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Abstract

We estimate the effect of state judiciary presence on rent extraction in Brazilian local governments. We measure rents as irregularities related to waste or corruption uncovered by central government auditors. Our unique dataset at the level of individual inspections allows us to separately examine the spread and depth of rent extraction in local administrations. The identification strategy is based on an institutional rule of state judiciary branches according to which prosecutors and judges tend to be assigned to the most populous among contiguous counties forming a judiciary district. Our research design exploits this rule by comparing counties that are largest in their district to counties with identical population size from other districts in the same state, where they are not the most populous. IV estimates suggest that state judiciary presence reduces the share of inspections with irregularities related to waste or corruption by about 10 percent or 0.3 standard deviations.

Keywords: Institutions, Judiciary, Corruption, Rents, Local Governments
JEL: D02, D72, D78, H41, H83

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1 Introduction

There is much debate among both academics and policy-makers whether institutions that constrain executive power are beneficial for economic development.[1] An important example of such institutions is the judicial check on executive (and legislative) power, enshrined in constitutions around the world. Cross-country comparisons have shown that judicial independence is positively correlated with measures of political and economic freedom (La Porta, López-de-Silanes, Pop-Eleches and Shleifer, 2004), but not with economic growth (Glaeser, La Porta, López-de-Silanes and Shleifer, 2004).[2] Open and contentious questions remain, however, because measures of checks on the executive should reflect permanent constraints, rather than policies or constraints that may exist only on paper. Moreover, there is considerable debate about econometric identification of the causal link between various outcomes and institutions more generally (since institutions themselves likely reflect collective choices). In part, these controversies are inherent to the nature of cross-country comparisons, which typically rely on aggregated measures of institutions and in which identification of causal effects is notoriously difficult. A complementary approach, advocated and summarized in Pande and Udry (2006), is to analyze institutions in a within-country context where measurement and identification issues can be more easily addressed.

This paper provides evidence on the role of the territorial organization of the judiciary in constraining rent extraction by the local (municipal) executive power in Brazil.[3] Rather than evaluating the extent of independence of the judiciary as in the cross-country literature, we focus on the physical *presence* of state judicial institutions in the local community.[4] State-level prosecutors and judges provide the checks on local officials within their entire jurisdictions but are not physically present in each municipality. Less than half of all municipalities in Brazil have a local judicial presence and if they do, it is a permanent feature of the local institutional environment, rather than

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[1]For the view that constraints on the executive cause economic growth see the work of Knack and Keefer (1995), Mauro (1995), Hall and Jones (1999), Acemoglu, Johnson and Robinson (2001, 2002, 2005), Acemoglu and Johnson (2005) among others. For the alternative view that economic growth causes institutional improvement see Barro (1999), Przeworski, Álvarez, Cheibub and Limongi (2000), Glaeser, La Porta, López-de-Silanes and Shleifer (2004), Przeworski (2004a, 2004b) and Glaeser and Saks (2006). Pande and Udry (2006) provide a comprehensive survey of the entire literature on institutions and development.

[2]Glaeser et al. (2004) measure independence of the judiciary based on term-length of judges. Feld and Voigt (2003) construct an index of *de facto* judicial independence using expert surveys, which turns out to be positively correlated with economic growth, while *de iure* judicial independence is not.

[3]Municipalities are the lowest level of government in Brazil (below the federal and state governments). The discussion refers to municipalities, counties, communities, local governments, or "the local level" interchangeably.

[4]For simplicity we refer to "state judicial presence", "local judicial presence" or simply "judicial presence", rather than "physical presence of state judicial institutions at the local level".

a policy that shifts with prevailing political winds. We use detailed knowledge about the institutional design of state judiciary systems across Brazil to identify the causal effect of state judicial presence on rent extraction by local government officials.[5] We measure rent extraction (including low effort on the job) as infractions of public management regulations by the local executive branch as revealed by federal government auditors. Our micro-data thus allow us to shed light on a key policy decision by the executive branch on which the judicial check might operate.

Theoretically, we think of judicial presence as a factor that deters rent extraction by local incumbent politicians and public servants.[6] Local officials might be exposed to a higher probability of detection in counties with local judicial presence compared to counties without such presence, because the general public faces lower transaction costs to report irregularities. Similarly, local officials may perceive a higher probability of punishment when the state prosecutor lives in town because he faces lower transaction costs for his investigations.[7] Alternatively, local elites might find it easier to capture state judiciary officials when they reside in the same county, which would presumably lower the probability of punishment and increase the incidence of infractions.[8] The effect of judicial presence on rent extraction by local public agents may thus work through a multitude of channels, and the net effect is a priori ambiguous.

We address potential endogeneity of local judicial presence by exploiting a common institutional feature across state judiciary systems that is mandated by federal law: although state judiciary branches provide services to all counties in a given state, only those counties that are sufficiently large in terms of observable characteristics may become a judiciary district (*comarca* in Portuguese) by themselves and get a physical presence of judges and prosecutors.[9] This territorial organization in terms of districts is an institutional feature of state judiciary systems only—unique and distinct from the territorial organization of local and state governments, for example.

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[5]In our context, local legislators play a minor role compared to the local executive (mayors and program administrators).

[6]See Persson and Tabellini (2000) for formal models that typically yield the prediction that equilibrium rents, defined as private gains from holding office, are decreasing in transaction costs.

[7]Unfortunately, information on prosecutions from the 26 state judiciary branches is not readily available for outside researchers, and in fact not even for the central government. It is not clear what we could learn from comparing prosecutions across counties even if we had access to these data, however, since prosecutions are endogenously determined. For example, if judicial presence increases the perceived probability of prosecution, local managers and politicians would commit fewer infractions and as a result there would be *fewer* prosecutions in counties with judicial presence, not more.

[8]See Bardhan and Mookherjee (2000) for the trade-off between local information and capture under centralized vs. decentralized delivery of public services. See Stigler (1971) on state capture by interest groups. See Ríos-Figueroa (2007) for an argument linking judiciary effectiveness to political fragmentation.

[9]Lei Complementar N° 35, de 14 de Março de 1979, Art. 95-97.

State-level laws specify necessary—although not sufficient—conditions for the creation of judiciary districts in terms of population size and typically a subset of other characteristics, such as geographical area, size of the electorate, county fiscal revenue, judicial caseload, and the existence of facilities for the courthouse, prison, police quarters and residence of the judge and prosecutor. Roughly 75% of all counties do *not* become their own judiciary district. These counties are grouped together with contiguous neighbors, and only one of them becomes the local judiciary seat (*sede da comarca*) and gets the physical presence of the judicial apparatus.[10] Although state laws typically do not specify which county should become the seat in multi-county judiciary districts, the internally used assignment rule is to locate the seat in the most populous county because this minimizes transaction costs to access judicial services for citizens.[11]

Our research design exploits this rule by using as an instrument for local judicial presence an indicator for whether or not a county has the highest population in its judiciary district. Essentially, our reduced form compares counties that are largest in their district to counties with identical population size from other districts in the same state, where they are not the most populous. Our instrumental variable (IV) approach explicitly allows for the possibility that judicial presence is correlated with unobserved factors that also affect outcomes—even conditional on population— since we only use variation in judicial presence induced by district-specific population rank.[12] The approach relies on three main identifying assumptions. First, conditional on population, district maximum population is mean independent of unobserved factors that affect outcomes (conditional independence). Second, district maximum population affects rent extraction only through local judicial presence, not through other channels (exclusion restriction). Third, the probability of having a judiciary presence in the municipality is higher when the municipality is largest within its district, conditional on population (first stage). In Section 3 we discuss how we test these assumptions (to the extent this is possible).

Our measure of rent extraction in local governments is based on audit reports stemming from a policy of randomly selecting Brazilian municipalities for an inspection of federal transfers, which

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[10]Judiciary district formation itself is potentially endogenous. Please see the end of Section 3.3 below for discussion of this possibility.

[11]This information is based on private correspondence with judges and technical judiciary staff in various states.

[12]This approach requires weaker identifying assumptions than assuming that judicial presence is exogenous conditional on population.

we refer to as the random audits program. Following the terminology of the federal internal audit agency (*Controladoria-Geral da União,* CGU), we usually refer to the infractions of public management regulations by local government officials revealed in these reports as irregularities in public management. The irregularities reported by auditors range from improper financial reporting, over lack of oversight in project implementation, to waste and actual theft of public resources. Because of the random sampling, the types and incidence of irregularities are representative of problems in the local public sector in Brazil. If compliance with homogeneous national regulations is socially beneficial, deviations from the standard provide an objective measure of rent extraction by local executive officials, either through outright corruption or low effort on the job.[13] For the vast majority of the regulations considered by auditors in Brazil, compliance is likely to be socially beneficial yet privately costly. Moreover, many of these standards reflect international best practices in public financial management (PEFA, 2006).[14]

Our measure of rents is based on the same reports as the corruption and mismanagement measures in Ferraz and Finan (2008, 2011) and Brollo, Nannicini, Perotti, and Tabellini (2013), but with two important differences.[15] The first difference is that we focus on irregularities overall, rather than likely instances of corruption. After all, corruption is only one type of rent extraction.[16] Moreover, corruption represents only a small fraction of irregularities uncovered by auditors as shown in Ferraz and Finan (2011).[17] In addition to inevitable ambiguities in the identification of corruption episodes, our main reason for focusing on overall irregularities is that the law is not limited to penalizing corruption, which requires a relatively high standard of proof because individuals can go to jail if convicted, but allows prosecutors to charge individuals with the lesser offense of "acts of administrative misconduct". Since the judicial check should operate on both waste and corruption, a comprehensive measure of rents is more appropriate for our purposes. However, we

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[13]Effort can be seen as negative rents as in Barro (1973) and Persson and Tabellini (2000).

[14]In the terminology of Bandiera, Prat and Valletti (2009) we think of most irregularities uncovered by auditors as a measure of active waste in government spending: compliance is socially beneficial yet privately costly. If, in contrast, public management regulations were essentially red tape—and compliance therefore of limited or no social value—irregularities would correspond to lower passive waste.

[15]Zamboni and Litschig (2014) also use the published audit reports, as well as non-public administrative data at the procurement process level, survey data on satisfaction with health service delivery, and data on household visits that are routinely conducted by auditors.

[16]See Rose-Ackermann (1999, 2004) for a review of the empirical literature on poor governance, corruption and development. See Aidt (2003) for a review of the theoretical literature.

[17]This is evident from the line "Share of audited items" in Table 1 of their paper, showing that the average number of corruption episodes per audited item, conditional on at least one irregularity in the municipality, is 0.067. In contrast, the (conditional-on-positive) average number of mismanagement episodes per audited item in their data is about 1.647.

do distinguish *management irregularities*, giving direct evidence of waste or corruption in the local provision of public services, from what we call *procedural irregularities*, such as irregular or non-existent financial reports, where the connection to inefficiency is only indirect, and for which local officials are less likely to get punished a priori. Section 4 compares the coding of irregularities across papers in more detail.[18]

The second difference with other codings of the Brazilian municipal audit reports is that our unique micro-data at the level of individual inspections allow us to separately examine the spread (share of inspections with at least one irregularity) and depth of rent extraction (number of irregularities per inspections with at least one irregularity). This decomposition is new and important because it allows us to distinguish a situation in which there are irregularities in most or all inspections from a situation in which many irregularities are concentrated in just a few inspections. The decomposition also allows us to test which margins of rent extraction respond to judicial presence. In Section 4 below we show that from a descriptive standpoint, the distinction between spread and depth matters a great deal: 35 percent of all inspections came up entirely clean—without any evidence of procedural or management irregularities—while a full 55 to 61 percent showed no direct evidence that public resources were wasted or stolen.

Our main empirical result is that local presence of state judicial institutions reduces the share of inspections with irregularities related to waste or corruption by about 10 percent or 0.3 standard deviations. While we show that judicial presence also reduces irregularities overall, the effect turns out to be driven exclusively by a reduction in irregularities indicating waste or corruption, rather than procedural irregularities. This result is consistent with the intuition that less serious infractions are less likely to be detected by the public and prosecuted by the judiciary. Consequently, such infractions should respond less to judicial presence or not at all. The result also suggests that the reduction in irregularities is unlikely to be driven by a better understanding of public management regulations and hence better compliance in counties with local judicial presence, rather than a deterrence effect, since better understanding would presumably affect procedural irregularities more than those indicating waste or corruption. As further discussed below, these results are robust

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[18]Zamboni and Litschig (2014) find that the results in their paper are invariant to alternative corruption codings used in prior literature. We could further explore the sensitivity of results to alternative coding choices in this paper as well if required.

to the inclusion of a long list of standard and context-specific controls, and to alternative definitions of mismanagement vs. procedural irregularities.

