Voter Buying:
Shaping the Electorate through Clientelism[*]

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February 20, 2013

Abstract

Studies of clientelism typically assume that political machines deliver rewards to influence
vote choices and turnout of the existing electorate. This assumption masks important clientelist
strategies because it focuses narrowly on citizens who are already registered in a machine's
district. Political machines distribute benefits not only to influence the *actions* of the electorate,
but also to shape the *composition* of the electorate. One such clientelist strategy, which we
term "voter buying," induces citizens in other districts to transfer their electoral registration
and vote for the machine. A formal model suggests that voter buying is incentive-compatible
and provides several testable predictions. Both qualitative and quantitative analyses provide
evidence of this strategy in Brazil. We investigate the impact of voter buying with a regression
discontinuity design, exploiting an intervention that required municipalities to re-register their
entire electorate if they exceed an arbitrary threshold. This intervention undermined voter
buying and reduced the likelihood of mayoral reelection by 18 percentage points. Consistent
with the voter-buying model, the treatment effect of the intervention is over twice as large (41
percentage points) if many voters recently transferred away from neighboring municipalities.

1   Introduction

A central area of inquiry in political science is how political institutions influence the practice of democracy. Formal rules that shape the electorate, ranging from suffrage restrictions to compulsory voting, are widely understood to have important effects on democracies across the world (e.g., Dahl, 1971; Lijphart 1997). Without denying the significant role of such formal rule changes, we argue that much scholarly research overlooks important ways in which informal institutions — such as clientelism — also shape the electorate. This paper investigates an understudied strategy of clientelism that alters the contours of the electorate and dramatically affects elections in some contexts.

Studies of clientelism typically assume that political machines deliver rewards to influence vote choices and turnout of the existing electorate (e.g., Cox, 2006; Nichter 2008; Stokes 2005). Such research typically takes the electorate as a given and therefore ignores important clientelist strategies. Machines provide rewards not only to influence the *actions* of the electorate, but also to shape the *composition* of the electorate. This paper examines one such clientelist strategy, which we term "voter buying." This strategy induces citizens in other districts to transfer their electoral registration and vote for the machine.

Historical accounts of elections in the U.S. are replete with examples of voter buying. Political machines in cities such as Baltimore, New York and St. Louis rewarded citizens from other districts for "colonizing" their electoral rolls (e.g., Argersinger, 1985, 675–683; Campbell 2005, 19, 161). For instance, an 1891 *New York Herald* article entitled "Colonization!" examines what is described as a "time honored" practice. Political operatives of the Tammany machine imported voters from other districts and paid them $10, alcohol and free lodging to "swear before the Board of Registration that they are qualified to vote" in the district.[1] In another example, a New York assemblyman was arrested during his reelection campaign in 1900 for "harboring colonizers" in

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[1] "Colonization!," *New York Herald*, October 16, 1891, Pg 5.

a hotel and "paying them to register fraudulently." The Elections Superintendent explained that colonizers, who did not live in the district, received "free lodging until after the election and $5 each for voting the Tammany ticket" in addition to "all they wanted to drink and car fare."[2] Overall, such examples suggest that voter buying deserves further investigation.

The understudied strategy of voter buying offers an alternative explanation to the secret-ballot puzzle examined by numerous studies of clientelism (e.g., Diaz-Cayeros, Estévez and Magaloni, forthcoming; Nichter 2008; Stokes 2005). Scholars typically understand "vote buying" as offering benefits in exchange for vote choices. But with the secret ballot, what prevents individuals from accepting rewards and then voting as they wish? The strategy of voter buying relaxes the frequent — yet often unrealistic — assumption that machines can violate ballot secrecy. Voter buying rewards outsiders who have no stake in the outcome of the election other than the material rewards they receive. Because recipients have no reason to defect once inside the ballot booth, voter buying only requires monitoring *whether* individuals transfer and show up on Election Day. Voter buying may be particularly attractive in contexts where stringent compulsory voting undermines the viability of turnout buying, another clientelist strategy that does not require monitoring of specific vote choices (Nichter, 2008).

This study advances research on clientelism by specifying and testing a mechanism that induces citizens to transfer their voter registration and vote for the machine. Neither of these analytical tasks has been addressed by the existing literature. Formal modeling suggests that voter buying is incentive-compatible, and provides several testable predictions: (1) machines will focus rewards on neighboring municipalities; (2) they will offer rewards where they can most effectively monitor citizens; and (3) they will target voters with weak or no political preferences. Both qualitative and quantitative analyses provide evidence of this strategy in Brazil. We investigate the impact of voter buying with a regression discontinuity design, exploiting an intervention that required municipalities to re-register their entire electorate if they exceed an arbitrary threshold.

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[2]"Tammany Hall Charged with Huge Registration Frauds," *New York Herald*, October 23, 1900, Pg 3.

This intervention undermined voter buying and reduced the likelihood of mayoral reelection by 18 percentage points. Consistent with the voter-buying model, the treatment effect of the intervention is over twice as large (41 percentage points) if many voters recently transferred away from neighboring municipalities. Furthermore, we find *no* effect of the intervention on mayoral reelection if few voters recently transferred away from neighboring municipalities.

In order to clarify the distinction between voter buying and other forms of clientelism, Figure 1 provides a typology of electoral strategies using rewards.[3] The typology identifies several important clientelist strategies that target different types of individuals.[4] The vast majority of studies on clientelism assume that political operatives use rewards to influence the vote choices or electoral participation of citizens registered to vote in their district. The left column of Figure 1 identifies four such clientelist strategies. The strategies of "vote buying" and "abstention buying" target registered voters who are likely to turn out. Vote buying induces opposing (or indifferent) voters to switch their vote choices (Lehoucq, 2003; Stokes, 2005), whereas abstention buying induces them to stay home on Election Day (Cox and Kousser, 1981; Heckelman, 1998). By contrast, the strategies of "turnout buying" and "double persuasion" target registered voters who are unlikely to turn out. Turnout buying distributes rewards to unmobilized supporters in exchange for showing up at the polls (Cox, 2006; Nichter, 2008). Double persuasion targets registered voters who are neither machine supporters nor likely vote to show up, and influences their vote choices and electoral participation (Chubb, 1982; Nichter, 2008).

Whereas the four strategies in the left column of Figure 1 influence the actions of the electorate, machines can also use rewards to shape the composition of the electorate. A central point of this paper is that some forms of clientelism target citizens who are *not* registered in a machine's district. The right column of Figure 1 identifies two strategies that target such citizens. "Voter buying,"

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[3]For the purpose of this analysis, rewards are cash or particularistic goods and services (including food and alcohol) given to individuals before an election. Post-election particularistic benefits and public programs are not considered rewards.

[4]The typology is by no means exhaustive (e.g., it does not include strategies of "relational clientelism" that involve ongoing benefits to loyal supporters).

Figure 1: Targeting of Electoral Strategies Using Rewards

**Reward Recipient Registered or Not
Registered in Politician's District**

	Registered	*Not Registered*
Inclined to Vote	**Vote Buying** **Abstention Buying**	**Voter Buying**
Inclined Not to Vote	**Turnout Buying** **Double Persuasion**	**Nonvoter Buying**

Reward Recipient Inclined to Vote or Not Vote

the focus of this paper, rewards citizens who are registered and likely to vote in *other* districts. In return for material inducements, citizens transfer their electoral registration and vote for the machine. "Nonvoter buying," a strategy not examined thoroughly in this study, targets citizens who are neither registered in the machine's district nor likely to vote. These citizens, who may live within or outside the machine's district, receive benefits in exchange for political support.[5]

Of course, parties may in fact engage in a combination of strategies in Figure 1, complicating both formal and empirical analyses. This paper focuses on identifying the understudied strategy of voter buying, and thus the formal model makes simplifying assumptions to illuminate this strategy.

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[5]For example, a dispensary owner in Michigan was charged in 2011 for providing "customers marijuana in exchange for voter registration." The dispensary also "allegedly encouraged the newly registered voters to support a certain slate of candidates running for Lansing City Council based on their position regarding regulation of marijuana dispensaries." See "Schuette, Johnson Announce Charge Against Dispensary Owner For Offering Free Marijuana for Votes," State of Michigan Attorney General, September 12, 2011.

The discussion section at the end of the paper returns to this issue, considering how parties may combine strategies.

Our analysis of voter buying suggests that research on machine politics should pay much closer attention to the relationship between clientelism and electoral fraud. Even though most scholars of clientelism are well aware that political machines often engage in fraud, they rarely examine linkages between the two political phenomena. In fact, most studies of clientelism eschew the topic of fraud altogether.[6] Yet our findings suggest that some machines distribute contingent rewards as a mechanism to induce registration fraud — voter buying is employed to induce both legal and illegal voter transfers.[7] Thus, by restricting their scope of analysis to ignore fraud, studies of clientelism risk misinterpreting why machines deliver some rewards.

Our findings also suggest that research on fraud should investigate more thoroughly the extent to which contingent rewards motivate voters to participate in a broad range of fraudulent activities. Unlike studies focused on clientelism, much of the literature on fraud discusses parties' use of clientelist strategies. In fact, some prominent studies even include clientelist strategies in their *definitions* of fraud (e.g., Lehoucq, 2003, 239; Ziblatt 2009, 4).[8] Despite this inclusiveness at a conceptual level, a crucial question often gets short shrift in the literature on fraud. In cases where electoral fraud involves complicit voters, what motivates citizens to break the law and risk imprisonment or fines?[9] Without denying the fact that voters may participate in fraud strictly due to partisanship, coercion or social ties, we argue that some citizens require material inducements. Our analysis encourages scholars to investigate how clientelist exchanges may underpin fraudulent activities.

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[6]For example, there is no mention of the term "fraud" in studies of clientelism such as Heckelman (1998), Nichter (2008), Rigger (2002), Stokes (2005), and Valenzuela (2002). Studies of clientelism that mention "fraud" only once in passing include: Baland and Robinson (2007), Desposato (2007), and Hicken (2007).

[7]Note that some scholars indicate that even legal electoral strategies may also be considered fradulent if they conflict with democratic norms (e.g., Schedler, 2002; Ziblatt 2009, 4).

[8] Lehoucq (2003, 239) extensively discusses vote buying as a form of electoral fraud, and Ziblatt (2009, 4) explicitly includes both vote buying and turnout buying in his definition of electoral fraud.

[9]One example is registration fraud that involve complicit actions by citizens. Of course, registration fraud need not involve complicit voters, such as when parties register deceased or fictitious "ghost voters."

Within the literature on fraud, the present paper is most closely related to excellent studies by Fukumoto and Horiuchi (2011) and Ichino and Schundeln (2012). Neither study investigates whether citizens receive contingent benefits. Similarly to Fukumoto and Horiuchi (2011), we find that transfers of voters across municipalities have a significant impact on the reelection rates of local politicians. Yet our findings suggest a much greater impact: whereas they find an average effect of just 11 voters in Japanese municipal elections (2011: 597), we find an average effect of 900 voters in Brazilian municipal elections. Fukumoto and Horiuchi (2011, 588) do not thoroughly examine why Japanese voters transfer: they briefly mention that citizens transfer to help candidates' friends or acquaintances, and also in response to pressure from employers or business partners.[10] Our analysis is also complementary to Ichino and Schundeln (2012), in that neighboring municipalities play an important role in both sets of findings. In contrast to our examination of voter buying, their study focuses on the spillover effects of election observers on registration fraud. Most crucially, unlike the present study, neither Fukumoto and Horiuchi (2011) nor Ichino and Schundeln (2012) examine whether citizens receive clientelist benefits.

To investigate the strategy of voter buying, this paper examines qualitative evidence and a regression-discontinuity design in the context of Brazil. But before turning to empirical evidence, we first develop a formal model of voter buying.

2   Formal Analysis

This section takes Stokes's (2005) vote-buying model and Nichter's (2008) turnout-buying model as a point of departure. In order to enhance comparability, we make one basic change — allowing citizens to transfer their voter registration to another district — while following their assumptions. This adaptation suggests that voter buying is incentive-compatible and provides several testable predictions.

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[10]In a closing footnote, the authors mention that "presumably" citizens transfer registrations to obtain "benefits from candidates or core supporters" (599). However, they do not further investigate this possibility.

2.1 Assumptions

Following Stokes (2005, 319) and Nichter (2008, 22), this analysis assumes a one-dimensional policy space. Two political parties compete in the district where voter i lives, with ideological positions of X_1 and X_2. In the absence of rewards, voter i is assumed to vote in her home district. Adapting the setup in Stokes (2005, 319) and Nichter (2008, 22), voter i receives utility:

$$u_i = -\frac{1}{2}(x_i - X_p)^2 - c_i \tag{1}$$

where x_i reflects voter i's position on the ideological spectrum, $X_p = \{X_1, X_2\}$ represents a vote for her preferred party, and c_i is the citizen's voting cost. Equation 1 suggests that voters whose preferences are closer to their preferred party's platform receive greater utility. In addition, voters face costs of voting, including both direct (e.g., transportation) and indirect (e.g., forgone earnings) expenses.[11]

Whereas previous studies ignore the strategy of voter buying, this analysis considers the case in which citizens can transfer their voter registration to another district in response to inducements. If citizen i votes in another district in exchange for a reward, she is assumed to receive the following utility:

$$u_i = b_i - k_i \tag{2}$$

where b_i is the value of the reward to the voter and k_i represents the cost of transferring her registration and voting in the machine's district on Election Day. We assume that $k_i \geq c_i$, which reflects the (weakly) greater costs incurred by a citizen who transfers and votes away from her district relative to costs incurred when voting at home.[12]

Equation 2 suggests that transferring voters do not have political preferences regarding can-

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[11] In contexts with abstention costs, c_i may be considered *net* voting costs (i.e., the cost of voting minus the cost of abstaining. The modification would not affect results.

