Essays on Political Corruption

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Abstract

This dissertation presents, in its three essays, a novel test of the so-called tradeoff hypothesis for the prevalence of political corruption; a new method for measuring corruption risk in public procurement; and tests of whether rent-seeking in local procurement can be prevented by accountability from different levels of government and of whether this rent-seeking negatively effects important public policy outcomes. The first set of analyses contradict the theoretical underpinning of the tradeoff hypothesis; the analyses using public procurement data present new and robust evidence that rent-seeking sands the wheels of government. This dissertation's methods, as well as its results and their implications, advance our understanding of the consequences of political corruption and present paths for future research on political corruption.

Dedication

To my mom, who believed in me because she loved me, and never the other way around.

To my dad, who could never tell anyone about anything I accomplished without getting teary eyed.

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I am deeply indebted to my committee chair, Sara Watson. This dissertation was only possible because of her mentorship, her insight, and her consistently excellent feedback. Without the opportunity to learn from Sara, I would not have been able to make any of my ideas come to fruition, and this dissertation would consist of 140 blank pages.

I must also express my huge debt of gratitude to Sarah Brooks, who offered advice and encouragement at times when I struggled to believe in myself. Without Sarah, I would not have been an Ohio State student to begin with, and my life would have been much worse. Jan Pierskalla has been a part of my journey from the start, and he has been extremely impactful in my professional life. I would not have considered a career as a data scientist without the work I did for the World Bank, for which I owe Jan.

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In my second year at Ohio State, I was assigned as a teaching assistant to an intimidating new faculty member who taught two quantitative methods courses. That assignment and that scary (but in reality sweet, funny, and deeply empathetic) professor changed me deeply. Brice Acree was a virtuoso, and I am unbelievably lucky to have had the chance to lean on him for his wisdom and humor for a year and a half. Working as his T.A. for those three semesters was a profoundly enriching experience that I will always remember. I wish I could thank him in person.

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Thank you (e obrigado, e um beijo) to my mom, Neide, for putting up with the distance for the past six years.

Thank you to my cat, Louise, for putting up with my presence for the past six years.

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1.1 Introduction

Many studies on the prevalence of political corruption have focused on voters' frequent preference for candidates who are known to be corrupt. This body of work has generated two broad classes of explanations, which Winters and Weitz-Shapiro (2013) term the information hypothesis and the tradeoff hypothesis. The former posits that information asymmetries lead voters to support corrupt candidates without knowing they are corrupt; the latter posits that voters knowingly support corrupt politicians because it is materially beneficial to them, either because corrupt politicians explicitly engage in clientelistic, exchange-based politics (as defined by Johnston (2005)) with their constituents or because of a hypothesized positive correlation between corruption and the ability or willingness to provide pork for one's voters. In other words, corrupt politicians receive support because they may be better at providing public services for their constituents. This is the implicit exchange hypothesis (Muñoz et al., 2016).[1]

This explanation, which derives from functionalist theory, is particularly applicable to developing democracies, as the hypothesized advantages of being represented by a skillful *and* corrupt politician increase in step with the level of dysfunction of a country's democratic institutions. It is more applicable yet to Latin America,

[1] The 'implicit exchange' hypothesis is contrasted by Rundquist et al. (1977) with an explicit type of exchange that they term the 'material inducement' hypothesis. The material inducement hypothesis is, in essence, vote buying. Rundquist et al. acknowledge that the survival of corrupt politicians cannot be explained by material inducements, as "the numerous explicit exchanges... necessary to control elections would require... large and complex organizations [that] are increasingly rare" (p. 956), and because, with large electorates, conducting enough explicit exchanges to decisively tip an election in one's favor would be too expensive.

where conventional wisdom sees a political culture of broken institutions and impunity for criminal behavior[2]. Not by coincidence, the implicit exchange hypothesis is best illustrated by a Latin American expression, originated in Brazil: "rouba, mas faz" (loosely, "[s]he steals but [s]he gets things done"), a phrase coined by an opponent of São Paulo governor Adhemar de Barros which was eventually adopted by Barros's supporters and later became an unofficial slogan for other Brazilian politicians. The existence of the slogan and its association with perennially popular politicians reflect the fundamental premise behind the tradeoff hypothesis: there is a positive correlation between politicians' propensity for corruption and their ability to deliver goods for their constituents, either because corruption is inherently correlated with performance (a hypothesis raised, for example, by Geddes and Neto (1992)) or because of a selection process through which corrupt politicians that do not perform well are voted out, but those who 'provide' for their constituents remain.

In this paper, I address the tradeoff hypothesis and test this fundamental premise: do corrupt politicians actually provide more public services to their constituents? Answering this question is necessary for the implicit exchange hypothesis to explain the phenomenon of political corruption in the aggregate. In other words, without understanding if the idea of "rouba mas faz" accurately represents politics in contexts with high levels of corruption, the implicit exchange hypothesis is unable to explain the *prevalence* of political corruption; it can only explain what could cause some corrupt politician to receive support. Indeed, that is the central question that is raised, but not necessarily answered, by works that test the implicit exchange hypothesis by means of survey experiments: do corrupt politicians actually deliver? Without answering that question, any relationship between a survey respondent stating that they would vote for a corrupt candidate that delivers pork and corrupt candidates actually winning elections is purely speculative.

I test my question on the 2015-2019 term of the Brazilian Chamber of Deputies, with a dataset of Brazilian municipalities I built using various official sources. The main hypothesis that I test is whether municipalities that vote in higher percentages for corrupt legislators receive more money from the federal government than municipalities that vote for non-corrupt legislators. The Brazilian Chamber of Deputies, as I explain below, is a window into the broader process of corruption prevalence and voter behavior; it is also a crucial case for

[2]This view is, of course, not exclusive to 'conventional wisdom'. See, for example, Geddes and Neto (1992), Rosenn (1971), and Silva (1999).

this test, due to the nature of the federal budgeting process, the highly personalistic character of Brazilian legislative elections (which has been found to be more conducive to corruption – for a review of that literature, see Manzetti and Wilson (2007)), the low electoral disproportionality (see e.g., Pellegata and Memoli (2018) on the effects of different electoral formulae on public perceptions of corruption), and the prevalence of corruption in the Chamber, which provides a source of variation between legislators[3].

The test, for which I utilize a generalized additive model for location, scale, and shape (GAMLSS) due to the extreme skew and kurtosis of the dependent variable, shows that voting for legislators known to be corrupt leads municipalities to receive less money in federal transfers, when compared to those that vote for legislators without known involvement with corruption. This suggests that the implicit exchange hypothesis may not explain the prevalence of corruption in the Brazilian Chamber of Deputies, and that mechanisms such as information asymmetries about the behavior of legislators or about their performance (i.e., an informational problem regarding the existence of tradeoffs) may be better suited to explain that phenomenon.

1.2 Why do voters re-elect corrupt politicians?

The literature on political corruption has consistently found that voters often fail to sanction representatives known to be corrupt (Banerjee et al., 2014; Chang et al., 2010; Klašnja & Tucker, 2013; Rundquist et al., 1977). This regularity is truly puzzling given that at the aggregate level, corruption has significant and negative consequences. The practice of corruption hinders institution-building (Praça, 2011), decreases the quality of democratic representation (Cinnanti, 2011; Johnston, 2005; Kunicová & Rose-Ackerman, 2005), and hurts developing countries' ability to attract foreign investment and reduce poverty and income inequality (Winters & Weitz-Shapiro, 2013); the *perception* of corruption by the public creates apathy in voters and increases rates of nonvoting (Canache & Allison, 2005; Davis et al., 2004) and threatens the survival of new democracies by fostering anti-system attitudes (Seligson, 2006).

[3] Of course, as the unit of analysis for this paper is the legislator and not the legislature, selecting a legislature with relatively high levels of corruption among its members does not constitute selection on the dependent variable.

One explanation for the persistence of corruption by elected officials in competitive democracies is that at lower levels of aggregation, corruption may actually be beneficial. Functionalist theory posits that corruption "provides immediate, specific, and concrete benefits to groups which might otherwise be alienated from society" (Huntington, 1968, p.64). In other words, corruption may be acceptable to voters who receive benefits from their corrupt representatives. These benefits may involve an illicit exchange between candidate and voter, such as outright vote-buying or the promise of public sector jobs to members of a candidate's patronage network. They may also (or instead) involve the provision of public goods: utilizing public resources to pay off supporters is, after all, a rational way for a resource-maximizing politician to retain the loyalty of her constituency. This is, again, what Winters and Weitz-Shapiro (2013) term the tradeoff hypothesis.

The functionalist framework supports the logic of the implicit exchange explanation: if political corruption is a "necessary evil to cut bureaucratic red tape, redistribute resources, and sustain socioeconomic development" (Manzetti & Wilson, 2007, p. 950) in poorly institutionalized or dysfunctional political systems, then communities that electorally support politicians willing to engage in corruption will benefit from those politicians' capacity to exploit the system in their favor. In other words, public goods provision to a corrupt politician's constituency is a plausible method of broad, network-based clientelistic exchange (as opposed to personal, specific patron-client relationships – see Heywood (1997)), and it may explain why corrupt politicians enjoy continued support even when their illicit behavior is publicly known.

This literature does not agree on the mechanism that links corruption and the proposed increase in the provision of public services. Corrupt politicians might maintain support from voters because politicians, upon being perceived as corrupt, engage in additional efforts to appease their constituents by providing pork (this is, for instance, the kind of tradeoff studied in Manzetti and Wilson (2007)). On the other hand, it is possible that corruption and public services correlate because corrupt politicians are perceived to be more competent. As Vera (2018) explains, corruption may be correlated with competence because voters overlook corruption by those officials whose illicit activity produces welfare-enhancing side effects. Thus, a selection process emerges: corruption and competence may be uncorrelated in the population of politicians who run for office, but positively correlated in the population of elected officials, particularly those who are not newcomers to politics. This selection explanation is different from the classic functionalist explanation,

which posits that corrupt politicians are perceived to be more competent because they are more likely to be well-connected, they are willing to bypass the regular procurement process for public works, and they have incentives expand the provision of public services at higher rates, because the contracting process provides opportunities for illicit enrichment on the part of officials.

Researchers have found some supporting evidence for different flavors of the tradeoff hypothesis in a variety of contexts: Rundquist et al. (1977) find that U.S. voters are more willing to overlook corruption by politicians whose programmatic appeals match their policy preferences; Fernández-Vázquez et al. (2016) find that in Spain, voters are more likely to re-elect corrupt mayors if they perceive that mayors' misuse of public funds created "positive externalities" for the welfare of the community; Zechmeister and Zizumbo-Colunga (2013) find that among voters in the Americas, those with better perception of personal economic well-being are more likely to tolerate political corruption; C. Pereira and Melo (2015), in perhaps the most striking finding in favor of the tradeoff hypothesis, find that corruption scandals have no effect on the reelection chances of Brazilian mayors who engage in high levels of public spending.

On the other hand, some studies have found only limited support for the tradeoff hypothesis. For example, Winters and Weitz-Shapiro (2013) find that Brazilian voters generally do not believe that people like them will vote for corrupt politicians who deliver public goods, with the exception of upper class and well-educated respondents; Banerjee and Pande (2007) and Banerjee et al. (2014) find that voters in the Indian state of Uttar Pradesh respond strongly to information on corruption by candidates, but that the ethnicization of voting increases the number of corrupt candidates who win seats in that state's legislature.

The studies above illustrate an omission that characterizes this literature. Investigations of the tradeoff hypothesis are concerned with two classes of questions. The first, best exemplified by Banerjee et al. (2014) and Winters and Weitz-Shapiro (2013), can be phrased as follows: are individuals willing to accept the 'tradeoff' between having corrupt representatives and receiving public goods if given the chance? The second, exemplified above by Fernández-Vázquez et al. (2016), is roughly: when corrupt politicians deliver public goods, are they more likely to receive public support?

Absent from this literature is a fundamental question, which I aim to answer in this chapter: do corrupt politicians actually provide higher levels of public goods to their constituencies? This question is at the center

of the tradeoff hypothesis, even though it is severely understudied[4]. The implicit exchange explanation cannot explain the high levels of support received by corrupt politicians in certain contexts (e.g., the aforementioned Uttar Pradesh legislature, or the Brazilian Congress) if these implicit exchanges do not actually take place.

1.3 Brazil as a crucial case for the tradeoff hypothesis

The federal legislature in Brazil contains all the conditions necessary for the tradeoff hypothesis to hold. First, the Brazilian government conforms the functionalist idea that corruption is beneficial in weakly institutionalized systems. The Brazilian federal legislature, on which I test the implicit exchange hypothesis, is one of the most fragmented in the world – which may indicate, as Roberts and Wibbels (1999) describe, weak and unstable parties and "unhinged" political representation. It is notoriously ineffective in its policymaking processes (see e.g., Samuels (2003)) and dysfunctional in its relationships with other branches of government (see e.g., Helmke (2017)). The federal legislature in Brazil is a crucial case for the hypothesis that, in the absence of strong formal institutions through which their demands may be heard and met by government, voters will prefer to be represented by politicians who know how to 'grease the wheels' of the system. It is, therefore, the institution I choose for the investigation below.

If Brazilian federal legislators participate in the politics of tradeoff, they most likely do so through the budgeting process. The federal budget's highly discretionary nature is one of the factors that makes the Brazilian legislature a crucial case for the tradeoff hypothesis. The 1988 Constitution states that federal legislators may submit individual amendments to the budget. The number and total monetary value of amendment proposals was unlimited until 2001, when Resolution 1/01 determined that every year, Congress would choose a limit to the total monetary value of budget amendments that can be approved (Praça, 2011). Further, Resolution 1/06 determined that no legislator may propose more than 25 amendments per year. Even within these limits, budgetary amendments can be substantial: in 2016, the approved amendments by members of

[4]One notable exception is Oliveros (2016), who investigates whether public servants in Argentina appointed through patronage agreements are more likely to provide services to voters. She finds support for her hypothesis. Her empirical design, like many of the other studies I cite above, involves a survey – more specifically, a list experiment with public employees of Argentinian municipalities.

the Chamber of Deputies added up to 7.86 billion BRL, or roughly 15.3 million BRL for each of the Chamber's 513 members. These funds are transferred to subnational entities, most commonly municipalities[5].

Praça (2011) lists other characteristics that make the Brazilian budgeting process particularly vulnerable to corruption, aside from the individual legislators's prerogative to individually amend the budget: a) the legislature can modify the rules of the budgeting process without needing to modify the constitution; b) both chambers of Congress craft the budget together in a joint committee, which eliminates horizontal checks; c) only this joint committee is responsible for budgeting, and it does so independently of the full Congress, which further eliminates scrutiny. Thus, if Brazilian voters elect knowingly corrupt politicians because they are better at 'working the system', or because they are more willing to engage in implicit exchanges, then those politicians, who have an incentive to provide pork to their constituents, will also have an incentive to do so through the highly permissive budgeting process.

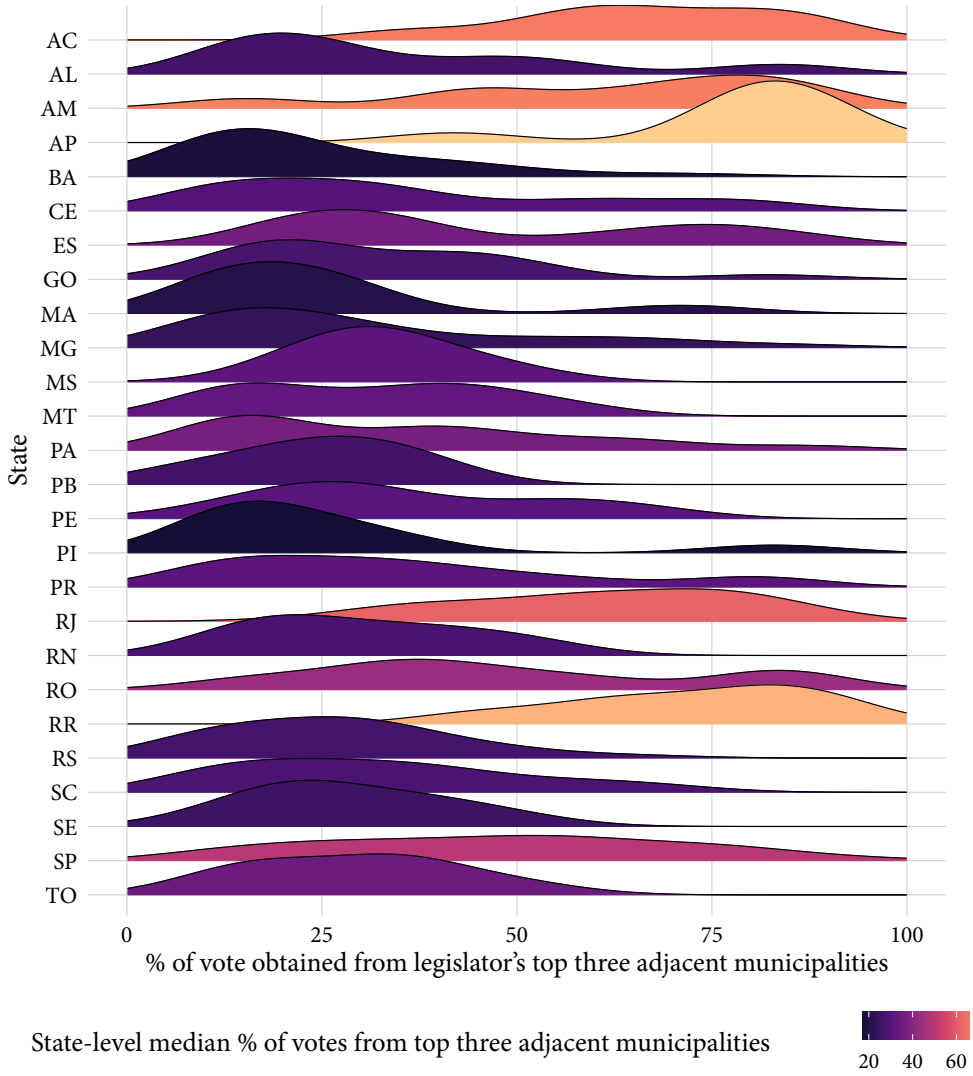
Finally, if Brazilian voters wish to build a relationship of implicit exchanges with their legislators, the electoral formula creates a further incentive: the Chamber of Deputies, elected by open-list PR with state-sized districts, offers candidates the chance to build relationships with very small constituencies, which can then be paid back with funds deriving from the federal budget. In Brazil, deputies are elected with very low numbers and percentages of the vote. In most Brazilian states, at least one federal deputy was elected with 50,000 or fewer votes in 2014; in states that elect more than the constitutionally defined minimum number of deputies, the mean winning candidate received only 5.4% of the vote. These very low barriers to entry make the implicit exchange strategy feasible: a voter who is part of a localized constituency, represented by a politician who engages in implicit exchanges through the budgeting process, is more likely to tolerate higher levels of corruption by her representative; a representative with a small enough constituency to buy off is more likely to satisfy the public goods demanded in return for corruption tolerance.

[5] More specifically, although the 'recipient' of a federal transfer may be a state, the 'beneficiary' of a transfer is almost always listed in federal data as a municipality. Simply put, the recipient is the entity that carries out a project (i.e., the entity to which the money is transferred), and the beneficiary is the location in which that project is carried out. In the analyses below, I consider the 'target' of pork to be the beneficiary of a transfer, not the recipient. The identity of a recipient is a legal matter, not one of public policy, and it is likely invisible to the public. The recipient/beneficiary distinction is usually due to the fact that certain policy areas are constitutionally designated as the exclusive responsibility of municipal governments (e.g., parks, public transport), while some are shared between states and municipalities (e.g., public education). Thus, federal transfers for certain types of public projects require that the recipient be a state. This is most commonly not the case, though, because the Federal Constitution gives great autonomy to municipalities (for details, see Articles 23, 30, and 186 of the Constitution).

Of course, the nature of open-list PR makes it possible that representatives do not have identifiable constituencies at all: a relatively small number of votes is needed to win a seat, but those votes can come from anywhere in a given state. If voters are scattered across a state, then no implicit exchange is possible: the smallest unit in Brazilian government is the municipality, and it is to municipalities that most discretionary federal transfers are directed. Thus, congresspeople who may wish to gain support from its constituency by engaging in tradeoffs would not be able to do so efficiently if their voter base is not sufficiently concentrated. I test a serious implication of this possibility on section 1.6, but I also attempt to address it descriptively here.

As Figure 1.1 shows, legislators tend to receive a significant share of their vote from very few localities. The figure depicts, as state-level density plots, the share of each legislator's vote obtained from her top three adjacent municipalities (i.e., the set of three neighboring municipalities from which each legislator obtained the highest share of her vote); each density plot is shaded by the median of that state-level vote share. In the figure, and in the descriptives reported below, the Federal District's eight deputies are not included, because there is only one municipality in the district — the federal capital, Brasília.

Figure 1.1: Distribution of vote concentrations among Brazilian federal legislators in the 2014 election



As the figure suggests, three neighboring municipalities alone are sufficient to account for around a quarter to a third of the vote of the median legislator. Indeed, the median vote share for the three neighbors from which legislators obtained the most support is 33.3%; the mean is 38.6%. 58 legislators — around one-eighth of the deputies considered here — received over three-fourths of their vote from three adjacent municipalities. For the state-level medians, the lowest is in Piauí, where the state's 10 congresspeople earned an average of 17% of the votes from their top three neighboring municipalities. On the other end of the spectrum, the median is above two-thirds in three states: Amazonas, Amapá, and Roraima (additionally, it is very close to

two-thirds — 66.4% — in Acre).

The choice of three adjacent municipalities for this calculation is, of course, arbitrary, but the pattern persists when considering non-adjacent municipalities as well. For example, the differences between the highest and second-highest municipalities for which legislators received votes are typically quite high: in the median case, the highest municipality contributes to 2.9 times more votes than the second highest; in the mean, 5.9 times more.

This suggests, at least for this sample of legislators elected in 2014, that it does not take a broad constituency to win a seat in the Chamber of Deputies — in many cases, a small, localized voter base will give a candidate almost all the votes she needs to obtain a seat. Additionally, in most cases, legislators will be able to identify a geographically clustered set of municipalities in which their voters live. This means that legislators are, indeed, incentivized to engage in the kind of tradeoff I hypothesize in this chapter; if they wish to provide pork in exchange for future votes, as a signal to voters that they will 'deliver', they should be able to identify the municipalities to which funds should be transferred.

1.4 Data and methods

1.4.1 Dependent variable

The dependent variable for this analysis is the amount of money added to the budget by legislators in amendments that list a municipality as the beneficiary(as opposed to a state government). Data on amendments to the 2016 and 2017 budgets was obtained from the Chamber of Deputies[6] This dataset contains every budget amendment by individual legislators for which money was earmarked to be transferred to subnational units. Crucially, this data is disaggregated at the level of the beneficiary (see footnote above). The data contains 8,036 unique budget amendments[7].

This data, at the amendment-beneficiary level, was aggregated to the level of the municipality-year by summing all transfers made to a municipality in a given year. This municipality-year sum is the dependent

[6] Data available at `https://www2.camara.leg.br/orcamento-da-uniao`.

[7] It may seem surprising that only 8,036 amendments exist in the data, given that each legislator may add 25 amendments. This would result in a total of 25,650 observations (513 legislators $\times$ 25 amendments $\times$ 2 years). The discrepancy is due to the fact that legislators are also limited by the amount of money they can add to the budget.

variable for the analysis below. The figures for 2017 were adjusted for inflation – thus, all transfers are expressed in constant 2016 Brazilian Reais (BRL). This dataset, excluding missing data, contains a total of 10,984 observations. As I discuss in further detail in subsection 1.4.4, this measure presents significant challenges for modeling, mostly due to its extreme skewness and its leptokurtosis.

1.4.2 Independent variables

The main independent variable in this analysis is the percentage of votes received in a municipality by deputies that were elected and who have been indicted with or convicted for a crime related to the abuse of their public functions.

Data on indictments and convictions was obtained from two sources: the first is a report from newspaper *Folha de S. Paulo* that collected data from the Supreme Court on federal deputies in April of 2016. The second is a more recent measure from a non-profit, *Ranking dos Políticos*, that collects data on federal legislators and applies a point-based system to 'rank' them[8].

The data from *Folha de S. Paulo* includes criminal charges and convictions against deputies that are unrelated to the abuse of their powers and privileges as public servants. I removed those before combining that data with the *Ranking dos Políticos* dataset, which includes only charges related to the abuse of public functions. Deputies whose crimes are excluded from the corruption variable include: Victório Galli of Mato Grosso, who has been charged with vehicular manslaughter; Franklin Lima of Minas Gerais, a pastor convicted for campaigning to his congregation during sermons; César Souza of Santa Catarina, indicted for an environmental crime; and Fausto Pinato of São Paulo, indicted for subornation of perjury in an attempt to frame an enemy of his father's for a crime. The final dataset contains 224 legislators with no indictments or convictions and 289 legislators who, in total, have faced 1,078 such charges.

[8]The project has been widely covered by the Brazilian news media, both positively (e.g., `https://politica.estadao.com.br/noticias/geral,plataformas-monitoram-atividade-de-politicos,70002352947`) and very negatively (e.g., `https://theintercept.com/2018/08/05/atencao-eleitor-nao-caia-no-engodo-chamado-ranking-dos-politicos/`) in the months preceding the 2018 general election. Notably, the criticism received by the project involves the politically biased 'ranking' it produces, not the information it publishes. Additionally, I have checked the *Ranking dos Políticos* dataset against the *Folha de S. Paulo* dataset for accuracy; most deputies have identical counts of indictments and convictions, and any differences reflect either the latter's broader scope (i.e., its inclusion of charges unrelated to corruption), or the former's more current data.

Around one in four indictments/convictions in this dataset fall under the Administrative Improbity Law of 1992, an anti-corruption law targeted at illicit enrichment by public officials. Among those cases, most refer to deputies' activities before joining the Chamber, as mayors, governors, or members of state legislatures or municipal councils. Generally, the data conforms to the observation of Ferraz and Finan (2011) that corruption schemes by Brazilian mayors usually involve "fraud in the procurement of public goods and services... overinvoicing of goods and services [and] embezzlement of public funds" (p. 1283).

The measure of a 'corrupt' politician by whether one has been indicted or convicted of a corruption-related crime is, of course, imperfect. I believe it is appropriate for this study, though, for a number of reasons: first, my objective is not to measure a simple relationship between corruption and the provision of public goods. Rather, the question I ask is whether voters — assumed to vote for corrupt politicians for material self-interest — actually have a rational basis for such behavior. Thus, measuring indictments and convictions is appropriate because these are the episodes of corruption that are publicized through the judiciary, a relatively trusted institution[9]. The role of the judiciary makes an indictment/conviction a credible signal to a citizen about a politician's 'type', while an accusation made in the press may not be seen as equally credible. The judiciary is also credible for a more elementary reason: as Rundquist et al. (1977) explain, most of the information on politicians' corrupt behavior is not at all credible to voters, as it comes from their political rivals and from the press, which is often mistrusted in developing countries without strong democratic traditions and a robust, professional news media (Winters & Weitz-Shapiro, 2013). Criminal charges and convictions, however, are more likely to be taken seriously, as "[only] when the corruption message comes from a source outside the electoral setting is it likely to be given credibility" (Rundquist et al., 1977, p. 955).

Criminal record data is "arguably a more objective measure of corruption [when compared to survey-based measures], *if* law enforcement and institutional setup are the same across all units of observation" (Lambsdorff & Schulze, 2015, p. 104); this is the case for my data, as it concerns federal crimes prosecuted at the federal level. The concern with asymmetrical enforcement of the law by courts is shared by Glaeser

[9]The 2013 wave of Latinobarómetro indicates that 41.1% of Brazilian respondents trust the judiciary, the third highest level among countries in the survey behind Costa Rica and Uruguay. The judiciary was the second most trusted institution in Brazil among those asked about in the survey, below then-President Dilma Rousseff but above the national and local governments, the public bureaucracy, Congress, and political parties.

and Saks (2006), who note that "in corrupt places, the judicial system is itself corrupt and fewer people will be charged with corrupt practices" (p. 1054) in justifying their choice to only use federal, and not local, law enforcement data in their measure of corruption in the United States. My measure differs from Glaeser and Saks (2006) and from e.g., Alt and Lassen (2012), however, in that I code both charges and convictions as evidence to voters that a legislator is corrupt, for reasons explained above. This makes my measure more similar to Schulze et al. (2016), whose measure of corruption in Russia is "the number of registered incidents of bribe acceptance" (p. 141). Schulze et al. justify the choice to use criminal reports instead of convictions for a variety of reasons, one of which also applies to this chapter: "the time span required... to complete criminal proceedings with a conviction" (p. 142) can be quite long in some legal systems. This is certainly the case in Brazil: Article 5, paragraph 57 of the Constitution [10] states that "no one shall be considered guilty before the issuing of a final and unappealable penal sentence". Given the length of the appelate process, particularly in cases with a federal legislator as a defendant, and given that corruption-related indictments are almost as widely publicized as the ensuing convictions (which may only happen many years later), coding only convictions as 'corruption' would lead to a severe undercount – especially because the point of my measure of corruption in this study is to capture the signals received by voters and how those voters respond.

I coded the main independent variable of interest as the percentage of votes in a municipality that were given to candidates who won seats and who have at least one indictment or conviction for a crime included in my dataset. Given that the relevant comparison for this test is between legislators known to be corrupt and those not known to be corrupt, I also built a variable for the percentage of the vote given to candidates who won seats and who have no charges. That is the relevant comparison; comparing the amounts of pork received by municipalities that help elect corrupt legislators with the amounts received by municipalities that vote for losing candidates is not an appropriate test of the tradeoff hypothesis.

[10] Official English translation available at https://bd.camara.leg.br/bd/handle/bdcamara/36019.

1.4.3 Controls

To control for the overall quality of life among a municipality's residents, I utilize median income, obtained from the 2010 Census. To control for violent crime rates, a highly salient political issue in Brazil[11], I utilize the murder rate per 100,000 population in 2014, obtained from the Ministry of Health's DATASUS database.

To control for poverty levels, I use the number of families in the municipality that are recipients of Bolsa Família[12] in 2014. This variable was obtained from a database of the Ministry for Social Development (MDS, *Ministério do Desenvolvimento Social*). Bolsa Família eligibility includes both families and individuals living in extreme poverty, defined by the Ministry of Social Development as earning less than 70 BRL (roughly 18.67 USD) per month[13], and families with children living in poverty (defined by the MDS as income per capita between 70 BRL and 140 BRL). To control for municipality wealth, I use municipality GDP, estimated by IBGE for 2014.

The control term for a municipality's population is the logged population in 2014, estimated by IBGE (*Instituto Brasileiro de Geografia e Estatística*, Brazilian Institute of Geography and Statistics). I built the models below with a logged population control, rather than building a federal-transfers-per-capita variable, because the effect of population on transfers is likely multiplicative, not linear; including a logged population term in the model captures this relationship, while dividing the outcome variable by population does not.

