral law and the weaknesses of the electoral bureaucracy, urban politicians and election officials succeeded in passing a major series of reforms[14] in 1955. The reforms instituted several changes to electoral procedures designed to combat fraud, particularly registration fraud. Rather than purge the existing voter registry, the new law required election officials to start anew. The reform mandated the election bureaucracy to re-register all eligible voters in time for the 1958 elections. To prevent multiple voting, newly registered voters were to be assigned to a polling station and would not be allowed to vote in another station for any reason. To aid in implementation, the electoral agencies charged with re-registering all eligible voters and instituting the new procedures were also given more resources. This increase in bureaucratic capacity is evidenced by a 10 percent increase in the number of polling stations (*sec cões eleitorais*) in elections after 1955 despite a decrease in the number of registered voters. Finally, the law replaced party-provided ballots with the Australian ballot in presidential and vice-presidential elections.

[13]The historiography of elections during the period documents many similar cases. For example, Rêgo (2008, 130) discusses how a political boss in rural Pernambuco, Heráclio do Rêgo, placed overwhelming importance on using the registration process to win elections. In another study of Pernambucan politics, Levine (1978, 94) describes a case where local election registrars operated out of the home of the municipality's political boss.

[14]Law 2.2250, passed on July 25, 1955 and Law 2.582, passed on August 20, 1955.

The effects of these reforms are clearly visible in the data. As displayed in figure 2.1b, with the exception of urban municipalities in the Southeast, almost all municipalities lost voters. In a three year span, the median municipality in Brazil lost 22 percent of its electorate. In the South, the drop was only 9 percent, but in the Northeast, the median municipality experienced a 28 percent plunge in the voter rolls. This decrease was especially dramatic in the Northeastern countryside, which lost 1,039,020 registered voters, as opposed to a 235,952 voter decrease in urban areas. After the 1955 reform, the gap between the rural and urban vote shrank considerably in the Northeast and the South, while in the Southeast, the urban electorate overtook the rural electorate for the first time. Overall, the 1955 voter registry reform amounted to a negative shock to the electoral weight of the countryside relative the cities.

2.3 Research Design and Data

To understand the causal effect of the 1955 registration reform on political dynamics within Brazil, I exploit the municipal-level variation in the change in the size of the electorate in two datasets. The first dataset contains data from all Northeastern municipalities and the second, more complete and extensive dataset focuses solely on the state of Pernambuco. While the larger Northeastern municipality dataset allows for an examination of the the broad effects of the 1955 reform on a large proportion of Brazilian municipalities, the Pernambuco data is much richer, allowing for a closer examination of the consequences of the reform, as well as more rigorous tests of the causal assumptions underpinning the statistical models.

Unlike typical fluctuations in electoral rolls that are a function of population change and political factors, the change or "treatment" I examine was imposed by federal election officials, making this cause more plausibly exogenous to hard-to-measure local factors that typically plague these types of studies. Additionally, I use the panel structure of the data to further protect against confounding variables. I rely on a "difference-in-differences" model, augmented by a non-parametric matching pre-processing step, for inference. Of course, the exogenously imposed treatment and panel nature of the data still may not be sufficient to guarantee the validity of my estimates, but it is reassuring that the design passes several placebo tests by recovering known effects of zero across multiple dependent variables.

The clarify the inferential goals of the quantitative analysis, the Neyman-Rubin potential outcomes framework as applied to continuous treatments is helpful. For each municipality i there exists the potential outcomes $Y_i(t)$ for $t \in \mathcal{T}$, which is a "dose response" function, a mapping for each unit between its response and its treatment dosage. In this case, treatment dosage is the degree to which the 1955 reform induced a decline in the electoral rolls. One quantity of interest is the average dose response function, $\mathbb{E}[Y_i(t)]$ for all t, which is the average response if all municipalities had received a given "dosage" of change in voter rolls. Given the continuous nature of the treatment, we

are interested in the *derivative* or the rate of change of the entire dose response function which may vary by the level of the treatment.

As is usually the case in observational studies, my conclusions depend on a conditional independence assumption, i.e. that conditional on a set of covariates, assignment to treatment dosage is independent of potential outcomes: $Y_i(t) \perp\!\!\!\perp T_i | X_i$ for all $t \in \mathcal{T}$. Because the panel structure of the data is particularly helpful for time invariant sources of confounding (Angrist and Pischke 2008, 230), the largest threat to the validity of my estimates are dynamic confounders, those variables correlated with the outcome that change at different rates among units with differing values of the treatment variable. To that end, it is particularly important to check that trends in important covariates, particularly values of the dependent variable, are statistically unrelated to the treatment *before* the reform. While not dispositive, lack of pre-treatment differential trends provides indirect evidence of the validity of research design. While such a test is not available for the Northeastern dataset, the more extensive panel structure of the Pernambuco dataset allows for such tests.

Data

A full description of data sources, as well as basic summary statistics, are reported in the data appendix, but it is worth discussing in some detail the definition of the main outcome and treatment variables. The main treatment variable is the percent decrease in the electorate induced by the 1955 reform. More specifically, it is the difference between the number of registered voters in 1954 (pre-reform) and the number of registered voters in 1955 (post-reform), divided by the number of registered voters in 1954, multiplied by -100. Thus, the larger the value of the variable, the more the reform affected the municipality.

The main outcome variable is turnout, which is the number of votes cast as a fraction of 1950 population. For the Pernambuco sample, auxiliary dependent variables include vote for the conservative gubernatorial candidate, and vote for the populist gubernatorial candidate. To adjust for differing population sizes, I divide these outcomes by the 1950 population[15], as measured by the census. For the Pernambuco dataset, classifying gubernatorial candidates as conservative or populist using historical sources is straightforward: those candidates belonging to the UDN[16] were considered conservative, in that the UDN was, according to all historical accounts, the main partisan vehicle

[15]One alternative is to divide the numerator by the number of voters who turnout, but since this variable is affected by treatment, it is easier to interpret effects when divided by a variable that is itself not a consequence of the phenomenon of interest. Technically, the franchise was restricted to literate voters, although many historical accounts argue that this restriction was easily overcome, particularly in rural areas. Data on the number of literate adults is only available for the Pernambuco sample, but when turnout is expressed as a fraction of literate adults, substantive results for the Pernambuco models are unchanged.

[16]In 1962, João Cleofas belonged to the Republican Party, but he ran in a coalition with the UDN.

used by the rural elite (Rêgo 2008, 122; Pandolfi 1984, 97; Albuquerque and Vilaça 1965). The populist coalition that supported candidates running against the UDN was more heterogenous, as it encompassed a wide variety of interests ranging from urban communists to sugar exporters, as well as the some of the "out" factions of the inland rural oligarchy (Lavareda 2008, 46). Despite its heterogenous character, the populist coalition was considerably more urban-oriented than the UDN as it was backed by industrial unions and received most of its votes from the state capital and its environs.

Difference-in-Differences

The statistical model I estimate is the following:

$$Y_{it} = \gamma_i + \lambda \cdot d_t + \delta(F_i \cdot d_t) + X_{it}\beta + \epsilon_{it}$$

where Y_{it} is the dependent variable in municipality i in election year t, γ_i is a municipal specific intercept (a "fixed effect") for each municipality, d_t is a dummy variable that takes a value of 1 for elections after the 1955 reform, λ is the common time "effect", F_i is the percent decrease in the electoral rolls in municipality i induced by the reform, and X_{it} is a vector of time varying covariates[17]. The municipality fixed effects account for time-invariant omitted variables, while the time fixed effect adjusts for common shifts in the dependent variable across all municipalities. The main causal parameter of interest is δ, which is a constant marginal effect of a 1 percent decrease in electorate on outcome Y_{it}. The estimate of δ can be interpreted as a linear approximation to the dose response function (Angrist and Pischke 2008, 77-80), if conditional on the municipality and year fixed effects and the covariates, the conditional independence assumption holds. Standard errors are computed from a bootstrap distribution created by resampling municipalities 200 times.

Semi-parametric Difference-in-Differences and Placebo Tests In results presented below, I augment parametric difference-in-differences results with a prior matching step (Abadie 2005). The "parallel paths" identifying assumption underlying the difference-in-differences estimand may be less compelling if municipalities with large drops in their election rolls differ on baseline variables from those municipalities with smaller decreases. In other words, if municipalities with very different levels of the treatment variable also differ substantially on variables such as prior population growth, levels of political competition, and economic prosperity, then one may legitimately question the identification assumption that the potential outcomes of both types of municipalities would have changed at similar rates in absence of any reform.

As documented in appendix 2.7, municipalities in the top quintile of the treatment variable were indeed different from municipalities with smaller decreases in the electorate on some variables. Municipalities that lost more than 45 percent of their voters

[17]Given the potential risk of post-treatment bias risked by including covariates that could be affected by the treatment, I present results both with and without covariates

tended to be more rural, less economically developed, and experiening slower population growth than municipalities that lost fewer voters. Of the available covariates, population growth between 1950 and 1960 most strongly correlated with treatment. Reassuringly, turnout, the dependent variable, was not correlated to the treatment variable prior to the reform.

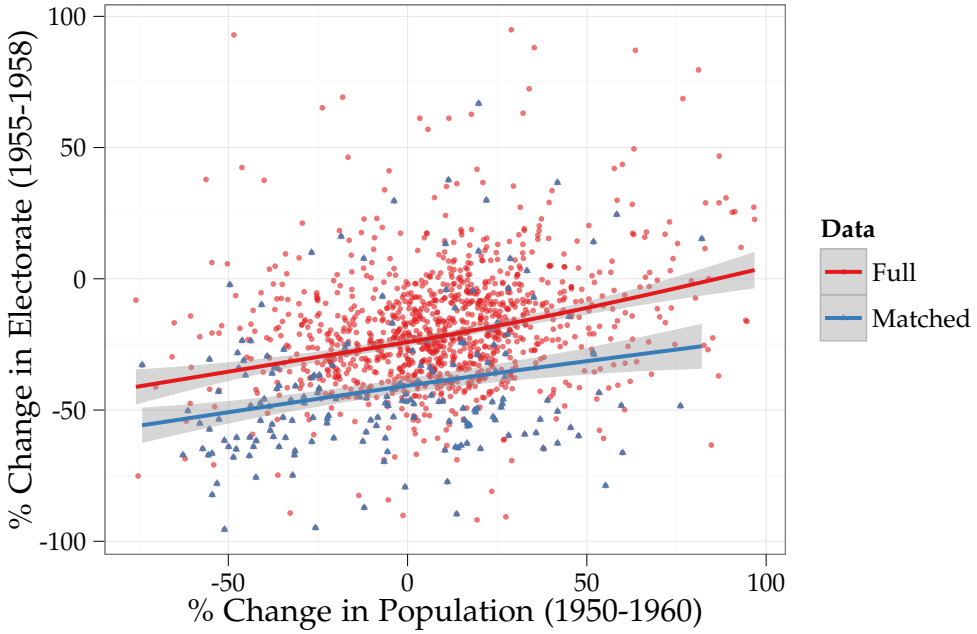


Figure 2.2: **Population and Electorate Change by Municipality.** In the full dataset (red circles), there is a positive correlation between population change and the change in the voter rolls induced by the 1955 reform. The red line is a loess estimate of the conditional expectation of electorate change as a function of population change. In the matched dataset (blue triangles), the relationship between the two variables (blue line) is largely attenuated.

The imbalance in population change is partiularly worrisome because differential trends in population growth is a potentially strong source of bias for treatment effect estimates. If the decline in turnout in the late 1950s and the decline in the voter rolls were jointly caused by the migration of voters from the countryside to the cities, then any correlation between changes in the size of the electorate and changes in turnout would be benign. In other words, the shrinking of the post-1955 electoral registry and declines in turnout could simply reflect demographic changes that had already occurred, not widespread fraud. The data, as evidenced by the red "loess" line in figure 2.2, show that population change in the full dataset is positively correlated with the changes in the voter rolls, albeit only moderately. As evident by red loess line, for most of the data, an expected change in the population is associated with a substantially larger change in

the electorate. Furthermore, the vast majority of municipalities that experienced population growth in the 1950s still lost registered voters after the reform. Given that most municipalities experienced population growth, but lost voters, it seems unlikely that any decrease in turnout would be driven by population change.

Nevertheless, the observed imbalance in popuation growth and other variables warrants covariate adjustment via matching to lessen the risk of bias. To deal with the continuous nature of the main causal variable, I split municipalities into treatment quintiles, with the municipalities in the quintile with the largest decrease in the electorate acting as the treated units, and municipalities in the four other quintiles acting as controls. I then used Genetic Matching (Sekhon 2011) to achieve covariate balance on a set of economic and political pre-treatment variables, with each treated unit matched to one control units. Both samples include basic demographic information from the 1940, 1950, and 1960[18] censuses and political data on the level of competition in city council elections and the results of the 1955 presidential election. The Pernambuco sample includes additional political variables and economic variables, particularly those related to land inequality, agricultural employment, and gubernatorial elections. The algorithm matches on a subset of available variables, but covariate balance is checked on a wide set of political and economic characteristics.[19] The genetic matching step, particularly for the national sample, produces good covariate balance on measured covariates (see 2.7). Population growth is largely uncorrelated with the treatment variable in the matched dataset, as evident by the blue loess line in Figure 2.2. Although the matched data is considerably smaller presumably estimates will be less threatened by bias.

In addition to covariate adjustment, I take advantage of the more comprehensive Pernambuco dataset, to implement a placebo test of the treatment effect on the turnout in elections *before* the 1955 reforms. If differential migration trends was truly a confounder, then decreases in turnout would be correlated with decreases in the electoral rolls in the early 1950s before the reform, as well as the late 1950s after the reform. In contrast, if

[18]The 1960 census data is post-treatment, but the only variables used from the 1960 census are population counts and measures of economic activity. These variables are used to construct interpolated measures that are used in either the difference-in-differences specifications that include covariates or as matching variables. It seems unlikely that the change in the voter rolls would have affected population change or economic activity in any meaningfully way, which would have made the variables inappropriate controls. It is reassuring, however, that the results below are robust to exclusion of these covariates.

[19]Because of the continuous nature of the treatment, I include in the matching procedure a requirement that control units are selected from the full range of the treatment variable. For the Northeast sample, the genetic matching algorithm minimized the maximum p-value from t-tests of equality of means and the Komolgorov-Smirnov tests of equality of distributions of the following covariates: log of population in 1950, log of population in 1940, total commercial revenues per capita in 1950, percent change in population between 1950 and 1960, percent change in population between 1940 and 1950, percent of population classified as rural, and sugar output per hectare in 1950. For the Pernambuco sample, the genetic matching procedure maximized balance on the following variables: vote for the conservative gubernatorial candidate in 1950, change in turnout between 1950 and 1952, percent change in population between 1940 and 1950, percent change in population between 1950 and 1960, the log of population in 1950, percentage of agricultural establishments that are rented, percent literate, and average sugar production per hectare.

the decline in the voter rolls is not attributable to changes in population, then the effect of the reform should only be evident after 1955. As shown in section 2.4, the placebo estimates is evidence that the treatment—particularly using the matched dataset—is not confounded by population change.

2.4 The Effect of Registration Reform

Case studies in the qualitative literature on politics in post-war Brazil suggests that registration fraud was an important means for traditional—and mostly rural—elite to control the outcome of elections. If so, the 1955 reform could have had the potentially large consequences for political dynamics in the late 1950s and early 1960s. Of course, an equally valid interpretation of the decrease in the electorate after the 1955 reform is that by updating the voter registry, election officials simply purged voters lists of deceased voters and non-residents. The fraud hypothesis would predict that a purging of the voter rolls and increased integrity at the polls would lead to a decrease in turnout. The main alternative and more inocuous hypothesis would predict no change or perhaps a small decrease due to the inconvenience of re-registering. In this section and relying on two datasets, I present evidence that the reduction in the voter rolls substantially reduced turnout, supporting the qualitative literature's contention that multiple voting or "ghost" voting played an important role in the political equilibrium of rural Brazil. First I present evidence from a larger, but less rich dataset of all municipalities in the Brazilian Northeast. Second, I use a more comprehensive dataset from the state of Pernambuco to better illustrate the political consequences of the reform.

