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Abstract

This paper estimates 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. 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. Instrumental variable estimates suggest that state judiciary presence reduces the share of inspections with irregularities related to waste or corruption by about 10 percent. The effect is concentrated among first-term mayors, suggesting that judicial presence operates through an increased probability of detection and prosecution rather than an increased probability of conviction, which should discipline second-term mayors as well.

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

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1 Introduction

Judicial checks on executive (and legislative) power are enshrined in constitutions around the world. Cross-country comparisons have shown that higher judicial independence is positively correlated with measures of political and economic freedom (La Porta et al., 2004), while the results for economic growth are mixed (Feld and Voigt, 2003; Glaeser et al., 2004).[1] Evidence from state governments in the U.S. suggests that corruption is generally lower in states with higher judicial independence, as measured by whether state supreme court judges are elected rather than appointed for example (Alt and Lassen, 2008; Cordis, 2009). As is well known, however, the survey and corruption convictions data used in these studies reflect at least in part variation in enforcement across states (Alt and Lassen, 2014). Moreover, measuring judicial independence is inherently difficult (Ríos-Figueroa and Staton, 2013) and identification is generally challenging because institutional variation often reflects a collective choice.

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.[2] Rather than evaluating judiciary independence as in existing studies, we focus instead on the conceptually simpler physical *presence* of state judicial institutions in the local community.[3] State-level prosecutors and judges provide the checks on local officials within their entire jurisdictions but they 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 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.[4]

[1] 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. Glaeser et al. (2004) measure independence of the judiciary based on term-length of judges and find no relationship with economic growth. Whether institutions that constrain executive power more generally are beneficial for economic development is unclear. 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, Alvarez, Cheibub and Limongi (2000), Glaeser, La Porta, López-de-Silanes and Shleifer (2004), Przeworski (2004a, 2004b) and Glaeser and Saks (2006). Pande and Udry (2005) provide a comprehensive survey of the entire literature on institutions and development.

[2] 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.

[3] 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".

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

Theoretically, we think of judicial presence as a factor that deters rent extraction by local incumbent politicians and public servants through increased odds of detection and prosecution.[5] 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 in local public service delivery. In line with this argument, we estimate that judicial presence reduces the travel distance from the municipality town hall to the judiciary seat by about 40 kilometers on average. Similarly, local officials may perceive a higher probability of punishment when the state prosecutor lives in town because he is more familiar with the setting and faces lower transaction costs for his investigations.[6] Alternatively however, local elites might also find it easier to capture state judiciary officials when they reside in the same municipality, which would presumably lower the probability of punishment and increase the incidence of infractions.[7] The effect of judicial presence on rent extraction by local public agents may thus work through several 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.[8] 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. 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 percent

[5] 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.

[6] 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.

[7] 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.

[8] Lei Complementar N^o 35, de 14 de Março de 1979, Art. 95-97.

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. New judiciary district formation is typically accompanied by municipality splitting and tends to respect prior district boundaries. Although state laws usually do not specify how to select 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.[9]

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. Since new district formation is constrained by contiguity requirements typically within prior district boundaries, the potential for selective groupings of municipalities is limited in practice. Moreover, since within-district population rank does not guarantee judicial presence, any potential lobbying is likely about becoming the seat county directly, rather than about having top population rank within the district. Our instrumental variable (IV) approach explicitly allows for the possibility that judicial presence is endogenous—even conditional on population—since we only use variation in judicial presence induced by district-specific population rank. More formally, the approach relies on three main identifying assumptions. First, conditional on population, district maximum population must be mean independent of unobserved factors that affect outcomes (conditional independence). Second, district maximum population can affect rent extraction only through local judicial presence, not through other channels (exclusion restriction). Third, the probability of having a judiciary presence in the municipality must be 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 with our data).