To account for a possible differential effect of political connectedness on the receipt of federal transfers, particularly in cases of favoritism by congresspeople, I include dummy variables for whether the mayor of a municipality belongs to one of the parties in the president's coalition in Congress – these are different groups of parties for the 2016 and 2017 budgets, as President Dilma Rousseff was impeached in 2016[14]. Additionally, to account for the possibility that a municipality's transfers of federal funds are in some way mediated by co-partisanship between municipal and state officials, I include a term for whether the mayor belongs to the

[11] 2016 UNODC data, available at rlhttps://dataunodc.un.org/crime/intentional-homicide-victims, shows that Brazil has the 8th highest intentional homicide rate in the world – 29.5 per 100,000 people – and the highest number of intentional homicide victims in the world. Data from the 2013 Latinobarómetro, the most recent wave prior to the legislative election considered here, shows that 1 in 5 Brazilian respondents point to violent crime as the most pressing political problem in the country.

[12] For a description of the program and its design, see Tepperman (2016).

[13] The 70 BRL figure is valid for 2014. In 2018, the cutoff for 'extreme poverty' is 89 BRL, roughly 23.73 USD.

[14] For the 2016 budget, passed in 2015 during President Rousseff's term, those parties were PT, PMDB, PSD, PP, PR, PRB, PDT, PROS, and PCdoB. For the 2017 budget, I use the congressional coalition of President Temer's: PMDB, PP, PSDB, PSD, DEM, PRB, PV, PTB, PR.

same party as her state's governor.

Finally, models include state- and year-fixed effects.

1.4.4 Modeling budget amendment amounts

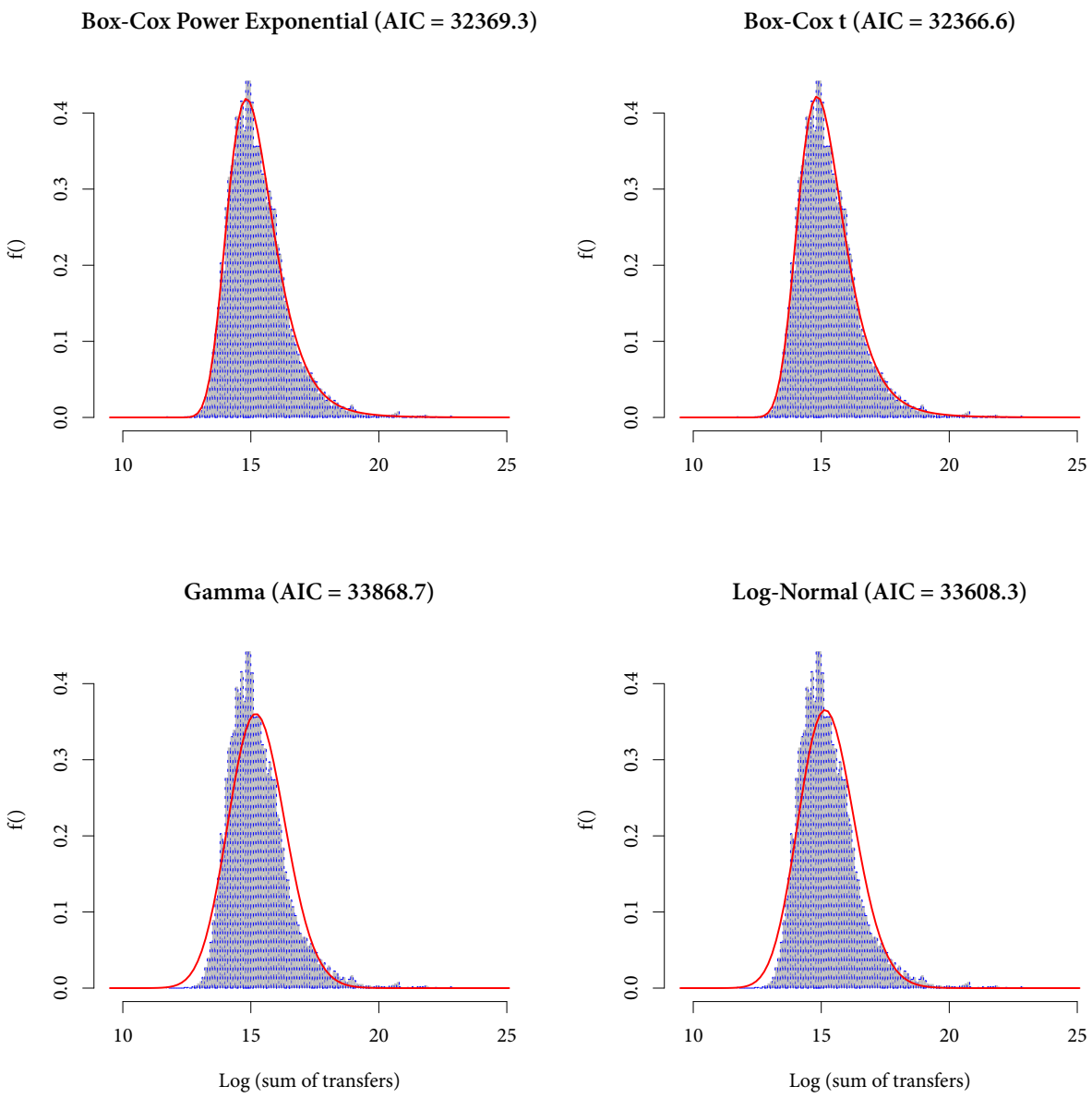
There are two significant obstacles to modeling the relationship described above. First, the dependent variable – the total amount of transfers to a given municipality in a given year introduced to the federal budget through amendments by Deputies – is highly zero-inflated. For this reason, a two-step model is used, one with a binomial outcome and the other with the non-zero, positive values (a setup similar to a hurdle model, but with a different choice for the second step, as described below). In the first stage, I build a logit model in which the outcome takes the value 1 *if the amount of money a municipality received is zero*, and 0 if the amount of money is *greater than* zero. This is a backwards logit: from a binomial dependent variable, this first step models the probability of a 'failure' rather than a 'success'. A predicted probability from this model is the predicted probability of an observation taking the value zero. This setup, although counter-intuitive, is the most appropriate for a two-step model of this kind: if the second step only includes observations that are included in the budget amendment process, then it makes sense for the first step to 'predict' the probability of an observation to be *excluded*.

The second obstacle is that the outcome variable is right-skewed and leptokurtic (the untransformed variable has excess kurtosis of 1478; the log-transformed variable, which I use in the models, has excess kurtosis of 3.29). To solve this problem, I build the models below with the Generalized Additive Model for Location, Scale, and Shape (GAMLSS) approach, developed by Rigby and Stasinopoulos (2005). GAMLSS is a semi-parametric regression model framework that allows for the modeling of parameters other than the mean (the 'location' parameter μ of a distribution) as a function of covariates. The GAMLSS framework explicitly models up to three other parameters (one 'scale' parameter σ and two 'shape' parameters, ν and τ – skewness and kurtosis, respectively) as an additive function of covariates.

With its greater flexibility, the GAMLSS framework relaxes the assumption of generalized linear models and generalized additive models that the response variable must follow a distribution in the exponential family. Thus, one can fit the data to a distribution that best fits it, in order to avoid misspecification (see e.g.,

Voudouris et al. (2012)). As an example, Figure 1.2 shows how the logged sum of transfers data fits four candidate distributions included in the R implementation of GAMLSS (Stasinopoulos, Rigby, et al., 2007). AICs listed on plot titles refer to the fitting method, which is a GAMLSS for each distribution with no covariates (i.e., parameters are determined fully as a function of the response variable). It is somewhat unsurprising that the Box-Cox t distribution would fit the data best: as Rigby and Stasinopoulos (2006) explain, the Box-Cox t distribution is particularly appropriate for models with strictly positive, heavily skewed, and leptokurtic dependent variables. Thus, all models below use a Box-Cox t distribution with a log link for μ.

Figure 1.2: Log of total transfers (constant 2016 values) and distribution fits with GAMLSS



The second challenge with the model is the possibility of spatial autocorrelation, both in the outcome (levels of federal transfers in a given year are likely to be more similar for municipalities that are close than for municipalities that are far apart because of demography, climate, etc., in ways that cannot be modeled with state-fixed effects) and in the independent variable of interest (a corrupt politician who seeks to build a small constituency that she can later 'buy off' with public money is more likely to look for supporters in

clusters of municipalities that are close). Indeed, Moran's I test on a GAMLSS identical to Model 1 below but without the spline leads to a rejection of the null hypothesis of no spatial autocorrelation, with a test statistic of 14.638 (p = 0.000).

To control for the spatial trend, I add a spherical smoothing spline to the models below with the longitudes and latitudes of the municipalities' centroids, with a penalty based on second derivatives[15]. For the models below, k was set to 200, a value chosen to avoid both the under-smoothing caused by `mgcv`'s default k of 50 and the over-smoothing that occurred at very high levels of k. Splines for the models below have estimated degrees of freedom between 109 and 110. All models below include this spline term. It effectively controls for the spatial trend: for example, Model 1 below has a Moran's I of -0.378 (p = 0.647).

1.5 Results and discussion

Table 1.1 shows results for the model described above. The model table is presented in two columns for legibility; the first column shows the results for the 'positive stage' – that in which only observations where $Y > 0$ are modeled – and the second shows results for the 'zero stage' (or the Zero Model); both are components of the same two-stage model. Models for μ, σ, and the Zero Model above have state-fixed effects. The model for μ and the Zero Model control for spatial autocorrelation with a thin-plate spline for longitude and latitude.

As the results show, municipalities receive more money in budget amendments if they vote for candidates who win seats; that is an expected, and entirely uninteresting, result. It does not mean, however, that voting for candidates with a known history of corruption pays off; the appropriate comparison in this case is between those with and without corruption charges and convictions. In that case, both the zero stage and the 'positive' stage of the model suggest that the support bases of corrupt legislators are not, in fact, engaging in a tradeoff — at least not in the most obvious, most straightforward way in which legislators can provide pork to their constituents.

Because the coefficients for the two independent variables of interest are quite small, due to the small effect sizes and due to the model using a log-link which naturally compresses the range of the dependent variable, I

[15] This spherical smoothing spline, as implemented in the R `mgcv` package, is a generalized thin plate spline (Wood, 2003) embedded in a 3D Euclidean space.

Table 1.1: Zero-adjusted GAMLSS model of voting for 'corrupt' vs. 'non-corrupt' legislators and budget amendment receipts

Dependent variable: Total amount received in budget amendments (Box-Cox T distribution)		
	'Positive' model	Zero model
μ (Intercept)	10.15^{***}	3.12^{***}
	(0.19)	(0.79)
μ Votes for legislators w. charges (1,000s)	0.00^{***}	-0.02^{*}
	(0.00)	(0.01)
μ Votes for legislators w/o charges (1,000s)	0.01^{***}	-0.03^{**}
	(0.00)	(0.01)
μ Population (log)	0.40^{***}	-0.48^{***}
	(0.03)	(0.10)
μ Median income	0.00^{***}	-0.00^{***}
	(0.00)	(0.00)
μ % illiterate adults	-0.00	0.00
	(0.00)	(0.01)
μ % rural	-0.00	-0.00
	(0.00)	(0.00)
μ Families in Bolsa Familia (log)	0.03	-0.19^{*}
	(0.03)	(0.08)
μ GDP (1,000,000s, log)	-0.07^{***}	0.13^{*}
	(0.02)	(0.05)
μ Murder rate (per 100,000s)	-0.00	0.00
	(0.00)	(0.00)
μ Mayor's party in coalition	-0.02	0.12^{*}
	(0.02)	(0.05)
μ Mayor in same party as governor	-0.05^{*}	0.17^{**}
	(0.02)	(0.06)
μ State capital	0.80^{***}	
	(0.15)	
σ (Intercept)	-0.44^{***}	
	(0.13)	
ν (Intercept)	0.14^{***}	
	(0.01)	
τ (Intercept)	1.95^{***}	
	(0.06)	
Num. obs.	10984	
Generalized AIC	215641.18	

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

present on Table 1.2 a series of predictions made on a simulated municipality that takes the median values of every continuous variable, is not a state capital, and is located in the state of Minas Gerais. For this simulated municipality, four possible voting profiles are compared, with the amount of vote for winning candidates with and without charges related to corruption varying between each.

Table 1.2: Predictions from the model on Table 1.1 (all covariates not displayed on table held constant at their mean)

Votes for legislators with charges	Votes for legislators without charges	$\hat{Y}\|Y > 0$	P(Y = 0)
0	6000	441,931.35	0.301
2000	4000	439,394.46	0.306
4000	2000	436,872.12	0.311
6000	0	434,364.27	0.316

As the table illustrates, this simulated municipality's predicted probability of receiving no money from federal budget amendments would be 1.5% higher if it had voted for legislators known to be corrupt rather than legislators without records of corrupt behavior; conditional on that municipality receiving some amount of federal funds through the individual budget amendment process, that amount would be roughly 1.7% smaller — around 7,500 BRL. This is an important result in light of the crucial, 'most-likely' nature of the Brazilian case, and of the implicit exchange explanation's implications regarding what types of corrupt politicians would receive popular support and win elections[16].

1.6 Mechanism test: the directed transfers assumption

The simplest, but also most damning, alternative explanation for the finding above is that congresspeople simply do not give preference to their electorate when allocating budget amendment funds. This could be due, for example, to the concern raised above that legislators' votes are not geographically concentrated. It

[16]This result is robust to less valid but more parsimonious model specifications, such as the non-inclusion of a spline for latitude and longitude.

could also happen because legislators simply do not consider the delivery of pork to their constituents important when allocating their funds to budget amendments,for whichever reason — lack of visibility, incentives toward bandwagoning behavior so that members of the same party, committee, or caucus allocate their funds together, etc.

This data, at the amendment-beneficiary level, was aggregated to the level of the legislator-municipality dyad to account for the fact that, even though not all legislators transferred money to all municipalities in their state, they could have done so. In other words, because a legislator may receive votes from any municipality in her state, the relevant unit of observation for this analysis is the legislator-municipality dyad for every municipality in a legislator's state.

In this legislator-municipality-level data, the dependent variable equals zero for 93.4% of observations (which is taken into account in the analysis below, in which I use zero-adjusted models). The 10,858 non-zero observations have mean 525,754 BRL and median 288,516 BRL.

Table 1.3 shows results for this mechanism test. Once again, the table shows the results of the same two-stage model in two columns for legibility. The first column shows the results for the 'positive stage' and the second shows results for the 'zero stage'; both are components of the same two-stage model.

The model's results suggest that the directed transfers assumption, necessary for the previous model, is reasonable in this context. It suggests that legislators are aware of their constituencies, either purposefully due to a tradeoff-like calculation, or (more plausibly) because candidates for office in an open-list, state-as-district election will receive votes from areas where they are well-known, well-connected, or where they focused their resources (with few exceptions, such as nationally-known celebrity candidates or the very top performing candidates).

As with the previous model, I present below a table of predicted values for this mechanism test. The middle column shows the predicted values for a typical municipality, conditional on the value not being zero; the right column shows the predicted probability for that value to be zero.

The results of this model are, of course, far from counter-intuitive: legislators transfer more funds from their individual amendments budgets to municipalities that voted for them in larger numbers. However, this model's results are important in lending further support to the idea I explain in section 1.3 when describing

Table 1.3: Zero-adjusted GAMLSS model of budget amendment receipts, by legislator × municipality dyads

Dependent variable: Total amount received in budget amendments (Box-Cox T distribution)		
	'Positive' model	Zero model
μ (Intercept)	12.14^{***}	2.52^{***}
	(0.11)	(0.27)
μ % of legislator's vote from municipality	0.05^{***}	-1.07^{***}
	(0.00)	(0.02)
μ Legislator had 0 vote in municipality	-0.11^{*}	2.01^{***}
	(0.05)	(0.08)
μ Mean NTL in municipality	0.00	0.02^{***}
	(0.00)	(0.00)
μ GDP (1,000s, log)	-0.02	0.03
	(0.02)	(0.03)
μ % illiterate adults	0.00	0.01^{***}
	(0.00)	(0.00)
μ % rural	-0.00	0.00^{**}
	(0.00)	(0.00)
μ Murder rate (per 100,000s)	-0.00	0.00^{***}
	(0.00)	(0.00)
μ Population (log)	0.10^{***}	-0.18^{***}
	(0.02)	(0.03)
μ Mayor & legislator in same party	0.22^{***}	-0.97^{***}
	(0.01)	(0.03)
σ (Intercept)	-0.68^{***}	
	(0.01)	
ν (Intercept)	0.04^{**}	
	(0.01)	
τ (Intercept)	1.31^{***}	
	(0.03)	
Num. obs.	164898	
Generalized AIC	360654.69	

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 1.4: Predictions for the model on Table 1.3 (all covariates not displayed on table held constant at their median)

Municipality provided % of legislator's votes	$\hat{Y}\|Y > 0$	P(Y = 0)
0	290,137.16	0.97
10%	326,910.13	0.80
20%	328,425.03	0.78
30%	329,946.94	0.76
40%	331,475.91	0.74
50%	333,011.97	0.72

how legislators' constituencies are quite concentrated, given that their districts comprise entire states. In those figures and descriptive statistics, I attempt to make the point that, if legislators are responding to the set of incentives that I assume in order to test the tradeoff hypothesis against this case, then they *should* be able to identify a personal constituency to transfer funds to. The models in this section suggest that legislators probably do engage in those calculations.

1.7 Concluding remarks

In this chapter, I have tested the underpinning of the hypothesis that politicians known to be corrupt by the public still win elections because the marker of 'corrupt' is a signal to voters that these politicians, for one reason or another[17], will provide to them once in power — the implicit exchange, or tradeoff, hypothesis. I have tested whether tradeoffs actually occur, in a case in which a tradeoff should be taking place (i.e., a circumstance where legislators are given the tools and the funds to provide pork legally, with nearly complete autonomy, and know their constituencies and are able to narrowly tailor their provision of material benefits to them). The analysis finds that there is no tradeoff. In fact, the reverse correlation takes place: corrupt politicians provide slightly less pork to their constituents via the channels I measure.

There is another question that is fundamental to the tradeoff hypothesis but usually goes unasked: do voters actually believe that corrupt politicians are better at delivering pork or public services to their constituents? Questions of this general category are asked somewhat frequently: for instance, Carreirão (2008) finds that there is no relation between support for rouba-mas-faz style politics and partisan identification among Brazilian voters, and Bonifácio (2013) finds that voters of lower educational attainment are more likely to approve of rouba-mas-faz politics. Both studies use data from the same survey, made up of value-laden questions about politically salient and sensitive topics, subject to significant levels of desirability bias. Moreover, this type of study has the same problem I described above when reviewing the broader tradeoff hypothesis literature. To explain why political corruption is common in a given context, it is not enough to

[17] In this case, the 'one reason' is that corrupt politicians understand that, due to their corruption having been revealed to voters, they must provide pork in order not to lose voters; the 'other' is that politicians willing to 'bend the rules' are better at navigating the systems that allow them to provide pork.

show that, if presented with a candidate who is corrupt and 'gets things done', a voter will choose her over an incompetent but honest challenger. Nonetheless, the question of whether voters *believe* that corrupt politicians deliver pork is arguably just as important as the question of whether it actually happens. Answering that question would, in conjunction with the findings in this chapter, reduce the tradeoff hypothesis to an information problem on the part of voters.

Of course, this study has untested assumptions (e.g., voters are aware of the pork from their federal legislators; voters are aware that their representatives are corrupt), and it also raises questions that it does not answer (e.g., in what other ways do corrupt legislators provide pork?). That is to say, as with almost any work of social science research, this chapter opens avenues for future research by not doing potentially important things. But it also advances the literature on the prevalence of political corruption in significant ways, by reframing the research question, by not relying on measures of corruption perception or surveys that take voters out of the social and political context under which they actually make decisions, and by shifting the focus to the investments made by political agents seeking to survive in power in a dysfunctional system.

2

You're Invited!: Partially closed procurement auctions as a proxy measure of rent-seeking behavior

2.1 Introduction

This chapter outlines the process of collecting data on municipal procurement in Brazil, describes the resulting data and some of its properties, and tests for potential biases arising from asymmetries in data availability. The dataset, which contains information on over one million different procurement processes — in some cases including data on every bidder and every item being procured — allows for comparisons across sub-national units under identical legal frameworks. This chapter is largely meant to set up the analyses presented in the next; the analyses in this chapter are meant to explore properties of the data itself, or investigate whether the data is rendered unrepresentative by the data availability problems that I describe below.

The dataset is important, first, because the public procurement market makes up a significant portion of Brazil's GDP. Estimates for the 2009-2012 period calculate the federal government's purchases at 8 to 9% of GDP (Mourão & Cantu, 2014; Ribeiro et al., 2018), and purchases by municipalities at around 3.5% of GDP (Ribeiro et al., 2018). Moreover, public procurement is largely where corruption occurs in local Brazilian governments. Ferraz and Finan (2011) mention several examples of procurement-related corruption schemes in Brazil: municipalities awarded contracts to phantom firms for non-existent construction projects so that public officials could illegally appropriate those resources; municipalities fabricated projects in order to award fake contracts to real firms (who were unaware that they had been awarded the contracts), so that public servants could create falsified copies of those firms' invoices and steal those resources; in one case, a municipality repeatedly published calls for bids for procurement auctions one hour before the auction was to take place, and in most of those auctions only one firm, owned by the mayor's brother, submitted bids.

Indeed, the three mechanisms that Ferraz and Finan (2011) mention as common ways for local politicians to appropriate resources ("fraud in the procurement of goods and services, diversion of funds, and [over-invoicing] of goods and services" (p. 1281)) are all closely related to procurement — the first quite obviously, and the other two as a result of it. Rose-Ackerman (1975) explains that essentially "two variables" can be affected by fraud in public contracting: "the identity of the seller and the terms on which the sale is made" (p. 188). Over-invoicing is a form of the latter and, as Fazekas et al. (2013) and Achua (2011) demonstrate, diversion of funds is commonly achieved through the procurement process and frequently involves manipulating both of Rose-Ackerman's 'variables'.

Particular to Brazil is a type of procurement auction, the invitation to bid[1], that facilitates corrupt action through decreased internal accountability and external transparency. In this paper, I show preliminary evidence that the invitation to bid is a venue for corruption. It allows for several 'red-flag' distortions in the procurement process: "shortened time [spans] for the bidding process" (Ferwerda et al., 2017, p. 250); frequent "tinkering with... expected contract value thresholds" (Fazekas et al., 2013, p. 20) so that auctions can be conducted by invitations to bid; and, most blatantly, giving "public officials... the power to decide which enterprises to invite to the tender" (Søreide, 2002, p. 14), which, combined with the power to shorten time spans for bidding, makes it so that "only the [companies] already informed have time to prepare" (Søreide, 2002, p. 14).

In the following sections, first I briefly describe the Brazilian legal arrangements on procurement and the different regimes through which public entities may procure goods and services or engineering projects. This next section is fundamentally important so that the measurement of rent-seeking behavior that I use for my analyses in the next chapter — a procurement modality that lends itself to corruption much more easily than to efficient acquisition processes — is properly motivated. It also serves as a potentially useful example of how procurement processes can be used to discern and detect 'odd' behavior that may be indicative of corruption, an idea that transcends the case study under consideration here and suggests the broader use of public procurement as a tool for detection and measurement in the literature on political corruption.

Second, I discuss the aforementioned invitation to bid modality, focusing on its marked differences to

[1] The translations to English of the names of Brazilian procurement modalities follow, for the most part, those of Mattos (2016).

other modalities and how those differences make the invitation to bid a particularly attractive venue for corruption. Third, I describe how the dataset on municipal procurement was collected, focusing on why that data exists at an aggregated level to begin with, why the data is available for some parts of Brazil but not for others, and why public data availability should not mean that some municipalities operate 'in the shadows', immune from government accountability. Fourth, I test whether municipalities for which procurement data is unavailable are more likely to fail random audits by the federal government — a proxy for whether those municipalities conduct their procurement processes more dishonestly — and find that there is no difference. Finally, I check whether the procurement data is reliable (i.e., whether it appears to have been fabricated by municipalities, and whether it conforms to substantive expectations of its distribution) by checking its values against Benford's law, a commonly used first-pass tool for fraud detection in finance and accounting.

2.2 Procurement in Brazil

Law 8666/93[2], Law 10520/02[3], and Decree 9412/18[4] establish eight modalities of contracting procedures that Brazilian government entities may conduct[5]. Two of them do not involve procurement: the 'auction' modality is intended for the *sale* of goods by a public entity, while the 'contest' modality is intended to reward particularly meritorious artistic or scientific works. These are not mentioned hereafter.

Out of the remaining six modalities, four directly refer to the procurement of goods and services. Table 2.1 briefly describes each. As the table shows, the first three modalities (invitation to bid, request for proposals, and competitive bidding) differ mostly in their openness (from least to most open to participation) as well as their price limits (from cheapest to most expensive contracts). The reverse bidding modality was created in 2002 to simplify the process of procuring "ordinary goods and services" (C. A. G. Pereira, 2012), that is, those in which the technical aspects of the bid are irrelevant (e.g., the purchase of gasoline for a municipality's vehicles). It takes place in two stages: in the first, closed bids may be submitted freely, along with documents

[2]Full text available at http://www.planalto.gov.br/ccivil_03/leis/l8666cons.htm.

[3]Full text available at http://www.planalto.gov.br/ccivil_03/Leis/2002/L10520.htm.

[4]Full text available at http://www.planalto.gov.br/ccivil_03/_Ato2015-2018/2018/Decreto/D9412.htm.

[5]Decree 5.450/05 (http://www.planalto.gov.br/ccivil_03/_ato2004-2006/2005/decreto/d5450.htm) and Decree 10.024/19 (http://www.planalto.gov.br/ccivil_03/_Ato2019-2022/2019/Decreto/D10024.htm) are also relevant: both relate to the reverse bidding modality, regulating how and under what conditions it may be conducted online.

showing that the bidder is qualified to fulfill the contract's required terms. In the second, the lowest bidder and every bidder whose bid did not exceed 110% of the lowest bid (or, if this rule does not produce at least three bidders, the three lowest bidders) engage in a round of open bidding. Only then must the procurement commission verify that the winner meets the requirements of the contract. In competitive biddings and requests for proposals, on the other hand, every bidder's eligibility (and, in the case of requests for proposals, registration status) must be reviewed by the commission before bidding can take place. In auctions by invitation, the commission or public servant responsible for the process must verify that participants are qualified before inviting them; participants who were not invited must submit evidence, which then must be reviewed before bidding takes place.

Table 2.1: Modalities of procurement in Brazil

Category	**Auction?**	**Bidder must submit proof of eligibility**	**Needs to be conducted by a commission?**
Invitation to bid	Yes	Only parties who were not invited	No
Request for proposals	Yes	Yes, and must be previously registered	Yes
Competitive bidding	Yes	Yes	Yes
reverse bidding (in-person or online)	Yes, in two stages	Yes, but only the winner's eligibility must be verified	No (conducted by auctioneer)

Table 2.2: Contract value limits for modalities of auctions

Category	**Contract value limits (in BRL, since 2018)**	**Contract value limits (in BRL, before 2018)**
Invitation to bid	Up to 330,000 for engineering works and services, up to 176,000 for goods and services	up to 150,000 for engineering works and services, up to 80,000 for goods and services
Request for proposals	Up to 3,300,000 for engineering works and services, up to 1,400,000 for goods and services	Up to 1,500,000 for engineering works and services, up to 650,000 for goods and services
Competitive bidding	Above 3,300,000 for engineering works and services, above 1,400,000 for goods and services	Above 1,500,000 for engineering works and services, above 650,000 for goods and services
reverse bidding (in-person or online)	None	None

Table 2.3: Types of procurement without auctions

Category	Description	Legal justifications
Waiver	Auction could be conducted, but would be against public interest	L. 8666/93 lists 35 valid causes for the auction requirement to be waived, for example: • Contract value is less than 10% of the limit for the invitation to bid modality • An auction for the same contract was conducted, but every bid far exceeded the market price of the good being procured • Good being procured is urgently needed (e.g., during a state of emergency)
Non-requirement	Competitive auction would be impossible or absurd	• Good can only be procured from one producer or distributor (brand preferences — Apple vs. Dell laptops — do not qualify under this item) • Services from highly specialized professionals (advertising agencies are explicitly mentioned in L. 8666/93 as not qualifying under this item) • Performances from artists or entertainers at events organized by a public entity

There is one other type of auction important to this study: the price registration (*registro de preços*, sometimes *sistema de registro de preços* or SRP). It serves a particular purpose defined by law that differs from an ordinary procurement process: "parties interested in selling goods or providing services to [a public entity] indicate their prices... and the amounts they can supply, for a future contract" (Medauar, 2008, p. 186). The price registration is meant for "routine purchases of standardized goods or services" (Mello, 2014, p. 587),

so that the government can conduct one auction and then make repeated purchases of the same good or service, as needed, from the winner of that auction. The winner of a price registration auction is registered with the government as the preferred supplier of a certain good or service, but price registrations are not used for immediate acquisitions.

It creates a binding legal relationship, but not a contract. According to Bado (2017), the winning bidder of a price registration auction must supply the good or service at the price and quantity informed to the government, for a period of up to a year, in quantities up to a limit stipulated by the government in the call for bids. In return, the government is barred from procuring the same good or service from any other supplier for the same price. This exclusivity does not obligate the government to buy that good or service at all, however.

The price registration is, therefore, not a modality for direct procurement. It serves a similar, but separate purpose. Given that those auctions do not lead to a contract right away, or to the guarantee of a contract at a later date, any price registration auction in my dataset is excluded from all analyses.

2.3 Invitations to bid as a measure of rent-seeking behavior

The invitation to bid is commonly practiced in Brazil — around 11% of municipal procurement processes in my dataset occur through that modality — even though it violates several 'best practices' for public procurement and even though its use was superseded by the online reverse bidding, formally instituted in 2005, which has caused "the invitation to bid [to become] an exception" (dos Reis & Bugni, 2017, p. 156) in procurement processes by federal government agencies, because it has none of the potential legal violations of the invitation to bid but provides greater savings to the government (dos Reis & Bugni, 2017), promotes competition instead of hindering it, and, on average, takes less time than an auction through invitation to bid (de Faria et al., 2011).

Invitations to bid allow local governments to leave proceedings entirely at the discretion of a single public official, which exacerbates the risk of corruption, particularly in a developing country such as Brazil (Ware et al., 2007); they also have very weak publicity requirements, which increases entry costs for firms that are

not invited to participate and thus reduces the number of entrants. For these reasons, Ortega (2015) argues that auctions by invitation to bid are unconstitutional. At least, they defeat the purpose of a system of public procurement through open, competitive auctions.

