

# Norms versus Action: Why Voters Fail to Sanction Malfeasance in Brazil\*

Taylor C. Boas  
Boston University

F. Daniel Hidalgo  
MIT

Marcus A. Melo  
UFPE

June 11, 2018

## Abstract

In this paper, we show that Brazilian voters strongly sanction malfeasant mayors when presented with hypothetical scenarios but take no action when given the same information about their own mayor. Partnering with the State Accounts Court of Pernambuco, we conducted a field experiment during the 2016 municipal elections in which the treatment group received information about official wrongdoing by their mayor. The treatment has no effect on self-reported voting behavior after the election, yet when informing about malfeasance in the context of a vignette experiment, we are able to replicate the strong negative effect found in prior studies. We argue that voters' behavior in the abstract reflects the comparatively strong norm against corruption in Brazil. Yet on election day, their behavior is constrained by factors such as attitudes toward local political dynasties and the greater salience of more pressing concerns like employment and health services.

Word count: 9995

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\*We are grateful to Mariana Batista for invaluable help throughout multiple phases of this project; to Marcos Nóbrega and the State Accounts Court of Pernambuco for their partnership; to Amanda Domingos, Julia Nassar, and Virginia Rocha for research assistance; and to seminar participants at Boston University, Harvard University, University of Notre Dame, Universidade Federal do Rio Grande do Sul, Universidade Federal de Pernambuco, Real Colegio Complutense, and the 2017 International Congress of the Latin American Studies Association. Thanks to Alejandro Avenburg, Jacqueline Behrend, Spencer Piston, Fabrício Pontin, and Matthew Singer for comments on previous versions. This study is part of the Metaketa Initiative on Information and Accountability, funded by Evidence in Governance and Politics (EGAP), and is pre-registered with EGAP (ID 20151118AA). Approval was obtained from the Institutional Review Boards of Boston University (protocol 4094X), MIT (protocol 1604551604), and the Universidade Federal de Pernambuco (número de parecer 1571592).

# 1 Introduction

Malfeasance by elected officials is an important problem in democracies around the world. Some politicians engage in actions that are corrupt: accepting bribes, diverting public funds into personal bank accounts, or otherwise using their office for private gain. Others stop short of outright corruption but engage in gross violations of the law, such as failing to pay pension contributions for state employees or ignoring mandated budgeting targets for social services. Both forms of malfeasance impinge upon citizens' welfare and impose significant economic costs on society. They can also contribute to disillusionment with democracy and support for authoritarian alternatives.

Democracy offers a solution to the problem of political malfeasance: vertical accountability. Provided that voters obtain credible information about official wrongdoing, they will have an opportunity to sanction politicians who break the law. Vertical accountability requires that voters condemn malfeasance by elected officials, versus believing that politicians are entitled to govern as they see fit, that accomplishments excuse illegal behavior, or that lawbreaking while in office amounts to a minor transgression. It also requires that voters act upon this norm when they go to the polls, rather than being constrained by personal loyalties, partisanship, clientelism, intimidation, or a belief that the opposition is no better than the incumbent.

In recent years, survey experiments in democracies around the world suggest that voters react negatively to malfeasance by public officials in the context of hypothetical vignettes. Such studies tell respondents to imagine a mayor or legislator who is running for reelection and ask about their likelihood of voting for him or her. In the treatment condition, voters are informed about some form of wrongdoing by the elected official. Vignette experiments have found significant negative effects on vote intention in countries ranging from Sweden to Peru to Moldova. The electoral punishment is particularly large in Brazil, where corruption has become highly salient in recent years.

In this study of Brazil, we argue that punishing malfeasance in the context of vignette experiments reflects norms against corruption that may not translate into action in real life. Partnering

with the State Accounts Court of Pernambuco, a governmental auditing agency, we conducted a field experiment during the 2016 municipal elections in which the treatment group received information about official wrongdoing, or lack thereof, by their mayor. The treatment has no effect on self-reported voting behavior after the election, yet when providing the same information in the context of a vignette experiment, we are able to replicate the strong negative effect found in prior studies. Voters' behavior in the abstract reflects a comparatively strong norm against corruption and other forms of malfeasance in Brazil. Yet behavior at the polls is constrained by other factors, including personal attitudes toward local political dynasties and tradeoffs with government performance in more tangible areas such as job creation and health.

Our primary objective in this paper is to investigate the effect of information about incumbent malfeasance on voting behavior. Doing so necessitates a discussion of the different methods that have been used to study this question. Hence, our secondary purpose is methodological. We argue that survey vignette experiments are well suited to studying voters' norms, whereas field experiments can tell us how these norms translate into action—or lack thereof—in the real world.

## **2 Malfeasance and Electoral Accountability: Prior Findings**

The general conclusion from prior research on malfeasance and electoral accountability is that voters punish politicians who break the law as long as they are sufficiently informed about the transgressions. This finding supports the prediction of formal models of political accountability (Fearon, 1999) that voters will act rationally on information about incumbent performance so as to incentivize politicians to govern with their preferences in mind. In settings as diverse as Brazil, Italy, and the United States, studies based on electoral results have shown that politicians accused of corruption fare worse in their reelection bids than those who are not implicated, at least when local or national media provide coverage of the scandals (Castro and Nunes, 2014; Chang, Golden and Hill, 2010; Ferraz and Finan, 2008; Jacobson and Dimock, 1994; Jucá, Melo and Rennó, 2016; Pereira, Melo and Figueiredo, 2009; Pereira, Rennó and Samuels, 2011; Pereira and Melo, 2015;

Peters and Welch, 1980; Rennó, 2008; Welch and Hibbing, 1997). While most of these studies rely on observational data, Ferraz and Finan (2008) leverage random audits of municipal governments in Brazil, providing a strong basis for causal inference.

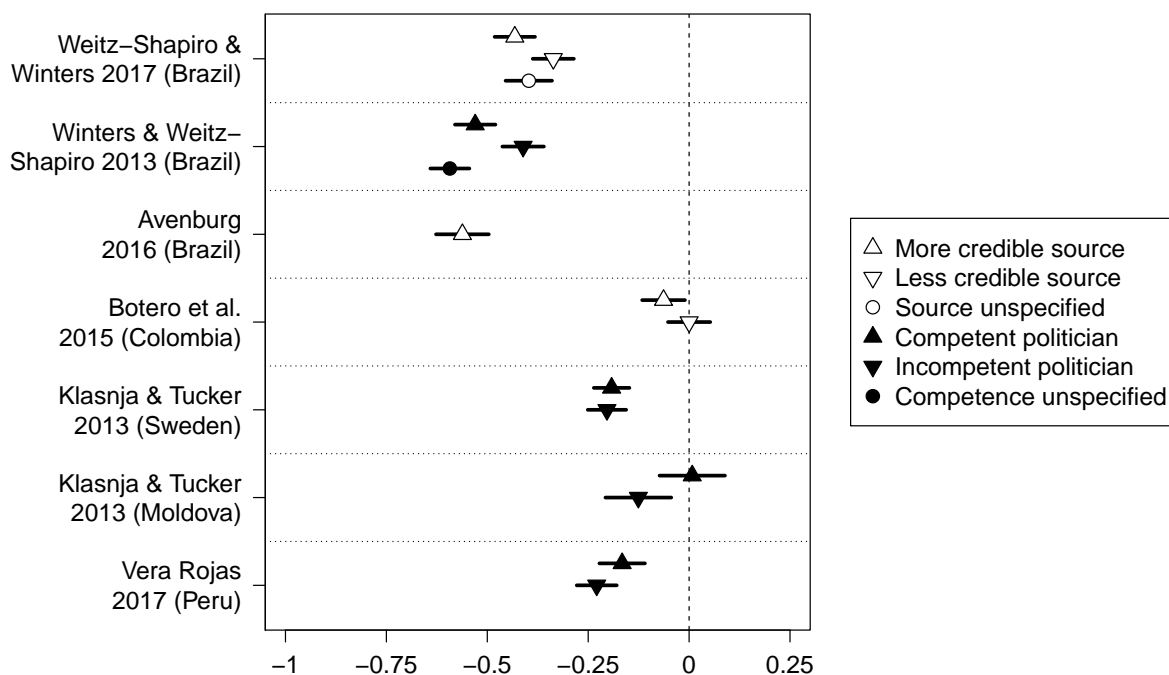
Yet studies based on electoral results alone provide no evidence of individual-level voting behavior, so there is room for uncertainty about the causal mechanism. Malfeasant politicians might perform worse because voters act upon this information, but news of a scandal could also hurt an incumbent's fundraising efforts or prompt stronger challengers to enter the race, diminishing her electoral prospects in a more indirect fashion (Jacobson and Dimock, 1994; Pereira, Rennó and Samuels, 2011).