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 we refer to as the random audits program. In contrast to survey- or convictions-based corruption

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

measures used in prior work on judicial independence, our outcome measures are not mechanically higher in places where local enforcement efforts are high. 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 to 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.[10] 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).[11]

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.[12] The first difference is that we focus on a broader set of irregularities, rather than likely instances of corruption. After all, corruption is only one type of rent extraction.[13] Moreover, corruption represents only a small fraction of irregularities uncovered by auditors as shown in Ferraz and Finan (2011).[14] However, we 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. In addition to inevitable ambiguities in the identification of corruption

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

[11] 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.

[12] Zamboni and Litschig (2018) 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.

[13] 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.

[14] 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.

episodes, our main reason for focusing on management 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 more comprehensive measure of rents is better suited for our purposes. The second difference with other codings of the Brazilian municipal audit reports is that we focus on the share of inspections with at least one irregularity rather than the number of irregularities per inspection. As further discussed below, the existence of irregularities is likely measured more accurately than the number of irregularities since auditor discretion in reporting plays much less of a role. In our data, 35 percent of all inspections come up entirely clean—without any evidence of procedural or management irregularities—while a full 55 percent show 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. While we show that judicial presence reduces irregularities overall, the effect turns out to be driven exclusively by a reduction of irregularities indicating waste or corruption. The null result for procedural irregularities is consistent with the intuition that less serious infractions are less likely to be reported 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. A further breakdown among management irregularities shows that the effect is entirely driven by those that are visible to the general public, such as substandard or delayed project execution, rather than unjustified or excessive payments for goods and services for example. As further discussed below, these results are robust to the inclusion of a long list of standard and context-specific controls, such as the population size of the judiciary district. We also show that population rank per se has no effect on any type of irregularity by comparing second- to lower-ranked municipalities, conditional on population.

In order to shed additional light on mechanisms, we investigate whether the impact of local presence of the judiciary on rent extraction depends on the mayor's re-election incentives. This is the main focus of Ferraz and Finan (2011) who show that mayors in their first term in office are less corrupt compared to mayors in their second term. Although these authors do not attempt to identify the causal effect of local judiciary presence, 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 effect of local judiciary presence.[15] 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. Importantly, this differential effect by mayoral term does not reflect differences in average income or education of the population or the presence of media across municipalities with first- versus second-term mayors. Together with the fact that the reduction of irregularities is concentrated among those that are visible to the general public, these results suggest that judicial presence operates through an increased probability of detection and prosecution, disciplining mayors with re-election incentives. This interpretation is consistent with evidence for Brazil showing that the revelation of corruption does have electoral consequences (Ferraz and Finan, 2008; Winters and Weitz-Shapiro, 2013).[16] If instead judicial presence increased the probability of conviction, we should see an effect for second-term incumbents as well. The null result for second-term mayors is in line with the fact that actual convictions of local officials were very rare events in Brazil during our sample period (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. Section 4 presents our dataset on irregularities in local public management. Section 5 discusses our estimation approach. Results are presented in Section 6. We conclude with a discussion of limitations and extensions.

[15]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.

[16]In general, the extent to which voters in Latin America punish corrupt politicians may depend on economic conditions (Zechmeister and Zizumbo-Colunga, 2013; Manzetti and Rosas, 2015).

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.[17] 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 percent of all counties in 2000) that have been audited through June 2006.[18] 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 percent 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

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

[18] 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.

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 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 for detected irregularities. If the auditors concede that there are valid arguments for non-compliance, we exclude these instances from our measures.

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.[19] Because the courts cannot initiate proceedings on their own, prosecutors play a key role in the criminal justice system.[20]

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.

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.[21] 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.[22] Table 1 gives details for each state.

Our research design is necessarily silent on the causal effect of judicial presence for single-

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

[20] 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.

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

[22] 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.

county districts since we lack information about the assignment rule in these cases.[23] However, roughly 75 percent 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 prosecutors and judges. New judiciary district formation is typically preceded by municipality splitting and tends to respect prior district boundaries. For example, municipalities Nova Bandeirantes and Nova Monte Verde both split away from Alta Floresta in Mato Grosso state in 1993 and became their own judiciary district in 2004. Nova Monte Verde became the seat with slightly larger population. Similarly, and also in Mato Grosso state, Ilha Solteira and Suzanapolis municipalities became independent from Pereira Barreto in 1993 but stayed members of the old judiciary district. None of the spin-off municipalities became the judiciary seat. This is not to say that prior district boundaries are always respected. For example in Minas Gerais state, Curral de Dentro and Divisa Alegre both split away from Aguas Vermelhas in 1997 and got incorporated into other existing judiciary districts. Nonetheless, our analysis suggests that new district formation is typically done within prior district boundaries.

