

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

Appendix

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Contents

1	Relationship between Analysis and Pre-Analysis Plan	1
2	Confidence in the State Accounts Court of Pernambuco	3
3	Example Fliers	3
4	Legality of the Intervention	4
5	Reasons for Accounts Rejection	5
6	Example Ballot	6
7	Vote Distribution: Sample versus Population	6
8	Vignette Experiment: Alternative Estimation Strategies and Samples	7
9	Field Experiment Results	7
10	Covariate Balance and Attrition	9
11	Mayoral Vote Share	10
12	Mayoral Approval and the Decision to Run Again	11
13	Effects on Intended Vote in the Pilot Study	11

14 Heterogeneity by Evaluation of the Government	11
15 Are Assumptions of Mayoral Malfeasance Pervasive?	12
16 Alternative Measures of Dynastic Politics	13
17 Heterogeneity by the Presence of Political Dynasties	14

1 Relationship between Analysis and Pre-Analysis Plan

As explained in the main text, our pre-analysis plan (PAP), 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. This section elaborates on the PAP and its relationship to the analysis we conducted.

As part of EGAP’s Metaketa Initiative on Information and Accountability (Metaketa I), our research design was coordinated with that of six other teams conducting similar projects on information and accountability around the world. These projects were on different timelines, according to the electoral calendar of each country; Brazil’s 2016 election was the last to occur. Each of the Metaketa project teams agreed to file a PAP before the results of any of the projects was publicly presented. In our case, this meant filing the PAP nearly a year before the election, and well before the most important details of our research design were worked out. Hence, on November 18, 2015, we filed “Information and Accountability in the Brazilian Northeast: A Preliminary Pre-Analysis Plan” with EGAP (<http://egap.org/file/1026/download?token=zk0TXQHm>), which outlined the basic approach of the project and specified that we would file our full PAP at a later date.

Our full PAP was filed on November 27, 2016 (Boas, Hidalgo and Melo, 2016). This was after the Brazilian municipal election of October 2016, but before we had received the outcome data from the survey firm. We told the firm that we intended to file the PAP on November 27, 2016 and asked that they not deliver the data prior to that date; they sent it to us the following day.

Our PAP contained a number of hypotheses related to different aspects of our research project, many of which are analyzed elsewhere (Boas, Hidalgo and Toral, 2018; Boas, Hidalgo and Melo, Forthcoming). The hypotheses that are directly relevant to the present article are the following (Boas, Hidalgo and Melo, 2016, 15–16):

Hypothesis 1a: Positive information relative to priors increases voter support for the incum-

bent.¹.

Hypothesis 1b: Negative information relative to priors decreases voter support for the incumbent.

Hypothesis 3: Negative or positive information will have a larger effect when respondents place more importance on the corresponding issue area.

Hypothesis 6: For respondents in municipalities where the mayor’s accounts were rejected by the TCE, the effect of this information on self-reported vote choice is smaller than the equivalent effect on intended vote in the context of a vignette experiment.

As is clear from hypotheses 1a and 1b, we did not hypothesize a null effect from the field experiment. Rather, consistent with the Metaketa Initiative and its Meta-Pre Analysis Plan (Dunning et al., 2015), with which ours was coordinated, we hypothesized that positive or negative information would have non-zero effects on voting behavior. Hence, our null findings for the field experiment do contradict these hypotheses. In this respect, our discussion of why malfeasance-related information has a null effect on voting behavior in real life represents a post-hoc interpretation of results.

On the other hand, we consider the most important hypothesis for the present article to be hypothesis 6, that the vignette experiment would yield larger effects on voting behavior than the field experiment. The theoretical section of the main text (“Malfeasance and Electoral Accountability: Prior Findings”) does not argue that the effect of malfeasance information on voting behavior should be zero in a field experiment, but simply that it should be smaller than the corresponding effect in a vignette experiment. The same is true of the theoretical section of our PAP (Boas, Hidalgo and Melo, 2016, 7). A null effect of negative information in the field experiment, which we found, is consistent with the field experiment effect being smaller in magnitude than the equivalent effect in the vignette experiment. Hence, the PAP, the theoretical expectations stated in the main

¹The phrase “relative to priors” was included only for consistency with the broader Metaketa Initiative. As explained in the PAP, given our binary treatment information and the way that “good news” and “bad news” are defined in Metaketa I, approval of accounts is always positive information relative to priors, and rejection of accounts is always negative information relative to priors

text, and the results are all aligned with respect to this crucial hypothesis regarding the relative magnitude of effects.

In terms of statistical tests, the analysis reported below, in section 9, exactly adheres to the specification from the PAP. The simpler specification presented in the main text is consistent with these results and supports the same conclusions. Regarding research design and procedures, there were no deviations from the PAP, since it was filed after the baseline survey had gone to the field and the intervention had been conducted.

Finally, as explained in the main text, the analysis in the section “Explaining the Divergence Between Norms and Action” goes beyond specific statistical tests described in the PAP, though the argument of this section is consistent with hypothesis 3.

2 Confidence in the State Accounts Court of Pernambuco

As discussed in the main text, citizens place a high degree of confidence in the State Accounts Court of Pernambuco (TCE-PE). Our baseline survey asked respondents about their level of confidence in the federal government, the justice system, their municipal government, and the TCE-PE. As shown in Figure 1, confidence in the TCE-PE was significantly higher than in any of the other institutions. The same relationship holds among sympathizers of the Workers’ Party (PT), suggesting that any doubts about the independence of federal court that rejected president Dilma Rousseff’s accounts do not affect relative levels of confidence in its state-level counterpart.

3 Example Fliers

Examples of the fliers used to deliver treatment information are contained in Figure 2 (accounts approved) and Figure 3 (accounts rejected).

