When Do Voters Punish Corrupt Politicians? Experimental Evidence from Brazil*

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Abstract

When do voters punish corrupt politicians? Heterogeneous views about the importance of corruption can determine whether or not increased information enhances accountability. If partisan cleavages correlate with the importance voters place on corruption, then the consequences of information may vary by candidate, even when voters identify multiple candidates as corrupt. We provide evidence of this mechanism from a field experiment in a mayoral election in Brazil where a reputable interest group declared both candidates corrupt. Informing voters about the challenger's record reduced turnout by 1.9 percentage points and increased the opponent's vote by 2.6 percentage points. Informing voters about the incumbent's record had no effect on behavior. We attribute this divergent finding to differences in how each candidate's supporters view corruption. Using survey data and a survey experiment, we show that the challengers' supporters are more willing to punish their candidate for corruption, while the incumbent's supporters lack this inclination.

1 Introduction

When do voters punish corrupt politicians? The question has important implications for institutions intended to keep politicians accountable. Electoral accountability is often perceived to be an important means of reducing incentives of politicians to engage in corruption. While there is a vast literature about the consequences of corruption (Johnston, 1986; Mauro, 1995; Olken, 2005), the literature focused on the causes of non-corrupt governments is still nascent. As Adserà, Boix and Payne (2003, p. 446) succinctly state: "[i]n contrast to the mounting scholarly research on the consequences of good governance, our knowledge about what causes governments to be clean and efficient is still at its infancy." One precondition for electoral accountability is sufficient knowledge by the citizenry of politicians' records. When voters are informed about accusations of corruption, most assume that voters will punish the corrupt candidates. This paper shows that information about candidate corruption given to voters can indeed result in the politician being punished by voters, but that some candidates are more accountable to voters when it comes to corruption than others. The degree to which voters view corruption as important to their decision-making can vary substantially and, furthermore, can be correlated with political cleavages. As a result, we show that the increased transparency can have divergent partisan consequences, even when two competing candidates are corrupt. Previous studies that merely treat corruption as a valence issue are likely to overlook this important dimension of the effects that corruption information can have on the electorate.

We find that when voters view corruption as important, then the increased provision of information can induce supporters of the corrupt candidate to abstain. Our results establish that transparency can suppress turnout even in the presence of mandatory voting, demonstrating that in some cases, voters are willing to bear costs not to vote. Additionally, despite a number of papers that have shown that corruption is not a salient issue in the consciousness of many voters in the developing world (Anderson and Tverdova, 2003; Chang and Golden, 2004; Rennó, 2007; Almeida, 2008), we find, under certain conditions, it still remains an important determinant of voting behavior.[1] While a host of non-governmental organizations (NGOs), international organizations, and governments have initiated various efforts to increase transparency and government accountability in elections, few have analyzed the impact of these initiatives in terms of their effect on electoral behavior in a manner that allows one to make valid causal inferences. Our study presents a step toward accomplishing such a goal, shedding light on the conditions under which corruption may or may not be subject to voter sanction.

In this study, we conduct a field experiment during the 2008 mayoral run-off election in São Paulo, Brazil, the seventh largest city in the world. In our study, to our knowledge the first field experiment involving elections in Latin America, we exploit the fact that both candidates in the run-off election had been convicted of corruption and inform selected voters of these convictions via the distribution of fliers. We randomly assigned whether or not households in the vicinity of a given polling station receive fliers containing the information. The experimental design allows us to make inferences with a high degree of internal validity about the effect of information on voting behavior, and unlike previous studies, we are able to examine the effects not only of the incumbent, but also of the challenger.

Specifically, we take advantage of a unique set of events that took place during the election period. The Brazilian Magistrates Association (*Associação dos Magistrados Brasileiros*, or AMB) published a document called the "Dirty List" (*Lista Suja*), which listed politicians running in the 2008 elections who had convictions involving impropriety while in government office. Both candidates

[1] Winters and Weitz-Shapiro (2010), in a nationally representative survey experiment in Brazil, similarly find that voters tend to reject corrupt politicians when information about the corruption is delivered in a specific, credible, and accessible manner.

running in the election for mayor of São Paulo – Gilberto Kassab of the Democratic Party (DEM) and Marta Suplicy of the Worker's Party (PT) – appeared on the AMB's Dirty List. During the week prior to the elections, we administered two treatments: the first was a flier informing voters that Kassab appeared on the Dirty List and gave information about the nature of his conviction, and the second was a flier that did the same for Suplicy. We then randomly assigned voting precincts that would receive the Kassab or Suplicy flier, and also had a control group of precincts that did not receive the flier. In all, households in the vicinity of 100 precincts received the Kassab flier, another set of households in the vicinity of 100 precincts received the Suplicy flier, and 200 precincts were in the control group. In the week prior to the election, we hired a direct marketing firm that distributed a total of 187,177 fliers to individual households. To measure the effect of the intervention, we examined electoral outcomes.[2]

Our results varied by individual candidate. The Kassab flier had no effect on vote choice, number of spoiled ballots or on turnout. The Suplicy flier, by contrast, moved votes on average relative to the control group by 2.6 percentage points, had no effect on spoiled ballots, and a negative 1.8-1.9 percentage point average treatment effect on voter turnout. The turnout results are particularly surprising given that Brazil has mandatory voting. We believe the results of our study suggest limits to theories positing that more informed voters are more likely to turn out (Wolfinger and Rosenstone, 1980; Palfrey and Poole, 1987; Feddersen and Pesendorfer, 1996).

To explain these divergent effects measured using aggregate vote returns in our field experiment, we rely on individual-level data from a survey and an embedded experiment that we fielded the week after the election. We find that Suplicy's and Kassab's voters do not differ in intensity of support, ex-ante knowledge about the corruption accusations, or the degree to which they view the accusations as serious. We do find, however, that Suplicy's voters place much greater importance on corruption when evaluating candidates than do Kassab's supporters. Not only do Suplicy supporters claim that a candidate's record on corruption is important to them at higher rate than Kassab voters, but we also find that they are much more sensitive to corruption accusations in our survey experiment. Kassab supporters do not change their evaluation of Kassab

[2]We obtained data from the São Paulo Regional Electoral Tribunal (*Tribunal Regional Eleitoral*, or TRE).

when they learn about his placement on the Dirty List. Suplicy voters, however, do judge their favored candidate more negatively upon learning about her record, which is consistent with our field experiment findings. Overall, these contrasting results place important scope conditions on when information campaigns are likely to increase accountability through the democratic process.

2 Corruption Information and Voting Behavior

An important precondition for electoral accountability is whether or not voters have access to information about corrupt behavior of public officials, which may prompt them to vote against such candidates on election day. A theoretical literature focused on the effects of information on voting behavior concludes that under certain conditions, information improves accountability to mass publics (Alvarez, 1998; Lupia and McCubbins, 1998; Przeworski, Stokes and Manin, 1999; Besley and Burgess, 2002). However, the empirical literature is still relatively scant on understanding the conditions under which information about corruption results in electoral accountability.[3] Making valid causal inferences about information effects is difficult, in large part because availability of information about the corruption of politicians is confounded by factors such as socioeconomic status and partisanship. A number of studies with non-experimental data that attempt to examine the effects of corruption charges on electoral performance find only modest effects (Peters and Welch, 1980; McCann and Dominguez, 1998).[4] In a study of municipal governments in Brazil, Ferraz and Finan (2008), exploiting randomized corruption audits, find relatively large effects that ultimately decrease the probability of incumbent politicians being reelected. Their important work, which examines the effects municipal-level corruption audits, however, does not include candidate-specific treatments, and their intervention also only involves incumbent politicians.

Field experiments that examine the effects of corruption on voting behavior have only recently emerged in the literature. To the best of our knowledge, our experiment conducted in October

[3]Notable exceptions include Adserà, Boix and Payne (2003); Chang (2005); Reinikka and Svensson (2005); Olken (2007); Ferraz and Finan (2008); Banerjee, Green, Green and Pande (2010); Banerjee, Kumar, Pande and Su (2010), and Chong et al. (2011).

[4]One observational study that is an exception is Pereira, Melo and Figueiredo (2009), which finds large negative effects on the probability of reelection when examining the effect of state corruption audits in the state of Pernambuco, located in northeast Brazil.

2008; Banerjee, Green, Green and Pande (2010)'s and Banerjee, Kumar, Pande and Su (2010)'s studies in India, conducted in March-April 2007 and December 2008, respectively; and Chong et al. (2011)'s work, conducted in Mexico in June and October 2009, are among the first field experiments that attempt to randomize informing voters about politicians' performance in order to examine the effects on voting behavior. Focusing on the consequences of transparency for *incumbent* electoral performance, these other studies have generally found that revelations about corruption have negligible effects on incumbent vote share relative to challengers' vote share. The effects on turnout have been more mixed. The Banerjee, Kumar, Pande and Su (2010) study found that distribution of information about the criminal records of New Delhi politicians increased turnout by about 3.6 percentage points. Chong et al. (2011), on the other hand, found a negative effect of 4 percentage points when voters are given information about corruption from government corruption audits in municipalities with highly corrupt incumbents.

