Voter Buying:
Shaping the Electorate through Clientelism[*]

F. Daniel Hidalgo[†]
Simeon Nichter[‡]

Abstract

Studies of clientelism typically assume that political machines distribute rewards to persuade or mobilize the existing electorate. We argue that rewards not only influence *actions* of the electorate, but can also shape its *composition*. Across the world, machines employ "voter buying" to import outsiders into their districts. Voter buying demonstrates how clientelism can underpin electoral fraud, and offers an explanation of why machines deliver rewards when they cannot monitor vote choices. Our analyses suggest that voter buying dramatically influences municipal elections in Brazil. A regression discontinuity design suggests that voter audits — which undermined voter buying — decreased the electorate by 12 percentage points and reduced the likelihood of mayoral reelection by 18 percentage points. Consistent with voter buying, these effects are significantly greater in municipalities with large voter inflows, and where neighboring municipalities had large voter outflows. Findings are robust to an alternative research design using a different dataset.

Keywords: Clientelism, Machine Politics, Fraud, Vote Buying, Election, Reelection, Electorate, Politics, Audit, Transfers, Brazil, Regression Discontinuity Design

[†]Massachusetts Institute of Technology; Department of Political Science; 77 Massachusetts Avenue; Room E53-470 Cambridge, MA 02139; dhidalgo@mit.edu.

[‡]University of California, San Diego; Department of Political Science; Social Sciences Building 301; 9500 Gilman Drive, #0521 La Jolla, CA 92093-0521; nichter@ucsd.edu.

1   Introduction

In many societies, clientelist parties (or political machines) distribute benefits to citizens in direct exchange for political support. Whereas scholars traditionally explored patterns of clientelism extending beyond electoral campaigns (e.g., Banfield and Wilson 1963; Scott 1972), many recent studies focus on how machines strategically target citizens during elections. A key debate in this recent strand of research is whether machines use rewards to persuade or mobilize the existing electorate (e.g., Stokes 2005; Cox 2006; Nichter 2008). We argue that this debate — and the broader literature on clientelism — ignores important strategies because it assumes the electorate is fixed. Machines provide rewards 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 outsiders 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. 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 news 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 register fraudulently (*New York Herald* 1891). In another example, a New York assemblyman was arrested during his reelection campaign in 1900 for "harboring colonizers" in 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" (*New York Herald* 1900). Despite such historic precedents for voter buying, recent studies of clientelism rarely consider the phenomenon.

Yet evidence from around the world suggests that contemporary political machines engage in voter buying. Kenya's Electoral Commission recently reported that politicians often distribute cash rewards in exchange for voter registration transfers. The agency called for efforts to block transfers of citizens failing to meet residency requirements (*East African Standard* 2007). In Mex-

ico, voter buying — frequently called "electoral tourism" — is also a familiar strategy. As just one example, busloads of illegitimate voters were intercepted on their way to the Yucatán during the 2013 election (*Proceso* 2013).[1] An imported voter explained she agreed to transfer her voter registration because she "didn't have money to eat for days" and was paid 1,000 pesos (US$75). Reports of voter buying also abound in the Philippines. During the 2013 election, operatives in San Lorenzo Village imported citizens — known as "flying voters" — and paid them ₱500 (US$11) each. One citizen urged officials to "remove the nonresidents" because "we can't allow outsiders to rule our elections" (*Philippine Daily Inquirer* 2013). Newspapers also report voter buying in Bolivia, Botswana, Bulgaria, Ghana, Jordan and Swaziland.[2] Voter buying is thus observed in various countries, motivating our investigation of the phenomenon in Brazil.

The present paper examines the logic of voter buying and offers several predictions: (1) the strategy is most likely in small communities, (2) it inflates the electorate by importing many voters, (3) incumbents import more voters than challengers, and (3) the strategy predominantly imports voters from nearby districts. Qualitative evidence of voter buying in Brazil is consistent with these predictions. Given that no direct quantitative measure of voter buying exists, we employ indirect tests of predictions with a regression discontinuity design of voter audits. These audits, which undermined voter buying, decreased the electorate by 12 percentage points and reduced the likelihood of mayoral reelection by 18 percentage points. Consistent with voter buying, these effects are significantly greater in municipalities with large voter inflows, and where neighboring municipalities had large voter outflows. Findings are inconsistent with alternative explanations and are robust to an alternative research design using a different dataset. Fixed-effects regressions confirm a link between voter transfers and audit removals. Furthermore, mayors perform more poorly in precincts with many removed voters, especially if those precincts recently imported many voters from neighboring municipalities.

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[1]See also complaint by four senators, *Camara de Senadores*, 5/8/2013.

[2]*Africa News* (March 25, 2009; August 29, 2013); *BBC* (October 9, 2007; November 14, 2008); *El Mundo* (December 3, 2014); *Sofia News Agency* (September 14, 2011).

The distinction between voter buying and several other forms of clientelism is elaborated by the typology in Figure 1. The commonly studied strategy of "vote buying" induces voters *already* registered in a machine's district to switch their vote choices (Lehoucq 2003; Stokes 2005). Scholars vigorously debate how vote buying coexists with the secret ballot (e.g., Stokes 2005; Nichter 2008; Diaz-Cayeros, Estévez and Magaloni forthcoming), and often contend that other strategies influence registered citizens' actions without monitoring vote choices. For example, "abstention buying" induces opposing voters to abstain (Cox and Kousser 1981), while "turnout buying" mobilizes supporting nonvoters (Cox 2006; Nichter 2008). By contrast, "voter buying" — the focus of this paper — shapes the electorate's *composition* by importing voters registered in other districts. Voter buying offers an alternative explanation to the secret-ballot puzzle: it rewards outsiders who are either indifferent or machine supporters, and thus have no reason to defect once inside the ballot booth. We focus on the understudied strategy of voter buying, but acknowledge clientelist strategies often coexist (Dunning and Stokes 2008; Gans-Morse, Mazzuca and Nichter 2014).[3]

[FIGURE 1 ABOUT HERE]

Voter buying lies at the intersection of clientelism and fraud, because it demonstrates how clientelist rewards can induce registration fraud. Although excellent studies focus on fraudulent voter registration (e.g., Fukumoto and Horiuchi 2011, Ichino and Schundeln 2012), they rarely consider clientelist benefits, and voter buying similarly receives scant attention in more expansive studies of electoral fraud (e.g., Lehoucq and Molina 2002, Simpser 2013). Voter buying may be considered a form of "retail" fraud that distorts individual votes before they are cast, as opposed to "wholesale" fraud that alters *ex post* vote tallies (Alvarez, Hall and Hyde 2008, 4). And unlike many forms of electoral fraud, voter buying involves participation of citizens casting ballots (Donsanto 2008, 24).

Our study therefore contributes by bridging two bodies of literature: it underscores that clientelism can underpin electoral fraud. Whereas most prominent studies of electoral fraud consider

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[3]The typology is not exhaustive. "Nonvoter buying" targets citizens neither registered in the machine's district nor likely to vote.

Figure 1: Clientelist Strategies During Elections

**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**	**Nonvoter Buying**

**Reward Recipient
Inclined to Vote or
Not Vote**

clientelism to be a *type* of fraud (e.g., Alvarez, Hall and Hyde 2008, 6; Lehoucq 2003, 237-9; Ziblatt 2009, 4), many studies of clientelism never even mention the word "fraud" (e.g., Scott 1972; Kitschelt 2000; Stokes 2005). Leaving conceptual issues aside, voter buying clarifies that scholars must not overlook clientelism as a potential mechanism of electoral fraud. Studies of clientelism should investigate whether rewards are used to induce fraud, lest they misinterpret why some machines distribute benefits. In parallel, studies of fraud should investigate mechanisms by which politicians motivate citizens' complicity — to what extent do rewards induce citizens to break laws and risk fines or imprisonment? The present study advances this agenda by theoretically and empirically linking clientelism to electoral fraud.

We also contribute to the empirical literature on how clientelism and fraud affect elections. Within this burgeoning literature, our study is most closely related to three excellent studies that do not investigate voter buying. First, our results corroborate Wantchekon's (2003) finding that clientelism disproportionately benefits incumbents. However, his field experiment examines the impact of clientelist campaign promises and does not consider voter buying. Second, we build on the incisive work of Fukumoto and Horiuchi (2011), who examine voter transfers across municipalities during Japanese city council elections. In contrast to their study, we investigate clientelism and directly examine effects on reelection rates. Third, as with Ichino and Schundeln (2012), we examine neighboring districts and voter registration. However, their study explores spillover effects of election observers on registration fraud, not voter buying. In sum, our unique focus on voter buying is a substantial contribution.

2 Hypotheses

The logic of voter buying suggests four key predictions tested in the present study:

H1 Small Districts: *Voter buying is more likely in small towns and villages than in cities.* Several factors render voter buying more cost effective in smaller communities. First, an imported voter is far more likely to be pivotal in small districts, so machines must typically import fewer citizens to change a candidate's vote share. This logic builds on Fukumoto and Horiuchi (2011, 593–4), which

argues registration fraud is more prevalent in small towns and villages because fewer votes separate winners from losers. Second, voter buying is often more cost effective in small communities because machines can more accurately monitor recipients' compliance with clientelist transactions (Stokes 2005, 322-3; Nichter 2008, 28). Third, politicians tend to shift campaign budgets away from clientelism as electorates increase, because returns to scale of advertising and other programmatic strategies are greater (Stokes et al. 2013, 181–2). Overall, voter buying is more likely in small districts.

H2 Registration: *Voter buying inflates electoral registration by importing many voters.* Studies of fraud, which do not investigate clientelist rewards, demonstrate that illegal voter registration inflates electorates. For example, Fukumoto and Horiuchi (2011) show Japanese politicians expand rolls by encouraging outsiders to register fraudulently before municipal elections. Likewise, Ichino and Schundeln (2012, 293) uncover fraudulent registration in Ghana and argue parties have "strong incentives to inflate the voters register." With voter buying, outsiders are induced to transfer their voter registration, which similarly expands rolls. Therefore, contexts with voter buying should exhibit electorate increases with many voter inflows.

H3 Incumbents: *Incumbents engage in more voter buying than challengers.* Incumbents are expected to engage in more voter buying than challengers due to greater access to resources. Many scholars argue incumbents have a competitive advantage at clientelism and patronage because they can disproportionately tap public funds, social programs or government employment (e.g., Gallego and Wantchekon 2012, 185; Stokes 2009, 14-5). Rigorous evidence is provided by Folke, Hirano and Snyder (2011), who show control over public employment provided an incumbency advantage in the historical U.S., as well as Schady (2000), who demonstrates Peru's incumbent president manipulated a social program for political gain. Moreover, experimental work in Benin suggests clientelism is more effective for incumbent candidates, potentially because they can more credibly promise to deliver rewards (Wantchekon 2003, 401, 421). Disproportionate access to resources may enhance such credibility by enabling incumbents to develop a track record of handouts (Chandra 1997, 94), as well as facilitating ongoing employment offers that are both credible

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and reversible (Robinson and Verdier 2013). Furthermore, incumbents may enjoy greater access to human resources, such as electoral staff who process registration transfers. These advantages suggest incumbents will typically conduct more voter buying than challengers.

H4 Neighboring Districts: *Voter buying imports many voters from geographically proximal districts.* Proximity renders voter buying more cost effective due to lower transportation costs. In contexts where machines physically transport voters to their districts, they must expend less on labor and fuel costs when voters live in neighboring districts. This logic builds on Ichino and Schundeln (2012, 295), who argue proximity affects the degree to which politicians fraudulently relocate voters due to "time and resource constraints." If politicians compensate voters for transportation costs instead of physically relocating them, a similar logic suggests nearby voters require smaller payments. Furthermore, many studies on voter behavior find distance to polling places reduces turnout propensity (e.g., Brady and McNulty 2011; Haspel and Knotts 2005). Analogously, distance may be expected to affect turnout propensity among induced voters, thereby undermining voter buying's effectiveness. Such considerations suggest voter buying tends to import many voters from proximal districts.

At the outset, we acknowledge these predictions are by no means exhaustive. For example, features of electoral systems may influence voter buying. Because this study focuses exclusively on Brazil, we do not thoroughly investigate why voter buying may be more prevalent in some countries than others, though the Discussion section suggests why Brazil may be a propitious environment for the strategy. We bracket such considerations and focus on providing evidence of voter buying and testing the above hypotheses.[4]

2.1 Indirect Test of Hypotheses

Given that no direct quantitative measure of voter buying exists, we test hypotheses indirectly by examining a Brazilian intervention designed to combat the phenomenon — voter audits. Voter

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[4]A previous version of this paper formalized several of these hypotheses, adapting the models in Stokes (2005) and Nichter (2008).

audits undermine voter buying by inspecting a municipality's electorate and removing ineligible voters. These audits have never been previously analyzed and provide substantial insight about voter buying.

The explicit purpose of voter audits — formally known as "electoral revisions" — is to combat fraudulent registration, including voters induced to transfer through voter buying. Impressive efforts in recent decades already curbed various other forms of registration fraud: one prominent researcher even argues Brazil "practically eliminated registration fraud" by digitizing its electoral registry and reissuing voter documents in the 1980s (Nicolau 2002, 28). To be sure, the Superior Electoral Court (TSE) regularly employs its computerized, national database to eliminate duplicate registrations (by cross-checking information), remove deceased voters (by processing death certificates), and identify counterfeit voting documents (by verifying ID numbers). However, illegal transfers remain a key concern and officials emphasize audits' crucial role in removing outsiders from the electorate. As the president of Minas Gerais's state electoral court 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" (TRE-MG 2011). Similarly, a state deputy in Mato Grosso argued audits improve mayoral elections by ensuring "only people who actually live there" will vote (*Várzea Grande* 2004).

Our quantitative analyses employ a regression discontinuity design, exploiting the fact that the TSE audits municipalities that surpass an arbitrary threshold.[5] To test predictions, we investigate whether evidence about audits is consistent with the hypotheses about voter buying elaborated above. As shown in Figure 2, a corollary about voter audits in the Brazilian context stems from each voter-buying hypothesis. Evidence consistent with these corollaries would thereby corroborate the voter-buying explanation. Before rigorously testing corollaries in Figure 2, we first provide direct qualitative evidence of voter buying in Brazil.

[FIGURE 2 ABOUT HERE]

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[5]As discussed below, the TSE is considered relatively well-insulated from political influence, and statistical tests find no evidence of partisan bias in the selection of municipalities for audits.

Figure 2: Voter Buying Predictions

Prediction	Hypothesis (Voter Buying)	Corollary (Voter Audits)
1. Small Districts	Voter buying is more likely in small towns and villages than in cities	Voter audits are more likely in small towns and villages than in cities
2. Registration	Voter buying inflates electoral registration by importing many voters	Voter audits reduce electoral registration in districts with many imported voters
3. Incumbents	Incumbents engage in voter buying more than challengers	Incumbents are undermined by voter audits more than challengers
4. Neighboring Districts	Voter buying imports many voters from geographically proximal districts	Voter audits remove many imported voters from geographically proximal districts

3 Qualitative Evidence

Newspapers and official documents provide substantial evidence of voter buying during Brazilian municipal elections. Mayors and councilors are elected concurrently every four years; federal elections are held two years later. Citizens may transfer voter registration once per year, at least 150 days before an election. They must personally submit paperwork in the new municipality and attest to living there at least three months.[6]

Qualitative evidence of voter buying in Brazil is consistent with above hypotheses. As predicted by Hypotheses 1 and 4, voter buying is typically observed in *small municipalities* — where Brazilians are more likely to be pivotal and interact with political operatives[7] — and frequently imports voters from *neighboring districts*. For example, an Amazonas state newspaper explains municipal elections are "historically marked by fraudulent transfers of voters on the periphery of Manaus [the state's capital] to neighboring towns," which typically "involve free transport on Election Day, cash payments, and promises of work" (*A Critica* 2011). Indeed, an investigation by the state's electoral court identified many voters induced with "money, gifts and transportation" to transfer from Manaus to small nearby towns (*A Critica* 2011b). In Pernambuco state, prosecutors found operatives provided up to R$40 (US$24) to voters who transferred from Recife to a small nearby municipality (*Ministério Público-PE* 2008). Likewise, Rodônia state imprisoned a mayoral candidate in Alvorada do Oeste (16,853 citizens) for promising "retirement benefits, cash and employment" to voters in neighboring municipalities in exchange for transferring their voter registration (*Agência Folha* 2000).

