

**Electoral accountability of  
corrupt politicians:  
Conditioning factors revisited**

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**Abstract**

In the past years, a growing literature has examined the impact of corruption accusations on voting behavior and found that, although incumbents appear to suffer some vote share loss after being associated with corruption scandals, a vast majority of them gets reelected nonetheless. If voters do not exercise electoral accountability against corrupt politicians as effectively as democratic theory would expect, what conditions explain this pattern? Which factors favor or hinder their decision to remove corrupt incumbents from office? The literature suggests a number of contextual factors and voters' attitudes that may condition corruption voting, but most studies examine them in a fragmented way. This paper seeks to address this gap in the existing scholarship on the topic by building a comprehensive model to test the validity of five central hypotheses discussed in previous works. Original corruption data from randomized audits in 383 Brazilian municipalities are used in the analysis. The results provide partial evidence for only two of the hypothesized mechanisms: (a) electoral accountability of corrupt incumbents is weakened by recent positive assessments of their performance in office, in particular in terms of improvements in economic conditions, and (b) voters appear to punish more strongly politicians facing more corruption accusations, but this is conditional on the timing of the audit.

**Keywords: corruption, electoral accountability, voters' behaviour, Brasil**

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## 1. Introduction

In the past years, the sustained electoral success of candidates accused of corruption in various contexts has puzzled many researchers. This apparent paradox of “unpopular corruption and popular corrupt politicians” (Kurer 2001, 63) has motivated two interconnected research streams: one concerned with estimating more precisely the extent to which voters retrospectively punish incumbents based on corruption information—what has been called corruption voting (Rundquist et al. 1977)—and another, more recent one seeking to understand what keeps voters from exercising electoral accountability against corrupt incumbents.

The first of those research streams has largely corroborated that voters, more often than not, fail to hold corrupt politicians accountable at the ballot box. On the one hand, the electorate seems to withdraw some support for corrupt incumbents, resulting in (small) losses in their vote shares (Bågenholm 2013; Chong et al. 2015; Costas-Pérez et al. 2012; Dimock and Jacobson 1995; Eggers and Fisher 2011; Lafay and Servais 2000; Peters and Welch 1980; Reed 1999; Welch and Hibbing 1997) and a somewhat lower probability of reelection (Chang et al. 2010; Ferraz and Finan 2008; Jucá et al. 2016; Pereira and Melo 2015). Nevertheless, still a vast majority of corrupt politicians are able to secure a new term, even in more consolidated democracies. Therefore, although voters are not completely oblivious to the involvement of their representatives with corruption, “[...] the modal corrupt politician is successfully reelected despite charges, or even convictions, of illegal behavior” (Golden 2006, 8). Moreover, the few cases of across-the-board rejection of corrupt politicians have <sup>1</sup>been so exceptional, that a more cynical observer may wonder not why voters sometimes fail to throw the rascals out, but instead why they are ever able to punish them at all (Johnston 2013).

So if we know that electoral accountability does not always work, the question that begs more attention is when does it work, i.e. under what circumstances are voters willing and able to remove corrupt politicians from office? The literature briefly discussed above has also raised a few hypotheses on factors that may condition voters’ response to corruption accusations, including contextual factors such as the information environment (Chang et al. 2010; Ferraz and Finan 2008) and the type of electoral system (Eggers and Fisher 2011; Reed 1999), as well as characteristics of the electorate, such as partisanship (Dimock and Jacobson 1995). These and other hypotheses on how corruption voting is in fact contingent on a wide range of macro- and micro-level factors (de Sousa and Moriconi 2013) have been further developed in numerous recent observational and experimental studies pertaining to the second stream of research mentioned earlier. Nevertheless, current scholarship on these conditioning variables is still largely fragmented, with partly contradictory findings and studies that often examine isolated hypotheses and overlook other relevant competing explanations.

The main objective of the analysis presented here is to contribute to this emerging empirical literature by assessing the determinants of electoral accountability against corrupt politicians more systematically, in a comprehensive model that emphasizes the main recurring hypotheses raised in the existing scholarship. In order to gain additional inferential leverage, the focus of the analysis will be on comparing the electoral performance of successful and unsuccessful corrupt politicians,

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<sup>1</sup> See Castro and Nunes (2014), Rennó Jr. (2008) and Chang et al. (2010) for a few examples from Brazil and Italy.

thus exploring a sub-sample that allows us to implicitly observe complex simultaneous interactions between corruption information and other explanatory factors that have been only partly modeled in previous studies.

The methodological approach relies on original corruption data from 383 Brazilian municipalities, thus exploiting both the availability of a concrete indicator of corruption and the advantages of a sub-national context with institutional commonalities. Overall, the findings show partial evidence for only two of the hypothesized mechanisms: (a) electoral accountability of corrupt incumbents is weakened by recent positive assessments of their performance in office, in particular in terms of improvements in economic conditions, and (b) voters appear to punish more strongly politicians facing more corruption accusations, but this is conditional on the timing of the audit.

The study is structured as follows: section 2 presents an overview of the core hypotheses discussed in the literature and the related findings; section 3 describes the methodological approach and data sources; section 4 presents the estimation procedures and results; and section 5 discusses the main findings and concludes with their implications for future research in this area.

## 2. Theoretical background and previous findings

The broader literature on electoral accountability has highlighted that its effectiveness can be affected or conditioned by a number of factors, from institutional structures<sup>2</sup> to individual voter characteristics (Anderson 2007; Ashworth 2012; Kayser 2014). In the past years, several studies have addressed this question in the particular case of corruption voting, by examining various isolated arguments often articulated in the theoretical literature on retrospective voting and voting behavior more generally. This section seeks to present an overview of the most recurring arguments in the relevant literature and the related empirical findings. This review will serve as the basis for the selection of hypotheses to be tested later.

One of the key hypotheses of interest here refers to the role of information. Indeed, we can consider information on corrupt behavior on the part of incumbent politicians as a pre-condition for electoral accountability, understood as a sanctioning mechanism. This argument has direct connection with contemporary models of electoral accountability that have emphasized the importance of the informational environment for voting behavior (Ashworth 2012; Besley 2006; Klašnja 2011). Based on a principal-agent framework, such models rely on the idea that there is some information asymmetry between voters and politicians, where the former cannot fully observe politicians' types (i.e. how honest or competent they are) and their actions in office. Therefore, it is argued that uninformed and ignorant voters are less likely to hold corrupt politicians accountable at the ballot box, since they lack the information to recognize the politicians' type (Chang and Kerr 2009; Golden 2009; Klašnja 2011; Rundquist et al. 1977; Winters and Weitz-Sha-

<sup>2</sup> As briefly mentioned earlier, formal institutional features such as electoral systems have been mentioned as potentially affecting corruption voting (Eggers and Fisher 2011; Reed 1999; de Sousa and Moriconi 2013). However, in the empirical literature of interest here, these factors have not been systematically explored, given that a vast majority of studies either takes an experimental approach or focuses on single countries, rendering such issues irrelevant. Two studies explore a moderating effect of institutional factors pertaining to the concept of clarity of responsibility, but both fail to find consistent evidence to support this, and comparability of results is weakened by the fact that the indicators used vary considerably (Ecker et al. 2016; Xezonakis et al. 2016). Given the limited emphasis that these factors have received so far in the existing literature, they are not included among the main hypotheses highlighted in this study, but are taken into consideration as control variables for the empirical analysis conducted later.

piro 2013). By the same logic, increased availability of information should improve signals of politicians' characteristics that are relevant for voters' decisions, thereby increasing voters' ability to punish corrupt incumbents and select better candidates.

Once information on the corrupt behavior of politicians is made available, either through public enforcement agencies, media reports or accusations from opponents, an essential factor influencing its potential electoral impact is the extent to which this information effectively reaches voters. In association with this argument, a few studies have highlighted the influence of the media on electoral accountability against corrupt politicians. Ferraz and Finan (2008), for instance, analyze the impact of corruption information from municipal audit reports on the electoral performance of incumbent mayors in Brazil. They find that the presence of local radio stations strengthens the negative effect of the audits' findings, when information on corruption dealings is uncovered. The study by Chang et al. (2010) on electoral accountability for corruption throughout Italian post-war legislatures also emphasizes the impact of the media in explaining the exceptional reaction of the electorate against corrupt legislators in the 1994 elections. The authors attribute this unique episode of strengthened electoral accountability to changes in the informational environment, due to an upsurge in press coverage of corruption issues at the time. A similar argument is made by Costas-Pérez et al. (2012), who find that more intense media coverage of corruption scandals involving Spanish mayors, measured as the number of news articles published, resulted in a larger loss of vote share in the subsequent elections. Klašnja et al. (2016) also argue that the effect of corruption perceptions on voting intention is moderated by its level of salience, which is intensified by increased media coverage on the issue.

Also in connection with the information hypothesis, a different approach is taken by Klašnja (2011), who uses a measure of political knowledge derived from survey data to capture the differential effects of corrupt accusations on voting behavior according to the level of political information that voters possess. He finds that a voter at a high level of political awareness is significantly less likely to vote for an incumbent involved in a corruption scandal, whereas less informed voters do not appear to have their decision influenced by the scandals.

Finally, an experimental study by Figueiredo et al. (2011) used the context of the 2008 runoff municipal elections in São Paulo to test how targeted information on corruption involvement of the candidates would affect their electoral performance. Both the incumbent mayor and the challenger candidate had been simultaneously included in a black-list of candidates convicted on administrative improbity charges that was disclosed by a magistrate's association prior to the election. The experiment involved distributing flyers with information on the conviction of one of the candidates to households in 100 randomly assigned voting precincts. The authors find a negative effect of the flyer treatment, but only in the case of the challenger candidate. These results partially confirm how ensuring that voters have access to information on corruption involvement of candidates can strengthen corruption voting, but the authors also point to other potential contingent effects related to candidate-specific attributes (e.g. party affiliation) or differences in the severity of the charges (Figueiredo et al. 2011, 28)<sup>3</sup>. These alternative hypotheses are discussed in more detail below.

One important reservation to be made about the impact of information on electoral accountability concerns its credibility and reliability (Botero et al. 2015; Jiménez and Caínzos 2004; de Sousa and Moriconi 2013; Weitz-Shapiro and Winters 2014). A key aspect refers to the source of the ac-

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3 Mr. Kassab received a conviction for improper use of public advertisement funds for an ad campaign that allegedly promoted his personal interests while he was Secretary of Planning in São Paulo, in 1997. The conviction was overturned on appeal. Ms. Suplicy, on the other hand, was convicted for having awarded a R\$ 2 million no-bid contract, during her term as mayor of São Paulo (2001-2004), to a NGO founded by her in 1990 and where she was honorary chairman until 2000. Her conviction was still on appeal at the time of the election.

cusations, and the perceived agenda that the accusers might have. It is argued, for instance, that accusations from independent enforcement or control agencies should be seen as less biased, and therefore more credible, by voters (Fernández-Vázquez et al. 2013; Klačnja 2011; Weitz-Shapiro and Winters 2014; cf. Sberna and Vannucci 2013);<sup>4</sup>, in contrast to accusations from adversary parties, that would be more likely discounted by voters as “partisan tricks” (Rundquist et al. 1977, 955; cf. Bågenholm and Charron 2014).

Two studies on corruption voting in Spain corroborate these arguments. A survey experiment conducted by Muñoz et al. (2012) found that the strategy of the accused incumbent’s political party of either acknowledging corruption allegations or denying them altogether has an impact on how credible voters perceive the accusations to be: respondents were more likely to dismiss corruption allegations from opposition parties as “noise”, i.e. empty accusations with little credibility, and continue to vote for the accused mayor when his/her party refused to acknowledge the charges. Another study by Costas-Pérez et al. (2012) shows that corruption scandals reported by the media and later corroborated by judicial investigations had a larger effect on electoral outcomes than those dismissed by the courts or not investigated, thus suggesting that voters assigned different degrees of credibility to the accusations based on the level of judicial involvement in the case. Winters and Weitz-Shapiro (2014) similarly find evidence for the impact of source credibility on corruption voting in a survey experiment conducted in Brazil. They show that respondents’ were significantly less likely to vote for an incumbent mayor accused of corruption, and that this reaction was stronger when the accusation came from federal audits instead of an opposition party.