State laws usually 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 this 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.

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

[23]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^o 35, de 14 de Março de 1979, Art. 97, 2nd paragraph).

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 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 percent) 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

Ideally, we would 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 in prior years.

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. More formally, let Y denote the outcome variable (share of inspections with at least one irregularity), 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 effect of judicial presence is constant, we can write the outcome and first stage equations as follows:

$$\begin{aligned} Y &= \beta_D D + \beta_X X + \beta_Z Z + U \\ D &= \pi_Z Z + \pi_X X + V \end{aligned}$$

We write a linear specifications for X in the outcome and first stage equations only for simplicity. In practice we 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:

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

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.

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. 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 size. 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 $E(U|Z,X) = E(\beta_W W + U'|Z,X) = \beta_W E(W|Z,X) + E(U'|Z,X)$. As long as $E(U'|Z,X) = E(U'|X)$ and $E(W|Z,X) \approx E(W|X)$ or $\beta_W \approx 0$, conditional mean independence will hold. We show below that although district size, urbanization and distance to the state capital are indeed correlated with being the top-ranked municipality in the district, the correlations

are either small in magnitude or the effects of these confounders on outcomes are close to zero, once we control for municipality population. We also show that estimates of the effect of judicial presence are robust to including more standard controls such as income per capita and average education of the local population (Glaeser and Saks, 2006), ease of access to information (Reinikka and Svensson, 2005), proxied by the presence of a local radio station and internet access, voter turnout (Zingales, 2004), and measures of local government capacity, such as whether there are digitized records of assets and whether accounting and budget control are computerized. Results are also robust to the inclusion of mayor party affiliation indicators and other mayor characteristics, such as age and whether the mayor has been re-elected as in Ferraz and Finan (2011). In addition, our controls include the number of civil servants as well as own revenue, federal and state transfers and total municipal spending, all in per capita terms, in order to account for the size of the local administration.

Of course there might be remaining unobserved factors that are correlated with population rank and that also have an effect on outcomes, even conditional on population. For example, judiciary district formation could 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 several reasons. First, since new district formation is constrained by contiguity requirements typically within prior district boundaries, the potential for selective groupings of municipalities is limited in practice. Second, top population rank within a district does not guarantee judicial presence, 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 within 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. Third, if district formation were indeed endogenous, this would likely show up in sizeable 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 operates might be through a higher presence of state police, which might reduce the cost of reporting malfeasance in the local administration.[24] 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

[24] 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.

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 74 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.[25] 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.[26]

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 indicated by the first stage of 74 percentage points—the estimated local effect might be fairly representative of the average effect among small- to medium-sized municipalities.

4 Data

The first subsection presents our measures of rent extraction in more detail. The second subsection discusses potential systematic measurement error. The last subsection summarizes the data on other municipality characteristics.

[25] 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.

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

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 percent of all counties in 2000) that have been audited through June 2006.[27] Our dataset is at the level of the inspection order and contains the year when the audited transaction was made, the amount involved in the audited program or project, as well as detailed audit findings. Following the practice of the comptroller general's office, we refer to the reported infractions of public sector management regulations as irregularities. 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.[28]

The violations reported by auditors range from improper financial reporting to lack of oversight in project implementation to waste and actual theft of public resources. The following 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.[29]
>
> 2) 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.[30]
>
> 3) 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.[31]

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

[27] 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.

[28] 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.

[29] 9th lottery, Salgado de São Félix municipality, Paraíba state, Primary and Preventive Health Care Program.

[30] 10th lottery, Farias Brito municipality, Ceará state, Programa Esporte Solidário.