Revealing the corruption record of a candidate could be loosely viewed as a negative attack, even if it does not come from the opposing campaign. From this perspective, a relevant body of work is the negative campaign advertising literature, which focuses overwhelmingly on the US context. This largely observational empirical literature has been inconclusive on the consequences of negative attack ads for candidate electoral performance (Lau, Sigelman and Rovner, 2007), though no studies have focused on advertisements that emphasize corruption. With respect to political participation, Ansolabehere and Iyengar (1995), initially relying on laboratory experiments, argued that negative advertising demobilizes the electorate. One of the mechanisms they cite is particularly relevant: negative campaigns could lower the probability of voting for the target of the attacks without simultaneously increasing the probability of voting for the attacking candidate, prompting voters to simply abstain. Thus, while not necessarily provoking vote switching, negative campaign advertising could still punish targeted candidates via decreased turnout of their supporters. The observational empirical evidence for this contention, however, has been mixed (Finkel and Greer, 1998) and has not been tested experimentally by examining actual turnout behavior.

The contrasting results of these studies motivate important questions about the mechanisms

that explain variation in the voting behavior in these different contexts. Whether or not a voter actually changes his behavior due to the revelation of information will depend on a number of factors, but a useful framework for explaining heterogenous effects is the spatial model of elections (Enelow and Hinich, 1984). The spatial model underscores the important mechanism of how political factors such as ideological attachments can mediate the effect of corruption information in the minds of voters. Voters receiving a large amount of subjective utility from a particular candidate's victory ("core" supporters) will be unlikely to change their vote or abstain unless the corruption charge is particularly serious or they place a high degree of importance on corruption in their vote decision. For more marginal ("swing") supporters, however, revelation about corruption is more likely to push voters to either abstain, cast a spoiled ballot, or switch their vote to the opposing party. When the costs of voting relative to abstaining are high, then marginal supporters who learn about corruption charges are particularly likely to abstain. Following a similar logic, learning about corruption could mobilize non-voters to vote for the opposition, as the difference in utility between abstaining and voting for the opposition would decrease after the information revelation.

Under this spatial framework, one would expect that the effects of experimental interventions revealing information about corruption would depend upon the distribution of core and swing voters in the electorate, as well as the importance supporters and nonvoters place on corruption.[5] If the proportion of marginal supporters is large, then one would expect that the revelation of corruption information would induce abstention (if the costs of voting relative to abstention are high) or casting spoiled ballots (if the costs of voting relative to abstention are low) or even vote switching. Casting spoiled ballots is a particularly attractive strategy for disappointed marginal voters when fines for abstention are substantial in countries with mandatory voting. Similarly, if voters place a high degree of importance on corruption as an issue, then increased transparency could induce many core supporters to abstain, spoil their ballot, or change their vote. On the other hand, if there are many abstainers with sympathies for the opposition party, then increasing

[5]Another important background condition that could explain divergent effects is pre-existing perceptions of the candidate's propensity to engage in corruption. If voters already believe that a given candidate is corrupt or know about the specific allegations, then increased transparency is unlikely to affect behavior since voters' decisions have already incorporated this information.

information about corruption could induce these citizens to vote on election day and thus increase total turnout. The key point is that the aggregate effect of information revelation on turnout and vote shares will depend on the ex-ante distribution of voters' preferences, the relative importance of corruption in voters' decision-making, and the cost of abstention relative to voting.

This discussion suggests that the effects of corruption revelation could vary by candidate, even if both are equally corrupt. If two candidates accused of corruption compete against each other, as is the case in our study, the impact of information revelation about each candidate's record could vary substantially because of any one of these factors. We offer evidence that partisan attachments can overlap with corruption preferences – a factor the extant literature on corruption overlooks – are an important mediating variable that shape whether voters punish corrupt politicians. Of course, while we have emphasized factors linked to the distribution of voter preferences, other variables such as candidate skill and background could shape voters' reaction to increased information. The existing experimental literature has largely ignored heterogeneity across different types of candidates, despite the fact it is quite likely that candidates' susceptibility to increased transparency is likely to depend on the contextual factors we have highlighted. In the following analysis, we pay particular attention how candidates' supporters differ and how these differences affect their response to increased information.

3 The Brazilian Electoral Context

3.1 São Paulo's 2008 Municipal Elections and the AMB's *Lista Suja*

On October 26, 2008, Kassab and Suplicy ran against each other in the run-off election for mayor of São Paulo. Kassab, the incumbent mayor, assumed the position in 2006 upon the resignation of José Serra, who became governor of the state of São Paulo and belonged to the Brazilian Social Democratic Party (PSDB or *Partido da Social Democracia Brasileira*). Kassab's Democratic Party is a center-right party that formerly was the PFL or *Partido da Frente Liberal*, one of parties that splintered from ARENA, the official party of the military regime that held power in Brazil from 1964 until 1985. Suplicy, who was mayor of São Paulo from 2001 until 2004, served as the Minister of

Tourism in the federal government for a year starting in 2007, before resigning to run for mayor. At the time of the election, President Luiz Inácio da Silva (Lula), a co-partisan of Suplicy, enjoyed widespread popularity; however, other PT candidates did not maintain the same level of support.

Suplicy's party, the PT, was traditionally associated with both leftist ideology and clean and participatory governance (Samuels, 2004). While the party moderated its ideological positions over time, the PT deliberately cultivated its brand as a party with a more ethical mode of governance and its leaders stressed the need for broader participation of the citizenry and civil society in policymaking (Hunter, 2010, p. 84). The PT heavily criticized Brazil's other major parties as corrupt and clientelistic and at least some of PT candidates' success could be attributed to the fact that they developed an image of promoting transparency in government. The São Paulo branch of the PT contributed to the creation of this brand in the early 1990s when Suplicy's former husband, Eduardo Suplicy, a federal senator, spearheaded corruption investigations against numerous municipal officials, including four past presidents of the city council (Hunter, 2010, p. 85). While the PT's reputation for clean government has been tarnished in recent years by national scandals involving bribery of legislators (including the recent 2005 *Mensalão* scandal) and illegal campaign finance, PT candidates still stress anti-corruption themes in their campaigns.

The brand of Kassab's party, the Democratic Party (DEM, formerly known as the PFL), was less distinctive than the PT's. Nominally a center-right party, the DEM was particularly strong in the poorer states in the Brazilian Northeast and its major leaders were frequently associated with extensive use of patronage while in office. The party did not have a notable anti-corruption record, given its image of being composed of "traditional" politicians with more particularistic styles of governance. One major blemish on its national record related to corruption was the party's strong support for the failed presidency of Fernando Collor de Mello, who resigned in the wake of an influence peddling scandal involving one of his main advisors. In São Paulo, the local DEM party supported the administration of Paulo Maluf, a two-time mayor of the city who was later convicted of corruption charges involving illegal government contracts, and is associated with the phrase *rouba mas faz* (he robs, but he gets things done). Perhaps because of its past record, Kassab did not tend to emphasize his party in his campaign appeals and instead stressed his

technocratic credentials and experience in government.

The AMB, the main professional association for Brazilian judges, established the Dirty List in order to publicize the corruption proceedings of candidates seeking political office. The Dirty List has generated controversy in Brazil, in terms of the criteria that one must meet to be on it and for selectively ignoring proceedings against politicians (Barros de Mello and Bragon, 2008). For example, the AMB's decision to include candidates that have been absolved by a court drew criticism from a number of judges and legal scholars. Gilmar Mendes, the president of the Federal Supreme Court (*Supremo Tribunal Federal*, or STF), for instance, declared the Dirty List as populist and politicized (D'Agostino, 2008).

The AMB included Kassab on the Dirty List because a court convicted him of "administrative impropriety" in 1997. At the time, Kassab served as the Secretary of Planning for the City of São Paulo. The case, launched by public prosecutors in São Paulo, accused Celso Pitta, mayor at the time, and his staff, which included Kassab, of taking out an advertisement paid for with municipal funds in which they allegedly defended their own "personal interests" in newspapers while they were under investigation. A lower court held that Kassab was guilty, but the decision was overturned on appeal. The public prosecutor appealed this decision, but it had yet to be resolved at the time of the election. Despite objections from the Kassab campaign, the AMB kept him on the Dirty List.

Suplicy's conviction was based on more serious charges. In 2005, a São Paulo court convicted her of inappropriately giving a R$2 million (approximately US$840,000 at the time of the election) no-bid contract to the Sexual Orientation Research Work Group (*Grupo de Trabalho e Pesquisa em Orientação Sexual*, or GTPOS), an NGO focused on advocacy for and increasing awareness of sexual orientation issues. The municipality awarded the contract to GTPOS to train São Paulo school teachers in issues pertaining to sexual orientation. Suplicy founded the NGO in 1990 and served as its honorary chairman until 2000 (MercoPress 2005). At the time of the election, the decision was under appeal.

Corruption featured prominently in the campaign, as both candidates accused each other of engaging in improprieties while in elected office, particularly after the first round in which Kassab

won with 34 percent of the vote versus Suplicy's share of 33 percent. Suplicy's campaign in particular emphasized corruption. In one Suplicy television advertisement, for example, Kassab was accused of using public funds for electoral purposes. Suplicy went so far as to petition the election authorities to make Kassab ineligible for re-election because of alleged politicized distribution of public benefits. Kassab's attacks tended to focus on Suplicy's record on public works when she was mayor between 2000 and 2004, but his campaign also raised corruption as an issue. In fact, Kassab's campaign, early in the election period before he was placed on the Dirty List, attacked Suplicy for being declared as "dirty" by the AMB. Despite the closeness of the first round, polls showed Kassab with a consistent lead throughout the second-round campaign and he subsequently won with a decisive 60 percent of the vote.