Observational studies based on survey data have reached similar conclusions. Though effects may vary based on co-partisanship, co-ethnicity, access to a politician's patronage networks, or the ideological polarization of the race, respondents choose to punish corrupt incumbents under at least some conditions (Chang and Kerr, 2017; Dimock and Jacobson, 1995; Rennó, 2007, 2011). Yet, as with all observational studies, those examining individual-level survey data leave room for uncertainty about causal effects. Perceptions of corruption might reduce support for an incumbent, but those who oppose the incumbent for other reasons may also be more inclined to see them as corrupt.

In recent years, scholars have used vignette experiments to gain new leverage on the causal effect of information about official malfeasance on voting behavior. In their most basic form, electoral accountability vignette experiments ask respondents to imagine a mayor or legislator who is running for reelection. In the control condition, the politician is either described as honest, or else no information about probity is provided; in the treatment condition, he or she is accused of corruption or illegal activity. The outcome measures self-reported likelihood of voting for the reelection of this fictitious incumbent (Avenburg, 2016; Botero et al., 2015; Klačnjaja and Tucker, 2013; Vera Rojas, 2017; Weitz-Shapiro and Winters, 2017; Winters and Weitz-Shapiro, 2013, 2016).

Vignette experiments have almost universally found that information about malfeasance by elected officials significantly reduces the likelihood of voting for their reelection. Figure 1 plots

**Figure 1: Malfeasance and Voting Behavior: Information Effects in Vignette Experiments**



NOTE: Icons give average treatment effects (rescaled 0–1) and lines indicate 95% confidence intervals.

average treatment effects (rescaled 0–1) and 95% confidence intervals from vignette experiments on electoral accountability in Brazil, Colombia, Moldova, Peru, and Sweden. Each of these studies involves a corrupt incumbent in the treatment condition; an honest one, or no information about probity, in the control condition; and an outcome measuring vote intention for the incumbent on a 4- or 7-point Likert scale. Though the size and significance of the treatment effect depend on whether the information comes from a credible source and whether the fictitious politician is otherwise competent at delivering public goods, each of these studies suggests that, under at least some conditions, voters will reduce their support for corrupt incumbents.

Evidence from field experiments points toward different conclusions than vignette experiments. In Mexico, Chong et al. (2015) found that information about incumbent corruption lowered support for the opposition more than it did for the incumbent party. In a Brazilian mayoral election, De Figueiredo, Hidalgo and Kasahara (2011) show that accusations of corruption against each candidate in the runoff reduced vote share only for the challenger, not for the incumbent. Other field

experiments informing about different aspects of incumbent performance, such as the provision of public goods, also suggest that sub-par elected officials escape punishment at the polls. If anything, the effect of providing information is to boost support for good performers rather than take votes away from bad ones (Banerjee et al., 2011; Humphreys and Weinstein, 2012).

To date, no study based on a vignette experiment can be directly compared to a field experiment, so prior literature provides no basis for saying whether behavior in hypothetical settings differs from real-world behavior or whether contradictory findings are attributable to differences in political context or research design. However, many studies based on vignette experiments acknowledge that electoral accountability effects in the real world are likely to be smaller than in hypothetical scenarios, for a variety of reasons.

A first set of factors concerns the information provided to voters (Winters and Weitz-Shapiro, 2013). Real-world accusations of malfeasance frequently lack details of what allegedly occurred and who is responsible. The source of the information—often an opposition party or candidate—may have low credibility because of a vested interest in the election outcome. Moreover, relevant information may be delivered weeks, months, or even years before the election, rather than immediately prior to the voting decision, as in a hypothetical vignette. Observational studies have shown that corruption-related information released closer to the election date has a larger effect on results (Pereira, Melo and Figueiredo, 2009).

A second reason why electoral accountability effects may be smaller in the real world concerns the campaign context in which voters make their decisions (Banerjee et al., 2014; Barabas and Jerit, 2010; Botero et al., 2015; Klačnja and Tucker, 2013). Charges of malfeasance are likely to be met with denials and counteraccusations from the incumbent who is targeted. The wealth of competing information circulating during campaign season may limit the effect of any single accusation. Moreover, the salience of other issues that are more directly relevant for individual welfare may reduce the weight that voters attach to information about politicians' probity in office. As Krosnick (1990, 62) argues, the impact of "policy attitude on a citizen's candidate preferences should depend on the personal importance of the policy attitude to the voter."

A third set of factors concerns features of the broader political context that serve to constrain campaign effects on voting behavior. In some places, strong partisanship may mean that there is little potential for new information to change voters' minds (Berelson, Lazarsfeld and McPhee, 1954; Campbell et al., 1960; Converse, 1962; Zaller, 1992). Dynastic politics and personal loyalties may have the same effect even where partisanship is weak. Vote buying and other forms of clientelism, or voter intimidation and threats of violence, may mean that voting decisions respond to material necessities and self-preservation rather than sincere preferences. Various studies have shown that “insiders”—those who belong to a candidate's patronage network or share the same ethnicity, ideological position, or partisan affiliation—are less responsive to information about corruption (Anduiza, Gallego and Muñoz, 2013; Barros and Pereira, 2015; Chang and Kerr, 2017).

Finally, the choices available to voters in a real election may also serve to limit electoral accountability (Muñoz, Anduiza and Gallego, 2016; Vera Rojas, 2017). Where the incumbent is dominant and the election uncompetitive, people may feel they have no capacity to punish malfeasance with their vote. Alternatively, a politician may be so unpopular for other reasons that it is difficult to further lower her base of support by providing specific information about malfeasance. Politicians' assessments of their electoral viability can affect the decision to run again; those who are vulnerable to accusations of malfeasance might opt out, leaving only those who feel confident that such charges will not hurt their performance (Jacobson and Dimock, 1994; Pereira, Rennó and Samuels, 2011). Moreover, where corruption and lawbreaking are endemic, opposition candidates may not be considered any better than the incumbent (Pavão, Forthcoming).

Seeking to replicate findings from hypothetical scenarios in the context of a field experiment thus presents a tough test. Yet the consensus in the survey experimental literature is that corresponding real-world effects, while smaller, should be non-zero (Banerjee et al., 2014; Barabas and Jerit, 2010; Winters and Weitz-Shapiro, 2013). One can maximize the likelihood of finding a significant effect from a real-world intervention through a combination of research design and case selection. Treatments should be designed in order to maximize the specificity, credibility, and availability of the information provided to voters. Moreover, it makes sense to focus on the country where

vignette experiments have demonstrated the largest electoral accountability effects: Brazil.