In contrast, there is no evidence of an effect when we use as dependent variable the total number of irregularities divided by either the number of inspections with at least one irregularity or by the number of total inspections. One interpretation of these findings is that the effect of judicial presence is heterogeneous across agents and some of them are simply not deterred. For example, while the mayor and his direct subordinates might worry more about negative electoral consequences from corruption investigations when prosecutors and judges are present, common local government employees might not care about potential electoral punishments and hence remain undeterred by judicial presence. Another interpretation is that there are fixed costs to rent-taking in a given opportunity and so it makes economic sense to limit only the extensive and not the intensive margin in response to a higher risk of being caught.

An alternative interpretation is that there might be significant measurement error in the number of irregularities as a result of non-standardized reporting and the interaction between the discovery of irregularities and further inspections. Indeed, some of the reported irregularities may simply describe various aspects of the same underlying problem that different auditors report in different ways. Random measurement error in the number of irregularities would lead to noisier estimates when this variable is used in the numerator. In contrast, the share of inspections with at least one irregularity should be measured more accurately since auditor discretion in reporting the extent of the underlying issue plays no role.

We also investigate whether the impact of local presence of the judiciary on rent extraction depends on the mayor's re-election incentives, the main focus of Ferraz and Finan (2011).[19] They show that mayors in their first term in office (those with re-election incentives and less experience in office) are less corrupt compared to mayors in their second term (who cannot run again immediately because of weak term limits). Although these authors do not attempt to identify the causal effect of local judiciary presence (they call it judiciary district), they find that in counties with judicial presence the effect of re-election incentives on corruption is reduced. Using our broader measure of rents, in contrast, we find just the opposite: re-election incentives increase the

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[19]Whether the mayor is in his first or second term is not correlated with our instrument, conditional on population.

effect of local judiciary presence.[20] In fact, for mayors in their second term, local judicial presence does not seem to matter at all. Put differently, our estimates suggest that judicial presence reduces rent extraction only among first-term mayors. This might indicate that judicial presence operates mostly through an increased probability of detection, which disciplines incumbents with re-election incentives, rather than an increased probability of prosecution or conviction, which should also discipline incumbents without re-election incentives. This interpretation is consistent with the fact that actual convictions of mayors and other local officials are very rare events in Brazil (Arantes, 2004, 2007).

The paper is organized as follows. In Section 2 we describe the audits program and give institutional background on the role of the state judiciary in providing a check on local executive power in Brazil. In Section 3 we discuss the territorial organization of the judiciary and our identification strategy. We present our dataset on irregularities in local public management and describe how it relates to existing datasets that are also based on the audit reports in Section 4. Section 5 discusses our estimation approach. Results are presented in Section 6. We conclude with a discussion of limitations and extensions.

2   Audits program and institutional background

2.1   The random audits program

The random audits program was initiated under the government of Luiz Inácio Lula da Silva in March 2003 with the explicit objective of fighting corruption and waste in local public spending. Most municipalities were eligible for federal audit from the start of the program with the exception of state capitals.[21] Several rounds of sampling occur each year through a public lottery. The machinery used for the selection of municipalities is the same as that used for a popular national (money) lottery and results are broadcast on television and through other media. Our empirical analysis is based on a sample of 1,064 counties (about 20% of all counties in 2000) that have been

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[20]There is no reason to expect our results to be similar to those reported in Ferraz and Finan (2011) because of our sample restrictions (excluding single-municipality judiciary districts, as well as municipalities with population above 40,000), a different outcome variable (broad rents vs. corruption), and our instrumentation for judicial presence.

[21]More specifically, eligibility for federal audit is based on a population threshold which was successively increased from 20'000 to 500'000.

audited through June 2006.[22] Sampling is geographically stratified by state. Larger states tended to have lower sampling probabilities in the beginning of the program but probabilities have converged to around 1% per lottery.

The program is implemented by the general comptroller's office (CGU), the internal audit institution of the federal government. When a county is selected, the CGU headquarters in Brasilia determines the specific aspects of programs and projects that are audited and issues detailed *inspection orders* (ordens de serviço)—standardized sets of program- or project-specific inspections—to state CGU branches. For simplicity we will usually refer to service orders as inspections, although technically service orders are *sets* of inspections. Teams of auditors that are based in the state CGU branches are then sent to the sampled county. Transfers eligible for audit include those that are earmarked to carry out national health and education policies (*legais*), direct transfers to citizens (*diretas*), as well as other negotiated transfers (*voluntarias*), but exclude revenue-sharing transfers, such as those from the *Fundo de Participação dos Municípios*. Inspections occur for a subset of eligible federal transfers made during the preceding two to three years.

The number of auditors dispatched depends on county size (area and population), the proportion of rural and urban areas and the number of inspection orders, which in turn depends on the number of programs and projects running in the municipality. For instance, a county with a small population and a low number of items to be checked, but with a large rural area may require more auditors than another county with larger population but more people living in urban areas. In addition, municipalities for which the CGU has received a lot of complaints or where the mayor was recently impeached, receive larger teams.

Within a week of the county sampling, auditors spend about two weeks in the county in order to carry out their inspection orders. The quality of public services is assessed through interviews with the local population and service staff members. Auditors then write a report which details all the irregularities encountered during their mission. Reports include the amounts of resources audited, and if possible, any fraction that was diverted, wasted or stolen. This fraction is just a preliminary estimate, however. The exact amount diverted can only be assessed through a more detailed inspection which occurs only if it is subsequently deemed appropriate by the prosecutor

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[22]The number of municipal audits carried out through round 21 is 1,091. 21 municipalities were audited twice, and for 6 municipalities we lack census characteristics because they were installed after the year 2000.

in charge of the municipality. County mayors are given the possibility to comment on the draft report within five business days. Auditors in turn explain whether or not they accept the mayor's justification of problems found.

2.2 The role of the judiciary as a check on local executive power

Final audit reports are sent to local legislatures, the federal ministries that are remitting the transfers, external audit institutions at state and federal levels, as well as state and federal prosecutors. Prosecutors then decide whether to further investigate the irregularities uncovered by auditors and whether and what charges to press against particular individuals. Administrative misconduct is prosecuted at the local level, while prosecution of corruption falls into the jurisdiction of the state attorney general and judgment is passed by the state court of justice.

If convicted of corruption, defendants may be imprisoned for 1 to 8 years, in addition to losing their mandate and incurring fines. If convicted of "acts of administrative misconduct" or "improbity", punishments include the loss of mandate, the suspension of political rights for 8 to 10 years, prohibition from entering into public contracts for 10 years as well as the obligation to reimburse public coffers. In addition to charging individuals with corruption or administrative improbity, prosecutors have the privilege to use civil requests, requiring the entity in question to change its practice or be fined and prosecuted.[23] Because the courts cannot initiate proceedings on their own, prosecutors play a key role in the criminal justice system.[24]

In Brazil, prosecutors and judges are not part of local governments but of the state government and they are granted substantial *de iure* and *de facto* independence. The 1988 Constitution stipulates that individual prosecutors cannot be fired and guarantees their salaries. Prosecutors are hired through highly competitive entry examinations. At the state level, the only formal political influence occurs through the appointment of the attorney-general by the state governor from a short-list of three candidates who are members of the state procuracy.

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[23]See Arantes (2004) on the organization and legal instruments at the disposal of the Brazilian *Ministerio Público.*

[24]Prosecutors do not have the monopoly to charge individuals with corruption or administrative improbity as Art. 5 of the Brazilian constitution gives that right to ordinary citizens as well. Citizens rarely press charges, however. In addition, legislatures have the right to hold the executive accountable through impeachment proceedings. This channel of accountability depends entirely on the power configuration inside the legislature.

3 Background on judiciary districts and identification

3.1 Background on judiciary districts

We are interested in estimating the causal effect of judicial presence on rent extraction in local governments. The main empirical challenge is that state judiciary officials might choose the location of the local judiciary seat at least partly in response to local conditions.[25] We address potential endogeneity of local judicial presence by exploiting a common institutional feature across state judiciary systems that is mandated by federal law: although state judiciary branches provide services to all counties in a given state, only those counties that are sufficiently large in terms of observable characteristics may become a judiciary district (*comarca* in Portuguese) by themselves and get a physical presence of judges and prosecutors. State-level laws specify necessary—although not sufficient—conditions for the creation of judiciary districts in terms of population size and typically a subset of other characteristics, such as geographical area, size of the electorate, county fiscal revenue, judicial caseload and the existence of facilities for the courthouse, prison, police quarters and residence of the judge and prosecutor.[26] Table 1 gives details for each state. Roughly 75% of all counties do *not* become their own judiciary district.[27] These counties are grouped together with contiguous neighbors, and only one of them becomes the local judiciary seat (*sede da comarca*) and gets the physical presence of prosecutors and judges.

For the purpose of our analysis, we need to know whether or not a municipality had a judicial presence at the time irregularities were committed. Since the audits in our dataset happened between April 2003 and June 2006, and since the typical audit goes back about two years, the relevant period ranges from January 2001 to June 2006. We use the last completed year for which we have irregularities data, 2005, as our benchmark year to measure judicial presence based on the relevant legislation from each state. Because judicial presence is highly persistent over time, almost all counties with judicial presence in 2005 already had it in 2001. Table 1 documents that half the states in Brazil created the last new judiciary seats in 2001 or earlier and those states that

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[25]This is what Becker's (1968) model of crime and punishment would suggest. In addition to reverse causality, omitted variable bias is also likely.

[26]Typically, these same observables are also used to rank judiciary districts and allocate judicial presence on the intensive margin in terms of number of courts, judges, etc.

[27]The vast majority of single-county judiciary districts meet the state-specific requirements even though exceptions—determined at the discretion of the judiciary—are explicitly allowed by law (Lei Complementar N° 35, de 14 de Março de 1979, Art. 97, 2nd paragraph).

created new seats later did so mostly until 2003.

Table 2 summarizes the territorial organization of the judiciary across Brazilian states at two points in time, 1999 and 2005. Information on local judicial presence in 1999 is from a nation-wide survey entitled "*Perfil dos Municípios Brasileiros: Gestão Pública*", conducted by the national statistical agency, *Instituto Brasileiro de Geografia e Estatística* (*IBGE*). Table 2 shows that there were slightly more than two counties per judiciary district averaged across Brazil in 2005. Because of a substantial number of single-county districts, the average district size for districts that grouped more than one county together was about three. The table also shows that the number of judiciary districts in Brazil has increased only little between 1999 and 2005. Although not shown in the table, the vast majority (95%) of counties that had a local judicial presence in 1999 also had it in 2005 (and vice versa), making judicial presence a permanent feature of the local institutional environment.

3.2 Identification

State laws typically do not specify which of the contiguous counties forming a multi-county judiciary district gets the physical presence of prosecutors and judges. The two exceptions we know of are the states of Mato Grosso and Mato Grosso do Sul, where the law explicitly prescribes that the seat of the judiciary district must be located in the most populous county or the one which is easiest to reach (Código de Organização e Divisão Judiciárias, Art. 8 and Art. 11, respectively). In states where the law is silent on the issue, we have verified with judges and technical judicial staff that the internally used assignment rule is to locate the judiciary seat in the most populous county at the time of district creation because this minimizes transaction costs to access judicial services for citizens.

Ideally, we would therefore use population rank at the time of district creation as our instrument for current judicial presence, controlling for population at the time of district creation. This strategy is not feasible because the required information would be extremely costly to obtain. Information on the year of district creation is at best scattered across Brazil's 26 judiciary systems, at worst across the 2,607 districts existing in 2005 (Table 2). Moreover, although districts were created at different points in time, we would probably end up controlling for municipality population at a

common point in time in any case, even if we knew their population at the time of district creation.

Instead, we use the fact that population rank within districts is very stable over time. For example, over the period from 1997 (the most recent year of municipality creation in our estimation sample) to 2005, only two percent of municipalities changed population rank within their district. As a result, population rank in 2005 likely provides a good approximation for rank at the time of district creation. The same is not true for population levels, however, and contemporaneous population levels might be themselves influenced by judicial presence. To address this issue, we control for population levels in 2000, which could not have been affected by the irregularities in our sample since these start in 2001. We use an indicator for judiciary-district-specific maximum population in 2005—the year for which we know the district composition based on the relevant legislation from each state—as our instrument for contemporaneous local judicial presence. In the online Appendix we show that results are robust to alternative choices of population rank and control.