Northeast Sample

The formal statistical estimates presented below are visible in the raw data depicted in Figure 2.3, which shows the scatter plot of the percent decrease in the size of the electoral rolls between 1955 and 1958 against the percent change in turnout between the 1955 and 1960 presidential elections. The relationship between the two variables is negative: a municipality with no change in size of the electoral rolls experienced about a four percentage point increase in turnout, while municipalities with 60 percent decrease in the size of the electorate experienced a fall of about two percentage points, indicating a causal effect of roughly six percentage points. Given that the median turnout in 1955 was 14.4 percent, a six percentage point effect would represent a very large effect.

The formal estimates of the effect of the reform presented in table 2.1 are consistent with the graphical evidence in figure 2.3. The point estimate from the simple difference-in-differences model (model 1) is -0.08 (95 percent confidence interval: [-0.09, -0.06]), which is similar in magnitude to the estimates from the model with covariates (model 2) and the model run on the matched dataset (model 3). The effect is large: going from the tenth to ninetieth percentile in the treatment variable, which amounts to a change from a

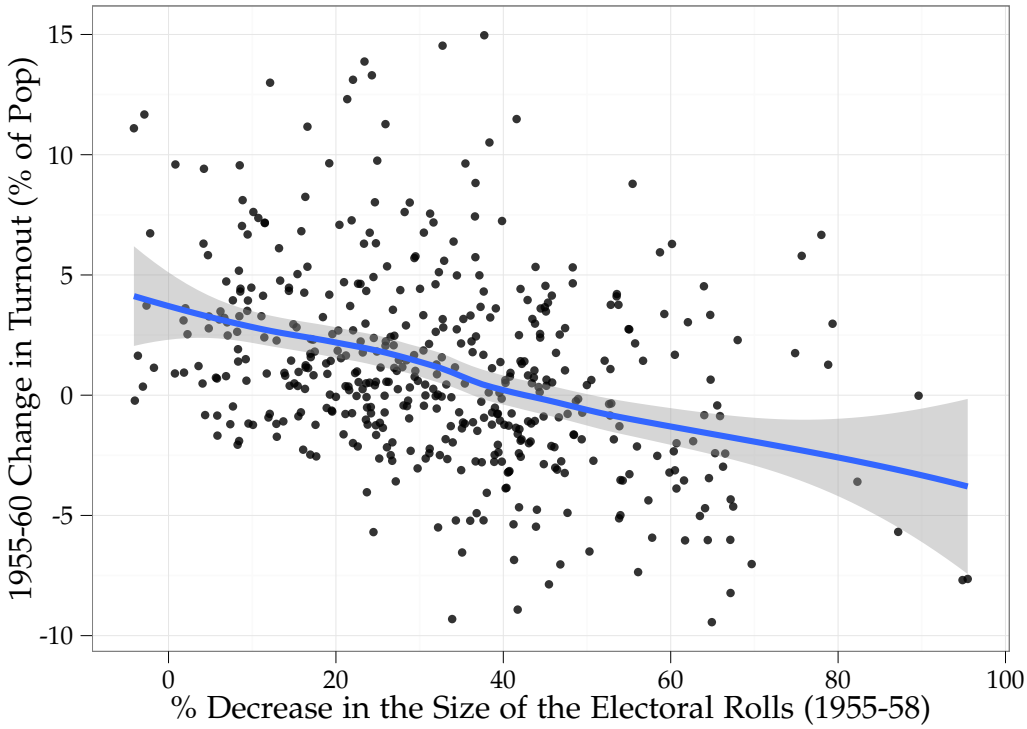


Figure 2.3: **The Relationship between Electorate and Turnout Change in Northeast Brazil.** This figure shows the strong negative relationship between decreases in the size of the voter rolls and change in turnout. Gray bands show 95 percent confidence interval.

4 percent increase in the voter rolls to a 56 percent decrease in the size of the electorate, would cause a change in turnout of approximately -5 percentage points, a large effect given that the median turnout in 1955 was about 14 percent of the 1950 population.

Did this reform have a real impact on Northeastern politics? To fully answer that question, a detailed state-level analysis would be required as is done for Pernambuco in the subsequent section. Still it is worth asking whether the reform could have meaningfully altered the political geography of the Northeast. The share of voters from rural Northeastern municipalities did fall from 68 percent to 64 percent between the 1955 and 1965 elections. Superficially, these figures suggest that the 1955 registry reform only modestly increased the weight of the urban vote. This inference, however, fails to account for the fact that despite the rapid growth of the cities during this period, the rural vote was growing more quickly than the urban vote prior the 1955 reform. Thus, in the absence of the reform, it would be quite likely that the weight of the rural electorate relative to the urban electorate would have been even more lopsided. To estimate the counterfactual distribution of voters in the 1960 election if there had been no electoral

	Model 1	Model 2	Model 3
% Electorate Decrease After Reform	-0.08^{***}	-0.07^{***}	-0.07^{***}
	(0.01)	(0.01)	(0.01)
After Reform	3.46^{***}	2.98^{***}	2.91^{***}
	(0.31)	(0.32)	(0.57)
Covariate Adjustment?	No	Yes	No
Matched Dataset?	No	No	Yes
Time periods	2	2	2
Municipalities	517	517	185

Bootstrapped errors in parentheses

† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 2.1: **Effect of the 1955 Electoral Registry Reform on Turnout in the Northeast of Brazil**. Dependent variable is turnout as a share of population. Treatment variable is the percent decrease in electorate due to the 1955 electoral registry reform. Regression coefficients are shown with blocked bootstrapped standard errors, where municipalities were resampled 200 times. Each regression is a difference-in-differences model using municipal-level data from two elections. Model 2 includes population, percent rural, sugar production, commercial revenues per capita, and employement per capita as covariates. Model 3 is run on a matched dataset, as described in the text.

reform, I use a modified version of the difference-in-differences model[20] to predict the turnout under the scenario in which the old voter registry had remained in place. Aggregating the predicted values across the Northeast leads to a prediction that in the absence of the reform, the rural vote would have accounted for 72 percent of all votes cast versus the 64 percent that were actually cast.

This 8 percent decrease in the rural share of the electorate could be substantively important for both national and state-level politics, as urban voters typically voted differently than voters in rural municipalities. For example in 1955, 43 percent of voters in Northeastern urban municipalities cast their ballots for the populist candidate, Juscelino Kubitchek, versus the 36 percent of the rural vote who did so. This urban-rural divide in the Brazilian electorate, combined with the typically competitive elections in the Northeast— the vote margins between the top two candidates in the 1955 and 1960 presidential elections were 1 percent and 4 percent, respectively—suggests that the 1955 reform could have had a large substantive impact on the outcome of elections. As is further discussed in the next section, the 1955 reform had substantial effects in Pernambuco and indirectly contributed to the increased polarization of the early 1960s.

[20] I augment the model by including interactions of the treatment variable with a rural dummy and population.

Pernambuco Sample

Historical Background

How did the 1955 electoral registry reform affect the trajectory of Pernambucan politics? The "disappearance"of hundreds of thousands of voters, I show, upset the prevailing political equilibrium in Pernambuco and resulted in the electoral victory, for the first time in state history, of a populist governor with an urban electoral base. Control of the state by a governor at the head of a viable populist coalition allowed for the emergence of open redistributive conflict in the countryside between agricultural workers and large landowners.

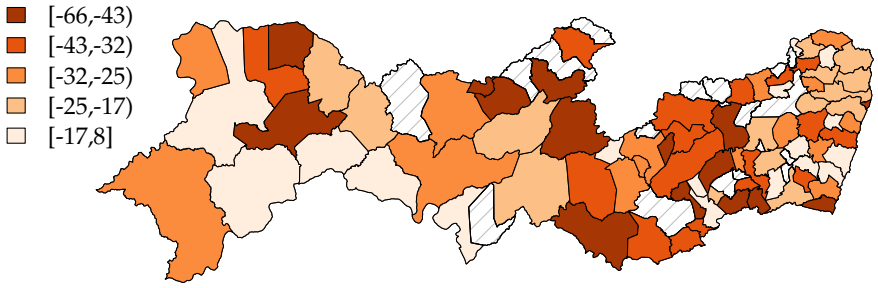


Figure 2.4: **The Effect of the 1955 Reform on the Size of the Electorate Across Pernambucan Municipalities.** Map depicts the extent to which electoral rolls decreased afer the 1955 reform in municipalities in Pernambuco. The darker the orange, the greater the decrease.

Like the rest of Brazil, the 1955 reform induced a sharp decrease in the total number of voters registered in Pernambuco: between 1955 and 1958, the voter registry fell from 836,176 voters to 594,003, a decrease of 29 percent. This aggregate figure, of course, masks substantial variation within Pernambuco. Figure 2.4 shows the spatial distribution of the change in the size of the electorate across 87 municipalities. The largest decrease was -66 percent of its 1955 electorate, while the median municipality lost -28 percent.

As visibile in figure 2.4, the biggest decreases in the electorate tended to be concentrated in the inland municipalities (a region known as the "Agreste") immediately

west of the state's more agriculturally important coastal region (known as the Zona da Mata" or "Forest Zone"). This inland zone, while less important economically than the sugar-producing coastal municipalities, was the geographic base of many of the rural political bosses (known as "coroneis") that were the electoral cornerstone of the conservative coalition that governed Pernambuco in the first decade after democratization (Lavareda 2008, 44). Gubernatorial and legislative candidates belonging to the PSD, the conservative ruling party between 1946 through 1958, relied on the large blocks of votes that their allied bosses could deliver in these interior municipalities. PSD party leaders negotiated directly with these local elites as they could deliver up to the 90 percent of votes cast in their municipality to PSD candidates of their choosing (Rêgo 2008, 131).[21] The ability of these elites to deliver overwhelming vote margins stemmed from their status as large landowners with many employees, as well as their use of coercion and fraud, particularly in the vote registration process (Rêgo 2008, 130; Mallon 1978, 54–55). That these bosses were particularly adept at taking advantage of the weakness of the electoral registration system is captured in the municipal election data.

The electoral support provided by the rural political bosses, both legitimate and fraudulent, was given in exchange for the state government's continued support of their local dominance. From the perspective of inland bosses, the most important policy concession on offer from the state government was the right to designate the local police and judiciary (Pandolfi 1984, 153). While the local elite desired a state government that shared their conservative ideological perspective, of greater importance was the maintenance of local control over the means of coercion. With control of these local government functions, political competitors could be kept out (Hagopian 2007, 49) and any peasant social movements could be suppressed, often violently.[22] The first governor of Pernambuco and a founder of the PSD, Barbosa Lima Sobrinho, described the political logic behind his party's alliance with the interior bosses thusly:

> "The main idea was to seek out those who could assure victory in the municipalities. Consider the case of Limoeiro in Pernambuco. We could care less about the ideas of Chico Herâclio [the local political boss]. What interested us was to know whether he had political prestige and a lot of votes. We offered him the support of the PSD and *the right to control the local authorities* once he had supported the party. Those who win elections are those who have the support of the political bosses with the most prestige" (Pandolfi 1984, 97).

As this quote illustrates, ensuring that the state coercive apparatus remained decentral-

[21] Albuquerque and Vilaça (1965), in their study of Pernambucan rural elite, have "smoking gun" evidence of such transactions: a photograph of a telegram sent by the governor to a boss requesting "all the votes" for a PSD candidate.

[22] The open coercion employed in the countryside was on vivid display in an American documentary filmed in Pernambuco in 1961, where a landowner bragged to the interviewer that "[m]y peasants are just lazy. If anyone comes here and tries to organize, I'll kill them".

ized was the chief goal of the rural bosses.[23]

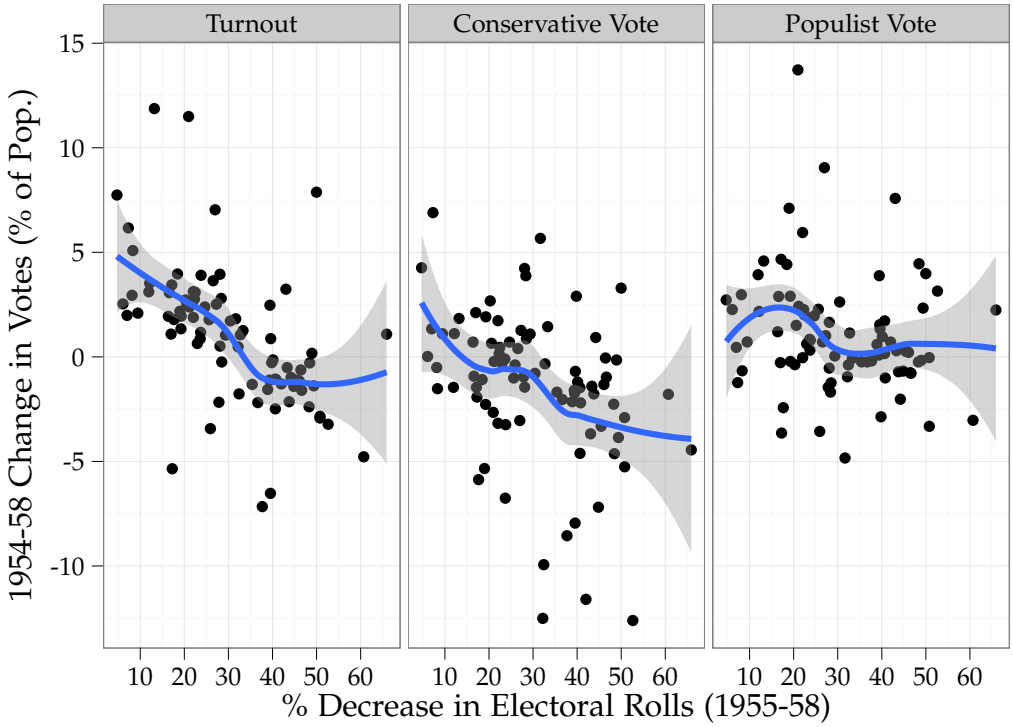


Figure 2.5: **The effect of the 1955 reform on the vote in Pernambuco.** The thick black line is a loess estimate of the mean of the dependent variable given different values of decreases in the electoral rolls. Gray bands are the associated 95 percent confidence intervals. There is a strong negative correlation between decreases in the electorate and turnout, as well as the vote for the conservative gubernatorial candidate. There is little or no correlation with the vote for the populist candidate.

Results

Prior to turning to the difference-in-differences estimates, it is useful to preview the results with graphical displays of the key correlations. Figure 2.5 shows the bivariate correlation between the percent change in the size of the electoral rolls (the treatment variable) and the change in the three main dependent variables between the 1954 and 1958 elections. The left-most panel shows a strongly negative correlation between decrease in the electoral rolls and turnout. The loess smoother (thick blue line) shows that,

[23] Indeed, one of the few times a large number of inland political chiefs broke with the PSD was prior to the 1954 elections when the governor sought centralize command of the military police (Rêgo 2008, 136).

on average, municipalities with small changes in the electoral rolls experienced much larger increases in turnout than those municipalities with larger decreases in the size of the electoral registry. The center and right panels suggests that the disappearing votes mostly hurt the conservative candidate for governor, as there is little or no correlation between the change in the electoral rolls and the votes for the populist candidate.

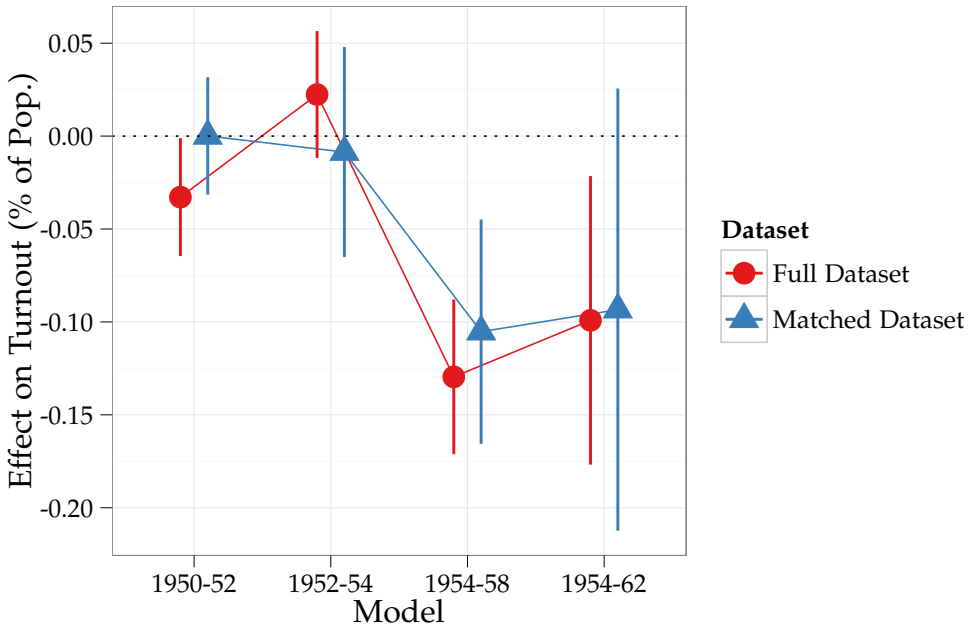


Figure 2.6: **Effect of the 1955 Electoral Registry Reform on Turnout in Pernambuco.** Dependent variable is turnout as a share of population. Treatment variable is the percent decrease in electorate due to the 1955 electoral registry reform. The 1950-52 and 1952-54 specications are placebo tests. The estimates using the full and matched dataset are shown, along associated 95 percent confidence intervals (vertical lines).

