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CORRUPTION AND ELECTORAL ACCOUNTABILITY IN BRAZIL

by

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Dedicated to my wife, Florencia, my son, Julián,

my father, Ricardo, my mother, Silvia, and my sister, Karen.

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CORRUPTION AND ELECTORAL ACCOUNTABILITY IN BRAZIL

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ABSTRACT

This dissertation examines how voters react towards candidates with records of misuse of public funds in the context of sub-national elections in Brazil. Its contribution to the extant literature on corruption and electoral accountability is twofold. First, it is the first study to inquire whether voters punish candidates with malfeasance records running for both executive and legislative office in the same electoral context and whether a number of contextual factors affect electoral accountability in these offices. Second, it presents and tests new hypotheses on the type of motivation that ground voters' rejection towards corrupt candidates.

In chapter 2, I examine whether voters punish candidates for mayor and city councilman with accounts rejected by the Brazilian Audit Courts and whether additional contextual factors affect electoral accountability. In particular, I study whether electoral accountability decreases as candidates (for mayor) have better records of social provision; whether local media promotes electoral accountability; and whether candidates with negative antecedents receive fewer campaign donations and are less likely to re-run. I combine large-N observational analysis, using an original dataset with candidates' accounts rejection records, with interviews with Brazilian Audit Court members and local politicians. In chapter 3 I use three online survey experiments

with a convenience sample of Brazilian voters to examine whether likelihood to support a corrupt incumbent is affected by the details that subjects learn about the corruption incident. I use these additional details to inquire whether subjects are sensitive to information emphasizing the public costs of corruption, the candidate's moral misbehavior, or his illicit enrichment.

Results presented in chapter 2 suggest that prior records of misuse of public funds have electoral consequences both for candidates for mayor and for city councilman. In addition, they suggest that the existence of local media does not increase electoral punishment; that public spending does not reduce electoral punishment; and that candidates with accounts rejected often receive fewer funds and are less likely to re-run. Results presented in chapter 3 suggest that voters' rejection towards corrupt candidates is stronger when they learn additional details on the candidate's illegal enrichment.

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1. Introduction

The spread of corruption in democratic polities is a key concern for the international community. While elections should provide strong selection effects against corrupt politicians, experience shows that they often manege to successfully participate in the electoral game. This paradox has stimulated a growing scholarly interest on how voters react towards candidates facing corruption accusations. Scholars have addressed various questions, including whether these candidates see their share of votes reduced, whether voters are typically informed about corruption accusations, and whether there are other factors that increase or decrease electoral accountability.

Although there seems to be a consensus that these candidates lose at least part of their share of votes when facing serious corruption accusations, there still remain various puzzles. First, an aspect still understudied is whether the electoral punishment is similar across different government levels and offices. Prior scholarship has mostly focused on candidates for executive office at the municipal level (Ferraz & Finan 2008, Costas-Pérez et al. 2012, De Figueiredo, Hidalgo & Kasahara 2010, Chong et al. 2015, Pereira & Melo 2015) and candidates for legislative office at the national level (Chang et al 2010, Peters & Welch 1980, Pereira, Renno & Samuels 2011). But little is known about whether electoral punishment varies for candidates for executive versus legislative office. There are a number of factors that might affect electoral accountability across different offices, such as the level of diffusion of the corruption accusation, the responsibilities for each office that might make the corruption accusation more or less severe, and the electoral system.

Second, studies have assessed a number of factors that might condition electoral punishment, such as availability of local media, or candidate's records of social provision. However, there are often contradictory findings. For instance, while Ferraz &

Finan (2008) find that the availability of local media is key to informs voters about corruption accusations, Pereira & Melo (2015) find a non statistically significant relationship between availability of local media and electoral punishment. Similarly, while Winters & Weitz-Shapiro (2013) find no evidence that voters condone corruption when the candidate has good records of social provision, Melo & Pereira (2015) find that as candidates have better records of social provision the electoral punishment disappears. Hence more empirical evidence is needed.

Third, there is little knowledge on the type of reasoning behind voters' assessment of corrupt candidates. Even when we know that all else being equal voters will reject corrupt candidates, their rejection might be grounded on different motivations, such as a moral rejection of the candidate's lack of integrity, or an evaluation of the possible costs of the corrupt misuse of public office.

This dissertation combines a variety of approaches to examine how voters react towards candidates with prior evidence of misuse of public funds in the context of municipal elections in Brazil. In particular, I address three questions: a) do voters punish candidates with records of malfeasance of public funds?; b) what is the impact, if any, of other contextual factors such as candidates' records of social provision and the availability of local media?; and c) is the voters' reaction affected by the specific details they learn on the candidates' malfeasance records? I use a variety of approaches to address these questions, including large-N observational analysis, survey experiments and interviews with Audit Courts members and local politicians in Brazil.

In chapter 1 I combine large-N observational analysis with interviews to examine if voters punish candidates for mayor and city councilman with malfeasance records in the context of the 2008 and 2012 elections. In addition, I examine whether electoral accountability decreases as candidates (for mayor) have better records of social

provision; whether local media promotes electoral accountability; and whether candidates with negative antecedents receive fewer campaign donations and are less likely to re-run in the following elections.

I use a unique database, built for this project, with candidates' records of misuse of public funds in the 34 Federal, State and Municipal Brazilian Audit Courts between 2004 and 2012. To run the analysis I use matching with difference-in-difference estimation techniques and regression analysis. This section is complemented with interviews with members of the Audit Courts, elected officials and their staff in four states in Brazil, two states in the relatively less developed Northeast region and two states in the more advanced Southeast region[1]. I use interviews to understand how Audit Courts' decisions are publicized by both the media and by the Courts themselves. With these interviews I aim to reconstruct how, and to what extent, voters are informed about candidates' records.

In chapter 2 I use three online survey experiments with a national sample of Brazilian voters to understand whether likelihood to support a corrupt incumbent is affected by the details that subjects learn about the corruption incident. The experiments present a hypothetical incumbent mayor running for reelection with records of misuse of public funds. Subjects are presented with either a "limited information" vignette in which they receive scant information on the negative records, or one of various "extended information" vignettes in which they learn more details about these records. I test whether providing additional information on the public costs of corruption, on the benefits (for the mayor) of the corruption incident, or additional information on the mayor's dishonest behavior affects likelihood to support his reelection.

[1]The four states are Pernambuco and Ceará in the Northeast and Rio de Janeiro and São Paulo in the Southeast.

This multi-methods approach is used to capture various aspects of electoral accountability. I use the experimental strategy in order to manipulate the treatment of interest -i.e. corruption- in a controlled setting. This method is the most appropriate to examine how small variations in the information received by subjects impacts on their subsequent support for a corrupt candidate. More generally, experimental designs can be used to incorporate and test new theories that could hardly be tested with alternative designs.

I use a large-N approach to understand whether voters punish candidates with negative antecedents and whether contextual factors -which can be measured in real settings- can affect their support. The observational setting enhances the external validity of findings and offers various advantages over experimental designs. The advantages and disadvantages of each methodological strategy are discussed below.

Studies dealing with electoral accountability and corruption typical address either of two questions: a) do voters support corrupt incumbents?; and b) what other factors have an impact on voters' support (rejection) for corrupt incumbents?

Survey experiments offer various advantages to address the second question. The possibility to manipulate the treatment of interest plus any additional factor makes it relatively easy to incorporate new theories and test them with strong internal validity. For instance, Weitz-Shapiro & Winters (2015a) and Botero et al. (2015) study whether the credibility of the source of information (of the corruption incident) affects voters' support for a corrupt incumbent; Weitz-Shapiro & Winters (2015b) examine whether the public reaction changes if the official accused is the mayor running for reelection or a non elected official under his responsibility; Klasnja and Tucker (2013) and Winters & Weitz-Shapiro (2013) study whether voters are less willing to punish a corrupt mayor if he has good records of public works completed and good records of economic growth; Anduiza et al. (2013) and Rundquist et al. (1977) study if voters

are less willing to punish a corrupt incumbent if he runs for the party they support or has similar preferences on specific issue positions. All these studies consider the effect of additional variables on electoral accountability.

However, survey (and lab) experiments could lead to less reliable estimates of the effect of corruption on electoral behavior as they can potentially overestimate the effect. First, in survey experiments the information of the corruption incident is always accessible and salient, as it was provided by the researcher. In real settings voters might not learn about those accusations and if they do, they will arguably learn them in a context where more issues are debated -within the campaign- which will make them less salient.[2] In addition, survey experiments measure the outcome immediately after subjects are informed of the corruption incident. Hence, this information is more likely to be salient in their mind when measuring the outcome variable. In contrast, in real setting elections might take place several months -or years- after voters first learnt about the incident. The effect of information on behavior might weaken its effect with time, which might be another source of overestimation of treatment effect in survey experiments[3].

Observational studies offer various advantages over experimental methods and have other limits as well. They don't require the assumption that voters know about the corruption accusation. In addition, they don't require making the information artificially salient for voters. Moreover, they allow us to capture the specific effect of information provision by specific outlets, such as the local media (Finan & Ferraz 2008, Pereira & Melo 2015) or national newspapers (Chang et al. 2010, Costas-Pérez et al. 2012).

[2]Field experiments provide an interesting alternative to overcome this problem as voters learn the accusation in a context of a real campaign, where other issues are in the debate.

[3]However, if other candidates use this information during the campaign voters, the treatment will be salient immediately before the election.

However, they tend to offer more limited possibilities in terms of developing new theories, as researchers can't manipulate the information received by subjects. The testing of very specific alterations in the information received by subjects might only be attainable in controlled -i.e. experimental- settings. In addition, testing the effect of real world corruption accusations could result in less reliable estimates given the heterogeneity of these cases, as some accusations could be more severe than others.

In sum, empirical studies on electoral accountability should use a wide array of methodological strategies to gauge different aspects of it. Among the various questions that this field of study still needs to address, we should consider: a) whether voters receive the information (on the candidates' corruption antecedents); b) the duration of effects; c) whether the treatment could be categorized as binary (i.e. corrupt/non corrupt) or ordinal (i.e. distinguishing various levels of corruption); d) what other contextual factors play a role in this process.

This dissertation presents new empirical evidence showing that prior records of misuse of public funds have electoral consequences. This finding is consistent across the two chapters, and consistent with prior studies. As chapter 2 shows, this is consistent for both candidates for mayor and for city councilman. In competitive elections, such the those for City Council legislator, the loss of votes that can be attributed to the accounts rejection might have a stronger effect on the likelihood of election. The first experiment in chapter 3 also presents strong evidence that prior misuse of public funds is strongly punished. In addition, results presented in chapter 2 suggests that this punishment is less contingent upon the existence of local media than argued in prior studies (Ferraz & Finan 2008); and that other elite based mechanisms of accountability, such as the amount of campaign donations that candidates receive, can have a relevant role in the process of accountability.

Evidence presented in chapter 3 shows that voters' rejection increases as they learn

additional information on candidate's illegal enrichment, while additional information on the public costs of corruption doesn't necessarily increase the electoral costs of corruption accusations. This is consistent with evidence presented in chapter 2, which shows no evidence that more public spending decreases electoral punishment. This evidence shows that voters do not weigh candidate's corruption antecedents in terms of the prospective costs on their wellbeing. Their judgment is more likely based on a rejection of the politicians' illegal use the public funds for their own enrichment.

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2. The Effect of Accounts Rejection on Electoral Outcomes: Evidence from Brazilian Municipal Elections

Abstract

Do voters punish candidates with records of misuse of public funds? I address this question in the context of the 2008 and 2012 municipal elections for mayor and City Council legislator in Brazil using interviews and econometric analysis. I use interviews with Audit Courts' board members and officials and local politicians across four Brazilian states to describe how and to what extent Audit Courts' decisions reach the public. Analysis of interviews shows significant diffusion of those decisions by various channels, including local media, internet, social media and electoral campaigns. I use a unique dataset built for this project listing all candidates with accounts rejection records by any of the 34 Brazilian Audit Courts between 2004 and 2012. To test the effect of accounts rejection on electoral returns I use a variety of methods, including matching and difference-in-difference and regression analysis. Results show strong evidence that voters punish both candidates for mayor and City Council legislator in most elections. I subsequently consider the effect of various moderators listed in prior studies, including the existence of local media, campaign donations received by the candidates, and the use of public spending to diminish electoral punishment (in the case of incumbent mayors). Results show evidence that contradicts findings from prior scholarship. The existence of local media doesn't increase electoral punishment -and it can even decrease it in some elections-; public spending doesn't reduce electoral punishment; and candidates with accounts rejected often receive fewer funds. Overall these results show that in the context of widespread information through various channels voters punish candidates for mayor and city councilman with records of accounts

rejection, and none of those mediators have a significant role.

2.1 Introduction

In democratic polities, electoral accountability is a key mechanism to oust corrupt elites from government. Despite the successful transitions to democracy in great part of the developing world, this mechanism has often failed to remove corrupt elites both at the national and sub-national level. This paradox has stimulated a number of empirical studies on electoral accountability and corruption.[4] The number of observational studies has remained relatively limited as compared to the growth of studies using field and survey experiments.[5] This can be explained by the practical difficulties that hamper the growth of observational studies: researchers need to identify and collect data of elections with a significant number of candidates facing corruption accusations for prior office holding. These data are often hard to find and its collection requires significant time and effort. Given these limitations, it is not surprising that there are no observational studies testing the effect of malfeasance accusations on electoral outcomes for both candidates in legislative and executive offices in the same electoral context.

This is the first study to examine electoral punishment to candidates with malfeasance records in elections for both legislative and executive office. It uses an original dataset that lists all candidates for mayor and City Council legislator with records of accounts rejection by any of the 34 Brazilian Audit Courts between 2004 and 2012. A few prior studies have analyzed the effect of corruption on subsequent electoral returns for legislative office at the national level (Peters and Welch 1980, Chang,

[4]There is a large number of studies tackling this question, including Rundquist, Strom & Peters (1977), Klasnja & Tucker (2013), Andiuza et al. (2013), Ferraz & Finan (2008), Chang, Golden & Hill (2010), Weitz-Shapiro & Winters (2013), Costas-Pérez, Solé-Ollé & Sorribas-Navarro (2012), Palau & Davesa (2013), Botero et al. (2015), Weitz-Shapiro & Winters (2015a, 2015b), Pereira & Melo (2015), Weitz-Shapiro & Winters (2013).

[5]Among the exceptions we should include Ferraz & Finan (2008), Andiuza et al. (2013), Pereira & Melo (2015), Peters and Welch (1980), Chang et al. (2010), Pereira, Renno & Samuels (2011).

Goldman & Hill 2010, Pereira, Renno & Samuels 2011) and for executive office at the municipal level (Ferraz & Finan 2008, Costas-Pérez et al. 2012, De Figueiredo, Hidalgo & Kasahara 2010, Chong et al. 2015, Pereira & Melo 2015) but this is the first study to analyze this issue in the same electoral context for both executive and legislative office, and the first one to study electoral accountability for legislative office at the sub-national level. It takes advantage of an electoral system for legislative office -open list proportional representation- that allows voters to select among candidates within the same party[6]. Hence, they can avoid selecting candidates with accounts rejection without the need to shift their party of choice.

In addition, I study the role of additional factors that have been listed as moderators in prior scholarship. In particular, I test whether the availability of local media increases electoral accountability, whether accountability diminishes as -incumbent-candidates have better records of social provision, and whether candidates with accounts rejected receive fewer campaign donations.

The econometric analyzes are complemented with interviews with Audit Court members and local candidates to investigate how this information is publicized. Interviews were conducted in four Brazilian states, two in the relatively more advanced Southeast region and two in the relatively less developed Northeast.[7] I aim to reconstruct to what extent and by what channels voters learn about these decisions.

2.2 Previous Studies

A majority of studies on electoral accountability towards corrupt candidates suggest

[6]Voters can choose either one party as a whole or an individual candidate within the party, but most voters select individual candidates

[7]The four states are Pernambuco and Ceará in the Northeast and Rio de Janeiro and São Paulo in the Southeast.

that they receive an electoral punishment (Ferraz and Finan 2008, Pereira, Renno and Samuels 2011, Chong et al. 2015, Winters & Weitz-Shapiro 2013, Weitz-Shapiro & Winters 2015a, 2015b, Klasnja & Tucker 2013, Pereira & Melo 2015), although this doesn't necessarily prevents their reelection (Peters and Welch 1980)[8]. This finding seems to hold for national legislative elections (Peters and Welch 1980, Chang, Golden & Hill 2010) and for local executive office (Ferraz & Finan 2008, Pereira & Melo 2015, Chong et al. 2015).

However, there is currently no scholarly consensus on whether punishment is affected by contextual factors such as media attention on corruption accusations and incumbent candidate's records of social provision. Ferraz and Finan (2008) find that voters are more likely to punish incumbents who received negative audits in Brazilian local elections in municipalities with at least one local radio (the most influential media in the majority of Brazilian municipalities). Similarly, Chang, Golden, and Hill (2010) find that electoral punishment towards Italian legislators accused of corruption since 1948 was virtually nonexistent until the early 1990s, when electoral accountability increased as a result of more media coverage of corruption scandals. Those studies suggest that media attention is a key factor in promoting electoral accountability.

Winters & Weitz Shapiro (2013) use a survey experiment in Brazil to test the "trade off" hypothesis (i.e. that voters are willing to condone corrupt candidates with good record of social provision) versus the "information hypothesis" (i.e. that voters will punish those candidates if they have sufficient information on the corruption records). They find that voters do punish corrupt incumbents regardless of their social provision records. This finding is challenged by Pereira & Melo (2015) in a study on the effect of negative audits on electoral outcomes in one Brazilian state.

[8]In contrast, Banerjee et al (2010) find no punishment effect in India, while De Figueiredo, Hidalgo & Kasahara (2010) find that only one of two competing candidates with corruption antecedents receive electoral punishment

They find that public spending decreases the negative impact of corruption on re-election likelihood (the "trade off hypothesis"). They also find that existence of local radios doesn't significantly decrease reelection chances of corrupt incumbents.

Observational studies are better suited to test the information hypothesis, as in both field and survey experiments the researcher is providing the treatment to subjects. With the only exception of Pereira & Melo (2015) those studies suggest that media has a prominent role. In particular, in the Brazilian context, Ferraz and Finan (2008) argue that local radios have a key role in making information on corruption accessible to the public. However, we also know that in Brazilian municipalities local radios are often owned or controlled by local political elites (Boas & Hidalgo 2011, Boas 2014). If mayors with negative audits are the owners of local radios, then they will make sure that either information on those records is not accessible to the public, or they will use the media to make their case against the procedures or findings of those audits.

A different hypothesis, posited by Pereira, Renno & Samuels (2011), is that corrupt incumbents see their share of campaign donations reduced. This would be an alternative channel by which those candidates receive an electoral punishment. They test this hypothesis in the context of federal legislative elections in Brazil and find that corrupt incumbents suffer a significant loss in their campaign donations.

This paper provides a new empirical test on the information and campaign donations hypotheses for both local legislative and executive elections and on the trade off hypothesis for local executive elections (which is the office that can claim credit on public spending).

2.3 Accounts Rejection in Brazilian Municipal Governments

Audit Court's role is sanctioned by the Brazilian constitution, and consists in controlling the use of public funds at all levels of public administration, including the three government branches. Although formally they take part in the legislative branch, they operate as quasi-independent judicial authorities, enjoying secure tenure until retirement (similar to the judicial authorities), and operate with procedures similar to the judicial bodies, such as right of reply and collegial decision making (Melo, Pereira and Figueiredo 2009: 1224).

All local governments -as well as the federal and state governments- are required to annually present reports of their use of public funds to show that they have been used according to the norms and procedures of public administration. Accounts submitted to the Audit Courts can be approved, approved with reservations, or rejected. Decisions are reach by a body of board members.

Mayors have the most important responsibilities in managing the municipal funds and consequently are particularly subjects of oversight by the Audit Courts. The most frequent cases of account rejection for mayors are: a) failure to comply with the laws requiring to spend at least 25% of municipal total expenditures in education and at least 15% in public health; b) over-expenses, no-bid purchases and use of fake receipts; c) mismanagement of pension funds; d) failure to present accounts to the Audit Court[9].