Essentially, our reduced form compares counties that are largest in their district to counties with identical population size from other districts in the same state, where they are not the most populous. Our research design is necessarily silent on the causal effect of judicial presence for single-county districts since we lack information about the assignment rule in these cases. Our instrumental variable approach relies on three main assumptions to identify the causal effect of judicial presence on rent extraction in local governments. First, conditional on population, district maximum population is mean independent of unobserved factors that affect outcomes (conditional independence). Second, district maximum population affects rent extraction only through local judicial presence, not through other channels (exclusion restriction). Third, the probability of having a judiciary presence in the municipality is higher when the municipality is largest within its district, conditional on population (first stage).

More formally, let Y denote the outcome variable (share of inspections with at least one ir-regularity), D treatment status (one for judicial presence, 0 otherwise), Z the instrument (one for judiciary-district-specific maximum population, zero otherwise), X municipality population, and U and V the influence of unobservables that affect Y and D, respectively. Assuming that the

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treatment effect is constant, we can write the outcome and first stage equations as follows:

$$Y = \beta_D D + \beta_X X + \beta_Z Z + U$$
$$D = \pi_Z Z + \pi_X X + V$$

Assuming linear specifications for X in the outcome and first state equations is without loss of generality as one could always include polynomial terms in X or a set of dummy variables for counties with similar values of X to flexibly control for population. Correlation between U and V (common factors determining both judicial presence and outcomes) leads to a correlation between D and U and hence endogeneity of D, even conditional on X. As a result, multiple regression and matching estimators will lead to inconsistent estimates under this data generating process.

Instead, our instrumental variable approach explicitly allows for a correlation between U and V since it only uses variation in D induced by Z to estimate β_D. Under the conditional independence assumption, district maximum population is mean independent of U and V, conditional on population: $E(U|Z, X) = E(U|X)$ and $E(V|Z, X) = E(V|X)$. And under the exclusion restriction $\beta_Z = 0$. We include Z in the data generating process (but not in the estimation equation) to emphasize that the exclusion restriction is a separate assumption from conditional mean independence (Angrist and Pischke, 2009). Instrument exogeneity amounts to both of these assumptions together. Without the exclusion restriction, the reduced form identifies $\beta_D \pi_Z + \beta_Z$, which in our context is arguably not a parameter combination that is of interest. With the first stage assumption, $\pi_Z > 0$, it can easily be shown that the ratio of reduced form coefficients on Z identifies β_D:

$$\frac{E(Y|Z = 1, X) - E(Y|Z = 0, X)}{E(D|Z = 1, X) - E(D|Z = 0, X)} = \frac{\beta_D \pi_Z}{\pi_Z} = \beta_D.^{28} \tag{1}$$

In what follows, we assess the plausibility of the three main identifying assumptions that lead to this result and discuss how we test them empirically with the data at hand.

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[28]To see this, substitute the equation for D into the equation for Y and evaluate the conditional expectations on the left-hand sided of equation (1). For example, we have $E(Y|Z = 1, X = x) = (\beta_D \pi_Z + \beta_Z) + (\beta_D \pi_X + \beta_X)x + E(\beta_D V + U|Z = 1, X = x) = \beta_D \pi_Z + (\beta_D \pi_X + \beta_X)x + \beta_D E(V|X = x) + E(U|X = x)$, where the second equality follows from the exclusion restriction and the conditional mean independence of Z given X. Proceeding analogously with the other three conditional expectations produces the right-hand side of equation (1).

3.3 Assessing conditional mean independence

The key threat to the conditional independence assumption is that unobserved factors that are correlated with population rank also have an effect on outcomes, even conditional on population. Both of these conditions must hold for conditional mean independence to fail, that is, the omitted factor must be both relevant and correlated with the instrument, conditional on population.[29] For example, a second-ranked municipality is by construction part of a larger district than a top-ranked municipality, once we compare municipalities of the same population size, and so population rank is mechanically correlated with *district* population. Similarly, the second-ranked municipality might be closer to large population centers and therefore more urban than the top-ranked municipality. Proximity to state capitals might also be related to outcomes through agglomeration effects for example. But conditional mean independence only fails if district population size, urbanization or proximity to state capitals also have direct effects on outcomes, conditional on municipality population.

More formally, let W denote a potential confounder, e.g. district size, β_W the effect of W on Y, and U' the influence of remaining unobserved factors that affect outcomes, so that we can write $E(U|Z,X) = E(\beta_W W + U'|Z,X) = \beta_W E(W|Z,X) + E(U'|Z,X)$. Even if $E(W|Z,X) \neq E(W|X)$, conditional mean independence will hold as long as $\beta_W \approx 0$ and $E(U'|Z,X) = E(U'|X)$. Empirically, it turns out that the effects of district size, urbanization or distance to the state capital on outcomes are indeed close to zero, once we control for municipality population. The more general point is that a correlation between unobserved factors and population rank by itself does not invalidate the conditional independence assumption, as long as these factors do not affect outcomes, conditional on population. Consequently, there is no point in showing estimates of the correlation between W and Z, conditional on X (and estimates of β_W) separately, although these results are available on request.

Instead, we show in Section 6 below that—conditional on population—our instrumental variable estimates of β_D are robust to including the potential confounders discussed above, as well as standard controls such as income per capita and the poverty rate, average education of the local population (Glaeser and Saks, 2006), ease of access to information (Reinikka and Svensson, 2005),

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[29]See Stock and Watson (2007), Appendix 13.3, for a textbook discussion of conditional mean independence.

proxied by the presence of a local radio station and internet access, voter turnout (Zingales, 2004), and measures of local government capacity, such as the extent of urban property tax collection, whether there are digitized records of assets, and whether accounting and budget control are computerized. We also prove our results robust to the inclusion of mayor party affiliation indicators and other mayor characteristics, such as the term he serves, education level, age, and gender as in Ferraz and Finan (2011).

In addition to these standard controls we also test for confounding factors that might be specific to our research design. For example, as noted above, state laws specify necessary conditions for the creation of judiciary districts in terms of observable characteristics such as geographical area, size of the electorate and county fiscal revenue, in addition to population.[30] Top-ranked municipalities might be systematically different along some or all of these dimensions and these characteristics might be correlated with rent extraction even conditional on population. It turns out however, that none of these potential confounders affect our results once we control for population (estimates available on request).

Another potential concern is that the legislation in each state only specifies necessary, not sufficient, conditions for the creation of judiciary districts. This means that there are municipalities that would qualify on their own to have a judicial presence, yet they are grouped together with other municipalities they share a border with. If—for some reason—fulfilling all the necessary conditions for district creation were correlated with population rank and if qualifying had an effect on outcomes, conditional on population, conditional mean independence would fail. It turns out, however, that neither of these conditions is satisfied (results are available on request).

Of course there might be other unobserved factors that are correlated with population rank and that also have an effect on outcomes, even conditional on population. For example, judiciary district creation might be endogenous in the sense that better managed counties might successfully pressure state officials to be grouped into judiciary districts with smaller neighbors, making the top-ranked counties systematically better managed than lower-ranked counties in other districts, even conditional on population. While we cannot rule out this possibility, we consider it unlikely for two main reasons. First, top population rank within a district does not guarantee judicial presence,

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[30]Other criteria shown in Table 1, such as judicial caseload, are also sometimes used but we do not have this data.

it only makes it more likely, as further discussed below. It is therefore more plausible that certain counties would lobby directly for judicial presence, rather than for rank with their district. Our IV approach allows for such behavior since it only uses variation in judicial presence for municipalities that comply with their assignment based on population rank. Second, if district creation were indeed endogenous, this would likely show up in observable differences between top and lower-ranked municipalities that matter for outcomes. It is reassuring in this respect that our estimate of interest is essentially unchanged when we include the long list of observables discussed above, once we condition on population.

3.4   Assessing the exclusion restriction

In addition to being independent of unobservables, conditional on population, being the largest county in the district should affect rent extraction only through local judicial presence, not by itself. It is worth emphasizing that other public or private institutions, such as local newspapers or TV stations, might of course use the same travel cost minimization logic as the judiciary to locate their headquarters in the most populous among a set of contiguous counties. But local newspapers or TV stations would presumably rank municipalities in terms of population within their respective media markets, not necessarily within judiciary districts. A violation of the exclusion restriction would only arise if local media markets were for some reason congruent with judiciary districts and media headquarters would locate in the largest municipality of the district, irrespective of whether the court is actually present. In that case there might be a direct effect of top population-rank on outcomes because of local media presence, even conditional on population.

A more likely scenario is that public or private institutions are choosing to locate in the municipality where the local court is based because of complementarities with activities of the judiciary. For example, many states explicitly require the existence of facilities for the prison and police quarters in the municipality in order to create a judiciary seat as shown in Table 1. Increased state police presence is therefore a direct consequence of judicial presence, not a violation of the exclusion restriction. Put differently, state police would not be more present in the top-ranked municipality within the district in terms of population if it were not for complementarities with judicial investigations. This implies that one of the channels through which judicial presence op-

erates might be through a higher presence of state police, which might reduce the cost of reporting malfeasance in the local administration.[31] Similarly, local media presence would be a channel of influence of judicial presence rather than a violation of the exclusion restriction.

A key advantage of our research design is that we can partially assess the validity of the exclusion restriction empirically, using a falsification test. If top population rank within the district had no direct effect on outcomes per se, conditional on population, it seems natural to expect no difference between second-ranked and lower-ranked municipalities either. The falsification test we perform therefore compares our measures of rent extraction between municipalities that are second-ranked in their district and those that are lower-ranked, conditional on population. As shown in Section 6 below, we find no effect of this "false" treatment, suggesting that population rank per se does not matter for rent extraction. As a result, it seems likely that the exclusion restriction holds, once we control for population.

3.5 Assessing the first stage

Finally, the first stage assumption requires that the probability of having a judiciary presence in the municipality is higher when the municipality is largest within its district, conditional on population. We show below that, controlling for population and other covariates, the first stage estimate is about 73 percentage points and highly significant.

If the effect of local judicial presence on rent extraction is heterogeneous, we estimate a local average treatment effect for small- to medium-sized municipalities in multi-county districts.[32] This average effect excludes those municipalities which—perhaps for political reasons—get a judicial presence irrespective of population rank, as well as those which do not get a judicial presence, irrespective of population rank. This result requires the monotonicity assumption, which in our case says that municipalities that got a judicial presence when they were not largest in their district, would have also gotten judicial presence had they been the most populous.[33]

Because the subpopulation of complier municipalities (for which district-specific population rank determines judicial presence) represents a sizeable share of all municipalities in Brazil—as

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[31]Judicial presence in general and state police presence in particular may also deter crime in the private sector. We have not explored this possibility due to space constraints.

[32]Abadie (2003) shows that if $P(Z = 1|X)$ is linear in X (and if appropriate regularity conditions hold), then the IV estimand with covariates provides a MMSE approximation to the average causal response for compliers.

[33]See Angrist and Imbens (1994) or Angrist and Pischke (2009) for background on local average treatment effects.

indicated by the first stage of 73 percentage points—the estimated local effect might be fairly representative of the average effect among small- to medium-sized municipalities.

4 Data

In the first subsection we present our measures of rent extraction in more detail. In the second subsection we relate our measures of rents to existing corruption and mismanagement measures in the literature that are also based on Brazilian local government audit reports. The third subsection discusses how we scale irregularities, as well as the distinction between the spread and depth of rent extraction. The fourth subsection discusses caveats. The last subsection summarizes the data on other municipality characteristics.

4.1 Data on irregularities in local public management

Audit report findings were compiled into a database by a team of researchers directed by Francisco Ramos at the federal university of Pernambuco. Our empirical analysis is based on a sample of 1,064 counties (about 20% of all counties in 2000) that have been audited through June 2006.[34] Our dataset is at the level of the inspection order and contains the year when the audited transaction was made, the amount involved, as well as detailed audit findings. Note that the amount involved or *valor envolvido* corresponds to the amount involved in the audited program or project, not the amount involved in corruption.[35] Following the practice of the comptroller general's office, we refer to the reported infractions of public sector management regulations as irregularities in public administration. It is worth emphasizing that each reported irregularity constitutes a breach of a specific legal norm by a local official and is potentially subject to prosecution by state procuracies.[36]

The violations reported by auditors range from improper financial reporting, over lack of oversight in project implementation, to waste and actual theft of public resources. The following

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[34]The number of municipal audits carried out through round 21 is 1,091. 21 municipalities were audited twice, and for 6 municipalities we lack census characteristics because they were installed after the year 2000.

[35]Ferraz and Finan (2011), Brollo et al. (2013) and Zamboni and Litschig (2014) impute the amount involved in corruption or mismanagement as the amount audited in a given inspection if at least one of the audit findings indicate a corruption or mismanagement irregularity. We did not pursue this approach here but would be open to do so if required.

