

# Imperfect Evaluations and Incumbency Effects\*

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## Abstract

Why do voters systematically reelect subnational officeholders in some new democracies and systematically oust them in others? Standard explanations stress clientelism and corruption, respectively, as sources of incumbency advantage and disadvantage. I propose an alternative account in which incumbency effects *emerge* and *vary* because voters use public goods spending to guide their electoral choices but partially attribute exogenous fiscal constraints – largely out of incumbent control – to incumbent competence. Using aggregate data from Brazilian municipalities, I find that mayors obtain an incumbency advantage when public goods spending increases relative to the past, but are otherwise at an incumbency disadvantage. An original survey experiment further shows that voters only partially discount exogenous fiscal windfalls when evaluating incumbent performance, and that they pay less attention to incumbent spending when informative party labels are available. This paper suggests that incumbency effects can coexist with partial electoral accountability in low information environments.

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# 1 Introduction

In many new democracies across the world, subnational incumbents enjoy an electoral advantage when seeking reelection. For example, Argentine sitting governors are 40% more likely to win an election than a challenger. Yet, in other new democracies, incumbents suffer from an electoral disadvantage. In India, the world's largest developing democracy, opposition candidates are 15% more likely to win an election than sitting mayors. Why do we observe variation in incumbency effects across and within countries?

Existing theories of incumbency effects cannot explain why incumbency is an advantage in some developing settings and a disadvantage in others. Explanations of incumbency advantage focus on clientelism and patronage. By disbursing targeted resources and selectively providing public employment, the argument goes, incumbents amass vote share sufficient enough to outpoll challengers. (Stokes 2005; Greene 2007; Bhavnani and Lupu 2016). Explanations of incumbency disadvantage stress corruption. This argument maintains that corruption is sufficiently widespread in some developing countries to customarily populate office with politicians that voters have no choice but to oust whoever sits in office (Aidt, Golden and Tiwari 2011; Klačnja 2015, 2016). These explanations focus on either incumbency advantage or disadvantage and cannot explain variation in the direction of incumbency effects within and across countries. These uniform predictions are rooted in the assumption that elite malfeasance is so entrenched that voters have no ability to use the ballot to create incentives for good behavior.

This is at odds with growing evidence that voters in developing settings punish and reward incumbents for good governance outcomes, such as economic growth (Carlin, Singer and

Zechmeister 2015; Bleck and van de Walle 2013), social services (Thachil 2011; Pereira, Melo and Figueiredo 2009), and public safety (Carlin, Love and Martínez-Gallardo 2015).

In this article, I shift the focus from elite to voter behavior. Building on growing evidence of robust electoral accountability in the global south, I argue that incumbency effects emerge and vary because voters make imperfect evaluations of incumbent performance. There is a growing recognition that voters in the developing world expect subnational incumbents, such as mayors or governors, to providing public goods, such as health, education and transportation (Kitschelt 2000; Baldwin 2012; Johannessen 2017). But subnational incumbents have limited policy and fiscal levers to garner the resources necessary to provide these goods. Incumbents are instead dependent on fiscal transfers from upper levels of government, transfers regulated by complex tax sharing agreement and sensitive to exogenous economic shocks, such as macroeconomic policies set at the national level and international economic cycles (Anderson 2006; Rodden and Wibbels 2011; Niedzwiecki 2016). Because voters seldom have detailed knowledge about policy making, they attribute exogenous factors to incumbent competence. As a result, reward incumbents for fiscally exogenous increases in spending and punish incumbents for analogous decreases.

This argument carries testable predictions about variations in incumbency effects across different political setting. I empirically test this prediction exploiting subnational variation across Brazilian municipalities. Both research and real political events suggest that Brazil is a case where rampant corruption and clientelism should be the main sources of incumbency effects. As a result, they pose a challenge to an argument that emphasizes public goods spending (Pereira, Melo and Figueiredo 2009; Pereira and Melo 2016; Bhavnani and Lupu 2016). From an empirical standpoint, Brazil has he added virtue of offers rich variation in the

electoral fortunes of incumbents across 5,564 municipalities and in the informative content of party labels (Samuels and Zucco 2014). I examine the predictions of my argument combining aggregate and individual level data from Brazilian municipalities.

My argument predicts that municipal changes in fiscal transfers should lead to changes in the size and direction of incumbency effects. The impact however, should only be present when transfers are accompanied by higher public goods spending. At the aggregate level, I test both predictions taking advantage of highly detailed fiscal panel dataset on Brazilian municipalities, coupled with municipal electoral returns. To obtain credible causal estimates, I employ a heterogeneous regression discontinuity design that allows me to examine how the fiscal transfers and spending affect incumbency effects. Consistent with my expectations, I find a strong positive correlation between changes in fiscal transfers and incumbency effects. Critically, though, the results show that mayors who fail to improve public goods provision suffer from an incumbency disadvantage, while those who oversee substantial increases in public goods spending build an incumbency advantage. The aggregate data also helps me rule out alternative explanations rooted in elite malfeasance. I find no evidence to suggest that the association between public spending and incumbency effects reflects unobserved patronage spending or corruption.

I rely on micro level data to examine voters perceptions and behavior more closely. My argument suggests that voters reward fiscal transfers that materialize in spending but not otherwise. I test this argument taking advantage of an original survey experiment that conducted in two Brazilian municipalities. Voters were randomly assigned voters to electoral scenarios that varied in terms of the presence of fiscal windfalls and spending in public goods by incumbents. Voters reward incumbents for exogenously funded increased spending, and but do

not react to spending-neutral fiscal shocks.

This paper contributes to the growing research agendas on incumbency effects in the developing world, and to ongoing debates about electoral accountability in comparative electoral behavior. I present a novel argument and supportive evidence that suggests that office-holding does not produce a uniform advantage or disadvantage, but that incumbency effects vary within countries, both across subnational settings and over time. Second, and related, the paper shows that these heterogeneous incumbency effects are not rooted in elite malfeasance but in voters rewarding representatives who serve the general interest of their communities by providing public goods.

The article also provides insights for debates about voter competence and democratic accountability. A growing body of research on electoral behavior has questioned the ability of voters to hold incumbents accountable for their actions. Some scholars hold the extreme view that voters are irrational; they respond to irrelevant outcomes, such as shark attacks or sporting events (Achen and Bartels 2016). Other scholars take a more nuanced view in which voters are rational but misinformed. While they note that voters cannot fully discount exogenous shocks, they argue that shocks influence elections through voter evaluations of relevant outcomes, such as economic growth and unemployment (Healy and Malhotra 2009; Healy, Malhotra and Mo 2010; Campello and Zucco Jr 2016).

The analysis of incumbency effects in this paper contributes to this nuanced view about the quality of democratic accountability. On the negative side of the ledger, voters make imperfect evaluations which weaken the power of elections to encourage politicians to maximize effort on behalf of citizens. But incumbency effects emerge because voters rationally respond to outcomes that affect their wellbeing and are partially capable of attributing competence by

benchmarking performance. The main normative lesson is that incumbency effects mitigate but do not fully crowd out electoral accountability.

## 2 Explaining Incumbency Effects

This article focuses on incumbency effects, or *the systematic difference in electoral success between incumbents and challengers that arises simply because incumbents hold office* (Erikson 1971; Gelman and King 1990).

Incumbency effects can operate in different directions. When incumbents systematically defeat challengers, incumbency effects manifest as incumbency advantage. When challengers systematically defeat incumbents, we are in a situation of incumbency disadvantage.

I argue that incumbency effects emerge and vary because of imperfect electoral accountability. It is widely documented that voters hold politicians accountable by evaluating performance in office (Duch and Stevenson 2008). This retrospective accountability acquires particular salience in developing democracies, where political parties are often weak and devoid of programmatic reputations (Mainwaring and Scully 1995; Lupu 2013, 2014). In the case of subnational incumbents, voters focus on public goods provision (Kitschelt 2000; Wantchekon 2003; Baldwin 2012).<sup>1</sup>

What is a good performance? Voters look at the past: they assess whether the sitting officeholder improved public goods provision relative to his or her predecessor (Downs 1957). But the past rarely looks like the present. Exogenous factors beyond the control of incum-

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<sup>1</sup>More generally, voters evaluate performance through outcomes that fall within the scope of the incumbent authority. For example, they judge presidents for growth and legislators for constituency service. Some scholars argue that voters often use irrelevant metrics such as shark attacks or flu outbreaks (Achen and Bartels 2016).

bents, such as economic crises and changes in commodity prices, affect the performance that voters observe. Subnational incumbents are particularly exposed because these factors condition the fiscal transfers they receive and, as a result, their ability to provide public goods. To correctly attribute competence from public goods spending, voters should fully discount exogenous factors from their evaluations. But voters have the information to make this kind of discounting. As a result, voters attribute exogenous changes in spending to incumbent competence. This misattribution leads to incumbency effects. When exogenous factors adversely affects incumbent performance, voters evaluations will be pushed downwards, increasing incumbency disadvantage. When exogenous factors positively affects incumbent performance, voters evaluations will be pushed upwards, increasing incumbency advantage.

The imperfect retrospective voting theory carries a series of observable implications about the nature of incumbency effects and their microfoundations. Regarding the former, the first prediction is that incumbency effects will be heterogeneous within countries:

**H1** (Heterogeneity) The electoral returns to incumbency increase with fiscal transfers, placing some incumbents at an advantage and others at a disadvantage.

The second hypothesis is that voters' misattribute competence by discounting external constrains through benchmarking:

**H2** (Misattribution) The spending in public goods that voters observe is reflects incumbent competence and external shocks in the form of fiscal transfers. I have argued that voters mistakenly attribute exogenous shocks to competence. If this is true, we should observe that both transfers and public goods spending will have an impact on incumbency effects, where the effect of transfers is mediated via public goods spending.

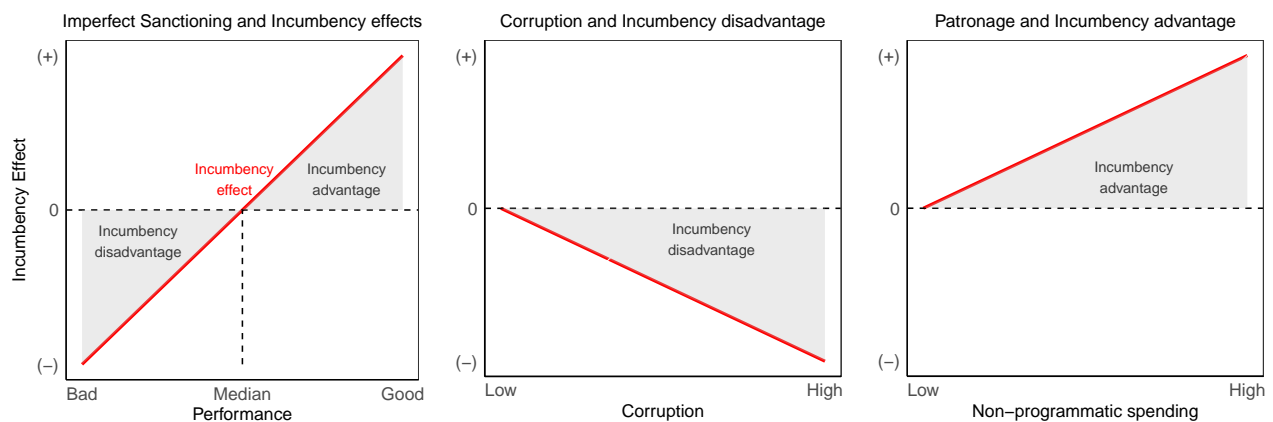
The third hypothesis follows from the claim that voters use party labels, whenever avail-

able, as an alternative source of information:

**H3 (Party Labels)** Informative party labels attenuate the link between public goods provision and incumbency effects.

### *Alternative explanations*

**Figure 1.** Direction of incumbency effects under different theories



These hypotheses help distinguish my account from explanations that stress bad governance. These theories maintain that incumbency advantage depends on spending in patronage (Stokes 2005; Greene 2007; Bhavnani and Lupu 2016), and incumbency disadvantage depends on corruption (Aidt, Golden and Tiwari 2011; Klašnja 2016; Klašnja and Titiunik 2017; Klašnja 2015). By contrast, I stress that incumbency effects – both advantage and disadvantage– may also be rooted in programmatic forms of spending, such as public goods provision. This implies that incumbency effects might coexist with incumbents possessing incentives for serving the general interest, to the detriment of personal enrichment or particularistic spending. A second distinction concerns the direction of incumbency effects.