The publicity requirement is worth further discussion. Publicity reduces entry costs for potential bidders and, through increased competition, prices paid by governments: Leslie and Zoido (2011), for example, find that public hospitals in Buenos Aires pay around 3% less for drugs when auctions are more widely publicized; Coviello and Mariniello (2014) identify that publicity significantly lowers the prices paid by Italian governments for a variety of goods, and that this is explained entirely by the increase in the number of bidders brought by increased amounts of publicity.

Auctions through invitations to bid in Brazil violate the publicity requirement in two main ways. First: in 'open' procurement processes, the government must announce any upcoming auction in its official gazette[6]. In invitations to bid, this requirement does not exist. Rather, the government must "affix, in an appropriate location, a copy of the invitation letter" (Law No. 8666/93, Art. 22, §3). The choice of an 'appropriate location' is, of course, open to an official's discretion, and it may be strategic to further hinder the ability of non-invited parties to participate. Second: auctions by invitation have the shortest period of time between the publishing (or 'affixing') of the call for bids and the deadline for submission of bids. In most other types of auction defined in Brazilian law, an upcoming auction must be publicized between 30 and 45 days before the deadline; in an auction by invitation, that minimum period is five business days.

These two violations of the publicity requirement are part of why the auction by invitation is "incomprehensible... [constituting] flagrant violation [of constitutional principles].... [conducive] to subjectivity, favoritism, and corruption" (Marra, 2006, p.199).

There is some support in the literature for the argument that invitations to bid are corrupt *per se*, and not simply a venue for rent-seeking. For example, Fazekas and Wachs (2020) define corruption in public

[6] In Brazil, municipalities, states, and the federal government must publish daily journals (known as *Diário Oficial*). These journals contain the texts of decrees, laws that come into force on that day, regulatory changes, etc. Upcoming auctions must be publicized in these journals. For example, the Diário Oficial of the municipality of São Paulo, on January 14, 2020, contains the text of a law signed by the mayor on January 13 that forbids the use of single-use plastic cups, plates, and straws by establishments such as restaurants and bars. It also contains 12 pages of information on upcoming auctions. Although the practice is not common among state and municipal governments in the United States, the federal U.S. government does publish the Federal Register, which is similar in content to a Brazilian Diário Oficial.

Table 2.4: Rates of publications of calls for bids on national holidays, by state and whether auction is by invitation to bid

State	% invitations published on a holiday	% non-invitation auctions published on a holiday
Ceará	0.64%	0.84%
Goiás	0.46%	0.41%
Minas Gerais	0.69%	1.12%
Pará	1.05%	1.32%
Paraná	0.63%	0.59%
Pernambuco	1.28%	1.70%
Piauí	0.94%	1.00%
Rio Grande do Sul	0.85%	1.26%
Roraima	1.48%	0.73%
Tocantins	1.24%	0.78%

procurement as "the allocation and performance of government contracts in violation of prior explicit rules and principles of open and fair public procurement to benefit a closed network while denying access to others" (p. 154). The definition does not apply fully to invitations to bid, because participation in those auctions is not entirely 'closed'; the violation of the principles of open public procurement is only partial — as mentioned above, the publication requirement is replaced by a requirement to "affix... a copy of the invitation letter".

Indeed, the partial nature of this violation is made clear when analyzing my dataset against one of the mechanisms for corruption described by Søreide (2002): governments may limit the participation of non-invited entrants by "making the tender public during [holidays]" (p. 14). As Table 2.4 shows, in most of the states for which I have data on publication, invitations to bid are slightly *less* likely to be published on holidays than auctions of other types[7]. It is difficult to draw conclusions from these figures alone: they may imply good faith from public officials, but they may imply that the requirements for publication for invitations to bid already accomplish the goal of excluding non-invited entrants, so publication on a holiday is unnecessary.

Indeed, these minimal publication requirements do not, in general, attract many non-invited entrants. To my knowledge, only Rio Grande do Sul includes data on which bidders were invited. Among the 12,539

[7] In this calculation, I only included national holidays, which are 11 per year but take up 12 days, as Carnaval is a two-day holiday. In practice, however, the number of days that are legal holidays in Brazil varies yearly, because it is common to extend holidays that fall on Tuesdays and Thursdays to also include the preceding Monday or the following Friday, respectively. Thus, for example, 2001 and 2018 had 17 'holidays', while 2002 and 2019 had 13. I count these 'extended' Mondays and Fridays as holidays; the row-wise comparisons on Table 2.4 would be essentially unchanged without their inclusion.

auctions conducted by invitations to bid in that state's municipalities between 2013 and 2019, 14.1% received a bid from a non-invited party. Among those auctions with bids from non-invited entrants, the median number of non-invited bidders is 1 (mean 1.92).

In every other state, it is not possible to find how many participants entered due to the publication, or affixing, of those invitation letters. But because the minimum number of parties that must receive invitations to bid is three, that number can be used for a rough estimation. For example, from 2003 to 2018 in Paraíba, only 5.9% of 'invitation to bid' auctions received more than three bids; that figure was 6.4% in Ceará between 2010 and 2018. Even then, receiving more than three bids does not indicate that a non-invited entrant submitted a bid: three is the minimum number of invitations that must be sent, but there is no maximum number; it is possible that some of the auctions with three or more bidders did not include a non-invited entrant. On the other hand, receiving an invitation to bid does not mean that one will submit a bid, thus it is also possible that some of the auctions with three bidders included a non-invited entrant.

In any case — and even if the argument that semi-open auctions are inherently corrupt — the rate of auctions by invitation to bid by a government is certainly a valid proxy for the creation of rents that could be captured in an illicit manner. The role of this rate is analogous to the role of the 'treatment' in an intention-to-treat design (for a short explanation, see Sheiner and Rubin (1995)), where an auction by invitation to bid represents the exposure or encouragement to engage in corruption, but not all events of exposure actually result in treatment. Given that auctions by invitation to bid are legally dubious *per se*, and that its original purpose (to make procurement faster for smaller contracts) has been superseded by the introduction of electronic modalities of procurement, conducting auctions by invitation to bid at high rates is an indication that, at least, a severe principal-agent problem exists between taxpayers and the government entity conducting the auction.

2.4 The dataset

My dataset of municipal procurement auctions uses a convenience sample of Brazilian states whose Court of Auditors publishes the relevant information online, open to the public[8]. In this section, I explain what the Courts of Auditors are, including their history, their legal powers, and why the 'convenience sample' is not necessarily a source of bias for my analysis (in short, this is because the federal constitution of Brazil binds every Court of Auditors to the same set of powers and legal obligations, which none can deviate from. Thus, the non-existence of publicly available data does not imply that the Courts do not collect such data.) I also explain how the data was scraped from each Court's website, to clarify that the differences in data availability do not imply differences in the 'thoroughness' of each Court's monitoring of the municipalities under their jurisdiction.

2.4.1 The legal framework of Brazilian Courts of Auditors

Municipalities send information on their procurement processes and the resulting contracts to their state's Court of Auditors (*Tribunal de Contas do Estado*, commonly referred to as 'TCE'). The basic functions of the Court of Auditors, at every level of government, are to monitor and audit accounts, contracts, and budget implementation of all public entities under the Court's jurisdiction. Courts of Auditors are independent bodies: they are not part of the executive, judiciary, or legislative powers. Their purpose, as Art. 71 of the Brazilian Constitution explains, is to "assist" the legislative power in external control of public accounts. However, a Court of Auditors is "not an auxiliary body to [the legislature] in the sense of hierarchical inferiority" (Britto, 2001, p. 3).

There are 33 Courts of Auditors in Brazil: the Federal Court of Auditors (*Tribunal de Contas da União*, commonly referred to as 'TCU'); a state Court of Auditors in each of the 27 states; a municipal Court of Auditors in São Paulo and one in Rio de Janeiro, the country's largest municipalities; and three state-level Municipality Courts of Auditors in the states of Bahia, Goiás, and Pará[9]. The current Brazilian constitution,

[8] There is one state, Goiás, that makes some information available to the public while withholding some behind a portal for which one has to register; I contacted the Goiás Court and was informed that registration is only open to employees of the state or of municipalities in the state.

[9] These are state Courts responsible for monitoring and auditing the municipalities in their respective states; they do not serve as additional accountability mechanisms on municipalities, as the functions of these Municipality Courts of Auditors are exercised

which came into effect in 1988, forbids the creation of new Courts of Auditors. Thus, the two municipal-level Courts and the three state-level Municipality Courts are exceptions to the more 'usual' administrative arrangement, in which a singular Court of Auditors is responsible for the accounts of state and municipal governments. Understanding the legal and institutional origins of these 'anomalous' cases is important, as their presence could (but, in practice, do not, as I explain below) mean that public officials face different sets of incentives in jurisdictions with or without Municipal or Municipality Courts.

The municipalities of Rio de Janeiro and São Paulo first created their own municipal-level Courts because Art. 22 of the Brazilian constitution of 1946 allowed each state's legislature to decide whether municipalities could have municipal Courts of Auditors. However, Art. 191 of the 1967 Constitution (introduced by amendment in 1969) extinguished all Courts of Auditors at the municipal level, except São Paulo's; Art. 16 of that amended constitution established that municipalities could only create new Courts of Auditors if its population and GDP were above certain thresholds. This provision, particularly the population threshold of two million inhabitants, excluded any municipality other than Rio de Janeiro (Instituto Brasileiro de Geografia e Estatística, 1975). That municipality created its Court of Auditors in 1980. As Art. 31 of the current Brazilian Constitution bans the creation of municipal Courts of Auditors, the only municipal Courts that can exist today are the two that already existed by 1988. Importantly for this work, municipalities with their own Courts of Auditors are *not* under the jurisdiction of their state's Court[10].

Because the municipalities of São Paulo and Rio de Janeiro have their own Courts of Auditors, those municipalities are excluded from my dataset, even though data for São Paulo is also available from the state Court.

The three state-level Municipality Courts were created before the 1988 Constitution came into effect: Bahia's in 1971, Goiás's in 1977, and Pará's in 1980. The state of Ceará also had a Municipality Court of Auditors; it was extinguished by state law in 2017. The existence of a Municipality Court does not imply, at least legally, that the municipalities in those states are subject to higher levels of scrutiny. Instead, in states

by the state Court in other states. The nomenclature is only slightly less confusing in Portuguese than it is in English: a municipal-level Court of Auditors is a 'Tribunal de Contas do Município', while a state-level Municipality Court is a 'Tribunal de Contas dos Municípios'.

[10] Art. 31 of the Constitution states that "municipal legislatures will oversee [government spending, accounts, etc.] with the assistance of the State Court of Auditors *or* [emphasis added] the Municipal Court of Auditors, where it exists."

with Municipality Courts, the state's Court is not responsible for assisting municipal legislatures in monitoring and auditing municipal accounts. As I explain below, federal law requires that the Court of Auditors in a state perform a certain set of functions; in states with Municipality Courts, those functions are split between that institution and the State Court. The budgets of State Courts can provide a crude illustration of how their functions are divided, and not duplicated, with Municipality Courts. In Bahia, the State Court had a budget of approximately BRL 258 million for 2019, while the Municipality Court's budget was approximately BRL 185 million (Governo do Estado da Bahia, 2018). The state of Paraná, closest to Bahia in number of municipalities and population, allocated BRL 481 million to its State Court (Governo do Estado do Paraná, 2018, p. 128-129) — only 8% more than Bahia's two Courts of Auditors combined. Another example is the budget of Ceará's State Court: it grew from BRL 77 million in 2016, when the Municipality Court was still in existence, to BRL 123 million in 2017, when it absorbed the functions of that Court, and to BRL 199 million in 2019[11].

2.4.2 Courts of Auditors and the power to monitor procurement auctions

The state Courts of Auditors have the same legal obligations and prerogatives of the Federal Court of Auditors. This normative symmetry appears on Article 75 of the Brazilian Constitution, which states that Articles 70-74, delineating the competencies of the TCU, apply to the Courts of Auditors in the states 'where applicable'. Although Article 75 gives each state's constitution the power to specify how its Court of Auditors will function, "they may only do so within the limits of the [Federal Constitution]" (Rodrigues & de Alcântara, 2013, p. 51). These limits do not function simply as an 'upper bound' on the powers of the Courts of Auditors: "states cannot run afoul of the arrangement given to the Federal Court of Auditors [by the Constitution]" (Fernandes, 1999, p. 170). According to Macieira (2009), the Brazilian Supreme Court has ruled in that respect: "states are [constitutionally] obliged to organize their Courts of Auditors according to the federal model" (p. 51). Therefore, although state Courts are all independent from each other and from their federal counterpart, each state does not have the right to set a different 'level' of accountability for itself, or to decide which aspects of its governance are subject to monitoring by the Court of Auditors. In every state, Courts of Auditors are

[11] Data available at `https://www.tce.ce.gov.br/despesas/comparativo-anual`.

responsible for monitoring "budget executions [and] all financial and administrative acts by governmental bodies" (Mileski, 2004, p. 10).

This mandate, of course, indicates that Courts of Auditors' powers extend to all contracts signed by government entities under their jurisdiction. And, as Mileski (2004) explains, the power to audit contracts implies the power to audit the auctions that led to those contracts: given that every purchase of goods and services must be preceded by an auction, determining the legality of a contract requires auditing the process through which that good or service was procured. Thus, Courts of Auditors have the power to request information about procurement processes of all public entities under their jurisdiction.

Many, but not all, state Courts make this data on municipality-level procurement auctions available online, in formats that range from completely machine-readable to actively hostile to collection. An example of the former is the Court of Auditors of Minas Gerais, which makes its data available as a series of comma-separated values (csv) files, easily readable in any statistical computing software. An example of the latter is the Court of Auditors of Acre, which publishes its data in a format that cannot be downloaded 'in bulk' or scraped efficiently[12] — each auction's information exists in a separate webpage, and each must be visited and parsed in order to collect the relevant data.

The amount of information available on each auction varies across states almost as much as the format in which data is presented. For instance, the Court of Auditors of Acre does not make important information available, such as the estimated values of contracts for auctions held before 2019.

2.5 Testing for potential sources of bias

In subsection 2.4.2, I explain that all Courts of Auditors must, by law, collect the data I seek here, even though some Courts do not make that data publicly available. Here, I test a possible source of non-representativeness of my convenience sample of municipalities only from states that publish data.

[12] For example with HTTP GET requests, which can easily be parallelized and will, in general, pull data from webpages much faster than a browser, as GET requests do not load scripts, 'cookies', images, etc., which most websites serve but which are usually unnecessary for the task I discuss here.

This potential non-representativeness would emerge if, given that publication of auction and contract data exposes a municipal government to greater levels of scrutiny, the behavior of municipal governments in 'open-data' states differs significantly from the behavior of municipal governments in 'closed-data' states. If this is the case, then any attempt at inference using my sample and involving corruption-related dependent variables will not capture the correct parameter values being estimated. Such bias does not depend on whether the change in behavior is a legitimate shift towards better governance or an attempt to hide corruption by, for example, submitting incomplete or false information. In either case, the sample would not allow me to estimate the true parameter values in a test as described above.

In this section and the next, I present the results of two series of tests for these mechanisms that would lead to non-representativeness. First, I test for whether public availability of data genuinely improves behavior, by testing whether municipalities in open-data states are less likely to 'fail' audits by the federal government. Then, I investigate whether municipalities submit fabricated data to state governments by testing the expected monetary values of contracts against the Benford distribution. The test, although somewhat rudimentary, is a well-recognized 'first-pass' attempt at detecting fraudulent data.

2.5.1 Are municipalities in open-data states less likely to fail federal audits?

To test whether public availability of procurement data has an effect on the behavior of municipal governments, I built a series of models with different measures of federal audit violations as outcome variables and a measure of exposure to accountability as the main independent variables.

Federal audit data is from the Comptroller General of Brazil (*Controladoria Geral da União*, 'CGU'), whose anticorruption program audited the expenditures of randomly selected municipal governments. This random audit program started in 2003 and continued until 2016. Since 2017, selection is no longer random; a risk assessment method is used to select which municipalities are audited. The dataset used in this analysis includes all audits selected by lottery since 2006. In that period, 1,184 municipalities were audited; 94 were audited twice, and 6 three times. As explained in subsection 2.4.1, the municipality of São Paulo (randomly audited in 2016) is excluded from the data. The dataset thus includes 1,289 observations, one for each audit.

The dependent variables are measured at the level of the audit, and the independent variables and controls are measured either at the municipality/year level or at the state/year level.

The data on audits from CGU includes, for each audit, a series of notes summarizing the auditors's findings. Each of those is classified according to the gravity of the violation detected: 'information' if there is no violation or a minor violation of norms[13], 'formal violation' if auditors find that laws or regulations were violated, but that those violations were not judged to have caused damage to the public coffers[14]; 'medium violation' if auditors uncover an illegal or illegitimate act that may have caused damage to the public[15]; and 'serious violation' if an illegal or illegitimate act is uncovered that grossly deviates from the law, or that gravely damages the public coffers[16] (Controladoria-Geral da União, 2018). For the models below, I code a municipality as having committed a 'violation' if it is listed as medium or serious by the CGU. There are two classes of dependent variables in the models: 'procurement-related' refers to violations regarding the procurement process generally, in any of its modalities (including waivers and non-requirements); 'invitation-related' specifically refers to violations in auctions conducted by the invitation to bid modality.

The main independent variable in these models is a binary variable for whether a state's Court of Auditors published data on municipal procurement auctions for the year the audit occurred. Figure 2.1 shows whether states make data publicly available, and the earliest year of data availability. Labels also include the number of audits in years for which data is publicly available. Of the 1289 audits in the CGU data, 437 fall within this category.

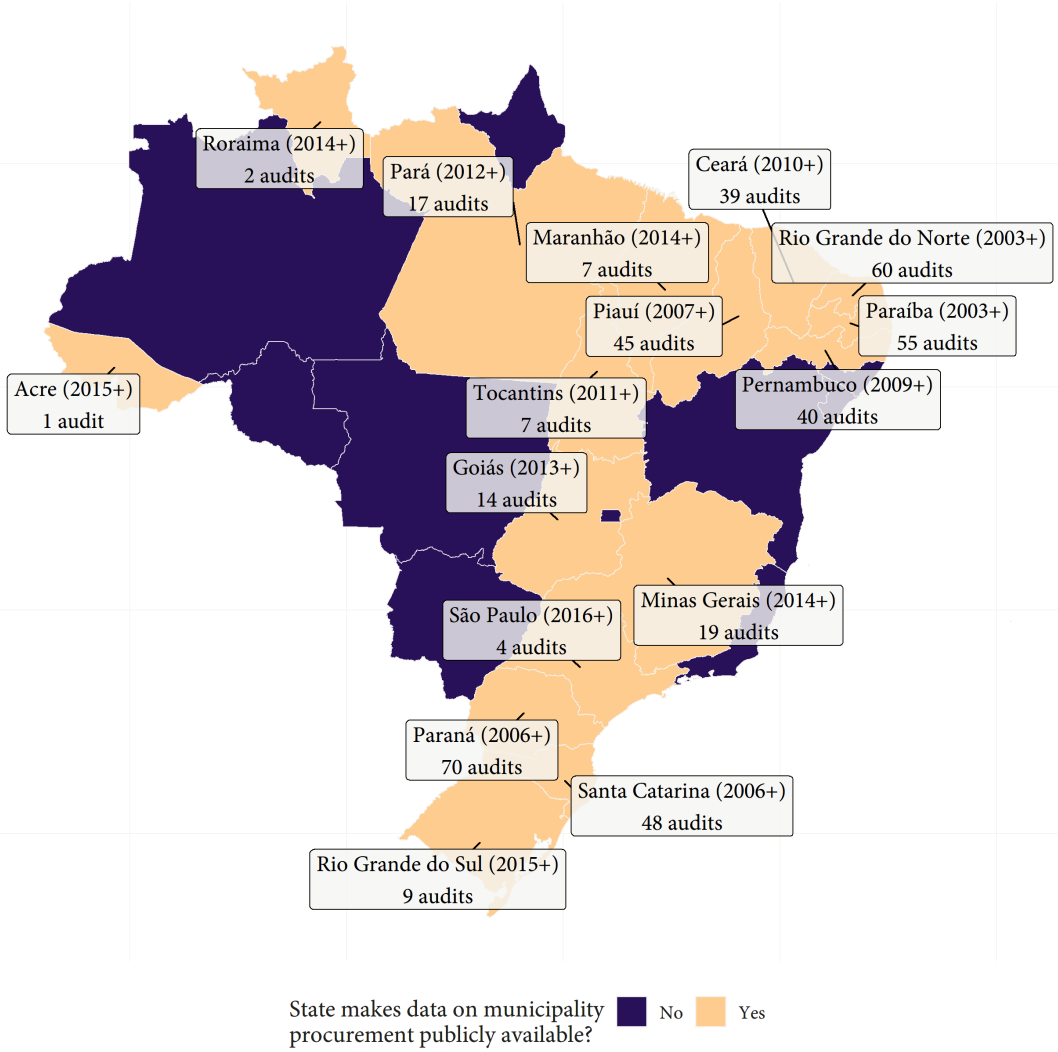
[13] A procurement-related example of an 'information' note, from Pancas-ES in 2015: "Eligibility requirements for a procurement auction were overly restrictive."

[14] An example of a formal violation, from Trombudo Central-SC in 2015: "Auction was waived [see Table 2.3] but no documentation was provided as to why, or to how the firm was chosen."

[15] An example of a medium violation, from Nova Odessa-SP in 2006: "An auction in the invitation-to-bid modality received less than three valid bids."

[16] An example of a serious violation, from Bocaiúva do Sul-PR in 2009: "The owners of a firm that won a procurement auction are related to the mayor."

Figure 2.1: Map of Brazilian states by earliest year of public data availability



The CGU data is the source of two control variables: first, a dummy variable for whether a municipality has been audited in the past. As mentioned above, that is true of 100 municipalities: 94 that were audited twice and 6 that were audited three times. Second: for each policy-area or federal government program under which federal funds were transferred to a municipality, the CGU data lists the amount of money under audit. To control for municipalities's use of federal funds, the models contain a term for the total amount of audited funds for each municipality/year.

I also include three demographic measures as controls. The first is the municipality's population in the most recent census: 2000 for audits that occurred between 2006 and 2009, and 2010 for audits between 2010

and 2016. The second is the municipality's GDP per capita on the year of the audit, as estimated by the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, IBGE). The third is the share of the population living in urban areas in the most recent census. These demographic variables (most importantly, the first two), as well as the total amount audited, are intended in part as proxies for the 'complexity' of an audit. Given that the number of auditors and the time spent in each municipality varies very little ("approximately 10 to 15.... [for] approximately ten days", per Ferraz and Finan (2008, p. 1282)), the possibility of "differential auditing" (Ferraz & Finan, 2011, p. 1305) exists. Moreover, there is evidence that demographic characteristics such as these are correlated with corruption (see e.g., Glaeser and Saks (2006)).

The models also contain a set of adjustments for mayoral and municipal characteristics: following Ferraz and Finan (2011), I include controls for: whether the Mayor is on their second consecutive term in office; the mayor's margin of victory in the most recent election (this variable is coded exactly as in Ferraz and Finan: "the difference in vote shares between the incumbent and the second-place candidate" (p. 1289)); the effective number of parties in the municipal legislature; the share of the municipal legislature with the same party affiliation as the mayor. To control for mayors's party affiliation, I specify each model in two different ways. In one set of models, I include a term for the mayor's party affiliation — these are the "mayor's party fixed effects" models in the tables below. In another set of models, I include a binary variable for whether the mayor's party was a part of the president's electoral coalition in the most recent election prior to the audit[17]. This specification, although less granular than the varying-intercepts model, has the potential advantage of adjusting for a purposeful, partisanship-driven kind of differential auditing. It also happens to fit the data better in all cases, which is unsurprising because there is a large number of parties represented in the dataset (27), but some parties appear less than a handful of times (there is one audited municipality with a mayor from PTN or PSOL, three with mayors from PRTB, four with mayors from PTdoB, etc.), which makes estimation challenging.

My choices of control variables are, for the most part, meant to adjust for the fact that the ten states without open-data Courts of Auditors tend to be smaller and poorer than their open-data counterparts. Apart

[17] In 2002, President Lula da Silva's electoral coalition consisted of PT, PL, PCdoB, PMN, and PCB. In 2006, Lula da Silva's coalition was PT, PCdoB, PRB, PL, and PSB. In 2010, President Rousseff's electoral coalition consisted of PT, PMDB, PDT, PCdoB, PSB, PR, PRB, PSC, PTC, and PTN. In 2014, Rousseff's coalition was PT, PMDB, PDT, PCdoB, PSD, PP, PR, PRB, and PROS.

from Bahia and Rio de Janeiro, none of them accounted for 2% or more of Brazil's GDP or 2% or more of the country's population as of the 2010 Census; five of those states had GDPs per capita smaller than three-quarters of the national average according to the 2010 Census. This difference is not unexpected: creating a system through which data can be accessed by the public requires investments in infrastructure and personnel; since those investments are not mandated by law, one may expect that generally poorer states will not make those investments. Given that being in a state that is willing and able to make these investments will plausibly affect municipalities in a variety of socioeconomic and political ways, I believe that adjusting for these socioeconomic and political variables is appropriate. Nonetheless, I try to accommodate the opposite argument: whether data is 'open' is a decision made by a state-level agency, and although municipalities are not randomly assigned to open- or closed-data states, procurement data availability is exogenous to municipal characteristics. For this reason, all model tables below also contain the results of bivariate models.

The models on Table 2.5 do not lend support to the hypothesis under consideration: that a dataset of municipalities only from states that make data publicly available is not representative of the universe of cases because public availability leads municipalities to improve how they conduct their procurement processes.

To ensure that this null result is not entirely due to the choice of model specification, I have recoded the dependent variable in two ways, both following Ferraz and Finan (2011): the first is the shares of audited resources that was involved in procurement-related and in invitation-related irregularities; the second is the count of procurement-related and invitation-related irregularities in an audit. Table 2.6 reports the results of the first alternative specification. Unlike Ferraz and Finan, who model these share-of-resources models with linear regression models, I follow the advice of Papke and Wooldridge (1996) and Meaney and Moineddin (2014) and fit fractional logits with robust/sandwich standard errors[18].

Those models, shown on Table 2.6, also do not support the hypothesis that public availability of data improves procurement auctions. In fact, the bivariate models for both outcomes lend weak support for the opposite idea: municipalities in open-data states are *more* likely to fail audits. However, that association disappears once controls are included. The models with controls lead to the same conclusions as the logit

[18]Meaney and Moineddin (2014) mention that fractional logit models have slightly higher power than linear models, and so do beta regression models; however, both dependent variables in my analyses contain zeros, which precludes me from fitting beta regression models — the support for those is in the open unit interval (0, 1), not the closed [0, 1].

Table 2.5: Logit models of procurement-related violations in audits

	Dependent variable:					
	Procurement-related violation			Invitation-related violation		
(Intercept)	1.86^{***}	-0.18	0.27	-0.55^{***}	-0.51	-0.18
	(0.10)	(1.12)	(1.26)	(0.07)	(0.85)	(0.94)
'Open-data' state	-0.11	-0.26	-0.34	-0.09	-0.19	-0.22
	(0.17)	(0.18)	(0.19)	(0.12)	(0.13)	(0.14)
Total amount audited (log)		0.45^{***}	0.45^{***}		0.09	0.12
		(0.09)	(0.09)		(0.07)	(0.07)
Population (log)		-0.38^{***}	-0.40^{***}		-0.04	-0.09
		(0.11)	(0.12)		(0.09)	(0.09)
GDP per capita (log)		-0.51^{***}	-0.53^{***}		-0.26^{**}	-0.27^{**}
		(0.12)	(0.12)		(0.09)	(0.10)
% urban population		0.50	0.59		0.25	0.35
		(0.45)	(0.46)		(0.32)	(0.33)
Munic. has been audited before		0.10	-0.08		-0.20	-0.27
		(0.31)	(0.32)		(0.24)	(0.25)
Effective n. parties in legislature		-0.10	-0.12		-0.16^{**}	-0.16^{**}
		(0.07)	(0.07)		(0.05)	(0.06)
Mayor's party's share in legislature		0.53	0.26		0.40	0.19
		(0.76)	(0.80)		(0.52)	(0.54)
Mayor is on second term		0.09	0.12		0.17	0.19
		(0.19)	(0.19)		(0.13)	(0.13)
Mayor's margin of electoral victory		-0.06	-0.16		0.03	-0.03
		(0.51)	(0.53)		(0.37)	(0.38)
Mayor and governor in same party		-0.26	-0.19		-0.00	-0.08
		(0.21)	(0.25)		(0.16)	(0.18)
Mayor's party in president's coalition		-0.10			-0.31^{*}	
		(0.18)			(0.14)	
Mayor's party f.e.			Yes			Yes
AIC	1045.87	961.37	980.09	1686.77	1594.62	1600.11
BIC	1056.20	1027.98	1169.70	1697.10	1661.23	1789.72
Log Likelihood	-520.94	-467.68	-453.05	-841.39	-784.31	-763.06
Deviance	1041.87	935.37	906.09	1682.77	1568.62	1526.11
Num. obs.	1289	1242	1242	1289	1242	1242

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

models from Table 2.5: there is no significant difference in the predicted share of resources in violations between municipalities in open-data and closed-data states.

Table 2.6: Fractional logit models of shares of audited resources in audit violations

	Dependent variable:					
	Share of audited funds involved in procurement-related violations			Share of audited funds involved in invitation-related violations		
(Intercept)	-1.64^{***}	-2.06^{***}	-2.07^{***}	-3.23^{***}	-3.00^{***}	-3.30^{***}
	(0.05)	(0.45)	(0.52)	(0.09)	(0.73)	(0.83)
'Open-data' state	0.02	0.00	-0.05	0.38^{*}	0.26	0.22
	(0.09)	(0.10)	(0.10)	(0.16)	(0.18)	(0.19)
Munic. has been audited before		0.18	0.15		-0.23	-0.22
		(0.16)	(0.17)		(0.33)	(0.32)
Population (log)		0.14^{**}	0.15^{**}		0.08	0.07
		(0.05)	(0.05)		(0.08)	(0.08)
GDP per capita (log)		-0.23^{**}	-0.23^{**}		-0.14	-0.08
		(0.08)	(0.08)		(0.12)	(0.12)
% urban population		-0.37	-0.43		-0.06	-0.11
		(0.22)	(0.23)		(0.37)	(0.37)
Effective n. parties in legislature		-0.06	-0.07		-0.13	-0.13
		(0.04)	(0.04)		(0.08)	(0.08)
Mayor's party's share in legislature		0.03	-0.07		0.40	0.17
		(0.39)	(0.40)		(0.69)	(0.70)
Mayor is on second term		0.08	0.06		0.12	0.12
		(0.10)	(0.10)		(0.17)	(0.17)
Mayor's margin of electoral victory		0.17	0.13		-0.76	-0.74
		(0.26)	(0.26)		(0.48)	(0.48)
Mayor and governor in same party		-0.15	-0.12		-0.02	-0.15
		(0.12)	(0.13)		(0.20)	(0.23)
Mayor's party in president's coalition		0.05			-0.04	
		(0.09)			(0.18)	
Mayor's party f.e.			Yes			Yes
AIC	891.20	845.76	879.42	282.39	276.82	318.72
BIC	901.52	907.25	1063.90	292.71	338.31	503.20
Log Likelihood	-443.60	-410.88	-403.71	-139.19	-126.41	-123.36
Deviance	388.92	359.94	352.15	211.88	195.53	189.41
Num. obs.	1289	1242	1242	1289	1242	1242

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

As another alternative attempt at investigating the hypothesis under consideration in this section, I have fit the same sets of models as the tables above, but with a reduced dataset to make causal identification (slightly) more plausible. This dataset only contains data from states that publish municipal procurement data, and I

only retain audits conducted in the earliest year without public data and the latest year without it (for example, the data for the state of Ceará begins in 2010; this reduced dataset contains 9 audits conducted in 2009 and 8 audits conducted in 2010.). Here, the comparison is still between 'open' and 'closed' data, but the variation being exploited is across time. In other words: in a dataset only of states that publish data, my independent variable of interest is zero in years prior to the data becoming available, and one in years after the data is available.