[FIGURE 3 ABOUT HERE]

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[6]The appendix provides additional details.

[7]In municipalities with populations below 100,000, only 652 votes separated first and second place mayoral candidates in 2012 — versus 29,296 votes in larger municipalities (median votes; data from TSE). The 2010 Brazilian Electoral Panel Study finds nearly three times the prevalence of campaign visits in small towns as in capital or large cities.

Figure 3: Voter Transfers, State Capitals and Periphery in Brazil
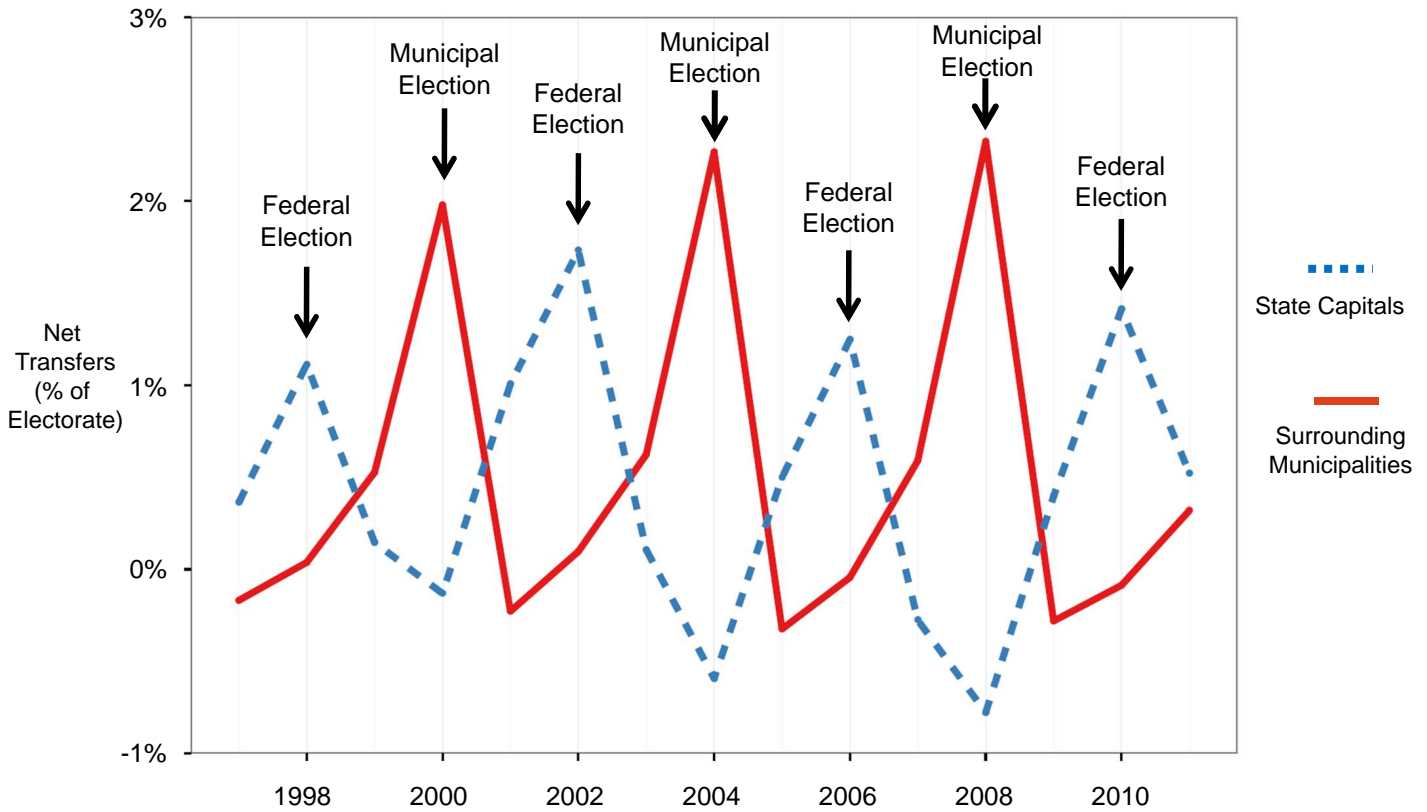
Note: Greater net transfers suggest a relatively higher share of voters transferred into than out of a municipality. Capitals are surrounded on average by 14 municipalities within 50 km. Only aggregate transfers are available for each municipality (i.e., inflows or outflows to all other municipalities combined), so net transfers shown do not sum to zero.

Voter buying contributes to suspicious patterns of voter transfers between state capitals (median population: 797,759) and their neighboring municipalities (median population: 25,160 each). Figure 3 shows small municipalities surrounding state capitals — but *not* capitals themselves — experience sharp net inflows of voters during municipal elections. Yet precisely the opposite is true during federal elections. Judicial officials point to clientelist benefits as a key explanation and investigate citizens "who change their voter residence every two years, in a pendular movement, and present false information" (*Correio Braziliense* 2010). A federal deputy proposed new legislation in response to a "pendular" shift of voters between Brasília and small neighboring municipalities in Goiás, arguing "this practice happens throughout the country."[8] For example, 11 voter-buying recipients were arrested in 2012 for using false addresses to transfer from Brasília to nearby Águas Lindas. According to a police chief: "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]" (*Globo* 2012).

Turning to Hypothesis 2, qualitative sources suggest voter buying inflates *electoral registration* by importing many voters. A newspaper in Rio Grande do Norte state describing voter buying in a small municipality quotes an ex-mayor as saying "Jardim de Piranhas is swollen with so many voter transfers ... that it could burst" (*O Poti* 1996). Across the country in Rio Grande do Sul, court documents discuss how registration inflows due to voter buying contributed to an "unacceptable inflation, absolutely artificial" of electorates in several small municipalities (TRE-RS 2000). And when a national civil society organization launched its anti-clientelism efforts in Mato Grosso state in 2004, its first filed complaint was that voter buying led to a "swelling of voters" in a small municipality neighboring the state's capital (*Diário de Cuiabá* 2004).

Turning to Hypothesis 3, *incumbent politicians* in Brazil enjoy a competitive advantage at voter buying. Mayors, in particular, can interfere with voter registration by wielding power over municipal employees. By law, electoral offices where transfers are processed should only be staffed with

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[8]Projeto de Lei 1866/2011, Câmara dos Deputados, 7/14/2011. See also *Correio Braziliense* (2010b).

civil service personnel of the judiciary. But in reality, understaffing means they often rely on workers granted by the mayor's office.[9] Consider the prosecution of the mayor of Caracol, Piauí (10,212 citizens) for distributing clientelist benefits and illegally transferring voters during his 2008 reelection campaign. During testimony, a municipal employee in the electoral office reportedly admitted "suffering pressure from the mayor" to process illegal transfers from neighboring municipalities (TRE-PI 2010; *Saraiva Reporter* 2011). In Goiás state, authorities arrested a municipal worker in Alexânia (23,814 citizens) for facilitating voter buying: she processed illegal transfers of induced voters from two neighboring municipalities after receiving documents from a candidate's nephew (*Jornal Estado de Goiás* 2010). Voter buying is also easier for incumbent mayors (and allied councilors) because of preferential access to public employment and programs. For example, the mayor of Rio da Conceição, Tocantins (1,714 citizens) was charged for offering public employment to an illegally imported voter during reelection efforts (Ministerio Público Federal 2008). In Nova Ipixuna, Pará (14,645 citizens), a councilor provided *Seguro Defeso* benefits as voter-buying rewards. According to federal charges in 2011, he manipulated this program — which assists poor fishermen during breeding season — to reward imported voters who didn't even fish (Ministerio Público Federal 2011). All in all, qualitative evidence from Brazil is consistent with voter-buying hypotheses in Figure 2.

4   Quantitative Evidence

Given this qualitative evidence of voter buying, we now employ a regression discontinuity design (RDD) to investigate voter audits. The voter-buying explanation is strengthened to the extent that voter audits are consistent with corollaries in Figure 2. While testing corollaries, analyses below focus on two outcome variables: voter registration and mayoral reelection. Consistent with voter buying, audits reduce both registration and reelection. Furthermore, effects are significantly greater in municipalities with large voter inflows, and where neighboring municipalities had large voter outflows.

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[9]Author's interview with Judge Marlon Reis, 8/9/2013.

Voter audits are conducted at the municipal level; the Superior Electoral Court (TSE) orders local electoral courts to re-register all voters in each specified municipality. The RDD exploits a TSE rule about voter audits: they are triggered if the electorate exceeds 80 percent of a municipality's population. Comparing outcomes for municipalities just below and above this arbitrary threshold enables us to estimate causal effects of audits. Strong internal validity is a substantial advantage of the RDD approach: it isolates causal effects of audits for municipalities at the threshold. This feature is particularly important because audited and unaudited municipalities exhibit various differences. For example, audited municipalities tend to be smaller, more rural, and exhibit less population growth. Although a common critique is that RDDs offer strong internal validity at the expense of external validity, an important feature of our dataset mitigates this concern. Many municipalities are near the 80-percent threshold, so our estimation sample contains over one-quarter of Brazilian municipalities. We also demonstrate robustness of findings to an alternative research design including municipalities farther from the threshold (a fixed-effects analysis of precincts in Bahia state).

In 2007, the average municipality's electorate represented 74.4 percent of its population; 1,483 of Brazil's 5,564 municipalities exceeded the 80-percent threshold. Two other criteria must also be met to trigger an audit, but the 80-percent threshold is reached by far fewer municipalities and thus has the greatest influence on the probability of an audit.[10] Another relatively minor source of audits is state electoral courts, which can authorize them in municipalities by randomly conducting home visits of five percent of voters and demonstrating substantial registration irregularities.

[FIGURE 4 ABOUT HERE]

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[10]The other two criteria are: (a) electorate is at least double the summed population of citizens aged 10-15 and over 70 years; and (b) voter transfers increased at least 10% over the past year. Whereas 27% of Brazil's municipalities fulfill the 80% criterion, over 99% fulfill (a) and over 60% fulfill (b). Three-fourths of municipalities over the 80% threshold meet both other criteria.

Figure 4: Electorate Trigger and Voter Audits

(a) Audit Frequency by Electorate Trigger

(b) Population by Electorate Trigger
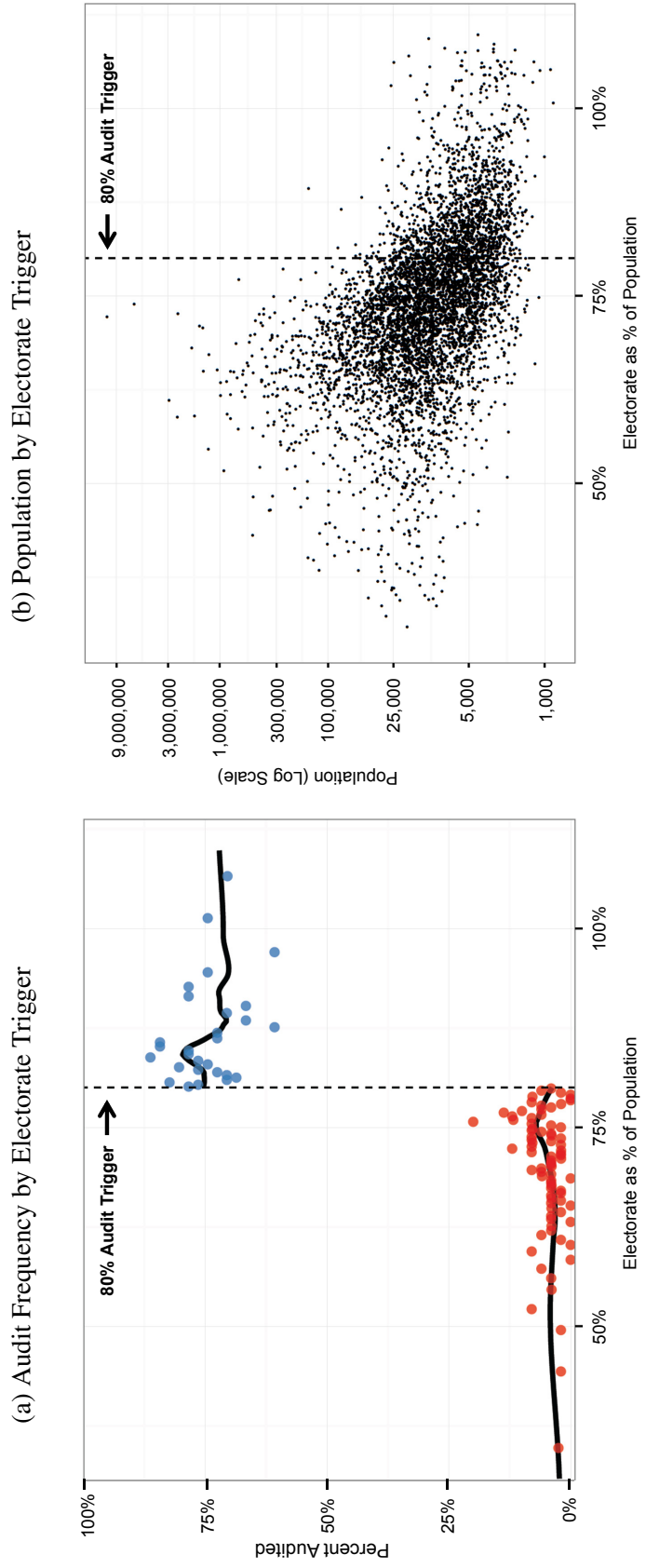
Note: Figure 4a shows the share of municipalities audited, using 1% bins.

We analyze the 2007-08 wave of voter audits, which was the most comprehensive in decades and constituted nearly one-third of all audits conducted over the past 15 years.[11] Figure 4a demonstrates the discontinuous increase in audits at the 80-percent threshold, though this rule was not completely deterministic. Election officials did not conduct audits in one-fourth of municipalities above the threshold, as they did not meet the other two TSE criteria. Furthermore, they conducted audits in five percent of municipalities below the threshold, due to the state-level process. Because audit assignment was not completely deterministic, "sharp" RDD methods are inappropriate as they would yield biased estimates of audits' effects. To account for this "fuzzy" discontinuity, we treat the issue as a non-compliance problem, as is standard in the program evaluation literature (Angrist and Lavy 1999, 259-267).

4.1 Corollary 1: Small Districts

Before analyzing the RDD, we note audits predominantly target small municipalities — consistent with Corollary 1. During the extensive 2007-08 audit wave, Brazil audited only two municipalities with populations over 90,000 citizens; three-fourths of audited municipalities have under 11,300 citizens. As shown in Figure 4b, the key audit trigger — the electorate-population ratio discussed above — is inversely related to municipality size. Due to the targeting of small municipalities, only 5.4 percent of the nation's electorate (6.8 million voters) had to re-register, but they lived in 23 percent of Brazil's municipalities (TSE 2007). In short, audits are most likely in small districts, consistent with voter buying.

4.2 Regression Discontinuity Design

To formally introduce the RDD, let R_i be a binary variable denoting whether municipality i underwent an audit and let E_i be the "forcing" variable (i.e., electorate-population ratio). A_i is a dummy variable indicating E_i exceeds the threshold. Using potential outcomes notation, $Y_i(R_i, A_i)$ is the realized outcome when a municipality receives an audit (or not), and when it is above the threshold

[11]Funding constraints inhibited previous waves of audits; this problem was rectified in 2007-08. See TSE Resolutions 20.769 (2001), 21.490 (2003), 22.050 (2005), and 22.586 (2007).

(or not). Similarly, let $R_i(A_i)$ indicate whether the municipality was audited when above or below the threshold. The RDD method assumes smoothness (continuity) of potential outcomes at the threshold (Hahn, Todd and Van der Klaauw 2001). One might question this assumption because mayors who employ voter buying might attempt to manipulate their electorate-population data to fall below the threshold. Evidence suggests no such manipulation. First, indicators such as margin of victory, campaign resources, and candidate age (a proxy for experience) do not vary significantly across different values of the forcing variable.[12] But even if mayors exert effort to influence their electorate-population ratio, prominent work by Lee (2008) demonstrates such manipulation does not bias RDD estimations unless agents have *precise* control over the forcing variable. And in Brazil, mayors do not have precise control, which would require changing the municipality's electorate *precisely*, and knowing *exactly* the population of her municipality (or vice versa). As shown in the appendix, both conditions are impossible. The electorate size was in flux for various reasons outside of mayors' precise control including deaths, cancellations, new registrations and outflows. Moreover, the TSE did not calculate the electorate statistics — and the census bureau did not release the population estimates — utilized in the ratio until *months* after the last date on which mayors could import voters. There is also no evidence of involvement of higher-level officials. The TSE is considered relatively well-insulated from political influence,[13] and balance tests discussed below suggest mayors who are co-partisans with the president or their governor are not disproportionately under (or over) the threshold. The appendix provides extensive further evidence against sorting, including results from a McCrary Density Test (McCrary 2008). Consistent with the smoothness assumption, this formal test similarly finds no evidence of manipulation.