While existing work argues that incumbency effects are either uniformly negative or positive, I anticipate that incumbency can be both an advantage and a disadvantage within the same political system. This contrasting implication is illustrated in Figure 1.

### **3 Empirical Setting: Brazilian Mayoral Elections**

I test the observable implications of my argument in the context of Brazilian mayoral elections. Brazil is the third-largest developing democracy, and offers a rich laboratory for examining the divergent electoral fortunes of incumbents across 5,564 municipalities over five electoral cycles.

Brazilian municipal governments consist of an executive branch, headed by a mayor, and a legislative branch, represented by a local council that exercises oversight. Mayors are elected through single-member plurality in municipalities with fewer than 200,000 registered voters, and using majority rules with a runoff system in the remaining municipalities. Mayors are eligible for one consecutive reelection.

Municipalities are responsible for providing fundamental public goods, such as health, primary education, and public transportation. As a result, voters care deeply about the performance of mayors when casting a ballot (Pereira, Melo and Figueiredo 2009; Pereira and Melo 2016; Johannessen 2017). But Brazilian voters need to grapple with the fact that mayors are conditioned by resources from upper levels of government (Melo, Pereira and Souza 2010; Arretche 2012). During the 2000-2016 period, federal and state transfers accounted, on average, for 90 percent of municipal budgets.<sup>2</sup> The lion's share of fiscal transfers are auto-

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<sup>2</sup>All calculations are based on data from the Brazil Finance Ministry.

matic, leaving limited discretion to upper level governments over their distribution (Arretche 2012).

Municipal spending is also constrained by regulations against corruption and fiscal profligacy (Arretche 2012). The most important constrain is the Fiscal Responsibility Law, which sets spending and debt ceilings, forbids hiring new employees in electoral years, and establishes strict transparency rules for reporting of government accounts (Afonso and Araújo 2006, p. 406-9).<sup>3</sup> Fiscal regulations are strongly enforced by the highly independent and professionalized State Audit Courts (*Tribunais de Contas dos Estados*), which can credibly sanction non-compliance (Melo, Pereira and Figueiredo 2009; Melo, Pereira and Souza 2010).

Mayors preserve significant discretion on how to allocate spending across policy areas (Johannessen 2017). This allows voters to extract a meaningful signal of incumbent competence from how much public goods are provided. The weakness of Brazilian local parties provides voters with further incentives to vote on performance. Most Brazilian parties lack programmatic reputations and cultivate low levels of party identification (Samuels 1999, 2003).<sup>4</sup> Parties are even less important in municipal elections, which revolve around local issues and personalities rather than on programmatic or partisan differences (Codato, Cervi and Perissinotto 2013; Novaes Forthcoming).

The Workers' Party (PT) is an exception to the rule of party weakness that characterizes Brazil. The PT, which held the Presidency for three consecutive terms during the 2002-2016

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<sup>3</sup>The constitution dictates that at least 25 percent of municipal revenues on education and 15 percent on health.

<sup>4</sup>Though national parties appear stronger in Congress (Figueiredo and Limongi 2000), in electoral coordination (Limongi and Cortez 2010), and on their impact on voters' positions (Samuels and Zucco 2014), municipal parties remain weak.

period, maintains an institutionalized national organization (Samuels 2004; Hunter 2010) and has the highest proportion of identifiers in the electorate.<sup>5</sup> There is also evidence to suggest that the PT label shapes the policy preferences of voters (Samuels and Zucco 2014). The PT also displays a distinctive programmatic character at the municipal level (Johannessen 2016). All these elements suggest that voters might pay less attention to incumbent performance when evaluating mayors from the PT than mayors from other parties.

## 4 Aggregate Evidence

In this section I use aggregate fiscal and electoral data to examine how performance and party labels shape incumbency effects across Brazilian municipalities. The next section uses a survey experiment to study this link at the micro level.

### *Estimating Incumbency Effects*

As discussed before, the central challenge in estimating incumbency effects is that incumbents and non-incumbents may differ along attributes that also influence electoral performance, such as candidate quality and underlying partisan support. Simply comparing the electoral performance of these types of candidates would therefore conflate the causal effect of incumbency with these attributes. Following current practice in the literature, I tackle this problem by adapting a regression discontinuity design (RDD) to Brazilian mayoral elections (Lee 2008).

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<sup>5</sup>Samuels and Zucco (2014) report that 45% of the Brazilian electorate identifies with a political party. Only three parties have at least 5% identifiers.

RDD relies on the assumption that assignment to incumbency is “as good as random” in close elections. In plurality elections, the winner has a positive vote margin and becomes the incumbent, and the runner-up has a negative vote margin and becomes a challenger. This means that incumbency status changes discontinuously at a vote margin score of 0 percent. Since it is plausible to assume that electoral outcomes in the vicinity of this cutoff are not determined by systematic differences between winners and losers, but by stochastic factors, incumbency is assumed “as-if” randomly assigned in close elections. Based on this identification strategy, the RDD estimator of incumbency effects focuses on districts that have very close elections at time  $t$  and compares the electoral performance of the “bare” losers and “bare” winners of those races in the subsequent electoral period, time  $t + 1$ .

I adapt RDD to the following features of the Brazilian political setting:<sup>6</sup>

**Attrition** Because many candidates who run at  $t$  do not run for another term, their electoral performance cannot be observed at  $t + 1$ . And focusing on repeated observations would bias findings toward an incumbency disadvantage, as only high-quality losers are likely to run again (De Magalhães 2015). To deal with the fact that some candidates do not run again, I follow Klašnja and Titiunik (2017) and use the unconditional probability of candidate victory at  $t + 1$  as the outcome of interest, that is, irrespective of whether candidates contest elections in  $t + 1$ .<sup>7</sup>

**Term limits** To accurately capture the link between performance evaluations and elec-

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<sup>6</sup>See Section A.1 in Supplemental Appendix for a detailed presentation.

<sup>7</sup>Consistent with this definition, I consider that parties that do not run again lose the election at  $t + 1$  ( $Victory_{i,m,t+1} = 0$ ). This conceptualization resonates with research on “scare-off” effects in the United States, where the decision to run in  $t + 1$  is interpreted as a component of the incumbency effect (Erikson and Titiunik 2015). A “scare-off” effect occurs when a challenger does not contest a seat to avoid confrontation with a sitting incumbent (Cox and Katz 1996). See De Magalhães 2015 and Appendix for a more detailed justification.

tions in a personalistic setting, I will only focus on non-term limited districts. Because in these districts there is no sitting mayor running at  $t$ , the incumbent is able to run in  $t + 1$ , thus allowing voters to engage in retrospective voting.<sup>8</sup> This decision retains 52 percent of all Brazilian municipalities in the analysis.

**Estimation** I use local polynomial regression to estimate incumbency effects (Imbens and Lemieux 2008; Calonico, Cattaneo and Titiunik 2014). This method entails fitting separate linear regressions of the probability of winning in  $t + 1$  on vote margin on each side of the discontinuity, within a small bandwidth of vote margin. I build an aggregate estimator that allows measuring incumbency effects for the full sample of Brazilian parties.<sup>9</sup> To this end, I construct an “All Candidates” sample including all pairs of winners and runner-ups from every municipality and party. The resulting dataset includes two party observations for every combination of municipality and year. To deal with temporal and spatial dependence, I cluster the standard errors of the aggregate estimator by municipality and year. Formally, the local linear specification takes the form:

$$Y_{i,m,t+1} = \alpha + \beta Incumbency_{i,m,t} + f(M_{i,m,t}) + \phi_{m,t} + \delta_{i,m,t}$$

$$\forall i, m, t \text{ s.t. } |M_{i,m,t}| < \epsilon$$

where the outcome variable  $Y_{i,m,t+1}$  is a binary victory measure for party  $i$  in municipality  $m$  at time  $t + 1$ . The incumbency indicator is  $Incumbency_{i,m,t}$ , which measures victory for

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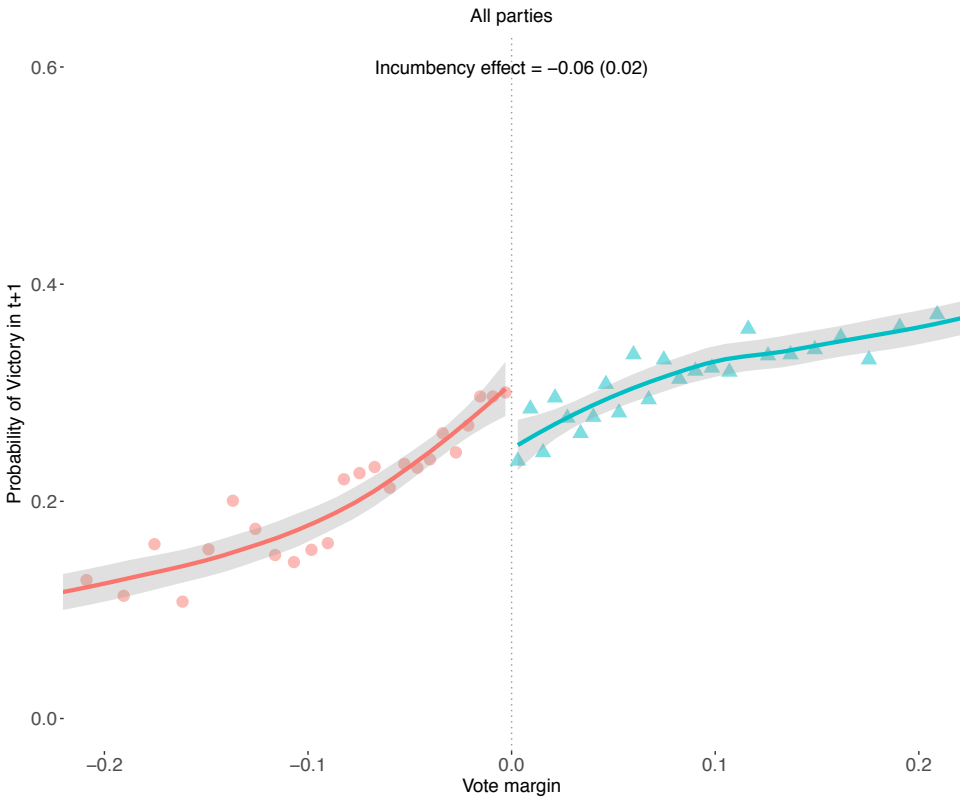
<sup>8</sup>Because parties decide to run before incumbency is assigned at time  $t$ , this sample is selected on the basis of a decision that is taken before  $t + 1$  incumbency is assigned. See Table A.5 in appendix for details on this sample.