Although the informational thesis remains prominent in the literature, it fails to account for many of the empirical cases that have been explored in previous studies, where punishment of corrupt politicians was limited despite highly visible scandals in countries with considerable press freedom and no apparent shortage of information (Chang and Kerr 2009). Therefore, other hypotheses have explored the perspective that voters may have other reasons to continue supporting corrupt politicians, despite being aware of their corrupt dealings, and thus face trade-offs in their voting decision. One such trade-off would involve an implicit exchange mechanism, whereby voters may rationally choose to support a corrupt candidate by weighing the corruption accusations against their evaluation of the politician’s type based on other criteria, such as competence or ideological congruence (Muñoz et al. 2012; Rundquist et al. 1977; Winters and Weitz-Shapiro 2013). Therefore, while facing the choice between a corrupt candidate that appears to be competent or shares their views on other important issues and a clean candidate seen as incompetent or as having contrary positions on those issues, they may prefer the former.

A few experimental studies from different settings have found evidence supporting the implicit exchange argument. Rundquist et al. (1977), for instance, show that respondents were much less likely to switch their vote after knowing that their preferred candidate was accused of corruption, whenever they shared his/her policy positions on issues that were particularly salient to the respective respondent. The experiment by Muñoz et al. (2012) in Spain also included treatment vignettes with information on the performance of the incumbent in office to test this hypothesis, showing that individuals who were told that the incumbent mayor had a good administration were significantly more likely to ignore the corruption accusation when declaring their vote preference, in comparison to those that were told the mayor’s administration was poor.

Another survey experiment conducted by Konstantinidis and Xezonakis (2013) in Greece also

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4 Nevertheless, De Sousa and Moriconi (2013) mention that this depends on the level of trust in those institutions. If, for instance, voters don’t trust the Judiciary, their confidence in information on prosecutions and convictions could be negatively affected. Botero et al. (2015) provide an example of this with their study on Colombia, where corruption information published in a reputable newspaper was seen as more trustworthy than that coming from the courts.

finds evidence in support of the implicit exchange hypothesis: respondents who were told that a mayor was accused of corruption, but had cut taxes during his/her administration, had a statistically significant higher propensity of voting for him/her in comparison to the control group that did not receive this information. Winters and Weitz-Shapiro (2013), on the other hand, find no robust evidence for the implicit exchange mechanism as a result of a survey experiment conducted in Brazil. Drawing on the Brazilian popular belief that voter behavior is commonly influenced by this logic—embodied in the well-known expression “rouba, mas faz” (“he robs, but gets things done”)—, they test whether voters, presented with a trade-off between a corrupt and competent<sup>5</sup> mayor and a non-corrupt incompetent one, are more likely to vote for the former. Their results show that respondents were on average 34% more likely to vote for the clean incompetent mayor than for the corrupt competent one<sup>6</sup>.

Some observational studies examine this type of trade-off hypothesis with different approaches, and find similarly confirmatory evidence. A study by Pereira and Melo (2015) on Brazilian municipalities tests whether voters are less reactive to accusations of corruption against governments that provide them with more public goods, measured as public expenditures on areas such as education, health, transportation, security, and housing. Indeed, they show that the impact of information about corruption on electoral outcomes exists, but disappears as the level of expenditures rises. Brollo (2010) also highlights a mediating effect of public expenditure. She finds that corruption information uncovered through municipal audits does impact mayors’ reelection chances, but argues that this effect is largely explained by the fact that municipalities with more irregularities are “punished” by the central government and receive less transfers. Thus, voters’ electoral response against corrupt mayors is in most cases a consequence of a decline in the provision of public infra-structure resulting from reduced federal transfers than a direct response to the corruption accusations per se.

Also examining the municipal level in Spain, Fernández-Vázquez et al. (2013) discuss a different version of the implicit exchange hypothesis, where they test whether voters’ punishment of corrupt mayors is conditional on the welfare impact of their corrupt dealings. They classify each case of corruption in their sample as welfare-enhancing or welfare-decreasing, based on an assessment of the economic externalities associated with the corrupt transactions<sup>7</sup>, and find that corruption accusations only have a statistically significant negative effect on the incumbent’s vote share for the welfare-decreasing cases. Therefore, consistent with the implicit exchange idea, voters appear to be less likely to exercise electoral accountability against corrupt mayors if their illegal activities have generated economic benefits for the municipality<sup>8</sup>.

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5 The understanding of competence applied in the study refers to the politician’s performance in providing public goods (Winters and Weitz-Shapiro 2013).

6 Nevertheless, they do find partial evidence for the trade-off argument in a sub-sample of high income respondents, among which the corruption information had no effect on voting behavior.

7 The authors consider corruption to be welfare-enhancing when it leads to an increase of economic activity in the municipality, at least in the short term. An example that they mention is the construction of a housing project in a protected area, which generates new jobs and local demand. Cases of procurement fraud and embezzlement, on the other hand, more clearly generate inefficiencies and are thus considered as welfare-decreasing. The classification is associated with the counterfactual of the economic impact if the mayor had abstained from corruption (Fernández-Vázquez et al. 2013, 10).

8 While testing the alternative hypothesis of the effect of information, though, they find that increased media attention to the scandal contributes to electoral retribution even in cases of welfare-enhancing corruption, which again highlights the conditioning effect of the information environment.

A few other works have approached this implicit exchange hypothesis from a different perspective, by testing whether the state of the economy conditions corruption voting. It is argued that voters may trade-off corruption for economic well-being: under favorable economic conditions, they would be more willing to overlook corruption and continue to support the government, whereas a bad economic situation would trigger a stronger response of voters against corrupt politicians (Zechmeister and Zizumbo-Colunga 2013). Two cross-national studies present evidence confirming this argument. A comparative analysis of 19 Latin American countries by Zechmeister and Zizumbo-Colunga (2013) shows that, as citizens' evaluations of the national economy improves, the effect of corruption perception on presidential support decreases, disappearing for very positive assessments of economic conditions. Another comparative study on 115 developing countries<sup>9</sup> also found evidence that corruption perceptions only affect the incumbent government's vote share negatively under low or negative economic growth (Choi and Woo 2010). An experimental study by Klašnja and Tucker (2012) also offers partial support for a moderating effect of economic conditions on corruption voting. They find evidence that corruption voting is conditional on bad economic circumstances in Moldova, identified as a high corruption country, but the same effect was not observed in a similar experiment conducted in Sweden, a low corruption country, where corruption voting took place regardless of the state of the economy.

The implicit exchange argument emerged as a counterpoint to a more traditional hypothesis in the literature, namely that voters' support for corrupt politicians may be related to another type of trade-off associated with more 'explicit' inducements, i.e. concrete material incentives that voters receive from a politician accused of corruption (Johnston 2013; Rundquist et al. 1977). This would most likely be related to practices such as clientelism, or patronage, which can be understood as "[...] a particular mode of 'exchange' between electoral constituencies as principals and politicians as agents in democratic systems [...]" (Kitschelt and Wilkinson 2007, 7), where clients usually receive material inducements such as jobs, contracts, cash transfers or other similar advantages in return for political support to their patrons (Manzetti and Wilson 2007). Voters that belong to clientelistic networks thus have an incentive to retribute material benefits that they receive or expect to receive from their patron politicians by voting for them, regardless of whether they are accused of corruption in office or not (Chang and Kerr 2009; Kurer 2001; Manzetti and Wilson 2007).

This hypothesis can be particularly difficult to test empirically due to the challenge of documenting good measures of clientelism (Kitschelt et al. 2009). Manzetti and Wilson (2007), for instance, examine the effect of individual corruption perceptions on support for the government conditioned by the level of clientelism in a sample of 14 countries. They try to go around the problem of measuring the latter variable by using an indicator of weak institutions—an index of government effectiveness—, based on the argument that clientelism and weak institutions are closely associated. They find that respondents who perceive corruption to be widespread in their country are more likely to be satisfied with the government when government effectiveness is low, and the authors argue that, in those conditions, voters are more likely to benefit from patronage<sup>10</sup>.

Chang and Kerr (2009) also explore the consequences of corruption on government support conditioned by the existence of patronage, but they develop a more specific, survey-based indicator of patronage at the individual level. Using Afrobarometer data for 18 sub-Saharan African democracies, they examine the effect of personal experience with corruption on government sup-

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9 It is important to note, however, that the sample used in this study also includes partial democracies, which raises questions about its adequacy for examining conditions for electoral accountability.

10 However, several questions may be raised about their assumptions and choice of indicator for clientelism, since government effectiveness may be linked to support for government through other mechanisms besides the one hypothesized by them.

port, conditioned on voters' status of "insiders", which includes both patronage insiders, i.e. voters that directly benefit from patronage networks, and identity insiders, who may identify with politicians on a partisan or ethnic basis. Respondents were coded as patronage insiders when their response to the question of what they would do to solve problems they might face due to bureaucratic red tape or government harassment was "use connections to influential people", which the authors interpret as a sign that those individuals are more likely to have such connections in the first place. Their empirical analysis finds that experience with corruption among insiders has no significant effect on support for the government, whereas it has a significant negative effect in the case of outsiders<sup>11</sup>.

Konstantinidis and Xezonakis (2013), on the other hand, do not find strong evidence that clientelism weakens voters' reaction to corruption accusations. Their experiment included a clientelism treatment in which participants were told that the mayor had implemented a program of temporary jobs for unemployed citizens in the municipal administration by-passing the usual hiring procedures in the public sector. They find that information on the mayor's attempt to build a clientelistic network had an estimated negative effect on the propensity to vote for this mayor, although this was not robustly significant<sup>12</sup>.

The studies by Chang and Kerr (2009) and Konstantinidis and Xezonakis (2013) discussed above draw attention to another factor that may influence electoral accountability of corrupt politicians, namely voters' **identity bias**. When voters identify with those politicians on aspects such as partisanship or ethnicity, they may be more inclined to dismiss corruption accusations either because they deem them unreliable or because they tend to be more tolerant of corruption involvement by politicians they support or identify with (Anduiza et al. 2013; Chang and Kerr 2009; Golden 2009; Jiménez and Caínzos 2004; Konstantinidis and Xezonakis 2013; Muñoz et al. 2012). Therefore, such identity-related factors appear to distort the cognitive process by which voters interpret information on corruption, and may thus reduce their disposition to punish corrupt incumbents with whom they share such characteristics<sup>13</sup>.

The empirical evidence on these arguments are inconclusive. Chang and Kerr (2009) partly provide support for this hypothesis in their study, in which they find that partisan and ethnic insiders' perception of corruption by the incumbent they support is significantly lower than for outsiders, i.e. those that identified with the opposition parties or other ethnic groups. However, they do not find confirmatory evidence that these insiders are indeed more tolerant of corruption. Dimock and Jacobson (1995) also offer evidence for the partisanship argument, showing that party identification had a significant impact in voters' reaction to the House Bank scandal. Ecker et al. (2016) also provide evidence for this hypothesis with their finding that individuals' inclination to vote for the opposition is influenced by their perception of corruption in the country, but only for those that are not particularly attached to any political party.

Konstantinidis and Xezonakis (2013), on the other hand, find no evidence that party identification affects voters' decision to support or punish corrupt incumbents. The study by Rundquist et al. (1977) even finds evidence contradicting the partisanship hypothesis, as the respondents with strong party identities in their survey were more likely to switch their vote to the opposition when their preferred candidate was accused of corruption. The study by Chong et al. (2015) on corruption voting in 12 Mexican municipalities also finds evidence countering the partisanship ar-

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11 One drawback from the study, though, is that the authors do not distinguish between patronage and identity insiders, thus making it impossible to identify to which degree each mechanism is driving these results.

12 It is important to note, however, that the study does not apply the clientelism hypothesis as involving whether respondents directly benefited from clientelistic exchange.

13 The impact of these factors can also be related to extensions of political agency models. One such analysis is offered by Besley (2006, 124–28).

gument, in that information of corruption appears to weaken party identification. A similar effect is found by Winters and Weitz-Shapiro (2015) in the Brazilian context, where accusations of corruption against certain parties appear to shift partisan attachments towards other parties within the more educated electorate.

At the same time, other studies offer additional evidence in support of the causal mechanism associated with the identity bias hypothesis. Anderson and Tverdova (2003), for instance, find that political allegiances more generally condition the impact of corruption on voters' attitudes toward government. They observe that the level of perceived corruption in the country has a negative impact on respondents' evaluation of the political system, but voters that support the opposition tend to have a significantly lower evaluation than voters that elected the incumbent government. A study by Davis et al. (2004) on Latin America similarly finds that perceptions of corruption are significantly affected by partisanship. In a survey with almost 3,400 respondents from Chile, Mexico and Costa Rica, supporters of the incumbent parties had on average lower perceptions of corruption than opposition supporters. Finally, a survey experiment conducted by Anduiza et al. (2013) in Spain also corroborates these claims. Participants were told that a hypothetical mayor had been accused of influence peddling, with the treatment vignette varying with regards to the party affiliation of the mayor: no party affiliation mentioned, the respondent's party (same party treatment) or the party opposed to the respondent's party (different party treatment). Respondents in the same party treatment group judged the accusation to be significantly less serious than respondents in the different party treatment group<sup>14</sup>.