Mandatory Voting In addition to the context-specific factors that took place during the 2008 municipal elections, mandatory voting also plays an important role in the electoral behavior of Brazilian voters. Although Brazil maintains a system of mandatory voting, absenteeism rates in recent elections have hovered around 15 to 20 percent nationally.[6] Specifically, citizens are required to vote from age 18 to 70, with some exceptions. Voting is voluntary from ages 16 to 18, and for those 70 or older.

Those who fail to vote without justifying their absence within 60 days are required to pay a small fine ranging from R$1.05 to R$3.51 (approximately US$0.44 to US$1.47). Non-pecuniary costs of absenteeism borne by the voter include the time involved in a three-step process to pay a fine in which the voter typically must: (1) go to the local electoral notary (*cartorio eleitoral*) and obtain a paper stating they are fined, (2) go to a bank to pay the fine, and (3) return to the electoral notary showing that he or she paid the fine. Until the fine is paid, citizens are barred from applying for government jobs and other services, such as receiving or renewing their passport or driver's licenses, or requesting loans with public funds. It is important to note that public services affected by unjustified abstentions tend to be important to middle class and educated voters, not working class and poor voters.[7] A voter is not penalized for absenteeism if he or she is out of town on

[6] These rates contrast with a number of other countries that maintain mandatory voting including countries like Argentina, Australia, Belgium, and New Zealand, all of which have voter absenteeism rates in single-digit percentages.

[7] Additionally, welfare payments are not suspended as a result of absenteeism.

election day (Brazil does not have absentee voting); voters may also file a form with a judge giving the reason why they did not vote in the election within 60 days. Electoral judges have discretion to determine whether the excuse is legitimate or not.

4 Research Design

Our empirical strategy for understanding how voters respond to information about a candidate's record on corruption relies on three distinct components: a survey, a field experiment, and a survey experiment. Before presenting estimated effects of information revelation on behavior in an election, we present basic descriptive statistics from a post-election survey that provides useful context for understanding our findings. Subsequently, we present results from the field experiment and then show survey experimental evidence that supplement our findings from the field experiment.

We conducted the research in São Paulo for a number of reasons. First, it was the only city in which both candidates in the run-off election appeared on the Dirty List. We received funding from non-profit U.S. universities, and U.S. law prohibits political advocacy of candidates in elections by non-profit (501(c)(3)) organizations.[8] As a result, we treated the same number of precincts and produced the same flier design for both candidates. Second, São Paulo is the financial center of Brazil, and the city's mayor carries significant weight in Brazilian politics. The 2008 election had an ex-governor of the state of São Paulo and the runner-up presidential candidate in the 2006 election; in addition, Brazil's most recently elected democratic presidents (Fernando Henrique Cardoso and Luiz Inácio da Silva (Lula)) maintain strong ties to the city. Finally, as a result of São Paulo's immense size – it is the largest city in Brazil and the seventh largest in the world with an estimated population of 11 million and 8,198,282 voters in 2008 within the municipality itself – the city offers considerable heterogeneity in the education and socioeconomic status of individual voters.

[8]For a more in-depth treatment of this issue, please see the discussion of legal and ethical issues in Appendix I.

4.1 Did Voters Already Know About the Dirty List?

A necessary, but insufficient, condition for information about candidates' corruption record to have an effect on voting behavior is voter ignorance about the candidates' placement on the Dirty List. To find out whether or not voters already knew about the accusations, in the week after the election, we conducted a survey (N=200) of São Paulo residents living near polling stations in the field experiment control group with the aim of obtaining information on voters' pre-treatment knowledge of the Dirty List, as well as their opinions related to corruption in government. Since the treatment was never administered in these precincts, knowledge among surveyed voters should reflect knowledge among voters prior to the intervention. We used cluster sampling, in which we chose 20 control group precincts, and then randomly sampled ten households with the vicinity of the selected precincts.

Was the São Paulo electorate aware of the Dirty List and the fact that the two major candidates were included on it? Our survey data suggests that this is not the case as only 25 percent of respondents answered that they had heard of the Dirty List. Of those who knew about the Dirty List, only 48 percent knew that both candidates were on it, 30 percent identified only Suplicy as being on the Dirty List, and 22 percent identified only Kassab as being on the Dirty List. Thus, only 12 percent of all respondents could correctly place both Kassab and Suplicy on the Dirty List. Given this relatively low level of knowledge, informing voters potentially could change their views of the candidates and consequently their behavior on election day.

5 Behavioral Responses to Increased Information: Field Experimental Evidence

5.1 The Intervention

To inform voters of the corruption convictions of politicians, we designed two fliers – one for each candidate in the run-off election. The fliers are pictured in Figure 1 with their respective

translations.[9] The flier design incorporates aspects of political propaganda that are similar to other political marketing material in Brazil, while also intending to have credibility in the information it is conveying. Both fliers have newspaper articles from *Folha de São Paulo*, one of the country's most respected periodicals, detailing the corruption allegations of each candidate. We also included the case numbers of each court case to increase the credibility of the information in the fliers.

5.1.1 The Unit of Analysis and the Randomization Group

The unit of analysis for the experiment is the *local de votação*, or voting precinct. Voting precincts are the smallest units for which we could administer a treatment, while obtaining vote share data for individual candidates and turnout data for voters. In selecting the group of precincts in the randomization group, we made a number of decisions based on our substantive interests and logistical constraints. We chose 400 of São Paulo's 1,759 precincts utilizing a constraint optimization algorithm that operated as follows:

(1) selected a relatively even mix of precincts based on the vote choice in previous elections. The specific covariates are discussed in greater detail in Section 5.2.

(2) chose precincts to maintain a relatively even mix of poor, lower middle class, and middle class precincts.

(3) maximized the distance between the treatment and control groups in order to minimize the potential for cross-over violations.

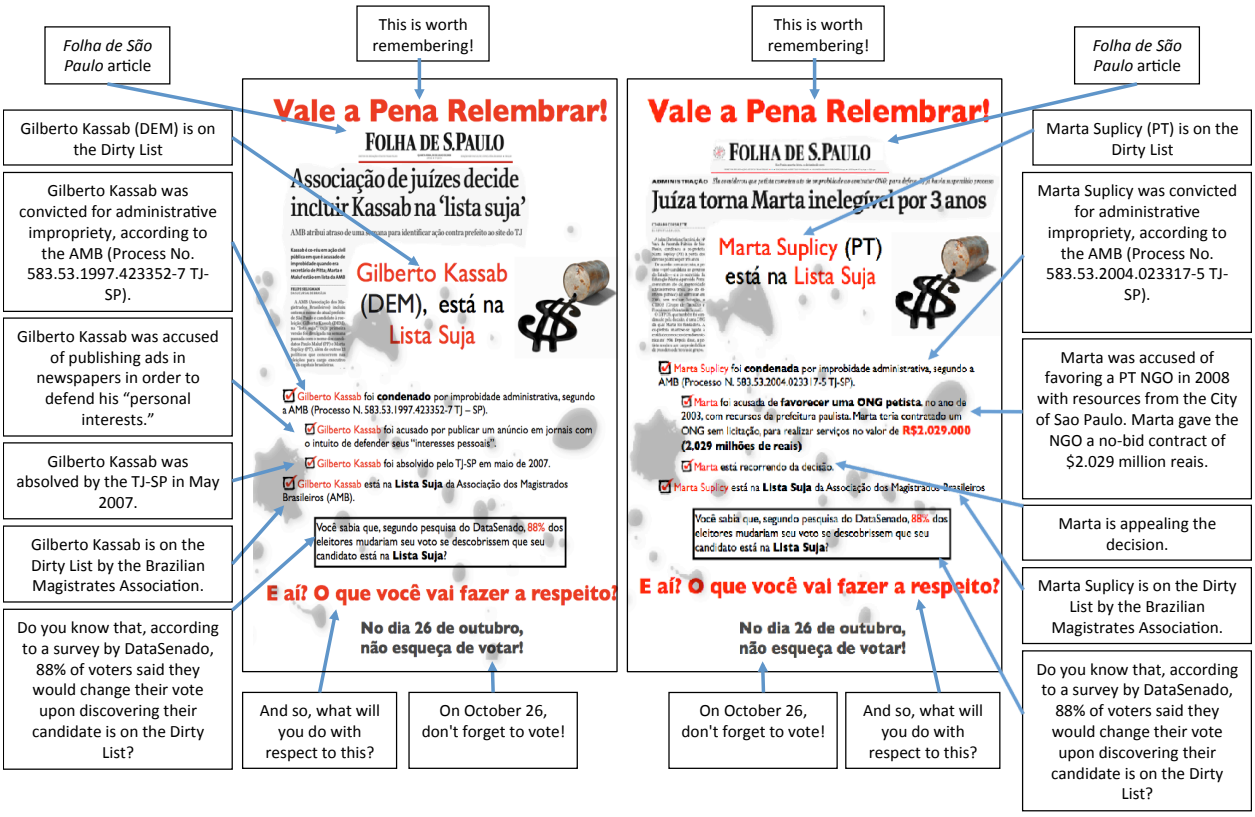
(4) selected the smallest polling locations in order to maximize statistical power.