The president of the City Council has various specific responsibilities audited by the Audit Courts. In particular, they are responsible for setting the annual expenses of the Council in accordance to the law, for managing the contributions to social pen-

[9]This enumeration is drawn from interviews with various Audit Court board members across four states in Brazil (interview with Ramalho 2016, interview with an anonymous board member of the State of Pernambuco Audit Court 2015, interview with Ponte 2015, interview with Massa 2015).

sions of city councilmen, for setting their salaries, and for managing the funds of the Council — although each city councilmen might manage funds allocated specifically for him. These responsibilities might render them subject to accounts rejection for the following reasons: a) exceeding the limit of annual expenses of the City Council, which should range between 5 and 8 percent of total municipal income[10]; b) exceeding the allowed limit on the annual salaries of city councilmen -also established by law-; c) failing to pay or provide proof of contribution to the social pensions of city councilmen; d) over-expenses, no-bid purchases and use of fake receipts in the use of Council funds for purchases or hiring staff.

In addition, all city councilmen might be subject to accounts rejection for the use of City Council funds provided for specific duty related expenses. The most common cases of account rejection for city councilmen are: a) over-expenses, no-bid purchases and use of fake receipts; b) exceeding reasonable expenses in hiring staff; c) use of public funds in travel expenses for trips unrelated to their City Council duties[11].

In case of a Court decision to reject accounts, there are five possible instances of appeal to reconsider the decision. Once all possibilities of appeal have been exhausted, the rejection of accounts is considered definitive. In the case of mayors, this decision needs to be confirmed by 2/3 of the City Council (the Audit Court's decision is labeled a "pre-assessment").

There are two pieces of legislation that affect candidates' eligibility for office after accounts rejection. The first one is a law passed in 1990 which sanctions that all citizens who had definitive accounts rejection by an Audit Court (i.e. there are no

[10]The annual budget of the municipal council is in practice determined by the council. While the mayor in theory can reject the municipal council budget this does not happen in practice. Hence, municipal councils have few incentives to abide to the constitutional limit (Mendes 2009).

[11]Interview with Ramalho 2016, interview with an anonymous board member of the State of Pernambuco Audit Court 2015, interview with Ponte 2015, interview with Massa 2015, interview with an anonymous board member substitute of the State of Pernambuco Audit Court 2015.

possibilities of appeal) due to an act of administrative mismanagement that involves intention are ineligible for 8 years[12]. In the case of mayors, this restriction only applies when the Audit Court's decision was confirmed by 2/3 of the City Council. More recently, in 2012, the Supreme Court determined the constitutionality of the recently sanctioned "Clean Records" law, which extends non eligibility for all officials who had accounts rejected by the Audit Court's collegial board, that is, in any instance even before all possible appeals have been exhausted.

Had these laws been effectively imposed, electors wouldn't have the choice to select a candidate with antecedents of accounts rejection. In practice, however, candidates have various ways to circumvent this restriction using different legal subterfuges. First, the candidacy to be banned should be impeached by the Electoral Tribunal, the Electoral Prosecutor's Office (*Ministério Público Eleitoral*), a political party or another candidate within 5 days of request of registration of candidacy to the Electoral Tribunal (Wargas Neto 2010). Once that period has passed, the candidate is eligible if his candidacy was not contested.

Second, even if the accounts rejection exhausted all possibilities of appeal in the Audit Court, the candidate can appeal the formal aspects of the decision to the Judiciary[13] (Wargas Neto 2010: 9). While the Judiciary reconsiders the decision, the impeachment of the candidacy is annulled and the candidate will be able to run for office, as the final decision will usually take place after the election. The raising of the impeachment takes place regardless of the merits of the demand, so candidates can present an appeal with any argument on the eve of the election and often the final decision of justice (which might attain the annulment of the votes received by

[12]According to the jurisprudence of the Superior Electoral Tribunal of Brazil this act should involve an administrative wrongdoing involving intentionality, affecting the public interest, seeking a private advantage of the official, which might be non material (Wargas Neto 2004: 11).

[13]In this case, the Judiciary cannot overrule the Courts decision based on its content, but it can object the procedural mechanisms followed to reach such decision.

the candidate) takes place long after, which obviously undermines the effectiveness of the whole process (Wargas Neto 2010: 13).

Various Audit Court members confirmed in interviews with the author that it is not hard for a candidate to appeal their decision using a variety of arguments, as long as the appeal doesn't involve the substance of the Court's decision but rather the formal procedures of it[14]. As an experienced local politician explains, the candidate might argue, for instance, that a specific document was not accepted in his defense under the Audit Court; this would be enough to open a process under the Judiciary -which will probably take years- and will temporarily annul the Audit Court's decision, allowing him to run in the election (interview with an anonymous legislator in the State Assembly of Ceará 2015). Some of the concepts used in the legislation required for ineligibility are rather vague, such as administrative incapability, leaving another possible subterfuge for appeals to the Judiciary (interview with Massa 2015). Candidates can also argue that the administrative action for which they were charged was not intentional (interview with an anonymous board member substitute of the State of Pernambuco Audit Court 2015, interview with Pimentel 2015). Within the Judiciary tends to prevail the idea of presumption of innocence and hence candidacies tend to be accepted (interview with Massa 2015). However, some candidates refrain from appealing to the Judiciary as they could find it difficult to follow the required judicial procedures while at the same time running the campaign (interview with Ramalho 2016).

In this fashion, 1,572 candidates who run for City Councils in 2008 had their accounts rejected between 2008 and 2012 (out of a total number of 321,127 candidates

[14]Interview with an anonymous official of the Public Ministry in the Municipal Audit Court of Ceará 2015, interview with an anonymous board member of the State Audit Court of Pernambuco 2015, interview with an anonymous state legislator in the State Assembly of Ceará 2015, interview with Pimentel 2015, interview with Massa 2015.

who run in the 2008 election); 510 of them attempted to re-run in the 2012 election (345 of them had been incumbents in the 2008 election); and 458 successfully registered to run in 2012. In the case of candidates for mayor, 1,027 candidates for mayor in 2008 had accounts rejected between 2008 and 2012 and 144 relisted in the 2012 election.

The number of candidates with records of accounts rejection in any given election doesn't restrict to incumbents. Candidates might have accounts rejected for any prior office holding -as the process of definitive accounts rejection might take several years-. The issue of whether accounts rejection might impact on candidates decision to re-run is further discussed in section 10.

There are two institutional features of Audit Courts that might make them less effective in punishing corrupt mayors. First, a portion of these members are appointed by the state executive[15] -subject to the approval of a simple majority of the state legislature- with few restrictions, or by the legislature itself. Hidalgo et al. (forthcoming) show that because many of these members are retired politicians with links to the local elites, they are less prone to reject municipal accounts[16]. This evidence is consistent with critics charging that the Audit Courts' design is ineffective (Hidalgo et al., forthcoming). This characteristic would potentially result in less punitive Courts (since some corrupt mayors might end up not being punished) but not biased towards prosecuting non-corrupt mayors (which could result in decisions of accounts rejection non credible for the public). Second, Melo, Pereira and Figueiredo (2009) show that the effectiveness of Audit Courts varies across states being positively related to power alternation and negatively related to voter volatility. This feature could make some state Audit Courts more effective in detecting corrupt politicians than others. This

[15]Or by the municipal executive in the few cases where there are specific Municipal Audit Courts.

[16]According to one interviewee this pro-government bias is more pronounced in the case of state governors that in the case of mayors (interview with Pacheco 2016).

wouldn't affect their negative decisions' credibility (insofar as this feature doesn't result in non corrupt officials being sanctioned), but would make less credible their positive decisions (to not reject officials' accounts).

2.4 Publicity of Audit Courts' Decisions

If electors are not aware of Audit Courts' decisions they will not punish condemned candidates. It is important, hence, to describe in detail how, and to what extent, Audit Courts' decisions receive publicity.

All Brazilian Audit Courts' sessions are open to the public and their most relevant decisions -such as those involving local governments' accounts- are published in their websites. In some states, all sessions are broadcasted online (interview with Ramalho 2016). In others, cases of larger public repercussion, such as those judging municipal governments' accounts, are broadcasted by local TV stations (interview with Ponte 2015).

Arguably, a more effective source of publicity comes from the Audit Courts' links with the media. Audit Courts often have specialized officials who work closely with the media on the diffusion of decisions that involve accounts rejection of elected officials (interview with Massa 2015, interview with Ramalho 2015, interview with Pimentel 2015). They daily select among all Courts decisions those that might be of interest for the press and submit them to local newspapers and radio stations. Audit Courts can also allocate specific budget for this purpose. For instance, the Audit Court of the state of Pernambuco has two weekly paid columns in the two main state newspapers. Those columns are used to inform the public on the main Audit Courts weekly decisions, including those affecting mayors' accounts (interview with an anonymous board member substitute in the State of Pernambuco Audit Court). As an Audit

Court Board Member explains, the Courts are expensive public structures; therefore, they understand that one of their main tasks is to show the public that they have a productive role for the community (interview with an anonymous board member substitute of the State of Pernambuco Audit Court 2015).

The media has a continuous interest in news involving elected officials' accounts rejection.[17] Some outlets might even have specialized reporters who attend daily the Audit Court sessions covering the most relevant decisions (interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015). Those specialized reporters often have their own blogs where they reprint and cover Audit Courts news with more detail (interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015). Accounts rejection involving use of federal funds (in which case it is responsibility of the Federal Audit Court to reject accounts) typically reaches the national media. According to one interviewee, it is very common to see in the *O Globo* headlines (one of the main newspapers in Brazil) news involving decisions by the Federal Audit Court (interview with Barbosa de Souza 2016).

Radio stations are largely the most influential media outlet in Brazil, as newspapers have very low rates of circulation (Ferraz & Finan 2008). Even when TV might reach an even higher number of Brazilians (97.1 % as compared to 72.1 % of Brazilians with access to radios) the number of municipalities with local TV outlets is rather limited (only 4%) (IBGE 2014, Speck & Cervi 2015). Hence, those outlets will not include coverage of local events in most municipalities. In contrast, 67% of municipalities have their own radio station, which will typically cover local events

[17]Interview with Ramalho 2015, interview with an anonymous official of the Public Ministry of the State Audit Court of Ceará 2015, interview with Pimentel 2015, interview with an anonymous legislator of the State Assembly of Ceará 2015, interview with Ponte 2015, interview with an anonymous board member substitute of the State of Pernambuco Audit Court 2015).

(Speck & Cervi 2015).

Local radio stations have a very active role in the diffusion of news of accounts rejection of local governments (interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015, interview with Pimentel 2015). Those stations have a multiplying effect on the diffusion of news on Audit Courts' decisions published in other outlets which might have a more limited circulation, such as newspapers or specialized blogs[18]. As one experienced local politician explains, blogs, social media, and newspapers are very active in publishing news on corruption and Audit Courts' decisions, and radio stations pick up those news and amplify the diffusion (interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015).

However, these stations are often owned or controlled by local political bosses (Boas 2014, Boas & Hidalgo 2011), in which case those news will receive little diffusion. The degree of influence might be conditioned by the size of the municipality: larger municipalities have more media diversity; making it harder for any local boss to have complete control of it. According to various interviewees, when mayors from larger municipalities have accounts rejected, that news will receive substantive media attention, at least by some non controlled outlets (interview with Pimentel 2015, interview with an anonymous board member substitute of the State of Pernambuco Audit Court 2015). That news might even reach the main state outlets, such as state newspapers and local TVs (interview with an anonymous legislator of the State of Ceará Assembly 2015). In contrast, the diffusion of news of accounts rejection of mayors from smaller municipalities might be contingent upon the existence of radio stations owned or influenced by opponent politicians (interview with an anonymous

[18]Interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015, interview with an anonymous legislator of the State Assembly of Ceará 2015, interview with Pimentel 2015, interview with Ramalho 2015.

official of the Public Ministry of the Municipal Audit Court of Ceará 2015).

Another important source of diffusion are blogs. Many specialized blogs (often owned by specialized journalists) regularly report on Audit Courts' publicly relevant decisions (interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015, interview with Pimentel 2015). Although their public reach might be limited, they are a common source of information for radio stations, which multiplies the diffusion of those decisions (interview with an anonymous legislator in the State Assembly of Ceará 2015).

One of the main events by which Courts' decisions are publicized take place shortly before the elections. Three months before each election all federal, state and municipal Audit Courts are required by law to provide the regional Electoral Tribunals with a list of all present and former officials -including candidates in the current election- whose accounts have been definitively rejected during the previous 8 years (as they might be subject to the ineligibility law). These lists are made public in the Audit Courts' websites.

More importantly, the publication of these lists is a major event for all media[19]. According to one Audit Court board member, it is one of the top news on the media agenda during the publication time (interview with Ponte 2015); as another Audit Court board members puts it: "it is part of the electoral climate" (interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015). Radio stations often read out load the lists in their broadcasts, and if there is any member of the local government in that municipality this will arguably be covered if there is a local radio run by a member of an opponent party (interview with

[19]Interview with an anonymous official of the Secretary of External Control of the Federal Audit Court 2016, interview with Ponte 2015, interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015, interview with an anonymous mayor of a municipality in the State of Ceará 2015, interview with an anonymous board member substitute of the State of Pernambuco Audit Court 2015.

an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015). In some states, the Audit Courts' presidents are interviewed on TV where they explain the significance of the list (interview with an anonymous board member substitute of the State of Pernambuco Audit Court 2015). As one mayor from a small municipality in the Northeast explains, "the media love those lists" (interview with a mayor of a municipality in the State of Ceará 2015).

Media attention to those lists is in part a product of the civil society mobilization through demonstrations and collection of signatures to promote the enactment of the "Clean Records" law[20] (interview with Pimentel 2015, interview with Massa 2015, interview with Ponte 2015, interview with an anonymous official of the Public Ministry of the Municipal Audit Court of Ceará 2015, interview with Pacheco 2016). The lists of candidates with accounts rejected became popularized as the "dirty records" lists, and candidates in that list became known as "dirty records candidates", undermining their reputation (interview with Pacheco 2016). According to one Audit Court prosecutor: "Now people pay a lot of attention to the lists of "dirty records", politicians have much more pressure" (interview with Massa 2015).

Political campaigns are another important form of diffusion, as challengers will discredit incumbents with records of accounts rejection (interview with an anonymous legislator of the State Assembly of Ceará 2015) and will provide the media with information on incumbents' negative records (interview with Ponte 2015). The accusation that the mayor used a certain amount of funds for a specific project without justification becomes part of their toolkit to attack incumbents (interview with an anonymous legislator of the State Assembly of Ceará 2015, interview with Ponte 2015, interview with an anonymous assistant of a city councilman in the city of Recife 2015).

[20]On the civil society mobilization leading to the enactment of the "Clean Records" Law see Doin et al. (2012)

2.5 Hypotheses

In the following sections I present results for the econometric analyzes. I test the following hypotheses:

Hypothesis 1: Candidates with accounts rejected have their share of votes reduced and are less likely to be reelected.

Hypothesis 2: Electoral accountability is higher in municipalities where there are local radio stations available.

Hypothesis 3: Electoral accountability decreases when the incumbent candidates with accounts rejected have positive records of public spending.

Hypothesis 4: Candidates with accounts rejected receive fewer campaign donations.

2.6 Data

2.6.1 Treatment and Outcome Variables

The treatment is a definitive accounts rejection by any of the 34 Brazilian Audit Courts when the official exhausted all possible appeals; which is prior to the City Council vote (in the case of mayors). The total number of candidates with accounts rejected in the mayoral elections is 611 (in the 2012 election) and 658 (in the 2008 election). The total number of candidates for City Council with accounts rejected is 1,767

(in the 2012 election) and 1,307 (in the 2008 election). As outcome variables I use the percentage of valid votes and a binary variable indicating if the candidate was elected.

2.6.2 Interaction Terms

For the interaction terms on local media I use a dummy indicating whether the municipality has at least one local radio AM, and another dummy indicating if the municipality has at least one local radio FM. For the interaction term on public spending I use data on the total per capita spending on education, health, transportation and housing at the municipal level from the National Secretary Treasury (*Secretaria do Tesouro Nacional*). Because only incumbent mayors can claim credit for the use of public spending, this interaction is only used in the analyses restricted to incumbent mayors.

2.6.3 Independent Variables

The independent variables included in the models are candidate's party, state, three dummy variables indicating if the candidate was incumbent in the 2008, 2004, or 2000 election, age, gender and education, all factors that arguably can have an impact on electoral outcomes. An additional campaign spending variable (relative to the total municipal spending) is included in specific models -and omitted in others-. Its inclusion will be specified in each analysis. For the analysis of city councilmen I include a dummy indicating if the candidate belongs to the same party than the elected mayor and a dummy indicating if he or she belongs to a party that is part of the ruling coalition.

2.7 Methods

To estimate the effect of accounts rejection on electoral outcomes I use two different estimators: matching with difference-in-difference and regression analysis. The difference-in-difference provides an estimation of the difference between "treated" candidates (i.e. candidates with accounts rejected) and "non-treated" candidates in the 2012 and 2008 election; that is, before and after the treatment of interest. To match candidates along a set of covariates I use propensity score matching, which measures the conditional probability of exposure to a treatment for the observed covariates (Rosenbaum 2010). While in the pre-matching analysis we expect that the units' conditional probability of being part of the treatment or control group differs, we match on a set of covariates to ensure that the treatment and control group will only differ in the treatment of interest. This quasi-experimental design results in potentially better estimates, as it compares trends in both treatment and control groups before and after the inclusion of the treatment. This analysis uses a smaller sample of "treated" candidates as this sample is restricted to those who had a first accounts rejection between 2008 and 2012 (before the election) and run in both elections. Because this sample includes only candidates with accounts rejection between 2008 and 2012 the diffusion is likely to be more salient in voters' mind at the time of the election, potentially leading to higher treatment effects.

A potential problem for the difference-in-difference estimate is that Audit Courts can publicize decisions to reject accounts that could be appealed -and hence are not definitive- over the course of various years. The entire institutional process leading to a definitive accounts rejection takes many years. This means that prior to 2008 some voters may have learnt about Audit Courts decisions, even if those decisions were not definitive. This institutional feature results in treatments that could be

conceptualized as a series of "dosages"; that is, information released over the course of various years. As a consequence, the treatment effect in the difference-in-difference analysis could be underestimated (as the pre-treatment estimate would contain some "dosage" of treatment).

For this reason I include regression analysis, which doesn't require to identify any pre-treatment estimate (and hence could provide a better model for a treatment released over the course of the years). In addition, regression analysis can use larger samples leading to more statistical power, as it also includes "treated" candidates with accounts rejected prior to 2008. Regression analysis also allows to estimate both the treatment effect for a complete sample of candidates and also for a sample restricted to only incumbents — this restriction is not possible in the difference-in-difference given the reduced sample size[21]. The regression analysis is also used to assess the effect of interaction terms.

The sample used in each estimate can be summarized as follows:

Matching and difference-in-difference:

Sampling frame: All candidates in the 2008 and 2012 elections.
Effective sample: Those who run in both elections.
Treatment group: Those with accounts rejected between 2008 and 2012.
Control group: All others.

[21]While in the difference-in-difference all "treated" candidates had accounts rejected between 2008 and 2012, those candidates are not necessarily incumbents, as the accounts rejection might be a product of an office holding prior to 2008. The interaction in the regression analyses -non reported- between incumbency and accounts rejection shows that incumbent candidates with accounts rejected suffer a stronger electoral punishment than non incumbents. The difference is significant in some models, although not in all models.

Regression analysis:

Sampling frame: All candidates in the 2008 and 2012 elections.
Effective sample: Candidates who run in 2008 and candidates who run in 2012 (the outcome measure is in each single election). Analyses with complete sample of candidates and with sample of incumbents only (specified in each model).
Treatment group: Those with accounts rejected between 2004 and 2008 (in analyses for the 2008 election) and those with accounts rejected between 2004 and 2012 (in analyses for the 2012 election).
Control group: All others.

2.8 The Effect of Accounts Rejection on Electoral Outcomes in Elections for Mayor

2.8.1 Matching and Difference-in-Difference

In table 2.1 I present pre and post matching balance statistics. It shows that matching significantly improved the covariate balance in most variables, with the exception of gender, which nonetheless still shows a relatively high p-value.

TABLE 2.1: Pre and Post-Matching Balance between Treatment and Control Groups
Candidates for Mayor

	Propensity Score Pre-Matching (P-Value)	Matching Post-Matching (P-Value)
Candidate Spending/Municipal Spending	0.09	0.81
Incumbent (2008)	0.20	0.81
Incumbent (2004)	0.00	0.99
Incumbent (2000)	0.03	0.82
Age	0.01	0.43
Municipal Spending (2005-2008)	0.00	0.53
Male	0.72	0.33
Education University Complete	0.77	0.82
Education High School Complete	0.90	0.85
Education Primary School Complete	0.05	0.66
Education Reads and Writes	0.00	1.00
Party PMDB	0.17	0.86
Party PSDB	0.08	0.19
Party PT	0.31	0.41
State RJ	0.64	0.64
State SP	0.00	0.72

In table 2.2 I present results for the difference-in-difference using percentage of valid votes as dependent variable. The estimate represents the electoral loss, in terms of percentage points, of "treated candidates" in the 2012 election (relative to their vote share in the 2008 election) minus the electoral difference in vote share of the "control candidates" in the 2012 election (relative to their vote share in the 2008 election). Results show a loss of 4.60 percentage points for candidates with accounts rejected.