[12] The discussion below focuses on Brazil, where absentee voting is not possible (with rare exception). Its inclusion would not change the comparative statics. However, it could increase the effectiveness of voter buying by reducing k_i and increasing q (discussed below).

didates in the machine's district. This setup reflects voter buying in contexts such as Brazil, in which the strategy is used during local elections and recipients transfer their registration for voting purposes rather than physically moving to the new district for long periods of time. Given this indifference regarding the remote district, we assume that if a citizen transfers her registration and shows up at the remote poll on Election Day, she will vote for the machine that induced her transfer. Hence, we relax Stokes's (2005, 318) monitoring assumption that machines can violate the secret ballot and monitor vote choices. Instead, the voter-buying model assumes that the machine can monitor, with probability q, whether a citizen transfers her registration and turns out. As with Stokes (2005) and Nichter (2008), we also assume that the machine has knowledge of individuals' ideal points, which are exogenous to rewards. For our purposes, this suggests that a machine can identify the strength of political preferences of a targeted citizen in another district.

Other modeling assumptions closely follow both models. An infinitely repeated Prisoner's Dilemma game is similarly used, in which credible promises and threats about future rewards can influence current behavior. The voter-buying model also assumes that "both sides forsee their interaction extending indefinitely into the future" (Stokes, 2005, 319). In line with Stokes (2005, 319) and Nichter (2008, 24), the voter-buying model assumes that the machine engages in a grim trigger strategy, providing rewards to a particular individual until he or she fails to cooperate, after which it never offers another reward. Also, the authors' structure of the game as one-sided uncertainty is adopted; therefore, no conditions are analyzed in which a political party chooses not to cooperate. The discount factor, β, corresponds to the value today of a dollar to be received one stage later, and is assumed to be sufficiently high to enable sustained cooperation. With these assumptions, we now examine a model of voter buying.

2.2 Model of Voter Buying

This model explores whether a machine can gain votes by providing incentives to voters in other districts. Voter buying is not effective unless interactions are repeated. Without the prospect of

future rewards, a voter in a remote district is better off if she simply accepts a reward and neither transfers her registration nor travel to the distant polls. Within a dynamic setting, cooperation is possible. When voter-buying interactions are repeated, the remote voter may be induced to transfer her registration and travel to the machine's district on Election Day, depending on the value of future rewards. Even though no single-stage outcome is a Nash equilibrium, a subgame-perfect outcome exists if the game is infinitely repeated. Following Stokes (2005, 319) and Nichter (2008, 24–5) , Inequality 3 shows the conditions under which sustained cooperation is possible:

$$\frac{1}{1-\beta}(b-k_i) \geq (-\frac{1}{2}(X_i - X_p)^2 + b - c_i) + \frac{\beta}{1-\beta}((1-q)(b-k_i)$$
$$+ (q)(-\frac{1}{2}(X_i - X_p)^2 - c_i)) \tag{3}$$

The left side of Inequality 3 represents the total discounted value of the rewards an remote voter receives if he or she cooperates during every stage. The right side of Inequality 3 represents the value of the reward a remote voter receives in a given stage if he or she defects by staying in her home district and voting for her preferred party, plus with probability $1 - q$ the discounted value of the future rewards received if he or she is not detected and cooperates in all future rounds. If detected with probability q, she will thereafter vote in her home district, receiving future utility streams from the payoffs described in Equation 1. Overall, Inequality 3 suggests that voter buying will be effective when the discounted value of the payoffs from sustained cooperation is greater than or equal to the discounted expected value if he or she defects in a given period.

Simplifying Inequality 3 shows the reward values (b) for which this condition is satisfied:

$$b \geq \phi[2(k_i - c_i) - (X_i - X_p)^2], \tag{4}$$

$$\text{where } \phi = \frac{1 + \beta(q-1)}{2\beta q}.$$

This inequality will bind, as a political party will use its bargaining power to expend the mini-

mum amount necessary to sustain cooperation. The voter-buying model yields numerous comparative statics. If we assume that a political party has a fixed budget, then voter buying becomes a less effective strategy for obtaining votes as the cost of rewards increases. Therefore:

1. **Cost:** The effectiveness of voter buying increases as the citizen's cost of transferring her registration and voting in the machine's district decreases ($\frac{\partial b}{\partial k} > 0$). Thus, we predict that machines will focus rewards on citizens in nearby municipalities, given lower transportation and opportunity costs.

2. **Monitoring:** The effectiveness of voter buying increases as the machine's ability to monitor whether a citizen transfers and turns out, q, increases ($\frac{\partial b}{\partial q} < 0$). Hence, we predict that machines in small communities will engage in relatively more voter buying, given that monitoring citizens is easier in such contexts (e.g., Stokes, 2005, 319; Nichter 2008, 28).

3. **Targeting:** Voter buying is more effective when machines target individuals whose ideal points (X_i') are far from those of their preferred party (X_p), because their cost of rewards is lower ($\frac{\partial b}{\partial (X_i - X_p)} < 0$). Thus, we predict that voter buying will target citizens who are indifferent or have weak political preferences.

We now investigate empirical evidence of voter buying in Brazil. Findings from qualitative sources and a regression discontinuity design are not only consistent with the model's predictions, but they also provide greater insight into mechanisms and effects of voter buying.

3 Voter Buying in Brazil

A broad range of qualitative evidence suggests that local political machines in Brazil engage in voter buying. This section investigates various examples of the strategy, and is based primarily on official reports and journalistic accounts of criminal cases involving voter buying. At the outset, we emphasize that the goal of this qualitative discussion is to provide insights into how politicians

induce voters to transfer their voter registration in Brazil. Insights gleaned from these sources set the groundwork for more systematic analysis in the next section, which employs a regression discontinuity design.

The qualitative sources reveal evidence consistent with predictions of the model regarding geographic proximity and community size. While the qualitative discussion below does not in any sense provide a rigorous test of the model, for illustrative purposes we report available data pertaining to both predictions. For geographic proximity, we indicate distances involved in voter transfers. For community size, we indicate the population of municipalities to which citizens transferred.[13] Given that reports do not indicate the political preferences of reward recipients, the qualitative evidence below cannot shed light on the third comparative static.

Voter buying is frequently observed across Brazil. Consistent with the model, politicians in smaller municipalities use the strategy to induce the transfers of voters in neighboring municipalities. For example, a local newspaper recently explained that the state of Amazonas is "historically marked by fraudulent transfers of voters on the periphery of Manaus to neighboring towns," which typically "involve free transport on Election Day, cash payments, and promises of work."[14] Indeed, an investigation by the state electoral court of Amazonas found that many voters are induced with "money, gifts and transportation" to transfer from Manaus to small nearby towns, in particular Iranduba (16 miles away; 40,781 citizens), Rio Preto da Eva (50 miles away; 25,719 citizens), Manacapuru (53 miles away; 85,141 citizens), and Presidente Figueiredo (66 miles away; 27,175 citizens).[15] In the state of Pernambuco, prosecutors found that political operatives induced voters from the capital city of Recife to transfer to the municipality of Santa Cruz do Capibaribe (90 miles away; 87,582 citizens). They provided R\$20 (US\$12) immediately and promised an additional R\$20 after the election.[16] In the state of Rio Grande do Norte, federal prosecutors have filed

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[13]Populations mentioned in this section are from the 2010 IBGE Census.

[14]"Juźes Apertam o Cerco Contra Fraudes Eleitorais no AM," *A Critica*, December 9, 2011.

[15]"Corregedor Flávio Pascarelli Anuncia Medidas para Sanear Pendências no Eleitorado de Silves e Manacapuru," *A Critica*, September 4, 2011.

[16]"Fraude Eleitoral é Descoberta," Ministério Público de Pernambuco, March 14, 2008. In this case, state prosecu-

over 100 cases regarding illegal registrations in Timbaúba dos Baptistas (2,295 citizens), many of which involve induced transfers from the neighboring municipality of Serra Negra (23 miles away).[17]

Voter buying often involves public programs. One example involves the municipality of Nova Ipixuna (14,645 citizens) in Pará, where a city councilman provided benefits from the *Seguro Defeso* program to citizens who transferred their voter registration and delivered political support. According to federal charges in 2011, he manipulated the federal program — which provides financial assistance to poor fishermen during breeding season — to reward imported voters who didn't even fish.[18] In another case in São Paulo, landless squatters (MST) were reportedly informed that they would receive plots of land conditional on transferring their voter registration to the municipality of Iaras (35 miles away; 6,376 citizens) and casting votes for the PT mayoral candidate.[19] Federal investigators found that 72 of the landless squatters had indeed transferred their registration to Iaras, a municipality with which they had no ties.

Whereas most voter buying delivers benefits before an election, some local machines rely exclusively on the promise of future benefits. For example, a mayoral candidate in Alvorada do Oeste, Rodônia (16,853 citizens) was convicted of promising future goods to voters in Urupá (26 miles away) and other unidentified neighboring municipalities in exchange for transferring their voter

tors arrested a voter caught red-handed while transferring illegally. They report allegations of similar schemes in other municipalities in Pernambuco. The case also mentioned instances of *nonvoter* buying (i.e., of citizens not registered anywhere). After receiving cash, some teenagers from Recife attempted to register in the smaller municipality, even though they were not yet 16 years old as required by law. The article names no other municipalities from which transfers occurred.

[17]"As Fraudes da Migração Eleitoral," *Correio Braziliense*, July 4, 2010. Prosecutors report that registration transfers were often conducted by partisan attorneys, even though by law voters must personally conduct transfers. As a result, the electoral office and judge are also under investigation.

[18]"Vereador de Nova Ipixuna é Denunciado por Fraude no Seguro-Defeso," Ministério Público Federal no Pará, July 15, 2011. Information was not provided about the locations from which recipients transferred. Similar schemes involving the program were also reported elsewhere in the state.

[19]"MPF/SP Denuncia Transferência Irregular de Títulos Eleitorais á Promotoria Eleitoral de Cerqueira César," Procuradoria da República em São Paulo, July 5, 2011. The landless squatters testified to authorities that workers from INCRA, the federal land reform program, provided them with forms necessary for transferring their voter registration.

registration.[20] According to the prosecutor, the candidate "offered retirement benefits, cash and employment, and warned that he would only pay if elected." At least 150 voters were identified as inappropriately registered in the municipality, and 15 witnesses admitted falsifying addresses when transferring in response to offers of benefits. The candidate, who previously served as mayor and state deputy, was fined and sentenced to nearly 3 years in prison.

Illegal voter transfers have not escaped the attention of Brazilian legislators. In 2011, Federal Deputy Roberto Policarpo Fagundes proposed new legislation in response to newspaper reports of a "pendular" shift of voters between Brasília and small neighboring municipalities in Goiás.[21] Many citizens vote in national elections in Brasília and then transfer their voter registration — often in response to inducements — for mayoral elections. For example, 11 voter-buying recipients were arrested in February 2012 for using false addresses to transfer their registration from Brasilia to Águas Lindas (27 miles away; 159,378 citizens). According to the police chief responsible for the arrests: "The voters in fact don't reside [in the town] and presented false documents. It's a recurring problem. As a rule, candidate promise jobs and benefits to those who transfer their [voter documents]."[22] Deputy Policarpo argued that "this practice happens throughout the country" and proposed a bill to make it more difficult to transfer.[23] In particular, the bill tried to increase the minimum time between registration transfers from one to three years. However, the Constitutional Committee of the Chamber of Deputies recently ruled against the bill because it could impede voting by citizens who genuinely move.

Voter buying in Brazil involves considerable effort by recipients, who must file transfer requests in the electoral office of the new municipality. They must apply in person and present a federally issued ID, birth or marriage certificate, and proof of military service or exemption.[24] In addition,

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[20]"Ex-Deputado é Preso por Fraude Eleitoral em Rondônia," Agência Folha, June 27, 2000.

[21]"Cerca de 47,4 Mil Moradores do Entorno Passam a Votar na Capital," *Correio Braziliense*, September 5, 2010.

[22]"Polícia Federal Prende 11 por Fraude Eleitoral no DF e Entorno," *Globo (G1)*, February 9, 2012.

[23]Projecto de Lei 1866/2011, Câmara dos Deputados, July 14, 2011. See also "Projeto Aumenta Tempo Mńimo para Mudança de Domicílio Eleitoral," Câmara dos Deputados, December 20, 2011.

[24]Since 2007, Brazilians can fill out their own information on the internet to expedite registration, but must still go to electoral offices to show ID and documentation.

they must have voted or justified their absence in all previous elections, or otherwise pay abstention fines.[25] Transfer requests must be filed at least 150 days prior to an election, and citizens may only transfer once per year. Citizens must also be residents of the new municipality for at least three months before applying for transfers.

