

The Role of Accountability in the Relationship between Democracy and Corruption

Bianca Vaz Mondo

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First supervisor

Alina Mungiu-Pippidi, PhD
Hertie School of Governance

Second supervisor

Carlos Pereira, PhD
Fundação Getúlio Vargas (FGV)

Third supervisor

Mark Andreas Kayser, PhD
Hertie School of Governance

Summary

Democracy is discussed in the comparative empirical literature as an important determinant of corruption. Several studies show that not only are democracies less corrupt than authoritarian regimes, but also that there is great variation in corruption levels across democratic regimes. This dissertation is concerned with examining the reasons behind this pattern. It begins by arguing that the study of democracy's impact on corruption requires a stronger focus on the causal mechanisms linking these two phenomena. Based on a review of causal arguments raised in the literature, a discussion of different dimensions of accountability—electoral, social and horizontal—is introduced as a useful framework for this purpose.

This sets the ground for the two main empirical contributions of the dissertation. In both cases, the study relies on original corruption data collected from over 600 audit reports on municipalities randomly selected through a federal audit program implemented in Brazil. First, a panel data set with 130 municipalities that have faced multiple audits between 2003 and 2013 is used to examine whether those three dimensions of accountability exert a deterrent effect on corruption over time. Second, a cross-section of 383 municipalities is the basis for analyzing the factors explaining when accountability effectively occurs, focusing on conditioning factors of electoral accountability against corrupt politicians.

In line with previous studies, the findings are not very optimistic. In the first case, no robust evidence is found for a deterrent effect of accountability on corruption over time, either through elections, increased civil society and media presence, or formal institutional sanctions. From the second analysis, we conclude that conditions for successful electoral accountability of corrupt incumbents may be more restrictive than usually expected. The study corroborates two hypotheses discussed in the literature: (a) electoral accountability is weakened by recent positive assessments of incumbents' performance in office, and (b) voters punish more strongly politicians facing more corruption accusations, but this may be conditional on the salience of corruption before elections. These results reinforce the existing literature suggesting that neither the expected corruption-reducing effects of accountability nor the effective occurrence of electoral accountability can be taken for granted in democratic regimes.

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Table of Contents

Summary	5
Acknowledgments	7
Table of Contents	9
List of Tables and Figures	11
List of Abbreviations.....	13
Introduction	15
Contextualizing democracy and accountability in Brazil.....	29
Electoral accountability.....	30
Social accountability.....	34
Horizontal accountability	37
PART I	47
Democracy and corruption: exploring the causal mechanisms	49
Abstract	49
Theory and empirical findings: a review of the literature	51
The linear effect hypotheses.....	51
The non-linear effect hypothesis	55
The conditional effect hypotheses.....	59
A note on mutual causality	64
Limitations of existing research and some conceptual issues	66
An alternative framework: the dimensions of accountability	69
Conclusion	74
PART II	77
Measuring political corruption from audit results: a new panel of Brazilian municipalities.....	79
Abstract	79
Data collection and description	82
Data description	89
Electoral accountability as a deterrent to corruption: an empirical application	92
Conclusion	97

Accountability as a deterrent to corruption: new data from Brazilian municipalities	99
Abstract	99
Theory and previous empirical evidence	101
Empirical strategy and data collection	107
Sample selection and coding procedure for the corruption data	108
Accountability measures and data sources.....	112
Municipal and mayor-level covariates	116
Estimation procedures and results	118
Data description	118
Difference-in-differences models.....	120
Panel regression models	131
Discussion and conclusion.....	138
PART III	143
Electoral accountability of corrupt politicians: conditioning factors revisited	145
Abstract	145
Theoretical background and previous findings	147
Methodological approach and data sources.....	160
Sample selection and coding procedure for the corruption data	164
Other variables and data sources	166
Empirical results	170
Discussion and conclusion.....	181
Conclusion	189
Bibliography	197
Appendix A	217
Appendix B	222
Appendix C	229
Annex: List of pre-publications.....	237

List of Tables and Figures

Table 1. Number of audits by year	83
Table 2. Distribution of observations with audit coverage across time periods	88
Table 3. Corruption violations by region.....	91
Table 4. Regression models with log number of corruption violations as dependent variable	96
Table 5. Difference-in-differences models with log number of corruption violations as dependent variable (narrow sample).....	121
Table 6. Difference-in-differences models with log number of corruption violations as dependent variable (broad sample).....	122
Table 7. Difference-in-differences models with log number of corruption violations as dependent variable and audit information release as electoral accountability treatment (narrow sample).....	126
Table 8. Difference-in-differences models with log number of corruption violations as dependent variable and audit information release as electoral accountability treatment (broad sample).....	127
Table 9. Estimated average treatment effects.....	131
Table 10. Fixed-effects models with log number of corruption violations as dependent variable (narrow sample).....	133
Table 11. Fixed-effects models with log number of corruption violations as dependent variable (broad sample).....	134
Table 12. Random-effects models with log number of corruption violations as dependent variable (narrow sample)	136
Table 13. Random-effects models with log number of corruption violations as dependent variable (broad sample)	137
Table 14. Logistic regression results with unambiguous corruption violations.....	171
Table 15. Predicted probabilities of electoral accountability for different levels of corruption	174
Table 16. Logistic regression results with unambiguous corruption violations and different indicators of government performance.....	176
Table 17. Comparison between sample and universe of cases	217
Table 18. Regional comparison of development indicators.....	218
Table 19. Pairwise correlations between corruption measure and development indicators	218
Table 20. Summary statistics.....	219

Table 21. Regression models with number of corruption violations as dependent variable	220
Table 22. Regression models with log number of corruption violations as dependent variable without selected influential observations.....	220
Table 23. Regression models with log number of corruption violations as dependent variable and change in vote share as indicator for electoral accountability	221
Table 24. Summary statistics.....	222
Table 25. Difference-in-differences models with log number of corruption violations as dependent variable without selected influential observations	223
Table 26. Difference-in-differences models with log number of corruption violations as dependent variable and audit information release as indicator for the electoral accountability treatment, without selected influential observations	224
Table 27. Fixed-effects models with log number of corruption violations as dependent variable and change in vote share as indicator for electoral accountability	225
Table 28. Random-effects models with log number of corruption violations as dependent variable and change in vote share as indicator for electoral accountability	226
Table 29. Description of variables	227
Table 30. Comparison between sample and universe of cases	229
Table 31. Summary statistics.....	230
Table 32. Logistic regression results with all corruption violations	231
Table 33. Logistic regression results using recoded corruption variables	232
Table 34. Linear regression models with vote share difference as dependent variable	233
Figure 1. WGI Control of Corruption and Freedom House scores, 2015	17
Figure 2. Distribution of the number of corruption violations per period.....	90
Figure 3. Timing of audits against the data set period structure	119
Figure 4. Treatment and control situations	125
Figure 5. Conditional marginal effects of unambiguous corruption violations for full models	172
Figure 6. Predicted probabilities for selected government performance measures.....	177
Figure 7. Conditional marginal effects of unambiguous corruption violations for unrestricted models.....	234
Figure 8. Conditional marginal effects of all corruption violations for full models	234
Figure 9. Conditional marginal effects of unambiguous corruption violations on vote share difference	235

List of Abbreviations

<i>Abbreviation</i>	<i>Name</i>
CGU	Federal Comptroller's Office (<i>Controladoria Geral da União</i>)
CNBB	National Conference of Brazilian Bishops (<i>Conferência Nacional dos Bispos do Brasil</i>)
CNJ	National Justice Council (<i>Conselho Nacional de Justiça</i>)
CPI	Parliamentary Committee of Inquiry (<i>Comissão Parlamentar de Inquérito</i>)
CPI	Corruption Perceptions Index
ENP	Effective number of parties
Firjan	Industry Federation of Rio de Janeiro (<i>Federação das Indústrias do Estado do Rio de Janeiro</i>)
GDP	Gross Domestic Product
HDI	Human Development Index
IBGE	Brazilian Institute of Geography and Statistics (<i>Instituto Brasileiro de Geografia e Estatística</i>)
ICRG	International Country Risk Guide
IFDM	Firjan Index of Municipal Development (<i>Índice Firjan de Desenvolvimento Municipal</i>)
MP	Public Prosecution Service (<i>Ministério Público</i>)
OLS	Ordinary Least Squares
PF	Federal Police (<i>Polícia Federal</i>)
PR	Proportional representation
PRI	Partido Revolucionario Institucional
PT	Workers' Party (<i>Partido dos Trabalhadores</i>)
STF	Supreme Federal Court (<i>Supremo Tribunal Federal</i>)
STN	National Treasury Department (<i>Secretaria do Tesouro Nacional</i>)
TCU	Federal Court of Accounts (<i>Tribunal de Contas da União</i>)
TSE	Superior Electoral Court (<i>Tribunal Superior Eleitoral</i>)
UNDP	United Nations Development Program
WGI	World Governance Indicators

Introduction

This dissertation is motivated by a daunting policy problem: how to fight systemic corruption, in particular when prevalent at high levels of government. In this case, corruption, also characterized as political corruption, may be understood as “[...] the behavior of public decision-makers where preferential treatment is provided to individuals and where narrow interests are advanced at the expense of the interests of broader segments of society” (Lambsdorff 2007, 82). Corruption in general has been shown to have significant detrimental effects to society by hurting economic growth, social well-being and regime legitimacy, among other things (Dahlström and Lapuente 2011; Holmberg *et al.* 2009; Mauro 1995; Seligson 2002), and political corruption is especially distortive with regards to policy-making and the allocation of public resources.

About two decades ago, when a number of studies were published pointing out in particular corruption’s negative consequences for economic activity (e.g. Mauro 1995), the previously held functionalist view of corruption as economically beneficial in some cases (Huntington 2002; Nye 1967) was strongly challenged. Corruption became a key issue in the international good governance agenda that was shaping, and at the same time increasingly attracted the interest of academics from different fields, who have continuously engaged in understanding the causes of corruption and the factors that contribute to reducing it in a society. In search for answers, an ever growing body of literature has analyzed the so-called determinants of corruption (see Andvig *et al.* 2001; Lambsdorff 2007; Seldadyo and de Haan 2006 for a review).

This line of comparative research was able to identify a number of relatively robust empirical regularities: low-corruption countries tend to be economically developed, Protestant, consolidated democracies that share common-law based legal systems; among former colonies, British colonial heritage emerges as a strong predictor for lower levels of corruption (Mungiu-Pippidi 2015; Serra 2004). As it turns out, most of those findings can hardly be translated into helpful policy advice for countries, especially at lower levels of development, currently struggling with corruption (Mungiu-Pippidi 2015). Moreover, the actual causal chains that might explain the connection between those, to a large extent, historically determined characteristics and corruption outcomes in contemporary political systems were not fully clear.

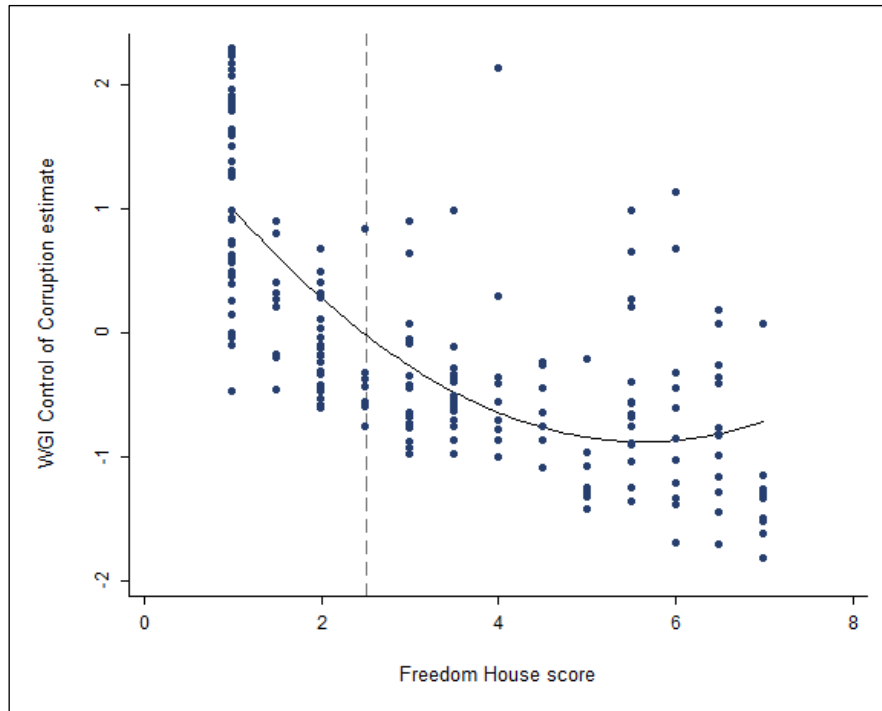
However, two factors do stand out as potential channels for broader anti-corruption efforts: economic development and democratic consolidation. It is true that even these avenues for tackling the corruption problem would require long-term efforts that are likely to pay off only after many years, if not decades. Nevertheless, if these factors can in fact help curb corruption in a society, they are at least feasible outcomes that are worth pursuing also for other benefits to society that they entail. This dissertation is concerned with the more political of those potential determinants of corruption: the impact of democracy.

What exactly do we know about the relationship between democracy and corruption? Several recent studies have corroborated earlier findings of an association between stronger democracy and lower levels of corruption, but the development of more elaborate hypotheses and the use of more sophisticated methods to test them have yielded additional insights, in particular about the non-linearity of this association (Kolstad and Wiig 2016; Montinola and Jackman 2002; Pellegata 2012; Saha *et al.* 2014; Treisman 2007). A simple scatter plot for a cross-section of countries, using the Freedom score by Freedom House as an indicator of democracy and the World Governance Indicators' (WGI) Control of Corruption estimate published by the World Bank, broadly illustrates this pattern, as shown in Figure 1. As indicated by the black line representing the quadratic fit of the data in the graph, a negative association between stronger democratic institutions is not observed throughout the whole democracy scale, but rather after a certain level, and it is actually most pronounced among countries that are already considered as democratic based on this scale, i.e. those which are to the left of the gray dashed line.

If, on the one hand, recent scholarly work seems to show little controversy about the empirical pattern observed between democracy and corruption, there is much less consensus about the causal mechanisms behind it. What is it about democracies that explains not only how more democratic polities manage to become less corrupt than their undemocratic counterparts, but also how the strengthening of democratic institutions even after a country has established itself as a democratic regime can contribute to curbing corruption even further? This is the broader research puzzle that this dissertation departs from. In addressing this puzzle, it argues that one of the reasons why this still remains largely unclear in the literature is that common approaches to examining the relationship between democracy and corruption empirically have considered the former as an aggregate phenomenon, without taking into account how distinct

features of democracy may impact this relationship. Therefore, existing research has been unable to disentangle the effects of different possible mechanisms.

Figure 1. WGI Control of Corruption and Freedom House scores, 2015



Source: Own elaboration. WGI Control of Corruption ranges from -2.5 to 2.5 (lowest corruption). Freedom House assesses democracy with scores from 1 to 7 (least democratic), and considers countries with a score of 2.5 or lower to be democracies (placed left of the gray dashed line).

With this perspective in mind, the first part of the dissertation seeks to make a contribution to this debate by thoroughly reviewing the multiple theoretical arguments proposed in the relevant literature on the link between democracy and corruption, the different hypotheses on the nature of this link and the associated empirical evidence accumulated in the last two decades of research in this field. This is done with a focus on the causal mechanisms described and how they are connected to different components of democracy.

From this review, it is possible to highlight four main features of democracy as central elements to the most recurring explanations for an expected negative effect of democratic institutions on corruption levels in a political system. The most prominent argument emphasizes the role of elections in enabling voters to exercise some form of control over their

representatives by threatening to vote them out of office when they “misbehave”. Under the assumption that politicians derive utility from staying in office and thus have an interest in maximizing their probability of being reelected, elections are expected to help deter self-interested behavior, of which corruption is an example, and to generate incentives for greater responsiveness on the part of elected officials (Adsera *et al.* 2003; Andvig *et al.* 2001; Montinola and Jackman 2002; Pellegata 2012; Przeworski *et al.* 1999). A second important explanation refers to the impact of democratic freedoms of information, expression and association which, by fostering the development of a free press and a vibrant civil society, create the conditions for active monitoring of governments by society. This should in turn contribute both to reducing incentives for corrupt behavior and to helping uncover malfeasance by public officials through watchdog activities. Another argument refers to the existence of institutional mechanisms of control, such as checks and balances and oversight agencies, which serve to ultimately put a check on government and restrict its ability to engage in corruption. Finally, the long-term consolidation of democratic norms is a fourth element that should contribute to strengthen anti-corruption sentiments in the population.

Drawing on the works of O’Donnell (1994, 1999, 2003) and Peruzzotti and Smulovitz (2006a), a conceptual framework is proposed where those democratic features are directly or indirectly linked to one common denominator, namely accountability, through its sub-dimensions. Accountability here is broadly understood in connection with “[...] subjecting power to the threat of sanctions; obliging it to be exercised in transparent ways; and forcing it to justify its acts” (Schedler 1999, 14). In particular in the case of the three first democratic features listed above, they can be directly associated with electoral and social accountability, as sub-dimensions of vertical accountability, and with horizontal accountability, respectively, while democratic norms are seen as feeding into these three channels of accountability and indirectly fostering their consolidation.

This perspective of the dimensions of accountability as representing the main causal mechanisms that might account for the association between stronger democracy and lower corruption is embedded in a theoretical framework of political corruption in democratic regimes as a principal-agent problem. According to this approach, the principal delegates functions to the agent, which are to be performed according to certain rules and taking into account the principal’s interests. In exercising the delegated functions, the agent may have the opportunity to follow his personal interests instead and extract rents from his entrusted

power, deviating from the rules imposed by the principle. This is possible due to the information asymmetry between the two, that is, the principal is not fully informed about the actions of the agent and cannot fully control them (Lambsdorff 2007). What determines the agent's decision to pursue his own interests is a calculation of the potential costs of doing so against the benefits that would entail from the transaction (Andvig *et al.* 2000).

The general principal-agent model has been used by several authors in the study of corruption (Klitgaard 1988; Klitgaard *et al.* 2000; Lambsdorff 2007; Rose-Ackerman 1999). Most commonly it is applied in the analysis of bureaucratic corruption, where the principal is understood as a high-level official, in charge of a government agency or even the public administration as a whole, and assumed to be committed to serving the public interest, and the agent in turn is another official at a lower level of hierarchy, who is entrusted with functions by the principal. However, the same approach can be easily applied taking society as the principal and public officials as agents (supposedly) acting in its interest. As this dissertation focuses on political corruption, the framework is considered with elected officials as agents and the citizenry as the principal; corruption thus represents a violation of the trust delegated by voters and can be considered to harm the public interest in benefit of private interests (Amundsen 1999; Lambsdorff 2007; Philp 2002)¹.

From this model's perspective, the key to curbing corruption lies in two main strategies: (a) reducing the information asymmetry between principal and agent, and (b) improving detection and punishment of corruption, in order to deter corrupt behavior. Both strategies are linked with strengthening accountability: minimizing the information asymmetry problem in the principal-agent relationship can be associated with what Schedler (1999) has called the answerability side of accountability, which refers to how accountability mechanisms serve to exercise pressure on office holders to be more transparent about their actions and justify them

¹ Conceptualizations of corruption in association with the public interest have the disadvantage of not clearly defining the normative substance of "public interest" (Philp 2002), but they are appropriate for this analysis because they reinforce the idea of an accountability link between voters and the government. More recent conceptual contributions in the field have addressed the lack of a universal normative standard in commonly used definitions of corruption by proposing that corruption be understood as the violation of specific principles for the exercise of public authority, such as impartiality (Rothstein 2011) and universalism (Mungiu-Pippidi 2006, 2015). Although these authors do not make an explicit reference to the public interest in their conceptual discussions, they also describe corruption i.e. partial or particular treatment as favoring private interests. It is thus possible to interpret these notions as representing an ultimate common denominator of what constitutes the public interest that should guide government decisions.

to the public, whereas strengthening detection and punishment of corrupt acts refers to what the same author has named the sanctioning side of accountability.

They can also be more specifically associated with the three dimensions of accountability discussed earlier. In the case of electoral accountability, efforts by the opposition to call the incumbent administration to account on its decisions or on corruption accusations raised against it would be an example of how this accountability dimension may help reduce the information asymmetry problem, and the electorate's potential reaction to information on malfeasance by voting the incumbent out of office would constitute the sanctioning component of electoral accountability. In the case of social accountability, efforts by a free media to investigate on government acts and abuses, as well as civil society initiatives to disseminate political information to the general public, illustrate channels that help increase society's ability to learn, as principal, about policy-makers' actions as its agents. The sanctioning component for this dimension of accountability, however, differently than with electoral accountability, would be of a more reputational nature, but could also create important costs for politicians (Peruzzotti and Smulovitz 2006b). Finally, agencies exercising horizontal accountability (e.g. Congressional Committees, Ombudsmen, Courts of Accounts) may similarly function as channels through which office holders are called to justify their actions and clarify suspicions of abuse, or may directly lead to concrete sanctions as a consequence of that abuse, through criminal prosecution, for instance.

It is important to emphasize, however, that the sanctioning element of these accountability channels is of greater relevance for the causal mechanisms proposed in the literature on how they are expected to impact corrupt behavior, because it is the threat of sanctions, or actors' assessment of their likelihood, that is believed to give them incentives to behave according to the interest of citizens (Manin *et al.* 1999a), thereby exercising a deterrent effect on corruption (Klitgaard 1988; Rose-Ackerman 1999).

With the proposed focus on the dimensions of accountability, the research described in this dissertation attempts to contribute to the broader literature on democracy and corruption in two ways. First, by applying a conceptual perspective in which democracy is expected to help reduce corruption mainly through different mechanisms of accountability, it seeks to go one step further towards a better understanding of the relationship between these two phenomena and its potential underlying causal mechanisms. Second, it also addresses the lack of clarity with regards to how democracy is conceptualized in this literature. By highlighting

the dimensions of accountability as key features of democracy for analyzing its potential impact on corruption, this perspective offers an alternative approach that bypasses the maze of conflicting definitions and measures of democracy observed across different studies on the subject.

Building upon the framework discussed above, two more specific research questions are formulated to guide the empirical contributions of the dissertation, which are presented in its second and third parts. First, the framework of dimensions of accountability is directly tested empirically to verify whether effective electoral, social and horizontal accountability indeed contribute to reducing corruption in a political system. The main goals of this analysis are to examine the validity of the hypothesized deterrence effect of these mechanisms of accountability on corruption, but at the same time going further than most previous studies in this field, in that it also attempts to disentangle the effects of those mechanisms which are potentially simultaneously at work. This question is addressed in the second part of the dissertation, which is composed of two related articles. The first one mainly presents an original panel data set of political corruption that was collected for this research, based on audit results from Brazilian municipalities, also contextualizing it as a broader contribution to recent developments in the field of corruption measurement. The second article engages more specifically in hypothesis-testing through a series of statistical procedures attempting to determine whether the data at hand offer support to the claims that accountability helps reduce corruption, in a contribution to a more specific, newer branch of empirical research about the deterrent effects of accountability on corruption.

Most studies in this line of work focus on electoral accountability (Bobonis *et al.* 2012; Crisp *et al.* 2014; Ferraz and Finan 2011; Pereira *et al.* 2009), but a few others offer interesting insights on the impact of the other dimensions of accountability as well (Avis *et al.* 2016; Brunetti and Weder 2003; Camaj 2013; Grimes 2013; Mungiu-Pippidi *et al.* 2011; Olken 2007). However, to the best of my knowledge, no study examining the relationship between accountability and corruption has so far fully accounted for the potential simultaneous effects of these three dimensions of accountability. Also, very few works have used longitudinal data to test the impact of accountability on future levels of corruption. Finally, most analyzes in the literature operationalize accountability only indirectly, often through institutional variables such as term limits, in the case of electoral accountability, or judicial independence, in the case of horizontal accountability. This dissertation seeks to address these three empirical gaps by developing a

single model to account for possible confounding effects of the different mechanisms of accountability, using an original panel data set that includes a concrete audit-based measure of political corruption and direct indicators of electoral and horizontal accountability.

The other specific research question addressed in the dissertation is concerned with what makes accountability effective in the first place, or more specifically, under which circumstances are corrupt politicians sanctioned for their malfeasance? As an assessment of this question applied to all three dimensions of accountability would certainly warrant another full research project, the analysis presented in the third part of this study focuses on examining the conditioning factors of electoral accountability in the case of politicians accused of corruption.

There are a few theoretical and strategic reasons that justify the choice to focus on this particular dimension of accountability. Firstly, it is directly linked to the electoral component of democracy, which can be considered as its most essential attribute. From the narrowest conception of democracy to its most comprehensive and demanding counterpart, the existence of regular competitive elections is the only omnipresent pre-requisite of a democratic regime. The centrality of this component of democracy becomes clear also in the discussion on sub-types of defective democracy (Merkel 2004), where the electoral regime is the only dimension that cannot be impaired without modifying the very democratic status of the regime. There are two main reasons for the primacy of this dimension: (a) it is the most manifest expression of democracy's underlying principle, i.e. the sovereignty of the people, and (b) it represents the central distinguishing element between democracies and autocracies (Merkel 2004; Møller and Skaaning 2010). It is thus an absolutely necessary attribute, hierarchically superior to other attributes of democracy (Møller and Skaaning 2010). O'Donnell also highlights this point in his discussion on the vertical and horizontal dimensions of accountability, claiming that the existence of vertical electoral accountability is in fact a definitional aspect of democracy, without which no democratic regime can exist (O'Donnell 1999, 29, 2003, 36).

Secondly, and also related to the previous point, electoral accountability is the most ubiquitous causal mechanism discussed in the literature on the effects of democracy on corruption. Indeed, the idea that elections can work to induce politicians to refrain from corruption is a widely held assumption in the literature on democracy and corruption. However, the empirical evidence on this link is still relatively weak. Recent empirical studies on

the effect of electoral accountability on corruption suggest that politicians respond to some electoral incentives that constrain corrupt behavior (Bobonis *et al.* 2012; Ferraz and Finan 2011; cf. Pereira *et al.* 2009), but at the same time, other findings put into question how effective this mechanism can be in substantially lowering corruption in a political system over time (Bobonis *et al.* 2012; Crisp *et al.* 2014).

In addition to doubts about the effects of electoral accountability on corruption outcomes, the literature on democratic accountability has also raised deeper concerns about the functioning of electoral accountability in democratic regimes (Diamond *et al.* 1999; Manin *et al.* 1999a; O'Donnell 1999; Peruzzotti and Smulovitz 2006b). A number of arguments, both from a theoretical and an empirical perspective, question the effectiveness of electoral control by voters more generally. On more theoretical grounds, it is argued, for instance, that the occurrence of electoral accountability predicted by certain theoretical models rely on assumptions² that are too restrictive with regards to how voters behave. As a consequence, electoral accountability would function as expected by economic theory only in very limited circumstances that do not appropriately reflect the context of actual democracies (Maravall and Sánchez-Cuenca 2008).

Other arguments emphasize that elections are a very limited instrument for voters to effectively punish or reward incumbents (Maravall and Sánchez-Cuenca 2008, I. 139; O'Donnell 1999, 30) and keep governments under control (Diamond *et al.* 1999, 2). One reason for that are the multiple roles of elections, which negatively impact their functioning as a mechanism of control by voters. Persson *et al.* (1997), for instance, highlight that providing a mechanism to hold politicians accountable for past performance is only one of several purposes that elections serve in a political system³. Therefore, voters may prioritize other purposes when making a voting decision, instead of punishing an incumbent for unsatisfactory performance or misconduct. Manin *et al.* (1999a) and Fearon (1999) argue that the incentives for “good behavior” that should result from voters’ threat to sanction bad performance and rent-seeking

² Common assumptions in models of retrospective voting include: citizens ignore promises and only care about outcomes; there is no candidate heterogeneity; voters’ distributive preferences over welfare are uniform; and they are able to coordinate on a voting rule for the welfare level that determines their vote for or against the incumbent (Maravall and Sánchez-Cuenca 2008, II. 129-132).

³ These include aggregating and representing voters' conflicting preferences, aggregating dispersed information about the correct political decisions, and allowing citizens to select the most competent individuals for public office (Persson *et al.* 1997, 1165).

are in fact weakened when the vote is at the same time used to select better governments, i.e. these two goals cannot be effectively simultaneously pursued. Manin *et al.* (1999a) mention yet another reason why electoral accountability is an inherently imperfect control mechanism: even under the most favorable conditions, where voters have enough information about politicians' behavior and can clearly attribute responsibility for specific outcomes, each voter still has only one vote to make a judgment on hundreds of decisions that the government has taken during the previous term. As they put it, "One cannot control a thousand targets with one instrument" (1999a, 50).

Additionally, an extensive empirical literature has examined the question of whether voters in fact punish incumbents based on their performance in office. A dominant perspective on this question is that of studies on economic voting, which seek to assess to what extent the state of the economy affects voters' decision to support the incumbent government or not. In general, findings point to an intermittent and substantively small effect of economic outcomes—usually proxied by measures of unemployment, income growth and inflation—on electoral results (Anderson 2007). Moreover, this impact is observed more with regards to government popularity and incumbent parties' vote share than governments' actual survival in office (Cheibub and Przeworski 1999; Manin *et al.* 1999b). These results also appear to be contingent on a number of institutional factors and voter characteristics (Anderson 2007; Kayser 2014). Another stream of research focusing more specifically on the impact of corruption accusations on voting behavior shows a similar picture: incumbents appear to suffer some vote share loss after being associated with corruption scandals, but a vast majority of them gets reelected nonetheless (Chang *et al.* 2010; Ferraz and Finan 2008; Golden 2006; Jiménez and Caínzos 2004; Muñoz *et al.* 2012). Therefore, understanding the conditions under which voters are (or are not) able and willing to uphold their role of principals and hold corrupt politicians to account remains an intriguing empirical question in political studies.

Finally, a strategic reason to focus this analysis on electoral accountability is that the literature on this dimension is much richer in terms of insights on the conditions for its effectiveness than scholarly work on social and horizontal accountability. The relatively advanced state of theoretical and empirical research on the factors that should affect electoral accountability, in particular in response to corruption, provides for numerous testable hypotheses on what could make it more or less effective. At the same time, there are still significant gaps that can be explored in further research. In contrast, the determinants of social and horizontal

accountability on corruption have been much less systematically explored, and more theoretical work is needed to articulate testable predictions on the factors that may affect or condition their effectiveness (Olsen 2013). Especially in the case of social accountability, we are dealing with a rather recent concept (Peruzzotti and Smulovitz 2006b; Smulovitz and Peruzzotti 2000) that has been so far mostly illustrated in case studies (see Grimes 2008 for a review). With horizontal accountability, even though the debate on its deficit in young democracies has been around for a longer time (O'Donnell 1994, 1999; Schedler *et al.* 1999), still little is known on what makes it effective.

In sum, the continuous discussion in the literature on the effectiveness of electoral accountability, in particular as a response to politicians' involvement in corruption, reinforces this study's motivation to further explore this question empirically, which is done in the third part of the dissertation. The current state of empirical research on electoral accountability suggests several factors that may foster or hinder the effective sanctioning of corrupt politicians by voters (see Jiménez and Caínzos 2004; de Sousa and Moriconi 2013 for a review), but they have been mostly examined in a fragmented way by studies that test isolated hypotheses, neglecting relevant alternative explanations and providing partly contradictory results. This dissertation seeks to make a contribution to this literature by conducting a more systematic and comprehensive analysis of those factors, testing the main competing hypotheses simultaneously in order to examine their relative strength in explaining effective electoral accountability against corrupt politicians, or what has also been described in the literature as corruption voting.

Based on a comprehensive review of existing empirical research, five hypotheses related to different conditioning factors were selected as most relevant for the empirical analysis. The first one highlights information as a necessary condition for voters to exercise electoral accountability. The second hypothesis refers to an "implicit exchange mechanism", through which voters may face trade-offs in their retrospective assessment of the government: while they may be inclined to punish incumbents accused of corruption, they may at the same time hold a positive view of their overall performance in office, or have congruent policy preferences or ideological leaning with the government, and the latter criteria could under certain conditions prevail in the voting calculus, thus weakening corruption voting. A third hypothesis deals with a more material kind of trade-off linked to clientelistic ties between corrupt incumbents and voters, which may lead the latter to "look the other way" when

casting their vote. A fourth explanation considered highlights that, in order to vote corrupt politicians out of office, voters need first to identify suitable alternatives among the challengers that they perceive both as “cleaner” than the incumbent and as sufficiently congruent with their own political preferences. In the absence of such alternatives, electoral accountability could also be jeopardized. Finally, the severity of corruption accusations may also be considered by voters as a relevant factor determining their voting decision. These five hypotheses are tested with a cross-section of Brazilian municipalities.

The choice of Brazilian sub-national units as a setting to base the empirical analyses described in the second and the third parts of the dissertation is justified by a series of advantages in terms of methodological possibilities and data availability. A central motivation for this choice of context is a valuable source of corruption data available for Brazilian municipalities, namely a series of audit reports produced as a result of a randomized audit program introduced by the Federal Comptroller’s Office (CGU) in 2003. This program represented a strong shift in anti-corruption efforts in Brazil, in particular considering the relatively weak and to some extent politicized monitoring mechanisms that are otherwise in place to oversee the use of public funds at the municipal level (Arantes *et al.* 2005; Hidalgo *et al.* 2016; Loureiro *et al.* 2009; Weitz-Shapiro *et al.* 2015). Between 2003 and 2013, the time frame considered for the data collection conducted as part of this research project, around 1,800 municipalities were audited through the program at least once, in a universe of over 5,500 municipalities eligible to be selected. Therefore, the wide geographical and temporal coverage of the program offers a broad range of cases to be explored in the analyses proposed for this dissertation. Moreover, the fact that a single federal agency, with a highly professionalized and well-trained body of auditors, runs the audits throughout the country further ensures comparability of the data for the different municipalities.

The analysis exploits three other key advantages associated with this data source: (a) the possibility to derive a concrete indicator of political corruption from the audit results, (b) the availability of longitudinal data for a sub-sample of the municipalities, and (c) the randomized selection of municipalities to be audited. These elements provide a great opportunity for improving empirical research on corruption through the use of concrete instead of perception-based indicators (Mondo 2016), for more than one time period in some cases, and the minimization of selection bias and its shortcomings for statistical inference.

Exploring the sub-national context in Brazil also taps into great potential for comparative analyses, while at the same time allowing us to hold constant a number of relevant institutional factors that would need to be controlled for in cross-national comparisons to assess the links between democracy, accountability and corruption. Being a large and diverse country, Brazil offers sufficient internal variation across a number of relevant municipal characteristics. Additionally, various data sources available for the sub-national level in this country provide rich and reliable data for the key dependent and independent variables examined in this study and many other relevant covariates considered in the analyses.

For the purpose of the specific analyses presented in the main empirical contributions of the dissertation, original data collection was conducted on two fronts. First, an indicator of political corruption was produced from the coding of over 600 audit reports published by the CGU following the municipal audits. One part of this process refers to a sample of 140 municipalities that have faced multiple audits in the program, which provides sufficient material to generate corruption estimates for different time periods. As a result, a unique panel data set covering five municipal administrative terms between 1997 and 2013 was compiled to be used in the analysis presented in the second part of the dissertation, which examines the impact of the three dimensions of accountability on future corruption levels. The second part of the coded reports is related to a cross-section of 383 municipalities audited between 2005 and 2008; this second data set was used in the analysis described in the third part of this work, which focuses on conditioning factors of electoral accountability against corrupt politicians, taking as a reference the 2008 municipal elections.

The second portion of original data collection consists of the coding of an indicator for the occurrence of horizontal accountability, also employed in the second part of the dissertation. For this variable, three possible channels of horizontal accountability were considered: (a) legal sanctions due to criminal or civil prosecution, (b) administrative sanctions associated with oversight procedures from Courts of Accounts, and (c) political sanctions deriving from impeachment procedures against mayors. In order to gather sufficient information on all three channels, data sources such as court records, rulings by Courts of Accounts and media reports were used.

In addition to these original indicators, the dissertation also makes use of data from existing sources to devise new forms to operationalize other relevant variables used in the empirical analyses which have not been explored in previous studies in this field. One example is a

measure of patronage, used in the analysis described in the third part of the dissertation to capture the clientelistic mechanism hypothesized as a potential moderator of corruption voting. In the same analysis, a municipal development index published by the Industry Federation of Rio de Janeiro (Firjan) is taken as one measure of incumbent performance to account for the implicit exchange mechanism also believed to impact electoral accountability. As a whole, these integrate a parallel effort in this dissertation to make a contribution in increasing the repertoire of useful indicators that could be further explored in future studies on the Brazilian context.

In terms of the findings that result from the two main lines of empirical inquiry pursued in this study, they partly reinforce the skepticism in the literature regarding the impact of accountability on corruption and the functioning of electoral accountability. The articles integrating the second part of the dissertation, focused on estimating the effect of electoral, social and horizontal accountability on future corruption, fall short of confirming the deterrence hypothesis put forth in the related literature, resonating with previous findings from recent studies (Bobonis *et al.* 2012; Crisp *et al.* 2014). Based on the analysis at hand, it is difficult to say whether certain factors particular to Brazil may in part explain why the expected effect of accountability doesn't appear to materialize in this context, whether there are other contingencies that need to be taken into account regarding this effect, or whether we should be looking somewhere else for evidence of the underlying causal mechanisms that might explain the broader patterns of association between democracy and corruption levels discussed in the beginning of this introduction. In any case, the results presented here indicate that the relationship between democratic institutions and corruption is likely more complex than depicted in some of the theoretical arguments usually raised in the literature, and other than simply assuming an effect of accountability on corruption, scholarly work in this field should go much deeper in trying to dissect the concrete causal mechanisms linking democracy and corruption.