4 Legality of the Intervention

Some scholars have questioned the legality of conducting electoral field experiments in Brazil that do not involve partnerships with parties or candidates, given strict regulations governing campaign advertising (Cunow and Desposato, 2015; Desposato, 2015). Doubts about the legality of interventions involving fliers spring from Article 38 of Law 9504 of September 1997, which governs elections. With respect to “the dissemination of electoral advertising via the distribution of leaflets, stickers, fliers, and other printed material,” it holds that such items “shall be published under the responsibility of the party, coalition, or candidate.” However, this law does not precisely define what counts as “electoral advertising.” Those sections that come closest to a definition suggest that, to be considered electoral advertising, a message must explicitly ask for votes. Article 26, paragraph 2 states that: “The following are considered electoral expenditures, subject to registry and to the limits set by this Law . . . direct and indirect advertising and publicity, via any medium of dissemination, intended to win votes.” Likewise, Article 36-A, which governs campaigning prior to the official start date, holds that references to potential candidates “do not count as early electoral advertising as long as they do not explicitly ask for votes.” Hence, a full reading of the law suggests that fliers that do not mention voting are not subject to the limits of Article 38 because they do not count as electoral advertising.

To check our interpretation of Law 9504, we submitted a request for clarification to Brazil’s Superior Electoral Court (TSE). Their response quoted Articles 26 and 38, as cited above, indicating that these were the relevant portions of the law bearing on the question of the legality of fliers. However, they told us that they could not provide any analysis or interpretation of the law, and that for that purpose we should contact a specialist attorney.

A condition of our partnership with the State Accounts Court of Pernambuco was that they would have the opportunity to review and approve all study materials before they went to the field. We submitted drafts of the fliers, which were reviewed by a TCE-PE Councilor who is a former judge of the Regional Electoral Court of Pernambuco (TRE-PE), as well as a Substitute Councilor

Table 1: Reasons for Accounts Rejection in Pernambuco, 2010-2013.

Reason	TCE Decisions (#)	Pct
Did Not Fully Fund Pensions	182	89
Excessive Spending on Personnel	104	51
Failed to Follow Budgeting or Planning Laws	62	30
Did Not Comply with Transparency Laws or Reporting Requirements	59	29
Excessive Deficits	55	27
Did Not Spend Minimum Required on Education	52	25
Excessive Spending Before and After Elections	52	25
Failed to Follow Laws Governing Education or Health Spending	52	25
Unexplained Discrepancies in Accounts	46	23
Excessive Spending on Politician Salaries	41	20
Cites Violations Detailed in Auditing Reports	32	16
Did Not Comply with Environmental Law	17	8
Did Not Spend Minimum Required on Health	15	7
Inadquate Provision of Education or Healthcare	11	5
Other	1	0

who is a law professor. They requested several changes to the draft version which we implemented prior to conducting the study.

Based on our inquiries with relevant legal authorities and specialists, we concluded that our intervention did not violate Brazilian law.

5 Reasons for Accounts Rejection

In the main text, we note that a mayor’s accounts are most commonly rejected for forms of malfeasance that do not involve self-enrichment, and that rejections based on “smoking-gun” evidence of corruption are extremely uncommon. When the TCE recommends the rejection of a mayor’s accounts, it justifies this recommendation in a report (*paracer prévio*) that lists specific infractions uncovered by auditors. We examined these reports for all 204 cases of rejected accounts in Pernambuco from 2010–2013, classifying the reasons that were listed as justification for these decisions. Table 1 summarizes these findings; the right-hand column gives the percentage of all rejection decisions that list a given infraction.

None of the reports that we examined cited smoking-gun evidence of corruption. The most com-

mon violations—failure to fund pensions and excessive spending on personnel salaries—are forms of malfeasance that do not involve self-enrichment. Two categories, unexplained discrepancies in accounts and failure to following budgeting or planning laws, frequently contain circumstantial evidence of corruption, such as awarding a contract without the required competitive bidding process. Often, no-bid contracts are the result of corruption, such as a bribe paid to the mayor, but the details typically only emerge after a judicial investigation which, if it happened, would occur after the review of accounts by the TCE.

6 Example Ballot

An example of the secret ballot used to measure vote is contained in Figure 4.

7 Vote Distribution: Sample versus Population

Comparing the vote distribution in the sample to the corresponding population figure suggests that respondents were honest about whether they voted for the incumbent mayor and also recalled their choices accurately. To construct a population figure for comparison, we took the electoral results in each sampled municipality and weighted them according to that municipality’s share of the final second-wave sample. Results are shown in Figure 5. The biggest discrepancy between sample and population is with respect to reported abstention, which is often subject to social desirability bias. However, official abstention rates include several groups of voters that had no chance of being sampled by our survey: 16- and 17-year-olds (for whom voting is optional) and those who moved out of town without changing their registration. When excluding abstentions, there is no significant difference between sample and population in the likelihood of voting for the mayor, our outcome variable of interest. In the analysis of treatment effects, we retain reported abstentions as zeros on the vote for incumbent indicator, both to avoid conditioning on our outcome variable and for consistency with the pre-analysis plan.

8 Vignette Experiment: Alternative Estimation Strategies and Samples

In the main text, we dichotomize the four-point vote intention scale in our vignette experiment, treating “a great chance” and “some chance” as indicating a vote for the incumbent. In Figure 6, we show that similar results are obtained when considering only the most likely category as equivalent to a vote or when treating the four-point scale as a continuous dependent variable. Regardless of the approach to measurement, our estimates are large, highly significant, and statistically indistinguishable from similar quantities calculated from the replication data provided by Weitz-Shapiro and Winters (2017).

For consistency with the approach of Weitz-Shapiro and Winters (2017), our estimates in Figure 6 are simple differences in means, without demeaned block fixed effects and treatment interactions. For the “Great” or “Some Chance” dependent variable, this approach yields the same point estimate as reported in the main text, with a slightly larger confidence interval.

In the main text, we present the vignette experiment treatment effect estimate for respondents who live in municipalities where the mayor’s accounts were rejected but who never received a flier with this information. Figure 7 shows this estimate along with those for a) all respondents, and b) those who never received a flier about their mayor’s accounts, regardless of whether they were approved or rejected. While treatment effect estimates for these alternative samples are slightly smaller, they would not alter our conclusions about voters’ behavior in hypothetical situations versus real life.