The magnitude of the difference is relatively high, although in the context of mayoral elections is lower than the average margin of difference of percentage votes between the winner and the second candidate. In the 2012 election the average percentage of votes for winners was 55.09 % with an average difference of 16 percentage points with the second. In the 2008 election the average percentage of votes for winners was 56.46 % with an average difference of 17.99 percentage points with the second. As we can see in table 2.3, accounts rejection doesn't have a statistically significant effect on the likelihood of being reelected (with a sample of 119 matched candidates).

TABLE 2.2: Effect of Accounts Rejection on Vote Share
Candidates for Mayor

	Propensity Score Matching	
	Estimate	Standard Error
Accounts Rejection	-4.60**	1.98
Matched Observations	119	

Difference in Difference. Estimate measured as percentage of valid votes.

Abadie-Imbens Standard Error. ***p<0.01, **p<0.05, *p<0.1 (two-tailed test).

TABLE 2.3: Effect of Accounts Rejection on Election Likelihood
Candidates for Mayor

	Propensity Score Matching	
	Estimate	Standard Error
Accounts Rejection	-0.06	0.06
Matched Observations	119	

Difference in Difference. Dependent Variable: Likelihood of Reelection.

Abadie-Imbens Standard Error. ***p<0.01, **p<0.05, *p<0.1 (two-tailed test).

2.8.2 Regression Analysis

Tables 2.4 and 2.5 present various models using the samples of all candidates and incumbents only with candidates who had accounts rejected between 2004 and 2012. The candidate spending variable (with the campaign funds that the candidate spent in 2012 as a share of total campaign funds spent in that municipality) was omitted in all models as it might partially reflect changes in campaign donations, which are affected by the treatment (as discussed in section 2.11). The inclusion of these controls doesn't substantively modify these results (although generates a marginal reduction in the size of the treatment coefficients). In table 2.4 I estimate the effect of accounts rejection in reelection likelihood and vote share in the 2012 election. Model 1 in that table shows a statistically significant reduction in the reelection likelihood of candidates for mayor. Model 2 uses the candidate's vote share as the dependent variable, and shows a significant loss of 3.11 percentage points. Models 3 and 4 show similar analyses with the sample restricted to incumbents only, also showing a significant reduction in the likelihood of being reelected and in the percentage of valid votes. Models 5 and 6 use an interaction between accounts rejected and existence of a local AM radio (Model 5) or FM radio (Model 6) with the sample of all candidates. None of these interactions are significant, suggesting that the existence of local media doesn't have a significant impact on the effect of accounts rejection on electoral outcomes. Model 7 uses an interaction between public spending and accounts rejected with the sample restricted to incumbents. Results show no significant interaction, suggesting that voters don't trade public spending for transparency.

TABLE 2.4: Effect of Account Rejection. Candidates for Mayor. 2012 Election.

	Model 1 Election	Model 2 Vote Share	Model 3 Reelection	Model 4 Vote Share	Model 5 Election	Model 6 Election	Model 7 Reelection
Accounts Rejected	-0.49***	-3.23***	-0.70***	-3.40**	-0.43**	-0.49**	-1.12*
(2004-2012)	(0.15)	(1.23)	(0.21)	(1.55)	(0.17)	(0.20)	(0.70)
Municipal Spending	-0.00	-0.00***	0.00	0.00	0.00***	0.00***	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Incumbent 2008	0.36***	7.93***	–	–	0.33***	0.33***	–
	(0.07)	(0.59)	–	–	(0.07)	(0.07)	–
Incumbent 2004	-0.18	-1.07	-0.30	-7.10	-0.12	-0.12	-0.08
	(0.15)	(1.23)	(0.87)	(6.26)	(0.16)	(0.16)	(0.07)
Incumbent 2000	0.17	2.56**	-0.07	-0.35	0.15	0.15	-0.07
	(0.14)	(1.09)	(0.20)	(1.43)	(0.14)	(0.14)	(0.20)
Party PT	-0.09	-2.36*	-0.31	-4.61***	-0.13	-0.13	-0.31
	(0.16)	(1.27)	(0.24)	(1.68)	(0.16)	(0.16)	(0.24)
Party PSDB	0.18	1.06	0.09	-1.83	0.15	0.15	0.09
	(0.15)	(1.24)	(0.23)	(1.62)	(0.16)	(0.16)	(0.23)
Party PMDB	0.14	1.26	0.10	-1.84	0.09	0.09	0.09
	(0.15)	(1.17)	(0.21)	(1.53)	(0.15)	(0.15)	(0.21)
State SP	0.08	2.00	0.92	12.6**	0.08	0.08	0.92
	(0.52)	(3.90)	(0.80)	(5.85)	(0.52)	(0.52)	(0.80)
State RJ	-0.40	-4.20	0.72	4.44	-0.29	-0.29	0.72
	(0.55)	(4.27)	(0.89)	(6.43)	(0.58)	(0.58)	(0.88)
Age	-0.03***	-0.29***	-0.03***	-0.32***	-0.03***	-0.03***	-0.03***
	(0.00)	(0.03)	(0.00)	(0.04)	(0.00)	(0.00)	(0.00)
Gender (Male)	0.35***	2.00**	0.52***	3.40***	0.34***	0.34***	0.52***
	(0.11)	(0.90)	(0.17)	(1.23)	(0.12)	(0.12)	(0.17)
University Complete	15.0	25.9	14.7	23.0	15.1	15.1	14.7
	(882.7)	(16.4)	(882.7)	(15.2)	(882.7)	(882.7)	(882.7)
High School Complete	15.2	25.5	14.9	24.1	15.2	15.2	14.9
	(882.7)	(16.6)	(882.7)	(15.2)	(882.7)	(882.7)	(882.7)
Primary School	15.0	25.9	14.6	24.4	15.0	15.0	14.6
Complete	(882.7)	(16.7)	(882.7)	(15.3)	(882.7)	(882.7)	(882.7)
Reads and Writes	15.1	29.1*	14.9	20.5	15.1	15.1	14.9
	(882.7)	(16.9)	(882.7)	(16.1)	(882.7)	(882.7)	(882.7)
Accounts Rejected * Radio AM					-0.28		
					(0.36)		
Accounts Rejected * Radio FM						0.10	
						(0.30)	
Accounts Rejected * Municipal Spending							-0.00
							(0.00)
Intercept	-14.0	29.9*	-13.9	35.7**	-14.0	-14.0	-13.9
	(882.7)	(17.1)	(882.7)	(16.5)	(882.7)	(882.7)	(882.7)
Incumbents Only	No	No	Yes	Yes	No	No	Yes
N	15,013	15,013	1.939	1,939	15,013	15,013	1,939
N Accounts Rejected	611	611	134	134	611	611	134

***p<0.01, **p<0.05, *p<0.1 (two-tailed test). Standard errors in parenthesis
Models 1, 3, 5, 6 and 7: Logistic regression. Models 2 and 4: OLS.
All models include full state and party dummies.
Models 5 & 6 include single interaction term as control (not reported).

In table 2.5 I present similar analyses for electoral and reelection outcomes in the context of the 2008 election. Here, in none of the models accounts rejection shows a significant coefficient. This suggests that contextual factors related to the 2012 elections -such as the mobilization for the enactment of the "Clean Records" law- might have facilitated the spread of information of candidates' accounts rejection antecedents.

As can be seen in models 5 and 6, the radio AM and radio FM interactions have a positive coefficient and significant in the case of the latter. However, the coefficient of the effect of accounts rejection in municipalities with radio stations (-0.11 + 0.39 = 0.28) is not significant, with a standard error of 0.15. The interaction of accounts rejection and municipal spending is negative and significant, providing evidence against the trade off hypothesis. In model 7 the positive sign and significance of the accounts rejection coefficient has little substantive implications[22].

[22]As suggested by Brambor, Clark and Golder (2006) this coefficient should be interpreted as conditional on the public spending interaction, and only true in the case when per capita public spending equals 0.

TABLE 2.5: Effect of Account Rejection. Candidates for Mayor. 2008 Election.

	Model 1 Election	Model 2 Vote Share	Model 3 Reelection	Model 4 Vote Share	Model 5 Election	Model 6 Election	Model 7 Reelection
Accounts Rejected	0.05	0.62	0.05	-1.69	0.03	-0.11	0.47**
(2004-2008)	(0.10)	(0.83)	(0.19)	(1.43)	(0.11)	(0.13)	(0.23)
Municipal Spending	-0.00***	-0.00***	0.00	0.00	0.00***	0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Incumbent 2004	1.15***	17.5***	–	–	1.51***	1.15***	–
	(0.05)	(0.40)	–	–	(0.05)	(0.05)	–
Incumbent 2000	0.01	6.60***	-0.40	-7.35	0.13	0.13	-0.30
	(0.01)	(0.71)	(0.78)	(6.40)	(0.08)	(0.08)	(0.79)
Party PT	-0.25***	-6.61***	0.19	-0.87	-0.24***	-0.24***	0.20
	(0.08)	(0.74)	(0.20)	(1.51)	(0.08)	(0.08)	(0.20)
Party PSDB	0.14*	1.18	0.15	0.47	0.14	0.14	0.15
	(0.08)	(0.72)	(0.18)	(1.35)	(0.09)	(0.09)	(0.18)
Party PMDB	0.13*	2.32***	-0.01	0.76	0.12	0.13	-0.01
	(0.08)	(0.67)	(0.16)	(1.27)	(0.09)	(0.08)	(0.16)
State SP	0.05	-1.04	0.44	16.4***	0.03	0.03	0.44
	(0.32)	(2.58)	(0.74)	(5.38)	(0.30)	(0.32)	(0.73)
State RJ	0.02	-1.19	0.30	16.5***	0.17	0.17	0.30
	(0.35)	(2.77)	(0.82)	(5.97)	(0.35)	(0.35)	(0.82)
Campaign Spending	0.00**	-0.00***	0.00	0.00	0.00	0.00**	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age	-0.02***	-0.18***	-0.04***	-0.37***	0.02***	0.02***	-0.04***
	(0.00)	(0.01)	(0.00)	(0.04)	(0.00)	(0.00)	(0.00)
Gender (Male)	0.35***	2.13***	0.35**	2.51**	0.36***	0.36***	0.35**
	(0.06)	(0.51)	(0.15)	(1.15)	(0.01)	(0.06)	(0.14)
University Complete	-17.0	-11.7	0.09	-0.81	-16.9	-16.9	0.08
	(240)	(18.4)	(0.16)	(1.26)	(240)	(240)	(0.16)
High School Complete	-17.0	11.0	0.24	-0.28	-16.9	-16.9	0.24
	(240)	(18.4)	(0.17)	(1.32)	(240)	(240)	(0.17)
Primary School	-17.1	11.2	0.24	-1.22	-17.1	-17.1	0.24
Complete	(240.0)	(18.4)	(0.20)	(1.55)	(240)	(240)	(0.20)
Reads and Writes	-16.9	-11.1	0.20	-2.99	-17.0	-16.9	0.22
	(240.0)	(18.4)	(0.41)	(3.27)	(240)	(240)	(0.41)
Accounts Rejected * Radio AM					0.06		
					(0.23)		
Accounts Rejected * Radio FM						0.39**	
						(0.19)	
Accounts Rejected * Municipal Spending							-0.00**
							(0.00)
Intercept	17.0	54.7***	2.47***	59.9***	17.0	17.0	2.49***
	(240)	(18.6)	(0.82)	(5.96)	(240)	(240)	(0.82)
Incumbents Only	No	No	Yes	Yes	No	No	Yes
N	14,865	14,865	3,022	3,022	14,865	14,865	3,022
N Accounts Rejected	658	658	195	195	658	658	195

***p<0.01, **p<0.05, *p<0.1 (two-tailed test). Standard errors in parenthesis
Models 1, 3, 5, 6 and 7: Logistic regression. Models 2 and 4: OLS.
All models include full state and party dummies.
Models 5 & 6 include single interaction term as control (not reported).

2.9 The Effect of Accounts Rejection on Electoral Outcomes in Elections for City Council

2.9.1 Matching and Difference-in-Difference

In this section I consider the effect of accounts rejection on electoral outcomes for the sample of candidates for City Council. Table 2.6 shows the propensity score of being in the treatment group given the set of covariates. Propensity score matching provides good balance in all variables, and in all cases the p-value increases after matching, with the exception of the first category of education (reads and writes) which nonetheless still has a high p-value. This ensures the "as if random" condition, there is equal probability for a candidate of being in the control group or in the treatment group given this set of covariates.

TABLE 2.6: Pre and Post-Matching Balance between Treatment and Control Groups
Candidates for City Council

	Propensity Score	Matching
	Pre-Matching (P-Value)	Post-Matching (P-Value)
Spending	0.00	0.51
Incumbent (2008)	0.00	0.62
Incumbent (2004)	0.00	0.32
Incumbent (2000)	0.00	0.98
Age	0.00	0.67
Party Mayor	0.00	0.88
Coalition Mayor	0.00	0.84
Male	0.00	0.57
Education University Complete	0.00	0.24
Education High School Complete	0.07	0.93
Education Primary School Complete	0.00	0.96
Education Reads and Writes	0.76	0.63
Party PMDB	0.75	0.99
Party PSDB	0.00	0.90
Party PT	0.00	0.64
State RJ	0.50	0.97
State SP	0.00	0.71

In Table 2.7 I present the results of the difference-in-difference. Results show a decrease in electoral returns of candidates with accounts rejected of 0.73 percentage points, and it is statistically significant (at the 0.01 level, two tailed test). In the context of highly competitive elections, this apparently small difference might be crucial. The average percentage of votes received by winning candidates in the 2012 election

was 4.74%, while the average percentage of votes received by the non winning candidates who received the highest percentage of votes in each municipality is 3.99%. The average difference between winners and candidates who lost for the smallest margin is 0.75 percentage points. In the case of the 2008 election, the average percentage of votes for the winner is 4.90%, while the average percentage of votes received by the non winning candidates with the highest share of votes in each municipality is 4.20%. The average margin of victory was 0.7 percentage points. The loss of votes that can be attributed to the accounts rejection could make the difference between being elected or not.

Table 2.8 uses likelihood to be elected as the dependent variable. Results also show a significant decrease in the likelihood of being elected for the candidates with accounts rejected.

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TABLE 2.7: Effect of Accounts Rejection on Vote Share
City Councilmen

	Propensity Score	Matching
	Estimate	Standard Error
Accounts Rejection	-0.73***	0.10
Matched Observations	458	

Difference in Difference. Estimate measured as percentage of valid votes.

Abadie-Imbens Standard Error. ***p<0.01, **p<0.05, *p<0.1 (two-tailed test).

TABLE 2.8: Effect of Accounts Rejection on Election Likelihood
City Councilmen.

	Propensity Score Matching	
	Estimate	Standard Error
Accounts Rejection	-0.09***	0.02
Matched Observations	458	

Difference in Difference. Estimate measured as percentage of valid votes.

Abadie-Imbens Standard Error. ***p<0.01, **p<0.05, *p<0.1 (two-tailed test).

2.9.2 Regression Analysis

In table 2.9 I present results of various models using OLS and logistic regression testing the effect of accounts rejection on the electoral outcomes in the 2012 election. In these analyses I exclude the variable candidate spending in the 2012 election to avoid introducing bias, since candidates with accounts rejected receive significantly fewer funds in this election (see results in section 2.11). The inclusion of this control doesn't introduce any significant changes in the results (beyond a marginal reduction in the size of the coefficients). Results show in the 7 models a significant reduction in the likelihood of being reelected and in the percentage of valid votes for candidates with accounts rejected. In models 5 and 6 the radio AM and FM interactions show no significant effect of accounts rejection on electoral outcomes.

TABLE 2.9: Effect of Accounts Rejection. 2012 Election. Candidates for City Council.

	Model 1 Election	Model 2 Vote Share	Model 3 Reelection	Model 4 Vote Share	Model 5 Election	Model 6 Election
Accounts Rejected	-0.65***	-0.63***	-0.78***	-0.87***	-0.66***	-0.59***
(2004-2012)	(0.08)	(0.05)	(0.09)	(0.09)	(0.09)	(0.11)
Party Mayor	0.10***	0.13***	0.04	-0.01	0.06**	0.09***
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.03)
Coalition Mayor	0.30***	0.77	-0.03	0.84***	-0.03	-0.03
	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
Incumbent 2008	1.22***	1.30***	–	–	1.23***	1.23***
	(0.02)	(0.01)	–	–	(0.02)	(0.02)
Incumbent 2004	0.66***	0.64***	0.54***	0.53***	0.67***	0.67***
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
Incumbent 2000	0.44***	0.33***	0.38***	0.33***	0.45***	0.45***
	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Party PT	-0.05	-0.17***	-0.09	-0.37***	-0.10**	-0.10**
	(0.04)	(0.03)	(0.07)	(0.06)	(0.05)	(0.05)
Party PSDB	0.05	0.01	0.02	0.02	0.06	0.06
	(0.05)	(0.02)	(0.07)	(0.06)	(0.05)	(0.05)
Party PMDB	0.13	0.10***	0.03	0.11**	0.08	0.08
	(0.04)	(0.03)	(0.06)	(0.05)	(0.05)	(0.05)
State SP	0.12	-0.09	0.37*	0.27	0.13	0.15
	(0.13)	(0.07)	(0.21)	(0.19)	(0.14)	(0.14)
State RJ	0.02	-0.29***	0.42*	-0.12	0.03	0.05
	(0.14)	(0.08)	(0.23)	(0.20)	(0.14)	(0.15)
Age	-0.03***	-0.02***	-0.04***	-0.05***	-0.03***	-0.03***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Gender (Male)	0.82***	0.57***	0.28***	0.02	0.81***	0.81***
	(0.03)	(0.01)	(0.04)	(0.03)	(0.03)	(0.03)
University Complete	0.21	0.11	0.53	-0.14	10.50	10.50
	(0.64)	(0.30)	(1.05)	(0.85)	(81.93)	(81.86)
High School Complete	0.02	0.25	0.41	0.26	10.32	10.31
	(0.64)	(0.30)	(1.06)	(0.85)	(81.93)	(81.87)
Primary School Complete	-0.02	0.36	0.40	0.51	10.29	10.28
	(0.64)	(0.30)	(1.06)	(0.85)	(81.93)	(81.87)
Reads and Writes	-0.20	0.25	0.36	0.48	10.15	10.14
	(0.64)	(0.31)	(1.06)	(0.85)	(81.93)	(81.87)
Accounts Rejected * Radio AM					0.11	
					(0.19)	
Accounts Rejected * Radio FM						-0.10
						(0.16)
Intercept	-1.74***	1.09***	0.07	4.03***	-12.07	-12.09
	(0.66)	(0.32)	(1.08)	(0.88)	(81.93)	(81.87)
Incumbents Only	No	No	Yes	Yes	No	No
N	420,555	420,555	36,727	36,727	420,577	420,577
N Accounts Rejected	1,767	1,767	671	671	1,767	1,767

***p<0.01, **p<0.05, *p<0.1 (two-tailed test). Standard errors in parenthesis
Models 1, 3, 5, and 6: Logistic regression. Models 2 and 4: OLS.
All models include full state and party dummies.
Models 5 & 6 include single interaction term as control (not reported).

In table 2.10 I use similar models for the 2008 City Council elections with the

inclusion of the candidate spending variable. Results show a negative and significant effect of accounts rejection on electoral outcomes in all models except for models 3 (only significant at the .1 level) and 4, which are the models including incumbents only. Models 5 and 6 show a positive and significant interaction between both radio AM and FM and accounts rejection. Consequently, the significant and negative coefficients in the accounts rejected variable (i.e. -0.46 and -0.62) represent a decrease in the likelihood of reelection for candidates with accounts rejected in municipalities without an AM or FM radio respectively. In municipalities with radio AM or FM the size of the coefficient is close to zero in both cases (0.02 in model 5 and 0.03 in model 6) and non significant (as the size of the standard errors is 0.18 in the first case and 0.14 in the second case). This suggests that electoral punishment is stronger in municipalities where there is no local media, which contradicts prior studies.

TABLE 2.10: Effect of Accounts Reelection. 2008 Election. Candidates for City Council.