(5) limited the geographic areas of polling locations to the north, east, and south zones of São Paulo. Due to budget constraints, the delivery company we used to deliver the fliers limited us to three geographic zones in São Paulo. These three zones best satisfied the other criteria on which we selected the precincts in the randomization group.

(6) included precincts in areas with a high penetration of individual household units with indi-

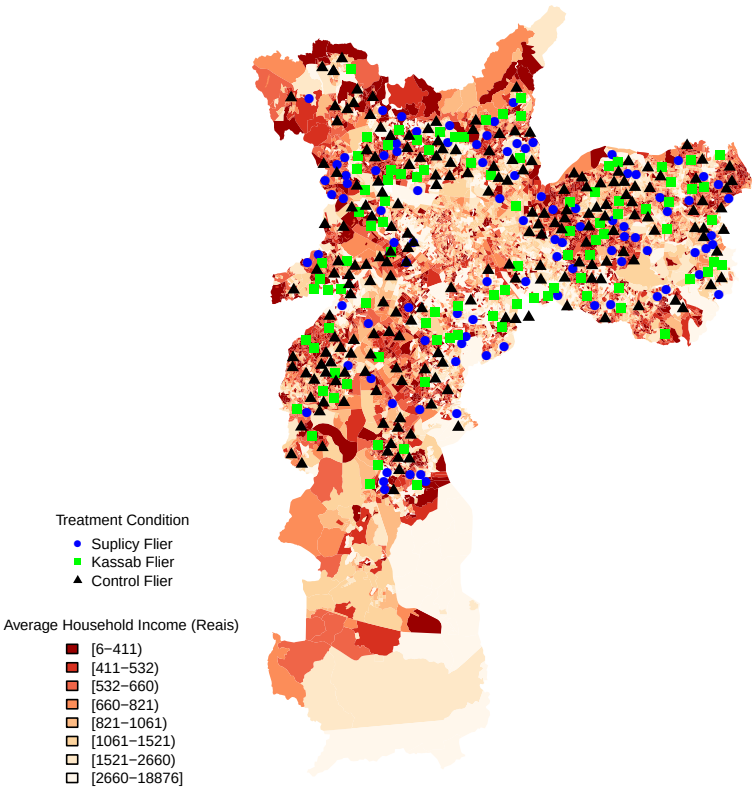
[9]The fliers were designed in consultation with a local graphics designer and political experts, with reference to a large sample of electoral propaganda. We also consulted with experts in constructing the design of the flier. In addition, we informally conducted semi-structured interviews with two dozen voters to get their reaction to various flier prototypes. Based on the responses of these individuals, we developed finalized versions of the fliers that would be used in the field and survey experiments.

Figure 1: The Fliers



vidual mailboxes. We intentionally avoided areas with a high percentage of high-rise and mid-rise apartment buildings, because of the high likelihood of fliers not being delivered by doormen or other personnel who would control access to the buildings.

Figure 2: Distribution of Voter Precincts



To reduce the risk of interference across experimental units, we ensured that precincts in the study were not closer than half a mile from other precincts in the study.[10] After ensuring some

[10]It is true that despite our precautions, some interference (sometimes referred to as "SUTVA" violations) could have occurred. The most plausible scenario is that a resident in a treatment precinct could have informed a voter living in a control precinct about the content of the flier. While we think that such violations were likely to have been few given that the election occurred only a few days after the distribution of the fliers, any interference that did occur

amount of distance between the experimental precincts, we grouped them into blocks of two based on longitude, latitude, PT vote share in the 2004 mayoral elections, and PT vote share in the 2006 presidential elections. More specifically, we matched precincts to their nearest neighbor on a Mahalanobis distance metric. Within blocks, each precinct had an equal probability of being selected into treatment. Figure 2 shows a map of São Paulo with the distribution of precincts in the treatment and control groups.

5.1.2 Flier Delivery

In order to deliver the fliers, we hired a direct marketing firm with extensive experience delivering marketing and political propaganda for prominent multinational and local retailers and political candidates. The firm delivered the fliers from October 22-25, 2008 (over the four days prior to the election), and had a number of enforcement measures in place to make sure that the correct fliers were delivered to households.[11]

Unlike in the United States, Brazilian voters are allowed to choose any voting precinct within an electoral zone located where he or she resides. In 2008, the municipality of São Paulo had 1,759 precincts located in 57 electoral zones. Unfortunately, in Brazil, data is not publicly available for the precincts to which voters are zoned. We spoke to political consultants and experts in voting behavior who stated that approximately 70 to 95 percent of voters vote at the location closest to their house in São Paulo. As a result, we were unable to determine the precise households that belonged to the voting precinct. Voters are, however, only able to vote in the precinct in which

would most likely result in downwardly biased (towards zero) treatment effect estimates. Under the assumption that receiving a flier with negative information about the candidate would not induce voters to vote for the candidate, our treatment effect is a lower bound on the true average treatment effect. More precisely, if the effect of receiving the flier on whether or not a voter votes for the candidate is non-positive in both treatment households and control households that inadvertently receive the information on the flier through interference, then reported treatment effect estimates of the average treatment effect in the absence of interference are downwardly biased. Our estimates would only overstate the treatment effect in the unlikely scenario that the fliers had opposing effects, i.e. that the flier caused voters in treatment precincts to vote against the candidate and caused control households to vote for the candidate. For a precise formulation of bounds in the presence of interference, see Manski (2011).

[11] First, the overwhelming majority of deliverers had worked with the firm previously, and had thus established a working relationship with the firm. Second, supervisors monitored deliverers and also performed random checks of mailboxes to ensure that the proper fliers were delivered. Third, delivery personnel carried hand radios and were monitored by a supervisor based at the office of the direct marketing firm. This supervisor had himself been a deliverer and had good local knowledge of the appropriate time it would take to complete a delivery route. Finally, the firm gave our research team unfettered access to monitor their work. We therefore conducted our own random checks of mailboxes to make sure the correct fliers were delivered and also accompanied the supervisors during the delivery.

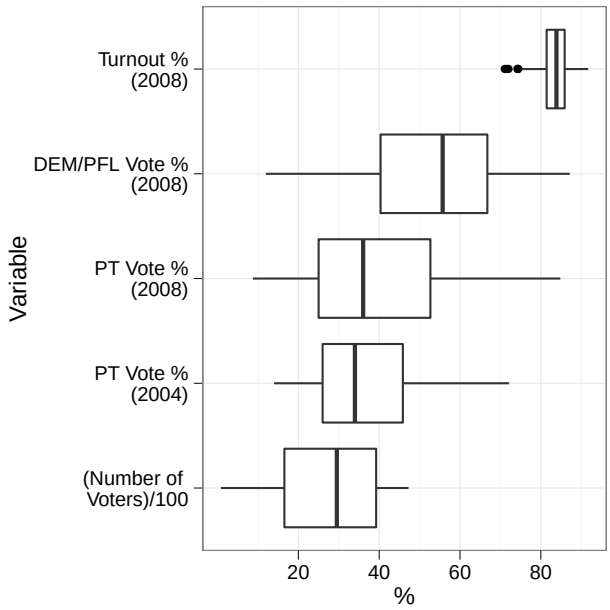
they are registered. In determining the appropriate number of households to deliver fliers for a given precinct, we knew the number of voters that were registered to vote at the precinct. We knew that the average number of voters per household in São Paulo at the time of the election was 3.1. In order to be conservative in our estimate of households for a given precinct, we took the number of voters in the precinct, and divided the number by 2.8 to obtain the number of households within a precinct to which we would deliver fliers. We also delivered an additional ten percent of fliers because of the high likelihood of dilution in the immediate area of the precinct. The direct marketing firm maintained a current database with the number of individual houses per city block. The delivery firm located the 200 precincts in the treatment group, and gave maps to the deliverers so that they would "spiral out" from the precinct delivering all of the fliers with the precinct as the center of a radius. Supervisors dropped off delivery personnel at the voting precinct (which almost always was a school). In the weeks after the election, we also asked respondents in the treatment group the distance they lived from their voting precinct, and 63.9 percent stated that they lived 1 kilometer or less from their polling location, and 77.5 percent reported living less than 2 kilometers away from their polling location.

As a result of the imprecision with which we were able to deliver the treatment, we believe that our treatment effects most likely underestimated the impact of the treatment. While the vast majority of voters assigned to a given precinct live in the immediate vicinity of the precinct's polling station, the small number of voters who live far from the polling station—most likely because they never bothered to change their registration after moving—would not have received the flier. Furthermore it is possible that some of the residents who received fliers actually voted in a control precinct, which would further attenuate our estimate. Because we do not have precise data on which voters no longer live near their precinct's polling station, we can only estimate an "intent-to-treat" effect that is likely to be lower in magnitude than the effect among those who actually received the flier.