Given the smoothness assumption, the RDD approach estimates 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 the effect of a municipality being above or below the threshold (the reduced form estimate) at the discontinuity point irrespective of audit status. As established by Angrist, Imbens

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[12]See online appendix.

[13]Metrics such as judge tenure and appointment also point to the TSE's independence (Rosas 2010).

and Rubin (1996), making three assumptions — (1) crossing the threshold only affects the outcome through implementation of audits ("exclusion" restriction), (2) there exists a "first stage" effect of crossing the threshold on audits, and (3) monotonicity[14] — enables us to estimate the following quantity using an instrumental variables approach:

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

In words, τ_R is the treatment effect for municipalities with $E_i = 80$ that are audited due to surpassing the threshold. The exclusion assumption is reasonable given that no other interventions are triggered at the threshold and there are no substantive reasons why being just above or below the threshold would directly change political outcomes. Figure 4a shows the first-stage condition is met as the proportion of municipalities audited jumps sharply at $E_i = 80$.[15]

4.2.1 Specification

The RDD identifies the average treatment effect for municipalities at the threshold that were audited in accordance with the 80-percent trigger. Estimating this quantity requires two primary choices: the estimator and the amount of data to retain around the discontinuity (i.e., the "bandwidth"). We employ a local linear estimator (with a bandwidth of ±4 percent) and show robustness to a difference-in-means estimator (with a bandwidth of ±1.5 percent). These bandwidths were selected to ensure covariate balance on a wide range of variables, while leaving sufficient data to estimate treatment effects precisely.[16] The appendix demonstrates robustness to a wide array of alternative bandwidth choices (even bandwidths of ±0.5 percent and ±0.25 percent, respectively),

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[14]The monotonicity assumption stipulates there are no municipalities that would receive an audit if they failed to cross the threshold, but that would not be audited if they *did* cross $E_i = 80$. This scenario is implausible in Brazil.

[15]The appendix presents formal "first stage" estimates. The first stage effect of being above the discontinuity on being audited is large (coefficient of .72) and highly statistically significant.

[16]Automatic bandwidth selection algorithms by Ludwig and Miller (2007) and Imbens and Kalyanaraman (2012) recommended implausibly large bandwidths that could introduce bias by using data far from the threshold.

as well as placebo tests showing insignificant results if thresholds other than the true 80-percent audit trigger are employed.

To estimate τ_A (the "reduced form"), we use OLS. For local linear estimates, we employ the following model:

$$Y_i = \alpha + \beta \cdot (E_i - 80) + \tau_A \cdot A_i + \gamma \cdot (E_i - 80) \cdot A_i + \epsilon_i$$

This model allows for a linear relationship between the outcome and the forcing variable to be estimated separately on each side of the discontinuity within the bandwidth window. For difference-in-means specifications, we estimate a bivariate regression with a treatment indicator for whether the municipality is above the 80-percent threshold. To estimate τ_R, we use the two stage least square analogues of the reduced form equations where we instrument compliance status (R_i) with A_i within the discontinuity window (Angrist and Pischke, 2008, p. 262). We use Hubert-White ("robust") standard errors, which account for any heteroskedasticity.

4.2.2 Data

We employ electoral data from the Superior Electoral Court (TSE) and demographic data from the Institute of Applied Economic Research (IPEA). The TSE's audit trigger utilized municipalities' electorates in June 2006 and populations in July 2006. Given Brazil's mayors may only serve two consecutive four-year terms, for all specifications examining incumbent reelection, we only consider municipalities with first-term mayors. We determined eligibility for reelection in 2008 by matching names of 2000 mayoral winners to names of 2004 mayoral winners. The online appendix provides descriptive statistics for all variables.

4.2.3 Assessing Validity of the Design

[FIGURE 5 ABOUT HERE]

To ensure RDD validity, we confirm balance of covariates on each side of the threshold. Figure 5 displays balance statistics for 30 covariates for three specifications (see caption). The figure suggests excellent overall covariate balance. Compared with the full sample, the RDD increases balance for nearly all covariates. For local linear specifications — our main specification — all
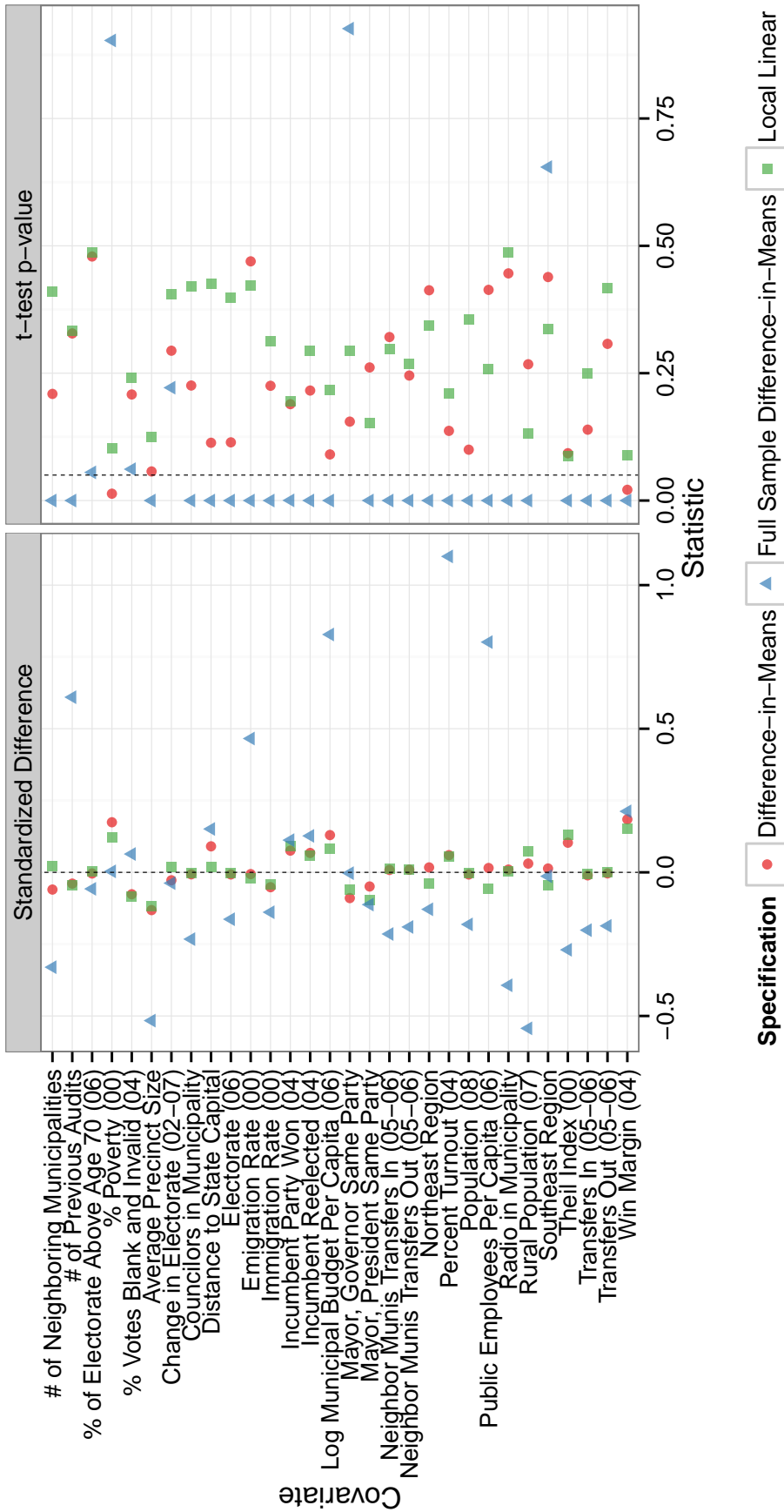
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Figure 5: Covariate Balance. Panel on left shows standardized differences (i.e., estimate divided by pooled standard deviation). Panel on right shows p-value from t-test of equality of means.

30 covariates are balanced (i.e., they are not statistically significant at the 5 percent level). For difference-in-means specifications — used as a robustness check — 28 of 30 covariates are balanced (at the 5 percent level). For this latter estimator, note that observed imbalances in win margin would likely bias our estimates *against* finding evidence in favor of our predictions. The online appendix shows all findings are robust to using parametric covariate adjustment to control for all variables with standardized differences higher than 0.10, as well as including state fixed effects.

4.3 Corollary 2: Registration

We turn to the registration corollary: if voter buying increases the electorate, then voter audits should reduce the electorate (see Figure 2). Consistent with this prediction, Figure 6a reveals audits removed a substantial number of voters from the rolls. The plot shows the conditional expectation of the change in the electorate size (as a percentage of the population) on both sides of the 80-percent audit trigger.[17] Table 1 presents both the reduced form estimates ($\hat{\tau}_A$) and estimates accounting for noncompliance ($\hat{\tau}_R$); our discussion focuses on local linear estimates adjusted for compliance. The electorate in municipalities just below the trigger increased 4.2 percentage points, compared to a decrease of 5.0 percentage points just above the trigger, corresponding to a reduced form estimated effect of -9.2 percentage points. When non-compliance is taken into account, the estimated treatment effect (τ_R) rises to -12.5 percentage points. In other words, audits removed roughly 1,900 voters from the typical municipality, consistent with Corollary 2 and voter buying. Table 1 shows this decrease in registered voters is also reflected in lower turnout (as a percentage of the population); the estimated treatment effect adjusting for non-compliance is -6.0 percentage points.

[FIGURE 6 ABOUT HERE]

[TABLE 1 ABOUT HERE]

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[17]Analyses compare electorate sizes before and after the TSE updated rolls with audit results (November 2007 and May 2008, respectively).

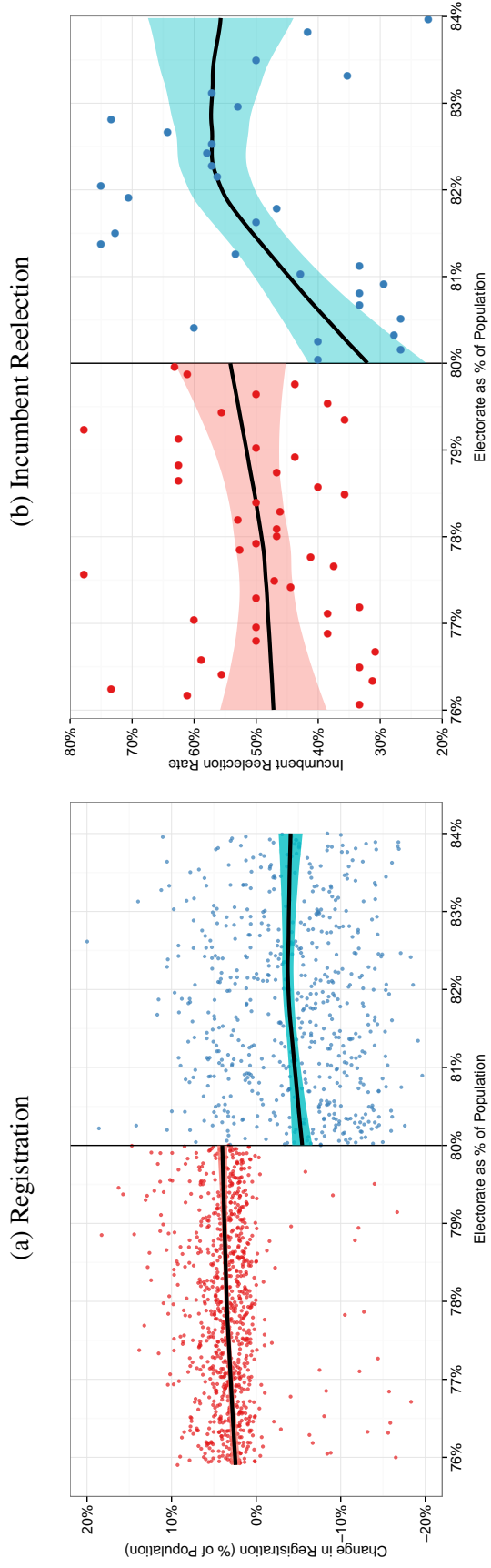Figure 6: Effect of Voter Audits on Brazil's 2008 Municipal Elections

Figure 6 shows effect of audit on (a) change in voter registration and (b) incumbent reelection (in 2008). For reelection plot, each dot represents the average reelection rate within a bin containing 20 municipalities. Conditional average (black lines) and 95% confidence interval (shaded region) are loess regression estimates.

Table 1: Effect of Voter Audits on Brazil's 2008 Municipal Elections (RDD Estimates)

Outcome	$\hat{\tau}_A$	SE_{τ_A}	$\hat{\tau}_R$	SE_{τ_R}	Baseline	n
Local Linear Specification						
Change in Registration (%)	−9.22	0.65	−12.46	0.70	4.18	1477
Change in Turnout (%)	−4.31	0.51	− 5.99	0.65	5.36	1477
Incumbent Reelected	−0.15	0.06	− 0.18	0.08	0.53	1107
Incumbent Party Reelected	−0.11	0.05	− 0.13	0.07	0.36	1477
Incumbent Runs for Reelection	−0.08	0.05	− 0.10	0.07	0.75	1107
Change in Incumbent Vote Share (%)	−3.73	1.41	− 5.20	1.94	3.73	802
Difference-in-Means Specification						
Change in Registration (%)	−8.73	0.55	−11.93	0.63	3.72	577
Change in Turnout (%)	−3.97	0.42	− 5.49	0.55	5.07	577
Incumbent Reelected	−0.12	0.05	− 0.16	0.07	0.52	428
Incumbent Party Reelected	−0.08	0.04	− 0.11	0.05	0.36	577
Incumbent Runs for Reelection	−0.10	0.04	− 0.14	0.06	0.76	428
Change in Incumbent Vote Share (%)	−3.25	1.17	− 4.72	1.69	3.53	307

$\hat{\tau}_A$: local average effect of municipality with electorate-population ratio over 80% ("reduced form"); SE_{τ_A} is its "robust" standard error. $\hat{\tau}_R$: estimated local average effect of audit; SE_{τ_R} is its "robust" standard error. Baseline estimates value of dependent variable among controls at $E_i = 80$. For *Incumbent Reelected*, *Incumbent Runs for Reelection*, and *Change in Incumbent Vote Share*, sample includes only municipalities with incumbents eligible for reelection.

4.4 Corollary 3: Incumbency

We turn to the incumbency corollary: if incumbents engage in more voter buying than challengers, then voter audits should disproportionately undermine incumbents' electoral prospects (see Figure 2). Audits can influence incumbent reelection 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 dependent variable is simply whether the incumbent mayor retained power, regardless of whether she ran for reelection. This formulation is optimal because the TSE announced which municipalities would be audited over a year before the 2008 election; some audited mayors may have thus considered reelection tougher and opted not to re-run. 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, which requires us to subset on municipalities where the incumbent re-ran. In addition to examining effects on incumbent victory and vote share, we examine audits' effect on whether the incumbent *party* wins again, which does not require conditioning on eligibility for reelection.

Findings are consistent with the incumbency corollary and voter buying. Figure 6b presents the effect of audits on incumbents' continuity of power. Each dot represents the percentage of incumbents reelected (in bins of 20 municipalities). A clear discontinuity exists between the average reelection rate immediately above and below the 80-percent threshold. As reported in Table 1, the audit's local average effect is -.18 (significant at the 5 percent level). To put this estimate in perspective, a typical incumbent has a baseline probability of reelection of about 53 percent, but after an audit her probability of victory falls to only 35 percent.

Focusing on incumbent *parties*, rather than just incumbent mayors, also yields effects consistent with predictions. Table 1 reports statistically significant, albeit smaller, negative audit effects on the probability of incumbent party victory. The incumbent-parties result appears to be entirely driven by the effect on the incumbent herself: when estimating the effect on the sample of municipalities with incumbents ineligible for reelection, the point estimate is small and statistically insignificant (not shown).