	Model 1 Election	Model 2 Vote Share	Model 3 Reelection	Model 4 Vote Share	Model 5 Election	Model 6 Election
Accounts Rejected	-0.31***	-0.18**	-0.24*	-0.17	-0.46***	-0.62***
(2004-2008)	(0.09)	(0.08)	(0.12)	(0.11)	(0.12)	(0.13)
Party Mayor	0.15	0.26**	0.19	0.39***	0.19	0.19
	(0.11)	(0.10)	(0.13)	(0.13)	(0.12)	(0.13)
Coalition Mayor	0.46***	1.27***	0.36	1.21***	0.38**	0.37**
	(0.13)	(0.11)	(0.15)	(0.14)	(0.13)	(0.13)
Incumbent 2004	0.65***	0.93***	–	–	0.61***	0.62***
	(0.09)	(0.08)	–	–	(0.10)	(0.10)
Incumbent 2000	0.50***	0.59***	0.48***	0.65***	0.56***	0.56***
	(0.08)	(0.08)	(0.11)	(0.10)	(0.09)	(0.09)
Party PT	-0.44	-0.62***	-0.20	-0.59*	-0.37	-0.35
	(0.24)	(0.22)	(0.31)	(0.30)	(0.26)	(0.25)
Party PSDB	-0.14	-0.12	0.06	-0.03	-0.04	-0.05
	(0.17)	(0.16)	(0.21)	(0.21)	(0.18)	(0.18)
Party PMDB	-0.09	-0.10	0.01	-0.20	-0.03	-0.04
	(0.17)	(0.15)	(0.20)	(0.20)	(0.18)	(0.18)
State SP	-0.74	-1.25	-0.19	-1.05	-0.62	-0.67
	(0.58)	(0.53)	(0.65)	(0.64)	(0.64)	(0.64)
State RJ	-0.90	-1.26	-0.25	-1.03	-0.70	-0.78
	(0.61)	(0.57)	(0.70)	(0.69)	(0.68)	(0.67)
Campaign Spending	8.30***	12.0***	4.87***	8.95***	8.15***	8.14***
	(1.12)	(0.88)	(1.12)	(1.01)	(1.18)	(1.18)
Age	-0.04***	-0.05***	-0.04***	-0.06***	-0.04***	-0.04***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Gender (Male)	0.46***	0.19	0.37**	-0.14	0.53***	0.53***
	(0.14)	(0.13)	(0.19)	(0.18)	(0.15)	(0.15)
University Complete	13.5	0.14	-0.21	-0.64***	13.3	13.3
	(882.7)	(2.04)	(0.18)	(0.18)	(882.7)	(882.7)
High School Complete	13.6	0.53	-0.04	-0.21	13.5	13.5
	(882.7)	(2.04)	(0.16)	(0.16)	(882.7)	(882.7)
Primary School Complete	13.5	0.49	0.06	0.13	13.3	13.3
	(882.7)	(2.04)	(0.18)	(0.18)	(882.7)	(882.7)
Reads and Writes	13.4	0.65	0.00	0.37	13.3	13.3
	(882.7)	(2.05)	(0.33)	(0.32)	(882.7)	(882.7)
Accounts Rejected					0.48**	
* Radio AM					(0.20)	
Accounts Rejected						0.65***
* Radio FM						(0.18)
Intercept	-12.1	5.13**	1.80***	7.37***	-12.1	-12.0
	(882.7)	(2.12)	(0.75)	(0.73)	(882.7)	(882.7)
Incumbents Only	No	No	Yes	Yes	No	No
N	321,137	321,137	37,136	37,136	321,137	321,137
N Accounts Rejected	1,307	1,307	710	710	1,307	1,307

***p<0.01, **p<0.05, *p<0.1 (two-tailed test). Standard errors in parenthesis
Models 1, 3, 5, and 6: Logistic regression. Models 2 and 4: OLS.
All models include full state and party dummies.
Models 5 & 6 include single interaction term as control (not reported).

2.10 The Effect of Accounts Rejection on Decision to Re-list

Candidates receiving negative audits from the Audit Courts might be less likely to run for reelection. In the case of this study, the risk of selection bias is two-fold. First, candidates with accounts rejected might decide not to run for reelection to avoid an electoral punishment. Second, as previously explained, candidates with accounts rejected might want to run for reelection but might become ineligible after attempting to launch their candidacy. If either of these selection biases exists, the estimate could underestimate the true causal effect, as candidates receiving negative reports would not run.

To assess the possibility of selection bias I run various models, displayed in appendix A. Table A.2.1 displays two models on likelihood to relist with the sample of candidates for mayor in the 2012 election who run in the 2008 election and had accounts rejected between 2008 and 2012 (i.e. the sample used in the matching and difference-in-difference estimation). In model 1 I assess whether candidates with accounts rejected are less likely to run for office. This might be a source of bias insofar as candidates who attempt to re-run have advantages over the ones that chose not to re-list, such as a more stable electorate based on the use of clientelistic networks, or better relations with the local media. The dependent variable in this model is a dummy indicating if the candidate re-listed in the 2012 election. I include as covariates the share of votes received in the prior election, the share of candidate spending (relative to the total spending in the municipality) in the prior election, age, gender and education. Note that in this model I am assessing whether candidates with accounts rejection are willing to run for reelection regardless whether the Electoral Tribunal finally decided that the candidate was ineligible. Results show that can-

didates with accounts rejected are less likely to attempt to re-list for re-election in the subsequent election. In table A.2.2 model 1 I display an analogous model for candidates for City Council.

Another issue of concern is if candidates with more offenses are less likely to re-list than candidates with minor offenses. Candidates might have different open processes in the Audit Courts, and might be condemned in more than one process. In model 2 (tables A.2.1 and A.2.2) I consider a sample restricted to candidates who had their accounts rejected between 2008 and 2012 to test if candidates with more processes of accounts rejection are less likely to re-list. Results, still significant, show that those candidates are less likely to re-list both for candidates for mayor and candidates for City Council.

In model 3 (table A.2.2) I test whether candidates with more accounts rejected are more likely to have their candidacy banned by the Electoral Tribunal when they attempted to re-list. Results again show that candidates with more episodes of accounts rejection are more likely to be banned by the Electoral Tribunal. This model is displayed only for candidates for City Council, as there are no formally registered candidates for mayor with accounts rejected between 2008 and 2012 attempting to relist and banned by the Superior Electoral Tribunal.

The present discussion holds for the models considering only incumbents running for reelection (in tables 2.4-2.5 and 2.9-2.10) and for the matching and difference-in-difference analyses (which consider only candidates who run in the 2008 and 2012 election). In those cases, results could be underestimating the real treatment effect.[23] For the samples with all candidates this interpretation doesn't necessarily hold, as these samples include both candidates who run -and won or were defeated- in the

[23]An alternative interpretation, following De Magalhaes (2015), is that the estimate shows the unconditional effect of accounts rejection on likelihood of reelection, taking the value of 1 if the candidate decides to re-run and wins the election, and 0 if either he loses or decides not to re-run.

prior election as well as candidates who didn't, a portion of which had accounts rejection for any prior office holding or for any prior responsibility in the use of public funds.

The selection bias identified in those models might be underestimating the true treatment effect, as candidates with accounts rejected who arguably would lose votes and would be less likely to be re-elected decide not to re-run. A Heckman selection model could overcome this bias and provide a better estimation of the true effect. A Heckman model depends on the inclusion of an instrument with a significant effect on the outcome variable in the selection model (i.e. likelihood to re-run) but not on the outcome variable in the outcome model (i.e. likelihood to be re-elected). There is one variable that could respond to this condition: the timing of the Audit Court decision. An empirical test shows that when the Audit Court decision is closer to the election, candidates with accounts rejected are more likely to re-run; and timing of decision doesn't affect electoral outcomes. However, running a Heckman selection model was not possible due to collinearity between the instrument and the treatment.

2.11 Effect of Accounts Rejection on Campaign Donations

Prior work suggests an elite based mechanism of accountability, as candidates facing corruption accusations can receive fewer campaign donations, which is likely to have an impact on the electoral outcomes (Pereira, Renno & Samuels 2011). In table 2.11 I present results using as dependent variable the percentage of campaign donations that candidates received (relative to total donations in their municipality) for both candidates for mayor and city councilman in the 2008 and 2012 elections.

Results show that only in the 2012 election there was a significant negative impact of accounts rejection on the percentage of campaign donations received by the candi-

date, although in the case of the city councilmen the magnitude of the coefficient is relatively small.

TABLE 2.11: Effect of Accounts Rejection on Campaign Donations. Elections 2008 and 2012
All Candidates for Mayor and City Councilman

	Model 1 Mayor 2012	Model 2 Mayor 2008	Model 3 City Council 2012	Model 4 City Council 2008
Accounts Rejected	-7.08***	0.05	-0.22***	-0.31
	(1.67)	(1.19)	(0.08)	(0.19)
Incumbent 2008	14.78***	–	2.05***	–
	(0.83)	–	(0.20)	–
Incumbent 2004	0.01	15.32***	0.71***	1.15***
	(1.72)	(0.60)	(0.02)	(0.20)
Incumbent 2000	5.58***	8.27***	0.33***	0.36*
	(1.52)	(1.06)	(0.02)	(0.18)
Party PT	-4.03**	-6.54***	0.09**	0.36
	(1.76)	(1.09)	(0.04)	0.49
Party PSDB	0.25	1.16	0.11***	0.81**
	(1.72)	(1.07)	(0.04)	(0.36)
Party PMDB	-1.51	2.32**	0.20***	0.48
	(1.63)	(1.00)	(0.04)	(0.35)
State SP	2.44	-0.72	-0.07	-0.99
	(5.54)	(3.60)	(0.12)	(1.21)
State RJ	1.02	-2.88	-0.31**	-0.38
	(6.04)	(3.90)	(0.12)	(1.27)
Age	-0.26***	-0.14***	-0.01***	-0.01
	(0.04)	(0.02)	(0.00)	(0.01)
Gender (Male)	-0.99	-0.58	0.51***	-0.11
	(1.25)	(0.75)	(0.01)	(0.29)
Education (University Complete)	-7.27	2.49	1.01**	-0.43
	(24.24)	(28.42)	(0.50)	(0.57)
Education (High School Complete)	-7.78	3.49	0.74	-0.54
	(24.25)	(28.42)	(0.50)	(4.57)
Education (Primary School Complete)	-5.57	3.02	0.69	-0.87
	(24.28)	(28.43)	(0.51)	(4.57)
Education (Reads and Writes)	-0.65	2.70	0.63	-0.67
	(24.64)	(28.51)	(0.51)	(4.59)
Intercept	57.13**	40.91	0.34	4.17
	(24.98)	(28.68)	(0.52)	(4.76)
N	15,013	14,865	420,555	321,137
N Accounts Rejected	611	658	1,767	1,307

***p<0.01, **p<0.05, *p<0.1 (two-tailed test). Standard errors in parenthesis.
All models include full state and party dummies.

2.12 Discussion

Results presented in this study suggest that both candidates for mayor and for city councilman with accounts rejected receive an electoral punishment. Even in elections for relatively less salient offices, such as City Council legislator, at least a portion of the electorate is informed about the negative audits and responds by supporting an alternative candidate. This suggests that the selection mechanism is effective in sub-national elections. When electoral systems promote highly competitive elections -such as the elections for city councilman- even a small reduction in the share of votes could make the difference between being elected or not.

Besides voters' punishment, there are other selection mechanisms that play a significant role. First, candidates with negative audits often opt not to re-run in the next election. These candidates might be anticipating electoral losses or they might desist because of restrictions in the electoral laws. In either of these cases, the result is a better quality of representatives in local politics. Second, donors often decide not to support these candidates, which also contributes to the selection mechanism.

In addition, the diffusion of the candidates' negative records by the Audit Courts, the media, and by other candidates during the campaigns also contributes to this selection mechanism. The spread of information reinforces all other mechanisms of electoral accountability. First, it increases voters' awareness and their electoral punishment. Second, donors might also be influenced by this diffusion, either because they are less willing to support candidates with negative records, or because they assume that these candidates have fewer chances in the electoral game. Third, the diffusion might create pressure for candidates to retire from the electoral game, as they perceive that voters are aware of their offenses.

Results also show weak evidence for other factors listed as relevant in prior litera-

ture, such as the availability of local media, and incumbents' public spending. In the case of the former, the existence of local radios doesn't increase electoral punishment. Voters do not depend on those radios to receive information, which contradicts prior scholarship. The analyses also show week support for the trade off hypothesis. Voters are not willing to condone incumbents with accounts rejected even if they show positive records of public spending.

Appendix A: Effect of Accounts Rejection on Likelihood to Relist

TABLE A.2.1: Effect of Accounts Rejection on the Possibilities of Re-listing for Reelection Candidates for Mayor

	Model 1 Coefficient (Std Error)	Model 2 Coefficient (Std Error)
Accounts Rejected	-0.80***	-0.31***
	(0.10)	(0.11)
Vote share (2008)	0.04***	0.02***
	(0.00)	(0.01)
Spending	-0.00***	0.00
	(0.00)	(0.00)
Age	-0.02***	-0.02
	(0.00)	(0.01)
Gender (Male)	0.07	0.30
	(0.06)	(0.33)
University Complete	-10.47	0.97
	(11.95)	(0.47)
High School Complete	-10.58	0.64
	(11.95)	(0.48)
Primary School Complete	-10.86	–
	(11.95)	–
Reads and Writes	-10.90	-14.01
	(11.95)	(664.52)
N	12,820	697

***p<0.01, **p<0.05, *p<0.1 (two-tailed test).
Model 1: Effect of account rejection on possibility of relisting for all candidates.
Model 2: Effect of number of processes of account rejection on possibility of re-listing.
Only candidates with accounts rejected.
Only candidates with accounts rejected.
Female is the baseline category for gender in the three models.
Illiterate is the baseline category in models 1 and 2.
Primary Complete is the baseline category for model 2 (there are no illiterates in this sample).

TABLE A.2.2: Effect of Accounts Rejection on the Possibilities of Re-listing for Reelection Candidates for City Council

	Model 1 Coefficient (Std Error)	Model 2 Coefficient (Std Error)	Model 3 Coefficient (Std Error)
Accounts Rejected	-1.14***	-0.38***	-0.56***
	(0.06)	(0.10)	(0.17)
Party Mayor	0.16***	0.34**	-0.10
	(0.01)	(0.15)	(0.24)
Coalition Mayor	-0.40***	-0.01	-0.15
	(0.02)	(0.15)	(0.27)
Vote share (2008)	38.22***	4.88*	7.11
	(0.26)	(2.52)	(5.14)
Spending	-2.72***	1.14	0.59
	(0.16)	(1.22)	(2.08)
Age	0.00	0.01	0.03***
	(0.16)	(0.01)	(0.01)
Gender (Male)	0.14***	0.14	0.14
	(0.01)	(0.18)	(0.33)
Education (University Complete)	0.34**	12.5	-0.31
	(0.16)	(324.7)	(0.36)
Education (University Incomplete)	0.33**	12.3	0.15
	(0.16)	(324.7)	(0.5)
Education (High School Complete)	0.45***	12.3	-0.03
	(0.16)	(324.7)	0.34
Education (High School Incomplete)	0.42**	12.3	-0.68
	(0.16)	(324.7)	(0.54)
Education (Primary School Complete)	0.42***	12.12	
	(0.16)	(324.7)	
Education (Primary School Incomplete)	0.34**	12.24	-0.53
	(0.16)	(324.7)	(0.38)
Education (Reads and Writes)	0.17	12.42	-1.23
	(0.16)	(324.7)	(0.63)
N	321,137	1,572	510

***p<0.01, **p<0.05, *p<0.1 (two-tailed test).
Model 1: Effect of account rejection on possibility of relisting for all candidates.
Model 2: Effect of number of processes of account rejection on possibility of re-listing.
Only candidates with accounts rejected.
Model 3: Effect of number of processes of account rejection on possibility of being eligible.
Only candidates with accounts rejected.
Female is the baseline category for gender in the three models.
Illiterate is the baseline category in models 1 and 2.
Primary Complete is the baseline category for model 3 (there are no illiterates in this sample).

Appendix B: Models Without Controls

TABLE B.2.1: Effect of Accounts Rejection on Electoral Outcomes. Incumbent Mayors

	Model 1 Election 2012	Model 2 Vote Share 2012	Model 3 Election 2008	Model 4 Vote Share 2008
Accounts Rejected	-0.79***	-4.07***	-0.22	-3.57**
	(0.18)	(1.44)	(0.15)	(1.28)
Intercept	0.34	50.81	0.78***	53.5***
	(0.04)	(0.38)	(0.04)	(0.33)

***p<0.01, **p<0.05, *p<0.1 (two-tailed test). Standard errors in parenthesis.

TABLE B.2.2: Effect of Accounts Rejection on Electoral Outcomes. Incumbent City Councilmen

	Model 1 Election 2012	Model 2 Vote Share 2012	Model 3 Election 2008	Model 4 Vote Share 2008
Accounts Rejected	-0.92***	-0.95***	-0.22	-3.57**
	(0.05)	(0.04)	(0.15)	(1.28)
Intercept	-1.85***	1.32***	0.78***	53.5***
	(0.00)	(0.00)	(0.04)	(0.33)

***p<0.01, **p<0.05, *p<0.1 (two-tailed test). Standard errors in parenthesis.

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3. Public Costs, Morality and Private Gain: Assessing the Effect of Alternative Information Treatments on Electoral Accountability

Abstract

Are voters' attitudes towards corrupt candidates affected by the details they learn about candidates' wrongdoing? This study examines the effect that including different types of details about the public costs, illicit enrichment and other forms of moral misbehavior by a corrupt incumbent mayor has on the probability of support for his re-election. Three surveys experiments, using an online convenience sample of Brazilian subjects, are used to test this question. Subjects are shown either a "limited information" vignette with the most basic information on those records, or one of several "extended information" vignettes which provide more details. The study presents a threefold categorization of treatments with information that emphasizes the public costs of corruption, the candidate's lack of honesty, and the mayor's illicit enrichment. In addition, I include alternative "extended information" treatments to test rival hypotheses. Results consistently show that information showing the mayor's illicit enrichment drives a stronger negative response than every alternative treatment.

3.1 Introduction

In recent years there have been a growing number of studies investigating the conditions that affect voters' reactions towards corrupt candidates. This has been the focus of many experimental and observational studies, including an initiative to support field experiments across various developing countries[24]. These studies have emphasized the importance of a host of mediators, such as subjects' identification with the accused candidate's party (Andiuza et al. 2013), subject's identification with the candidate's position on specific issues (Rundquist, Strom & Peters, 1977), the degree of party system polarization (Eggers 2014), the politician's public expenditures records (Pereira & Melo 2015, Weitz-Shapiro & Winters 2013), the overall state of the economy (Klasnja & Tucker 2013), and the role of information provided by the media and oversight agencies (Ferraz & Finan 2008, Chang, Golden & Hill 2010, Weitz-Shapiro & Winters 2013, Costas-Pérez, Solé-Ollé & Sorribas-Navarro 2012, Palau & Davesa 2013, Botero et al. 2015, Weitz-Shapiro & Winters 2015a). Most of these studies have found consistent evidence that all else being equal, voters will reject corrupt candidates. However, we know little about whether additional information on the same corruption incident can generate a stronger rejection. Plausibly, the kind of information that spurs a stronger reaction will depend on the kind of motivation driving the rejection of corrupt candidates. Voters' reaction can be driven by a variety of motivations, such as an utilitarian costs assessment of the corruption incident or a rejection of the candidate on moral grounds.

This is the first study, to the author's knowledge, that introduces a variety of manipulations on the information that subjects learn about a corruption incident and

[24]EGAP Metaketa Initiative on Political Accountability in the Developing World. More information available at: http://egap.org/metaketa/metaketa-information-and-accountability

tests their effect on subsequent support for the politician responsible for that incident. In particular, the study uses different versions of a similar vignette presenting a corrupt mayor running for reelection. The vignettes include manipulations to the information that subjects receive about the source of misappropriated funds, the destination of those funds and about other forms of dishonest behavior by the candidate. I introduce these variations to inquire whether subjects are sensitive to information emphasizing the public costs of corruption, the candidate's moral misbehavior, and his illicit enrichment.

The study was conducted using three online survey experiments presented to a convenience sample of Brazilian subjects. The pre-analysis plan for the second and third experiments were pre-registered (Avenburg 2015, Avenburg 2016). All hypotheses and analyses were included in the pre-registration except for analyses and hypotheses of experiment #1 (which was not pre-registered). In all experiments, I use vignettes presenting a fictitious incumbent mayor running for reelection with accounts rejected by an Audit Court. A baseline ("limited information") vignette presents only basic information on the Court's decision. I use alternative "extended information" vignettes with different pieces of additional information to test several theories on the factors that increase rejection for a corrupt candidate. Results consistently show that additional information showing the private spoils of corruption generate a stronger negative response. The alternative "extended information" vignettes (including those that emphasize the public costs of corruption) show significant additive effects with respect to the baseline vignette only in some cases. As such, results in this respect are less conclusive.