5.2 Descriptive Statistics and Baseline Balance

Box plots showing the distributions of the data used in the analyses below are presented in Figure 3. Consistent with the overall election results though with a smaller spread between the two candidates, the center-right candidate, Gilberto Kassab, received about 14 percentage points more of the vote than the center-left candidate from the PT.[12] Furthermore, turnout is high, with an average of 83 percent of registered voters casting a ballot. To check baseline balance on observables, as well as to improve precision in some of our estimates, we also use election data from the most recent past elections.

Figure 3: Descriptive statistics for the field experiment.



Data obtained from the São Paulo TRE (Regional Electoral Tribunal); N=400

To check if our randomization procedure was successful, we examined whether pre-existing differences existed across treatment and control precincts. As is well known, in expectation there will be no differences between treatment assignment groups, but for any given randomization,

[12] In the actual election, Kassab received 60.7 percent of the vote, while Suplicy received 39.3 percent of the vote.

some imbalances can remain. To check baseline balance, we conducted simple difference-in-means tests across thirteen baseline covariates. The covariates include voting outcomes from previous elections, as well as voting results from the first round of the 2008 election. In addition to testing balance on each variable separately, we use an omnibus test found in Hansen and Bowers (2008) that jointly appraises balance on each covariates, as well as their linear combinations.[13] Table 1 shows the results for each separate variable, reporting mean differences, standard errors of the difference, t-test p-values, and the Kolmogorov-Smirnov Test p-values. For twelve of the thirteen covariates, we find no substantial imbalances. The number of voters variable, however, exhibits some imbalance, with a mean difference of 298 additional voters in treatment precincts versus control. In some of our analyses below, we check the robustness of our findings to adjust for this imbalance. The omnibus test which tests the hypothesis of no difference on any of the baseline variables, as well their linear combinations, has a p-value of 0.17. Thus, while we find some imbalance on the number of registered voters, on all other variables, treatment and control are statistically indistinguishable overall.

Table 1: Balance on Baseline Variables (N=400)

Variable	Mean Diff	Standard Error	t-Test p-Value	KS-test p-Value
Number of Registered Voters	-298.38	133.42	0.03	0.09
PT Mayor Vote % (2004)	-0.32	0.98	0.74	0.54
PT Pres. Vote % (2006)	0.07	1.12	0.95	0.86
PT Congress Vote % (2006)	-0.06	0.72	0.93	0.99
PSDB Congress Vote % (2006)	0.32	0.63	0.62	0.14
1st Round Suplicy Vote % (2008)	-1.10	1.37	0.42	0.54
1st Round Kassab Vote % (2008)	0.14	0.74	0.86	0.79
1st Round Blank Vote % (2008)	-0.02	0.08	0.78	0.92
1st Round Invalid Vote % (2008)	-0.07	0.08	0.41	0.79
1st Round Turnout % (2008)	0.20	0.32	0.52	0.18
PT City Council Vote % (2008)	-0.65	0.83	0.43	0.54
PSDB City Council Vote % (2008)	0.86	0.60	0.15	0.33
DEM City Council Vote % (2008)	0.22	0.44	0.62	0.47

[13]The omnibus statistic, called as d^2 in Hansen and Bowers (2008), is a weighted sum of squares of differences in means, though in our application, the weights are constant. This statistic has a large sample χ^2 distribution.

5.3 Results

We present two sets of results for each of our three dependent variables: vote share, turnout, and spoiled ballots. Our quantity of interest is the average treatment effect on precincts, not individual voters, as individual level data is unavailable. The first estimator is the simple "intent-to-treat" estimator, which is the average within-block difference in treatment and control precinct means. Our second set of estimates are from a simple linear regression of the outcome variable on a treatment indicator, a vector of covariates, and block dummy variables. The model we estimate is as follows:

$$Y_i = \beta_0 + \beta_1 T_i + \sum_{k=1}^{K-1} \gamma_k B_{ki} + \lambda_1 X_1 + \lambda_2 X_2 + u_i$$

Y_i is the outcome of interest, T_i is the treatment indicator, X_1 and X_2 are two pre-treatment covariates, and u_i is the disturbance term. To account for the fact that randomization occurred within matched pairs or blocks (k), we add fixed effects (B_{ki}) for all but one matched pair. Since we are interested in the separate effects of each type of flier, we estimate this model separately for the Suplicy intervention and the Kassab intervention. We adjust for two covariates: PT vote share in the 2004 mayoral election and the number of registered voters in the precinct. 2004 PT vote share is an important covariate because it is highly predictive of our outcome variables and can potentially increase the precision of our estimates. We also adjust for number of voters because we detected some imbalance in this covariate after randomization, as discussed in the previous section. Finally, all standard errors account for heteroskedasticity, as "robust" standard errors are used in covariate adjusted results and the intent-to-treat estimates do not assume equal variance across treatment conditions.

Table 2 presents the effect of the distribution of the fliers with information on the corruption convictions of the PT mayoral candidate on the vote share of the candidate, turnout, and spoiled ballots.[14] For vote share (votes as a percent of total votes cast), we find a negative effect of about 2.6 percentage points, which amounts to about 15 percent of a standard deviation. The 90 and 95

[14]Spoiled ballots in all presentations of results are measured by the blank votes cast in the election. We also estimated treatment effects on invalid votes and the sum of invalid votes and blank votes, and found that all estimates were statistically indistinguishable from 0.

Table 2: The effect of distributing information on corruption convictions involving Marta Suplicy, the PT mayoral candidate, on election outcomes. N=200 precincts, with 100 treated units. Estimates without covariates are from the simple ITT estimator. Estimates with covariates are from a linear model, including a treatment indicator, PT vote share in 2004, total number of registered voters in the precinct, and block fixed effects.

	Vote Share (%)		Turnout (%)		Spoiled Ballots (%)	
Estimate	-2.6	-2.6	-1.9	-1.8	0.03	0.01
Standard Error	1.99	0.93	0.46	0.45	0.08	0.08
95 % Conf. Int.	[-6.5, 1.3]	[-4.4, -0.7]	[-2.7, -0.9]	[-2.7, -0.9]	[-0.1, 0.2]	[-0.1, 0.2]
p-value	0.2	0.01	0	0	0.72	0.86
Covariates	No	Yes	No	Yes	No	Yes

percent confidence intervals of the unadjusted estimate overlaps with 0 (p-value = 0.2), but the adjusted estimate, which is also -2.6 percentage points, is statistically significant at conventional levels. While estimated with some imprecision, this result does suggest that receiving the flier induced some voters who otherwise would have supported Suplicy to abstain or vote for Kassab.

Our estimates support the hypothesis that providing information about Suplicy's corruption convictions lowered the candidate's vote totals, but where did these votes go? Theoretically, the lower vote share in treatment precincts could be due to either increased abstention by Suplicy supporters or vote switching to Kassab by voters who previously supported the PT candidate. Our data is more consistent with the former story as opposed to the latter. When we estimate the effect of the Suplicy intervention on total votes received by Kassab as a percent of registered voters (not vote share as a percent of ballots cast), we find an insignificant increase of about 1.5 percentage points (standard error of 1.7). Thus, while it is likely that some Suplicy voters changed their vote and cast a ballot for Kassab, it would appear that abstention was the primary response by voters to the intervention.

Further evidence that the intervention affected electoral outcomes primarily through decreased turnout is presented in the second two columns of Table 2, where we find a significant negative effect of -1.9 percentage points. This effect estimate represents an average decline of about 450 voters. Results using covariate adjustment are substantively equivalent to the unadjusted results (point estimate of -1.8 percentage points). For spoiled ballots, we find a small positive difference,

Table 3: The effect of distributing information on corruption convictions involving Gilberto Kassab, the DEM/PFL mayoral candidate, on election outcomes. N=200 precincts, with 100 treated units. Estimates without covariates are from the simple ITT estimator. Estimates with covariates are from a linear model, including a treatment indicator, PT vote share in 2004, total number of registered voters, and block fixed effects.

	Vote Share (%)		Turnout (%)		Spoiled Ballots (%)	
Estimate	1.9	1.5	0.1	0	-0.05	-0.09
Standard Error	1.87	0.99	0.42	0.41	0.12	0.13
95 % Conf. Int.	[-1.8, 5.5]	[-0.5, 3.4]	[-0.7, 0.9]	[-0.8, 0.8]	[-0.3, 0.2]	[-0.4, 0.2]
p-value	0.32	0.15	0.77	0.95	0.68	0.49
Covariates	No	Yes	No	Yes	No	Yes

but both estimates are statistically indistinguishable from 0.

This abstention response is particularly surprising given that failing to vote is punished with fines, albeit very small ones (less than US $2). The fact that turnout is sensitive to the distribution of a flier suggests that either voters find the fines to be trivial or that the consequences of not paying the fine are low for many Suplicy voters. Nonvoters who fail to pay the fine or provide an adequate excuse are prevented from receiving benefits that are most valuable to educated, middle-class Brazilians such as the ability to renew a passport and drivers license and eligibility for the civil service. In our survey data, we found that Suplicy's base is substantially poorer than Kassab voters, with about half of Suplicy voters earning less than twice the minimum wage, compared to 30 percent of Kassab supporters. While we cannot know for sure why voters are willing to bear the costs of abstention, one possibility is that the punishment for not voting is inconsequential for a substantial portion of Suplicy's base. Kassab voters, because of their comparative education and wealth, are more likely to view the administrative restrictions that result from abstention as more costly.