This residential requirement does not pose a substantial barrier for voter buying in Brazil for two reasons. First, voters self-declare where they live "under penalty of law," and their statements are typically "presumed to be true" without official verification.[26] And second, the concept of "electoral residence" extends well beyond where someone lives. Brazilian electoral law employs a definition that is "more flexible and elastic, identifying residence with the place where the applicant has political or social links."[27] If a citizen does not physically reside in a municipality, a citizen can claim various other links to the municipality. It is at the discretion of the local electoral judge, "on a case-by-case basis," whether the rationale is deemed sufficient to warrant a transfer.[28] For example, an electoral judge in Amazonas even approved a voter's claim of residence on the basis that he was romantically involved with someone in the municipality (Ramalho 2008: 23).

Political operatives in local electoral offices often facilitate voter buying. Electoral judges, who approve transfer requests, are unlikely to be involved in such schemes.[29] Relatively more likely to be complicit are municipal employees appointed by the mayor who work within electoral offices.[30] One such municipal worker was arrested in 2010 for facilitating voter buyer in Alexânia, Goiana (23,814 citizens): she processed illegal transfers upon receiving voter documents from a

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[25]This requirement applies for all citizens for whom voting is mandatory (i.e., literate voters aged 18 to 70 years).

[26]See Electoral Laws 6.996/1982, 7.115/1983, and 21.538/2003. However, in some instances, voters may be asked to show proof of residence.

[27]Tribunal Superior Eleitoral, Acordão 16.397/2000.

[28]Interview with Cibelle Barbosa, Tribunal Regional Eleitoral de Minas Gerais, January 30, 2011.

[29]Although electoral judges were heavily influenced by local politicians during much of Brazil's history, most observers now consider them to be relatively well insulated from municipal politics. Electoral judges are appointed for two-year terms from the corpus of state judges, who are selected by competitive exam (Artigo 121, Constituição da Republica Federativa do Brasil, 1988). Electoral judges can be consecutively reappointed at most once, in which case they are rotated to a new municipality.

[30]Since 2004, civil service workers selected by competitive exam have been appointed to every electoral office in Brazil (Lei 10.842/2004). However, many electoral offices have insufficient civil service personnel and therefore rely additionally on municipal employees appointed at the local level.

candidate's nephew.[31] The transferred voters, who received transportation money (and likely other benefits), came from the neighboring municipalities of Anápolis and Pirenópolis (41 and 40 miles away, respectively). The Federal Police arrested the municipal worker as well as two candidates for city council, and reported that 2,000 irregular voting documents were used in the municipality during the 2008 election.

Overall, the qualitative evidence above provides considerable insight about the mechanisms and logic underlying voter buying in Brazil. In addition, findings from qualitative sources are consistent with two predictions derived from our model's comparative statics: (1) machines focus rewards on neighboring municipalities; and (2) voter buying is particularly employed by machines in small municipalities. We now discuss an intervention that enables us to investigate more rigorously how voter buying affects the continuity of power during local elections Brazil.

4 Voter Buying and Revisions

4.1 Overview of Revisions

We investigate the impact of voter buying by an examining the consequences of an intervention designed to combat the phenomenon. Brazil's electoral governance body, the *Tribunal Superior Eleitoral* (TSE), requires municipalities to re-register all voters — a procedure called "electoral revisions" — if they meet specific criteria. The explicit purpose of these revisions is to combat fraudulent registration in the electoral rolls, including voters induced to transfer through voter buying. As the president of the state electoral court in Minas Gerais recently explained, "what justifies a revision is the existence, in the electoral rolls, of voters who don't have links with a municipality ... In municipal elections, one vote determines the selection of the mayor."[32] Similarly, a state deputy in Mato Grosso argued that revisions improve elections of municipalities by ensuring

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[31]"PF Ouve Suspeitos de Fraude," *Jornal Estado de Goiás*, April 10, 2010.
[32]"Presidente do TRE Acompanha Início da Revisão com Biometria em Itaguara," *Tribunal Regional Eleitoral de Minas Gerais*, September 1, 2011.

that "only people who actually live there" will vote.[33] To examine the impact of voter buying on mayoral elections, our analysis takes advantage of the fact that municipalities are forced to re-register the entire electorate if they exceed an arbitrary threshold.

Electoral revisions are mandated in municipalities that meet three criteria: (1) the electorate exceeds 80 percent of the population; (2) the electorate is at least double the summed population of citizens aged 10-15 and over 70 years old; and (3) the total number of voter transfers increased at least 10 percent over the past year. Our analysis focuses on first criterion, which is frequently discussed in TSE documents as the most important factor influencing qualification. In the average municipality during 2007, the electorate represented 74.4 percent of the population, and 1,483 of Brazil's 5,564 municipalities exceeded the 80 percent threshold. In 140 municipalities, the electorate was even greater than the population, which is possible given the flexibility of electoral residency requirements. As an extreme example, the municipality of Serrano do Maranhão (MA) had 7,158 registered voters despite a population of just 3,972.[34]

The TSE conducts a wave of revisions in between election years at which time it determines which municipalities must re-register all voters. We focus on the 2007 electoral revisions process, for which a relatively broader range of data were available. Given that revisions are not typically permitted during years in which elections are held, there had not been a wave of revisions since 2003.[35] Another relatively minor source of revisions is state electoral courts, which can authorize them in municipalities with substantial complaints of registration irregularities. Before states can order revisions in this manner, they must make home visits to five percent of voters (randomly selected) in the municipality and show that a significant number of selected voters are erroneously registered.[36] The quantitative analysis, which focuses on the 80-percent criterion discussed above,

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[33]"Rezende Acredita que Correição Garante Eleições Mais Transparentes," *Várzea Grande*, August 10, 2004. Rezende explains that both revisions and corrections have this benefit by resolving registration irregularities.

[34]"TREs Agendam Revisões de Eleitorado em 1023 Municípios até o Dia 31 de Dezembro," *Tribunal Superior Eleitoral*, October 27, 2007.

[35]There were municipal elections in 2004, a national arms referendum in 2005, and a national election in 2006.

[36]What constitutes a "significant" number of irregularities is at the prerogative of each state. States must file TSE documentation to confirm that such random samples have been conducted and that there are sufficient irregularities to

controls for factors such as the ability of states to conduct additional revisions as well as the two other national-level (TSE) criteria.

Given that a major objective of the revisions process is combating illegal transfers, it is important to note that revisions predominantly target small municipalities. Such contexts are precisely where the model predicts that voter buying will occur, given the greater ability of machines to monitor reward recipients. Figure 2 shows that the key variable that mandates revisions — electorate as a percentage of population — is inversely related to municipality size. Also consider that in 2007, revisions were conducted in 23 percent of Brazilian municipalities (1,278 of 5,565 municipalities). Because revisions target small municipalities, only 5.4 percent of Brazil's electorate (6.81 million voters) were required to re-register.[37] Overall, we emphasize that this targeting of small municipalities: (1) is consistent with the model's prediction of voter buying where machines can monitor citizens; and (2) suggests that municipalities near the discontinuity would be a particularly relevant sample to test for the effects of voter buying.

Under TSE supervision, each state electoral court (*Tribunal Regional Eleitoral*, or TRE) organizes the re-registration of voters in selected municipalities. Municipalities must commence the revision process within one month of receiving TRE instructions. Local electoral judges use various methods to publicize the requirement that all citizens re-register, such as television, radio, and newspaper announcements for at least three days before the revision period. The revision period lasts at least 30 days, with locations open for re-registration at least six hours daily including Saturdays. To re-register, a citizen must personally travel to one of the offices and present his or her identification card, proof of residence and voting card. Proof of residency can be shown using various documents, such as utility bills (issued between 3 and 12 months before the revision) or checkbooks (showing the voter's name and address). Electoral residency can also be established on the basis of professional, family or community ties. The electoral judge has discretion over

warrant a revision.

[37]"Quase 7 Milhões de Eleitores Deverão Comparecer ao Cartório Eleitoral para Regularizar Sua Situação," *Tribunal Superior Eleitoral*, September 12, 2007.

Figure 2: Relationship between Municipality Size vs. Revision Trigger
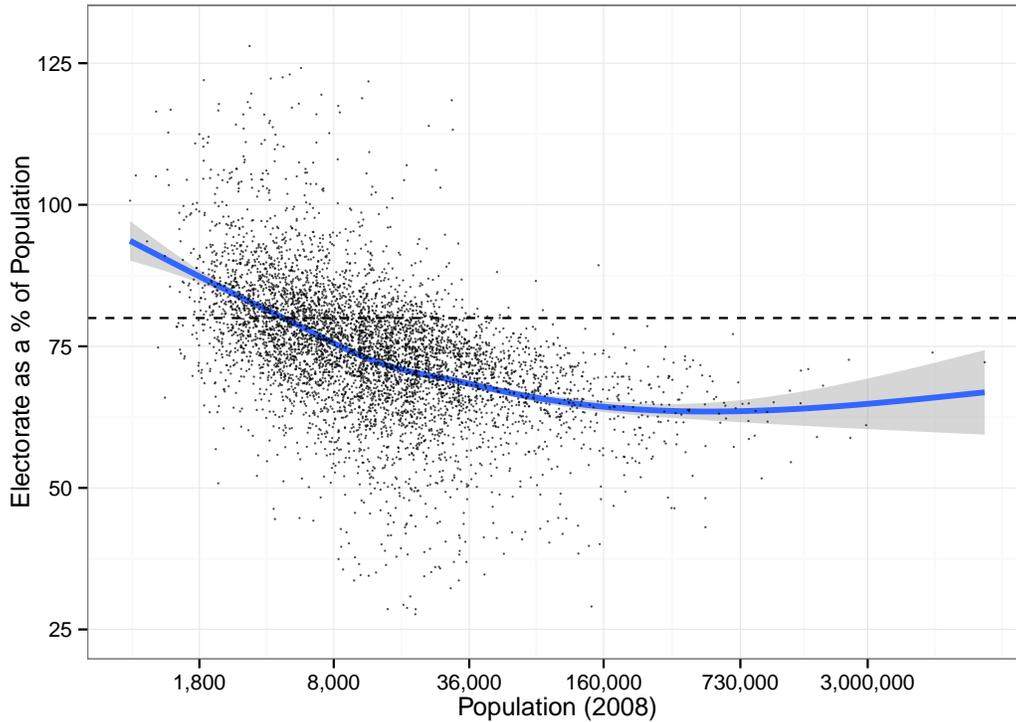
Horizontal axis is logged population. Each dot is a municipality and the blue line represents the conditional mean.

whether a voter meets residency requirements, and can request additional documentation or even order a home visit. Voters who show up and meet the requirements are issued new voting documents. After the revision is completed, the local electoral office publicizes a list of citizens who have not re-registered, providing them with another opportunity to meet requirements. After a waiting period and approval of the revision, the state electoral courts cancel the voter registration of citizens who failed to re-register.[38]

There are considerable differences between municipalities with and without revisions, highlighting the benefits of using a regression discontinuity design. As discussed above, population sizes differ substantially — the median size of municipalities with revisions is less than half of

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[38]TREs manage the voting rolls under the supervision of the national TSE.

those without revisions. In addition, there is considerable geographic heterogeneity of revisions. During 2007, revisions were conducted in 42 percent of municipalities in Bahia and 40 percent of municipalities in Piauí. By contrast, in the same year, no revisions were conducted in the states of Amapá, Amazonas, Roraima or Sergipe. Given various differences between municipalities with and without revisions, we exploit the 80 percent electorate-to-population discontinuity threshold to examine the causal effect of revisions. While this method enables us to isolate the causal effects of revisions from such differences, we acknowledge that its strong internal validity involves an important tradeoff. Given that the analysis relies on a relatively small subset of Brazilian municipalities, we must be careful when drawing inferences regarding the full country. For example, the effects of revisions in the smaller municipalities that comprise our sample may be quite different than the effects in major cities. Notwithstanding this caveat, the regression discontinuity design offers rigorous evidence about revisions, consistent with both the voter-buying model and qualitative findings.

4.2 Hypothesized Effects of Revisions

Before turning to quantitative analyses, we hypothesize the ways in which revisions undermine voter buying and thereby affect elections in Brazil. This discussion is based primarily on predictions of formal model developed in Section 2.2. The model suggests that the effectiveness of voter buying is inversely related to a citizen's cost of transferring her registration and voting in the machine's district. Because revisions require citizens to incur additional costs of re-registering, they consequently act as a negative shock to the effectiveness of voter buying. As a result, some voter-buying recipients will not re-register, contributing to a decline in the electorate. Given that these recipients are relatively more likely to participate in elections than other citizens, we also expect turnout to fall. After all, the model predicts that individuals who receive sufficient voter-buying rewards are induced to turn out because by complying with their side of the bargain, they can receive future rewards.