These findings suggest that corruption is not an undifferentiated treatment. What citizens thinks about a politician's behavior is largely a function of what sort of details are revealed (generally through the media) about that behavior. More specifically,

when they learn details about how a corruption scheme is used for the politician's personal benefit their rejection of that candidate will be stronger.

The findings have important implications for both the scholarly literature and for assessments of the effect of corruption on public opinion at large. Regarding the former, it provides new evidence on the specific type of motivation that spurs a stronger response towards corrupt candidates. Citizens rejection is not prompted by an estimation of the possible consequences of corruption on their wellbeing. They reject the politician's ambition to use the public office for personal gain. However, this rejection cannot be extended to other spheres of immoral behavior (such as using lies to conceal a malfeasance).

In terms of the implications for assessments of the impact of corruption scandals on public opinion, these results imply that corruption news articles focusing on aspects such as illicit enrichment by politicians will generate a stronger public reaction than articles covering other aspects of corruption. In addition, evidence showing that more detailed information drive stronger responses suggests that the effect of anti-corruption campaigns may be limited when the information they spread is limited. This type of campaign has been carried out recently in places like Brazil and India as part of anti-corruption non-governmental organizations grass-root campaigns.[25]

3.2 Assessing Costs or Judging Morality?

What kind of information about a corruption incident spurs a stronger rejection

[25]In Brazil both the Movement to Fight Against Electoral Corruption (*Movimento Contra à Corrupção Eleitoral*) and Brazilian Magistrates Association (*Associação dos Magistrados Brasileiros*) have recently published a "Dirty List" of candidates with a variety of negative records (such as corruption convictions or accounts rejected by Audit Courts). In India the Association for Democratic Reforms carried out a similar campaign. Those lists typically present very limited or no details on the specific accusation affecting each candidate.

towards politicians responsible for such malfeasance? Plausibly, how voters react depends on the type of judgment they invoke to evaluate politicians' behavior. Their judgment might rely on a variety of grounds. One possibility is that voters rely on an estimation of the costs of malfeasance. As the "trade off" hypothesis posits, voters will condone corrupt candidates if they perceive that despite corruption politicians have good records of social provision (Winters & Weitz Shapiro 2013). Prior scholarly work testing the "trade off" hypothesis provided mixed empirical support, as some studies have found no empirical evidence supporting this theory (Winters & Weitz Shapiro 2013), others have found empirical evidence only in some contexts (Kasnja and Tucker 2013) and others did found suggestive evidence (Pereira & Melo 2015). Alternatively, voters might rely on a moral judgment of the candidate. Various studies show that a candidate's traits, such as integrity and trustworthiness, have an important impact on voters' evaluation (Funk 1996, Fridkin & Kenney 2011).

These different grounds for judgment pertain to different conceptions of ethics. According to the deontological conception, there are ethical rules that define the morality of an action, regardless of its anticipated consequences. These ethical rules should be weighed by the intent of the actor. In contrast, the consequentionalist conception considers that the morality of an actions should be weighed based on the anticipated costs and benefits of such action. This distinction has been the focus of various experimental papers, which examine whether in dilemmatic situations subjects' moral psychology is consistent with either of the two(see Ditto and Liu 2012 and Ditto and Liu 2011 for a summary). In this study, I present subjects with scenarios that do not provide moral dilemmas, but rather alternative moral psychological foundations to punish corrupt candidates.

I propose three alternative hypotheses assessing the moral psychology that grounds voters rejection of corrupt candidates: a) voters can rely on an evaluation of the costs

of the corruption incident —i.e. how corruption will affect their wellbeing; b) they might ground their rejection on a moral judgment of the politician's misbehavior, assessing equally any type of moral offense associated with the incident —e.g. using lies to conceal the malfeasance or diverting the funds for private enrichment; c) they might specifically reject the politician's illicit enrichment.

The first hypothesis holds that voters reject corrupt candidates based on the anticipated costs of corruption on their wellbeing; that is, they rely on a consequentional conception of ethics. When officials misappropriate public funds, programs and expenditures are defunded as a result. Consequently, the social programs and public goods voters expect to receive will suffer from this misappropriation. This might also determine different individual reactions to the same treatment. For example, subjects who regularly benefit from public programs and expenditures may punish the candidate more strongly, as they expect to be particularly affected by the corruption incident.

This theory has some similarities with the "trade off" hypothesis, which holds that voters are willing to tolerate the negative impact of corruption as long as the candidate shows overall good records of public goods provision (Winters & Weitz Shapiro 2013, Pereira & Melo 2015). The two differ in that the "trade off" hypothesis assumes that other goods are distributed to compensate corruption, while the consequentionalist ethics assumes that corruption will be more punished when there is a tangible negative outcome for a specific corruption incident —i.e. regardless of the overall performance of the politicians. The intuition that voters assess corrupt candidates with a consequentionalist ethics is behind the message used by Banerjee et al (2010: 20) to prime subjects on corruption: "Corrupt politicians steal money set aside for development funds and do nothing for you".

A second theory holds that voters rejection is based on a negative moral judgment

of the candidate's integrity, regardless of the consequences of the corrupt behavior. Following a deontological moral conception, voters would be punishing the intent, not the consequences, of a candidate's wrongdoing. Candidates facing corruption accusations show lack of integrity, which has a negative impact on subjects' evaluation. If voters' rejection is based on a deontological conception of ethics, when any type of additional information on the moral misbehavior associated with the corruption incident is included, such as using corruption for illegal enrichment, or using lies to cover up any misuse of public funds, voters' negative reaction will increase.

A third theory holds that voters' rejection may be based on a moral judgment that specifically rejects the candidate's intent to use the public office for personal gain. If this is true, then voters will be particularly sensitive to any additional information showing that the candidate is using this office for illegal enrichment. This could be considered a specific type of deontological ethics, as voters weigh the moral integrity of the candidate regardless of the consequences of his action. However, voters are specifically sensitive to information on the illegal enrichment of the candidate as compared to other forms of misbehavior, such as lying to conceal his wrongdoing.

The three main hypotheses of this study can be summarized as follows:

Public Costs/Consequentionalist Morality (Hypothesis 1): Voters' rejection for a corrupt candidate will increase as they learn additional details about the source of the misappropriated funds and about the costs of such misappropriation.

Hypothesis 1.1: This rejection will be stronger among subjects who regularly benefit from the programs and expenditures that were misappropriated.

Deontological Morality (Hypothesis 2): Voters' rejection for a corrupt candidate will

increase as they learn additional details emphasizing any kind of moral misbehavior associated with the corruption incident (e.g. lying to conceal the incident or using those funds for illegal personal gain).

Private Benefit (Hypothesis 3): Voters' rejection for a corrupt candidate will increase as they learn additional details specifically emphasizing the use of funds for illegal personal gain.

3.3 Costs, Benefits and Morality in Corruption Treatments

In the three experiments I present alternative versions of a vignette featuring a hypothetical incumbent mayor running for reelection. A "limited information" vignette informs subjects of the corruption incident without further details; various "extended information" vignettes include more details on the incident. In particular, the various "extended information" vignettes present additional information about which specific program or expenditure was used to divert funds, where funds were diverted, and whether the candidate tried to conceal such misuse with lies. The additional information on the source of misappropriated funds -i.e. which expenditure was affected- is used to emphasize the costs of the corruption incident. When informed about the source of misappropriated funds, subjects learn additional details on which particular program is affected and hence can assess what will be the anticipated costs of this incident. In addition, in study # 3, I introduce a vignette that makes explicit the costs of the incident. In contrast, when subjects learn additional details on where those funds are destined, they learn who is benefiting from the corruption. The lying treatment is included by informing the subjects that the candidate used lies to conceal a malfeasance in a typical procedure of horizontal accountability. I use additional

"extended information" vignettes to test various rival hypothesis.

To introduce these vignettes I use formats typical of news articles drawn from existing Brazilian journals. A stylized version of an article covering a corruption case involving local officials includes either information on the source of the misappropriated funds or information on the destination of those funds (or information on both). Appendix K presents a number of summarized news pieces on corruption in Brazilian municipalities used to create the experimental vignettes with a realistic format.[26]

A number of examples of recent scandals in Brazil -among the most prominent ones in Brazilian history- might be useful to illustrate how information on the source and destination of misappropriated funds conveys information on the public costs and private benefits in corruption schemes. In 2005- 2006 two major scandals hit Brazilian politics: the Mensalão ("Monthly Payments") case and the Sanguessugas ("Bloodsuckers") case. The first was a scheme in which various members of the PT government offered monthly bribes to allied legislators in exchange for political support. Evidence showing that legislators were receiving monthly bribes is an example of information on the private benefit of corruption; subsequently, the press reported that the diverted funds might have originated from state owned companies' advertisement budget. This is an example of the public costs of corruption. The Sanguessugas case involved a scheme of overpayments and money diversion in public purchases of ambulances; hence, news stories focusing on overpayments and payments without acquisition of those ambulances show the public costs of such fraud.

A number of prior experimental studies have provided subjects with information either on the source of misappropriated funds or on the destination of those funds. For instance, Chong et al. (2015) in a field experiment distribute leaflets informing

[26]These examples are drawn from a non random sample and cannot be used to infer any systematic pattern of news format from a larger population.

citizens of the amount of funds that mayors spent in a corrupt manner and their responsibility in terms of public goods provision, which suggests the costs of a corrupt administration[27]. Other experimental treatments include information on the politician's illicit enrichment (Botero et al. 2015) or information on bribe-taking, which also emphasizes the "private benefit" of corruption (Winters and Weitz-Shapiro 2013, Klasnja and Tucker 2013, Banerjee, Green, McManus, Pande 2012, Weitz-Shapiro & Winters 2015a, 2015b). However, the present study is the first to manipulate various versions of this information in a single project to assess the effect of emphasizing the public costs and private benefits of the corruption incident on subjects' responses.

3.4 First Study

3.4.1 Experimental Design

Following Samuels & Zucco (2012), I recruited subjects online with Facebook advertisements. The ads targeted Brazilian users over the age of 18, offering the chance to win an iPad after completing a survey. Following Samuels & Zucco's approach, in order to attract a wider sample of subjects, I did not include any reference to politics in the advertisement. Participants clicking on the ad were redirected to an external website that presented the consent form and hosted the survey.

All vignettes presented a fictitious incumbent mayor, not identified by name, who is running for reelection. Following Weitz-Shapiro & Winters (2015a, 2015b), I asked

[27]More generally, Audit Court decisions will typically provide information that emphasizes public costs, as they rule on whether there was a proper use of public funds. Their ruling often informs on the type of expenditure that has been mismanaged. For instance, Ferraz and Finan (2008: 710-11) in their study on electoral punishment towards candidates with negative audits provide the illustrative examples of mismanagement in funds spent for infrastructure and health related programs.

subjects to imagine they live in a different municipality where this mayor is running for reelection, and informed them that since he has been in office, the overall conditions in the municipality improved, with new public works completed and improved street cleaning. Because I am interested in comparing evaluation across different candidates who have corruption records, I used positive information on prior records, aside from the corruption records themselves. The purpose of providing a positive reference point is to allow a relatively high benchmark from which the negative evaluation point of the corruption record will presumably drop, hence allowing enough variation in the dependent variable. A similar approach was used in Weitz-Shapiro & Winters (2015a, 2015b).

The corruption treatment was provided by informing subjects that the State Audit Court examined how the candidate used public funds and detected irregularities such as over-invoicing and no-bid public purchases. In Brazil, State Audit Courts have an extensive role in auditing accounts of officials at the municipal level (Ferraz and Finan 2008; Melo, Pereira & Figueiredo 2009, Pereira & Melo 2015), and prior research has found that they are viewed as a credible source of accusation (Weitz-Shapiro & Winters 2015a).

The outcome variable (the probability of voting for the mayor) was measured with a 7 point Likert scale. An alternative outcome measure, satisfaction with the mayor, was also measured using a second 7 point Likert scale.

I included a pre-treatment questionnaire with several demographic and socio-economic questions, along with questions measuring subject's sources of political information, whether he or she regularly follow political news and discusses politics, whether he or she can properly identify the role of Audit Courts, and whether he or she attends public or private hospitals[28]. In addition, I included a screener question.

[28]More specifically, the question asks subjects if the last time they went to a hospital they attended

The structure of the screener replicated the structure of the vignette in that the variation in the additional information details in the treatment were introduced in the fourth line, while the screener's instruction to skip the question were introduced in the third line of the paragraph. The complete screener questions wording is presented in appendix L. Hence, assuming equal attention, subjects who passed the screener were also the most likely to capture variation introduced across the different treatment conditions.[29]

3.4.2 Hypotheses and Expected Results

The first study was designed to test whether the extension of information and the source and destination of misappropriated funds affect subjects' responses. The first hypothesis posits that when subjects learn more details about the corruption incident, they are more likely to reject the candidate than when they learn only limited information (H1). The second hypothesis posits that when subjects learn that the corruption incident involves misuse of funds destined to public services, they are more likely to reject the candidate than when they learn that those funds were destined to expenses where the social benefits are less obvious (H2). The third hypothesis posits that corruption used for the candidate's private gain generates a stronger response than corruption used to support other members of the party (H3).

To test these hypotheses, I used a six-treatment design with one condition where subjects learn that the candidate has no antecedents of corruption (Tr1), one condition where subjects only learn that the candidate has antecedents of administrative

a public or a private hospital.

[29]Berinsky et al. (2014) contend that subjects can be assumed to pay equal attention across an entire questionnaire. Following their recommendation I present both results for all subjects and for the sample of subjects who passed the screener question.

irregularities, and four additional treatment conditions with complementary information on the source of misappropriated funds (expenditures destined to public hospitals or expenditures in technical services to repair the computers in municipal offices) and on the destination of those funds (a mayor's bank account or to support other party members) with a 2x2 factorial design. The two alternative sources of funds attempt to capture a continuum from expenditures that are generally more valued by voters (social services) to expenditures where the benefits for the public are less obvious (regular expenditures). In particular, I use the example of public health, for which there is shared responsibility between the municipal and federal government. Recent surveys show that this is a top concern among the Brazilian public[30]. Both examples were drawn from real-world cases. The first example is a frequent case of accounts rejection for mayors. Many municipalities receive federal transfers from a specific program[31] geared to improve the quality of public health and are subsequently audited by the Federal Audit Court[32]. The second example is drawn from a particular case of account rejection of a city councilman in a municipality of the State of Ceará, although similar cases of account rejection for misuse of funds spent in technical services are not uncommon.

The two alternative destinations of misappropriated funds attempted to assess whether personal gain was punished more strongly than diversion of funds for the benefit of other party members. The specific language of each vignette can be found

[30]http://www.ibope.com.br/pt-br/noticias/Paginas/Brasileiro-elege-saude-seguranca-e-educacao-como-prioridades-para-2014.aspx. Accessed 4/21/2015.

[31]The name of the program is the *Fundo Nacional de Saúde* (FNS) or National Health Fund. For more information see: http://www.fns.saude.gov.br/indexExterno.jsf

[32]In Brazil mayors are only audited by the Federal Audit Court for the use of funds transferred from the federal government; the State Audit Court (or the Municipal Audit Court in the few cases where there is a specific Audit Court at the municipal level) is in charge of auditing municipal funds. Technically, the fact that the treatment refers to a State Audit Court (and not to the Federal Audit Court) would mean that misused funds were either state or municipal funds, which is the same hypothetical source of the funds for the alternative treatment.

in appendix A.

Summarizing the six treatment conditions:

Tr1 No Corruption: Information that the candidate has no antecedents of misuse of public funds.

Tr2 Baseline (Limited Information): Information that the Audit Court found various irregularities when analyzing how the mayor used public funds.

Tr3 Health Care/Bank Account (Extended Information): Baseline plus information that the misused funds come from a social program and that those funds were diverted to a bank account owned by the mayor.

Tr4 Health Care/Reelection (Extended Information): Baseline plus information that the misused funds come from a social program and that funds misused were diverted to support party members who supported the mayor in the electoral campaign.

Tr5 Computer/Bank Account (Extended Information): Baseline plus information that the misused funds come from regular expenditures (expenditures in technical services to repair computers in municipal offices) and that misused funds were diverted to a bank account owned by the mayor.

Tr6 Computer/Reelection (Extended Information): Baseline plus information that the misused funds come from regular expenditures (expenditures in technical services to repair computers in municipal offices) and that funds misused were diverted to support party members who supported the mayor in the electoral campaign.

The hypotheses and expected results can be summarized as follows:

Extended Information (H1): Providing more information on the source and destination of funds from the corruption incident elicits a stronger negative response from subjects.

Expected results (H1): Support for candidate in Tr3 = Tr4 = Tr5 = Tr6 < Tr2 < Tr1.

Different Effects/Public Costs (H2): Voters are particularly sensitive to information on the public costs of corruption when the source of misappropriated funds comes from a social program.

Expected results (H2): Support for candidate in Tr3 = Tr4 < Tr5 = Tr6.

Different Effects/Private Benefits (H3): Voters are willing to condone corruption when misappropriated funds are being diverted to support other party members.

Expected results (H3): Support for candidate in Tr3 = Tr5 < Tr4 = Tr6.

3.4.3 Results

The experiment was run between August and September 2015. In total 1,598 subjects completed the survey (sample descriptive statistics are available in Appendix B) and a total of 774 subjects passed the screener (48.4 % of the total sample). The experimental sample is largely representative, although individuals with lower levels of education are underrepresented. This pattern has also been found in prior studies recruiting subjects with Facebook advertisements in Brazil (Boas 2014). The sample also shows a larger share of women. Both patterns are consistent across the three studies.

In this section, I discuss results from OLS regression analyses, using both a basic model with no controls and additional models controlling for a number of covariates. The full models are included to control for possible sample imbalances (see results of a multinomial logistic model testing sample balance in appendix C). In all analyses, I will particularly focus on results from the sample of subjects who passed the screener, as I consider screener passage the best proxy for subject compliance. Subjects who

don't read the entire vignette would not capture any difference across the various treatments and hence could be considered non-compliant[33].

In table 3.1, I report mean support for each treatment condition.

TABLE 3.1: Mean Vote Intention
Study # 1. One Screener

No Corruption	Baseline	Health Care/ Bank Account	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
5.48	2.11	1.59	1.49	1.43	1.64
(0.18)	(0.16)	(0.12)	(0.11)	(0.11)	(0.14)
N=123	N=124	N=141	N=133	N=122	N=131

Standard Deviation in Parenthesis. DV: Likelihood to Vote for Candidate.

In table 3.2 I present results from the OLS analyses both for the entire sample and for the sample that passed the screener. Vote intention is the dependent variable. The baseline "limited information" condition is the reference category. The first column shows a basic model without controls. The second column reports results from the entire sample. The third column presents results including a number of socio-demographic controls. In column four I additional control for knowledge of the Audit Court's role. In the last column, I include a treatment interaction with the dummy variable indicating if the subject attends public hospitals.

All analyses show that subjects are significantly less likely to support the corrupt candidate in any of the "extended information" treatments as compared to the "limited information" treatment. This is consistent for both the entire sample and for the samples that passed the screener. In the first column I consider results for the sample that passed the screener without controls. It shows effect sizes ranging from an almost 9 percentage point decrease in the probability of voting the mayor to

[33]This was specified in the pre-analysis plans for studies # 2 and #3, the only studies pre-registered.

an almost 10 percentage point decrease. Results from the other samples also show results that suggest that subjects in any of the "extended information" conditions are less likely to support the candidate

In addition, the no corruption interaction with the dummy indicating whether the subject attends public hospitals is significant, while all other interaction terms are not significant. This suggests little support for hypothesis 1.1. as the interaction terms of vignettes specifying that misappropriated funds came from health related expenditures do not present significant results. Results also show that voters are less likely to support a corrupt candidate in any of the information conditions with respect to the no corruption condition, which is consistent with the literature.