[36]Not all irregularities reported by auditors are under the control of local officials. We exclude those (few) instances from our measures where auditors report on state or federal government failures or where reported irregularities are otherwise beyond local government control.

quotes, translated from actual audit reports, illustrate the types of irregularities encountered by auditors.

> 1) We verify the existence of improper payments to administrative staff at the expense of service personnel in the health care center. This situation is contrary to health ministry regulation which explicitly prohibits the use of federal transfers to this end.[37]

> 2) The mayor's office failed to organize a competitive tender for the procurement of school textbooks under the pretext that these books were unique although equivalent alternative textbooks were in fact available. The same administration had purchased different textbooks in the past.[38]

> 3) Our inspection of the project execution for two sanitary units reveals that they were constructed in smaller dimensions than projected. We also found that the height of the ceramic masonry in the bathroom was constructed below project specifications.[39]

Most of the irregularities uncovered by auditors are not easily classified as corrupt practices, in the sense of indicating abuse of public office for private (material) gain, although they very often do reflect bad public management practices.[40] Indeed, none of the examples above appear to unambiguously involve corruption. In all examples, however, managers were circumventing regulations that are intended to benefit end-users of public services or were not exerting enough effort on their job. They diverted public funds from their intended use, circumvented procurement procedures that are privately costly to carry out, and failed to oversee project implementation by contractors, which led to sub-standard project execution.

As these examples illustrate, distinguishing corruption from bad management is very difficult in practice. In fact, even with the support from prosecutors who—contrary to auditors—can request authorization from a judge to use wiretaps and to obtain suspect bank account records, identifying corrupt schemes is very costly and time-consuming.[41] As a result, auditors themselves deliberately

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[37] 9th lottery, Salgado de São Félix municipality, Paraíba state, Primary and Preventive Health Care Program.

[38] 11th lottery, Abaetetuba municipality, Pará state, Programa Brasil Alfabetizado.

[39] 10th lottery, Farias Brito municipality, Ceará state, Programa Esporte Solidário.

[40] Other existing objective measures typically capture corruption together with more general forms of government inefficiency. This issue is most pronounced with unit cost measures (Golden and Picci 2005) and input prices for hospital supplies (Di Tella and Schargrodski 2003). It also seems likely that at least part of the difference between funds disbursed by the central government and funds reported by recipients (schools) reflects management quality, i.e. adequate bookkeeping, rather than corruption (Reinikka and Svensson, 2004). Similarly, at least part of the difference between reported expenditure on road construction and estimated actual expenditure may be due to project management, i.e. attention to materials lost in the construction process, rather than corruption (Olken 2007).

[41] A good example of this is given by the "Sanguessuga" scandal. The first hints about the scheme came from inspections in several municipalities, spread across 10 Brazilian states, where auditors identified apparently small problems in a number of procurement processes that were won by the same ambulance seller. Once this pattern was identified, CGU auditors

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abstain from coding particular irregularities as corruption episodes and our paper follows their example. Fortunately for our purposes it is not necessary to distinguish between corruption and bad management, since the law is not limited to penalizing corruption, allowing prosecutors to charge individuals with the lesser offense of administrative misconduct.

However, it is also clearly the case that not all irregularities are equally serious. In line with CGU headquarter guidelines, we distinguish practices that indicate waste or corruption in the local provision of public services, which we label *management irregularities*, from practices where the connection to inefficiency is only indirect, such as irregular or non-existent financial reports, which we refer to as *procedural irregularities*. The distinction between management and procedural irregularities is also important as a robustness check on our results because local official are *a priori* less likely to get punished for procedural irregularities and hence judicial presence should matter less for the incidence of these practices, if at all.

Appendix I at the end of the paper gives a descriptive summary of the types of irregularities as they are reported by auditors, as well as our own classification into management (M) or procedural (P) varieties. We also indicate the types of irregularities we think could be reasonably classified either way (M/P). In the examples above, 1) would be a diversion of project resources, which we classify as a management irregularity. 2) would be an unjustified direct purchase, which we think could be classified as either a management or a procedural irregularity. 3) would be an example of substandard project execution, which we deem a management irregularity.

4.2 Relation to existing corruption and mismanagement measures

In Appendix I we compare our (LZ) measure of rents to existing corruption and bad management measures in the literature that are also based on the CGU audit reports. Ferraz and Finan (FF, 2011) define a corruption and a mismanagement measure, which essentially correspond to mutually exclusive subsets of our management irregularities. For example, one of the corruption categories in Ferraz and Finan, which they call "diversion of funds" is when funds "disappear from municipal bank accounts", which might roughly correspond to our "emission of checks without justification"

decided to dig deeper, and more irregularities were found, but still there was no clear evidence of corruption. Eventually, federal police and prosecutors joined the investigation and after many hours of recorded phone calls and bank account searches, the whole scheme was uncovered and hundreds of individuals, including mayors and deputies, were charged with corruption or administrative misconduct. Since 2003, over 30 operations of similarly large scale have been conducted.

type of irregularity. Another instance of diversion of funds they consider is when "the municipality claimed to have purchased goods and services that were never provided, which is determined when there is no proof of purchase and community members confirm that the goods were in fact not delivered". This corruption category would correspond to a subset of our "irregular/non-existent receipts" type, for which non-delivery was somehow confirmed. Their "over-invoicing", in which "auditors determined that the goods and services were purchased at a value above market price", corresponds to our "unjustified or excessive payments for goods and services" type. Finally, their "irregular public procurement", which is when "there is an illegal call-for-bids where the contract was awarded to a "friendly firm" and the public good was not provided" corresponds to a subset of our "simulated tender process", and "evidence of favoritism", types, where non-provision of the good or service was somehow confirmed, which we do not distinguish in our data.

The mismanagement measure in Ferraz and Finan is based on separate types of irregularities, illustrated mostly by way of examples. In procurement, a mismanagement episode occurs when "less than three firms bid for a public contract", corresponding to our "invitation for bids to less than three firms". Another example is "misuse of resources", which corresponds to "diversion of project resources" in our Appendix I. Other examples are "medicines were not being properly stored", "schools were serving lunches that were past their expiration dates", or "the mayor's office was not keeping school attendance for children participating in a federal school program", which would fall into our "inadequate equipment/inventory maintenance", "inadequate project inputs", and "non-existent school attendance verification" categories, respectively.

Brollo et al. (BNPT, 2013) also use the CGU audit reports to construct a narrow and a broad corruption measure, both of which basically correspond to a subset of our management irregularities as shown in Appendix I. Their narrow corruption measure includes cases of "limited competition", corresponding roughly to our "evidence of favoritism" category, "fraud", corresponding to our "simulated tender process", and "manipulation of the bid value", which we label "fractionalizing of procurement amounts". Their narrow definition of corruption also includes cases of "favoritism in the good receipt", which we do not distinguish in our data, as well as "over-invoicing", which amounts to our "unjustified or excessive payments for goods and services" category. In their broad measure of corruption, Brollo et al. include "an irregular firm wins the bid process", correspond-

ing roughly to our "participating ineligible firm", "the minimum number of bids is not attained", which we label "invitation for bids to less than three firms", as well as "the required procurement procedure is not executed", which is our "procurement modality too restricted". In their broad measure of corruption, Brollo et al. also include "diversion of funds", corresponding to our "diversion of project resources", as well as "paid but not proven", which we label "irregular/non-existent receipts".

4.3 Clean inspections, spread and depth of irregularities

An important challenge for any measure of rents is how to deal with issues of scale. The raw reported number of irregularities or corruption episodes is a problematic measure of rents because it mechanically increases with local government size (more locally administered programs, more scope for irregularities) and with the number of inspections that are carried out (more inspections, more reported irregularities). In order to address this issue, we construct a unique dataset at the level of the service order by obtaining those inspections from the audit reports which turned up no irregularities at all, and by relating each irregularity to its corresponding service order. A key feature of our inspections-level dataset is that it allows us to separately examine the spread (share of inspections with at least one irregularity) and depth (number of irregularities per inspections with at least one irregularity).

Table 3 presents the distribution of irregularities per inspection (technically per service order). The first striking finding is that 35 percent of all inspections in the sample came up entirely clean. The median is one irregularity per inspection. This is in stark contrast with the mean number of irregularities per inspection (not shown) which is about 2. The mean is more than twice the median because of many irregularities in just a few inspections: 10% of inspections turn up six or more irregularities. Table 3 also gives summary statistics broken down by type of irregularity for two definitions of management vs. procedural irregularities. Under our two definitions of management irregularities, 55 percent and 61 percent of all inspections, respectively, came up clean, that is, without direct evidence that public resources were wasted or stolen. Again, the average of management irregularities per inspection is about 1 (not shown), the median is zero, and the difference is largely driven by two and one percent of inspections, respectively, which

turned up six or more management irregularities.

Table 4 presents summary statistics on irregularities and inspections, aggregated by municipality. The average number of service orders is about 29 and, conditional on population, it is uncorrelated with population rank (results available on request). The average of total irregularities divided by total inspections across municipalities in our data is 2.2 (median 1.9), while the mean (and median) share of inspections that turn up at least one irregularity is 67%, and the mean number of irregularities per inspections with at least one irregularity is 3.2 (median 2.8). This means that in the "typical" municipality a full 33% of inspections came up entirely clean. Looking only at irregularities that indicate waste or corruption, the average total of such irregularities per total inspections is about 1 (median 0.8), while the mean (and median) share of inspections involving such irregularities is about 0.45, and the conditional-on-positive mean number of such irregularities per inspections is 2.1 (median 1.9). That is, in the typical municipality, 55% of all inspections give no indication that public resources were wasted. This number increases to about 65% when we drop irregularities that we consider could be classified as either of the management or of the procedural type.

4.4 Caveats

There are three caveats worth pointing out regarding our measures of rent extraction. First, we assume that existing rules and regulations which define both management and procedural irregularities make sense, that is, they serve a legitimate purpose in a reasonable way.[42] Put differently, we take irregularities to be generally detrimental to public service delivery, rather than reflecting attempts by well-meaning officials to circumvent inefficient red tape. As mentioned above, mayors and managers have the possibility to comment on the audit report. Sometimes auditors concede that there are valid arguments for non-compliance and we exclude these instances from our measures. Based on our reading of the regulations considered here, we believe that reported irregularities are for the most part undesirable from a social point of view because they either involve a direct waste or loss of public resources or complicate the detection of such mismanagement. It is also worth noting that the regulations pertaining to public financial management reflect international

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[42]Without this assumption we are still evaluating compliance.

best practices.[43]

The second caveat is that we need to assume that auditors themselves were not bribed into manipulating audit findings. If this manipulation were for some reason more likely in municipalities with judicial presence, it would bias our estimates. However, we believe that the institutional setup makes it very unlikely that auditors are corrupt. First, auditors are paid by the federal government, not by local governments, which makes it less likely that they are captured by local special interests. Second, auditors are relatively well paid, and therefore have a lot to lose in case collusion gets detected. Third, auditors work in teams of about 10 people on average. This makes it hard to sustain collusion on any significant scale because the whole team has to be bribed in order to conceal irregularities. Fourth, the interaction between auditors and local officials is at a single point in time (unknown ex ante), which again makes it harder to sustain collusion. Finally, CGU auditors' work is itself subject to periodic inspection from the external audit agency of the central government, the *Tribunal de Contas da União* and we are not aware of any reported cases of collusion between CGU auditors and local administrations.

The third caveat is that even if auditors were incorruptible, the local elite might somehow manage to manipulate what gets uncovered and what remains unnoticed. While this scenario is plausible in general, it is unlikely in our case because local elites play no direct role in carrying out the audit. Auditors go into a county with specific orders to investigate particular programs and projects and the items on their list are not subject to local review. Neither is it likely that local managers succeed in systematically concealing more irregular transactions in counties with judicial presence because the audit is very thorough, involving both financial auditing and detailed inspection of public works and services in the field. Since hiding malfeasance is costly, there will necessarily be instances where the extra cost induced by judicial presence exceeds the expected benefits of committing the offense, thus leading to less offenses (Becker 1968).

It is also important to keep in mind that Olken (2007) finds that administrative irregularities in road construction detected by central government auditors are positively correlated with missing expenditures as determined by independent engineers. It seems likely that there is less underdetection of corruption based on an unexpected type of audit as conducted by engineers, compared

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[43]See PEFA (2006) for an overview of international standards in public financial management.

to irregularities reported in routine audits. If missing expenditures and administrative irregularities are positively correlated not only in the Indonesian but also in the Brazilian setting, then at least part of the impact of judicial presence we find reflects a real reduction in rent extraction.