Effect on Turnout Table 2.2 and figure 2.6 present the difference-in-differences estimates of the effect of the 1955 registry reform on turnout, as measured as a fraction of the 1950 population.[24] Each column shows the parameter estimates from using data from two sets of elections, either using the full dataset or the smaller matched dataset.

The placebo specifications use pairs of elections that occurred before the 1955 reform, and thus the causal effect of the 1955 decrease in the size of the electorate is known to be zero. Using the full dataset, the parameter estimates are relatively close to zero and

[24]I drop Recife, the state capital, from the analysis given that its much higher population and level of industrialization make it fundamentally incomparable to other municipalities in the sample.

	1950-52	1950-52	1952-54	1952-54	1954-58	1954-58	1954-62	1954-62
% Electorate Decrease					-0.13^{***}	-0.11^{***}	-0.10^{*}	-0.09
					(0.02)	(0.03)	(0.04)	(0.06)
After Reform					4.88^{***}	3.93^{**}	5.58^{***}	5.33^{*}
					(0.68)	(1.25)	(1.36)	(2.62)
Placebo	-0.03^{*}	0.00	0.02	-0.01				
	(0.02)	(0.02)	(0.02)	(0.03)				
After Placebo Reform	-2.84^{***}	-4.01^{***}	4.71^{***}	-5.25^{***}				
	(0.57)	(0.66)	(0.55)	(1.45)				
Covariate Adjustment?	No	No	No	No	No	No	No	No
Matched Dataset?	No	Yes	No	Yes	No	Yes	No	Yes
Time periods	2	2	2	2	2	2	2	2
Municipalities	83	31	83	31	83	31	83	31

Bootstrapped errors in parentheses
† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 2.2: **Effect of the 1955 Electoral Registry Reform on Turnout in Pernambuco**. Dependent variable is turnout as a share of population. Treatment variable is the percent decrease in electorate due to the 1955 electoral registry reform. Regression coefficients are shown with blocked bootstrapped standard errors, where municipalities were resampled 200 times. Each regression is a difference-in-differences model using municipal-level data from two elections. Model 2 includes population, percent rural, sugar production, commercial revenues per capita, and employement per capita as covariates. Model 3 is run on a matched dataset, as described in the text.

negative: -0.03 (95 percent confidence interval: [-0.065, -0.001]) for the 1950-52 specification and 0.02 (95 percent confidence interval: [-0.012, 0.057]) for the 1952-54 specification. Troubling for the plausibility of the conditional independence assumption in this case, however, is the significance—albeit only at the 10 percent level—of the 1950-52 "effect". The matched dataset, however, lacks any of this imblance in the pre-treatment trajectory of the dependent variable. As is evident in figure 2.6, the parameter estimates for the placebo models are insignificant at the 5 percent level and very close to 0: 0 (95 percent confidence interval: [-0.032, 0.032]) for the 1950-52 specification and -0.01 (95 percent confidence interval: [-0.065, 0.048]) for the 1952-54 specification. Overall, these placebo tests suggest that inferences using the matched datasets are less likely to be prone to bias.

The right half of figure 2.6 and the four left-most columns of table 2.2 shows my estimates of the effect of the 1955 reform on turnout in the 1958 and 1962 elections. Using both the full and matched dataset, I find that a decrease in the electorate induced by the reform had a large negative effect on turnout in the subsequent elections. The estimates from both datasets are qualitatively similar, although the matched estimates are somewhat smaller and—due to the smaller n—less precisely estimated. According to estimates using the matched data, in the first post-reform election of 1958, Pernambucan municipalities experienced, on average, a decrease of -0.11 percentage points (voters as

share of 1950 population) (95 percent confidence interval: [-0.17, -0.04]) for every percent fall in the voter rolls. More concretely, going from the tenth to ninetieth percentile in the treatment variable, which amounts to a change from -9 percent to -48 percent, would cause a decrease of approximately -4 percentage points. This is a very large effect, given that the median turnout in the 1954 election was about 13 percent. The estimated effect from the matched dataset for the 1962 elections is -0.09 percentage points (95 percent confidence interval: [-0.22, -0.03]), which is somewhat smaller than the effect on 1958 turnout. For this year, going from the tenth to the ninetieth percentile in the treatment variable would cause a decline of -4 percentage points, still a large effect relative to turnout rates in the typical municipality.

	1950-52	1950-52	1952-54	1952-54	1954-58	1954-58	1954-62	1954-62
% Electorate Decrease					-0.09^{***}	-0.09^{*}	-0.08^{*}	-0.10
					(0.02)	(0.04)	(0.04)	(0.08)
After Reform					1.19	2.05	1.01	2.50
					(0.73)	(1.87)	(1.41)	(3.75)
Placebo	0.00	-0.02	0.03	0.04				
	(0.02)	(0.02)	(0.02)	(0.04)				
After Placebo Reform	$1.24^{\dagger}$	2.54^{*}	0.40	-0.20				
	(0.63)	(1.11)	(0.69)	(1.71)				
Covariate Adjustment?	No	No	No	No	No	No	No	No
Matched Dataset?	No	Yes	No	Yes	No	Yes	No	Yes
Time periods	2	2	2	2	2	2	2	2
Municipalities	83	31	83	31	83	31	83	31

Bootstrapped errors in parentheses
† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 2.3: **Effect of the 1955 Electoral Registry Reform on Conservative Gubernatorial Candidate**. Dependent variable is percentage of vote for conservative gubernatorial candidate as a percentage of population. Specification details are described in text and caption of table 2.2.

Effect on Vote Share Table 2.3 and Table 2.4 show the effect of the electoral registry reform on vote shares of the conservative candidate and the populist candidate, respectively. As before, the first four columns of each table show the results of placebo models using either the full or matched dataset that test for differential trends in the outcome before the reform occurred. There are no statistically significant placebo coefficients, with the partial exception of 1950-1952 change in the vote share of the populist candidate using the full dataset (first column of table 2.4), which is significant at the 10 percent level. The matched dataset, however, passes all the placebo tests.

The last four columns of Tables 2.3 and 2.4 suggest that the conservative gubernatorial candidate lost the most votes due to fictitious voters being purged from the rolls as a result of the reform. Both in the full and unmatched dataset, the candidate representing

	1950-52	1950-52	1952-54	1952-54	1954-58	1954-58	1954-62	1954-62
% Electorate Decrease					-0.04^{*}	0.01	-0.00	0.03
					(0.02)	(0.02)	(0.04)	(0.06)
After Reform					2.36^{**}	-0.15	0.55	-0.88
					(0.73)	(1.02)	(1.23)	(2.45)
Placebo	$-0.03^{\dagger}$	0.02	0.00	-0.04				
	(0.02)	(0.03)	(0.02)	(0.03)				
After Placebo Reform	-3.88^{***}	-6.23^{***}	4.75^{***}	6.44^{***}				
	(0.64)	(1.37)	(0.71)	(1.40)				
Covariate Adjustment?	No	No	No	No	No	No	No	No
Matched Dataset?	No	Yes	No	Yes	No	Yes	No	Yes
Time periods	2	2	2	2	2	2	2	2
Municipalities	83	31	83	31	83	31	83	31

Bootstrapped errors in parentheses
† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 2.4: **Effect of the 1955 Electoral Registry Reform on Populist Gubernatorial Candidate**. Dependent variable is percentage of vote for populist gubernatorial candidate as a percentage of population. Specification details are described in text and caption of table .

rural interests lost a substantial amount of votes in municipalities with larger decreases in the electorate. In comparison, the parameter estimates (table 2.4) for the populist candidates are unstable across specifications and years, and are all smaller (or even positive) than the conservative candidate estimates. In the 1958 election, for each percentage drop in the number of registered voters, the conservative gubernatorial candidate Jarbas Maranhão, on average, lost -0.09 percentage points (95 percent confidence interval: [-0.008, -0.181]) for each percent decrease in the voter rolls (matched dataset). The parameter estimate for the 1962 elections, is slightly larger: the conservative candidate João Cleofas lost about -0.1 ([0.068, -0.257]) for every percent decrease in the electoral registry. These are politically important effects: a change from a -9 percent decrease (tenth percentile) to -48 percent (ninetieth percentile) decrease in 1958 would induce a change of about -4 percentage points, which is large relative the median 6 percent vote share for the conservative candidate.

The 1955 reform appears to have cost populist gubernatorial candidates considerably fewer votes than the conservative candidate and possibly even won them votes. In 1958, the point estimate (matched dataset) is a statistically insignificant 0.01 percentage points (95 percent confidence interval: [0.057, -0.039]). In 1962, the coefficient is positive and statistically insignificant as well: 0.03 percentage points (95 percent confidence interval: [0.14, -0.086]). The parameter estimates are somewhat unstable across matched and unmatched datasets, but overall the data is consistent with the reform mainly hurting the conservative candidate and leaving the populist candidate basically unaffected.

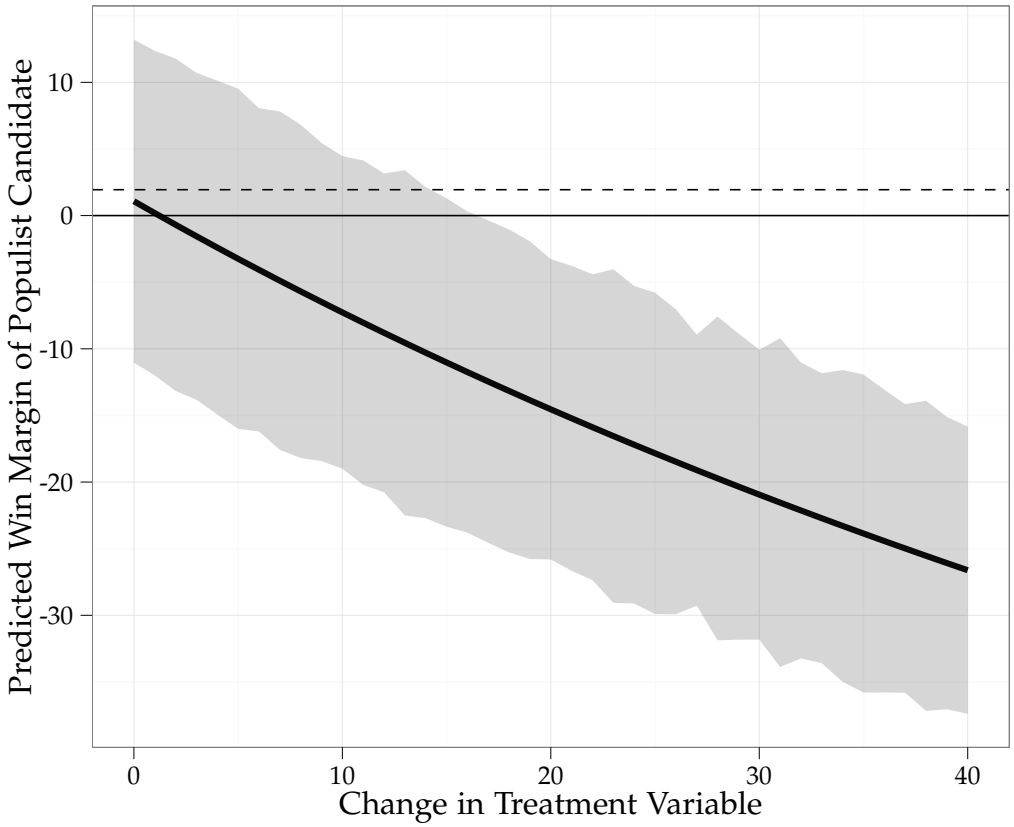


Figure 2.7: **Counterfactual Election Outcomes.** This graph plots the predicted win margin of the populist gubernatorial candidate in Pernambuco as a function of different values of the treatment variable. The x-axis is the is degree to which the decrease in the electorate is reduced by a constant shift. At the actual ("realized") value of the treatment variable , the populist candidate has a bare plurality. As the electorate is decreased by smaller amounts, the conservative candidate is predicted to win a pluarlity. Gray bands are 95 percent confidence intervals calculated using the bootstrap.

Counterfactual Election Outcome The 1955 voter registry reform suppressed turnout by purging "ghost voters" who had principally voted for the conservative, rural-oriented, candidate, but did the reform make a difference for election outcomes? To answer this question, at least tentatively, I simulated election outcomes in 1958 under different dosages of the treatment variable using the matched dataset. Specifically, I used the difference-in-differences model to produce predicted values for each municipality when a constant shift of 0 to 40 percentage points is subtracted from the decrease in the electorate actually induced by the reform. This shift preserves the variance of the treatment variable but shifts its mean towards 0. I can use this simulation to ask how much smaller would the decrease in the electorate have to be to result in a different election outcome.

The results of this simulation can be seen in figure 2.7. The dashed horizontal line is the realized aggregate vote margin between the populist and conservative candidates, which in this data was narrow populist plurality of only 2 percent. As the effects of the reform decrease due to a hypothesized constant shift in the value of the treatment variable, the predicted vote margin quickly swings in favor of the conservative candidate. The point where a positive conservative vote margin is statistically significant is at a constant shift of about 15 percent , which translates into a counterfactual mean decrease of about 22 percent in the size of the electorate, about half of the actual 37 percent mean decrease experienced by Pernambucan municipalities. In other words, an average decrease in the electorate of about half of what was actually experienced would have led to a different electoral outcome. This simulation suggest that in the matched data sample at least, the 1955 reform was decisive in the victory of the populist gubernatorial candidate over his conservative opponent.

Aftermath of the Reform

If the 1958 registration reform contributed to the first-time victory of an urban-oriented gubernatorial candidate not allied with traditional conservative rural elites, as suggested by the estimates presented above, then what were the consequences for trajectory of Pernambucan politics? As discussed in detail in the historiography of the state (Azevedo 1982; Barros Jaccoud 1990; Mallon 1978), the election of Cid Sampaio as governor initiated a period of popular mobilization by rural and urban workers. Under Sampaio, state police forces became less repressive towards protests, land invasions, and other forms of collective action which consequently increased the visibility and membership of peasant and worker groups. In the most concrete sign that the government's stance toward the countryside had changed, Sampaio's government began cautiously promoting the redistribution of agricultural land. The first-ever expropriation of a sugar plantation occurred in 1959 in direct response to a land invasion supported by the radical *Ligas Camponesas* (Peasant Leagues) (Mallon 1978, p. 55). This state intervention would have been unthinkable under previous governments.

Pernambucan politics further radicalized after the 1962 election of Miguel Arraes, former mayor of the state capitol. Unlike Sampaio, Arraes openly identified with the left and his election was viewed with great alarm by Pernambuco's traditional rural elite. This suspicion was only heightened when one of the first acts of the Arraes government was to order the police to not persecute rural workers involved in social movements. Arraes, early in his adminsitration, proclaimed: "[w]hat is certain is that the Secretariat of Public Security will not, in this Government, be an instrument of pressure, much less of oppression" (Rogers 2010, p. 137). The left-wing governor further challenged the local dominance of rural elites when he created a state bureaucracy charged with regulating and resolving rural conflict, removing this responsibility from the hands of the police, who often were under the sway of local politicians (J. Soares 1982, p. 91). Not only did he reduce rural elites' capacity to suppress peasant and worker organization, but the

new governor also directly attacked the economic interests of the agricultural sector by moving to enforce the minimum wage in the countryside, something previously never attempted by previous governors (J. Soares 1982, p. 94).