In the case of electoral accountability against corrupt politicians, we see that, as already shown in myriad empirical studies on this topic, voters' reaction to corruption accusations in the form of electoral punishment of corrupt incumbents is anything but an automatic response, differently than oftentimes assumed in the literature on democracy and corruption. Indeed, the results presented in the third part of the dissertation further corroborate that corruption voting is highly contingent on other factors. In particular, partial evidence is found for a

conditioning effect of the incumbent's performance on areas such as the economy or the provision of public services—thus offering some confirmation for the implicit exchange hypothesis—and of the severity of corruption charges. However, even these moderating effects appear to be conditional on third factors, such as the time frame of the performance assessment and the timing of release of the corruption information, which in turn could be linked to how salient this information becomes for the public through other dissemination channels. In sum, these findings illustrate how the conditions for successful electoral accountability against corrupt incumbents may be even more restrictive than usually expected.

Throughout the empirical sections of the dissertation, several particularities related to the relevant mechanisms of accountability in Brazil are briefly mentioned in connection to the respective data sources and later in the discussion of the results. As a complement to those more isolated discussions, a general overview of recent developments with regards to those accountability institutions might be useful to provide unfamiliar readers with a more comprehensive background about the country's institutional environment. For this purpose, the last section of this introduction below seeks to contextualize some topics of interest for this dissertation in the scope of Brazil's recent political history.

Contextualizing democracy and accountability in Brazil

Brazil offers an interesting context for a study of the relationship between democracy, in particular with a focus on accountability channels, and corruption. It is a relatively young democracy, which underwent its latest democratic transition in 1985, but has by now accumulated three consecutive decades of democratic experience⁴, thus offering a time frame where certain consolidation patterns could be observed in terms of building effective accountability mechanisms, and its potential impact on corruption outcomes. Indeed, a number of recent studies have argued that a trajectory towards the strengthening of accountability institutions, but also of other democratic features more generally, such as

⁴ The Polity IV indicator classifies Brazil as a democracy since 1985, with a stable score of 8—in a scale with a maximum of 10 points—since 1988. Similarly, Freedom House considered Brazil as a free country after its democratic transition in 1985, with slight deterioration in the scores between 1993 and 2002, but the country has recovered and maintained the “free” status ever since, with a stable score of 2—in a scale of 1 to 7 (least free)—in recent years.

political competition and participation, has in fact materialized in the country since re-democratization (Hagopian 2005; Power and Taylor 2011; Praça and Taylor 2014; Rennó *et al.* 2010; Weyland 2005). Despite these improvements, Brazil still faces serious deficits in terms of the quality of its democracy, especially regarding effective rule of law, equal access to justice and the full protection of civil liberties, in particular for less privileged and marginalized social groups (Hagopian 2005; Kingstone and Power 2000; Weyland 2005).

If the general perspective in the literature on accountability institutions in Brazil is of a positive development in the last decades, what does this mean in concrete terms? What specific improvements have been observed in recent times, and what are the weaknesses that potentially remain? These are the questions that this brief overview attempts to address. In order to better structure the discussion provided here, the three dimensions of accountability applied throughout the dissertation will also serve as a reference in the assessment of accountability mechanisms in Brazil.

Electoral accountability

An assessment of how well electoral accountability is exercised by the electorate in Brazil should include two considerations. The first one refers to voters' ability to use the ballot as a means of signaling their retrospective evaluation of their representatives and withdrawing support when they are not satisfied with their performance in office. This ability, in turn, is influenced by several institutional factors and other characteristics of the political environment.

A number of elements related to the way elections are structured in Brazil can be construed as favoring electoral accountability, in that they provide voters with sufficient possibilities of retrospectively rewarding or punishing their representatives. Due to separation of powers, direct elections for Executive office at all levels of government allow voters to directly decide whether to further support an incumbent administration or to replace it with an opposition candidate in a highly personalized manner (Hellwig and Samuels 2007; Nicolau 2002). The existence of a second round run-off election for the federal and state Executives, as well as for mayoral office in municipalities with more than 200,000 voters, gives the electorate an additional opportunity to voice their discontent (Rennó 2011). Since the introduction of reelection for one consecutive term through a constitutional amendment in 1997, the

accountability link between incumbents and voters was further strengthened. Additionally, the concurrence of Executive and Legislative elections⁵ has also been argued to contribute to retrospective voting (Hellwig and Samuels 2007). Legislative elections, in turn, are based on a system of open list proportional representation (PR)⁶ with large district magnitude⁷. Although this system has been argued to weaken the accountability link between voter and candidate (Melo *et al.* 2009), at the same time it has allowed Brazilian voters to base their retrospective assessment on the personal performance of the representative (Nicolau 2002)⁸. The large number of parties in the Brazilian political environment also contributes to offer the electorate a wide array of candidates to choose from both for the Executive and the Legislative.

Another dimension in which institutional design may condition voters' ability to exercise electoral accountability refers to the role of clarity of responsibility, i.e. how easy it is for voters to attribute responsibility for policy outcomes to the relevant actors (Hellwig and Samuels 2007; Hobolt *et al.* 2013; Nicolau 2002). Under high clarity of responsibility, electoral accountability is facilitated. In this respect, however, the Brazilian political environment can be seen as characterized by relatively low clarity of responsibility. Despite the high concentration of powers in the hands of the Executive, separation of powers, coupled with bicameralism (at the national level) and federalism, contribute to dispersing power and responsibility within governments and across administrative levels (Hobolt *et al.* 2013; Pereira *et al.* 2011). This situation is the more exacerbated by the extreme fragmentation of the party system, which is fostered by very permissive rules regarding the creation of new parties and their access to representation in Parliament and to public funding (Power 2000). As a consequence, the party elected for the Executive most often holds only a minority share of legislative seats, thus

⁵ In Brazil, elections for the Executive and Legislative both at the federal and at the state level occur at the same time every four years; elections for the municipal Executive and Legislative are also held together, but in two-year intervals from federal and state elections.

⁶ The only exception to the PR system in legislative elections is the application of plurality rule in elections to the Senate.

⁷ For the lower House, districts are the 27 units of the federation, including the Federal District, and the number of seats for each district varies between a minimum of 8 and a maximum of 70. In the Senate, each unit of the federation has three seats.

⁸ Even though less than half of all votes cast in legislative elections are "successful" in winning a seat to their selected candidate, those voters whose preferred candidate has not been elected still appear to engage in some sort of retrospective voting, using information on the performance of other legislators that somehow "stand out" in Parliament to guide their voting decision in the subsequent election (Nicolau 2002).

creating the need for governments to negotiate broad-based coalitions, sometimes at the expense of ideological cohesion, in order to be able to push their agenda through the Legislative (Melo and Pereira 2013; Pereira *et al.* 2011; Power 2000; Taylor 2010).

The second relevant question regarding the effectiveness of electoral accountability refers to whether voters indeed make use of this instrument to punish or reward incumbents. One favorable aspect in the behavior of Brazilian voters is the high level of participation in the electoral process, largely due to mandatory voting, with average turnout rates above 80% in the recent democratic period (Power 2009)⁹. A number of studies on elections and voting behavior in Brazil have demonstrated that retrospective assessments of government performance indeed play a strong role in elections for different levels of government (Almeida 2008; Cavalcante 2015; Lavareda and Telles 2011; Nicolau 2002; Rennó 2011). An early example of this behavior was already seen only a few years after re-democratization, when Fernando Collor de Mello, the first directly elected president of the current democratic period, was impeached by Congress, and the electorate punished those parties and Congressmen that had voted against the impeachment by withdrawing support in the 1992 municipal elections (Geddes and Ribeiro Neto 1992). Research focusing more specifically on electoral accountability against politicians directly accused of corruption also suggests that information on corruption disseminated by the media or other institutional channels does have an impact on voters' decision at the ballot box (Ferraz and Finan 2008; Pereira *et al.* 2011; Rennó 2011; Rennó Jr. 2008).

However, findings from this dissertation and other studies in the literature highlight the contingencies associated with the electoral sanctioning of politicians accused of corruption. Episodes of stronger electoral accountability appear to be influenced by the level of salience of corruption at the moment, such as the recent exposure of a scandal. In 2002, for instance, survey data shows that corruption was not among the most salient problems to Brazilians, and therefore corruption accusations do not seem to have impacted the electoral outcome so strongly; in 2006, on the other hand, when a major scandal erupted shortly before the elections, allegations of corruption had much more impact on the electoral outcome and accused legislators were punished across the board (Hagopian 2005; Pereira *et al.* 2011; Rennó

⁹ It is important to note that, despite relatively high turnout, there are still high rates of blank or spoiled votes (Nicolau 2002, 2015, Power 2000, 2009).

Jr. 2008). This impact appears to be limited also by other factors that voters take into account in their calculus, such as pre-existing loyalties and their overall assessment of the incumbent government's performance (Figueiredo *et al.* 2011; Pereira and Melo 2015; Rennó 2011), or voter characteristics such as education level (Weitz-Shapiro and Winters 2014). Moreover, the considerable reelection rates of politicians accused of corruption still indicates that part of the electorate is willing to overlook corruption charges and put more weight on other criteria (Ferraz and Finan 2008; Pereira *et al.* 2011; Rennó 2011; Rennó Jr. 2008).

Another potential obstacle to electoral accountability in this context is the presence of clientelistic linkages between voters and politicians, especially at the local level. As already mentioned above, the Brazilian electoral system creates strong incentives for personalized politics and individualistic strategies. For many politicians, electoral success hinges on their ability to access resources controlled by the Executive in order to guarantee the provision of private goods or locally targeted public goods to their constituencies (Desposato 2006; Macaulay 2011; Samuels 2002, 2006a). However, the prevalence of clientelistic linkages over programmatic ones appears to have decreased over the years in the recent democratic period, and while politicians still have their local bases as the main foundation for electoral support, their strategies appear to move away from direct exchanges of private goods towards indirect ones based either on local public goods or on broader policy appeals (Lyne 2006). This process is geographically uneven, though, with regions where clientelism is still widespread, and others where programmatic politics are well-established (Desposato 2006).

Overall, we see that, regarding this dimension of accountability, the institutional environment appears to produce effects both promoting and hindering electoral accountability. Nevertheless, voters seem to have effectively used this mechanism as an instrument to reward or sanction incumbent politicians on the basis of their performance in office throughout the recent democratic period, and in some occasions have even more strongly demonstrated their discontent with politicians' involvement in corruption. At the same time, it is difficult to identify the existence of great change over time. It appears that electoral accountability, in particular as a response to corruption, comes and goes in waves, according to other conditioning factors such as the economic situation or the issues most salient to voters at the time. Moreover, clientelism is still a central feature of politics in many regions of the country, which may further weaken electoral accountability.

Social accountability

Social accountability also shows positive developments throughout the democratic period. The assessment presented here focuses on the two main actors related to this dimension: civil society and the media (Peruzzotti and Smulovitz 2006b). Civil society has demonstrated its strength on a number of occasions in the past decades. Already under the military regime, social movements displayed strong dynamism and capacity for coordination and mobilization in opposition to the regime, which culminated in massive protests in favor of direct elections on the eve of the democratic transition (Hochstetler 2000). The potential for large mobilization in the form of street demonstrations continued into the new period. A clear example was seen during the impeachment proceedings against President Collor, when the masses took to the streets and were successful in generating pressure over legislators for voting in favor of impeaching the president (Hagopian 2005; Hochstetler 2000). More recently, in 2013, mass demonstrations again exploded all over the country, first motivated by increases in public transportation fares in the country's largest cities, but quickly the message mutated into an expression of generalized dissatisfaction with state inefficiency, corruption and the poor quality of public services (Singer 2013). After the uncovering of a massive corruption scandal involving the state-controlled oil company Petrobrás in 2014, repeated nation-wide demonstrations followed in 2015 and 2016 with a more focused agenda: direct opposition to the Workers' Party (PT) administration and the impeachment of President Dilma Rousseff (Gohn 2016).

After re-democratization, important foundations were laid down for new forms of civic mobilization and participation, especially through institutionalized *loci* of interaction with the state (Cavalcanti 2006). Traditional social movements from the previous period, which had strongly focused on demanding democratization, gave way to new movements and a wide range of organizations such as interest associations and NGOs (Hochstetler 2000; Weyland 2005). Overall, civil society appears to have successfully adjusted to the new political environment through the use of new strategies of mobilization, increased advocacy efforts, and also close cooperation with governments on several occasions (Hochstetler 2000). One example of an innovative sphere for such cooperation is the system of participatory budgeting first introduced in the city of Porto Alegre in the late 1980s. This has by now been used in many other cities and has established itself as a strong tool of social accountability and a positive example from Brazil replicated also in other parts of the world (Grimes 2008;

Hagopian 2005). Other participatory structures for increased involvement of civil society in policy-making and in monitoring of the public administration exist also in the form of government councils at different levels of government (Cavalcanti 2006; Hochstetler 2000). The Council for Public Transparency and Anti-Corruption (*Conselho da Transparência Pública e do Combate à Corrupção*), created by the CGU, for instance, housed intense discussions with civil society for the drafting of Freedom of Information legislation since 2005, culminating in the passing of a law with this purpose in 2012 (Loureiro *et al.* 2012). Another important trend in civil society initiatives to monitor governments is the close cooperation with institutions of horizontal accountability, in particular the Public Prosecution Services (MP) (Cavalcanti 2006).

A key participatory instrument instituted in the 1988 Federal Constitution is the citizen initiative to propose legislation, which requires support from 1% of the electorate from at least five states as a pre-condition to present a bill to Congress. Following strong mobilization efforts, this instrument has been increasingly used by civil society in recent years and has successfully led to the enactment of public interest legislation in four occasions; several initiatives of this type are currently under analysis in Congress (Beltrão and Vidigal 2013). Two of the pieces of legislation that originated from popular initiatives are particularly important as instruments in the fight against corruption. The first one is a law created to punish vote-buying. Led by the National Conference of Brazilian Bishops (CNBB), the mobilization to gather support for the bill collected over a million signatures. The initiative was approved in Congress in 1999 and has had considerable impact on elections since, with hundreds of politicians being removed from office due to vote-buying charges, predominantly at the local level (Melo 2014; Taylor 2011). More recently, a citizen initiative also initiated by the CNBB and supported by a broad movement of associations was the “Clean Slate” bill, which was passed into law in 2010. The law generally precludes individuals with a legal conviction by a collegial court—as a rule, this means a confirmation of a conviction after a first appeal¹⁰—to run in elections for a period of eight years after the conviction (Melo 2014; Taylor 2011).

Although these examples indicate that an organized civil society is in place and is ready to make use of the different instruments that the democratic order has offered them to increase their influence over policy-making, it is also true that certain obstacles remain to a more

¹⁰ Exceptions to this are usually cases involving elected officials or cabinet members who, while in office, have special standing (*foro especial*) to be tried by higher instances of the Justice system (Taylor 2011). In that case, a conviction by a collegial court could be the first conviction in the case, prior to an appeal.

effective impact of these actors on governments' decisions and legislation. In many cases, governments offer great resistance to concrete input from civil society (Cavalcanti 2006). Government councils, for instance, are sometimes deliberately underfunded or simply ignored by policy-makers; governments may also try to co-opt representatives of social movements (Hochstetler 2000). The use of the popular initiative to propose legislation is also hindered by the demanding requirements for gathering minimal support—currently, the hurdle is about 1.4 million signatures—and by the formal procedures to validate the necessary signatures (Beltrão and Vidigal 2013). In sum, capacity for mobilization and coordination seems high within the organized civil society in Brazil, and there are good channels for participation, but at the same time there is still room to foster stronger input from these groups in the policy process. With regards to corruption, the public has successfully mobilized in a number of occasions, not only to exercise pressure for the political sanctioning of governments involved in corruption scandals, but also to directly influence the introduction of anti-corruption legislation.

The media have also gone through interesting developments in this period. Since the 1980s, there has been a strong trend towards modernization, professionalization and increased “objective” reporting, thus weakening previous patterns of partisanship observed in the main news organizations in earlier periods (Porto 2011). Among the main news media groups, the market is quite competitive, diversified and economically autonomous from the state¹¹, although some degree of market concentration exists especially with regards to the *Globo* group, the most influential media conglomerate in the country (Hervieu *et al.* 2013; Melo 2014; Porto 2011). Despite the existence of broader conditions for independence, there are still a couple of factors that leave room for political influence over certain outlets, in particular in the case of television and radio. For this type of media, licenses are essentially allocated by the federal government, which creates room for politicization of the licensing process (Boas and Hidalgo 2011; Porto 2011; da Silva 2000)¹². Moreover, the phenomenon of radio and television stations owned by local political elites is well-documented in the Brazilian case, in

¹¹ Smaller media companies, on the other hand, may be more financially dependent on government advertisement (Hervieu *et al.* 2013).

¹² Indeed, Boas and Hidalgo (2011) provide empirical evidence of this by showing that incumbent city councilors are much more likely to have their requests for community radio licenses approved than unelected candidates.

particular in less developed regions, which also contributes to hamper press freedom (Boas and Hidalgo 2011; Hervieu *et al.* 2013; de Lima and Lopes 2007; Porto 2011; Taylor 2010)¹³.

Since relatively early in the new democratic period, the media have performed an important role in investigating and disseminating information on corruption cases, and the number of exposés of this sort published in the period quickly increased (Geddes and Ribeiro Neto 1992; Melo 2014; Porto 2011). Print media has been at the forefront regarding most corruption exposés and, despite the relatively low penetration of this type of media, their outreach is amplified by their influence on the news agenda of other outlets, such as radio and television, which have much higher penetration in Brazilian households (Porto 2011). The new political context of more openness and freedom afforded media groups a radically different environment with more room for investigative work, but more importantly, a great share of reporting on corruption investigations has originated from an ever closer relationship that news media have developed with institutions of horizontal accountability; in the opposite direction, the media have also significantly contributed to accountability by activating other formal accountability channels (Porto 2011).

Horizontal accountability

Finally, it is important to discuss the environment of institutions of horizontal accountability. In this dimension, Brazil's web of accountability displays several agencies or bodies with oversight, investigation and enforcement powers, pertaining to all three branches of government and also to the independent *Ministério Público* (MP), which concentrates prosecutorial and investigative functions and works as a fourth branch of government (Melo 2014; Porto 2011; Power and Taylor 2011; Praça and Taylor 2014; Taylor 2010).

Under the Executive branch on the national level, agencies such as the CGU and the Federal Police have been responsible for great advances in anti-corruption efforts. In particular in the case of the CGU, since its establishment in 2003 as an agency centralizing internal control

¹³ Although the direct ownership of media outlets by elected politicians while in office is not legally permitted, they usually find ways to circumvent this prohibition by transferring management to relatives or other associates (Hervieu *et al.* 2013; de Lima and Lopes 2007).

functions at the federal Executive¹⁴, there have been considerable efforts to strengthen its role in detecting and preventing corruption, turning it into a *de facto* anti-corruption agency (Loureiro *et al.* 2012). One example is a randomized audit program targeting the use of federal funds by municipalities, which has been in place also since 2003 and is the source of most of the original data collected for this dissertation. Indeed, empirical studies suggest that the program has had a significant effect in reducing corruption at the municipal level (Avis *et al.* 2016; Zamboni Filho 2011). In addition to its top-down control activities, the CGU has also taken up an important and innovative role in fostering bottom-up monitoring by civil society, through the dissemination of information on public administration procedures, the promotion of public transparency and capacity building initiatives directed at local actors (Loureiro *et al.* 2012). The CGU has also contributed to the diffusion of its institutional model to the sub-national level (Loureiro *et al.* 2012), with Comptroller's Offices or equivalent agencies being introduced in almost all of the 26 states (Corbari *et al.* 2013) and in a number of larger cities.

The Federal Police (PF) has gone through an even deeper process of institutional development in the last years. Formally subject to the Ministry of Justice, the PF used to have a secondary role in the investigation of relevant crimes in comparison to the state civil and military police forces, and this was also reflected in its more precarious institutional structure (Arantes 2011). Since the early 2000s, however, a radical re-orientation of the Ministry of Justice towards increased investments and the complete modernization of the PF's apparatus took place in the end of the Cardoso administration (1995-2002), and was pursued even more strongly under President Lula (2003-2010); during this period, the PF's budget and staff increased significantly (Arantes 2011; Praça and Taylor 2014). As a result, it has taken a much more prominent role in investigating corruption and organized crime through hundreds of special operations in recent years; these have contributed immensely to increasing the PF's visibility and to shaping its image in the public opinion (Arantes 2011).

Other instruments of horizontal accountability exist under the Legislative branch, such as the possibility of impeaching the head of the Executive and the establishment of parliamentary

¹⁴ Previously, internal control functions were exercised in a decentralized manner, as attributions of separate units within each ministry. Part of those functions were already somewhat centralized under the *Corregedoria Geral da União*, created in 2001, but its transformation into the present CGU in 2003 came with a wider scope of functions, more powers and a higher status under the Presidency. The emergence of the CGU thus represented an important shift towards greater efficiency and independence in internal oversight (Loureiro *et al.* 2012; Praça and Taylor 2014).

committees of inquiry (CPIs), illustrating the wide amplitude of formal instruments that Legislative bodies may use to check the Executive at all levels of government (Figueiredo 2003; Hagopian 2005). In particular, the CPIs can have an important investigative role because their scope of action is extremely broad (Figueiredo 2003; Power and Taylor 2011). At the national level, several CPIs have contributed to revealing important information on a number of corruption scandals. Another positive long-term implication of parliamentary investigations is that, since they are often under constant media scrutiny and help maintain high visibility of the scandals under their investigation, some important reforms have resulted from their findings. After the scandal involving President Collor, for instance, changes in political finance regulation and in procurement legislation were introduced; another scandal uncovered in 1993 involving malfeasance by members of the Parliamentary Budget Committee led to a deep re-structuring of the budget formulation and oversight process inside Congress (Melo 2014; Praça and Taylor 2014).

At the same time, both CPIs and impeachment procedures are essentially led by a political institution, and are consequently affected by the calculations of the actors involved and the political balance of power (Arantes 2011; Melo 2014; Pérez-Liñán 2007; Power and Taylor 2011; Taylor 2010; Taylor and Buranelli 2007). More specifically, due to the dynamics of Executive-Legislative relations, such instruments may be weakened in practice, and occupants of Executive office that manage to secure sufficient legislative support may become practically immune to sanctions through these channels (Figueiredo 2003). As an example, several scholars have argued that the impeachment of President Collor was fundamentally driven by his inability or outright refusal to manage political relations with the traditional elite represented in Congress; added to the pre-existing inclination of those politicians to turn against a president that had violated the “rules” of the political game, the public pressure that arose after the corruption scandal involving his administration made the outcome inevitable (Pereira *et al.* 2011; Pérez-Liñán 2007; Samuels 2006; da Silva 2000). Similarly, majority government coalitions may use different strategies to simply block or sabotage the work of CPIs that threaten the government’s interests. Indeed, although a few CPIs in Congress in the recent democratic period were relatively successful and uncovered important evidence regarding corruption, the general effectiveness of this instrument of accountability to the Legislative has been limited, since the majority of CPIs is either never installed or fail to produce a final report (Figueiredo 2003).

These instruments may also be used by Congress to investigate and punish its own members. Several corruption scandals involving Members of Parliament have indeed resulted in some of them facing expulsion, or resigning in fear of this more drastic type of sanction that also results in a temporary suspension of political rights (Pereira *et al.* 2011; Power 2000; Taylor and Buranelli 2007; Weyland 2005). However, episodes where legislators have cooperated to save their peers despite severe corruption accusations are not uncommon, especially when internal voting on the expulsion is made secret (“Conselho de Ética Da Câmara Absolve 59% Dos Deputados Desde a Sua Criação” 2009; Pereira *et al.* 2011)¹⁵.

Also linked to the Legislative branch are the Federal Court of Accounts (TCU), responsible for overseeing the Federal government’s finances, and the State Courts of Accounts, which oversee the use of public funds by state and municipal governments. Their legal mandate includes relatively broad audit powers and one of their main attributions is making an assessment of the accounts presented yearly by the administrative bodies (e.g. state agencies, municipal governments) under their respective jurisdictions, and making a recommendation to the Legislative to approve, approve with reservations or reject those accounts (Figueiredo 2003; Hidalgo *et al.* 2016; Melo *et al.* 2009; Speck 2011)¹⁶. The ultimate call on whether to endorse those assessments lies with the respective Legislative body, but such recommendations have a considerable weight nonetheless and, in the case of municipalities, can only be overruled by qualified majorities in the Legislative (Speck 2011). They may also imply other political costs, as politicians with accounts rejected by the Legislative become ineligible for candidacy for a period of eight years¹⁷.

Although legal sanctions remain outside the scope of action of these audit bodies, they can directly impose a number of other relevant punishments, including: (a) financial sanctions, such as fines or the repayment of lost funds by public officials considered responsible for

¹⁵ At the national level, secret voting in such cases was only abolished in December 2013 through a constitutional amendment and a change in the Congress internal bylaws (“Câmara Aprova Mudança No Regimento Pelo Voto Aberto” 2013). At the local level, each State Assembly or Municipal Council can autonomously determine whether voting for the expulsion of members is to be secret or not.

¹⁶ The Courts of Accounts’ functions were further expanded after the introduction of the Fiscal Responsibility Law in 2000, after which the courts were re-structured and modernized in order to effectively monitor compliance with the new law across the federation (Loureiro *et al.* 2009).

¹⁷ Originally, this sanction was valid for a period of five years, but this time frame was extended by amendments introduced with the “Clean Slate” bill.

irregularities, (b) blacklisting of public contractors for a period between three and five years, and (c) a ban from civil service for a period between five and eight years (Speck 2011). These wide range of political and civil sanctions that may derive from Courts of Accounts' decisions shows that they are, at least in principle, not toothless as agencies of oversight. Nevertheless, the main challenge is guaranteeing the effective enforcement of those sanctions. One problem refers to the time frame in which these courts usually manage to conclude their assessments, which can take several years, thus delaying the imposition of definitive sanctions and the activation of other accountability channels through the Justice system (Hidalgo *et al.* 2016; Melo 2014; Taylor and Buranelli 2007). Moreover, in the case of financial sanctions or ineligibility for elected office, those "convicted" by Courts of Accounts have been very successful in using litigation against their decisions before the actual Justice system to avoid facing those penalties (Hidalgo *et al.* 2016; Melo 2014; Speck 2011). Probably as a result of these shortcomings, auditing bodies such as the TCU have increasingly resorted to other types of sanctions in the last years, such as bans on contracting or from civil service, which they can unilaterally enforce (Speck 2011).

In recent years, new instruments for strengthened cooperation between the TCU and Congress have provided for significant improvement in the oversight of large government projects by those institutions. These are closely monitored by the TCU and, if irregularities are identified, the Court notifies the Budget Committee in Congress to immediately halt disbursements related to the project in question, thus preventing further damage while the irregularities can still be remedied; this new procedure has also ensued a reaction by the Executive, which now urges the TCU to speedily reassess the projects in order to get clearance for further disbursements, instead of employing strategies to delay its decisions (Speck 2011).

In formal terms, the several Courts of Accounts in Brazil have a considerable degree of functional autonomy, with independence in its internal management and secured budget allocation for their activities¹⁸. Independence is strengthened by the fact that they are headed by a collegial structure of councilors with life-long tenure and are well-protected against removal from office (Hidalgo *et al.* 2016; Melo *et al.* 2009; Speck 2011). Furthermore, they usually have a professionalized and specialized technical bureaucracy that performs the main

¹⁸ At the state level, however, there is considerable variation in terms of the share of the state budget allocated to finance the audit courts (Melo 2014; Melo *et al.* 2009).

auditing functions (Melo 2014). However, a key weakness with regards to these institutions' *de facto* independence from the political forces of the day refers to the system of appointment of councilors (Taylor and Buranelli 2007).

Several authors have highlighted that the structure of appointments allows the Executive and the Legislative to appoint members to the Courts that are politically aligned with them, who could therefore show leniency towards political allies while exercising their oversight functions; interestingly, the level of political influence actually exercised by politicians varies at the sub-national level (Figueiredo 2003; Loureiro *et al.* 2009; Melo *et al.* 2009; Paiva and Sakai 2014; Speck 2011). Indeed, some studies have corroborated these arguments by showing that the majority of councilors appointed to these courts are former politicians or relatives of politicians (Loureiro *et al.* 2009; Paiva and Sakai 2014), that councilors appointed through more politicized channels tend to be more biased in their functions than their more professionalized counterparts (Hidalgo *et al.* 2016), and that characteristics of the local political environment, such as the level of political competition, influence the performance of the courts of accounts in different states (Melo *et al.* 2009).

The Judiciary also plays an essential role in the web of accountability institutions, and has established itself as a truly independent branch in the recent democratic period, as exemplified by several occasions in which the country's higher courts have acted against the government's interest (Melo 2014; Melo and Pereira 2013; Power and Taylor 2011). This branch is also characterized by a very professionalized bureaucracy, selected on a meritocratic basis and paid competitive salaries (Melo 2014). Nevertheless, at the sub-national level, significant regional variation in terms of funding and efficiency is observed across the lower courts, which are based in the different states of the federation; at this level, courts also tend to be sometimes less autonomous than their federal counterparts, and a stronger degree of political influence by the State Executive may exist (Macaulay 2011; Melo 2014).

The functioning of the courts has become a main bottleneck for the effective exercise of horizontal accountability and rule of law in Brazil. This is partly explained by excessive formalism and outdated rules of procedure allowing seemingly endless possibilities of recourse, which in turn cause unreasonable delays until a final decision can be reached by the judges (Taylor 2011). As a result, well-off defendants, i.e. members of the political and economic elites who are able to afford more competent legal counsel are the ones who most profit from these deficiencies and often succeed in escaping punishment, which brings about

(justified) perceptions of injustice and impunity (Hagopian 2005; Melo 2014; Taylor 2010, 2011; Taylor and Buranelli 2007). This has been confirmed by surveys in the 1990s showing that only a minority of Brazilians believe that all are equal before the law (Hagopian 2005), and in later surveys we still see that most Brazilians do not trust the Justice system to effectively punish criminals (Rennó *et al.* 2010).

However, recent developments may contribute to changing this view in the country. A large judicial reform implemented since 2004 is helping reduce the inefficiency of the courts, and the creation of a National Justice Council (CNJ) has represented an important move towards increased internal oversight and rationalization of procedures across the different components of the Justice system (Macaulay 2011; Taylor 2011). At the national level, the country finally witnessed in 2010 the first criminal conviction of a sitting Congressman by the Supreme Federal Court (STF) (Praça and Taylor 2014; Taylor 2011). In 2012, the spectacle of the six-year long *Mensalão* process, with a total of 37 defendants, finally came to an end with the conviction of 25 people also by the STF, including a former Chief of Staff of President Lula and leaders of several political parties, who were then sent to jail (Melo and Pereira 2013; Praça and Taylor 2014; Zampier 2012). Most recently, the extensive investigations conducted under the Car Wash operation (*Operação Lava Jato*) have so far led to the imprisonment of dozens of politicians, public officials and executives of government contractors, many of which already face harsh first-level convictions (Richter 2016).

The Public Prosecution Service (MP) has also established itself as an independent and professionalized enforcement body, with strong powers granted under the 1988 Constitution (Melo 2014; Porto 2011; Taylor and Buranelli 2007). In an international comparative perspective, it stands out from other prosecutorial bodies due to its independence from other branches of government and its explicit role in also defending the collective interest of the society (Arantes 2011; Cavalcanti 2006). As a result, an internal culture of the MP as the ultimate “protector” of society became deeply rooted among public prosecutors, which has contributed to great initiative on their part to fight corruption and misconduct in the public administration (Arantes 2004, 2011)¹⁹.

¹⁹ This role of the MP is largely endorsed by high levels of public trust shown in several opinion surveys; at the same time, this so-called “political voluntarism” has raised concerns about this institution’s

While legal action against corrupt politicians at the federal level has rarely produced any concrete sanctions, state prosecutors seem to have been more successful in pursuing charges against mayors accused of misusing public funds, and hundreds of convictions have ensued, even already in the late 1990s (Arantes 2011; Cavalcanti 2006; Hagopian 2005). In particular, prosecutors have made ample use of a legal innovation in the Brazilian system, namely the Administrative Improbity Law, enacted in 1992, which provides for civil prosecution against politicians accused of malfeasance and can result in sanctions that include removal from office, suspension of the defendant's political rights for up to 10 years and reimbursement of financial losses to the public coffers (Arantes 2011). Nevertheless, despite this activism among prosecutors, an abysmally low number of lawsuits results in definitive sentences, mostly as a consequence of the slow Justice system (Arantes 2011; Taylor 2011; Taylor and Buranelli 2007).

In general, we see remarkable improvements in the functioning of horizontal accountability due to a series of important reforms in the last years. These have been in great part the result of a learning process motivated by multiple corruption scandals that identified key institutional weak links, but also fruit of a series of incremental changes, through the introduction of new procedures and institutional routines as well as increased interaction between different bureaucracies, which slowly set in motion a sort of endogenous dynamic of institutional change (Melo 2014; Praça and Taylor 2014). Moreover, the dynamics of the political environment in the country have produced incentives for strengthening the independence of mechanisms of checks and balances, such as the Judiciary and the MP (Melo and Pereira 2013).

A positive aspect is that Brazil's oversight and enforcement institutions are underscored by modern principles, such as meritocratic recruitment and high levels of professionalism, which should in theory contribute to their effectiveness. On the other hand, a number of challenges end up diminishing their impact. One central issue is the wide array of agencies with overlapping functions, which often results in inter-agency competition and lack of coordination across the numerous bodies that are in charge of government oversight and corruption control (Macaulay 2011; Melo 2014; Power and Taylor 2011; Taylor and Buranelli 2007). In some areas, such as criminal investigations conducted jointly by the MP and the Federal Police, with

increasing political appeal, when it is not subject to any form of political accountability (Arantes 2004, 2011).

oversight by the courts and sometimes with further support from other federal agencies, cooperation efforts have improved in the past years (Arantes 2011), but as a rule the maze of different institutions would require more fine-tuning across the respective responsibilities and scopes of action in order to bring about significant gains in efficiency to the accountability system as a whole (Power and Taylor 2011; Taylor and Buranelli 2007).

Another problem are the obstacles to these agencies' insulation from political influence: although institutions such as the Judiciary and the Prosecution services function with more independence, the mechanisms of accountability linked to the Legislative branch are still subject to the political machinations between the Executive and the Legislative, which generates a considerable degree of bias in how they operate. Finally, a key challenge to effective accountability is the internal variation observed across organizations at different levels of the public administration. While substantial advances have been observed at the national level, significant threats still exist with regards to the autonomy of institutions of horizontal accountability at the sub-national level, where the potential for politicization is higher and the level of public scrutiny by society and the media is reduced (Macaulay 2011); clear disparities across accountability institutions in the different states also show that they may be more vulnerable to the influence of local elites in some regions than in others (Melo 2014; Melo and Pereira 2013).

In sum, this broad panorama of the three dimensions of accountability reveals a generally positive picture, where mechanisms of vertical accountability, especially in the case of social accountability, appear to function relatively well and have seen improvements throughout the recent democratic period. In the case of horizontal accountability, there have also been several positive developments in recent years, which may have contributed to weakening the widespread perception of impunity. However, we see that these improvements seem to concentrate at the national level, whereas mechanisms of accountability at the sub-national level sometimes still show limited insulation from the influence of local elites, either through clientelism, the capture of local media or political influence over institutions of oversight. This is particularly relevant for the analyses conducted as part of this dissertation, which focus on the municipal level in Brazil. Against this background regarding the challenges that may hinder accountability at the local level, the empirical work developed in this study seeks to contribute to shedding more light on whether electoral, social and horizontal accountability can still be

successfully exercised in this environment, and the extent to which these mechanisms fulfill the promise of helping curb corruption in the long term.

PART I

Democracy and corruption: exploring the causal mechanisms¹

Abstract

Democracy has been highlighted, theoretically and empirically, as a main factor associated to both cross-national variation in corruption and changes in corruption patterns over time. This paper critically reviews a number of hypotheses and empirical findings that have been presented on the relationship between these two phenomena, pointing out that most studies have only scratched its surface, examining it at a high level of aggregation. An argument is made about the need for a more disaggregated perspective of democracy in order to better capture the essential causal mechanisms for an effect of democratic institutions on corruption, emphasizing the role of accountability in explaining this effect. A discussion of different dimensions of accountability is introduced as a useful framework for this purpose.

In the past two decades, corruption has become one of the main concerns in the international political agenda, in particular due to mounting evidence of its detrimental effects in terms of economic development and social welfare (Holmberg *et al.* 2009; Mauro 1997). Despite increasing efforts to tackle this problem in different contexts, recent studies have emphasized its elusive and persistent nature: on one side, assessments of countries' performance in controlling corruption show very little improvement in the last years (Kaufmann *et al.* 2009; Mungiu-Pippidi *et al.* 2013), and on the other, many evaluations point out that most anti-corruption policies promoted in developing countries have been largely ineffective (Heeks and Mathisen 2012; Heilbrunn 2004; Mungiu-Pippidi *et al.* 2014; Persson *et al.* 2013).

A few streams of the literature have addressed this generally pessimistic picture in different ways, but two in particular have attempted to understand the very foundations of this phenomenon. One of them has focused on understanding the underlying causes of corruption, and has gained greater momentum with the development of various aggregate corruption indicators at a cross-national level since the mid-1990s. This literature has mainly relied on a broad definition of corruption as the misuse or abuse of public power for private gain (Rose-

¹ An earlier version of this paper was published in December 2014 with the title "Democratization and corruption: the state of the art", as a deliverable for the research project ANTICORRP, co-funded by the European Commission within the Seventh Framework Programme (http://anticorpp.eu/wp-content/uploads/2014/12/D1.1_Part2_Democratization-and-corruption.pdf).