[31] 11th lottery, Abaetetuba municipality, Pará state, Programa Brasil Alfabetizado.

often do reflect bad public management.[32] 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 they were not exerting enough effort on their job. They diverted public funds intended for health service providers, failed to oversee project implementation by contractors, which led to sub-standard project execution, and circumvented procurement procedures that are privately costly to carry out. 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.[33] As a result, auditors themselves deliberately abstain from labeling 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 as discussed in Section 2.2.

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 officials 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. We also subdivide management irregularities into those that are likely visible to the general public, such as substandard or delayed project execu-

[32] 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).

[33] 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 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.

tion, and those irregularities that are likely only revealed by a formal audit, such as unjustified or excessive payments for goods and services or favouritism in contract awarding. 1) and 2) above are examples of diversion of program resources and substandard project execution, respectively, which we classify as visible management irregularities. 3) is an unjustified direct purchase, which we code as a nonvisible management irregularity. In appendix I below we enumerate all types of irregularities as they are reported by auditors, as well as our own classification into procedural, visible and nonvisible management varieties. In the online appendix we discuss how our measure of rents relates to existing corruption and mismanagement codings by Ferraz and Finan (2011) and Brollo, Nannicini, Perotti and Tabellini (2013).

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.

Since judicial presence varies at the level of the municipality, we construct our main outcome as the share of inspections with at least one irregularity of a given type. We discard information on the number of irregularities per inspection because of likely 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. In fact, 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 and we find indeed that standard errors are an order of magnitude larger when the count of irregularities is used in the numerator. Results are available on request. 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.

4.2 Systematic measurement error

A first caveat related to systematic measurement 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 second 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 not all irregularities are easy to conceal. While nonvisible irregularities, such as favouritism in contract awarding, may potentially go unnoticed even by auditors, visible irregularities, such as substandard project execution or insufficient front-line staff at the health post are much harder to hide from the public. Last but not least, 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 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.

4.3 Data on county characteristics

Data on county characteristics come from several sources. We obtained the composition of 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 number of municipal civil servants, presence of a radio station, internet access, the extent of digitized records of municipal assets and computerized accounting and budget control. Official local population data for the years 2000 and 2005 are also from IBGE.[34] Data on local income distribution, schooling, 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*). Finally, we extract total municipal spending, own revenue and state and federal transfers received by the municipality from the *Finanças do Brasil* compiled by the Brazilian national treasury. Especially in the data on local government budgets there are some missing values. In each of these cases we impute the corresponding sample average in order not to lose the entire observation. There are 42 observations with at least one imputed covariate value in the estimation sample. Our regressions always include a dummy for whether an imputation took place.

[34] 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).

5 Estimation approach

We use an indicator for district-specific top population rank as an instrument for judicial presence. Since population size of the municipality is a key confounder, it is crucial for our approach to adequately control for population. 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. 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. We also control for population nonparametrically with a set of indicators for population within bins of width 2,500. All estimations 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. 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 lose districts without variation in the instrument (recall that we have outcome measures only for audited municipalities, not for all municipalities within a given district).

We also investigate whether the impact of judicial presence depends on the mayor's re-election incentives. In order to account for differences in average income or education of the population or the presence of media across municipalities with first- versus second-term mayors, we run instrumental variable regressions with covariate interactions. We demean the covariates so that the coefficient on judiciary presence retains its interpretation as an average effect. Demeaned covariates are interacted with the instrument in the first stage and with judiciary seat in the second stage.

6 Results

6.1 Covariate balance conditional on population

Table 3 shows the correlation between judicial presence, top population rank within the district and 20 covariates, both unconditionally and conditional on population. The sample consists of 721 municipalities that were audited through round 21, excluding single-county judiciary districts, as well as municipalities with population above 40,000. Column (1) gives the mean and standard deviation of each covariate among municipalities without judicial presence. Column (2) shows the difference in means compared to municipalities with judicial presence and in parentheses the corresponding standard error. Columns (3) and (4) show a similar comparison but by population rank within the judiciary district, that is, top-ranked versus not-top-ranked. There are sizeable differences in the raw sample means in both columns (2) and (4), making it clear that controlling for population is crucial for our approach.