<sup>9</sup>The conventional application of RDD estimates separate incumbency effects for individual parties. This procedure is highly inefficient in Brazil’s multiparty system, where 27 different parties have held mayoral office during 2000-2008. See Section A.1 in Supplemental Appendix.

candidate  $i$  in municipality  $i$  at time  $t$ .  $M_{i,m,t}$  is the vote margin between the top ranked candidates and  $\epsilon$  is an arbitrarily small vote margin, which I estimate using the optimal bandwidth algorithm developed by Calónico, Cattaneo and Titiunik (2014).

*Is incumbency an advantage or disadvantage in Brazil?*

**Figure 2.** Average Incumbency effects



Note: The x-axis takes values of vote margin over .1 bins, ranging from -.2 to .2. The circles represent mean probability of victory at  $t + 1$  for each bin, the red and blue lines are loess regression curves, and the shaded areas represent 95 percent confidence intervals. The dashed vertical line at 0 is used to separate winners and losers in  $t$ .

Is incumbency an electoral advantage or disadvantage in Brazilian local elections? Figure

2 plots the probability of victory parties in  $t + 1$  as a function of their vote margin in  $t$ .<sup>10</sup> Following the RDD identification strategy, we can obtain a visual estimate of the incumbency effect by comparing the electoral performance of winners and losers around the 0 percent cutoff. The results suggest that winning mayoral office produces a considerable incumbency disadvantage for Brazilian parties. The electoral handicap is sizable: formal estimation indicates that incumbent parties are 6 percent less likely to win an election than challengers.

This average, however, masks significant heterogeneity in incumbency effects across time periods. Figure 3 shows that an incumbency disadvantage of 13 percent in 2004 and 16 percent in 2016. There is no evidence of a statistically significant incumbency effect in 2008 and 2012. Why do the electoral returns to incumbency vary so widely across elections? The next section explores the answer provided by imperfect retrospective voting.

### *Heterogeneous incumbency effects*

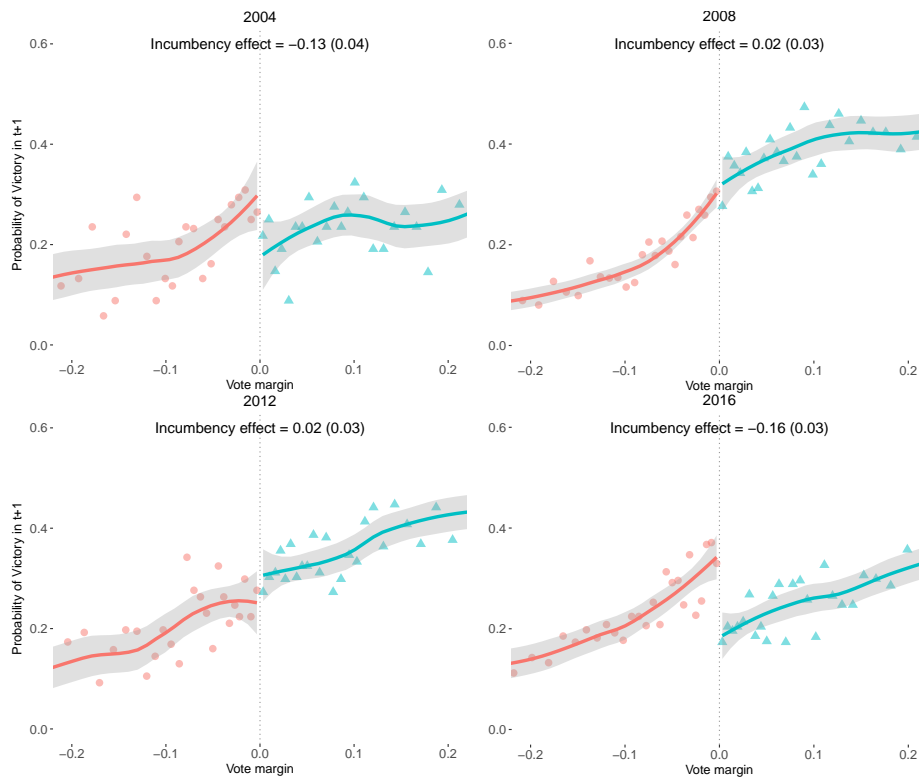
In this section, I study how public goods spending shapes incumbency effects in Brazilian mayoral elections, and how this relationship is influenced by party labels and voters benchmarking strategies.

I measure spending using the Finances of Brazil dataset (FINBRA) compiled by the Brazilian Ministry of Finance. This dataset provides highly detailed information on the revenues and expenditures of municipal governments, disaggregated by issue area. Tied to the transparency efforts of the fiscal responsibility law, FINBRA is monitored by a strong audit

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<sup>10</sup>The analyses rely on data from Brazil's Superior Electoral Tribunal, which includes candidate electoral returns, party affiliation, coalition, gender, and occupation. In instances of runoff elections, I use the first round outcome. The data can be accessed at [www.tse.jus.br/eleicoes/repositorio-de-dados-eleitorais](http://www.tse.jus.br/eleicoes/repositorio-de-dados-eleitorais).

**Figure 3. Incumbency effects over time**



Note: The x-axis takes values of vote margin over .1 bins, ranging from -.2 to .2. The circles represent mean probability of victory at  $t + 1$  for each bin, the red and blue lines are lowest regression curves, and the shaded areas represent 95 percent confidence intervals. The dashed vertical line at 0 is used to separate winners and losers in  $t$ .

court that can credibly sanction misreporting. This dataset minimizes the risk that reported spending does not match spending on the ground, a common problem in studies of distributive politics (Stokes et al. 2013).

My measure of public goods spending considers spending in health, education, and transportation. Following prior studies of Brazil, I consider spending in personnel as proxy for patronage (Bhavnani and Lupu 2016; Brollo et al. 2013). Table 1 provides summary statistics on the main spending measures in the overall sample and in the sample of municipalities that



**Table 1.** Spending measures. Descriptive statistics in the full sample and optimal bandwidth sample

	<i>Change in</i>		
	Transfer revenues	Public Goods Spending	Personnel Spending
<i>Full Sample</i>			
Median	0.30	0.27	0.33
Std. dev.	0.35	0.41	0.47
<i>Optimal Bandwidth</i>			
Median	0.30	0.26	0.33
Std. dev.	0.38	0.43	0.50

Source: FINBRA.

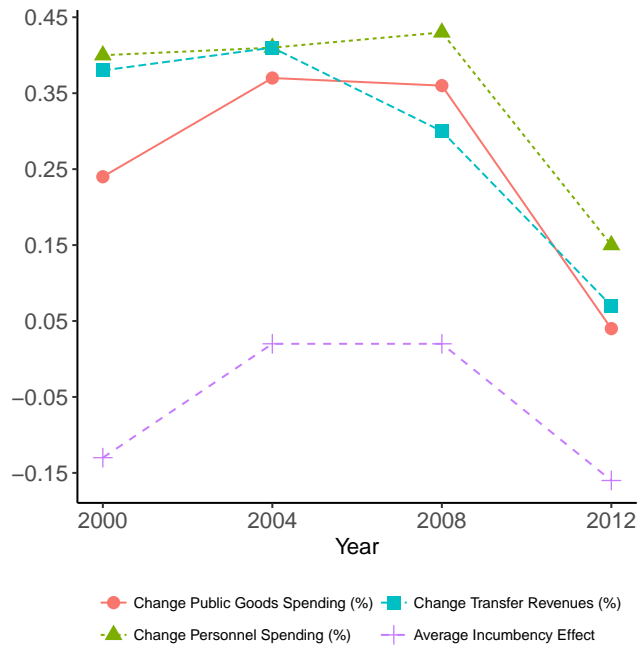
fall within the optimal bandwidth of vote margin.

To study how performance shapes electoral returns to incumbency, I merge the fiscal dataset with electoral returns, and augment it with social and economic indicators collected from the Brazilian Statistics Institute. The resulting dataset combines municipal-level fiscal and economic variables with electoral variables – such as incumbency and electoral performance – measured at the party level.<sup>11</sup>

As a preliminary test of the plausibility of my argument, Figure 4 compares trends in incumbency effects with trends in the changes in public goods spending, personnel spending, and fiscal transfers. Consistent with my argument, incumbency disadvantage coincided with electoral years in which public goods spending increased at a lower rate relative to the past. While fiscal transfers also matter, they only influence incumbency effects when they are accompanied by increases in public goods spending. The plot also reveals that personnel spending does not track incumbency effects as well as public goods spending.

<sup>11</sup>To deal with the spatial and temporal correlation of observations, I use standard errors clustered by municipality and year in the estimation.

**Figure 4.** Trends in Performance and Incumbency effects



Note: Trends in percentage changes in public goods and personnel spending, revenues from transfers, and in average incumbency effects. Average incumbency effects estimated using RDD and local polynomial linear regression. Fiscal data based on FINBRA.

To more rigorously estimate the impact of performance on incumbency effects, I use two approaches to studying treatment effect heterogeneity within the RDD (see Hainmueller, Mummolo and Xu 2016). The first “parametric” approach entails augmenting the local linear regression used to estimate average incumbency effects with a multiplicative interaction between the incumbency indicator and the measures of spending. This model keeps all the observations within the optimal bandwidth used for RD estimation and allows the effect of incumbency to vary as a function of spending. For each party  $p$  in municipality  $m$  at time  $t$ , this expanded model of heterogeneous incumbency effects takes the form:

$$Y_{p,m,t+1} = \alpha + \beta_1 Inc_{p,m,t+1} + \beta_2 P_{m,t} + \beta_3 (Inc_{p,m,t} \times P_t) + \beta_4 Z_{m,t} + \beta_5 (Inc_{p,m,t} \times Z_{m,t}) +$$

$$f(M_{p,m,t}) + \psi_{m,t+1} + \delta_{p,m,t+1}$$

$$\forall p, m, t \text{ s.t. } |M_{p,m,t}| < \epsilon$$

where  $Y_{p,m,t+1}$  is a binary measure of victory of party  $p$  in municipality  $m$  at  $t + 1$ .  $P_{m,t}$  denotes municipal performance, operationalized through different measures of change in spending. In keeping with the RD design, I include a control function for the vote margin  $f(M_{p,m,t})$  and focus on municipalities within narrow vote margins ( $\epsilon$ );  $\psi_{p,m,t+1}$  and  $\delta_{p,m,t+1}$  denote, respectively, municipality and year fixed, effects and  $\delta_{p,m,t+1}$  is an error term assumed to be mean independent on the basis of the RDD identification strategy. One quantity of interest is  $\beta_3$ , which captures how much the incumbency effect vary as a function of performance  $P_{m,t}$ . I am also interested in  $\beta_1 + (\beta_3 \times P_{m,t})$ , which denotes the predicted incumbency effect for specific values of change in municipal spending.

I also rely on a nonparametric approach in which I estimate separate incumbency effects for different values of the fiscal spending variables. While less efficient, this binning methodology provides reassurance that the results obtained in the parametric approach are not a result of linear extrapolation (Hainmueller, Mummolo and Xu 2016).