Ackerman 1999; Treisman 2000). In this line of research, historical, cultural, economic and political factors have been explored as potentially explaining the large variation of corruption levels observed across countries (see Treisman 2007 for a review).

Another more recent stream of research, to some extent critical of the first, has sought to conceptualize and understand not the determinants of corruption *per se*, but of its opposite. Focusing on the concept of good governance more broadly, this literature is mainly interested in explaining how a handful of societies have historically managed to achieve a governance regime characterized by a low corruption equilibrium and by the predominance of a norm of ethical universalism and impartiality in the treatment of citizens by the state (Mungiu-Pippidi 2006, 2015). These studies thus take a more societal and historical institutional approach to understanding this phenomenon, and generally define corruption in terms of a violation of those norms of impartiality and universalism in the exercise of public authority (Mungiu-Pippidi 2006, 2015; Rothstein and Teorell 2008).

In both of these streams, democracy has been highlighted, theoretically and empirically, as a main factor associated to both cross-national variation in corruption and changes in corruption patterns over time. The literature on causes of corruption has found a strong association between more advanced and consolidated democracies and lower levels of corruption (Kolstad and Wiig 2016; Pellegata 2012; Serra 2004; Treisman 2000, 2007). Works from the historical institutional literature have also discussed the role of democratization and increased political pluralism in a society's transition to a "good governance" regime characterized by a low corruption equilibrium (Mungiu-Pippidi 2006, 89; Mungiu-Pippidi *et al.* 2011, 29)².

This paper offers a critical and systematic assessment of this literature, showing that existing research has only scratched the surface of the relationship between democracy and corruption, leaving several questions partly unanswered and unexplored. In particular, most studies examine this relationship at a high level of aggregation and propose multiple, and at times contradictory, causal mechanisms through which an effect of democracy on corruption could take place. As a consequence, the extensive empirical literature has explored a number of contrasting hypotheses on the nature of this relationship, offering some insightful findings,

² At the same time, however, this line of research has also shown that some countries where a low corruption equilibrium was achieved reached this under reforms promoted by absolutist regimes, long before the introduction of representative democracy under universal suffrage (Mungiu-Pippidi 2015).

and other less conclusive ones. Moreover, at a conceptual level the debate on the link between these two phenomena is still fuzzy, and a clear discussion of what is understood by both the dependent and the independent variables is often lacking in a large part of the scholarly work on this issue. In order to address some of the gaps identified in the surveyed literature, an argument is made about the need for a more disaggregated perspective of democracy in order to better capture the essential causal mechanisms for an effect of democratic institutions on corruption, emphasizing the role of accountability in driving this effect.

The paper is structured as follows: the next section reviews the main hypotheses and findings present in the relevant literature, succeeded by a discussion of the related conceptual issues and gaps; subsequently, a conceptual framework of dimensions of accountability is presented as a tool to analyze the relationship between democracy and corruption; finally, the conclusion points to open questions and challenges ahead for future research on this topic.

Theory and empirical findings: a review of the literature

In the literature on democracy³ and corruption, a number of hypotheses have been put forth about how the former should affect the latter. This section summarizes the main theoretical arguments and the empirical evidence associated with different hypotheses, in order to provide an overview of what is known so far and what remains unclear based on existing research.

The linear effect hypotheses

The simplest hypothesis discussed in the literature claims that democracy should lead to lower corruption, or in other words, that it has a linear negative effect on corruption. This rather optimistic expectation derives from the very philosophical and normative ideals of democracy,

³ This discussion in the literature refers both to mechanisms associated to a process of democratization, thus with the establishment of certain democratic institutions in a political regime, and to other linked to a process of democratic consolidation over time. In order to maintain a uniform terminology and avoid confusion while referring to these distinct processes, the term “democracy” is used throughout the text to include both these developments.

based on principles such as equality, justice, citizenship, openness and accountability, i.e. values that are antithetical to corruption (Morris 2009). It can also be traced back to theories of democratic representation, which portray representative democracy as a system where rulers are systematically induced to act according to the interests of the citizenry (Przeworski *et al.* 1999). As corruption, by all definitions, implies actions that benefit private interests at the expense of the collective interest, true representation would in theory preclude corrupt acts. More specifically, democracy should induce representation on the part of elected officials because voters can ultimately threaten to remove them from office, should they act in ways detrimental to the public interest (Przeworski *et al.* 1999)⁴.

This hypothesis is also linked to the public choice literature, which has explored how the introduction of elections and political competition changes the incentive structure of politicians (Montinola and Jackman 2002). Under the assumption that politicians are self-interested and care about retaining office, incumbents would anticipate sanctioning by voters and therefore act in a way representative of their interests and refrain from corruption (Adsera *et al.* 2003; Andvig *et al.* 2001; Montinola and Jackman 2002; Pellegata 2012). Moreover, electoral competition would give the opposition an incentive to expose their adversaries' involvement in corruption (Andvig *et al.* 2001; Kolstad and Wiig 2016; Lederman *et al.* 2005; Rose-Ackerman 1999; Seldadyo and de Haan 2006)⁵.

Additionally, democratic freedoms and transparency should contribute to reducing the information asymmetry between voters and office holders and are thus expected to foster voters' ability to monitor the government (Kolstad and Wiig 2016; Lederman *et al.* 2005; Montinola and Jackman 2002; Sandholtz and Koetzle 2000). Finally, the adoption of mechanisms of checks and balances associated with democratic regimes is expected to restrict the ability of government officials to engage in corruption (Kolstad and Wiig 2016; Lederman *et al.* 2005; Seldadyo and de Haan 2006).

A second hypothesis on the effect of democracy on corruption claims that the duration of democratic regimes is negatively associated with corruption. This is linked to the argument

⁴ Nevertheless, several contributions in the volume edited by Przeworski *et al.* (1999) argue that the retrospective sanctioning of incumbents by voters does not always take place.

⁵ Kunicová and Mattes (2006) develop a model to show that this is not always the case, however, because competitors may collude, share rents or alternate in power.

that democracy implies, in the long term, the consolidation of democratic norms that foster a rejection of corruption by the citizenry, which should also contribute to reinforcing social monitoring of the government (Sandholtz and Koetzle 2000). Additionally, a longer experience with democracy contributes to more solid and established mechanisms of accountability that are expected to constrain corruption (Lederman *et al.* 2005; Pellegata 2012).

Although the theoretical discussion around the first hypothesis refers to the effect of democratization and implies that democratic regimes are less corrupt in comparison to non-democratic ones, recent empirical analyses have to a large extent used continuous indicators of democracy or the strength of democratic institutions, thus essentially testing for the effect of different “degrees” of democracy and comparing corruption levels not only between democracies and non-democracies, but also across what may be construed as different stages of democracy. Such studies present rather mixed findings, with results often varying according to different measures of democracy or the co-variants controlled for in the models.

Some authors fail to find a statistically significant relationship between democracy and corruption. Goldsmith (1999), for instance, examines this hypothesis for a cross-section of 34 low- and medium-income countries and finds no statistically significant effect of democracy—measured with Freedom House’s freedom indicator—on corruption, once per capita income is controlled for. Treisman (2000) also tests for the effect of democracy on corruption as part of a more comprehensive model of determinants of corruption. In cross-sectional analyses of different samples, varying from 34 to 64 countries, he similarly finds the estimated effect of democracy—both measured as a dummy variable of democratic status in 1995 and as the Political Rights index from Freedom House—to be not statistically significant when taking into account a series of other potential explanatory variables, including economic development, trade openness, legal tradition, Protestantism, colonial past, among others.

Other scholars have found some evidence of a linear negative of democracy on corruption, but this is not always robust. Paldam (2002) estimates the effect of democracy on corruption for a cross-section of almost 100 countries, taking into account the impact of cultural areas⁶ and economic factors (e.g. per capita, economic growth, income inequality) as well. He finds that

⁶ The countries in the sample are divided across six cultural groups: Western Europe, Latin America, former Communist countries, Africa, Oriental countries, and a residual group with remaining countries (Paldam 2002).

the effect of democracy is sensitive to model specification, once economic factors are controlled for. In a 2007 paper that essentially replicates his initial analysis with more recent data and much larger samples (up to 162 countries), Treisman (2007) does find significant results, but they are not robust to changes in the democracy indicator⁷. Sandholtz and Koetzle (2000) examine the same relationship for a cross-section of 50 countries, also using Freedom House's freedom indicator for democracy, and find a negative association between corruption and the strength of democratic institutions, but it is mostly significant at the 10%-confidence level, while controlling for economic freedom, average income, trade, Protestantism and age of the democratic regime. Adsera *et al.* (2003) also estimate this effect in a more sophisticated analysis, with panel data from the International Country Risk Guide (ICRG) for the period 1980-1995 (averaged for each 5-year period) for more than 100 countries, and find results not to be robust to changes in the model specification⁸. Bohara *et al.* (2004) introduce an alternative measure of democracy that emphasizes political participation over time⁹ and find stronger democracy to be significantly associated with lower corruption both in cross-section and panel analyses. Finally, Lederman *et al.* (2005) similarly use panel data to conduct their analysis, also based on the ICRG corruption index for the 1984-1999 period, and they do find a significant negative effect of democracy—measured as a binary variable—on corruption, both for ordered probit¹⁰ and ordinary least squares (OLS) estimation, while controlling for a series of cultural, policy-related and development factors and other political institutional characteristics. However, when they replicate this analysis for a cross-section of 70 countries with corruption data from the World Bank for 1999, the effect of democracy on corruption becomes insignificant.

⁷ Alternatively to Freedom House's Political Rights index, Treisman (2007) uses the Polity IV Democracy indicator.

⁸ These authors estimate the effect of democracy on corruption together with a variable called free newspaper circulation, which is measured as an interaction between the level of democratic liberties and the newspaper circulation in the respective countries (Adsera *et al.* 2003). The authors do not discuss the effect of collinearity between the two variables and how it may reduce the significance level of the effect of democracy.

⁹ Based on Dahl's discussion of the main dimensions of polyarchy, these authors create a measure of compound democracy, generated as "the cumulative sum of the product of participation and competition for each country for each year using yearly participation (turnout/population ratio) and competition (100 minus percent vote for largest party) data collected by Vanhanen (1990)", with 1950 as the starting date.

¹⁰ This method of estimation is justified by the fact that the ICRG corruption measure varies discretely, and not continuously, between 0 to 6 (Lederman *et al.* 2005).

Evidence supporting the second hypothesis, on the other hand, appears to be more robust¹¹. Sandholtz and Koetzle (2000) also test a variable of the length of democracy in their sample of countries, and find this to have a significant effect on corruption levels at the 10%-level in most of their models. Treisman (2000) finds no effect of a variable measuring years of democracy for his sample of countries, but does find a statistically significant negative effect of a dummy variable for whether the country has been a democracy uninterruptedly from 1950 to 1995. Lederman *et al.* (2005) also consider the effect of democratic stability—measured as the time of uninterrupted democratic experience—on corruption levels, and find it to have a significant negative association with corruption throughout the different models estimated. Pellegata (2012) also finds supportive evidence for the effect of cumulative democratic experience on corruption, uninterrupted or not, for a cross-section of 112 countries. Even stronger support for this hypothesis is presented by Serra (2004), who conducts an Extreme-Bounds Analysis of 16 determinants of corruption for a sample of 62 countries, and finds the negative effect of uninterrupted democracy—measured as a dummy variable for the period 1950-1995—to be one of the five determinants to remain robust to different model specifications¹².

The non-linear effect hypothesis

Other studies have proposed that democracy has a non-linear effect on corruption, whereby it first results in increasing corruption and only in the long term, at a more advanced stage, does it help reduce corruption levels. Two different sets of theoretical explanations can be associated with this idea. The first one implies that the non-linear effect is explained by a lagged negative effect of democracy on corruption, i.e. the mechanisms through which democracy helps curb corruption take a long time to be set in motion. One of the arguments behind this proposition is that, at early stages of democracy, the mechanism of electoral

¹¹ At the same time, there are a few case studies that explore some counter-examples of this thesis (e.g. India) and question the assumption that a longer experience with democracy is naturally associated with deeper democracy (von Soest 2013; Sun and Johnston 2009).

¹² Extreme Bound Analysis (EBA) is a strong test of the robustness and sensitivity of explanatory variables through a series of estimations changing the composition of the set of control variables included. If the coefficients of the variable of interest remain statistically significant in the same direction from its extreme upper bound (its highest value plus twice its standard error) to its extreme lower bound (its lowest value minus twice its standard error), the variable is considered to be robust to specification changes. In the analysis conducted by Serra (2004), the other four explanatory variables found to be robust are economic development, political instability, Protestantism and colonial heritage.

control is still weak and is not able to guarantee effective accountability between voters and politicians (Pellegata 2012). It is also argued that top-down control exercised by authoritarian governments over the economic and political spheres contributes to suppress corruption to some extent, but once these forms of control collapse after the transition to democratic rule, other mechanisms such as accountability structures and checks and balances are not yet fully consolidated, which leads to an increase of corruption in the short term (Andvig *et al.* 2001; Bäck and Hadenius 2008; Saha *et al.* 2014).

The second set of explanations, on the other hand, relies on the notion that different developments associated with democracy may have contradictory effects on corruption (Blake and Morris 2009; Rose-Ackerman 1999). This view similarly considers that the aforementioned mechanisms of control take time to consolidate and to produce the expected negative effect on corruption. At the same time, though, it challenges the argument that elections have predominantly positive effects on corruption. Instead, it claims that electoral competition and the “uncertainty” associated with it create both opportunities and incentives for politicians to subject to increasing pressures from business or to engage in electoral corruption through vote-buying, clientelism and illegal party-financing in order to maximize voter support (Blake and Morris 2009; Moran 2001; Rose-Ackerman 1999)¹³. Eventually, once solid mechanisms of checks and balances, increased transparency and a free press are in place, this may be counterbalanced and surpassed by an effect in the opposite direction (Goldsmith 1999; Kolstad and Wiig 2016; Sung 2004).

Mungiu-Pippidi (2006, 2015) also discusses the non-linear effect of democracy on corruption, but offers an alternative explanation. In her model, the transition from authoritarian, patrimonial regimes to incipient democracies rarely represents a challenge to the fundamental social norm of particularism and unequal treatment that governs these regimes. Instead, the introduction of political competition contributes to breaking the former monopoly of power of the ruling elite, only to allow other elite groups to compete for state rents¹⁴. In this situation of

¹³ The political machines described by Scott (1972) illustrate this dynamics. Similarly, documented cases of illegal party financing and clientelistic networks even in more advanced democracies (Della Porta and Mény 1997) may be seen as evidence of this.

¹⁴ The economic literature on rent-seeking also discusses how political liberalization in the beginning opens up access to rent-seeking activities and leads to an increase in the number of rent-seekers (Mohtadi and Roe 2003).

“competitive particularism”, corruption increases, and democracy only brings about a reduction of corruption once the introduction of elections is followed by normative changes that weaken social acceptability of particularism and push for stronger accountability and universalism (Mungiu-Pippidi 2006, 89–90).

The non-linear effect hypothesis was largely supported by earlier case studies on new democracies after the so-called Third Wave of democratization, showing that democratic transition was followed by an apparent increase in corruption in several countries (Geddes and Ribeiro Neto 1992; Harriss-White and White 1996; Mohtadi and Roe 2003; Moran 2001; Rock 2007; Weyland 1998; Whitehead 2002)¹⁵. Rock (2007) mentions examples from Indonesia and Thailand, where the democratic transition led to the collapse of centralized corruption networks and to increased corruption by local political actors. Sidel (1996) also discusses how local bossism appeared to have re-emerged in Thailand and the Philippines after democratization¹⁶. Finally, studies on the re-democratization of Latin American countries after military dictatorships also point to evidence of an increase in corruption, especially in the realm of political financing and clientelism (Little 1996; Whitehead 2002)¹⁷. A more recent study on the impact of democracy on corruption in Mexico also corroborates this hypothesis. Focusing on the changes that took place in the country’s political environment after the alternation in power in 2000, when the former hegemonic party Partido Revolucionario Institucional (PRI) lost the presidential elections and stepped down after more than seven decades of dominance, Morris (2009) finds that democracy has brought about new opportunities for corruption through state capture, new forms of clientelism and campaign finance.

The studies mentioned above have contributed with qualitative evidence of increased corruption in a number of younger democracies, but they largely concentrate on a short time

¹⁵ A large part of these studies makes reference to numerous corruption scandals that came to light a few years after the democratic transitions they examine (Whitehead 2002). However, due to the lack of straightforward measures of corruption, it is impossible to distinguish whether such cases represent in fact an increase of corruption or merely of its visibility, due to increased press freedom, for instance (Montinola and Jackman 2002; Weyland 1998).

¹⁶ A similar structure is described by Shleifer and Vishny (1993) as the “industrial organization” of corruption, illustrated by examples from post-Communist Russia, India and some African countries.

¹⁷ It is important to notice that several of these studies also attribute the perceived increase in corruption in those countries to economic liberalization and new opportunities for corruption linked to processes of deregulation and privatization, for instance (Whitehead 2002).

horizon after democratic transition, covering mostly the first decade after the regime change. Therefore, none of them illustrates fully the hypothesized non-linear nature of the effect of democracy on corruption as predicted by the theoretical arguments previously discussed, in the sense that no “turning point”, to the effect that corruption eventually is reduced as a consequence of stronger democratic institutions, is documented for those cases. More recent comparative studies testing this hypothesis for a larger sample of countries, on the other hand, have offered more consistent evidence of a non-linear association between democracy and corruption.

Montinola and Jackman (2002) were the first to find statistical evidence of a quadratic relationship between democracy and corruption examining two cross-sections of 51 and 66 countries, respectively, while controlling for size of government, per capita income, oil exporting countries and regional dummies. They found non-democratic states to be slightly less corrupt than partial democracies, and a negative association between levels of democracy and corruption to exist only for countries where democratic institutions are stronger. Treisman (2007) partly supports these findings, but notes that the marginal effect of advances in democracy on corruption at intermediate levels of democracy is erratic and inconsistent. Bäck and Hadenius (2008) explore a similar relationship and, while they focus on the effect of democracy—measured as a composite indicator including both Freedom House and Polity democracy indices—on state capacity, the dependent variable is also operationalized with corruption indicators¹⁸. Similarly, they find evidence for this non-linear effect for a panel of 125 countries over a period of 19 years, with a positive effect of democracy on corruption at low levels of democracy and a negative effect at high levels of democracy, while controlling for per capita income, openness to trade and British colonial heritage. Pellegata (2012) also tests and finds statistical support for this non-linear effect. Rock (2007), in turn, tests this hypothesis in panel regressions with samples between 75 and 104 countries, considering alternative measures of democracy, including one corresponding to the duration of democracy and another multiplying measures of duration and strength of democratic institutions—both based on Polity IV data. He finds a significant non-linear effect for the duration of democracy and for the combined measure. Saha *et al.* (2014) also test the non-linear effect of different components of democracy on corruption for a panel of 100 countries, and find evidence of

¹⁸ Their variable state capacity is measured by combining ICRG’s Bureaucratic Quality and Control of Corruption indices.

heterogeneous effects, with stronger effects for higher levels of democracy¹⁹. Finally, Sung (2004) tests other functional forms for this relationship in a sample of 103 countries and finds statistical evidence for a cubic relationship between democracy and corruption, although this is more difficult to interpret in substantive terms.

What is intriguing in these studies is that they have also shown that the negative effect of democracy on corruption comes about rather at higher democracy levels, thus after countries are more advanced in their democratic consolidation. This is in a way corroborated by findings of a negative association between the “age” of a democratic regime and its level of corruption, discussed above. These empirical findings thus suggest that a negative effect of democracy on corruption is observed only after a country passes a certain threshold of consolidation of its democratic institutions.

The conditional effect hypotheses

Finally, some researchers have attempted to explain the non-uniform effect of democracy on corruption by arguing that it is in fact dependent on other factors. Charron and Lapuente (2010) subscribe to the classic political economy assumption that democracy leads politicians to become more responsive to citizens’ demands, but argue that those demands vary according to the level of economic development in a society. In poorer societies, people’s preferences are likely to favor immediate consumption instead of future consumption. Therefore citizens would be more likely to demand the provision of goods through clientelistic exchanges and patronage, instead of the allocation of public resources to improve administrative capacity and the provision of public goods in the long term. In richer societies, on the other hand, citizens would be more likely to pressure rulers to invest in improving quality of government and reducing corruption, because they are more able to afford the short term costs of policies in this direction (Charron and Lapuente 2010, 451). The empirical analysis conducted by the authors for a panel of 127 countries finds evidence of the hypothesized interaction effect, with a negative effective of democracy on their indicator of

¹⁹ The authors use Freedom House’s Political Rights, Civil Liberties and Press Freedom indices to develop indicators of narrow and broad democracy, where the former includes only the Political Rights index, and the latter consists of all three dimensions. They also use the combination of Civil Liberties and Press Freedom as a measure for Rule of Law. Moreover, the effect of each measure on corruption is also tested individually.

quality of government—also a combination of ICRG’s Bureaucratic Quality and Control of Corruption, as used by Bäck and Hadenius (2008)—at lower levels of per capita income, and a positive effect at higher levels of per capita income. They also control for alternative hypotheses, including the one for an effect of the duration of democracy and for a quadratic relationship between democracy and corruption, and find no evidence to support them.

A more recent study by Jetter *et al.* (2015) also finds support for this hypothesis, although their theoretical explanation for the conditioning effect of income is a bit different: democracy opens up access to rent-seeking activities to a larger part of society; below a threshold per capita income level—estimated at about US\$ 2000 (in 2005 US\$)—, individuals have low income prospects in the productive sector, and therefore have more incentives to engage in corruption whereas at higher income levels, they can profit from sufficient income in other economic activities unrelated to the state.

Other authors have proposed that the effect of democracy on corruption is also non-linear, but varies with the degree of media freedom existent in a society²⁰. Bhattacharyya and Hodler (2012) develop a formal game-theoretical model where higher quality of democratic institutions²¹ contributes to reducing corruption, and this effect is potentialized under increased media freedom. In their model, voters choose between “good” and “bad” politicians, where the former act in people’s best interest and the latter in their own self-interest. The role of media freedom is to improve voters’ knowledge of corrupt behavior by the incumbent, allowing them to update their belief of the incumbent’s type and accordingly make their voting decision for the next term. The empirical tests to the model’s predictions, taking into account data for 126 countries from 1980 to 2008, find strong support for the complementary effect of democracy and media freedom on corruption.

Similarly, Kalenborn and Lessmann (2012) test the joint effect of democracy and press freedom on corruption levels for a large sample of countries, both in cross-section and panel regressions. They argue that both a free press and democratic elections are necessary for

²⁰ Evidence of a significant independent negative effect of press freedom on corruption have also been presented in the literature on determinants of corruption (Adsera *et al.* 2003; Brunetti and Weder 2003; Treisman 2007).

²¹ In the authors’ definition, high quality democratic institutions imply that the incumbent is likely to stay in office when supported by the people, and unlikely to stay in office without popular support (Bhattacharyya and Hodler 2012).

voters to effectively exercise accountability against corrupt politicians, as the former provides for detection of corrupt behavior and the latter for punishment by voters. In accordance with the principal-agent framework, they discuss that a free press has mainly the role of reducing the information asymmetry between voters and politicians. As Bhattacharyya and Hodler (2012), they also find empirical evidence to support the conditional effect of democracy and press freedom on corruption.

Yet another strand of research considers that the effect of democracy may be conditional on the institutional design of democratic regimes, i.e. “the devil is in the details” (Morris 2009; Persson and Tabellini 2004; Rose-Ackerman 1999). This stream in the literature is mainly concerned with explaining variation in corruption outcomes across democracies, and examines how contrasting institutional features of democracies affect corruption levels. The theoretical arguments behind these analyses usually emphasize two potential effects of certain political institutions: (a) they may influence the ability of voters to hold corrupt politicians accountable, and (b) given institutional setups may be more effective in constraining corrupt behavior by those in power. However, competing arguments are presented about how and in which direction different institutional characteristics bring about these effects (Kunicová 2006).

One of the factors discussed in the literature refers to executive-legislative relations, more specifically to the effect of presidentialist and parliamentarist forms of government. This theoretical debate is full of controversies. Theoretical models suggest that Presidentialism should be associated with less corruption, as it favors accountability by allowing voters to exercise direct control over the Executive. Additionally, its strong separation of powers would contribute to restricting opportunities for rent extraction in the government, by providing strengthened checks and balances between the Executive and the legislature (Persson and Tabellini 2004). Similarly, it is argued that the increased number of veto-players under Presidentialism should reduce discretionary power and, consequently, corrupt behavior (Kunicová 2006). At the same time, other authors argue that Presidential systems in practice often result in considerable concentration of power in the hands of the Executive, despite separation of powers, and therefore may be associated with more corruption. Together with fixed terms in office, this would in practice reduce oversight of the Executive, thus contributing to less accountability and more corruption (Kunicová and Rose-Ackerman 2005). Finally, there are claims that higher centralization of power under Parliamentarism is conducive to stronger centralized, top-down accountability, and thus less corruption (Gerring and Thacker 2004).

Empirical evidence on these arguments are quite limited and inconclusive. Persson and Tabellini analyze a sample of 60 democracies, using various estimation techniques, and find that Presidentialism to be significantly associated with higher corruption (Persson and Tabellini 2004)²². Blume *et al.* (2009) replicate their analyses on a larger sample, with 31 additional countries, and with alternative classification of presidentialist and parliamentarist regimes, and find the negative effect of Presidentialism on corruption not to be robust to alternative measures of both the dependent and the independent variable. Gerring and Thacker (2004), on the other hand, find that Parliamentarism is significantly associated with less corruption, in a sample of over 100 democracies and partial democracies and controlling mainly for per capita income, energy dependence, democracy, Protestantism and legal origin.

A second institutional factor that is considered to affect corruption outcomes is the electoral system. A number of studies has examined how different electoral rules may affect voters' ability to hold corrupt politicians accountable at the ballot box (Kunicová 2006; Persson *et al.* 2003), and three main mechanisms are discussed. Firstly, different systems may create stronger or weaker accountability links between voters and incumbents. It is argued that plurality systems, where voters vote on specific candidates, favor electoral accountability, as there is a direct accountability link between incumbents and their constituency and reelection is more dependent on performance in office (Kunicová 2006; Persson *et al.* 2003). In systems based on proportional representation (PR), on the other hand, voters vote on party lists determined by the parties, thus establishing a weaker accountability link between the voters and the individual candidates. This is most extreme in the case of closed lists, where the rank of elected candidates is decided internally by each party. For open lists, differently, it may be argued that electoral accountability not necessarily weakened as with closed lists, because voters are also able to vote for specific candidates and influence the rank of elected candidates (Kunicová 2006; Persson *et al.* 2003).

Secondly, different electoral systems may create different levels of political competition, thus affecting the range of alternatives that voters have to choose from when they are dissatisfied with incumbents. These effects are hypothesized to work in the opposite direction as the

²² Their analysis makes a distinction between "good" and "bad" democracies and finds the opposite association between Presidentialism and corruption among good democracies. This classification of countries is based on the level of constraints on the Executive and freedom of political participation, measured according to indicators from Polity IV and Freedom House (Persson and Tabellini 2004).

previous arguments, however. In this debate, proportional representation is believed to increase political competition due to reduced entry barriers to new parties and candidates, especially by inducing larger district magnitude, which in turn should contribute to electoral accountability by offering voters more options of honest candidates (Kunicová 2006; Persson *et al.* 2003).

Thirdly, electoral rules, by affecting the level of competition, impact the incentives of competitors and voters to monitor the behavior of incumbents, and thus indirectly affect the amount of information that voters may have at their disposal about how incumbents have behaved in office. Kunicová and Rose-Ackerman (2005) propose this line of thought, arguing that increased political competition may also have a negative effect on electoral accountability: in a two-party system, the opposition has more incentive to expose corruption by the incumbents because access to power depends on defeating them; under a higher number of parties, on the other hand, the chances of each individual party coming to power on its own is reduced, and the perspective of forming coalitions with the incumbent in the future may limit the incentives of exposing corrupt behavior.

The empirical evidence for the effect of different electoral rules on corruption do not offer clear-cut answers on which system is most advantageous for controlling corruption. Persson *et al.* (2003) test the effect of certain electoral rules on corruption on a sample of 80 democracies—classified according to Freedom House’s ratings—, both in cross-section and panel analyses, and find that voting on individual ballots (such as in plurality systems) and larger district magnitude are robustly associated with lower corruption. Blume *et al.* (2009) confirm these findings in their extended analysis. However, these features are not usually implemented together: plurality systems are often coupled with low-magnitude districts, and PR is mostly combined with larger district magnitude. Therefore, comprehensive electoral reforms could have counteracting effects on corruption, depending on how these characteristics are designed (Persson *et al.* 2003). Moreover, there could be interaction effects across different characteristics of electoral systems that should be explored further (Kunicová 2006).

Finally, the literature also considers the effect of federalism on corruption outcomes. Federalism has been argued to affect corruption mainly through the effects of decentralized

government, but there are claims that it may contribute to either increased or reduced corruption (Lessmann and Markwardt 2009; Treisman 2000)²³. This controversy is present in discussions on how federalism impacts the structure of public service provision. If different levels of government have some kind of monopoly over the provision of particular or complementary services, this may lead to excessive bribing by officials across those levels; if there is competition in the provision of services, on the other hand, this may drive bribery down (Shleifer and Vishny 1993). Moreover, sub-national governments could be more prone to corruption due to limited control by centralized enforcement agencies and increased pressure from local interest groups (Kunicová 2006; Lessmann and Markwardt 2009; Rose-Ackerman 1999; Treisman 2000). At the same time, however, there are claims that corruption may be lower in federal structures, as sub-national governments can be better monitored and held accountable from below due to more proximity to local constituencies (Gerring and Thacker 2004).

The empirical evidence has so far failed to clarify these controversies. Goldsmith (1999) and Treisman (2000) tests both hypotheses and finds evidence that federalism is significantly associated with more corruption. However, a later replication of these tests by Treisman (2007) in a larger sample no longer finds a statistically significant relationship. Gerring and Thacker (2004) apply the opposite concept of unitarism—conceptualized as a combination of non-federalism and unicameralism—and find it to be significantly associated with lower corruption. Adsera *et al.* (2003), on the other hand, find federalism to significantly reduce corruption. Finally, Bohara *et al.* (2004) also include federalism in their analysis, but find no significant relationship with the corruption indicator.

A note on mutual causality

As the several hypotheses and theoretical formulations discussed above illustrate, the relationship between democracy and corruption is one of high complexity. One main issue of concern in this literature is, naturally, if causality runs both ways between these two phenomena. In fact, there are several claims that corruption has detrimental effects on

²³ There is another stream in the literature that looks specifically at the effects of fiscal and administrative decentralization on corruption, but this analysis differs somewhat from the one on federalism (see Bardhan and Mookherjee 2006 for a review).

democracy. Johnston argues that competitive political processes can be undermined by corruption (2005, 28), and other authors mention corruption as one of the biggest obstacles and threats to democratic consolidation (Diamond *et al.*, 1999, 1; Mungiu-Pippidi, 2006, 86). Mungiu-Pippidi *et al.* highlight how corruption, in the form of clientelism and patronage, “subverts democracy” (2011, 12). As Warren puts it, “corruption [...] breaks the link between collective decision-making and people’s powers to influence collective decisions through speaking and voting, the very link that defines democracy” (2004, 328). In essence, “Corruption [...] violates norms of openness and equality that would seem to be central to democracy” (Sandholtz and Koetzle 2000, 31). The literature on consequences of corruption also points to its detrimental effects on democratic legitimacy (Andvig *et al.* 2001; Seligson 2002).

This issue has, indeed, important methodological implications for the empirical analysis of the effect of democracy on corruption, especially in the case of the numerous quantitative analyses that have been conducted, as their results may be biased if the danger of reverse causality is not taken into account. For this reason, many researchers have made use of different strategies to minimize this problem.

Pellegata (2012), for instance, argues in favor of a concept of democracy based on electoral competition only, arguing that indicators based on this narrower concept, in contrast to more substantial notions of democracy, limit the potential for endogeneity when examining the effect of democracy on corruption. However, this rationale is questionable, if we take into consideration that corrupt practices such as vote-buying and clientelism are likely to affect the electoral dimension of democracy as well.

Other authors have relied on statistical techniques such as the use of lagged measures of democracy (Bäck and Hadenius 2008; Treisman 2007) or estimation with instrumental variables to circumvent the issue of reverse causality. However, in particular the identification of good instruments for democratic institutions, required for the latter type of analysis, is very challenging, as highlighted by Treisman (2007). Kalenborn and Lessmann (2012), for instance, use latitude and dummy variables for Europe and Central Asia and Scandinavian legal origin as instruments for democracy, but their choice of instruments is not grounded in detail. Rock (2007) also uses latitude and share of Protestant population as instrumental variables for democracy. There are issues particularly with Protestantism, which has been pointed by the

literature as having an independent effect on corruption (Mungiu-Pippidi 2013; Treisman 2000, 2007).

Kolstad and Wiig (2016) similarly employ instrumental variable analysis in their estimation of the effect of democracy on corruption, but devise their instrument for democracy based on the democratic peace thesis, i.e. an indicator of whether a country has been at war with another democracy between 1946 and 2009. These authors find evidence of a stronger negative effect of democracy on corruption than in models where potential endogeneity is not corrected for, suggesting that the causal effect may have been underestimated in previous studies. They also find evidence consistent with heterogeneous effects of democracy on corruption, with a stronger negative impact observed for more mature (i.e. older) democracies and for countries at higher levels of democracy—measured with the Polity IV and the Freedom House indicators—, thus in line with the non-linear effect hypothesis.

These brief remarks on the complexity of the causal links between democracy and corruption aim to stress that, although the extensive comparative literature on this subject has contributed much to advancing our knowledge on the issue, there are still no absolute foolproof strategies to examine this question empirically.

Limitations of existing research and some conceptual issues

As demonstrated in the previous section, the literature on democracy and corruption offers insights on a complex relationship, where multiple hypotheses and causal pathways for an effect of the former on the latter are discussed. Despite considerable research on this topic, however, a critical assessment of the existing literature shows that a number of open questions remain regarding this relationship.

A first point of concern for the empirical analysis of this relationship is the matter of endogeneity, due to a likely feedback effect of corruption on democracy. This problem is still poorly addressed in the literature, and remains ignored in some of the studies surveyed. There are analyses that have attempted to address this problem through the employment of more sophisticated statistical techniques, but some of the solutions offered are not fully convincing. This is likely to remain one of the main challenges for the study of a causal relationship between these two phenomena.

Secondly, what seems to emerge rather strongly in the latest research is that democracy and corruption appear to be associated in a non-linear way. A number of plausible explanations for this pattern can be found in the literature, but so far the empirical evidence remains inconclusive on whether this might be due to a lagged effect of democracy, to the need for democratic regimes to cross a certain threshold of consolidation of their institutions, to the conditioning role of other factors (e.g. economic development, press freedom etc.) or to contradictory effects of different processes associated with democratization itself. Moreover, these different hypotheses allude to distinct mechanisms, associated with several dimensions of democracy, and how they should trigger a causal effect on corruption levels, but the empirical literature has largely overlooked how the multidimensional character of democracy is relevant for its impact on corruption levels, making use mainly of composite measures of democracy. Therefore, the existing empirical literature fails to shed further light into the black box of which mechanisms are in fact at work and how different dimensions of democracy concretely affect corruption outcomes in a political system.

Finally, the existing literature on democracy and corruption suffers from considerable conceptual gaps. Both democracy and corruption lack universally accepted definitions, and the respective academic literature on each of these topics includes extensive conceptual debates. In the case of democracy, for instance, definitions vary along a wide spectrum of attributes that are considered necessary to characterize a regime as democratic. A minimalist definition of democracy may require only the selection of rulers by competitive elections (Przeworski 2003); other less thin procedural conceptions establish additional criteria, such as the existence of full suffrage and minimal civil liberties (Collier and Levitsky 1997; Coppedge 2005; Dahl 1971). At the same time, there are more substantive conceptions of democracy that refer not only to procedures, but also to effective social and political outcomes of democratic regimes. Recent discussions in this direction have articulated the concept of quality of democracy, which considers to what extent the “goals” of an ideal democracy—e.g. popular sovereignty, political equality—are achieved in specific democratic regimes (Diamond and Morlino 2005). In the case of corruption, similarly, numerous issues regarding the scope of the concept—e.g. public vs. private sector corruption, legal vs. illegal corruption, universal vs. culture-specific concepts etc.—continue to be under debate (e.g. Heidenheimer and Johnston 2002; Johnston 1996, 2014; Kaufmann and Vicente 2011; Kurer 2005).

Despite such controversies, it is interesting to notice that existing scholarly work on the relationship between democracy and corruption rarely engages in a clear conceptual discussion of either. Different studies employ distinct definitions and, at times, conceptualizations of democracy and corruption are implicit at best. In particular in the case of democracy, only a handful of studies deliberately discuss the conceptual approach taken and which indicators most adequately correspond to the particular concept applied (Bohara *et al.* 2004; Pellegata 2012; Rock 2007; Saha *et al.* 2014).

It is clear that democracy is still an “essentially contested concept” (Collier and Levitsky 1997; Coppedge 2005), and a single, consistent definition is unlikely to emerge. Nevertheless, the study of how democracy impacts corruption would benefit from a more consistent conceptual framework on how this relationship functions, and this is essentially still lacking in the present literature. More importantly, this gap in current research also has considerable implications for empirical analyses on the subject, given the associated difficulties of establishing reliable and valid measures for such a blurry concept. In fact, a large part of existing research appears to be more driven by the convenience offered by the wide country and time coverage of some aggregate indicators of democracy than by conceptual clarity and concerns with validity²⁴. Moreover, in the particular case of democracy and corruption, concepts may overlap depending on how these two phenomena are conceptualized. Therefore, some essential questions regarding appropriate definitional and empirical approaches for these variables have only been superficially touched upon by the existing literature.