The activation of the peasantry in rural Pernambuco had national and international reverberations. To national conservative elite, the radicalization of Pernambucan politics was further evidence of the risks democracy posed for their interests and the spectre of rural conflict spreading to other states played a prominent role in the military's justification for toppling the civilian government in 1964. An editorial in the prominent conservative São Paulo newspaper on the conciliatory response of a newly elected populist government to the growth of peasant based social movements in Pernambuco captures the flavor of elite rhetoric:

> The movement grows larger and larger, affecting the working classes in the cities, with the invasion of offices, the occupations of factories, assaults on homes, and the vandalism of banks and other commercial centers. It is a revolution. In its blindess, the government of Pernambuco started a revolution (O Estado de São Paulo 1960).

Pernambucan politics also drew international attention as a possible breeding ground for a Cuba-style revolution. A front page article on the conflict in Pernambuco in the *The New York Times* entitled "Northeast Brazil Poverty Breeds Threat of a Revolt" warned that "the makings of a revolutionary situation are increasingly apparent" and argued that poverty was "exploited by rising Leftist influences", particularly in the "Communist-infiltrated Peasant Leagues" (Szulc 1960b). A similarly alarmist article on Pernambuco entitled "Marxists are Organizing Peasants in Brazil: Leftist League Aims at a Political Army 40 Million Strong" was published the same year (Szulc 1960a). The alarm provoked in part by the conflict in Pernambuco also likely played a role in the US government's decision to covertly invest millions of dollars into influencing the 1962 congressional elections.

2.5 Conclusion

Rapid economic change often unleashes forces that challenge the position of political elites, but these elites, of course, are not passive observers of the structural transformations that accompany economic development. Political elites may recognize the potential for economic change to affect politics and seek to forestall such changes via institutional engineering. Students of constitution-making have long recognized that the rules governing elections and representations are often crafted with the goal of preserving the political power of elites who stand to lose from ongoing demographic and economic change. Legislative malapportionment that favors rural constituencies during periods of urbanization is perhaps the most common example of such institutional engineering. Less studied are *informal* practices that can have similar consequences. In this chapter, I

identified a form of electoral fraud that takes advantage of urban-rural differences in the capacity of the state to accurately count and categorize the eligible electorate through the voter registration process. Rural elites can take advantage of their comparative advantage in manipulation of the voter registry to fraudulently inflate the rural vote and thus blunt the impact of urbanization on electoral politics.

I exploit an episode of electoral reform in post-war Brazil to explore the consequences of this phenomenon. As I document in this chapter, the 1958 re-registration of the entire electorate resulted in a sudden and sharp increase in the urban electorate and this had political consequences. Focusing on the politically-important state of Pernambuco, I present evidence that the reform resulted in the electoral loss of a gubernatorial candidate backed by the rural elite for the first time. While the consequences of the reform in Pernambuco were particularly dramatic, it is likely that the disappearance of a large block of rural votes affected the political equilibrium in other states as well. An important open question is the degree to which the 1956 electoral reform ultimately contributed to the political polarization of the early 1960s throughout Brazil. The results from this chapter suggest that the increase in the electoral weight of cities strengthened populist parties and consequently heightened the threat posed by electoral politics to the power of traditional elites. But more empirical work is required to assess the claim of broader effects beyond Pernambuco and the Northeast region.

The informal practice identified in this chapter is a specific type of fraud that occurs at the intersection of state weakness and electoral democracy. While most research on electoral fraud emphasizes acts that take place on election day, the electoral fraud I discuss requires subversion of a basic responsibility of a state in a democracy: the counting and categorization of the populace. In some respects, this form of fraud is particularly difficult to combat. Election-day fraud can often be reduced through technical reforms or improved voting technology. Combatting registration fraud, however, may in some instances require more fundamental reforms, such as changes to how the state is staffed, expanding the degree to which the state is present throughout the national territory, and the insulation of the bureaucracy from politicians. Needless to say, these types of structural reforms to the state apparatus represent a quite difficult political and administrative challenge.

This research also suggests a mechanism by which the timing of democratization can matter for the quality of future governance. Whether democratization occurs before or after the emergence of a competent Weberian administrative bureaucracy is a key variable in debates over the origins of states that are both democratic and bureaucratically effective. As the "election norm" has taken hold throughout most of the world, critics (P. Collier 2010) have argued that holding elections before the existence of an effective state capable of fully exercising its authority can in fact be counter-productive because open political competition can generate incentives for politicians to weaken the state even further. The phenomenon in this study is perhaps one such mechanism. Rural elites taking advantage of weaker state monitoring in the countryside to commit fraud and win elections will hardly be supportive of state-enhancing reforms. Given this ad-

vantage, it is unlikely that political parties which depend on fraudulent votes would seek to support policies that insulate the bureaucracy from political pressure when creating and maintaining the electoral registry. In the context of transitions to democracy, this argument would suggest that in order to forestall a state-weakening political dynamic, reformers should ensure that the procedures governing voter registration are resistant to manipulation *before* the first elections are held.

2.6 Appendix: Data

Data Sources The data used to estimate the effects of the 1955 reform were constructed from a variety of historical sources. The municipal-level measures of the number of registered voters and presidential election results were found in archival documents provided by the Supreme Electoral Tribunal ("Tribunal Superior Eleitoral"). For the Pernambuco dataset, the municipal level political variables were found in the official register of the state government, which published disaggregated electoral results in the weeks after each election. A small set of demographic variables from the 1940, 1950 and 1960 censuses (commercial revenues, population, number and area of farms, sugar production) were obtained in digital format from Institute of Applied Economic Research ("Instituto de Pesquisa Econômica Aplicada", `http:///www.ipeadata.gov.br`). More detailed data on employment and land inequality for the state of Pernambuco were obtained from original 1950 and 1960 census reports (IBGE 1954, 1967).

Municipality Boundary Changes One difficulty with working with Brazilian municipality data from this period is that the number of municipalities increased substantially over time. This growth in the number of municipalities complicates the definition of the unit of analysis and construction of a panel dataset because many municipalities in 1960 used different boundaries than the similarly named municipalities in earlier years. To adjust for this problem, I used the Brazilian Institute of Geography and Statistics ("Instituto Brasileiro de Geografia e Estatística") municipality histories (`http://biblioteca.ibge.gov.br/`) to track the evolution of all municipalities between 1950 and 1962. Using this historical data, I then aggregated all data to the 1950 municipal boundaries.

Rural vs Urban To classify municipalities as urban or rural, I relied on IBGE (2008). This study used a detailed government survey of demographic and economic information to create a categorization scheme of Brazil's major urban centers and their economic satellites for regular intervals beginning in 1968. Population centers were classified as "national metropolitan centers" (i.e. São Paulo, Rio de Janeiro), "regional metropolitan centers" (i.e. Belo Horizonte), "macro-regional centers" (i.e. Curitiba), "regional centers", "subregional centers", and "local centers". I defined a municipality as "urban" if it belonged to any of these categories. An alternative method would be to use the government's official statistics on the rural population in each municipality as measured by the census. These statistics, as documented in Veiga (2008, 31-34) however, vastly over-estimate the urban population because they categorize any person living in the municipality seat as an urban resident. In about 40 percent of municipalities, however, the municipality seat would not be classified as urban using traditional economic and demographic criteria. This unusual definition of "urban" stems from a 1938 law that

was never revised despite heavy criticism by demographers. As a result, I rely on the alternate categorization scheme.

Variable	n	Min	$\tilde{x}$	$\bar{x}$	Max	s	#NA
Commercial Revenues per Cap	1086	0.0	0.1	0.2	3.8	0.3	54
Employed in Commerce per Cap	1034	0.0	0.0	0.0	0.0	0.0	106
Population	1086	1622.0	24115.0	31711.8	797234.0	45428.4	54
Rural Municipality	1140	0.0	1.0	0.9	1.0	0.3	0
Sugar Prodcution (Tons per Hectare)	1086	0.0	0.6	63.7	1856.4	211.3	54

Table 2.5: Descriptive statistics for the Northeast municipalities panel sample.

Summary Statistics Descriptive statistcs for the Northeast municipality panel are in table 2.5. The dataset includes 570 municipalities and 2 election years.

Variable	n	Min	$\tilde{x}$	$\bar{x}$	Max	s	#NA
Commerce Revenues Per Cap	172	0.0	0.1	0.2	3.8	0.4	258
Employed in Commerce Per Cap	172	0.0	0.0	0.0	0.0	0.0	258
Population	172	8.9	10.3	10.3	13.6	0.6	258
Sugar Production (Tons per Hectare)	172	0.0	4.8	231.9	1856.4	411.7	258

Table 2.6: Descriptive statistics for the Pernambuco municipalities panel dataset.

Descriptive statistcs for the Pernambuco municipality panel are in table 2.6. The dataset includes 86 municipalities and 5 election years.

2.7 Appendix: Covariate Balance

Figure 2.8 presents balance statistics comparing Northeastern municipalities in the top quintile of the percent decrease in the size of the electorate ("treatment") to those in the bottom four quintiles ('"control") before and after matching. The plot shows p-values from t-tests of equality of means and Kolmogorov-Smirnov tests of equality of distributions. Treatment and control means are from the matched dataset.

Figure 2.9 presents balance statistics comparing Pernambucan municipalities in the top quantile of the decrease in the electoral registry due to the 1955 reform to those in the bottom five quantiles before and after matching.

Figure 2.8: Balance statistics before ("BM") and after ("AM") matching for the Northeast municipality sample. Means are post-matching values.

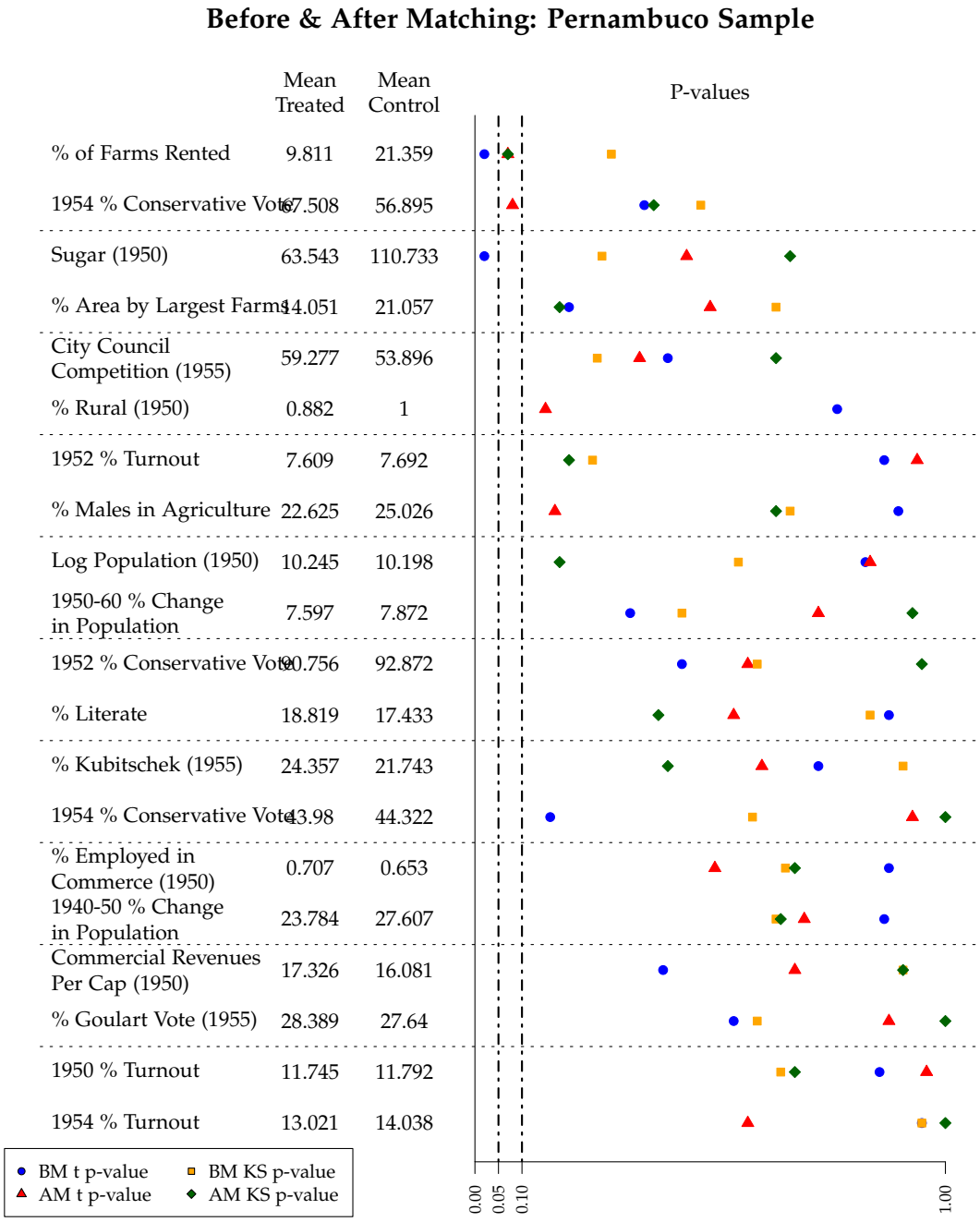


Figure 2.9: Balance statistics before ("BM") and after ("AM") matching for the Pernambuco municipality sample. Means are post-matching values.

Chapter 3

When Do Voters Punish Corrupt Politicians? Experimental Evidence from Brazil[1]

[1]This chapter was coauthored with Miguel de Figueiredo and Yuri Kasahara.

3.1 Introduction

When do voters punish corrupt politicians? The question has important implications for institutions intended to keep politicians accountable. Electoral accountability is often perceived to be an important means of reducing incentives of politicians to engage in corruption. While there is a vast literature about the consequences of corruption (Johnston 1986; Mauro 1995; Olken 2007), the literature focused on the causes of non-corrupt governments is still nascent. As Adserà, Boix and Payne (2003, pg. 446) succinctly state: "[i]n contrast to the mounting scholarly research on the consequences of good governance, our knowledge about what causes governments to be clean and efficient is still at its infancy." One precondition for electoral accountability is sufficient knowledge by the citizenry of politicians' records. When voters are informed about accusations of corruption, most assume that voters will punish the corrupt candidates. This study shows that information about candidate corruption given to voters can indeed result in the politician being punished by voters, but that some candidates are more accountable to voters when it comes to corruption than others. The degree to which voters view corruption as important to their decision-making can vary substantially and, furthermore, can be correlated with political cleavages. As a result, we show that the increased transparency can have divergent partisan consequences, even when two competing candidates are corrupt. Previous studies that merely treat corruption as a valence issue are likely to overlook this important dimension of the effects that corruption information can have on the electorate.

We find that when voters view corruption as important, then the increased provision of information can induce supporters of the corrupt candidate to abstain. Our results establish that transparency can suppress turnout even in the presence of mandatory voting, demonstrating that in some cases, voters are willing to bear costs not to vote. Additionally, despite a number of papers that have shown that corruption is not a salient issue in the consciousness of many voters in the developing world (Anderson and Tverdova 2003; Chang and Golden 2004; Rennó 2007; Almeida 2008), we find, under certain conditions, it still remains an important determinant of voting behavior.[2] While a host of non-governmental organizations (NGOs), international organizations, and governments have initiated various efforts to increase transparency and government accountability in elections, few have analyzed the impact of these initiatives in terms of their effect on electoral behavior in a manner that allows one to make valid causal inferences. Our study presents a step toward accomplishing such a goal, shedding light on the conditions under which corruption may or may not be subject to voter sanction.

In this study, we conduct a field experiment during the 2008 mayoral run-off election in São Paulo, Brazil, the seventh largest city in the world. In our study, to our knowledge the first field experiment involving elections in Latin America, we exploit the fact

[2]Winters and Weitz-Shapiro (2010), in a nationally representative survey experiment in Brazil, similarly find that voters tend to reject corrupt politicians when information about the corruption is delivered in a specific, credible, and accessible manner.

that both candidates in the run-off election had been convicted of corruption and inform selected voters of these convictions via the distribution of fliers. We randomly assigned whether or not households in the vicinity of a given polling station receive fliers containing the information. The experimental design allows us to make inferences with a high degree of internal validity about the effect of information on voting behavior, and unlike previous studies, we are able to examine the effects not only of the incumbent, but also of the challenger.