19

If local political machines use voter buying as a strategy to maintain power, we would expect revisions to impinge on mayoral performance in elections. More specifically, revisions' shock to the effectiveness of voter buying would reduce incumbent machines' likelihood of winning reelection.[39] This adverse effect would be expected to depend on how much they rely on voter buying. Thus, we predict a greater impact on mayoral reelection when many voters recently transferred into the municipality. Given the model's prediction that machines target citizens in nearby municipalities, we also expect reelection to fall more sharply when many voters recently transferred out of neighboring municipalities. Another source of heterogeneity involves municipality size, given that the model predicts that machines tend to use voter buying where they can more effectively monitor citizens. Thus, we expect revisions to more seriously damage mayoral reelection prospects in less populous municipalities, where easier monitoring leads machines to rely more heavily on the strategy.

To summarize, the logic of voter buying offers the following hypotheses about the effects of electoral revisions:

1. **Electorate:** Revisions decrease the electorate in municipalities with voter buying.

2. **Turnout:** Revisions decrease turnout in municipalities with voter buying.

3. **Incumbent Re-election:** Revisions decrease the likelihood of mayoral reeleection where incumbent machines engage in voter buying.

4. **Heterogeneity by Incoming Transfers:** The negative effect on mayoral reeleection will be greater if many voters recently transferred into the municipality.

5. **Heterogeneity by Neighbor Transfers:** The negative effect on mayoral reeleection will be greater if many voters recently transferred out of neighboring municipalities.

6. **Heterogeneity by Municipality Size:** The negative effect on mayoral reeleection will be greater in less populous municipalities.

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[39]An implicit assumption is that machines engaging in voter buying are relatively more likely to be incumbents than challengers. This assumption is consistent with most of the literature on clientelism. It by no means suggests that *all* machines are incumbents.

We now test these predictions using a regression discontinuity design. At the outset, it should be emphasized that we do not claim that voter buying is the *only* factor that would cause any given one of these predictions. However, the empirical results confirm all six predictions, which taken together provide compelling evidence that voter buying explains important patterns in the Brazilian data.

5   Quantitative Analysis

5.1   Research Design

We now investigate a regression discontinuity (RD) design that exploits the 2007 electoral revisions (i.e., mandatory re-registrations of all voters in a municipality). Most revisions were triggered by the criterion of a municipality's electorate exceeding 80 percent of its population. The discontinuous increase in revision status at the 80 percent threshold can be observed in Figure 3, a histogram showing the number of municipalities (in 1 percent bins) by revision status. As shown in Figure 3, the 80-percent rule was not completely deterministic. Election officials did not conduct revisions in 25 percent of the municipalities above the threshold, as they did not meet the other two TSE criteria mentioned in Section 4. Furthermore, they *did* conduct revisions in five percent of municipalities below the threshold, due to the state-level process described in Section 4. Because assignment of the revision was not completely deterministic, standard methods used with "sharp" discontinuity designs are inappropriate as they would yield biased estimates of revisions' effects. To account for this "fuzzy" discontinuity, we treat the issue as a non-compliance problem.

Formally, let R_i be a binary variable denoting whether or not municipality i underwent a revision and let E_i be the "forcing" variable (i.e., electorate as a percentage of the population). If $E_j \geq 80$, then $A_i = 1$, otherwise let $A_i = 0$. Using the Neyman-Rubin (Neyman, 1990; Rubin, 2005) potential outcomes notation, we write $Y_i(R_i, A_i)$ for the realized outcome when a municipal-

Figure 3: Forcing Variable Histogram
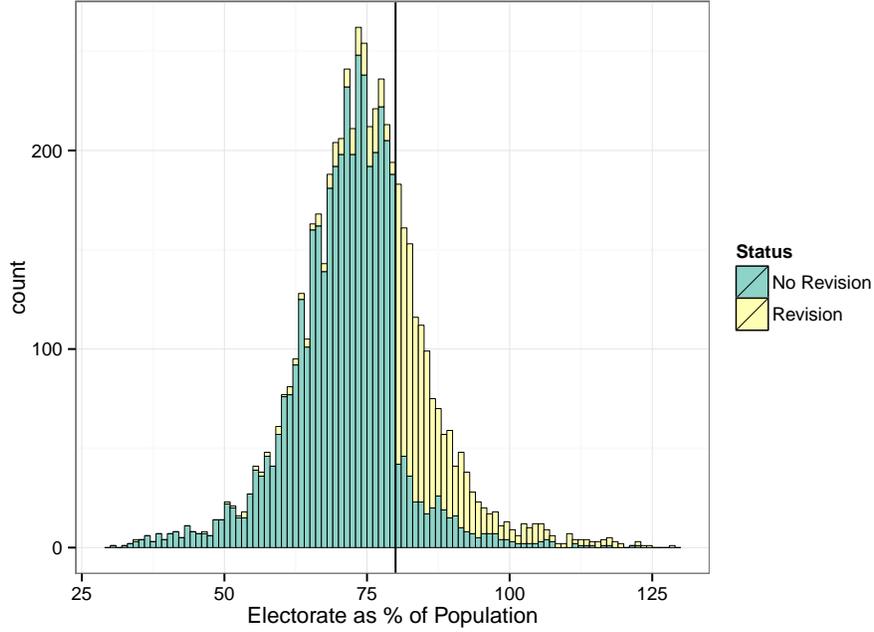
Dark bars show the number of municipalities in each bin with no revision and light bars show the number of municipalities with a revision. The black vertical line is the discontinuity threshold.

ity receives a revision (or not), and when it is above the threshold (or not). A necessary assumption in applications of RD designs is the smoothness of potential outcomes at the discontinuity (Hahn, Todd and Van der Klaauw, 2001). This assumption is reasonable when agents lack precise control over the realized value of the forcing variable (Lee, 2008), as is the case with revisions. While continuity of potential outcomes has observable implications that we test for below, it is a fundamentally unverifiable assumption. Assuming continuity, our RD design allows us to estimate the following quantity:

$$\tau_A = \mathbb{E}[Y_i(R_i, 1) - Y_i(R_i, 0)|E_i = 80]$$

In our case, τ_A is effect of a municipality being above or below the 80 percent threshold (the reduced form estimate) at the discontinuity point. While such a quantity is of interest and we estimate it below, of greater interest is the effect of the revision itself. We make the following

exclusion restriction:

$$Y_i(R_i, 1) = Y_i(R_i, 0)$$

This exclusion restriction says that being above or below the threshold only influences the outcome through its effect on whether or not a revision occurs and not through any other channels. In our case, this assumption is reasonable given that no other interventions — to the best of our knowledge — are triggered at this threshold, nor are there substantive reasons why being just above or below the threshold would directly change political outcomes.

In addition to the exclusion restriction and smoothness of potential outcomes, identification of our desired estimand requires $\mathbb{E}[R_i(1) - R_i(0)|E_i 0] \neq 0$, or in words, that there is a non-zero average effect of being above or below the 80 percent threshold for revision status. As is clear from Figure 3, this condition is met in our data as there is a sharp increase in the number of revisions precisely at $E_i = 80$. The combination of the smoothness assumption and the exclusion restriction enable the following expression to be written:

$$
\begin{aligned}
\tau_R &= \mathbb{E}[Y_i(1, A_i) - Y_i(0, A_i)|E_i = 80, R_i(1) > R(0)] \\
&= \frac{\mathbb{E}[Y_i|A_i = 1, E_i = 80] - \mathbb{E}[Y_i|A_i = 0, E_i = 80]}{\mathbb{E}[R_i|A_i = 1, E_i = 80] - \mathbb{E}[R_i|A_i = 0, E_i = 80]}
\end{aligned}
$$

In words, τ_R is the treatment effect for those municipalities with $E_i = 80$ that received a revision as a result of being above the 80 percent threshold.[40] This quantity can be estimated from the data, under the assumptions maintained above, by computing the ratio of the difference in outcomes to the difference in compliance status at the discontinuity.

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[40]This estimand is known as the local average treatment effect for compliers, i.e. those municipalities that received the revision as a result of being above the discontinuity threshold with an $E_i = 80$. Because non-compliance for municipalities with $E_i < 80$ is very low in the data, in our particular case, this estimand is very close to local average treatment effect on the treated. For more on this point see Angrist and Pischke (2008, 164).

5.2 Specification

As established above, our design identifies the average treatment effect on the treated for munic-
ipalities with pre-revision electorates that are 80 percent of their population. Actually computing
an estimate of this quantity requires two primary choices: the estimator and the amount of data
to retain around the discontinuity (i.e., the "bandwidth"). We present results using two common
specifications in the regression discontinuity literature: a local linear estimator and a difference-
in-means estimator. For the local linear estimator, we use a bandwidth of ± 4 percent, and for the
difference-in-means estimator, we use a bandwidth of ± 1.5 percent. A certain degree of arbitrari-
ness is inevitable when choosing the amount of data around the threshold to retain, so our main
criterion was whether or not our chosen bandwidth produced good covariate balance on a wide
range of variables, while leaving a sufficient amount of data to estimate treatment effects with
some precision.[41]

The difference-in-means estimator of τ_R is simply the ratio of the difference in average out-
comes within the discontinuity window (80 ± 1.5) to the difference in the fraction of municipalities
that received the revision between municipalities above and below $E_i = 80$ within the discontinu-
ity window. For the local linear estimates, we use the ratio of two parameter estimates from the
following expressions:

$$\underset{\alpha_1, \beta_1, \tau_{FS}, \gamma_1}{\operatorname{argmin}} \sum_{i=1}^{N} 1\{-4 \geq E_i - 80 \leq 4\}(R_i - \alpha_1 - \beta_1(E_i - 80) - \tau_{FS} \cdot A_i - \gamma_1 \cdot (E_i - 80) \cdot A_i)^2$$

$$\underset{\alpha_2, \beta_2, \tau_{FS}, \gamma_2}{\operatorname{argmin}} \sum_{i=1}^{N} 1\{-4 \geq E_i - 80 \leq 4\}(Y_i - \alpha_2 - \beta_2(E_i - 80) - \tau_A \cdot A_i - \gamma_2 \cdot (E_i - 80) \cdot A_i)^2$$

The first expression is the local linear estimator for the first stage and the second expression is used

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[41] An alternative is to use automatic bandwidth selection algorithms such as the cross-valdiation procedure suggested
by Ludwig and Miller (2007) and the algorithm proposed by Imbens and Kalyanaraman (2009). We 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, we chose to use covariate balance as our main criterion for bandwidth selection.

to estimate the effect of being above or below the discontinuity on the outcome of interest. The estimated effect of the revision at $E_i = 80$ is

$$\hat{\tau}_R = \frac{\hat{\tau}_A}{\hat{\tau}_{FS}}$$

For statistical inference, we use Hubert-White ("robust") standard errors which account for any heteroskedasticity.

5.3 Data

In order to test our hypotheses, we constructed a dataset based on electoral data from Brazil's electoral governance body (TSE),[42] and demographic data from the Institute of Applied Economic Research (IPEA).[43] For the forcing variable, we employed TSE documents to determine the exact numbers used to determine eligibility for treatment. Specifically, the correct forcing variable is the ratio of June 2006 electorate to the July 2006 population.

Mayors are only permitted to serve two consecutive four-year terms in Brazil. Thus, for all specifications examining incumbent re-election in 2008, we consider municipalities with first-term mayors. We determined eligibility for re-election in 2008 by matching names of 2000 mayoral winners to names of 2004 mayoral winners. Descriptive statistics for all variables are shown in the appendix.

5.4 Assessing the Validity of the Design

Figure 4 displays balance statistics for 19 covariates for three different specifications. For each variable and for three different specifications, we plot the standardized difference (estimated difference scaled by the pooled standard deviation) and the p-value from a t-test of equal means. We

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[42]Most of the electoral data are available at `http://www.tse.gov.br`, though some data was obtained directly from TSE officials.