9 Field Experiment Results

In the main text, we present results in graphical form for a specification that involves simple mean differences (with demeaned block fixed effects and treatment interactions to ensure that our estimator is consistent for the average treatment effect when treatment probabilities vary by block).

Table 2: ATE Estimates for the Effect of Treatment on Vote for Incumbent

	(1)	(2)	(3)	(4)
ATE Estimate	0.02	0.01	-0.00	0.01
SE	0.02	0.02	0.03	0.03
n	847	847	818	818
Accounts Status	Approved	Approved	Rejected	Rejected
Covariate Adjusted?		✓		✓

NOTE: All estimates adjust for blocks used in the randomization procedure. Covariate adjusted results include covariates selected using a Lasso regression.

In our pre-analysis plan, we specified this same approach, with the addition of also controlling for a vector of pre-treatment covariates in order to increase precision. Rather than select the covariates ex-ante, we pre-specified that we would employ a data-adaptive procedure that selects a small number of covariates from all available pre-treatment covariates based on how well they predict the outcome. By using a procedure that optimizes for out-of-sample predictive performance, we sought to maximize the efficiency of our estimates. Specifically, we follow Bloniarz et al. (2016) and use the “least absolute shrinkage and selection operator” (Lasso) to select a parsimonious set of relevant covariates to include in our estimating equation for each specification. The precise algorithm we use is the $cv(Lasso + OLS)$ adjusted estimator described in Bloniarz et al. (2016, 7388). In short, we estimate separate Lasso models in each treatment and control group. We then employ 10-fold cross-validation on the combination of the Lasso and OLS to select optimal tuning parameters for out-of-sample prediction. Finally, the non-zero coefficients in the Lasso model using the optimal tuning parameter are used in our main estimating equations. Because we use separate models for the treatment and control groups, the final set of covariates are the union of the covariates selected for each group. Eligible covariates for selection are all those listed as “moderators” and “covariates” in our pre-analysis plan.

As shown in Table 2, we obtain nearly identical estimates with both approaches, so we have opted to present the simpler set of results in the main text.

10 Covariate Balance and Attrition

Table 3: Covariate Balance Tests

Variable	Estimate	SE	Perm. P-Value
2012.000 Incumbent Vote	0.011	0.018	0.338
2012.000 Turnout	-0.044	0.014	0.017
Age	0.328	0.670	0.375
Education	-0.052	0.180	0.421
Female	-0.050	0.020	0.052
Finds Info from Surveyors Credible	0.010	0.010	0.220
Free and Fair Scale	-0.046	0.052	0.260
Prob Vote Can't Be Monitored	-0.017	0.041	0.382
Prob. Vote Buying	-0.085	0.046	0.110
Prob. Vote Count Accurate	-0.030	0.033	0.278
Relative Wellbeing	0.030	0.081	0.404
Same Party as Mayor	-0.024	0.018	0.190
Same Race as Mayor	0.050	0.022	0.074
Wants Info about Corruption	0.020	0.016	0.171

NOTE: Estimates are based on the full sample.

Covariate balance tests on pre-specified covariates are presented in Table 3. These estimates are from a model with block fixed effects run on the full sample. “Perm. P-Value” is the p-value from the hypothesis test that the treatment had no effect on the covariate whatsoever (the “sharp” null), calculated using permutation inference.

Because our survey is a panel, we also check whether the field experiment treatment is correlated with respondent attrition. The estimated effect of treatment on attrition is found in Table 4. We find a small but statistically detectable correlation between treatment status and attrition. The correlation is larger in municipalities with rejected accounts, where assignment to treatment appears to increase the propensity to attrit by about 4 percentage points.

The mechanism that most likely explains this pattern is that voters who were given negative information about the incumbent and went on to vote for him or her were more likely to refuse the re-interview. With this mechanism, the bias in our estimate would likely be in a negative direction, meaning that the true treatment effect would be even smaller than what we report.

Table 4: ATE Estimates for the Effect of Treatment on Attrition

	(1)	(2)	(3)
ATE Estimate	0.03	0.02	0.04
SE	0.01	0.02	0.02
Perm. P-Value	0.038	0.28	0.03
n	2049	1024	1025
Accounts Status	Both	Approved	Rejected

NOTE: All estimates adjust for blocks used in the randomization procedure.

While we believe it is unlikely, we should consider the possibility that attrition bias is masking a large negative effect on the propensity to vote for the incumbent. To check for this possibility, we compute “trimming” bounds (Lee, 2002) for the accounts rejected sample, which demonstrate the range of estimates possible under all forms of attrition bias when we assume that treatment never induces respondents to stay in the sample. In other words, we assume that the treatment may cause respondents to drop out, but it does not cause any respondents to remain in. Under this assumption, treatment effects are contained within the range $[-0.15, 0.10]$. While this set contains larger effect sizes (in both directions) than those that we estimate, they are still far smaller than those of the vignette experiments we compare our results to.

11 Mayoral Vote Share

As discussed in the main text, we found little evidence that incumbents with rejected accounts had unusually low baseline levels of support, which might limit the potential for treatment effects. Specifically, we 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. Figure 8 plots vote share for all rerunning incumbents in 2012 versus 2016. Those with rejected accounts straddle the regression line; on balance, they are quite similar to those with approved accounts.

12 Mayoral Approval and the Decision to Run Again

As discussed in the main text, we do find evidence that, among mayors with rejected accounts, only the politically stronger ones choose to run again. Figure 9 plots mayoral approval rates from our $N = 2000$ pilot study conducted in July 2016, prior to the candidate registration deadline. Mayors with rejected accounts who went on to run for reelection did not differ in terms of popularity from rerunners with approved accounts. However, those with rejected accounts who eventually chose not to run again were highly unpopular. Their median evaluation was “terrible” (*péssimo*), the lowest category.