Specifically, we take advantage of a unique set of events that took place during the election period. The Brazilian Magistrates Association (*Associação dos Magistrados Brasileiros*, or AMB) published a document called the "Dirty List" (*Lista Suja*), which listed politicians running in the 2008 elections who had convictions involving impropriety while in government office. Both candidates running in the election for mayor of São Paulo – Gilberto Kassab of the Democratic Party (DEM) and Marta Suplicy of the Worker's Party (PT) – appeared on the AMB's Dirty List. During the week prior to the elections, we administered two treatments: the first was a flier informing voters that Kassab appeared on the Dirty List and gave information about the nature of his conviction, and the second was a flier that did the same for Suplicy. We then randomly assigned voting precincts that would receive the Kassab or Suplicy flier, and also had a control group of precincts that did not receive the flier. In all, households in the vicinity of 100 precincts received the Kassab flier, another set of households in the vicinity of 100 precincts received the Suplicy flier, and 200 precincts were in the control group. In the week prior to the election, we hired a direct marketing firm that distributed a total of 187,177 fliers to individual households. To measure the effect of the intervention, we examined electoral outcomes.[3]

Our results varied by individual candidate. The Kassab flier had no effect on vote choice, number of spoiled ballots or on turnout. The Suplicy flier, by contrast, moved votes on average relative to the control group by 2.6 percentage points, had no effect on spoiled ballots, and a negative 1.8-1.9 percentage point average treatment effect on voter turnout. The turnout results are particularly surprising given that Brazil has mandatory voting. We believe the results of our study suggest limits to theories positing that more informed voters are more likely to turn out (Wolfinger and Rosenstone 1980; Palfrey and Poole 1987; Feddersen and Pesendorfer 1996).

To explain these divergent effects measured using aggregate vote returns in our field experiment, we rely on individual-level data from a survey and an embedded experiment that we fielded the week after the election. We find that Suplicy's and Kassab's voters do not differ in intensity of support, ex-ante knowledge about the corruption accusations, or the degree to which they view the accusations as serious. We do find, however, that Suplicy's voters place much greater importance on corruption when evaluating candidates than do Kassab's supporters. Not only do Suplicy supporters claim that a candidate's record on corruption is important to them at higher rate than Kassab vot-

[3]We obtained data from the São Paulo Regional Electoral Tribunal (*Tribunal Regional Eleitoral*, or TRE).

ers, but we also find that they are much more sensitive to corruption accusations in our survey experiment. Kassab supporters do not change their evaluation of Kassab when they learn about his placement on the Dirty List. Suplicy voters, however, do judge their favored candidate more negatively upon learning about her record, which is consistent with our field experiment findings. Overall, these contrasting results place important scope conditions on when information campaigns are likely to increase accountability through the democratic process.

3.2 Corruption Information and Voting Behavior

An important precondition for electoral accountability is whether or not voters have access to information about corrupt behavior of public officials, which may prompt them to vote against such candidates on election day. A theoretical literature focused on the effects of information on voting behavior concludes that under certain conditions, information improves accountability to mass publics (Alvarez 1998; Lupia and McCubbins 1998; Przeworski, Stokes, and Manin 1999; Besley and Burgess 2002). However, the empirical literature is still relatively scant on understanding the conditions under which information about corruption results in electoral accountability.[4] Making valid causal inferences about information effects is difficult, in large part because availability of information about the corruption of politicians is confounded by factors such as socioeconomic status and partisanship. A number of studies with non-experimental data that attempt to examine the effects of corruption charges on electoral performance find only modest effects (Peters and Welch 1980; McCann and Dominguez 1998).[5] In a study of municipal governments in Brazil, Ferraz and Finan (2008), exploiting randomized corruption audits, find relatively large effects that ultimately decrease the probability of incumbent politicians being reelected. Their important work, which examines the effects municipal-level corruption audits, however, does not include candidate-specific treatments, and their intervention also only involves incumbent politicians.

Field experiments that examine the effects of corruption on voting behavior have only recently emerged in the literature. To the best of our knowledge, our experiment conducted in October 2008; Banerjee, Green, et al. (2010)'s and Banerjee, Kumar, et al. (2010)'s studies in India, conducted in March-April 2007 and December 2008, respectively; and Chong et al. (2011)'s work, conducted in Mexico in June and October 2009, are among the first field experiments that attempt to randomize informing voters about politicians' performance in order to examine the effects on voting behavior. Focusing

[4]Notable exceptions include Adserà, Boix, and Payne (2003); Chang (2005); Reinikka and Svensson (2005); Olken (2007); Ferraz and Finan (2008); Banerjee, Green, et al. (2010); Banerjee, Kumar, et al. (2010), and Chong et al. (2011).

[5]One observational study that is an exception is Pereira, Melo, and Figueiredo (2009), which finds large negative effects on the probability of reelection when examining the effect of state corruption audits in the state of Pernambuco, located in northeast Brazil.

on the consequences of transparency for *incumbent* electoral performance, these other studies have generally found that revelations about corruption have negligible effects on incumbent vote share relative to challengers' vote share. The effects on turnout have been more mixed. The Banerjee, Kumar, et al. (2010) study found that distribution of information about the criminal records of New Delhi politicians increased turnout by about 3.6 percentage points. Chong et al. (2011), on the other hand, found a negative effect of 4 percentage points when voters are given information about corruption from government corruption audits in municipalities with highly corrupt incumbents.

Revealing the corruption record of a candidate could be loosely viewed as a negative attack, even if it does not come from the opposing campaign. From this perspective, a relevant body of work is the negative campaign advertising literature, which focuses overwhelmingly on the US context. This largely observational empirical literature has been inconclusive on the consequences of negative attack ads for candidate electoral performance (Lau, Sigelman, and Rovner 2007), though no studies have focused on advertisements that emphasize corruption. With respect to political participation, Ansolabehere and Iyengar (1995), initially relying on laboratory experiments, argued that negative advertising demobilizes the electorate. One of the mechanisms they cite is particularly relevant: negative campaigns could lower the probability of voting for the target of the attacks without simultaneously increasing the probability of voting for the attacking candidate, prompting voters to simply abstain. Thus, while not necessarily provoking vote switching, negative campaign advertising could still punish targeted candidates via decreased turnout of their supporters. The observational empirical evidence for this contention, however, has been mixed (Finkel and Greer 1998) and has not been tested experimentally by examining actual turnout behavior.

The contrasting results of these studies motivate important questions about the mechanisms that explain variation in the voting behavior in these different contexts. Whether or not a voter actually changes his behavior due to the revelation of information will depend on a number of factors, but a useful framework for explaining heterogenous effects is the spatial model of elections (Enelow and Hinich 1984). The spatial model underscores the important mechanism of how political factors such as ideological attachments can mediate the effect of corruption information in the minds of voters. Voters receiving a large amount of subjective utility from a particular candidate's victory ("core" supporters) will be unlikely to change their vote or abstain unless the corruption charge is particularly serious or they place a high degree of importance on corruption in their vote decision. For more marginal ("swing") supporters, however, revelation about corruption is more likely to push voters to either abstain, cast a spoiled ballot, or switch their vote to the opposing party. When the costs of voting relative to abstaining are high, then marginal supporters who learn about corruption charges are particularly likely to abstain. Following a similar logic, learning about corruption could mobilize non-voters to vote for the opposition, as the difference in utility between abstaining and voting for the opposition would decrease after the information revelation.

Under this spatial framework, one would expect that the effects of experimental in-

terventions revealing information about corruption would depend upon the distribution of core and swing voters in the electorate, as well as the importance supporters and non-voters place on corruption.[6] If the proportion of marginal supporters is large, then one would expect that the revelation of corruption information would induce abstention (if the costs of voting relative to abstention are high) or casting spoiled ballots (if the costs of voting relative to abstention are low) or even vote switching. Casting spoiled ballots is a particularly attractive strategy for disappointed marginal voters when fines for abstention are substantial in countries with mandatory voting. Similarly, if voters place a high degree of importance on corruption as an issue, then increased transparency could induce many core supporters to abstain, spoil their ballot, or change their vote. On the other hand, if there are many abstainers with sympathies for the opposition party, then increasing information about corruption could induce these citizens to vote on election day and thus increase total turnout. The key point is that the aggregate effect of information revelation on turnout and vote shares will depend on the ex-ante distribution of voters' preferences, the relative importance of corruption in voters' decision-making, and the cost of abstention relative to voting.

This discussion suggests that the effects of corruption revelation could vary by candidate, even if both are equally corrupt. If two candidates accused of corruption compete against each other, as is the case in our study, the impact of information revelation about each candidate's record could vary substantially because of any one of these factors. We offer evidence that partisan attachments can overlap with corruption preferences – a factor the extant literature on corruption overlooks – are an important mediating variable that shape whether voters punish corrupt politicians. Of course, while we have emphasized factors linked to the distribution of voter preferences, other variables such as candidate skill and background could shape voters' reaction to increased information. The existing experimental literature has largely ignored heterogeneity across different types of candidates, despite the fact it is quite likely that candidates' susceptibility to increased transparency is likely to depend on the contextual factors we have highlighted. In the following analysis, we pay particular attention how candidates' supporters differ and how these differences affect their response to increased information.

3.3 The Brazilian Electoral Context

São Paulo's 2008 Municipal Elections and the AMB's *Lista Suja*

On October 26, 2008, Kassab and Suplicy ran against each other in the run-off election for mayor of São Paulo. Kassab, the incumbent mayor, assumed the position in 2006

[6] Another important background condition that could explain divergent effects is pre-existing perceptions of the candidate's propensity to engage in corruption. If voters already believe that a given candidate is corrupt or know about the specific allegations, then increased transparency is unlikely to affect behavior since voters' decisions have already incorporated this information.

upon the resignation of José Serra, who became governor of the state of São Paulo and belonged to the Brazilian Social Democratic Party (PSDB or *Partido da Social Democracia Brasileira*). Kassab's Democratic Party is a center-right party that formerly was the PFL or *Partido da Frente Liberal*, one of parties that splintered from ARENA, the official party of the military regime that held power in Brazil from 1964 until 1985. Suplicy, who was mayor of São Paulo from 2001 until 2004, served as the Minister of Tourism in the federal government for a year starting in 2007, before resigning to run for mayor. At the time of the election, President Luiz Inácio da Silva (Lula), a co-partisan of Suplicy, enjoyed widespread popularity; however, other PT candidates did not maintain the same level of support.

Suplicy's party, the PT, was traditionally associated with both leftist ideology and clean and participatory governance (D. J. Samuels 2004). While the party moderated its ideological positions over time, the PT deliberately cultivated its brand as a party with a more ethical mode of governance and its leaders stressed the need for broader participation of the citizenry and civil society in policymaking (Hunter 2010, pg. 84). The PT heavily criticized Brazil's other major parties as corrupt and clientelistic and at least some of PT candidates' success could be attributed to the fact that they developed an image of promoting transparency in government. The São Paulo branch of the PT contributed to the creation of this brand in the early 1990s when Suplicy's former husband, Eduardo Suplicy, a federal senator, spearheaded corruption investigations against numerous municipal officials, including four past presidents of the city council (pg. 85). While the PT's reputation for clean government has been tarnished in recent years by national scandals involving bribery of legislators (including the recent 2005 *Mensalão* scandal) and illegal campaign finance, PT candidates still stress anti-corruption themes in their campaigns.

The brand of Kassab's party, the Democratic Party (DEM, formerly known as the PFL), was less distinctive than the PT's. Nominally a center-right party, the DEM was particularly strong in the poorer states in the Brazilian Northeast and its major leaders were frequently associated with extensive use of patronage while in office. The party did not have a notable anti-corruption record, given its image of being composed of "traditional" politicians with more particularistic styles of governance. One major blemish on its national record related to corruption was the party's strong support for the failed presidency of Fernando Collor de Mello, who resigned in the wake of an influence peddling scandal involving one of his main advisors. In São Paulo, the local DEM party supported the administration of Paulo Maluf, a two-time mayor of the city who was later convicted of corruption charges involving illegal government contracts, and is associated with the phrase *rouba mas faz* (he robs, but he gets things done). Perhaps because of its past record, Kassab did not tend to emphasize his party in his campaign appeals and instead stressed his technocratic credentials and experience in government.

The AMB, the main professional association for Brazilian judges, established the Dirty List in order to publicize the corruption proceedings of candidates seeking political office. The Dirty List has generated controversy in Brazil, in terms of the criteria

that one must meet to be on it and for selectively ignoring proceedings against politicians (Barros de Mello and Bragon 2008). For example, the AMB's decision to include candidates that have been absolved by a court drew criticism from a number of judges and legal scholars. Gilmar Mendes, the president of the Federal Supreme Court (*Supremo Tribunal Federal*, or STF), for instance, declared the Dirty List as populist and politicized (D'Agostino 2009).

The AMB included Kassab on the Dirty List because a court convicted him of "administrative impropriety" in 1997. At the time, Kassab served as the Secretary of Planning for the City of São Paulo. The case, launched by public prosecutors in São Paulo, accused Celso Pitta, mayor at the time, and his staff, which included Kassab, of taking out an advertisement paid for with municipal funds in which they allegedly defended their own "personal interests" in newspapers while they were under investigation. A lower court held that Kassab was guilty, but the decision was overturned on appeal. The public prosecutor appealed this decision, but it had yet to be resolved at the time of the election. Despite objections from the Kassab campaign, the AMB kept him on the Dirty List.

Suplicy's conviction was based on more serious charges. In 2005, a São Paulo court convicted her of inappropriately giving a R$2 million (approximately US$840,000 at the time of the election) no-bid contract to the Sexual Orientation Research Work Group (*Grupo de Trabalho e Pesquisa em Orientação Sexual*, or GTPOS), an NGO focused on advocacy for and increasing awareness of sexual orientation issues. The municipality awarded the contract to GTPOS to train São Paulo school teachers in issues pertaining to sexual orientation. Suplicy founded the NGO in 1990 and served as its honorary chairman until 2000 (MercoPress 2005). At the time of the election, the decision was under appeal.

Corruption featured prominently in the campaign, as both candidates accused each other of engaging in improprieties while in elected office, particularly after the first round in which Kassab won with 34 percent of the vote versus Suplicy's share of 33 percent. Suplicy's campaign in particular emphasized corruption. In one Suplicy television advertisement, for example, Kassab was accused of using public funds for electoral purposes. Suplicy went so far as to petition the election authorities to make Kassab ineligible for re-election because of alleged politicized distribution of public benefits. Kassab's attacks tended to focus on Suplicy's record on public works when she was mayor between 2000 and 2004, but his campaign also raised corruption as an issue. In fact, Kassab's campaign, early in the election period before he was placed on the Dirty List, attacked Suplicy for being declared as "dirty" by the AMB. Despite the closeness of the first round, polls showed Kassab with a consistent lead throughout the second-round campaign and he subsequently won with a decisive 60 percent of the vote.

Mandatory Voting In addition to the context-specific factors that took place during the 2008 municipal elections, mandatory voting also plays an important role in the electoral

behavior of Brazilian voters. Although Brazil maintains a system of mandatory voting, absenteeism rates in recent elections have hovered around 15 to 20 percent nationally.[7] Specifically, citizens are required to vote from age 18 to 70, with some exceptions. Voting is voluntary from ages 16 to 18, and for those 70 or older.

Those who fail to vote without justifying their absence within 60 days are required to pay a small fine ranging from R$1.05 to R$3.51 (approximately US$0.44 to US$1.47). Non-pecuniary costs of absenteeism borne by the voter include the time involved in a three-step process to pay a fine in which the voter typically must: (1) go to the local electoral notary (*cartorio eleitoral*) and obtain a paper stating they are fined, (2) go to a bank to pay the fine, and (3) return to the electoral notary showing that he or she paid the fine. Until the fine is paid, citizens are barred from applying for government jobs and other services, such as receiving or renewing their passport or driver's licenses, or requesting loans with public funds. It is important to note that public services affected by unjustified abstentions tend to be important to middle class and educated voters, not working class and poor voters.[8] A voter is not penalized for absenteeism if he or she is out of town on election day (Brazil does not have absentee voting); voters may also file a form with a judge giving the reason why they did not vote in the election within 60 days. Electoral judges have discretion to determine whether the excuse is legitimate or not.

3.4 Research Design

Our empirical strategy for understanding how voters respond to information about a candidate's record on corruption relies on three distinct components: a survey, a field experiment, and a survey experiment. Before presenting estimated effects of information revelation on behavior in an election, we present basic descriptive statistics from a post-election survey that provides useful context for understanding our findings. Subsequently, we present results from the field experiment and then show survey experimental evidence that supplement our findings from the field experiment.