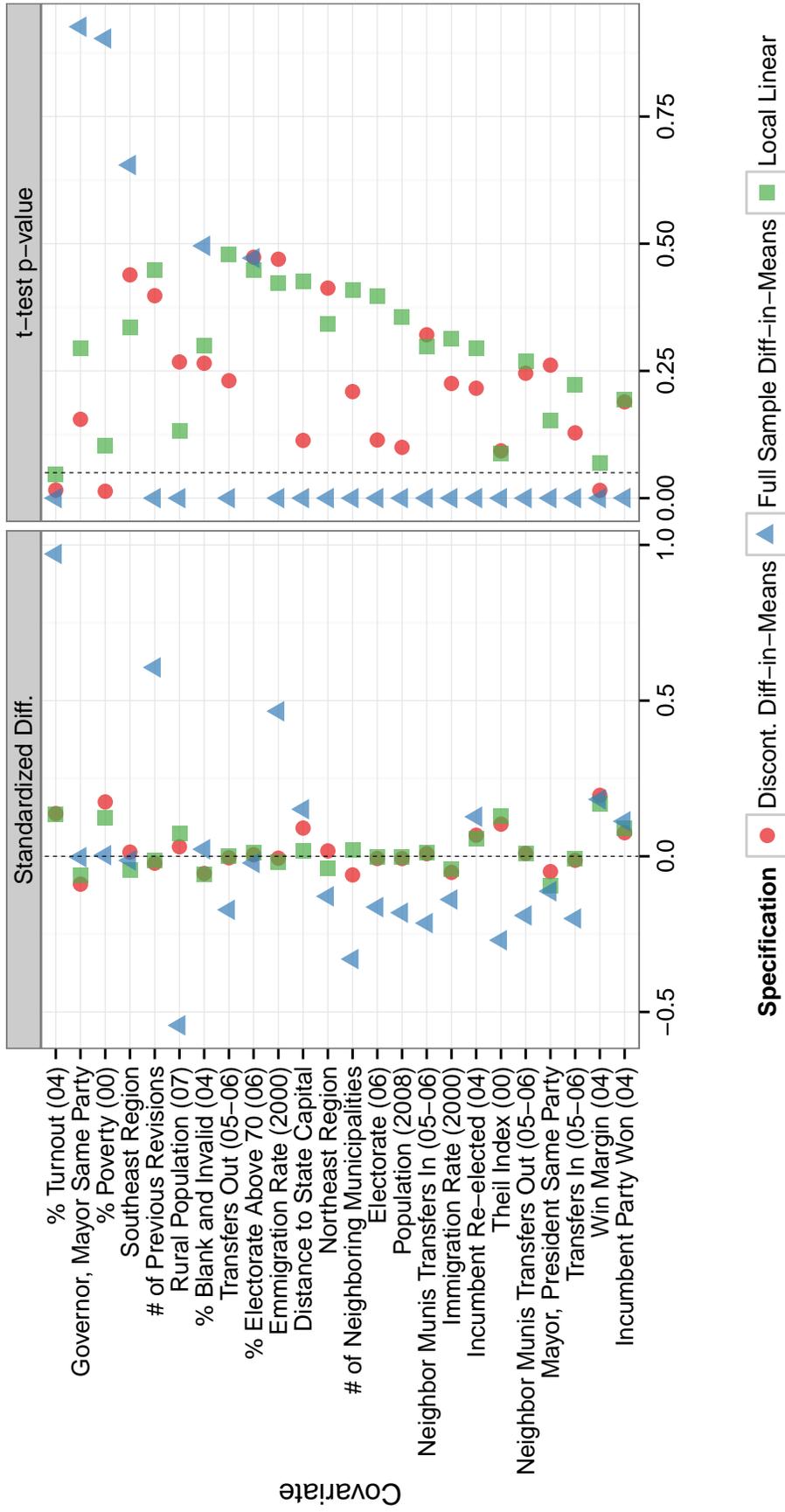[43]These data are available from `http://www.ipeadata.gov.br`.

Figure 4: Covariate Balance. This plot shows covariate balance using three different specifications: a difference-in-means estimator using the full sample, a difference-in-means estimator with a bandwidth of 1.5 percent, and a local linear estimator with a bandwidth of 4 percent. The panel on the left shows standardized differences, which is the estimate divided by the pooled standard deviation. The panel on the right shows a p-value from t-test of equality of means.

show balance statistics for the full sample of 5,547 municipalities (difference-in-means), as well as the local linear estimate and the difference-in-means using a discontinuity sample with bandwidths of 4 percent and 1.5 percent, respectively.

Figure 4 demonstrates that balance increases dramatically on most covariates according to both metrics. For the local linear specifications, 18 of the 19 covariates are balanced in that they are not statistically significant (using a .05 level). The only unbalanced variable is turnout (in 2004). For the difference-in-means specifications, 16 of the 19 covariates are balanced (also using a .05 level). The unbalanced variables are turnout (in 2004), win margin (in 2004), and poverty (in 2000). One consideration that mitigates potential concern about the imbalance of win margin and turnout (both in 2004) is that the sign of the coefficients actually make it even *less* likely that our empirical results would confirm Hypotheses 2 and 3 in Section 4.2. In particular, in the difference-in-means discontinuity sample, treatment municipalities have slightly *higher* turnout rates and vote margins. Potential bias arising from the observed imbalance on these variables would reduce the likelihood that we would find the hypothesized negative effects on turnout and incumbent electoral performance (in 2008). To check for the robustness of our results, we control for all variables with standardized differences higher than 0.1 using parametric covariate adjustment and find no change in our substantive conclusions. Furthermore, when examining vote shares, we use the difference between 2008 and 2004 values as our dependent variable. This step is equivalent to introducing a municipality-specific fixed effect, which would help to control for time-invariant sources of bias.

5.5 Results

Revisions removed a substantial number of voters from the electoral rolls, as shown in Figure 5a. This finding, which we would expect given the overall objectives of the revisions, is consistent with voter buying (Hypothesis 1 in Section 4.2). The plot shows the conditional expectation of the size of the electorate (as a percentage of the voting age population) on both sides of the 80 percent

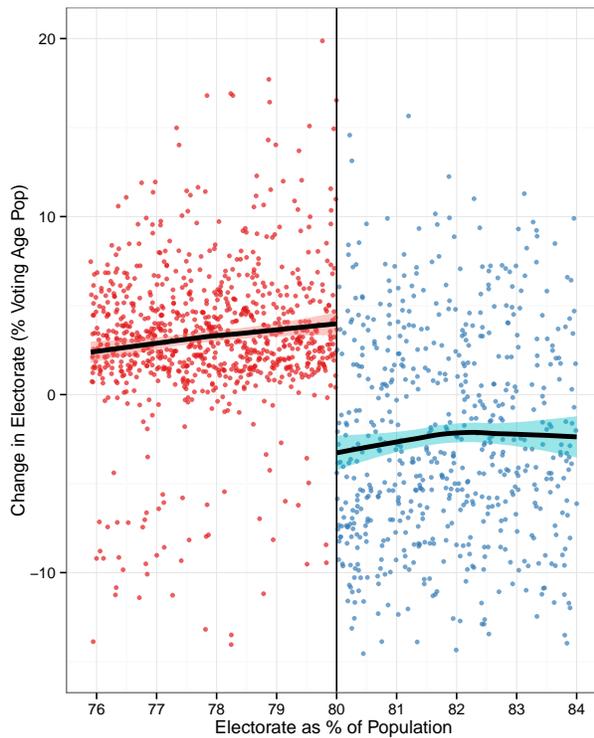Figure 5: (a) Effect on Electorate   Figure 5: (b) Effect on Turnout
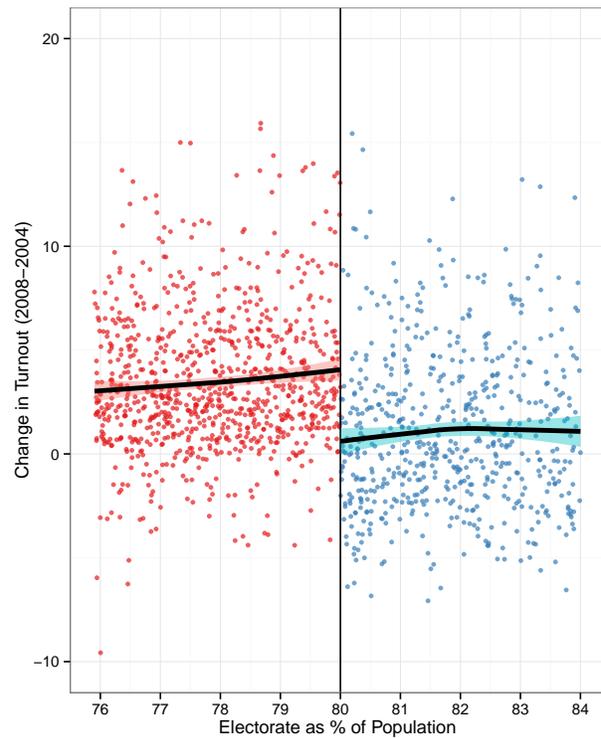

Figure 5a shows the effect of the revision on change in the size of the electorate (as a percentage of the voting age population). Figure 5b shows the effect on change in turnout (as a percentage of the voting age population). Each dot is a municipality. Dots to the right of the vertical line exceed the 80 percent threshold for revision eligibility. The black line is the estimated conditional expectation of the dependent variable as estimated using a locally weighted polynomial regression.

eligibility threshold. Municipalities on the left side of the 80 percent discontinuity had a 4 percent increase in population between 2004 and 2008, while those on the right side experienced a decrease of 3 percentage points, corresponding to a reduced form estimated effect of -7.1 percentage points. As shown in Table 1, when non-compliance is taken into account, the estimated treatment effect (τ_R) rises to -9.7 percentage points. This treatment effect implies that the revision removed roughly 1,900 voters from the typical municipality.

The logic of voter buying also predicts that revisions decrease turnout (Hypothesis 2 in Section 4.2). Findings support this hypothesis. As shown in Figure 5b, there is a clear difference in turnout

Table 1: Main Results: Effect on Size of the Electorate, Turnout, and Incumbent Performance

Outcome	$\hat{\tau}_A$	SE_{τ_A}	$\hat{\tau}_R$	SE_{τ_R}	Baseline	n
Difference-in-Means Specification						
Electorate Change (%)	−6.64	0.44	−9.13	0.49	3.85	577
Turnout Change (%)	−3.05	0.34	−4.22	0.45	4.01	577
Incumbent Wins	−0.12	0.05	−0.16	0.07	0.52	428
Incumb. Party Wins	−0.08	0.04	−0.11	0.05	0.36	577
Incumbent Runs Again	−0.10	0.04	−0.14	0.06	0.76	428
Change in Incumbent Vote Share (%)	−3.15	1.17	−4.58	1.71	6.12	307
Local Linear Specification						
Electorate Change (%)	−7.09	0.55	−9.67	0.58	4.22	1477
Turnout Change (%)	−3.33	0.41	−4.64	0.53	4.25	1477
Incumbent Wins	−0.15	0.06	−0.18	0.08	0.53	1107
Incumb. Party Wins	−0.11	0.05	−0.13	0.07	0.36	1477
Incumbent Runs Again	−0.08	0.05	−0.10	0.07	0.75	1107
Change in Incumbent Vote Share (%)	−3.11	1.40	−4.43	1.95	6.16	802

$\hat{\tau}_A$ is the local average effect of a municipality having an electorate more than 80% of its population ("reduced form") and SE_{τ_A} is its associated standard error. $\hat{\tau}_R$ is the estimated local average effect of the revision and SE_{τ_R} is its standard error. Standard errors are White-Huber ("robust") standard errors. Baseline is the estimated value of the dependent variable among controls at $E_i = .8$ For the "Incumbent Wins", "Incumbent Runs Again", and "Incumbent Vote Share" variables, only municipalities where the incumbent is eligible for re-election is retained in the sample.

trends right at the $E_i = 80$ threshold. As shown in Table 1, the magnitude of this decrease in turnout is 3.3 percentage points, or 4.6 percentage points once non-compliance is adjusted for (using the local linear specification). According to these estimates, a substantial number of voters who would have voted in 2008 were prevented from doing so due to revisions. For the typical municipality, this treatment effect represents a decrease of about 900 voters. Comparing the turnout and electorate effects, approximately half of the voters purged from the rolls would have otherwise participated in the 2008 election.

If machines use voter buying as a strategy to retain power, we also expect revisions to undermine the electoral performance of incumbents (Hypothesis 3 in Section 4.2). The effect of revisions on incumbent reelection could occur through two channels: (1) the decision to run for office again, and (2) the share of votes received if the incumbent decides to run. Our primary

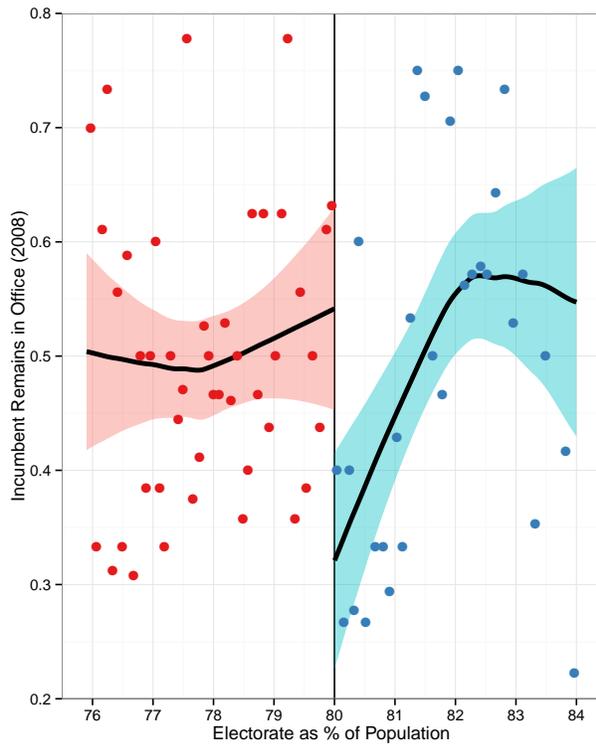Figure 6: (a) Effect on Incumbent Victory   Figure 6: (b) Effect on Incumbent Party Victory
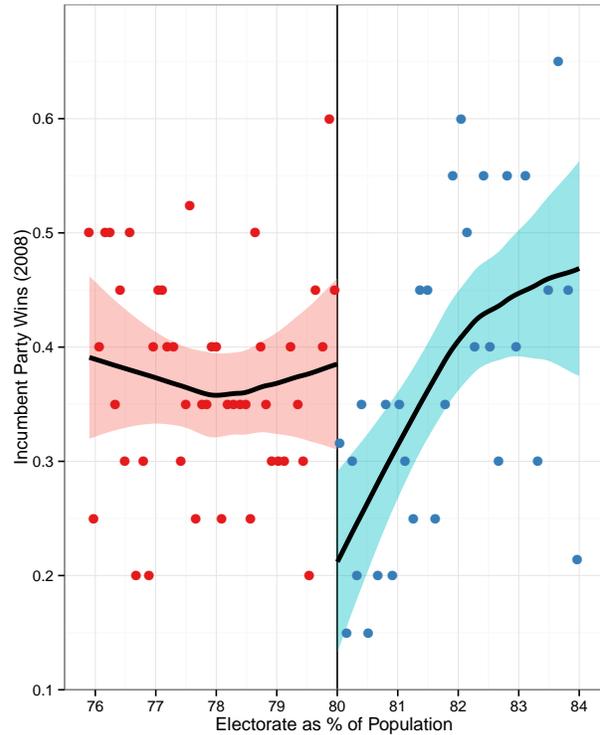

Figure 6(a) shows the effect of a revision on incumbent reelection in 2008. The right panel shows the effect on incumbent party reelection in 2008. Each dot represents the average of the dependent variable in a bin containing 20 municipalities.

dependent variable is simply whether or not the incumbent retained power, regardless whether he or she ran for reelection. The reason we formulate the variable in this way is that incumbents who rely on voter buying may strategically choose to not re-run because they know victory is more challenging after a revision. As a result, conditioning on the decision to re-run would risk post-treatment bias (Rosenbaum, 1984). However, as a robustness check, we also present treatment effects on incumbent vote share. In addition to examining effects on incumbent victory and vote share, we also examine revisions' effect on whether or not the incumbent *party* wins again. This estimand does not require us to condition on eligibility for re-election, thus allowing for a larger sample.