13 Effects on Intended Vote in the Pilot Study

Despite evidence that only the stronger mayors with approved accounts choose to run again, we argue in the main text that this form of self-selection cannot account for our null findings. Our pilot study was conducted in all thirteen municipalities where mayors with approved accounts were eligible to run for reelection, including the six where they eventually bowed out. 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 Table 5, we obtain null results when informing voters about the rejection (or approval) of their mayor’s accounts in this study. This finding suggests that even if the potentially more vulnerable mayors with rejected accounts had chosen to run for reelection and been included in our field experiment, our conclusions would not have changed.

14 Heterogeneity by Evaluation of the Government

As an additional check on whether the self-selection involved in rerunning could explain our null results, we also examine treatment effect heterogeneity by baseline evaluation of the mayor’s ad-

Table 5: ATE Estimates for the Effect of Treatment on Vote for Incumbent in the Pilot Study

	(1)	(3)
ATE Estimate	-0.02	0.00
SE	0.026	0.0308
n	518	488
Accounts Status	Approved	Rejected

NOTE: All estimates adjust for blocks used in the randomization procedure.

ministration. If our null results are explain by the fact that only popular mayors choose to run again, we might find that voters who evaluate the mayor more negatively would react more strongly to information about malfeasance, even if the average treatment effect is close to zero. To test this hypothesis, we check for heterogeneous treatment effects in accounts rejected municipalities by baseline evaluation of the municipal government (measured on a 5-point Likert scale). As shown in the marginal effect plot in Figure 10, we find no evidence of heterogeneity, suggesting that mayoral popularity is an unlikely explanation for our null effects.

15 Are Assumptions of Mayoral Malfeasance Pervasive?

If voters assumed *a priori* that all politicians—or even just their own mayor—were guilty of malfeasance, their vote intention might not be moved by information confirming this fact. However, our survey confirms that many voters assume their mayor is honest. Table 6 shows pretreatment priors on a question about the TCE’s decision on the mayor’s accounts in 2013, as well as answers to the question “How surprised would you be if you learned from a reliable source of cases of corruption involving Mayor [NAME]?” The latter are shown for the control group only since this question was asked post-treatment. Even in municipalities where the mayors accounts were rejected, a majority assume that they were approved. Moreover, a sizable minority of voters in accounts rejected municipalities report that they would be very or somewhat surprised to learn of mayoral corruption.

Table 6: Baseline Assessment of the Likelihood of Mayoral Malfeasance

	Prior on Accounts Status		How Surprised to Learn of Corruption			
	Approved	Rejected	Very	Somewhat	Not very	Not at all
Accounts Status						
Approved	71%	29%	36%	19%	10%	35%
Rejected	56%	44%	27%	16%	11%	45%

NOTE: Priors on accounts status were measured in the baseline survey just before delivery of treatment information. The question about how surprised respondents would be to learn of credible cases of corruption involving the mayor was asked in the endline survey; responses shown are for the control group.

16 Alternative Measures of Dynastic Politics

In the main text, we identify dynastic candidates for mayor by taking all distinct candidates in 2012 and 2016 who share at least one surname (maternal or paternal) and then conducting Internet research to rule out those for whom the match is coincidental. This approach, while likely to rule out false positives, probably involves false negatives. In particular, by looking only at surname matches in 2012 and 2016, we miss those cases in which a family member ran in a prior election, including instances in which mayors from dynastic families ran for a first term in 2012 and a second term in 2016. In theory, one could extend the approach backward in time, but Internet research on potential matches is laborious and likely to be less accurate as one examines older elections.

As an alternative approach, we examined the surnames of the top two finishers in every mayoral election from 1988 to the present without conducting additional background research to rule out false positives. For each municipality, we eliminated repeat candidacies and then took the ratio of candidates with non-overlapping surnames—that is, neither their maternal nor paternal surname matches that of any other candidate—to unique top-two finishers in these elections. Figure 11 plots the distribution of this ratio for the 184 municipalities in the state; at the bottom, short thick lines show the values for the fourteen case study municipalities. In only one municipality did the top candidates for mayor share no surnames in common; in the median municipality, only 40% of these candidates had non-overlapping surnames. Obviously, candidates who share surnames do not necessarily share a close family relation. However, the plot clearly shows that the case study

municipalities, where we can observe the prevalence of dynastic politics in greater qualitative detail, are typical of the rest of the state on this measure.