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Recall that audits can affect the continuity of power through an incumbent's decision about whether to run and/or her electoral performance. We find evidence of both mechanisms. The point estimates on the incumbent's probability of re-running are negative and on the order of -.10, albeit imprecisely estimated. While not consistently significant, the point estimates are large enough to suggest strategic dropout could be a mechanism by which audits affect whether incumbents remain in power.

Whereas above analyses examine the share of incumbent mayors retaining power — a measure robust to strategic candidate dropout — we also consider incumbent vote share. This alternative measure requires conditioning on the incumbent re-running. However, note 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. Ignoring this issue, we find a statistically significant effect of about -5.2 percentage points.[18] The magnitude of this effect exceeds the vote margin in over a third of Brazil's 2008 municipal elections, suggesting why relatively small swings in vote share can reduce reelection rates.

4.5 Corollary 4: Neighboring Districts

Just as RDD estimates are consistent with the registration and incumbency corollaries, so too are heterogeneous treatment effects of the RDD consistent with the neighboring districts corollary. Audits are expected to harm a machine's electoral prospects more sharply if it recently imported many voters from neighboring districts through voter buying. Thus, the negative effect on registration and mayoral reelection should be greater if: (1) many voters recently transferred out of neighboring municipalities, and (2) many voters recently transferred into the municipality.[19]

[FIGURE 7 ABOUT HERE]

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[18]Vote share measured as a percentage of population. Following Manski (1995), assuming audits would have a greater negative effect on dropouts' vote share, an audit's effect unconditional on re-running is contained within nonparametric bounds of -5.8 and -37.9 percentage points.

[19]Precinct-level data below provides an even more direct test.

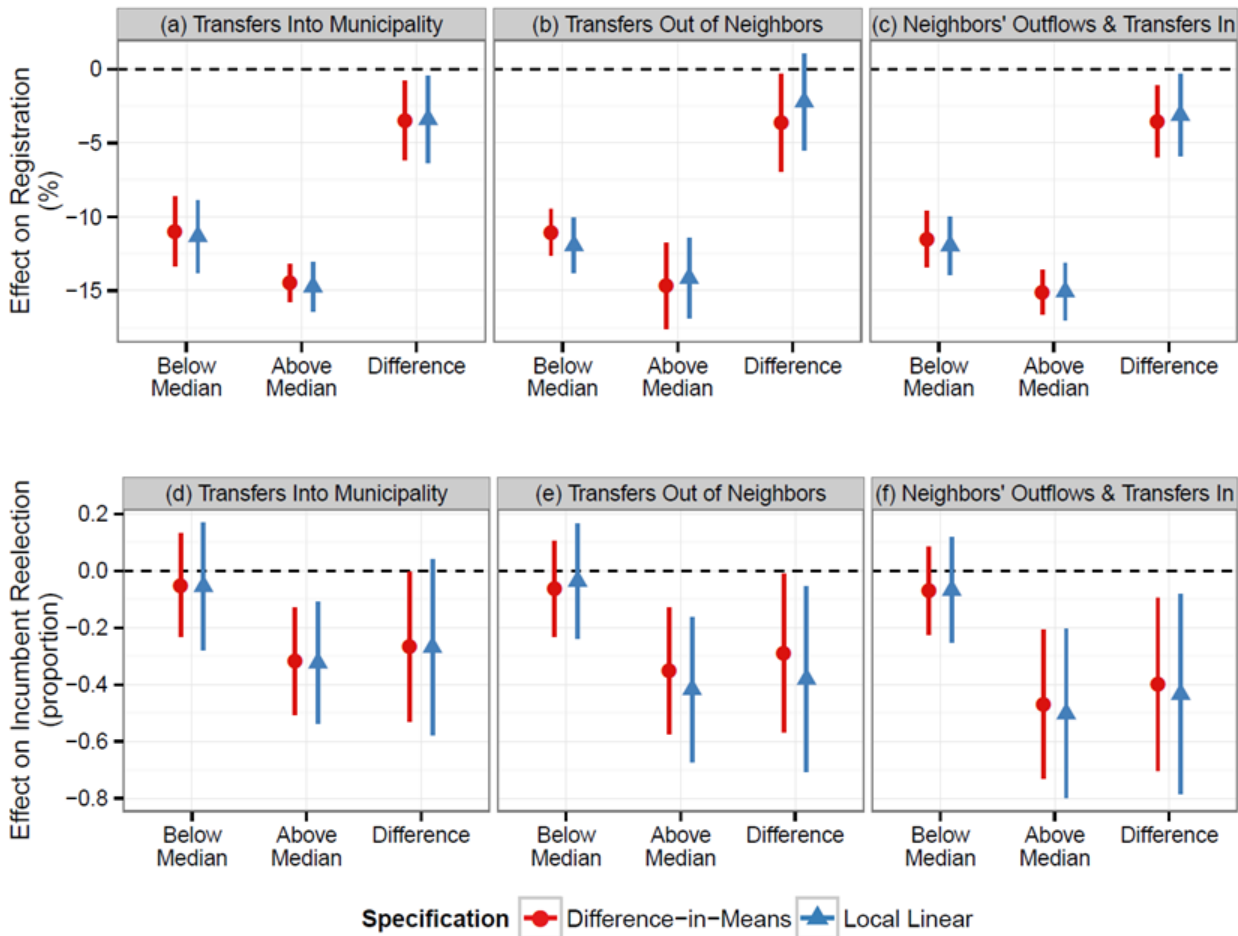Figure 7: Effects of Voter Audits by Level of Voter Transfers (RDD Estimates)

Figure 7 shows how audit effects on registration and incumbent reelection vary by whether municipality has above-median voter transfers into the municipality (panels a and d) and out of neighboring municipalities (panels b and e). Panels c and f show how effects vary by whether municipality has above-median levels of both transfers in *and* transfers out of neighbors. Difference between strata effects is also shown (i.e., the "interaction"). Vertical lines represent 95% confidence intervals.

To test predictions, we first split our sample by median transfers *into* municipalities (180 voters). Figures 7a and 7d show how an audit's effect on voter registration and incumbents' continuity of power varies by whether inflows are above or below-median. For both registration and reelection, the difference is substantial. The effect on registration in below-median municipalities is just -11.3 percentage points, versus -14.7 percentage points in above-median municipalities. The difference between these estimates (labeled "difference" in the plot) is statistically significant. Incumbent reelection is even more striking. We find *no* effect of audits on incumbent reelection in below-median municipalities: the point estimate of -0.05 is statistically insignificant. By contrast, in above-median municipalities, the effect is a -0.32 change in probability of winning. For both outcomes, consistent with voter buying, audit effects are substantially larger if many voters recently transferred into the municipality.

Furthermore, a similarly large degree of heterogeneity emerges when splitting the sample by median transfers out of neighboring municipalities (1,646 transfers). Figures 7b and 7e reveal effects on both registration and reelection are substantially larger in municipalities with above-median neighbor outflows. Similar to findings above, audits do not affect the average probability of incumbent victory when neighboring municipalities have relatively few outflows.

We next stratified data by whether a municipality met both criteria — that is, above-median inflows *and* above-median transfers out of neighboring municipalities. Figures 7c and 7f show heterogeneity across municipalities meeting both criteria versus those that did not. Once again, differences across strata are dramatic. Both registration and mayoral reelection are cut significantly more in districts meeting both criteria. And the effect of audits on reelection is statistically indistinguishable from 0 in municipalities that do not meet both transfer criteria. In sum, findings for both registration and incumbent reelection are consistent with the neighboring districts corollary and voter buying.

4.6 Alternative Explanations

Evidence thus far corroborates the four corollaries in Figure 2, consistent with voter buying. We now show RDD findings are inconsistent with four key alternative explanations.

First, consider emigration. According to this explanation, audits remove voters who previously emigrated from small municipalities to cities but failed to transfer their voter registration. One major problem with this explanation: Brazil automatically cancels the voter registration of "missing voters" — citizens who fail to vote or justify their absence in three consecutive elections. Furthermore, some elections have two rounds, with each round counting as a missed election. In addition, absentee voting is generally forbidden in Brazil. Thus, emigration resulting from longer-term patterns of urbanization can account for audit removals only in cases where city dwellers return to their birthplaces every two years to vote (or alternatively, repeatedly justify their absence).

Emigration is also inconsistent with RDD findings. If emigration were a valid explanation, one would expect audits to have a greater impact on registration and mayoral reelection in municipalities with higher emigration rates. To test this possibility, we obtained 2010 census data on the percentage of citizens in each municipality who emigrated between 2005 and 2010. As shown in Figure 8a, treatment effects on both outcomes are not heterogeneous across emigration rates, pointing away from emigration as an alternative explanation.[20] Overall, evidence does not support the emigration explanation.

[FIGURE 8 ABOUT HERE]

A second alternative explanation is that audits undermine incumbents' reelection prospects by disproportionately removing *legitimate* voters who are incumbent supporters. Undermining incumbents requires asymmetric cancellations, so the most compelling argument employing this logic involves elderly voters. Voters at least 70 years old are exempt from compulsory voting laws and consequently may be less likely to re-register after an audit. If elderly voters are disproportionately pro-incumbent, then incumbents could conceivably lose at higher rates after an audit. To test this possibility, Figure 8b compares audit effects in municipalities with above versus below-median percentages of citizens aged 70 years and older. The elderly hypothesis does not hold

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[20]The appendix shows effects are not heterogeneous by three additional proxies of emigration, including pre-audit voter outflows.

Figure 8: Test of Alternative Explanations (Heterogeneity of RDD Estimates)
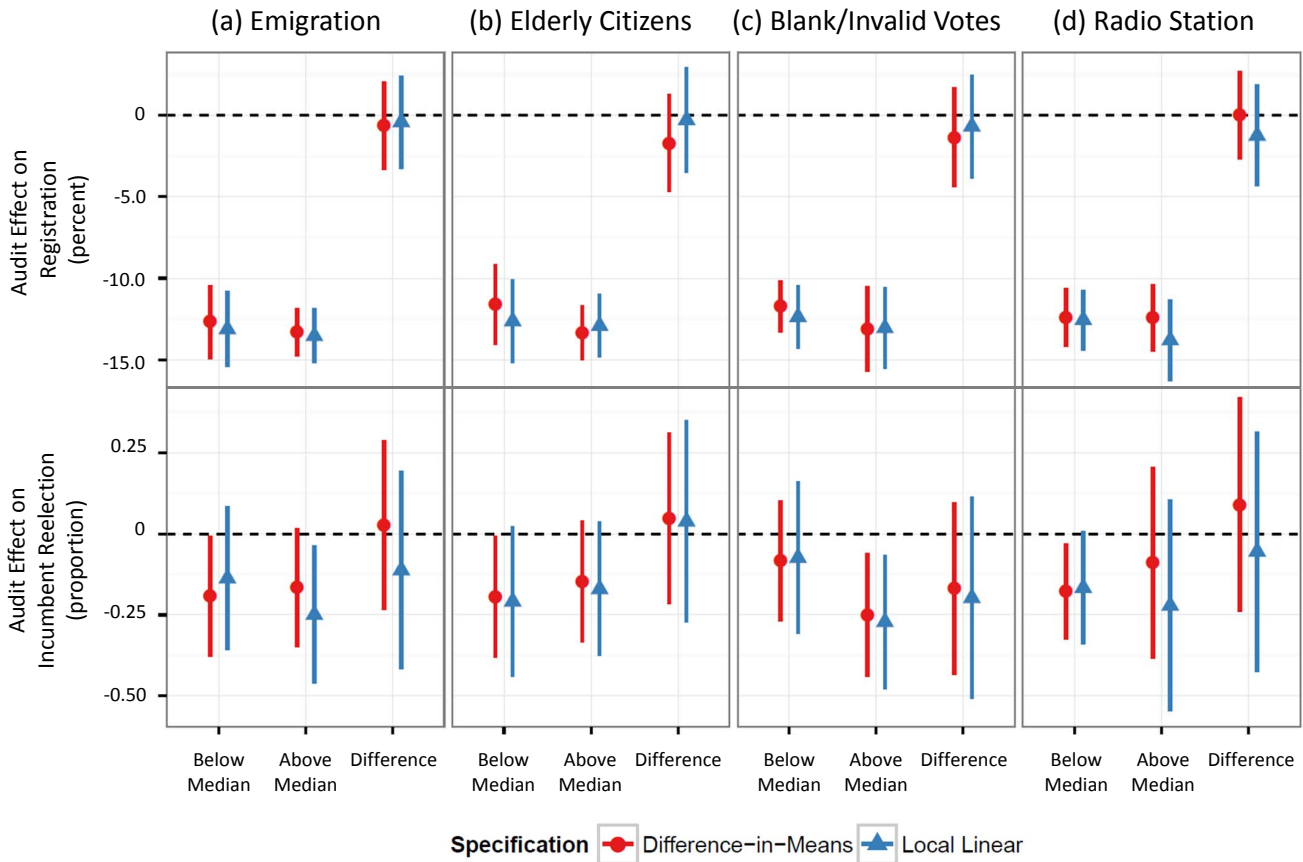
Note: Figure 8 suggests audit effects do not significantly vary by whether municipality has above-median values for measures of alternative explanations described in text. Vertical lines represent 95% confidence intervals.

up — one would expect audits to have a greater impact on registration and mayoral reelection in municipalities with disproportionately more elderly voters. Yet treatment effects are not heterogeneous across this variable. Moreover, analyses suggest the elderly account for only 15 percent of the overall electorate decrease experienced during audits (not shown). Overall, evidence points away from the elderly hypothesis.

A third alternative explanation involves disinterested voters. One might hypothesize such voters are less likely to re-register and are disproportionately pro-incumbent. The 2010 Brazilian Electoral Panel Study suggests blank and invalid votes can be used as a proxy for disinterest in politics (see appendix). If this alternative hypothesis holds, one would expect audits to have a greater impact on registration and mayoral reelection in municipalities with above-median blank and invalid votes. But as shown in Figure 8c, treatment effects are not heterogeneous across this variable.

Finally, a fourth alternative explanation is that audits reduce reelection rates by exposing mayors' malfeasance regarding electoral rolls. The underlying logic is that when audits uncover incumbents' illicit tactics, voters learn this information, update perceptions about incumbents, and punish them electorally. One would thereby expect audit effects on reelection to be magnified in municipalities with local media to report about audits. Excellent work by Ferraz and Finan (2008) — which investigates audits of federal funds (in 373 Brazilian municipalities) instead of voter audits — finds precisely this result. But voter audits exhibit no such pattern, belying this alternative explanation. We follow Ferraz and Finan (2008, 2011) by examining heterogeneity by presence of radio station. Unlike their study, Figure 8d shows voter audits are not heterogeneous by whether municipalities have radio stations. This explanation also cannot explain findings *within* municipalities, as discussed below. Overall, evidence is inconsistent with alternative explanations.

5 Robustness at the Precinct Level

As further corroboration of predictions, we employ an alternative research design using precinct-level data from Bahia, the state where audits are most prevalent. Fixed-effects regressions ex-

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ploiting *within*-municipality variation establish that transfers from neighboring municipalities are correlated with the share of voters purged from the electorate during audits ("audit removals"). Furthermore, incumbents perform more poorly in precincts with many audit removals, especially those recently importing voters from neighboring municipalities. At the outset, we note the RDD approach above offers greater internal validity; inferences from this fixed-effects analysis rest on stronger assumptions. Also, we do not claim Bahia is representative of the entire country; for instance, it is relatively poor and is the most populous state in Northeast Brazil (with 15.1 million citizens).[21] Despite these limitations, specifications below offer two advantages. First, because specifications below suggest audit effects are robust at the *precinct* level, they help to rule out any potential alternative explanation for audit effects that operates at the municipal level (e.g., campaign effects). Second, our data requests yielded richer transfer data to test hypotheses — in addition to the number of transfers, for Bahia we know where voters departed from and arrived to. These granular data enable us to investigate more directly whether effects are larger where more transfers originated from neighboring municipalities. When the RDD and fixed-effects results are considered jointly, they provide compelling evidence of voter buying.