In order to address some of the gaps and inconsistencies discussed above, this paper proposes an alternative conceptual framework for the analysis of the relationship between democracy and corruption. This framework seeks to make a contribution to a better understanding of how democracy affects corruption by highlighting the role of different components of democracy in

²⁴ Freedom House’s Freedom Index and Polity IV’s indicator of democracy are among the most commonly used measures of democracy, and are often used as alternative indicators or combined into a single measure in specific studies, even though they lie on distinct conceptions of democracy. While Polity IV measures institutionalized democracy according to the competitiveness and openness of executive recruitment, constraint on Chief Executive and competitiveness of political participation (<http://www.systemicpeace.org/polity/polity4.htm>), Freedom House’s measure is more comprehensive and includes a civil liberties dimension. An additional problem with Freedom House’s indicator that is virtually ignored in most studies is that its Political Rights dimension includes a component called Functioning of Government, which explicitly considers the absence of corruption as a sub-indicator (<http://freedomhouse.org/report/freedom-world-2012/checklist-questions-and-guidelines>). For a critical assessment of different democracy measures, see Munck and Verkuilen (2002).

this relationship and by establishing a stronger focus on the causal mechanisms discussed in the literature for this effect. This strategy offers an essential advantage, since it takes a disaggregate approach to understanding the role of democratic institutions in the scope of this relationship, highlighting specific features of democracy that are, according to a series of theoretical arguments, expected to have a causal effect on corruption, and thus by-passes the labyrinth of broader conceptualizations of democracy. The next section elaborates the proposed framework in detail.

An alternative framework: the dimensions of accountability

The framework proposed here addresses the conceptual gaps discussed above by taking a disaggregated understanding of democracy and bringing together the various theoretical arguments presented in the literature about the mechanisms through which democracy is expected to impact corruption. This perspective has been rarely used in the existing literature (Bohara *et al.* 2004; Lederman *et al.* 2005; Saha *et al.* 2014), and it offers a few advantages to avoid some of the pitfalls that have been pointed out in the previous section. In order to introduce this framework, a brief summary of the causal mechanisms discussed in the literature is offered to highlight the dimensions of democracy that have so far been considered as plausibly affecting corruption. The objective here is not to engage in a comprehensive conceptualization of democracy *per se*, as the mechanisms discussed are certainly not exhaustive with regards to all relevant dimensions and attributes of democracy, but to offer a solid framework for those dimensions that are portrayed in the literature as most relevant for the relationship between democracy and corruption.

The most prominent arguments discussed in previous scholarly work refer to the electoral competition component of democracy, i.e. the selection of rulers through free and fair competitive elections. This dimension is discussed as having a negative effect of corruption through two main mechanisms: a) by empowering voters to sanction corrupt politicians in the next elections; and b) by motivating the opposition to expose corrupt behavior by their competitors. There is a controversy, though, regarding the effect of electoral competition on corruption, as some authors also argue that it may at the same time create certain incentives for corruption, or only allow new rent-seekers to compete for state resources.

Other claims highlight the role of democratic freedoms of information, association and expression, also linked to the emergence of a free press, in contributing to the monitoring of governments by the citizenry and the consequent reduction of corruption in society. Furthermore, the importance of institutional mechanisms of control to put a check on the government is discussed. Finally, a normative dimension of democracy is also included in this debate, with regards to the consolidation of democratic norms that crystallize the belief among the citizenry that corruption is antithetical to democracy and the common interest.

The framework proposed here interprets these four dimensions as directly or indirectly associated with the dimensions of accountability discussed in the literature on democratic accountability. In the extensive and long-living conceptual debate on democracy, accountability has in fact been highlighted by some authors as being one of democracy's most important components (Schmitter 2007) and a core feature of representative democracy (Morlino 2007, 130), being thus one of the elements that distinguishes liberal representative democracies from other types of regime (Peruzzotti and Smulovitz 2006b). Accountability implies "[...] subjecting power to the threat of sanctions; obliging it to be exercised in transparent ways; and forcing it to justify its acts" (Schedler 1999, 14). Schedler associates the first part of this definition with a dimension of enforcement, and the second and third parts with a dimension of answerability.

It is important to make a distinction between this understanding of accountability and other meanings that appear in the literature. Accountability here refers to the existence of concrete mechanisms (formally institutionalized or not) through which public office holders can be called to provide an account of their actions and be sanctioned for misconduct²⁵. This differs from other more normatively loaded conceptualizations of accountability that portray it in association with a sense of individual responsibility or moral obligation, or even as a synonym of responsiveness (Mulgan 2000). This distinction is what allows us to focus on how the existence and functioning of those mechanisms, in particular the possibility of sanctions, may affect corrupt behavior.

²⁵ Although the predominant view in the literature emphasizes sanctions as a core element of accountability (Kenney 2000; Mulgan 2000; Schedler 1999), there are also other opinions that do not consider the possibility of sanctions as a necessary attribute for an accountability relationship (Bovens 2006; Philp 2009).

Several types of accountability have been proposed in the literature (Bovens 2006), but particularly important is the contribution of O'Donnell (1994, 1999) with his discussion of the vertical and horizontal dimensions of accountability. According to O'Donnell,

“Elections, social demands that usually can be articulated without suffering state coercion, and regular coverage by the media of at least the more visible of these demands and of apparently wrongful acts of the public authorities are dimensions of [...] ‘vertical accountability’.” (1999, 30)

Horizontal accountability, on the other hand,

“[...] is the existence of state agencies that are legally enabled and empowered, and factually willing and able, to take actions that span from routine oversight to criminal sanctions or impeachment in relation to actions of omissions by other agents of the state that may be qualified as unlawful.” (O'Donnell 1999, 38)

More specifically, these agencies undertake actions

“[...] with the explicit purpose of preventing, cancelling, redressing and/or punishing actions (or eventually non-actions) by another state agency that are deemed unlawful, whether on grounds of encroachment or of corruption.” (O'Donnell 2003, 35)

The vertical dimension of accountability can also be divided into two components. When it is exercised through elections, this represents electoral accountability (O'Donnell 1999). However, O'Donnell also acknowledges the role of social demands and monitoring by the media as part of vertical accountability. Peruzzotti and Smulovitz have called this component “social accountability”, defined as “[...] a nonelectoral yet vertical mechanism of control of political authorities that rests on the actions of an array of citizens’ associations and movements and the media” (2006b, 10).

It becomes clear from this discussion that these three sources of accountability vary with regards to their degree of institutionalization and formalization. In the case of electoral and social accountability, the two components of vertical accountability, the answerability side of accountability may take a more informal nature, in the sense that the demand for transparency and justification for acts taken by the government can materialize not only through formal channels of inquiry that may be available to individuals, media and civil society, but also through popular pressure. With regards to the sanctioning aspect of accountability, on the other hand, electoral accountability involves a formal channel through which voters may

collectively “punish” a corrupt incumbent, whereas social accountability involves only reputational sanctions, which may nonetheless be of considerable weight to politicians (Grant and Keohane 2005; Philp 2009). In the case of horizontal accountability, on the other hand, both answerability and sanctioning are linked to formal institutional channels and procedures.

Taking into consideration these dimensions of accountability, it is possible to directly link them to the four main dimensions of democracy and the related causal pathways through which they are expected to affect corruption, according to the literature. In other words, accountability emerges as a kind of “common denominator” of those theoretical explanations, and we can interpret the effect of democracy on corruption as being essentially associated with the consolidation of vertical (electoral and social) and horizontal accountability²⁶. Electoral competition, for instance, is directly associated with the electoral component of vertical accountability²⁷, by providing the citizenry with a means of removing corrupt politicians from office, and by motivating the opposition to expose information that enables voters to exercise electoral accountability. Democratically safeguarded rights to expression, association and information are in turn connected to the social component of vertical accountability, as they provide for the monitoring of government by an organized civil society and a free press. Institutional mechanisms of control, in the form of checks and balances and separation of powers, for instance, are a direct manifestation of horizontal accountability, as they often institutionalize channels through which the Legislative and the Judiciary may hold rulers accountable for misconduct. Finally, democratic norms can also be associated to all dimensions of accountability more generally and indirectly, in the sense that they foster and

²⁶ Recent conceptual work on democracy has highlighted this connection between defining attributes of democracy and these dimensions of accountability. Møller and Skaaning (2010), for instance, consider electoral rights, civil liberties, and the rule of law to be the three key attributes of democracies, and claim that they can be tied together via the concept of accountability and its vertical and horizontal dimensions: “Vertical accountability entails interactions between rulers and the ruled, in particular the bottom-up control of the former by the latter via elections as well as restrictions on the top-down exercise of power via fundamental rights and the absence of judicial arbitrariness. Horizontal accountability has to do with interactions between branches of the state, in particular via the separation of powers” (Møller and Skaaning 2010, 275).

²⁷ In this discussion, electoral accountability is linked to the theoretical arguments for a negative effect of electoral competition on corruption. Empirical applications of this framework would thus require that claims regarding a positive effect of elections on corruption levels be taken into account through the operationalization of other variables capturing those mechanisms, such as clientelism and campaign financing.

enable their consolidation and effectiveness once democratic principles become embedded in social norms.

In fact, several authors in the surveyed literature on democracy and corruption implicitly or explicitly talk about mechanisms of accountability when describing the theoretical foundations of their analysis. Lederman *et al.* (2005) even operationalize it with indicators of political competition, the existence of checks and balances and freedom of the press, which correspond roughly to the three dimensions discussed above, but they do not use the vertical and horizontal accountability terminology. Bäck and Hadenius (2008) talk about the effect of democracy on corruption through dimensions of control from above and below, where the latter is a clear reference to what has been described as vertical accountability by other authors. Kolstad and Wiig (2016) also refer to democracy as a combination of horizontal and vertical accountability, but do not elaborate this in detail. This structure similarly resonates with empirical analyses stressing the role of media freedom and civil society as explanatory factors of corruption (Adsera *et al.* 2003; Brunetti and Weder 2003; Mungiu-Pippidi 2013). Mungiu-Pippidi (2015) also applies a more general model of corruption that highlights the role of legal and normative constraints as forces to control corruption, where these may also be seen as having parallel with horizontal and social accountability, respectively. Additionally, a number of studies alludes to how these different dimensions may impact, or reinforce, one another (Bhattacharyya and Hodler 2012; Grimes 2013; Kalenborn and Lessmann 2012; Mainwaring and Welna 2003; Peruzzotti and Smulovitz 2006a; Zhang 2016).

This discussion can also be directly related to long-standing theoretical approaches to the study of corruption, based on a principal-agent model. This perspective highlights the agency relationship between voters and elected officials, whereby the former, in the role of principal, entrust the latter, as agents, with power to make decisions on behalf of their interests, in order to achieve a set of preferred outcomes determined by the principal (Kitschelt *et al.* 2009; Lancaster and Montinola 1997; Rose-Ackerman 1978). Corruption thus constitutes a violation of the obligation taken by the agent to act according to the principal's interests. The general principal-agent framework has been applied by several authors in the analysis of corruption and anti-corruption policies (Klitgaard 1988; Klitgaard *et al.* 2000; Lambsdorff 2007; Rose-Ackerman 1999). From this model's perspective, strengthening accountability by reducing the information asymmetry between principal and agent is a key element to fighting corruption,

and can be achieved through active and investigative media, increased transparency in the public administration, and improved detection and punishment of corruption.

The framework discussed here aims to re-define an effect of different dimensions of democracy on corruption as corresponding mainly to an effect of electoral, social and horizontal mechanisms of accountability. This structure helps to address some conceptual and empirical gaps observed in the literature on democracy and corruption. Firstly, this approach lies on a clear conceptualization of democracy that puts accountability as a central element, in line with several contributions in the democracy literature, but at the same time providing for a nuanced discussion of its components and their respective consequences. Secondly, this framework allows us to examine more closely the individual impact of these components on corruption and to directly test the functioning of several mechanisms that are discussed in the literature, instead of only assuming their existence and effectiveness. In contrast to existing analyses based on aggregated indicators of democracy, this enables us to advance in understanding how and why democracy may affect corruption and in disentangling the effects of different mechanisms. Finally, efforts to document concrete indicators of effective electoral, social and horizontal accountability are an important contribution to innovation in the measurement of democracy and to providing more body to the concept of accountability, which has so far remained very abstract in the literature. Certainly, an attempt to develop such indicators also implies significant empirical challenges, especially for the purpose of comparative analyses, but some resources for initial endeavors in this direction already exist.

Unfortunately, it must be acknowledged that redirecting the analysis of democracy and corruption towards a focus on dimensions of accountability cannot circumvent the issue of endogeneity referred to earlier. All three dimensions of accountability can theoretically be affected by corruption in ways that jeopardize their effectiveness in producing the hypothesized negative effect on corrupt behavior. Therefore, empirical approaches making use of this framework should take that issue into account and devise strategies to minimize this obstacle for causal inference.

Conclusion

This paper has sought to demonstrate that the literature on democracy and corruption offers insights on a complex relationship, where multiple hypotheses and causal pathways for an

effect of the former on the latter are discussed. However, a solid conceptual foundation for the analysis of the potential mechanisms proposed is still missing. Insights from the literature on dimensions of accountability were discussed as potentially offering a framework to study this relationship in a more consistent way, by considering the impact of effective mechanisms of vertical (electoral and social) and horizontal accountability on corruption.

The link between accountability and corruption is less trivial than it may appear, though. Considering the different dimensions of accountability poses interesting questions to the relative relevance of each dimension in contributing to reducing corruption in a democratic system. Are all of them necessary for control of corruption, or are some more important than others? Historical case studies of older democracies where a lower level of corruption is observed show that the biggest strides to reaching a low corruption equilibrium took place before the introduction of universal suffrage (Mungiu-Pippidi *et al.* 2011; Rose and Shin 2001; Rothstein 2011), for instance, which suggests that electoral accountability may not be necessary after all.

Moreover, although the effect of electoral accountability remains a strong assumption in the literature, it still has to be examined empirically. A large part of the studies on the electoral effects of corruption scandals suggests that voters are not always likely to punish corrupt incumbents by removing them out of office, even in established democracies (see Jiménez and Caínzos 2004 for a review). At the same time, there are a few examples where voters appear to have sanctioned more strongly politicians accused of corruption (Chang *et al.* 2010; Ferraz and Finan 2008; Pereira *et al.* 2011). Therefore, the conditions under which electoral accountability is effectively exercised by voters against corrupt politicians still needs further examination. Similarly, additional research on the workings of horizontal and social accountability is required.

At the same time, some studies also suggest that these three dimensions of accountability may interact with one another (Bhattacharyya and Hodler 2012; Kalenborn and Lessmann 2012), but thorough analyses on how they may be associated are still scarce in the literature (see Mainwaring and Welna 2003 for a few contributions). Kunicová and Mattes (2006), for instance, argue that the effect of electoral competition has limitations, and that the “iron fist” of independent monitoring, either by other institutions, the press or civil society, is essential to motivate politicians to refrain from corruption. Therefore, there is much to learn about

whether the different dimensions of accountability complement or substitute each other in some cases, with regards to their impact on corruption.

Furthermore, the analysis of these issues also implies empirical challenges in the documentation of good indicators of effective accountability, as to allow the systematic analysis of its effect on corruption. This adds to the already acknowledged challenges of devising better indicators of corruption to start with.

In sum, several questions still need to be addressed for a better understanding of whether and how effective electoral, social and horizontal accountability may contribute to lowering corruption. This, of course, does not solve the larger puzzle of understanding what makes institutional mechanisms of accountability effective in the first place, but hopefully looking at the relationship between democracy and corruption from this perspective will help generate new insights for better-targeted and more effective anti-corruption policies.

PART II

Measuring political corruption from audit results: a new panel of Brazilian municipalities¹

Abstract

Comparative research on corruption has always faced challenges on how to reliably measure this phenomenon. Indicators based on perceptions of or experience with corruption are the most common approaches, but these methods have also faced criticism regarding limitations to their conceptual and measurement validity. More recently, a number of scholars have sought to develop alternative, more objective measures of corruption. Following this line of research, this paper relies on audit reports from Brazilian municipalities to construct a concrete indicator of political corruption. The data collection exploits the setup of randomized multiple audit rounds to construct a unique panel of 140 municipalities, covering five administrative terms between 1997 and 2013. A first empirical application of the data is presented, testing the potential deterrent effect of electoral accountability on future corruption levels.

Comparative research on corruption has always faced challenges on how to reliably measure this phenomenon. Given the generally obscure and secretive nature of corrupt transactions², devising ways to quantify its occurrence is no easy task and usually involves some trade-offs. A prominent approach in the last two decades has been the development of aggregate perception-based indices, such as Transparency International's Corruption Perceptions Index (CPI) and the World Bank's control of corruption measure, both publically available since the mid-1990s³. Indeed, cross-national comparative work on corruption has profited immensely from the availability of those indicators for a wide range of countries, which has contributed to an upsurge of studies in this field in recent years. However, such data sources have also encountered considerable criticism from scholars and practitioners alike: the aggregation of

¹ This article has been published online in the *European Journal on Criminal Policy and Research* in February 2016, and in print in Volume 22, Issue 3, pp. 477–498 (September 2016).

² This is naturally associated with the fact that corruption acts are most often illegal, but recent discussions have highlighted the existence of legal forms of corruption, understood as "abuses of public office or entrusted power for private gain", in explaining different patterns of corruption across countries (Kaufmann and Vicente 2011). The political influence of economic actors exercised through legal campaign financing is a good example of this (Johnston 2005).

³ These indices are largely based on the perceived frequency of corruption reported in different surveys of country experts, business people and citizens (Treisman 2007).

different sources leads to some conceptual imprecision, and the subjective nature of the indicators makes them vulnerable to other factors that may bias people's opinions on how prevalent corruption is, resulting in that they may not accurately reflect the extent to which corruption is concretely occurring (Abramo 2007; Escresa and Picci 2015; Knack 2006; Treisman 2007).

The latter argument against indicators of perceived corruption finds support in several analyses comparing them to alternative measures based on reported experience with corruption, usually drawn from surveys with citizens and business people, showing that these two types of corruption indicators indeed correlate poorly with one another in many cases (Razafindrakoto and Roubaud 2010; Seligson 2006; cf. Charron 2015)⁴. One possible explanation for this divergence is that they are actually capturing different things: experience-based surveys most often refer to direct experience with petty bribery in public services, and aggregate indicators may include perceptions of political corruption as well (Treisman 2007). On the other hand, there are claims that perception-based surveys may be considerably biased by respondents' beliefs and characteristics (Olken 2009; Razafindrakoto and Roubaud 2010). At the same time, although experience-based measures may be less affected by opinion bias, they are likely to suffer some degree of response bias and underestimate corruption, since respondents tend to be reticent in admitting their involvement in a corrupt transaction (Escresa and Picci 2015; Kraay and Murrell 2013). Moreover, responses might not be fully comparable if individuals from different countries interpret questions differently (Treisman 2007).

In face of the drawbacks associated with those two approaches, scholars have sought to develop more objective measures of corruption, which can more precisely capture the concrete occurrence of corrupt transactions and at the same time offer stronger conceptual consistency by relying on narrower conceptualizations of this phenomenon. A few efforts in this direction have been made through different and innovative sources of data, such as micro-surveys, expenditure-tracking or audit results (Brollo 2010; Escresa and Picci 2015; Ferraz and Finan 2008; Golden and Picci 2005; Olken 2007; Reinikka and Svensson 2006), but ensuring

⁴ Another study that puts into question the validity of perception-based indicators is the analysis by Olken (2009), who finds discrepancies between corruption perceptions and a more concrete measure of corruption, based on an estimate of missing expenditures in the implementation of road projects in Indonesian villages.

sufficient case coverage for large-scale comparative analyses, especially at a cross-national level, remains a fundamental challenge to such approaches. As a result, most of the studies exploring such types of indicators resort to data at the sub-national level.

This paper seeks to further contribute to this literature by presenting a novel data set of selected Brazilian municipalities. It draws on the results of a randomized federal audit program to compose an original panel of corruption levels covering up to five administrative terms during the period 1997-2013 in 140 municipalities. The data compiled aim to add to the repertoire of concrete corruption indicators in two main aspects. Firstly, it makes use of an existing and reliable source for longitudinal corruption data, which has not yet been explored in previous studies⁵. Secondly, it is based on information reflecting the occurrence of a specific type of corruption that is particularly difficult to measure empirically, namely political corruption, broadly understood as “[...] the behavior of public decision-makers where preferential treatment is provided to individuals and where narrow interests are advanced at the expense of the interests of broader segments of society” (Lambsdorff 2007, 82).

Despite its limitation to a single country and the difficulty of replication in other settings, the data described here present several advantages when contrasted to other approaches to measuring corruption previously applied. They provide a “hard” corruption indicator that is not subject to some of the biases that may affect perception-based measures and experience/victimization surveys. Moreover, by exploiting information at the sub-national level they offer great potential for comparison across a large number of units. This has important advantages relative to cross-country comparisons, as sub-national entities share important commonalities at the macro-level that would otherwise need to be controlled for in a cross-national setting. At the same time, the sub-national level in Brazil presents considerable variation across municipalities on several different dimensions that may be correlated with corruption and to a large extent are comparable to variation on a cross-national level. Finally, the randomized character of the audit program also makes it possible to avoid some potential pitfalls associated with selection bias in the sample, and the fact that all audits are conducted by the same external agency strengthens comparability across municipalities.

⁵ Ferraz and Finan (2008, 2011) and Brollo (2010) have compiled corruption indicators from audit reports originating from the same audit program in Brazil, but did not employ the same panel structure.

The article is structured as follows: the next section describes the data sources, the procedure for data collection and the resulting data set; this is followed by an application of the data set in an empirical exercise analyzing the impact of electoral accountability on future corruption levels, and the final section concludes.

Data collection and description

In 2003, the Federal Comptroller's Office (CGU) launched a large-scale program to audit the application of federal funds transferred to municipalities. Brazilian municipalities have, as a rule, very large portions of their budget financed through governmental transfers⁶. These finance a highly decentralized policy implementation system, either through so-called voluntary transfers, related to permanent federal programs in areas such as health and education and received as fixed monthly installments, or through specific co-financing grants for infra-structure and other large investment projects. Therefore, this level of the public administration gains particular relevance as a main locus for basic public service delivery in the country. In such a context, the CGU audit program emerged as a very promising initiative to complement other mechanisms of control and substantially improve monitoring of how public funds are used at the local level⁷.

The municipalities to be audited are selected through a lottery system. Since it was introduced, the program has audited around 2000 municipalities in a universe of about 5,600 Brazilian municipalities, selected in a total of 40 lottery draws. Each lottery typically selects 60 municipalities under a population threshold of 500,000 inhabitants⁸, with a pre-defined

⁶ Based on data published by the National Treasury Department (STN) for 2012, a mean of 86% of municipal revenue comes from intergovernmental transfers (from both federal and state levels), and the share of transfers from the federal government reaches 48% on average.

⁷ As a rule, the jurisdiction for external oversight regarding municipal administrations lies with state-based Courts of Accounts, which are responsible, among other things, for assessing and passing opinion on yearly financial and managerial reports submitted by each municipality. However, their effectiveness in performing this control function is often weakened by factors such as delays in the analysis of such reports, the limitation of audits to formal aspects and a considerable degree of politicization in the appointment of the Courts' members, which reduces their independence as watchdogs (Arantes *et al.* 2005; Loureiro *et al.* 2009; Weitz-Shapiro *et al.* 2015).

⁸ Earlier lottery rounds had slightly different rules, and the number of municipalities selected gradually increased from only 5 in the first edition to 26 in the second, 50 from the third to the ninth rounds, and

number of municipalities to be selected from each of the 26 states⁹, in order to keep some degree of proportionality. Once a municipality is selected, it is excluded from the pool for the three subsequent draws. The exact timing of the lottery is not previously known and is announced by the CGU only shortly prior to its taking place.

For the compilation of the panel data set described here, the data collected focus on a sub-sample of 227 municipalities that have been selected in at least two audit rounds, which offers the possibility to estimate corruption levels for those cases for at least two time periods. The time span considered includes the audits conducted until 2013, thus covering the first 38 audit rounds. Table 1 illustrates the distribution of audit rounds by year since the beginning of the program.

Table 1. Number of audits by year

<i>Year</i>	<i>Number of audits</i>
2003	7
2004	7
2005	5
2006	3
2007	3
2008	2
2009	3
2010	3
2011	2
2012	2
2013	1

As the data collection effort described was undertaken with the objective of analyzing the relationship between corruption and electoral accountability, certain conditions considered necessary for the occurrence of electoral accountability were taken into account to further

finally 60, which has been the standard since then. Also, the population threshold was 250,000 or 300,000 in earlier rounds, and was eventually increased as well.

⁹ An exception is made with regards to one group of five less populated northern states, from which a total of two municipalities from two different states is selected in each round.

delimit the sample for which the corruption indicator was ultimately compiled¹⁰: (a) at least one election for municipal office took place between audits, and (b) the mayor in power during audit 1 ran for reelection—a narrow interpretation of electoral accountability—or, when this was not the case¹¹, either a candidate from the same party, from a coalition party¹², a relative of the mayor¹³ or a member of the administration (typically a cabinet member) was presented as candidate for succession—a broader interpretation of electoral accountability. Based on these criteria, 140 possible cases were identified to be included in the data set¹⁴. Although these constitute only a small portion of all Brazilian municipalities, this final sample still preserves some main characteristics in terms of the regional distribution, population size and development levels across the municipalities considered, with only small deviations when compared to the full universe of cases (see Table 17 in Appendix A), thus making it reasonably representative of the larger population of Brazilian municipalities.

For those selected municipalities, a measure of corruption was extracted from the irregularities identified in the respective audits. Detailed audit reports describing the individual irregularities found by the auditors in each municipality are available on the CGU website¹⁵. These generally reflect the lack of formal compliance with federal regulations in the application of transferred funds, unsatisfactory quality in public service provision or the ineffectiveness of institutionalized social monitoring mechanisms. What is of particular interest for the development of a corruption indicator is the fact that the auditors often uncover cases of non-compliance that signal favoritism or even outright fraud in the use of public resources.

¹⁰ The coding of the material is a relatively cumbersome process and, due to time and resource constraints, it was not possible to collect the data for the municipalities that did not fulfill these criteria.

¹¹ In Brazil, mayors are elected for a four-year term and are allowed to run for only one consecutive term after that. They may run again only after a hiatus of four years in which another mayor has been in power.

¹² This criterion could only be applied in the case of elections until 2004, as after that no data was available on electoral coalitions.

¹³ It is not uncommon in Brazilian municipalities for local politics to be marked by the dominance of certain political dynasties that remain in power for longer periods of time. In such cases, incumbent mayors that cannot be reelected often present a relative or even their spouse as a candidate to continue their “legacy” in office.

¹⁴ A few other cases were excluded in which audit 1 took place before an election, but its results were released only shortly before or after election day, since the relevant information about corruption could not have reached voters. Cases in which the relevant election had only one registered candidate were also disregarded.

¹⁵ <http://sistemas.cgu.gov.br/relats/relatorios.php>

Therefore, such violations can be seen as reflecting actual cases of corruption involving the municipal administration, and can be used as a measure to capture meaningful variation of corruption prevalence in the different municipalities audited.

Following previous work that has used concrete indicators of corruption retrieved from the same sources (Brollo 2010; Ferraz and Finan 2008, 2011), the data constructed here are based on the coding of irregularities related to procurement fraud, diversion of public funds, and over-invoicing as corruption violations. This emphasizes instances that may be characterized as political corruption, as the cases described generally involve decisions and administrative acts at higher levels of the local administration. Indeed, in several cases the involvement of cabinet members or mayors themselves is explicitly mentioned in the reports. Also for the subject of the empirical analysis presented later, this conceptualization of corruption is considered as more relevant for the study of a deterrent effect of electoral accountability on corruption.

The coding procedure thus consisted of classifying the numerous situations described as irregularities by the auditors as pertaining (or not) to the three broader categories mentioned above. This step also used as initial reference the types of occurrences considered in the coding of corruption violations by Ferraz and Finan (2008, 2011) and Brollo (2010)¹⁶. Cases of procurement fraud include, apart from instances of evident simulation or manipulation of tenders, situations in which procurement regulations were circumvented to avoid the required competitive awarding procedures (e.g. use of less competitive procedures when more competitive ones are legally required), restrict competition among potential bidders (e.g. excessive eligibility criteria, restricted publication of the call for bids) or favor particular suppliers and service providers (e.g. award to companies that did not fulfill all tender requirements, evidence of pre-selected winning bidders). Situations in which companies owned by members of the administration or their relatives were awarded public contracts were also considered as violations in this category, since this indicates favoritism as well¹⁷.

¹⁶ Dozens of specific concrete situations exemplifying each category were identified in the coding. As they are not always explicitly related to the occurrence of corruption in the audit reports, the coding process included an inductive component as well, through which the different occurrences described by the auditors were considered as associated with corruption violations when at least part of the reports characterized them as linked to suspicions of fraud, favoritism, diversion or marked-up prices. A detailed list with all the types of occurrences considered in the classification of corruption violations can be obtained from the author.

¹⁷ Due to the conflict of interest associated with such cases, procurement legislation in Brazil (Law 8666/1993) explicitly prohibits the hiring of companies owned by public officials employed in the

Instances of diversion of funds encompass, besides obvious embezzlement by authorities, cases where the provision of purchased goods or hired services did not take place or could not be confirmed/documented, the destination of payments could not be identified (e.g. payments without documented justification, irregular withdrawals from bank accounts) or evidence of fraud in payments or in the documentation of expenditures (e.g. forged/doctored invoices or receipts) was found. Over-invoicing refers largely to cases where goods were purchased or services hired at prices above market level, or when the municipal administration paid for higher amounts than what was actually delivered.

The main corruption indicator computed for each municipality is based on a simple count¹⁸ of all individual instances of corruption identified in the audit reports, according to the classification listed above. It is important to highlight that the coding of corruption occurrences did not necessarily follow the same structure of the individual items as presented in the audit reports. This is because the documents are sometimes structured in a way that an item describing the findings actually includes information on multiple corruption instances of the same type, such as with cases of procurement fraud, where the irregularity described is often seen in several tenders conducted in a given program. Also, auditors may describe situations involving the combination of corruption situations from the different coding categories. The manipulation of procurement procedures is often associated with over-invoicing or fake invoices, for instance. In such cases, each instance was counted separately, in order to avoid measurement error and more accurately reflect the extent of corruption found in the municipality. This coding decision introduces some differences between the indicator estimated here and similar indicators used in earlier studies¹⁹.

respective hiring agency, with the understanding that this implies potential access to privileged information and favoritism. Analogously, the jurisprudence of the Federal Court of Accounts (TCU) has applied the same understanding to consider illegal the hiring of companies owned by relatives of public officials as well.

¹⁸ Previous studies (Ferraz and Finan 2008, 2011) included the share of funds linked to corruption violations in the total amount of audited funds as an alternative measure of corruption. This was also calculated for the sample considered in this study, but was not used due to reliability and measurement error concerns, given that many of the reports do not include this information while describing some of the irregularities.

¹⁹ Ferraz and Finan (2011), for instance, find the mean number of irregularities per municipality to be around 2.5, and Brollo (2010) estimates 1.8 as the mean number of violations in her sample. The data collected for the present study, on the other hand, finds the mean number of corruption violations for all audited periods at 8.4.

Naturally, one potential caveat regarding the measurement validity and reliability of an audit-based corruption indicator is related to limitations in the auditors' ability to uncover such corruption cases, and whether this systematically varies across the observations in the sample. On the one hand, it is certainly the case that auditors are not able to identify all corruption instances that may have occurred in a given administration, therefore some degree of measurement error is unavoidable in this context. On the other hand, even if the indicator presented cannot capture the full extent of corruption in the municipalities examined, two factors contribute to strengthening its validity as a means for comparison across the units in the sample. Firstly, all audits are conducted by auditors from the same external agency, managed by the federal government. Even though they are stationed at the respective state-based offices of this agency, they are recruited through a national examination procedure, thus guaranteeing a good degree of comparability in their qualifications. Secondly, the methods and resources applied by the auditors in verifying the use of federal funds in the different municipalities follow the same standards and guidelines, and do not focus only on formal compliance with federal legislation, but also include a more thorough investigation of the administrative procedures conducted in the municipality. Taken together, these factors contribute to increase our confidence in the comparability of the indicator across the different municipalities.

In order to take full advantage of the longitudinal characteristic of the data, the coding procedure takes as reference the four-year administrative terms partially or fully covered by the different audits. The resulting data set consists of a panel including five administrative periods: 1997-2000 (prior to the audit program, but partly covered by the audits in some municipalities), 2001-2004, 2005-2008, 2009-2012 and 2013-. The time periods are coded in two ways: chronologically and relative to the occurrence of audit 1, in order to take into account that the audits occurred at different points in time and with different intervals between each audit in the different municipalities. This is done as follows: the period in which audit 1 took place receives a value of 0 ($T(0)$); periods prior to this reference period are coded with negative sequential numbers going back in time, and the subsequent periods are coded also sequentially with positive values. This relative coding is used to define the sample used in the empirical application presented later.

As the reports often refer to more than one administrative term, some periods are covered by more than one audit. Therefore, for each period the corruption level calculated is the sum of

all violations associated with that period uncovered in the different audits. For each of the 140 municipalities, the corruption measure produced thus includes an estimate for at least two of the five possible periods, resulting in a total of 437 observations in the data set as a whole. Table 2 displays the distribution of these observations across the five time periods.

Table 2. Distribution of observations with audit coverage across time periods

<i>Time period</i>	<i>Narrow sample</i>		<i>Broad sample</i>	
	<i>Observations</i>	<i>% coverage</i>	<i>Observations</i>	<i>% coverage</i>
1 (1997-2000)	33	32%	53	38%
2 (2001-2004)	92	90%	129	92%
3 (2005-2008)	98	96%	134	96%
4 (2009-2012)	78	76%	107	76%
5 (2013-)	13	13%	14	10%
Total	314		437	

Two additional variables were coded from the reports, in order to capture variation in the range and temporal scope of audited programs. For each audit, a number of service orders were issued corresponding to specific federal programs and projects for which funds were transferred to the municipal administration. This information was computed in the data set for each period considered and, similarly to the corruption data, the number of service orders issued for different audit rounds but covering the same administrative period was combined. Regarding the time scope, each audit tends to focus on the two previous years, but funds related to some projects executed in earlier years are often also audited. Data on the time scope of the audits were included in the data set as the number of years in each period that was covered by any of the audits²⁰. Since each administrative period has four years, this variable ranges from 0 to 4.

These two indicators are considered essential for inferential purposes about the corruption levels observed in the different municipalities. Intuitively it is to be expected that the larger the scope of the audit work, the higher the likelihood that the auditors will find corruption violations, all else being equal. This is confirmed for the full sample across all audited periods,

²⁰ The coding for this variable was based on the time periods mentioned in association with each of the service orders covered in the reports, complemented by data from *Portal da Transparência* (<http://transparencia.gov.br/convenios/>) on co-financing grants implemented in the municipalities.

where the number of corruption violations correlates at 0.42 with the number of service orders and at 0.37 with the number of years covered by the audit in each period. In fact, in the econometric models explored later these two variables are among the most robust predictors of corruption levels, even after the inclusion of the independent variable of interest and several additional controls.

Data description

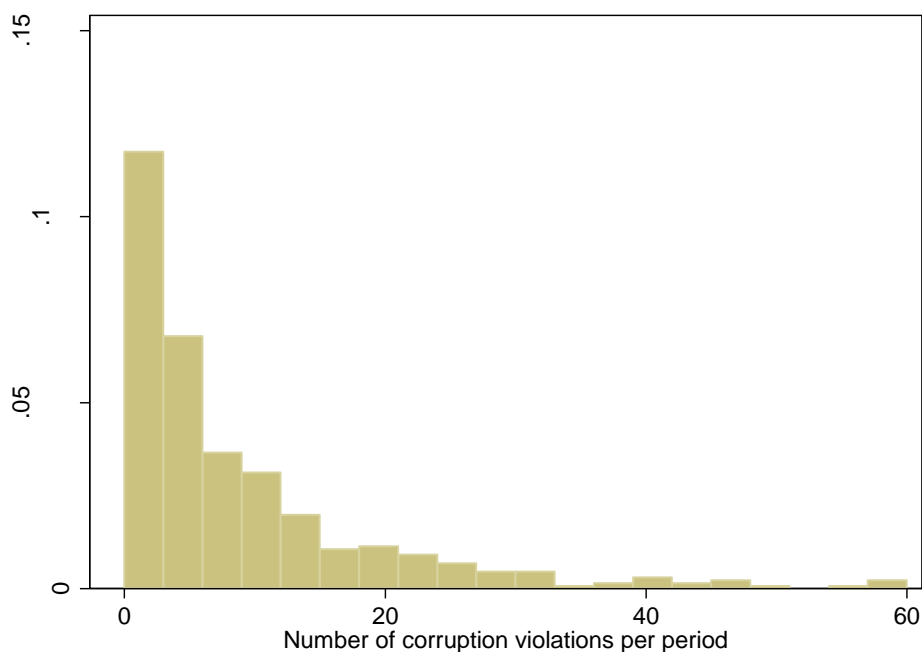
A more detailed look at the resulting indicator provides interesting insights about the prevalence of local corruption in Brazil. Considering the individual observations in the data set for each period covered by any of the audits conducted in the different municipalities, in only 71 out of the 437 (16.2%) no single corruption violation was identified. However, if we look at the different periods per municipality, all 140 municipalities had at least one corruption violation uncovered in any of the periods for which the data was collected. This indicates that political corruption, at least in the form of the three categories of violations considered for composing the indicator, is a very widespread phenomenon at this level of the public administration in Brazil.