Figure 6a presents our main result on incumbent continuity of power. Findings are consistent

with the hypothesis that revisions — by acting as a negative shock to the effectiveness of voter buying — harm the political fortunes of the incumbent machines. Each dot in Figure 6a represents the fraction of incumbents who win reelection (in bins of 20 municipalities). A clear discontinuity exists between the average rate of reelection immediately above and below the 80 percent cutoff. As reported in Table 1, the reduced form estimate of the revision on the probability of the incumbent remaining in office is -.15 for the local linear estimate and -.12 for the difference-in-means estimate. The local average effect of the revision is -.18 (or -.16, depending upon the specification) and is statistically significant at the 5 percent level. To put these estimates in perspective, an incumbent in a typical municipality has a baseline probability of reelection of 52 percent, but after a revision his or her probability of victory falls to only 34 percent.

Figure 6b is also consistent with Hypothesis 3. Rather than focusing on incumbent mayors, this plot considers the effect of revisions on the reelection prospects of incumbent *parties*. We find statistically significant, albeit smaller, negative effects on the probability of incumbent party victory. As presented in Figure 6b and Table 1, the effect is statistically significant using both specifications. Note that this party effect result appears to be entirely driven by the effect on the incumbent himself: when we estimate the effect on the sample of municipalities with non-incumbent candidates of the incumbent party, the point estimate is small and statistically insignificant (not shown).

As noted above, the effect of the intervention on the incumbent retaining power could occur through the incumbent's decision about whether to run and/or their performance in the election. We find evidence of both mechanisms. The point estimates on the probability of the incumbent re-running are negative and on the order of about -.14 (difference-in-means) or -.10 (local linear estimate), albeit imprecisely estimated and only significant at conventional levels when using the difference-in-means specification. While the estimate is not consistently significant, the point estimates are large enough to suggest that strategic dropout could be one of the mechanisms by which revisions affect whether incumbents remain in power.

Despite sample selection problems potentially induced by strategic candidate dropout, we also

31

estimate the effect of the revision on incumbent vote share, conditional on the incumbent re-running. Note that the sample selection bias induced by differential candidate dropout would most likely downwardly bias our estimates since weaker candidates would be less likely to run again. Under the assumption that these weaker candidates would have been more affected by the revision than those who chose to stay in the race, sample selection bias would result in an estimated effect *smaller* in magnitude than the true effect. Ignoring this issue, we find a statistically significant effect of about -4.5 percent. It is interesting to note that this point estimate is very close to our estimated effect on turnout, which suggests that the missing voters were overwhelmingly pro-incumbent.

Overall, the results discussed thus far suggest that revisions not only reduce the electorate and turnout, but also dramatically reduce incumbents' reelection prospects. These findings are consistent with logic of voter buying, as they directly correspond to the first three hypotheses in Section 4.2. To examine the next three hypotheses, we now turn to heterogeneous treatment effects.

5.6 Heterogenous Treatment Effects

An investigation of heterogeneous treatment effects lends additional support to the voter-buying argument. Hypotheses in Section 4.2 indicate that the adverse impact of revisions on incumbent reelection prospects is expected to depend on how much a machine relies on voter buying. For example, we expect that the negative effect on mayoral reeleection will be greater if: (1) many voters recently transferred into the municipality, and (2) many voters recently transferred out of neighboring municipalities. To test these hypotheses with regards to the 2007 revisions, we obtained data on the number of voter registration transfers to and from every municipality in 2005 and 2006. For each municipality, we utilized these data to determine the total number of transfers into the municipality, as well as the total number of transfers out of neighboring municipalities.[44]

To examine heterogeneity in revision effects, we first split our sample by the median value of

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[44]We used GIS maps provided by the Brazilian census agency to identify the neighbors of each municipality.
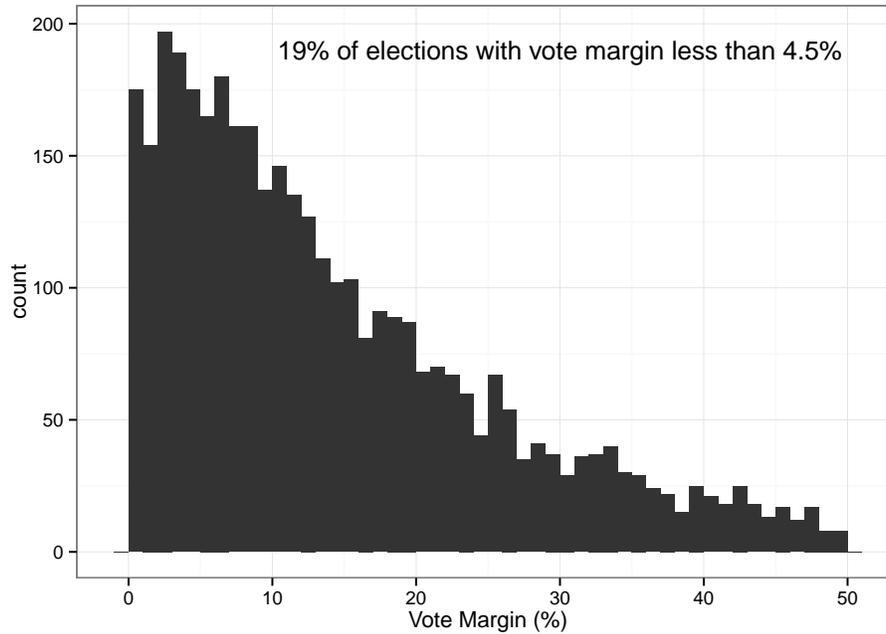
Figure 7: Histogram of Vote Margins for 2008 Municipal Elections

Figure 7 shows number of municipalities with each vote margin, and includes all municipalities with at least two candidates for mayor.

the number of transfers out of neighboring municipalities and the median value of the number of registration transfers into the municipality. Figure 8a shows how the effect of the revision on the incumbent remaining in power varies by whether the total number of transfers from the municipality's neighbors is above or below the median (2,705 transfers). The difference in the effect of the revision across the two strata is stark. The estimated effect on incumbent electoral victory in municipalities with lower than the median number of transfers from neighbors is about -.05 and statistically insignificant. By contrast, in municipalities with higher than the median number of transfers from neighbors, the effect is about -.41 using the local linear estimate and -.32 using the difference-in-means estimator. The difference between these two estimates (labeled "difference" in the plot) is statistically significant according to both of our specifications. In other words, consistent with the predictions of voter buying, the treatment effect of the intervention is over twice as large (41 percentage points) if many voters recently transferred away from neighboring munic-

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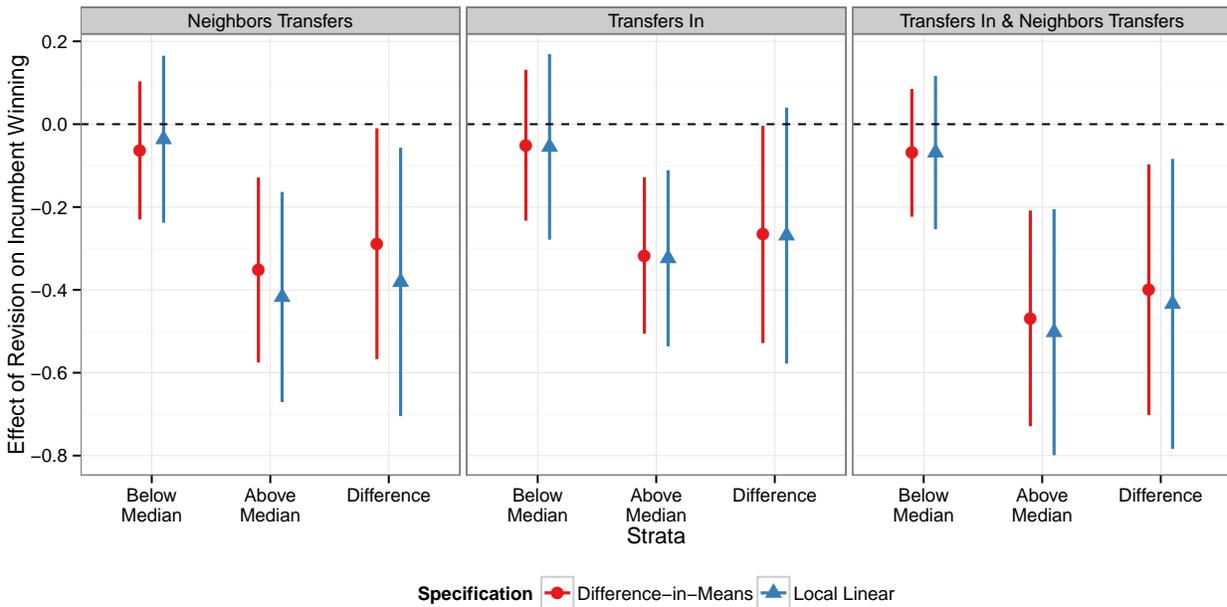
Heterogeneity in Effect of Revisions on Incumbent Winning by Number of Transfers. This plot shows how the effect of revisions varies by the amount of voters transfering out of neighboring municipalities (a), the amount of voters transfering into the municipality (b), and the interaction of the two covariates (c). In (a) and (b), the sample is split by the median value of each covariate. In (c), the sample is split by whether the municipality is above the median value of both covariates. In addition, the difference between effects in each stratum are plotted (the "interaction"). Vertical lines represent 95% confidence intervals.

ipalities. Furthermore, we find *no* effect of the intervention on mayoral reelection if few voters recently transferred away from neighboring municipalities.

Also in line with the logic of voter buying, we find a similarly large degree of heterogeneity by the number of voters transferring into the municipality (see Figure 8b). The effect on incumbent performance is substantially larger in municipalities with higher than median transfers (180 voters) than in those municipalities with fewer than median transfers. The difference between the effects in the two strata is statistically significant in the difference-in-means specification, but only marginally so using the local linear specification (p-value of .08).