17 Heterogeneity by the Presence of Political Dynasties

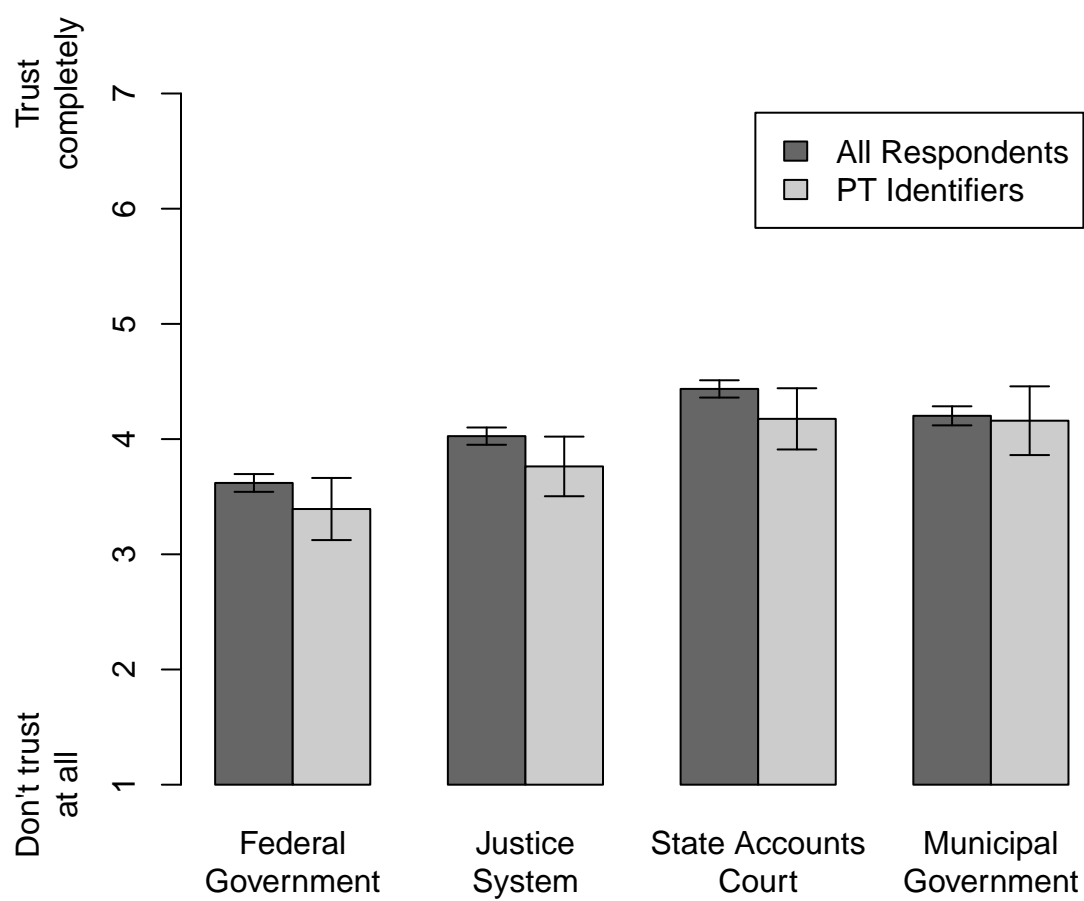
Table 7: Heterogeneity by Dynastic Politics

Sample	Estimate	t-statistic	n
Approved Accounts, Non-Dynastic	0.053	1.94	550
Approved Accounts, Dynastic	-0.042	-1.05	297

NOTE: All estimates adjust for blocks used in the randomization procedure.

As discussed in the main text, our argument about dynastic politics implies that the effect of information on voting behavior should be larger in places not dominated by family dynasties. We cannot test this hypothesis with any degree of precision in the seven municipalities with rejected accounts, since there is little variation in our measure of dynastic politics. However, we can test it in municipalities with approved accounts. These estimates are reported in Table 7. In non-dynastic municipalities, informing respondents that the mayor’s accounts were approved has a positive effect on vote for the incumbent, significant at the 0.05 level for a one-tailed test or the 0.1 level for a two-tailed test. Meanwhile, we obtain null results in dynastic municipalities.

Figure 1: Confidence in Institutions in Pernambuco



NOTE: Lines give 95% confidence intervals.

Figure 4: Secret Ballot for Measuring Vote Choice

PARA PREFEITO DE ABREU E LIMA

	<u>NOME</u>	<u>NÚMERO</u>	<u>PARTIDO</u>	
	KATIANA GADELHA	12	PDT	<input type="checkbox"/>
	FLAVIO GADELHA	15	PMDB	<input type="checkbox"/>
	PR. MARCOS JOSÉ	40	PSB	<input type="checkbox"/>
	BRANCO / NULO			<input type="checkbox"/>