5.1 Data and Statistical Model

The fixed-effects analysis examines precinct-level data in Bahia, where 42 percent of municipalities were audited during the 2007-08 wave. To examine effects of audits on incumbent reelection, we analyze audited municipalities where the incumbent mayor ran for reelection in 2008. In Bahia, 140 audited municipalities had mayors eligible for reelection; 89 of these mayors ran for reelection. This decision to seek reelection is potentially affected by the degree to which an audit reshapes the electorate, which would induce sample selection bias. Reassuringly, however, there is no correlation between the decision to re-run and the percentage of voters removed by audits. Even still, if weak incumbents disproportionately drop out when audited for fear of losing, comparatively

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[21]We obtained precinct-level data on audit removals and transfers directly from Bahia's state electoral court during fieldwork. Multiple requests for comparable data nationally were unsuccessful.

stronger incumbents will be over-represented in the estimation sample. If strong incumbents are less affected by audits, then our coefficient estimates for incumbent vote share will be downwardly biased and thus *conservative*.

We estimate the effect of transfers from neighboring municipalities on audit voter removals using this specification:

$$R_i = \delta T_i + c_m + \mathbf{X}_i \boldsymbol{\beta} + \epsilon_i$$

The dependent variable R_i is the number of voters purged during an audit as a percentage of 2004 registered voters in precinct i. The primary independent variable T_i is a variable measuring the number of transfers from neighboring municipalities. The term c_m is a municipality-specific fixed effect. $\mathbf{X}_i$ is a vector of control variables including a rural-precinct dummy, logged registered voters in 2004, percentage of voters affiliated with the incumbent's party, and percentage affiliated with any party. We estimate audit effects on electoral outcomes employing this specification:[22]

$$y_{i,2008} - y_{i,2004} = \tau R_i + c_m + \mathbf{X}_i \boldsymbol{\beta} + \epsilon_i$$

The dependent variable $y_{i,2008} - y_{i,2004}$ is the change in votes (as a percent of valid votes) received by the incumbent between 2004 and 2008.[23]

Table 2 presents results of the precinct-level analysis. Columns 1 and 2 show a statistically significant positive correlation between transfers from neighboring municipalities and voters removed by audits. In column 1, we follow our RDD specifications by examining a dummy variable

[22]Because we employ a first-differenced dependent variable and municipality fixed effects, this specification is equivalent to a difference-in-differences model with precinct-specific fixed effects and municipality-by-year fixed effects. We model R_i's effect as linear because inclusion of higher order terms for a more flexible specification showed no improvement in model fit.

[23]Due to growth in the number of precincts, 20% of 2008 precincts could not be matched to 2004 precincts. However, missingness in 2004 is uncorrelated with audit cancellations and incumbent vote share in 2008 (conditioning on municipality fixed effects), suggesting sample selection will not contaminate estimates. Estimates employ data for precincts observed in both periods.

indicating above or below-median inflows from neighbors. We find precincts with above-median inflows experienced significantly more audit removals than below-median precincts. The estimated coefficient represents 18 percent of a standard deviation of audit removals in precincts within our sample. Column 2 employs logged neighbor transfers and similarly finds a significant positive association.

In columns 3 through 5, we examine the effect of audit removals on changes in incumbent vote shares between 2004 and 2008. Column 3 shows that the percentage of voters purged due to audits is negatively associated with change in incumbent vote share. This magnitude implies in a typical precinct, a 10 percentage point increase in audit removals is associated with a 1 percentage point decrease in incumbent vote share.

[TABLE 2 ABOUT HERE]

Considerably stronger effects on reelection are observed if a precinct recently imported many voters from neighboring municipalities. Paralleling the RDD heterogeneous treatment effects analysis, column 4 examines whether incumbent vote shares fall more in precincts that had above-median transfers from neighbors over the previous two years. Consistent with Section 4.5, the negative effects of audits in columns 1-2 are almost entirely driven by such precincts. In precincts with below-median transfers from neighbors, the point estimate is negative but small and statistically insignificant. In precincts with above-median transfers from neighbors, the effect of a 10 percentage point increase in audit removals is a substantially larger 1.8 percentage point decrease in incumbent vote share — over 10 times that in below-median precincts. Column 4 shows a similar pattern using logged transfers as the interaction variable. To contextualize these estimates, the median Bahian precinct lost about 15 percent of its electorate due to audits, resulting in a 3 percentage point loss in incumbent vote share (based on column 4) in precincts with above-median transfers from neighbors. Given the high competitiveness of many mayoral elections in Bahia — 13 percent had vote margins of less than 3 percent — voter audits likely tipped the electoral outcome in many municipalities.

Table 2: Precinct-Level Results for Bahia State

	Dependent variable:				
	Audit Removals		Change in Incumbent Vote Share (04-08)		
	(1)	(2)	(3)	(4)	(5)
Transfers from Neighbors (Above Median)	0.97**			2.27*	
	(0.16)			(0.97)	
Transfers from Neighbors (Logged)		0.94**			2.03
		(0.20)			(1.13)
% Audit Removals			−0.10**	−0.01	−0.04
			(0.04)	(0.05)	(0.04)
% Audit Removals x Transfers from Neighbors (Above Median)				−0.18**	
				(0.06)	
% Audit Removals x Transfers from Neighbors (Logged)					−0.18*
					(0.07)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	3,535	3,535	3,224	3,224	3,224

Note: *p<0.05; **p<0.01. Robust standard errors in parentheses. See text for definitions of variables.

Overall, the fixed-effects specifications are remarkably similar to RDD findings, even though they employ a distinct research design and a different dataset. Taken together, they provide substantial evidence in line with predictions of voter buying.

6 Discussion

Evidence of voter buying challenges a fundamental assumption underlying studies of clientelism. Although scholars typically assume rewards influence actions of the existing electorate, they may also *shape* the electorate. Beyond influencing vote choices and turnout, clientelist benefits can induce outsiders to transfer their voter registration and deliver political support. Voter buying also bridges the literature on clientelism and fraud, because it demonstrates how contingent benefits serve as a mechanism of registration fraud. Our empirical analyses (using both RDD and fixed-effects specifications) suggest voter buying has substantial effects on both voter registration and mayoral reelection in some Brazilian municipalities. Our findings lay the groundwork for an important research agenda: conducting theoretical and empirical work on the various ways in which clientelism may induce citizens' participation in fraud.

The concept we introduce — voter buying — travels far beyond Brazil. This paper's introduction provides evidence about its existence in 10 additional countries: Bolivia, Botswana, Bulgaria, Ghana, Jordan, Kenya, Mexico, the Philippines, Swaziland, and the historic U.S. To test external validity more thoroughly, cross-national survey research should investigate voter buying's prevalence. The external validity of hypotheses posited in this paper are also an important research question; in different contexts, other hypotheses may prove to be salient. For example, politicians across districts could plausibly compete or collude with each other for voters, with variation across co-partisanship. No such patterns are observed in Brazil, potentially reflecting weak partisanship in local-level politics. Other hypotheses might involve how specific contextual differences shape the extent to which *challengers* as well as incumbents pursue voter buying and other forms of fraud (see Cantú 2013). Overall, the generalizability of voter buying and specific hypotheses deserve further investigation.

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Another important avenue for future research is examining how machines combine voter buying and other strategies of clientelism. Machines may employ more voter buying in contexts with rigorous ballot secrecy, because it does not require monitoring vote choices. Voter buying may also be attractive where stringent compulsory voting inhibits turnout buying. These considerations suggest why Brazil may be a propitious environment for voter buying: Brazil became the first country in the world to introduce fully electronic voting in 2000 (undermining vote buying), and penalizes abstention with substantial bureaucratic hassles (undermining turnout buying). As a next step, formal and empirical research should investigate how various factors — including ballot secrecy and compulsory voting — affect portfolios of clientelism that include voter buying.

Future research should also examine how politicians trade off voter buying with other forms of electoral fraud. For example, machines may find it cheaper to pad electoral rolls with fictitious "ghost voters" before an election, or rejigger ballot totals after an election. In Brazil, voter buying became relatively more attractive as a national computerized registry undermined other registration fraud, and electronic voting hampered fraud *after* voting. The latter technology obviated various common strategies, such as adding votes to tabulation sheets, claiming opposition votes were illegible, and filling out candidates' names on blank ballots (*Estado de São Paulo* 1994, 1994b). On the other hand, voter audits investigated in this study had implications for preventing voter buying. The strategy became relatively less attractive as audits expunged many imported voters from the rolls, and as politicians in audited municipalities faced charges of voter buying (e.g., *Ministerio Público Federal* 2008). The impact of future policy and technological shifts remains uncertain. Brazil is installing biometric voting, which could suppress voter buying by requiring broad re-registration, but could also stimulate substitution towards voter buying if other fraudulent strategies become impossible. How such factors affect trade-offs between voter buying and broader tools of electoral fraud warrants further attention.

Overall, this study challenges scholars to deepen their understanding about the logic by which machines distribute contingent benefits. Our analyses encourage researchers to pay closer attention to the ways in which clientelist exchanges may underpin electoral fraud. Given that voter buying

dramatically influences municipal elections in Brazil, such unexplored phenomena may well have substantial effects on the practice of democracy across the world.

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Appendix

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A Additional Details about Voter Transfers

Voter buying in Brazil involves the active participation of 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, 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).

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[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.

[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.

B   Additional Details about Voter Audits

Voter audits are always conducted at the *municipal* level. That is, regardless whether upcoming elections are municipal or federal, the TSE examines municipal-level data and assigns audits. The local electoral court is then ordered to re-register all voters in a specified municipality.

Descriptive evidence suggests that audits are most likely during municipal elections in Brazil. First, consider an important temporal pattern: the TSE orders most audits (78 percent in 1997-2012) during the six-month window before it finalizes the electoral rolls for each upcoming election. During such windows over the last 15 years, nearly 89 percent of audits were ordered before *municipal* elections. A similar pattern holds when examining voters removed from rolls during audits — nearly 90 percent of such removals took place during audits before municipal elections.[29] In addition, official statements about voter audits focus almost entirely on municipal elections. In sum, audits are most likely during municipal elections.

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[29]This pattern also holds when examining annual data. Due to data availability, figures reflect 1997-2012 for audits (3 municipal and 3 federal elections) and 1997-2008 for voters removed (2 municipal and 2 federal elections). Regardless of timing, audits are conducted of municipalities. Thus, even though the electoral districts in federal elections are entire states, municipalities are the units at which audits occur.

C No Precise Sorting Around the Threshold

As discussed in Section 4.2, the RDD method assumes smoothness (continuity) of potential out-comes at the threshold. Given that sorting precisely around the threshold would be inconsistent with the smoothness assumption, we build on the work of Lee (2008), investigate TSE procedures, and employ McCrary (2008) density tests to show that such sorting is not observed.

Lee (2008)

Lee (2008) demonstrates formally that the smoothness assumption does not require the forcing variable to be "as good as randomized." Rather, randomized variation stems from agents being unable to control precisely the forcing variable near the threshold (see also Lee and Lemieux 2009). In order words, sorting does not bias RDD estimates unless politicians have *precise* control over the realized value of the electorate-population ratio. In order to have such control, a mayor would need to change her municipality's electorate precisely, and know exactly the population of her municipality (or vice versa). We now discuss why this is impossible for both the electorate and population figures in the Brazilian context. It should be emphasized, however, that so long as *either* the numerator or denominator cannot be precisely known, then Lee's (2008) analysis demonstrates that the smoothness assumption holds.

Electorate (Numerator of Ratio)

During the time that electoral rolls are open to voter transfers, municipal officials cannot precisely manipulate or even know exactly the number of voters that will be used in the numerator of the electorate-population ratio. For the 2007-2008 wave of voter audits analyzed in this paper, the TSE calculated the ratio using the electorate in June 2006, but the last date that transfers could be processed was May 6, 2006. Before and after this May 6 "closing date" by which voters could be imported, the electorate was in substantial flux due to factors that could not be precisely controlled by municipal politicians. The following information is from official TSE documents:

- During April 12-16, 2006, the TSE removed all voters who had missed the past three consecutive elections without justifying their absence or paying fines. Municipal officials would not know how many voters had justified their absence or paid fines in other municipalities or states just before the deadline.

- Until May 6, 2006, voters could transfer into a politician's municipality and first-time voters could register to vote. In addition, unbeknownst to the politician, her municipality's voters living elsewhere in Brazil could transfer out (subject to residency requirements). The former would increase the municipality's electorate, and the latter would decrease the electorate. A surge of registrations and transfers happens in the days leading up to the closing of the rolls, making it difficult for a politician to project how the electorate will fluctuate (especially with regards to outward transfers conducted outside her own municipality). For example, in the state of Mato Grosso do Sul, 20,676 voter transfers and 21,539 voter registrations were processed in just ten days before the rolls closed in 2006, jointly representing nearly 3 percent of the state's electorate.[30]

- During April 15-16, 2006, the TSE removed any voters whose death — in any location in Brazil, not just in the voter's municipality — had been reported by the electronic federal death registry (*Sistema Informatizado de Controle de Óbitos*) in the prior month.

- During May 27-28 and June 24-26 of 2006, the TSE again removed any voters whose death had been reported by the electronic federal death registry (*Sistema Informatizado de Controle de Óbitos*) in the prior month. These removals happened *after* any voter transfers could be executed on May 6.

- On June 22, 2006, the TSE concluded an additional data checking process, which removed or added voters from the electorate whenever errors had been made.

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[30]Data from the Tribunal Regional Eleitoral of Mato Grosso do Sul. Reported in "Mais de 53 mil Pessoas Procuraram Cartórios Eleitorais de MS," *RMT Online*, May 6, 2006.

To sum up, the last day the TSE's national, electronic voter database would permit any transfers was May 6, 2006. Before this closing date of May 6, 2006, municipal politicians could precisely control the electorate, given voter registrations, transfers, cancellations and deaths that happen (some outside their locality). And after May 6, when no more voter transfers could be conducted, the electorate continued to change due to TSE actions.

Population (Denominator of Ratio)

During the time that electoral rolls are open to voter transfers, municipal officials cannot precisely know the population used in the denominator of the electorate-population ratio. For the 2007-2008 wave of voter audits analyzed in this paper, the electoral rolls closed to transfers on May 6, 2006. But the census bureau (IBGE) did not release the July 2006 population estimate used in TSE calculations until August 31, 2006, per regulations in Article 102 of Law 8443. Thus, the population estimate was released nearly four months after the last date on which a politician could import voters. To calculate these population estimates, the census bureau employs a complex mathematical model involving the fertility, mortality and migration rates not just of the municipality itself, but also of other areas of the state.

Overall, municipal officials are unable to manipulate precisely or know exactly the electorate or population figures used. Thus, Lee's (2008) conditions for RDD as-if randomization described above are sufficiently met.

Various TSE court cases also suggest that politicians have erroneously estimated the electorate/population ratio. For example, the PMDB party in Iacanga, Sao Paulo, filed a court case claiming that a voter audit should have been conducted because the ratio reached 80.91%, but the TSE ruled that in fact the 80% threshold had not been exceeded.[31]

McCrary (2008) Density Test

Empirical analyses employing a McCrary (2008) Density Test also finds no evidence of manipulation. This test examines whether or not "lumping" exists on one side of the discontinuity or

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[31]Tribunal Superior Eleitoral, Resolution 21.583, December 9, 2003.