We conducted the research in São Paulo for a number of reasons. First, it was the only city in which both candidates in the run-off election appeared on the Dirty List. We received funding from non-profit U.S. universities, and U.S. law prohibits political advocacy of candidates in elections by non-profit (501(c)(3)) organizations.[9] As a result, we treated the same number of precincts and produced the same flier design for both candidates. Second, São Paulo is the financial center of Brazil, and the city's mayor

[7] These rates contrast with a number of other countries that maintain mandatory voting including countries like Argentina, Australia, Belgium, and New Zealand, all of which have voter absenteeism rates in single-digit percentages.

[8] Additionally, welfare payments are not suspended as a result of absenteeism.

[9] For a more in-depth treatment of this issue, please see the discussion of legal and ethical issues in Appendix I.

carries significant weight in Brazilian politics. The 2008 election had an ex-governor of the state of São Paulo and the runner-up presidential candidate in the 2006 election; in addition, Brazil's most recently elected democratic presidents (Fernando Henrique Cardoso and Luiz Inácio da Silva (Lula)) maintain strong ties to the city. Finally, as a result of São Paulo's immense size – it is the largest city in Brazil and the seventh largest in the world with an estimated population of 11 million and 8,198,282 voters in 2008 within the municipality itself – the city offers considerable heterogeneity in the education and socioeconomic status of individual voters.

Did Voters Already Know About the Dirty List?

A necessary, but insufficient, condition for information about candidates' corruption record to have an effect on voting behavior is voter ignorance about the candidates' placement on the Dirty List. To find out whether or not voters already knew about the accusations, in the week after the election, we conducted a survey (N=200) of São Paulo residents living near polling stations in the field experiment control group with the aim of obtaining information on voters' pre-treatment knowledge of the Dirty List, as well as their opinions related to corruption in government. Since the treatment was never administered in these precincts, knowledge among surveyed voters should reflect knowledge among voters prior to the intervention. We used cluster sampling, in which we chose 20 control group precincts, and then randomly sampled ten households with the vicinity of the selected precincts.

Was the São Paulo electorate aware of the Dirty List and the fact that the two major candidates were included on it? Our survey data suggests that this is not the case as only 25 percent of respondents answered that they had heard of the Dirty List. Of those who knew about the Dirty List, only 48 percent knew that both candidates were on it, 30 percent identified only Suplicy as being on the Dirty List, and 22 percent identified only Kassab as being on the Dirty List. Thus, only 12 percent of all respondents could correctly place both Kassab and Suplicy on the Dirty List. Given this relatively low level of knowledge, informing voters potentially could change their views of the candidates and consequently their behavior on election day.

3.5 Behavioral Responses to Increased Information: Field Experimental Evidence

The Intervention

To inform voters of the corruption convictions of politicians, we designed two fliers – one for each candidate in the run-off election. The fliers are pictured in Figure 1 with their

respective translations.[10] The flier design incorporates aspects of political propaganda that are similar to other political marketing material in Brazil, while also intending to have credibility in the information it is conveying. Both fliers have newspaper articles from *Folha de São Paulo*, one of the country's most respected periodicals, detailing the corruption allegations of each candidate. We also included the case numbers of each court case to increase the credibility of the information in the fliers.

The Unit of Analysis and the Randomization Group

The unit of analysis for the experiment is the *local de votação*, or voting precinct. Voting precincts are the smallest units for which we could administer a treatment, while obtaining vote share data for individual candidates and turnout data for voters. In selecting the group of precincts in the randomization group, we made a number of decisions based on our substantive interests and logistical constraints. We chose 400 of São Paulo's 1,759 precincts utilizing a constraint optimization algorithm that operated as follows:

(1) selected a relatively even mix of precincts based on the vote choice in previous elections. The specific covariates are discussed in greater detail in Section 5.2.

(2) chose precincts to maintain a relatively even mix of poor, lower middle class, and middle class precincts.

(3) maximized the distance between the treatment and control groups in order to minimize the potential for cross-over violations.

(4) selected the smallest polling locations in order to maximize statistical power.

(5) limited the geographic areas of polling locations to the north, east, and south zones of São Paulo. Due to budget constraints, the delivery company we used to deliver the fliers limited us to three geographic zones in São Paulo. These three zones best satisfied the other criteria on which we selected the precincts in the randomization group.

(6) included precincts in areas with a high penetration of individual household units with individual mailboxes. We intentionally avoided areas with a high percentage of high-rise and mid-rise apartment buildings, because of the high likelihood of fliers not being delivered by doormen or other personnel who would control access to the buildings.

To reduce the risk of interference across experimental units, we ensured that precincts in the study were not closer than half a mile from other precincts in the study.[11] After en-

[10]The fliers were designed in consultation with a local graphics designer and political experts, with reference to a large sample of electoral propaganda. We also consulted with experts in constructing the design of the flier. In addition, we informally conducted semi-structured interviews with two dozen voters to get their reaction to various flier prototypes. Based on the responses of these individuals, we developed finalized versions of the fliers that would be used in the field and survey experiments.

[11]It is true that despite our precautions, some interference (sometimes referred to as "SUTVA" violations) could have occurred. The most plausible scenario is that a resident in a treatment precinct could have informed a voter living in a control precinct about the content of the flier. While we think that such violations were likely to have been few given that the election occurred only a few days after the distribution of the fliers, any interference that did occur would most likely result in downwardly biased

Figure 3.1: The Fliers

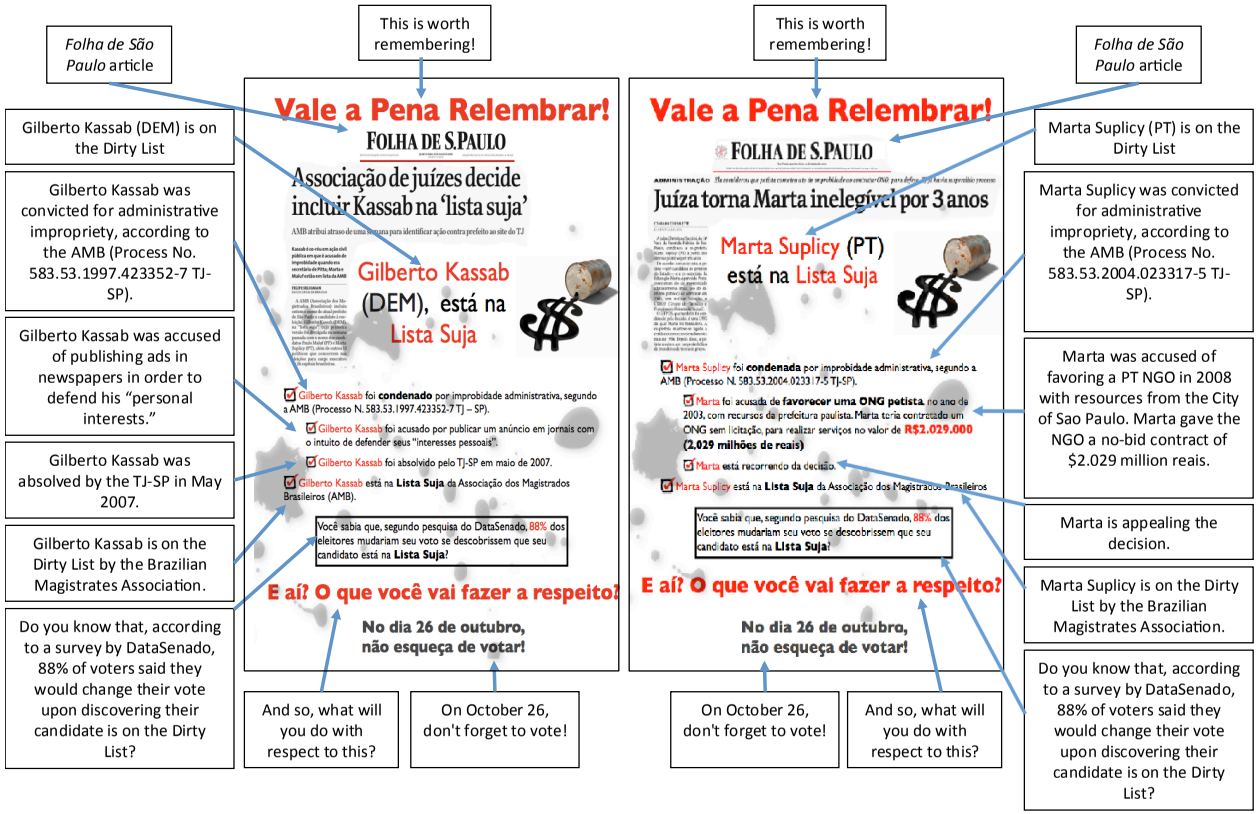
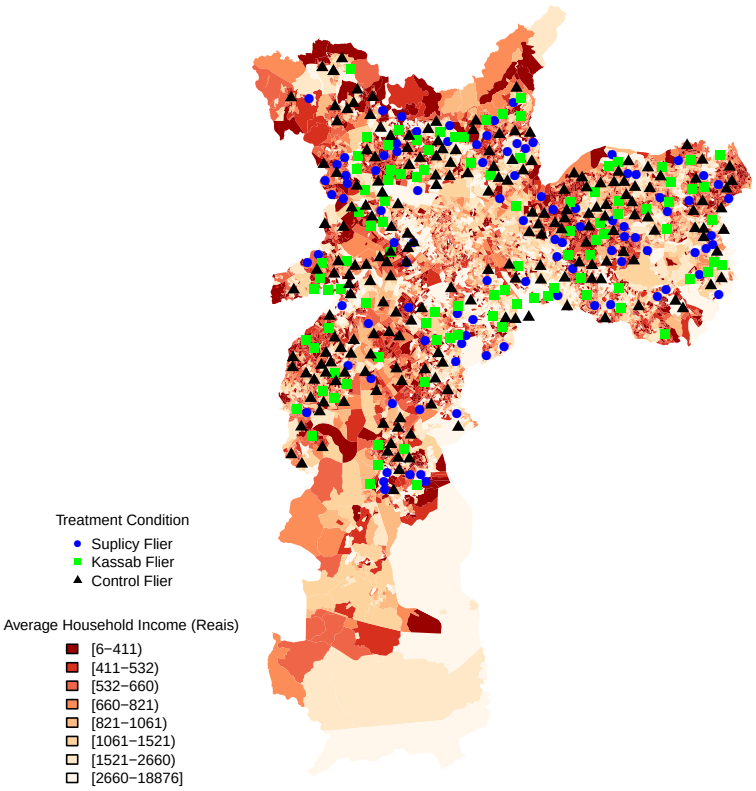


Figure 3.2: Distribution of Voter Precincts



suring some amount of distance between the experimental precincts, we grouped them into blocks of two based on longitude, latitude, PT vote share in the 2004 mayoral elections, and PT vote share in the 2006 presidential elections. More specifically, we matched precincts to their nearest neighbor on a Mahalanobis distance metric. Within blocks, each precinct had an equal probability of being selected into treatment. Figure 2 shows a map of São Paulo with the distribution of precincts in the treatment and control groups.

Flier Delivery

In order to deliver the fliers, we hired a direct marketing firm with extensive experience delivering marketing and political propaganda for prominent multinational and local retailers and political candidates. The firm delivered the fliers from October 22-25, 2008 (over the four days prior to the election), and had a number of enforcement measures in place to make sure that the correct fliers were delivered to households.[12]

Unlike in the United States, Brazilian voters are allowed to choose any voting precinct within an electoral zone located where he or she resides. In 2008, the municipality of São Paulo had 1,759 precincts located in 57 electoral zones. Unfortunately, in Brazil, data is not publicly available for the precincts to which voters are zoned. We spoke to political consultants and experts in voting behavior who stated that approximately 70 to 95 percent of voters vote at the location closest to their house in São Paulo. As a result, we were unable to determine the precise households that belonged to the voting precinct. Voters are, however, only able to vote in the precinct in which they are registered. In determining the appropriate number of households to deliver fliers for a given precinct, we knew the number of voters that were registered to vote at the precinct. We knew that the average number of voters per household in São Paulo at the time of the election was 3.1. In order to be conservative in our estimate of households for a given precinct,

(towards zero) treatment effect estimates. Under the assumption that receiving a flier with negative information about the candidate would not induce voters to vote for the candidate, our treatment effect is a lower bound on the true average treatment effect. More precisely, if the effect of receiving the flier on whether or not a voter votes for the candidate is non-positive in both treatment households and control households that inadvertently receive the information on the flier through interference, then reported treatment effect estimates of the average treatment effect in the absence of interference are downwardly biased. Our estimates would only overstate the treatment effect in the unlikely scenario that the fliers had opposing effects, i.e. that the flier caused voters in treatment precincts to vote against the candidate and caused control households to vote for the candidate. For a precise formulation of bounds in the presence of interference, see Manski (2011).

[12]First, the overwhelming majority of deliverers had worked with the firm previously, and had thus established a working relationship with the firm. Second, supervisors monitored deliverers and also performed random checks of mailboxes to ensure that the proper fliers were delivered. Third, delivery personnel carried hand radios and were monitored by a supervisor based at the office of the direct marketing firm. This supervisor had himself been a deliverer and had good local knowledge of the appropriate time it would take to complete a delivery route. Finally, the firm gave our research team unfettered access to monitor their work. We therefore conducted our own random checks of mailboxes to make sure the correct fliers were delivered and also accompanied the supervisors during the delivery.

we took the number of voters in the precinct, and divided the number by 2.8 to obtain the number of households within a precinct to which we would deliver fliers. We also delivered an additional ten percent of fliers because of the high likelihood of dilution in the immediate area of the precinct. The direct marketing firm maintained a current database with the number of individual houses per city block. The delivery firm located the 200 precincts in the treatment group, and gave maps to the deliverers so that they would "spiral out" from the precinct delivering all of the fliers with the precinct as the center of a radius. Supervisors dropped off delivery personnel at the voting precinct (which almost always was a school). In the weeks after the election, we also asked respondents in the treatment group the distance they lived from their voting precinct, and 63.9 percent stated that they lived 1 kilometer or less from their polling location, and 77.5 percent reported living less than 2 kilometers away from their polling location.

As a result of the imprecision with which we were able to deliver the treatment, we believe that our treatment effects most likely underestimated the impact of the treatment. While the vast majority of voters assigned to a given precinct live in the immediate vicinity of the precinct's polling station, the small number of voters who live far from the polling station—most likely because they never bothered to change their registration after moving—would not have received the flier. Furthermore it is possible that some of the residents who received fliers actually voted in a control precinct, which would further attenuate our estimate. Because we do not have precise data on which voters no longer live near their precinct's polling station, we can only estimate an "intent-to-treat" effect that is likely to be lower in magnitude than the effect among those who actually received the flier.

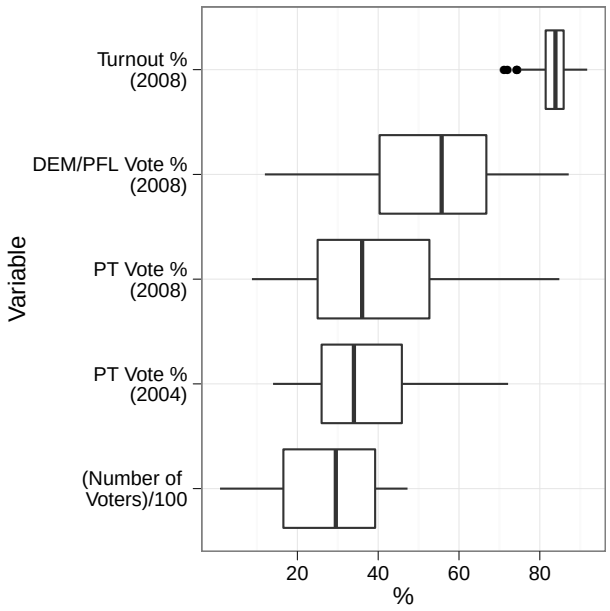
Descriptive Statistics and Baseline Balance

Box plots showing the distributions of the data used in the analyses below are presented in Figure 3. Consistent with the overall election results though with a smaller spread between the two candidates, the center-right candidate, Gilberto Kassab, received about 14 percentage points more of the vote than the center-left candidate from the PT.[13] Furthermore, turnout is high, with an average of 83 percent of registered voters casting a ballot. To check baseline balance on observables, as well as to improve precision in some of our estimates, we also use election data from the most recent past elections.

To check if our randomization procedure was successful, we examined whether pre-existing differences existed across treatment and control precincts. As is well known, in expectation there will be no differences between treatment assignment groups, but for any given randomization, some imbalances can remain. To check baseline balance, we conducted simple difference-in-means tests across thirteen baseline covariates. The covariates include voting outcomes from previous elections, as well as voting results

[13]In the actual election, Kassab received 60.7 percent of the vote, while Suplicy received 39.3 percent of the vote.