Another aspect that may condition corruption voting is voters' perceptions about the **availability of viable alternatives** in the political market (Charron and Bågenholm 2016; Jiménez and Caínzos 2004; Kurer 2001; Muñoz et al. 2012; Pavão 2015; de Sousa and Moriconi 2013). This issue can be directly linked to another central element of the contemporary political agency literature, namely the distribution of good and bad types in the pool of politicians available to voters. Differently than earlier models of retrospective voting where the electorate was expected to simply decide to retain an incumbent based on his/her performance in office, more recent variations of political agency models emphasize that voters in fact seek to select the best available candidate as well (Ashworth 2012; Besley 2006; Fearon 1999). Therefore, voters' decision to keep or replace an incumbent depends also on their expectations about the probability that challengers are of a better type. In a context where voters become cynical and perceive the political elite as a whole as being corruption, they may have little incentive to replace a corrupt incumbent (Kurer 2001; Muñoz et al. 2012; Pavão 2015).

Although this argument is raised in several more theoretical discussions in the literature, only two studies were found that directly test it empirically, and they point to different findings. The experimental study by Muñoz et al. (2012), for instance, tests whether voters who are led to believe that all candidates are corrupt are more likely to vote for a corrupt incumbent. However, they fail to confirm this hypothesis. A recent work by Pavão (2015), on the other hand, argues that voters, especially in contexts with more widespread corruption, tend to be more cynical about corruption and to see all the options of parties and candidates available to them as relatively undifferentiated on this dimension. Using survey data from Brazil and also from a cross-national setting, she provides empirical evidence of this type of cynicism and that this attitude weakens corruption voting.

It may also be the case that voters do identify possible "clean" alternatives, but none that correspond to their ideological preferences (Jiménez and Caínzos 2004). Reed (1999) presents this as one of the reasons that hindered electoral accountability against the scandal-ridden Liberal Democratic Party (LDP) in Japan for most of the post-war period. This argument is developed in a

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14 The authors highlight that this result was apparently driven by individuals with lower political sophistication, since the same pattern was not observed among those with a higher level of political knowledge.

more sophisticated manner by Charron and Bågenholm (2016), who use data from a recent European survey to show that voters with more extreme ideological positions, either to the left or the right of the scale, are less likely to react to corruption accusations and change their vote than their more centrist counterparts, as they face fewer alternatives in the political spectrum. In line with their argument, they find that this is less so the case as the effective number of political parties increases. Another study by the same authors provides additional evidence for the alternatives hypothesis. Bågenholm and Charron (2014) find that the vote share gains for European political parties that use anti-corruption rhetoric, including accusations against opponents, in their electoral campaigns are most pronounced in the case of new parties. A similar finding is presented by Engler (2016), who also provides evidence that changes in perceptions of corruption across different countries and time periods has favored the electoral performance of new parties. These findings may be construed as an effect of signaling to voters that the entry of new players into the political market may strengthen electoral accountability by increasing the number of legitimate alternatives available.

Finally, another relevant factor in conditioning corruption voter is the severity of the irregularities attributed to those accused (Jiménez and Caínzos 2004). Ferraz and Finan (2008), for instance, show evidence of this in their study of the electoral effects of audit reports in Brazilian municipalities. They find that the level of corruption found in the audits conditions the effect of the audit results on mayor reelection rates, significantly reducing the probability of reelection for those accused of more violations. Chong et al. (2015) also find that the level of corruption impacted voters' electoral reaction in Mexican municipalities, where they larger vote share losses for incumbent mayors in municipalities where the share of diverted resources was above 66%. Figueiredo et al. (2011) also allude to the potential impact of this factor in explaining the different degree of corruption voting against the two mayoral candidates in São Paulo, where an impact of information of corruption was only found in the case of the candidate with more severe charges.

As revealed by the literature review presented above, existing research on the conditioning factors of electoral accountability against corrupt politicians is considerably fragmented. Firstly, we find a variety of relevant hypotheses that have been repeatedly examined in numerous empirical studies, but often with contradictory findings. Secondly, although a number of studies consider more than one hypothesis in the analysis conducted, they still fail to take other important alternative arguments into account, and therefore do not provide a consistent and comprehensive empirical analysis of the factors that explain why electoral accountability in such cases is sometimes effective and others not.

One clear example are the studies examining two of the central hypotheses discussed in the literature, namely the role of information and clientelism. In the case of the former argument, the contributions on the Italian (Chang et al. 2010) and Brazilian (Ferraz and Finan 2008; Figueiredo et al. 2011; Winters and Weitz-Shapiro 2013) contexts do not address the potential role of clientelism in political systems where clientelistic practices are still very relevant (Desposato 2006; Kitschelt and Wilkinson 2007, 3). Similarly, the studies assessing the role of clientelism in explaining electoral support to corrupt politicians, such as the ones by Manzetti and Wilson (2007) and by Chang and Kerr (2009), do not take potential variation in the availability of information into account. The many experimental studies in this field tend to do a better job at controlling for (most) other potential explanations, either through randomization or in the experimental design itself, but limitations to their external validity and the extent to which survey responses indeed reflect concrete behavior demand some caution in the interpretation of such results (Charron and Bågenholm 2016; Winters and Weitz-Shapiro 2015).

Another weakness that emerges from the literature at hand relates to the type of data used in a considerable part of the studies surveyed. Many of them rely on perceptions of corruption, such as aggregate perception indicators at the country-level or measures of individual perceptions from a

variety of surveys. This raises a few issues about potential inherent biases present in such sources of data (Anderson and Tverdova 2003; Davis et al. 2004; Mondo 2016) and endogeneity problems associated with identifying the direction of causality in the relationship between perception data and voting preferences (Kayser 2014).

A third limitation that we can identify is that most of the studies mentioned focus on examining the effect of corruption information on the reelection of politicians, and thus establish a comparison between corrupt and non-corrupt politicians. However, there is great variation in how strongly politicians accused of corruption are held accountable by voters, and it is important to examine what factors explain this variation. If the studies find, in general, that the majority of corrupt politicians manage to get reelected, it is relevant to understand the factors that may explain why the rest does not, and this perspective has only rarely been explored (Reed 1999; Rennó Jr. 2008).

This study will attempt to address these limitations by developing a more comprehensive model of corruption voting, including the main hypotheses highlighted in the discussion above<sup>15</sup> and other potential confounding factors raised in the literature on voting behavior. Additionally, this work seeks to make an empirical contribution to this area of research by exploring a concrete indicator of corruption in Brazilian municipalities, derived from audit reports, and by testing unexplored indicators for some of the main independent variables analyzed. Finally, the focus will be set on the factors explaining the electoral fate of successful and unsuccessful corrupt politicians, which allows us to implicitly model simultaneous interactions of all the independent variables considered with the occurrence of corruption, something that has only been partly modeled in previous studies. With this approach, the objective here is to help improve our understanding of how electoral accountability of corrupt politicians is contingent on other factors and under which conditions it is more likely to be effective. The details on the methodology and the data applied are discussed in the next section.

### 3. Methodological approach and data sources

As already highlighted, the main goal of this study is to make a contribution to research on the conditions that favor or hinder electoral accountability against corrupt politicians by assessing the validity of the main hypotheses discussed earlier in a single, more comprehensive model, allowing us to better evaluate their relative weight in explaining the electoral performance of corrupt incumbents. In order to do that, the empirical setting selected for the analysis is that of Brazilian municipalities.

Although focusing on the aggregate level to analyze a phenomenon ultimately connected to individual behavior may involve the danger of incurring in ecological fallacies, the choice of setting for the study was motivated by several important advantages that this context offers in terms of inferential leverage and data availability. Firstly, focusing on the sub-national level in a single country allows us to control for several commonalities across sub-national units, including a wide range of formal institutions and also cultural factors that may affect electoral accountability and its covariates. At the same time, we still observe considerable internal variation among Brazilian municipalities on many dimensions of interest to this study. Secondly, data sources for Brazilian municipalities are very rich, and offer the opportunity to explore novel indicators for some variables that are rarely available for other levels of analysis.

In particular, the availability of several randomized audit rounds conducted by a federal agency

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15 The only explanation that will not be directly addresses, due to contextual conditions related to the Brazilian setting, is the identity bias hypothesis. The reasons for this methodological choice are discussed in detail in the next section.

in hundreds of municipalities provides us with valuable and credible information on the concrete occurrence of corruption at this administrative level. This is an important methodological advantage, in contrast to several previous works that have used data on corruption perceptions instead. As suggested by a number of studies (Anderson and Tverdova 2003; Davis et al. 2004), individual corruption perceptions may not be the most adequate measures to identify an effect of corruption on voting, as these perceptions may be themselves affected by individual biases<sup>16</sup>. For this reason, given some of the trade-offs involved in studying corruption voting at the individual level versus an aggregate level of analysis, the points raised above are believed to tip the balance in favor of the latter for the purpose of this study.

Brazil is also an ideal case for this analysis due to the fact that almost all the hypotheses discussed above are highly relevant for the Brazilian context, as shown by the large number of studies focusing on this country to examine the conditions for electoral accountability (Ferraz and Finan 2008; Figueiredo et al. 2011; Pavão 2015; Pereira and Melo 2015; Rennó Jr. 2008; Weitz-Shapiro and Winters 2014; Winters and Weitz-Shapiro 2013). And even though no study has directly tested the material inducement hypothesis in this particular context, the specific literature on clientelism points out that this is also a relevant issue in Brazilian politics, with considerable regional variation (Desposato 2006; Speck 2003). Therefore, five main hypotheses were selected from the literature review presented earlier as the central focus of the empirical analysis developed here. They are summarized as follows:

#### Information

H1: In contexts where the electorate has access to more information on corrupt behavior by politicians, the likelihood that corrupt incumbents are voted out of office is higher.

#### Implicit exchange

H2: In contexts where the administration of corrupt incumbents has improved economic well-being and public service provision, the likelihood that they are voted out of office is lower.

#### Material inducement

H3: In contexts where clientelism and patronage are more prevalent, the likelihood that corrupt incumbents are voted out of office is lower.

#### Alternatives

H4: In contexts where voters find more alternatives to vote for, the likelihood that corrupt incumbents are voted out of office is higher.

#### Severity of accusations

H5: Corrupt incumbents that are accused of several acts of corruption are more likely to be voted out of office.

As we can see, the only hypothesis discussed in the previous section that is not included for testing in this study is the role of identity bias, either based on ethnicity or in the form of partisanship. The main reason for this is that the specific political literature on voting behavior in Brazil suggests that these factors are of limited relevance for this particular context. In the case of ethnic identities, they do not appear to reflect a significant characteristic of the linkage between voters

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<sup>16</sup> This is not only pointed out in the case of corruption voting, but also in the literature on performance voting and economic voting more generally: voters that are more sympathetic to the government tend to have more positive perceptions of that government's performance than those who are more critical to begin with (Kayser 2014).

and candidates in Brazil. Ethnic voting has been examined more often with a focus on the black electorate, and the existing literature shows very little evidence that ethnic identities significantly influence voting behavior in Brazil, highlighting rather that black voters tend to vote more according to other types of social and ideological cleavages (Campos 2015; Mitchell 2009). Moreover, there is no official data available on the racial profile of candidates for the 2008 election which would enable an analysis of this hypothesis as part of this study.

As for the effect of partisanship, a few factors contribute to weaken our a priori expectation that partisan identities should play a strong role in voting decisions among the electorate. Several studies on elections in Brazil highlight that voting behavior is much more influenced by personalistic aspects, such as candidates' attributes and image, than partisan preferences (Almeida 2008; Ames et al. 2009; Kinzo 2005; Nicolau 2002, 2015; Samuels 2006)<sup>17</sup>. Even though voters may, on legislative elections, vote for a specific party label, a small share of votes is cast like this (Nicolau 2002, 2015; Samuels 1997). Additionally, the behavior of the political elites, marked historically by very prevalent party switching, indicates that their electoral strategies do not strongly rely on party identification, thus suggesting that this is not a strong orientation among the electorate (Nicolau 2002; Novaes 2014; Samuels 1997)<sup>18</sup>. At the individual level, several studies have shown that the share of voters that identify with a particular party is relatively low in comparison to more established democracies, and these identities appear to be quite malleable and fluid over short periods of time, being in particular negatively affected by corruption scandals (Kinzo 2005; Nicolau 2014; Samuels 2006; Winters and Weitz-Shapiro 2015)<sup>19</sup>. Finally, recent evidence indicates that, even for the minority of voters that have a specific party preference, this preference is rarely translated into their actual voting decision (Nicolau 2015). As a consequence of these arguments suggesting a weak role of partisanship in voting behavior in Brazil, we can also infer that an attempt to capture partisan preferences at the aggregate level, which would have to rely on indicators such as electoral volatility or historical vote shares of different parties, would not be a reliable strategy.