Figure A.1: McCrary Density Test

Full Sample   **Municipalities with Mayors Eligible for Re–election**
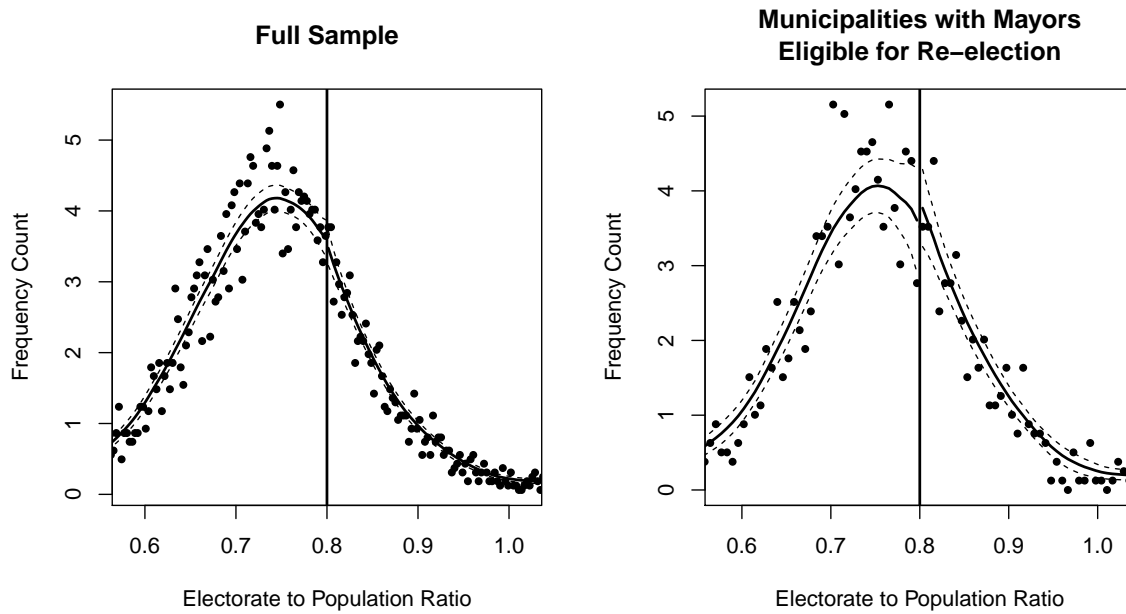
another, which might indicate that some units manipulate the forcing variable so as to avoid or receive the treatment. If a substantially larger number of municipalities were clustered below the 80% threshold than above the threshold, for example, this would be evidence for mayors successfully manipulating the electorate-population ratio.

We find no evidence of this clustering. The p-value on the null hypothesis of "smoothness" of the forcing variable—known as the McCrary Density Test—is .82 and .67 for the full sample and the sample of municipalities where mayors are eligible for reelection, respectively. These results are consistent with the absence of manipulation of the forcing variable. A visual depiction of this test can be found in Figure A.1.

Table A.1: First Stage Results

	Dependent variable:	
	Revision	
	(1)	(2)
Above .8	0.72***	0.72***
	(0.03)	(0.03)
Baseline	0.03***	0.01
	(0.01)	(0.01)
Specification	Difference-in-Means	Local Linear
Observations	577	1,477
Note:	*p<0.1; **p<0.05; ***p<0.01	

D First Stage Results

The first stage results for the regression discontinuity design are shown above in Table A.1. The F-statistic on the excluded instrument is 725 and 651 for the local linear and difference-in-means specifications, respectively.

E Robustness to Alternative Bandwidths

In Figures A.2 and A.3, we report the robustness of our estimates to alternative bandwidth choices. At the outset, it deserves emphasis that: (1) estimates using overly small bandwidths are likely to have relatively larger standard errors due to small sample size; and (2) estimates using overly large bandwidths are prone to bias as covariates become imbalanced above and below the threshold.

In Figure A.2, we show local linear estimates with bandwidths ranging from 0.5% to 6% in steps of 0.5%. Bandwidths over 6% would include a very large proportion of all municipalities in the estimation dataset and covariate balance noticeably deteriorates. For example, at a bandwidth of 6.5%, 42% of all municipalities are included in the sample. Using our benchmark bandwidth of 4%, about 26% of all municipalities are included in the estimation sample.

For the difference-in-means estimator (Figure A.3), we test bandwidths between 0.25% and 2.75% in steps of .25%. Note that the difference-in-means estimator can be biased if the conditional expectation of the outcome variable with respect to the forcing variable has a slope, an issue mitigated with smaller bandwidths.

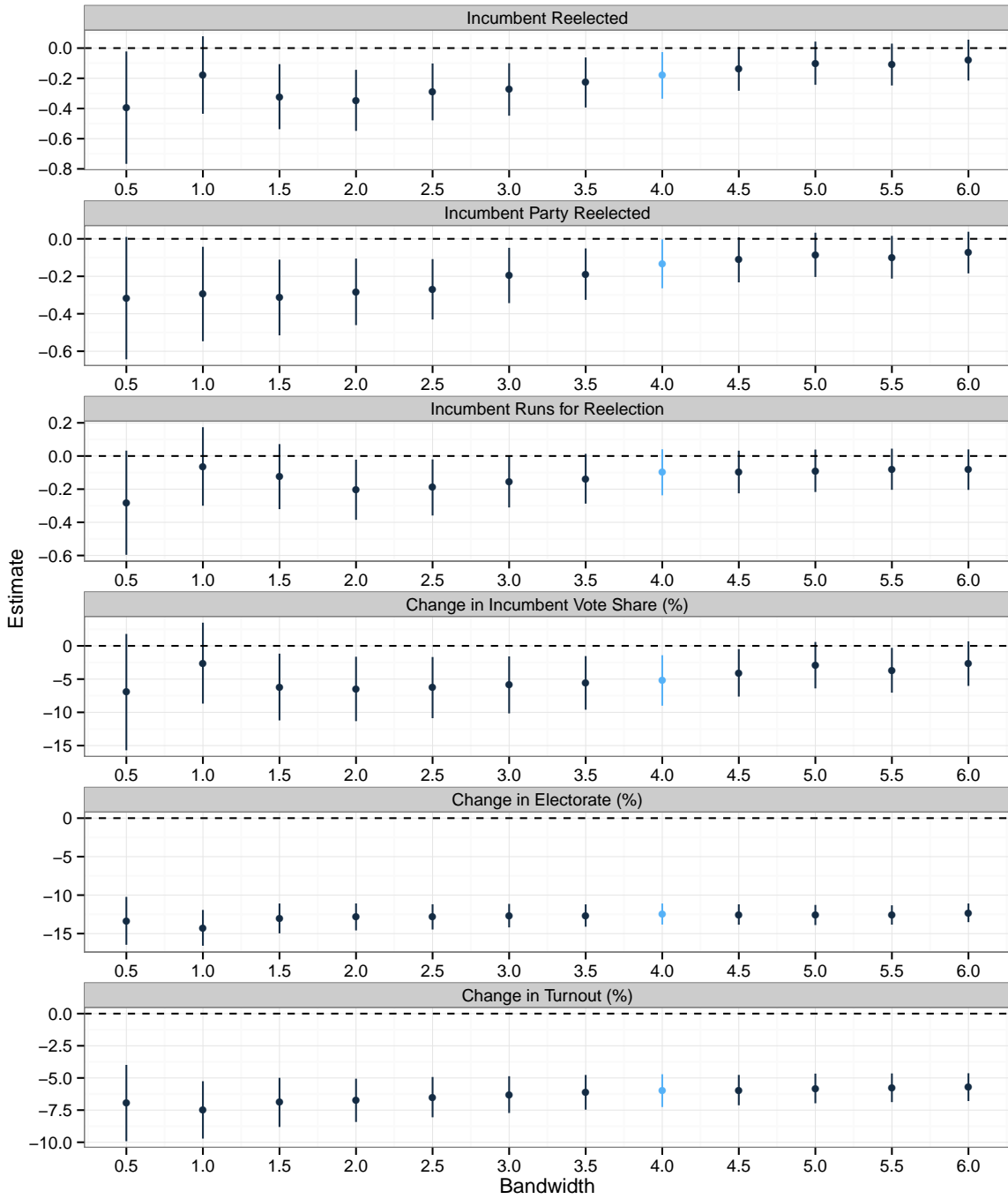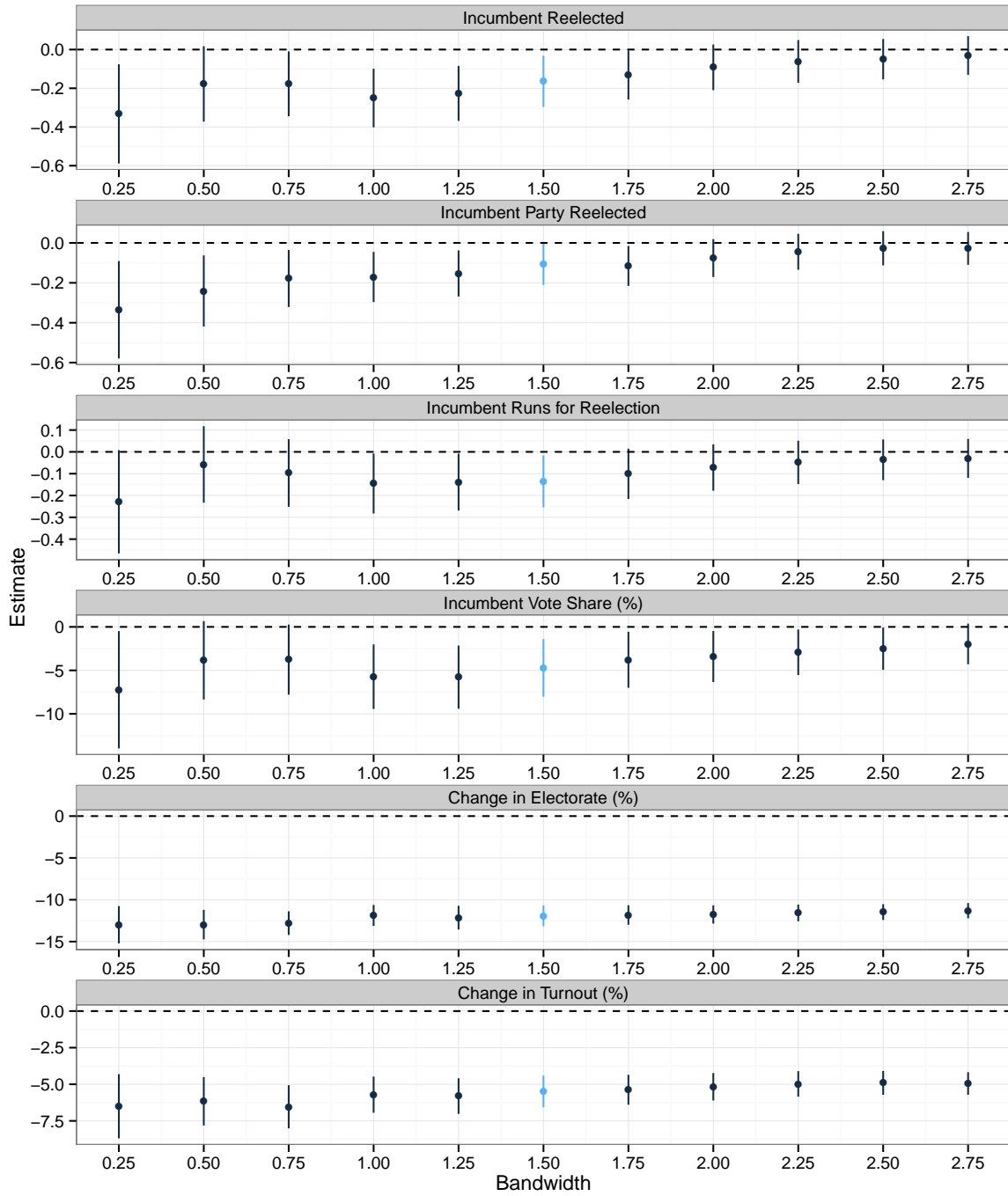Figure A.2: Robustness to Alternative Bandwidths (Local Linear Specification)

Figure A.3: Robustness to Alternative Bandwidths (Difference-in-Means)

F Descriptive Statistics

Table A.2: Descriptive Statistics

Variable	n	Mean	Median	SD
Electorate as % of Pop	5476	75.98	76.14	10.70
Turnout (08) as % of Pop	5547	68.01	67.28	10.86
Incumbent Party Won (08)	5547	0.34	0.00	0.48
Ran for Re-election (08)	4273	0.73	1.00	0.45
Incumbent Vote (08) as % of Pop	3112	33.39	32.76	10.84
Blank and Invalid (08) as % of Pop	5547	5.69	4.21	6.00
Incumbent Party Won (04)	5547	0.26	0.00	0.44
Turnout (04) as % of Pop	5547	63.83	63.74	10.86
Electorate Above 70 (06) as % of Pop	5504	4.83	4.81	1.55
Pct Poverty (00)	5494	46.47	44.76	22.85
Theil Index (00)	5494	0.52	0.51	0.11
Incumbent Re-elected (04)	5501	0.23	0.00	0.42
Rural Population (07)	5385	5325.39	3381.00	6002.17
Distance to State Capital	5494	253.23	227.49	163.64
Num of Previous Audits	5547	0.25	0.00	0.44
Transfers In (05-06)	5512	641.86	180.00	3256.79
Transfers Out (05-06)	5530	654.10	300.00	3006.31
Neighbor Munis Transfers Out (05-06)	5493	3129.60	1646.00	9823.51
Neighbor Munis Transfers In (05-06)	5493	3181.97	1178.00	10059.72
Blank and Invalid (04) as % of Pop	5547	4.41	3.75	4.01
Northeast Region	5547	0.32	0.00	0.47
Southeast Region	5547	0.30	0.00	0.46
Num of Neighboring Municipalities	5493	5.96	6.00	2.02
Population (08)	5547	33686.07	11131.00	197884.09
Electorate (06) as % of Pop	5504	74.03	74.52	9.79
Win Margin (04) as % of Pop	5426	8.22	5.98	7.54

Figure A.4: Histogram of Forcing Variable
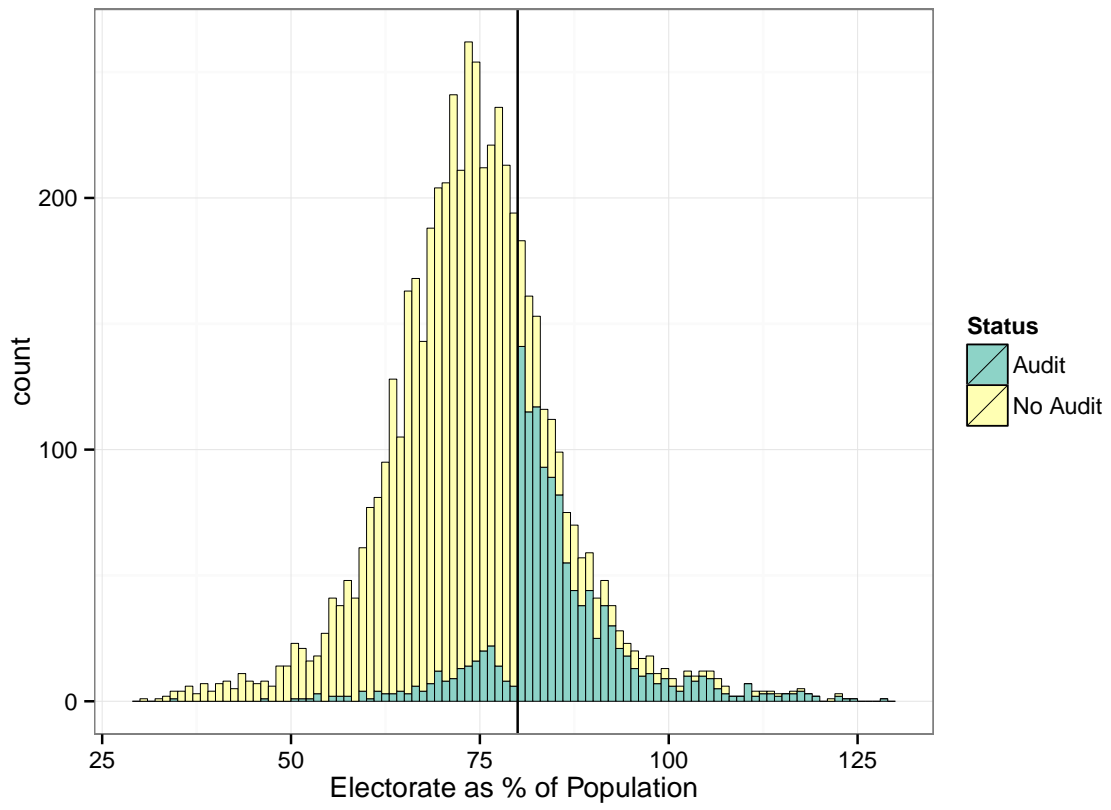
G   Alternative Specifications

In Tables A.3 and A.4, we demonstrate robustness of results using alternative specifications to those used in the main text. Specifically, in Table A.3, we show the local linear estimates once we control for state fixed effects and covariates that exhibited an imbalance of more than .1 standardized differences. In Table A.4, we show our estimates when using a fifth order polynomial to model the relationship between the outcome and the forcing variable with a bandwidth of 30%. Tables show reduced form estimates ($\hat{\tau}_A$). Instrumental variable estimates (not shown) are larger and similarly statistically significant.