4.5   Data on county characteristics

Data on county characteristics come from several sources. We obtained data on judiciary districts and the indicator for local judicial presence from each state's law on the organization and territorial division of the judiciary branch (*Código da Organização e Divisão Judiciárias*). For most states, the data on judiciary districts and local judicial presence is from the year 2005. To construct our instrument, we therefore rank municipalities within each judiciary district in terms of year 2005 population. Information on local judicial presence in 1999 is from a nation-wide survey entitled "*Perfil dos Municípios Brasileiros: Gestão Pública*", conducted by the *Instituto Brasileiro de Geografia e Estatística* (*IBGE*). The same source also has information on the extent of urban property tax collection, digitized records of assets, and computerized accounting and budget control. Official local population data for the years 2000 and 2005 are also from IBGE.[44] Data on local income distribution, schooling and health outcomes, and distance to state capitals are from the *Instituto de Pesquisa Economica Aplicada* (*IPEA*) based on the 2000 census. Mayor characteristics and political participation data are from the *Tribunal Superior Eleitoral* (*TSE*).

Table 5 presents sample means of the covariates used in the empirical analysis. Column 1 gives sample means of all 4,442 municipalities in Brazil for which we do *not* have audits data. Columns 2 through 4 give sample means for mutually exclusive subsamples of municipalities for which we have audits data available. Column 2 gives sample means for the 275 single-county judiciary districts in our dataset, column 3 for the 304 municipalities with judicial presence in multi-county districts and column 4 for the 485 municipalities without judicial presence. Columns 5 and 6 present summary statistics for our main estimation sample: multi-county districts with and without judicial presence that were no larger than 40,000 inhabitants according to year 2000 census figures. Counties with and without judicial presence show sizeable differences in the raw sample

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[44]For intercensal years, such as 2005, official population estimates are produced using a forecasting procedure that ensures consistency of estimates for lower level units (municipalities) with the higher levels (states and the country as a whole) (IBGE, 2002).

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means for many municipality mayor characteristics, making it clear that controlling for population is crucial for our approach.

5 Estimation approach

We use an indicator for district-specific top population rank as an instrument for judicial presence to estimate β_D.[45] Because the population size of the municipality has a direct effect on rent extraction, controlling for population in equation (1) is crucial for our approach. Figure 1 shows that for small- to medium-sized municipalities up to about 40,000 inhabitants, there is a common support of population among those ranked second or lower in terms of population in their district (Z=0) and those that are top-ranked (Z=1). In order to ensure a common support, we therefore drop top-ranked municipalities with population above 40,000 from the sample. We only trim from the top because the two supports overlap much better at the bottom, as is evident from Figure 1. We also drop single-county judiciary districts, which satisfy all requirements by themselves and are therefore intrinsically different from those that do not.[46] Note that these two sample restrictions are dictated by our knowledge of the assignment rule for multi-county judiciary districts and the fact that we lack such institutional information about single-county judiciary districts.

We control for the direct effect of population on outcomes using polynomial terms in year 2000 census population, determined prior to the audit results used in this study. Results are quantitatively very similar when we use a set of indicators for bins of *X* as controls (available on request). All estimations below include state fixed effects because the probability of having a local judicial presence varies systematically across states (as evident from Table 2), as does our measure of rent extraction. Note that we cannot include judiciary district fixed effects for two reasons: first, with population held constant we necessarily compare counties from different districts, and second, we would loose districts without variation in the instrument (recall that we have outcome measures only for audited municipalities, not for all municipalities within a given district).

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[45]We use the IV estimator (rather than indirect least squares (ILS) as in the discussion in Section 3.2 above) because in addition to providing a point estimate (numerically identical to the ILS estimate), the IV estimator also directly calculates the correct (robust) standard errors. Reduced form estimates are available on request.

[46]Conceptually, our instrument is also not well defined in single-county judiciary districts because maximum population requires a comparison of at least two municipalities per district.

6 Estimation results

6.1 First stage

Table 6 presents linear probability model (OLS) estimates of the first stage relationship between local judicial presence (judiciary seat) and the indicator for judiciary-district-specific maximum population (maximum population), conditional on population. The estimates of π_Z in the first three columns of Table 6 with linear, quadratic, and cubic population controls, respectively, suggest that the probability of having a local presence of the judicial apparatus increases by 80 percentage points if the county is largest in its district, for counties with the same population size. Columns four, five and six consecutively add municipality characteristics, a set of mayor party affiliation dummies, and other mayor characteristics. Municipality characteristics are the only set of covariates that jointly predict judicial presence (p-value=0.00) and some of these seem to be somewhat correlated with the instrument since the point estimates of π_Z decreases to about 73 percentage points. The implied first stage F-statistic is $t^2 = (0.73/0.04)^2 = 330$, well beyond conventional critical values for the weak instrument test based on TSLS size (Stock and Yogo 2005). The last two columns of Table 6 show that the first stage is similar in magnitude and statistical significance, irrespective of whether the municipality is run by a first- or second-term mayor. Overall, these results suggest that, controlling for population and other covariates, the first stage estimate is about 73 percentage points.

Figure 2 presents graphical evidence of the first stage. Each dot in Figure 2 corresponds to the sample proportion of municipalities that are judicial seats for a given judiciary-district-specific population rank (top or not top) and in a given population bin. Consistent with Figure 1 above, there is a lot of overlap in the two distributions of population in top- and lower-ranked municipalities except at the boundaries of the support. In fact, there are no top-ranked municipalities in the population range 0 to 2,500 and no lower-ranked municipalities in the range 37,500 to 40,000. Perhaps the most striking feature of Figure 2 is that the first-stage relationship of about 80 to 90 percentage points is approximately constant irrespective of the level of population, again with the exception of the bins that are closest to the boundaries of the support.

6.2 Impact on the share of irregular inspections

Table 7 gives IV estimates of β_D using the share of inspections that turn up at least one irregularity as the dependent variable (the extensive margin of rent extraction). The point estimates of β_D in the first three columns of Table 7 are decreasing from the linear population control to the quadratic and then stabilize (no change when the cubic control term is added) at -0.051, significant at the 5 percent level. This suggests that while the linear specification of population yields an upward biased estimate of β_D, the quadratic and cubic specifications control for any direct effect of population size on the incidence of irregularities. Columns four through eight show that the effect size of about -0.05 is remarkably robust to the inclusion of municipality characteristics, mayor party affiliation dummies, as well as other mayor characteristics. At the same time, the R-squared increases by about 5 percentage points, going from about 19 percent in column 3 to about 24 percent in column 8.[47] Together, the unchanged estimates and increased fit with covariates beyond population imply both that the covariates that are correlated with the instrument (such as district size or urbanization) are not relevant predictors of the outcome and that those that are relevant (such as mayors' party affiliations) are not correlated with the instrument, conditional on population. These results thus provide some evidence in favor of conditional mean independence.

Figure 3 presents graphical evidence of the reduced form relationship between population rank and the share of inspections that turn up at least one irregularity, conditional on population. Each dot corresponds to the sample average of the share of inspections with at least one irregularity in deviation from the state average for a given judiciary-district-specific population rank (top or not top) and in a given population bin. The two solid lines in Figure 3 show that the share of irregular inspections is about 4 percentage points lower in top-ranked municipalities compared to those that are lower-ranked on average, conditional on population. Figure 3 also shows that the share of irregular inspections is reduced in top-ranked municipalities in almost every bin, although the magnitude of the reduction varies across bins.

Table 8 presents the main empirical result, which is that local presence of state judicial institutions reduces the share of inspections with irregularities related to waste or corruption by about 10

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[47]While the R-squared can be negative in an IV regression, it cannot be above one because the sum of squared residuals cannot be negative. It therefore makes sense to interpret the R-squared even in an IV regression. In particular, R-squared increases if and only if the estimated coefficients on additional covariates are non-zero.

percent or 0.3 standard deviations. As in Table 7, the IV estimate of β_D in the first three columns of Table 8 decreases as the quadratic population term is added but stabilizes at about -0.046, again significant at the 5 percent level. And again, the effect size is remarkably robust to the inclusion of municipality characteristics, mayor party affiliation dummies, as well as other mayor characteristics. Compared to the mean share of inspections with management irregularities of 0.44 and standard deviation of 0.16, the effect amounts to about -10% or 0.3 standard deviations. Figure 1 in the online Appendix presents graphical evidence of the reduced form relationship between population rank and the share of inspections with irregularities related to waste or corruption, conditional on population.

6.3 Impact on total number of irregularities per inspection

In contrast, there is no evidence of an effect when we use as dependent variables the total number of irregularities divided by either the number of inspections with at least one irregularity or by the number of total inspections. Table 9 shows results for total irregularities divided by the total number of inspections. The point estimates change sign several times across specifications and they are nowhere near statistical significance, mainly because the standard errors are an order of magnitude larger compared to Tables 7 and 8 above. We omit the results for total irregularities divided by "dirty" inspections to save space (see Table 1 in the online Appendix).

6.4 Management vs. procedural irregularities

Table 2 in the online Appendix shows that the lower incidence of infractions is driven exclusively by a reduction in management irregularities, rather than procedural irregularities. This result is consistent with the intuition that less serious infractions are less likely to be detected by the public and prosecuted by the judiciary. The result also suggests that the reduction in management irregularities is unlikely to be driven by a better understanding of public management regulations and hence better compliance in counties with local judicial presence, since this would presumably affect procedural irregularities more than irregularities indicating waste or corruption. Tables 3 and 4 in the online Appendix show that our main result is robust to an alternative definition of mismanagement vs. procedural irregularities which re-classifies those types of irregularities that

we think could be classified either way as procedural.

6.5 Falsification test of the exclusion restriction

Table 10 shows that the incidence of irregularities is no different between municipalities that are second-ranked in their district and those that are lower-ranked, conditional on population. Although most of the estimates are negative, they are an order of magnitude smaller than the estimates of β_D discussed above and they are nowhere near statistical significance. This test result increases our confidence in the exclusion restriction, that is, were it not through judicial presence, population rank per se would have no effect on rent extraction.

6.6 Impacts by term of the mayor

In Table 11 we test whether the effect of local presence of the judiciary on rent extraction depends on the mayor's term in office. The estimates suggest that judicial and political accountability complement each other, with judicial presence reducing the share of inspections with irregularities more strongly for first-term mayors compared to second-term mayors. Among first-time mayors, judicial presence reduces the share of irregular inspections by about 8 to 10 percentage points (sum of the first two coefficients), significant at 5% throughout (p-value on F-statistic). In contrast, for mayors in their second term, local judicial presence does not seem to matter at all. The estimate of the differential effect in Table 11 becomes a bit larger and statistically significant at 10 percent when covariates are added.[48]

Table 9 in the online Appendix shows that the differential effect is again driven by a reduction in irregularities related to waste or corruption. For mayors in their first term, the point estimate is about -7 to -9 percentage points. For mayors in their second term in contrast, local judicial presence seems to have no effect. The estimate of the differential effect becomes again a bit larger and statistically significant at 5 percent when covariates are added. These results suggest that judicial presence operates mostly through an increased probability of detection of irregularities, which disciplines incumbents with re-election incentives, rather than an increased probability of

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[48]We find quantitatively similar results when we restrict the sample to municipalities that were audited during 2003 or 2004, the last two years of the 2001-2004 term, to which our first-term indicator corresponds. Since auditors inspect transfers that were made during the two preceding years, audit results from 2005 and the first half of 2006 correspond at least in part to management practices in the 2001-2004 term.

prosecution or conviction, which should also discipline incumbents without re-election incentives.

6.7 Additional robustness checks

Table 5 in the online Appendix shows that the main result is robust to an alternative cutoff value for trimming the population support. The cutoff value above 51,000 residents is chosen because this includes the next two municipalities which are second- or lower-ranked in their district, as evident from Figure 1. While the choice of cutoff value is clearly arbitrary, we have found quantitatively similar results for a range of cutoffs and these results are available on request. Table 6 in the online Appendix shows that the main result is robust to excluding judiciary seats that were created at some (unknown) point in time between 1999 and 2005 and that could have been created in response to irregularities uncovered by auditors. Table 7 in the online Appendix shows that the main result is also robust to using population rank in 2001, the earliest year for which audit results are available—rather than rank in 2005—as the instrument for judicial presence in 2005. Finally, Table 8 in the online Appendix shows that the main result is robust to controlling for population in 1997, the most recent year of municipality creation in our estimation sample.