Figure 3.3: Descriptive statistics for the field experiment.



Data obtained from the São Paulo TRE (Regional Electoral Tribunal); N=400

from the first round of the 2008 election. In addition to testing balance on each variable separately, we use an omnibus test found in Hansen and Bowers (2008) that jointly appraises balance on each covariates, as well as their linear combinations.[14] Table 3.1 shows the results for each separate variable, reporting mean differences, standard errors of the difference, t-test p-values, and the Kolmogorov-Smirnov Test p-values. For twelve of the thirteen covariates, we find no substantial imbalances. The number of voters variable, however, exhibits some imbalance, with a mean difference of 298 additional voters in treatment precincts versus control. In some of our analyses below, we check the robustness of our findings to adjust for this imbalance. The omnibus test which tests the hypothesis of no difference on any of the baseline variables, as well their linear combinations, has a p-value of 0.17. Thus, while we find some imbalance on the number of registered voters, on all other variables, treatment and control are statistically indistinguishable overall.

[14]The omnibus statistic, called as d^2 in Hansen and Bowers (2008), is a weighted sum of squares of differences in means, though in our application, the weights are constant. This statistic has a large sample χ^2 distribution.

Table 3.1: Balance on Baseline Variables (N=400)

Variable	Mean Diff	Standard Error	t-Test p-Value	KS-test p-Value
Number of Registered Voters	-298.38	133.42	0.03	0.09
PT Mayor Vote % (2004)	-0.32	0.98	0.74	0.54
PT Pres. Vote % (2006)	0.07	1.12	0.95	0.86
PT Congress Vote % (2006)	-0.06	0.72	0.93	0.99
PSDB Congress Vote % (2006)	0.32	0.63	0.62	0.14
1st Round Suplicy Vote % (2008)	-1.10	1.37	0.42	0.54
1st Round Kassab Vote % (2008)	0.14	0.74	0.86	0.79
1st Round Blank Vote % (2008)	-0.02	0.08	0.78	0.92
1st Round Invalid Vote % (2008)	-0.07	0.08	0.41	0.79
1st Round Turnout % (2008)	0.20	0.32	0.52	0.18
PT City Council Vote % (2008)	-0.65	0.83	0.43	0.54
PSDB City Council Vote % (2008)	0.86	0.60	0.15	0.33
DEM City Council Vote % (2008)	0.22	0.44	0.62	0.47

Results

We present two sets of results for each of our three dependent variables: vote share, turnout, and spoiled ballots. Our quantity of interest is the average treatment effect on precincts, not individual voters, as individual level data is unavailable. The first estimator is the simple "intent-to-treat" estimator, which is the average within-block difference in treatment and control precinct means. Our second set of estimates are from a simple linear regression of the outcome variable on a treatment indicator, a vector of covariates, and block dummy variables. The model we estimate is as follows:

$$Y_i = \beta_0 + \beta_1 T_i + \sum_{k=1}^{K-1} \gamma_k B_{ki} + \lambda_1 X_1 + \lambda_2 X_2 + u_i$$

Y_i is the outcome of interest, T_i is the treatment indicator, X_1 and X_2 are two pre-treatment covariates, and u_i is the disturbance term. To account for the fact that randomization occurred within matched pairs or blocks (k), we add fixed effects (B_{ki}) for all but one matched pair. Since we are interested in the separate effects of each type of flier, we estimate this model separately for the Suplicy intervention and the Kassab intervention. We adjust for two covariates: PT vote share in the 2004 mayoral election and the number of registered voters in the precinct. 2004 PT vote share is an important covariate because it is highly predictive of our outcome variables and can potentially increase the precision of our estimates. We also adjust for number of voters because we detected some imbalance in this covariate after randomization, as discussed in the previous section. Finally, all standard errors account for heteroskedasticity, as "robust" standard errors are used in covariate adjusted results and the intent-to-treat estimates do not assume equal variance across treatment conditions.

Table 3.2: The effect of distributing information on corruption convictions involving Marta Suplicy, the PT mayoral candidate, on election outcomes. N=200 precincts, with 100 treated units. Estimates without covariates are from the simple ITT estimator. Estimates with covariates are from a linear model, including a treatment indicator, PT vote share in 2004, total number of registered voters in the precinct, and block fixed effects.

	Vote Share (%)		Turnout (%)		Spoiled Ballots (%)	
Estimate	-2.6	-2.6	-1.9	-1.8	0.03	0.01
Standard Error	1.99	0.93	0.46	0.45	0.08	0.08
95 % Conf. Int.	[-6.5, 1.3]	[-4.4, -0.7]	[-2.7, -0.9]	[-2.7, -0.9]	[-0.1, 0.2]	[-0.1, 0.2]
p-value	0.2	0.01	0	0	0.72	0.86
Covariates	No	Yes	No	Yes	No	Yes

Table 3.2 presents the effect of the distribution of the fliers with information on the corruption convictions of the PT mayoral candidate on the vote share of the candidate, turnout, and spoiled ballots.[15] For vote share (votes as a percent of total votes cast), we find a negative effect of about 2.6 percentage points, which amounts to about 15 percent of a standard deviation. The 90 and 95 percent confidence intervals of the unadjusted estimate overlaps with 0 (p-value = 0.2), but the adjusted estimate, which is also -2.6 percentage points, is statistically significant at conventional levels. While estimated with some imprecision, this result does suggest that receiving the flier induced some voters who otherwise would have supported Suplicy to abstain or vote for Kassab.

Our estimates support the hypothesis that providing information about Suplicy's corruption convictions lowered the candidate's vote totals, but where did these votes go? Theoretically, the lower vote share in treatment precincts could be due to either increased abstention by Suplicy supporters or vote switching to Kassab by voters who previously supported the PT candidate. Our data is more consistent with the former story as opposed to the latter. When we estimate the effect of the Suplicy intervention on total votes received by Kassab as a percent of registered voters (not vote share as a percent of ballots cast), we find an insignificant increase of about 1.5 percentage points (standard error of 1.7). Thus, while it is likely that some Suplicy voters changed their vote and cast a ballot for Kassab, it would appear that abstention was the primary response by voters to the intervention.

Further evidence that the intervention affected electoral outcomes primarily through decreased turnout is presented in the second two columns of Table 3.2, where we find a significant negative effect of -1.9 percentage points. This effect estimate represents an

[15]Spoiled ballots in all presentations of results are measured by the blank votes cast in the election. We also estimated treatment effects on invalid votes and the sum of invalid votes and blank votes, and found that all estimates were statistically indistinguishable from 0.

Table 3.3: The effect of distributing information on corruption convictions involving Gilberto Kassab, the DEM/PFL mayoral candidate, on election outcomes. N=200 precincts, with 100 treated units. Estimates without covariates are from the simple ITT estimator. Estimates with covariates are from a linear model, including a treatment indicator, PT vote share in 2004, total number of registered voters, and block fixed effects.

	Vote Share (%)		Turnout (%)		Spoiled Ballots (%)	
Estimate	1.9	1.5	0.1	0	-0.05	-0.09
Standard Error	1.87	0.99	0.42	0.41	0.12	0.13
95 % Conf. Int.	[-1.8, 5.5]	[-0.5, 3.4]	[-0.7, 0.9]	[-0.8, 0.8]	[-0.3, 0.2]	[-0.4, 0.2]
p-value	0.32	0.15	0.77	0.95	0.68	0.49
Covariates	No	Yes	No	Yes	No	Yes

average decline of about 450 voters. Results using covariate adjustment are substantively equivalent to the unadjusted results (point estimate of -1.8 percentage points). For spoiled ballots, we find a small positive difference, but both estimates are statistically indistinguishable from 0.

This abstention response is particularly surprising given that failing to vote is punished with fines, albeit very small ones (less than US $2). The fact that turnout is sensitive to the distribution of a flier suggests that either voters find the fines to be trivial or that the consequences of not paying the fine are low for many Suplicy voters. Nonvoters who fail to pay the fine or provide an adequate excuse are prevented from receiving benefits that are most valuable to educated, middle-class Brazilians such as the ability to renew a passport and drivers license and eligibility for the civil service. In our survey data, we found that Suplicy's base is substantially poorer than Kassab voters, with about half of Suplicy voters earning less than twice the minimum wage, compared to 30 percent of Kassab supporters. While we cannot know for sure why voters are willing to bear the costs of abstention, one possibility is that the punishment for not voting is inconsequential for a substantial portion of Suplicy's base. Kassab voters, because of their comparative education and wealth, are more likely to view the administrative restrictions that result from abstention as more costly.

The estimated effects of the distribution of fliers with information on the center-right candidate of the DEM/PFL are found in Table 3.3. Surprisingly, the point estimate on the DEM/PFL candidate's vote share is positive at about 1.5-1.9 percentage points, depending on the specification. This result, however, is estimated rather imprecisely and consequently not statistically significant at conventional levels. Furthermore, the estimate appears to be somewhat sensitive to covariate adjustment. The estimates for the other two outcome variables – turnout and spoiled ballots – are small and not statistically

insignificant.[16]

To contextualize these estimated effects, it is worth comparing their magnitude to effects documented in other studies using experimentally administered interventions to increase voters' awareness about corruption.[17] In the Chong et al. (2011) study on Mexican mayoral elections, a flier campaign informing voters of the result of corruption audit had a negative overall effect of 1.10 percentage points on turnout or slightly more than half of what we document. Their flier intervention, however, had a larger turnout effect of -4 percentage points in municipalities with higher rates of corruption and, in contrast to our findings, had similar effects on both incumbent and challenger supporters. An important difference between our results and their study, however, is that they informed voters only about incumbent performance, as well as the fact that abstention in Mexico is not fined.[18] In the Banerjee, Kumar, et al. (2010) study in New Delhi that provided voters with "report cards" on incumbent performance, additional information *increased* turnout by about 3.6 percentage points, but these leaflets included information on a range of activities. While the report cards did not have corruption indicators, they did report whether or not the candidate was a criminal. The effects of the intervention, however, did not vary by candidate's criminal status. Thus, our results are more in line with the Chong et al. (2011) study, with the important caveat that we compare two candidates in the same municipality while their study emphasizes comparisons across municipalities.

3.6 Testing Mechanisms: Individual Level Evidence

To understand the heterogenous behavioral effects of the fliers observed in the aggregate electoral data, we now turn to individual level data. This data, gathered in the survey, described in Section 3.4, contains descriptive data on voters opinions about the corruption record of each candidate, ex-ante evaluations of the candidates, and the importance that voters place on corruption in their political decision-making. In addition to collecting basic attitudinal data, we also use an embedded survey experiment to observe individual level attitudinal responses to the information contained on the fliers used in the intervention.

[16] The point estimate for the difference in the effect of the two fliers on vote share (with covariates) is 4.1 percent, with a standard error of 1.4. Without covariates, this difference is estimated less precisely but it is still significant at the 10 percent level. Similarly, the point estimate for the difference in the effect on turnout between the Suplicy and Kassab fliers—with or without adjusting for covariates—is statistically significant. For the covariate adjusted estimates, the difference is 1.8 percent with a standard error 0.61.

[17] In the extensive experimental get-out-the-vote (GOTV) literature on US elections, we are not aware of studies that provide information about politicians' corruption records. The literature on the effects of distributing GOTV leaflets or fliers on turnout suggest very small effects on the order of about 0.5 percentage points (Green and Gerber 2008, pg. 51).

[18] While voting is technically compulsory in Mexico, there are no legal sanctions for not voting.

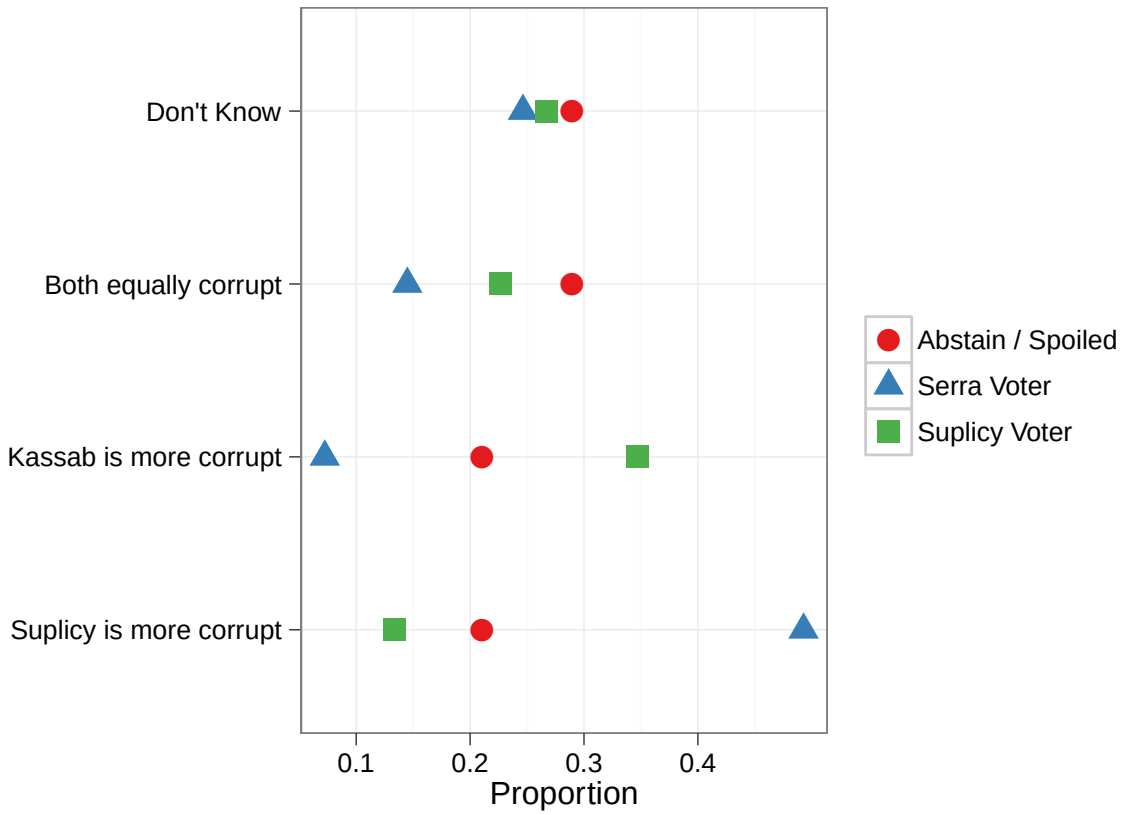
As discussed in Section 2, voters may already have existing beliefs about how corrupt each candidate is and these beliefs will affect their response to new information. If voters already perceive a candidate to be corrupt, learning about their placement on the Dirty List may not change their attitudes or their behavior. It is possible, for example, that voters already assumed that Kassab was corrupt and thus the flier would not affect their evaluation of the candidate. To check for this possibility, the survey asked voters to rank each candidate by their perceived level of corruption. On average, voters' evaluations of the candidates on this quality differed in that 29 percent of voters identified Suplicy as the most corrupt candidate, while 20 percent of voters named Kassab. 20 percent of voters said both were equally corrupt, while another 30 percent stated they did not know. These figures suggest that while a plurality of voters considered Suplicy the more corrupt candidate, the vast majority of voters believed the candidates to be equally corrupt or could not make the comparison. Overall, these figures suggest that the differential effects we detected in the field experiment are unlikely to be attributable to diverging ex-ante evaluations of the candidates on the corruption issue as Suplicy, on average, was viewed as somewhat more corrupt already.

These aggregate figures, however, mask considerable heterogeneity when voters are disaggregated by their past political behavior. Figure 3.4 shows how voters rank each of the candidates in subgroups defined by their self-reported vote in the run-off for the 2004 mayoral election.[19] Suplicy, the incumbent in 2004, lost the election against former presidential candidate José Serra. It is clear that the political leanings of each voter strongly predicts how voters evaluate each candidate on the corruption issue. 34 percent of voters who cast a ballot for Suplicy in 2004 viewed Kassab as the more corrupt candidate, while only 7 percent of Serra voters felt similarly. The views of Suplicy and Serra voters are not completely symmetric: Serra voters are more likely to believe that Suplicy is more corrupt (49 percent) than Suplicy voters are to believe that Kassab is more corrupt (34 percent). As one might predict, voters who abstained or cast a spoiled ballot in the 2004 election were more likely to claim that each candidate was equally corrupt.