Table 2.7 and Table 2.8 show the results of those models. As with the previous models, the first table shows the results of logit models for whether a 'medium' or 'serious' violation was detected, and the second table shows the results of fractional logit models for the share of resources involved in those violations.

I also subset the data to within two years of the data being publicly available (in the case of Ceará, that means the addition of 6 audits from 2008 and 8 audits from 2011). This is because the appropriate window of time is not obvious from the data or the hypothesis being tested, and because subsetting the dataset to within one year of procurement information being public leaves only 128 observations, which severely impacts the performance of those models. The models built with data from two years before and two years after data was public are shown in Appendix A. As with previous sets of alternative model specifications, both datasets lead to the same substantive conclusions: municipalities that are audited in years after data is publicly available are no more or less likely to have irregularities uncovered in their procurement processes.

Having scraped the procurement data, I find the lack of an effect unsurprising. Public availability of these data is not advertised, and the information is not simple to obtain and analyze (for example, data for the state of São Paulo before 2018 only exists as a PostgreSQL database dump[19]; data for the state of Maranhão cannot be scraped with HTTP requests and requires a tool such as Selenium[20] to automatically emulate interaction with the webpage from a real browser). I speculate that the publication of municipal procurement data by a state's Court of Auditors is unlikely to strike fear into the hearts of public servants, and particularly unlikely to prevent rent-seeking through added public monitoring. Open data policies certainly have the potential to increase government transparency, but as Janssen et al. (2012) emphasize, "poor usability, weak application of

[19] Available at `https://transparencia.tce.sp.gov.br/conjunto-de-dados`.

[20] "[An] umbrella project for a range of tools and libraries that enable and support the automation of web browsers", available at `https://www.selenium.dev/documentation/en/`.

Table 2.7: Logit models of violations in audits among open-data states within one year of data being publicly available

	Dependent variable:			
	Procurement-related violation		Invitation-related violation	
(Intercept)	1.90^{***}	-3.95	-0.61^{*}	-2.79
	(0.41)	(4.84)	(0.28)	(3.11)
Procurement data is available	0.35	1.11	-0.31	-0.24
	(0.57)	(0.83)	(0.38)	(0.45)
Total amount audited (log)		0.99^{*}		0.31
		(0.43)		(0.28)
Munic. has been audited before		15.02		-1.15
		(1707.87)		(0.88)
Population (log)		-0.70		0.08
		(0.51)		(0.33)
GDP per capita (log)		-1.18^{*}		-0.96^{*}
		(0.54)		(0.41)
% urban population		-1.54		0.15
		(2.61)		(1.30)
Effective n. parties in legislature		0.23		-0.25
		(0.31)		(0.20)
Mayor's party's share in legislature		-0.55		-3.29
		(2.90)		(1.88)
Mayor is on second term		1.97		0.59
		(1.21)		(0.48)
Mayor's margin of electoral victory		-3.12		-0.29
		(1.82)		(1.32)
Mayor and governor in same party		0.61		0.25
		(1.05)		(0.62)
Mayor's party in president's coalition		-0.20		0.09
		(0.84)		(0.51)
AIC	91.98	77.23	162.33	156.86
BIC	97.69	113.68	168.03	193.31
Log Likelihood	-43.99	-25.62	-79.16	-65.43
Deviance	87.98	51.23	158.33	130.86
Num. obs.	128	122	128	122

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 2.8: Fractional logit models of shares of audited resources in audit violations among open-data states within one year of data being publicly available

	Dependent variable:			
	Share of audited funds involved in procurement-related violations		Share of audited funds involved in invitation-related violations	
(Intercept)	-1.51^{***}	-2.52	-2.68^{***}	-1.53
	(0.20)	(2.07)	(0.34)	(2.28)
Procurement data is available	-0.29	-0.29	-0.48	-0.46
	(0.28)	(0.28)	(0.47)	(0.44)
Munic. has been audited before		0.77		-1.16
		(0.40)		(0.97)
Population (log)		0.27		0.08
		(0.20)		(0.25)
GDP per capita (log)		-0.27		-0.64^{*}
		(0.24)		(0.32)
% urban population		-0.88		0.18
		(0.74)		(1.41)
Effective n. parties in legislature		-0.17		-0.19
		(0.12)		(0.22)
Mayor's party's share in legislature		-0.50		0.27
		(1.14)		(1.76)
Mayor is on second term		0.10		0.35
		(0.34)		(0.53)
Mayor's margin of electoral victory		0.42		-2.19
		(0.96)		(2.13)
Mayor and governor in same party		0.14		-0.12
		(0.37)		(0.64)
Mayor's party in president's coalition		0.52		0.60
		(0.30)		(0.50)
AIC	88.49	101.27	28.96	46.38
BIC	94.20	134.91	34.66	80.03
Log Likelihood	-42.25	-38.63	-12.48	-11.19
Deviance	41.83	36.41	25.70	21.77
Num. obs.	128	122	128	122

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

stewardship principles, lack of data feedback and improvement mechanisms, and inadequate meta-data" (p. 265) are common among government open-data projects, and data published in those conditions go against the basic principles of open data ("data must be available as a whole... in a convenient and modifiable form" (Open Knowledge Foundation, 2019)). In theory, the publishing of granular data on government processes could potentially affect the conduct of local governments — but that would require an entirely different set of open-data policies.

2.6 Contract values and Benford's law

Benford's law, or the significant-digit law, is the empirical observation that the leading digits in naturally-occurring sets of numbers follow a certain distribution. Originally a "[mere] mathematical curiosity with no apparent useful application" (Bolton & Hand, 2002, p. 237), Benford's law has been demonstrated to be a useful tool for the initial steps in the process of detecting fraud in accounting data (see e.g., Durtschi et al. (2004) and Nigrini and Mittermaier (1997)). Before testing auction data against Benford's law, first I briefly describe its applications and discuss when one may reasonably expect that data will deviate from it.

2.6.1 When should Benford's law hold?

The significant-digit law, as originally formulated, states that "the law of probability of the occurrence of numbers is such that all mantissae of their logarithms are equally probable" (Newcomb, 1881, p. 40). The mantissa is the part of a logarithm after the decimal point; for logarithms with base 10, numbers that are powers of 10 apart have the same mantissae (e.g., $\log_{10} 3.14 = 0.497$; $\log_{10} 31.4 = 1.497$; $\log_{10} 314 = 2.497$). If the mantissae of the base-10 logarithms of a set of numbers is uniformly distributed, then the first digits of those numbers will follow a particular logarithmic distribution, first specified by Benford (1938):

$$P(first\ digit = d) = log_{10}\left(1 + \frac{1}{d}\right) \tag{2.1}$$

Thus, in a set that follows Benford's law, the first digit is expected to be 1 in 30.1% of cases, 2 in 17.6% of cases, etc. Hill (1995) extends this formulation to sequences of leading digits of any length. For any number d of length n,

$$P(first\ n\ digits = d) = log_{10}\left(1 + \frac{1}{d}\right) \tag{2.2}$$

For instance, in a set that satisfies Benford's law, the probability that the leading three digits of a randomly chosen number are 154 is $log_{10}\left(1 + \frac{1}{154}\right) = 0.0028$.

Benford's law is described variously as "statistical folklore" (Hill, 1995, p. 354), a "quirky law" (Durtschi et al., 2004, p. 17), and a "mathematical curiosity" (Bolton & Hand, 2002, p. 237). This is, I speculate, partly because the applications of Benford's law were not immediately obvious to those who first articulated it. Newcomb (1881) merely suggests that it can be used to determine "whether a large collection of independent numerical results were composed of natural numbers or logarithms" (p. 40), presumably because the mantissae of logarithms are uniformly distributed and the mantissae of logarithms of logarithms are not. Benford (1938) demonstrates that the law applies to a variety of seemingly unrelated sets of data (e.g., baseball statistics, numbers on the front page of a newspaper, street addresses printed in a directory) but does not explain the phenomenon beyond stating that it only holds for 'anomalous', unrelated sets of numbers.

In fact, a widely accepted explanation was not given until Hill (1995), who notes that none of Benford's numerical tables conformed to the significant-digit law as closely as the set obtained from *combining* all of those tables. Hill shows that Benford's law holds when "probability distributions are selected at random and random samples are then taken from each of these distributions" (p. 360). This explains why, for example, tables of baseball statistics satisfy the significant-digit law, as they comprise data on plate appearances, home runs, stolen bases, etc. It also explains why the set of monetary values of all procurement contracts in my data should follow Benford's law: such a set combines auctions for purchases of goods and services with auctions for engineering projects; those purchases involve different products, in different amounts; and the contracts' prices span several orders of magnitude.

To illustrate the principle explained by Hill (1995), consider Figure 2.2. Each panel shows the distribution

of the first two digits of a set of data on Brazilian municipalities for 2014, and the expected distribution against Benford's law. The top panel shows the results for municipal GDPs, and the bottom panel shows results for a combination of municipal GDPs and municipal populations. For the bottom panel, I took a random sample of size 5570 without replacement, so that both panels have the same sample size. As the histograms show, the relative proportions of the two first digits in the bottom panel are visibly closer to the Benford distribution.

The mean absolute deviation (MAD) of the bottom panel is also smaller than the MADs of the individual datasets – 0.0009 vs. 0.0013 for GDPs alone and 0.0012 for populations alone. The MAD, as the name implies, is given by:

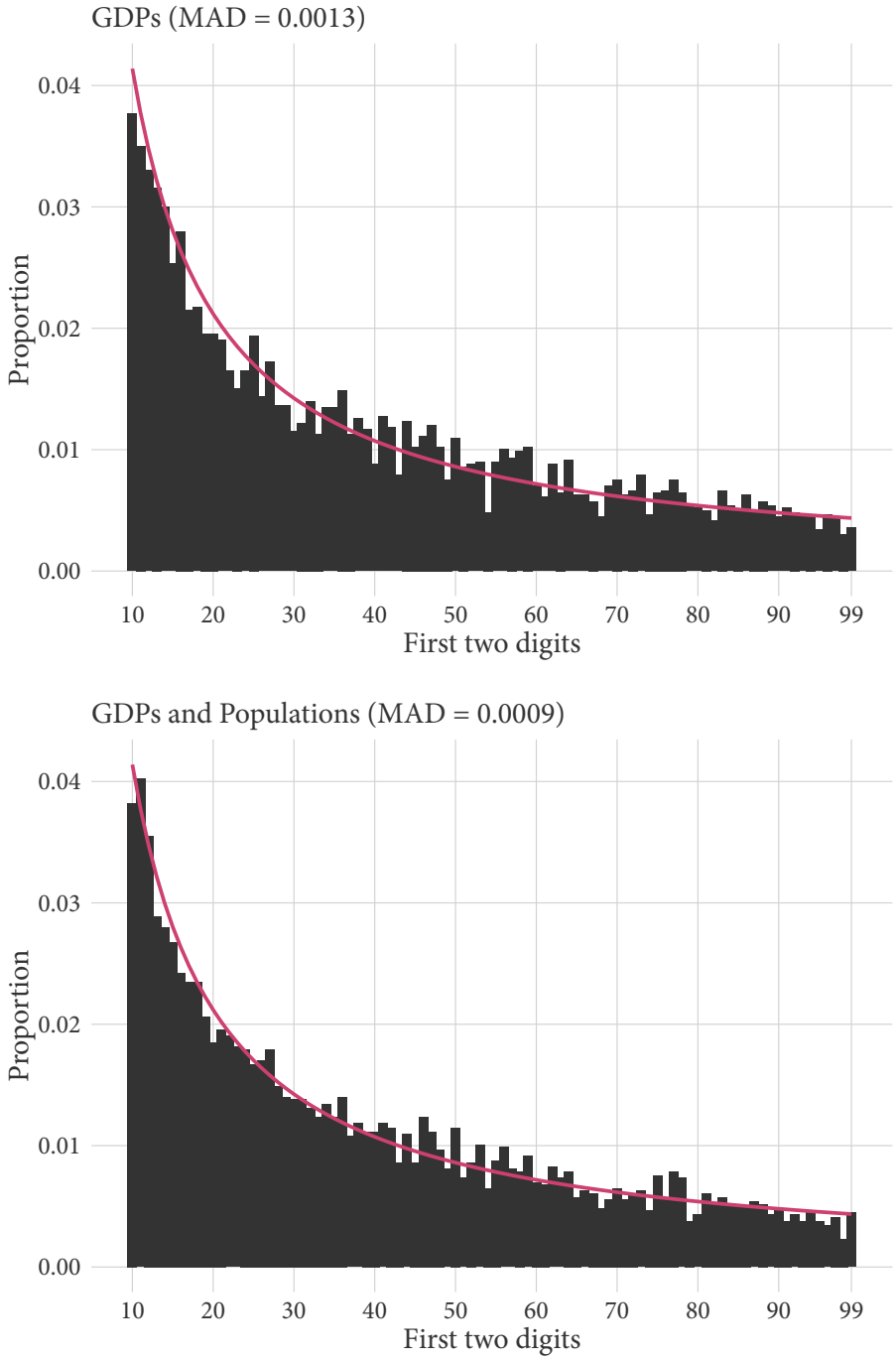
$$MAD = \frac{\sum_i^K |AP_i - EP_i|}{K}$$

where K is the number of bins (90 for the two-digit distribution I use throughout this paper), AP_i is the actual proportion of leading digits i in the data, and EP_i is the expected proportion under the Benford distribution (Nigrini, 2012, p. 158). I use the MAD as a measure of conformity throughout this paper. I prefer it as a measure of conformity (as opposed to, for example, reporting the results of Kolmogorov-Smirnov tests or chi-squared tests) for two main reasons. First, some widely-used tests for Benford conformity are either not valid or not appropriate: the Kolmogorov-Smirnov test, used in e.g., Horton et al. (2020), is only valid for continuous distributions and therefore cannot be used to test data against Benford's distribution; the widely used Kuiper test, a rotation-invariant modification of the Kolmogorov-Smirnov test, used in e.g., Deckert et al. (2011), is equally inappropriate when testing data against discrete distributions, and moreover it is too sensitive to small deviations from Benford's distribution (Cho & Gaines, 2007); the chi-squared test is "very sensitive to sample size [and] too rigid to assess goodness-of-fit" (Cho & Gaines, 2007, p. 220). Sensitivity to sample size makes the chi-squared test inappropriate because Benford's law is not an asymptotic property of real data.

Second, and most importantly: given, again, that Benford's law is not an asymptotic property of real-life datasets, I do not find it useful to use a null-hypothesis significance test for Benford conformity. Instead, the MAD is described by Nigrini (2012) as a measure without "objective critical scores... [but only] some

guidelines" (p. 159). It is a descriptive tool without the potentially misleading veil of authority of a null-hypothesis significance test. The guidelines given by Nigrini for the first two digits are: datasets show 'close conformity' when the MAD is under 0.0012; 'acceptable confirmity' when it is between 0.0012 and 0.0018; 'marginally acceptable conformity' when it is between 0.0018 and 0.0022; and 'nonconformity' when it is above 0.0022. Thus, the MAD for Brazilian municipal GDPs is just above the threshold between close and acceptable conformity; combining it with another set of data brings it to the close conformity category.

Figure 2.2: Conformity of Brazilian municipal data to Benford's law



Notes: Line shows Benford's distribution for two leading digits.
Data is from IBGE estimates for 2014. N = 5700 for both panels.

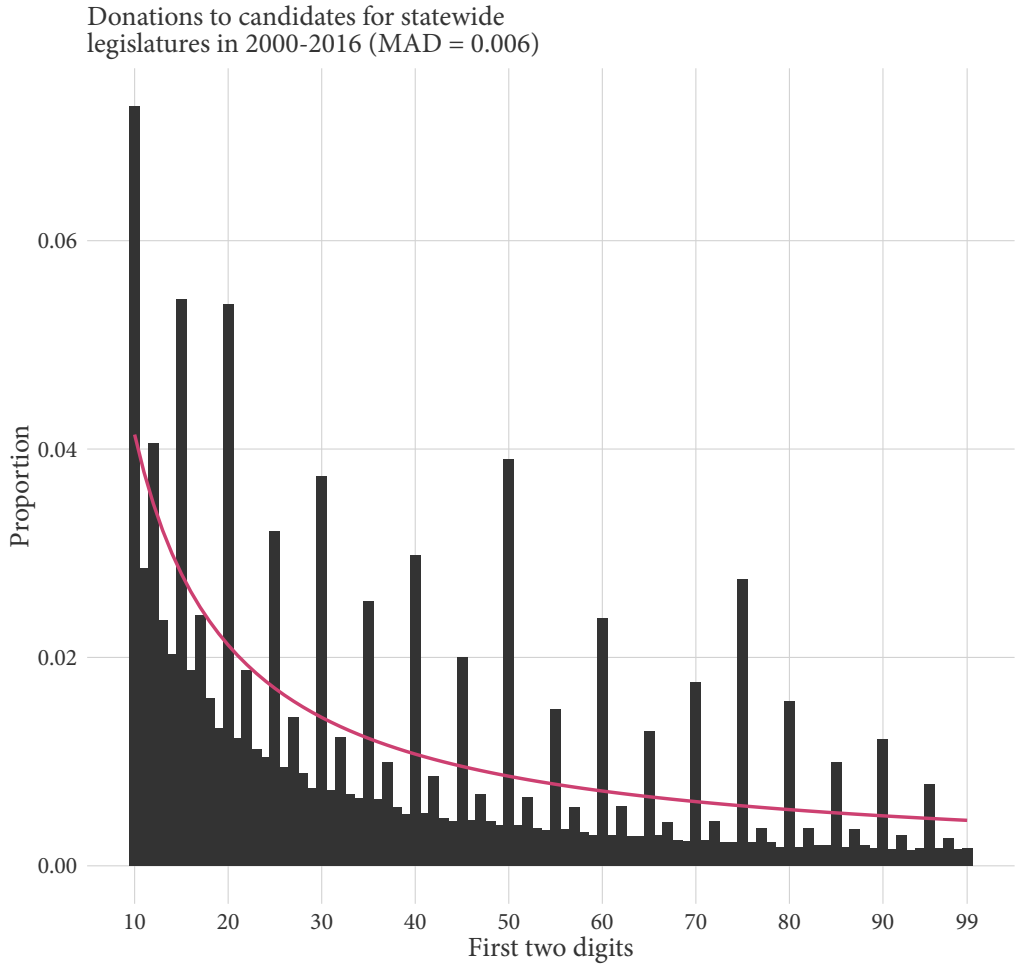
2.6.2 When should Benford's law not hold?

Although Hill explains why and under what conditions random draws from probability distributions should satisfy Benford's law, its practical uses for fraud detection — for example, in election results (Deckert et al., 2011; Mebane, 2006), in campaign finance (Cho & Gaines, 2007), and in scientific publications (Hein et al., 2012; Horton et al., 2020; Hüllemann et al., 2017) — only become clear when exploring when and why datasets are expected *not* to conform to it. Put simply, "when individuals invent numbers, these numbers do not conform to Benford's law" (Nigrini & Mittermaier, 1997, p. 56). As Nigrini (2012) describes, invented numbers tend to follow predictable patterns: when asked to invent sets of random numbers, experiment subjects commonly chose sequences of digits (e.g., 1234) or repeated digits: one respondent in an experiment entered "the numbers 999 followed by 666 and then 777.... [another] person used 999 and 11111" (Nigrini, 2012, p. 306).

Numbers arbitrarily chosen by individuals in real-life situations also tend to follow such patterns. For instance, using data from Reuning (2019), I analyzed the first two digits for the monetary amounts of campaign donations by various groups (e.g., labor unions, companies, party committees), for all legislative elections between 2001 and 2016. The dataset contains 721 elections for all fifty American states, and a total of 282,011 observations[21]. Figure 2.3 shows the distribution of leading digits for those contributions. Unsurprisingly, multiples of five and ten make up a majority of the data: 30.2% of contributions begin with multiples of ten, and another 20.5% begin with odd multiples of five. 79, 91, and 93 appear with less than one-third of the frequency predicted by Benford's law; 50, 60, and 75 appear at least three times as frequently as predicted.

[21] Reuning's data, obtained from the National Institute on Money in Politics, is aggregated by group/election — individual data points are not individual donations, but sums of donations for each group/election/year.

Figure 2.3: An example of nonconformity to Benford's law when numbers are chosen arbitrarily



Notes: Line shows Benford's distribution for two leading digits. Data source is Reuning (2019). N = 282,011.

As I describe below, the estimated contract values in the procurement data partly conform to this pattern of peaks at multiples of ten: those estimated values are in part chosen arbitrarily, but are also tied to the market prices of goods and services. Given that many auctions are not for a single product, but a combination of several goods of the same category or used for the same purpose, these contract values are a set of values drawn from a set of probability distributions, and thus should follow the data-generating process described by Hill (1995).

Using Benford's law as a tool for detecting fraud is particularly useful in a dataset such as the estimated contract values data: the distribution of the leading digits come from a seemingly Benford-conforming DGP;

any deviations are partial (multiples of five and ten should be more frequent, but not to the extent of Figure 2.3), expected, and explained.

There is, however, another relevant reason for a set of numbers not to conform to Benford's law, which appears in the procurement data in a more pronounced way. In this case, distortions are caused by a data-generating process that constrains behavior — particularly, but not exclusively, of bad-faith actors — such as to 'force' some combinations of leading digits to occur more often. One example of behavior driven by these constraints is 'smurfing', the illegal practice of structuring financial transactions in order to evade reporting by financial institutions to governments. As Welling (1989) explains, in 1970 the United States Congress passed the Bank Secrecy Act, which required financial institutions to report any cash transactions over $10,000 to the federal government. As a result, money launderers "began to conduct multiple cash transactions just below the $10,000 threshold" (Welling, 1989, p. 288), which was a successful tactic until the early 1980s when federal law enforcement intensified its efforts to prosecute transaction structuring. In one case, United States v. Tobon-Builes (1983), the defendant attempted to conceal $185,200 that he had ostensibly won playing poker, so that he could avoid paying federal taxes on those earnings. In order to keep his transactions from being reported by financial institutions, he and a companion drove to ten different banks in a six-hour period; in each bank, they entered separately, went to different tellers, and each purchased one cashier's check for $9,000, always using false names. They were caught not because of suspicious bank tellers or because of reporting to the Department of the Treasury, but because a police officer saw the defendants purchasing $9,000 cashier's checks, became suspicious, and followed them to other banks, thus uncovering their scheme. This case of transaction structuring is an instructive, if somewhat silly, example of Benford's law being subverted: due to the legal reporting threshold of $10,000, what could have been one deposit of $185,200 became 10 transactions of $18,000 each, and subsequently 20 cashier's checks worth $9,000 each.

Public procurement in Brazil imposes a similar set of incentives on government officials, due to the modality-specific limits described on Table 2.2. The invitation to bid is a frequent reason for local governments to engage in 'auction structuring', as I show below.

2.6.3 How well do estimated procurement auction values conform to Benford's law?

For the analysis below, I use a subset of municipalities from fifteen states[22], who collectively conducted 1,910,903 procurement processes (this excludes those conducted in 2018 and 2019, as the price limits for each modality changed in 2018). The relevant variable that I will test against the Benford distribution is the total estimated value of the goods, services, or construction projects being procured.

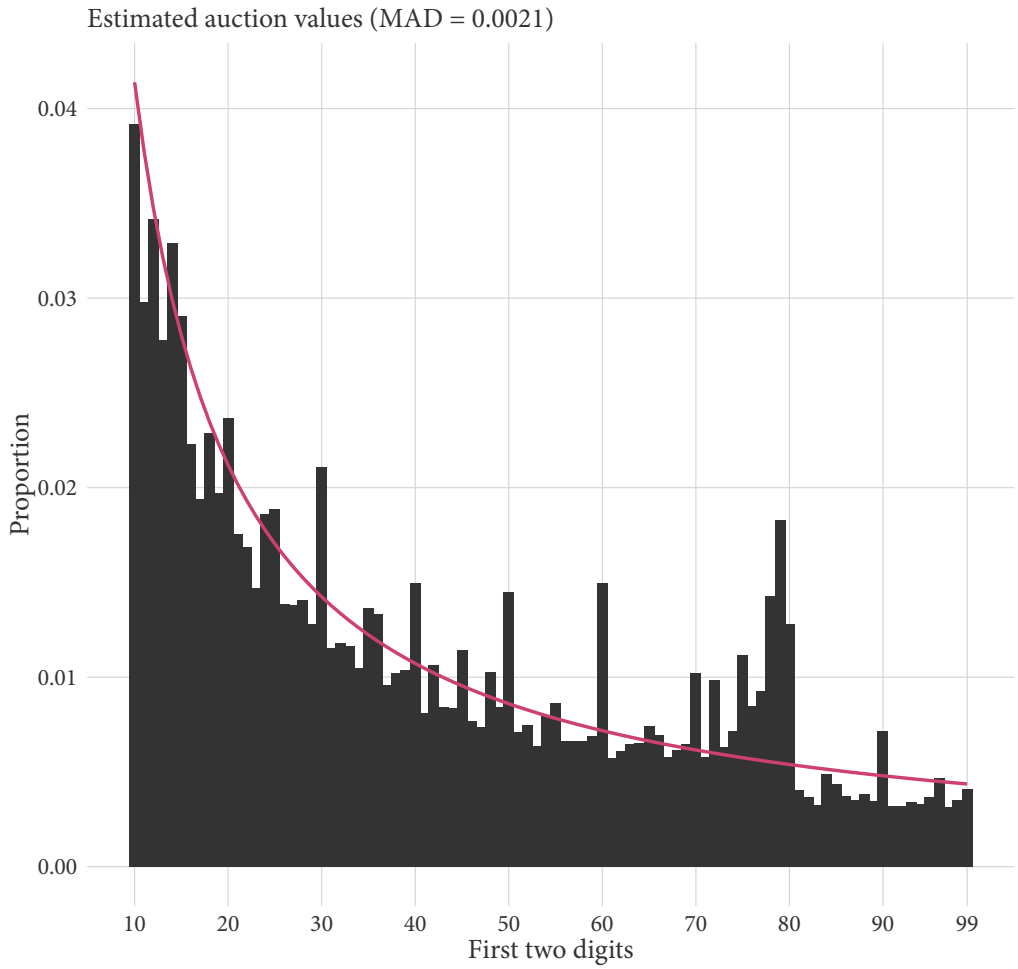
Estimating the total value of the procured goods/services/projects is mandatory pursuant to federal Law 8666/93, Art. 7, which states that the public entity interested in acquiring goods or services must publish a request for proposals that includes an estimated budget for each of the contract's components[23]. That estimated value determines the possible modalities under which an auction may be conducted, per the limits on Table 2.2.

Law 8666/93 establishes limits on how auctions can be structured in relation to their estimated prices and the modalities that can be used (Jardim de Amorim, 2017). Importantly, Art. 23 forbids "[using] the 'invitation to bid' modality... for parts of the same service or project, or for services or projects of the same nature and in the same locality that can be undertaken concurrently" (L. 8333/93, Art. 23, §5). In other words, the law expressly forbids a government from conducting invitation-to-bid auctions by transforming one single acquisition of e.g., R$237,000 into three auctions with estimated values of R$79,000 each (to recapitulate Table 2.2, invitations to bid had a price limit of R$80,000 until 2018). However, an analysis of the first two leading digits of these estimated auction prices shows that this kind of structuring may be normal – or, at least, that invitation-to-bid auctions coincidentally are estimated between R$78,000 and R$79,999 rather frequently. The analysis also shows that auction waivers are frequently invoked to sign contracts between R$7,800 and R$7,999 without an auction – as Table 2.3 describes, one of the valid legal causes for a waiver is that the contract value is less than 10% of the limit for the invitation to bid: R$8,000 until 2018.

[22] Ceará, Goiás, Maranhão, Minas Gerais, Pará, Paraíba, Paraná, Pernambuco, Piauí, Rio Grande do Norte, Rio Grande do Sul, Roraima, Santa Catarina, São Paulo, and Tocantins.

[23] Art. 40 of the same act clarifies that these estimated prices are not 'ceilings' for auctions, although such limits may be established at the discretion of the public entity responsible for the purchase.

Figure 2.4: Distribution of the two leading digits of estimated contract values in Brazilian municipal procurement processes



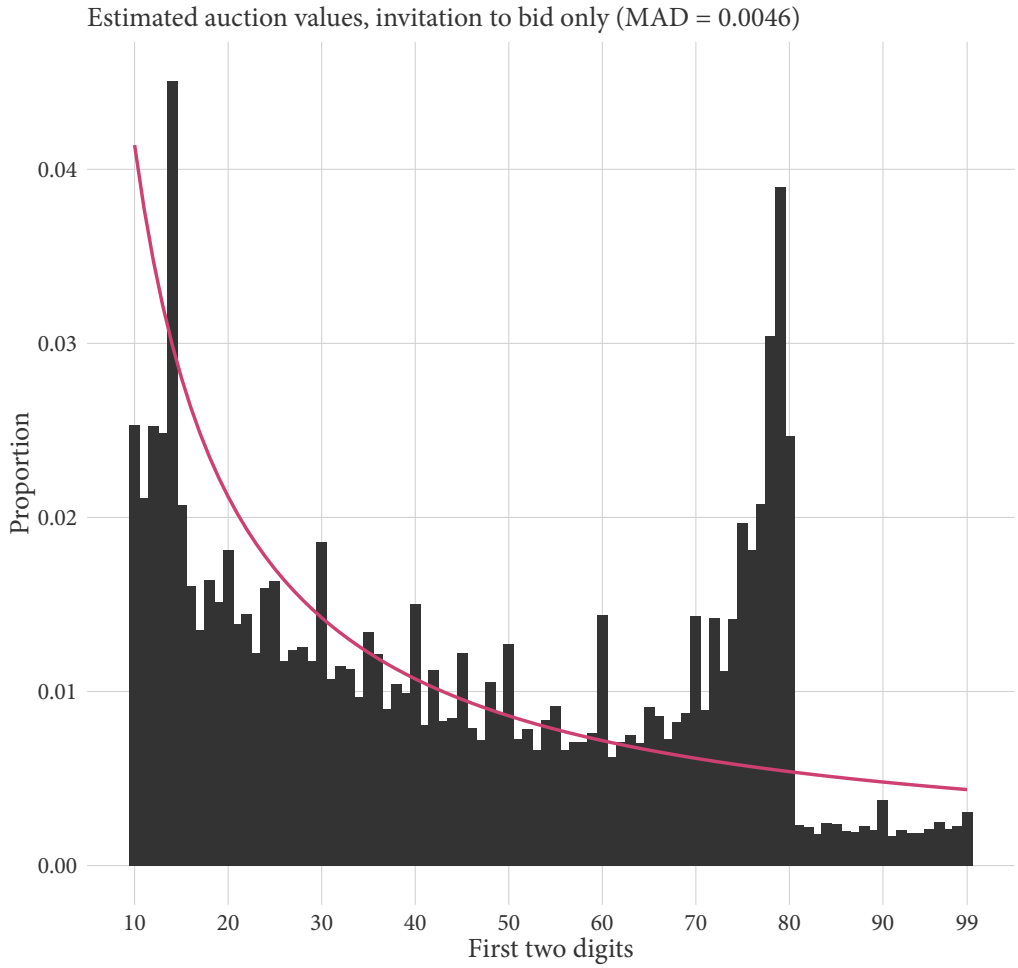
Notes: Line shows Benford's distribution for two leading digits. Only observations before 2018 were used. N = 1,910,903.