7 Conclusion

This paper provides evidence on the role of the territorial organization of the judiciary in constraining rent extraction by the local (municipal) executive power in Brazil. We show that local presence of state judicial institutions reduces the share of inspections with irregularities related to waste or corruption by about 10 percent or 0.3 standard deviations. In addition, we provide new evidence on the interaction between electoral and judicial accountability. In particular, our estimates suggest that judicial presence reduces rent extraction only when mayors have re-election incentives. This might indicate that judicial presence operates mostly through an increased probability of detection, which disciplines incumbents with re-election incentives, rather than an increased probability of prosecution or conviction, which should also discipline incumbents without re-election incentives.

Although this evidence is suggestive of a particular mechanism, ultimately we cannot say precisely *how* judicial presence leads to a reduction in rent extraction by local government officials. The results are nonetheless of interest from the perspective of evaluating whether to scale up judi-

cial presence at the local level, since pinpointing exact channels is typically not viewed as necessary (nor sufficient) for this purpose (Ludwig, Kling and Mullainathan, 2011).[49] Given that about 75% of all municipalities belong to multi-county districts and that the vast majority of them is of small to medium size, and given the high proportion of municipalities that followed the assignment rule, the (local average) treatment effect we identify in this study is in fact fairly general. From a policy perspective, our results therefore suggest that scaling up judicial presence at the local level in Brazil would likely reduce irregularities related to waste or corruption in the local public sector.

Judicial presence should be scaled up if and only if the net benefits of such a policy are positive. While the costs of an expansion of judicial presence are relatively easy to quantify, assessing the benefits in monetary terms is difficult as we would need to know the value of a marginal increase in compliance with existing public sector rules and regulations (and other benefits of local judicial presence). A necessary first step in this direction would be to quantify the cost savings and ideally even service delivery improvements stemming from judicial presence. Unfortunately, however, the audit reports considered here do not systematically include an estimated amount of funds that were diverted, wasted or stolen. More detailed data is therefore required to better quantify the benefits of local judicial presence in terms of cost savings and service improvements.

Whether judicial presence reduces rent extraction in other countries and institutional contexts as well is an open and important question. We speculate that our results help explain the fact that state district attorneys, the U.S. institutional equivalent of Brazilian state prosecutors, today are present in most counties in the U.S., although historically this was not the case. Since budget constraints often require that a choice has to be made where to place the judicial apparatus, similar research designs to the one introduced in this paper might be applicable to historical U.S. data or to contemporary data from developing countries other than Brazil.

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[49]Learning more about mechanisms would of course be desirable in itself in order to interpret the results and to help assess whether they would generalize to other settings (Deaton, 2010; Ludwig, Kling and Mullainathan, 2011).

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Table 1: Judiciary district requirements and judiciary seat creations prior to 2005

State	Judiciary district minimal requirements	Source	Last creation of judiciary seat prior to 2005	Source
Acre	P, C, E	CODJ Art. 24	unknown	CODJ 2005, 2010
Amapá	P, C, B, Q	CODJ Art. 4	1999	CODJ 2005
Amazonas	B	CODJ Art. 10	1997	CODJ 2004
Pará	P, E, R, C, A, B, Q	CJ Art. 10	2002	CJ 2003, 2006
Rondônia	P, E, R, C, B, Q	CODJ Art. 83	2003	CODJ 2003, 2006
Roraima	P, E, C, B	COJ Art. 28	2001	COJ 2001
Tocantins	P, E, C, B, Q	LOPJ Art. 6	2002	LOPJ 2002
Alagoas	P, E, R, C, A, B, Q	COJ Art. 125-6	1998	COJ 2005
Bahia	P, E, R, C, A, B, Q	LOJ Art. 7-8	unknown	LOJ 2005
Ceará	P, E, R, C, A, B, Q	COJ Art. 57	1997	COJ 2005
Maranhão	P, E	CDOJ Art. 6	2004	CDOJ 2008
Paraíba	P, E, R, C, A, B, Q	LOJ Art. 7	2002	LOJ 2005
Pernambuco	P, E, R, C	COJ Art. 5	2004	COJ 2006
Piauí	P, E, R, C, A, B, Q	LOJ Art. 6	unknown	LOJ 2008
Rio Grande do Norte	P, E, C, B	LDOJ Art. 7	1999	LDOJ 2005
Sergipe	P, E, R, C, A	COJ Art. 3	prior to 1999	COJ 2003, 2008
Goiás	P, E, R, C, B	COJ Art. 6	1999	COJ 2005
Mato Grosso	P, E, R, C, A, B, Q	CODJ Art. 11	2001	CODJ 2003
Mato Grosso do Sul	P, E, R, C, A, B, Q	CODJ Art. 14	2001	CODJ 2001, 2006
Espírito Santo	P, E, R, C	CODJ Art. 5	2002	CODJ 2002, 2008
Minas Gerais	P, E, C, B, Q	CODJ Art. 5	2001	CODJ 2001, 2009
Rio de Janeiro	P, E, R, C	CODJ Art. 11	2000	CODJ 2000, 2005
São Paulo	E, C, R	CODJ Art. 12	2003	CODJ 2003
Paraná	P, E, R, C, B, Q	CODJ Art. 216	2003	CODJ 2003
Rio Grande do Sul	P, E, R, C	COJ Art. 3	2000	COJ 2003
Santa Catarina	P, E, R, C, A, B, Q	CDOJ Art. 8-10	2003	CDOJ 2005

Notes: Requirements: Population (P), Caseload (C), Electorate (E), Judiciary Buildings, including prison (B), Revenue (R), Area (A), Police Quarters (Q). Sources: Código de Organização e Divisão Judiciárias (CODJ), Código Judiciário (CJ), Código de Organização Judiciária (COJ), Lei Orgânica do Poder Judiciário (LOPJ), Lei de Divisão e Organização Judiciárias (LDOJ).

Table 2: Judiciary districts in Brazil, 1999 and 2005

State	Region	# of Counties		# of Districts		Counties per District 2005			
		2000	2005	1999	2005	Mean	Std.	Min	Max
Acre	N	22	22	14	22	1.00	0.00	1	1
Amapá	N	16	16	10	11	1.45	0.52	1	2
Amazonas	N	62	62	56	62	1.00	0.00	1	1
Pará	N	143	143	96	105	1.36	0.77	1	5
Rondônia	N	52	52	20	25	2.08	1.08	1	6
Roraima	N	15	15	5	7	2.14	1.07	1	4
Tocantins	N	139	139	42	45	3.09	1.61	1	7
Alagoas	NE	101	102	63	63	1.61	0.85	1	4
Bahia	NE	415	417	268	272	1.53	0.81	1	5
Ceará	NE	184	184	137	137	1.34	0.60	1	4
Maranhão	NE	217	217	79	125	1.74	0.79	1	5
Paraíba	NE	223	223	70	72	3.10	1.73	1	9
Pernambuco	NE	185	185	112	148	1.22	0.48	1	3
Piauí	NE	221	223	89	97	2.30	1.58	1	9
Rio Grande do Norte	NE	166	167	62	65	2.57	1.47	1	7
Sergipe	NE	75	75	37	37	2.03	1.09	1	5
Goiás	CW	242	246	113	119	2.07	0.97	1	6
Mato Grosso	CW	126	141	49	55	2.56	1.45	1	6
Mato Grosso do Sul	CW	77	78	45	51	1.53	0.70	1	4
Espírito Santo	SE	77	78	68	69	1.13	0.34	1	2
Minas Gerais	SE	853	853	282	309	2.76	1.73	1	11
Rio de Janeiro	SE	91	92	71	73	1.25	0.55	1	3
São Paulo	SE	645	645	228	224	2.88	1.79	1	10
Paraná	S	399	399	156	155	2.57	1.33	1	6
Rio Grande do Sul	S	467	496	157	162	3.06	2.06	1	14
Santa Catarina	S	293	293	86	97	3.01	1.65	1	8
Brazil		5,506	5,563	2,415	2,607	2.13	1.47	1	14

Notes: The source for judiciary districts in 1999 is a nationwide survey administered by the statistical institute IBGE. For 2005 the sources are the Códigos de Organização e Divisão Judiciárias of each state as detailed in Table 1.

Table 3: Distribution of irregularities per service order

Variable	0	1	2	3	4	5	6	Above 6
Irregularities	35%	25%	13%	8%	6%	3%	3%	7%
Management irregularities	55%	25%	9%	5%	2%	2%	1%	1%
Procedural irregularities	54%	21%	10%	5%	3%	2%	1%	4%
Clear management irregularities	61%	23%	8%	4%	2%	1%	0%	1%
Serious procedural irregularities	51%	22%	10%	6%	4%	3%	1%	3%

Notes: N=30,874. The unit of observation is the *service order*, which is a standardized set of inspections on programs and projects issued by the CGU headquarters. Management irregularities include those classified as M/P in Appendix I, while clear management irregularities exclude those. Procedural irregularities exclude those classified as M/P in Appendix I, while serious procedural irregularities include those.

Table 4: Irregularities and inspections, aggregated by municipality

Variable	Mean	Median	Std. Dev.	Min	Max
Number of inspections (technically service orders)	28.9	27	12.4	2	112
Number of irregularities	59.5	51	40.3	3	402
Number of irregularities/number of inspections	2.21	1.89	1.48	0.07	18.3
Share of inspections with at least one irregularity	0.67	0.67	0.19	0.07	1
# of irregularities/# of inspections with at least one irregularity	3.17	2.84	1.56	1	18.3
Management irregularities	27.3	22	21.7	0	251
Number of management irregularities/number of inspections	0.99	0.84	0.69	0	6.66
Share of inspections with at least one management irregularity	0.45	0.44	0.17	0	1
# of management irregularities/# of inspections with at least one management irregularity	2.06	1.86	0.86	1	7.6
Procedural irregularities	31.3	26	25.5	0	292
Number of procedural irregularities/number of inspections	1.18	1	0.96	0	11.5
Share of inspections with at least one procedural irregularity	0.48	0.5	0.22	0	1
Clear management irregularities	22.2	17	20.0	0	244
Share of inspections with clear management irregularities	0.37	0.35	0.17	0	1
Serious procedural irregularities	36.3	30	29.0	0	320
Share of inspections with serious procedural irregularities	0.53	0.54	0.22	0	1

Notes: N=1,064. The unit of observation is the municipality. Management irregularities include those classified as M/P in Appendix I, while clear management irregularities exclude those. Procedural irregularities exclude those classified as M/P in Appendix I, while serious procedural irregularities include those.

Table 5: Municipality summary statistics (sample means)

	No	Yes				
Audited	No	Yes				
Judiciary seat in 2005		Yes	Yes	No	Yes	No
Single-municipio judiciary district in 2005		Yes	No	No	No	No
Year 2000 population support ('000)		Full	Full	Full	<40	<40
Number of municipalities	4,442	275	304	485	240	481
Year 2000 municipality characteristics						
Municipality population	31,876	41,896	34,744	8,390	18,389	7,823
Judiciary district population	68,150	41,896	50,490	73,865	32,600	7,100
Income per capita	171.8	154.2	191.3	156.4	178.1	156.1
Average years of schooling (25 years and older)	4.05	3.97	4.41	3.76	4.2	3.76
Percentage of residents living in urban areas (%)	58.6	61.9	68.9	52.4	66.1	52.3
Poverty headcount ratio (national poverty line, %)	24.6	29.7	22.7	26.1	24.4	26.1
Poverty gap (%)	49.5	51.6	50.1	50.2	49.7	50.1
Gini coefficient	0.56	0.58	0.58	0.55	0.58	0.55
Radio station (0/1)	0.44	0.59	0.69	0.26	0.65	0.26
Internet access (0/1)	0.22	0.33	0.45	0.08	0.36	0.08
Size of electorate	20,593	26,259	22,514	5,629	12,292	5,315
Electoral turnout	0.87	0.83	0.85	0.87	0.86	0.87
Log distance to state capital	5.29	5.00	5.31	5.34	5.36	5.35
Mayor's party affiliation during 2001-2004 term						
Partido do Movimento Democrático Brasileiro	21.2	19.2	23.3	19.6	24.6	19.8
Partido da Social Democracia Brasileira	19.3	19.5	16.7	19.6	15.0	19.8
Partido do Frente Liberal	18.1	16.2	16.4	19.0	17.5	19.0
Partido Progressista Brasileiro	10.5	5.6	11.0	14.1	11.3	14.0
Partido Trabalhista Brasileiro	7.7	6.7	6.3	5.8	5.3	5.8
Mayor characteristics during 2001-2004 term						
Some higher education (completed or not) (0/1)	0.47	0.48	0.62	0.36	0.58	0.36
Some secondary education (completed or not) (0/1)	0.29	0.35	0.22	0.33	0.24	0.34
Some primary education (completed or not) (0/1)	0.23	0.17	0.15	0.29	0.17	0.29
No formal education (0/1)	0.01	0.00	0.01	0.02	0.01	0.01
Age	49.6	50.9	50.2	48.7	50.0	48.6
Male (0/1)	0.93	0.93	0.93	0.95	0.93	0.95
First-term (0/1)	-	0.65	0.63	0.59	0.63	0.58

Notes: See Section 4.5 for data sources. Only the most important political parties are given in the table.