Figure 2 illustrates the distribution of the number of corruption violations per period for all 437 observations in the data set. As we can see, there is considerable variation in the level of corruption identified, with the number of violations ranging between 0 and 60. At the same time, the indicator is heavily skewed toward lower values, with a mean of approximately 8.4 and a median of 5 violations, and about two thirds of the observations present values below the mean.

Considering the three different categories of violations included in the indicator, procurement fraud is by far the most common type, with an average 5.1 violations, whereas diversion of funds and over-invoicing have a mean of 1.8 and 1.5 violations, respectively. This certainly results from the coding decision of counting individual procurement procedures as separate corruption violations, but it also reflects a substantive characteristic of how such corruption practices occur in Brazilian municipalities: cases of procurement irregularities appear to be often associated with the replication of the same corruption “techniques” across several

tenders conducted in a given municipality²¹, revealing some sort of learning effect in corrupt administrations²². Even though instances of procurement fraud are the most numerous, in the majority of municipalities—91 of the 140 in the sample—all three types of corruption violations were found across the different periods, also reflecting that different forms of corrupt practices are usually combined in order to extract rents from the municipal administration.

Figure 2. Distribution of the number of corruption violations per period



²¹ Interestingly, this is not necessarily limited to single municipalities, and in a few cases the replicated “techniques” indicate the functioning of more organized corruption schemes extending to several municipalities or even regions. In the sample at hand, for instance, a similar practice of procurement fraud with restricted competition in the award of school transportation services, combined with blatant over-invoicing in the contract amounts, is visible in almost all the municipalities in the state of Ceará. In another state, Rio Grande do Norte, one of the audit reports makes explicit mention to a parallel investigation that uncovered an accounting firm responsible for fabricating documents of simulated tenders with fake firms in multiple municipalities. An even larger case of fraud in the purchase of ambulances, which became know nationwide in 2006 as the *Sanguessugas* (“Leeches”) scandal, was uncovered after the CGU audits found evidence of fraud in tenders favoring the same group of companies in municipalities of several states.

²² Actually, this learning effect is not restricted to cases of procurement fraud. A similar replication pattern can be observed in many cases with the other kinds of violations as well, where the same corruption practices are often used in the application of funds from different programs.

We can also observe some regional differences in the level of corruption found across municipalities. Brazil's 26 states are divided into five main regions: North (N), Northeast (NE), Southeast (SE), South (S) and Center-West (CO). This division reflects a number of essential geographic, demographic and cultural differences, in addition to different patterns in social-economic indicators (see Table 18 in Appendix A). Since several of these factors may be related to the occurrence of corruption, similar discrepancies appear when we compare the extent of corruption across all five regions, as illustrated in Table 3. The mean number of corruption violations is the highest in the Northeast—which is also the poorest and least developed region in the country—, with 11.6 violations, significantly higher than the sample mean at the 95% confidence level. The northern region also presents higher levels of corruption on average in comparison to the full sample. The Southeastern and Southern regions, which include the most economically developed areas in Brazil, display much lower numbers of corruption violations—4.0 and 3.3, respectively—, and these are statistically significantly distinct from the means observed in the North and Northeast.

Table 3. Corruption violations by region

<i>Region</i>	<i>Municipalities</i>	<i>Observations</i>	<i>Mean corruption violations</i>	<i>95% confidence interval</i>	
North	21	66	9.5	6.6	12.3
Northeast	63	197	11.6	9.9	13.4
Southeast	23	69	4.0	2.7	5.3
South	20	60	3.3	2.3	4.2
Center-West	13	45	6.0	3.9	8.1
Total	140	437	8.4	7.4	9.4

These regional corruption patterns, to the extent that they are paralleled by regional differences in the level of development as well, constitute a first indication for the construct validity of the corruption measure presented here, as the variation that it reflects across the sampled municipalities appears to corroborate a negative association between corruption levels and development indicators already identified in the cross-national literature (e.g Treisman 2000, 2007). Analyzing this more systematically, we see that the number of corruption violations identified for each observation in the data set indeed presents a statistically significant negative correlation with some development indicators, such as municipal GDP per capita, municipal HDI and literacy rate (see Table 19 in Appendix A). This

holds for both the absolute number of violations and its log transformation. If we extract the variation in the corruption indicator that is explained by the audit scope indicators, this same correlation (though somewhat weaker in the case of the municipal HDI) is still observed. Given that no alternative corruption measures for Brazilian municipalities are available for further verification of the indicator's validity, the association with these other factors in the expected direction contributes to offer additional support that the corruption measure presented here is likely capturing real corruption variation at the sub-national level in Brazil.

As reflected in this brief description of the corruption data collected, the context of Brazilian municipalities offers valuable variation for comparative analyses not only in the extent to which corruption occurs, but also in terms of other relevant factors characterizing the different regions and individual municipalities. This variation is explored in the next section in an application of the data set, where the potential role of electoral accountability as a deterrent to corrupt behavior is analyzed.

Electoral accountability as a deterrent to corruption: an empirical application

As a first application of the data set described above, the empirical exercise presented in this section seeks to estimate the effect of electoral accountability on future levels of corruption. Several studies have examined the impact of democracy on corruption (Bäck and Hadenius 2008; Lederman *et al.* 2005; Pellegata 2012; Saha 2008; Sandholtz and Koetzle 2000; Serra 2004; Sung 2004; Treisman 2000), and electoral accountability emerges as the main causal mechanism described to explain a consistently negative association between stronger and older democratic institutions and lower corruption levels: the anticipation of punishment by voters at the ballot box should induce incumbent politicians to be more responsive towards voters' interests and refrain from corruption.

Direct empirical evidence on whether we can indeed observe the predicted deterrent effect of electoral accountability on corruption is still limited, and the emerging literature presents ambiguous results. Some studies find indirect evidence supporting the mechanism described above, i.e. the potential for electoral accountability in the future seems to be associated with lower corruption in the present (Bobonis *et al.* 2012; Ferraz and Finan 2011). However, whether this effect is sustainable over time and indeed contributes to explain the apparent long-term benefits of democratization on corruption levels remains unclear, and the existing

comparative literature has so far failed to find evidence to corroborate this argument (Bobonis *et al.* 2012; Crisp *et al.* 2014).

The empirical analysis developed here attempts to further examine the impact of electoral accountability on future corruption with the newly collected data on Brazilian municipalities. The Brazilian local context offers a good setting for studying this question. Due to mandatory voting, political participation at the local level is strong and turnout rates have remained above 80% in the last municipal elections. Moreover, studies on voter behavior in Brazil have shown that voters' decisions at the local level are strongly driven by the retrospective evaluation of the incumbent mayor, with issues and political alliances at the state and national levels having limited influence in the outcome of municipal disputes (Almeida 2008; Lavareda and Telles 2011). Therefore, this particular environment appears to offer favorable conditions for the exercise of electoral accountability by voters.

The analysis presented below tests the following hypothesis:

H1: Future levels of corruption will be lower in municipalities where the previous mayor/administration involved in corruption was voted out of office.

As already implied in the formulation above, the operationalization of electoral accountability is drawn from actual electoral results and seeks to reflect whether mayors whose administration had corruption violations uncovered in audit 1 were effectively sanctioned by voters in the subsequent election. Based on electoral records from the Superior Electoral Court (TSE), for each period this variable takes a value of 0 if the previous incumbent mayor/administration was successful in the reelection bid and a value of 1 if they were defeated by a challenger. As an alternative indicator for a robustness check, the change in vote share obtained by the former mayor/administration relative to the previous election was also used.

In addition to the main independent variable of interest, the empirical analysis controls for other factors that may exercise confounding effects on corruption. Basic municipal characteristics such as municipal area, the year the municipality was founded, total population, percentage of urban population, percentage of literate population and municipal GDP are available from the Brazilian Institute of Geography and Statistics (IBGE). Alternatively, a municipal Human Development Index (HDI) published by the United Nations Development Program (UNDP) office based on 2000 and 2010 Census data is considered as a measure of

development. Its education component is used as another measure for educational attainment. The presence of radio stations in the municipality was also included as a proxy for voters' access to information (Ferraz and Finan 2011). Regional dummies are considered to control for remaining unobserved regional discrepancies²³. Other variables attempt to capture variation in the level of rents available in each municipality: percentage of intergovernmental transfers and the percentage of natural resource royalties in total municipal revenue, both calculated as period averages based on yearly data available from the STN; period averages of GDP growth (calculated based on yearly data published by IBGE); and the share of municipal employees selected exclusively through direct appointments, as a proxy for patronage through the use of public positions as resources for corruption.

Additional political factors are also taken into consideration: the mayor's margin of victory, as a proxy for the level of electoral competition, the share of seats held by the mayor's party in the local legislative chamber, whether mayors are in their first or second consecutive term, and the amount of campaign funds raised by the mayor in power in the previous election. Finally, mayor-specific characteristics such as gender and education, their level of political experience and whether they are affiliated to the party in the federal government are also considered as potential confounders.

The sample used in the analysis below considers 130 of the 140 municipalities included in the data set. This slight reduction in sample size is due to the application of an additional criterion to define the sample, namely the identification of corruption violations in audit 1, since this is assumed as a pre-condition for the occurrence of electoral accountability based on the availability of corruption information from the pre-electoral audit. Two alternative samples are used in the estimations: a "narrow" sample including 93 municipalities with mayors running for reelection, and a "broad" sample of all 130 municipalities with both incumbent mayors and successor candidates disputing the municipal Executive²⁴. The summary statistics for all the

²³ More importantly, regional fixed-effects may also capture unobserved cultural differences that cannot be accounted for by the other indicators available.

²⁴ The latter is based on a broader interpretation of electoral accountability where politicians may face a longer time horizon, and even if incumbent mayors themselves are not directly eligible for reelection, they still have a stake in their party's or political group's remaining in power. Therefore, they may still face some incentives to refrain from corruption in order to improve the chances of that group's securing another term in office.

observations available for the sample and all the variables included in the models are presented in Table 20 in Appendix A.

The analysis was conducted with a pooled cross-section regression including all available time periods after audit 1, i.e. after period T(0). The objective here is to verify whether future levels of corruption are substantially lower in municipalities that have experienced effective electoral accountability in the previous period²⁵. Table 4 presents the results for both the narrow and the broad samples with different model specifications, including distinct groups of controls. The dependent variable used in the estimation was log-transformed, given that the original variable is heavily skewed, creating difficulties for estimation through linear regression²⁶.

The coefficient for the effect of electoral accountability is mainly negative, as expected by the hypothesis formulated, with the exception of the fully specified model for the broad sample. However, in none of the models is the estimated effect statistically significant at conventional confidence levels. A verification of variance inflation factors for the electoral accountability variable shows that these estimates are not explained by inflated standard errors due to high collinearity. These results thus show no support for statistically significant lower corruption outcomes in municipalities where accountability is more effective.

²⁵ An alternative approach to estimating the effect of electoral accountability on corruption over time would be to observe the change in corruption levels across periods. However, this would be associated with endogeneity problems, because corruption in the period before the election is expected to be affected *ex ante* by the probability of electoral accountability in the future. The use of future corruption levels, i.e. corruption after the occurrence of accountability as a dependent variable has the objective of minimizing endogeneity concerns, since future corruption is posterior to the occurrence of electoral accountability and arguably does not affect it *ex ante*.

²⁶ The same models presented in Table 4 were also tested with the original (not log-transformed) values for corruption violations. However, even with robust estimation the observed residuals in all models are not normally distributed, as generally assumed in linear regression. Moreover, all models do not pass a Ramsey test for omitted variables, which indicates that there are problems with model fit regarding the functional form. In addition to linear regression, Poisson and negative binomial regression with the original variable were tested as well, given the count nature of the corruption indicator. Nevertheless, those models were also found to offer an inadequate fit, with the exception of the negative binomial model with the broad sample, the only one to indicate a correct link function for the data structure. In any case, none of these models show a statistically significant effect of electoral accountability on corruption. The results for the fully specified models are reported in Table 21 in Appendix A.

Table 4. Regression models with log number of corruption violations as dependent variable

	Narrow sample			Broad sample		
	(1)	(2)	(3)	(4)	(5)	(6)
EA previous mayor	-0.213 (0.185)	-0.137 (0.182)	-0.245 (0.232)	-0.193 (0.138)	-0.0743 (0.122)	0.226 (0.198)
Observations	95	94	94	156	154	154
R-squared	0.241	0.505	0.639	0.309	0.530	0.598
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	No	Yes	Yes	No	Yes	Yes
Mayor controls	No	No	Yes	No	No	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area, GDP growth and local radio presence. Mayor characteristics include, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support, log campaign revenue and term in office (only in models 4-6). Robust standard errors in parentheses.

In order to check the robustness of these results, an analysis of *dfbetas* was conducted for the fully specified models, and some very influential observations with *dfbetas* above one standard error were identified. However, even after their exclusion from the models, the results are not substantially altered, and electoral accountability continues to display no statistically significant effect on future corruption levels. A separate robustness check was conducted replacing the binary measure for electoral accountability with an alternative indicator, namely the change in vote share experienced by the incumbent mayor/administration in the previous period. Similarly, these models also fail to present evidence for a deterrent effect of accountability on future corruption. The results of these additional analyses are shown in Table 22 and Table 23 in Appendix A.

Based on this empirical exercise, we thus conclude that the data at hand do not offer support to the hypothesized negative effect of electoral accountability on future corruption levels. The results echo previous studies that also failed to find evidence for a sustained effect of electoral accountability on corruption over time (Bobonis *et al.* 2012; Crisp *et al.* 2014), at least in the short term.

Conclusion

This paper aims to make a contribution to the empirical literature on corruption by introducing an original panel data set of political corruption in Brazilian municipalities, based on information resulting from a randomized audit program conducted by the federal government in Brazil. The data collected explore a rich source for longitudinal corruption information to compose a concrete corruption indicator reflecting variation in the occurrence of corruption at the municipal level in Brazil. In doing so, the paper follows previous researchers that have made an effort to develop objective measures of corruption as an alternative to perception- and experience-based approaches relying on cross-national survey data, in particular by exploring novel sources of corruption information at the sub-national level.

The data collection effort described here also sought to address another central issue related to the measurement of corruption, namely the need for measurement strategies that take into account the existence of diverse forms of corruption. Together with more refined and nuanced conceptual discussions on different types of corruption, the development of corruption indicators capturing more specific manifestations of this phenomenon is an important step to generate increased inferential leverage in the analysis of a number of empirical questions that remain open in the existing literature. This matter is of the utmost relevance in the study of corruption and its causes and consequences, in particular considering that different types of corruption may be explained by different mechanisms and may have different implications for society.

In the case of the data gathered in this study, for instance, the indicator compiled focuses on the occurrence of some types of political corruption that were found to be prevalent in the context at hand. Based on the audit material used to produce the indicator, the corruption violations considered encompass situations that at least implicitly—and also often explicitly—involve decisions from occupants of higher office in the respective municipal administrations. The empirical application presented in the previous section sought to take advantage of the higher conceptual congruence between the corruption measure developed and theoretical arguments about how corruption levels are expected to be affected by incentives of political actors, in order to re-examine a recurring hypothesis on the deterrent effect of electoral accountability on corrupt behavior. Similarly, this indicator could be further explored for more sound analyses of how several other factors may impact or be impacted by corruption measured specifically in relation to the behavior of political officials.

The data presented here also illustrate some of the advantages of relying on sub-national units for comparative purposes (Snyder 2001). The richness of the data on Brazilian municipalities makes the setting explored in this analysis a favorable universe of cases for the study of the relationship between corruption and a number of other factors for which reliable indicators are available from different sources²⁷. Additionally, at the same time that the sub-national comparison helps strengthen the internal validity of the analysis by holding some factors constant at the macro-level, the considerable degree of variation found on several dimensions across the municipalities in the sample is to a large extent similar to that observed on a cross-national setting, which to a certain degree contributes to also improve the external validity of the results for other contexts and levels of analysis. Moreover, the Brazilian case exemplifies how the occurrence of corruption may vary significantly within a single country, and how the collection of corruption data at the sub-national level may be extremely informative for furthering our understanding of this phenomenon beyond the lens of cross-national comparisons alone.

Finally, this data make an effort to incorporate a temporal dimension in the measurement of corruption by exploring a data source that offers potential for more refined panel analyses in the future. Naturally, these data still have a limited time frame, and the empirical exercise developed above could only take into account a relatively short time horizon in examining the impact of one potential determinant of corruption levels. Nevertheless, even in the presence of such constraints the use of panel data in this line of research could offer stronger inferences on the causes of this phenomenon and produce new insights with policy relevance in the field of anti-corruption interventions. Even though there are important challenges regarding the availability of longitudinal data on corruption, especially for the elaboration of objective measures, further steps to explore and generate indicators allowing the analysis of corruption patterns over time are much needed to complement other existing survey-based indicators. Along with the other issues mentioned above, this remains an essential part of the agenda for future scholarly work in this field.

²⁷ The recent literature on corruption in Brazil has seen the emergence of a number of studies exploiting the availability of the audit-based corruption data in the examination of the relationship between corruption and several other factors. A few examples include studies on the impact of e-government (Vieira 2012), participatory mechanisms of oversight (Vieira 2014), increased government transfers (Brollo *et al.* 2013) and gender (Brollo and Troiano 2015) on corruption and mismanagement at the municipal level.

Accountability as a deterrent to corruption: new data from Brazilian municipalities

Abstract

Democracy has been highlighted in the comparative empirical literature as a strong determinant of corruption, explained mainly by the expected effect of different accountability mechanisms as deterrents of corrupt behavior. Although some authors point to an association between those mechanisms and lower corruption levels, there is little evidence on whether this effect subsists over time. This paper exploits the setup of multiple audit rounds conducted in randomly selected Brazilian municipalities to further assess the effect of accountability on future corruption levels. It aims to contribute to the emerging empirical literature on this field by employing a unique panel of 130 municipalities including a corruption measure compiled from the audit results and original indicators of effective electoral, social and horizontal accountability. Difference-in-differences and panel estimations are used, as well as alternative identification strategies to handle a potential endogeneity bias associated with the estimated effect of electoral accountability. In line with a few recent studies, we do not find any robust evidence that effective accountability contributes to lower future corruption.

Democracy has been highlighted in the comparative literature as an important determinant of corruption. Numerous studies have found longer democratic experience and stronger democratic institutions to be consistently associated with lower levels of corruption (Bäck and Hadenius 2008; Lederman *et al.* 2005; Pellegata 2012; Saha *et al.* 2014; Sandholtz and Koetzle 2000; Serra 2004; Sung 2004; Treisman 2000). Three main arguments are most commonly raised to explain this negative association between the two phenomena. The first and most prominent mechanism described refers to electoral incentives: as incumbents anticipate being punished by voters at the next election, they are induced to refrain from corrupt behavior in order to increase their chances of reelection. Secondly, the role of democratic freedoms of information, association and expression in enabling the monitoring of governments by society is discussed, in particular linked to the emergence of a free press. Thirdly, the role of control institutions that serve to put a check on the government is emphasized as well.

These three causal pathways can be interpreted as directly linked to different dimensions of accountability discussed in the literature on democratic accountability. Accountability implies “[...] subjecting power to the threat of sanctions; obliging it to be exercised in transparent

ways; and forcing it to justify its acts” (Schedler 1999, 14). Particularly relevant here is the discussion of O’Donnell (1994, 1999) on the vertical and horizontal dimensions of accountability. The vertical dimension has two components: when exercised through elections, it is referred to as electoral accountability (O’Donnell 1999), and when manifested through social demands and monitoring by the media, it can be considered as “social accountability” (Peruzzotti and Smulovitz 2006a). Horizontal accountability, in turn, refers to

“[...] the existence of state agencies that are legally enabled and empowered, and factually willing and able, to take actions that span from routine oversight to criminal sanctions or impeachment in relation to actions of omissions by other agents of the state that may be qualified as unlawful.” (O’Donnell 1999, 38)

Although the arguments linking these three forms of accountability to corruption are common in the literature, direct empirical evidence on whether they indeed produce the predicted effect on corruption outcomes is still limited, and the few studies that have examined this question raise doubts about their efficacy in deterring corrupt behavior. In the case of electoral accountability, the still emerging literature shows ambiguous results: while some studies find that the threat of punishment by voters is associated with less corruption in some cases (Bobonis *et al.* 2012; Ferraz and Finan 2011), others show findings in the opposite direction (Pereira *et al.* 2009) or suggest that this effect is not sustained over time (Bobonis *et al.* 2012; Crisp *et al.* 2014). For horizontal accountability, some evidence for an effect of judicial independence (Camaj 2013; Mungiu-Pippidi *et al.* 2011) and increased external control (Avis *et al.* 2016; Olken 2007; Zamboni Filho 2011) exist, but this may also be weakened in the long run if actors can adapt their behavior to less detectable forms of rent-seeking (Olken 2007). Finally, the case for a significant impact of social accountability seems to be stronger in the case of media presence and freedom (Adsera *et al.* 2003; Brunetti and Weder 2003; Ferraz and Finan 2011; Grimes 2013), but less conclusive regarding the monitoring role of civil society (Olken 2007), which may be conditional on the existence of a favorable institutional context (Grimes 2013).

Building upon this line of research, this paper further explores whether the different accountability mechanisms suggested in the literature can be empirically associated with lower levels of corruption. One of the aims proposed here is to contribute to a more systematic analysis of how these dimensions independently and simultaneously impact corruption. This

angle of analysis has been seldom explored¹ and is particularly relevant in light of arguments regarding how these dimensions of accountability may affect one another (Mainwaring and Welna 2003; Peruzzotti and Smulovitz 2006a), which raises questions about potential confounding effects of the different dimensions of accountability if they are not adequately modeled. Additionally, the analysis focuses on whether effective accountability affects *future* corruption levels, i.e. whether its deterrent effects are sustainable over time, taking as background the overarching discussions and findings about the long-term impact of democracy on corruption. For this purpose, the empirical strategy exploits the unique setup of multiple randomized audits conducted in Brazilian municipalities. Finally, the paper seeks to make an empirical contribution by further developing a concrete measure of corruption based on audit results, complemented by original data on the functioning of accountability at the local level in Brazil.

The next section of this paper reviews the relevant theoretical and empirical literature on accountability and corruption. The subsequent parts describe the methodological approach and the data collection and present the empirical results. The final section concludes and discusses the implications for further research.

Theory and previous empirical evidence

The connection between accountability and corruption can be derived mainly from the political agency literature, with a strong focus on how electoral accountability creates incentives for political actors in office to be more responsive to voters' preferences (Manin *et al.* 1999). The general theoretical framework is based on an agency relationship between voters and elected officials, whereby the former, in the role of principal, entrust the latter, as agents, with power to make decisions on behalf of their interests (Kitschelt *et al.* 2009; Lancaster and Montinola 1997; Rose-Ackerman 1978).

Under the assumptions that politicians seek to maximize their own utility and that this may often conflict with the interests of the electorate, the information asymmetry between the

¹ One recent exception is a study by Avis *et al.* (2016), which attempts to disentangle the corruption-reducing effects from elections and from non-electoral mechanisms such as external audits and legal sanctions.

two allows the former to betray their representative mandate and pursue their own interests without the knowledge of voters. Corruption thus constitutes an example of how agents can violate their obligation of acting according to the principal's interests, and in connection with this framework is commonly conceptualized as the misuse of public or entrusted power for private gain (Lambsdorff 2007; Rose-Ackerman 1999). At the same time, elections give voters an opportunity to replace corrupt incumbents, which in turn should give politicians incentives to refrain from corruption while in office in order to avoid losing their position in the next elections.

What determines agents' decisions to pursue their own interests instead of the electorate's is a calculation of the probability of facing the potential costs of corruption against the benefits that it would bring (Andvig *et al.* 2000). In all its forms, the sanctioning component of accountability mechanisms implies potential costs that may offset those benefits. In the case of electoral accountability, this entails the risk of losing office, should information on the malfeasance of agents become available to voters (Andvig *et al.* 2001; Kolstad and Wiig 2016; Lambsdorff 2007; Rose-Ackerman 1999).

The logic of a deterrent effect through social and horizontal accountability mechanisms is very similar. Although social accountability does not involve strict formal sanctions, the watchdog role of civil society and the media may involve reputational costs for politicians (Grant and Keohane 2005; Philp 2009). Moreover, social accountability agents can contribute to electoral accountability by exposing information on wrongdoing that potentially influences voter behavior, and to horizontal accountability by activating formal mechanisms and institutions to redress abuses by office holders (Peruzzotti and Smulovitz 2006a, l. 148). Horizontal accountability, in turn, can lead to formal penalties for office misconduct, ranging from administrative sanctions to criminal sentences. Thus the anticipation of costs from these different mechanisms of accountability, and an assessment of their respective probabilities, can all produce similar incentives that serve to *ex ante* discipline politicians and constrain corruption.

In recent years some empirical studies have sought to identify more concretely the effect of these mechanisms on corruption. Ferraz and Finan (2011) look specifically at the impact of electoral accountability by indirectly modeling the effect of reelection incentives on corruption in Brazilian municipalities. A common prediction derived from political agency models is that the deterrent effect of electoral accountability should be present only when reelection

incentives exist, i.e. this mechanism should not impact incumbents facing term limits. The authors compare corruption levels² across municipalities with first- and second-term mayors, as the latter cannot run for reelection in the Brazilian system. They find on average 27% less corruption under first-term mayors, and these results are robust to different specifications, indicators and alternative explanations. Pereira *et al.* (2009) conduct a similar test of this hypothesis, also using concrete measures of corruption derived from audits conducted by the State Audit Court of Pernambuco. In their sample of 184 municipalities from that state, however, they find that first-term mayors engage in more corruption than their lame-duck counterparts, contradicting the deterrence hypothesis.

Bobonis *et al.* (2012) also analyze the impact of electoral accountability indirectly, using the release of information on corruption before and after an election to capture this effect. They exploit the setting of an audit program for municipalities in Puerto Rico to verify whether incumbent mayors that have their municipality audited in the period before an election have incentives to engage in less corruption than mayors whose municipalities are audited only after the election³. The main assumption is that voters' behavior can be affected by the audit results only if they are disclosed before the election. They find that the pre-electoral disclosure of audit results is associated with 67% less corruption.

These findings relate to the static, *ex ante* effect of accountability, i.e. how the expectation of punishment by voters in the future affects incumbents' present behavior. A different approach examines the effect of electoral accountability on future levels of corruption. Although this perspective is seldom explicitly discussed in the political agency literature, we can interpret the dynamic effect of accountability on corruption as related to how political actors adjust their behavior in the face of changes in the probability of sanctioning over time. By observing the effective functioning of accountability mechanisms in the present, these actors can reassess their expectation on being sanctioned in the future. In this way, the same mechanism functions repeatedly over time, only with updates in the probability of sanctions taking place.

² Similar to a previous study (Ferraz and Finan 2008), they use original concrete corruption indicators derived from municipal audit reports.

³ The research design is made possible by the structure of the audit program, which relies on a pre-determined ordering of the municipalities that will be audited. This should allow mayors to predict whether they will be audited before the next elections or not.

The study by Bobonis *et al.* (2012) also tests whether this is the case in the Puerto Rican context. As the centralized audit program has been implemented since 1987, each municipality in the country has been audited several times, which provides the authors with sufficient longitudinal data for the analysis. Interestingly, however, they find no effect of the pre-electoral release of audit results on corruption levels in the subsequent administrative term. The authors interpret this as evidence that corrupt incumbents try to improve their reputation by refraining from corruption before an election, thus mimicking the behavior of non-corrupt politicians, in order to reap higher rents in the next term in office.

A more recent article by Crisp *et al.* (2014) also tries to capture this effect indirectly through a measure of electoral volatility. These authors postulate that electoral accountability of corrupt politicians should generate increased electoral volatility, once voters move their support to other parties. This should in turn contribute to reducing corruption in the future, as voters signal to politicians that their threat of sanctioning bad incumbents is real. They estimate a reciprocal relationship between electoral volatility and perceptions of legislative corruption for 249 elections in 74 democracies and partial democracies. The authors find that stronger perceptions of corruption do indeed lead to more electoral volatility, but no statistically significant effect of electoral volatility on future corruption levels is identified. They attribute this to two possible explanations: (a) high electoral volatility may lead the disciplining mechanism to break down, as politicians interpret it as a sign that their chances of retaining office are slim regardless of their behavior, thus having incentives to extract more rents while still in office, and (b) even if politicians do change their behavior as a result of electoral accountability, this could become visible in voters' perceptions of corruption only after some time lag.

A few other studies offer insights on the impact of the other accountability dimensions on corruption outcomes. A very influential article by Olken (2007) studies the effect of increased oversight, both in the form of top-down audits—representing a form of horizontal accountability—and bottom-up monitoring—an example of social accountability—, on corruption levels in road construction projects in Indonesian villages. In an experimental design, municipalities were randomly selected to be informed that the implementation of the project would be audited, thus increasing the probability of audit from 4% to 100%. Additionally, two experiments were conducted to assess the effect of increased social monitoring: (a) the distribution of invitations to the village's "accountability meeting", where officials give an

account of how public resources have been used, and (b) the distribution of comment forms together with the invitations, in order to allow villagers to anonymously give information on the account given at the meeting. The author found that the amount of missing expenditures was lower by 8 percentage points in villages that were informed about a future audit. At the same time, nepotism seemed to increase in jobs related to the project, suggesting that one form of corruption may have been substituted for with another. The social monitoring treatments, on the other hand, had no significant effect on missing expenditures, even though they were successful in increasing participation at the accountability meetings.

Zamboni Filho (2011) takes a similar experimental approach to estimate the impact of a randomized auditing program targeting Brazilian municipalities. In a given round of audits, 120 municipalities were selected to be informed that, among them, 30 would be audited in the following year. Given that the usual probability of a municipality's being selected in the program was between 3% and 6%, the municipalities selected for the experiment faced an audit probability around 20% higher. The audit results from the 30 municipalities that had been previously informed about a higher probability of facing an audit, i.e. the treatment group showed a significantly lower number of procurement irregularities, and a lower share of less competitive procurement procedures, in comparison to the 30 municipalities randomly selected and assigned to the control group in the same audit round.

A recent contribution by Avis *et al.* (2016) offers so far the most solid body of evidence suggesting a deterrence effect from horizontal accountability. Also using data from the Brazilian audit program to test for its long-term effect on corruption, they exploit the fact that some municipalities were audited more than once throughout the duration of the program. They find that municipalities being audited for a second time present 8% less corruption violations than those facing their first audit. Moreover, they present evidence of spillover effects, as municipalities with an audited neighbor also display lower corruption. This is conditioned on the presence of local media, though. The authors point also to an indirect effect of the audits on horizontal accountability in the form of legal sanctions, showing that the occurrence of an audit also brings about a statistically significant increase in the probability of a mayor's facing legal action due to corruption allegations. Finally, they conduct a series of simulations to disentangle the mechanisms that might be driving the reduction in corruption observed in their sample and are able to demonstrate that the lower levels of corruption appear to be driven by non-electoral mechanisms, associated either to an increase in the

perceived probability of being audited in those municipalities that have been audited in the past, or by the increased probability of facing legal consequences of corruption.

Ferraz and Finan (2011) also consider alternatively that the presence of prosecutors and radio stations may condition the effect of reelection incentives on their corruption estimates. The presence of prosecutors would imply a higher probability of legal procedures against the mayor, thus increasing the likelihood of horizontal accountability. Similarly, the presence of local radio would represent a larger probability of revealing corruption to voters. The authors predict that the difference in corruption between municipalities with first-term and second-term mayors should be lower in the presence of either prosecutors or local radio. They find evidence supporting these hypotheses, but they only examine them in separate models. They also find a significant independent negative effect of local media on corruption levels.

Finally, the comparative literature on determinants of corruption includes a number of cross-national studies providing additional evidence for the impact of factors associated with social and horizontal accountability on corruption. In the case of social accountability, different measures of press freedom (Brunetti and Weder 2003; Camaj 2013) or media penetration (e.g. free newspaper circulation (Adsera *et al.* 2003), internet access (Mungiu-Pippidi *et al.* 2011)) have been found to be significantly associated with lower corruption. Freedom of association (Camaj 2013) and higher civil society density (Mungiu-Pippidi *et al.* 2011) also appear to be linked to lower corruption, although in the latter case the effect may depend on minimal levels of political competition, transparency and press freedom (Grimes 2013). For horizontal accountability, some evidence for an association between judicial independence (Camaj 2013; Mungiu-Pippidi *et al.* 2011) and lower corruption levels exists. Brunetti and Weder (2003), on the other hand, also consider the role of rule of law as an indicator for checks and balances and solid legal institutions, but fail to find any significant connection between that variable and corruption.

The analysis conducted here seeks to contribute to this existing knowledge by examining the different accountability dimensions more systematically, accounting for potential simultaneous effects of the distinct mechanisms of accountability. Moreover, differently than some of the studies reviewed above, this study seeks to implement direct measures of accountability, which are expected to capture more closely the extent to which the mechanisms in question are really effective and not just potential. A similar effort is made regarding the indicator for the dependent variable, which follows on the contribution of other

studies employing concrete measures of corruption from audits, instead of measures of perceived corruption more commonly used in comparative contexts. The details on the empirical strategy and the data collected for the estimation procedures are described in the next section.

Empirical strategy and data collection

The empirical analysis presented here seeks to estimate the effect of accountability on future levels of corruption, taking into consideration mechanisms of electoral, social and horizontal accountability. As already discussed, the functioning of accountability mechanisms is expected to produce a deterrent effect on corrupt behavior based on the threat of sanctions. Therefore, corruption levels at a certain point in time are influenced by politicians' expectation of punishment in the future. At the same time, the concrete functioning of accountability mechanisms informs politicians about the probability of sanctioning, allowing them to update their expectations and adapt their behavior accordingly. Therefore, in contexts where accountability mechanisms are seen to be effective, we should expect political actors to update the expected probability of sanctioning upwards⁴ and refrain more from corruption in the future. Based on this dynamics, we should see a reduction in corruption over time where accountability mechanisms are more effective.

In order to test this hypothesis, this study conducts a comparative analysis with data on 130 Brazilian municipalities. The choice of setting was motivated mainly by the availability of concrete corruption data from a randomized municipal audit program conducted by the Federal government since 2003. These municipalities constitute a sub-sample of all municipalities audited until 2013, and they have all been targeted by at least two audits during the period in question. This allows us to estimate corruption levels for these municipalities for at least two time periods.

⁴ This argument would probably not apply in contexts where the probability of effective accountability is already very high. However, the analysis conducted here considers the context of a relatively young democracy where accountability mechanisms can be understood as undergoing a process of consolidation, which makes this assumption more plausible.

The selected setting offers some other advantages as well. Firstly, random selection of municipalities in the audit program eliminates selection bias in the conduction of the audits. Secondly, all audits were conducted by the same external agency, thus ensuring comparability across audit results in different municipalities. Thirdly, the possibility of employing a concrete measure of corruption is an important resource for research in this field, as an alternative to commonly used perception indicators, and it also allows us to focus on instances of political corruption, which are more relevant for the relationship between accountability and corruption⁵. Finally, the sub-national level in Brazil offers great richness of data for other relevant variables that potentially affect both corruption and the occurrence of accountability, and at the same time holds constant other country-specific characteristics that would need to be taken into account in a cross-national comparison.

The following sub-sections discuss in more detail the sample selection, data sources and the coding procedures used. All variables are described in Table 29 in Appendix B.

Sample selection and coding procedure for the corruption data

The sample used in the analysis was defined according to the availability of audit information from a randomized audit program instituted by the Brazilian Federal Comptroller's Office (CGU) since 2003⁶, and it includes municipalities selected in at least two audit rounds until 2013. For this period there are 227 municipalities that fit this criterion. However, since electoral accountability can materialize only at specific points in time, the actual sample of analysis was further restricted to those cases where it could have concretely occurred, given the following conditions: (a) at least one election for municipal office took place between audits, and (b) either the mayor in power during audit 1 ran for reelection—a narrow interpretation of electoral accountability—or, when this was not possible⁷, a successor

⁵ Broader perception-based measures often do not distinguish corruption involving higher political offices from low-ranking officials, which would not be ideal for the analysis in question, since the latter are less likely to be affected by some of the accountability mechanisms described.

⁶ For more detailed background information on the program, see Mondo (2016).

⁷ In Brazil, incumbent mayors are only allowed to run for one consecutive term at a time.

candidate was presented—a broader interpretation of electoral accountability⁸. In the latter case, the following types of candidates were identified as successors: candidates from the same party, from a coalition party⁹, a relative of the mayor¹⁰ or a member of the administration (e.g. a cabinet member).

Based on these criteria, 140 municipalities were identified as potential cases. Finally, the identification of corruption violations in audit 1 was used as an additional criterion, since it is considered as a pre-condition for some measures of accountability applied, which are conceptualized to reflect the concrete occurrence of sanctions. This results in a final sample of 93 municipalities with mayors running for reelection (referred to as the “narrow” sample) and 130 municipalities with both incumbent mayors and successor candidates disputing the municipal Executive (referred to as the “broad” sample).

For the selected municipalities, a measure of corruption was derived from irregularities described in the respective audit reports. Following previous studies that have employed corruption data from CGU municipal audit reports as well (Brollo 2010; Ferraz and Finan 2008, 2011), the indicator compiled for this study includes irregularities related to procurement fraud, diversion of public funds, and over-invoicing as instances of corruption¹¹. This emphasizes situations that may be construed as cases of political corruption, understood as “[...] the behavior of public decision-makers where preferential treatment is provided to individuals and where narrow interests are advanced at the expense of the interests of

⁸ The adoption of a broad interpretation of electoral accountability allows for a larger alternative sample of cases, but it also implies different assumptions about the incentives faced by incumbent politicians. In the narrow interpretation, only first-term mayors are subject to electoral accountability. This is associated with political agency models structured as a finite two-period game where the incumbent’s action in period 1 is followed by an election where voters decide either to keep or to replace the incumbent, and the election winner is in a “lame-duck” situation in period 2, where no accountability incentives are present (Ashworth 2012; Besley 2006; Fearon 1999). However, this model structure overlooks the possibility that politicians face a longer time horizon, and even if incumbent mayors themselves are not directly eligible for reelection, they still have a stake in their party’s or political group’s remaining in power. Therefore, it is also reasonable to consider electoral accountability from the perspective of an infinitely repeated game where an incumbent party or political group subjects a candidate to the approval of the popular vote for continuing in office (Ferejohn 1986), which corresponds to the broader interpretation of electoral accountability.