The estimated effects of the distribution of fliers with information on the center-right candidate of the DEM/PFL are found in Table 3. Surprisingly, the point estimate on the DEM/PFL candidate's vote share is positive at about 1.5-1.9 percentage points, depending on the specification. This result, however, is estimated rather imprecisely and consequently not statistically significant at conventional levels. Furthermore, the estimate appears to be somewhat sensitive to covariate

adjustment. The estimates for the other two outcome variables – turnout and spoiled ballots – are small and not statistically insignificant.[15]

To contextualize these estimated effects, it is worth comparing their magnitude to effects documented in other studies using experimentally administered interventions to increase voters' awareness about corruption.[16] In the Chong et al. (2011) study on Mexican mayoral elections, a flier campaign informing voters of the result of corruption audit had a negative overall effect of 1.10 percentage points on turnout or slightly more than half of what we document. Their flier intervention, however, had a larger turnout effect of -4 percentage points in municipalities with higher rates of corruption and, in contrast to our findings, had similar effects on both incumbent and challenger supporters. An important difference between our results and their study, however, is that they informed voters only about incumbent performance, as well as the fact that abstention in Mexico is not fined.[17] In the Banerjee, Kumar, Pande and Su (2010) study in New Delhi that provided voters with "report cards" on incumbent performance, additional information *increased* turnout by about 3.6 percentage points, but these leaflets included information on a range of activities. While the report cards did not have corruption indicators, they did report whether or not the candidate was a criminal. The effects of the intervention, however, did not vary by candidate's criminal status. Thus, our results are more in line with the Chong et al. (2011) study, with the important caveat that we compare two candidates in the same municipality while their study emphasizes comparisons across municipalities.

6 Testing Mechanisms: Individual Level Evidence

To understand the heterogenous behavioral effects of the fliers observed in the aggregate electoral data, we now turn to individual level data. This data, gathered in the survey, described in

[15] The point estimate for the difference in the effect of the two fliers on vote share (with covariates) is 4.1 percent, with a standard error of 1.4. Without covariates, this difference is estimated less precisely but it is still significant at the 10 percent level. Similarly, the point estimate for the difference in the effect on turnout between the Suplicy and Kassab fliers—with or without adjusting for covariates—is statistically significant. For the covariate adjusted estimates, the difference is 1.8 percent with a standard error 0.61.

[16] In the extensive experimental get-out-the-vote (GOTV) literature on US elections, we are not aware of studies that provide information about politicians' corruption records. The literature on the effects of distributing GOTV leaflets or fliers on turnout suggest very small effects on the order of about 0.5 percentage points (Green and Gerber, 2008, p. 51).

[17] While voting is technically compulsory in Mexico, there are no legal sanctions for not voting.

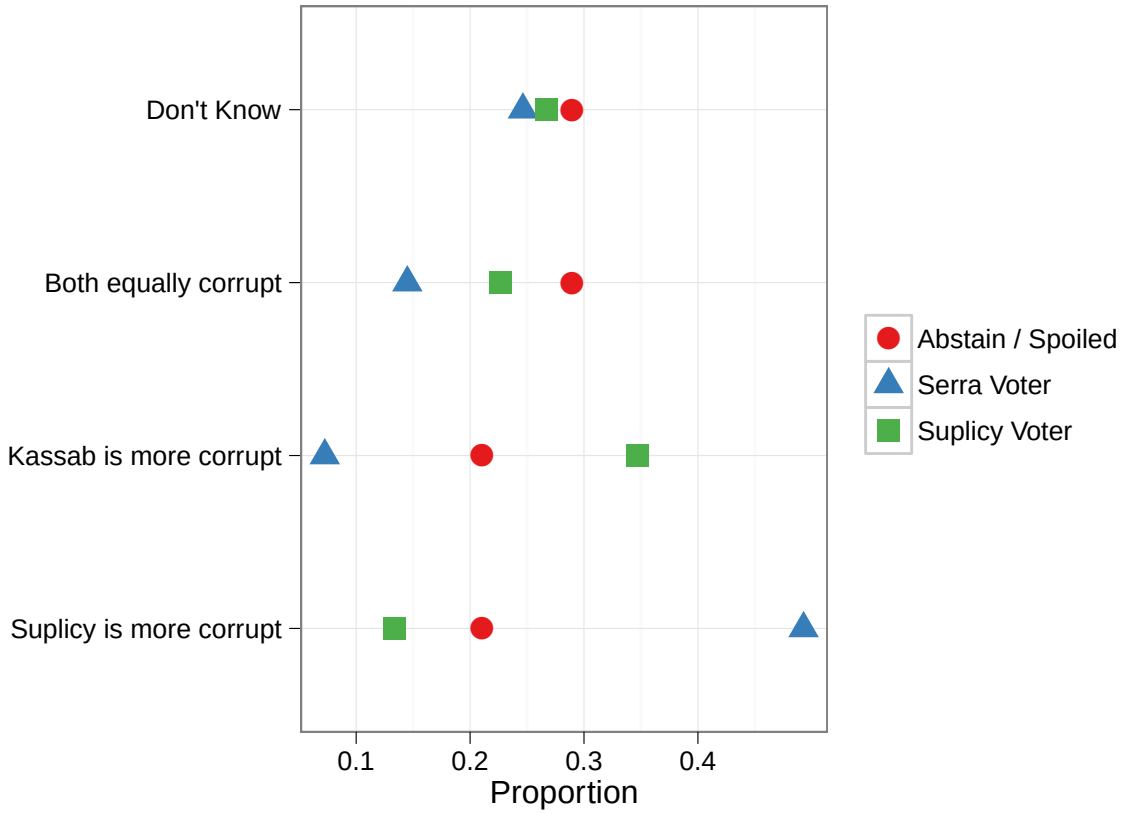
Section 4.1, contains descriptive data on voters opinions about the corruption record of each candidate, ex-ante evaluations of the candidates, and the importance that voters place on corruption in their political decision-making. In addition to collecting basic attitudinal data, we also use an embedded survey experiment to observe individual level attitudinal responses to the information contained on the fliers used in the intervention.

As discussed in Section 2, voters may already have existing beliefs about how corrupt each candidate is and these beliefs will affect their response to new information. If voters already perceive a candidate to be corrupt, learning about their placement on the Dirty List may not change their attitudes or their behavior. It is possible, for example, that voters already assumed that Kassab was corrupt and thus the flier would not affect their evaluation of the candidate. To check for this possibility, the survey asked voters to rank each candidate by their perceived level of corruption. On average, voters' evaluations of the candidates on this quality differed in that 29 percent of voters identified Suplicy as the most corrupt candidate, while 20 percent of voters named Kassab. 20 percent of voters said both were equally corrupt, while another 30 percent stated they did not know. These figures suggest that while a plurality of voters considered Suplicy the more corrupt candidate, the vast majority of voters believed the candidates to be equally corrupt or could not make the comparison. Overall, these figures suggest that the differential effects we detected in the field experiment are unlikely to be attributable to diverging ex-ante evaluations of the candidates on the corruption issue as Suplicy, on average, was viewed as somewhat more corrupt already.

These aggregate figures, however, mask considerable heterogeneity when voters are disaggregated by their past political behavior. Figure 4 shows how voters rank each of the candidates in subgroups defined by their self-reported vote in the run-off for the 2004 mayoral election.[18] Suplicy, the incumbent in 2004, lost the election against former presidential candidate José Serra. It is clear that the political leanings of each voter strongly predicts how voters evaluate each candidate on the corruption issue. 34 percent of voters who cast a ballot for Suplicy in 2004 viewed Kassab as the more corrupt candidate, while only 7 percent of Serra voters felt similarly. The views of Suplicy and Serra voters are not completely symmetric: Serra voters are more likely to believe

[18]The pattern is very similar if we stratify by 2008 vote choice. We use 2004 vote choice as a stratification variable to show that heterogeneity in voters' evaluations coincide with political cleavages that existed prior to the 2008 election.

Figure 4: Ranking Candidates on Perceived Corruption by Vote in 2004



that Suplicy is more corrupt (49 percent) than Suplicy voters are to believe that Kassab is more corrupt (34 percent). As one might predict, voters who abstained or cast a spoiled ballot in the 2004 election were more likely to claim that each candidate was equally corrupt.

Given the fact that voters' ex-ante perception of the candidates on corruption varied markedly by their political leanings, any intervention designed to increase voters' information could have highly heterogenous effects depending on the candidate the voter intends to support. If a Suplicy supporter received information about Suplicy and viewed the new information as credible, for example, then she might be less inclined to turnout or cast a ballot for Suplicy. This is especially the case if Suplicy voters were more likely to be weak supporters of the candidate. Thus, a potentially important distinction between the two candidates is the intensity of their voters preferences since a candidate with many weak supporters would likely suffer more as a result of the revelation of

information. In this election, we find no evidence of a divergence in the intensity of preferences among the supporters of each candidate. To assess this, we asked each voter to rate the candidates with a 1 to 10 "feeling thermometer" score. The distribution among each candidate's voters were almost identical with a mean score of 7.8 for Suplicy among Suplicy voters (median of 8) and a mean score of 7.6 (median of 8) for Kassab among Kassab voters. The similarity across the two groups of voters suggests that intensity of preferences is unlikely to be an explanation for the divergent effects found in the field experiment.