In appendix D I present results of the ANOVA difference-in-means test. Vote intention is the dependent variable. Each row presents results of the difference-in-means test with respect to the condition specified in top of each column. For instance, the health care/bank account condition has difference-in-means of -0.52 with respect to the baseline condition. In parenthesis I report Holm adjusted p-values[34]. In brackets I report non adjusted p-values. When considering Holm adjusted p-values, results show that the combinations health care/reelection and computer/bank account are significant with respect to the baseline condition. The combination health care/bank account is very close to the significance level (p=0.05). When using non adjusted p-values all "extended information" vignettes show significant results with respect to the "limited information" condition. If the dependent variable is shifted to satisfaction with mayor all "extended information" treatments are significant with respect to the baseline condition using both Holm adjusted p-values and non adjusted p-values, with effect sizes range within the same magnitude (between a 7 and an almost 10

[34]I report adjusted p-values, in addition to the non adjusted p-values, to control for family wise error term, as in all studies I will be running multiple comparisons.

percentage point decrease). Results are displayed in table E.3.1 in appendix E. In addition, in appendix F I present difference-in-means results for the entire sample. Only the health care/reelection and computer/bank account show significant differences with respect to the baseline condition when using non adjusted p-values.

In table G.3.1 in the appendix I report results of a logistic regression assessing whether specific subgroups are more likely to pass the screener. Results show that more educated, older, and female subjects, as well as those who more frequently talk about politics are more likely to pass the screener.

TABLE 3.2: OLS Regression on Vote Intention. Baseline is the Reference Category
Study # 1

	Screener	All Subjects	Screener	Screener	Screener
No Corruption	3.37**	2.80***	3.29***	3.29***	2.88***
	(0.20)	(0.17)	(0.20)	(0.21)	(0.29)
Health Care/Bank Account	-0.52***	-0.37**	-0.57***	-0.57***	-0.77***
	(0.19)	(0.02)	(0.19)	(0.20)	(0.27)
Health Care/Reelection	-0.62***	-0.42**	-0.63***	-0.61***	-0.69**
	(0.19)	(0.01)	(0.19)	(0.20)	(0.28)
Computer/Bank Account	-0.69***	-0.43**	-0.69***	-0.65***	-0.61**
	(0.20)	(0.01)	(0.20)	(0.20)	(0.28)
Computer/Reelection	-0.49**	-0.37**	-0.50**	-0.47**	-0.55**
	(0.20)	(0.17)	(0.20)	(0.20)	(0.28)
Education		-0.05**	-0.01	-0.01	-0.02
		(0.03)	(0.03)	(0.03)	(0.02)
Income		-0.01	-0.01	-0.00	-0.02
		(0.02)	(0.03)	(0.04)	(0.04)
Sex		-0.01	-0.07	0.04	0.02
		(0.97)	(0.13)	(0.13)	(0.13)
Knowledge Audit Court				-0.13	-0.14
				(0.12)	(0.12)
Public Hospital					-0.07
					(0.29)
No Corruption					0.91**
*Public Hospital					(0.42)
Health Care/Bank Account					0.47
*Public Hospital					(0.40)
Health Care/Reelection					0.22
*Public Hospital					(0.41)
Computer/Bank Account					-0.13
*Public Hospital					(0.41)
Computer/Reelection					0.18
*Public Hospital					(0.41)
Intercept	2.11***	2.56***	2.09***	2.17***	2.37***
	(0.14)	(0.00)	(0.00)	(0.00)	(0.00)
N	774	1,598	774	774	774

Standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.1.
Public Hospital=1 if subject attends public hospital. Sex coded as Female=1, Male=0

In sum, results from study # 1 show strong support for hypothesis 1 and no support for hypotheses 2 and 3. As subjects learn more about either the sources or the destinations of misappropriated funds, they are more likely to reject the candidate. However, these results do not show which specific piece of information explains the stronger effect. The set-up of the second experiment attempts to test various competing hypotheses on the type of additional information driving the effect.

3.5 Second Study

The second experiment was designed to identify which specific piece of information, the source of misappropriated funds or the destination of those funds, explains voters' stronger rejection of the corrupt candidate. I used a set-up in which subjects were provided either with information about where the misappropriated funds come from or where those funds were diverted. To test the public costs hypothesis, I used the example of the public health expenditure (which would presumably have a higher social impact); to test the private benefit hypothesis, I used the bank account example, which highlights the corrupt's candidate private gain. Each treatment included specific details of the accusation to test the rival hypotheses (see below).

3.5.1 Rival Hypotheses: The Role of Additional Information

I test a set of alternative hypotheses assessing the role of additional information. I maintain three alternative hypotheses. The first one states that any more extensive information, regardless of what kind of information it is, reinforces the treatment effect. Consequently, any "extended information" treatment will have a stronger effect than the baseline treatment. According to this hypothesis, more information means

a stronger treatment (by providing a stronger treatment dosage). Subjects' reactions will be stronger not due to any particular piece of information, but simply because there is more information on the wrongdoing.

The second hypothesis states that information providing specific details about the accusation generates a stronger effect. Here I rely on prior research, grounded in dual-coding theory (Paivio 1986), which has shown that concrete news has a stronger effect than abstract news (David 1998). Plausibly, the additional information that subjects receive in the "extended information" treatments might increase the effect because this information is making the news more concrete, creating a "story" out of information that otherwise can be perceived as abstract. To test this hypothesis I included an additional vignette with more abstract procedural information (the procedural information condition). When subjects are provided with more information either on the source of misappropriated funds or where those funds were diverted they get a real, concrete story, as opposed to those confronted with the abstract procedural treatment. Consequently, the treatment effect in those cases will be stronger.

Finally, I included a third hypothesis that focuses on the often insufficient background information that subjects have on the role of Audit Courts. I posit here that subjects in experiment # 1 used the additional information to interpret what a negative audit means, which not all subjects might completely understand. In each of the three experiments I ran I included the question asking subjects to identify the role of Audit Courts. The percentage of subjects who correctly answered this question was 53.6 % in the first study, 49.8% in the second study, and 49.3 % in the third study. To test this hypothesis, the procedural information condition provides additional information on the role of the Audit Court. Only abstract information is provided to accommodate hypothesis # 2. In particular, this hypothesis will be tested with the sample of subjects who are less knowledgeable of the role of Audit Courts.

3.5.2 Experimental Design

The set-up of the second experiment is similar to the first experiment. Subjects were recruited with similar Facebook ads. I used the same pre- and post-treatment questionnaires. I included a second screener question (following Berinsky, Margolis, & Sances 2014) with the same structure as the first screener, instructing subjects in the third line of the paragraph to choose two specific and nonsensical choices in a multiple choice question (see appendix L for the exact screener wording). In addition, I measured the time subjects spent reading the treatment vignettes to test if those who passed the screeners also spent more time reading the vignette. This experiment was run between November and December 2015.

I introduced a few relatively minor variations in the information on the candidate's prior positive records. First, in contrast to the first experiment, positive records were included after the information on the irregularities detected (while in the first experiment those records were introduced before the treatment). In addition, I mentioned a new area of positive records (public transport) as well as a line stating that the candidate is still very popular and has good chances of being reelected (the exact vignette wording is available in appendix A). The vignette also made explicit that the Audit Court rejected the candidate's accounts, something that was not explicitly stated in the first experiment. I informed subjects the despite the decision of the Audit Court, the candidate was allowed to run for reelection (in Brazil candidates with accounts rejected are not allowed to run, but they often circumvent this restriction by appealing in the judiciary).

The baseline treatment informed subjects that the Audit Court found irregularities and consequently decided to reject the mayor's accounts. The procedural details

treatment added to the baseline treatment information on the role of the Audit Court, as well as information on the session and chamber of the Audit Court that decided to reject accounts (all information that is frequent in news articles). The public costs version was similar to the baseline, with additional information that the irregularities included over-invoicing and no-bid contracts in expenditures intended to build a primary health care center. The private benefits treatment was similar to the baseline condition, with additional information that the Audit Court found that the mayor created a civil association to divert public funds to his own bank account. I used the Audit Court as the agency that investigates diversion of funds, though in Brazil this crime is more often investigated by the regular court system, to avoid varying the institution making the accusation. Presumably, subjects are more familiar with the procedures of the regular court system than with those of the Audit Court system. If so, introducing two different institutions might bias comparisons across groups.

Summarizing the four treatment conditions:

Tr1 Baseline (Limited Information): Information that the candidate has accounts rejected by the Audit Court.
Tr2 Procedural (Extended Information): Baseline plus information on the Audit Courts procedures and mechanisms leading to that decision.
Tr3 Health Care (Extended Information): Baseline plus information that misappropriated funds came from a program to build a primary care health center.
Tr4 Bank Account (Extended Information): Baseline plus information that funds misappropriated were diverted to the mayor's bank account.

3.5.3 Hypotheses and Expected Results

Hypothesis 1 (Private Benefit): When voters learn additional details about where money was diverted to, they are less likely to support a corrupt candidate than when they learn additional details on the source of misappropriated funds.

Expected results (H1): Support for candidate in Tr4 < support for candidate in Tr3, Tr2 & Tr1.

Hypothesis 2 (Public Costs/Consequentional Morality): When voters learn additional details about the source of misappropriated funds, they are less likely to support a corrupt candidate than when they learn additional details about where funds were diverted.

Expected results (H2): Support for candidate in Tr3 < support for candidate in Tr4, Tr2 & Tr1.

Hypothesis 3 (Specific Details): When voters learn specific details of the accusation, they are less likely to support a corrupt candidate.

Expected results (H3): Support for candidate in Tr4 = Tr3 < Tr2 = Tr1

Hypothesis 4 (Procedural Information): When voters learn more about the procedures of the Audit Court, they are more likely to understand and believe in the accuracy of the accusation and hence less likely to support a corrupt candidate. The effect is expected to be higher for the subset of subjects who did not answered correctly a question on knowledge of Audit Courts' role.

Expected results (H4): Support for candidate in Tr2 < Tr3 = Tr4

Hypothesis 5 (Any Kind of Information): When the treatment includes more information (of any kind), voters are less likely to support a corrupt candidate.

Expected results (H5): Support for candidate in Tr4 = Tr3 = Tr2 < Tr1

3.5.4 Results

In total, 4,894 subjects responded the survey. A total of 1,506 subjects passed at least one screener question (31% of respondents) and a total of 701 subjects passed both screeners (14% of respondents). In terms of time spent on the vignette's screen, subjects who passed at least one screener spent an average of 88.6 seconds, while subjects who did not pass any screener spent an average of 79.1 seconds. Subjects who passed the two screeners spent an average of 89 seconds. This suggests that subjects who passed screeners did spend more time reading the vignettes.

In table 3.3, I present mean support in each treatment condition for the sample that passed at least one screener using vote intention as the dependent variable. In table 3.4 I present results from the OLS models. Bank account is the reference category. Results fully support the private benefits hypothesis, as the bank account vignette significantly reduces probability to support candidate with respect to all other conditions. The first column reports results for the sample that passed at least one screener without controls. In this column, the size of the effect ranges from a decline in 4.4 percentage points (with respect to the health care condition) to a decline in 9 percentage points (with respect to the baseline condition). Results across all other columns also show that subjects in the bank account condition are significantly less likely to support the candidate.

The interaction with the Audit Courts knowledge dummy is not significant, suggesting no support for H4. In appendix J, I present results of a difference in means for this subset of subjects. This analysis also provides evidence against H4, as only the bank account condition shows significant differences with respect to the other conditions (with the exception of its difference with the health care condition and with the baseline condition using Holm adjusted p-values). As for the interaction with public hospital users, results also are not statistically significant. In tables I.3.3

and I.3.4 (in appendix I) I use ANOVA analysis for heteregeneous causal effects with split samples of subjects who attend private and public hospitals. Results suggest that subjects who attend public hospital are less likely to support the candidate in the health care condition with respect to the baseline and procedural conditions; and results are not significantly different from the bank account condition. In contrast, among private hospital users the health care condition is not significantly different from the other conditions (with the exception of the difference between the health care condition with respect to the baseline condition when using non adjusted p-values). Results from the split samples in this study provide some support for hypothesis 1.1 (as public public hospital users seem to be more information sensitive when there are costs affecting public hospitals). However, the interaction results in table 3.4 is not significant, and all other analyses in the three studies (i.e. interaction terms and split sample difference-in-means analyses) provide little empirical support for this hypothesis.

Table D.3.2 in the appendix shows results of difference-in-means tests with vote intention as dependent variable for subjects who passed at least one screener. Results using both Holm adjusted p-values and non adjusted p-values show that subjects in the bank account condition are significantly less likely to support the candidate. In addition, these results also show that subjects in the health care condition are significantly less likely to support the candidate than subjects in the baseline and procedural conditions.

Table E.3.2 in the appendix presents results for the estimated differences in satisfaction with mayor for the sample that passed at least one screener. Results also fully support the private enrichment hypothesis, as the bank account condition significantly reduces support for the candidate with respect to all the other conditions (both using Holm adjusted p-values and non adjusted p-values). No other significant

results are observed.

In tables F.3.3 and F.3.4 in the appendix, I present results for the complete sample using both vote intention and satisfaction with mayor as dependent variables. Again, results fully support the private enrichment hypothesis. In addition, in both tables the differences in vote intention and satisfaction with the candidate in the procedural and health care condition are negative and significant with respect to the baseline condition.

In tables H.3.1 and H.3.2 in the Appendix I present results for the sample of subjects who passed both screeners. Results again show consistent support for the private enrichment hypothesis. Only the bank account condition does not significantly differ from the health care condition when using probability to vote as the dependent variable (with both Holm adjusted p-values and non adjusted p-values), and when using satisfaction with mayor as the dependent variable (with Holm adjusted p-values). There are no other statistically significant differences.

In sum, overall results in experiment 2 fully support the private benefits hypothesis.

TABLE 3.3: Mean Vote Intention
Study # 2. At Least One Screener

Baseline	Procedural	Health Care	Bank Account
2.55	2.54	2.23	1.92
(1.83)	(1.76)	(1.69)	(1.58)
N=381	N=381	N=384	N=360

Standard Deviation in Parenthesis. DV: Likelihood to Vote for Candidate.

TABLE 3.4: OLS Regression on Vote Intention. Bank Account is the Reference Category. Study # 2

	Screener (1 or 2)	All Subjects	Screener (1 or 2)	Screener (1 or 2)	Screener (1 or 2)	Screener (1 or 2)
Baseline	0.63***	0.48***	0.63***	0.64***	0.46**	0.86***
	(0.13)	(0.08)	(0.00)	(0.13)	(0.20)	(0.19)
Procedural	0.62***	0.29***	0.59***	0.57***	0.52***	0.59***
	(0.13)	(0.00)	(0.00)	(0.13)	(0.19)	(0.19)
Health Care	0.31**	0.23***	0.27**	0.27**	0.35*	0.47**
	(0.13)	(0.00)	(0.12)	(0.13)	(0.20)	(0.19)
Education		-0.06***	-0.06***	-0.06**	-0.06**	-0.05**
		(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Income		-0.01	0.03	0.04	0.04	0.01
		(0.01)	(0.28)	(0.03)	(0.03)	(0.03)
Sex		-0.06	0.14	0.11	0.13	0.09
		(0.06)	(0.16)	(0.10)	(0.10)	(0.10)
Knowledge Audit Court				-0.28***	-0.34*	-0.28**
				(0.09)	(0.19)	(0.09)
Public Hospital						0.10
						(0.18)
Baseline*Audit Court					0.31	
					(0.26)	
Procedural*Audit Court					0.09	
					(0.25)	
Health Care*Audit Court					-0.15	
					(0.26)	
Baseline*Public Hospital						-0.40
						(0.11)
Procedural*Public Hospital						-0.06
						(0.25)
Health Care*Public Hospital						-0.47
						(0.25)
Intercept	1.92***	2.69***	2.01***	2.16***	2.16***	2.22***
	(0.09)	(0.14)	(0.24)	(0.25)	(0.27)	(0.29)
N	1,506	4,894	1,506	1,506	1,506	1,506

Standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.1.
Sex coded as Female=1, Male=0

Table G.3.2 in appendix G presents the results of a logistic regression on likelihood to pass the screeners. Similar to study # 1, more educated subjects and those who more frequently talk about politics are more likely to pass the screeners. In contrast to study # 1, younger subjects are more likely to pass the screener and there is no significant difference between women and men. In addition, wealthier subjects are more likely to pass the screeners, and subjects leaning on the left are more likely to pass one screener.

3.6 Third Study

Results of the second study showed that subsequent information on the candidate's illicit enrichment drives a stronger punishment effect. The purpose of the third study is twofold. First, the bank account and health care vignettes were modified to make them more homogeneous to improve internal validity. In the second study, those vignettes included specific stories on the corruption episodes found out by the Audit Court to accommodate its format to the one of typical real news and as a test for the specific details versus abstract news hypothesis. The trade off is that as different forms of specific stories are developed, other details of the story besides the treatment itself might have an impact on the outcome of interest. In this new experiment, those conditions are displayed in more homogeneous vignettes, without developing concrete stories, so that other background conditions are kept similar.

Second, I include a new vignette with a new form of moral misbehavior to test the "deontological morality" hypothesis. In particular, I test if the same treatment effect could be detected when adding information that the mayor lied to the Audit Court when justifying his use of public funds. If subjects weigh the mayor's intentions more than the consequences of his behavior, then we should detect a similar punishment effect when they learn that the mayor diverted funds as when they learn that he lied. In contrast, if subjects are particularly concerned with elected officials' illicit enrichment, we should still detect a stronger effect in the private gain vignette.

In addition, I add a new vignette increasing the negative stimulus of the public costs as a harder test for the private gain hypothesis. The new stimulus makes explicit the costs associated with the misuse of funds by informing subjects that as a consequence, the primary care health center couldn't be finished. This is the costs

condition[35]. The health care treatment is used as the new baseline condition (see additional details in the summary of treatment conditions section).

3.6.1 Experimental Design

The experiment was run in January 2016. The recruitment method was similar to the one used in studies 1 and 2, with one modification. To avoid repeated survey takers, I used a Facebook pixel that identified and prevented showing the ad to users who completed those surveys. In addition, I set the survey in the same Qualtrics platform that was used in experiment 2; the survey was set to prevent re-taking the survey (as consequence subjects who completed experiment # 2 who attempted to complete for the first time experiment# 3 would be considered re-takers). Finally, I included a question asking subjects if they took a similar survey during the months that the first and second experiments were being run.

The baseline vignette was identical to the one used in study # 2 with only one modification; it included the information that irregularities included over-invoicing and no-bid contracts and that misappropriated funds came from a health care program. That means that the new baseline condition had more information than the baseline condition in studies # 1 and # 2. A second vignette included the information that the candidate lied to the Audit Court when justifying expenses (lies condition). A third vignette included the information that because of the misuse of funds, the primary care center was not built (costs condition). A fourth vignette included the information that the candidate diverted the misappropriated funds to his own bank

[35]It is not uncommon to see similar real cases, in particular when the municipal government depends on federal transfers for education or health related expenses. In those cases, when federal audits detect irregularities, the federal government will stop transfers. See last example in appendix K for a similar case in real news.

account (bank account condition). The same screener questions used in study # 2 were included. The four treatment conditions can be summarized as follows:

Tr1 Baseline (Limited Information): Information that misappropriated funds come from a health care program.
Tr2 Lies (Extended Information): Baseline plus information stating that the mayor lied when justifying expenses.
Tr3 Costs (Extended Information): Baseline plus information that as a result of the misuse of funds, the primary care center was not built.
Tr4 Bank Account (Extended Information): Baseline plus information that the misappropriated funds were diverted to the mayor's bank account.

3.6.2 Hypotheses and Expected Results

H1 Deontological Morality Hypothesis: Likelihood to support a corrupt candidate drops as subjects learn additional details emphasizing the candidate's moral misbehavior (lying or diverting funds to his bank account).
Expected results (H1): Support for candidate in Tr2 < Tr1 & Tr3. Support in Tr4 < Tr1 & Tr3
H2 Private Benefits Hypothesis: Likelihood to support a corrupt candidate drops specifically when subjects learn details on the illicit enrichment of the candidate.
Expected results (H2): Support for candidate in Tr4 < Tr1, Tr2 & Tr3
H3 Public Costs Hypothesis: Likelihood to support a corrupt candidate drops as subjects learn additional details about the program from which funds were misused and on the specific costs of that mismanagement.
Expected results (H3): Support for candidate in Tr3 < Tr1, Tr2 & Tr4

3.6.3 Analysis and Results

A total of 4,918 subjects completed the survey. A total of 563 subjects of those 4,918 subjects had taken one of the previous surveys. These subjects were dropped from the final analysis (resulting in a total N of 4355 subjects). Subjects who passed at least one screener spent on average 100 seconds reading the treatment vignette, while subjects who failed to pass any screener spent an average of 90 seconds. Subjects who passed both screeners spent an average of 103 seconds reading the vignette. This suggests, again, that screener passage is a good indicator of treatment compliance.