⁹ Data on coalitions are found in election records only until 2004. Therefore, for later elections this type of successor could not be identified.

¹⁰ Political “dynasties”, with several generations of politicians maintaining power in the hands of the same family for a long period, are relatively common at the local level in Brazil.

¹¹ For a more detailed description of the coding procedure, see Mondo (2016).

broader segments of society” (Lambsdorff 2007, 82). As argued earlier, an indicator capturing this type of corruption should be more appropriate for analyzing the deterrent effect of accountability.

The corruption indicator computed is based on the total number of corruption violations—pertaining to the three categories mentioned above—found by the auditors in each municipality, taking into account all audits conducted in that municipality. In order to translate the information from the repeated audits in each municipality, covering different time periods, into a panel data set, the corruption violations identified were then assigned to the respective four-year administrative term in which they took place, according to the timing information provided in the reports and complementary information from official databases on federal transfers. The resulting data set encompasses five administrative periods: 1997-2000 (prior to the audit program, but partly covered for some municipalities), 2001-2004, 2005-2008, 2009-2012 and 2013- . For each period, the corruption level measured thus corresponds to a count of all violations found associated with that period; if the period in question was covered in more than one audit, the corruption violations found in different audits were added to compose the value computed for that period.

Given that the corruption indicator collected represents only the number of violations identified in the audits, it is important to take into account that this is likely to be influenced by the scope of the audits, both in terms of the time period covered and of the range of administrative procedures analyzed by the auditors. Indeed, there is considerable variation on those two dimensions across the different audit reports. Therefore, two variables were also coded from the reports in order to account for this variation in the subsequent analyses: the total number of service orders issued for the auditors¹², also assigned to each time period included in the data set, and the number of years in each period—ranging from 0 to 4—that was covered by any of the audits.

One important reservation that must be made about the collected data regards the corruption information extracted from the subsequent audits, i.e. the second and, in some cases, third audits conducted in the sampled municipalities. According to information provided in response

¹² Each service order largely corresponds to a specific federal program or co-financing grant to be audited, for which funds were transferred to the municipality during the time considered in the audit.

to an inquiry sent to the regional CGU offices¹³, which are responsible for all on-site audit work in each state, the scope of later audits conducted in municipalities that had already been selected through the program takes into account the results of the previous audits. In particular, areas where irregularities were identified before are included in the scope of the next audit for further investigation, and the audit protocol followed in subsequent audits tends to be more thorough and detailed.

An implication of this procedure for the analysis developed here is that the results between the different audits conducted in the same municipality are not fully independent from each other. Moreover, the fact that the occurrence of irregularities in the previous audits is associated with a more careful examination by the auditors in the next audit indicates that municipalities with more corruption violations in the first audit face a higher probability of detection of new violations in subsequent audits. This could thus introduce some degree of measurement bias in the indicator. However, there are reasons to believe that the potential bias should be limited. Firstly, the correlation between the results of the first and second audits for the sample at hand is not so strong, with a Pearson coefficient of 0.25. Secondly, the temporal dimension of the panel data set employed is structured not according to the different audits that took place in each municipality, but based on four-year administrative periods. Therefore, the corruption violations found in each audit were assigned to the respective administrative periods in which they occurred, which contributes to dissipating the possible bias emerging from a higher detection probability in later audits.

Finally, it is important to reflect upon the implications of this potential bias for the inference made from the analysis. In this respect, the main possible distortion introduced in the auditing procedure would be a relative “inflation” of corruption violations in later periods¹⁴. As a consequence, it might be more difficult to observe whether corruption is decreasing as a

¹³ High-level officials from the offices in Amazonas, Bahia and Rio Grande do Sul clarified some of the criteria used to define the scope for the audits conducted under the CGU program. As a rule, the central office would occasionally establish priority areas, but an important part of the selection criteria for the programs to be audited included a risk assessment considering the amount of funds transferred, the program’s relevance and the observed propensity for irregularities. In the case of municipalities selected more than once through the program, this risk assessment factored in the results of the previous audit as well.

¹⁴ Actually, the increased likelihood of detecting corruption in later audits would allow auditors to find an amount of corruption violations closer to the reality; the real distortion would then be that the level of corruption observed in the first audit would be artificially low in comparison.

consequence of effective accountability, thus making it more difficult to confirm the deterrence hypothesis. Therefore, the main danger would be a type II error, where the analysis might fail to reject the null hypothesis. At the same time, however, it stands to reason that a more thorough audit in the second time the municipality is selected will only be able to find more corruption than previously detected if corruption has not decreased, i.e. if no deterrence effect took place. Logically, the auditors, no matter how careful they look, cannot find more corruption where there is not more corruption to be found. As a consequence, the potential bias in the estimation of the effect of interest in the analysis conducted here is arguably decreasing as the “true” deterrence effect becomes stronger. Based on all these considerations, the potential bias in the indicator should *a priori* not pose major obstacles to the empirical analysis, and its more concrete implications will be again assessed in light of the statistical results presented in the next section.

Accountability measures and data sources

The indicators for the three dimensions of accountability considered in this study seek to capture, whenever possible, the extent to which those mechanisms of accountability are effective in generating sanctions against corrupt politicians. However, there are some challenges in finding measures that adequately reflect this.

The operationalization of electoral accountability is the most straightforward and more closely reflects whether mayors whose administration was associated with corruption violations uncovered by the audits were effectively sanctioned by voters. Based on electoral records from the Superior Electoral Court (TSE), we look, for each period, whether the mayor running for reelection—in the case of the narrow sample—or presenting a successor candidate—in the case of the broad sample—in the previous period succeeded in securing a new term. If yes, electoral accountability is considered as absent, which is coded with a value of 0, and if the previous incumbent mayor/administration was voted out of office, the variable is coded with a 1. Alternatively, the change in vote share of the incumbent mayor/administration relative to the previous election was also considered.

The operationalization of horizontal accountability takes into account three main sanctioning mechanisms that Brazilian mayors can face, which are considered to be particularly relevant for their calculations of risks associated with corruption. The most clear-cut of these is the

imposition of legal sanctions. Mayors may be criminally prosecuted for malfeasance in office, when characterized either according to corruption-related provisions of the Penal Code or as so-called “responsibility crimes”, applicable to holders of Executive office at different levels of the public administration. Additionally, they may be subject to civil prosecution under the Administrative Improbity Law for actions resulting in undue private advantage or enrichment through public office, either for themselves or for other private actors. This type of civil procedure can lead to penalties such as the return of lost or diverted funds to municipal coffers and the suspension of political rights for up to 10 years, which imposes a particularly high cost on politicians’ career prospects. Secondly, mayors are required to submit a yearly report of municipal accounts and expenditures to the respective State Court of Accounts, and to the Federal Court of Accounts (TCU) in the case of federal funds received. In case these institutions reject the accounts due to grave administrative irregularities—and the decision is later ratified by the Legislative—, office holders may also become ineligible to run for political office for eight years. Finally, mayors accused of malfeasance or unlawful actions may face an impeachment procedure conducted by the local Legislative body. The consequence in this case is immediate removal from office and the replacement by the deputy mayor elected on the same ticket.

For the two latter cases, the concrete occurrence of sanctions can be more easily captured. In the case of legal penalties, on the other hand, there are fundamental empirical challenges in operationalizing the sanctioning dimension of horizontal accountability. A strict notion of a definitive sanction would require that mayors face an irreversible conviction by the courts. However, in order to successfully capture the causal mechanism of deterrence that is hypothesized, the mayor(s) in power in each administrative period would need to have observed the conviction of the former mayor prior to that period, so that the level of corruption found under their administration can reflect a behavioral response to having observed that punishment. For the Brazilian context in general, this is not a realistic expectation, given that legal procedures often take several years and this time frame is too short for a final conviction to take place in most cases. Therefore, the measure used in the analysis applies a less strict criterion, which considers horizontal accountability as occurring when the previous mayor has faced criminal or civil prosecution associated with corruption-

related charges prior to the administrative period in question¹⁵. Even though this does not necessarily reflect the occurrence of definitive legal sanctions, it is taken as a sign that horizontal accountability is more effective than where such procedures have not been initiated.

For the coding of the horizontal accountability variable, a search with the names of the mayor in power in the previous period was conducted on the databases of both the Federal and the respective State courts, in order to identify ongoing or closed criminal and civil proceedings (restricted to those related to administrative improbity) in which they appear as defendants. Complementary information from the associated court rulings was considered to ensure that the lawsuits in question were linked to corruption-related actions and not other crimes or irregularities¹⁶. In the case of Courts of Accounts, a similar search was conducted on databases of previous rulings by those instances to check whether the accounts for any of the years in the period in question had been rejected or disapproved. Impeachment cases were coded through the identification of changes in power across different data sources: municipal profiles published by the Brazilian Institute of Geography and Statistics (IBGE), mention in official documents and Court of Accounts proceedings and media reports. The final coding received a value of 1 when the mayor in the previous period was prosecuted, impeached or had the accounts rejected, otherwise the variable received a value of 0.

For social accountability, two separate indicators were considered to operationalize distinct facets of this dimension. The main limitation faced here is that none of the available measures concretely reflects the extent to which civil society and media actors effectively monitor and expose corruption in the local administration. The only available measures thus cannot completely fulfill the goal of capturing effective social accountability, but are considered as important indicators that certain conditions are in place for social accountability to be effectively exercised.

¹⁵ In cases in which only the latter part of an administrative period was covered by the audits, the coding considered the occurrence of prosecution up until that point in time. For instance, if in a given municipality in the period 2005-2008 the audits cover projects implemented only in 2007 and 2008, the time reference for the coding of horizontal accountability was the occurrence of sanctions until the year before that, i.e. 2006. For the coding of sanctions by the Courts of Accounts, the same time frame of reference was applied.

¹⁶ In the cases in which insufficient information on the substance of the case was available, the lawsuit was disregarded for coding purposes.

The first indicator reflects the presence of local media in each municipality. Following previous related studies, emphasis was put on the presence of local radio stations¹⁷. This is also a binary variable reflecting whether at least one local radio station is present in the municipality. The coding of the variable was based on information from IBGE municipal profile surveys¹⁸. The second indicator is the number of private non-profit associations registered in each municipality, as previous studies have also applied a similar measure for comparative purposes at the cross-national level (Grimes 2013; Mungiu-Pippidi *et al.* 2011) This data is available also from IBGE with estimates for 2002, 2005, 2006, 2008 and 2010. Unfortunately, a change in the methodology of classification of non-profit organizations since 2008 creates some problems for comparability across all data points. Nevertheless, data for 2006 was later adapted and published according to both methodologies. Two variables were created based on the data estimated according to the old and the new methodologies, each with two data points for each municipality. From the old methodology, the values from the 2002 and 2006 studies were input for the periods 2001-2004 and 2005-2008; from the new methodology, 2006 and 2010 data were respectively assigned to the periods 2005-2008 and 2009-2012. For the others periods, data was coded as missing.

These measures for different accountability mechanisms should, at least to some degree, capture how effectively electoral, horizontal and social accountability *de facto* work in the different municipalities. They seek to make a contribution to new approaches for measuring this characteristic of democratic regimes, in comparison to other indicators of accountability that take into consideration only the formal existence of certain institutional mechanisms. The data collected here for Brazilian municipalities indeed illustrate how actual variation on the functioning of different dimensions of accountability may emerge even in an environment where formal institutional uniformity based on national-level regulation exists.

¹⁷ This has been argued to be a more relevant source of political information than printed media in the Brazilian context, given the relatively low level of education and literacy in the country (Brollo 2010; Ferraz and Finan 2008, 2011).

¹⁸ Between 1999 and 2012, IBGE has conducted almost on a yearly basis (with the exceptions of 2003 and 2007) nation-wide surveys with municipal administrations in order to collect a wide range of data on all municipalities. The surveys from 1999, 2001, 2005, 2009 and 2012 include information on the existence of some local media outlets and were used as the main source for this variable.

Municipal and mayor-level covariates

The empirical analysis described in the next section also includes a number of other variables that may have confounding effects on corruption. Municipal characteristics such as population, percentage of urban population, literacy rate, log municipal GDP, municipal area and the year the municipality was founded were taken from IBGE data. A municipal Human Development Index (HDI) published by the United Nations Development Program (UNDP) office is also considered as an alternative measure of development, as well as its education component as another indicator for educational attainment. Finally, regional dummies are used to control for any remaining unobserved regional discrepancies¹⁹. Mayor attributes considered include gender, education, political experience and affiliation to the party in the federal government, all coded from the TSE candidate registry for all elections since 2000.

Variation in the level of rents available is also discussed in political agency models as a relevant factor influencing political actors' decision to engage in corruption. Moreover, the availability of resources is likely to impact mayors' electoral prospects as well. In order to capture that potential confounding effect, two indicators related to municipal finances were used: the percentage of intergovernmental transfers²⁰ and the percentage of natural resource royalties²¹ in total municipal revenue, both calculated as period averages based on yearly data available from the National Treasury Department (STN). Additionally, the level of GDP growth experienced in the period is also considered as a relevant control variable potentially affecting the level of rents. Period averages were calculated based on yearly data published by IBGE. Finally, the share of municipal employees selected exclusively through direct appointments²² is

¹⁹ Brazil is divided into five main regions: North (N), Northeast (NE), Southeast (SE), South (S) and Center-West (CO). This division reflects a number of essential geographic and demographic differences, and also different patterns in social-economic indicators, but may capture cultural differences as well.

²⁰ Additional resources in the form of intergovernmental transfers are mentioned in the literature as impacting both the electoral performance of incumbents and the level of corruption observed in Brazilian municipalities (Brollo *et al.* 2013; Litschig and Morrison 2010).

²¹ The impact of natural resources has also been considered in some cross-national analyses of determinants of corruption (Montinola and Jackman 2002; Treisman 2000, 2007). Moreover, anecdotal evidence suggest that cases of corruption are common in oil-producing municipalities in Brazil (Caselli and Michaels 2009).

²² As a general rule, public positions in the local bureaucracy in Brazil are filled by competitive examinations. Management and advisory positions, however, may be filled through direct appointments. In such cases, appointees are either existing public employees selected through the examinations, who then receive a sort of promotion, or individuals from outside the bureaucracy. The

taken as an additional proxy for the use of public positions as sources of rents and/or political support. This reflects patronage and clientelistic practices that may be associated with corruption. The data for this variable were calculated based on information extracted from IBGE municipal profiles in 2004, 2008, 2012 and 2013, where information on the number of employees in the municipal administration is presented.

Political factors that potentially reduce the constraints to corrupt behavior were also taken into consideration. The level of electoral competition, for instance, may be negatively related to corruption: mayors who face more competition should be more inclined to refrain from corruption, since the risk of losing office in the next elections is higher. For each period, this was operationalized as the margin of victory obtained by the mayor in power in the previous election. Legislative support, calculated as the share of seats held by the mayor's party in the local legislative chamber, was considered as an indicator of other political constraints that may impact corrupt behavior. Whether mayors are in their first or second consecutive term was also considered in the analysis, as political agency models often predict that term-limited mayors should have fewer incentives to refrain from corruption than first-term mayors²³. Additionally, the amount of campaign funds²⁴ raised by the mayor in power in the previous election is taken into account, as political finance may influence corruption by creating more pressure on politicians to repay campaign donations with political favors when in power (Blake and Morris 2009; Moran 2001; Rose-Ackerman 1999). At the same time, campaign funds have been shown as positively associated with the reelection chances of candidates in Brazil (Jucá *et al.* 2016; Rennó Jr. 2008), and are likely to affect voter behavior by increasing the visibility of candidates.

data used here considers only the latter case. Office holders usually have considerable discretion in creating and allocating such positions, and this is illustrated by the considerable variation observed across Brazilian municipalities: although the mean share of exclusively appointed officials is around 10%, this figure may extend to more than 70% in some cases.

²³ For the narrow sample, it is not necessary to account for this effect, since this difference is already captured by the electoral accountability indicator itself (except in the few cases where there was a change in power). In the case of the broad sample, the assumption is that this lame-duck effect does not fully apply, but incumbents may still face stronger incentives when they are themselves running for reelection.

²⁴ This information is originally available from TSE, but the data used in the analysis were extracted from project *As Claras* (<http://www.asclaras.org.br/@index.php>), maintained by the non-profit organization *Transparência Brasil*, which presents the TSE data in a structured and searchable format for all candidates in national and local elections from 2002 to 2012.

Estimation procedures and results

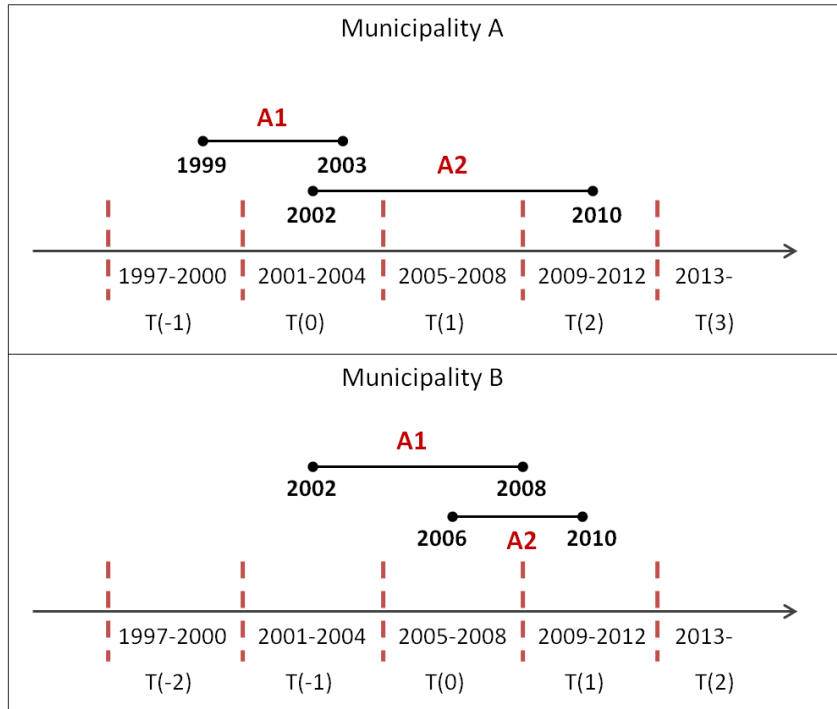
Data description

The sample used in the analysis presents substantial variation in all the main variables of interest. This is true also for the additional control variables, especially the municipal characteristics that define some contextual aspects related to the cases at hand. This constitutes another advantage of working with a sample of Brazilian municipalities, because the within-country diversity offers a setting that to a large extent mimics cross-national variation, therefore offering considerable potential for external validity of the findings and applicability beyond the sub-national level. Table 24 in Appendix B shows the summary statistics for all the variables considered in the analysis.

The data set compiled is structured as a panel of 140 municipalities and five four-year time periods corresponding to different consecutive administrative terms. This amounts to a total of 700 observations. However, since there isn't complete audit coverage of all these periods in all municipalities, Table 24 refers only to the observations for which there is corruption data, i.e. the periods that were at least partly covered by any of the audits conducted in the respective municipalities. Nevertheless, the statistical analyses focus only on the observations for the 130 municipalities in which corruption violations were found in audit 1.

In addition to the chronological coding of the time periods, another variable was created to identify each period relative to the occurrence of audit 1 in each municipality, as the audits occurred at different points in time and with different intervals in the different municipalities. For this variable, the period in which audit 1 took place receives a value of 0 ($T(0)$) with subsequent periods after that coded sequentially with positive values (e.g. $T(1)$, $T(2)$). Similarly, periods prior to $T(0)$ are coded with negative numbers going back in time (e.g. $T(-1)$, $T(-2)$). Figure 3 illustrates this structure with examples from two municipalities. As already mentioned in the previous section, for the periods in which different audits overlap, the corruption estimate includes data from both audits. This relative coding is used later to describe the time periods of interest in the estimation procedures applied.

Figure 3. Timing of audits in the data set period structure



Note: The red vertical dotted lines represent the period divisions. The periods at the bottom of each diagram indicate the relative coding of each period with regards to the occurrence of audit 1 (A1). The black lines indicate the temporal scope of audit 1 (A1) and audit 2 (A2), with the year on the left-hand side representing the beginning of the period covered by the audit, and the end of each line depicting the year in which the respective audit took place, and therefore the end of its temporal scope.

The estimation procedures discussed below aim to test a set of hypotheses associated with the expectation of a reduction in corruption over time following the successful functioning of accountability. For the different accountability mechanisms under analysis, we can formulate them as follows:

H1: Municipalities where the previous mayor/administration involved in corruption was voted out of office should experience a reduction in corruption levels.

H2: Municipalities where the previous mayor involved in corruption has faced legal (civil or criminal prosecution), administrative (municipal accounts rejected by Court of Accounts) or political (impeachment) sanctions should experience a reduction in corruption levels.

H3a: Municipalities with stronger media presence should experience a reduction in corruption levels.

H3b: Municipalities with higher civil society density should experience a reduction in corruption levels.

In order to test these hypotheses, a difference-in-differences strategy and panel estimations with fixed- and random-effects were applied. These are complemented with additional estimation procedures to circumvent endogeneity problems and to verify the sensitivity and robustness of the results. These procedures are described in the sub-sections below.

Difference-in-differences models

The difference-in-differences estimation was applied with a reduced data set comprising only two periods for each municipality. For this method, the binary accountability variables were considered as “treatments”, and each time period was assigned a before and after treatment status. The period before treatment corresponds to T(0) in the panel data set, i.e. the administrative period in which audit 1 took place. The period after treatment corresponds to T(1), i.e. the first administrative period after that.

The treatment status was assigned according to the occurrence of electoral and horizontal accountability until T(1)²⁵, and the presence of local radio stations at the beginning of T(1) as the main indicator of social accountability. Since the second indicator of social accountability, civil society density, is a continuous variable, it is not possible to apply the treatment logic to this indicator, so it was included in some specifications as a municipal covariate²⁶. All models were estimated for the narrow and broad samples. The analysis was conducted with ordinary least squares (OLS) regression, and the estimated effect for an interaction term between the time period and the treatment assignment is considered as indicating the average treatment effect of the respective accountability treatments.

²⁵ In the case of electoral accountability, the treatment variable indicates its occurrence for the municipalities in which the incumbent mayor/administration was voted out of office in the election occurring at the end of T(0). In the case of horizontal accountability, the treatment variable considers sanctions observed until before the period in T(1) that was actually covered by audit 2.

²⁶ In order to keep the comparability of this indicator over time, the two measures available based on different methodologies were tested separately in the models. Since they individually provide information only for two periods in the panel data set, the inclusion of this variable leads to a reduction in the sample size.

Table 5. Difference-in-differences models with log number of corruption violations as dependent variable (narrow sample)

	(1)	(2)	(3)	(4)	(5)	(6)
After treatment period	-0.0581 (0.273)	0.0839 (0.335)	-0.0440 (0.292)	-0.119 (0.293)	-0.200 (0.389)	-0.0343 (0.298)
EA treatment	-0.0922 (0.165)	-0.0392 (0.180)	-0.0611 (0.179)	-0.0393 (0.193)	-0.0140 (0.234)	-0.0371 (0.186)
EA treatment x after	-0.252 (0.236)	-0.257 (0.324)	-0.360 (0.248)	-0.327 (0.260)	-0.126 (0.355)	-0.394 (0.258)
HA treatment	-0.183 (0.143)	-0.115 (0.150)	-0.380** (0.189)	-0.389* (0.200)	-0.252 (0.233)	-0.397** (0.195)
HA treatment x after	0.175 (0.214)	-0.0868 (0.293)	0.366 (0.247)	0.423* (0.249)	0.261 (0.375)	0.373 (0.253)
Radio treatment	-0.0356 (0.154)	0.00895 (0.182)	0.0643 (0.196)	0.0358 (0.193)	0.266 (0.249)	0.0838 (0.203)
Radio treatment x after	0.0481 (0.227)	0.0254 (0.322)	-0.0716 (0.258)	0.00554 (0.259)	-0.291 (0.390)	-0.103 (0.271)
Log number CSOs (old)		-0.118 (0.126)			-0.158 (0.178)	
Log number CSOs (new)			-0.0752 (0.117)			-0.0797 (0.118)
Constant	13.21 (8.372)	2.213 (10.19)	21.03** (10.31)	18.91* (10.01)	14.00 (11.60)	21.69** (10.39)
Observations	165	120	120	123	78	119
R-squared	0.592	0.607	0.679	0.680	0.728	0.680
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2, and 3). Robust standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 6. Difference-in-differences models with log number of corruption violations as dependent variable (broad sample)

	(1)	(2)	(3)	(4)	(5)	(6)
After treatment period	-0.252 (0.245)	-0.146 (0.273)	-0.336 (0.290)	-0.326 (0.304)	-0.387 (0.340)	-0.346 (0.293)
EA treatment	-0.0286 (0.141)	0.00768 (0.153)	-0.0105 (0.171)	-0.00194 (0.181)	-0.0181 (0.217)	-0.0162 (0.178)
EA treatment x after	-0.0502 (0.256)	-0.185 (0.314)	0.107 (0.274)	0.102 (0.289)	0.0849 (0.367)	0.134 (0.282)
HA treatment	-0.174 (0.135)	-0.139 (0.142)	-0.320* (0.167)	-0.347** (0.171)	-0.323 (0.200)	-0.323* (0.170)
HA treatment x after	0.201 (0.194)	0.0203 (0.231)	0.354 (0.218)	0.371* (0.222)	0.325 (0.284)	0.359 (0.220)
Radio treatment	-0.0604 (0.139)	-0.0445 (0.152)	-0.102 (0.185)	-0.0887 (0.179)	-0.160 (0.218)	-0.0981 (0.188)
Radio treatment x after	-0.0695 (0.211)	-0.0469 (0.265)	-0.153 (0.228)	-0.0535 (0.235)	0.0251 (0.316)	-0.146 (0.234)
Log number CSOs (old)		-0.109 (0.113)			-0.210 (0.134)	
Log number CSOs (new)			-0.194** (0.0965)			-0.195** (0.0956)
Constant	-0.248 (6.348)	-6.719 (7.841)	8.407 (7.406)	5.605 (7.262)	3.980 (10.44)	8.165 (7.415)
Observations	244	182	174	178	116	173
R-squared	0.497	0.535	0.652	0.618	0.703	0.652
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2 and 3). Robust standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

As the corruption indicator taken as dependent variable for the analysis is a count variable, it is not appropriate for simple linear regression. Its distribution is heavily skewed, with values concentrated at the lower end of the scale, creating difficulties for estimation through linear regression²⁷. In order to handle some of these problems, the models were estimated with a log-transformed dependent variable. The results are displayed in Table 5 and Table 6, for the narrow and broad samples, respectively.

The estimated coefficients for the accountability treatment effects are not statistically significant at the 95% confidence level, both in the narrow²⁸ and broad samples. Also, some sensitivity to changes in the sample is observed. In the case of electoral accountability, the sign of the coefficients is mainly negative in the models with the narrow sample, but becomes somewhat unstable with the broad sample. For horizontal accountability, estimates are mostly positive, contradicting theory, and significant at the 90% confidence level in some specifications. The sign of the estimates for the radio presence treatment effect oscillates across specifications with both samples. Interestingly, the coefficients for civil society density (measured through the new methodology) are negative and statistically significant in the models with the broad sample, indicating that corruption is significantly lower as the number of non-profit organizations increases. An analysis of variance inflation factors shows no evidence that these results might be motivated by strong collinearity and highly inflated standard errors. Additional specifications with alternative indicators for some of the main control variables (not reported) provide very similar results to the ones shown below²⁹.

Regression diagnostics for these models reveal that they perform well in terms of normality of residuals (with the exception of models 3 and 6 in Table 5) and also pass a Ramsey test for

²⁷ Indeed, the models estimated with the number of corruption violations as dependent variable violate some assumptions of linear regression, such as the normality of residuals, and present problems of model fit with regards to functional form, as evidenced by a Ramsey test of omitted variables. Other estimation methods commonly used with count variables, such as Poisson and Negative Binomial regression, were also tested, but they failed to produce adequate model fits. These results are not reported.

²⁸ For all models reported with the narrow sample—also in the other estimation procedures described later—, the variable indicating the term in office for the mayor in power was not included. Since this variable strongly correlates with the binary electoral accountability measure, its inclusion creates severe collinearity problems for the estimation.

²⁹ The alternative specifications included HDI as an indicator of development, instead of its education component and log GDP, and literacy rate as an alternative to the education component of the HDI.

omitted variables, which points to an adequate fit in terms of functional form³⁰. Finally, an analysis of *dfbetas* identified a few very influential observations with *dfbetas* above one standard error³¹. However, the models excluding those presented similar results to the previous models for the main independent variables (see Table 25 in Appendix B).

This first estimation strategy presents no evidence of a significant deterrent effect of any of the accountability variables on corruption levels. In some models, the direction of the effect is unstable, or even contradicts the deterrence hypothesis, as is the case with horizontal accountability. An important issue with this approach, however, is a potential endogeneity bias. Accountability is expected to affect corruption *ex ante*, i.e. the occurrence of corruption in the present is affected by the expectation of sanctions in the future. At the same time, accountability in the form of sanctioning can only happen if corruption has taken place. Therefore, these two factors are not independent from each other.

In order to circumvent this problem, another identification strategy was applied, exploring the exogenous release of information from the audits in period $T(0)$ to identify the effect of electoral accountability on corruption levels³². Since there was no audit in period $T(-1)$, the comparison between corruption levels observed in that period and in $T(0)$ is used as a “control” situation for electoral accountability, whereas the comparison between corruption levels observed in $T(0)$ and $T(1)$ constitutes the “treatment” situation³³. The assumption here is that the probability of electoral accountability for the treatment situation—referring to the election occurring at the end of $T(0)$ —is potentialized by the release of information on the occurrence of corruption after audit 1, which is absent in the control situation—no audit took place prior to the election in $T(-1)$). Therefore, the change in the probability of electoral accountability between these two periods is captured by this exogenous factor. Moreover, the

³⁰ This Ramsey test verifies the significance of predicted values (\hat{y}) and their powers (\hat{y}^2) as predictors of the observed values of the dependent variable. In all cases, the null hypothesis of no omitted variable could not be rejected.

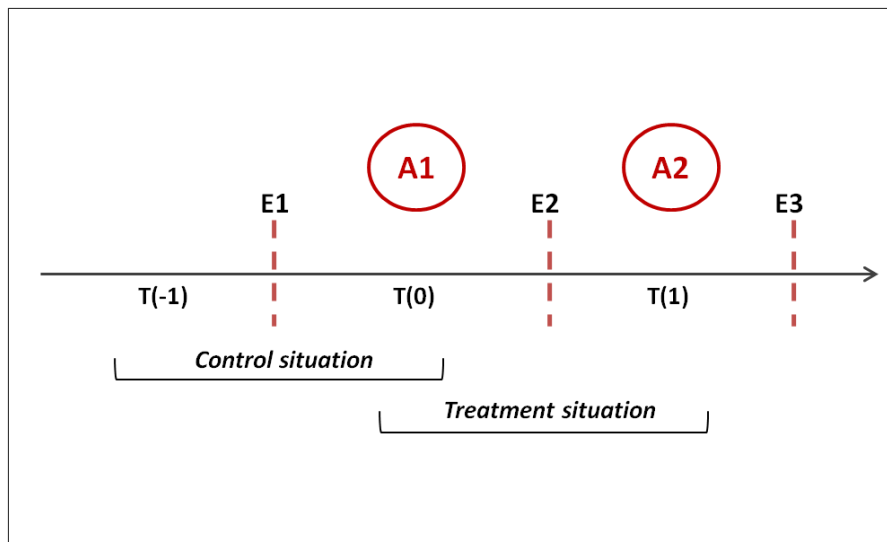
³¹ Different cutoff values for the identification of influential observations are suggested in the literature (Bollen and Jackman 1990). One of them takes sample size into consideration with the formula $2/\sqrt{n}$. As an alternative, the less demanding value of one standard error can be used.

³² Although the horizontal accountability variable is also possibly affected by an endogeneity bias, it was not possible to find a similar exogenous factor as a substitute indicator in that case.

³³ Control and treatment are referred to as “situations” instead of “cases” or “groups”, as to avoid an interpretation that these cases are associated with different units of observation. All municipalities have a control and treatment situation, and what varies are the time periods associated with each situation.

fact that the occurrence and timing of audit 1 is fully randomized adds inferential leverage to this approach. This setup is better illustrated in Figure 4, which shows a typical scenario where, by definition, audit 1 (A1) takes place in period T(0) and audit 2 (A2) occurs in the subsequent period, T(1); at the end of each period, elections (e.g. E1, E2, E3) are held. Due to the absence of an audit before E1, voters did not have access to information of corruption prior to that election, whereas the occurrence of audit 1 prior to E2 made such information available to voters. Therefore, electoral accountability should be stronger in E2, and we should see a deterrent effect from electoral accountability between T(1) and T(0) (treatment situation), but not between T(0) and T(-1) (control situation).

Figure 4. Treatment and control situations



For this analysis, a separate data set was constructed with four observations for each municipality, including data for T(-1), T(0) and T(1). The observations for T(0) are duplicated; one of them is considered as the after-treatment period for the control situation, and the other is designated as the before-treatment period for the treatment situation. The horizontal accountability and radio presence treatment are also included in the models³⁴.

³⁴ In order to account for the time discrepancy between control and treatment, these variables were recoded as follows: horizontal accountability for the control situation received values reflecting its occurrence prior to the period audited in T(0), and retained the original values (sanctions occurring prior

Table 7. Difference-in-differences models with log number of corruption violations as dependent variable and audit information release as electoral accountability treatment (narrow sample)

	(1)	(2)	(3)	(4)	(5)	(6)
After treatment period	-0.384** (0.175)	-0.450** (0.188)	-0.739** (0.296)	-0.768*** (0.282)	-0.925*** (0.339)	-0.733** (0.302)
EA treatment (audit information)	-0.250 (0.155)	-0.333** (0.159)	-0.567** (0.266)	-0.569** (0.256)	-0.771** (0.316)	-0.569** (0.271)
EA treatment x after	0.0168 (0.202)	0.168 (0.216)	0.326 (0.315)	0.357 (0.316)	0.412 (0.366)	0.315 (0.328)
HA treatment	-0.0625 (0.116)	-0.0397 (0.115)	-0.301* (0.156)	-0.322* (0.164)	-0.259 (0.167)	-0.310* (0.161)
HA treatment x after	0.0996 (0.164)	0.0302 (0.174)	0.366* (0.194)	0.405** (0.201)	0.349 (0.219)	0.377* (0.201)
Radio treatment	-0.183 (0.122)	-0.184 (0.129)	-0.0635 (0.140)	-0.0780 (0.145)	0.00116 (0.169)	-0.0540 (0.145)
Radio treatment x after	0.135 (0.160)	0.148 (0.172)	0.0662 (0.182)	0.0942 (0.189)	0.0386 (0.221)	0.0545 (0.186)
Log number CSOs (old)		-0.0752 (0.0929)			-0.0761 (0.138)	
Log number CSOs (new)			-0.0527 (0.0827)			-0.0533 (0.0836)
Constant	0.595 (6.493)	-8.431 (6.998)	14.62* (7.875)	13.11 (7.933)	8.662 (7.752)	14.78* (7.904)
Observations	286	237	167	169	120	165
R-squared	0.581	0.610	0.692	0.694	0.744	0.692
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2, and 3). Robust standard errors in parentheses (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

to period audited in T(1)) for the treatment situation; radio presence for the control periods received the value reflecting the presence of local radio in the first year of T(0), and also retained the original values for the treatment periods (presence of radio at beginning of T(1)).

Table 8. Difference-in-differences models with log number of corruption violations as dependent variable and audit information release as electoral accountability treatment (broad sample)

	(1)	(2)	(3)	(4)	(5)	(6)
After treatment period	-0.316* (0.176)	-0.335* (0.183)	-0.865** (0.364)	-0.882*** (0.297)	-0.957*** (0.324)	-0.848** (0.364)
EA treatment (audit information)	-0.237 (0.150)	-0.268* (0.151)	-0.680** (0.332)	-0.681** (0.262)	-0.720** (0.278)	-0.671** (0.331)
EA treatment x after	-0.0607 (0.205)	-0.00187 (0.219)	0.493 (0.364)	0.554* (0.303)	0.566* (0.322)	0.498 (0.363)
HA treatment	-0.0521 (0.104)	-0.0535 (0.105)	-0.265* (0.145)	-0.296** (0.146)	-0.254 (0.159)	-0.263* (0.147)
HA treatment x after	0.127 (0.145)	0.0939 (0.157)	0.333* (0.177)	0.349* (0.178)	0.300 (0.199)	0.329* (0.180)
Radio treatment	-0.116 (0.110)	-0.0944 (0.114)	-0.152 (0.155)	-0.132 (0.152)	-0.151 (0.173)	-0.147 (0.157)
Radio treatment x after	0.0298 (0.145)	0.0303 (0.153)	0.0523 (0.186)	0.0700 (0.189)	0.149 (0.223)	0.0444 (0.189)
Log number CSOs (old)		-0.0882 (0.0766)			-0.197* (0.109)	
Log number CSOs (new)			-0.152* (0.0777)			-0.154** (0.0766)
Constant	-4.837 (4.486)	-10.79** (4.730)	9.533 (5.919)	7.738 (5.919)	6.429 (7.304)	9.359 (5.916)
Observations	419	352	237	240	173	235
R-squared	0.510	0.537	0.663	0.644	0.699	0.665
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2 and 3). Robust standard errors in parentheses (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The models originally presented in Table 5 and Table 6 were re-estimated in this new setup. The results are shown in Table 7 and Table 8. This alternative approach for estimating the effect of electoral accountability yields consistently positive coefficients for the treatment effects in the case of the narrow sample, and also mostly positive in the case of the broad sample. Although these results are not statistically significant at the 95% level, only at the 90% level in some specifications, it is important to point out that variance inflation factors are considerably high (around 16) for the electoral accountability treatment, indicating that standard errors are inflated and about four times higher than they should be in the absence of high collinearity with other variables. For the other accountability treatments, the estimated treatment effects become consistently positive across all models with both samples, contrary to the expected deterrence role of accountability. However, these results are still not statistically significant at conventional levels, with the exception of the estimate for horizontal accountability in one specification with the narrow sample.