Table 6: First stage

<u>Dependent variable: judiciary seat (0/1)</u>

Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Maximum population (0/1)	0.80***	0.80***	0.80***	0.73***	0.79***	0.79***	0.73***	0.73***	0.71***	0.78***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.06)	(0.06)
Municipality characteristics	N	N	N	Y	N	N	Y	Y	Y	Y
F-statistic				2.22			2.23	2.25	1.60	1.03
(p-value)				(0.00)			(0.00)	(0.00)	(0.07)	(0.43)
Mayor's party affiliation	N	N	N	N	Y	N	Y	Y	Y	Y
F-statistic					1.37		1.26	0.94	0.94	0.53
(p-value)					(0.13)		(0.20)	(0.52)	(0.54)	(0.92)
Mayor's characteristics	N	N	N	N	N	Y	N	Y	Y	Y
F-statistic						0.91		0.98	0.52	1.00
(p-value)						(0.52)		(0.45)	(0.84)	(0.44)
Mayor's term	Any	Any	Any	Any	Any	Any	Any	Any	First	Second
Observations	721	721	721	718	721	713	718	710	425	285
Percent predicted	94	94	94	94	94	94	94	94	94	94

Notes: OLS estimations. Dependent variable equals 1 if the municipality served as the local seat of the state judiciary branch in 2005 and 0 otherwise. Maximum population equals 1 if the municipality had the largest population in its district based on 2005 population and 0 otherwise. Municipality characteristics: judiciary district population, urban property tax collection, digital records of assets, computerized accounting, computerized budget control, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 7: Impact on the share of inspections with management or procedural irregularities

Dependent variable: share of inspections with at least one irregularity; mean 0.67, std 0.19

Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Judiciary seat (0/1)	-0.038	-0.051**	-0.051**	-0.052*	-0.054**	-0.049**	-0.055*	-0.057*
	(0.023)	(0.024)	(0.024)	(0.029)	(0.025)	(0.025)	(0.030)	(0.030)
Municipality characteristics	N	N	N	Y	N	N	Y	Y
F-statistic				0.57			0.82	0.84
(p-value)				(0.91)			(0.67)	(0.64)
Mayor's party affiliation	N	N	N	N	Y	N	Y	Y
F-statistic					24.6		3.86	3.10
(p-value)					(0.00)		(0.00)	(0.00)
Mayor's characteristics	N	N	N	N	N	Y	N	N
F-statistic						0.50		
(p-value)						(0.87)		
Observations	721	721	721	718	721	713	718	710
R-squared	0.193	0.191	0.191	0.199	0.217	0.197	0.232	0.237

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics: judiciary district population, urban property tax collection, digital records of assets, computerized accounting, computerized budget control, income per capita, average years of schooling, urbanization, poverty headcount ratio, poverty gap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 8: Impact on the share of inspections with management irregularities

Dependent variable: share of inspections with at least one management irregularity; mean 0.44, std 0.16

Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Judiciary seat (0/1)	-0.029	-0.046**	-0.046**	-0.044**	-0.045**	-0.044**	-0.046*	-0.045*
	(0.021)	(0.022)	(0.021)	(0.022)	(0.022)	(0.022)	(0.026)	(0.027)
Municipality characteristics	N	N	N	Y	N	N	Y	Y
F-statistic				0.80			0.95	0.92
(p-value)				(0.68)			(0.51)	(0.55)
Mayor's party affiliation	N	N	N	N	Y	N	Y	Y
F-statistic					103.8		7.44	5.58
(p-value)					(0.00)		(0.00)	(0.00)
Mayor's characteristics	N	N	N	N	N	Y	N	Y
F-statistic						0.53		0.78
(p-value)						(0.85)		(0.64)
Observations	721	721	721	718	721	713	718	710
R-squared	0.184	0.188	0.188	0.200	0.216	0.196	0.232	0.241

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics: judiciary district population, urban property tax collection, digital records of assets, computerized accounting, computerized budget control, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 9: Impact on management or procedural irregularities per inspection

Dependent variable: number of irregularities/number of inspections; mean: 2.09, std: 1.22

Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Judiciary seat (0/1)	-0.110	-0.113	-0.117	0.103	-0.148	-0.128	0.053	0.024
	(0.189)	(0.203)	(0.204)	(0.220)	(0.199)	(0.198)	(0.216)	(0.214)
Municipality characteristics	N	N	N	Y	N	N	Y	Y
F-statistic				0.59			0.62	0.50
(p-value)				(0.89)			(0.87)	(0.95)
Mayor's party affiliation	N	N	N	N	Y	N	Y	Y
F-statistic					541		59.78	30.24
(p-value)					(0.00)		(0.00)	(0.00)
Mayor's characteristics	N	N	N	N	N	Y	N	Y
F-statistic						0.44		0.44
(p-value)						(0.91)		(0.92)
Observations	721	721	721	718	721	713	718	710
R-squared	0.258	0.258	0.260	0.279	0.314	0.260	0.331	0.334

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics: judiciary district population, urban property tax collection, digital records of assets, computerized accounting, computerized budget control, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 10: Impact on the share of inspections with management or procedural irregularities, falsification test

Dependent variable: share of inspections with at least one irregularity; mean 0.66, std 0.19

| Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Second population rank (0/1)	-0.001	0.001	-0.002	-0.004	-0.003	-0.002	-0.006	-0.006
	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)	(0.020)
Municipality characteristics	N	N	N	Y	N	N	Y	Y
F-statistic				0.68			0.71	0.88
(p-value)				(0.82)			(0.75)	(0.60)
Mayor's party affiliation	N	N	N	N	Y	N	Y	Y
F-statistic					1.32		1.37	1.63
(p-value)					(0.18)		(0.15)	(0.06)
Mayor's characteristics	N	N	N	N	N	Y	N	Y
F-statistic						0.91		0.99
(p-value)						(0.52)		(0.45)
Observations	479	479	479	479	479	476	476	473
R-squared	0.179	0.179	0.184	0.200	0.212	0.198	0.228	0.250

Notes: OLS estimations. Second population rank indicates that the municipality has the second-largest population in its judiciary district based on 2005 estimated population. Municipality characteristics: judiciary district population, urban property tax collection, digital records of assets, computerized accounting, computerized budget control, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, top-ranked municipalities in multi-county districts, as well as municipalities with population above 40,000. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 11: Impact on the share of inspections with management or procedural irregularities, first- vs. second-term mayors

Dependent variable: share of inspections with at least one irregularity; mean 0.67, std 0.19

| Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Judiciary seat (0/1)	0.002	-0.007	-0.006	0.003	0.006	-0.010	0.017	0.012
	(0.035)	(0.036)	(0.036)	(0.042)	(0.038)	(0.036)	(0.045)	(0.044)
Judiciary seat (0/1) × First-term mayor (0/1)	-0.066	-0.071	-0.071	-0.101*	-0.089*	-0.062	-0.125**	-0.121*
	(0.047)	(0.049)	(0.049)	(0.059)	(0.051)	(0.049)	(0.063)	(0.064)
F-statistic for H_0: $\beta_1+\beta_2=0$	4.27	5.54	5.62	5.49	5.82	4.70	5.69	5.40
(p-value)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Municipality characteristics	N	N	N	Y	N	N	Y	Y
F-statistic				0.76			0.77	0.87
(p-value)				(0.83)			(0.82)	(0.68)
Mayor's party affiliation	N	N	N	N	Y	N	Y	Y
F-statistic					16.1		4.99	4.52
(p-value)					(0.00)		(0.00)	(0.00)
Mayor's characteristics	N	N	N	N	N	Y	N	Y
F-statistic						0.55		0.59
(p-value)						(0.92)		(0.87)
Observations	721	721	721	718	721	713	718	710
R-squared	0.202	0.200	0.200	0.217	0.244	0.210	0.263	0.272

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. β_1 is the coefficient on judiciary seat among second-term mayors. β_2 is the difference between coefficients on judiciary seat in the first- and second-term mayor samples. Municipality characteristics: judiciary district population, urban property tax collection, digital records of assets, computerized accounting, computerized budget control, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

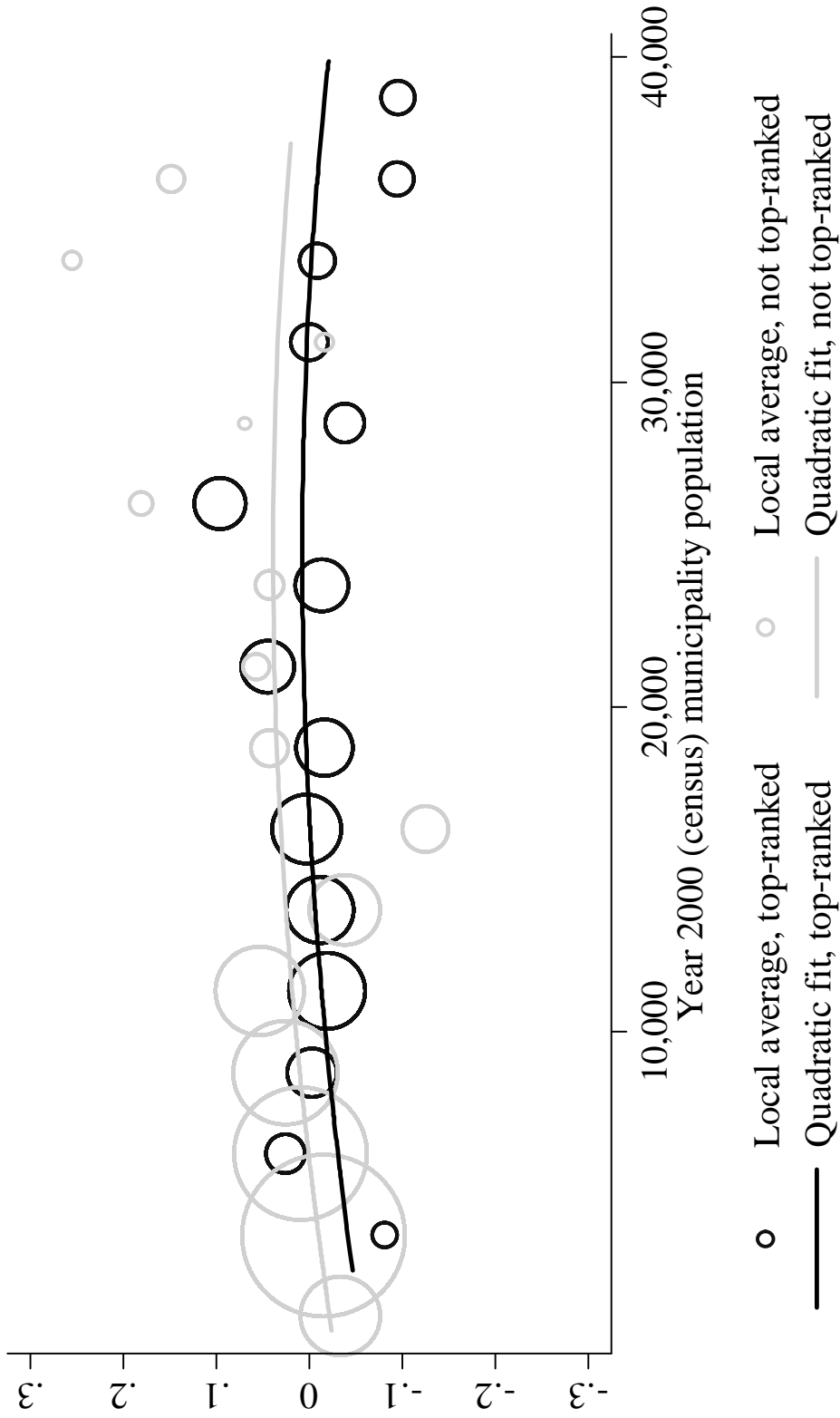Figure 1: Histograms of municipality population by population rank
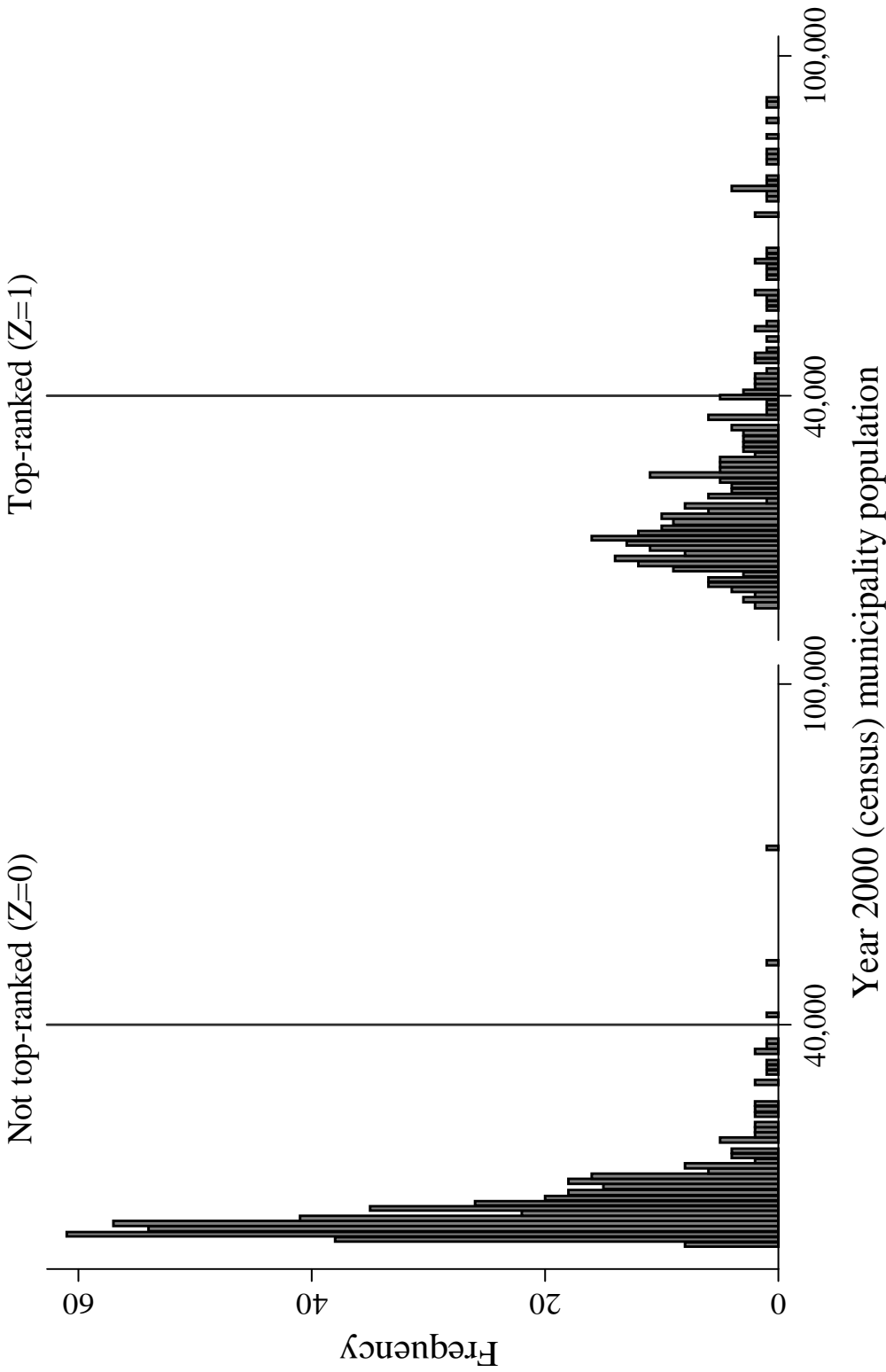
Notes: Z indicates judiciary-district-specific top rank in terms of official 2005 population. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 100,000.