In table 3.5, I present mean vote intention for the sample of subjects who passed either screener.

TABLE 3.5: Mean Vote Intention
Study # 3. At Least One Screener

Baseline	Lies	Costs	Bank Account
2.15	2.25	2.24	1.84
(1.68)	(1.72)	(1.65)	(1.44)
N=397	N=380	N=371	N=386

Standard Deviation in Parenthesis. DV: Likelihood to Vote for Candidate.

In table 3.6, I present results of the OLS analyses. Bank account is the reference category. Results fully support the private benefit hypothesis. In the first column I presents results using the sample that passed at least one screener without controls. Subjects in the bank account condition are significantly less likely to support the candidate than subjects in all other conditions. The magnitude of the effect ranges from a 4.4 to a 5.8 percentage point decline in the probability of voting for the mayor. The

relative decline in the magnitude with respect to studies # 1 and # 2 is probably a result of the additional information provided in the baseline condition. Results from the sample of all candidates presents similar results with the only exception that the difference between the lies and the bank account condition is not significant. In the third and fourth columns I use the sample that passed at least one screener with various controls. Results also fully support the private benefits hypothesis. In the last column I include the interactions with the public hospital dummy. In this analysis, the difference between the bank account condition and the other conditions is not significant (at the .05 level) and the interaction is not significant. This is the only model, though, where the differences are not significant at the .05 level.

In table D.3.3 in appendix D, I show results for the difference-in-means test for subjects who passed at least one screener. Results also fully support the private benefits hypothesis as the difference between this condition and all other conditions is significant (with both Holm adjusted p-values and with non adjusted p-values). No other significant differences are observed. Table E.3.3 in appendix E presents results of similar analyses using the alternative dependent variable. These results also provide full support for the private benefits hypothesis. Results with the sample of all subjects (in tables F.3.5 and F.3.6 in the appendix) are also consistent, with the only exception that the differences between the bank account condition and the lies condition are not significant (using both vote intention and satisfaction with mayor as dependent variables and with any of the p-value estimations). Results with the sample that passed both screeners (in tables H.3.3 and H.3.4 in the appendix) are also consistent, although some differences between the bank account condition and the other conditions are not statistically significant at the .05 level; in particular, the differences between the bank account and the lies conditions.

Tables I.3.5 and I.3.6 in the appendix provide evidence against hypothesis 1.1, as

public hospital users are not information sensitive when provided with evidence of the costs of corruption. We should note, however, that in study # 3, the health care information is present across all conditions.

Table G.3.3 in the appendix presents results of the probability to answer correctly the screener questions, with results consistent with those of the previous surveys (although in this case women are less likely to pass screener).

TABLE 3.6: OLS Regression on Vote Intention. Bank Account is the Reference Category
Study # 3

	Screener (1 or 2)	All Subjects	Screener (1 or 2)	Screener (1 or 2)	Screener (1 or 2)
Baseline	0.31***	0.19**	0.33***	0.33***	0.29*
	(0.12)	(0.08)	(0.12)	(0.12)	(0.17)
Lies	0.41***	0.12	0.45***	0.43***	0.30*
	(0.12)	(0.12)	(0.12)	(0.12)	(0.17)
Costs	0.40***	0.28***	0.44***	0.44***	0.33*
	(0.12)	(0.00)	(0.12)	(0.12)	(0.18)
Education		-0.03**	-0.06***	-0.06***	-0.06***
		(0.01)	(0.00)	(0.02)	(0.02)
Income		-0.05***	-0.07***	-0.07***	-0.06**
		(0.02)	(0.00)	(0.03)	(0.03)
Sex		-0.07	0.01	0.00	0.01
		(0.21)	(0.83)	(0.08)	(0.08)
Audit Court Knowledge				-0.16*	-0.15*
					(0.09)
Baseline*Public Hospital					0.07
					(0.23)
Lies*Public Hospital					0.27
					(0.23)
Costs*Public Hospital					0.20
					(0.24)
Intercept	1.84***			2.60***	2.57***
	(0.08)			(0.22)	(0.26)
N	1,538	4,355	1,538	1,538	1,538

Standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.1. Sex coded as Female=1, Male=0

3.7 Conclusions

This study provides evidence that different types of information on the same corruption incident affect subjects' reaction towards the corrupt candidates involved in the incident. The effects of these manipulations on subjects' response were used to infer the type of ethical considerations behind their rejection of corrupt candidates. Results from study # 1 show that voters' rejection is not contingent upon the type of expenditure from which funds were misappropriated, nor on whether funds were used for self-enrichment or for the benefit of other party members. Results from studies # 2 and # 3 provide consistent evidence that subjects' judgment does not rely on a cost evaluation of the consequences of corruption, nor it is based on a strict deontological conception, insofar as not every type of immoral behavior is equally punished. The strongest rejection is specifically triggered by information on illicit enrichment. This means that in subjects' evaluation of corrupt candidates, their judgment is grounded on a restricted moral conception that considers that the public office should never be used for illicit enrichment.

These results show that corruption is viewed more as a candidate's trait than as a malfeasance bearing negative consequences. Learning about the costs of corruption doesn't increase salience of the corruption treatment, as subjects weigh corruption as a candidate's attribute rather than as a cause of specific negative outcomes.

We should note, however, that all vignettes presented incumbent mayors who had positive backgrounds records in a number of areas. A limited interpretation of this study, therefore, is that the findings are only valid for the cases of candidates with good records (outside of the corruption incident). That is, voters might be more information sensitive to public costs when incumbents show worse background records. Whether information sensitivity varies across this or other dimensions of candidacy

could be tested in future research.

These findings have important repercussions as we interpret the political consequences of corruption scandals. They suggest that when the evidence of corruption is grounded on proofs of illicit enrichment, the effect of such a scandal on public opinion will be stronger. In contrast, when the information is grounded in offenses such as over-invoicing or no bid-purchases of specific government expenditures, the impact on public opinion will be weaker.

Appendix A. Vignettes

First Study Vignettes:

Treatment 1

Imagine that you were living in another municipality of Brazil similar to the one in which you currently live and that current the mayor in this municipality is running for reelection. Since he has been mayor conditions in his municipality have significantly improved. He completed new public works, and the street cleaning has significantly improved. The State Audit Court examined how he used public funds and concluded that there were no irregularities in his use of public funds.

Versions 2-6

Imagine that you were living in another municipality of Brazil similar to the one in which you currently live and that the current mayor in this municipality is running for reelection. Since he has been mayor, conditions in his municipality have significantly improved. He completed new public works, and the street cleaning has significantly improved. The State Audit Court examined how he used public funds and concluded that there were many irregularities such as purchases without bidding and over-invoicing [in funds spent to improve the quality of public hospitals/ in funds spent in technical services for maintenance of computers in several municipal offices/ (OR NOTHING)]. Subsequent investigations of the State Audit Court showed that funds were diverted [to the mayor's bank account/ to benefit members of the mayor's party who worked with him in the electoral campaign/ (OR OMIT THIS LINE)].

Second and Third Study Vignettes:

Imagine that you were living in another municipality of Brazil similar to the one in which you currently live and that the current mayor in this municipality is running for reelection. The State Audit Court examined how he used public funds when he was mayor and concluded that there were many irregularities and consequently rejected his accounts [Insert 1/ Insert 2/ Insert 3/ Insert 4].

Despite the decision, the candidate was allowed to run for reelection. The candidate is still very popular and has good chances of being reelected. Since he was elected, conditions in the municipality have significantly improved. As mayor he completed new public works, improved street cleaning, as well as the quality of public transport.

Second Study:

Insert 1 (Baseline): [Nothing]

Insert 2 (Procedural): The decision to reject accounts was made by the first chamber of the Audit court, the agency in charge of auditing the use of public funds. The investigation determined that the use of public money was not in conformity with the principles established by law.

Insert 3 (Health Care): The investigation of the Audit Court showed irregularities in the use of resources destined for a health care program. The Court determined that the mayor is responsible for over-invoicing in hiring a company to build a primary

care health center and for non bidding in a purchase of material that was intended to be used for this center.

Insert 4 (Bank Account): Subsequent investigations of the State Audit Court showed that funds were diverted to a mayor's bank account. According to the investigation, the mayor created a civic association through which he signed a contract used to divert public funds to his own bank account.

Third Study:

Insert 1 (Baseline): The investigation of the Court showed that the mayor is responsible for over-invoicing and no-bid purchases in funds that were intended to build a primary care health center.

Insert 2 (Lies): The investigation of the Court showed that the mayor is responsible for over-invoicing and no-bid purchases in funds that were intended to build a primary care health center. The Court also showed that the mayor lied when he testified that he did the required bid for various purchases as several of the alternative budgets he presented were faked.

Insert 3 (Costs): The investigation of the Court showed that the mayor is responsible for over-invoicing and no-bid purchases in funds that were intended to build a primary care health center. The Court also showed that as a consequence of those irregularities the primary health care center could not be finished.

Insert 4 (Bank Account): The investigation of the Court showed that the mayor

is responsible for over-invoicing and no-bid purchases in funds that were destined to build a primary care health center. The Court also showed that the over expenses and no-bid purchases were used by the mayor to divert part of those funds to his own bank account.

Follow up Questions (Common for the Three Studies):

How likely would you vote for a candidate like this?

Not at all o o o o o o o Very likely

How satisfied would you feel with a mayor like this?

Not at all satisfied o o o o o o o Very satisfied

Appendix B. Sample Descriptive Statistics.

TABLE B.3.1: Sample Descriptive Statistics. All Subjects

	Study 1	**Study 2**	**Study 3**	**Census**
Household Income				
0-2 x minimum wage	42.3	48.0	44.1	38.5
2-5 x minimum wage	32.0	30.2	30.9	36.4
5+ X minimum wage	16.3	15.1	17.7	25.1
No Response	9.4	6.7	7.3	
Region				
North	4.6	3.9	4.1	7.4
Northeast	15.3	16.2	16.0	26.6
Center-West	6.5	6.7	7.4	7.3
Southeast	50.9	49.2	47.5	43.8
South	14.4	14.9	14.5	14.9
No Response	8.3	9.1	10.5	
Education (18 years old or older)				
Primary Incomplete or Less	5.5	9.6	8.2	45.1
Primary Complete or Secondary Incomplete	14.8	18.9	16.4	16.6
Secondary Complete or Tertiary Incomplete	44.1	45.8	43.1	27.9
Tertiary Complete	35.3	25.6	32.3	10.0
No Response	0.2	0.1	0.1	
Other				
Age (median)	34	30	31	38
Male	33.1	29.5	34.2	48.2

TABLE B.3.2: Sample Descriptive Statistics. At Least One Screener

	Study 1	**Study 2**	**Study 3**	**Census**
Household Income				
0-2 x minimum wage	37.3	36.1	31.1	38.5
2-5 x minimum wage	34.9	37.3	35.9	36.4
5+ X minimum wage	20.7	22.4	28.1	25.1
No Response	7.1	4.2	4.9	
Region (NA omitted)				
North	3.4	4.7	4.0	7.4
Northeast	15.2	13.6	13.9	26.6
Center-West	6.5	6.8	7.6	7.3
Southeast	51.0	50.4	49.3	43.8
South	14.7	17.3	16.1	14.9
No Response	9.2	7.2	9.0	
Education (18 years old or older)				
Primary Incomplete or Less	3.6	2.8	2.7	45.1
Primary Complete or Secondary Incomplete	11.8	11.8	8.5	16.6
Secondary Complete or Tertiary Incomplete	42.1	48.6	43.7	27.9
Tertiary Complete	42.2	36.8	45.2	10.0
No Response	0.3	0.1	0.0	
Other				
Age (median)	35	31	33	38
Male	29.2	30.0	38.0	48.2

TABLE B.3.3: Sample Descriptive Statistics. All Subjects

	Study 1	**Study 2**	**Study 3**	**Americas Barometer**
Party				
None	72.6	77.0	75.9	77.1
PT	7.1	5.3	6.2	12.1
PSDB	5.8	4.4	4.9	2.4
PMDB	4.4	3.2	3.1	3.9
Other	10.1	10.1	9.8	4.5

TABLE B.3.4: Sample Descriptive Statistics. At Least One Screener

	Study 1	**Study 2**	**Study 3**	**Americas Barometer**
Party				
None	72.9	79.0	77.0	77.1
PT	8.0	5.2	6.3	12.1
PSDB	6.3	5.0	5.6	2.4
PMDB	3.2	1.8	2.1	3.9
Other	9.6	9.1	9.1	4.5

Appendix C. Sample Balance Tests

TABLE C.3.1: Balance Across Treatment Groups. Multinomial Logistic Regression
Passed Screener. Study # 1

	No Corruption	Baseline	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
Education	0.79	0.26	0.04	0.75	0.10
	(0.62)	(0.06)	(0.06)	(0.06)	(0.06)
Income	-0.24	-0.04	-0.01	-0.06	-0.11
	(0.07)	(0.07)	(0.07)	(0.07)	(0.75)
Gender (Male)	0.87	-0.01	0.27	-0.14	-0.00
	(0.29)	(0.28)	(0.29)	(0.28)	(0.28)
Knowledge Audit Court	0.28	0.58**	0.38	0.52*	0.68**
	(0.27)	(0.27)	(0.26)	(0.27)	(0.26)
Intercept	-0.95	-0.49	-0.94	-0.54	-0.79
	(0.70)	(0.68)	(0.68)	(0.69)	(0.69)
N (Total = 776)					

Standard error in parenthesis. *** p<0.01, ** p<0.05, *p<0.1.
Health Care/Bank Account is the reference category.

TABLE C.3.2: Balance Across Treatment Groups. Multinomial Logistic Regression
At Least One Screener. Study # 2

	Procedural	Health Care	Bank Account
Education	-0.00	-0.03	0.05
	(0.03)	(0.04)	(0.04)
Income	-0.02	0.01	-0.02
	(0.05)	(0.05)	(0.05)
Gender (Male)	-0.13	-0.03	-0.14
	(0.16)	(0.16)	(0.17)
Knowledge Audit Court	-0.12	-0.23	0.02
	(0.27)	(0.15)	(0.15)
Intercept	0.37	0.25	-0.10
	(0.39)	(0.39)	(0.40)
N (Total = 1,506)			

Standard error in parenthesis. *** p<0.01, ** p<0.05, *p<0.1. Baseline is the reference category.

TABLE C.3.3: Balance Across Treatment Groups. Multinomial Logistic Regression
At Least One Screener. Study # 3

	Lies	Costs	Bank Account
Education	-0.15	-0.01	-0.08
	(0.04)	(0.04)	(0.04)
Income	0.11**	0.07	-0.02
	(0.04)	(0.04)	(0.05)
Gender (Male)	0.12	-0.04	-0.14
	(0.15)	(0.16)	(0.15)
Knowledge Audit Court	-0.01	-0.10	0.01
	(0.15)	(0.15)	(0.15)
Intercept	-0.48	0.24	0.43
	(0.38)	(0.38)	(0.38)
N (Total = 1,538)			

Standard error in parenthesis. *** p<0.01, ** p<0.05, *p<0.1. Baseline is the reference category.

Appendix D. Replication of Results using ANOVA. At Least One Screener

TABLE D.3.1: Estimated Difference in Vote Intention (One Way ANOVA)
Passed Screener. Study # 1

	Baseline	Health Care/ Bank Account	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
No Corruption	-3.37	-3.89	-3.99	-4.05	-3.85
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Baseline		-0.52	-0.62	-0.69	-0.49
		(0.05)	(0.01)	(0.01)	(0.09)
		[0.01]	[0.00]	[0.00]	[0.01]
Health Care/Bank Account			-0.10	-0.16	0.04
			(1.00)	(1.00)	(1.00)
			[0.60]	[0.40]	[0.84]
Health Care/Reelection				-0.06	0.14
				(1.00)	(1.00)
				[0.75]	[0.47]
Computer/Bank Account					0.20
					(1.00)
					[0.31]
Computer/Reelection					
N (Total = 776)	124	141	133	122	131

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE D.3.2: Estimated Difference in Vote Intention (One Way ANOVA)
At Least One Screener. Study # 2

	Procedural	Health Care	Bank Account
Baseline	-0.02 (0.90) [0.90]	-0.32 (0.04) [0.01]	-0.63 (0.00) [0.00]
Procedural		-0.31 (0.04) [0.01]	-0.62 (0.00) [0.00]
Health Care			-0.31 (0.04) [0.02]
Bank Account			
N (Total = 1,506)	381	384	360

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE D.3.3: Estimated Difference in Vote Intention (One Way ANOVA)
At Least One Screener. Study # 3

	Lies	Costs	Bank Account
Baseline	0.09 (1.00) [0.42]	0.09 (1.00) [0.46]	-0.31 (0.03) [0.00]
Lies		-0.01 (1.00) [0.95]	-0.41 (0.00) [0.00]
Costs			-0.40 (0.00) [0.00]
Bank Account			
N (Total = 1,538)	380	371	386

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Appendix E. Analyses using Satisfaction As DV. At Least One Screener

TABLE E.3.1: Estimated Difference in Satisfaction with Mayor (One Way ANOVA)
Passed Screener. Study # 1

	Baseline	Health Care/ Bank Account	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
No Corruption	-3.48	-4.08	-3.97	-4.17	-3.97
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Baseline		-0.60	-0.49	-0.69	-0.49
		(0.00)	(0.03)	(0.00)	(0.03)
		[0.00]	[0.00]	[0.00]	[0.00]
Health Care/Bank Account			0.11	-0.09	0.11
			(1.00)	(1.00)	(1.00)
			[0.51]	[0.59]	[0.50]
Health Care/Reelection				-0.20	0.01
				(1.00)	(1.00)
				[0.24]	[0.98]
Computer/Bank Account					0.20
					(1.00)
					[0.23]
Computer/Reelection					
N (Total = 776)	124	141	133	122	131

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE E.3.2: Difference in Satisfaction with Mayor (One Way ANOVA)
At Least One Screener. Study # 2

	Procedural	Health Care	Bank Account
Baseline	-0.03	-0.22	-0.69
	(0.81)	(0.15)	(0.00)
	[0.81]	[0.05]	[0.00]
Procedural		-0.20	-0.66
		(0.17)	(0.00)
		[0.08]	[0.00]
Health Care			-0.46
			(0.00)
			[0.00]
Bank Account			
N (Total = 1,506)	381	384	360

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE E.3.3: Estimated Difference in Satisfaction with Mayor (One Way ANOVA)
At Least One Screener. Study # 3

	Lies	Costs	Bank Account
Baseline	-0.02	0.05	-0.25
	(1.00)	(1.00)	(0.10)
	[0.89]	[0.67]	[0.02]
Lies		0.06	-0.24
		(1.00)	(0.12)
		[0.57]	[0.03]
Costs			-0.30
			(0.04)
			[0.01]
Bank Account			
N (Total = 1,538)	386	380	371

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Appendix F. Analyses for All Subjects.