The two leading digits of contracts' estimated values are shown in Figure 2.4. As expected, the distribution shows peaks at multiples of ten; it also shows a high peak between 77 and 80. This peak suggests that municipalities may engage in auction structuring. The pattern becomes somewhat clearer when plotting the two leading digits only for auctions conducted through the invitation to bid modality, as in Figure 2.5. Aside from the peak between 77 and 79, the figure also shows a high peak at 14, which can be explained by the limit of R$150,000 for engineering projects.

Of course, perhaps the strange distribution of the data on Figure 2.5 is due to the truncation itself — because invitations to bid have legal limits of R$80,000 and R$150,000, subsetting the data to only that pro-

curement regime would introduce artificial nonconformity to Benford's law, and that could explain the pattern. To address this potential concern, Appendix B includes a plot with the same data as the right panel on Figure 2.2, but truncated to values below 80,000. That distribution certainly presents signs of nonconformity — including very low values between 81 and 99, similar to Figure 2.5 — but generally, that distribution does not resemble the distribution of estimated contract values for auctions conducted by invitation to bid.

Figure 2.5: Distribution of the two leading digits of estimated contract values in Brazilian municipal procurement processes conducted by invitations to bid



Notes: Line shows Benford's distribution for two leading digits. Only observations before 2018 were used. N = 360,936.

Figure 2.4 and Figure 2.5 show many of the patterns I discuss above: partial conformity because each contract's estimated value is a series of random draws from a series of random distributions (i.e., sums of values of individual items, which often are different items purchased together); partial nonconformity due to

peaks at multiples of ten because the estimated values are, at least in part, chosen arbitrarily; very high peaks at aberrant values because of structural limits that influence behavior.

As a result, the figures do not seem to lend evidence to the idea that the information submitted to state Courts of Auditors is fabricated by municipalities. Patterns in the data reflect potentially dishonest behavior that can be explained in relation to the incentives set by legal constraints, which lends some credibility to the data in general and to its use as a measure of corrupt behavior. Of course, the former conclusion is more strongly supported by the data than the latter, simply because the Benford's law test is more efficient at detecting made-up data than it is at detecting e.g., accounting fraud. Thus, it must be emphasized, the kind of visualizations and tests performed in this section do not allow one to deduce that corruption takes place. I believe that it supports the idea that the invitation to bid is a useful proxy for corruption, because it shows that municipal governments illegally distort estimated contract values in order to use the invitation to bid. That, in addition to the rest of the information in this chapter, suggests that there is *some* incentive to conduct invitations to bid that municipal authorities respond to. My analyses in this chapter and the next depend on the credibility of the argument that this incentive is directly linked to rent-seeking — a plausible argument, given that the incentive is probably not about efficiency (because the reverse bidding auction, and particularly its electronic version, takes less time and has been shown to help the federal government save money in acquisitions), and that the invitation to bid has directly caused a series of legal imbroglios for municipal governments.

2.7 Concluding remarks

In this chapter, I discussed the collection process, sources, and some characteristics of a dataset I assembled of Brazilian municipal procurement processes. In my effort to utilize the dataset to test hypotheses relevant to the literature on political corruption, I also draw from literatures on Brazilian constitutional law, public policy, and public procurement. That these literatures are disproportionately represented in this chapter is an unfortunate artifact of the complexity of the task of making the very low-level data useful for analysis. It is also a result of this chapter's main purpose, which is to set up the analysis reported in the next chapter. To

that end, the empirical work presented here aimed to clarify or preempt some questions about the quality of the data: are states that make their data public somehow more effective at monitoring the municipalities under their jurisdiction? Does the data present patterns that would suggest that its values are, in general, fraudulent?

Other problems certainly exist, and some questions cannot be answered in this paper with this data (why do municipalities generally not publish calls for bids for invitations to bid on national holidays, despite the incentives Søreide (2002) describes?), but the questions I have addressed in this chapter are fundamental in order for the data to be useful for any research on any topic related to political corruption. In the next chapter, I use this dataset to investigate one such set of questions: do randomly assigned audits change behavior by municipal governments? If so, under what conditions, and for what period of time? If any effect exists, does it extend past the boundaries of the audited municipalities — i.e., is the effect contagious?

I hope that the data is, at some point, put to use outside of these narrow questions. It has clear shortcomings (e.g., some information, such as the dates of publication of calls for bids, is not made available by every state), but its scope and overall level of detail should hopefully allow it to be, by itself, a contribution to the discipline.

3

"You think it's a nonvite?": Rent-seeking in procurement by local governments and its consequences

3.1 Introduction

What are the effects of the type of partially closed procurement discussed in the previous chapter? Additionally, what are the effects of federal government oversight on the procurement practices of municipal governments? Aside from their clear law enforcement function, do intra-government accountability mechanisms fulfill latent, 'good-governance' functions?[1]

Even though the Brazilian federal government's program of state and municipal government audits has been extensively studied (e.g., Avis et al. (2018), Ferraz and Finan (2008), Gerardino et al. (2017), Lauletta et al. (2020), and Zamboni and Litschig (2018)), partly due to its random assignment of municipalities and states to the auditing process, many of the well-known studies that use that data focus narrowly on the corruption detected by those audits — for instance, Ferraz and Finan (2011) investigate whether reelection incentives cause mayors to engage in less misappropriation of resources; Avis et al. (2018) ask whether audits increase the likelihood of legal actions being taken against mayors whose corrupt behavior is detected by an audit. The audits' secondary policy outcomes are more rarely studied; one notable exception is Lichand et al. (2018), who find that although auditing leads to a decrease in some forms of corrupt activity among municipal governments, other forms of 'mismanagement' of funds increase in the years following an audit.

Using audits to measure corruption, however, limits the possible empirical strategies available to researchers, because municipality that is audited can only be included for selection in the lottery "after several lotteries have elapsed" (Ferraz & Finan, 2018, p. 260) – at first, a municipality could only be audited again

[1] The title quotes from Mehlman et al. (1997).

after three 'rounds' of the lottery; eventually this rule was changed to twelve rounds.[2] Thus, the audits have very limited potential to answer whether audits reduce corruption: out of a total of 1,183 municipalities that were chosen by lottery to be audited between 2006 and 2016 (from the 20th to the "41st" lotteries, for which I have the most complete data), only 100 were audited more than once; only eight municipalities were audited twice in the same mayoral term, and all of those occurred in the 2009-2012 term.[3] Therefore, while the random audits program provides a useful measurement of corruption that overcomes the problems inherent to surveys of corruption perceptions (Lambsdorff & Schulze, 2015; Mondo, 2016), it effectively impedes certain questions from being investigated, because the measurement is very rarely repeated on the same units.

This is an unfortunate reality, given that this literature on Brazilian municipal auditing, largely created by the random-assignment feature of the audits program, breaks the paradigm that "[auditing] has put itself beyond empirical knowledge about its own effects in favor of a constant programmatic affirmation of its potential" (Johnsen et al., 2001, p. 584). Taking advantage of the random assignment of audits to municipalities facilitates the production of knowledge on which politicians are most affected by audits (Ferraz & Finan, 2011), whether audits produce positive policy outcomes aside from its corruption-fighting intent (Bologna & Ross, 2015; Lauletta et al., 2020; Lichand et al., 2018), and how central governments can use their accountability powers strategically to maximize the efficiency of audits (Beylis et al., 2016). In this chapter, I also try to use auditing data to investigate whether it reduces the behavior associated with rent-seeking by public officials.

To do so, I exploit a new measure of potentially corrupt behavior: the invitation to bid, a controversial (and, as of April 1, 2021, no longer legal) type of procurement auction. The invitation to bid, designed to be a faster, cheaper, and more efficient modality of procurement, has been superseded in all of those metrics by the online reverse bidding, a modality regulated in 2005 and widely adopted at all levels of Brazilian public

[2] Between 2003 and 2007 there were an average of five lotteries per year; between 2008 and 2013, there were 2.1 rounds per year on average. This decrease coincided with an increase in the number of municipalities chosen in each round: 5 in the first lottery, 26 in the second, 50 in the third to the ninth rounds, and 60 from the tenth to the fortieth, with two exceptions (Mondo, 2016). In 2015, the audit program ceased to select municipalities by lottery only. Since then, two rounds of municipal audits have taken place in which municipalities were chosen by a risk assessment model, and one round of audits by lottery (in 2016) occurred. In this "41st" round, 67 municipalities were audited.

[3] Incidentally, these eight municipalities that were audited twice in the same mayoral term had a total of 466 'medium' or 'serious' violations detected in their first audits (42 directly about procurement auctions), and 419 in their second (22 directly about procurement auctions).

administration, but particularly by the federal government (dos Reis & Bugni, 2017). The invitation to bid procedure endures in local governments, however; their use has been increasingly scrutinized by Brazilian legal scholars (see e.g., (Mascarenhas et al., 2012; Ortega, 2015)). Arguably, its persistence is in part due to the ease with which it can be used to generate rents.

In this chapter, I use the data described in the previous chapter: a dataset of Brazilian municipal procurement processes, encompassing fifteen Brazilian states and over two and a half million procurement auctions and auction waivers. I discuss two different classes of problems regarding corruption risk to which my data can help provide evidence.

First, following the literature on auditing and corruption prevention, I investigate whether federal auditing of municipal governments leads to a decrease in the incidence of the use of the invitation to bid procedure.

Then, in order to estimate the possible stakes involved in the answer to the first question, and following e.g., Bologna and Ross (2015) and Lichand et al. (2018), I ask: do invitations to bid have consequences to municipal public service provision and to the local economy? Particularly, do municipalities that conduct more of its auctions by the invitation to bid procedure see worse public health outcomes? Additionally, is the use of the invitation to bid associated with worse economic outcomes, which may signal that the modality does not serve the purpose of fomenting local economic activity, one of the 'secondary goals" (Telgen et al., 2012, p. 20) of public procurement?

Before discussing in more detail, in the next section, how the invitation to bid's idiosyncrasies support its use as a measure of corrupt behavior, an important caveat must be given. Although my dataset on municipal procurement can be quite detailed, that amount of detail is not uniform. Not all public databases of every state's Court of Auditors make the same amount of information available for the municipal contracts. I construct my measures for the analyses below only using data available for (nearly) every contract, for every municipality: whether there was an auction or not, the modality of auction or the legal justification for the lack of an auction, and the estimated value of the acquisition. This is, in my view, this chapter's greatest shortcoming. Were all the data as rich as the most detailed observations, I could have tested many of the assumptions I make in this chapter: do municipalities pay more for the same goods, in the same quantities, if they acquire through invitations to bid, as dos Reis and Bugni (2017) claims is the case for the federal gov-

ernment? How effective is the invitation to bid at keeping firms out when the goal is to direct the contract to a pre-selected winner or to an in-group of politically connected firms (in the previous chapter, I mention that in Rio Grande do Sul between 2013 and 2019, 85.9% of auctions by invitation to bid do not receive bids from non-invited parties, but is that applicable to other parts of Brazil)?

That caveat notwithstanding, I believe that this chapter contributes to the literature on political corruption in two important ways. First, more specifically, by introducing a new measure the concept of 'corruption risk' (as described e.g., in Fazekas and Wachs (2020)) and using that measure to test claims related to corruption, its causes, and its consequences. Second, more generally, by focusing on the public procurement process, which is a treasure trove of information on the process of governing.

3.2 The invitation to bid as a measure of corrupt behavior

The invitation to bid violates several 'best practices' of public procurement, and while its practice is not corrupt *per se*, it is a frequent and convenient venue for corrupt exchanges. Its use has been cited in Brazilian federal courts as a deciding factor on whether a bidder in a procurement process was given an illegal advantage: in 2011, the Superior Court of Justice (*Superior Tribunal de Justiça, STJ*) ruled that the mayor of Carvalhópolis-MG had violated the law when, in 2002, the municipality contracted a business owned by his daughter and his son-in-law for the provision of services to municipal employees. According to the Court's opinion, a town signing a contract with a business owned by the mayor's relatives is not by itself a crime under the Administrative Improbity Act. Instead, the criminal act was in choosing that business as the winner of the contract "based on an inadequate procurement process and a bad-faith choice of modality" (REsp. 1.245.765, STJ, 2011[4]). Carvalhópolis made use of 'auction structuring' (as I explain in the last chapter) so that a contract that ultimately cost the municipality 176,129 BRL could be procured through invitations to bid, with the ultimate goal of favoring that firm.

The illegal use of invitations to bid in Carvalhópolis was not an isolated case: one or more invitation-to-bid related violations were found in over one-third of municipalities randomly audited by the federal government

[4]Decision available at https://stj.jusbrasil.com.br/jurisprudencia/21102991/recurso-especial-resp-1245765-mg-2011-0040108-7-stj.

between 2006 and 2016. In six cases,[5] the owners of two or more of the firms invited were related to each other. Pesqueira-PE's 2010 audit report,[6] for example, mentions an auction to purchase cleaning supplies for municipal schools in which invitations to bid were sent to two firms whose sole proprietors are brothers. The audit uncovered that, five days before the bids were submitted and a winner was selected, the municipality coincidentally earmarked for the contract the exact value (24,287.90 BRL) that the winning firm, owned by one of the brothers, would bid in the auction. This demonstrated, to the satisfaction of the Court, that the auction had been rigged and that the 'winner' was known to the municipality before any bids had been submitted; the use of the invitation to bid procedure, and the inclusion of a firm owned by the winner's brother as one of the three legally mandated invitees, was necessary for the scheme to work.

This sort of distortion — abusing the process to steer contracts towards politically connected bidders — is the most common in the literature on corruption in public procurement (Auriol et al., 2016; Goldman et al., 2013).

The invitation to bid, in giving governments (and, within government bodies, individual public officials) considerable latitude in the acquisition process, provides a rather simple mechanism for corruption, chiefly because invitations to bid may be conducted by an ad-hoc 'committee' consisting of a single public official, and because public notices of those auctions do not have to be published in an official government gazette. These two characteristics, designed to simplify acquisition processes, create a clear "lack of proper preventive capacity" (Stefanov & Karaboev, 2015, p. 49) for the detection of corrupt acts that is inherent to the invitation-to-bid process, and may help explain why audits routinely detect violations related to invitations to bid. The relative obscurity of auctions, and the relative autonomy of one-person 'committees', may lead to corrupt outcomes explicitly, in the form of preferential distributions of invitations to favor firms or individuals with high levels of political connectedness, or more implicitly in the form of highly restrictive conditions for firms, invited or not, that wish to submit bids (for instance, in the Pesqueira-PE case mentioned above, the winning bidder did not submit the lowest bid for some of the items being acquired, but it was declared a winner for those items because the other firms failed to submit 'samples' of the products they wished to sell the

[5] Herveiras-RS, audited in 2007, Chapada do Norte-MG and Ilha das Flores-SE, audited in 2009, Pesqueira-PE and Terra Santa-PA, audited in 2010, and João Pessoa-PB, audited in 2016.

[6] Available for download at `https://eaud.cgu.gov.br/relatorios/download/867599`.

government).

These distortions are intrinsic to the modality, as its original purpose was to facilitate the acquisition of goods and services of lower value (Jardim de Amorim, 2017), and to "allow for greater flexibility and speed in the selection process" (Carvalho Filho, 2019, "Modalidades: Convite", para. 3). However, the invitation to bid ceased to be the most expeditious modality of procurement in 2005, when Decree 5.450/05 formalized the online reverse bidding for the acquisition of ordinary goods and services. The online reverse bidding is highly efficient, mainly due to its inversion in the order of the determination of bidders' eligibility. As I explained on Section 2 of the last chapter, every form of procurement requires the government to verify the eligibility of every bidder before bidding can take place. In a reverse bidding process, only the eligibility of the winner must be checked – the eligibility of runners-up must only be verified if the winner is deemed ineligible to fulfill the contract. This requirement saves time and money not only to the public administration, but also to potential bidders, given that "in auctions in which the eligibility stage precedes the bidding stage, every bidder has an incentive to challenge the eligibility documents [submitted by its competitors]" (Rezende, 2011, p. 28), which often causes potential bidders to incur legal costs; firms that cannot afford such legal representation are 'commonly' driven away from the public procurement market (Ceccato, 2012).

Thus, like the invitation to bid, the online reverse bidding creates a solution to the sluggishness and labor intensity of public procurement processes by local governments — however, unlike the invitation to bid, the online reverse bidding modality solves those problems while simultaneously increasing competition and, according to Ceccato (2012), Mattos (2016), and Rezende (2011), hindering the creation and operation of cartels. The relative efficiency of reverse bidding auctions, particularly those carried out online, reduces the credibility of the most obvious explanation for why municipalities conduct invitations to bid. Another possible explanation is that invitations to bid are necessary for low-value contracts because not enough firms are interested to seek out information on those auctions, prepare the necessary documentation, and place bids. At first glance, my dataset does not lend support to this idea: out of 790,110 low-value municipal auctions – i.e., those with estimated values between 8,000 BRL (the threshold below which auctions can be waived and contracts can be signed directly with a chosen individual or firm) and 80,000 BRL (the threshold above which invitations to bid cannot be conducted), 53.7% were conducted through the reverse bidding modality, and

39.6% were invitations to bid (the remaining 6.7% were either requests for proposals or competitive bidding processes).

Nevertheless, the invitation to bid is not by itself corrupt: the literature and the jurisprudence discussed here simply indicate that it is a "poor governance [structure]" (Podumljak & David-Barrett, 2015, p. 74) that evidences a significant "corruption risk" (Fazekas & Wachs, 2020). In other words, the procedure is less conducive to efficient public acquisitions than to corruption. This is why, in section 3.3, I test whether the Brazilian federal government's random audits program is associated with a decrease in the use of the invitation to bid by municipal governments. While the random audits program's effect on political corruption is difficult to measure, due to reasons stated previously, it is generally hypothesized to reduce corruption; this may be because audits reduce politicians' ability to deliver on their promises of clientelistic goods, which damages those clientelistic networks and the politicians' reputations as credible partners (Bobonis et al., 2019), because audits expose corrupt politicians who are punished electorally (Ferraz & Finan, 2011), or simply because audits increase the likelihood of legal actions being taken against corrupt mayors (Avis et al., 2018). In any case, the literature suggests that there should be a negative correlation between being randomly audited and engaging in 'odd' policy implementation strategies that are more indicative of an incentive to engage in corruption than of an incentive to procure efficiently. I test whether that is true below.

3.3 Accountability of municipal governments through federal audits

In this section, I investigate whether the auditing of municipalities by the federal government is associated with changes in the use of the invitation to bid. The federal auditing program was instituted in 2003; these audits are conducted by the Comptroller General of Brazil (*Controladoria Geral da União*, 'CGU'). Municipalities are selected by lotteries conducted, in publicly available sessions, by the same federally-owned bank responsible for the administration of the federal government's lotteries.[7] (Caldas et al., 2016; de Carvalho Casalecchi & de Oliveira, 2010) As mentioned in the introduction to this chapter, the audits occur in 'rounds',

[7] Audit lotteries also follow the same methods of actual lotteries; the CGU announcement for the 3rd lottery, available at `https://www.gov.br/cgu/pt-br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores/legislacao/convocacoes/sort3.pdf`, cites a list of rules for how municipalities will be selected. Those rules strongly suggest that the drawing is done with balls being drawn from a large bingo cage.

usually two to five per year, each corresponding to one lottery draw. The first round of audits occurred in 2003; the 40th took place in 2015. From 2015 onwards, the program added a method based on a predictive model to select municipalities for audit rounds. In 2016, a (so far) final lottery took place.

The audits are conducted by 10 to 15 CGU auditors who travel to municipalities and "examine... documents and financial statements [and] inspect... construction projects and [public] services" (Caldas et al., 2016, p. 243). At the end of this process, the auditors submit a report with their findings, detailed lists of violations including responses by municipal governments, and sometimes photographic evidence of violations. These reports are publicly available. The scope of the audits is limited to "programs implemented by local officials with funds [from] discretionary transfers by the federal government" (de Carvalho Casalecchi & de Oliveira, 2010, p. 50). This excludes transfers that are constitutionally mandated (e.g., transfers of 'royalties' from the federal government to coastal municipalities for offshore oil extraction), and programs that are exclusively funded by the municipality's own revenues.

Importantly, the criterion is not that CGU audits municipal use of federal money, but municipal *programs* funded with federal money. Thus, for example, the first section of the report regarding the audit conducted in Vargem Alegre-MG in 2008[8] is about a Ministry of Education program to fund school meals in municipal primary schools. The violation listed as 1.1.2 is about the invitation to bid: the municipality conducted seven invitation-to-bid auctions, three in 2007 and four in 2008, to purchase food products for school lunches. In all seven, the same firms were invited, and only one of those firms submitted bids. If an invitation to bid receives less than three valid bids, federal law mandates that the auction be repeated, with invitations also sent to different potential sellers; instead, in all seven auctions, the government of Vargem Alegre awarded contracts to the sole bidder (violation 1.1.3 in the same report is about the same seven contracts, and mentions that Vargem Alegre used what I call 'auction structuring' in section 2.6 in order to conduct those seven auctions as invitations to bid). When discussing those violations, the report does not mention whether those contracts were funded entirely with money from federal transfers; school meals (and other basic public service programs) are often funded with money from a combination of sources — municipal, state, and federal. That there was a transfer of federal money for the purpose of funding school lunches is sufficient for those

[8] Available at `https://eaud.cgu.gov.br/relatorios/download/867981`.

auctions to be eligible for auditing.

Broadly speaking, the questions I aim to answer using data from that auditing program speak to the literature on political institutions and how their design may reduce political corruption. More specifically, these questions are relevant to a growing literature on a form of accountability that, as far as I can tell, has not been properly defined in that literature.

To clarify: the auditing of municipal accounts by the federal government does not represent a form of vertical accountability as defined by O'Donnell (1998), i.e., accountability of the state by voters/citizens (although this is a possible, and desirable, indirect result of the audits, as they represent a credible and public source of information on the performance of elected officials and therefore may inform voters). The audits are also not a form of horizontal accountability in O'Donnell's definition, which refers to accountability of the state, by the state, but in a Madisonian sense. In other words, horizontal accountability is "accountability between more or less equal institutions" (Lührmann et al., 2020, p. 812).

The concept of 'diagonal accountability' also exists in the literature, in different formulations, none of which applies here. For example, Goetz and Jenkins (2001) use the term to describe efforts by non-governmental organizations in India to convince governments to include civil society groups in formal oversight processes; Lührmann et al. (2020), on the other hand, use it to mean the "broad range of actions [taken by] civil society organizations, an independent media, and engaged citizens... to provide and amplify information about the government, thereby holding it accountable" (p. 813). Strictly speaking, the latter is not 'accountability', which means "subjecting power to the threat of sanctions; obliging it to be exercised in transparent ways; and forcing it to justify its acts" (Schedler, 1999, p. 14). Thus, Lührmann et al.'s definition is not accountability, but monitoring: the media and civil society pressure those in power to act transparently and to justify their acts, but without the threat of sanctions. Nonetheless, it is clear that neither definition of diagonal accountability applies to the phenomenon I study, or to the literature to which I will contribute. Rather, this is a form of accountability that is both horizontal and vertical – accountability between 'unequal equals'.

This type of accountability is quite consequential, and an important tool to improve local governance. Oversight of local governments by state governments can help avoid fiscal crises (Kloha et al., 2005), and the effectiveness of this monitoring may increase by making it more formalized, and more meticulous; audit

programs may lead to positive outcomes before audits actually occur, as "local governments [that] are aware that an audit was going to take place... [learn] from the irregularities found at other local governments" (Sepsey, 2011, p. 419). Similarly, Beylis et al. (2016) find that an increased probability of being audited reduces corrupt behavior by Brazilian mayors, and that the effect is stronger for mayors in their first term, as they are incentivized both "through the penalty they may be required to pay but also through the electoral incentive" (p. 21). Indeed, this "electoral incentive" is the principal mechanism proposed by Ferraz and Finan (2011) for the effect that the auditing program in Brazil has on the actions of municipal governments.

Of course, if my hypothesis is correct on why local governments use invitations to bid, then the ways in which auditing shapes the incentives of mayors should extend to the use of the procedure. There is limited evidence to support this expectation: Gerardino et al. (2017) find that, in Chile, audits significantly impact public procurement strategies (although, in the Chilean case, auctions are much more closely scrutinized than direct contracts, which is certainly not the case in the Brazilian context). The effect of auditing on procurement strategies seems to mostly apply to those that are backward-looking, i.e., designed to investigate and punish past wrongdoing by public officials, rather than those that are forward-looking, i.e., those that aim to discover problems and craft solutions (Sabet, 2020); in Honduras, according to Sabet, forward-looking audits of public procurement have had no direct effect on procurement portfolios, although they led to reforms in the country's procurement processes.

Thus, my analyses below aim to answer: are federal audits an effective tool against the use of the invitation to bid? I test this question in two ways: first, by analyzing only municipalities that were audited, and comparing their rates of invitation-to-bid auctions in the years before and after they were audited. Second, following e.g., Avis et al. (2018) and Ferraz and Finan (2018) by analyzing municipalities whose neighbors were audited.

3.3.1 Data and methods

The dataset used for the analyses below is described in detail in the previous chapter. It contains data on procurement processes by Brazilian municipalities from fifteen states[9] that make such data publicly available.[10]

Data on federal audits of municipalities conducted by the Comptroller General do not come directly from that office, but were collected from three sources: from the 20th lottery onwards (2006-2016), data on audits that include summaries of each finding is available from `basedosdados.org`, a project for archiving and preserving data from Brazilian government sources (Carabetta et al., 2021). Data on which municipalities were chosen in the 8th-19th lotteries is from Barberia et al. (2016); that dataset contains information up to the 33rd lottery, and was used to verify the accuracy and completeness of the data from Carabetta et al. It does not include textual summaries of audit findings. Data on which municipalities were chosen in the 2nd-7th lotteries is only available, among all publicly available sources that I could find, from Ferraz and Finan (2019) — the replication dataset for Ferraz and Finan (2011). However, that data is unusable, because it does not include municipalities' names, and the municipality codes column does not follow any known standard.[11] The replication scripts available does not show how those codes were constructed or whether they are modified versions of the actual IBGE codes. Therefore, the data for the analyses below only consider the 8th-41st lotteries. That encompasses the 2004-2016 period, and a total of 1,238 municipalities from states in my procurement data.

In my first analysis, I test whether municipalities conduct more auctions by invitation to bid after an audit. That analysis is presented in two ways: first, I compare the year before an audit with the year after (due to

[9] Those states are Ceará, Goiás, Maranhão, Minas Gerais, Pará, Paraíba, Paraná, Pernambuco, Piauí, Rio Grande do Norte, Rio Grande do Sul, Roraima, Santa Catarina, São Paulo, and Tocantins. I have made requests to the Courts of Auditors of other states for their municipal procurement data: the Court of Auditors of the state of Amapá, after publishing my taxpayer ID (*Cadastro de Pessoa Física, CPF*) on their website in a list of outstanding information requests, responded to my request by stating that they do not have a system through which to share the data; the state of Goiás, which has procurement data, responded to my request for more information (e.g., on numbers of invited parties that actually submitted bids) by stating that the level of detail I was asking about is only made available for employees of the Court and municipal government officials that interact with their systems to submit data. No Court of Auditors that I contacted stated that they did not collect, or did not possess, the data I had inquired about.

[10] The state of Acre makes municipal procurement data publicly available, but that data is not used for the analyses in this chapter due to the quality of the data on the website of the state's Court of Auditors. Most importantly, information on the estimated value of the auction is missing for about 80% of auctions.

[11] The variable is named 'cod_ibge6', which suggests that those are IBGE's six-digit municipality identifiers (*many* official methods of assigning numerical codes to municipalities exist: IBGE has six- and seven-digit codes; the Superior Electoral Tribunal (*Superior Tribunal Eleitoral*, TSE) has its own codes as well). None of the codes correspond to any of IBGE's six-digit municipality identifiers; none of my attempts to transform those identifiers to IBGE-conforming codes succeeded.