These models also perform well in terms of the normality of residuals and the model fit. As in the original models, a few observations appear to be highly influential on some coefficients. After they are removed from the sample³⁵, the estimates for the main treatment variables do not change significantly and retain the positive sign in almost all models, but these results are still not statistically significant at conventional levels. The only accountability indicator that appears to be significantly associated with lower corruption is civil society density (based on the new methodology) in the models with the broad sample. This is still the case in the estimations without influential observations. These results are displayed in Table 26 in Appendix B.

It is puzzling to see, for all three treatment variables, positive effects on the corruption indicator, which are even sometimes statistically significant at least at the 90% confidence level, as this goes against the expectation derived from the deterrence hypothesis under test. Indeed, based on the theoretical discussion used to fundament the analysis, it is difficult to derive an explanation why corruption might in fact increase as a result of effective accountability, as the estimated treatment effects displayed above suggest. In light of these results, it is important to consider a potential bias from the measurement issues discussed in

³⁵ The criterion applied here was again to select only the observations with *dfbetas* above the absolute value of one standard error.

the previous section. As described earlier, the corruption estimates for later periods may be influenced by a higher probability of detection of irregularities in the second audit. Looking at the results presented so far, it is plausible that this measurement bias is resulting in an apparent increase of corruption.

This begs the question whether the positive coefficients could be so biased as to mask a true significant negative treatment effect. However, as already argued before, it seems unlikely that a strong bias would persist in the presence of a true deterrent effect of accountability on corruption, as in that case the true level of corruption subject to detection in the second audit would be lower, thereby minimizing the potential for an apparently inflated number of corruption violations. Moreover, since the dependent variable used in the estimation is the log-transformed number of violations, the risk that observations with extreme values are driving the results is also reduced, and the sensitivity analysis has verified that results do not change significantly when influential observations are removed.

This alternative setup to circumvent endogeneity problems in the case of electoral accountability offers yet another possibility to identify the effect of this variable on corruption levels. Given that the data is structured with one control situation and one treatment situation for each municipality, it is possible to explore the difference in corruption levels observed between $T(-1)$ and $T(0)$, i.e. in the control situation almost as a true counterfactual of the difference observed in the treatment situation for each panel unit, only accounting for the time difference between the two. Differently than in the standard difference-in-differences estimation, this offers stronger inferential leverage due to the fact that the primary comparison is between observations for the same municipality, which makes it possible to eliminate the effect of potential unobservable municipal characteristics.

In order to extract the confounding effect of the time difference between the control and treatment situations, we estimate a regression model of corruption based on all time-varying covariates considered in the original difference-in-differences models, and predict the respective residuals for all observations³⁶. These residuals reflect the remaining level of

³⁶ These models were also estimated with the log corruption violations as dependent variable, and included audit scope controls (number of service orders and time coverage for each period in years), period fixed-effects, municipal controls (education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, log % of municipal employees directly appointed, and GDP growth), and mayor

corruption that should be explained only by the exogenous release of audit information in T(0). In order to better illustrate the basis for comparison here, let us consider C_{rk1} as the residual for T(-1) for municipality k , C_{rk2} the residual corruption for T(0), and C_{rk3} the residual for T(1). We are interested in the following differences:

$D_{Tk} = (C_{rk3} - C_{rk2})$, which reflects the difference before and after treatment for the treatment situation, and

$D_{Ck} = (C_{rk2} - C_{rk1})$, representing the difference between the before and after periods for the control situation.

The “individual” treatment effect is the difference between D_{Tk} and D_{Ck} for each municipality. If we were to observe a deterrence effect of electoral accountability, we would expect D_{Tk} to be negative, with lower residual corruption levels in the after treatment period, and this difference should also be larger in magnitude than D_{Ck} . Therefore, in the presence of a deterrence effect of electoral accountability, the estimated treatment effect should be negative. In a first step, t-tests comparing the mean D_T to the mean D_C were conducted, showing no statistically significant difference between the two for any of the possible underlying models tested; all differences are also very small in magnitude, with standard errors at least twice as large, depending on the different model specification tested and the sample used. However, this procedure is comparing only the averages for each period.

The next step was to calculate the actual “individual” treatment effects for each panel unit, and then to calculate the average treatment effect. Due to missing data for part of the observations in the data set, we unfortunately have much fewer panel units for which the direct comparison is possible, and this is the case only for residuals generated from models excluding number of civil society organizations and log campaign revenue. In order to have a slightly larger alternative sample to conduct the analysis, the residuals calculated with a reduced specification excluding two other control variables were also used. The resulting

characteristics (gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support). Alternative specifications including also the number of civil society organizations and log campaign revenue were tested as well, but did not yield enough observations for direct comparison through this procedure.

average differences and the confidence intervals are displayed in Table 9. Through this procedure, although the estimated effects have the expected sign, we similarly fail to find an average treatment effect statistically distinguishable from zero in all variations tested.

Table 9. Estimated average treatment effects

	<i>N</i>	<i>ATE</i>	<i>Confidence interval</i>	
<i>Full specification</i>				
Narrow Sample	34	-0.1112126	-0.5656146	0.34318943
Broad Sample	48	-0.0969416	-0.4822255	0.28834238
<i>Reduced specification</i>				
Narrow Sample	44	-0.0466971	-0.493363	0.39996882
Broad Sample	74	-0.1011003	-0.4347948	0.23259409

Note: The reduced specification excludes the log % of natural resource royalties in municipal revenue and the log % of municipal employees directly appointed. Confidence intervals calculated at 95% level.

Over all, we see again that, even with alternative approaches to remove the endogeneity bias at least for the effects of electoral accountability, there is no robust evidence for any deterrent effect of accountability on future corruption levels, even taking into consideration a potential bias due to measurement issues.

Panel regression models

In addition to the difference-in-differences approach, panel regression with fixed- and random-effects was conducted on data for all the five periods available. The tables below show the results for the main models with the full set of controls³⁷. In the fixed-effects models with the narrow sample (Table 10), the estimates for electoral accountability are all negative, as predicted, and statistically significant at the 95% confidence level in three of the four models. In the case of social accountability, the coefficients for both indicators are mostly negative and statistically significant with the expected sign in part of the specifications. For horizontal

³⁷ Due to insufficient overlap between the time periods with data for civil society density based on the old methodology and for campaign funds, models including both variables could not be estimated. Therefore, for comparative purposes only models with the new measure for civil society are reported.

accountability, the estimated coefficients are also partly statistically significant, but their signs are still mainly positive.

These first results appear to be sensitive to changes in the sample. For the same models estimated with the broad sample (Table 11), only the coefficients for horizontal accountability remain statistically significant, retaining the positive sign. The sign of the coefficient for electoral accountability becomes unstable. The estimates for radio presence and civil society density are still mostly negative, but no longer statistically significant. It is important to highlight that the models with the broad sample offer better conditions for verifying within-unit changes, as they take into consideration more observations per municipality, on average. This is because the electoral accountability variable has several missing data points for the municipalities in the narrow sample, as the coding for this variable is based on a more restricted notion of electoral accountability. Therefore, the models with the narrow sample include several panel units with only one observation.

Alternative specifications with a measure of change in vote share as the indicator for electoral accountability show that the results observed are not robust (see Table 27 in Appendix B). It is important to note that we expect the opposite relationship when using this variable: lower values of the change in vote share are associated with stronger accountability, thus we should expect coefficients to be positive. In only one of the models with the narrow sample do we see a statistically significant effect in the expected direction. The estimates for horizontal accountability and for civil society density become more erratic, with varying signs, although statistically significant in some specifications. The models with the broad sample are even less consistent with those yielded with the binary electoral accountability indicator, since all estimates have the opposite sign than what we would expect, and in two of the models the estimates are statistically significant at the 90% confidence level.

Table 10. Fixed-effects models with log number of corruption violations as dependent variable (narrow sample)

	(1)	(2)	(3)	(4)
EA previous mayor	-0.542* (0.309)	-2.110*** (0.464)	-3.272*** (0.372)	-1.929*** (0.432)
HA previous mayor	-0.164 (0.238)	0.611*** (0.193)	0.371*** (0.115)	0.211* (0.109)
Radio presence	0.556 (0.339)	-0.281* (0.145)	-0.451** (0.197)	-0.296*** (0.106)
Log number CSOs (new)		-0.0315 (0.240)		-0.856*** (0.164)
Constant	-13.98 (21.41)	-25.91* (13.80)	-26.31*** (9.602)	-5.666 (7.454)
Observations	137	105	111	104
R-squared	0.590	0.963	0.974	0.991
Number of panel units	85	80	84	80
Audit scope controls	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1 and 2). Robust standard errors in parentheses (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11. Fixed-effects models with log number of corruption violations as dependent variable (broad sample)

	(1)	(2)	(3)	(4)
EA previous mayor	-0.0575 (0.179)	0.0163 (0.215)	-0.0249 (0.170)	0.0482 (0.222)
HA previous mayor	0.171 (0.146)	0.369*** (0.140)	0.359** (0.138)	0.375*** (0.139)
Radio presence	0.148 (0.156)	-0.185 (0.136)	-0.0151 (0.147)	-0.222 (0.144)
Log number CSOs (new)		-0.286 (0.209)		-0.250 (0.221)
Constant	-13.35 (10.00)	4.524 (14.18)	-2.121 (13.38)	7.703 (14.10)
Observations	269	200	210	199
R-squared	0.524	0.694	0.746	0.698
Number of panel units	130	127	129	127
Audit scope controls	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1 and 2). Robust standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

The models from the tables above were also estimated with random-effects, in order to better account for the impact of some accountability variables that may vary little over time within some of the units in the sample (e.g. radio presence). The results for those models are presented in Table 12 and Table 13. For the narrow sample, the coefficients for electoral accountability are still negative, but no longer statistically significant. Horizontal accountability presents unstable signs in the narrow sample, and the estimates are not statistically significant either. The estimated effect for radio presence is now positive, contrary to what was hypothesized, but not statistically significant. Finally, only the coefficients for civil society density suggest a statistically significant effect of this indicator for social accountability, at least at the 90% confidence level.

The results for the broad sample are to a large extent similar to those for the narrow sample. None of the accountability measures show a statistically significant effect, except civil society density. The coefficients for electoral accountability become also unstable and present a negative sign in one specification. The estimates for horizontal accountability continue to show positive signs. The coefficient for radio presence also becomes unstable and shows a positive sign in one of the models. The alternative specifications with change in vote share as a measure of electoral accountability also fail to find evidence for a deterrent effect of this accountability dimension on corruption (see Table 28 in Appendix B). The coefficients for this indicator present volatile signs across specifications with the narrow and broad samples. The results for the other accountability indicators are very similar to those observed in the original models.

A Hausman test was conducted to confront the consistency and efficiency of the random-effects results to those originating from the fixed-effects estimation. According to the test, the models with the narrow sample are generally less consistent than the fixed-effects alternative, except for model 1 in Table 12. For the broad sample, however, the random-effects results are preferred. In either case, however, these panel estimations also fail to offer robust evidence of a deterrent effect of the three dimensions of accountability on corruption levels in Brazilian municipalities.

Table 12. Random-effects models with log number of corruption violations as dependent variable (narrow sample)

	(1)	(2)	(3)	(4)
EA previous mayor	-0.0521 (0.314)	-0.372 (0.466)	-0.244 (0.270)	-0.269 (0.490)
HA previous mayor	-0.107 (0.154)	-0.0188 (0.164)	0.0757 (0.181)	0.0932 (0.161)
Radio presence	0.136 (0.147)	0.0775 (0.178)	0.0984 (0.180)	0.0688 (0.190)
Log number CSOs (new)		-0.273* (0.144)		-0.364** (0.160)
Constant	11.17 (10.36)	19.33 (12.82)	10.11 (13.33)	11.83 (14.32)
Observations	137	105	111	104
Number of panel units	85	80	84	80
Audit scope controls	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1 and 2). Robust standard errors in parentheses (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 13. Random-effects models with log number of corruption violations as dependent variable (broad sample)

	(1)	(2)	(3)	(4)
EA previous mayor	0.0517 (0.143)	-0.00544 (0.156)	0.0840 (0.144)	0.0198 (0.158)
HA previous mayor	0.0570 (0.108)	0.0490 (0.108)	0.0760 (0.108)	0.0562 (0.107)
Radio presence	-0.0163 (0.104)	-0.0186 (0.102)	0.0241 (0.108)	-0.0208 (0.101)
Log number CSOs (new)		-0.269*** (0.0982)		-0.261*** (0.0984)
Constant	-0.857 (5.342)	6.345 (6.200)	3.735 (5.704)	5.191 (6.192)
Observations	269	200	210	199
Number of panel units	130	127	129	127
Audit scope controls	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1 and 2). Robust standard errors in parentheses (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Discussion and conclusion

The empirical results described above do not present robust evidence for a deterrent effect of the distinct dimensions of accountability on corruption levels over time. Throughout the different approaches explored we observe that, in most cases, the very direction of the estimated relationships cannot be unequivocally identified, often pointing at different directions. In the case of electoral accountability, the results echo previous studies that also failed to find evidence for a sustained effect of electoral accountability on corruption over time (Bobonis *et al.* 2012; Crisp *et al.* 2014), at least in the short term. Only the fixed-effects models with the narrow sample show a statistically significant effect of electoral accountability on corruption. However, as already pointed out, these results are based on a relative small and unbalanced panel, for which not enough longitudinal information is available for all cases. Furthermore, this was not corroborated by the other estimation exercises.

For this variable, we also see some discrepancy between results with the narrow and broad samples. The results for the broad sample seem to be more volatile, with the direction of the effect varying more across specifications than with the narrow sample. On the one hand, the estimates for the broad sample may indeed reflect more accurately the relationship (or lack thereof) between the dependent and independent variables, as they are based on a larger amount of data. At the same time, however, it is important to highlight that there is an essential conceptual difference between the two samples with regards to how electoral accountability is operationalized. As a consequence, the observed discrepancy may suggest a difference in the strength of reelection incentives for candidates running themselves for reelection, as in the narrow sample, and those that present a successor candidate, who may still have some incentives to refrain from corruption, but less so than when their personal career and continuance in power is at stake.

The case for a deterrent effect of social accountability is also relatively weak based on the evidence collected here. The effects of radio presence are mostly volatile, pointing to opposite directions depending on the model specification and the sample used. A statistically significant negative effect of this variable on corruption is seen only for the narrow sample in the fixed-effects models, and this finds no support in the other estimation procedures used. A negative effect of civil society density, on the other hand, is partially supported by the data and is found to be statistically significant, mainly for the broader sample, in some of the estimations conducted. Moreover, this is the only accountability variable for which the expected negative

sign of the coefficients is consistent across virtually all models and specifications tested. Nevertheless, the evidence presented is insufficient to confirm a robust deterrent effect of social accountability through increased civil society presence.

The most puzzling case is certainly that of horizontal accountability. For this variable, the estimated models indicate, more often than not, a positive association with future corruption levels, although this is only statistically significant at conventional levels in a few of the models tested. Here the data more clearly contradicts the postulated hypothesis, although the evidence is not robust to all specifications.

A key concern associated with these results, however, is the possibility that they are influenced by a potential bias in the measurement of the corruption indicator, due to the fact that the results from later audits are influenced by the existence of irregularities identified in previous audits. This characteristic of the auditing process implies an increased likelihood of detecting corruption violations in subsequent audits, which could cause the number of corruption violations found in later audits to be higher than before. Indeed, in several of the procedures used, the estimated effect of the accountability variables showed a positive sign, suggesting that the occurrence of accountability is associated with increased corruption in future periods. These results would be consistent with the potential measurement bias discussed: corruption appears to be on the increase because later audits have looked for it more carefully. In the case of horizontal accountability, for which the estimates were almost always positive across the different models tested, it could be that this problem is even more pronounced, because it likely correlates with the probability of horizontal accountability occurring in the first place. Indeed, in municipalities with more violations found in audit 1, the probability that the mayor has faced formal sanctions as a consequence should be higher simply because there are more facts to prosecute, administrative irregularities would be more severe, and a reaction leading to his potential removal from office could also be more likely. Similarly, the likelihood of a more thorough second audit should increase with the number of violations found in audit 1, as auditors would have more motivation to verify whether the administration has incurred in the same abuses as before.

Even though this means that the estimated effects cannot be fully taken at face value, there are reasons to believe that the nature and magnitude of the bias are not occulting a deterrent effect of accountability on corruption. In fact, the impact of this bias should decrease the stronger the deterrent effect. After all, auditors would not be able to detect more corruption if

it had in fact decreased. The often positive effects found for electoral and horizontal accountability, and also for radio presence, which in some cases were even statistically significant with a 90% confidence level, can thus be interpreted as an indication that the expected deterrent effect is not taking place. Moreover, the fact that the analysis is still able to find more consistent evidence of a negative effect of civil society presence in several of the models further supports this interpretation; otherwise, the estimates for this variable should be equally distorted by measurement bias in the corruption indicator.

These methodological shortcomings notwithstanding, we can therefore conclude that the evidence at hand does not provide indication for the hypothesized deterrent effect of accountability. Why do these dimensions of accountability apparently fail to bring about the expected deterrent effect on corruption, at least based on the available evidence from the Brazilian case? Despite the universal aspect of the hypotheses considered, and the favorable variation across Brazilian municipalities, there are some contextual elements at the macro level that may contribute to understanding why the data at hand does not offer any solid support to the deterrence thesis related to the role of accountability.

In the case of electoral accountability, the Brazilian context suggests that the probability of mayors losing office, considering the sample at hand, is not so low. The sample distribution for the electoral accountability indicators in the data set vary between 43% and 46% of observations where electoral accountability was observed, i.e. the mayor or administration in power was voted out of office. In contrast to arguments and evidence from other contexts of a so-called incumbency advantage, it seems that this phenomenon is not as prevailing in the Brazilian case³⁸. On the contrary, recent studies even suggest that Brazilian mayors may face an “incumbency disadvantage” instead (Brambor and Ceneviva 2012). In such a context, politicians may perceive their probability of losing office as already considerable, regardless of their performance. Incumbents may consider this as a certain level of unpredictability or “noise” in the behavior of voters, which creates a higher degree of uncertainty that becomes counterproductive for electoral accountability (Besley 2006). In such cases, their preferred strategy may become to take advantage of available rents while they can (Crisp *et al.* 2014).

³⁸ Studies on the incumbency advantage often rely on the American electoral context, in particular the high reelection rates for members of Congress, which are in many cases above 90%. Similar studies about Brazilian representatives have found an average reelection rate of only 50% (Brambor and Ceneviva 2012).

This could be an alternative mechanism by which an increased probability of electoral accountability results rather in the opposite effect and weakens incentives to refrain from corruption.

Contextual idiosyncrasies related to the effectiveness of horizontal accountability by Brazilian enforcement and oversight institutions, in particular in the case of the judicial system and the Courts of Accounts, could also shed some light on why we tend to see the opposite effect of this dimension of accountability on corruption outcomes for the sample under analysis. This is also connected to the operationalization of this independent variable: due to empirical limitations in measuring effective punishment, the horizontal accountability indicator reflects in most cases only potential sanctions. Moreover, it is also possible that, even in the cases where legal and administrative sanctions are comparatively more likely, political actors still perceive the general probability of facing concrete punishment in the foreseeable future as sufficiently low to motivate them to continue engaging in corruption³⁹.

In the case of social accountability through increased media presence, the often observed pattern of media capture by local elites in Brazil (Boas and Hidalgo 2011; Hervieu et al. 2013; de Lima and Lopes 2007; Porto 2011) could be a mechanism behind the absence of a deterrent effect of radio presence on corruption levels. In such an environment, the presence of local media would likely not result in increased scrutiny over the local government and in more information reaching voters, thus reducing the potential for a virtuous impact of the media through this channel of accountability.

It is also possible that the potential payoffs of corruption are still high enough to offset the incentives produced by accountability (Pereira *et al.* 2009), and this cannot be fully captured in the present analysis. Non-monetary payoffs, such as local power and influence or other perks related to executive office, probably play a significant role in the local context in Brazil. Such factors are likely to be associated with the particularistic allocation of resources, and although the analysis here sought to take into account cross-unit variation in terms of budgetary

³⁹ The rejection of municipal accounts by the Courts of Accounts, for instance, must be ultimately confirmed by the municipal Legislative in order to have binding effects. In the case of legal prosecution, judicial procedures in Brazil tend to take several years before a final conviction is reached, and in criminal cases this may even lead to charges being dismissed altogether if procedures take longer than the time limitations for sentencing established by law. This sort of inefficiency may be yet another factor contributing to a perception of generally low risks of concrete legal sanctions (de Alencar and Gico Jr. 2011).

resources and the potential use of patronage, such indicators probably only capture part of the picture.

Although the present analysis suggests a rather grim perspective for the anti-corruption power of accountability mechanisms, the few points discussed above suggest that its study requires further efforts to understand how certain contextual dimensions may condition that potential effect. The recent literature on how different accountability mechanisms may impact corruption outcomes is relatively scarce. New attempts to more systematically examine this question empirically in other contextual settings could offer valuable additional insights on this relationship and also contribute to refining the theoretical foundations behind the deterrence hypothesis.

Additionally, more efforts are necessary to incorporate a temporal dimension into the study of accountability and corruption. The analyses conducted here sought to take a further step into that direction, but the empirical strategies explored are based on a relatively short time horizon, looking only at the immediate effects of accountability on future corruption levels. Perhaps this limitation is another reason why the expected deterrent effect could not be confirmed by data. The effective functioning of accountability mechanisms is ultimately associated with the consolidation of democratic institutions, and the deterrent effects proposed by the theoretical literature may indeed take longer periods of time to crystallize. Future research in this area should thus aim at devising empirical strategies in order to examine this question also from a longer temporal perspective. Such remaining gaps in the literature offer a promising agenda for future scholarly work in this field.

PART III

Electoral accountability of corrupt politicians: conditioning factors revisited

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.

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 been so exceptional¹, 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, 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.

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

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 first presents an overview of the core hypotheses discussed in the literature and the related findings. The next section then describes the methodological approach and data sources, followed by the estimation procedures and results. The final section discusses the main findings and concludes with their implications for future research in this area.

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 structures² 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.

² 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.

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 precondition 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-Shapiro 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 run-off 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)³. 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 accusations, and the perceived agenda that the accusers might have. It is argued, for instance, that accusations from independent enforcement or control agencies

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.

should be seen as less biased, and therefore more credible, by voters (Fernández-Vázquez *et al.* 2013; Klačnjaja 2011; Weitz-Shapiro and Winters 2014; cf. Sberna and Vannucci 2013;)⁴, 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

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.

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 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 competent⁵ mayor and a non-corrupt incompetent one, are more likely

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

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 one⁶.

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 transactions⁷, 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

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).

against corrupt mayors if their illegal activities have generated economic benefits for the municipality⁸.

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 countries⁹ 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,

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.

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.

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 patronage¹⁰.

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 support, 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

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.

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 outsiders¹¹.

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 significant¹².

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 characteristics¹³.

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,

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).

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 argument, 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 group¹⁴.

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

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.

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 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 conditioning corruption voting 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 observed 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 above¹⁵ 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.

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

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.

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 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 measure to identify an effect of corruption on voting, as these perceptions may be themselves affected by individual biases¹⁶. 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:

¹⁶ 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).

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 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)¹⁷. 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)¹⁸. 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 2006b; Winters and Weitz-Shapiro 2015)¹⁹. 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.

¹⁷ 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).

¹⁸ 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.

¹⁹ 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 2006b; Samuels and Zucco 2014).

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). 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 2015²⁰. So far, over 2,000 municipalities have been audited at least once, representing almost 40% of all Brazilian municipalities²¹. 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 place²².

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.

During the selected period, a total of 720 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 analysis²³. 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 30 in Appendix C).

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 violations²⁴. 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 simulation²⁵. This may ultimately impact how voters perceive the corruption information 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,

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 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.

and those described with more technical language were coded as “ambiguous”²⁶. 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 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

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.

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 areas²⁷. Therefore, this indicator appears to be particularly useful for testing this hypothesis because it 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 voting²⁸.

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,

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.

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.

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 cases²⁹. 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 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,

²⁹ It is important to acknowledge that this is certainly not the only means mayors have to distribute material resources to their clienteles. 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).

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 government³⁰. 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).

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 2006b; Samuels and Zucco 2014).

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 31 in Appendix C, which displays the summary statistics for all the variables of interest.

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 baseline 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 14 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 14. 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 violations (unambiguous)	-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 violations*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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

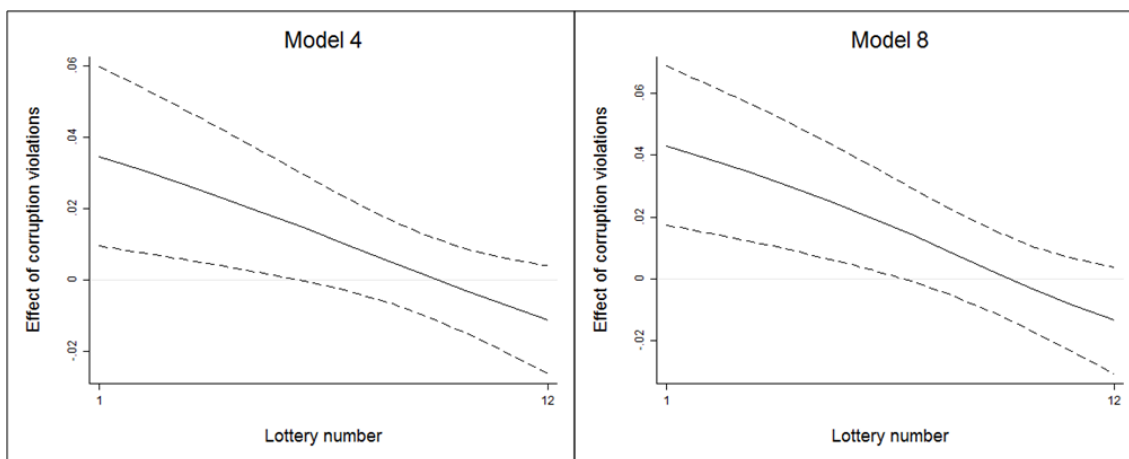
A first comparative glance across these two groups reveals sizeable differences in the estimated coefficients for the different samples, with larger estimated effects for the corruption sub-sample. Although the effects are in most cases not statistically significant, these differences already offer some 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 5 for models 4 and 8. The marginal effects for Models 2 and 6, which do not include the controls, are shown in Figure 7 in Appendix C.

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 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 32 in Appendix C), 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 8 in Appendix C). 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.

Figure 5. Conditional marginal effects of unambiguous corruption violations for full models



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 15 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 15. Predicted probabilities of electoral accountability for different levels of corruption

Number of unambiguous corruption violations	Full sample		Corruption Sample A	
	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

Number of total corruption violations	Full sample		Corruption Sample B	
	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 16 illustrates models with equivalent specifications to the ones shown in Table 14, 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.

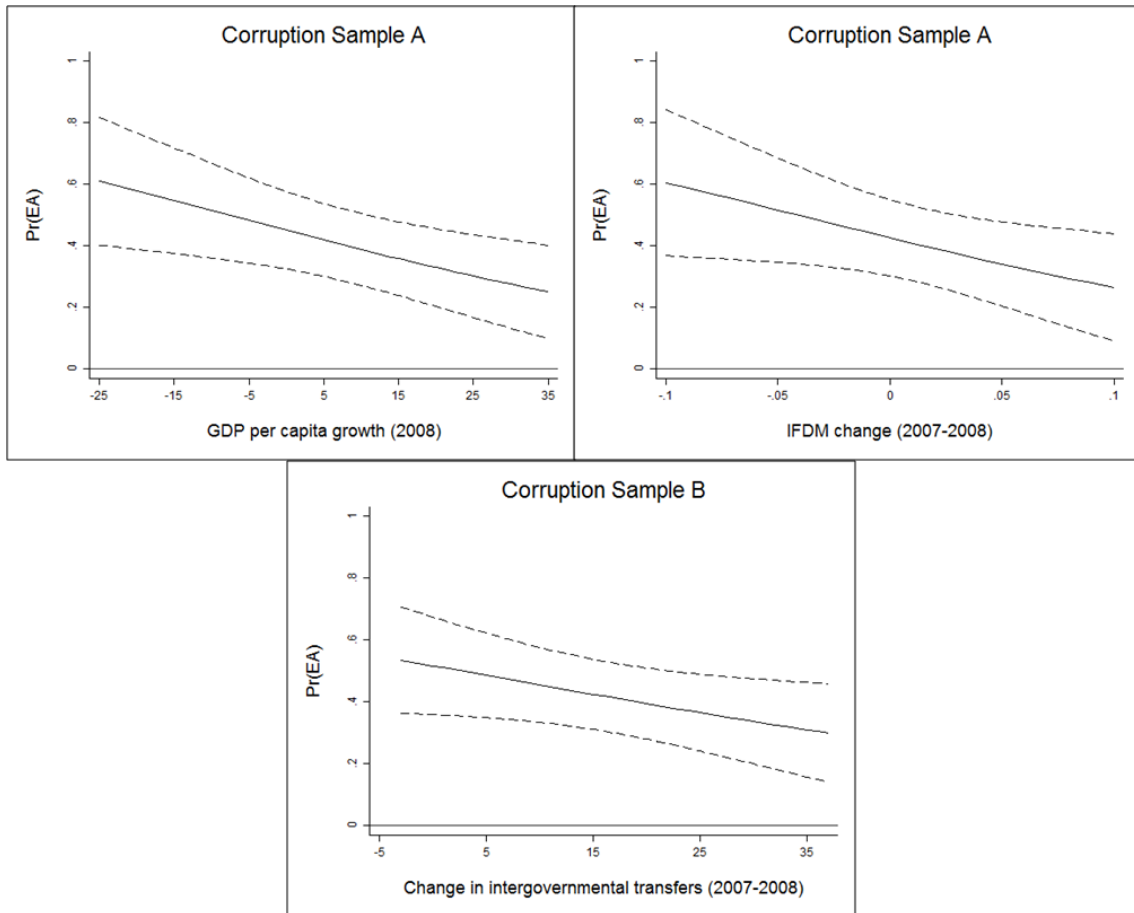
These effects become clearer once we observe the change in the predicted probability of electoral accountability against corrupt incumbents for different values of the indicators of interest, illustrated in Figure 6. In the case of GDP per capita growth, a change from a value of one standard deviation 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 16. 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) (2.049)	-0.0161** (0.00749) (1.944)	-0.0167** (0.00724) (1.914)	-0.0167** (0.00725) (1.911)	-0.0160 (0.0105) (2.099)	-0.0254** (0.00999) (2.650)	-0.0220** (0.00933) (2.511)	-0.0213** (0.00871) (2.455)	-0.0214** (0.00882) (2.469)	-0.0189 (0.0130) (2.726)
Observations	358	370	383	383	341	240	248	259	259	228

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 6. 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 33 in Appendix C) 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 34 in Appendix C). After calculating the corresponding marginal effect for corruption violations, we observe a statistically significant negative effect on the change in vote share for early audit rounds in the models using the number of unambiguous corruption violations (see Figure 9 in Appendix C). 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.

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 dissemination 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 years³¹.

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

³¹ 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).

years of 2005 and 2006 saw the emergence of two major corruption scandals of national amplitude³², 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). One possible confounding factor that would interfere in this process is the occurrence of media capture by local political groups, which is as relatively common phenomenon in Brazil (Boas and Hidalgo 2011; de Lima and Lopes 2007), but could not be taken into account in the analysis due to data limitations. 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

³² 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.

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 become 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 2006a).

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 weakening 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; Klašnja 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 reinforcing 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.

Conclusion

This dissertation set out to address two related research puzzles that have been the focus of much scholarly work in the last decades: what is the impact of democracy on corruption, and what explains the large variation in corruption levels observed across democratic regimes? Differently than most previous research on these issues, it sought to tackle these questions from a distinct perspective, considering the multidimensional nature of democracy and the consequent multiplicity of causal mechanisms that might be behind the association between stronger democratic institutions and lower corruption observed in a number of cross-national comparative studies (e.g. Kolstad & Wiig, 2016; Pellegata, 2012; Saha *et al.* 2014).

Indeed, as shown in the first part of the dissertation, the vast literature on democracy and corruption includes several theoretical arguments alluding to the role of different dimensions of democracy as potentially explaining its impact on corruption outcomes in a political system. Based on a review of those arguments, it was argued that, although the theoretical explanations for an effect of democracy on corruption do make reference to various possible causal mechanisms, they can be connected by a common thread, namely accountability, and can be linked to three sub-dimensions of this key attribute of democracy: electoral, social and horizontal accountability. With this discussion, the dissertation sought to make a first contribution to this broader literature, by taking an approach that emphasizes the need to examine more closely and more directly the actual causal mechanisms linking democracy and corruption, and proposing that this be done from the angle of those three dimensions of accountability as a key causal link.

The conceptual framework proposed set the ground for the two main empirical analyses developed in the dissertation. The first one, addressed in the second part of the study, drew on theoretical principal-agent models to test whether those dimensions of accountability exert a commonly hypothesized deterrent effect on corrupt behavior, thus reducing corruption levels over time. The second one, discussed in Part III, looked more specifically at the factors that might explain when accountability—in the form of concrete sanctions—takes place, focusing on the conditioning factors of electoral accountability against corrupt politicians, or what is also described in the literature as corruption voting. These empirical analyses sought to

make a contribution to more specific strands of research, dealing respectively with the impact of accountability on corruption and the determinants of corruption voting.

In both cases, the study relied on original political corruption data collected from over 600 audit reports on municipalities randomly selected through a federal audit program implemented by the Brazilian government. For the analyses presented in Part II, a sub-sample of 140 municipalities that have faced multiple audits in the period between 2003 and 2013 was taken as the basis for a novel panel data set covering five municipal administrative terms. In Part III, the analyses conducted employed corruption data for a cross-section of 383 municipalities.

In line with previous findings from the empirical literature in both lines of research addressed in this study, the overall picture that emerges from the work presented here is not very optimistic. In the first case, no robust evidence is found for a deterrent effect of accountability on corruption over time, be it through elections, increased civil society and media presence, or formal institutional sanctions. These findings might seem counterintuitive, given the fact that the idea of accountability as a deterrent to corruption has established itself as a strong assumption in several areas of political research. However, empirical evidence to support it is still limited. In the case of electoral accountability, for instance, a few studies indicate that incumbent politicians facing the threat of losing office in the next election engage in less corruption in the present (Bobonis *et al.* 2012; Ferraz and Finan 2011); at the same time, other findings indicate that the threat of accountability may not always be sufficient to hinder rent-seeking behavior (Pereira *et al.* 2009), and that even when incumbents seem to refrain from corruption in the short-term, they may compensate with increased predatory behavior after they have been able to secure an additional term in office (Bobonis *et al.* 2012).

In the case of horizontal accountability, some evidence suggesting a deterrent effect of public audits on corruption has been found (Avis *et al.* 2016; Olken 2007; Zamboni Filho 2011); however, it may be also the case that corrupt officials adapt their behavior to increased detection risks in some areas, moving to other forms of rent-seeking that remain under the radar of monitoring institutions (Olken 2007). Regarding social accountability, some authors present direct evidence of a negative effect of increased media presence on corruption (Ferraz and Finan 2011), and others find a broader association between media freedom and lower corruption (e.g. Adsera *et al.* 2003; Brunetti and Weder 2003), but the impact of civil society presence still has not been systematically studied (Schatz 2013). In the analysis conducted

here, the evidence points more strongly to an effect of civil society presence in reducing future corruption, but this too is not fully robust to all the estimation procedures applied.

From the second empirical analysis, we also conclude that conditions for successful electoral accountability of corrupt incumbents may be more restrictive than usually expected. Out of the five main hypotheses tested, the study corroborates only two conditioning factors discussed in the literature on corruption voting. First, the severity of corruption accusations seems to be a relevant criterion for voters while making their decision, but only under certain conditions. More specifically, the analysis found that the effect of corruption information varied with the timing in which this information was made available to the public. However, a more careful consideration of this finding suggests that this conditional effect may be associated with the level of salience that corruption had at the time of the election. In the particular context of the 2008 municipal elections in Brazil, media attention on the municipal audit results may have declined throughout the years prior to the election, either because the audit results were disseminated in a less accessible format, because the original excitement around the audit program slowly died down, or also because other national corruption scandals may have attracted more media attention. This is consistent with evidence from other contexts indicating that increased salience of corruption driven by stronger media attention seems to play a key role in how corruption information impacts electoral results (Chang *et al.* 2010; Costas-Pérez *et al.* 2012; Klašnja *et al.* 2016).

The second hypothesis that is supported by the evidence from Brazilian municipalities is the impact of an implicit exchange mechanism, whereby voters weigh the corruption information against other relevant criteria, in particular the more general performance of the incumbent government. In this case, voters' inclination to remove corrupt incumbents from office appears to be significantly weakened by positive economic results and, to some extent, improvements in the provision of public services. However, here voters seem to behave myopically and give more importance to recent improvements in performance. This finding resonates with results from several previous studies showing a moderating effect of perceptions of incumbents' performance on corruption voting (Choi and Woo 2010; Klašnja *et al.* 2016; Muñoz *et al.* 2012; Pereira and Melo 2015; Zechmeister and Zizumbo-Colunga 2013).