Table A.3: Regression Discontinuity Results Controlling for Covariates and State Fixed Effects

	Dependent variable:					
	Incumb. Reelected	Incumb. Party Reelected	Incumb. Re-runs	Incumb. Vote Share	Electorate Change	Turnout Change
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\tau}_A$	−0.21***	−0.18***	−0.09	−5.23***	−9.45***	−4.34***
	(0.06)	(0.05)	(0.06)	(1.49)	(0.67)	(0.53)
Baseline	−0.0001	−0.0001	0.0005	−0.02	0.04**	0.02*
	(0.002)	(0.001)	(0.001)	(0.03)	(0.02)	(0.01)
Observations	1,069	1,408	1,069	775	1,386	1,408
Note:						*p<0.1; **p<0.05; ***p<0.01

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Table A.4: Regression Discontinuity Results with a 5th Order Polynomial Specification

			Dependent variable:			
	Incumb. Reelected	Incumb. Party Reelected	Incumb. Re-runs	Incumb. Vote Share	Electorate Change	Turnout Change
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\tau}_A$	−0.18***	−0.15***	−0.12*	−3.67**	−9.84***	−4.63***
	(0.07)	(0.06)	(0.06)	(1.62)	(0.76)	(0.61)
Baseline	0.54***	0.39***	0.77***	3.89***	4.31***	5.47***
	(0.05)	(0.04)	(0.04)	(1.13)	(0.31)	(0.38)
Observations	4,085	5,358	4,085	2,999	5,289	5,358
Note:					*p<0.1; **p<0.05; ***p<0.01	

H Placebo Thresholds

As an additional test of the validity of the research design, we calculate treatment effects at placebo thresholds which—if the design is valid—should all be close to 0. These placebo estimates (using the local linear estimator) are shown in Figure A.5. Results do not change if we use the difference-in-means estimator.

Figure A.5: Placebo Thresholds
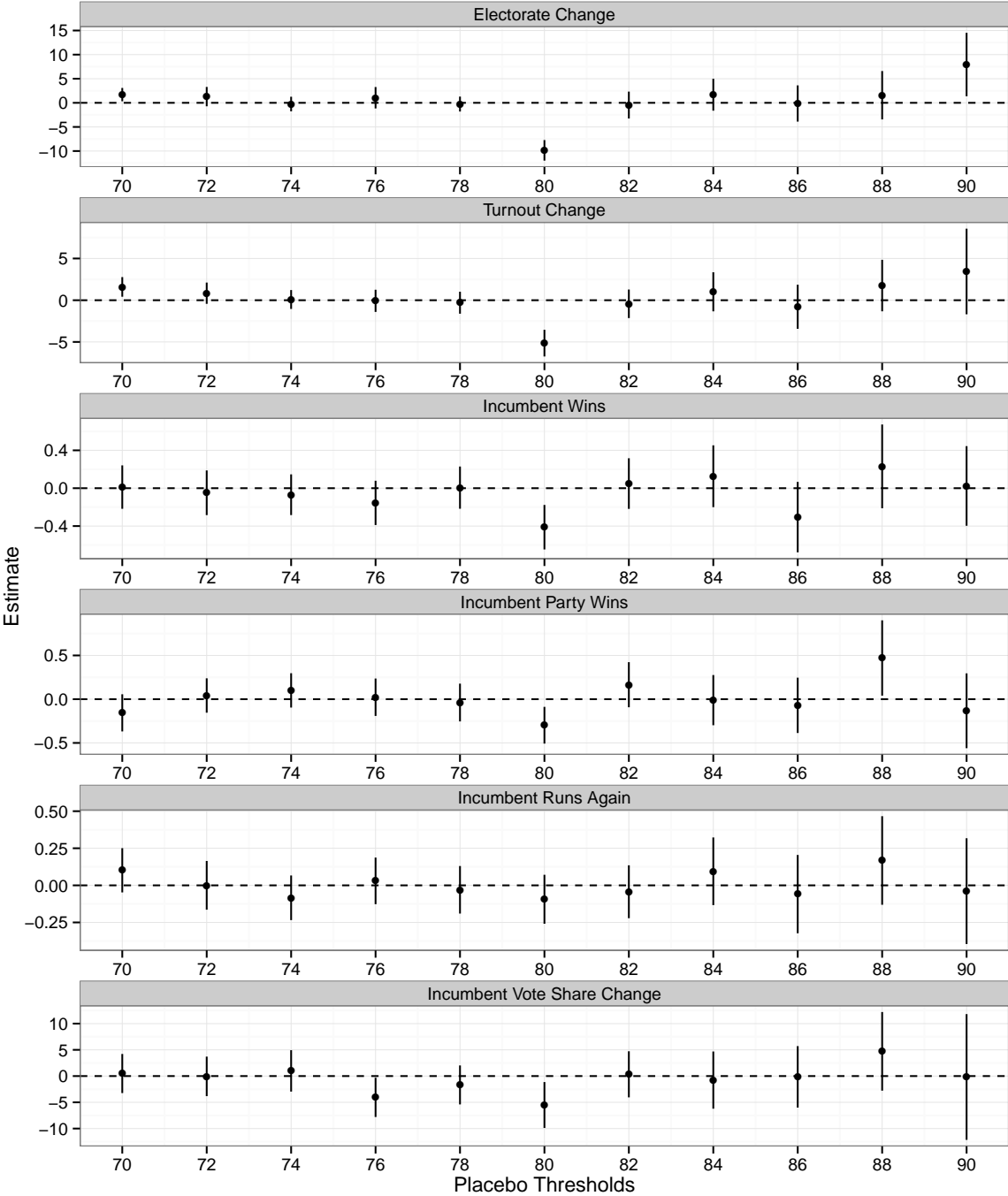
I Additional Alternative Explanations Results

In figure A.6, we provide the results of additional tests of emigration as an alternative hypothesis. First we employ data on number of transfers out of a municipality over a shorter and longer time frame (2002-2007 and 1997-2007). Another potential test of the emigration hypothesis is whether audits hindered incumbent performance more in municipalities where voters have a pattern of missing consecutive elections. As shown in Figure A.6c, treatment effects are not heterogeneous across municipalities with above versus below-median "missing voters" (discussed in text). Overall, the evidence does not support the emigration explanation.

Figure A.6: Additional Tests of Alternative Explanations (Heterogeneity of RDD Estimates)
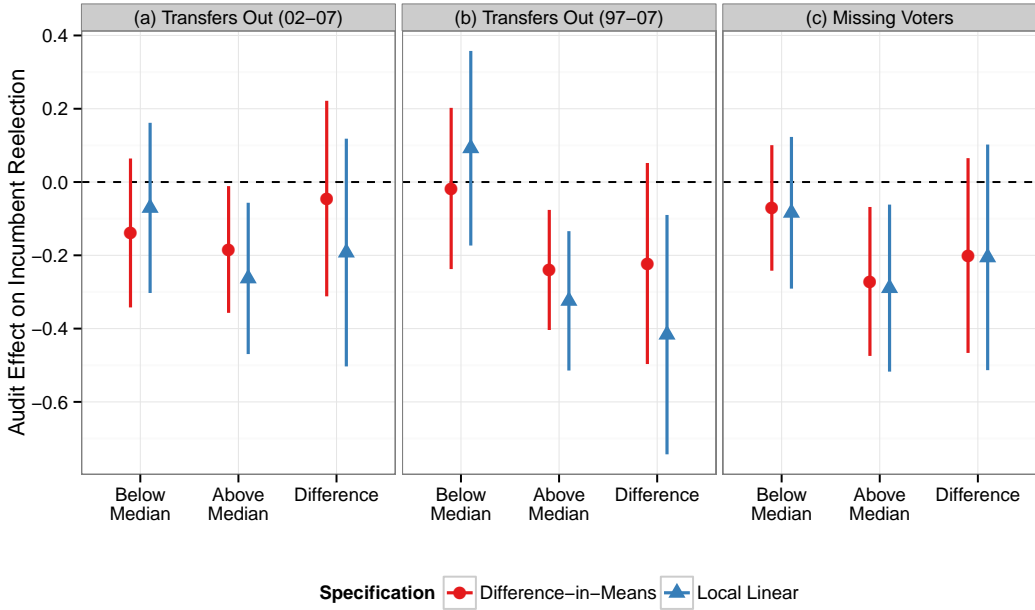
Note: Figure A.6 suggests an audit's effect does not significantly vary by whether municipality has above-median values for alternative migration measures. Solid lines are 95% confidence intervals.

J   Blank & Invalid Votes versus Indifferent Voters

Figure A.7 demonstrates that voters casting blank or invalid votes are substantially less interested in politics than voters casting votes for candidates. Data is drawn from 2010 Brazilian Electoral Panel Study (Ames et al. 2013).

Figure A.7: Blank & Invalid Votes versus Indifferent Voters

How Much Respondent Cares About Politics

		Much	Some	Little	None	Don't Know	No Response	
Respondent's 2010 Presidential Vote in Second Round	Dilma Rouseff	9.32	17.06	44.23	26.86	0.32	2.21	100.00
	Jose Serra	9.14	17.74	42.20	29.57	0.54	0.81	100.00
	Blank/Invalid	4.08	10.20	32.65	(53.06)	0.00	0.00	100.00
	Didn't Vote	6.32	11.58	40.00	41.05	0.00	1.05	100.00
	Don't Know	0.00	20.00	60.00	20.00	0.00	0.00	100.00
	No Response	8.96	4.48	46.27	37.31	0.00	2.99	100.00
		8.76	15.89	43.00	30.38	0.33	1.64	100.00

How Much Respondent Cares About Politics

		Much	Some	Little	None	Don't Know	No Response	
Respondent's Governor Vote in Previous Election	Current Governor	8.58	17.17	43.61	29.61	0.52	0.52	100.00
	Another Candidate	11.26	16.21	44.47	27.08	0.40	0.59	100.00
	Didn't Vote	8.65	9.46	40.27	40.54	0.27	0.81	100.00
	Blank/Invalid	3.33	15.00	30.83	(50.83)	0.00	0.00	100.00
	Don't Know	2.81	7.23	37.35	51.41	0.40	0.80	100.00
	No Response	6.94	16.67	36.11	31.94	2.78	5.56	100.00
		8.26	14.71	41.82	34.00	0.48	0.73	100.00

Note: Data from 2010 Brazilian Electoral Panel Study. Entries reflect percentage of row; i.e., the top circled entry indicates that 53 percent of respondents who reported casting Blank/Invalid votes in the 2010 presidential election reported that they had no interest in politics (roughly double that of Dilma Rouseff voters).

K   Change in Electorate Over Time

In Figure A.8, we show the evolution of the electorate around the discontinuity in the 5 years before the audit wave (top panel) and the 5 years after the audit wave (bottom panel). As is evident from the figure, immediately after the audits, municipalities above the discontinuity lose a substantial part of their electorate. In the years following the audit, the difference between the two types of municipalities diminish.

Figure A.8: Evolution of the Electorate Over Time
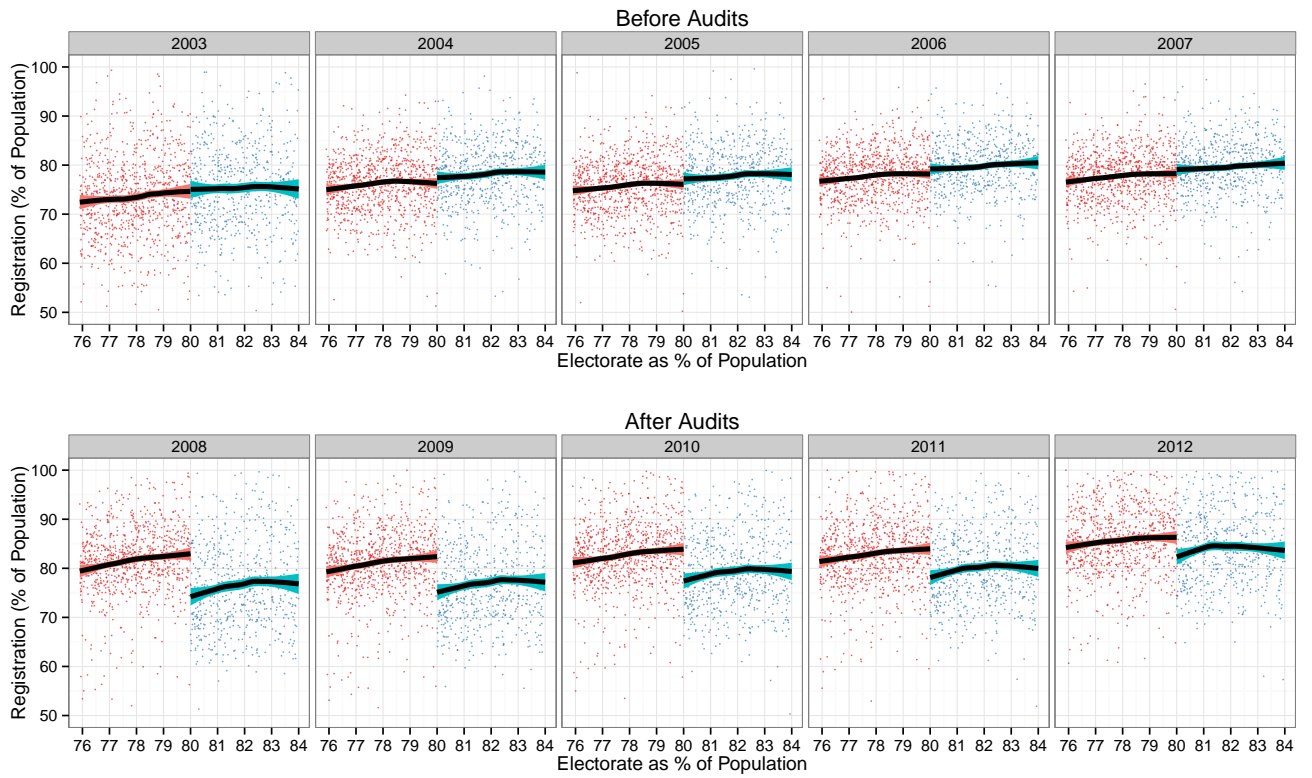
L   Incumbent, Challenger, and Election Characteristics

To assess whether candidates and electoral competition in municipalities near the 80% threshold are unusual on measurable characteristics, in Figure A.9 we present scatter plots showing the distribution of four variables across a wide range of the forcing variable. We also plot a lowess curve, which estimates the moving average of the variable (span parameter is set to .3). The four variables are: vote margin in 2004 mayoral election, difference in total donations received by the winner and runner up in 2008 mayoral election, incumbent mayor's age in 2008, and the age of the challenger (to the incumbent mayor) in 2008.