Finally, we further stratified the data by whether or not a municipality had *both* a higher than

median number of voter inflows and higher than median number of voter outflows from neighboring municipalities. Figure 8c shows the heterogeneity across strata defined by municipalities that met both of these criteria, versus those that did not. Once again, the difference across strata is dramatic. The revision among municipalities that had either below the median inflows or neighbor outflows is statistically indistinguishable from zero. Among municipalities that fit both criteria, the estimated effect is -.44 (difference-in-means specification) or -.49 (local linear specification). To put these estimates into perspective, in the average municipality, incumbents return to power 52 percent of the time. When compared to this benchmark, our point estimate for the "above median" strata is striking.
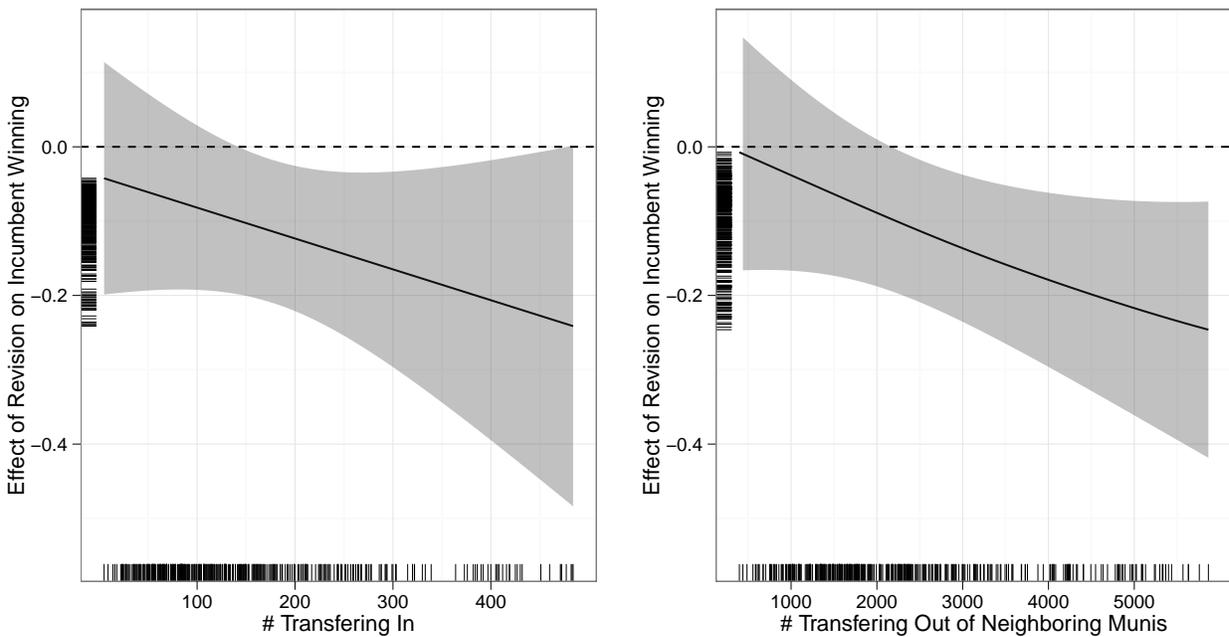

Figure 9: Heterogeneity in the Effect of Revisions on Incumbent Winning Using Continuous Covariates. This plot shows how the effects of the revision vary by a continuous measure of the number of voters transfering into the municipality (left panel) and the number of voters transfering out of neighboring municipalities (right panel). Estimates are from a generalized additive model. These are reduced form estimates. Tick marks on the margins show the marginal distribution of the data. The gray ribbon represents a 95% confidence interval.

To confirm that these findings are not an artifact of arbitrarily splitting the sample by the median

value of transfers, Figure 9 shows heterogenous treatment effect results using the continuous measures of the number of voters transferred out of neighboring municipalities and number of transfers into the municipality. These are reduced form estimates using the difference-in-means discontinuity sample and produced using a generalized additive model (Hastie and Tibshirani, 1990).[45] The advantage of generalized additive models over traditional OLS interaction models is that they do not impose a linear functional form on the interaction between the covariate and the effect of the treatment. As evident in Figure 9, the effect of the transfers on incumbents' ability to win elections is negative and statistically significant.
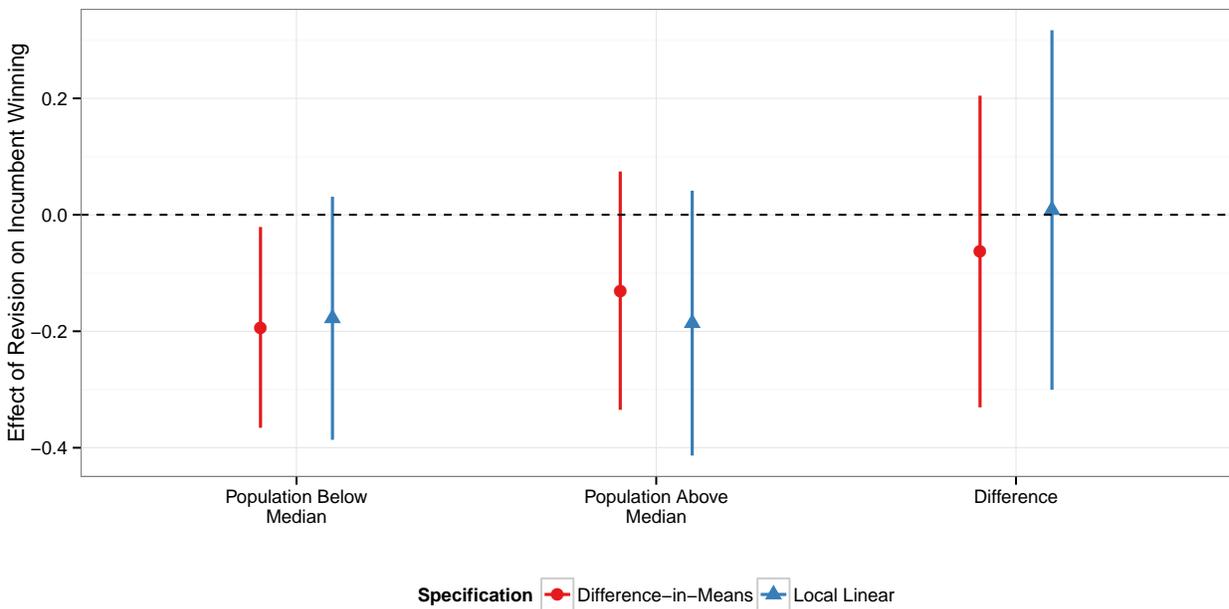

Figure 10: Heterogeneity in the Effect of Revisions on Incumbent Winning by Population Size. This plot shows how the effect of revisions varies by whether or not a municipality's population is larger than the median population size. In addition, the difference between the two estimates is plotted (the "interaction"). Vertical lines represent 95% confidence intervals. Sample is comprised of those municpalities with higher than median transfers.

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[45]Specifically, we estimate the difference between the predicted values from a generalized additive model that is fitted seperately on each side of the discontinuity threshold. We used the `gam` function from the R package `mgcv` to estimate the relationship between the outcome and each covariate. The algorithm estimates the functional form by using penalized regression splines with smoothing parameters chosen by a generalized cross validation criterion (Wood, 2006).

We now consider the final voter-buying hypothesis in Section 4.2, which pertains to municipality size. The prediction is that the negative effect of revisions on mayoral reeleection will be greater in less populous municipalities, where lower monitoring costs increase use of the strategy. As noted above, the revision targets smaller municipalities, so the variance on population size in our discontinuity sample is limited.[46] Nevertheless, to test this prediction we estimate treatment effects separately in small and large municipalities among the municipalities where many voters transferred in during 2005 and 2006. Specifically, among the municipalities with higher than median transfers, we further split the sample by the population sample median (about 11,000) and estimate the effect of the revisions in each strata.

The results are shown in Figure 10. While stratifying the sample in this way significantly reduces the statistical power of our tests, the pattern of results is consistent with the voter-buying hypothesis. We find that in municipalities with a population smaller than the median, the effect of the intervention is to lower the probability of an incumbent retaining political power by a very large -.7. This estimate, however, is estimated with a substantial amount of uncertainty and only significant at the 5% level using the difference-in-means specifications. Still, this point estimate is much larger in magnitude than the effect among municipalities with larger than the median population, which is about -.5. The difference across the two strata, however, is not statistically significant. While small sample sizes make this evidence necessarily tentative, the pattern of coefficient magnitudes supports our prediction that voter buying will be more common in small municipalities where monitoring is easier.

In sum, specifications employing a regression discontinuity design suggest that revisions have substantial effects on the continuity of power in local Brazilian elections. Heterogeneous treatment effects provide further evidence that voter buying is a compelling explanation for observed patterns in the data. Taken together, the empirical findings are consistent with all six voter-buying

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[46]As mentioned above, given that revisions are designed to target illegal transfers, this targeting of small municipalities in and of itself provides evidence consistent with the voter-buying model's prediction.

Table 2: Testing Alternative Explanations: Effect of Revision on the Age Distribution of the Electorate and Blank/Invalid Votes

Outcome	$\hat{\tau}_A$	SE_{τ_A}	$\hat{\tau}_R$	SE_{τ_R}	Baseline	n
Difference-in-Means Specification						
Above 70 Electorate Change (%)	−13.09	0.81	−18.00	0.87	5.43	577
16-24 Electorate Change (%)	− 4.84	0.43	− 6.66	0.52	3.72	577
Blank & Invalid Change (%)	0.08	0.51	0.12	0.70	0.97	577
Local Linear Specification						
Above 70 Electorate Change (%)	−13.55	1.01	−18.55	1.01	5.96	1477
16-24 Electorate Change (%)	− 5.06	0.53	− 6.95	0.62	4.00	1477
Blank & Invalid Change (%)	0.02	0.64	− 0.09	0.87	1.04	1477

$\hat{\tau}_A$ is the local average effect of a municipality having an electorate more than 80% of its population ("reduced form") and SE_{τ_A} is its associated standard error. $\hat{\tau}_R$ is the estimated local average effect of the revision and SE_{τ_R} is its standard error. Standard errors are White-Huber ("robust") standard errors. Baseline is the estimated value of the dependent variable among controls at $E_i = 80$

hypotheses discussed in Section 4.2.

5.7   Alternative Explanations

Although the analyses above provide compelling evidence of voter buying in Brazil, we emphasize that voter buying is by no means the *only* factor affecting the political phenomena explored above. But while other factors may underlie some findings, we find it highly implausible that another mechanism could provide a better account of the full range of results. We now briefly discuss some alternative explanations and suggest why they fail to explain important patterns in the data.

One alternative hypothesis is that revisions undermine the incumbents' reelection prospects by removing legitimate voters from the electorate who are their supporters. Such an explanation would rely on a systematic pattern of asymmetric cancellations, so perhaps the best argument employing this logic would involve elderly voters. Voters over 70 are no longer covered by compulsory voting laws and consequently may be less likely to re-register after a revision. If elderly voters are disproportionately pro-incumbent, then incumbents could conceivably lose at higher rates after a
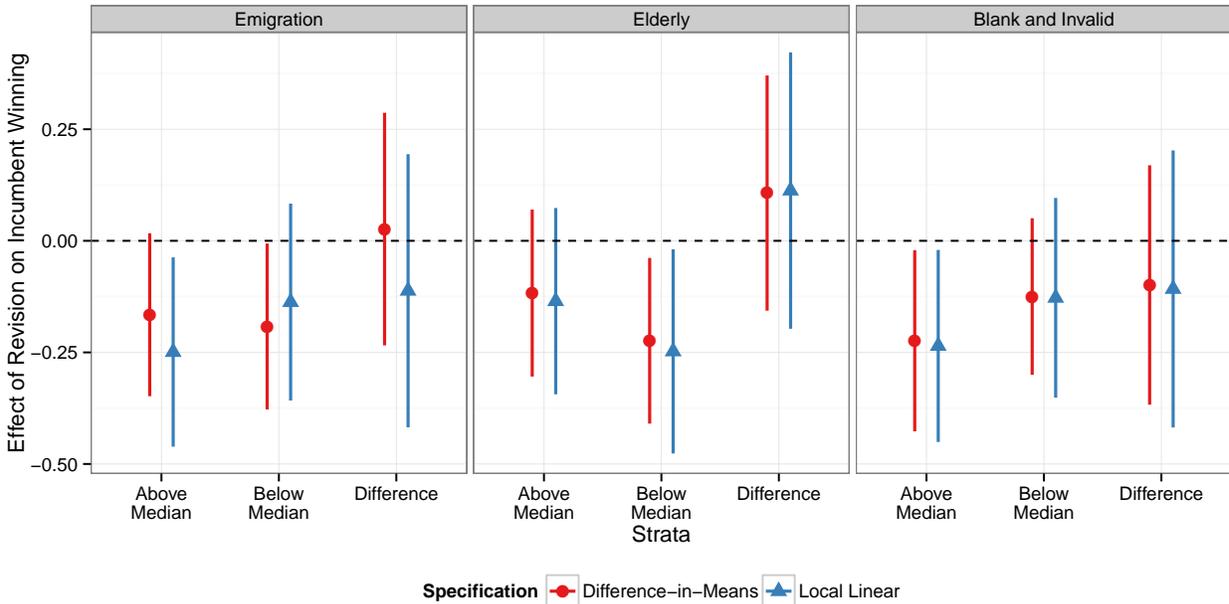
Figure 11: XX

revision. Indeed, as documented in Table 2, the revision decreased the size of the elderly electorate (age 70 and above) more than younger voters (age 16 to 24). The percentage point decrease in the size of the elderly electorate as a result of the revision was about -18 percentage points (depending on specification), approximately two and half times the magnitude of the effect on the size of the younger electorate (-7 percentage points). The large decrease in the elderly electorate may include the removal of some deceased elderly voters, but "ghost voting" (i.e., voting with documents of the deceased) is relatively unlikely given that digitalized electoral rolls are automatically updated with vital statitics whenever a death certificate is issued. The more plausible explanation is that many elderly voters simply do not re-register because they are exempt from compulsory voting. The major flaw with this rival explanation is that the drop in the elderly electorate only accounts for about 15 percent of the overall decrease in the electorate. In other words, the vast majority of voters during the revision were *younger* than 70.

Another possible explanation is that voters with little interest in politics failed to re-register.

As with elderly voters, it is perhaps plausible that disinterested voters tend to vote at higher rates for incumbents than the general population of voters. To test for such a mechanism, we examine the effect of the revision on blank and invalid votes cast. Voters have the option of casting a blank vote or an invalid vote (known as "branco" and "nulo" votes, respectively), and these votes are correlated with levels of political information and interest. If the revision purged voters with little interest in politics from the rolls, then we would expect a decrease in the share of blank and invalid votes cast. As shown in Table 2, we find no such effect. According to both specifications, the effect of the revisions on blank and invalid votes is close to 0 and statistically insignificant.[47] This result suggests that effects are unlikely to be attributable to the purging of the rolls of politically disinterested voters.