In both approaches, the main empirical challenge is that omitted municipal characteristics might influence both spending and elections. RDD partially mitigates this problem, because focusing on close elections balances the quality of incumbents and challengers in each type of municipality.<sup>12</sup> But RDD does not rule out municipal sources of confounding. Thus, for example, wealthier municipalities might have a stronger fiscal base and this feature can

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<sup>12</sup>For balance statistics see Table A.11 in Appendix.

facilitate performance and reelection.<sup>13</sup>

I rely on multiple strategies to lend more credibility to my findings. First, I use municipality and year fixed effects to control for underlying differences between municipalities or time periods in changes in spending. Thus, I control for changes in the resources from transfers received by municipalities to account for underlying differences in fiscal capacity (Bhavnani and Lupu 2016) or political connections with upper levels of government (Brollo et al. 2013). I also control for municipal GDP per capita and human development to assess whether the impact of spending is driven by economic or human development. The last control is the size of the local population to address the concern that larger municipalities attract more resources or increase the electoral efficiency of spending. Because these omitted variables may shape the effect of incumbency conditional on spending, I model these controls additively ( $Z_{m,t}$ ) and interacted with incumbency and vote margin.

### ***Do incumbency effects vary with performance?***

I first test Hypothesis 1: do Brazilian mayors improve their electoral fortunes by increasing public goods spending? Table 2 presents results from different parametric specifications of heterogeneous incumbency effects. Across models, public goods spending is associated with a large and statistically significant increase in the advantage of incumbent parties. Substantively, a one standard deviation increase in public goods spending leads to a 10 percent increase in the returns to office. These results hold irrespective of whether covariates or municipality fixed effects are included in the model. Once public goods spending is accounted

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<sup>13</sup>Some scholars use natural experiments, which exploit oil rents (Bhavnani and Lupu 2016; Ferraz and Monteiro 2012) or discontinuities in tax-sharing rules (Brollo et al. 2013) .

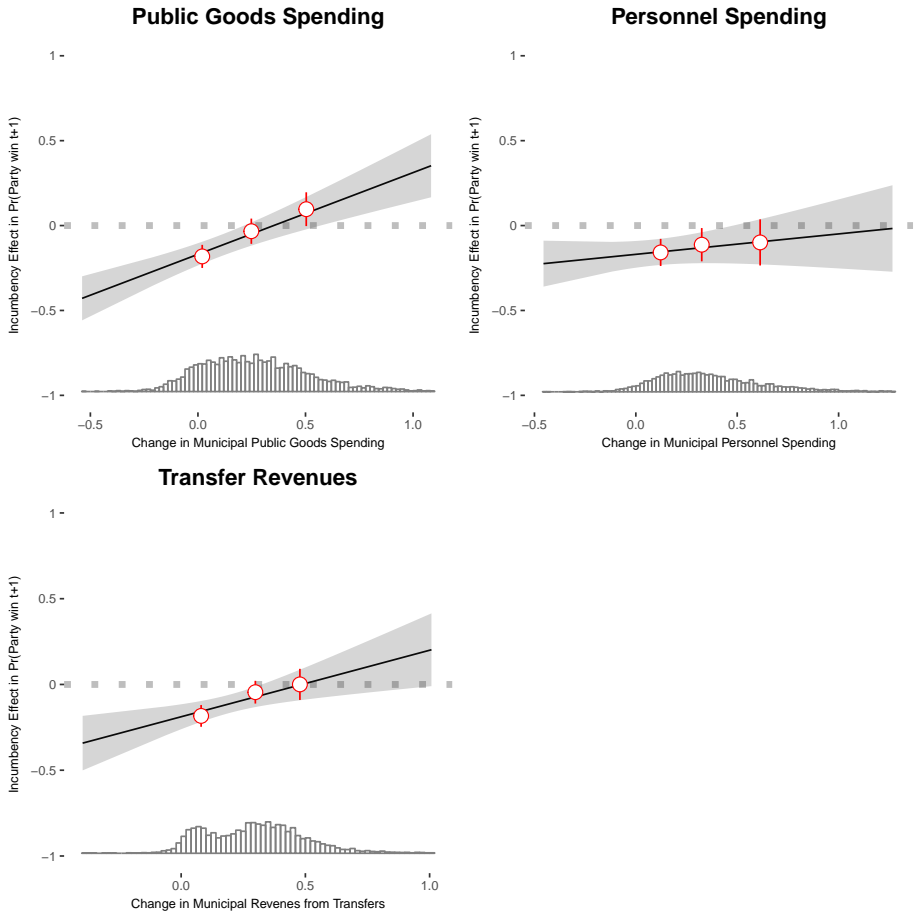
**Table 2.** Heterogeneous Incumbency Effects. Parametric Estimation

	<i>Dependent variable: Pr(Victory<sub>t+1</sub>)</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Incumbency	-0.17*** (0.03)	-0.16*** (0.03)	-0.22 (0.26)	-0.17*** (0.04)	-0.16*** (0.04)	-0.22 (0.33)
Incumbency × Change Public Goods (%)	0.47*** (0.03)	0.51*** (0.06)	0.49*** (0.05)	0.47*** (0.04)	0.51*** (0.07)	0.49*** (0.06)
Incumbency × Change Personnel (%)		-0.003 (0.04)	0.02 (0.05)		-0.003 (0.04)	0.02 (0.06)
Incumbency × Change Transfers (%)		-0.06 (0.09)	-0.10 (0.12)		-0.06 (0.11)	-0.10 (0.15)
Incumbency × log(GDP)			0.02 (0.02)			0.02 (0.03)
Incumbency × log(Population)			-0.01 (0.02)			-0.01 (0.02)
Incumbency × log(HDI)			0.06 (0.12)			0.06 (0.14)
Change Public Goods (%)	-0.23*** (0.02)	-0.21*** (0.04)	-0.19*** (0.03)	-0.12 (0.08)	-0.11 (0.09)	-0.10 (0.09)
Change Personnel (%)		-0.08** (0.04)	-0.04 (0.04)		-0.06 (0.06)	-0.07 (0.06)
Change Transfers (%)		0.04 (0.04)	0.07 (0.06)		0.04 (0.08)	0.08 (0.09)
log(GDP)			-0.003 (0.01)			-0.01 (0.02)
log(Population)			-0.02** (0.01)			0.18* (0.09)
log(HDI)			0.20*** (0.07)			-0.001 (0.11)
Observations	9,192	9,192	9,192	9,192	9,192	9,192
Municipal fixed effects				✓	✓	✓

Note: Incumbency effects on probability of victory in  $t + 1$  conditional on public goods spending and controls Estimates from RDD with a local linear specification where the incumbency indicator was interacted with each spending measure, the vote margin, and controls. The analysis focuses on the optimal bandwidth following Calonico, Cattaneo and Titiunik (2014). All models include year fixed effects and robust standard errors clustered by municipality-year. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

for, there is no evidence to suggest that personnel spending or trends in transfers have a direct impact on incumbency effects.

**Figure 5.** Heterogeneous incumbency effects conditional on spending. Parametric and nonparametric estimation



Note: Conditional incumbency effects estimated using parametric and nonparametric methods. Parametric estimates are based on RDD using the parametric model, where solid lines represent conditional incumbency effects (the sum of the interaction coefficient and the main effect of the spending measure) and dotted lines 95 percent confidence intervals. The nonparametric method relies on the binning estimator proposed by Hainmueller, Mummolo and Xu (2016). White dots represent incumbency effects estimated for the first, median, and third terciles of the distribution of the underlying spending measure. Red vertical segments are the confidence intervals of each of these estimates. The underlaid histogram indicates the density of each spending measure.

Not only does spending increase the returns to office, but it also appears to explain variations from incumbency disadvantage to advantage. This is illustrated in Figure 5, which plots the predicted incumbency effects for every level of change in public goods spending, personnel spending, and fiscal transfers.<sup>14</sup> Besides the linear interaction effect, Figure 5 depicts results from the nonparametric binning estimator of heterogeneous incumbency effects. High performers, who are in the top tercile of the distribution of change in spending, earn an incumbency advantage vis-à-vis challengers of 10 percent. Low performers, by contrast, who are in the bottom tercile of public goods spending, suffer an incumbency disadvantage of 15 percent. Mayors do not benefit from personnel spending at all. Interestingly, transfer revenues do not have a direct impact on incumbency effects once public goods spending is considered. This can be observed in the lack of a statistically significant difference between the top and medium bins.

### *Alternative explanations*

These aggregate results are subject to alternative interpretations. One possibility is that spending matters because it helps incumbent establish clientelistic relationships. However, personnel spending, a standard measure of patronage, makes no contribution to incumbency effects. Another possibility is that these heterogeneous incumbency effects are an artifact of targeted resources from an upper level of government, especially in a setting where most municipal resources originate in fiscal transfers (see Brollo and Nannicini 2012). But this conjecture is

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<sup>14</sup>The figure is based on the models included in columns 2 in Table 2. Because of severe outliers in terms of changes in spending, I only focus on municipalities within 2.5 standard deviations of the median of spending change for ease of graphical presentation. The shape of the relationship and magnitude of the conditional incumbency effects remains unchanged when all the data is used.

inconsistent with two pieces of evidence already presented. If politicized transfers explained whether mayors have an incumbency advantage or disadvantage, the PT, which dominated the presidency in the period under analysis, should be the primary beneficiaries of spending. However, the analyses show that they benefit the least. The evidence shows that revenues from transfers do not drive incumbency effects. While these resources may matter, they only do so when mayors are able to perform well.

The last alternative explanation worth considering is corruption. It could matter in two complementary ways. First, spending in public goods might simply be a proxy for how (little) corrupt politicians are and, therefore, not the source of incumbency effects. Second, corruption may be an even more important consideration for voters than public goods spending and may represent a stronger factor accounting for variation in incumbency effects. To compare the explanatory power of my explanation against corruption, I draw on the dataset from Brollo et al. (2013) and replicate the prior analysis considering public goods spending, corruption, and patronage.<sup>15</sup> Since the measure of corruption is only available for municipalities that underwent corruption audits, I can only compare explanations for that subset of the data. And because it does not include repeated municipal observations, I have to rely on purely cross-sectional variation and controls. The results of this exercise are presented in the Appendix.<sup>16</sup> Neither corruption nor patronage spending bears a relationship with incumbency effects. Public goods spending, by contrast, remains a powerful predictor, even in the

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<sup>15</sup>These authors use the randomized corruption audits conducted by the Brazilian Audit Agency to code the level of malfeasance identified in municipal accounts. They build two measures: broad corruption, which includes irregularities that could also be interpreted as bad administration rather than as overt corruption, and narrow corruption, which only includes severe irregularities that are also more likely to be visible to voters.

<sup>16</sup>See Figure A.13 and Table A.6.



reduced sample under consideration.<sup>17</sup>

## 5 Incumbency Effects at the Micro Level

In this section, I investigate the main predictions of my theory at the individual level through an original survey experiment carried out in 2013 in two Brazilian state capitals.<sup>18</sup> In the survey experiment, I randomly assign voters to hypothetical incumbent-challenger electoral scenarios that vary in terms of mayoral public goods spending, fiscal windfalls, and informative party labels. Besides examining incumbency effects at the micro level, random assignment ensures that the observed associations between spending and party labels, on the one hand, and incumbency effects, on the other, are not driven by unobserved municipal characteristics.

The first prediction is misattribution. The aggregate evidence that voters do not fully benchmark performance is consistent with this prediction. To test it at the appropriate micro level, the survey experiment includes a treatment informing voters that incumbents benefit from a fiscal windfall, and another treatment in which this fiscal windfall is spent in public goods. If voters partially attribute competence from performance, they should only partially discount external shocks from their evaluations and handsomely reward exogenously financed spending (Hypothesis 2).