Figure A.9: Incumbent, Challenger, and Election Characteristics
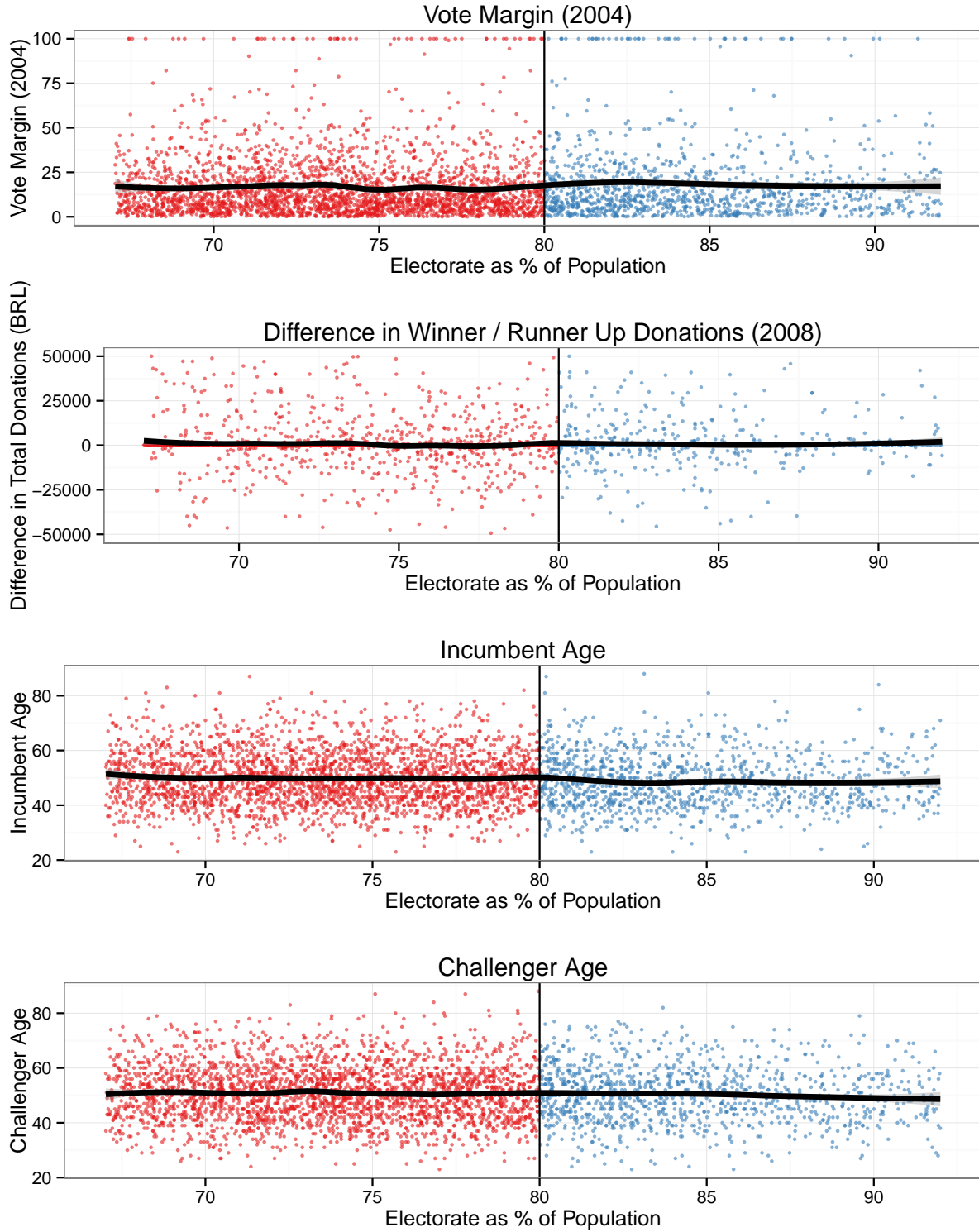
M   Discontinuity Plots for Selected Pre-Treatment Covariates

In Figure A.10, we show discontinuity plots for 8 pre-treatment covariates. These covariates are: vote margin in 2004, log municipal budget per capita in 2006, public employees per capita in 2006, incumbent age in 2008, challenger age in 2008, presence of an AM radio station, ratio of electorate to number of city councilors, and average precinct size in 2008. Across all 8 covariates, we find no evidence for sorting around the threshold.

Figure A.10: Discontinuity Plots for Selected Pre-Treatment Covariates
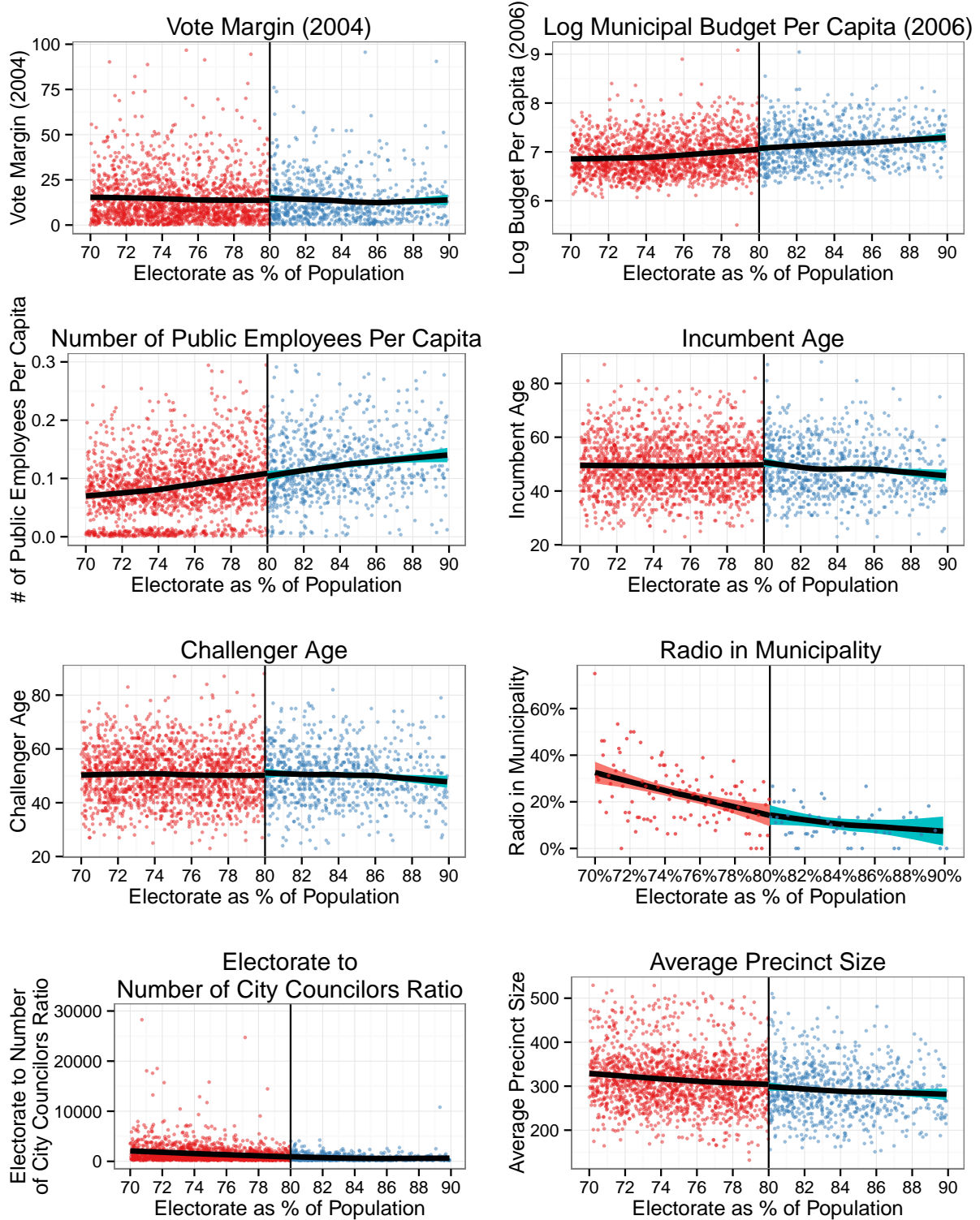
N   Additional Discontinuity Plots

Figure A.11: Effect of Audit on Incumbent Reelection, Full Sample
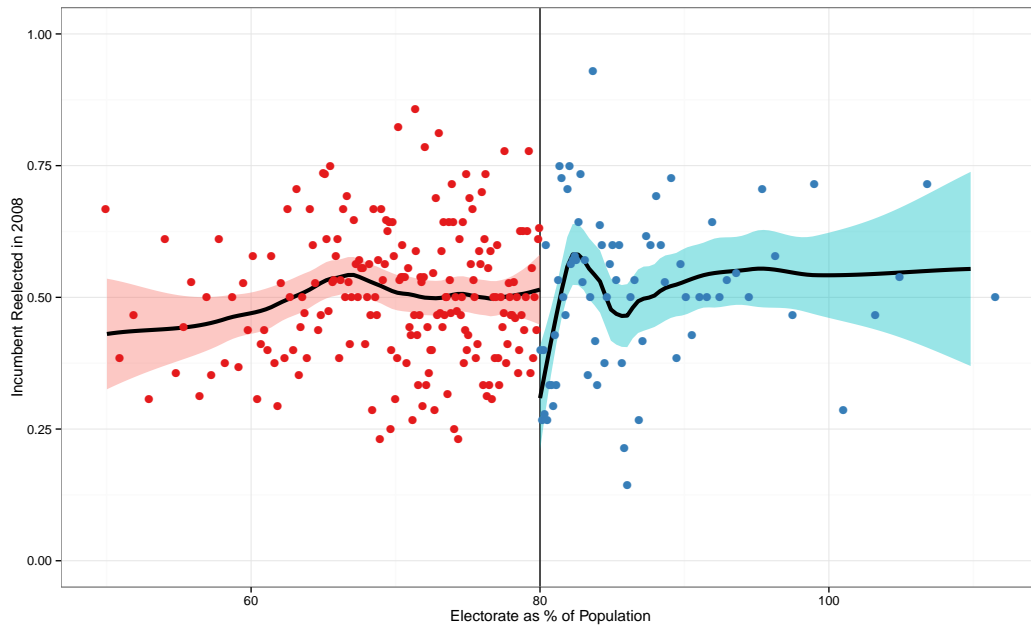

Figure A.12: Effect of Audit on Incumbent Decision to Run for Reelection, Full Sample
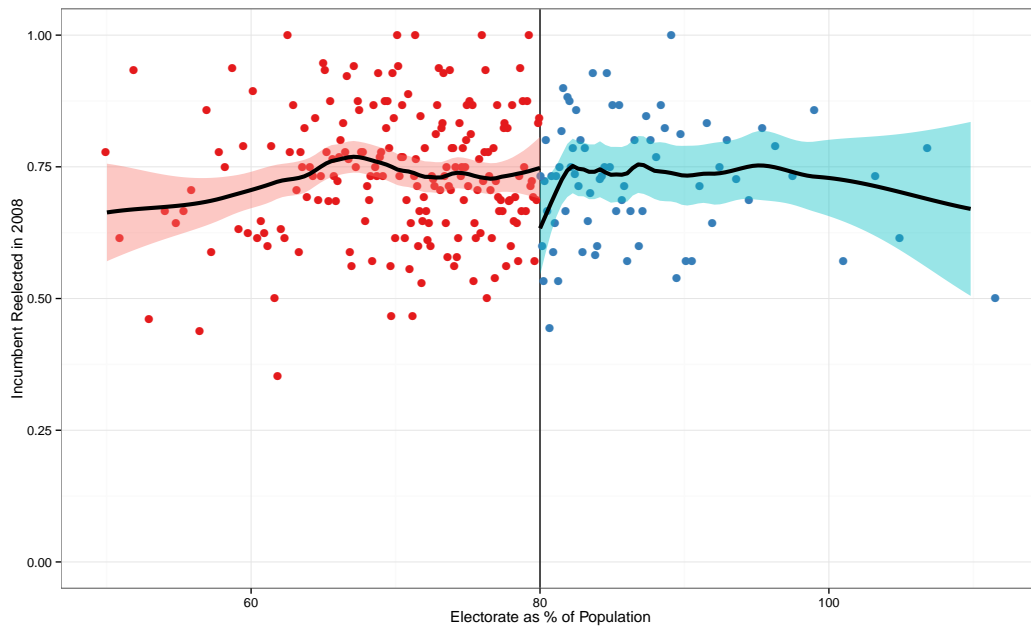
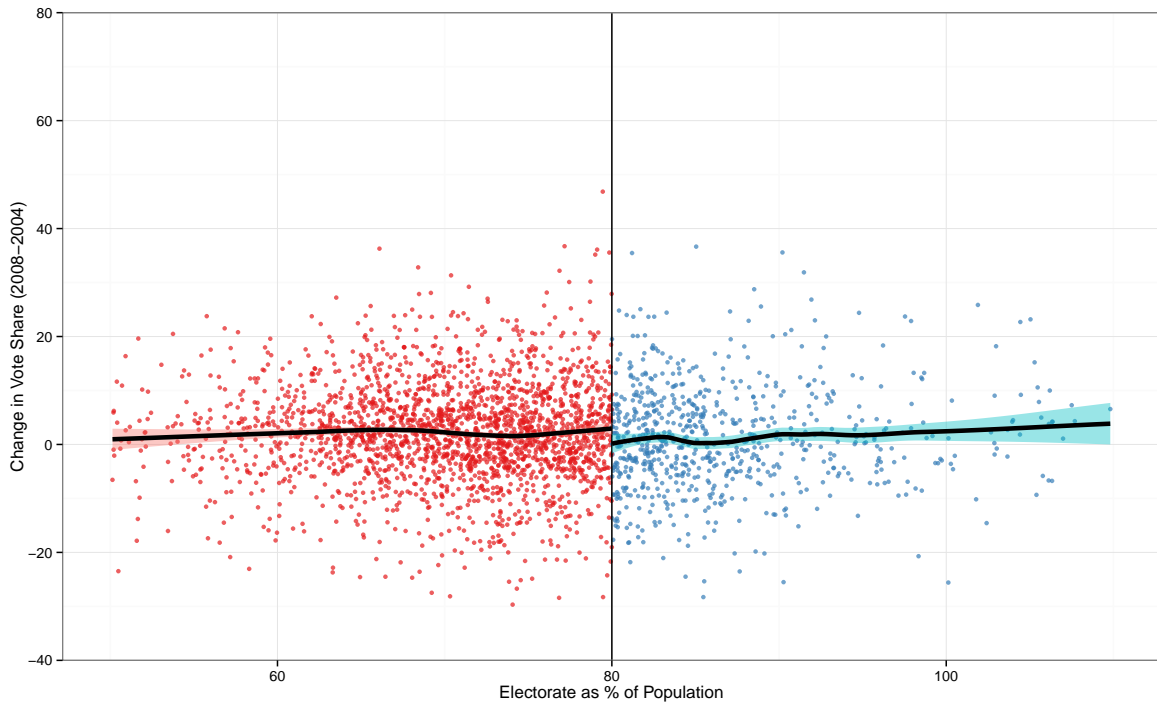Figure A.13: Effect of Audit on Change in Incumbent Vote Share, Full Sample

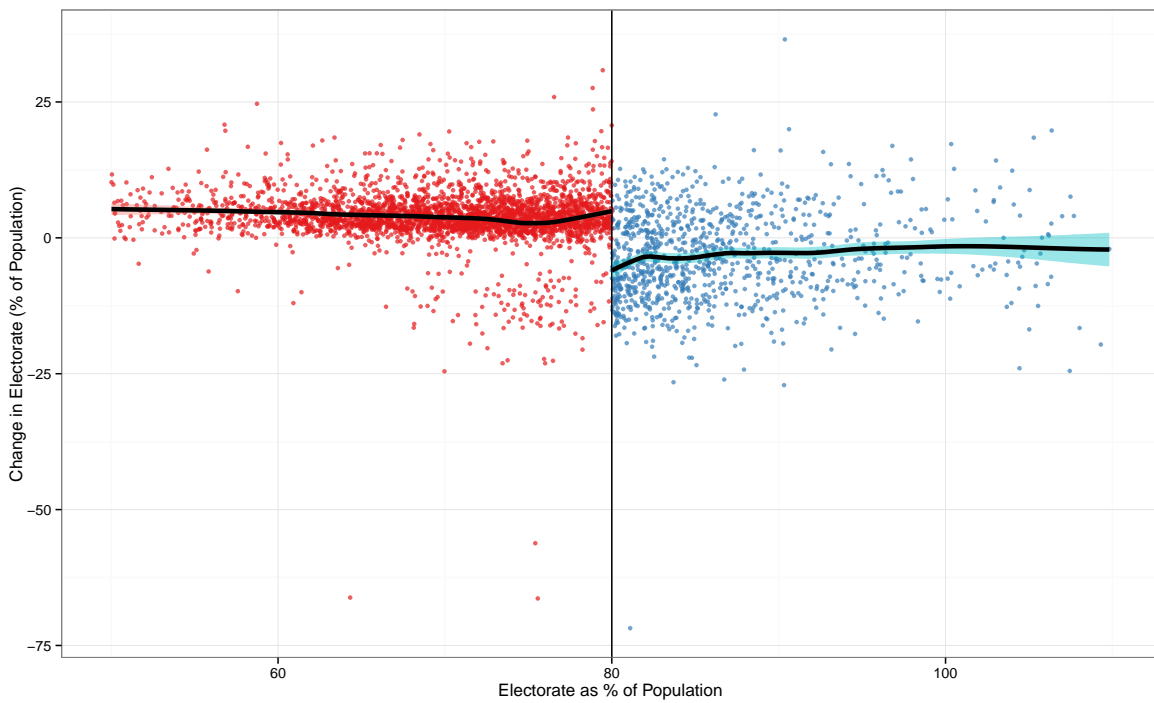Figure A.14: Effect of Audit on Change in Electorate, Full Sample

Figure A.15: Effect of Audit on Change in Turnout, Full Sample