### **3 Anti-Corruption Norms and Institutions in Brazil**

As shown in Figure 1, when presented with hypothetical scenarios, voters in Brazil judge official malfeasance much more harshly than those from other countries. In this section, we argue that they do so because of a particularly strong anticorruption norm.

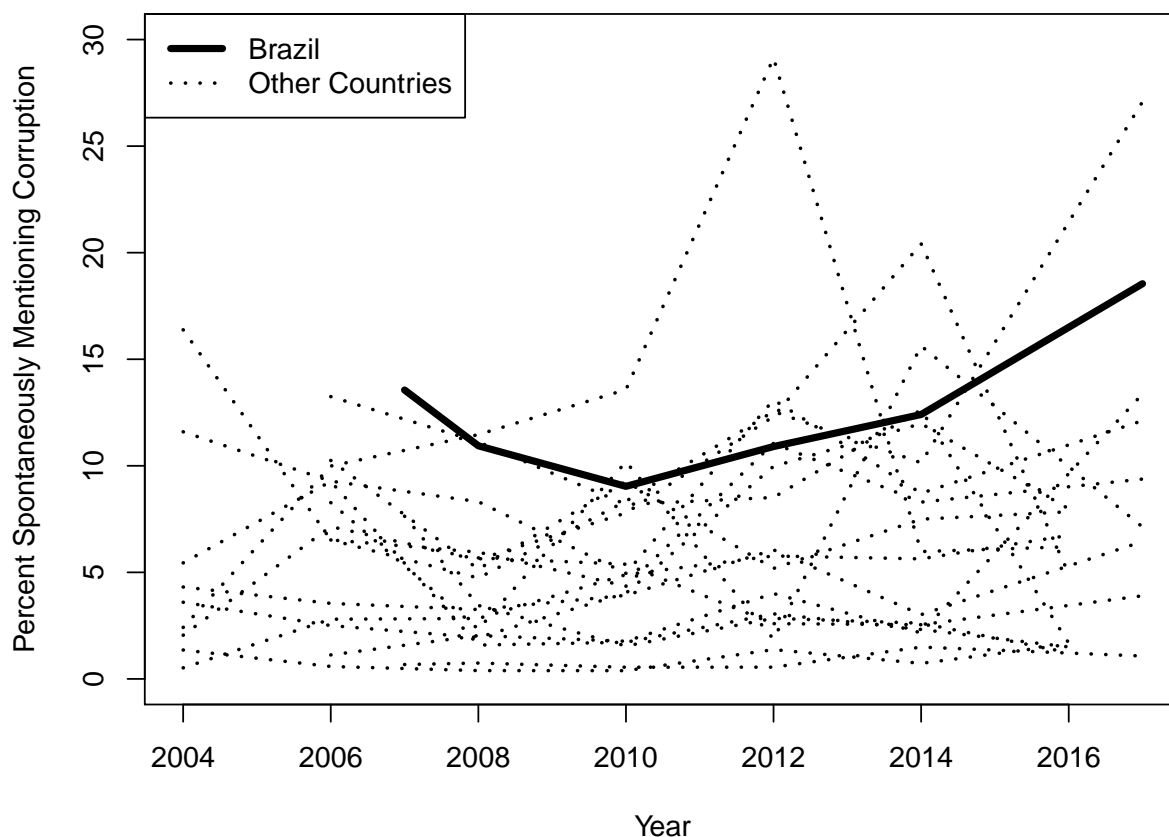
Data from cross-national public opinion surveys demonstrate the strength of anti-corruption sentiment in Brazil. The biennial AmericasBarometer asks respondents an open-ended question about the most serious problem facing the country. Figure 2 plots the percentage in each Latin American country who spontaneously mentioned corruption. While some countries have had higher spikes in response to particular scandals, on average Brazil has the highest levels of popular concern with corruption in the region. Moreover, in 2017, corruption was tied with the economy as the most commonly cited problem.

Public opinion regarding corruption reflects the deep roots of this problem in Brazil’s political system. Yet there have also been significant efforts to create laws and institutions that can prevent and punish malfeasance by elected officials. Brazil’s Constitution establishes auditing institutions—the Federal Accounts Court (Tribunal de Contas da União, TCU) and State Accounts Courts (Tribunais de Contas dos Estados, TCEs)—that are charged with monitoring government compliance with laws regarding budgeting and public administration. The main form of supervision is through an annual audit of accounts, followed by a recommendation as to whether these accounts should be approved or rejected. For audits of executives, the recommendation is then sent to the corresponding legislature—federal, state, or municipal—for a final decision (Avenburg, 2016; Speck, 2011).

Decisions taken by Brazil’s auditing institutions have potentially severe consequences for politicians. In 2010, the passage of the “Clean Slate” (*Ficha Limpa*) Law allowed candidates to be barred from running for office for eight years if the TCU or TCE had recommended rejection of their



**Figure 2: Corruption as the Most Important National Problem**



NOTE: Data are from the AmericasBarometer by the Latin American Public Opinion Project, for all Spanish- and Portuguese-speaking countries of Latin America (except Cuba) and all available years from 2004–2017. Country-level means apply sampling weights.

accounts. This effort was spearheaded by the non-governmental organization Movement to Combat Electoral Corruption, which gathered 1.5 million signatures to introduce the bill in Congress via the popular initiative process (Breuer and Groshek, 2014). In August 2016, a Supreme Court decision significantly weakened the Clean Slate Law, ruling that the rejection of an executive's accounts could only be grounds for disqualification if the decision had been upheld by the corresponding legislature. Nonetheless, the successful passage of this law speaks to the strength of the anti-corruption norm in Brazilian society.

Support for the Clean Slate Law's original sanctions regime remains strong, even after the

Supreme Court decision that weakened it. In the post-electoral wave of the survey analyzed below, we asked respondents whether mayors who had their accounts rejected by the TCE should have the right to run for reelection. In the full sample of respondents, 91% answered “no.” Even among respondents who reported voting for the incumbent mayor and had been informed of the rejection of his or her accounts, 84% said that such mayors should not have the right to run again—effectively claiming that the candidate they supported should not have been on the ballot.

Moreover, the issue of malfeasance by elected officials was made unusually salient by developments in national politics at the time of our study. Impeachment proceedings against President Dilma Rousseff—who was formally removed from office one week before our baseline survey went to the field—were based on charges of fiscal irresponsibility raised by the TCU during its annual review of the federal government’s accounts. In addition, much of Brazil’s political class was engulfed in the massive Lava Jato corruption scandal, which, at the time of our study, had recently led to the expulsion from Congress of a former president of the Chamber of Deputies. High-profile incidents of malfeasance might also be expected to induce cynicism in the electorate, reducing the potential for treatment effects. Yet as we argue below, voters were quite varied in their pre-treatment assessments of their mayor’s honesty, suggesting that there was room for information about the probity of government to affect voting decisions.

## **4 Research Design**

To examine the effect of information about incumbent malfeasance on voting behavior in Brazil, we implemented a field experiment in the state of Pernambuco informing voters about mayors running for reelection in 2016, as well as a vignette experiment that provided similar information about a hypothetical mayor. Our pre-analysis plan, including the hypothesis that the vignette experiment would yield larger effects on voting behavior than the field experiment, was pre-registered with Evidence in Governance and Politics (EGAP) prior to our access to the outcome data.