Therefore, given the several arguments in the literature suggesting the limited relevance of ethnic identities and partisanship to understanding voting behavior in this context, together with the empirical challenges of measuring these variables, these hypotheses are not included for testing in the models developed here. The operationalization of variables for testing the other five hypotheses selected, as well as the criteria for sample selection, are described in the sub-sections below.

#### *Sample selection and coding procedure for the corruption data*

The sample used in this study was chosen in connection to the availability of data on corruption occurrences from audit reports published by the Brazilian Federal Comptroller's Office (CGU).

<sup>17</sup> This is likely fostered by a candidate-centered electoral system, based on plurality rule (with runoff election in some cases) for Executive offices and open list proportional representation for the legislative bodies at all levels of government (Kinzo 2005; Samuels 1997).

<sup>18</sup> Party-switching appears to have somewhat decreased since a ruling by the Superior Electoral Court (TSE) from 2007 established that legislative seats belong to the party and not to the candidate, thus opening the possibility that turncoat legislators lose their seat as a result of going to a different party after elected (Novaes 2014). However, despite the reduced incentives for opportunistic party-switching after this ruling, this type of behavior is still widespread, as politicians have made use of the few loopholes still legally allowed (e.g. founding of new parties or mergers, allegations of internal persecution due to ideological differences) to avoid potentially losing office.

<sup>19</sup> It is important to note that partisan identity is heavily skewed towards the Worker's Party (PT), which seems to be the only party with a more solid base of supporters (Kinzo 2005; Samuels 2006; Samuels and Zucco 2014).

Since the criteria for sample selection and the data collection from those reports are interlinked, they are both presented together in this sub-section. The variable derived from the coding of the audit reports is used here for two main purposes: (a) to determine a key sub sample of interest for the analysis, namely municipalities where at least one corruption occurrence was found, and (b) to operationalize the main independent variable for testing H5.

All municipalities included in the sample have been previously audited through a lottery program conducted by the CGU. The program was introduced in 2003 and has conducted 40 lottery rounds until 2015<sup>20</sup>. So far, over 2,000 municipalities have been audited at least once, representing almost 40% of all Brazilian municipalities<sup>21</sup>. The time period covered by the program includes three electoral cycles: 2004, 2008 and 2012. However, due to time and resource constraints for the data collection, the analysis was restricted to the 2008 electoral cycle, i.e. municipalities selected in any of the 12 audit rounds that took place between 2005 and October 2008, when the municipal elections took place<sup>22</sup>.

During the selected period, a total of 721 municipalities were audited through the lottery system. However, since we are interested in the impact of corruption information on the electoral performance of incumbent mayors after the release of the reports, only those municipalities where the mayor in power ran for reelection in 2008 can be included in the analysis<sup>23</sup>. Therefore, the final sample for which corruption data from the audit reports was collected includes 383 municipalities. Although this represents only slightly less than 7% of all Brazilian municipalities, this sample is still quite representative of the whole universe of cases, in terms of regional distribution, development levels and population (see Table 5 in the Appendix).

For the selected municipalities, the content of the audit reports published by the CGU was coded to generate a variable representing the number of corruption violations found by the auditors. Only violations associated with the period under the incumbent's mayor administration, i.e. the period 2005-2008 were included in the database. Following other works based on these audit reports (Ferraz and Finan 2008, 2011), irregularities associated with either diversion of public funds, overinvoicing or procurement fraud were coded as corruption violations<sup>24</sup>. Differently than previous studies, however, this study considered an additional nuance for the coding of this information, namely the variation in the language used by the auditors to describe the various situations coded as instances of corruption. For instance, similar situations are portrayed in some of the reports with more technical language, which may not be immediately perceived as associated with corruption by the general public, while others use less ambiguous terms such as fraud, favoritism, or simulation<sup>25</sup>. This may ultimately impact how voters perceive the corruption information

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20 The randomized lottery is still being implemented, although under the scope of a new audit strategy and no longer as a stand-alone audit program.

21 For more detailed background information on the program, see Mondo (2016).

22 Two other reasons motivated this decision. First, previous studies using data from this series of audit reports have already analyzed its impact on the 2004 elections (Brollo 2010; Ferraz and Finan 2008), so covering a different time period is an opportunity to test whether previous findings also hold for another time frame. Second, data coverage on other relevant covariates for the following period, namely the 2012 electoral cycle, is more limited, and fewer municipalities were audited between 2009 and 2012.

23 Brazilian mayors are only allowed to run for one consecutive term at a time. Therefore, the sample excludes municipalities with second-term mayors and those where the incumbent mayor decided not to run again.

24 For a more detailed description of the coding procedure and the criteria applied, see Mondo (2016).

25 Despite this type of variation across the different audit reports, comparability on the substance of the information is still warranted by the high level of professionalization of the auditors

and how they use it to ponder their voting decision. All irregularities describing situations pertaining to any of the three categories mentioned above were coded as corruption violations, but those portrayed with terms more clearly associated with corruption were coded as “unambiguous” violations, and those described with more technical language were coded as “ambiguous”<sup>26</sup>. The variables based on both coding strategies will be tested in the empirical analysis.

#### *Other variables and data sources*

A wide range of data sources for Brazilian municipalities offer several useful indicators for testing the hypotheses described earlier. Starting with the dependent variable of interest, namely electoral accountability of corrupt mayors, the main indicator used is a binary variable indicating whether the incumbent mayor associated with corruption found in the audit was voted out of office (1) or not (0) in the 2008 election. An alternative indicator for robustness checks is the change in vote share experienced by the incumbent mayor between 2004 and 2008. Both indicators are derived from official electoral records published by TSE.

In order to test the information hypothesis (H1), two alternative indicators derived from municipal surveys conducted by the Brazilian Institute of Geography and Statistics (IBGE) were selected in order to capture variation in local media presence across municipalities. The first one indicates the presence of local radio stations, and the second one the presence of local newspapers (Ferraz and Finan 2008, 2011). For both indicators, the year of reference is 2006. Given that information from the audit reports was disseminated by the CGU through summaries of the findings made available on to the media (Ferraz and Finan 2008, 2011), the indicators chosen are believed to reflect a key feature of the local information environment that potentially captures variation in the intensity with which the corruption information may have reached the local electorates. Additionally, since the corruption data collected originated from audits conducted by an external federal agency, credibility issues associated with the information hypothesis becomes less relevant for our context, which is another advantage from using a concrete indicator of corruption.

To assess the validity of the implicit exchange hypothesis (H2), the analysis relies on three types of indicators. The first one is based on an index of municipal development (IFDM) published by the Industry Federation of Rio de Janeiro (Firjan), which is derived from different sub-indicators compiled from official sources. The index ranges from 0 to 1 and includes three components: Employment and Income, Education and Health, thus reflecting both an assessment of the economic conditions of the moment and the quality of basic public service provision in two key areas<sup>27</sup>. Therefore, this indicator appears to be particularly useful for testing this hypothesis because it

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employed by the CGU, and also by the fact that all audits follow the same guidelines and standards and use the same techniques to identify administrative irregularities in the audited units. Therefore, it is unlikely that it reflects systematic variation in terms of how effective auditors are in uncovering corruption violations.

26 Based on an inductive approach, a list of terms and expressions was identified as more clearly depicting corruption irregularities according to their language connotation and their common use in media reports about corruption.

27 Information on the index is available on <http://www.firjan.com.br/ifdm/>. The sub-indicators used to compile each dimension are the following: the Employment and Income component reflects change in formal employment, share of formal occupation rate in the local market, change in average income, average wages and the Gini coefficient; the Education component includes coverage of childcare, age-grade distortion in basic education, share of teachers with a higher education, average daily school hours, abandonment rate and performance test results; and the Health component considers coverage of regular pre-natal care, share of unexplained deaths, avoidable infant mortality and avoidable hospital admission due to lacking basic healthcare.

reflects the state of affairs in three areas that voters care about and are likely to consider in their retrospective evaluation of the municipal government. The indicator is available for each year of our period of interest, and in order to capture what voters might assess in terms of improvement or worsening of the conditions in those three areas, we use the change in the indicator observed between 2005 and 2008. A second indicator applied in the analysis measures the real percentage change in total intergovernmental transfers during the incumbent's administration as compared to the previous term, therefore between 2004 and 2008. This indicator, similar to the first one, also follows approaches found in the literature to assess the implicit exchange hypothesis based on potential changes in the provision of public services (Brollo 2010; Pereira and Melo 2015). The third indicator used is the average municipal GDP per capita growth for the incumbent's term, which captures a change in the state of the economy as a potential conditioning effect of corruption voting<sup>28</sup>.

For the material inducement hypothesis (H3), the main indicator applied in the study is a novel measure of patronage derived from the IBGE municipal surveys, namely the share of municipal employees selected exclusively through direct appointment, i.e. those that have not been recruited through any kind of competitive recruitment procedure (e.g. public examinations). This is considered as a useful measure of patronage at the municipal level because it reflects the degree of discretion used by office holders in the appointment of positions in the municipal bureaucracy, which may be used also for political reasons (Barone 2010). Indeed, the high variation across Brazilian municipalities is quite telling about how mayors make use of this discretion to different degrees: although the mean share of exclusively appointed officials is around 10%, this figure may extend to more than 70% in some cases<sup>29</sup>. The indicator for 2008 is applied in the analysis, and alternatively we also employ an indicator of change in the share of directly appointed employees in the municipal administration, using as reference for comparison the years of 2004 and 2008, which might better capture the extent to which the incumbent mayors' electoral performance may have been affected by increases or decreases in the distribution of patronage during their administration.

Testing the alternatives hypothesis (H4) involves some empirical challenges, since it is essentially impossible to precisely observe and measure the range of politicians' types available to voters for the mayoral election in each municipality. Given this difficulty, this study draws on insights from Charron and Bågenholm (2016), who use the effective number of parties (ENP) as a proxy for the options available to voters. The logic is that, where voters have elected more parties to the local legislative, it is likely that they perceive a wider range of alternatives in the local electoral market. For this indicator, the average for 2004 and 2008 is used, as to minimize the effect of potential shocks from each election year on the resulting indicator.

Finally, the number of corruption violations in each municipality derived from the CGU audit reports, as described in the previous sub-section, is the measure used to operationalize the severity of accusations faced by incumbent mayors (H5). The number of each audit round is also included in the analysis to account for a potential moderating effect of the timing of the audit, as voters

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28 In order to test for potential voter "myopia" regarding government performance information (Kayser 2014), the GDP per capita growth only for the election year is also considered, as well as the change in the IFDM and in intergovernmental transfers only between 2007 and 2008.

29 It is important to acknowledge that this is certainly not the only means mayors have to distribute material resources to their clientele. One example are cash transfer programs, and there is some evidence that even a federal program of the kind has been used for clientelistic purposes at the municipal level in Brazil, since the registration of beneficiaries is conducted by the municipal administration (Tudball 2016). Nevertheless, an indicator of patronage can still reflect an important part of clientelistic exchanges at this level of the public administration in the Brazilian context (Barone 2010; Santos 2013).

may react more strongly to information on corruption made public closer to the election (Brollo 2010; Costas-Pérez et al. 2012).

In addition to the key independent variables described above, the empirical analyses developed here take into account a number of other potential confounders. Municipal characteristics such as total population, percentage of urban population, literacy rate, municipal GDP, municipal area, and the year the municipality was founded are included, based on data from IBGE. Regional dummies are included in some models as well to account for remaining unobserved differences across regions. Also, characteristics of the incumbent candidates, such as gender and education level, are included, as well as their margin of victory from the previous election.

Other factors mentioned in the literature as affecting electoral accountability were considered. A large discussion in the literature on performance voting, more strongly in the case of economic voting, refers to the impact of institutional factors on how well voters can hold incumbent governments accountable at the ballot box. For the analysis conducted here, however, the confounding effects of political institutions can be, at least in part, eliminated, as factors related to formal institutions are not varying across observations. However, some variation may exist in the composition of local governments that affects voters' ability to allocate responsibility for outcomes to the right individuals—what is referred to as clarity of responsibility (Hobolt et al. 2013). In order to take this into account, two indicators are included as controls: the share of legislative seats held by the incumbent mayor's coalition, and the number of coalition parties (Xezonakis et al. 2016).