Figure 5: Vote for Mayor: Sample versus Election Results

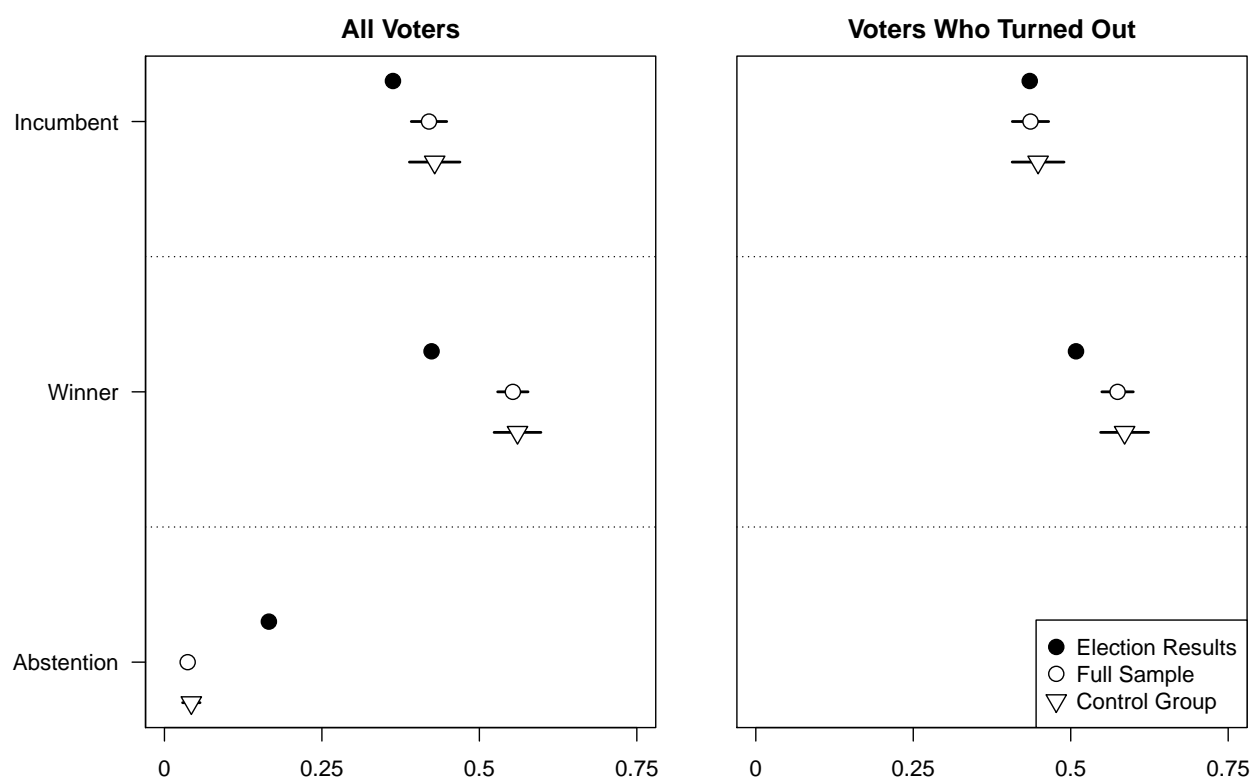
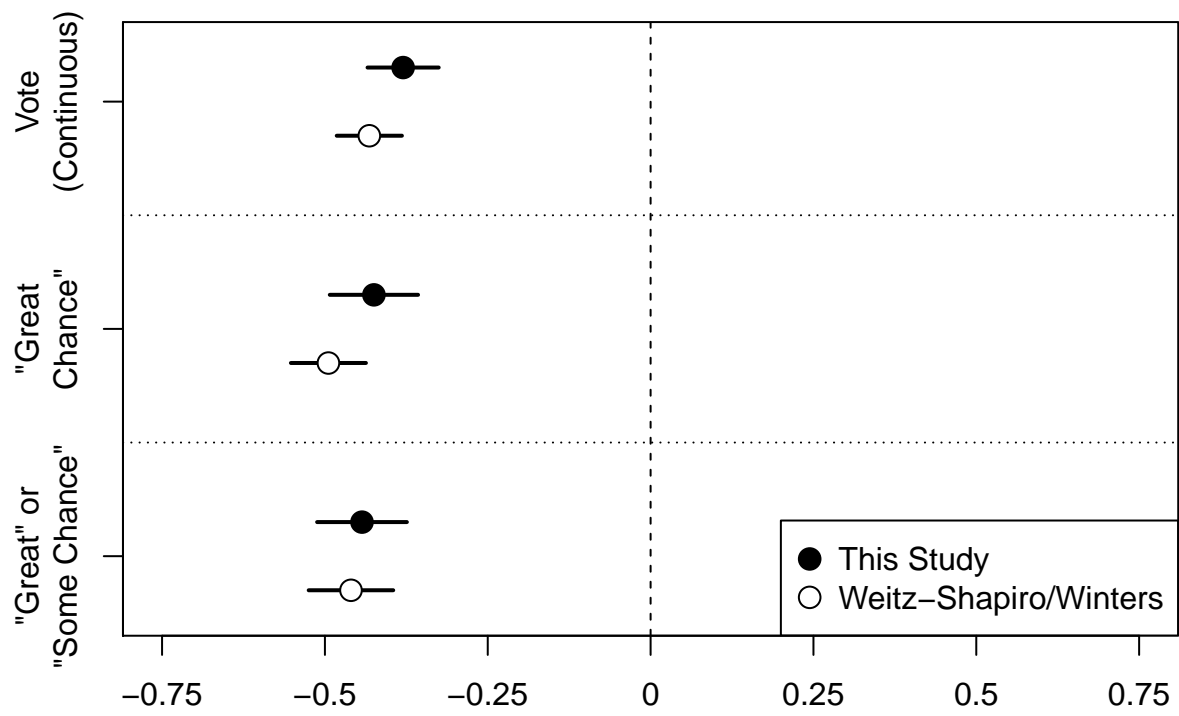
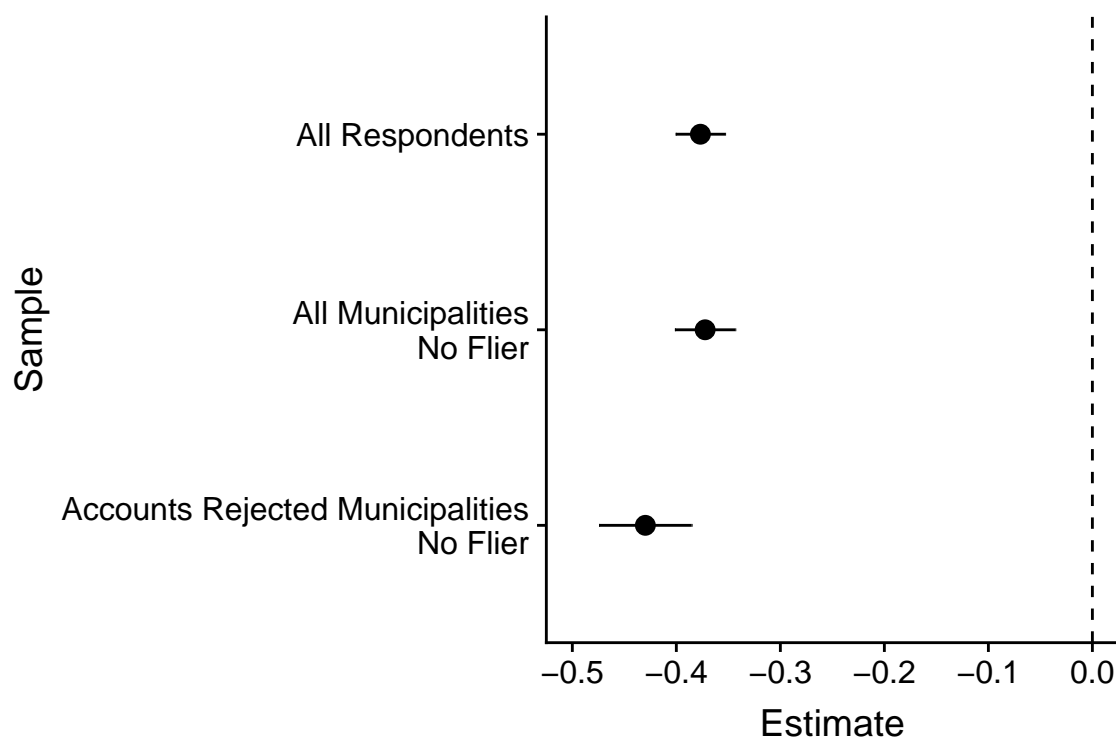