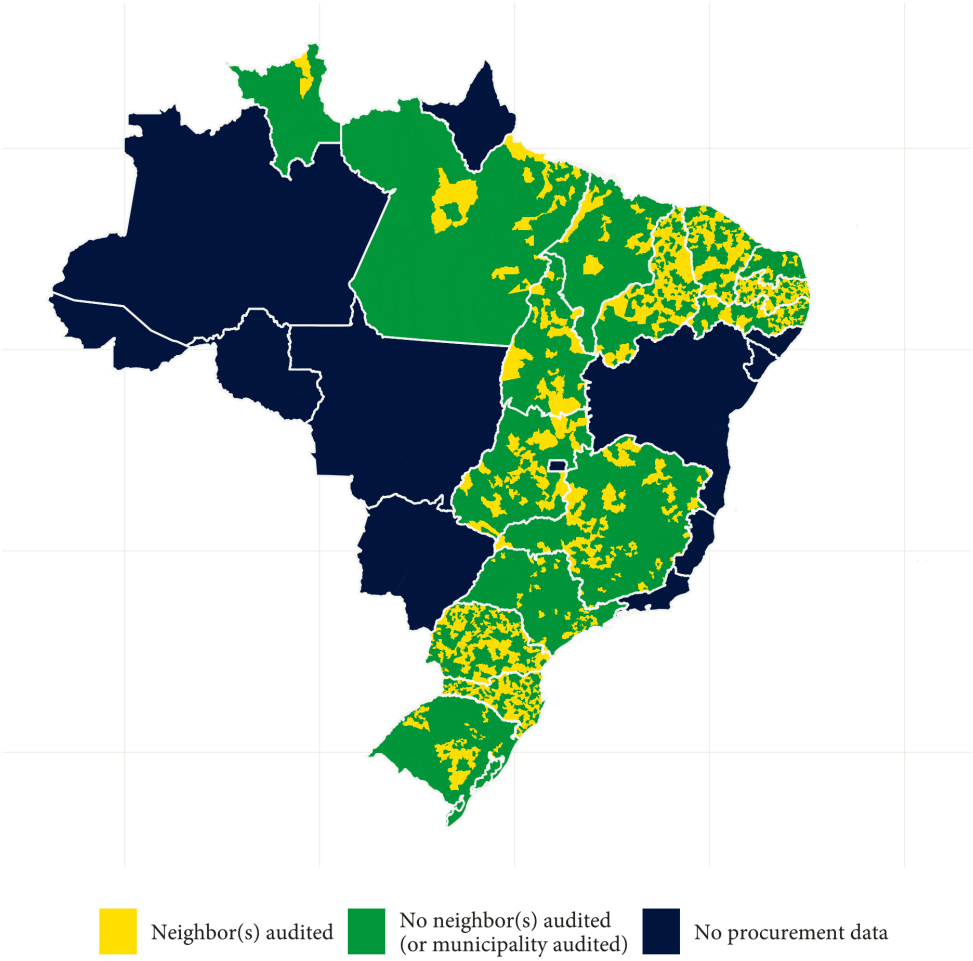
differences in the timing of the audit, and the publication of its results, I exclude the year when the audit happened, as some acquisitions in that year will have happened before and some after the audit), in a model that only includes observations for which I have procurement data for both years: 206 municipalities, audited between 2004 and 2016. Second, I compare the two years before an audit with the two years after. This second analysis constitutes a smaller sample of municipalities (139) but a larger sample of procurement processes.

In my second analysis, I test the hypothesis, put forth by Avis et al. (2018) and Ferraz and Finan (2018), that the auditing program's hypothesized effects on political corruption carry over to those municipality's neighbors, particularly through the audits' law-enforcement mechanism. For those models, the appropriate scope conditions should exclude municipalities that were audited at any point in the period under consideration (e.g., if the model includes observations for a municipality two years before its neighbor was audited and two years after, then that is a five-year period in which that municipality should not have been audited).

However, studies of this auditing program have found that being audited produces long-lasting changes: e.g., Lichand et al. (2018) find that "the effect [of auditing on corruption] does not dissipate during the subsequent term" (p. 3); Avis et al. (2018) find, for municipalities that were audited twice, "no evidence of a differential effect [of a past audit on the number of violations detected in subsequent audits] based on how much time had elapsed since the last audit" (p. 1948). Therefore, I exclude from the audited-neighbor model any municipality that was audited at any time in the past. This is then, purely a model of 'unaudited' municipalities whose neighbor(s) were audited. Figure 3.1 shows the 1,124 municipalities in the data that meet those conditions. To find a municipality's neighbors, I use queen contiguity (which defines two polygons as 'neighbors' even if their shared border is a single point), in order to account for the possibility of error introduced by simplified polygons in the shapefile I use.[12]

[12]The shapefile used for the creation of neighbors lists is from IBGE, available at `ftp://geoftp.ibge.gov.br/organizacao_do_territorio/malhas_territoriais/malhas_municipais/`.

Figure 3.1: Map of Brazilian municipalities by whether at least one of its neighbors was audited between 2004 and 2016



The unit of analysis for the models below is the municipality-year. The outcome variable is the share of procurement processes that qualify for the invitation to bid procedure and that were conducted that way.[13] The main independent variable, shown in the tables as 'year (or years) after audit', is a dummy variable for whether that observation occurred in the year(s) after an audit, considering the 8th-41st lotteries.

Each model is run in three ways: first, because the assignment to treatment is actually random, I fit a bivariate model. Second, because the number of audited municipalities changes across lotteries, as presumably does the amount of resources dedicated to the auditing program and the format of the audits, I then

[13] In the calculation of this variable, I exclude: procurement processes with estimated values under 8,000 BRL; procurement processes with estimated values over 80,000 BRL before 2018, or over 176,000 BRL in 2018 or 2019; 'price registrations' (for reasons explained in the previous chapter).

fit a varying-intercepts model with terms for the lotteries. Finally, following the literature on this particular auditing program, I fit a model with a series of controls to adjust for municipality- and mayor-specific characteristics that may affect how an audit may affect the behavior of a municipal government. Those terms are: the count of procurement processes (with or without auction) that fit the boundaries specified above for modalities and estimated prices;population, logged; GDP per capita, logged; annual GDP growth; the percentage of the municipality's population with at least 8 years of education, which in Brazil indicates having completed primary education (*ensino fundamental*); following Ferraz and Finan (2011), whether the municipality has a local AM radio station; whether the mayor is in her second term; whether the mayor's political party was a part of the electoral coalition of the president in the most recent election; whether the mayor is in the same party as her state's governor. In the models with these controls, I also include lottery- and state-fixed effects.

The models for whether a neighbor having been audited correlates with procurement choices contain four additional controls to adjust for characteristics of the audited neighbor. First: Avis et al. hypothesize that the spillover effect is, in part, due to the presence of local news media, although they do not test whether the mechanism only works if these media outlets are located in the unaudited municipality, or if news media presence in the audited neighbor also produces the effect (in other words, it is uncertain whether the effect of the media also 'spills over'). For that reason, the audited-neighbor models include a term for whether the audited neighbor has a local AM radio station. Second: to account for the possibility that the demographics of the audited neighbor may have an impact on whether its audits influence other municipalities' procurement choices, I include a term for the audited neighbor's population. Third, to account for the possibility that the hypothesized spillover may be affected by political or partisan connectedness, I add a control for whether the audited neighbor's mayor is in the same party as the unaudited municipality. Finally, I add a term for the number of neighbors that the audited municipality has. Given that this affects the probability of receiving treatment, I add that term to the model with only lottery-fixed effects as well as to the model with controls.

Before proceeding to the results of those models, an important caveat regarding data availability: the presence of a local AM radio station is not reported by IBGE every year; I have found that data in IBGE's municipality profiles[14] for 2001, 2006, 2009, 2012, 2014, and 2018. Thus, I take the most recent year of data

[14] Available at `ftp://ftp.ibge.gov.br/Perfil_Municipios/`.

as the value for that variable in the analyses below. This is, of course, a potential source of bias for years that are not directly included and whose values are assumed by these proxies. However, the presence of AM radio stations is quite stable across data years; their absence is even more so. For example, 82.4% of municipalities that had a local AM radio station in 2001 also had one in 2006 and 2009; 93% of municipalities that did not have an AM station in 2001 also lacked one in both 2006 and 2009.

Education data for every municipality is only available for census years — in this data, the relevant censuses were conducted in 2000 and 2010. Like the AM radio variable, the percentage of population with at least a primary education variable in the models below correspond to the municipality's value in the last year for which there is data.

3.3.2 A note regarding São Paulo-SP

São Paulo is an extreme outlier in every possible metric: according to a 2018 UN report, São Paulo is the largest city in the world other than Tokyo, Delhi, and Shanghai.[15] The city's GDP in 2019 (761.4 billion BRL) was 10.7% of Brazil's; its economy is larger than that of the 4,357 bottom municipalities (78% of the total) combined. In 2019, the city conducted 3,720 procurement processes that fit the parameters I specify above (i.e., that would have qualified for the invitation to bid modality) — nearly four times as many as the second-highest municipality/year in the data (Recife in 2018 had 1,030 such acquisitions). Of relevance to subsection 3.4.2, São Paulo in 2019 administered doses of the BCG vaccine to 131,862 children under age 1; that figure is higher than the total populations of 96% of Brazilian municipalities.

Thus, although São Paulo is of fundamental importance to the study of Brazil as a case,[16] it is also inevitably a high-leverage observation in models of Brazilian municipalities. For that reason, São Paulo is excluded from every model in this chapter. The inclusion of São Paulo would not have altered the results of any models in this chapter.

[15] https://www.un.org/en/events/citiesday/assets/pdf/the_worlds_cities_in_2018_data_booklet.pdf.

[16] The city is also fundamental to the existence of this dissertation, as I am a (clearly proud) São Paulo native.

3.3.3 Results and discussion

Table 3.1 shows the results for the first set of fractional logit models, with the year before municipalities were audited compared to the year after. Table 3.2 shows results for models fit with data from the two years before and two years after an audit. To account for the possibility that the effects on Table 3.2 are caused by those observations that occur the year before or the year after an audit (i.e., the observations that comprise the sample in the models of Table 3.1), I fit these same models only including observations two years before or two years after an audit. Those are shown in Appendix C.

Table 3.1: Models of invitation to bid incidence 1 year before and after an audit

Dependent variable: *Rate of invitation-to-bid use among qualifying procurement processes*			
(Intercept)	0.26^{**}	2.29^{***}	3.86^{***}
	(0.10)	(0.10)	(0.77)
Observation is after audit	-0.51^{***}	-0.66^{***}	-0.64^{***}
	(0.14)	(0.13)	(0.13)
No. of acquisitions in year			0.00
			(0.00)
GDP per capita (log)			-0.34
			(0.19)
GDP growth in previous year			-0.53
			(0.52)
Local AM radio in munic.			-0.23
			(0.22)
Population (log)			-0.12
			(0.10)
% over 15 w. 8+ years of education			0.59
			(1.13)
Mayor's party in president's coalition			-0.72^{***}
			(0.13)
Mayor in same party as state governor			0.12
			(0.16)
Mayor is in second term			0.03
			(0.14)
Lottery f.e.		Yes	Yes
State f.e.			Yes
AIC	567.07	465.03	424.65
BIC	575.12	597.72	637.77
Log Likelihood	-281.54	-199.51	-159.33
Deviance	246.57	144.88	90.77
Num. obs.	412	412	412

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 3.2: Models of invitation to bid incidence 2 years before and after an audit

Dependent variable: *Rate of invitation-to-bid use among qualifying procurement processes*			
(Intercept)	0.36^{***}	3.79^{***}	3.77^{***}
	(0.08)	(0.11)	(0.69)
Observation is after audit	-0.80^{***}	-1.01^{***}	-0.97^{***}
	(0.12)	(0.11)	(0.12)
No. of acquisitions in year			-0.00
			(0.00)
GDP per capita (log)			-0.69^{**}
			(0.25)
GDP growth in previous year			0.12
			(0.37)
Local AM radio in munic.			-0.32
			(0.18)
Population (log)			-0.06
			(0.08)
% over 15 w. 8+ years of education			1.37
			(0.93)
Mayor's party in president's coalition			-0.35^{**}
			(0.12)
Mayor in same party as state governor			0.06
			(0.12)
Mayor is in second term			-0.07
			(0.12)
Lottery f.e.		Yes	Yes
State f.e.			Yes
AIC	744.01	604.15	548.41
BIC	752.65	720.81	742.84
Log Likelihood	-370.00	-275.07	-229.20
Deviance	303.02	189.40	127.01
Num. obs.	556	556	556

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

In the year(s) after an audit, municipalities conduct their acquisitions through the invitation to bid procedure at substantially lower rates. That finding is robust to the temporal cutoff chosen; indeed, the estimates are consistently larger in magnitude for the models that include four years of acquisitions (two before the audit and two after).

This may be due to the mechanisms proposed by Avis et al. (2018) and by Zamboni and Litschig (2018). Avis et al. find that audits reduce corruption mostly due to non-electoral incentives they provide. Those two hypotheses are plausible in this case. Non-electoral punishments are more likely than electoral ones to change a local government's procurement portfolio, because the distribution of a government's procurement modalities is largely unobserved by voters and, as I discuss in the next sections, the relationships between rent-creating procurement procedures and policy outcomes appear to be relatively weak. Important among those non-electoral incentives is that proposed by Zamboni and Litschig, who find that the greatest impact on procurement-related corruption comes from increasing a municipality's perceived audit risk. That is a possible reason for the stronger association in the models that compare two years after an audit with two years before: due to the rule that audited municipalities are excluded from subsequent lotteries, perhaps the effect of an audit (assuming that these results reflect any 'effect' at all) on rent-seeking is initially dampened by the reduced audit risk in the subsequent year.

Nonetheless, municipalities that are audited conduct significantly fewer auctions by invitation to bid in the year(s) after an audit. As Table 3.3 and Table 3.4 show, an association also exists, albeit weaker, in unaudited municipalities whose neighbors are audited.

In these models for unaudited municipalities with audited neighbors, the estimates for the dummy variable for whether the observation took place after the audit are all in the same direction as the models for audited municipalities; they are all moderately smaller, regardless of model specification; the models that include data from two years before and after an audit have higher estimates for the after-audit period than the models that exclude those years. Of course, if there is an 'effect' to speak of here, it would come from different mechanisms: one candidate from the literature is, again from Avis et al. (2018), the increased probability of prosecution that an audit leads to. This mechanism is implied by Avis et al.'s idea of a spillover effect. According to the authors, it is not exactly the spilling over of knowledge that an audit occurred, or what auditors

Table 3.3: Models of invitation to bid incidence 1 year before and after a neighbor is audited

Dependent variable: *Rate of invitation-to-bid use among qualifying procurement processes*			
(Intercept)	0.27^{***}	2.00^{***}	3.49^{***}
	(0.05)	(0.21)	(0.58)
Observation is after audit	-0.43^{***}	-0.59^{***}	-0.55^{***}
	(0.07)	(0.06)	(0.06)
No. of acquisitions in year			0.00
			(0.00)
GDP per capita (log)			-0.28^{**}
			(0.11)
GDP growth in previous year			0.13
			(0.24)
Local AM radio in munic.			-0.12
			(0.09)
Population (log)			0.00
			(0.05)
% over 15 w. 8+ years of education			-1.75^{***}
			(0.50)
Mayor's party in president's coalition			-0.11
			(0.07)
Mayor in same party as state governor			-0.06
			(0.08)
Mayor is in second term			0.02
			(0.07)
AM radio in audited neighbor			0.08
			(0.10)
Mayor in same party as audited neighbor's			-0.05
			(0.09)
Audited neighbor's population (log)			-0.05
			(0.05)
No. of neighbors		-0.03^{*}	-0.02
		(0.02)	(0.02)
Lottery f.e.		Yes	Yes
State f.e.			Yes
AIC	2493.70	1851.93	1635.03
BIC	2504.71	2050.16	1976.44
Log Likelihood	-1244.85	-889.96	-755.52
Deviance	1112.72	657.22	472.95
Num. obs.	1820	1820	1820

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 3.4: Models of invitation to bid incidence 2 years before and after a neighbor is audited

Dependent variable: *Rate of invitation-to-bid use among qualifying procurement processes*			
(Intercept)	0.33^{***}	1.73^{***}	4.72^{***}
	(0.04)	(0.26)	(0.51)
Observation is after audit	-0.69^{***}	-0.97^{***}	-0.95^{***}
	(0.05)	(0.05)	(0.06)
No. of acquisitions in year			0.00^{***}
			(0.00)
GDP per capita (log)			-0.38^{***}
			(0.09)
GDP growth in previous year			0.11
			(0.22)
Local AM radio in munic.			-0.02
			(0.08)
Population (log)			-0.13^{**}
			(0.04)
% over 15 w. 8+ years of education			-0.50
			(0.40)
Mayor's party in president's coalition			-0.05
			(0.06)
Mayor in same party as state governor			-0.08
			(0.07)
Mayor is in second term			-0.03
			(0.05)
AM radio in audited neighbor			0.13
			(0.09)
Mayor in same party as audited neighbor's			-0.08
			(0.07)
Audited neighbor's population (log)			-0.07
			(0.04)
No. of neighbors		-0.02	-0.01
		(0.01)	(0.01)
Lottery f.e.		Yes	Yes
State f.e.			Yes
AIC	3728.52	2718.65	2407.75
BIC	3740.37	2914.21	2751.45
Log Likelihood	-1862.26	-1326.33	-1145.88
Deviance	1676.53	983.76	719.27
Num. obs.	2768	2768	2768

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

looked into, that matters, but actually the spilling over of information on any findings of criminal wrongdoing by public officials, or any resulting legal sanctions.

If this is the case, then the idea of a delay appears reasonable. Simpler explanations exist, of course, although they require assumptions that may be stronger than those supported by the literature. For instance, maybe the auditing process actually results in transmission of knowledge from the federal auditors to the local public officials. Then, even though the audits are backward-facing (Sabet, 2020), they still lead to improvements in the governing practices of well-meaning bureaucrats who lack information on how to efficiently conduct their acquisitions. This process would explain the findings on Table 3.1 and Table 3.2, and also add to the information that spills over from an audited municipality to its unaudited neighbors.

Regarding the models of unaudited municipalities with audited neighbors, it is quite likely, of course, that the spillover effect results from all of the kinds of information I speculate about in this section, and more. The spillover I discuss itself involves two different mechanisms: the first is transmission of information *between* adjacent municipal governments, the second is transmission of information *about* an adjacent municipal government, but directed at the public. They may all happen following an audit, through different channels. Testing which kind of information matters — and at whom such information should be directed to have an effect on corrupt behavior — is unfortunately beyond the scope of this chapter, but it may be an interesting avenue for future research.

Still, these models lend support to the idea that the random audits program is an effective tool in reducing rent-seeking behavior by local governments, even if such activities are largely hidden to the public. As I attempt to show in the next sections, this finding could be enormously consequential from a policy perspective.

3.3.4 Testing for the possibility of a spurious relationship due to a temporal effect

The main concern with the analyses above is that they are all before-and-after comparisons of the same units. Perhaps the effect seen above is entirely attributable to that fact — a national decrease in the rate of invitations to bid over time leads to the estimates above, and the relationship between the occurrence of an audit and the rates of invitation-to-bid auctions is simply a relationship between an 'after' and a 'before'.

To test whether this is the case, I first build the same model as the model with covariates and fixed effects from Table 3.2, but with a simulated dataset with 'audit' years that I randomly assign to municipalities that were not audited in reality. The dataset is constructed by first excluding any municipality that was ever audited, or that ever had a neighbor audited; out of 4,419 municipalities in the original procurement data, 593 remain in this step.[17] Second, I also remove any municipality for which I have less than five years of procurement data, leaving 393 municipalities. Then, randomly assigning a fake 'audit' year between the second and the second-to-last years of procurement data (but never the first or the last, such that there is always a 'before' and an 'after' to compare). The model reported below includes two years before and two years after each fake audit, such that municipalities that were randomly assigned a fake audit on the second or the second-to-last years of data are also excluded. Despite these rules, the fake audit model still has a larger sample size (168 municipalities) than the model that compares two years before to two years after a real audit (139 municipalities).

Nonetheless, as Table 3.5 shows, the estimate for the 'after' period is much smaller than the estimates in the actual models, and does not achieve statistical significance under the usual thresholds. To test the robustness of this result to the random assignment procedure, I repeated it 300 times. Because the year of the fake audit determines whether a municipality is included in the sample, each of the 300 samples would be of a different size. To make each simulation comparable, I randomly sampled 139 municipalities from each synthetic dataset.

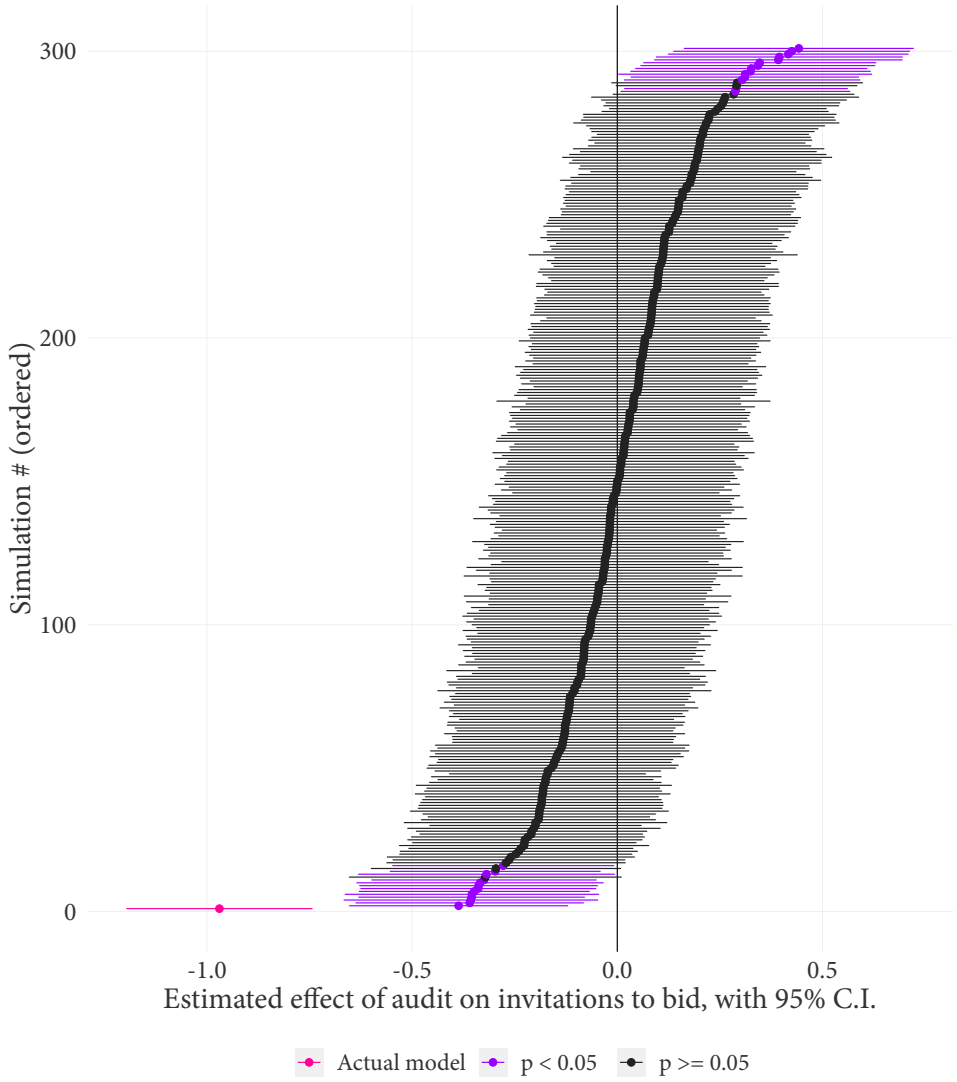
[17] The small number of non-audited municipalities without any audited neighbors is why I could not implement another model idea, in which I took, for each year of data, a sample of never-auditeds of equal size to the number of municipalities actually audited in that year. Having 593 total candidates for the control group, and wanting to select each municipality no more than once, meant not being able to take a sample for years prior to 2008 (because there is not enough procurement data for the two years before those target years.) Once I sampled 36 municipalities for 2008 and 60 for 2009, thus excluding 96 municipalities from further selection, it became impossible to sample 57 municipalities for 2010.

Table 3.5: Models of invitation to bid incidence 2 years before and after a simulated 'audit' year, among municipalities that were not audited

	Dependent variable:
Rate of invitation-to-bid use among qualifying processes	
(Intercept)	4.35^{***}
	(1.09)
Observation is after audit	-0.09
	(0.13)
No. of acquisitions in year	-0.00
	(0.00)
GDP per capita (log)	-0.12
	(0.13)
GDP growth in previous year	0.29
	(0.27)
Local AM radio in munic.	-0.42^{*}
	(0.17)
Population (log)	-0.17^{*}
	(0.08)
% over 15 w. 8+ years of education	0.54
	(0.85)
Mayor's party in president's coalition	-0.13
	(0.11)
Mayor in same party as state governor	-0.36^{**}
	(0.13)
Mayor is in second term	0.29^{*}
	(0.11)
Year f.e.	Yes
State f.e.	Yes
AIC	572.13
BIC	729.99
Log Likelihood	-251.07
Deviance	182.28
Num. obs.	672

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Figure 3.2: Distribution of coefficients from models built on 300 simulated datasets with 'fake' audit years



The point estimates and confidence intervals shown on Figure 3.2 correspond to the 'after audit' term in models identical to that on Table 3.5. The simulations are ordered by the value of the point estimate, with the estimates from the model built on actual data on the bottom; points shown in purple are those for which the simulated model indicated that the estimate is statistically significant from zero at $\alpha = 0.05$. There are 27 such simulations; the sign of the coefficient is negative in only 14 of those. Thus, even though the relationship shown in the models built on actual data may be spurious for other reasons, it is most likely not an artifact of a before-after comparison.

3.4 Is partially closed procurement good for your health?

The invitation to bid is used mostly for the purchase of goods and services; the procedure is used relatively rarely for construction projects or engineering services. For instance, in Rio Grande do Sul, 91.5% of invitations to bid are classified as being for the acquisition of goods or services; that figure is 85.9% for other modalities (excluding the reverse auction, which cannot be used for construction projects or engineering services). In Pernambuco, 83.5% of invitations to bid are for the purchase of goods or services, or for the rental of goods; across other modalities excluding reverse auctions, that is true of only 74.3% of procurement processes.

More specifically, and more importantly to this section, the invitation to bid is widely used for purchases of consumable goods in procurement-intensive sectors, such as healthcare. Healthcare is additionally important to any study of local procurement in Brazil because that country's public health system is highly decentralized, with municipal governments "having the largest share of responsibility for providing services in the sector" (Machoski & de Araujo, 2020, p. 670), both with their own tax receipts and with large transfers of state and federal funds.

My data suggests that, indeed, the invitation to bid is slightly over-utilized in the public health sector. For example, 9.2% of Ceará's 338,000 procurement processes in my data mention healthcare, hospitals, or medication in their calls to bids or waiver/non-requirement justifications. Out of those 31,000 purchases, one-quarter was conducted by an invitation to bid (only about 13% of all procurement processes in that state were invitations to bid). Over one in six invitations to bid in Ceará were for purchases of goods or services directly related to healthcare. Thus, the first downstream effects of the invitation to bid modality that I test are related to public health.

The quality of public health service provision is investigated, for example, by Lichand et al. (2018), who use the federal random audits program to test the effects of political corruption on policy outcomes related to public health. They focus on public health, in part, because it is a particularly procurement-intensive government activity (the authors do identify certain aspects of public health service provision that have 'low procurement intensity'; those are mostly related to long-term healthcare infrastructure, which I adjust for in

the analyses below). Indeed, due to the fact that healthcare provision is highly dependent on consumable goods, and thus procurement-intensive, the effectiveness of public health policy should be highly dependent on the effectiveness of procurement; in the absence of proper checks on rent-seeking in procurement, investment may even be counter-productive (Keefer & Knack, 2007). Thus, due to the procurement-intensity of public health and in this section, I test whether the invitation to bid procedure is associated with worse public health outcomes.

Counter-intuitively, Lichand et al. find that auditing does not improve public health outcomes, and hypothesize that, even though corruption arguably has "high social costs, from... resource misallocation to inefficient investment" (Lichand et al., 2018, p. 27), combating corruption may also produce inefficiencies, as measures to decrease corruption are often designed to limit discretion by public officials, thus discouraging certain types of decision-making. Although they do not focus directly on this point, it exemplifies an interesting conundrum in the use of any auditing data to study political corruption: given that the random audits generate data on corruption by investigating corruption in an administrative/legal context, how can researchers measure the effect of corrupt behavior without, instead, measuring the effect of the auditing program itself?[18] In fact, this problem manifests itself in the article that describes it: in estimating the effect of the introduction of audit program on public health outcomes, Lichand et al. cannot distinguish between the effect of the audits as a corruption-fighting measure and the effect of the audits as external interference on the discretion of bureaucrats and their ability to extract rents in exchange for providing public service, even though part of their paper's main claim is that the latter has a negative effect that nullifies any possible positive effect of the former.

I believe that the use of invitations to bid as a measure of rent-seeking behavior may ameliorate this problem, while elucidating questions about both the invitation to bid as a practice, and about the effects of rent-seeking behavior on important policy outcomes. Estimating the effect of auctions by invitation to bid on public health may shine light on the nature of the procedure because, much like the federal audits, the effects of the invitation to bid on the ability of governments to provide public services are not immediately obvious.

[18] A problem that does not apply, of course, to the literature mentioned on section 3.3 that focuses on the process of auditing, and not only on the audits' findings as a measure of corruption.

On the one hand, it improves efficiency by allowing bureaucrats to bypass some of the more onerous aspects of public procurement. At the same time, it potentially generates inefficiencies, in two different ways: first, by allowing those public officials to engage in corrupt particularistic exchanges (as discussed by e.g., Podumljak and David-Barrett (2015) and in examples of nepotism in procurement that I mention above), which leads to sub-optimal outcomes in terms of the contracts signed by governments. Second, by allowing public agents to soak the public coffers, thus depleting budgets — a particularly salient, and particularly harmful, problem in local governments in the developing world.

Thus, the invitation to bid is a candidate for a measure with which to test the hypothesis of Lichand et al. on the effects of political corruption on public health, especially because it provides the possibility of measuring corruption without simultaneously measuring any form of monitoring that would curb the discretion of bureaucrats: the frequent use of a questionably legal procurement modality is not a particularly conspicuous act, it does not make for bombastic news coverage or winning political attack ads, and (considering how many illegal invitation-to-bid auctions are uncovered by federal audits) it seems to be frequently overlooked by internal accountability processes within municipal governments.

Thus, in this section, I test whether the frequency with which invitations to bid are conducted is associated with worse public health outcomes. Public health is a particularly appropriate policy area for this study because, as Machoski and de Araujo (2020), Nishijima et al. (2016), and Peixoto et al. (2012) point out, public health policymaking in Brazil has undergone a process of decentralization since 1988. As a result, federal healthcare funds are increasingly transferred to states and municipalities instead of being spent directly by the federal government; the federal government establishes strict controls on the programmatic destination of those funds but, relevant to this analysis, the federal government does not control the process of procuring goods and services for healthcare provision under state and municipal hospitals and clinics.

In this analysis, I depart from Lichand et al. in one important respect: due to their empirical strategy, they choose not to use infant mortality as a 'downstream' public health outcome, because it "is affected by *all* health transfers" (p. 17). Infant mortality is the first outcome I test. It is, across all issue areas, one of the most relevant measures of a government's performance in a developing country. In terms of public health outcomes, it is perhaps the most important and the most consequential. Additionally, infant mortality is "[sensitive]

over a short time frame to state investments in medical care and public health" (Conley & Springer, 2001, p. 769), in part because it is "more likely than adult mortality to result from preventable causes and thus to be reducible by appropriate policies" (McGuire, 2013, p. 55). There are, then, clear mechanisms that would link the effectiveness (or, alternative, the 'wastefulness') of procurement to infant mortality rates. If an external shock (e.g., a federal audit) leads to a change in a municipality's procurement portfolio away from invitations to bid, that may be – if my hypothesis holds – equivalent to a short-term increase in investment. Conversely, if a municipality chooses to conduct its purchases in wasteful ways, then it should see worsening conditions in public health service provision, which would lead to negative effects (e.g., lower quality of prenatal care, less capacity to treat those "preventable causes" pointed out by McGuire) that would be reflected in increased rates of infant mortality.