In the specific literature on voting behavior in Brazil, other political factors are mentioned as impacting the electoral performance of incumbent mayors. An association with politicians in power at other levels of government has been shown to impact electoral results in some occasions (Lavareda and Telles 2011; Oliveira and Fernandes 2013; Pereira and Melo 2015; Pereira et al. 2009). Therefore, we consider both whether the incumbent mayor was affiliated to the same party as the state governor at the time, and to the Worker's Party (PT), the one in power in the federal government<sup>30</sup>. Finally, the amount of funds used to finance the campaign appears as a relevant determinant of electoral performance in this context (Jucá et al. 2016).

## 4. Empirical results

The context of Brazilian municipalities offers an interesting setting for this study not only due to methodological advantages from a sub-national sample, but also due to a considerable degree of variation observed across the municipalities on the several indicators of interest for the empirical analysis conducted here. This is illustrated in Table 1, which displays the descriptive statistics for all the variables of interest.

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30 Even though the partisanship argument is not directly tested in the models presented here, this variable should contribute to eliminating part of the potential, although arguably limited bias that the absence of partisanship data could cause, as it should capture some effects of party identification with the PT, which is the strongest among the electorate (Kinzo 2005; Samuels 2006; Samuels and Zucco 2014).

Table 1. Summary statistics

	<i>N</i>	<i>Mean</i>	<i>Median</i>	<i>St. Deviation</i>	<i>Min</i>	<i>Max</i>
<i>Dependent variables</i>						
EA: incumbent mayor voted out	383	0.324	0	0.469	0	1
EA: change in vote share	383	-0.842	-0.4	14.742	-46.8	50.7
<i>Independent variables</i>						
Local radio presence	383	0.637	1	0.481	0	1
Local newspaper presence	383	0.379	0	0.486	0	1
IFDM change, 2005-2008	358	0.052	0.048	0.058	-0.134	0.25
IFDM change, 2007-2008	370	0.008	0.008	0.038	-0.126	0.135
Real % change in total intergovernmental transfers, 2004-2008	341	60.637	58.7	24.718	-18.5	198.4
Real % change in total intergovernmental transfers, 2007-2008	339	17.152	16.5	10.184	-44.8	73.5
Mean yearly % GDP per capita growth, 2005-2008	383	6.147	5.7	6.702	-16.7	55.5
% GDP per capita growth, 2008	383	6.740	5	14.180	-31.9	108.9
% of directly appointed municipal employees	383	10.284	8.1	7.650	0	44.9
Change in % of directly appointed municipal employees, 2004-2008	382	1.910	1.5	7.696	-33.4	31.2
Mean ENP	383	4.574	4.5	1.206	1.8	10.6
Number of corruption violations (unambiguous)	383	4.248	2	7.199	0	55
Number of corruption violations (all)	383	7.426	4	9.771	0	63
<i>Municipal characteristics</i>						
Municipal area (km <sup>2</sup> )	383	1754	434	6167	11	84213
Population, 2008	383	24667	11196	41234	1643	331412
% Urban population	383	57.7	58.4	22.8	4.2	100
Literacy rate	383	76.8	79.2	12.7	39.3	97.1
GDP per capita, 2008	383	11019	8554	8444	2117	63157
Year the municipality was founded	383	1963	1959	22	1909	1997
<i>Incumbent characteristics</i>						
Gender	383	1.084	1	0.277	1	2
Education level	383	6.527	7	1.690	2	8
Total campaign funds	383	128954	59787	305151	0	
Margin of victory, 2004	383	13.734	9.6	14.732	0	100
Affiliation to federal government party	383	0.094	0	0.292	0	1
Affiliation to state government party	383	0.211	0	0.409	0	1
Coalition's share of seats	383	0.487	0.44	0.176	0	1
Number of coalition parties	383	4.355	4	2.434	1	16

For the models estimated here, three different samples are considered: (a) the full sample of 383 observations, (b) a sample of 259 municipalities where at least one “unambiguous” corruption violation was found (“Corruption sample A”), and (c) an alternative sample with 315 municipalities where any corruption violation was found (“Corruption sample B”). The full sample is used for ba-

seline models of determinants of electoral accountability, without taking into account potential moderating effects of the main independent variables. Corruption Samples A and B, in turn, are used to estimate models that essentially compare only electorally successful versus unsuccessful corrupt mayors, allowing us to identify differential effects of the independent variables considered.

The first estimation strategy employed is a logistic regression with the binary indicator of electoral accountability as a dependent variable. Table 2 provides results for a selected set of models including local radio as an indicator of media presence, IFDM four-year change as an indicator of government performance, the share of directly appointed municipal employees in 2008, and the number of unambiguous corruption violations, with both unrestricted specifications including only the main independent variables and full specifications with basic municipal and incumbent characteristics. Regarding the impact of the number of corruption violations on electoral accountability, part of the models also test for an additional conditioning effect of the timing of release of the corruption information, operationalized simply by the lottery number associated with the audit conducted in each municipality. Models 1 to 4 consider the full sample of municipalities, and models 5 to 8 include only municipalities with at least one unambiguous corruption violation

**Table 2. Logistic regression results with unambiguous corruption violations**

	Full sample				Corruption Sample A			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local radio	0.120 (0.246)	0.160 (0.249)	0.0360 (0.276)	0.0735 (0.280)	0.305 (0.306)	0.363 (0.311)	0.157 (0.343)	0.204 (0.351)
IFDM change (2005-2008)	-2.012 (1.999)	-2.318 (2.014)	-1.929 (2.188)	-2.045 (2.210)	-2.493 (2.474)	-2.995 (2.506)	-2.653 (2.731)	-3.050 (2.786)
% directly appointed employees	0.00225 (0.0150)	0.000798 (0.0151)	0.00799 (0.0158)	0.00668 (0.0159)	0.00429 (0.0177)	0.00412 (0.0179)	0.0111 (0.0188)	0.0116 (0.0191)
Mean ENP	-0.0322 (0.0970)	-0.0437 (0.0973)	0.00178 (0.112)	-0.0143 (0.113)	0.0761 (0.115)	0.0472 (0.116)	0.132 (0.134)	0.0884 (0.137)
Corruption vio- lations (unambi- guous)	-0.0152 (0.0182)	0.130* (0.0743)	-0.0104 (0.0198)	0.180** (0.0770)	-0.0196 (0.0207)	0.167* (0.0910)	-0.0151 (0.0226)	0.240** (0.0941)
Lottery number		0.0180 (0.0408)		0.0121 (0.0424)		0.0557 (0.0603)		0.0414 (0.0627)
Corruption vio- lations*lottery number		-0.0155* (0.00814)		-0.0194** (0.00818)		-0.0198** (0.00991)		-0.0254** (0.00999)
Constant	-0.565 (0.498)	-0.681 (0.549)	-3.937** (1.988)	-4.559** (2.049)	-1.127* (0.598)	-1.527** (0.724)	-3.996 (2.507)	-5.109* (2.650)
Observations	358	358	358	358	240	240	240	240

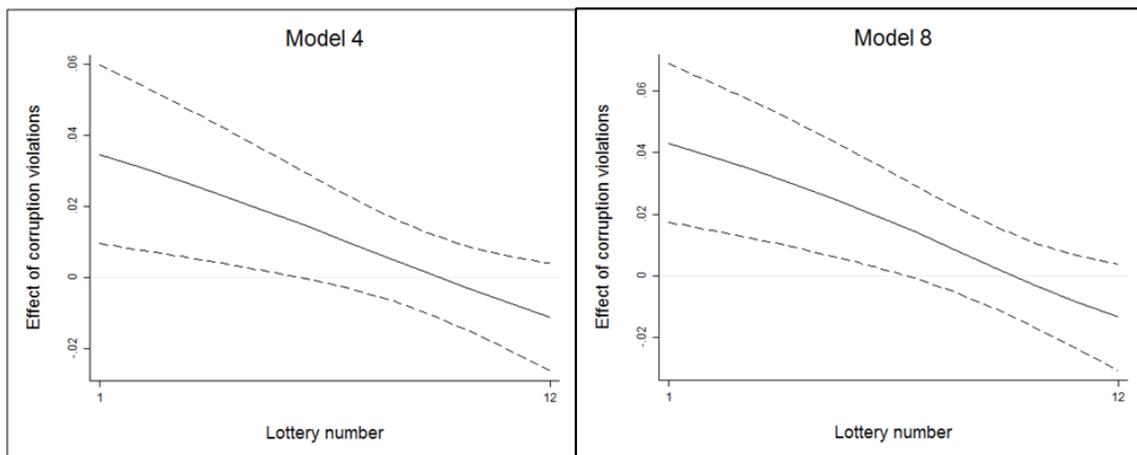
Municipal characteristics	x	x	x	x
Incumbent characteristics	x	x	x	x

*Note: Municipal controls include population, % of urban population, literacy rate and log municipal GDP. Mayor controls include gender, education level, margin of victory in previous election, affiliation to federal government party, affiliation to state government party and campaign revenue. Standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$*

A first comparative glance across these two groups reveals sizeable differences in the estimated coefficients for the different samples, with larger estimated effects for the corruption sub-sample. Although the effects are in most cases not statistically significant, these differences already offer some indication of differential effects of our independent variables once we restrict the sample only to municipalities with incumbents accused of corruption. This is in line with our expectation of moderating effects of these variables on corruption voting.

Nevertheless, these first models do not offer empirical support for hypotheses H1 to H4. The coefficients for radio presence, IFDM change and ENP mean have the expected sign, but are not statistically significant, and the coefficient for the percentage of directly appointed municipal employees, our indicator for patronage, is positive, contrary to the effect postulated in H3, and similarly not statistically significant. We find evidence only in favor of H5. In the models without the interaction term, the coefficients for the number of corruption violations are negative, contrary to our expectation, and not statistically significant. The coefficients estimated with the interaction, however, become positive and statistically significant at conventional levels for 3 of the 4 models presented; the coefficient for the interaction term is also statistically significant. Interestingly, these effects appear to become stronger and more significant once the two sets of control variables are introduced. In order to better grasp the nature of the total effect of corruption conditional on the varying lottery number, it is necessary to estimate the marginal effects, which are illustrated in Figure 1 for models 4 and 8<sup>31</sup>.

Figure 1. Conditional marginal effects of unambiguous corruption violations for full models



The graphs show that the number of corruption violations has an initially positive effect on the probability of incumbent mayors accused of corruption being voted out of office, but this

31 The marginal effects for Models 2 and 6, which do not include the controls, are shown in Figure 3 in the Appendix.

effect decreases for later audits. In the case of the full sample, the effect is statistically significant until the 5th audit, and for the corruption sub-sample it still holds for the 6th audit conducted in the period, which took place in early 2006. Surprisingly, this goes against the expectation that the effects becomes stronger the closer the audit is to the election. We also observe these positive effects of corruption on electoral accountability for models estimated with the alternative corruption indicator, including both ambiguous and unambiguous violations (see Table 6 in the Appendix), but the estimated marginal effect for both the full sample and the corruption sub-sample is considerably smaller and less significant, disappearing after the 4th audit round (see Figure 4 in the Appendix). The difference between the results based on the two different codings of the corruption violations suggests that the manner in which corruption accusations are communicated may also impact how voters perceive that information and incorporate it into their voting calculations.

The magnitude of the effects becomes clearer by looking at the difference in the predicted probability of electoral accountability for different levels of corruption, as illustrated in Table 3 below. For example, taking the first audit round as reference in the case of Corruption Sample A, the probability of being voted out of office for a typical corrupt mayor in a typical municipality increases from 0.48 to 0.81 as the number of unambiguous corruption violations found in the audit moves from the mean (4) to one standard deviation above that value (11). For the 5th audit round, this difference is much smaller, but still large, as the probability of electoral accountability for the same mayor increases 20 percentage points for the equivalent change in the number of corruption violations. In general, the significant positive effect of the number of corruption violations on electoral accountability, conditional on the timing of the audit, holds for all models tested, except for specifications including change in intergovernmental transfers as an indicator of government performance to test H2.