I also study whether informative party labels attenuate the impact of spending on incumbency effects (Hypothesis 3). Suggestively, the previous analysis reveals a weaker link for

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<sup>17</sup>This reduction in sample also explains why the coefficient for public goods spending is smaller than the ones observed before.

<sup>18</sup>I chose them to tap the northeast-south divide that characterizes the development and political traditions of Brazil.

the programmatic PT than for other parties. But the aggregate data cannot establish whether this occurs because of the information conveyed by party labels, or because municipalities governed by the PT vary in other respects. In the survey experiment, I explore this question by randomly assigning candidates to party labels, where one of the candidates is from the PT and the other candidate is from a center-right party.

## *Design*

The survey was conducted in June 2013 and consists of a representative sample of 1000 Brazilians residing in the cities of São Paulo (N = 600) and Salvador (N = 400).<sup>19</sup> Borrowing from Winters and Weitz-Shapiro (2013)'s study of corruption and accountability in Brazil, respondents were randomly assigned to hypothetical electoral scenarios that varied in terms of fiscal windfalls, public goods spending, and party labels (See Table 3). Following these authors, scenarios are presented the third person. The goal is to force respondents to abstract away from the idiosyncratic experience with their own mayors, so as to provide a realistic approximation of how they would behave if presented with the scenario.<sup>20</sup>

The *control group* is presented with the following preface:

*Now, I want to talk about elections. Imagine a person called Maria (João). She (He) is a*

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<sup>19</sup>*Destaque*, a Brazilian survey firm, conducted the fieldwork. I relied on multi-stage cluster sampling. A random sample of census tracts was drawn following a probability-proportional-to-size principle, and then a random sample of blocks was drawn for each census tract. Within each block, one interview was conducted in every fourth household based on the last birthday method. The sample used quotas for gender, age and social class.

<sup>20</sup>This third-person approach could introduce a different kind of bias if Brazilians possess negative stereotypes about others. But results show that mayors are rewarded for public goods spending, which is hard to equate with a negative stereotype. Besides this evidence, a recent study comparing approaches to vignettes in Brazil found no difference between first and third-person vignettes (Weitz-Shapiro and Winters 2014).

**Table 3.** Experimental Design. Treatment Groups and Number of Observations.

Performance	Party Labels	
	Yes	No
Control	N = 125	N = 125
Fiscal Windfall	N = 125	N = 125
Spending	N = 250	N = 250

*person like you who lives in a different city. This year, Maria (João) has to vote to elect the mayor of her (his) city. In the election, the current mayor, who seeks reelection, runs against an opposition candidate*<sup>21</sup>

Respondents randomly assigned to performance treatments receive an expanded version of the preface which, following the introduction, provides them with information about incumbents' performance. The *fiscal windfall* treatment reads as follows:

*It is important to recall that during his last term in office, the mayor benefited from an increase in funds from federal transfers*

This treatment aims to inform voters that mayors benefit from an ostensibly exogenous fiscal windfall.<sup>22</sup> Importantly, the source of funds matches the origins of most municipal revenues in Brazil. The *spending* treatment consists of the following text:

*It is important to recall that during his last term in office the mayor benefited from an increase in funds from federal transfers and that he spent these funds in health and education*

<sup>21</sup>The gender of the hypothetical voter is chosen to match the gender of the respondent.

<sup>22</sup>Of course, voters could think that resources increased due to the lobbying efforts of the mayor. However, there is no evidence that voters infer more competence from mere resource availability. See Figure A.14 in the Appendix.

The treatment pursues two goals. First, it provides respondents with information of good incumbent performance on public goods. This is the spending area stressed in my argument, and found relevant in the aggregate analysis. It also matches the competencies of the mayor and are funded by federal transfers. Second, by building the fiscal windfall into the vignette, this treatment helps assess whether voters reward incumbents for spending funded by exogenous resources.

To test the assumption that incumbency effects are moderated when informative party labels are available, half the questionnaires were randomly assigned to candidates with real party affiliations while the remaining half did not include party affiliations. All conditions with party labels pit the center-left PT against a center-right party. In São Paulo the center-right party was the PSDB and in Salvador the DEM.<sup>23</sup> This difference sought to include only parties that had held mayoral office in the prior eight years, so that voters would know them.

The outcome variable is an ordinal vote choice question, introduced immediately after the experimental prompt: *On a scale of 1 to 4, where 1 is very unlikely and 4 very likely, how likely is it that Maria (João) will vote for the mayor?*<sup>24</sup> Comparing average support for the hypothetical mayor across conditions provides estimates of the causal effect of different treatments on incumbency effects.

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<sup>23</sup>Party labels were also randomly assigned to incumbency and challenger status across conditions. For example, respondents assigned to a Spending-Party Label treatment with a PT mayor receive the following treatment: “Now, I want to talk about elections. Imagine a person called Maria (João). She (He) is a person like you who lives in a different city. This year, Maria (João) has to vote to elect the mayor of her (his) city. In the election, the current mayor, from the PT, who seeks reelection, runs against an opposition candidate, from the PSDB.”

<sup>24</sup>A binary measure –coded 1 for “likely” and “very likely” responses and zero otherwise– leads to stronger effects.

## Results

### Figure 6. How Likely is María to Support the Incumbent Mayor?

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Note: Bars depict mean support for the incumbent across the performance treatment groups pooling conditions with and without party labels. Vertical segments represent 95% confidence intervals for the mean.

To test how voters process information on fiscal windfalls and spending, I first consider the average effect of performance treatments on electoral support for the incumbent pooling conditions with and without party labels.<sup>25</sup> Figure 6 depicts the mean support for the incumbent and associated confidence intervals in the control and treatment groups. Located almost at the midpoint of the 1-4 range (2.4), voters in the sample do not have a positive or negative predisposition towards incumbents.

But the content of incumbency cues changes when information on performance is introduced. Consistent with voters discounting external constraints only partially, information on the fiscal windfall leads to a statistically significant but small reduction in incumbent support relative to the control group ( $p < .1$ ). But voters are willing to handsomely reward incumbents when these ostensibly exogenous resources are spent in the municipality. Support for incumbents is 2.75 in the spending group, a 10 percent increase relative to the control group ( $p < .01$ ).<sup>26</sup>

I now turn to test whether party labels attenuate voters' response to incumbent spending.

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<sup>25</sup>Section A.3 in the Supplemental Appendix includes robustness checks for the survey experiment. *t-tests* comparing mean pre-treatment scores across treatment groups show excellent covariate balance. "Worst case bounds" imputation suggests that differential non-response is not biasing the results.

<sup>26</sup>The alternative binary measure yields 61% support for the incumbent, corresponding to an 18% increase in incumbent vote share.

## Figure 7. Do Party Labels Attenuate Incumbency Effects?

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Note: Bars depict mean support for the incumbent across the performance treatment groups disaggregating conditions with and without party labels. Vertical segments represent 95% confidence intervals for the mean.

Recall that half the sample was randomly assigned to party label scenarios, which always include a candidate from the PT, known to cultivate programmatic brand. Figure 7 disaggregates support for the incumbent by performance treatment and by whether party labels are available. Consistent with my expectations, the spending treatment has no effect in scenarios with party labels. In scenarios where hypothetical candidates do not have a party affiliation, spending leads to a 0.65 in incumbent support, an effect twice as large as the one observed in the overall sample. Quite strikingly, the effect of spending on incumbent support disappears when party labels are present.

The individual-level results from the survey experiment match the findings from the aggregate analyses. Brazilian voters are willing to reward incumbents for providing public goods, particularly when they cannot rely on informative party labels to guide their choices. But voter evaluations of incumbent performance are imperfect. While voters correct their evaluations by discounting exogenous constraints, they do so only partially.

As is often the case with experimental designs, the internal validity gains associated with random assignment come at the expense of external validity. A reasonable criticism of my approach is that the conclusions drawn from a hypothetical scenario may not travel to real electoral settings. I do not intend the survey experiment to provide a perfect mapping of reality. My goal, instead, is to investigate the microfoundations that underpin the aggregate

findings. In this spirit, I designed the survey experiment to include the party labels, fiscal constraints, and forms of spending at the core of aggregate analysis. The fact that the aggregate- and individual-level analysis point in the same direction lends more credibility to the overall evidence.

## *Conclusion*

This paper studies why subnational incumbents have an electoral advantage or disadvantage in the developing world. I argue that voters' imperfect evaluations of performance are a central factor explaining incumbency effects. I draw aggregate and survey experimental data from Brazilian mayoral elections to show that the returns to incumbency depend on public goods spending, and that voters perceive incumbent spending as a signal of competence even when incumbents are exposed to exogenous constraints. The evidence also suggests that voters pay less attention to incumbency cues when party labels are available.

The findings of this paper do not present an ideal model of democratic accountability. Indeed, they suggest that voters respond to spending because they make attribution errors. Though an imperfect form of citizen oversight, this interpretation represents a sharp departure from accounts based on clientelism and corruption. In these views, bad governance compromises electoral accountability, and creates a perverse equilibrium in which bad policies persist over time. As long as voters can partially attribute competence from performance, my argument and evidence suggest that incumbency effects are compatible with electoral accountability. From a policy perspective, it implies that there is room for policy interventions to improve the ability of voters to hold incumbents accountable for outcomes they are

responsible.<sup>27</sup>

The theory advanced in this paper may help understand incumbency effects in other settings. One set of expectations concerns variations in party systems and political institutions. An observable implication is that incumbency effects should be smaller when parties are programmatically differentiated. Consistent with this hypothesis, Jacobson (2015) shows that party polarization has reduced the size of the incumbency advantage in U.S. Congress during the last decade, and Eggers and Spirling (forthcoming) show that British members of parliament obtain a smaller incumbency advantage in districts where they run against ideologically more distant parties. This evidence from two different settings suggests that incumbency effects may respond to the informativeness of party labels, and offers an avenue for future comparative exploration.

My argument also implies that electoral institutions may shape the incidence of incumbency effects. One reasonable conjecture is for incumbency effects to grow when electoral rules create incentives to cultivate a personal vote. Country case studies suggest that this conjecture is plausible. While legislators build a personal incumbency advantage in the Brazilian Congress (Pereira and Rennó 2003, 2007), incumbency effects are weaker in settings with party-centered electoral rules, such as the Argentine (Jones et al. 2002) and Italian national legislatures (Golden and Picci 2015).

The general logic of imperfect retrospective voting can travel beyond subnational executive elections, to offices where voters infer competence from observable outcomes and cannot fully distinguish external constraints. Consider, once again, the U.S. Congress, known for its

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<sup>27</sup>Other research has shown that providing voters with information on incumbents' responsibility and on benchmarks can improve accountability (See *inter alia* Gottlieb 2016).



incumbency advantage. Incumbent Congressmen are evaluated for constituency service and voters have limited information about their resource constraints. As another example, consider presidential elections, where incumbents are evaluated for macroeconomic performance but are exposed to international economic factors.<sup>28</sup> Future research could explore whether imperfect retrospective voting systematically places Congressmen and presidents in an incumbency advantage or disadvantage across different countries.

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<sup>28</sup>Campello and Zucco Jr show that the electoral performance of Latin American presidents is strongly contingent on exogenous factors such as commodity prices and international interest rates.