Given the fact that voters' ex-ante perception of the candidates on corruption varied markedly by their political leanings, any intervention designed to increase voters' information could have highly heterogenous effects depending on the candidate the voter intends to support. If a Suplicy supporter received information about Suplicy and viewed the new information as credible, for example, then she might be less inclined to turnout or cast a ballot for Suplicy. This is especially the case if Suplicy voters were more likely to be weak supporters of the candidate. Thus, a potentially important distinction between the two candidates is the intensity of their voters preferences since a candidate with many weak supporters would likely suffer more as a result of the revela-

[19]The pattern is very similar if we stratify by 2008 vote choice. We use 2004 vote choice as a stratification variable to show that heterogeneity in voters' evaluations coincide with political cleavages that existed prior to the 2008 election.

Figure 3.4: Ranking Candidates on Perceived Corruption by Vote in 2004



tion of information. In this election, we find no evidence of a divergence in the intensity of preferences among the supporters of each candidate. To assess this, we asked each voter to rate the candidates with a 1 to 10 "feeling thermometer" score. The distribution among each candidate's voters were almost identical with a mean score of 7.8 for Suplicy among Suplicy voters (median of 8) and a mean score of 7.6 (median of 8) for Kassab among Kassab voters. The similarity across the two groups of voters suggests that intensity of preferences is unlikely to be an explanation for the divergent effects found in the field experiment.

Even in the absence of differences in the intensity of support across each candidate's voters, the effects of information about corruption accusations could diverge if a candidate's supporters differ in the importance they place on clean government. There is reason to believe that divergence would exist given that Suplicy's party, the PT, has a long history of emphasizing transparency in government and this may cause voters who care about this issue to support her. In fact, we do find a marked difference between Suplicy supporters and other voters in the importance placed on corruption. For vot-

ers who supported Suplicy, 70 percent professed that when deciding who to vote for in the 2008 election, corruption was "very important" or "important" in their decision. In contrast, a considerably fewer 48 percent of Kassab supporters said that corruption was "very important" or "important." This 22 percentage point difference suggests that Suplicy voters would, on average, be considerably more sensitive to learning about Suplicy's placement on the voters list.

Overall, the findings of the survey suggest that the most substantial difference across Kassab and Suplicy voters is the weight each candidate's supporters place on corruption in their decision making. Perhaps because of the PT's historical image as not engaging in the corrupt practices used by other parties, more Suplicy voters than Kassab voters say that corruption is an important factor when choosing among candidates. This suggests that learning about one's preferred candidate's placement on the Dirty List would have a larger effect on behavior among Suplicy supporters than Kassab supporters, which is consistent with the results of the field experiment.

Evidence from a Survey Experiment

To better understand our findings and to take advantage of individual-level data, we embedded a survey experiment modeled after the field experiment in the post-election survey discussed in Section 3.4. While we are interested in the overall impact of the fliers on voter attitudes, the survey experiment also allows us to test some of our hypotheses explaining the divergent effects found in the field experiment. In particular, we take advantage of the survey experiment to test our hypothesis that Suplicy's supporters' views are more affected by learning about her placement on the Dirty List than the views of Kassab's supporters when they learn about his corruption record.

Working only in field experiment control precincts, we randomly assigned 200 respondents with equal probability to be given the Kassab flier, the Suplicy flier, or a placebo flier showing basic biographical information for both candidates (shown in Appendix II).[20] After the respondents read the fliers, the interviewers asked the interviewed voters to "grade" Kassab and Suplicy on a scale from 0 to 10, where 0 indicated being strongly against the candidate and 10 indicated that the respondent was strongly in favor of the candidate.

After asking respondents to read the fliers, we asked the interviewed voters about the believability of the accusations, as well as their seriousness. If voters perceived the Suplicy accusations to be more believable or serious, then this difference could explain the disparate behavioral response to the fliers. We find no evidence for either explanation.

[20]Like with the field experiment, we checked to see if the randomization procedure achieved reasonable balance in pre-treatment covariates. We checked for differences on pre-treatment feeling thermometer scores of the two candidates, self-reported turnout, self-reported vote intention, party identification, presidential vote, and household income. None of the differences across the three different treatment conditions were statistically significant from each other. The Hansen and Bowers (2008) omnibus test that jointly appraises covariate balance and their linear combinations gives a p-value of 0.56.

Only 30 percent thought the flier was mostly or completely false; most voters exposed to the Suplicy flier said that the accusations were mostly or completely true. For those exposed to the Kassab flier, the proportions are very similar: only 28 percent thought the flier was mostly or completely false. When it comes to the seriousness of the accusations, once again there were few differences by flier. 80 percent and 78 percent of voters exposed to the Suplicy flier and Kassab flier, respectively, thought the accusations were very serious or serious. The similarity in voters perceptions of the two fliers provides evidence that differences in the fliers or their content are not an explanation for why the Marta flier was more effective at changing voting behavior than the Kassab flier.

Table 3.4: Survey experiment results for the Suplicy (PT) and Kassab (DEM/PFL) fliers. The dependent variable is the post-treatment minus pre-treatment candidate evaluation on feeling thermometer on a scale of 0 to 10.

	Suplicy vs Placebo	Kassab vs Placebo	Suplicy vs Kassab
Estimate	-0.78	-0.36	-0.54
Standard Error	0.32	0.34	0.42
95 % Conf. Int.	[-1.41, -0.15]	[-1.03, 0.31]	[-1.36, 0.28]
p-value	0.02	0.3	0.2

We also examine the overall effects of the fliers on voters' evaluations of the candidates by comparing voters' evaluations of Suplicy (Kassab) when they view the Suplicy (Kassab) flier versus when they are given the placebo flier. These results are shown in the first two columns of Table 3.4. The effect of the Suplicy flier on voters' evaluations of Suplicy is larger than the effect of the Kassab flier on voters' evaluations of Kassab, although the difference between the two effects is not significantly different than 0. After being exposed to the Suplicy flier, respondents in the treatment group on average adjusted their evaluations downward by an estimated 0.78 points on a 10-point scale, which amounts to about 60 percent of a standard deviation. The point estimate for the Kassab flier was an insignificant -0.36. The third column compares those receiving the Suplicy flier to those receiving the Kasab flier. The Suplicy flier more negatively affects attitudes, though this difference is not statistically significant. Overall, these individual-level estimates are in keeping with the field experiment evidence: the Suplicy flier harms voters evaluations of her, while the Kassab flier has weaker effects. Thus, the design also contributes to a nascent literature (Barabas and Jerit 2010) that examines the external validity of survey experiments through its pairing of a field experiment with a survey experiment.

To test whether or not Suplicy voters respond differently to increased information about their favored candidate's corruption record than Kassab voters, we estimated treatment effects separately in strata defined by vote choice. The first two columns of Table

Table 3.5: Heterogeneity in survey experiment results for the Suplicy (PT) and Kassab (DEM/PFL) fliers. The dependent variable is the post-treatment minus pre-treatment candidate evaluation on feeling thermometer on a scale of 0 to 10.

	Suplicy vs Placebo		Kassab vs Placebo	
	Non-Suplicy Voters	Suplicy Voters	Non-Kassab Voters	Kassab Voters
Estimate	-0.38	-1.29	-1.24	0.42
Standard Error	0.32	0.53	0.5	0.5
95 % Conf. Int.	[-1.01, 0.25]	[-2.33, -0.25]	[-2.22, -0.26]	[-0.56, 1.4]
p-value	0.24	0.02	0.02	0.4
n	85	48	62	72

3.5 show the effect of the Suplicy flier, as compared to the placebo flier, on those who did not vote for Suplicy and those who did (self-reported). The estimate for Suplicy voters is more than three times the size of the estimate for Non-Suplicy voters: -1.29 versus -0.38. Unsurprisingly given the small samples, however, the difference between the two estimates (the interaction) is not statistically significant. Still, the difference in magnitudes certainly suggests that Suplicy voters are more sensitive to corruption-related information than supporters of other candidates.

When we examine heterogeneity in the effect of the Kassab flier, the contrast with the effect of Suplicy flier is striking. As shown in the third and fourth columns of Table 3.5, the heterogeneity observed is the opposite of what we found for the Suplicy flier. Kassab voters who read the flier, on average, give a *higher* evaluation of the candidate. Although this estimate is not statistically distinguishable from 0, it is distinguishable from the effect of the flier among non-Kassab voters. Among non-Kassab voters, reading the Kassab flier induced a statistically significant 1.24 point decrease in their evaluation of the candidate. The difference in the size of the effect between Kassab voters and non-Kassab voters is 1.7 points (standard error of 0.7).

Overall, the results from the survey experiment provides further evidence that Suplicy's voters have a larger reaction to increased information about their candidate's corruption record than Kassab's voters. Upon learning of Suplicy's position on the Dirty List, Suplicy's voters perceive her more negatively, on average. When Kassab voters learn about their candidate's placement on the Dirty List, their evaluation of their candidate is essentially unchanged. Furthermore, our survey evidence shows that, when asked, Suplicy's base professes to place more importance on corruption than Kassab's base. It is plausible that this difference in how each candidate's voters view the importance of corruption resulted in a differential behavioral response to the release of information in our field experiment. In general, our evidence indicates that Suplicy's voters viewed their candidate more negatively after learning about her record and became more likely

to abstain as well as, to a lesser degree, switch their vote to Kassab. Kassab voters, because they view corruption as less central to their political decision-making, failed to change their views or their behavior.

3.7 Conclusion

Good government activists and reformers frequently argue that increased transparency about politicians' records can make democratic institutions more effective at incentivizing clean governance. Increased transparency, the argument typically goes, will induce voters to punish corrupt politicians at the voting booth and thus better align the interests of politicians with the electorate. As we document in this work, publicizing a candidate's record on corruption does have the potential to alter voters' behavior, but its effects are contingent upon the importance voters place on clean governance in their decision-making. Furthermore, the degree to which voters view a candidate's corruption record as important can be correlated with political cleavages, an important mechanism previously unexplored in the experimental literature on corruption. As a result, the effects of increased transparency may result in outcomes wherein one politician may be punished when his corruption record is revealed while another may not be.

In the case of São Paulo, we document the existence of a partisan cleavage in how voters perceive the importance of corruption. Furthermore, we argue that this cleavage has real consequences for the effectiveness of an intervention designed to inform voters about candidates' placement on a so-called Dirty List compiled by a civil society organization. Despite the fact that voters viewed the accusations against each candidate as equal in seriousness, our field experimental evidence revealed that only the PT's candidate, Marta Suplicy, was punished at the ballot box when voters learned about her placement on the Dirty List. Data from our public opinion survey and an embedded survey experiment provides evidence of a mechanism: Suplicy's supporters are much more sensitive to information about corruption than are Kassab's supporters. As a result of this increased sensitivity, the information provided to voters induced some of Suplicy's supporters to abstain and, to a lesser degree, to switch their vote to her opponent.

An important question raised by these results is why are Suplicy voters more willing to change their behavior when they learn about their candidate's record? We speculate that the PT's historical cultivation of a brand as a "different" type of party that has a distinct "mode of governance" (*"modo petista de governar"*) emphasizing transparency and citizen participation may have raised PT voters' expectations on the corruption issue. For many PT voters, corruption and ethical issues may be central to their political identity. Kassab's party, the Democrats (formerly the PFL) if anything, has developed a brand as a party whose candidates may rob, but "get things done." As a result, Kassab voters may have had lower expectations about their candidate's probity in office and consequently new information about past misdeeds failed to change their behavior.

More generally, our results suggest voters can develop a "norm of accountability"

but that this norm can be less than universally held. In a different context, Stokes (2006) documented variation in the degree to which voters across Argentina abided by informal voting rules that sanctioned poor performance. In towns where democratic institutions worked more effectively, voters tended to expect politicians to govern ethically and were quite willing to withdraw their support when this was not the case. Our findings suggest that this norm of accountability can interact with partisanship and have important consequences for the outcomes of campaigns where corruption is an issue. The historical factors that explain how a party becomes particularly trusted on the issue of corruption and that cause its supporters to vote based on candidates' corruption records is an important area for future research.

One troubling possibility raised by our findings is that increased transparency may disadvantage candidates from parties with a reputation for clean governance when they compete against candidates from parties with no such reputation. In the case of Brazil, PT candidates may be particularly vulnerable to attacks by opposing parties on the corruption issue. Increased transparency could paradoxically, at least in the short term, reduce the chances of PT candidates from winning office, even if they tend to be less corrupt than candidates from parties like the Democrats.[21] Of course, this may be an acceptable outcome to PT voters, as long as it creates a long-term incentive for PT politicians to govern without resorting to corruption. Still, the heterogeneity across candidates that we document suggests the possibility that information campaigns can actually increase the incidence of corruption in government by disproportionately punishing "clean" parties.

More broadly, our findings suggests that future experimental work on information and accountability will find varying effects across different political contexts. As we found in São Paulo, the existence of information effects can depend on highly contextual factors associated with particular candidates, parties, and the distribution of preferences in the electorate. Future work on the effects of information on political accountability will need to address more systematically how these antecedent factors affect voters' response to increased transparency. As we have documented, the relationship between information and accountability is by no means a simple one.

[21] It is by no means clear, however, that PT politicians are still less corrupt than non-PT politicians. In recent years, political observers have actively debated whether or not the PT has abandoned its historical position as a more ethical party and now is just a "normal" (that is a *corrupt*) party. The unlikely success of Green Party candidate Marina Silva in the 2010 presidential election may have been partly fueled by former PT voters disappointed in the party's record on the corruption issue.

Appendix I: Legal and Ethical Issues

We faced some legal and ethical issues in carrying out this project, and responded by having a number of safeguards in place. The concerns involved legal and ethical issues not only in Brazil, but also in the United States.

We received funding from the University of California, Berkeley, and Yale University to carry out the project. Both are non-profit (501(c)(3)) institutions that are prohibited from engaging in political advocacy. We inquired with Yale Law School's Non-Profit Organizations Clinic to make sure that we complied with this restriction, and drew on the experience of previous electoral field experiments done in the United States as a precedent for complying with this prohibition. This prohibition partly factored into our choice of São Paulo as the site where we conducted the field experiment. We not only performed the intervention in a place where both candidates had corruption convictions, but we chose the run-off election so as not to have effects on the vote shares of other candidates that could affect the outcome of the election. We also obtained approval from human subjects committees at Berkeley and Yale.

Polls immediately prior to the election from prominent organizations such as Datafolha and Ibope showed that Kassab had roughly a twenty percentage point lead over Suplicy. Our treatment of 187,177 households reached an estimated six to seven percent of the electorate of São Paulo. Even if every voter responded to the treatment, we believe the likelihood of the field experiment affecting the overall outcome was extremely unlikely. Though to our knowledge there were no prior electoral field experiments of this sort conducted in Latin America, we examined the findings of electoral field experiments conducted in other regions. The largest treatment effect for this sort of project that we found was slightly below nine percentage points (Gerber, Green, and Larimer 2008; Green and Gerber 2008). In addition, we delivered the fliers immediately prior to the election (from October 22 until October 25, 2008) to minimize the likelihood of the information spreading to other areas, and also to decrease the chances of the parties reacting strategically to the experiment.

While in São Paulo, we sought counsel from an election lawyer to make sure we were in compliance with Brazilian electoral laws. The lawyer assured us that so long as we were not affiliated with any candidate or party, we would be in compliance with the Brazilian Electoral Code. We also sought the opinion of a former electoral judge, who felt that the study was in compliance with local laws. Finally, we informed an electoral judge of the research design and also gave him the fliers prior to the launch of the field experiment.

Appendix II: Placebo Flier for the Survey Experiment

Information about the Candidates

Informação sobre os Candidatos

- •Marta Suplicy nasceu em 1945 em São Paulo, SP.
- •Marta é psicóloga.
- •Marta tem três filhos.
- •Marta estudou na Pontifícia Universidade Católica de São Paulo.

Marta Suplicy was born in 1945 in Sao Paulo, SP

Marta is a psychologist.

Marta has three children.

Marta studied at the Catholic University of Sao Paulo.

- •Gilberto Kassab nasceu em 1960 em São Paulo, SP.
- •Kassab é engenheiro civil e economista.
- •Kassab estudou na Universidade de São Paulo.

Gilberto Kassab was born in 1960 in Sao Paulo, SP.

Kassab is a civil engineer and economist.

Kassab studied at the University of Sao Paulo.

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