Figure 2: Graphical first stage
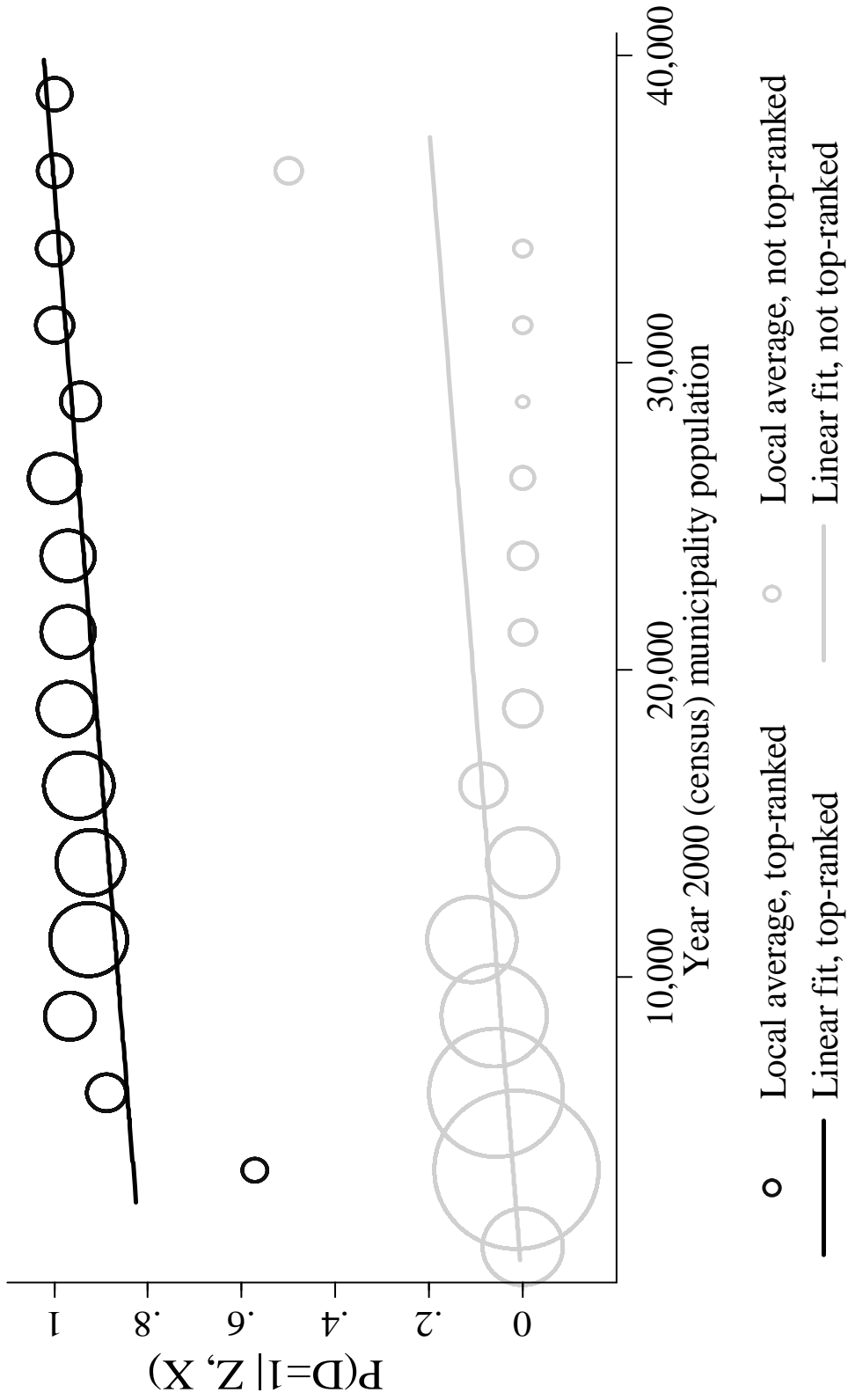
Notes: The dependent variable on the vertical axis is the indicator for judiciary seat. Each dot corresponds to the sample proportion of municipalities that are judicial seats for a given judiciary-district-specific population rank (top or not top) based on official 2005 population and in a given bin. The size of each dot is proportional to the number of municipalities in a given bin. The bin width is 2,500.

Figure 3: Impact on the share of inspections with management or procedural irregularities, reduced form

Year 2000 (census) municipality population

○ Local average, top-ranked
── Quadratic fit, top-ranked

○ Local average, not top-ranked
── Quadratic fit, not top-ranked

Notes: The dependent variable on the vertical axis is the share of inspections with at least one irregularity in deviation from the state average. Each dot corresponds to the sample average of the dependent variable for a given judiciary-district-specific population rank (top or not top) based on official 2005 population and in a given bin. The size of each dot is proportional to the number of municipalities in a given bin. The bin width is 2,500.

9 Appendix I

Auditor classification of irregularities	%	Corruption codings[a]		
		LZ	FF	BNPT
Civil society oversight of government programs	4.97			
- non-existent civil society council	10.32	P		
- ineffective/non-existent oversight	70.93	P		
- irregular composition of oversight council	9.94	P		
- evidence of council capture by mayor	1.33	P		
- no meeting records	0.35	P		
- formal errors	7.13	P		
Quality and timeliness of financial reporting	12.88			
- irregular/non-existent financial report	66.34	P		
- irregular/non-existent receipts	29.04	P	C[b]	M
- delayed reporting	4.62	P		
Financial program and project management	3.58			
- emission of checks without justification	7.88	M/P	C	
- excess cash holdings (opportunity cost)	43.71	M/P		
- unjustified payment of bank fees	1.40	M/P		
- irregular account management	36.57	P		
- spending without appropriation	4.51	P		
- failure to return residual project funds	5.39	P		
- premature withdrawal of funds	0.53	P		
Procurement for programs and projects	15.01			
- simulated tender process	3.05	M	C[c]	C
- evidence of favoritism	4.11	M	C[c]	C
- invitation for bids to less than three firms	7.79	M/P	M	M
- non-selection of the lowest bid	2.07	M		
- evidence of price collusion	0.62	M/P		
- inappropriate procurement modality (restricted competition)	4.60	M/P		M
- unjustified direct purchase (no competition)	7.50	M/P		
- irregular composition/capture of the procurement commission	1.90	M/P		
- participating ineligible firm	9.12	M/P		M
- fractionalizing of procurement amounts	4.11	M/P		C
- absence of preliminary price survey	6.58	P		
- inadequate publication of the call	4.38	P		
- incomplete specification of the call	3.90	P		
- inadequate publication of results	2.05	P		
- tender process without funding	0.07	P		
- formal errors	35.61	P		
Social security contribution collection	1.74	P		

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[a] LZ: Litschig and Zamboni, FF: Ferraz and Finan, BNPT: Brollo, Nannicini, Perotti, and Tabellini, P: Procedural, M: Management/Mismanagement, C: Corruption
[b] Only if "community members confirm that the goods were in fact not delivered".
[c] Only if "the public good was not provided".

Auditor classification of irregularities	%	LZ	FF	BNPT
Execution of programs and capital projects	36.54			
- unjustified or excessive payments for goods and services	7.82	M	C	C
- project not implemented	9.29	M		
- partial project execution	4.78	M		
- substandard project execution	26.01	M	M	
- inadequate project inputs	1.70	M	M	
- diversion of project resources	9.68	M	M	M
- time overruns	0.49	M/P		
- project delays	2.00	M/P		
- project on hold	1.05	M/P		
- inadequate infrastructure to run program	5.29	P		
- lacking oversight of project implementation	5.72	P		
- irregular sub-contracting	0.12	P		
- irregular change of work plan	2.03	P		
- irregular project documentation	12.05	P		
- matching grant requirements are not met by local governments	3.26	P		
- staff members have inadequate training	3.75	P		
- irregular contract	0.47	P		
- late payment to suppliers	0.14	P		
- failure to notify community of resource receipt	2.77	P		
- formal errors	1.58	P		
Inventory and equipment management	13.56			
- inventory or equipment unaccounted for	49.43	M		
- irregular sale of inventory or equipment	1.28	M		
- unusable or only partially usable equipment	6.91	M		
- non-existent equipment utilization control	4.26	P		
- non-existent inventory control	15.31	P		
- inadequate equipment/inventory maintenance	12.90	P	M	
- inappropriate use of equipment	4.26	P		
- inappropriate political propaganda	0.42	P		
- equipment without appropriate label	5.23	P		
Remittance management	10.26			
- irregular fees/other requirements to obtain benefits	6.04	M		
- remittance to ineligible individuals	17.84	M		
- benefit not remitted	4.65	M		
- partial remittance	1.06	M		
- duplication of remittance	5.00	M		
- program beneficiary not found	10.34	M		
- delayed remittances	5.17	M	M	
- non-existent school attendance verification	7.27	M		
- number of beneficiaries below target	2.83	M		
- irregularities in the payment process	5.34	P		
- incomplete register of beneficiaries	33.32	P		
- costly access to obtain benefits	1.10	P		
- formal errors	0.03	P		
Other irregularities	1.46			