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# A Supporting information to Imperfect Evaluations and Incumbency Effects

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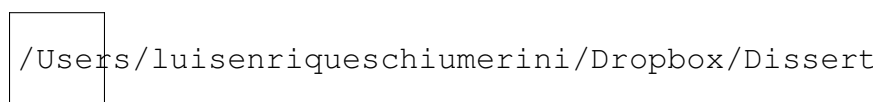
### A.1 *Adjusting the Regression Discontinuity Design*

I adapt Lee (2008)'s regression discontinuity design (RDD) to the Brazilian local context by making adjustments that address multipartism, attrition, personalism, and term limits.

### A.1.1 Multipartism

As described in the manuscript, Lee's original application of RDD to the US Congress and most extensions to other settings proceed by establishing an individual party (ie. Democrats or Republicans) as reference and by estimating incumbency effects for this reference party. Focusing on one party keeps one observation per year and district in the data, so that for every election the unit of analysis and the unit of assignment of incumbency is the district. However, disaggregating the analysis to focus on one party is statistically inefficient in a multiparty system like Brazil. As Figure A.9 shows, 27 parties won mayoral elections and became incumbents in the 2000-2008 period. Given this fragmentation, conducting separate RDD analysis for each party would be highly statistically inefficient and would deprive the analysis of an overall conclusion of the extent of incumbency effects. Thus, while in the manuscript I use this method to obtain estimates of incumbency effects for Brazil's main three parties, my main conclusions are based on an adaptation of Lee that allows me to obtain an aggregate estimate of incumbency effects in the Brazilian multiparty context. I formalize it below:

**Figure A.8.** Number of Victories per party, 2000-2008



**Figure A.9.** Note: Number of mayoral victories by each party in the 2000, 2004 and 2008 elections. Data provided by the Superior Electoral Tribunal.

Let municipality  $m$  at election  $t$  have  $n$  parties contesting mayoral elections. Parties are ordered according to the vote share that they receive at time  $t$ , which is denoted by  $V_{p,m,t}$ . The winner of the election has subscript  $p = 1$  and the runner-up  $p = 2$ . The vote margin

$M_{p,m,t}$  of the winner of election  $t$  in municipality  $m$  is  $V_{1,m,t} - V_{2,m,t}$  and the corresponding vote margin for the runner-up is  $V_{2,m,t} - V_{1,m,t}$ . The incumbency status of party  $p$  takes value 1 if  $M_{p,m,t} > 0$  and zero otherwise.

The outcome of interest,  $Y_{p,m,t+1}$ , is the  $t + 1$  probability of victory of party  $p$  in municipality  $m$ . The outcome for the incumbent is  $Y^1_{p,m,t+1}$  and the outcome for the non incumbent  $Y^0_{p,m,t+1}$ . The causal effect of incumbency can thus be defined as:

$$IE = E[Y^1_{p,m,t+1} - Y^0_{p,m,t+1}]$$

But the same party cannot be observed simultaneously under incumbency and non incumbency status. The key assumption of RDD is that the outcome  $Y_{p,m,t+1}$  is continuous around  $M_{p,m,t} = 0$ . Continuity implies that the causal effect of incumbency may be locally identified by comparing the electoral performance in  $t + 1$  of winners and losers in the vicinity of a vote margin cutoff of 0 percent. On the basis of this identification strategy, the RDD estimator of incumbency effects can be formalized as:

$$IE_{RDD} = \lim_{M_{p,m,t} \leftarrow 0^+} E[Y_{p,m,t+1} | M_{p,m,t}] - \lim_{M_{p,m,t} \rightarrow 0^-} E[Y_{p,m,t+1} | M_{p,m,t}]$$

I estimate RDD using *local polynomial regression*, which is based on fitting two separate local polynomial estimators with a triangular kernel on each side of the discontinuity (Calónico, Cattaneo and Titiunik 2014). The RDD model that I use keeps two observations per municipality and year (a winner and a loser). For implementation, I use the bandwidth selection algorithm advocated by Calónico, Cattaneo and Titiunik (2014), and for inference I use the robust standard errors, clustered by municipality and year, with biased correction developed by these authors.

### A.1.2 Personalism and Attrition

Because the dependent variable in the RD design is electoral performance in  $t + 1$ , outcomes cannot be observed for parties who run in  $t$  but fail rerun in  $t + 1$ . In addition to undefined outcomes, the higher rates of rerunning of incumbents –true in the US and Brazil– than challengers produces differential attrition across treatment and control. Differential attrition can cause bias if the observations that drop out are systematically different from the observations that stay in the sample. To address attrition, Lee (2008) focuses on party rather than personal incumbency. I follow this approach. While not perfect, Brazilian parties do offer a significantly more stable unit of analysis than candidates (See Table A.4)

**Table A.4.** Rerunning Rates by Unit of Analysis. Non Term Limited districts

Unit of analysis	Outcome at $t$	Full Sample		Optimal Bandwidth	
		N	%	N	%
Party	Loser	4154.00	39.15	2311.00	44.63
	Winner	6423.00	60.53	3094.00	59.75
Candidate	Loser	3617.00	34.09	1804.00	44.19
	Winner	7061.00	66.54	2635.00	64.55

Note: The table compares the proportion of candidates or parties contesting elections in a given municipality at time  $t$  that rerun in  $t + 1$ . The rerunning rates are disaggregated by vote margin bandwidth and unit of analysis. The table only reports these rates for non term limited districts. These are the ones included in the data analysis.

### A.1.3 Term Limits

My analysis focuses on non term limited districts – those where the  $t - 1$  winner (the incumbent at  $t$ ) does not run for reelection at  $t$ .<sup>29</sup> Term limited districts, by contrast, are those where the incumbent wins in  $t - 1$  and runs in  $t$ , thus potentially being term limited at  $t + 1$ .<sup>30</sup> In a personalistic setting like Brazil incumbency effects estimated in the non term limited are primarily personal, while those estimated in the term limited sample are partisan.

**Table A.5.** Number of observations by type of district

Sample	Full Sample		Optimal Bandwidth	
	Non term-limited	Term-limited	Non term-limited	Term-limited
All parties	21244	20030	10356	8906
PMDB	4165	4165	4165	4165
PSDB	2848	2950	1380	1289
PT	1907	1605	874	689

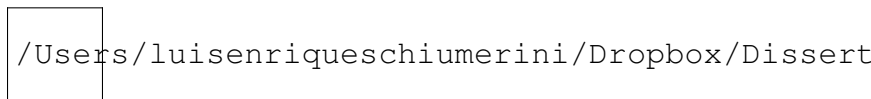
Table A.5 describes the number of observations broken down by type of district, party affiliation and bandwidth. Note that the unit of analysis is the party-year so that there are more observations than municipalities.

<sup>29</sup>Strictly speaking, thus, these candidates are potentially term-limited. Indeed, if they do not win in  $t$ , term limits would not bind for them in  $t + 1$ . This makes it tempting to focus on effectively term limited candidates at  $t + 1$ . But this rule would condition on winning in  $t$  and would therefore produce post-treatment bias. By contrast, my suggested procedure of conditioning on the incumbent running in  $t$  excludes candidates based on a pre-treatment characteristic, as the decision to run is made before the incumbency treatment is assigned in  $t$ .

<sup>30</sup>Fowler and Hall (2014) propose subtracting the incumbency effect observed in term limited elections from the one observed in non term limited elections to isolate a pure personal incumbency effect. This hinges on the assumption that term-limited elections are not systematically different from open seat elections in anything other than the possibility of personal incumbency. However, term limits can shape elections in multiple other ways that should influence the electoral success of the incumbents. Term limits could reduce incentives for incumbent performance, could induce succession crises, and could also create voter fatigue. Due to this difference, I leverage the fact that Brazilian elections are inherently personalistic to assume that non term limited elections provide a good approximation of a purely personal incumbency effect.

## A.2 *Internal Validity of Regression Discontinuity Design*

In this section, I conduct the two conventional tests used in the RDD literature to evaluate the internal validity of the design in the Brazilian local setting.



**Figure A.10. Note: Histogram of Vote Margin and Density tests in  $t$ .** Frequency of non term limited candidates by vote margin in  $t$ , considering full party sample and main party subsamples. The histogram only considers a vote margin range between  $-.1$  and  $.1$

### A.2.1 Density tests

The validity of the regression discontinuity design would be compromised if candidates had control over the vote margin which assigns them to incumbency status. This can be assessed visually by evaluating whether the density of vote margin is smooth around the 0 percent vote margin cutoff. Evidence of bunching on either side of zero would violate the assumption that observations cannot sort themselves. The histogram in Figure A.10 conducts this exercise in the All parties sample and in the individual party subsamples. In keeping with the analysis, the data used for these plots comes from non term limited candidates. The visual inspection suggests that there is no sorting. The plot also includes the p-values of formal density tests of the null hypothesis that the two densities are indistinguishable near the threshold, and fails to provide evidence of sorting (McCrary 2008).



## A.2.2 Balance tests

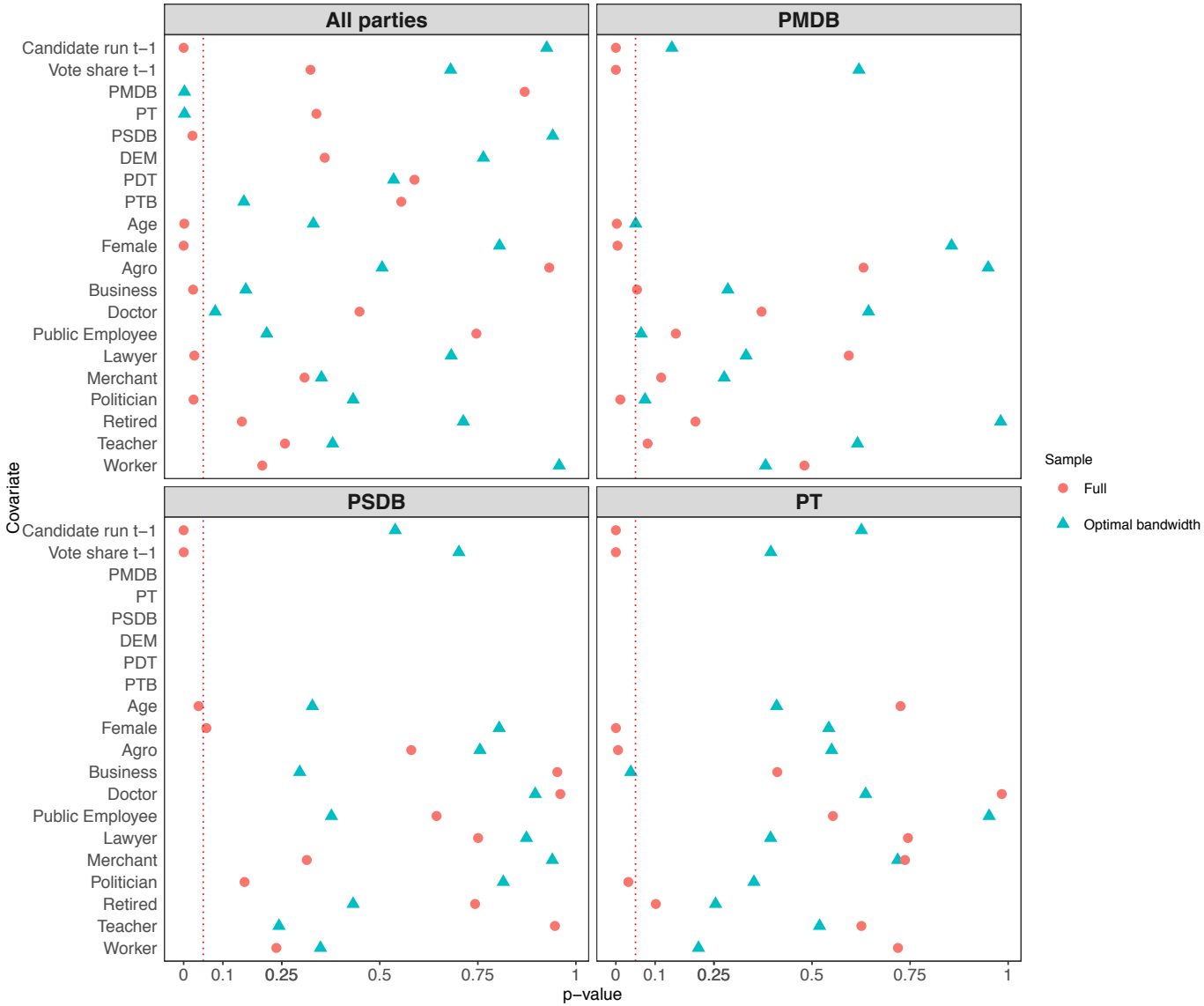
If incumbency status is as-if randomly assigned in close elections, the samples of "bare" winners and "bare" losers should be statistically indistinguishable on observable pre-treatment characteristics.<sup>31</sup> To test this observable implication, I conduct placebo tests that compare winners and losers of close elections along pre-treatment covariates that tap experience, partisan affiliation, demographics and various binary indicators of professional affiliation.