Figure 6: Vignette Experiment Effects on Intended Vote



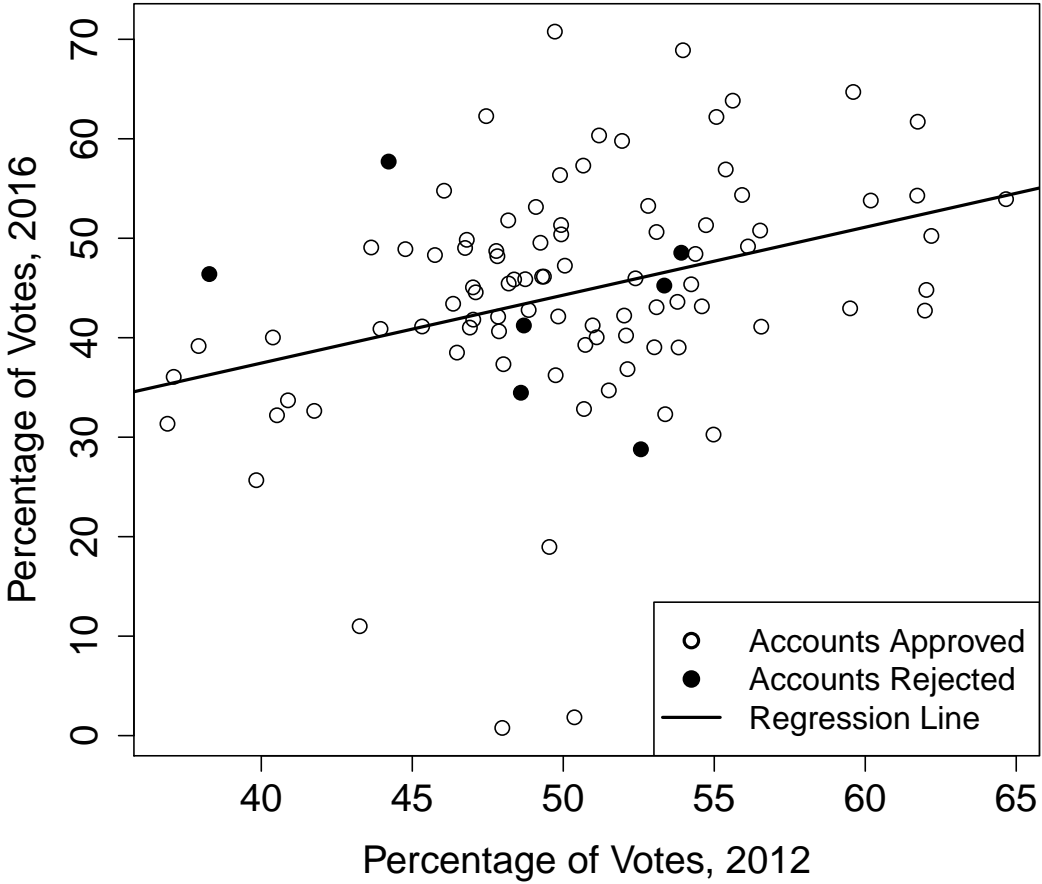
NOTE: Lines give 95% confidence intervals.

Figure 7: Vignette Experiment Effects in Alternative Samples



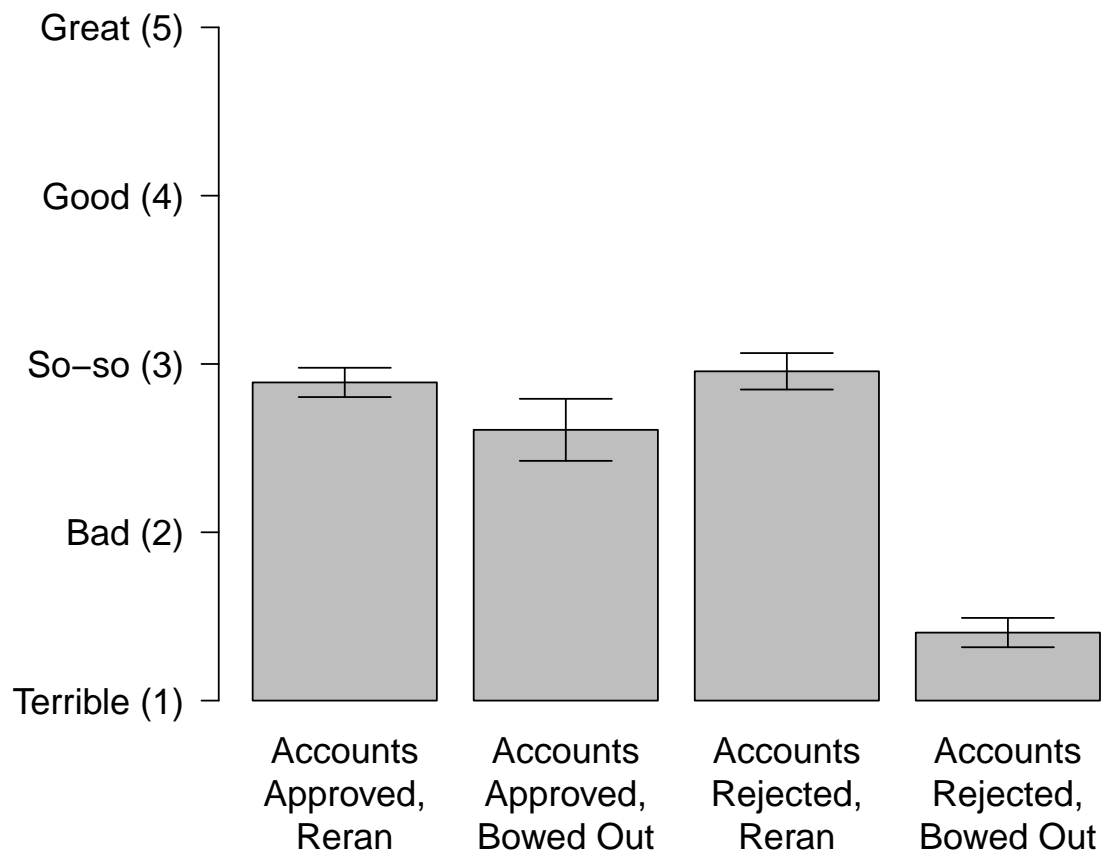
NOTE: Lines give 95% confidence intervals.