**Table 3. Predicted probabilities of electoral accountability for different levels of corruption**

	Full sample		Corruption Sample A	
<i>Number of unambiguous corruption violations</i>	Audit 1	Audit 5	Audit 1	Audit 5
1	0.36	0.36	0.33	0.34
4 (mean)	0.48	0.42	0.48	0.42
11 (1 sd above mean)	0.74	0.56	0.81	0.62
18 (2 sd above mean)	0.90	0.70	0.95	0.78
	Full sample		Corruption Sample B	
<i>Number of total corruption violations</i>	Audit 1	Audit 5	Audit 1	Audit 5
1	0.33	0.35	0.34	0.35
7 (mean)	0.46	0.41	0.48	0.42
17 (1 sd above mean)	0.68	0.52	0.72	0.55
27 (2 sd above mean)	0.84	0.62	0.88	0.67

*Note: Predicted probabilities calculated for model specifications including basic municipal and mayor controls, with IFDM change (2005-2008), % directly appointed employees, mean ENP, population, % of urban population, literacy rate, log municipal GDP, mayor education level, margin of victory in previous election and campaign revenue held at their means, and local radio presence, mayor gender, affiliation to federal government party and affiliation to state government party held at their median values.*

The results described above remain largely the same across other specifications tested with alternative indicators for the independent variables, as well as additional control variables such as municipal area, the municipality's year of foundation, regional dummies and also two indicators of clarity of responsibility. In the case of H1, for instance, we find no evidence for a moderating effect of local newspaper presence on corruption voting either. For this variable, we see a different pattern across the coefficients than the one observed with radio presence: the estimates for the full sample are larger than the ones for Corruption Samples A and B. Although the differences are not statistically significant, this might indicate that radio indeed played a more important role in the dissemination of the corruption information than printed media, as suggested by Ferraz and Finan (2008, 2011). For H3, using an indicator of change in the distribution of patronage during the incumbent's administration does not substantially alter the results and also falls short of confirming the hypothesis. However, one puzzling result is that this variable is found to be a statistically significant predictor of electoral accountability in some models with the full sample, but also with a positive effect on the dependent variable, contrary to the theoretical expectation that increased patronage should decrease the probability of mayors' being voted out of office.

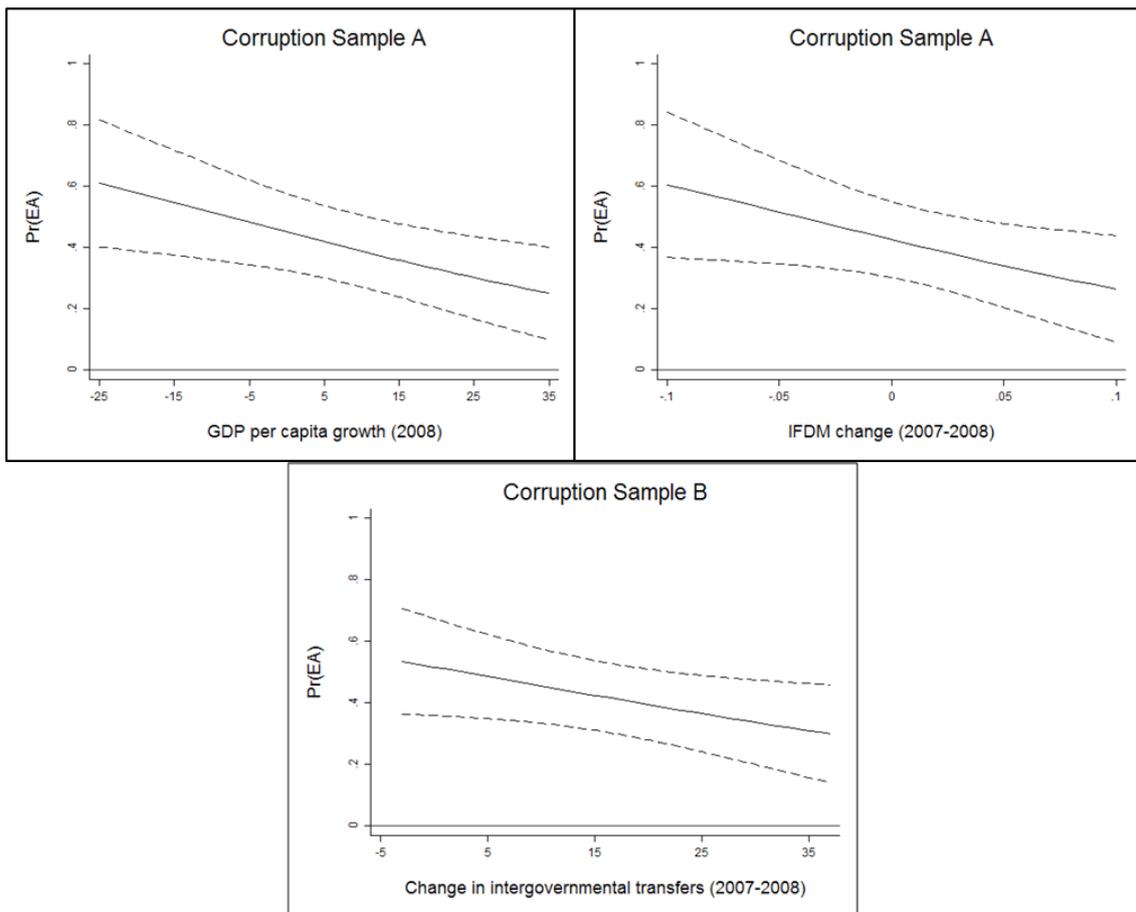
One interesting exception is found regarding the validity of H2, though. Table 4 illustrates models with equivalent specifications to the ones shown in Table 2, only varying the performance indicators used to test H2. All three indicators used to measure incumbent government performance for the whole administrative period of 2005–2008 show no statistically significant effect on the probability of electoral accountability. However, two alternative indicators capturing the change only for the last year of the incumbent mayor's term, namely the change in the IFDM between 2007 and 2008 and GDP per capita growth for 2008, are found to have significant negative effects, although only at the 90% confidence level for the former, in models with Corruption Sample A. Models with a one-year change in intergovernmental transfers were also estimated (not reported), and with this variable we see negative effects significant at the 90% level only for Corruption Sample B, and no significant effects for the other samples. These results also hold across specifications and with additional controls. This provides partial confirmation of H2, but conditional on the time frame for which government performance is measured.

These effects become clearer once we observe the change in the predicted probability of electoral accountability against corrupt incumbents for different values of the indicators of interest, illustrated in Figure 2. In the case of GDP per capita growth, a change from a value of one standard deviation below the mean to one standard deviation above it is associated with a decrease in the probability of the incumbent being voted out of office of about 15 percentage points, and in the case of IFDM change the same difference is associated with a decrease of 12 percentage points. For the one-year change in intergovernmental transfers, in the case of Corruption Sample B, the corresponding decrease in the probability of electoral accountability is of about 10 percentage points.

Table 4. Logistic regression results with unambiguous corruption violations and different indicators of government performance

	Full sample				Corruption Sample A					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Local radio	0.0735 (0.280)	0.130 (0.273)	0.174 (0.266)	0.173 (0.267)	0.0261 (0.290)	0.204 (0.351)	0.413 (0.348)	0.406 (0.333)	0.410 (0.340)	0.306 (0.369)
IFDM change (2005-2008)	-2.045 (2.210)					-3.050 (2.786)				
IFDM change (2007-2008)		-1.487 (3.164)					-7.233* (4.066)			
GDP per capita growth (yearly average, 2005-2008)			-0.00720 (0.0176)					-0.0131 (0.0239)		
GDP per capita growth (2008)				-0.00868 (0.00829)					-0.0257** (0.0116)	
Change in intergovernmental transfers (2004-2008)					-0.00713 (0.00529)					-0.0103 (0.00685)
% directly appointed employees	0.00668 (0.0159)	0.00496 (0.0159)	0.00882 (0.0152)	0.00767 (0.0151)	0.00136 (0.0163)	0.0116 (0.0191)	0.00944 (0.0188)	0.0162 (0.0181)	0.0145 (0.0181)	0.00924 (0.0191)
Mean ENP	-0.0143 (0.113)	-0.00344 (0.110)	-0.00805 (0.108)	-0.0180 (0.108)	-0.0323 (0.117)	0.0884 (0.137)	0.0843 (0.134)	0.0940 (0.129)	0.0774 (0.130)	0.0898 (0.137)
Corruption violations (unambiguous)	0.180** (0.0770)	0.157** (0.0737)	0.171** (0.0712)	0.170** (0.0714)	0.135 (0.102)	0.240** (0.0941)	0.210** (0.0908)	0.212** (0.0855)	0.212** (0.0868)	0.147 (0.127)
Lottery number	0.0121 (0.0424)	0.0171 (0.0407)	0.0184 (0.0400)	0.0166 (0.0401)	0.0440 (0.0443)	0.0414 (0.0627)	0.0461 (0.0604)	0.0461 (0.0582)	0.0404 (0.0585)	0.0637 (0.0665)
Corruption violations * lottery number	-0.0194** (0.00818)	-0.0161** (0.00749)	-0.0167** (0.00724)	-0.0167** (0.00725)	-0.0160 (0.0105)	-0.0254** (0.00999)	-0.0220** (0.00933)	-0.0213** (0.00871)	-0.0214** (0.00882)	-0.0189 (0.0130)
	(2.049)	(1.944)	(1.914)	(1.911)	(2.099)	(2.650)	(2.511)	(2.455)	(2.469)	(2.726)

Figure 2. Predicted probabilities for selected government performance measures



Two important things can be inferred from these results. First, we see that electoral accountability of corrupt incumbents can be significantly weakened when their administration is associated with positive outcomes in other dimensions that are important to voters, such as the economy and the provision of public services. Secondly, voters appear to behave myopically in this regard, and give more weight to their perception of the incumbent's performance in the recent past, which is in line with findings from the economic voting literature (Kayser 2014).

It is also worth noting the estimation results for some of the control variables considered in the models. The specifications including indicators of clarity of responsibility, discussed in the literature as potentially conditioning factors of corruption voting and performance voting more generally, do not provide any evidence of these factors as relevant predictors of electoral accountability in the context at hand. Also, the incumbent's affiliation to the state governor's party emerges as one of the stronger predictors of the dependent variable, with a considerable negative effect on the probability of electoral accountability. This is extremely robust across all specifications tested, and the effect appears to be even stronger for Corruption Sample A. This finding suggests that securing direct support from the state governor is a very effective strategy to improve a mayor's chances of reelection, in particular for those accused of corruption.

In order to verify the robustness of the results described above, two approaches were taken. First, the models were checked for the presence of influential observations that might be driving the results observed, through which one observation with abnormally high leverage was identified, but the results remain essentially the same once this observation is excluded from the samples. However, there were a number of other observations with relatively high leverage remaining. Therefore, a second approach was used to minimize the potential influence of observations with extreme values in the corruption variables. For this purpose, both variables—the one with

only unambiguous violations and the one with all corruption violations—were recoded, collapsing values above two standard deviations from the mean under the same maximum value of 18 violations for the unambiguous measure and 27 violations for the alternative measure. The recoded observations correspond roughly to the top 5% of the distribution in each case. The results with the recoded variables (see Table 7 in the Appendix) broadly confirm the initial estimates for the models with the unambiguous measure of corruption, and the marginal effects are also very similar in terms of statistical significance and magnitude, but the estimated marginal effects for the total number of corruption violations, in particular for Corruption Sample B, are no longer statistically significant for any audit round.

This suggests that the severity of the accusations against incumbent mayors seems to, under certain conditions, play a moderating role on how corruption information affects voters' decision to hold them accountable or not in the next election, thus still partly confirming H5. Again, the fact that the effect is significant for Corruption Sample A, but not for Corruption Sample B offers further corroboration that there seem to be cognitive barriers at play regarding how corruption information in different formats is received and interpreted by the public.

As to the estimated effects of the other independent variables, the results remain largely unchanged. The indicators used to verify H1, H3 and H4 are still without any statistically significant effects across all models, whereas the measures of IFDM change and GDP per capita growth for the election year maintain their statistical significance in models with Corruption Sample A, at the 95% and the 90% confidence levels, respectively, and change in governmental transfers is still significant at the 90% confidence level for Corruption Sample B. The magnitude of their effects is also very similar.