## 4.1 Field Experiment

Our field experiment was conducted in partnership with the State Accounts Court of Pernambuco (TCE-PE). We chose Pernambuco largely because of the professionalism and efficiency of the TCE-PE. The auditing agencies of Brazil's states vary in the degree to which they are considered independent, professional organizations free from overt political meddling; the reputation of Pernambuco's court is among the best (Melo, Pereira and Figueiredo, 2009). In a 2018 study of Brazil's 32 accounts courts by the Association of Accounts Courts Members of Brazil, adopting a methodology developed by the International Organization of Supreme Audit Institutions, the TCE-PE ranked in the top tercile in terms of Independence and third overall on a summary measure of eight different areas of performance (ATRICON, 2018). Moreover, unlike courts in some other states, the TCE-PE typically completes its review of accounts in three years or less, meaning that most mayors have their first year's accounts judged prior to the next election.

The TCE-PE's professional reputation means that citizens place a high degree of confidence in the institution—an important condition for information to affect voting behavior. As shown in the Appendix, in our baseline survey, confidence in the TCE-PE was significantly higher than in the federal government, the justice system, or respondents' municipal government.

The magnitude of informational effects on voting behavior is also likely to depend on the specificity of the charges and the timing of information delivery relative to election day (Winters and Weitz-Shapiro, 2013). With respect to both factors, our treatment sought to maximize the potential for large effects, subject to practical limitations and external validity concerns. Information delivery took place 2–3 weeks prior to the election, after candidacies had been declared, the campaign was in full swing, and voters were likely to be thinking about their decisions. This is much more proximate than a charge communicated by the media several years before the election.

Our treatment also sought to provide as many details as were practical and to deliver the information in a fashion that would maximize comprehension. Treatments informed voters as to whether the accounts of the mayor in their municipality were approved or rejected by the TCE-PE

in 2013, along with the percentage of other municipalities in the state that fell into the same category (12% rejected and 88% approved). Information was delivered in the form of a flier handed out by enumerators during the baseline wave of our panel study; examples are contained in the Appendix. Enumerators also summarized the information orally to maximize information retention and facilitate comprehension.<sup>1</sup>

In some respects, the fliers were intentionally less specific than they could have been. At the TCE-PE's request, we omitted the mayor's name from the flier and corresponding survey question, in keeping with the court's practice of not personalizing its decisions. However, the mayor's name was mentioned in four prior questions, including one, just before the delivery of treatment information, that measured pre-treatment knowledge of whether accounts had been approved or rejected. We also chose not to include the specific reasons why accounts were rejected. The TCE's rejection of accounts usually happens for a variety of reasons. It would be difficult to summarize these reasons succinctly; picking and choosing among them would require arbitrary decisions; and including municipality-specific details would have made treatments less comparable to one another.

Our treatment information does not necessarily imply egregious acts of corruption, such as bribetaking, as some vignette experiments have done. As shown in the Appendix, accounts are most often rejected for activities that impinge upon public welfare without lining the mayor's pockets, such as excessive spending on personnel salaries or failing to fund pensions. While accounts could be rejected for smoking-gun evidence of corruption, circumstantial evidence, such as unex-

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<sup>1</sup>Some scholars have questioned the legality of conducting electoral field experiments in Brazil, given strict regulations governing campaign advertising (Cunow and Desposato, 2015; Desposato, 2015). Our fliers were carefully designed not to meet the legal definition of campaign advertising; they said nothing about elections, voting, or specific candidates. Furthermore, they were reviewed and approved not only by the Ethics in Research Committee of the Brazilian institution of one of the authors, but also by lawyers at the TCE-PE. See Appendix for further discussion.

plained discrepancies in accounts, is much more common. Given the reality of how malfeasance is conducted and uncovered in Brazil, studies of electoral accountability that rely on observational data routinely cast a much broader net than those cases involving unambiguous evidence of self-enrichment (Ferraz and Finan, 2008, 710; Jucá, Melo and Rennó, 2016, 16–18; Pereira and Melo, 2015, 89). Our treatment information—also based on observational data—is similarly broad and realistic. That said, given strong support for the Clean Slate Law’s original sanctions, it is likely that the vast majority of respondents *perceive* the rejection of a mayor’s accounts as involving corruption or something similarly severe.

The experimental sample consisted of 3,200 adult registered voters in 47 municipalities in the state of Pernambuco. The initial sampling frame included those municipalities in which the mayor was running for reelection in 2016 and the TCE-PE had already judged the 2013 accounts. We included all 7 municipalities where a mayor with rejected accounts chose to run for reelection, and we randomly sampled an additional 40 municipalities where the mayor’s accounts had been approved.<sup>2</sup> Enumerators interviewed 40 voters in each of the accounts approved municipalities, and between 80 and 416 voters in each of the accounts rejected municipalities, for a total of 1,600 respondents from each group. Respondents were randomly assigned with equal probability to a treatment group that received information about approval or rejection of their mayor’s accounts, a pure control group that received no information, and a second treatment group that received information about the performance of municipal schools, which we analyze elsewhere (omitted self-citation). Assignment was block-randomized at the census tract level.

Our outcome variable, *vote*, was measured during a second wave of the survey that was fielded 2–4 weeks after the election and reinterviewed 2,577 respondents. *Vote* takes on a value of 1 if the respondent reported voting for the incumbent mayor, and 0 otherwise (including abstention or a blank or null vote). Nonresponse was not an issue; only one person refused to answer. To

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<sup>2</sup>We opted for a larger number of accounts approved municipalities in order to minimize clustering and the impact of our intervention in any one locale.

reduce social desirability bias and demand effects, we used municipality-specific printed ballots, which respondents were asked to deposit in an envelope carried by the enumerator; an example is contained in the Appendix. While Zucco and Power (2013) warn against the accuracy of retrospective measures of vote choice, our measure improves significantly upon those they criticize—spontaneous recall questions from the 2007 AmericasBarometer asking about vote in the 2002 and 2006 elections. We measure vote choice a few weeks after the election, and our paper ballots provided candidate names, party affiliations, and photos, all of which are used in Brazil’s electronic voting system. As shown in the Appendix, comparing the vote distribution in the sample to the corresponding population figure suggests that respondents both accurately recalled and honestly reported whether they voted for the incumbent mayor.

## 4.2 Vignette Experiment

To facilitate a direct comparison of field and vignette experiments, we replicated the vignette experiment analyzed in Weitz-Shapiro and Winters (2017) and Winters and Weitz-Shapiro (2016), substituting our accounts rejection treatment for their bribery treatment and using the original Portuguese-language text for everything else. Our vignette experiment thus presents the following scenario:

Imagine that you live in a neighborhood like yours, but in a different city in Brazil. Let’s call the mayor of the city where you live Carlos. Now imagine that Mayor Carlos is running for reelection. During the four years that he was mayor, the city had various improvements, with economic growth and improved public health and public transport services. *Also in that city, the State Accounts Court rejected the accounts of Mayor Carlos in the year 2013 because it found serious problems in the administration of the budget.*

Respondents were then asked how likely they were to vote for Mayor Carlos, on a four-point scale. Comparing treatment effects on this outcome to those in the field experiment requires dichotomiz-

ing the scale, which we do by treating “a great chance” and “some chance” as indicating a vote for the incumbent (as in Winters and Weitz-Shapiro, 2013). In the Appendix, we show that similar results are obtained with alternate ways of measuring vote choice.