Figure 8: Mayoral Vote Share: 2012 versus 2016



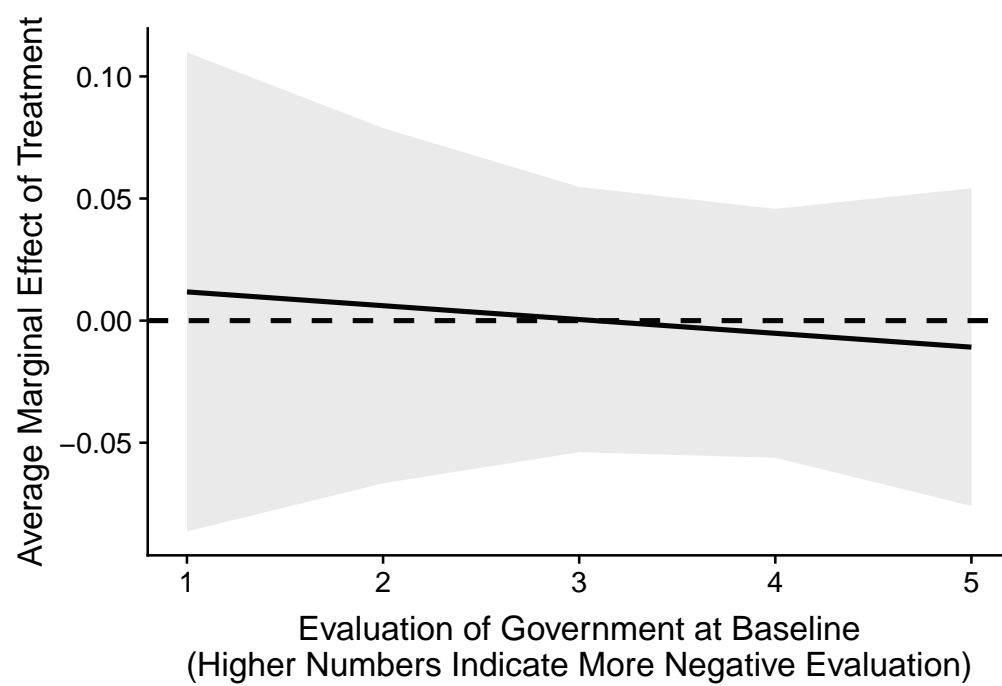
NOTE: Vote share calculated based on votes cast, including those that were subsequently declared invalid due to disqualification of a candidate.

Figure 9: Mayoral Approval by Accounts Status and Rerunning Decision



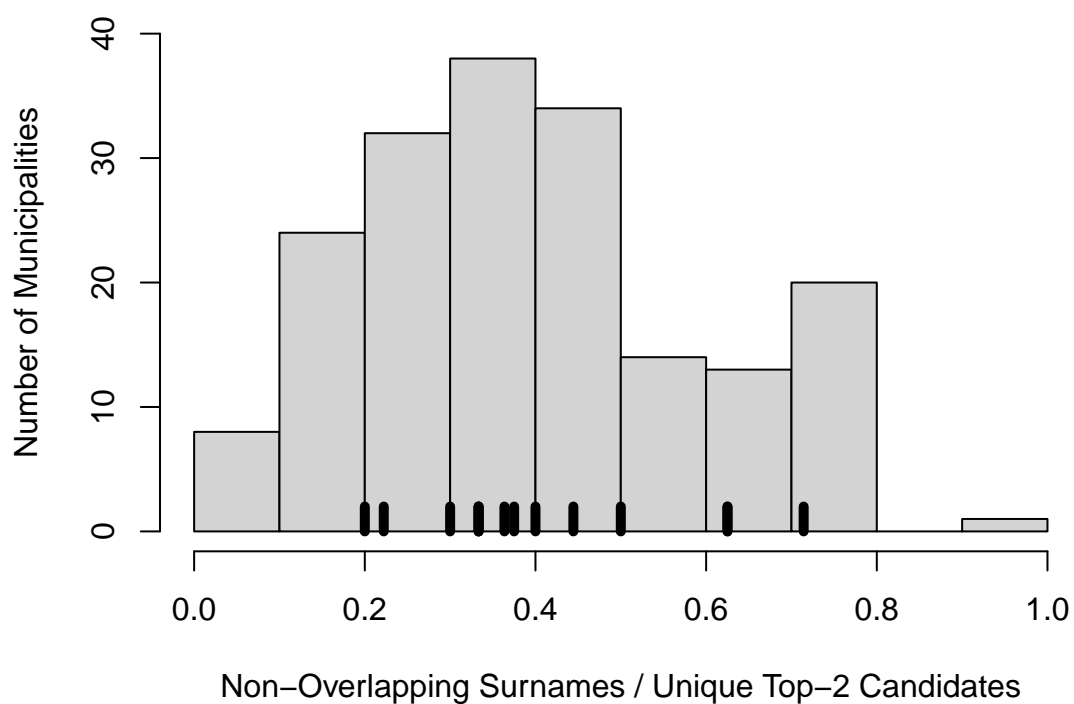
NOTE: Lines give 95% confidence intervals.

Figure 10: Heterogeneity by Baseline Evaluation of the Municipal Government



NOTE: Shaded area give 95% confidence intervals.
Sample is respondents in Accounts Rejected Municipalities.

Figure 11: Political Dynasties in Pernambuco, 1988–2016



NOTE: Short thick lines at the bottom of the plot show values for the case study municipalities listed in the main text.

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