Indeed, the 'wastefulness' of procurement can be anecdotally traced to worse public health outcomes in federal audit reports. For instance, the audit of Água Nova-RN in 2009[19] shows that: the municipality used auction structuring so that it could acquire essential medicine in two invitation-to-bid auctions worth 79,362 BRL and 79,780 BRL, conducted on February 6th and February 9th, 2009 (violations 4.3.3 and 4.3.4); during an inspection in September 3rd, 2009, CGU auditors found that the municipality's pharmacy lacked essential medications that it must have in stock according to Ministry of Health policy. Those missing medications include amoxicillin, an antibiotic commonly prescribed to infants and children as "first line therapy for uncomplicated [acute otitis media], sinusitis, and mild to moderately severe pneumonia" (Klein, 2003, p. S140), and erythromycin, an antibiotic used to treat pertussis in newborn infants and various infections in pregnant people that may otherwise negatively affect the fetus (Louik et al., 2002). The audit report credits the lack of these medications to "an absence of planning [regarding] the management of resources" (p. 49) of the Ministry of Health's pharmaceutical assistance program. This is not, of course, a causal link between the use of invitations to bid and the lack of essential medications in the pharmacy run by the municipality. It is an example, however, of illegal procurement practices affecting a public service area with direct impacts on infant healthcare.

[19] Available at `https://eaud.cgu.gov.br/relatorios/download/867835`.

Although infant mortality is an important indicator, I also model another outcome below, this time following the lead of Lichand et al.: the rate of Bacillus Calmette-Guérin (BCG) vaccination in infants under one year of age. The BCG vaccine, first used in the 1920s and effective at preventing tuberculosis and leprosy (Cernuschi et al., 2018), is the most appropriate choice for this analysis for four main reasons.

First, both the WHO (Organization, 2018) and the Brazilian Ministry of Health (Ministério da Saúde, 2014) recommend administering the BCG vaccine to healthy newborns as close to birth as possible, "preferably within the first 12 hours after birth" (Ministério da Saúde, 2014, p. 69) but later under certain conditions, e.g., for infants under 2kg at birth. Although some studies show that delaying BCG to eight or ten weeks of age may provide stronger immune responses (Dockrell & Smith, 2017), BCG vaccination is almost universally recommended for children under one year of age. This makes for a straightforward calculation of the denominator of a yearly 'vaccination rate' variable: because BCG is administered so soon after birth, that denominator should be the number of live births in a given year, as provided by SINASC.

Second, it is a single-dose vaccine. This allows for a straightforward calculation of the numerator of that 'vaccination rate' variable, as the data reported by the Ministry of Health is at the level of the dose, and not the patient. This dose-level dataset would make it difficult to calculate, for example, how many infants were fully vaccinated against hepatitis B, considering that some hepatitis B vaccines require three doses and some require four, and that the final dose of the three-dose schedule is administered six months after the first (Ministério da Saúde, 2014), which results in doses being given to children born the year prior – greatly complicating the calculation of a yearly rate. To that complication, one may add the additional problem that between 9% and 10% of patients do not get all three doses of the hepatitis B vaccines (Ministério da Saúde, 2015).

Third, there is substantial variation in compliance of Brazilian municipalities with BCG vaccination goals set by the federal government. The Ministry of Health establishes a target of 90% coverage for BCG (Ministério da Saúde, 2015). Between 2003 and 2019, only 4.4% of municipalities met that target every year, and only 62% of municipalities met that target on a majority of years (i.e., at least 9 out of the 17 years in the period), despite the national rate exceeding the target every year.

Finally, the rate of BCG vaccination in a given municipality/year is a proxy for several important aspects

of public health policy: because the vaccine is preferably given at birth, rates are significantly higher among children born in hospitals than among children born at home (Hu et al., 2018; Tsehay et al., 2019); because the vaccine is not given immediately to children born underweight, rates are correlated with the availability and quality of antenatal care, and with the overall health of mothers (Babalola & Lawan, 2009; Tsehay et al., 2019). Similarly, because BCG is given at birth, it avoids certain confounding causes that affect the coverage rates of other vaccines. For instance, Nankabirwa et al. (2010) find that maternal education significantly affects whether children miss doses of vaccines that require multiple appointments.

3.4.1 Data and methods

Dependent variables

The outcome variable for my first set of analyses is the municipality's yearly infant mortality rate, defined as deaths of infants (under one year of age) per 1,000 live births in a given year. The data is from the Ministry of Health's open-data service DATASUS.[20]: the SIM (*Sistema de Informação de Mortalidade* 'Mortality Information System') database provides data on deaths – 13.2 million from 2003 to 2019; the SINASC (*Sistema de Informação sobre Nascidos Vivos*, 'Live Births Information System') database contains data on live births – 50.3 million from 2003 to 2019.

I enter the infant mortality variable in two ways in the models below: first, the 'raw' infant mortality data: the number of infant deaths in a given year, divided by 1,000 times the number of live births in that year.[21]

This variable has a crucial problem, also identified e.g., by Swanson (2019) and Toson and Baker (2003): many small localities had zero infant deaths reported for at least one year. Given that, in small municipalities, these zeros do not reflect a 'zero infant mortality rate', but rather an artifact of the data-generating process, those authors discuss methods of data imputation in order to replace those zeros. I also created an 'imputed' infant mortality variable, by: creating a dataset of infant death counts at the municipality/year level, but with

[20] https://datasus.saude.gov.br/.

[21] SIM and SINASC data both report the municipality associated with each birth or death event in two ways: for births, the municipality in which the mother resides, and the municipality in which the birth occurred; for deaths, the municipality in which the person resided, and the municipality in which the death occurred. The place of birth/death measures would introduce measurement error due to e.g., municipalities without hospitals, whose residents travel for medical treatment. Therefore, I use the place of residence for both variables.

all zeros erased (i.e., set to missing); building a hierarchical time series with municipality and state as the levels of aggregation; fitting a time-series linear model with a trend term and the count of live births as an independent variable; imputing the pseudo-missing values; rounding them to the nearest integer, without 'forcing' non-zero values. This is a more conservative estimate than, for instance, the strategy of Silcocks et al. (2001)[22] to replace all zero values with a small positive value (in their case, 0.693, "the value for the Poisson rate for which the chance of observing 0 events is a half" (p. 40)). Because I am rounding the predicted values to the nearest integer but still allowing zeros, my model-based imputation still leaves about half of the zero observations intact (3,905 observations are imputed as zero, out of 7,889 original zeros – 81% of the remaining become 1, and 14% become 2).

For my second set of analyses, the dependent variable is the rate of BCG vaccination in infants, defined as the number of BCG doses given to infants under age 1 divided by the number of live births in a given municipality, in a given year. Data on the number of doses given is from the Brazilian Ministry of Health's National Immunization Program (*Programa Nacional de Imunizações*, 'PNI') database. That database differentiates between first doses of BCG and booster shots given in case of exposure to tuberculosis; those booster shots are not included in the calculation of the vaccination rate variable.

For many small municipalities, the rate of BCG vaccination exceeds 100%. This is a common problem in vaccination coverage data for smaller areas, as reported e.g., by the WHO[23] or by the Brazilian Ministry of Health.[24] Arroyo et al. (2020), Bueno and Matijasevich (2011), and Moraes (2007) speculate that these errors are due to incorrect reporting of the municipality of residence of a newborn's parent(s) – in other words, the number of live births in some municipalities, for some years, may be incorrect. This 'denominator problem', as Bueno and Matijasevich describe it, is "widely discussed" (p. 352) in the public health literature on vaccination rates, but in the reports that I have found – including the three articles and two data sources cited in this paragraph – it is never 'corrected' by truncating data to 100%. I follow that practice here. Instead of truncating the variable or otherwise modifying the reported vaccination rates, I estimate the model with

[22] Note: Silcocks et al. (2001) do not estimate infant mortality, but life expectancy at birth; still, their imputation strategy is also meant to solve the problem of zero deaths reported in some localities for some age bands.

[23] Report available at `https://www.who.int/data/gho/indicator-metadata-registry/imr-details/2442`, retrieved on June 7th, 2021.

[24] Page available at `http://tabnet.datasus.gov.br/cgi/idb1998/fqf11.htm`, retrieved on June 7th, 2021.

different exclusion rules (because observations with very high rates are almost inevitably very high-leverage points). Thus, the models for BCG rates below exclude municipalities with reported rates higher than 200%, 300%, and 500% (these rules exclude 210, 35, and 8 observations respectively).

Independent variables

The main independent variables in the analyses below are: the proportion of procurement processes that qualify for the invitation-to-bid modality and that were conducted that way in the previous year (i.e., a lagged version of the dependent variable from the previous section), and the proportion of procurement processes that qualify for the invitation-to-bid modality that were conducted without auctions (i.e., under the 'waiver' or the 'non-requirement' legal justifications) in the previous year. The latter variable is not only interesting from a substantive standpoint, but also useful for the interpretation of the estimates of the former because it creates a useful counterfactual: the proportion of acquisitions conducted by invitations to bid plus the proportion of acquisitions conducted without auction equals one minus the proportion of acquisitions conducted by a truly 'open' method (requests for proposals, competitive bidding, or reverse auctions, either in-person or electronically). Thus, for example, when discussing in subsection 3.4.2 the predicted effects of an increase in the proportion of invitations to bid, that increase corresponds to a decrease in the rate of 'open' procurement, as the rate of auction-less procurement is held constant.

The choice of cutoff points for procurement process values to only include those that would qualify for the invitation to bid stems not only from the scope conditions of this study, but also from a substantive consideration: most purchases by local governments are relatively small. Out of the 1.78 million acquisitions in my data over 8,000 BRL, 61% fall below the contract value limits for the invitation to bid for goods and services (of course, a higher share falls below the limits for the invitation to bid for engineering works). Around one-third of municipalities in my sample (1,409 out of 4,421) did not conduct any acquisitions above the invitation-to-bid limits on at least one year. Moreover, although larger contracts are more attractive to corrupt officials because they allow for higher bribes (as Lichand et al. (2018) correctly point out), those contracts may also be less attractive to officials under the risk of audits, particularly because, from its inception in 2003,

the federal audit lottery program only included small municipalities (initially, those with population under 100,000) where unusually large contracts would be subject to higher levels of scrutiny.

Everyday purchases give opportunity for everyday corruption, hidden unless and until examined by federal auditors. Consider the anecdotes that open section 3.2: nepotism does not require titanic infrastructure projects.

Regarding the universe of cases, note that in these analyses I take *all* procurement into account, and not only those purchases described by local authorities as related to public health. This is because the effects of mismanagement, inefficiencies, and corruption are not sector-specific: waste in acquisitions related to e.g., public transportation removes funds from the budget that could otherwise have been used for public health. Additionally, the allocation of expenditures to sectors is endogenous to the practice of corruption (Delavallade, 2006; Mauro, 1998), which implies that measuring the share of invitations to bid only among health-related acquisitions, for example, would not capture the effect of the modality on public health outcomes, because perhaps the procedure, being conducive to the extraction of rents, would be used more often to procure goods in sectors that are also more conducive to the extraction of rents.

Thus, the main independent variable in these analysis is the percentage of *all* purchases (regardless of the sector associated with the acquisition) that were conducted through invitation-to-bid auctions, among those that legally qualified for the invitation-to-bid procedure – that is, procurement processes with estimated value over 8,000 BRL (because contracts under that amount can be signed without an auction) and under 80,000 BRL if the acquisition was before 2018, or under 176,000 BRL if the acquisition was on or after 2018.

Controls

As a proxy for the size of the municipal government and the procurement market in a given municipality/year, I add a term for the number of procurement processes conducted in the previous year within the limits specified above. To adjust for the effect of randomly-assigned federal audits on the choices of procurement procedures and (potentially) on policy outcomes, I add a term for whether the municipality was audited at any point in the two previous years, and (partially following Lichand et al. (2018)) a term for whether any of the municipality's neighbors were audited at any point in the two previous years.

I include three terms in the model to adjust for municipal demographics. The first is the municipality's GDP per capita in a given year, which has obvious effects on infant mortality but also on procurement, because governments in wealthier localities are likely to have different policy priorities, more financial power through which to pursue those policy goals, and thus will have different procurement portfolios. GDP and population data are from the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, IBGE). For non-census years (the only census in the period of my data was conducted in 2010), population data is estimated by IBGE. The second variable is the municipality's population density, entered in the models as population per km^2, mainly as a proxy for urbanization rates, which are only reported by IBGE for years in which a census is conducted. Geographical areas are also from IBGE. The third is a dummy variable for whether the municipality is a state capital.

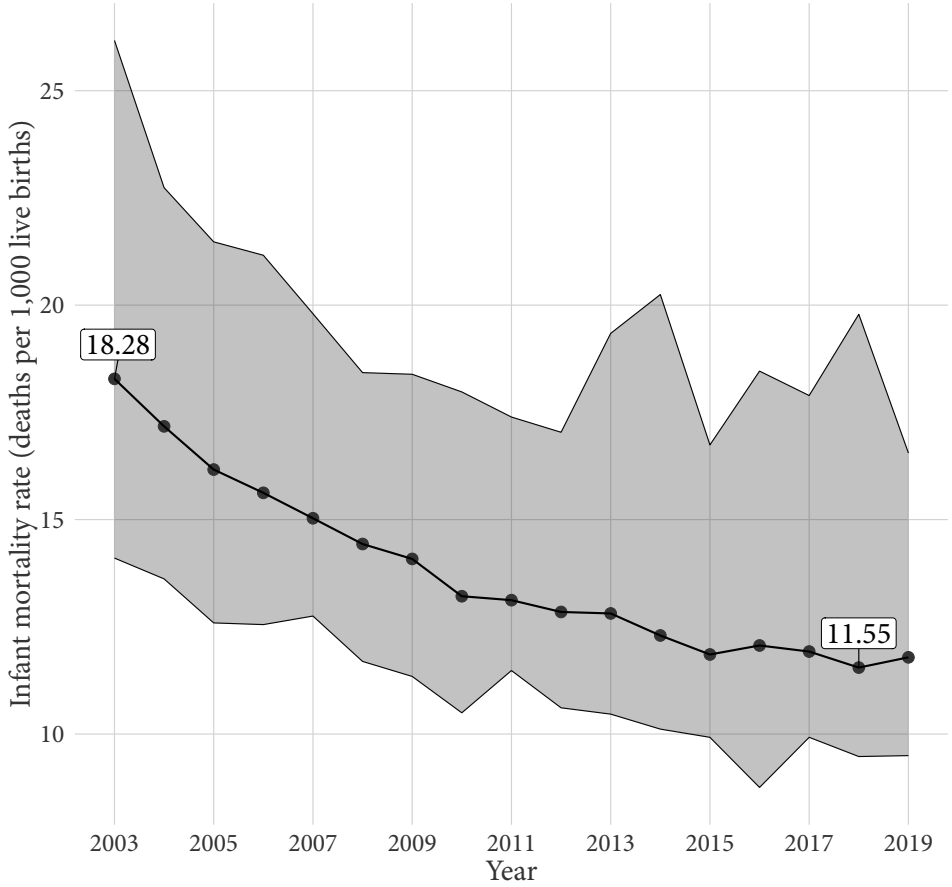
I also include terms for whether the mayor is in her second term, and for the mayor's margin of electoral victory in the previous election. Data on electoral results is from the Superior Electoral Tribunal (*Superior Tribunal Eleitoral*, TSE).

Finally, following works from the public health literature that model or otherwise discuss infant mortality as an effect of political causes (e.g., Factor and Kang (2015) and Hogue and Hargraves (1993)), I add two additional control variables. First, I add a term meant to adjust for maternal education. Given the asymmetries in data availability on actual maternal education present in the SINASC database, I choose a proxy, which appears in IBGE's decennial census data. It is the percentage of women over the age of 15 with at least eight years of education, which signifies having completed primary education. Second, the models require a control for a municipality's existing health infrastructure. That is because, again, the use of infant mortality in these models is partly meant to reflect its sensitivity to short-term governing decisions (Conley & Springer, 2001), which is what my independent variables are meant to capture. Additionally, my independent variables of interest being based on small government purchases gives rise to the possibility of confounding due to ignoring the effect of larger expenditures on the procurement portfolio of smaller, everyday acquisitions, and also on the 'downstream' public health outcomes. Thus, I include as a control the number of hospital beds per 1,000 inhabitants in a municipality.

For every outcome, I report three model specifications: one with 'pooled' estimates, one with year-fixed

effects, and one with year- and state-fixed effects. The need for including year-fixed effects is illustrated by Figure 3.3, in which I show infant mortality data for the fifteen states in the municipal procurement dataset (data is reported for all states in all years, regardless of whether the state reported municipal procurement data in that year). The across-state average, plotted as a thicker line with points for yearly averages, shows that between 2003 and 2018 there was a 37% reduction in infant mortality rates across the fifteen states. The shaded area shows, however, that this temporal trend does not fully represent the multilevel structure of the data: across-state variability is quite high in most years, owing in large part to inter-regional disparities in wealth and quality of life. For every year of data except 2011 and 2012, the across-state, within-year variability is higher than the across-year variability for the entire sample of states.

Figure 3.3: Infant mortality in Brazilian states, 2003-2019



Note: data only for the fifteen states included in the procurement dataset.
Shaded area represents the range of infant mortality rates across those states for each year.

3.4.2 Results and discussion

Table 3.6 shows the results of the models with the 'actual' infant mortality rates as the dependent variable, and Table 3.7 shows the results of the models with the imputed infant mortality rates. For the infant mortality models, zero values (or values that were imputed as zero) are removed. For all models in this chapter, observations (municipality/year units) that reported fewer than three acquisition processes within the boundaries delineated above are excluded. That represents an exclusion of 8% of the observations in the data.

Two different estimators are reported for each model, following the advice of Manning and Mullahy (2001) regarding models with "nonnegative measurements of the outcomes [and] a positively skewed empirical distribution of the nonzero [values]" (p. 462). First, the three specifications discussed above (a 'simple' model without fixed-effects, a model with year-fixed effects, and a model with year- and state- fixed effects) are estimated using OLS models with log-transformed dependent variables and heteroskedasticity-consistent standard errors; second, the third of those models is estimated using a gamma model with a log link. Similarly to the simulation results reported by Manning and Mullahy, the estimates of the gamma model for my independent variables of interest do not massively differ from those of the OLS model with log-transformed outcomes.

The models, regardless of imputation of the outcome variable and estimator, lend support to the hypothesis that rates of procurement by invitation to bid are negatively correlated with infant mortality. Note, first, that the estimated effect sizes are quite small: for a 'typical' municipality[25] that increases its share of acquisitions by the invitation to bid modality from 20% to 80%, the OLS model with year- and state-fixed effects on Table 3.7 predicts an increase in the infant mortality rate from 15.3 to 15.7 per thousand live births. Given that the mean number of live births in the data is roughly 466, that corresponds to an increase of 0.18 in the expected number of yearly infant deaths: from 7.13 to 7.31. The gamma model, again from Table 3.7, predicts an increase in the infant mortality rate from 15.9 to 16.5 – for the mean number of live births, that corresponds to an increase of 0.28 in the expected number of deaths, from 7.41 to 7.69. That there is an effect at all is still substantively significant, however, considering that these models only include small government

[25] That is, a simulated observation, with values for all covariates fixed at their median (or mode, for dummy variables) on which predicted values were calculated. Because the outcome variable is logged, every predicted value given in the text is transformed according to the smearing estimate of Duan (1983).

Table 3.6: Models of infant mortality and procurement methods

	Dependent variable: Infant mortality rate			
	OLS (logged dependent variable)			Gamma
(Intercept)	3.39^{***}	3.44^{***}	3.34^{***}	3.54^{***}
	(0.02)	(0.05)	(0.05)	(0.05)
% invitations to bid in previous year	0.07^{***}	0.05^{**}	0.04^{*}	0.06^{**}
	(0.02)	(0.02)	(0.02)	(0.02)
% procurement w/o auction in prev. year	-0.06^{**}	-0.05^{*}	-0.10^{***}	-0.12^{***}
	(0.02)	(0.02)	(0.02)	(0.03)
No. of acquisitions in prev. year	-0.00^{***}	-0.00^{***}	-0.00^{***}	-0.00^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Munic. was audited in last 2 years	-0.05^{*}	-0.05^{*}	-0.05	-0.05^{*}
	(0.02)	(0.02)	(0.02)	(0.03)
A neighbor was audited in last 2 years	-0.19	-0.19	-0.08	-0.07
	(0.14)	(0.14)	(0.14)	(0.11)
Population density (log)	-0.10^{***}	-0.10^{***}	-0.11^{***}	-0.12^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
GDP per capita (log)	-0.01	-0.01	-0.06^{***}	-0.06^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
State capital	0.49^{***}	0.48^{***}	0.51^{***}	0.54^{***}
	(0.03)	(0.03)	(0.03)	(0.04)
Hospital beds per 1,000 inhabitants	-0.03^{***}	-0.03^{***}	-0.03^{***}	-0.03^{***}
	(0.00)	(0.00)	(0.00)	(0.01)
% of women w. 8+ years of education	-0.68^{***}	-0.62^{***}	-0.50^{***}	-0.62^{***}
	(0.06)	(0.07)	(0.07)	(0.09)
Mayor is in second term	0.04^{***}	0.04^{***}	0.04^{***}	0.04^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Mayor's margin of electoral victory	-0.05^{*}	-0.05^{*}	-0.03	-0.05
	(0.02)	(0.02)	(0.02)	(0.02)
Year f.e.		Yes	Yes	Yes
State f.e.			Yes	Yes
R^2	0.12	0.12	0.14	
Adj. R^2	0.12	0.12	0.13	
Num. obs.	17561	17561	17561	17561
AIC				123909.98
BIC				124236.46
Log Likelihood				-61912.99
Deviance				5304.62

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 3.7: Models of (imputed) infant mortality and procurement methods

	Dependent variable: Imputed infant mortality rate			
	OLS (logged dependent variable)			Gamma
(Intercept)	3.37^{***}	3.43^{***}	3.33^{***}	3.52^{***}
	(0.02)	(0.04)	(0.05)	(0.05)
% invitations to bid in previous year	0.07^{***}	0.05^{**}	0.04^{*}	0.05^{**}
	(0.01)	(0.02)	(0.02)	(0.02)
% procurement w/o auction in prev. year	-0.05^{**}	-0.04^{*}	-0.10^{***}	-0.11^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
No. of acquisitions in prev. year	-0.00^{***}	-0.00^{***}	-0.00^{***}	-0.00^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Munic. was audited in last 2 years	-0.05^{*}	-0.05^{*}	-0.04	-0.05^{*}
	(0.02)	(0.02)	(0.02)	(0.02)
A neighbor was audited in last 2 years	-0.18	-0.18	-0.08	-0.06
	(0.14)	(0.14)	(0.14)	(0.11)
Population density (log)	-0.10^{***}	-0.10^{***}	-0.11^{***}	-0.12^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
GDP per capita (log)	-0.02^{**}	-0.02^{*}	-0.06^{***}	-0.07^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
State capital	0.49^{***}	0.47^{***}	0.50^{***}	0.52^{***}
	(0.03)	(0.03)	(0.03)	(0.03)
Hospital beds per 1,000 inhabitants	-0.03^{***}	-0.03^{***}	-0.03^{***}	-0.03^{***}
	(0.00)	(0.00)	(0.00)	(0.01)
% of women w. 8+ years of education	-0.59^{***}	-0.54^{***}	-0.40^{***}	-0.49^{***}
	(0.05)	(0.06)	(0.07)	(0.08)
Mayor is in second term	0.03^{***}	0.03^{***}	0.03^{***}	0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Mayor's margin of electoral victory	-0.06^{**}	-0.05^{*}	-0.03	-0.05^{*}
	(0.02)	(0.02)	(0.02)	(0.02)
Year f.e.		Yes	Yes	Yes
State f.e.			Yes	Yes
R^2	0.12	0.12	0.13	
Adj. R^2	0.12	0.12	0.13	
Num. obs.	20637	20637	20637	20637
AIC				144713.14
BIC				145046.40
Log Likelihood				-72314.57
Deviance				5925.61

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

purchases. That an increase in the use of the 'rent-creating' procedure may add up to an additional death every four years, for a mean number of live births, is also notable.

Of course, the effect may be spurious, for reasons difficult to measure: perhaps municipalities that conduct invitations to bid at high rates are plagued by mismanagement and rent-seeking of other kinds. The differences in expected infant mortality rates, under that hypothesis, may have no relation to the actual procurement modality directly. If this is the case, though, the effect of that incompetence or rent-seeking being captured by invitations to bid would make the procedure a canary in the coal mine; such a publicly available 'warning' would be particularly useful to the CGU, who changed their auditing program so that the random selection is complemented by an 'algorithm' that chooses high-risk municipalities to audit.

The effect of procuring without auctions is stronger, and in the opposite direction. The obvious explanation is that auction-less acquisitions are more efficient; of course, they are also potentially much more prone to corruption — and they allow local governments to violate all principles of openness and fairness that are central to public procurement. That risk may be counterbalanced, I speculate, by the quite strict restraints placed by federal procurement law on the use of the waiver and non-requirement justifications for procurement without auctions. If that is the case (a massive 'if', I admit), then the focus of these counter-intuitive results should not be on the fact that this is procurement without auction; rather, it should be on the fact that acquisitions without auctions can only be conducted under very specific circumstances while the use of invitations to bid is only constrained by the estimated cost of the contract, which itself is a factor under the control of municipal governments. Alternatively, it may be the case that the kinds of truly open procurement auctions that are allowed by federal law are particularly slow, expensive, and prone to inefficiencies.

Table 3.8 shows results for models of BCG vaccination rates in infants, with the approximately 260 observations with reported rates over 200% excluded. These results suggest the same about the invitation-to-bid procedure as do the models of infant mortality. On Appendix D, I report the results of models fit with larger sets of data, only removing municipalities with rates over 300% and 500%. Those cutoff points add, respectively, 208 and 236 observations to the models on Table 3.8; the results are virtually unchanged.

This set of models, which indicate that the rates of procurement without auction are associated with lower immunization rates, provides contradictory evidence to the infant mortality models. Does procure-

Table 3.8: Models of BCG vaccination rates and procurement methods

	Dependent variable: BCG vaccination rates in infants		
(Intercept)	0.29^{***}	0.59^{***}	0.94^{***}
	(0.02)	(0.03)	(0.04)
% invitations to bid in previous year	-0.07^{***}	-0.11^{***}	-0.04^{***}
	(0.01)	(0.01)	(0.01)
% procurement w/o auction in prev. year	-0.15^{***}	-0.14^{***}	-0.06^{***}
	(0.01)	(0.01)	(0.01)
No. of acquisitions in prev. year	0.00^{***}	0.00^{***}	0.00^{*}
	(0.00)	(0.00)	(0.00)
Munic. was audited in last 2 years	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)
A neighbor was audited in last 2 years	-0.05	-0.05	-0.06
	(0.14)	(0.14)	(0.13)
Population (log)	0.03^{***}	0.03^{***}	0.03^{***}
	(0.00)	(0.00)	(0.00)
Population density (log)	-0.02^{***}	-0.03^{***}	-0.01^{***}
	(0.00)	(0.00)	(0.00)
GDP per capita (log)	0.07^{***}	0.07^{***}	0.02^{***}
	(0.00)	(0.00)	(0.01)
State capital	0.12^{***}	0.08^{*}	0.21^{***}
	(0.04)	(0.03)	(0.04)
Any TB deaths in last year	0.01	0.00	0.01^{**}
	(0.01)	(0.01)	(0.01)
Hospital beds per 1,000 inhabitants	0.03^{***}	0.02^{***}	0.02^{***}
	(0.00)	(0.00)	(0.00)
% of women w. 8+ years of education	0.22^{***}	0.56^{***}	0.17^{***}
	(0.03)	(0.04)	(0.04)
Mayor is in second term	-0.01^{*}	-0.01	-0.00
	(0.01)	(0.01)	(0.00)
Mayor's margin of electoral victory	0.02	0.02	-0.00
	(0.01)	(0.01)	(0.01)
Year f.e.		Yes	Yes
State f.e.			Yes
R^2	0.12	0.14	0.23
Adj. R^2	0.12	0.14	0.23
Num. obs.	21906	21906	21906

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

ment without auction, then, both grease and simultaneously sand the wheels of government? One possible explanation for this, in light of the evidence provided so far in this chapter, could be that this finding supports the speculation I provide above regarding the strict legal controls on waivers and non-requirements. In the list of 35 possible justifications for an auction waiver, the most closely related to the present analysis are that a waiver is justifiable in a state of 'emergency or public calamity' (reason 4), or for acquisitions that result in the technology transfer of a strategically important product to the national health service (reason 32). Additionally, a non-requirement can be justified for goods that can only be acquired by one supplier. None of those reasons apply to tuberculosis in infants, or to the century-old BCG vaccine.

However, given that infant mortality is affected by a wide variety of policies, it is plausible that more types of waivers would apply to the kinds of spending that affect infant mortality; for example, since 2010 contracts have been eligible for a waiver (reason 30) if they support the National Policy of Technical Assistance and Rural Extension (*Política Nacional de Assistência Técnica e Extensão Rural*, 'PNATER'), a federal government program that provides assistance to subsistence farmers, in order to both introduce sustainable practices and improve their quality of life (Diesel & Miná Dias, 2016); since 2013, contracts have been eligible for a waiver (reason 32) when nonprofits are contracted for the construction of cisterns "or other technologies [to facilitate] access to water for human consumption... to benefit low-income families affected by droughts or lack of regular access to water" (L. 8666/93, Art. 24[26]).

Thus, the outcomes in these two sets of models differ substantially with regards to the mechanisms that link them to procurement, even though the literature links both of them to some of the same demographic causes (e.g., maternal education, proper prenatal care). Changes in infant mortality are a result of a nearly uncountable number of policies and, consequently, of any gains or losses in efficiency in the process of implementing those policies. On the other hand, the administration of BCG vaccines is the result of a much more specific set of policy priorities, public acquisitions, and structural conditions. Still, both are negatively associated with the rates of partially-closed, rent-creating procurement procedures. This relationship may be consequential to our understanding of political corruption in procurement, specifically, and the consequences of political corruption in general — particularly, who bears the burden of corruption. These find-

[26] Full text available at `http://www.planalto.gov.br/ccivil_03/leis/l8666cons.htm`.

ings carry significant implications to the findings in the section below, and the importance of functioning accountability mechanisms in local governments.