Even in the absence of differences in the intensity of support across each candidate's voters, the effects of information about corruption accusations could diverge if a candidate's supporters differ in the importance they place on clean government. There is reason to believe that divergence would exist given that Suplicy's party, the PT, has a long history of emphasizing transparency in government and this may cause voters who care about this issue to support her. In fact, we do find a marked difference between Suplicy supporters and other voters in the importance placed on corruption. For voters who supported Suplicy, 70 percent professed that when deciding who to vote for in the 2008 election, corruption was "very important" or "important" in their decision. In contrast, a considerably fewer 48 percent of Kassab supporters said that corruption was "very important" or "important." This 22 percentage point difference suggests that Suplicy voters would, on average, be considerably more sensitive to learning about Suplicy's placement on the voters list.

Overall, the findings of the survey suggest that the most substantial difference across Kassab and Suplicy voters is the weight each candidate's supporters place on corruption in their decision making. Perhaps because of the PT's historical image as not engaging in the corrupt practices used by other parties, more Suplicy voters than Kassab voters say that corruption is an important factor when choosing among candidates. This suggests that learning about one's preferred candidate's placement on the Dirty List would have a larger effect on behavior among Suplicy supporters than Kassab supporters, which is consistent with the results of the field experiment.

6.1 Evidence from a Survey Experiment

To better understand our findings and to take advantage of individual-level data, we embedded a survey experiment modeled after the field experiment in the post-election survey discussed in Section 4.1. While we are interested in the overall impact of the fliers on voter attitudes, the survey experiment also allows us to test some of our hypotheses explaining the divergent effects found in the field experiment. In particular, we take advantage of the survey experiment to test our hypothesis that Suplicy's supporters' views are more affected by learning about her placement on the Dirty List than the views of Kassab's supporters when they learn about his corruption record.

Working only in field experiment control precincts, we randomly assigned 200 respondents with equal probability to be given the Kassab flier, the Suplicy flier, or a placebo flier showing basic biographical information for both candidates (shown in Appendix II).[19] After the respondents read the fliers, the interviewers asked the interviewed voters to "grade" Kassab and Suplicy on a scale from 0 to 10, where 0 indicated being strongly against the candidate and 10 indicated that the respondent was strongly in favor of the candidate.

After asking respondents to read the fliers, we asked the interviewed voters about the believability of the accusations, as well as their seriousness. If voters perceived the Suplicy accusations to be more believable or serious, then this difference could explain the disparate behavioral response to the fliers. We find no evidence for either explanation. Only 30 percent thought the flier was mostly or completely false; most voters exposed to the Suplicy flier said that the accusations were mostly or completely true. For those exposed to the Kassab flier, the proportions are very similar: only 28 percent thought the flier was mostly or completely false. When it comes to the seriousness of the accusations, once again there were few differences by flier. 80 percent and 78 percent of voters exposed to the Suplicy flier and Kassab flier, respectively, thought the accusations were very serious or serious. The similarity in voters perceptions of the two fliers provides

[19] Like with the field experiment, we checked to see if the randomization procedure achieved reasonable balance in pre-treatment covariates. We checked for differences on pre-treatment feeling thermometer scores of the two candidates, self-reported turnout, self-reported vote intention, party identification, presidential vote, and household income. None of the differences across the three different treatment conditions were statistically significant from each other. The Hansen and Bowers (2008) omnibus test that jointly appraises covariate balance and their linear combinations gives a p-value of 0.56.

evidence that differences in the fliers or their content are not an explanation for why the Marta flier was more effective at changing voting behavior than the Kassab flier.

Table 4: Survey experiment results for the Suplicy (PT) and Kassab (DEM/PFL) fliers. The dependent variable is the post-treatment minus pre-treatment candidate evaluation on feeling thermometer on a scale of 0 to 10.

	Suplicy vs Placebo	Kassab vs Placebo	Suplicy vs Kassab
Estimate	-0.78	-0.36	-0.54
Standard Error	0.32	0.34	0.42
95 % Conf. Int.	[-1.41, -0.15]	[-1.03, 0.31]	[-1.36, 0.28]
p-value	0.02	0.3	0.2

We also examine the overall effects of the fliers on voters' evaluations of the candidates by comparing voters' evaluations of Suplicy (Kassab) when they view the Suplicy (Kassab) flier versus when they are given the placebo flier. These results are shown in the first two columns of Table 4. The effect of the Suplicy flier on voters' evaluations of Suplicy is larger than the effect of the Kassab flier on voters' evaluations of Kassab, although the difference between the two effects is not significantly different than 0. After being exposed to the Suplicy flier, respondents in the treatment group on average adjusted their evaluations downward by an estimated 0.78 points on a 10-point scale, which amounts to about 60 percent of a standard deviation. The point estimate for the Kassab flier was an insignificant -0.36. The third column compares those receiving the Suplicy flier to those receiving the Kasab flier. The Suplicy flier more negatively affects attitudes, though this difference is not statistically significant. Overall, these individual-level estimates are in keeping with the field experiment evidence: the Suplicy flier harms voters evaluations of her, while the Kassab flier has weaker effects. Thus, the design also contributes to a nascent literature (Barabas and Jerit, 2010) that examines the external validity of survey experiments through its pairing of a field experiment with a survey experiment.

To test whether or not Suplicy voters respond differently to increased information about their favored candidate's corruption record than Kassab voters, we estimated treatment effects separately in strata defined by vote choice. The first two columns of Table 5 show the effect of the Suplicy flier, as compared to the placebo flier, on those who did not vote for Suplicy and those who

Table 5: Heterogeneity in survey experiment results for the Suplicy (PT) and Kassab (DEM/PFL) fliers. The dependent variable is the post-treatment minus pre-treatment candidate evaluation on feeling thermometer on a scale of 0 to 10.

	Suplicy vs Placebo		Kassab vs Placebo	
	Non-Suplicy Voters	Suplicy Voters	Non-Kassab Voters	Kassab Voters
Estimate	-0.38	-1.29	-1.24	0.42
Standard Error	0.32	0.53	0.5	0.5
95 % Conf. Int.	[-1.01, 0.25]	[-2.33, -0.25]	[-2.22, -0.26]	[-0.56, 1.4]
p-value	0.24	0.02	0.02	0.4
n	85	48	62	72

did (self-reported). The estimate for Suplicy voters is more than three times the size of the estimate for Non-Suplicy voters: -1.29 versus -0.38. Unsurprisingly given the small samples, however, the difference between the two estimates (the interaction) is not statistically significant. Still, the difference in magnitudes certainly suggests that Suplicy voters are more sensitive to corruption-related information than supporters of other candidates.

When we examine heterogeneity in the effect of the Kassab flier, the contrast with the effect of Suplicy flier is striking. As shown in the third and fourth columns of Table 5, the heterogeneity observed is the opposite of what we found for the Suplicy flier. Kassab voters who read the flier, on average, give a *higher* evaluation of the candidate. Although this estimate is not statistically distinguishable from 0, it is distinguishable from the effect of the flier among non-Kassab voters. Among non-Kassab voters, reading the Kassab flier induced a statistically significant 1.24 point decrease in their evaluation of the candidate. The difference in the size of the effect between Kassab voters and non-Kassab voters is 1.7 points (standard error of 0.7).

Overall, the results from the survey experiment provides further evidence that Suplicy's voters have a larger reaction to increased information about their candidate's corruption record than Kassab's voters. Upon learning of Suplicy's position on the Dirty List, Suplicy's voters perceive her more negatively, on average. When Kassab voters learn about their candidate's placement on the Dirty List, their evaluation of their candidate is essentially unchanged. Furthermore, our survey evidence shows that, when asked, Suplicy's base professes to place more importance on corruption than Kassab's base. It is plausible that this difference in how each candidate's voters

view the importance of corruption resulted in a differential behavioral response to the release of information in our field experiment. In general, our evidence indicates that Suplicy's voters viewed their candidate more negatively after learning about her record and became more likely to abstain as well as, to a lesser degree, switch their vote to Kassab. Kassab voters, because they view corruption as less central to their political decision-making, failed to change their views or their behavior.

7 Conclusion

Good government activists and reformers frequently argue that increased transparency about politicians' records can make democratic institutions more effective at incentivizing clean governance. Increased transparency, the argument typically goes, will induce voters to punish corrupt politicians at the voting booth and thus better align the interests of politicians with the electorate. As we document in this work, publicizing a candidate's record on corruption does have the potential to alter voters' behavior, but its effects are contingent upon the importance voters place on clean governance in their decision-making. Furthermore, the degree to which voters view a candidate's corruption record as important can be correlated with political cleavages, an important mechanism previously unexplored in the experimental literature on corruption. As a result, the effects of increased transparency may result in outcomes wherein one politician may be punished when his corruption record is revealed while another may not be.