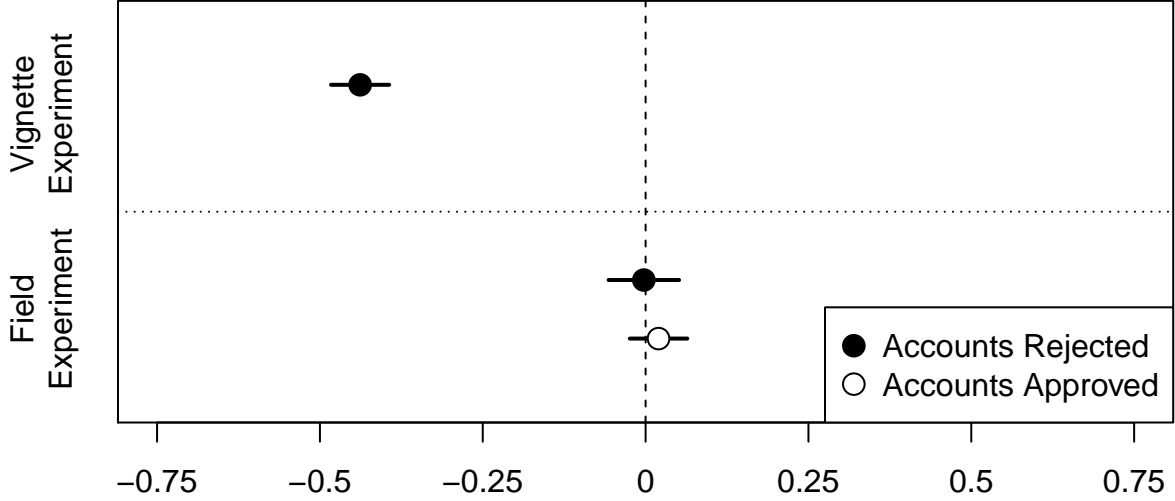
Though our vignette experiment treatment conveys a generic reason for accounts rejection, while our fliers did not, the same language from the vignette was used immediately prior to treatment delivery in the field experiment. When measuring respondents’ pre-treatment knowledge of the mayor’s accounts status, respondents were told that “generally, the accounts are rejected if the Court finds serious problems in the administration of the budget.” Hence, common reasons for rejection form part of the informational context in which the field experiment treatment is delivered. This design makes the two treatments more comparable than if we had listed actual reasons for rejection, which vary by municipality, on each flier.

To ensure comparability of the field and vignette experiments while avoiding contamination between the two, we examine vignette experiment treatment effects only for respondents who live in municipalities where the mayor’s accounts were rejected but who never received a flier with this information. We thus ensure that the respondents used to estimate the effect of information about accounts rejection in the field experiment are valid counterfactuals for those used to estimate the effect in the vignette experiment, since each respondent could have ended up in either group but not both. In the Appendix, we report the effect of the vignette experiment in the full sample, which is somewhat smaller but would not alter our conclusions.

## **5 Results**

For simplicity and consistency with most prior studies, we estimate average treatment effects as mean differences, controlling only for block fixed effects. Specifically, we use an estimating equation with a treatment dummy, demeaned block dummies, and their interaction, which is consistent

**Figure 3: Average Treatment Effects on Vote for Mayor**



NOTE: Lines give two-sided 95% confidence intervals. N = 619 (vignette experiment), 818 (field experiment, accounts rejected), and 847 (field experiment, accounts approved).

for the average treatment effect when treatment probabilities vary by block (Lin, 2013):

$$Y_i = \beta_0 + \beta_1 \mathbf{T}_i + \sum_{k=1}^{K-1} (\mu_k B_{ki} + \gamma_k B_{ki} \cdot \mathbf{T}_i) + \epsilon_i \quad (1)$$

$Y_i$  is the outcome variable for individual  $i$ ,  $\mathbf{T}_i$  is the treatment indicator,  $B_{ki}$  is the  $k$ th demeaned block (or census tract) dummy,  $\mu_k$  is the  $k$ th block effect,  $\gamma_k$  is coefficient on the interaction between the demeaned block and treatment dummy, and  $\epsilon_i$  is the disturbance term. For the standard error of our estimates, we employ the “HC2” heteroskedastic consistent estimator. In the Appendix, we present similar results obtained when controlling for a vector of pre-treatment covariates chosen by the data-adaptive Lasso procedure, as specified in our pre-analysis plan. We also present checks for covariate balance and differential attrition by treatment status.

Our results, summarized graphically in Figure 3, show a clear contrast between voters’ behavior in the context of a hypothetical vignette versus real life. In the vignette experiment, telling respondents that Mayor Carlos’s accounts were rejected reduces the likelihood of voting for him by 44 percentage points. As shown in the Appendix, this estimate is statistically indistinguishable from



that obtained by Weitz-Shapiro and Winters (2017).

By contrast, in the field experiment, informing respondents about either the approval or the rejection of their mayor's accounts has no significant effect on voting behavior. Though in the expected direction, these effects are substantively small and statistically insignificant. In particular, the estimated effect of the accounts rejection treatment on vote for the incumbent is almost exactly zero.<sup>3</sup>

In sum, our study provides clear evidence that voters in Pernambuco, like those in the rest of Brazil, respond to the strong norms against corruption and malfeasance in hypothetical scenarios. However, when it comes to real-world voting decisions, those norms do not translate into action at the polls.

## **6 Explaining the Divergence Between Norms and Action**

Why do norms regarding the punishment of malfeasant officeholders not influence voters' behavior on Election Day? We argue that the divergence between norms and action is primarily attributable to two factors. First, while corruption is seen as a major problem at the national level, malfeasance by local officials is a particularly low-salience concern compared to health, job creation, and other issues that people directly experience in their day-to-day lives. Second, while mass partisanship is weak in Brazil, attitudes toward local political dynasties often serve as a functional equivalent to strong party identification, limiting the potential for information to change voting behavior. The analysis below goes beyond specific statistical tests described in the pre-analysis plan, though it does speak to our general hypothesis that "information will have a larger effect when respondents place more importance on the corresponding issue area" (omitted self-citation).

In this section, in addition to electoral results and survey data, we leverage several sources of

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<sup>3</sup>As discussed elsewhere, positive and negative news also have null effects on turnout (omitted self-citation).

qualitative evidence. For the fourteen municipalities listed in Table 1—all seven with rejected accounts, and another seven, largely similar in terms of population, region, and electoral competitiveness, where the mayor’s accounts had been approved—we had Brazilian research assistants write background reports on the local political climate and campaign dynamics. In three of these municipalities—Tabira, Flores, and Itaíba—we commissioned post-electoral focus groups with local residents. One of us attended these focus groups as an observer; the discussion below draws upon our own notes as well as reports prepared by the survey firm.

**Table 1:** Case Study Municipalities

Region	Municipality	Accounts	Population	Vote Margin
West	Bodocó	Approved	35,158	4.1
West	Santa Maria da Boa Vista	Approved	39,435	3.7
West	Santa Filomena	Rejected	13,371	3.9
West	Trindade	Rejected	26,116	3.1
North	Sertânia	Approved	33,787	8.6
North	Tabira	Approved	26,427	2.1
North	Custódia	Rejected	33,855	24.7
North	Flores	Rejected	22,169	4.1
East	Primavera	Approved	13,439	12.1
East	Gameleira	Rejected	27,912	14
South	Caetés	Approved	26,577	18.4
South	Pedra	Approved	20,944	9.4
South	Bom Conselho	Rejected	45,503	27.1
South	Itaíba	Rejected	26,256	9.2

## 6.1 Unlikely Explanations

As discussed in section 2, a variety of factors could potentially explain why effects in a vignette experiment are larger than in a field experiment. We argue that the divergence between norms and action is unlikely to be attributable to uncompetitive elections, weak incumbents, the self-selection involved in standing for reelection, voters’ assumptions that all politicians break the law, or differences between the research design of the vignette and field experiments.