Another approach to test the robustness and sensitivity of the results was to estimate the same models through linear regression with an alternative indicator for the dependent variable, namely the change in vote share observed by the incumbents between the 2004 and 2008 elections. The results of this procedure differ from the previous models in a few respects. For the first models estimated, we used the original corruption variables, which yielded statistically significant estimates for the corruption variable and the interaction term with lottery number in 3 of the 4 models (see Table 8 in the Appendix). After calculating the corresponding marginal effect for corruption violations, we observe a statistically significant negative effect on the vote share difference for early audit rounds in the models using the number of unambiguous corruption violations (see Figure 5 in the Appendix). It is important to note that the expected sign for the effect is indeed the opposite than with the other dependent variable, as lower values in the vote share difference are indicative of stronger electoral accountability. Therefore, this finding is apparently in line with H5. This effect becomes less negative and turns positive for later audits, in a pattern similar to the one observed with the original models presented earlier. However, the estimated marginal effect is positive and statistically significant for the last audit rounds, which is a quite puzzling result. This part of the effect appears to be more robust across different specifications than the negative effect observed for the first audits. In models with Corruption Sample B, only the positive effect for later audits is statistically significant. Again, in models using change in intergovernmental transfers as an indicator of government performance, we no longer see statistically significant effects for the corruption variables.

Regarding the estimates for the other independent variables, these models also fail to provide any evidence in favor of H1, H3 and H4. For H2, we again see a statistically significant positive effect of GDP per capita growth, relative to the election year only, in models with Corruption Sample A, but the effect of the one-year IFDM change, which was significant at the 90% confidence level in the original models, is no longer significant here. A striking change with regards to the original models is that the third indicator for incumbent government performance tested, namely change in intergovernmental transfers, appears as one of the strongest predictors of

vote share difference, displaying statistically significant positive effects in all models tested. These effects are also of substantial magnitude: taking the estimate for the full samples, they represent an additional 7.5 percentage points in the vote share difference in the case of a municipality with an average value in this variable, and the effect is even a bit stronger for Corruption Sample A, with additional 9 percentage points in the mayor's vote share between elections. The one-year change in transfers is also statistically significant with positive effects in all models except those with Corruption Sample A.

For the linear models with the alternative dependent variable we also replicated the same specifications with the recoded corruption variables, in order to similarly reduce the potential influence of observations with extreme values in those variables. The results reveal that this was a justified concern: the estimated marginal effects for the corruption variables are no longer statistically significant at conventional levels for any of the different specifications tested, indicating that the previous results were likely driven by outliers. The results for the other independent variables remain largely unchanged.

Overall, the analysis of the data at hand reveals a mixed picture. Out of the five hypotheses considered, we find partial evidence in support only of the implicit exchange mechanism (H2) and of a moderating effect of the severity of corruption accusations (H5). In models where electoral accountability is associated with survival in office, the data suggest that voters take accusations of corruption into account in their voting calculus, punishing more strongly mayors charged with more corruption violations. However, this effect is observed only in the case of earlier audits. At the same time, voters' impetus to punish corrupt mayors appears to be weakened by recent improvement in economic conditions. When we consider electoral accountability as reflected in the vote share of incumbent mayors, on the other hand, we find no robust moderating effect of corruption violations, but we still find similar confirmatory evidence for the moderating role of the incumbent government's performance as measured by recent GDP per capita growth and real change in intergovernmental transfers. The implications of those findings for research in this field and for future lines of inquiry are discussed in the next section.

## 5. Discussion and conclusion

This study sought to empirically reexamine key hypotheses discussed in the literature on conditioning factors of electoral accountability against corrupt politicians, drawing on original corruption data from 383 Brazilian municipalities. In contrast to the fragmented way in which those hypotheses have been often assessed in previous studies, it aimed at constructing a more comprehensive model accounting for a wider range of competing explanations, and explicitly focused on analyzing their relatively explanatory power with regards to the electoral performance of a sub-sample of incumbents accused of corruption.

The empirical analysis presented above suggests that, indeed, not all of the explanations offered in the literature appear to account equally well for variation in the strength of electoral accountability of corrupt incumbents in different contexts. In the case of the information hypothesis, for instance, we did not find confirmation that stronger media presence contributes to increasing the likelihood that corrupt incumbents will be punished by voters. This result essentially contrasts with findings by Ferraz and Finan (2008), also in the context of Brazilian municipalities, that voters' reaction to corruption information in the 2004 election was stronger in municipalities with local radio presence.

One explanation for these conflicting findings could be related to the different time period analyzed here, and the possibility that the audit results received less local media attention in the 2005-2008 period than in the previous one. Indeed, already in 2005 the CGU changed the dis-

semination strategy regarding the audit results, publishing only the full audit reports online without making a summary of the findings available in the form of press releases (Avis et al. 2016). Therefore, as results became less accessible to the local media, it may be that it reached voters less and less throughout the years<sup>32</sup>.

It is also possible, for instance, that the audit results became particularly newsworthy in the first years after the program was introduced in 2003, due to the novelty of the lottery policy itself and the fact that more audits took place in 2003 and 2004, with seven audits being conducted in each year. Since 2005, however, the program was slowly scaled down, with five audit rounds in 2005, three in 2006 and 2007 and only two conducted in 2008. Moreover, the years of 2005 and 2006 saw the emergence of two major corruption scandals of national amplitude<sup>33</sup>, which may have dominated corruption reporting thereafter also at the local level. Even though one of those scandals involved corruption acts in municipalities as well, its national dimension, in particular the involvement of Members of Parliament, received more attention from national news agencies, which are a main source of news for local outlets.

The possibility that local corruption information derived from the audits became less salient in the period at hand, and therefore may have not reached the electorate or impacted voters' decision so strongly, is a plausible underlying mechanism that would be worth exploring, but the lack of reliable data on local media coverage of corruption prevents further investigation under the scope of this study. It is certainly an important limitation of the present analysis that the modeled relationship of local media presence as a potential moderator of corruption voting can only rely on an assumed connection between local media presence and increased dissemination of corruption information. The impossibility to more directly capture the variation in this connection is also a possible explanation why the lack of support for the information hypothesis similarly contradicts others studies highlighting a moderating effect of information on corruption voting through increased media coverage of corruption (Chang et al. 2010; Costas-Pérez et al. 2012; Klačnja et al. 2016). Additionally, one potential issue regarding the role of local media is the influence of political groups over certain media channels. These are nuances that could be explored in further studies examining the Brazilian case and could shed additional light about the validity of the information hypothesis for this context.

How do we reconcile this possible scenario with the finding of a significant positive effect of the number of corruption violations on electoral accountability of corrupt mayors? If voters are indeed knowingly punishing more those mayors facing graver, or at least more numerous accusations of corruption, this information must have reached them somehow. Indeed, the analysis described in the previous section shows that this may be the case, but only for earlier audits. Initially, this result appears to be counterintuitive, since the original suspicion was that voters would be more able to use corruption information that has reached them closer to the election. However, this finding would still be consistent with a process in which the audit findings beco-

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32 The study mentioned is actually co-authored by Ferraz and Finan and briefly mentions that, in a replication of their original analysis for the 2008 elections, they also failed to find an electoral disciplining effect through increased punishment of corrupt mayors by voters (Avis et al 2016).

33 In 2005, revelations emerged about a scheme allegedly put in place by the Federal Executive to "purchase" legislative support in Congress through monthly payouts, which became known as Mensalão. In the following year, the Sanguessugas ("Leeches") scandal came to light with the uncovering of a massive fraud scheme in the procurement of ambulances, which articulated corruption dealings both at the national and at the municipal level: first, budgetary amendments to finance new ambulances were proposed by representatives in Congress, then approval for the execution of the purchase would be secured through the involvement of officials in the Ministry of Health, and finally procurement procedures in numerous municipalities all over the country were manipulated to favor suppliers connected to the scheme.

me less salient over the years, in particular following the interruption in the dissemination of the press releases by the CGU, and therefore only findings from earlier audits would have been reported sufficiently to have informed voters' decision at a later point.

There are two other possible mechanisms, though, that could be behind the observed effect of the number of corruption violations on electoral accountability, conditional on the timing of the audit. Firstly, what we see as an effect of the corruption information disseminated only through earlier audits may be actually driven by the fact that those corruption violations are more likely to have led to criminal or civil charges against the respective mayors by the time the election took place. In this case, the effects estimated in the models here would at least in part be picking up the effect of an alternative source of information for voters, originating from a parallel mechanism of horizontal accountability (O'Donnell 1999), and this information would certainly have been reported closer to the election than the original audit reports. Since data about this is outside the scope of the present study, this possible mechanism cannot be directly examined here, but it alludes to an important limitation of the present analysis and of others studies on the electoral accountability of corrupt politicians, which fail to adequately assess and account for the effects of potential interactions between different accountability channels that may reinforce each other (Mainwaring and Welna 2003; Peruzzotti and Smulovitz 2006).

Another interpretation of the results presented here that we are unable to rule out, however, speaks to the findings by Brollo (2010), who argues that the apparent negative electoral effects of corruption information uncovered by the municipal audits in Brazil are in fact explained by reduced intergovernmental transfers faced by municipalities where irregularities are found, as a sort of administrative sanction imposed by the federal government as a consequence of the audits. The results from the models estimated with the change in intergovernmental transfers appear to be consistent with this claim. Indeed, the conditional effects of corruption violations are not observed in any of the models including this variable as an indicator for testing the implicit exchange hypothesis. And even though the change in intergovernmental transfers is not found to be a strong predictor of electoral accountability in the logistic regression models, it becomes a very strong explanatory variable for vote share difference in the linear regression models.

Brollo's (2010) discussion of the mechanism behind this argument essentially reflects the implicit exchange hypothesis: the reduction in intergovernmental changes received by the municipalities would negatively affect the provision of public infrastructure, which would directly be felt by the population as a decrease in welfare and in the provision of public goods by the local administration. However, this would take some time to materialize, which again could be an explanation why the apparent effects of corruption violations are found only for earlier audits. This explanation seems consistent with part of the results presented here, but it essentially implies that the effect attributed to corruption violations is in fact coming from an underlying implicit exchange mechanism. However, in alternative model specifications tested with the IFDM change and GDP per capita growth as indicators for this same mechanism, we still find evidence of that the number of corruption violations has an independent conditional effect on the electoral performance of corrupt incumbents. Therefore, there might be other micro-mechanisms connecting changes in intergovernmental transfers to voters' reactions that have not been captured here and remain unclear, and which could thus be further examined in future research, at least applicable to the Brazilian context.

The possibility that the conditional effects of the number of corruption violations might be associated with an underlying implicit exchange mechanism only reinforces the other findings presented earlier regarding the partial validity of this hypothesis for the context at hand. Indeed, the empirical analysis conducted here finds evidence that a positive performance by the incumbent government in terms of economic conditions, measured as GDP per capita growth, and less strongly in the case of a broader measure encompassing also an improvement in basic public

services, contributes to weaken electoral accountability of corrupt incumbents. However, voters appear to be myopic when using this assessment in their voting decision, given that the identified effect was only seen for performance measures referring to the immediate past before the election. In any case, the findings from the present study suggest without a doubt that, among the five main hypotheses tested, this appears to be the strongest conditioning factor affecting corruption voting for the context at hand.

Regarding the two remaining hypotheses, namely the role of patronage and of the range of alternatives available to voters, we fail to find corroborating evidence for either. It is important to acknowledge that the empirical strategy employed in this study faces considerable challenges regarding measurement of these variables, which are inherently difficult to capture empirically. The proxy indicator for patronage explored in this analysis, for instance, yielded estimates that, although not statistically significant, pointed consistently in the opposite direction than the original expectation. This may be an indication that the measure chosen is not capturing the phenomenon of interest adequately, i.e. it may be either capturing other information unaccounted for in the model, or it may be missing an important part of the picture of how the clientelism mechanism works in reality. As mentioned before, the distribution of patronage is certainly only one resource that mayors have at their disposal as a way of distributing material advantages to supporters. Therefore, future attempts to examine this hypothesis further would likely require improvements in how to more adequately capture this mechanism empirically. A similar problem possibly occurs with the attempt to measure the political alternatives available to voters: the effective number of parties may only poorly reflect how voters perceive the range of options that the political market of candidates offers them in terms of corrupt and non-corrupt types. Again, measurement advances with especially ingenious strategies may be required in future research to better grasp the mechanism of interest here.

Another interesting insight from the findings presented here refers to the different results observed for Corruption Samples A and B. The distinction between the two samples lies essentially in the fact that, for the former, only corruption information presented in more accessible language was considered. Based on this coding decision, our expectation would be that the observed effects related to corruption voting are more pronounced for Corruption Sample A, which is largely corroborated by the fact that the effects in support of the implicit exchange mechanism and of the number of corruption violations are either stronger, or observed exclusively in the case of samples defined according to the “unambiguous” corruption information. This suggests that there are also relevant cognitive barriers related to voters’ ability to use corruption information as a useful criterion.