Figure A.11 presents the results of the placebo tests comparing winners and losers. Each panel corresponds to a different party subsample. The first pattern to note is that winners and losers differ on multiple observable characteristics, such as having contested the prior election, partisanship, age, gender, as well as across several occupational indicators. Winners and losers become almost indistinguishable if we compare them within a small vote margin of bandwidth using local linear regression. While some differences do reach statistical significance, it never applies to more than one covariate. This is exactly what one would expect as a result of chance alone. The failure to find systematic differences in the observable characteristics of winners and losers of close bolsters the validity of the RD design in Brazilian mayoral elections.

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<sup>31</sup>An influential article by Caughey and Sekhon (2011) questioned the internal validity of Lee's study of US Congressional elections precisely on the grounds of covariate imbalance. While such evidence may invalidate RD in one particular context, the assumption must be tested –rather than assumed false– in other settings. This principle motivated the study by Eggers et al. (2015). Drawing data from other US offices and time periods, and from nine other countries, they show that recent US Congressional elections are the only setting showing covariate imbalance in close races.

**Figure A.11. Placebo Tests of Covariate Balance**

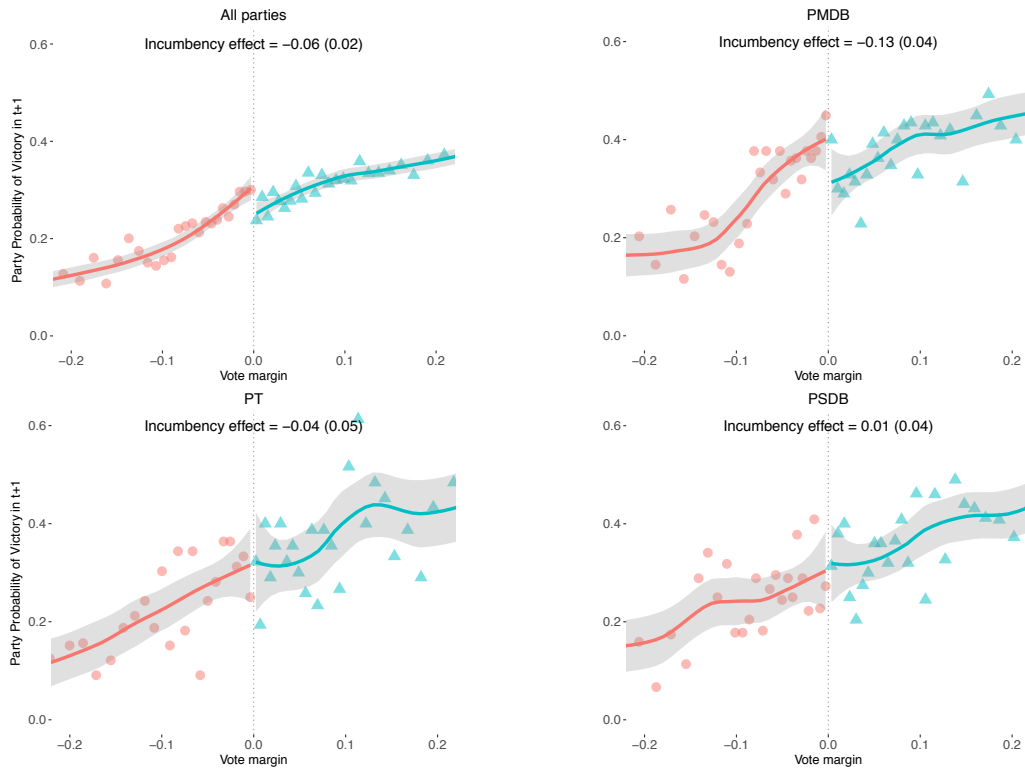


Note: The coefficients represent differences between winners and losers on observable covariates. Balance was estimated for the All parties sample as well as for individual party subsets. Estimation was conducted within the full sample using OLS as well as within an optimal bandwidth of vote margin using local linear regression. Robust standard errors, clustered by municipality and year in parentheses.

### A.3 Further Aggregate Results

#### A.3.1 Average Incumbency Effects by Party

**Figure A.12.** Average Incumbency effects by Party



Note: The x-axis takes values of vote margin over .1 bins, ranging from -.2 to .2. The circles represent mean probability of victory at  $t + 1$  for each bin, the red and blue lines are lowess regression curves, and the shaded areas represent 95 percent confidence intervals. The dashed vertical line at 0 is used to separate winners and losers in  $t$ .

The manuscript only presented average incumbency effects pooling data from all parties. Figure A.12 compare the incumbency effect for all parties with the one estimated for the main party subsamples. The results suggest that the PMDB, Brazil's largest party, has a severe incumbency disadvantage of 15% in probability of victory. No average incumbency

effect is observed for the PT or PSDB.

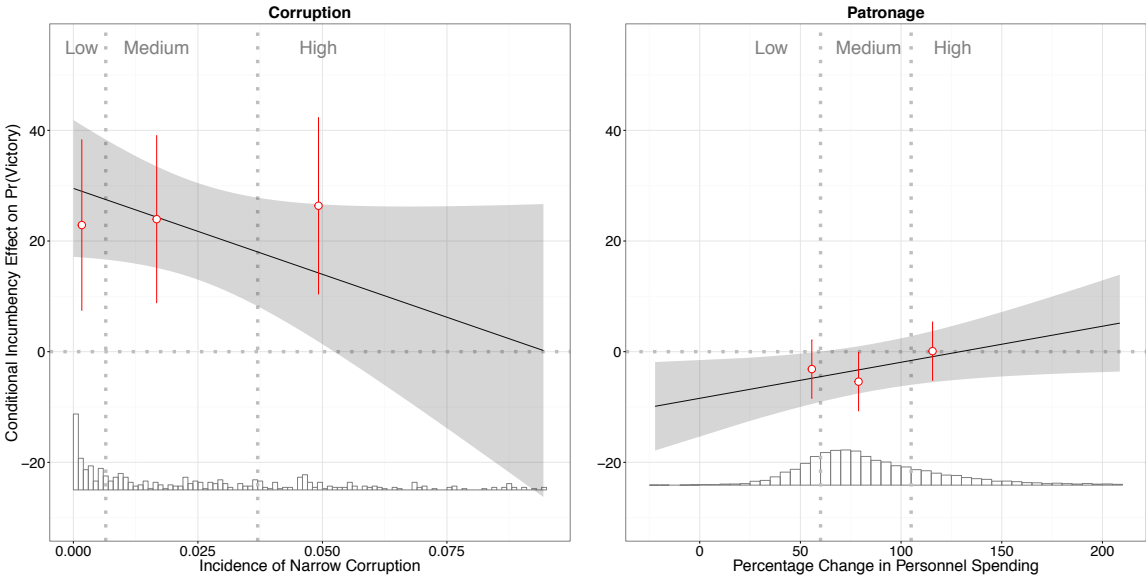
### **A.3.2 *Alternative Explanations***

**Table A.6.** Comparing Good and Bad Governance

	<i>Dependent variable: Pr(Victory<sub>t+1</sub>)</i>				
	(1)	(2)	(3)	(4)	(5)
<i>Incumbency Effects</i>					
Incumbency	0.065 (0.280)	0.051 (0.276)	0.024 (0.277)	0.037 (0.280)	0.018 (0.278)
Inc × Broad Corruption	0.008 (0.069)			-0.002 (0.069)	
Inc × Narrow Corruption		0.086 (0.057)			0.078 (0.057)
Inc × Public Goods Spending			0.140*** (0.041)	0.140*** (0.041)	0.138*** (0.041)
Inc × Personnel Spending			-0.021 (0.038)	-0.021 (0.038)	-0.021 (0.038)
<i>Main Effects and Controls</i>					
Broad Corruption	-0.053 (0.041)			-0.048 (0.041)	
Narrow Corruption		-0.049 (0.034)			-0.046 (0.034)
Public Goods Spending			-0.060*** (0.023)	-0.059*** (0.022)	-0.059*** (0.023)
Personnel Spending			0.017 (0.023)	0.018 (0.023)	0.018 (0.023)
$\log(\text{Population})$	0.053 (0.036)	0.059 (0.037)	0.058 (0.036)	0.060* (0.036)	0.066* (0.037)
$\log(\text{GDP})$	-0.041 (0.031)	-0.046 (0.031)	-0.045 (0.031)	-0.046 (0.031)	-0.050 (0.032)
Incumbency × $\log(\text{Population})$	0.039 (0.047)	0.025 (0.048)	0.019 (0.048)	0.016 (0.048)	0.004 (0.049)
Incumbency × $\log(\text{GDP})$	-0.041 (0.041)	-0.031 (0.041)	-0.029 (0.041)	-0.028 (0.042)	-0.018 (0.042)
Observations	1,578	1,578	1,578	1,578	1,578

Note: Corruption measures based on Brollo et al 2013. Estimates from local linear RDD where the incumbency indicator was interacted with each spending measure, the vote margin, and controls. Based on optimal bandwidth following Calonico et al 2014. All models include state-year fixed effects and robust standard errors clustered by municipality-year. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

**Figure A.13. Bad or good governance?**



Note: The figure compares the relationship between two measures of bad governance and incumbency effects for different Brazilian parties. See note to Figure 5 for estimation details.

The manuscript reports tests for two alternative explanations of the aggregate association between changes in public goods spending and incumbency effects: corruption and personnel spending. These tests are based on analyses of heterogeneous incumbency effects where the incumbency indicator is interacted with proxies of both factors. Measures of broad and narrow corruption are borrowed from the coding developed by Brollo et al. (2013) based on random municipal audits. Because this is a smaller sample than the one used in the manuscript, I also replicate the analysis examining the association between personnel and public goods spending. The results, presented in Table A.6 and Figure A.13, show that while public goods spending remain statistically and substantively significant predictors of incumbency effects, corruption and personnel spending play a much weaker role.