We found little evidence that uncompetitive elections should have limited treatment effects in the

field experiment. The median margin of victory in the 2016 mayoral elections in all of Brazil was 11.7 percentage points. Pernambuco was somewhat more competitive, at 10 percentage points; our 47 sampled municipalities were even more competitive, at 9.4 percentage points; and the seven accounts rejected municipalities were the most competitive of all, at 9.2 percentage points. While a lead of this size might feel comfortable in a heavily-pollled presidential election, there are few published surveys of vote intention in small towns, so residents have little basis for deciding that the race is wrapped up and their vote does not matter.

We also found little evidence that incumbents with rejected accounts had unusually low baseline levels of support, which might limit the potential for treatment effects. While mayoral approval at baseline is significantly higher in municipalities with approved accounts, the difference is relatively small: 0.15 points on a 5-point scale, or about one-tenth of a standard deviation. We can also compare mayors in terms of their change in vote share vis-à-vis the prior election to see if voters punished those with rejected accounts more severely, perhaps due to other aspects of poor performance in office. As shown in the Appendix, the seven incumbents with rejected accounts are similar to other rerunning incumbents in terms of the relationship between their vote share in 2012 and in 2016.

One might suspect that self-selection into the sample of candidates—i.e., the strongest incumbents with rejected accounts choosing to run for reelection, while the more vulnerable ones opt out—accounts for our null finding in the field experiment. Prior studies of Brazil have shown that being accused of corruption reduces the likelihood of running for reelection (Jucá, Melo and Rennó, 2016; Pereira, Rennó and Samuels, 2011; Rennó, 2008). We find a similar bivariate relationship among mayors in Pernambuco: of 13 first-term mayors whose accounts were rejected, 6 chose not to run for reelection (46%), versus 27 out of 120 (22.5%) whose accounts had been approved or not yet judged. In a full-scale ( $N = 2000$ ) cross-sectional pilot study conducted prior to the candidate registration deadline, we included respondents from all 13 municipalities where mayors with rejected accounts were eligible to run for reelection. As shown in the Appendix, accounts-rejected mayors who bowed out were much less popular than those who chose to run

again.

Of course, strong reelection prospects do not automatically imply weak treatment effects; a popular mayor might choose to run again because she feels she has a sufficient base of support to weather the inevitable loss of votes from charges of malfeasance. Still, it is worth considering how our results might change if all reelection-eligible incumbents had been included in the field experiment.

Several pieces of evidence suggest that the lack of a punishment effect in the field experiment is not driven by candidate self-selection. The first concerns treatment effects in the pilot study. The design of the pilot was identical to that of the panel, with the exception that our vote question, asked immediately after delivering the treatment information, inquired about intended vote for the mayor if he or she were to run for reelection. As shown in the Appendix, we obtained similarly null results when informing voters about the rejection of their mayor's accounts in the pilot study. As a second piece of evidence, we can look at heterogeneous treatment effects by mayoral evaluation. As shown in the Appendix, there is no significant treatment interaction with the prior evaluation of accounts-rejected mayors; we find null effects among supporters, opponents, and fence-sitters. Taken together, both results suggest that the inclusion of a few more poorly-evaluated, vulnerable incumbents would not have changed our conclusions from the field experiment.

One might also posit that our null finding is attributable to voters assuming that all candidates—incumbent and opposition alike—are equally guilty of malfeasance. In the elections in our sampled municipalities, some opponents were quite clearly corrupt. In Custódia, the incumbent mayor's accounts were rejected in 2013, but his opponent, a former vice-mayor, had been convicted of rigging bids for municipal contracts to benefit his own company. In the vignette experiment, voters might have been willing to punish Mayor Carlos because they were asked to evaluate a single, hypothetical candidate without considering the opposition, whereas in a real election, they were weighing a choice among multiple alternatives and may have known or assumed that transgressions were committed on all sides. Alternatively, voters might perceive corruption as so pervasive, above and beyond their choices in the mayoral contest, that they cease to use it as a criterion for evaluating

politicians in general (Pavão, Forthcoming).

Several pieces of evidence suggest that assumptions of pervasive corruption cannot account for our null effects. First, as shown in the Appendix, respondents often assume their own mayor is honest, even when there is evidence to the contrary. In accounts rejected municipalities, 59% of respondents believed that their mayor’s accounts had been approved before being told otherwise, and 44% said they would be surprised to learn from a credible source of cases of corruption involving the mayor. Second, if voters’ priors are that all politicians are dishonest, we should see larger positive effects when informing them that their mayor’s accounts were approved. Yet, as shown in Figure 3, we do not. Finally, we also obtain null effects on vote intention in our pilot study, which was, like the vignette experiment, a simple referendum on the incumbent without mentioning specific opponents. Since mayoral candidates had not yet registered at the time this survey was fielded, our vote intention question only included options for the incumbent, “another candidate,” and abstention or a null or blank vote.

Finally, one might suspect that certain features of the research design account for the difference in effect sizes. In order to maintain comparability with the design of Weitz-Shapiro and Winters (2017) and Winters and Weitz-Shapiro (2016), our vignette experiment contained details—the name of the (hypothetical) mayor, and the reason for rejection of accounts—that were not included in the flier. The vignette experiment also described several positive features of the mayor’s tenure, whereas the flier did not. And the vignette experiment measured the outcome of interest immediately after treatment delivery; in the field experiment, several weeks elapsed, potentially allowing the effect to decay.

We believe that these differences in research design are unlikely to account for much of the difference in effect size. As discussed in section 4, both the name of the mayor and similar language about common reasons for the rejection of accounts were conveyed in the survey just prior to treatment delivery. Moreover, several Brazilian vignette experiments that did not name the hypothetical mayor obtained similar results (Avenburg, 2016; Winters and Weitz-Shapiro, 2013). While characterizing Mayor Carlos as otherwise competent might have raised baseline vote inten-

tion and allowed for a larger treatment effect, prior vignette experiments have found that Brazilian voters punish incompetent mayors for corruption almost as much as competent ones (Winters and Weitz-Shapiro, 2013). As noted above, we also find no variation in the null effects of the field experiment based on how competent respondents considered their own mayor. Finally, with respect to the elapsed time between treatment delivery and realization of outcomes, our full-scale pilot asked about vote intention immediately after providing information about the mayor, yet we still obtained null effects.