Further evidence against an "indifference" or "laziness" hypothesis can be gleaned from the process by which state electoral courts can authorize revisions. As described above, officials must make home visits to five percent of voters (randomly selected) in a municipality to investigate if a significant number of selected voters are erroneously registered. Data from these random home visits suggests that many citizens are illegally registered, not simply too lazy to re-register. Although systematic data are unavailable, we searched through the court documents in the state of Bahia, and identified 132 municipalities that inspected random samples of the electorate in 2007 and 2008. Of these, on average 7.8 percent of voters were cancelled due to irregularities. These cancellations occurred only after detailed investigations. When officials are unsuccessful locating voters upon visiting listed addresses, they follow up with other methods such as checking if the voter has a bank account or telephone registered in the municipality, if they have local health credentials, or if they possess land or a house. Furthermore, they investigate the possibility that the voter has local relatives.[48] In short, the "indifference" or "laziness" does not explain many cancellations.

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[47]Estimating the effect separately on blank and invalid votes gives a null result in each case.
[48]Interview with Cibelle Barbosa, Tribunal Regional Eleitoral de Minas Gerais, January 30, 2011.

Some observers would offer another explanation for voter cancellations during revisions: over the years, voters have migrated away from small municipalities to cities but simply failed to transfer their voter registration. However, Brazil automatically cancels the voter registration of citizens who do not vote or justify their absence in three consecutive elections. And furthermore, national elections — as well as municipal elections in large cities — typically have two rounds, and both rounds count as missed elections. So longer-term patterns of urbanization could only affect voter cancellations during revisions in cases where city dwellers return to their birthplaces every two years to vote in elections (or repeatedly justify their absence). These voters may be removed from the voter rolls as a result of the revision. This mechanism would explain the decrease in turnout, but not necessarily the effect on incumbents' political fortunes, unless they tended to disproportionately vote for the incumbent. And while some voters may return for every election and consequently maintain their registration, the numbers involved would have to be unrealistically large to explain our results. Another consideration is that return trips may also involve voter-buying inducements. Especially in small municipalities, candidates seeking political support often pay the travel expenses of former residents who have migrated to live in cities.[49]

Our further research will continue to explore alternative hypotheses. However, given this preliminary evaluation of potential rival explanations, we argue that while voter buying may not be only the mechanism explaining parts of our results, it best explains the full breadth of findings explored in this paper.

6   Discussion

This paper challenges the literature on clientelism. Although most studies assume that political machines distribute contingent benefits to influence the actions of a given electorate, we argue that machines also distribute benefits to shape the composition of the electorate. Local politicians

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[49]"TSE Faz Revisão Eleitoral em 658 Municipios," *Tribunal Superior Eleitoral*, September 27, 1999.

need not focus on influencing the vote choices and turnout of citizens registered in their districts. Through voter buying, they can induce voters in other districts to transfer their voter registration and deliver political support. Formal modeling shows that voter buying is incentive-compatible and offers several testable predictions. Both qualitative and quantitative analyses provide evidence consistent with the voter-buying model, and also suggest that voter buying has substantial effects on local politics in Brazil. We use a regression discontinuity design, exploiting an intervention that required municipalities to re-register their entire electorate if they exceed an arbitrary threshold. This intervention undermined voter buying and reduced the likelihood of mayoral reelection by 18 percentage points. Consistent with the voter-buying model, the treatment effect of the intervention is over twice as large (41 percentage points) if many voters recently transferred away from neighboring municipalities. Furthermore, we find no effect of the intervention on mayoral reelection if few voters recently transferred away from neighboring municipalities. And the effects of voter buying may well extend even further than this analysis suggests. Beyond these dramatic direct effects on the municipalities that import induced voters, there may also be important indirect effects on municipalities from which they transfer.

While our empirical results provide strong evidence that voter buying is an important means by which incumbents retain power in Brazil, further analyses would help to rule out alternative explanations more definitively. For example, in order to conduct further tests that transfers are not a proxy for migration, we plan to use census data to examine heterogenous treatment effects by migration flows. As another robustness check — assuming that we can acquire the needed transfers data — we plan to examine the effect of the wave of revisions conducted before the 2004 elections. Finally, we hope to examine the longer-term effects of revisions. For example, incumbent machines may compensate for the effect of the revisions by inducing more voters in neighboring municipalities to transfer in the years following the 2008 elections. Obtaining additional transfers data would allow us to explore the possibility of lasting treatment effects, shedding additional light on the strategies used by political machines amidst pressure from electoral officials.

An important avenue for future research is examining how machines combine voter buying with other strategies of clientelism. Political machines can engage in various forms of clientelism — such as vote buying and turnout buying — and formal analysis by Gans-Morse, Mazzuca and Nichter (2010) predicts they are more effective when combining several strategies. When a machine chooses how much voter buying to include in its portfolio of clientelism, one factor that is likely to play an important role in its decision is ballot secrecy. Voter buying involves a less stringent monitoring requirement than vote buying because it does not require machines to observe how citizens vote. This key difference points to one explanation why voter buying may be particularly attractive in Brazil: the advent of electronic voting. With the previous paper-based system, political machines enforced vote-buying exchanges by marking ballots and using strategies such as the Tasmanian Dodge. Brazil's recent technological shift — it became the first country in the world to have fully electronic voting in 2000 — undermined the ability of political machines to violate the secret ballot. Vote buying has thus become relatively less effective, as it is more difficult to ensure that opposition voters follow through on their promises to deliver electoral support. As such, electronic voting in Brazil has increased the relative attractiveness of voter buying.

Voter buying may also be particularly attractive where rigorous compulsory voting reduces the viability of turnout buying, another strategy that does not rely on violating the secret ballot. Turnout buying is rare in Brazil due partly to exceptionally strong compulsory voting requirements. The 1988 Constitution compels all Brazilians aged 18 to 70 years to vote, unless they are illiterate. As one politician explains, "everyone knows that if you don't vote, you harm yourself ... your voting document becomes irregular." With irregular voting documents, Brazilians cannot obtain identification cards, work in the public sector, qualify for government loans, or enroll in public educational institutions. Citizens exert substantial effort to avoid such consequences.[50] Partly

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[50]For example, if voters are out of town and cannot report to their designated polling places, they frequently report to other locations on Election Day to fill out "justification" forms. Although voters cannot cast ballots while away, they avoid all abstention penalties by submitting such forms. Over 7.8 million voters took the time to justify their absence in the 2008 election (TSE 2010), suggesting that voting is indeed considered obligatory.

due to strict enforcement of compulsory voting, electoral participation in Brazil is high: turnout reached 81.9 percent of registered voters in the presidential elections of 2010, and 85.5 percent in the most recent municipal elections of 2008 (TSE 2010). In addition, our analysis of micro-level data from Bahia suggests that the turnout of citizens for whom voting is mandatory exceeds 90 percent - only 2.9 percent simply didn't show up at the polls, while 6.5 percent officially justified their absence.[51] In sum, voter buying may be especially attractive in contexts such as Brazil where most citizens already vote due to enforced mandatory voting laws.

Yet voter buying and other forms of clientelism represent only a subset of the strategies by which machines can manipulate elections. For example, in some cases it might be cheaper to pad voter rolls with fictitious "ghost voters" before an election or rejigger ballot totals after an election. A key topic for future study involves the conditions under which political machines rely more heavily on clientelist strategies than such forms of fraud. In the case of Brazil, voter buying became relatively more attractive as technological shifts undermined various fraudulent strategies. For example, the digitalization of voter rolls dramatically curtailed the possibility of using fabricated or duplicated voter registrations. Throughout the history of Brazil, local political machines often used *fosforos* (matchsticks), citizens who would vote in place of deceased, disinterested or itinerant voters (Nicolau, 2002). When Brazil digitalized its electoral rolls in 1985, "fraud in voter registration became almost impossible" due to national cross-checking of records to eliminate duplications and many other irregularities (Nicolau 2002: 68; Angelim 2007). Moreover, electoral rolls are regularly updated with vital statistics data, which to a large degree inhibits the ability of machines to cast votes with documents of deceased voters. Another technological shift discussed above, electronic voting, also significantly constrained the ability of local political machines to commit fraud *after* voting. With its introduction in 2000, various strategies common with paper balloting became impossible to use, such as adding votes to polling station tabulation sheets,

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[51]By contrast, 38.0 percent of Bahians aged 16-17 and 70+ didn't show up at the polls (their voting is optional, and thus they are not required to justify).

claiming that opposition votes were illegible, and filling out candidates' names on blank ballots.[52] As one observer indicates, "few doubts persist about the manipulation of voting results" (Taylor 2011: 166). By undermining such fraudulent strategies, these technological shifts have increased the relative attractiveness of voter buying in Brazil.

The impact of further technological shifts in Brazil on voter buying remains uncertain. The TSE intends to install biometric voting, including fingerprint scanning, in the entire country within the next decade.[53] Over one million voters voted biometrically in the 2010 national election, and this figure will reach approximately 10 million in this year's municipal elections.[54] On the one hand, because biometric voting requires the re-registration of the entire Brazilian electorate, it may undermine the strategy of voter buying. But on the other hand, it will also impinge on various forms of registration fraud. As a chief minister of the TSE explained, "no one will appropriate the voter ID of anyone who died or disappeared, or have more than one voter ID."[55] Thus, it remains unclear whether this technological advance will lead local political machines to engage in more or less voter buying.

Another important consideration involves contexts where multiple political machines operate in each other in the same geographic territory. As Kitschelt (2011, 9) explains, while clientelism is sometimes a "unilateral, monopolistic affair concentrated in the hands of a single party," elsewhere it involves competition between several machines.[56] Voter buying may be a particularly attractive strategy in contexts with competing political machines. As an electoral judge in Maranhão explained, voter buying is often more attractive than vote buying in Brazil because the strategy enables machines to bypass direct competition over voters. Voter-buying recipients live in other municipalities, and as a result opposing politicians frequently do not even know who they are or

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[52]See "Encontrei uma Quadrilha, Afirma Juíz," *Estado de São Paulo*, 1994 and "Juízes Comandam Máquinas das Fraudes," *Estado de São Paulo*, 1994.

[53](Tribunal Superior Eleitoral, Resolution 23.062.

[54]"Biometria na Justiça Eleitoral," *Tribunal Superior Eleitoral*, 2012

[55]Tribunal Superior Eleitoral, Resolution 23.062.

[56]Some scholars, such as Stokes (2005, 324), argue that even in such settings, different machines do not compete directly because they develop links with different constituents.

where to find them. And because transfers are only permitted once per year, the machine knows that once a citizen transfer her registration, she cannot transfer back and vote in her own municipality for the next year. By contrast, many politicians interviewed suggest that vote buying entails intense competition; local residents who receive inducements to switch their votes may then receive additional inducements from the opposition.

Scholarly research should also investigate the distinction between forms of voter buying that involve legal versus illegal registration by citizens. Some forms of voter buying do not require illegal registration. For instance, unlike the examples discussed in Section 1, the Tammany machine in New York sometimes circumvented voter registration laws. Indeed, some paid "colonizers" received rooms in boarding houses "long enough before election to comply with the law regarding residence."[57] But on the other hand, as many examples above demonstrate, voter buying often involves illegal registration by citizens. For instance, the Electoral Commission of Kenya reported in 2007 that politicians frequently paid money to citizens in exchange for transferring their voter registrations, and emphasized the need to block fradulent transfers of those who did not meet residency requirements.[58] Such examples suggest the need for greater analytical precision regarding the analytical and normative implications of the distinction between voter buying that does vs. does not involve illegal registration.

Overall, this study has challenged scholars to deepen their understanding about the various ways in which clientelism and other informal institutions can shape the electorate. If the understudied strategy of voter buying can dramatically influence elections by altering the electorate, it is likely that many other important strategies are also overlooked. Such phenomena may substantially influence the practice of democracy in many countries.

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[57]"Tammany Method," *Syracuse Daily Journal*, October 24, 1893, Pg. 1. According to current law, even if such registrations were not fradulent, the inducements would still be criminal. Providing inducements for voter registration is presently illegal in federal elections (42 U.S.C. 1973i(c)).

[58]"Kivuitu Order on Transfer of Voters," *The East African Standard*, February 21, 2007 and "We Elect to Take Kivuitu Seriously," *East African*, March 20, 2007.

Appendix

Table 3

	Change in Electorate (%)		Incumbent Vote (%)	
	Dependent variable:			
	(1)	(2)	(3)	(4)
Pct. Above 70	−0.3***	−0.3***	0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
Emigrants	−2.4***	−0.9***	−2.8***	−0.6*
	(0.1)	(0.2)	(0.2)	(0.4)
Blank and Invalid	0.01	−0.04	0.1	0.1
	(0.04)	(0.04)	(0.1)	(0.1)
Constant	24.7***	46.7***	48.1***	58.8***
	(1.2)	(5.6)	(1.6)	(4.1)
State Fixed Effects	No	Yes	No	Yes
Covariates	No	Yes	No	Yes
Observations	1,279	1,276	2,323	2,310
Note:			$^{*}p < 0.1;^{**}p < 0.05;^{***}p < 0.01$	

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