This issue becomes particularly important when we consider its implications for the discussion about the credibility of information sources. Several authors have highlighted that official sources, such as enforcement bodies, tend to be a more credible source of information on corruption (Fernández-Vázquez et al. 2013; Klačnja 2011; Weitz-Shapiro and Winters 2014). At the same time, however, information from such agencies are likely to be made available in more technical language that is also less accessible to the public, in particular to less sophisticated voters. Therefore, their effective impact on voting behavior could depend on the existence of other channels that manage to “translate” corruption accusations from such sources adequately, so that they become more informative to the electorate and have the desired effect of contributing to electoral accountability against politicians accused of malfeasance.

Regarding the methodological approach taken in this study, the results shown here strengthen the case for empirical analyses looking specifically at sub-samples of incumbents accused of corruption. Even though the estimated effects for the main independent variables were not all found to be statistically significant, we see in almost all cases sizeable differences in the coefficients between the full and the “corruption” samples that indicate potential differential effects

beyond those that the analysis was able to identify more clearly, such as in the case of performance indicators. Indeed, this illustrates the level of complexity to adequately modeling the phenomenon of interest, and on top of this the analysis developed here provides evidence of yet another intervening variable, namely the timing of release of the corruption information. Other studies should consider this perspective to try to better capture these multiple conditional effects more accurately than past studies have done.

As to the chosen level of analysis, it is important to acknowledge that this study highlights the added value of using a concrete aggregate measure of corruption, but there certainly are costs to measuring other determinants of voting behavior at the aggregate level, in particular those that are meant to capture variation in individual voter characteristics. The study of electoral accountability inherently involves the interaction between contextual and individual-level variables, and it is challenging to model this adequately mainly due to data constraints. Although new approaches have been tried to overcome this difficulty in studies looking at performance voting more broadly (e.g. Hellwig 2011), analyses of corruption voting face an additional challenge related to the difficulty of empirically capturing aggregate corruption levels. For most studies in this area, still the mainstream approach is the use of perception-based indicators, which may themselves involve considerable biases. Therefore, an important agenda for future research on determinants of electoral accountability against corrupt politicians is without a doubt the development of new indicators that allow for cross-level analyses that better reflect the underlying mechanisms associated with voters' reaction to corruption.

Overall, the findings presented in this study contribute to reinforce the general picture that already comes across from the literature on the electoral accountability of corrupt politicians as an inherently complex phenomenon, likely contingent on a broad spectrum of contextual and attitudinal factors (Jiménez and Caínzos 2004; de Sousa and Moriconi 2013). Fully understanding its determinants may be an unattainable goal for empirical research in this field, but the current state of knowledge offers a great deal of fertile ground for exploring this issue further, with a number of empirical and methodological challenges yet to be overcome. In particular, innovative approaches regarding improvements in the measurement of corruption itself and of other indicators for addressing the hypotheses analyzed here are much needed for advancing our comprehension of the workings of electoral accountability as an essential feature of democratic regimes.

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## Appendix

Table 5. Comparison between sample and universe of cases

	<i>Sample</i>	<i>Brazil</i>
<i>Regional distribution</i>		
North	10%	8%
Northeast	36%	32%
Southeast	27%	30%
South	18%	21%
Center-West	9%	8%
<i>Distribution by population bracket</i>		
Up to 5,000	22%	23%
5,001-10,000	23%	22%
10,001-20,000	23%	25%
20,001-50,000	21%	19%
50,001-100,000	6%	6%
100,001-500,000	4%	5%
More than 500,000	0%	1%
<i>Mean development indicators</i>		
HDI (2010)	0,652	0,659
HDI education (2010)	0,550	0,559
% Literacy (2010)	82,9	83,8
% Urban population (2010)	63,3	63,8
GDP per capita in 2008 (2012 constant BRL)	11019	12754

Table 6. Logistic regression results with all corruption violations

	Full sample			Corruption Sample B				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local radio	0.128 (0.246)	0.145 (0.248)	0.0430 (0.277)	0.0542 (0.280)	0.223 (0.275)	0.252 (0.277)	0.137 (0.310)	0.162 (0.314)
IFDM change (2005-2008)	-2.054 (1.997)	-2.117 (2.005)	-1.936 (2.188)	-1.886 (2.207)	-2.664 (2.227)	-2.737 (2.237)	-1.966 (2.479)	-1.882 (2.507)
% directly appointed employees	0.00197 (0.0151)	0.000215 (0.0152)	0.00777 (0.0158)	0.00556 (0.0160)	0.00112 (0.0166)	0.000346 (0.0166)	0.00738 (0.0173)	0.00630 (0.0175)
Mean ENP	-0.0358 (0.0970)	-0.0384 (0.0969)	-0.00157 (0.112)	-0.000542 (0.112)	-0.0128 (0.105)	-0.0201 (0.106)	0.0364 (0.121)	0.0348 (0.122)
Corruption violations (all)	-0.00983 (0.0129)	0.0703 (0.0451)	-0.00835 (0.0144)	0.103** (0.0475)	-0.0136 (0.0142)	0.0674 (0.0503)	-0.00793 (0.0159)	0.115** (0.0535)
Lottery number		0.0263 (0.0448)		0.0292 (0.0472)		0.0244 (0.0552)		0.0272 (0.0573)
Corruption violations*lottery number		-0.00899* (0.00512)		-0.0120** (0.00532)		-0.00881 (0.00567)		-0.0127** (0.00591)
Constant	-0.539 (0.502)	-0.728 (0.558)	-3.919** (1.989)	-4.667** (2.058)	-0.597 (0.547)	-0.810 (0.645)	-4.228* (2.210)	-5.092** (2.299)
Observations	358	358	358	358	294	294	294	294
Municipal characteristics			x	x			x	x
Incumbent characteristics			x	x			x	x

Note: Municipal controls include population, % of urban population, literacy rate and log municipal GDP. Mayor controls include gender, education level, margin of victory in previous election, affiliation to federal government party, affiliation to state government party and campaign revenue. Standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 7. Logistic regression results using recoded corruption variables

	Full sample		Corruption sample A	Corruption sample B
	(1)	(2)	(3)	(4)
Local radio	0.0788 (0.280)	0.0643 (0.279)	0.217 (0.350)	0.174 (0.312)
IFDM change (2005-2008)	-2.022 (2.209)	-1.864 (2.202)	-3.011 (2.782)	-1.834 (2.502)
% directly appointed employees	0.00658 (0.0159)	0.00523 (0.0160)	0.0108 (0.0190)	0.00565 (0.0174)
Mean ENP	-0.0176 (0.113)	-0.00483 (0.112)	0.0808 (0.137)	0.0306 (0.122)
Lottery number	0.00837 (0.0446)	0.0250 (0.0501)	0.0400 (0.0674)	0.0181 (0.0622)
Corruption violations (unambiguous)	0.182** (0.0860)		0.245** (0.109)	
Corruption violations (unambiguous) *lottery number	-0.0194** (0.00942)		-0.0262** (0.0117)	
Corruption violations (all)		0.0915 (0.0557)		0.101 (0.0652)
Corruption violations (all) *lottery number		-0.0111* (0.00620)		-0.0114 (0.00711)
Constant	-4.434** (2.040)	-4.447** (2.047)	-4.847* (2.627)	-4.807** (2.286)
Observations	358	358	240	294

Note: All models include municipal (population, % of urban population, literacy rate and log municipal GDP) and mayor controls (gender, education level, margin of victory in previous election, affiliation to federal government party, affiliation to state government party and campaign revenue). Standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table 8. Linear regression models with vote share difference as dependent variable

	Full sample		Corruption sample A	Corruption sample B
	(1)	(2)	(3)	(4)
Local radio	-0.601 (1.571)	-0.512 (1.550)	-0.672 (1.914)	-0.475 (1.739)
IFDM change (2005-2008)	20.12 (12.53)	19.07 (12.47)	19.35 (15.13)	19.41 (14.43)
% directly appointed employees	-0.133 (0.102)	-0.117 (0.101)	-0.160 (0.130)	-0.135 (0.112)
Mean ENP	-0.804 (0.675)	-0.824 (0.672)	-0.514 (0.772)	-0.213 (0.729)
Lottery number	0.316 (0.252)	0.111 (0.278)	0.188 (0.342)	0.232 (0.329)
Corruption violations (unambiguous)	-0.999** (0.478)		-1.140** (0.547)	
Corruption violations (unambiguous) *lottery number	0.114** (0.0452)		0.129** (0.0511)	
Corruption violations (all)		-0.580* (0.330)		-0.581 (0.370)
Corruption violations (all) *lottery number		0.0793** (0.0311)		0.0758** (0.0350)
Constant	-2.948 (12.26)	-2.600 (12.35)	14.34 (14.73)	6.170 (13.68)
Observations	358	358	240	294
R-squared	0.232	0.236	0.257	0.239

Note: All models include municipal (population, % of urban population, literacy rate and log municipal GDP) and mayor controls (gender, education level, margin of victory in previous election, affiliation to federal government party, affiliation to state government party and campaign revenue). Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Figure 3. Conditional marginal effects of unambiguous corruption violations for unrestricted models

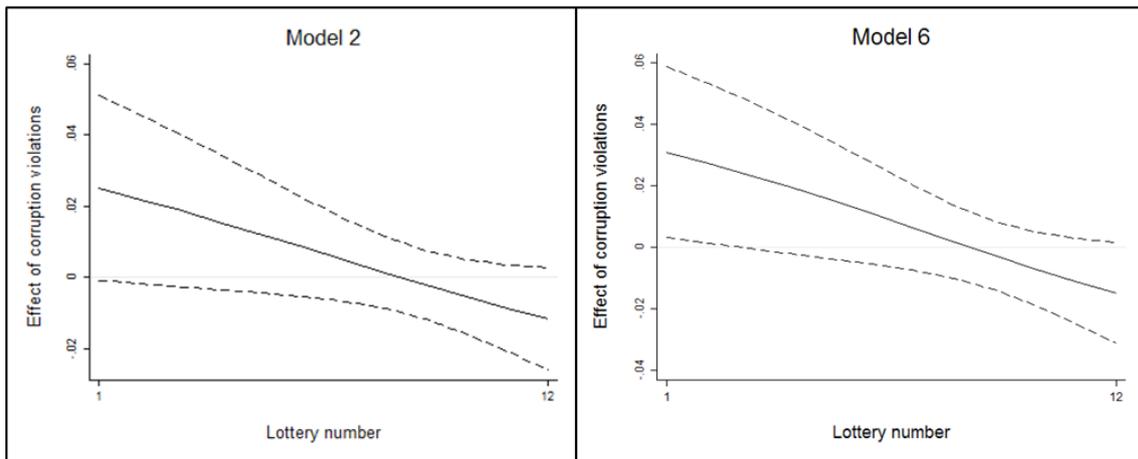


Figure 4. Conditional marginal effects of all corruption violations for full models

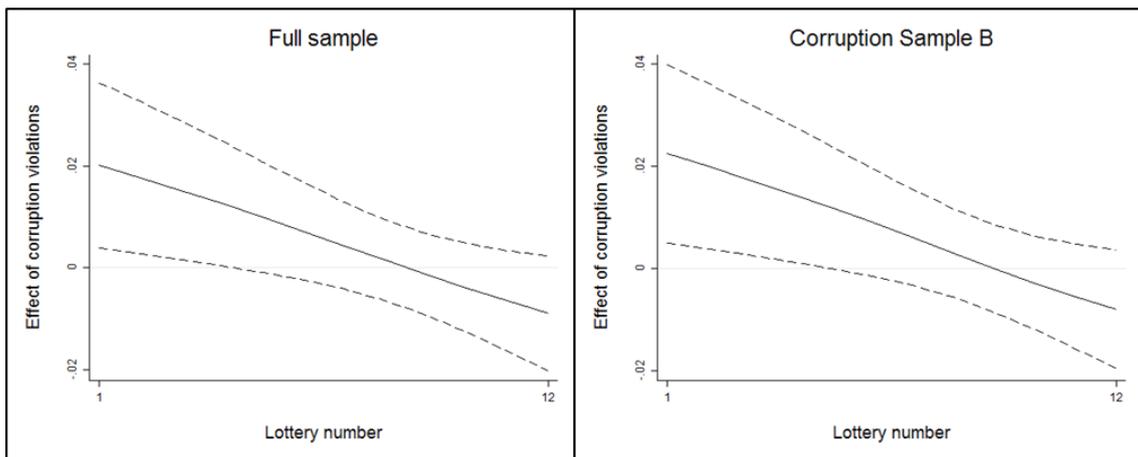


Figure 5. Conditional marginal effects of unambiguous corruption violations on vote share difference