## A.4 Robustness tests for Survey Experiment

**Table A.7.** Balance Tests

Covariate	<b>Control</b>	<b>Fiscal Windfall</b>		<b>Spending</b>	
	Mean	Mean	Difference	Mean	Difference
Age	43.49 (15.87)	44.39 (15.01)	0.9 (1.5)	44.32 (15.5)	0.82 (1.28)
Female?	0.54 (0.5)	0.52 (0.5)	-0.01 (0.05)	0.56 (0.5)	0.02 (0.04)
Unemployed	0.13 (0.31)	0.11 (0.33)	-0.02 (0.03)	0.11 (0.31)	-0.02 (0.03)
Partisan?	0.25 (0.42)	0.23 (0.44)	-0.02 (0.04)	0.24 (0.43)	-0.01 (0.04)
Ideology	4.68 (2.37)	4.87 (2.41)	0.2 (0.24)	4.77 (2.24)	0.09 (0.21)
Tax preferences	1.87 (1.16)	1.8 (1.21)	-0.07 (0.12)	1.65 (1.07)	-0.22 (0.1)
News watching	4.5 (1)	4.47 (0.88)	-0.02 (0.09)	4.43 (0.97)	-0.07 (0.08)
Information	0.35 (0.46)	0.29 (0.48)	-0.06 (0.06)	0.28 (0.45)	-0.07 (0.05)

Results from  $t$ -tests of difference in means between treatment groups and the control group. Standard errors based on unpaired and unequal variance in parentheses.

To assess whether assignment to the treatment conditions in the survey experiment was effectively random, I evaluate whether respondents in the treatment and control groups differed systematically along observable characteristics. Table A.7 presents results from  $t$ -tests of difference in means based on unpaired and unequal variances. Across covariates, the results suggest that treatment and control groups were well balanced.

#### A.4.1 Perceptions of incumbent competence

**Figure A.14.** How Competent is the Incumbent Mayor?

/Users/luisenriqueschiumerini/Dropbox/Dissertation

Note: Bars depict mean competence evaluation for the incumbent across the performance treatment groups pooling conditions with and without party labels. Vertical segments represent 95% confidence intervals for the mean.

The survey instrument includes a post-treatment question asking respondents to rate competence of the incumbent on a 1-7 scale. Figure A.14 plots mean ratings across treatment groups. While voters barely change their competence perceptions after learning about fiscal windfalls, learning that the windfall was spent in public goods increases these perceptions by more than one point ( $p < .01$ ).

#### A.4.2 Dealing with survey non response

	Missing	%
Fiscal transfer	32	13%
Control	39	16%
Spending	29	6%
Total	100	

**Table A.8.** Response Rates by Experimental Group.

The manuscript reported missing survey data on the key outcome measuring the likelihood of voting for the hypothetical incumbent. As Table A.8 documents, exactly 100 observations are missing, amounting to 10 percent of the sample. Though relatively small, the proportion of missing observations varies across treatment groups. The spending group is 10 percent less likely to suffer from missingness than the control.



(Intercept)	0.15*** (0.02)
Fiscal Transfer	-0.03 (0.03)
Spending	-0.10*** (0.02)
Party labels	0.02 (0.02)
$N$	1000
Standard errors in parentheses	
† significant at $p < .10$ ; * $p < .05$ ; ** $p < .01$ ; *** $p < .001$	

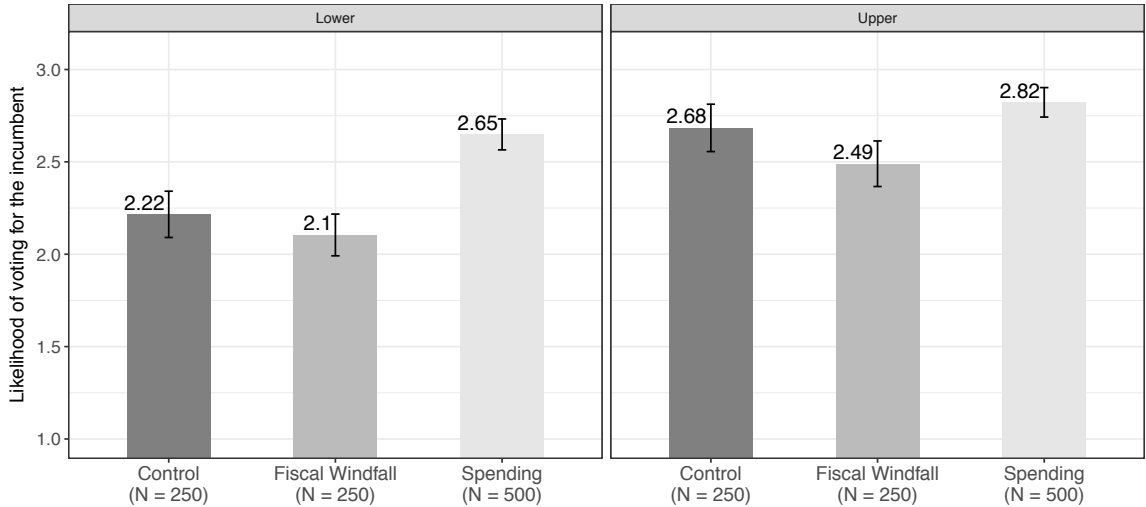
**Table A.9. Differential Non Response.** Results from regression of binary missingness indicator on dummies capturing membership in experimental groups.

Table A.9 confirms that the differential non-response is statistically significant. While differential non response may cause bias if it reflects respondent characteristics, the hypothetical nature of the survey experiment provides another plausible explanation. Recall that respondents are presented with hypothetical scenarios about which they have little information. Consistent with this interpretation, missingness reaches its highest point in the control group, where respondents know little about the hypothetical candidates. Given the low levels of information provided, the 84 percent response rate in the control group is remarkable. As respondents are provided with more information, non response decreases, reaching its minimum in the spending group.

To further evaluate whether missing data is biasing the results, I imputed the outcome using “worst case” bounds (See Manski 1995). This procedure simultaneously assumes that missing responses take the maximum and minimum value and compares treatment effects under each assumption. Because the true treatment effect should be bounded within the

estimates derived from each extreme assumption, if the results obtained each assumption are still consistent with the main findings, we can be sure that the treatment effect would exist even if data were not missing.

**Figure A.15.** Average Treatment Effect. ‘Worst Case Bounds Imputation’



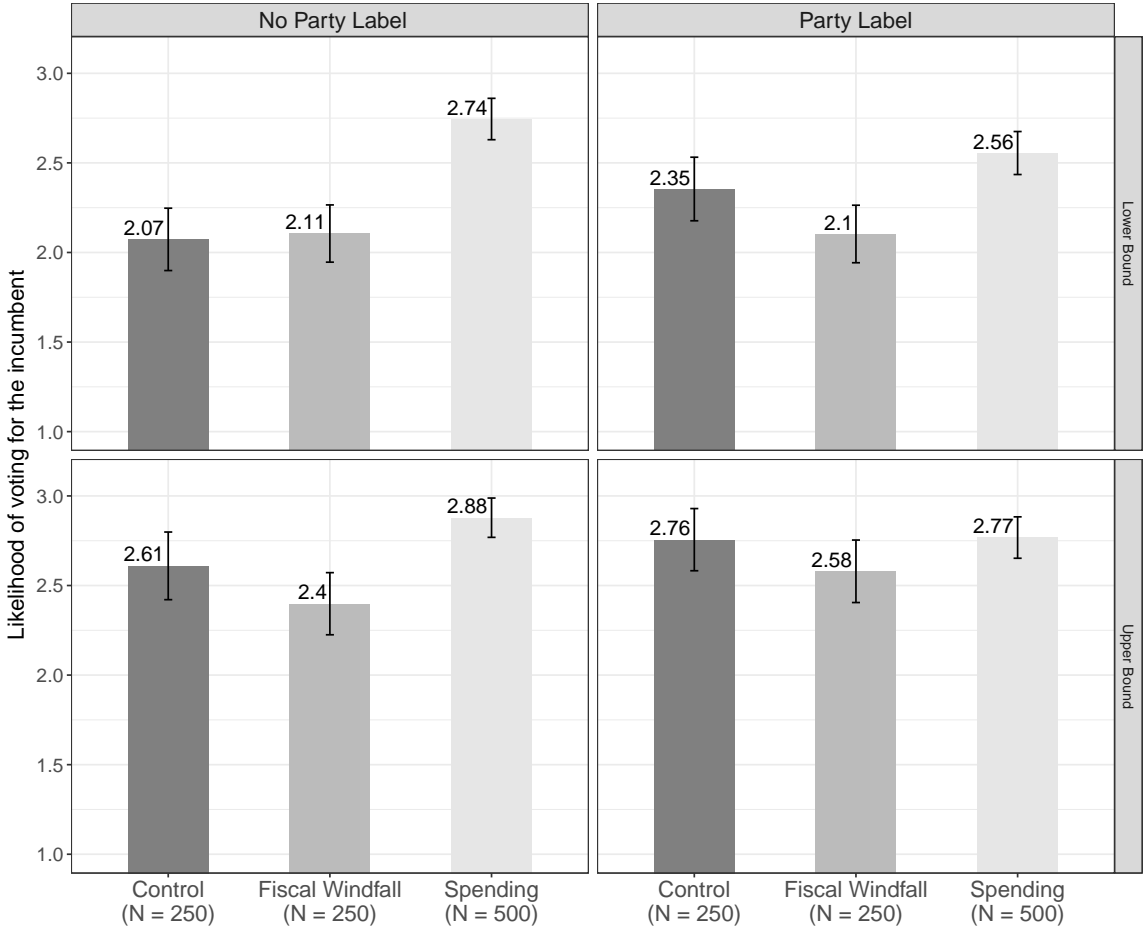
Bars depict mean support for the incumbent across the survey experimental treatment groups pooling conditions with and without party labels. The left panel recodes missing observations as taking the minimum outcome value and the right panel assumes that they take the maximum. Vertical segments represent 95 percent confidence intervals for the mean.

Figure A.15 applies this procedure to the main survey results presented in Figure 6 of the manuscript. The panel on the left hand-side assumes that non respondents assign the minimum likelihood of voting for the incumbent, and are coded with a ‘1’. The panel on the right hand-side assumes that non respondents assign the maximum likelihood of voting for the incumbent, and are coded with a ‘4’. The main comparison between the groups assigned to spending and the control provides stronger treatment effects than the analysis without imputation. Whereas the manuscript reported a treatment effect of .31, the lower bound yields a treatment effect of .55 and the upper bound yields treatment effect of .14.

All estimates are statistically significant. The upper bound yields a stronger and statistically significant difference between the fiscal windfall and the control group one documented in the manuscript. Though still negative, this difference is smaller and becomes statistically insignificant when using upper bound imputation

In keeping with the manuscript, I extend the worst case bounds imputation to the test about the influence of party labels. Figure A.16 plots the results. The first row assumes the lower bound of the outcome variable and the second row assumes the upper bound. Meanwhile, the first column presents results for conditions without party labels while the second column presents results for conditions with party labels. The results confirm the patterns observed without imputation. Under both assumptions –lower and upper bound– the absence of party labels increases the treatment effect of spending. The effects are substantively large and statistically significant under both assumptions. In sum, imputing worst case bounds provides reassurance that the survey experimental are not biased by missing outcome data and differential non response.

**Figure A.16.** Average Treatment Effect Conditional on Party Labels. “Worst case bounds imputation”.



Bars depict mean support for the incumbent across the resource treatment groups disaggregating conditions with and without party labels, and with and without program divergence. The upper panel recodes missing observations as taking the minimum outcome value and the lower panel assumes that they take the maximum. Vertical segments represent 95 percent confidence intervals for the mean.