In the case of São Paulo, we document the existence of a partisan cleavage in how voters perceive the importance of corruption. Furthermore, we argue that this cleavage has real consequences for the effectiveness of an intervention designed to inform voters about candidates' placement on a so-called Dirty List compiled by a civil society organization. Despite the fact that voters viewed the accusations against each candidate as equal in seriousness, our field experimental evidence revealed that only the PT's candidate, Marta Suplicy, was punished at the ballot box when voters learned about her placement on the Dirty List. Data from our public opinion survey and an embedded survey experiment provides evidence of a mechanism: Suplicy's supporters are much more sensitive to information about corruption than are Kassab's supporters. As a result of

this increased sensitivity, the information provided to voters induced some of Suplicy's supporters to abstain and, to a lesser degree, to switch their vote to her opponent.

An important question raised by these results is why are Suplicy voters more willing to change their behavior when they learn about their candidate's record? We speculate that the PT's historical cultivation of a brand as a "different" type of party that has a distinct "mode of governance" (*"modo petista de governar"*) emphasizing transparency and citizen participation may have raised PT voters' expectations on the corruption issue. For many PT voters, corruption and ethical issues may be central to their political identity. Kassab's party, the Democrats (formerly the PFL) if anything, has developed a brand as a party whose candidates may rob, but "get things done." As a result, Kassab voters may have had lower expectations about their candidate's probity in office and consequently new information about past misdeeds failed to change their behavior.

More generally, our results suggest voters can develop a "norm of accountability" but that this norm can be less than universally held. In a different context, Stokes (2006) documented variation in the degree to which voters across Argentina abided by informal voting rules that sanctioned poor performance. In towns where democratic institutions worked more effectively, voters tended to expect politicians to govern ethically and were quite willing to withdraw their support when this was not the case. Our findings suggest that this norm of accountability can interact with partisanship and have important consequences for the outcomes of campaigns where corruption is an issue. The historical factors that explain how a party becomes particularly trusted on the issue of corruption and that cause its supporters to vote based on candidates' corruption records is an important area for future research.

One troubling possibility raised by our findings is that increased transparency may disadvantage candidates from parties with a reputation for clean governance when they compete against candidates from parties with no such reputation. In the case of Brazil, PT candidates may be particularly vulnerable to attacks by opposing parties on the corruption issue. Increased transparency could paradoxically, at least in the short term, reduce the chances of PT candidates from winning office, even if they tend to be less corrupt than candidates from parties like the Democrats.[20] Of

[20]It is by no means clear, however, that PT politicians are still less corrupt than non-PT politicians. In recent years, political observers have actively debated whether or not the PT has abandoned its historical position as a more ethical

course, this may be an acceptable outcome to PT voters, as long as it creates a long-term incentive for PT politicians to govern without resorting to corruption. Still, the heterogeneity across candidates that we document suggests the possibility that information campaigns can actually increase the incidence of corruption in government by disproportionately punishing "clean" parties.

More broadly, our findings suggests that future experimental work on information and accountability will find varying effects across different political contexts. As we found in São Paulo, the existence of information effects can depend on highly contextual factors associated with particular candidates, parties, and the distribution of preferences in the electorate. Future work on the effects of information on political accountability will need to address more systematically how these antecedent factors affect voters' response to increased transparency. As we have documented, the relationship between information and accountability is by no means a simple one.

party and now is just a "normal" (that is a *corrupt*) party. The unlikely success of Green Party candidate Marina Silva in the 2010 presidential election may have been partly fueled by former PT voters disappointed in the party's record on the corruption issue.

Appendix I: Legal and Ethical Issues

We faced some legal and ethical issues in carrying out this project, and responded by having a number of safeguards in place. The concerns involved legal and ethical issues not only in Brazil, but also in the United States.

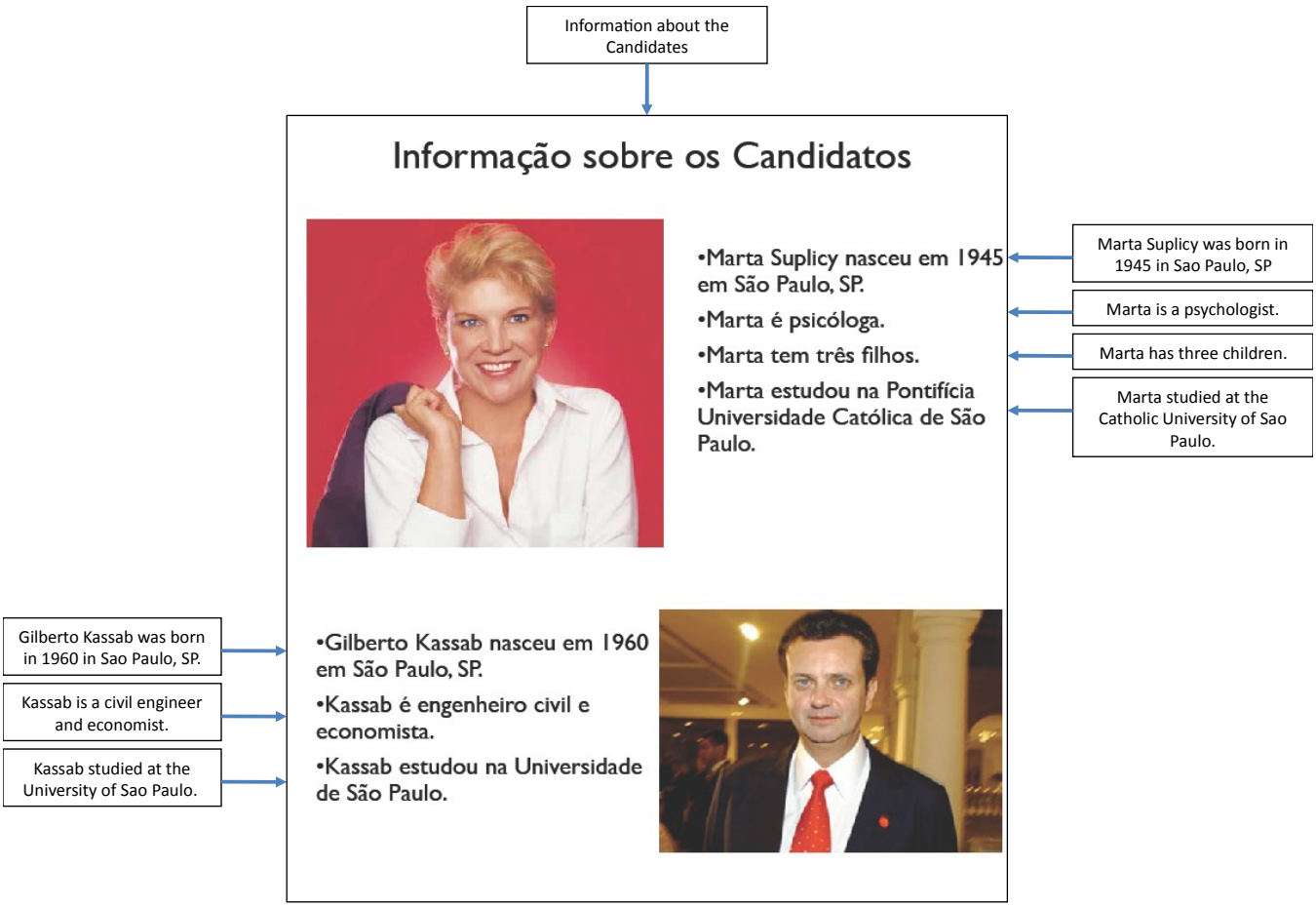
We received funding from the University of California, Berkeley, and Yale University to carry out the project. Both are non-profit (501(c)(3)) institutions that are prohibited from engaging in political advocacy. We inquired with Yale Law School's Non-Profit Organizations Clinic to make sure that we complied with this restriction, and drew on the experience of previous electoral field experiments done in the United States as a precedent for complying with this prohibition. This prohibition partly factored into our choice of São Paulo as the site where we conducted the field experiment. We not only performed the intervention in a place where both candidates had corruption convictions, but we chose the run-off election so as not to have effects on the vote shares of other candidates that could affect the outcome of the election. We also obtained approval from human subjects committees at Berkeley and Yale.

Polls immediately prior to the election from prominent organizations such as Datafolha and Ibope showed that Kassab had roughly a twenty percentage point lead over Suplicy. Our treatment of 187,177 households reached an estimated six to seven percent of the electorate of São Paulo. Even if every voter responded to the treatment, we believe the likelihood of the field experiment affecting the overall outcome was extremely unlikely. Though to our knowledge there were no prior electoral field experiments of this sort conducted in Latin America, we examined the findings of electoral field experiments conducted in other regions. The largest treatment effect for this sort of project that we found was slightly below nine percentage points (Gerber, Green and Larimer, 2008; Green and Gerber, 2008). In addition, we delivered the fliers immediately prior to the election (from October 22 until October 25, 2008) to minimize the likelihood of the information spreading to other areas, and also to decrease the chances of the parties reacting strategically to the experiment.

While in São Paulo, we sought counsel from an election lawyer to make sure we were in compliance with Brazilian electoral laws. The lawyer assured us that so long as we were not affiliated

with any candidate or party, we would be in compliance with the Brazilian Electoral Code. We also sought the opinion of a former electoral judge, who felt that the study was in compliance with local laws. Finally, we informed an electoral judge of the research design and also gave him the fliers prior to the launch of the field experiment.

Appendix II: Placebo Flier for the Survey Experiment



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