## 6.2 Likely Explanations: Issue Salience and Dynastic Politics

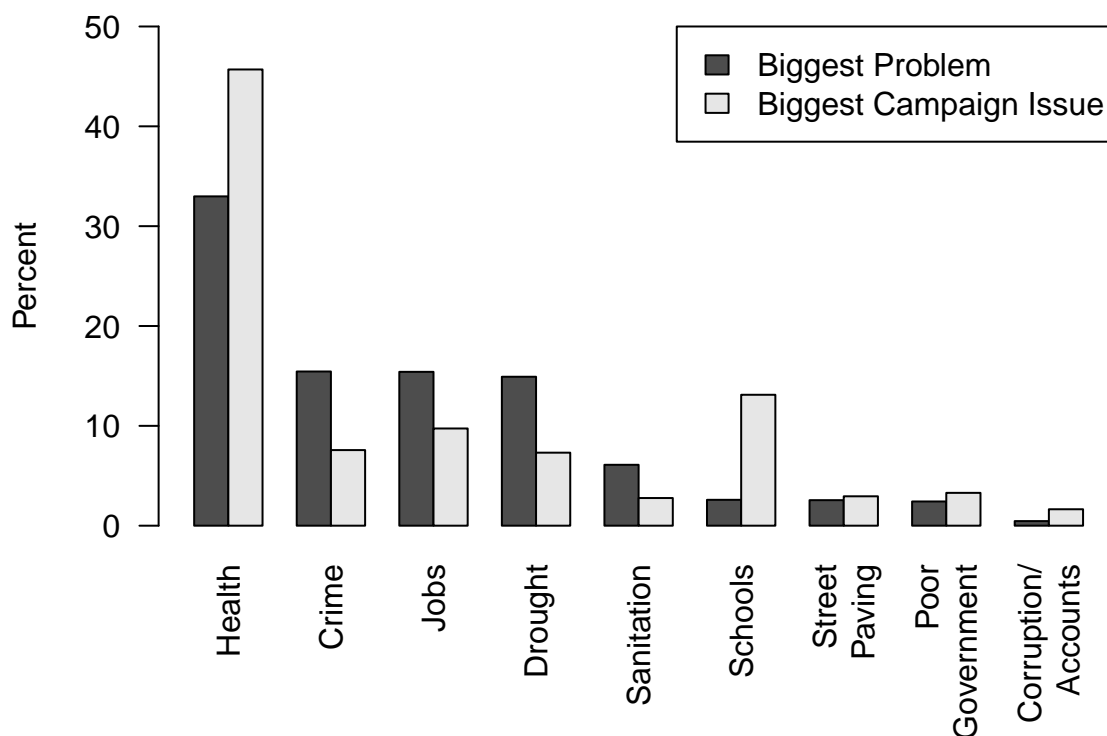
Rather than uncompetitive elections, unpopular incumbents, the self-selection of rerunners, voters' assumptions of pervasive malfeasance, or features of the research design, we argue that the divergence between norms and action is attributable to tradeoffs with more salient performance criteria and voters' attitudes toward local political dynasties. Below, we examine evidence for each explanation.

First, while Brazilians often list corruption as the country's biggest problem, this issue is much less salient in municipal politics. In the baseline survey, we asked respondents to name the biggest problem in their municipality, and in the endline survey, we asked what issue candidates had most discussed during the campaign. Figure 4 shows the results for those issues in the top ten on both lists, plus the issue of corruption or accounts management. The most frequently mentioned issues are those that impact people directly on a day-to-day basis—health services, crime, employment, and dealing with a severe drought. Municipal corruption and malfeasance are clearly at the bottom of the priority list for both voters and candidates.<sup>4</sup> In part, this finding may reflect the fact that

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<sup>4</sup>We might expect a larger treatment effect among the few voters who do prioritize municipal corruption or malfeasance. Treatment effects do appear larger in this subgroup, though the sample size is so small—37 in accounts rejected municipalities and 26 in accounts approved municipalities—that these effects cannot be estimated with any precision.

**Figure 4: Biggest Problem and Biggest Campaign Issue in the Municipality**



Brazil does comparatively well at controlling petty corruption—e.g., needing to pay a bribe to access a basic service—even as it struggles with massive schemes at the national level (Pring, 2017).

Evidence from the focus groups accords with these findings from our survey. Asked about problems in their municipality, participants most often mentioned poor employment prospects, especially in agriculture, which has been severely affected by recent droughts. There were also major complaints related to health services, such as a shortage of doctors and medications in local clinics and needing to travel outside of town for emergency care. Issues related to corruption and municipal accounts never arose spontaneously, even in municipalities where the mayor’s accounts had been rejected. When asked about the quality of the municipal government’s “financial management,” a term used in the survey to refer to the status of the mayor’s accounts, participants talked instead about whether the municipal government paid public servants on time—a major issue in places where the town is a major employer but budgets often run short.

Following the political behavior literature on “attitude importance” (e.g., Boninger, Krosnick and Berent, 1995; Krosnick, 1990; Krosnick, Berent and Boninger, 1994) as well as the spatial modeling literature on candidate valence (Enelow and Hinich, 1982), we argue that information about municipal malfeasance is likely to carry relatively little weight in an individual’s voting calculus, given the greater salience of more tangible performance criteria. Our argument encompasses the familiar notion of “rouba mas faz,” or “he steals but he gets things done”—voters are likely to excuse the transgressions of a mayor who delivers in terms of health services and job creation. Yet it is also more general. If a mayor gets *nothing* done, his poor performance in salient areas is likely to push voters toward support of the opposition; additional information about malfeasance should make little difference in their voting decisions.

A second explanation for the divergence of results between the vignette and field experiments concerns aspects of the broader political environment that might limit the effects of information—even about salient issues—on voting behavior. In advanced democracies, strong partisan attachments are a traditional explanation for why information gleaned during campaigns often has limited effects on how people vote (Berelson, Lazarsfeld and McPhee, 1954; Campbell et al., 1960; Converse, 1962; Zaller, 1992). In Brazil, as in many newer and developing democracies, mass partisanship is much weaker, leaving more room for informational effects on voting behavior (Baker, Ames and Renno, 2006). At the start of 2016, only around 30% of Brazilians identified with a political party (Samuels and Zucco, Forthcoming); in our survey, the figure was 26%. Moreover, given Brazil’s vast array of parties (28 won seats in the 2014 congressional election) and the differences between national and local-levels patterns of competition, a partisan preference does not necessarily provide voters with a clear choice in local elections. In our survey, only 16% of voters identified with a party that was running a candidate for mayor in their town.

While levels of traditional partisanship may be low in Pernambuco, dynastic politics serves as a functional equivalent in many towns. In the majority of our fourteen case study municipalities, one or more of the principal candidates for mayor in 2016 was a close relative of a former mayor in that municipality. In some instances, candidates’ families had dominated municipal politics for

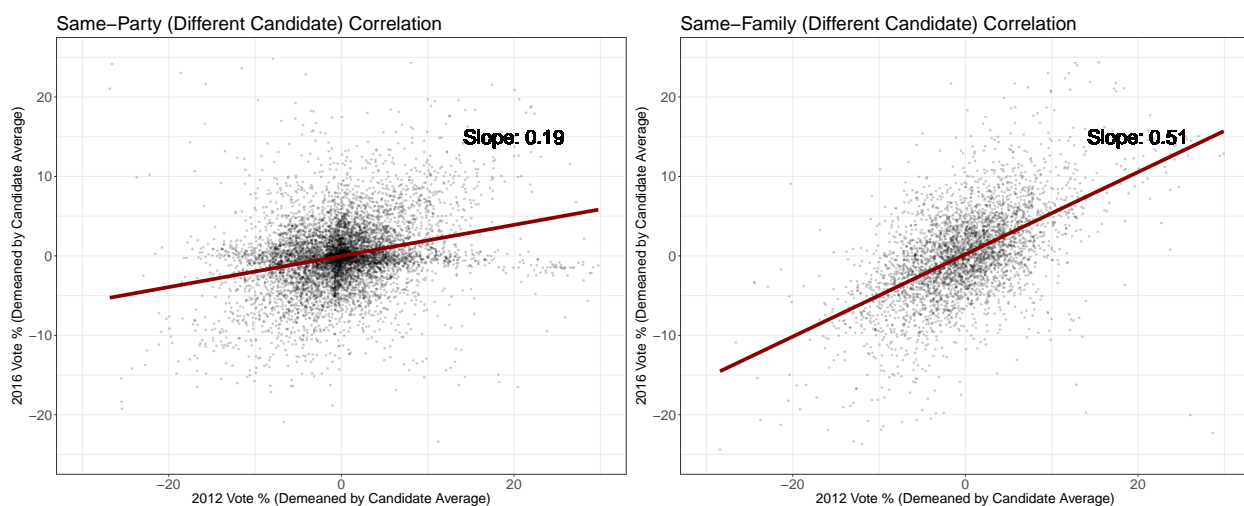


decades. In Gameleira, all but one mayor from 1988 to the present was from the two families that presented the major candidates in 2016. In Flores, challenger Marconi Santana, a two-term former mayor himself, was related to seven prior mayors in the town. Candidates' campaign strategies often make these family ties explicit. For example, in Custódia, the son of a former mayor who had gone by the nickname "Zé do Povo" ran as "Manuca de Zé do Povo." Oftentimes, dynastic candidates are widely seen as stand-ins for former mayors who cannot run again due to term limits or disqualification. For example, in the Tabira focus group, one participant explained that a candidate who was the wife of a former mayor was jokingly referred to as "the mute" during the campaign because "she never spoke... he was the one who spoke."