Surely, some limitations to the present analyses must be acknowledged. Similar to virtually every other study dealing with elusive concepts such as accountability and corruption, measurement challenges relating to these variables are among the main obstacles for the

empirical work presented. Despite such difficulties, this dissertation sought to make an additional empirical contribution to the operationalization of these constructs. In the case of accountability, original data for the occurrence of horizontal accountability was collected; regarding corruption, an indicator of political corruption was devised and extracted from the results of repeated randomized municipal audit rounds in Brazil and was also explored in a longitudinal dimension that is rarely available for this type of data. However, some data limitations could not be fully overcome. In the first case, the horizontal accountability indicator could only capture stronger potential for formal sanctions, and as a consequence may not fully reflect how a deterrent effect of this accountability dimension on corruption might work in reality. The corruption estimates used in the longitudinal analysis, in turn, are likely to suffer from a certain degree of measurement bias, due to lack of independence between the corruption information found in later audits and the results from previous audits. Although these issues do not eliminate the applicability of the indicators for the analyses conducted, they exemplify some of the practical difficulties that are faced in attempts to more directly measure these phenomena.

Similar measurement problems also impact other indicators used in the analyses, such as the proxy for clientelism applied in Part III, which is maybe capturing only a part of this phenomenon and its potential impact on electoral accountability. Moreover, other relevant factors could not be taken into account in the study due to data constraints. One example is the possibility of local media capture that could impact the reliability of the radio presence variable employed both as an indicator of social accountability and as a moderating factor of corruption voting to capture variation in the availability of corruption information to voters. Indeed, a more refined analysis of the impact of local media would ideally take into account data on the ownership of media by local politicians, in order to provide a more accurate picture of the media environment at this level of analysis in Brazil. However, such data are not easily obtainable and would have required additional efforts to data collection that were simply beyond the scope and the resources available for the conclusion of this study.

There are also some limitations regarding the inferential scope of both analyses. For the empirical exercises described in Part II, the availability of panel data for a relatively short time frame implies that the estimated effects for the different dimensions of accountability on future corruption levels comprehend only potential immediate effects. Therefore, the possibility that a longer period of time is necessary for the accountability mechanisms to

exercise a suppressing effect on corruption cannot be tested by the data at hand. In the analysis presented in Part III, a central issue related to the inferences drawn from the analysis refers to the use of aggregate level data to assess the validity of some hypotheses that are associated with mechanisms at the individual level. This has been a challenge also in the broader literature on electoral accountability and voting behavior (Hellwig 2011), and approaches to conduct multi-level analysis in the case of corruption voting, which are still hindered by limited availability of adequate data, remain a part of the agenda for further research in this area.

Another relevant question regards the potential for external validity of the findings presented here. It is true that the analyses include data from a specific context, and the results must also be considered in the light of certain idiosyncrasies associated with the particular institutional and political environment in Brazil. The lack of a deterrent effect of horizontal accountability, for instance, may be linked to the fact that formal sanctioning mechanisms in Brazil are still not effective enough to significantly increase the expected probability of punishment in order to tip the balance in political actors' calculations of the costs associated with corrupt behavior. Similarly, specific dynamics of local media ownership and their potential impact on media freedom seem to be common in the Brazilian case and could be an important mechanism behind the patterns observed in the data employed here. Nevertheless, the large variation observed among Brazilian municipalities on a number of other factors still contributes to show that this context has parallel with cross-national variation in numerous respects, thus contributing to strengthening the external validity of the results. Moreover, the empirical findings described in this dissertation go in a similar direction of findings from previous studies covering other countries or even large-N cross-national comparisons, suggesting that they are likely reflecting a more general pattern on how the connection between accountability and corruption works.

In spite of the several limitations mentioned above, the work conducted as part of this dissertation provides additional pieces to the more general puzzle of the relationship between democracy, accountability and corruption and adds to earlier evidence showing that neither the expected corruption-reducing effects of accountability nor the effective occurrence of electoral accountability can be taken for granted in democratic regimes. These ideas have been treated as universal assumptions in studies on democracy and corruption, but an emerging empirical literature—this study included—examining the proposed causal mechanisms

more closely has consistently put their validity into question. Considering the point of departure of the dissertation, which emphasized the theoretical discussions on the causal mechanisms possibly linking democracy and corruption as reflecting the hypothesized impact of different dimensions of accountability, the findings presented here have important implications with regards to those theoretical underpinnings.

Indeed, the scant evidence for an anti-corruption effect of democracy through the described accountability channels indicates that the general causal claims found in the literature should not be taken at face value. However, there may be different reasons why these claims still lack strong empirical support. For example, it may be the case that the hypothesized effect of accountability does take place, but under specific conditions that have not been fully identified. In this case, existing theories would need some refinement with regards to the conditions that might impact the expected effect of accountability on corruption. One such possible condition could be the time horizon considered. From the analysis presented here, given the limitation in the time frame available for the data collected, we cannot rule out that a longer period of time might be needed for a possible deterrent effect to materialize or become perceptible. This would be consistent with earlier comparative studies indicating an association between older democracies and lower corruption (e.g. Lederman *et al.* 2005; Pellegata 2012; Serra 2004).

Another possibility briefly mentioned in Part II of this dissertation is that the effect of accountability on corruption may sometimes be non-linear. In the case of electoral accountability, there are some discussions in the literature that suggest this is the case. In the Brazilian municipal context, where some authors speak of an incumbency disadvantage (Brambor and Ceneviva 2012), the level of electoral uncertainty and the risk of losing office may be so high as to offset the expected disciplining effects of electoral accountability and create the opposite incentives. Indeed, engaging in more corruption while they can may become a more feasible strategy for politicians in office if they perceive the risk of losing office as high (Crisp *et al.* 2014). Other studies also suggest that electoral competition may have a non-linear impact on the responsiveness of elected officials more broadly (Kayser and Lindstädt 2015), and on corruption more specifically, indicating that incentives for corrupt behavior might be lowest under an intermediate level of competition (Afridi *et al.* 2016). Studies examining these types of hypotheses in different contexts could make an invaluable

contribution to improving theoretical arguments on the causal nexus between accountability and corruption.

Moreover, even if accountability would in fact impact corruption negatively under certain favorable conditions, it is also unclear whether all dimensions of accountability are equally important to help reduce corruption over time. Studies examining the potential impact of horizontal and social accountability on corruption are still scarce. More research considering the potential confounding effects of the different accountability dimensions, as well as how they may interact with one another, is necessary too. In this respect, new studies taking into account the potential anti-corruption effect of different accountability mechanisms, possibly covering distinct contexts, would also be useful to help clarify these issues and generate new hypotheses on how contextual factors might be relevant for a better understanding of the links between accountability and corruption.

Finally, it may also be the case that the empirical association between stronger democracy and lower corruption observed in the comparative literature is explained by other mechanisms besides accountability. The existing literature still seems to offer limited insights in this regard, but future discussions in this direction, exploring alternative mechanisms, would also be an important contribution to shedding additional light on this topic of research and providing for new lines of inquiry on the impact of democracy on corruption.

In addition to these possibilities for future research in this area, we should not lose sight of the need for further improving available indicators of corruption. In this respect, this study follows a much welcome trend that has developed in recent years towards the development of concrete indicators of corruption, departing from more traditional measurement approaches based on public perceptions or bribery victimization (see Fazekas *et al.* 2016 for a review). Advances in this area are important for two reasons. First, concrete indicators should help avoid some of the biases often pointed out in the literature with regards to perception-based indicators, which may reveal more about the beliefs of respondents than about the actual prevalence of corruption in a particular context (Abramo 2007; Escresa and Picci 2015; Knack 2006; Olken 2009; Razafindrakoto and Roubaud 2010; Treisman 2007). This could lead to substantial improvements in the validity and reliability of existing indicators. Second, these types of indicators allow researchers to capture different facets of the corruption phenomenon (Fazekas *et al.* 2016). As many have argued, corruption is not a uniform practice, it comes in different shapes and sizes; consequently, different types of corruption may be

associated with different underlying causes and dynamics (e.g. Bauhr 2016; Hellman *et al.* 2000; Johnston 2005, 2014). This is still a relatively little explored terrain in the area of corruption studies, and efforts to slowly overcome measurement challenges that help provide a more accurate picture of the problem from different angles could open up new possibilities for better understanding corruption and finding more effective ways to tackle it in its different manifestations.

Other efforts to produce more fine-grained depictions of how and to what extent corruption occurs in different contexts should include measures that capture in-country variation as well. As illustrated by the case of Brazil explored in this dissertation, internal variation in the prevalence of corruption across sub-national units can be significant, and this is another useful dimension of analysis for studying this topic. Finally, the longitudinal perspective is still a big gap in corruption measurement, and new approaches to gathering indicators of corruption and their variation over time could have a groundbreaking impact on corruption research. Needless to say, these are certainly challenging goals, given the inherent difficulties of measuring corruption, but novel and ingenious strategies to capture this phenomenon empirically are being elaborated more and more often by researchers.

All in all, recent research on democracy, accountability and corruption has helped produce new insights on their underlying relationships, showing that some of the assumptions commonly held in the literature need more careful investigation. Indeed, the latest findings suggest that the link between these factors may be more complex than usually acknowledged, and more empirical work is needed to improve our understanding of the causal mechanisms linking democracy and corruption. Fortunately, several avenues of research should emerge from the existing limitations of scholarly works in this field, providing a promising ground for new approaches and perspectives.

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Appendix A

Table 17. Comparison between sample and universe of cases

	Sample	Brazil
<i>Regional distribution</i>		
North	15%	8%
Northeast	45%	32%
Southeast	16%	30%
South	14%	21%
Center-West	9%	8%
<i>Distribution by population bracket (IBGE 2013)</i>		
Up to 5,000	13%	23%
5,001-10,000	20%	22%
10,001-20,000	27%	25%
20,001-50,000	24%	19%
50,001-100,000	7%	6%
100,001-500,000	9%	5%
More than 500,000	0%	1%
<i>Mean development indicators</i>		
HDI (2010)	0.637	0.659
HDI education (2010)	0.538	0.559
% Literacy (2010)	64.0	83.8
% Urban population (2010)	81.3	63.8
GDP per capita (2012 BRL)	11,531	14,897

Table 18. Regional comparison of development indicators

Region	IDH (2010)		GDP (2012 BRL)		% Literacy (2010)		% Urban population (2010)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
North	0.608	0.609	515,329.81	122,036.00	82.7	83.4	56.7	55.8
Northeast	0.591	0.588	331,874.16	73,960.50	72.7	72.3	55.2	54.5
Southeast	0.699	0.705	1,453,240.60	144,654.50	89.2	90.7	74.9	79.0
South	0.714	0.716	598,366.97	126,190.00	92.1	92.6	60.7	61.6
Center-West	0.689	0.693	923,741.17	156,047.50	87.7	87.9	71.8	74.8
All	0.659	0.665	789,235.22	109,731.00	83.8	86.9	63.8	64.7

Table 19. Pairwise correlations between corruption measure and development indicators

	Number of corruption violations	Residual number of violations without audit scope variables	Log number of corruption violations	Residual log number of violations without audit scope variables
GDP per capita	-0.2138*	-0.2352*	-0.2053*	-0.2333*
HDI	-0.2443*	-0.1629*	-0.2313*	-0.1494*
Literacy	-0.2532*	-0.2860*	-0.2706*	-0.2697*

Note: * $p < 0.05$.

Table 20. Summary statistics

<i>Variable</i>	<i>N</i>	<i>Mean</i>	<i>Median</i>	<i>St. Deviation</i>	<i>Min</i>	<i>Max</i>
<i>Dependent variable</i>						
N. of corruption violations	408	8.659	5	10.599	60	0
<i>Independent variables</i>						
EA: previous mayor voted out (narrow sample)	229	0.454	0	0.499	1	0
EA: previous mayor/administration voted out (broad sample)	330	0.476	0	0.500	1	0
EA: change in vote share	334	-7.048	-5.1	19.04	57.6	-61.7
<i>Audit scope</i>						
Number of audit service orders	408	19.122	15	15.753	103	1
Time period covered (in years)	408	2.818	3	1.192	4	1
<i>Municipal characteristics</i>						
Municipal area	408	1746	527	3780	33595	33
Population	408	31563	14564	59529	486487	1948
% of urban population	408	60.0	60.8	21.7	100	15.5
% of municipal employees directly appointed	358	9.983	7.7	8.264	57.9	0
Municipal HDI	408	0.534	0.537	0.109	0.808	0.293
Education component of municipal HDI	408	0.381	0.37	0.147	0.768	0.102
Municipal GDP (1000 BRL)	408	329723	85426	888377	9584190	4385
Municipal GDP per capita	408	8238	6285	5618	40418	2242
Municipal GDP growth (yearly average)	408	6.515	5.8	12.562	202.6	-14.9
% of transfers in municipal revenue	405	90.208	93	8.479	99.7	50.3
% of natural resource royalties in municipal revenue	353	1.077	0.53	2.284	21.95	0
Year of foundation	408	1963	1960	21	1997	1933
Local radio presence	404	0.582	1	0.494	1	0
<i>Mayor characteristics</i>						
Mayor affiliated to federal government party	408	0.105	0	0.307	1	0
Term in office	408	1.272	1	0.446	2	1
Gender	358	1.103	1	0.305	2	1
Education level	357	6.482	7	1.811	8	2
Political experience	408	0.463	0	0.499	1	0
Margin of victory in previous election	403	18.375	12.4	19.643	100	0
Legislative support (% of mayor's party seats in local council)	407	0.263	0.22	0.158	1	0
Total campaign revenue in previous election	236	133466	69801	218928	2080263	0

Table 21. Regression models with number of corruption violations as dependent variable

	OLS		Poisson		Negative binomial	
	NS	BS	NS	BS	NS	BS
	(1)	(2)	(3)	(4)	(5)	(6)
EA previous mayor	-1.782 (2.002)	0.294 (2.406)	-0.307 (0.251)	-0.101 (0.263)	-0.322 (0.257)	0.150 (0.211)
Observations	94	154	94	154	94	154
R-squared	0.495	0.424				
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area, GDP growth and local radio presence. Mayor characteristics include, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support, log campaign revenue and term in office (only in models 2, 4 and 6). Robust standard errors in parentheses. NS = Narrow sample; BS = Broad sample.

Table 22. Regression models with log number of corruption violations as dependent variable without selected influential observations

	Narrow sample	Broad sample
	(1)	(2)
EA previous mayor	-0.223 (0.237)	0.235 (0.197)
Observations	92	152
R-squared	0.639	0.616
Audit scope controls	Yes	Yes
Period fixed-effects	Yes	Yes
Municipal controls	Yes	Yes
Mayor controls	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area, GDP growth and local radio presence. Mayor characteristics include, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support, log campaign revenue and term in office (only in model 2). Influential observations with $dfbetas$ above 1 excluded (PR-Saudade do Iguaçu in period 3 removed in models 1 and 2; BA-Nazare in period 4 removed in model 1; RO-Rolim de Moura in period 3 removed in model 2). Robust standard errors in parentheses.

Table 23. Regression models with log number of corruption violations as dependent variable and change in vote share as indicator for electoral accountability

	Narrow sample			Broad sample		
	(1)	(2)	(3)	(4)	(5)	(6)
EA (change in vote share)	0.00229 (0.00532)	0.000680 (0.00473)	-0.00135 (0.00491)	0.000842 (0.00394)	0.000165 (0.00351)	-0.000367 (0.00335)
Observations	106	105	105	159	157	157
R-squared	0.282	0.513	0.610	0.305	0.526	0.586
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	No	Yes	Yes	No	Yes	Yes
Mayor controls	No	No	Yes	No	No	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area, GDP growth and local radio presence. Mayor characteristics include, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support, log campaign revenue and term in office (only in models 4-6). Robust standard errors in parentheses.

Appendix B

Table 24. Summary statistics

	<i>N</i>	<i>Mean</i>	<i>Median</i>	<i>St. Deviation</i>	<i>Min</i>	<i>Max</i>
<i>Dependent variable</i>						
N. of corruption violations	437	8.368	5	10.531	0	60
<i>Independent variables</i>						
EA: previous mayor voted out (narrow sample)	245	0.437	0	0.497	0	1
EA: previous mayor/administration voted out (broad sample)	355	0.462	0	0.499	0	1
EA: change in vote share	334	-7.048	-5.1	19.040	-61.7	57.6
HA: previous mayor sanctioned	408	0.402	0	0.491	0	1
SA: N. of non-profit organizations (old methodology)	263	81.384	36	185.072	2	1752
SA: N. of non-profit organizations (new methodology)	241	75.224	29	177.437	1	1775
SA: local radio presence	433	0.573	1	0.495	0	1
<i>Audit scope</i>						
Number of audit service orders	437	19.119	16	15.697	1	103
Time period covered (in years)	437	2.796	3	1.193	1	4
<i>Municipal characteristics</i>						
Municipal area	437	1865	527	4061	33	33595
Population	437	32248	14541	60245	1948	486487
% of urban population	437	60.1	60.9	22.2	15.5	100
% of literate population	437	76.4	77.1	12.3	47.5	97.9
% of municipal employees directly appointed	384	9.978	7.7	8.185	0	57.9
Municipal HDI	437	0.535	0.537	0.109	0.293	0.808
Education component of municipal HDI	437	0.381	0.37	0.147	0.102	0.768
Municipal GDP (1000 BRL)	437	363832	86154	1043257	4385	10100000
Municipal GDP per capita	437	8483	6529	6134	2242	40418
Municipal GDP growth (yearly average)	437	6.566	5.8	12.216	-14.9	202.6
% of transfers in municipal revenue	434	90.095	93	8.585	50.3	99.7
% of natural resource royalties in municipal revenue	379	1.080	0.53	2.301	0	21.95
Year of foundation	437	1963	1960	21	1933	1997
<i>Mayor characteristics</i>						
Affiliation to federal government party	437	0.112	0	0.316	0	1
Term in office	437	1.279	1	0.449	1	2
Gender	384	1.102	1	0.302	1	2
Education level	383	6.462	7	1.818	2	8
Political experience	408	0.463	0	0.499	0	1
Previous terms in office	408	0.554	0	0.659	0	3
Margin of victory in previous election	431	18.065	12.1	19.369	0	100
Legislative support (share of mayor's party seats in local council)	424	0.263	0.22	0.158	0	1
Total campaign revenue in previous election	253	132626	68754	213577	0	2080263

Table 25. Difference-in-differences models with log number of corruption violations as dependent variable without selected influential observations

	Narrow Sample					Broad Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
After treatment period	-0.0704 (0.274)	-0.0444 (0.289)	-0.285 (0.316)	-0.190 (0.385)	-0.196 (0.317)	-0.261 (0.243)	-0.394 (0.336)
EA treatment	-0.0820 (0.165)	-0.0641 (0.176)	0.0300 (0.202)	-0.0442 (0.236)	0.0361 (0.195)	-0.0165 (0.142)	-0.0370 (0.215)
EA treatment x after	-0.259 (0.232)	-0.349 (0.242)	-0.314 (0.258)	-0.144 (0.358)	-0.387 (0.256)	-0.0570 (0.255)	0.114 (0.366)
HA treatment	-0.163 (0.141)	-0.344* (0.188)	-0.354* (0.201)	-0.249 (0.241)	-0.354* (0.194)	-0.168 (0.134)	-0.349* (0.202)
HA treatment x after	0.168 (0.212)	0.336 (0.242)	0.449* (0.247)	0.303 (0.391)	0.387 (0.247)	0.205 (0.192)	0.339 (0.282)
Radio treatment	-0.0389 (0.154)	0.0794 (0.194)	0.0678 (0.194)	0.389 (0.265)	0.130 (0.204)	-0.0673 (0.140)	-0.169 (0.215)
Radio treatment x after	0.0648 (0.228)	-0.0836 (0.258)	0.0380 (0.262)	-0.417 (0.411)	-0.0824 (0.270)	-0.0636 (0.211)	0.0212 (0.314)
Log number CSOs (old)				-0.190 (0.193)			-0.228* (0.136)
Log number CSOs (new)		-0.0628 (0.120)			-0.0938 (0.119)		
Constant	13.77 (8.369)	20.68** (10.24)	16.80* (9.493)	18.63 (11.74)	19.93** (9.865)	0.492 (6.341)	3.473 (10.33)
Observations	164	119	121	76	117	243	115
R-squared	0.597	0.682	0.687	0.731	0.690	0.500	0.707
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office (excluded in models 1-5), gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2 and 6). Influential observations with $dfbetas$ above 1 excluded. Robust standard errors in parentheses (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 26. Difference-in-differences models with log number of corruption violations as dependent variable and audit information release as indicator for the electoral accountability treatment, without selected influential observations

	Narrow Sample					Broad Sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After treatment period	-0.398** (0.178)	-0.746** (0.287)	-0.793*** (0.278)	-1.272*** (0.338)	-0.949*** (0.325)	-0.323* (0.176)	-0.500* (0.267)	-0.479* (0.259)
EA treatment (audit information)	-0.259* (0.156)	-0.540** (0.264)	-0.527** (0.255)	-1.128*** (0.335)	-0.797** (0.327)	-0.242 (0.150)	-0.353 (0.221)	-0.343 (0.214)
EA treatment x after	0.0292 (0.201)	0.265 (0.312)	0.192 (0.311)	0.734* (0.383)	0.502 (0.380)	-0.0543 (0.205)	0.171 (0.264)	0.174 (0.260)
HA treatment	-0.0533 (0.116)	-0.231 (0.151)	-0.249 (0.162)	-0.103 (0.167)	-0.168 (0.156)	-0.0485 (0.104)	-0.248* (0.144)	-0.246* (0.145)
HA treatment x after	0.0958 (0.163)	0.352* (0.186)	0.423** (0.196)	0.234 (0.211)	0.289 (0.192)	0.127 (0.145)	0.315* (0.176)	0.312* (0.179)
Radio treatment	-0.177 (0.122)	-0.0648 (0.138)	-0.0634 (0.148)	0.0469 (0.158)	-0.0210 (0.145)	-0.115 (0.111)	-0.0996 (0.154)	-0.0935 (0.155)
Radio treatment x after	0.146 (0.161)	0.126 (0.175)	0.186 (0.184)	0.0896 (0.199)	0.0818 (0.177)	0.0341 (0.146)	0.00325 (0.184)	-0.00589 (0.187)
Log number CSOs (old)				-0.0545 (0.126)				
Log number CSOs (new)		-0.0434 (0.0808)			-0.0289 (0.0843)		-0.171** (0.0758)	-0.173** (0.0746)
Constant	1.543 (6.721)	17.20** (7.724)	14.35* (7.461)	11.40 (7.779)	17.18** (7.766)	-4.325 (4.525)	9.479 (5.944)	9.299 (5.936)
Observations	285	165	166	117	162	418	236	234
R-squared	0.583	0.704	0.713	0.765	0.707	0.511	0.668	0.669
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office (excluded in models 1-5), gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2, 6 and 7). Influential observations with $dfbetas$ above 1 excluded. Robust standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 27. Fixed-effects models with log number of corruption violations as dependent variable and change in vote share as indicator for electoral accountability

	Narrow Sample				Broad Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EA (change in vote share)	0.00217 (0.00595)	-0.0103* (0.00532)	0.0137* (0.00731)	0.0151*** (0.00479)	-0.00615* (0.00336)	-0.00752 (0.00471)	-0.00622 (0.00424)	-0.00757* (0.00451)
HA previous mayor	-0.167 (0.246)	0.758*** (0.154)	0.206 (0.144)	-0.0438 (0.166)	0.159 (0.141)	0.396*** (0.139)	0.385*** (0.138)	0.399*** (0.140)
Radio presence	0.541 (0.335)	-0.123 (0.171)	-0.459 (0.286)	-0.397** (0.154)	0.162 (0.160)	-0.151 (0.136)	0.0369 (0.160)	-0.192 (0.153)
Log number CSOs (new)		0.400 (0.255)		-1.354*** (0.193)		-0.313 (0.203)		-0.283 (0.212)
Constant	-18.33 (21.56)	-11.21 (16.10)	-20.80* (12.50)	6.363 (6.205)	-12.53 (9.833)	3.297 (13.84)	-4.865 (13.50)	6.506 (13.93)
Observations	136	104	110	103	264	199	209	198
R-squared	0.580	0.941	0.935	0.988	0.545	0.707	0.753	0.712
Number of panel units	84	79	83	79	129	126	128	126
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2, 5 and 6). Robust standard errors in parentheses (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 28. Random-effects models with log number of corruption violations as dependent variable and change in vote share as indicator for electoral accountability

	Narrow Sample				Broad Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EA (change in vote share)	-0.00116 (0.00315)	0.00411 (0.00472)	0.00356 (0.00424)	0.00493 (0.00419)	-0.00363 (0.00276)	-0.000363 (0.00294)	-0.000710 (0.00273)	-0.000266 (0.00291)
HA previous mayor	-0.112 (0.151)	-0.0445 (0.159)	-0.00770 (0.167)	0.0713 (0.163)	0.0318 (0.106)	0.0533 (0.110)	0.0702 (0.109)	0.0592 (0.109)
Radio presence	0.142 (0.145)	0.0646 (0.170)	0.139 (0.160)	0.0476 (0.182)	-0.0118 (0.107)	-0.0153 (0.103)	0.0232 (0.113)	-0.0203 (0.104)
Log number CSOs (new)		-0.237* (0.142)		-0.345** (0.158)		-0.268*** (0.102)		-0.260** (0.102)
Constant	11.34 (10.37)	19.20 (12.58)	14.68 (12.29)	12.14 (13.78)	0.790 (5.225)	6.322 (6.161)	4.262 (5.628)	5.275 (6.134)
Observations	136	104	110	103	264	199	209	198
Number of panel units	84	79	83	79	129	126	128	126
Audit scope controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mayor controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Audit scope controls include number of service orders and time coverage for each period in years. Period fixed-effects refer to time period identification coded from 1 to 5. Municipal controls include region fixed-effects, education component of HDI, log GDP, log population, % of transfers in municipal revenue, log % of natural resource royalties in municipal revenue, % of urban population, year of foundation, log % of municipal employees directly appointed, log municipal area and GDP growth. Mayor characteristics include term in office, gender, education level, previous political experience, affiliation to federal government party, margin of victory, legislative support and log campaign revenue (excluded in models 1, 2, 5 and 6). Robust standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 29. Description of variables

<i>Variable</i>	<i>Description</i>	<i>Source</i>
Time period	4-year administrative period (1=1997-2000; 2=2001-2004; 3=2005-2008; 4=2009-2012; 5=2013-)	
Time period (relative)	Time period with respect to occurrence of audit 1 (0=period in which audit 1 took place; -1=period prior to occurrence of audit 1; 1=period subsequent to occurrence of audit 1)	
Corruption violations	Number of corruption violations found in audits (total for all audits covering the period)	Own calculations based on CGU audit reports
Electoral accountability	Whether previous mayor/administration was voted out (cases in which they did not run/present a successor coded as missing)	Electoral records (TSE)
Change in vote share	Change in vote share of mayor/administration in power in the previous period	Electoral records (TSE)
Horizontal accountability	Whether previous mayor suffered prosecution, accounts rejection or impeachment prior to period covered by audits in time period in question	Federal and State Court records; Courts of Accounts' records; media reports
Radio presence	Presence of local radio station in time period in question	IBGE municipal profiles (1999, 2001, 2005, 2009 and 2012)
Civil society density	Number of non-profit organizations (for old methodology, data available for 2002 and 2006, for new methodology, data available for 2006 and 2010)	IBGE (2002, 2006 and 2010)
Audit service orders	Number of service orders applied in audits (total for all audits covering the period)	Own calculations based on CGU audit reports
Audit time period	Number of years covered in audits (considering all audits covering the period)	Own calculations based on CGU audit reports
Region	Region where municipality is located (1=North; 2=Northeast; 3=Southeast; 4=South; 5=Center-West)	IBGE
Municipal area	Municipal area in km ²	IBGE 2013 estimates
Population	4-year average population for the period (except for 2013)	IBGE
Urban population	Percentage of urban population (periods 1-3=2000 data; periods 4-5=2010 data)	IBGE Census (2000 and 2010)
Literacy	Percentage of literate population (periods 1-3=2000 data; periods 4-5=2010 data)	IBGE Census (2000 and 2010)
Municipal employment	Percentage of municipal employees (direct and indirect administration) that are exclusively directly appointed (T2=2004;T3=2008;T4=2012;T5=2013)	IBGE municipal profiles (2004, 2008, 2012 and 2013)
HDI	Municipal Human Development Index (periods 1-3=2000 data; periods 4-5=2010 data)	UNDP estimates based on IBGE Census (2000 and 2010)
HDI education	Education component of municipal Human Development Index (periods 1-3=2000 data; periods 4-5=2010 data)	UNDP estimates based on IBGE Census (2000 and 2010)
GDP	Municipal GDP in constant 2012 prices (R\$ 1000), yearly average for the period (exceptions: T1=1999-2000; T5=data for 2012)	Own calculations based on IBGE data

GDP growth	Growth in municipal GDP, yearly average for the period (exceptions: period 1=data for 2000; period 5=data for 2012)	Own calculations based on IBGE data
GDP growth deviation	Deviation between municipal GDP growth and national GDP growth, yearly average for the period (exceptions: T1=1999-2000; T5=data for 2012)	Own calculations based on IBGE data
Transfers revenue	Percentage of revenue from current transfers in total municipal current revenue, yearly average for the period (exception: period 5=data for 2012)	Own calculations based on STN data
Natural resource revenue	Percentage of revenue from natural resource exploitation in total municipal current revenue, yearly average for the period (exceptions: period 2=2002-2004; period 5=data for 2012)	Own calculations based on STN data
Year founded	Year municipality was founded	IBGE
Federal government party	Whether mayor in power was affiliated to the party in the federal government	Electoral records (TSE)
Term in office	Whether mayor in power was 1st or 2nd term	Electoral records (TSE)
Mayor gender	Gender of mayor in power (1=Male; 2=Female)	Electoral records (TSE)
Mayor education	Educational level of mayor in power (2=Literate;3=Basic education incomplete;4=Basic education complete;5=Secondary education incomplete;6=Secondary education complete;7=Higher education incomplete;8=Higher education complete)	Electoral records (TSE)
Mayor political experience	Whether mayor in power has occupied a mayor's office previously	Electoral records (TSE), official documents, mayor's profiles, media reports
Mayor previous terms	Number of terms previously served as mayor (also in other municipalities)	Electoral records (TSE), official documents, mayor's profiles, media reports
Mayor margin	Margin of victory of mayor of victory in previous election (in cases in which the deputy mayor took office, this is the same as the previous mayor; cases in which the mayor was not directly elected, data is missing)	Electoral records (TSE)
Mayor campaign revenue	Total campaign revenue raised by mayor in power in previous election	Electoral records (TSE), Transparência Brasil
Mayor political support	Share of seats obtained by the mayor's party in the local legislative	Electoral records (TSE)

Appendix C

Table 30. Comparison between sample and universe of cases

	Sample	Brazil
<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 31. 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 ²)	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	4817067
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

Table 32. 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 33. 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 34. 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 7. Conditional marginal effects of unambiguous corruption violations for unrestricted models

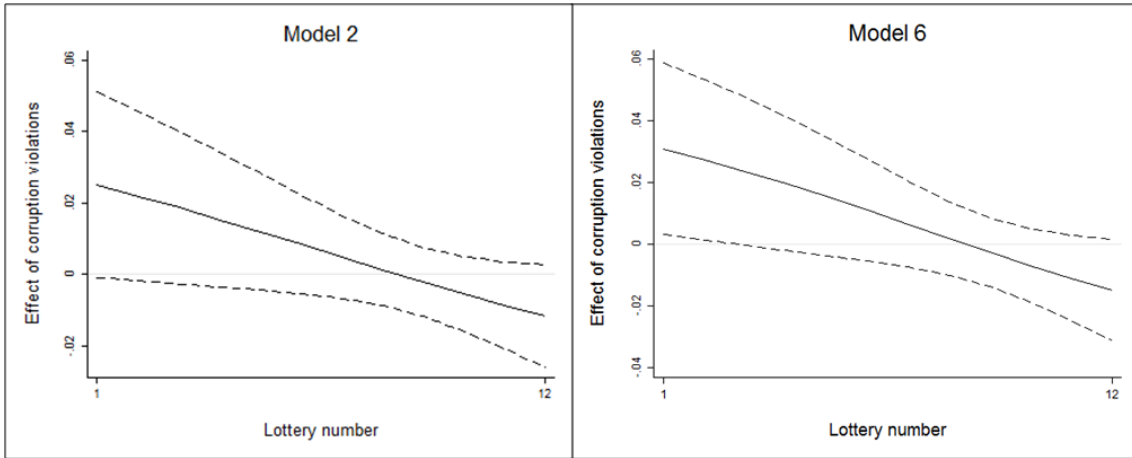


Figure 8. Conditional marginal effects of all corruption violations for full models

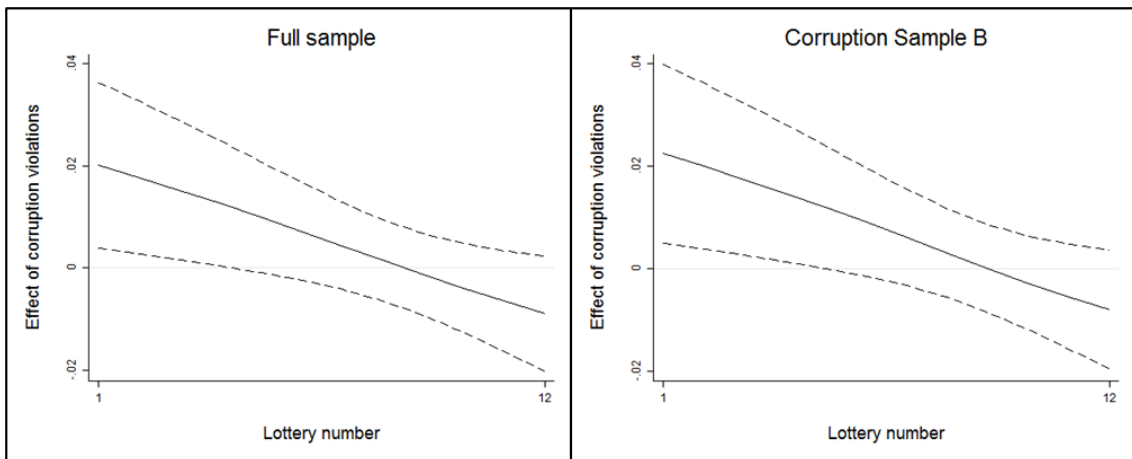
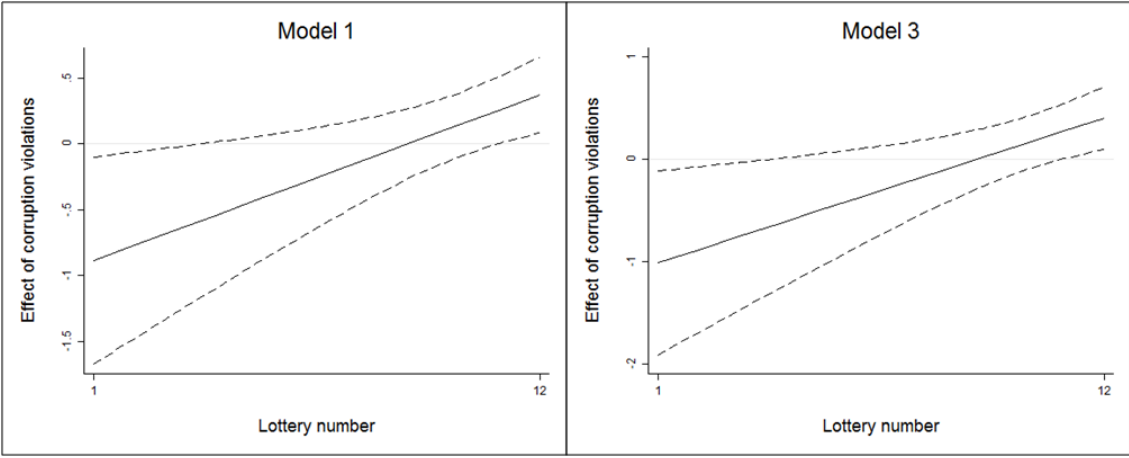


Figure 9. Conditional marginal effects of unambiguous corruption violations on vote share difference



Annex: List of pre-publications

Mondo, Bianca Vaz. "Democratization and corruption: the state of the art." Deliverable for project Anti-Corruption Policies Revisited (ANTICORRP), December 2014. Available at http://anticorrrp.eu/wp-content/uploads/2014/12/D1.1_Part2_Democratization-and-corruption.pdf

Mondo, Bianca Vaz. "Measuring Political Corruption from Audit Results: A New Panel of Brazilian Municipalities." *European Journal on Criminal Policy and Research* 22(3) (2016): 477-498.

This dissertation is the result of cumulative research presented in the following articles:

Part I (pp. 47-76)

Mondo, Bianca Vaz. "Democracy and corruption: exploring the causal mechanisms". Partially published with the title "Democratization and corruption: the state of the art", as a deliverable for project Anti-Corruption Policies Revisited (ANTICORRP), December 2014.

Part II (pp. 77-142)

Mondo, Bianca Vaz. "Measuring Political Corruption from Audit Results: A New Panel of Brazilian Municipalities." *European Journal on Criminal Policy and Research* 22(3) (2016): 477-498.

Mondo, Bianca Vaz. "Accountability as a deterrent to corruption: new data from Brazilian municipalities". Unpublished.

Part III (pp. 143-187)

Mondo, Bianca Vaz. "Electoral accountability of corrupt politicians: conditioning factors revisited". Unpublished.