3.5 A short note on the effects of invitations to bid on local economies

This section, somewhat less important than the previous, is motivated by two main concerns. First, I realize that the evidence from the previous section that the use of invitations to bid is associated with worse public health outcomes may be sector-specific, perhaps because public health is procurement-intensive due to its dependence on consumable goods. Second, although I have discussed procurement in terms of accountability, transparency, and fairness to entrants, public procurement laws are not designed exclusively to that end. They are also tailored to advance broader state interests: public procurement policy can "stimulate industrial development, entrepreneurship, and R&D" (Sorte Junior, 2016, p. 30), in part because "the government can... bear higher entry costs, create critical mass [and] signal the market" (Kattel & Lember, 2010, p. 377) and promote innovation, whilst taking on the risk associated with the early adoption of such innovations. Public procurement is also used to promote "fair labor conditions and fair wages" (McCrudden, 2004, p. 258), as well as fight unemployment — both through the kind of extravagant public works that characterized the developmental state in the mid-20th century, but also through everyday purchases, if conducted with attention to that end. Additionally, because of the usual requirements for individuals and firms that wish to sell to the government, public procurement can also shift the economy away from informality. These are "secondary goals" (Telgen et al., 2012, p. 20) of public procurement, and their importance appears throughout the literature on public procurement.

The effect of public procurement on local firms goes beyond the promotion of innovation: by working "alongside the market by utilizing the government's purchasing power" (Preuss, 2011, p. 805), procurement policies provide governments with ways to stimulate small and medium firms because the public sector provides "predictable demand and near guaranteed payment" (Woldesenbet & Worthington, 2019, p. 1664), as well as reputational gains of being a supplier to the public sector (Loader, 2013). But this is only true, according to Woldesenbet and Worthington (2019), if the barriers to entry to the public procurement market

are not prohibitive to small firms.

Thus, it is possible that the invitation to bid actually foments economic activity among local small businesses. Because these firms are not able to bear the costs of the bureaucratic requirements of public procurement, they tend to benefit from the kinds of features of the invitation to bid modality that I have, thus far, treated as harmful distortions: "unbundling large contracts" (Loader, 2013, p. 50), for instance, is not necessarily a sign of malicious 'auction structuring', but may be an attempt to create smaller contracts that would increase the competitiveness of small firms; the invitation process itself, and the fact that an invited bidder does not need to prove its eligibility (which is ostensibly verified by the government's auctioneer prior to the submission of invitation letters), may alleviate the "fear and ambivalence generated by.... the procurement process and the obligatory legal [requirements]" (Woldesenbet & Worthington, 2019, p. 1672).

Thus, in this section I test whether the use of auctions by invitation to bid by local governments is associated with better outcomes for the local economy. Below, I explain my operationalizations and describe the sources of the new data for this analysis; then, I discuss the results of the models.

3.5.1 Data and methods

I follow Bologna and Ross (2015) in using only small businesses in my models, although I depart from them in the choice of dependent variable. Their main outcome of interest is the yearly change in the number of establishments with less than 10 employees (or, in a different specification, less than 20 employees — a measure that I adopt below) in a municipality. However, as Bologna and Ross acknowledge, they cannot correct for the possibility that an establishment may have shrunk. This omission is due to their data on establishments being anonymized. Without a way to track individual firms over time, there is no way to track which 'new' firms with less than 10 employees represent a new firm and which represent a larger firm that has reduced its size.

For that reason, my measure of local economic activity is the total number of jobs created by small firms in a year. That data, like the data on establishments used by Bologna and Ross (2015) is from the Annual Social Information Report (*Relação Anual de Informações Sociais*, 'RAIS') "an annual record of all formal

establishments across Brazilian municipalities" (Bologna & Ross, 2015, p. 66) collected yearly by the Ministry of Labor.

This is possible because, apart from containing data on all establishments in Brazil, RAIS also contains data on all formal employment, as reported by firms to the Ministry of Labor. The data, although also anonymized at the levels of the firm and the worker, is rich in detail: it includes, for instance, each worker's average monthly wage for the year under consideration, expressed both in nominal terms and in terms of the minimum wage.[27] It also includes length of employment, which allows me to count new jobs only. While firms do hire new workers during periods of economic difficulty, an aggregate increase in job creation cannot (under almost all normal circumstances) signify economic depression — unlike an increase in the number of firms that employ 19 or fewer people. Moreover, counting the number of new jobs allows for a crude measure of a shift from informal to formal employment. While informal employment is, of course, not measured comprehensively by governments, a shift toward formal employment should be reflected in job creation figures; it would not necessarily be reflected in the number of *firms* that employ a certain number of people.

In operationalizing job creation by small firms, I follow Almeida et al. (2017), who also use RAIS data, and exclude public sector jobs.

I run the models with three cutoff points for average monthly wages. First, I count any new jobs that pay at least the minimum wage — almost all jobs in the data. Second, I count all jobs that pay at least two times the minimum wage. That corresponds to around 17-20% of the total, depending on the year. Finally, I only count jobs that pay at least three times the minimum wage — 5-6% of the total. This choice is due to the fact that the minimum wage in Brazil is, by international standards, quite low: in 2019, the final year of my data, it was the fifth-lowest in the Americas in purchasing power parity (International Labour Organization, 2020); in a table of minimum wages in PPP that includes 31 OECD members plus Brazil,[28] the country consistently ranks second- or third-to-last, ahead only of Mexico and, until 2017, Russia. Additionally, according the Inter-Union Department of Statistics and Socioeconomic Studies (*Departamento Intersindical de Estatística*

[27] The minimum wage in Brazil is adjusted yearly by the federal government, (ideally) correcting for inflation and, depending on the inclinations of the federal administration, providing low-wage workers with a small increase in their purchasing power. For that reason, expressing wages in terms of the minimum wage provides a better snapshot of the labor market in a given year than, for example, correcting nominal wages for inflation in order to express them in constant terms.

[28] Available at `https://stats.oecd.org/index.aspx?DataSetCode=RMW`.

e Estudos Sócio-Econômicos, 'DIEESE'), a labor organization that compiles a cost of living index in order to measure the purchasing power of the minimum wage,[29] the minimum wage places workers in a state of underemployment, unable to afford a basic basket of goods that would be sufficient to feed a family for a month. Therefore, when investigating how procurement policies affect the job market, it may not be appropriate to include underemployed workers.

A final note: IBGE only publishes 'actual' data on population broken down by age group for census years. DATASUS publishes IBGE's estimates of population by age group, but only until 2012. As DATASUS's documentation explains,[30] the estimation method changed in 2007, therefore the forecasts for 2007-2009 are not comparable with those for 2001-2006. For 2013 on, DATASUS's estimates are from a different source, the Ministry of Health's Health Surveillance Department (*Secretaria de Vigilância em Saúde*, 'SVS'), which uses a different method. Thus, given that there are four different estimators of population by age group in a period of 17 years (actual counts for the 2010 census, plus three different, non-reconcilable forecasting methods), I choose not to enter the dependent variable as the number of new jobs divided by the working-age population in a given year. Instead, I use the municipality's total population as the denominator for the dependent variable in the analyses below.

Otherwise, the models are generally similar to those above, other than the choices of controls: in the models below, I exclude those directly related to public health (count of hospital beds per 1,000 inhabitants, and the dummy variable for whether any of the municipality's residents died of tuberculosis) and include a term for the municipality's GDP growth in the previous year, a term for the number of firms that employ less than 20 workers, a term for the share of the population over age 15 with at least 8 years of education, which corresponds to having completed primary education, and a term for the municipality's unemployment rate. Education and unemployment rate are only reported by IBGE for every municipality in census years, so for years before 2010 the value of these variables is the value in the 2000 census; for the 2010-2019 period, the value of these variables is the value in the 2010 census.

[29] Data and methodological notes available at `https://www.dieese.org.br/analisecestabasica/salarioMinimo.html`.
[30] Available at `http://tabnet.datasus.gov.br/cgi/ibge/popdescr.htm`.

3.5.2 Results and discussion

Table 3.9 shows results for the gamma models of new jobs per capita, per municipality and year. The first model includes all jobs that pay at least the monthly minimum wage. The second and third models only include jobs that pay two or three times the minimum wage, respectively. Every model excludes municipality-years in which no new jobs exist in the data.

Table 3.9: Models of small-business employment and procurement methods

	Dependent variable: No. of new jobs per capita in last 12 months in firms with < 20 employees		
	1× min. wage	2× min. wage	3× min. wage
(Intercept)	-7.19^{***}	-9.04^{***}	-9.15^{***}
	(0.06)	(0.10)	(0.12)
% invitations to bid in previous year	-0.05^{*}	-0.06^{*}	-0.09^{**}
	(0.02)	(0.03)	(0.04)
% procurement w/o auction in prev. year	-0.04	-0.08^{*}	-0.07
	(0.03)	(0.04)	(0.05)
No. of acquisitions in prev. year	0.00^{***}	0.00	0.00
	(0.00)	(0.00)	(0.00)
Munic. was audited in last 2 years	-0.02	-0.03	-0.05
	(0.03)	(0.04)	(0.05)
A neighbor was audited in last 2 years	-0.37^{*}	-0.44	-0.20
	(0.18)	(0.27)	(0.32)
GDP growth in previous year	0.07^{*}	0.18^{***}	0.14^{**}
	(0.03)	(0.04)	(0.05)
GDP per capita (log)	0.77^{***}	1.00^{***}	0.84^{***}
	(0.01)	(0.02)	(0.02)
% of pop. over 15 w. 8+ years of education	2.99^{***}	2.44^{***}	1.86^{***}
	(0.07)	(0.11)	(0.13)
No. of establishments w. $<$ 20 employees	-0.00^{*}	0.00	0.00^{*}
	(0.00)	(0.00)	(0.00)
Unemployment rate in prev. census	1.15^{***}	0.99^{***}	0.93^{**}
	(0.15)	(0.24)	(0.30)
Mayor is in second term	-0.02	0.03	0.02
	(0.01)	(0.02)	(0.02)
Mayor's margin of electoral victory	-0.02	0.00	0.05
	(0.03)	(0.04)	(0.05)
Year f.e.	Yes	Yes	Yes
State f.e.	Yes	Yes	Yes
AIC	-152596.16	-217606.67	-228834.47
BIC	-152249.69	-217263.34	-228496.31
Log Likelihood	76341.08	108846.33	114460.23
Deviance	10013.08	11550.53	10285.43
Num. obs.	23326	21682	19228

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

As I explain on subsection 3.5.1, this test is primarily motivated by a possible objection one may raise to my depicting the invitation to bid procedure as harmful. Invitations to bid incentivize municipalities to

break down acquisitions into smaller contracts; they shift the burden of bidder eligibility verification from the firms to the government; they allow the public sector to encourage small local firms to participate by sending invitation letters.

The results above do not support that idea. Instead, at every wage cutoff level, the use of the invitation to bid is associated with a decreased number of new hires by small firms. The estimates are, as in previous models, quite small: an increase in the rate of invitations to bid from 20% of a municipality's small purchases to 80% is associated with the loss of 1.27 new jobs in the first model, 1.35 in the second, and 1.47 in the third. Considering that, for a synthetic 'new' observation created by holding every predictor at its mean for numeric variables or modal category for binary variables, the year as 2011 and the state as Ceará, the second model predicts 29.5 new jobs, the difference of 1.35 in the predictions from that model represents 4.5% of the 'expected' new jobs.

3.6 Concluding remarks

In this chapter, I have investigated two classes of questions relevant to the literature on political corruption. I found supporting evidence to the idea that local governments can change their practices, and increase the rate at which they engage in rent-seeking behavior, if mechanisms exist through which to hold them accountable. Perhaps more interestingly, I found that the effect of having a neighbor being audited is only moderately weaker than the effect of a municipality itself being audited — which may suggest, albeit quite preliminarily, that the visibility of accountability mechanisms may be nearly as important as the monitoring/auditing program itself.

Additionally, the second set of analyses in this chapter found that the incidence of partially closed, corruption-risk-laden procurement is associated with worse outcomes in public health, as well as not fulfilling one of the government-as-consumer goals of public procurement. These findings seem to support the idea that corruption 'sands the wheels' of development — or at least that some kinds of corruption do. This is an important finding in light of the literature on corruption risk in procurement, which has variously found that those forms of corruption have positive or negative downstream policy effects.

Other than these findings, of course, this chapter raises many questions that may inform future research: what are the mechanisms through which auditing decreases the rates of corrupt behavior in local governments? How and under what conditions does the audited-neighbor effect (which this chapter suggests, but does not measure — the models simply show a correlation) hold?

Most importantly, this chapter directs attention to public procurement as a locus of political corruption and as a potential source of robust measurements of corruption risk. The public acquisition process is fundamentally important to policymaking, and distortions related to corruption are extremely consequential. My use of Brazil as a case is largely a result of the country's legal arrangements allowing public officials to engage in 'cargo-cult' auctions, but the country also provides a timely and unfortunate example of the importance of studying corruption in procurement: in late June 2021, the day before the country's death toll from COVID-19 reached half a million (Londoño & Milhorance, 2021), it was reported that a Ministry of Health official had allegedly been pressured to sign a contract for the purchase of 20 million doses of the Covaxin vaccine at the price of 15 USD per dose — despite the vaccine costing 1.34 USD per dose in India, where it was developed, and despite the Ministry of Health having turned down an offer from Pfizer because the asked price of 10 USD per dose was too high. This Ministry official and his brother, a congressman, spoke directly to Brazil's president about their suspicions regarding the contract. The president took no action, and a month later the congressman was reportedly offered a bribe of 1.2 million USD in exchange for his silence regarding the contract. Later, it emerged that the firm acting as an intermediary in the purchase was involved in a case of corruption regarding a contract signed with the Ministry of Health in 2017(BBC News, 2021; Chaib et al., 2021).

After a Senate ad-hoc investigative committee heard the allegations, the federal government ended negotiations with the firm. In the month of June, an average of 1,700 to 1,800 people died of COVID-19 per day in Brazil.

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Appendix A: Re-specifications of models in subsection 2.5.1

Table A.1: Logit models of procurement-related violations without logged terms

	Dependent variable:					
	Procurement-related violation			Invitation-related violation		
(Intercept)	1.86^{***}	2.68^{***}	2.75^{***}	-0.55^{***}	-0.08	0.10
	(0.10)	(0.53)	(0.68)	(0.07)	(0.40)	(0.50)
'Open-data' state	-0.11	-0.20	-0.31	-0.09	-0.18	-0.20
	(0.17)	(0.18)	(0.19)	(0.12)	(0.13)	(0.14)
Total amount audited (centered & scaled)		0.41^{*}	0.39		0.15	0.17
		(0.20)	(0.20)		(0.12)	(0.12)
Munic. has been audited before		0.10	-0.11		-0.18	-0.26
		(0.30)	(0.31)		(0.24)	(0.25)
Population (centered & scaled)		-0.70	-0.81		-0.65	-0.87
		(0.42)	(0.45)		(0.52)	(0.55)
GDP per capita (centered & scaled)		-0.27^{*}	-0.26^{*}		-0.38^{*}	-0.38^{*}
		(0.12)	(0.12)		(0.15)	(0.16)
% urban population		-0.41	-0.36		0.17	0.22
		(0.40)	(0.41)		(0.30)	(0.30)
Effective n. parties in legislature		-0.10	-0.12		-0.13^{*}	-0.14^{*}
		(0.07)	(0.07)		(0.06)	(0.06)
Mayor's party share in legislature		0.22	0.11		0.45	0.25
		(0.74)	(0.78)		(0.52)	(0.54)
Mayor is on second term		0.13	0.15		0.18	0.21
		(0.19)	(0.19)		(0.13)	(0.13)
Mayor's margin of electoral victory		-0.05	-0.15		0.06	0.01
		(0.50)	(0.52)		(0.37)	(0.38)
Mayor and governor in same party		-0.34	-0.30		-0.03	-0.12
		(0.20)	(0.24)		(0.16)	(0.18)
Mayor's party in president's coalition		-0.13			-0.32^{*}	
		(0.18)			(0.14)	
Mayor's party f.e.			Yes			Yes
AIC	1045.87	1002.28	1018.80	1686.77	1595.36	1598.50
BIC	1056.20	1068.90	1208.41	1697.10	1661.98	1788.10
Log Likelihood	-520.94	-488.14	-472.40	-841.39	-784.68	-762.25
Deviance	1041.87	976.28	944.80	1682.77	1569.36	1524.50
Num. obs.	1289	1242	1242	1289	1242	1242

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Variables were centered and scaled by subtracting the mean and dividing by one standard deviation.

Table A.2: Fractional logit models of shares of audited resources in audit violations without logged terms

	Dependent variable:					
	Share of audited funds involved in procurement-related violations			Share of audited funds involved in invitation-related violations		
(Intercept)	-1.64^{***}	-1.02^{***}	-1.01^{**}	-3.23^{***}	-2.91^{***}	-3.19^{***}
	(0.05)	(0.29)	(0.36)	(0.09)	(0.60)	(0.66)
'Open-data' state	0.02	-0.03	-0.09	0.34^{*}	0.18	0.14
	(0.09)	(0.10)	(0.10)	(0.16)	(0.17)	(0.19)
Munic. has been audited before		0.16	0.13		-0.21	-0.21
		(0.16)	(0.16)		(0.33)	(0.32)
Population (centered & scaled)		0.53	0.58		-0.55	-0.59
		(0.32)	(0.34)		(0.44)	(0.46)
GDP per capita (centered & scaled)		-0.04	-0.04		-0.12	-0.08
		(0.07)	(0.06)		(0.21)	(0.14)
% urban population		-0.54^{**}	-0.58^{**}		-0.11	-0.11
		(0.20)	(0.20)		(0.34)	(0.33)
Effective n. parties in legislature		-0.06	-0.06		-0.08	-0.09
		(0.04)	(0.04)		(0.08)	(0.08)
Mayor's party share in legislature		-0.08	-0.17		0.77	0.58
		(0.39)	(0.40)		(0.69)	(0.68)
Mayor is on second term		0.09	0.07		0.13	0.14
		(0.10)	(0.10)		(0.17)	(0.16)
Mayor's margin of electoral victory		0.16	0.11		-0.70	-0.69
		(0.26)	(0.26)		(0.48)	(0.48)
Mayor's party in president's coalition		0.00			-0.02	
		(0.09)			(0.18)	
Mayor's party f.e.			Yes			Yes
AIC	891.20	862.92	896.36	275.80	271.25	312.91
BIC	901.52	924.42	1080.84	286.12	332.73	497.36
Log Likelihood	-443.60	-419.46	-412.18	-135.90	-123.62	-120.46
Deviance	388.92	366.34	358.11	206.46	189.81	183.44
Num. obs.	1289	1242	1242	1288	1241	1241

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Variables were centered and scaled by subtracting the mean and dividing by one standard deviation.

Table A.3: Logit models of violations in audits among open-data states within two years of data being publicly available

	Dependent variable:			
	Procurement-related violation		Invitation-related violation	
(Intercept)	1.49^{***}	-6.37^{*}	-0.64^{***}	-5.84^{*}
	(0.23)	(2.91)	(0.18)	(2.52)
Procurement data is available	0.17	0.02	-0.28	-0.51
	(0.34)	(0.42)	(0.28)	(0.33)
Total amount audited (log)		0.92^{***}		0.59^{*}
		(0.25)		(0.25)
Munic. has been audited before		0.15		-0.17
		(0.67)		(0.53)
Population (log)		-0.46		-0.26
		(0.29)		(0.28)
GDP per capita (log)		-0.48		-0.62^{*}
		(0.28)		(0.27)
% urban population		-1.21		0.53
		(1.13)		(0.92)
Effective n. parties in legislature		-0.02		-0.12
		(0.17)		(0.13)
Mayor's party's share in legislature		0.86		-0.15
		(1.63)		(1.25)
Mayor is on second term		0.06		0.09
		(0.45)		(0.33)
Mayor's margin of electoral victory		-0.61		0.26
		(0.99)		(0.85)
Mayor and governor in same party		-0.79		-0.27
		(0.49)		(0.46)
Mayor's party in president's coalition		0.11		-0.06
		(0.43)		(0.34)
AIC	227.11	194.12	305.72	282.06
BIC	234.09	238.92	312.70	326.86
Log Likelihood	-111.56	-84.06	-150.86	-128.03
Deviance	223.11	168.12	301.72	256.06
Num. obs.	242	232	242	232

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

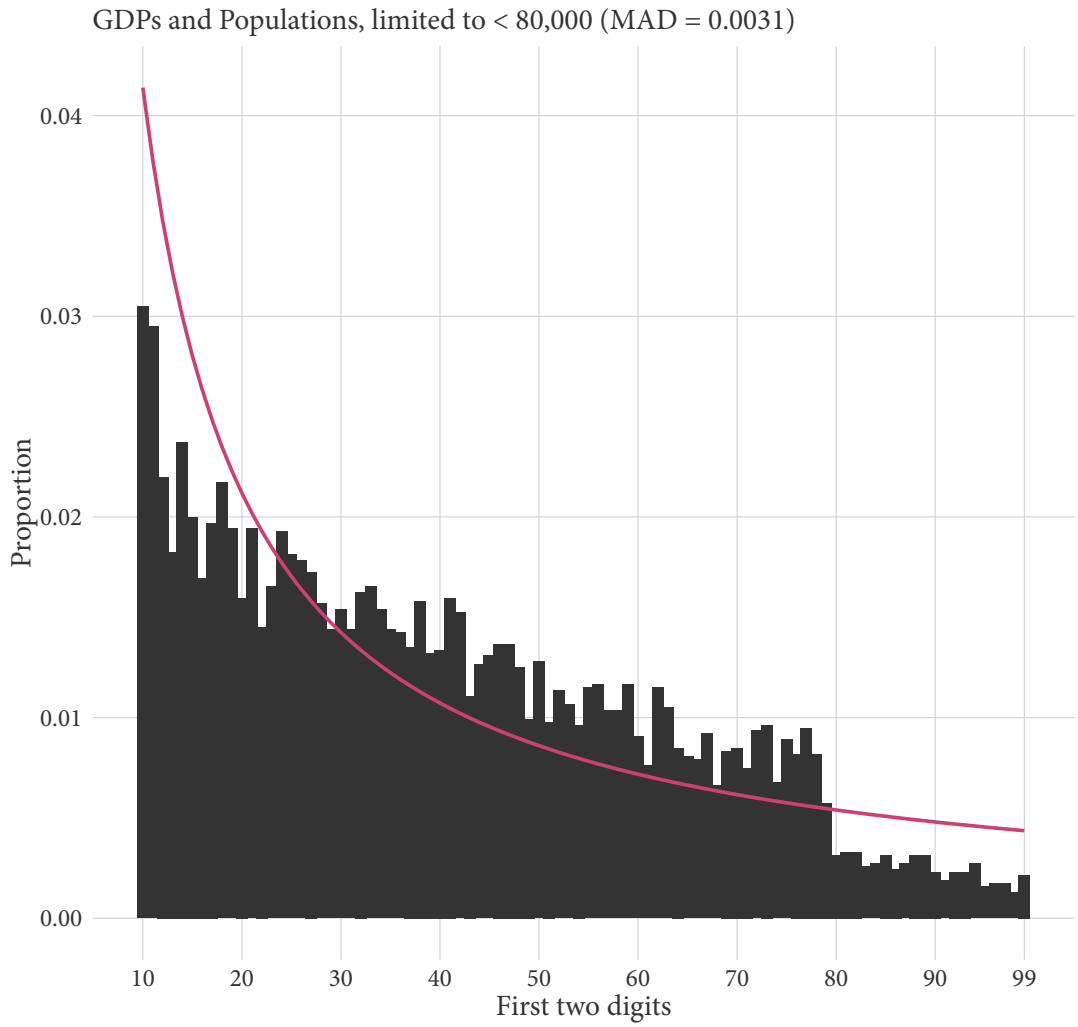
Table A.4: Fractional logit models of shares of audited resources in audit violations among open-data states within two years of data being publicly available

	Dependent variable:			
	Share of audited funds involved in procurement-related violations		Share of audited funds involved in invitation-related violations	
(Intercept)	-1.50^{***}	-3.64^{**}	-2.66^{***}	-4.25^{**}
	(0.13)	(1.17)	(0.23)	(1.62)
Procurement data is available	0.14	0.14	-0.28	-0.21
	(0.20)	(0.22)	(0.38)	(0.42)
Munic. has been audited before		0.27		-0.72
		(0.30)		(0.57)
Population (log)		0.40^{***}		0.33
		(0.11)		(0.17)
GDP per capita (log)		-0.19		-0.53
		(0.21)		(0.41)
% urban population		-0.62		1.34
		(0.62)		(1.23)
Effective n. parties in legislature		-0.24^{**}		-0.29
		(0.08)		(0.16)
Mayor's party's share in legislature		-0.44		-0.28
		(0.83)		(1.48)
Mayor is on second term		0.22		0.09
		(0.25)		(0.39)
Mayor's margin of electoral victory		0.22		-0.44
		(0.65)		(1.05)
Mayor and governor in same party		0.16		0.13
		(0.27)		(0.55)
Mayor's party in president's coalition		0.18		0.21
		(0.23)		(0.41)
AIC	209.56	211.42	88.57	104.29
BIC	216.53	252.78	95.55	145.65
Log Likelihood	-102.78	-93.71	-42.28	-40.14
Deviance	95.96	83.33	61.43	54.51
Num. obs.	242	232	242	232

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Appendix B: Histogram of municipal GDPs and populations, truncated to values below 80,000

Figure B.1: Distribution of the two leading digits of estimated contract values in Brazilian municipal procurement processes conducted by invitations to bid



Notes: Line shows Benford's distribution for two leading digits.
Data is from IBGE estimates for 2014. N = 6,954.

Appendix C: Models from subsection 3.3.3, excluding the year before and the year after an audit

Table C.1: Models of invitation to bid incidence only 2 years before or after an audit

Dependent variable: *Rate of invitation-to-bid use among qualifying procurement processes*			
(Intercept)	0.66^{***}	3.20^{***}	4.50^{***}
	(0.11)	(0.72)	(1.28)
Observation is after audit	-1.06^{***}	-1.41^{***}	-1.33^{***}
	(0.16)	(0.15)	(0.18)
No. of acquisitions in year			0.00
			(0.00)
GDP per capita (log)			-0.95^{*}
			(0.40)
GDP growth in previous year			1.11^{*}
			(0.48)
Local AM radio in munic.			-0.18
			(0.26)
Population (log)			-0.07
			(0.11)
% pop. over 15 w. 8+ years of education			1.78
			(1.21)
Mayor's party in president's coalition			-0.16
			(0.16)
Mayor in same party as state governor			0.16
			(0.17)
Mayor is in second term			-0.14
			(0.16)
Lottery f.e.		Yes	Yes
State f.e.			Yes
AIC	396.79	341.66	335.63
BIC	404.24	445.92	506.91
Log Likelihood	-196.40	-142.83	-121.81
Deviance	162.92	96.64	68.58
Num. obs.	306	306	306

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Appendix D: Re-specifications of the models on Table 3.8

Table D.1: Models of BCG vaccination rates, with cutoff for exclusion set at 300%

	Dependent variable: BCG vaccination rates in infants		
(Intercept)	0.25^{***}	0.53^{***}	0.83^{***}
	(0.03)	(0.04)	(0.04)
% invitations to bid in previous year	-0.06^{***}	-0.11^{***}	-0.05^{***}
	(0.01)	(0.01)	(0.01)
% procurement w/o auction in prev. year	-0.14^{***}	-0.13^{***}	-0.06^{***}
	(0.01)	(0.01)	(0.01)
No. of acquisitions in prev. year	0.00^{***}	0.00^{***}	0.00^{**}
	(0.00)	(0.00)	(0.00)
Munic. was audited in last 2 years	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)
A neighbor was audited in last 2 years	-0.07	-0.07	-0.06
	(0.13)	(0.13)	(0.13)
Population (log)	0.04^{***}	0.04^{***}	0.04^{***}
	(0.00)	(0.00)	(0.00)
Population density (log)	-0.02^{***}	-0.03^{***}	-0.01^{***}
	(0.00)	(0.00)	(0.00)
GDP per capita (log)	0.06^{***}	0.06^{***}	0.02^{***}
	(0.00)	(0.00)	(0.01)
State capital	0.06	0.01	0.12^{**}
	(0.04)	(0.04)	(0.04)
Any TB deaths in last year	0.01^{*}	0.01^{*}	0.02^{***}
	(0.01)	(0.01)	(0.01)
Hospital beds per 1,000 inhabitants	0.03^{***}	0.03^{***}	0.03^{***}
	(0.00)	(0.00)	(0.00)
% of women w. 8+ years of education	0.26^{***}	0.61^{***}	0.29^{***}
	(0.03)	(0.04)	(0.04)
Mayor is in second term	-0.01	-0.00	0.00
	(0.01)	(0.01)	(0.01)
Mayor's margin of electoral victory	0.03^{*}	0.03^{*}	0.01
	(0.02)	(0.02)	(0.01)
Year f.e.		Yes	Yes
State f.e.			Yes
R^2	0.11	0.13	0.19
Adj. R^2	0.11	0.13	0.19
Num. obs.	22114	22114	22114

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table D.2: Models of BCG vaccination rates, with cutoff for exclusion set at 500%

	Dependent variable: BCG vaccination rates in infants		
(Intercept)	0.24^{***}	0.52^{***}	0.82^{***}
	(0.03)	(0.04)	(0.04)
% invitations to bid in previous year	-0.06^{***}	-0.11^{***}	-0.04^{***}
	(0.01)	(0.01)	(0.01)
% procurement w/o auction in prev. year	-0.13^{***}	-0.13^{***}	-0.06^{***}
	(0.01)	(0.01)	(0.01)
No. of acquisitions in prev. year	0.00^{***}	0.00^{***}	0.00^{**}
	(0.00)	(0.00)	(0.00)
Munic. was audited in last 2 years	0.01	0.01	0.01
	(0.02)	(0.02)	(0.02)
A neighbor was audited in last 2 years	-0.08	-0.08	-0.06
	(0.13)	(0.13)	(0.13)
Population (log)	0.04^{***}	0.04^{***}	0.04^{***}
	(0.00)	(0.00)	(0.00)
Population density (log)	-0.02^{***}	-0.03^{***}	-0.01^{***}
	(0.00)	(0.00)	(0.00)
GDP per capita (log)	0.06^{***}	0.06^{***}	0.03^{***}
	(0.00)	(0.00)	(0.01)
State capital	0.05	0.00	0.11^{**}
	(0.04)	(0.04)	(0.04)
Any TB deaths in last year	0.02^{**}	0.02^{**}	0.03^{***}
	(0.01)	(0.01)	(0.01)
Hospital beds per 1,000 inhabitants	0.03^{***}	0.03^{***}	0.03^{***}
	(0.00)	(0.00)	(0.00)
% of women w. 8+ years of education	0.26^{***}	0.60^{***}	0.30^{***}
	(0.03)	(0.04)	(0.04)
Mayor is in second term	-0.00	-0.00	0.00
	(0.01)	(0.01)	(0.01)
Mayor's margin of electoral victory	0.03^{*}	0.04^{*}	0.02
	(0.02)	(0.02)	(0.02)
Year f.e.		Yes	Yes
State f.e.			Yes
R^2	0.10	0.12	0.18
Adj. R^2	0.10	0.12	0.18
Num. obs.	22142	22142	22142

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$