To measure quantitatively the degree of family dominance of local politics in Pernambuco, we examined the extent to which mayoral candidates in 2016 had family relationships with candidates in 2012. Using Internet searches, we investigated all 2012–2016 candidate pairs from the same municipality that shared at least one surname. We found that 24% of municipalities had at least one mayoral candidate in 2016 with a family tie to a candidate in the previous election. This approach almost certainly understates family dominance of local politics in Pernambuco, as it only looks for family matches in 2012; first-term mayors are likely to run for reelection before passing the torch to a relative. In the Appendix, we discuss similar results from an alternative measurement strategy.

In many towns, attitudes toward political dynasties serve as a functional equivalent to partisanship, leading voters to make up their mind about the election well before the campaign. Local political groups sometimes maintain a consistent partisan affiliation, but often they do not. In Flores mayoral elections from 1988 to the present, members of the Santana clan have run with four different parties. Yet focus group members often used the term "party" to refer to voters' loyalty to these groups and the stability of political competition among them. According to one participant in Flores, "all my life it's been two parties, either one of them has 5000 votes guaranteed, and there are 2–3000 votes left for them to dispute... the candidate can be Joe Nobody, he enters and gets 5000 votes." In Tabira, another participant said that "whoever votes for that party never ceases to

**Figure 5: Vote Share Over Time: Same-Party versus Same-Family Correlations**



NOTE: Left plot excludes pairs of candidates belonging to the same family.

be [loyal]. . . it’s a real tradition. They are people that put on the shirt of their team and never take it off.”

As a measure of the extent to which political dynasties structure vote choice, we examine the over-time correlation in electoral results for families versus parties. Figure 5 presents a scatterplot of precinct-level vote share in 2012 versus 2016 for distinct candidates belonging to same party (left panel) and those belonging to the same family (right panel).<sup>5</sup> We normalize vote share by subtracting off the municipality-specific party or family average to account for the fact that different parties and families have different overall levels of support across municipalities. The thick line is an OLS best fit for the normalized data. The over-time correlation in votes for candidates belonging to the same party is substantially weaker than the correlation between candidates belonging to the same family.

We are agnostic as to the particular psychological mechanisms underlying the effect of dynastic attitudes on voting behavior. As with party affiliation, a candidate’s membership in a local political family might serve as a cue of competence or policy positions or as a proxy for clientelistic net-

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<sup>5</sup>We exclude same-party candidates who are in the same-family sample.

works. Attitudes toward local dynasties might prompt motivated reasoning—for example, a belief that charges of corruption are fabricated by the opposing family—or simply constitute a strongly held preference that cannot be moved by even credible evidence of malfeasance. The key point is that, as with partisanship in established democracies, opposition or loyalty to local dynasties can lead many voters to make up their minds in advance, leaving less room for information gleaned during the campaign to influence voting behavior.

Our argument about dynastic politics implies that the effect of information on voting behavior should be larger in places not dominated by family dynasties. As shown in the Appendix, informing voters that the mayor’s accounts were approved has a significant positive effect on vote intention in non-dynastic municipalities (as operationalized above) but a null effect in dynastic municipalities. Unfortunately, there is little variation in our measure of dynastic politics in the seven municipalities with rejected accounts, so we cannot test the hypothesis for these respondents with any precision.

## **7 Conclusion**

In the context of a strong norm against corruption, survey vignette experiments have shown that Brazilians are willing to punish incumbent politicians when confronted with hypothetical corruption scenarios. In this paper, we show that they act similarly when presented with vignettes in which a mayor was charged with malfeasance by the State Accounts Court. Clearly, respondents are not interpreting the news about Mayor Carlos as a form of creative accounting that should be rewarded, nor are they dismissing it as a harmless administrative error. In the abstract, Brazilian voters sanction mayoral malfeasance.

Yet our field experiment also shows that Brazil’s strong anti-corruption norm fails to translate into action at the polls in a real municipal election. Informing voters of the acceptance or rejection of their mayor’s accounts has no effect on the decision to vote for the mayor’s reelection. We argue that the divergence between norms and action can be attributed to the greater salience of more tangible concerns such as job creation and health services, as well as the degree to which attitudes

toward local political dynasties structure voting decisions.

In highlighting the greater weight of tangible, everyday concerns in voters' evaluation of municipal politicians, our study underscores that information about incumbent performance must be salient if it is to facilitate electoral accountability. In separate work emerging out of this project, we have reached similar conclusions. When informed about their municipality's record in hiring public health workers to combat mosquito-borne illnesses, only respondents who personally know someone affected by microcephaly or the Zika virus choose to punish poor-performing mayors (omitted self-citation). Likewise, only parents of children enrolled in municipal schools, for whom information about educational performance ought to be most salient, vote against incumbent mayors when informed of poor standardized test scores in the municipality (omitted self-citation). Information *can* prompt electoral accountability among those for whom it matters most. The challenge of corruption and malfeasance is that they impose diffuse costs on society; people's personal stake in the issue rarely approaches that of a parent whose child is enrolled in a failing school or someone whose best friend's baby was borne with microcephaly.

Our findings also cast doubt upon the ability of horizontal accountability institutions to induce vertical accountability through public information campaigns. Dissemination of informational fliers signed by an impartial government agency is a relatively "low-dose" treatment compared to opposition political campaigns or denunciations in the media. On their own, auditing agencies are unlikely to do much more than was done in our study—provide factual information to the public and let citizens draw their own conclusions—whereas opposition campaigns will repeat charges ad nauseam, embellish them with innuendo, and generally increase the dosage of an information treatment. Journalists will often do the same—a potential explanation for the divergence between our findings and those of Ferraz and Finan (2008), who show that negative audits have large negative effects on incumbents' reelection prospects in municipalities with local radio stations. Widespread dissemination of information may further amplify its effects by facilitating coordination among citizens, who might be reluctant to sanction incumbents when acting alone (Adida et al., 2016).

Finally, our study makes clear that vignette and field experiments have different strengths and

weaknesses for estimating electoral accountability effects. Field experiments are clearly the best option for understanding how information about incumbent performance affects voters in a real election. But vignette experiments, by offering insights into societal norms, have their advantages as well. Electoral accountability effects as estimated in vignette experiments may provide a useful upper bound or goal for reformers seeking to create environments in which voters are unconstrained by dynastic politics, overriding material necessities, or other factors and can freely punish incumbents for official malfeasance. Moreover, vignette experiments offer advantages for estimating heterogeneous treatment effects—how electoral accountability varies according to source credibility, co-partisanship, different types of corruption, or other factors. Variables such as these are difficult or impossible to experimentally manipulate in the real world, so vignette experiments may be our best bet for learning about the moderators of electoral accountability, even if the magnitude of these effects differs in real life.

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