TABLE F.3.1: Estimated Difference in Vote Intention (One Way ANOVA)
All Subjects. Study # 1

	Baseline	Health Care/ Bank Account	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
No Corruption	-2.77	-3.07	-3.13	-3.17	-3.03
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Baseline		-0.29	-0.35	-0.39	-0.25
		(0.56)	(0.30)	(0.19)	(0.91)
		[0.07]	[0.03]	[0.02]	[0.13]
Health Care/Bank Account			-0.06	-0.10	0.04
			(1.00)	(1.00)	(1.00)
			[0.71]	[0.54]	[0.80]
Health Care/Reelection				-0.04	0.10
				(1.00)	(1.00)
				[0.81]	[0.55]
Computer/Bank Account					0.14
					(1.00)
					[0.40]
Computer/Reelection					
N (Total = 2,405)	246	296	267	266	257

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE F.3.2: Estimated Difference in Satisfaction with Mayor (One Way ANOVA)
All Subjects. Study # 1

	Baseline	Health Care/ Bank Account	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
No Corruption	-2.96	-3.27	-3.34	-3.30	-3.22
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Baseline		-0.31	-0.37	-0.34	-0.26
		(0.24)	(0.11)	(0.21)	(0.61)
		[0.03]	[0.01]	[0.02]	[0.09]
Health Care/Bank Account			-0.06	-0.02	0.06
			(1.00)	(1.00)	(1.00)
			[0.67]	[0.88]	[0.68]
Health Care/Reelection				0.04	0.12
				(1.00)	(1.00)
				[0.78]	[0.41]
Computer/Bank Account					0.08
					(1.00)
					[0.58]
Computer/Reelection					
N (Total = 2,405)	246	296	267	266	257

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE F.3.3: Estimated Difference in Vote Intention (One Way ANOVA)
All Subjects. Study # 2

	Procedural	Health Care	Bank Account
Baseline	-0.18	-0.25	-0.48
	(0.04)	(0.01)	(0.00)
	[0.02]	[0.00]	[0.00]
Procedural		-0.07	-0.30
		(0.39)	(0.00)
		[0.39]	[0.00]
Health Care			-0.24
			(0.00)
			[0.00]
Bank Account			
N (Total = 4,894)	1,260	1,224	1,177

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE F.3.4: Estimated Difference in Satisfaction with Mayor (One Way ANOVA)
All Subjects. Study # 2

	Procedural	Health Care	Bank Account
Baseline	-0.25	-0.27	-0.63
	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]
Procedural		-0.11	-0.38
		(0.87)	(0.00)
		[0.87]	[0.00]
Health Care			-0.39
			(0.00)
			[0.00]
Bank Account			
N (Total = 4,894)	1,260	1,224	1,177

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE F.3.5: Estimated Difference in Vote Intention (One Way ANOVA)
All Subjects. Study # 3

	Lies	Costs	Bank Account
Baseline	-0.06	0.08	-0.20
	(0.58)	(0.58)	(0.04)
	[0.47]	[0.29]	[0.01]
Lies		0.14	0.15
		(0.25)	(0.25)
		[0.07]	[0.06]
Costs			-0.29
			(0.00)
			[0.00]
Bank Account			
N (Total = 4,355)	1,054	1,041	1,043

P-values in parenthesis for a t-test of the null hypothesis with holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE F.3.6: Estimated Difference in Satisfaction with Mayor (One Way ANOVA)
All Subjects. Study # 3

	Lies	Costs	Bank Account
Baseline	-0.16	-0.03	0.19
	(0.12)	(1.00)	(0.04)
	[0.03]	[0.65]	[0.01]
Lies		0.12	-0.04
		(0.25)	(1.00)
		[0.08]	[0.57]
Costs			-0.16
			(0.11)
			[0.02]
Bank Account			
N (Total = 4,355)	1,054	1,041	1,043

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Appendix G. Likelihood to Pass Screener.

TABLE G.3.1: Probability to Answer Correctly the Screener Question
Study # 1

	Screener
Intercept	-0.24***
	(0.08)
Education	0.03***
	(0.01)
Age	0.01***
	(0.00)
Sex	0.11***
	(0.00)
Income	0.01
	(0.01)
Ideology	0.01
	(0.01)
Frequency of Political Conversation	0.06***
	(0.01)
Frequency of News Following	0.01
	(1.00)
N	2,405

Standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.1. All results from logistic regression. Sex coded as Female=1, Male=0. Ideology measured in a scale from 0 (left) to 10 (right).

TABLE G.3.2: Probability to Answer Correctly the Screener Question
Study # 2

	One Screener	Two Screeners
Education	0.05***	0.03***
	(0.00)	(0.00)
Age	-0.01**	-0.01***
	(0.00)	(0.00)
Sex	0.02	-0.01
	(0.21)	(0.35)
Income	0.01***	0.01***
	(0.00)	(0.00)
Ideology	-0.01**	-0.01
	(0.04)	(0.58)
Frequency of Political Conversation	0.04***	0.02***
	(0.00)	(0.00)
Frequency of News Following	0.01	0.01
	(0.20)	(0.50)
N	4,894	4,894

Standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.1. All results from logistic regression. Sex coded as Female=1, Male=0. Ideology measured in a scale from 0 (left) to 10 (right).

TABLE G.3.3: Probability to Answer Correctly the Screener Question
Study # 3

	One Screener	Two Screeners
Education	0.04***	0.03***
	(0.00)	(0.00)
Age	-0.01**	-0.01***
	(0.00)	(0.00)
Sex	-0.03**	-0.04***
	(0.02)	(0.01)
Income	0.02***	0.02***
	(0.00)	(0.00)
Ideology	-0.00	-0.00*
	(0.00)	(0.00)
Frequency of Political Conversation	0.04***	0.03***
	(0.01)	(0.01)
Frequency of News Following	-0.01	0.01
	(0.01)	(0.88)
N	4,355	4,355

Standard errors in parentheses. *** p<0.01, ** p<0.05, *p<0.1. All results from logistic regression. Sex coded as Female=1, Male=0. Ideology measured in a scale from 0 (left) to 10 (right).

Appendix H: Analyses for Subjects who Passed Both Screeners.

TABLE H.3.1: Estimated Difference in Vote Intention (One Way ANOVA)
Both Screeners. Study # 2

	Procedural	Health Care	Bank Account
Baseline	0.02	-0.27	-0.54
	(0.90)	(0.38)	(0.01)
	[0.89]	[0.12]	[0.00]
Procedural		-0.29	-0.57
		(0.38)	(0.00)
		[0.09]	[0.00]
Health Care			-0.27
			(0.38)
			[0.13]
Bank Account			
N (Total = 701)	173	173	164

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE H.3.2: Estimated Difference in Satisfaction with Mayor (One Way ANOVA)
Both Screeners. Study # 2

	Procedural	Health Care	Bank Account
Baseline	0.03	-0.27	-0.64
	(0.83)	(0.19)	(0.00)
	[0.82]	[0.09]	[0.00]
Procedural		-0.30	-0.68
		(0.19)	(0.00)
		[0.06]	[0.00]
Health Care			-0.38
			(0.09)
			[0.02]
Bank Account			
N (Total = 701)	173	173	164

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE H.3.3: Estimated Difference in Vote Intention (One Way ANOVA)
Both Screeners. Study # 3

	Lies	Costs	Bank Account
Baseline	-0.13	0.04	-0.40
	(0.85)	(0.85)	(0.04)
	[0.41]	[0.78]	[0.01]
Lies		0.17	-0.27
		(0.85)	(0.27)
		[0.28]	[0.07]
Costs			-0.45
			(0.03)
			[0.00]
Bank Account			
N (Total = 777)	203	177	199

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

TABLE H.3.4: Estimated Difference in Satisfaction with Mayor (One Way ANOVA)
Both Screeners. Study # 3

	Lies	Costs	Bank Account
Baseline	-0.20	0.01	-0.34
	(0.52)	(0.90)	(0.10)
	[0.16]	[0.90]	[0.02]
Lies		0.22	-0.13
		(0.52)	(0.72)
		[0.13]	[0.36]
Costs			0.35
			(0.10)
			[0.01]
Bank Account			
N (Total = 777)	203	177	199

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Appendix I: Heterogeneous Treatment Effects. Private Versus Public Hospital Attendance.

Table I.3.1: Estimated Difference in Vote Intention (One Way ANOVA)
Public Hospital Only. Passed Screener
Study # 1

	Baseline	Health Care/ Bank Account	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
No Corruption	-2.93	-3.70	-3.72	-3.69	-3.60
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Baseline		-0.77	-0.80	-0.76	-0.67
		(0.03)	(0.03)	(0.04)	(0.09)
		[0.00]	[0.00]	[0.00]	[0.01]
Health Care/Bank Account			-0.02	0.01	0.10
			(1.00)	(1.00)	(1.00)
			[0.93]	[0.96]	[0.68]
Health Care/Reelection				0.04	0.12
				(1.00)	(1.00)
				[0.89]	[0.63]
Computer/Bank Account					0.09
					(1.00)
					[0.73]
Computer/Reelection					
N (Total = 427)	62	82	71	72	73

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Table I.3.2: Estimated Difference in Vote Intention (One Way ANOVA)
Private Hospital Only. Passed Screener
Study # 1

	Baseline	Health Care/ Bank Account	Health Care/ Reelection	Computer/ Bank Account	Computer/ Reelection
No Corruption	-3.86	-4.13	-4.30	-4.56	-4.15
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Baseline		-0.26	-0.44	-0.70	-0.28
		(1.00)	(1.00)	(0.21)	(1.00)
		[0.37]	[0.14]	[0.02]	[0.34]
Health Care/Bank Account			-0.17	-0.44	-0.02
			(1.00)	(1.00)	(1.00)
			[0.56]	[0.15]	[0.95]
Health Care/Reelection				-0.26	0.15
				(1.00)	(1.00)
				[0.39]	[0.61]
Computer/Bank Account					0.42
					(1.00)
					[0.18]
Computer/Reelection					
N (Total = 325)	59	57	55	50	53

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Table I.3.3: Estimated Difference in Vote Intention (One Way ANOVA)
Public Hospital Attendance. At Least One Screener
Study # 2

	Procedural	Health Care	Bank Account
Baseline	0.03	-0.41	-0.48
	(1.00)	(0.04)	(0.02)
	[0.85]	[0.01]	[0.00]
Procedural		-0.44	-0.52
		(0.03)	(0.02)
		[0.01]	[0.00]
Health Care			-0.08
			(1.00)
			[0.66]
Bank Account			
N (Total = 812)	216	199	180

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Table I.3.4: Estimated Difference in Vote Intention (One Way ANOVA)
Private Hospital Attendance. At Least One Screener
Study # 2

	Procedural	Health Care	Bank Account
Baseline	-0.17	-0.38	-0.82
	(0.54)	(0.14)	(0.00)
	[0.38]	[0.04]	[0.00]
Procedural		-0.21	-0.66
		(0.54)	(0.00)
		[0.27]	[0.00]
Health Care			-0.44
			(0.07)
			[0.02]
Bank Account			
N (Total = 660)	155	174	172

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Table I.3.5: Estimated Difference in Vote Intention (One Way ANOVA)
Public Hospital. At Least One Screener
Study # 3

	Lies	Costs	Bank Account
Baseline	0.21	0.12	-0.35
	(0.58)	(0.90)	(0.12)
	[0.19]	[0.45]	[0.03]
Lies		-0.09	-0.57
		(0.90)	(0.00)
		[0.57]	[0.00]
Costs			-0.47
			(0.02)
			[0.00]
Bank Account			
N (Total = 823)	194	213	206

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Table I.3.6: Estimated Difference in Vote Intention (One Way ANOVA)
Private Hospital. At Least One Screener
Study # 3

	Lies	Costs	Bank Account
Baseline	-0.01	0.04	-0.27
	(1.00)	(1.00)	(0.55)
	[0.98]	[0.83]	[0.11]
Lies		0.04	-0.26
		(1.00)	(0.55)
		[0.81]	[0.12]
Costs			-0.30
			(0.50)
			[0.08]
Bank Account			
N (Total = 698)	182	156	172

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Appendix J. Difference in Vote Intention. Subjects who Didn't Answer Correctly the Audit Court Question.

TABLE J.3.1: Estimated Difference in Vote Intention (One Way ANOVA)
No Audit Court Knowledge. At Least One Screener
Study # 2

	Procedural	Health Care	Bank Account
Baseline	0.13	-0.07	-0.45
	(1.00)	(1.00)	(0.16)
	[0.53]	[0.73]	[0.03]
Procedural		-0.20	-0.58
		(0.98)	(0.03)
		[0.33]	[0.00]
Health Care			-0.38
			(0.28)
			[0.07]
Bank Account			
N (Total = 650)	171	164	152

P-values in parenthesis for a t-test of the null hypothesis with Holm adjustment.
P-values in brackets for a t-test of the null hypothesis with no adjustment.

Appendix K. Public Costs Versus Private Gain in Real News Examples

Accused	Public Costs	Private Gain	Source
Mayor of municipality in the state of Paraíba.	Over-invoicing in public purchases of 66,400 scholar kits.	–	See source reference #1.
Mayor of municipality in the state of Rio Grande do Norte.	No-bid contracts and money diversion from an education program.	Evidence that the money was diverted for mayor's illegal enrichment.	See source reference # 2.
Mayor, vice-mayor and various city councilmen in the state of Piauí.	Money diverted from programs to support public schools.	Money diverted benefited mayor's relatives with over-priced salaries.	See source reference # 3.
Mayor of municipality in the state of Amapá.	–	Mayor created a fake civil association to divert public funds to benefit himself, relatives and allies.	See source reference # 4.
Mayor in municipality in the state of Santa Catarina.	Over-priced contracts and fake bids in expenses for public works.	Diverted funds distributed among mayor and participants in the scheme.	See source reference # 5.
Mayor in municipality in the state of Alagoas.	Money diverted in funds for public health programs.	–	See source reference # 6.
Mayor in municipality in the state of Espíritu Santo.	Money diverted in funds allegedly used to support a charity association.	Funds were distributed among mayor and other participants in the scheme.	See source reference # 7.
Mayor in municipality in the state of Maranhão.	Over-expenses in funds destined to improve health conditions in rural households.	–	See source reference # 8.
Mayor in municipality in the state of São Paulo.	Over-expenses in contract for solid waste collection.	–	See source reference # 9.
Two mayors in municipality in the state of Alaogas.	Irregular use of funds that the federal government transferred to build a primary health care center. As a consequence of those irregularities, the construction of the health care center could not be completed.	–	See source reference # 10.

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Appendix L: Screeners

First Study Screener

Imagine now that this candidate was defeated in the elections and that another opponent candidate won for a small difference. But the difference is not as important as the problem that many persons do not read the instructions carefully. To show that you read this don't choose any of the options below, just type the letter 'k' on your keyboard and continue.
How satisfied would you feel with the results of the elections?

Not at all o o o o o o o Very satisfied

Second and Third Study Screeners:

First Screener:

In the last national elections candidates talked about different issues, such as the state of the economy and corruption. Many times candidates talk about their plans and people do not pay attention. Now we want to see if you are paying attention to what you are reading. To show that you are paying attention, ignore the questions below and choose None of the above and Poverty as your two answers.
In your opinion, what was the most important issue that candidates debated on during the last election?
(1) Unemployment
(2) The state of the economy

(3) Crime

(4) Poverty

(5) Inflation

(6) Corruption

(7) Drugs

(8) Education

(9) Health

(10) None of the above

Second Screener:

There are many forms in which people receive information about their municipality. Some persons get their news from the radio, other talking with neighbors, and other talks with coworkers or classmates. There are also persons who don't pay attention to the questions that researchers do. We want to see if you are paying attention. To show that you read this, please ignore the question below and just choose the '-' option at the very bottom of the list.

How do you regularly receive information about the situation in your municipality?

(1) Radio

(2) Newspaper

(3) TV

(4) Talking with neighbors

(5) Talking with family members

(6) Talking with coworkers

(7) Talking with classmates

(8) At Church

(9) Internet websites

(10) Another form

(11) I don't receive information

(12) -

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Anonymous. Official of the Public Ministry of the Municipal Audit Court of Ceará. March 24, 2015.

Anonymous. Legislator in the State Assembly of Ceará, March 23, 2015.

Anonymous. Mayor in a municipality in the State of Ceará, March 17, 2015.

Anonymous. Assistant of a City Councilman in the City of Recife, Pernambuco, March 10, 2015.

Anonymous. Board Member Substitute of the State of Pernambuco Audit Court, March 11, 2015.

Anonymous. Board Member of the State of Pernambuco Audit Court, March 12, 2015.

Anonymous. Official in the Secretary of External Control, Federal Audit Court, January 25, 2016.

Barbosa de Souza, Marcos Cesar. Official of the Secretary of External Control, Federal Audit Court, Rio de Janeiro, January 25, 2016.

Massa, Gustavo. Public Prosecutor of the Public Ministry of the State of Pernambuco Audit Court, March 16, 2015.

Pacheco, Marcio Emmanuel. Official of the Secretary of External Control of the Federal Audit Court, Rio de Janeiro, January 25, 2016.

Pimentel, Cristiano. Public Prosecutor of the Public Ministry of the State of Pernambuco Audit Court, March 11, 2015.

Ponte, Edilberto. Board Member of the State of Ceará Audit Court, March 18, 2015.

Ramalho Dimas, Board Member of the State of São Paulo Audit Court, January 28, 2016.

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Education

Ph.D. in Political Science (2009 Present).

Department of Political Science, Boston University, GPA: 3.86.

Dissertation Title: Corruption and Electoral Accountability in Brazil. Dissertation Committee: John Gerring, Taylor Boas, Dino Christenson.

Master in International Relations and Negotiations (2009).

FLACSO (Latin American School of Social Sciences)-San Andrés University, Argentina. GPA: 9.27 / 10.

B.A. in Political Science (2003)

University of Buenos Aires, School of Social Sciences, Argentina. GPA: 7.78 / 10

Other Graduate Level Training

Summer Program on Quantitative Methods for the Social Sciences, Interuniversity Consortium for Political and Social Research. Ann Arbor, Michigan, July-August 2012.

Fellowships

2015: Boston University Summer Fellowship.

2014: National Science Foundation, Dissertation Improvement Grant. Project Title: Corruption and Electoral Accountability in Brazil.

2013: Boston University, Graduate Research Fellowship Award.

2013: Latin American Studies Program Fellowship, Boston University.

2011 (September December): Pardee Center for the Study of the Longer-Range Future (Boston University) Research Fellow.

2011 (June - August): Pardee Center for the Study of the Longer-Range Future (Boston University) Summer Fellowship.

2009 2011: Fulbright scholarship for graduate studies in the U.S., Argentine Fulbright Commission.

2007 2009: University of Buenos Aires scholarship for graduate studies.

2006-2007: Argentine Ministry of Education scholarship to assist in the Project "Twenty Years of Democracy in Argentina. Creation of an Oral History Archive", Political History Department, University of Buenos Aires.

Publications in Refereed Journals

"Between Pressure and Support: The U.S. Human Rights Policy and the Argentine Military Government (1976-1978)" (in Spanish), Desarrollo Económico, Revista de Ciencias Sociales, Number 215, Vol. 55, May-August 2015.

"The Changing Roles of The International Monetary Fund and Latin America" (in Spanish), co-authored with Roberto Frenkel, Desarrollo Económico, Revista de Ciencias Sociales, Number 194, Volume 49, July September 2009.

"The Inter American Human Rights Commission in Argentina: Between Democratization and Human Rights" (in Spanish), co-authored with Marcos Novaro, Desarrollo Económico, Revista de Ciencias Sociales, Number 193, Volume 49, April July 2009.

"The Role of Ideas and Strategies in the Creation of IZL and LEHI in Palestine" (in Spanish), Revista Relaciones Internacionales, National University of La Plata, Year 17, Number 34, December 2007-May 2008.

Publications in Non Refereed Journals

"Social Conflicts and Anti-Semitism during the Semana Trágica (Tragical Week)" (in Spanish), co-authored with Matías Obludziner, Report on Anti-Semitism in Argentina 2006, Center for Social Studies, DAIA.

Teaching Experience

Summer 2013: Lecturer in the course "Introduction to Comparative Politics", Political Science Department, Boston University.

Spring 2013: Teaching Assistant in the course "U.S. Media and Politics", Political Science Department, Boston University.

Fall 2012: Lecturer in the course "Democracy in Latin America and its Challenges", International Relations Department, Boston University.

Spring 2012: Teaching Assistant in the course "U.S. Media and Politics", Political Science Department, Boston University.

2006-2007: Teaching Assistant in the course "Methodology of the Social Sciences" (B.A.), School of Economics, University of Buenos Aires, Argentina.

Research Experience

2013-2014: Research Assistant in project Collaborative Research: Center for Historical Information and Analysis (CHIA), supported by the National Science Foundation. Director: John Gerring, Boston University.

2006 - 2009: Research Assistant in the Political History Program, University of Buenos Aires.

2007- 2009: Research Assistant at CEDES (Center for the Study of State and Society), Economics Division.

2005-2006: Research Assistant in the Political History Department, FLACSO (Latin American School of Social Sciences).

Awards

2013: Outstanding Teaching Fellow Award, Boston University.

Conference Papers

2015: "Misuse of Public Funds and Electoral Accountability in Brazil". Argentine Society of Political Analysis Annual Conference, Mendoza, Argentina.

2013: "Corruption and Electoral Accountability in Brazil", Midwest Political Science Association Annual Conference, Chicago, Illinois.

2012: "Corruption, Accountability, and Citizen Participation in Protests in Latin America", Midwest Political Science Association Annual Conference, Chicago, Illinois.

2010: "NGOs, Civilian Leaders and the Carters Human Rights Policy to Argentina (1976-1978)", New England Council for Latin American Studies (NECLAS) Annual Meeting, University of Connecticut.

2008: "Between Pressure and Support: U.S. Human Rights Policy to Argentina after the Military Coup of 1976 and the Moderates", Society for Historians of American Relations (SHAFR) Annual Meeting, Ohio.

2007: "American Bureaucracy and Foreign Policy Change: The Transition to the James Carter Human Rights Policy to Argentina (1976-1977)", Argentine Society of Political Science VII National Congress.

2006: "The Role of Ideas and Strategies in the Creation of IZL and LEHI in Palestine", presented during the Sixth Congress on Middle East, National University of La Plata.

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