Column (5) shows the adjusted difference estimate, including dummies for population within bins of width 2,500 as well as state fixed effects. The estimate for municipality population in the first row is 120, suggesting that the nonparametric approach successfully eliminates the difference in average population of almost 11,000 inhabitants between top- and not-top-ranked municipalities shown in column (4). Other covariates still exhibit sizeable differences by population rank however, even controlling for population. For example, top-ranked municipalities are part of judiciary districts with 77,000 fewer inhabitants on average, compared to not-top-ranked municipalities. While this difference seems large, it is a bit difficult to interpret because it does not account for the fact that larger districts tend to have more courts and prosecutors. Similarly, top-ranked municipalities are about 44 kilometers further away from their respective state capital on average, compared to an average distance of 266 kilometers among not-top-ranked municipalities. Whether conditional on municipality population, the size of the judiciary district or distance to the state capital matter for rent-taking are empirical questions which we investigate below.

The remaining adjusted difference estimates are rather small in magnitude, even though some are statistically clearly different from zero. For instance, income per capita, average years of schooling, the proportion living in urban areas, the size of the electorate and the number of mu-

nicipal civil servants are only about 9 percent higher in top-ranked municipalities, conditional on population. Similarly, potentially important determinants of rent-taking, such as federal and state transfers and total municipal spending per capita are only about 9 percent lower in top-ranked municipalities, conditional on population. Overall, there are thus few sizeable observable differences between top- and lower-ranked municipalities, once we condition on population. Moreover, as shown below, these differences do not seem to matter much for outcomes, conditional on population, as the estimate of interest is essentially unchanged when we include the entire list of observables from Table 3 plus a set of dummies for the mayor's party affiliation.

6.2 First stage

Table 4 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. All regressions include state fixed effects. Estimates with linear, quadratic, cubic and nonparametric population controls are shown in columns (1), (3), (5) and (7) respectively. Columns (2), (4), (6) and (8) show the corresponding estimates when all the controls from Table 3 plus a set of dummies for mayoral party affiliations are also included. The first stage estimates for the full sample in Panel A suggest that without additional controls, the probability of having a local presence of the judicial apparatus increases by about 80 percentage points if the county is largest in its district, for counties with the same population size. Some of the additional controls are slightly correlated with the instrument since the point estimates with controls decrease to about 74 percentage points. The implied first stage F-statistic is $t^2 = (0.74/0.04)^2 = 342$, well beyond conventional critical values for the weak instrument test based on TSLS size (Stock and Yogo 2005). Panels B and C of Table 4 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.

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 municipal-

ities 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 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.

6.3 Impact on the share of irregular inspections

Table 5 shows instrumental variable estimates of the effect of judicial presence using the share of inspections that turn up at least one irregularity as the dependent variable. Panel A shows results for any irregularity, while Panels B and C show estimates for management and procedural irregularities, respectively. Point estimates for the share of inspections with at least one irregularity are about -4 percentage points in the linear and nonparametric specifications and about -5 percentage points in the quadratic and cubic specifications. Six out of eight estimates are statistically different from zero at the 5 or 10 percent level. Turning to management irregularities in Panel B, the results suggest that the overall reduction in the share of inspections with irregularities is driven entirely by management irregularities. Point estimates are only slightly smaller than in Panel A and four out of eight estimates are significant at 5 or 10 percent. Compared to the mean share of inspections with management irregularities of 0.45, the effect of judicial presence on rent extraction amounts to about -10 percent.

Panel C shows that for procedural irregularities, effect estimates are essentially zero. This result is consistent with the intuition that less serious infractions are less likely to be reported by the public and prosecuted by the judiciary. The null result for procedural irregularities 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.

Another important feature of the results in Table 5 is that the effect size is remarkably robust to the inclusion of all the municipality and mayor characteristics from Table 3 as well as mayor party affiliation dummies. At the same time, the R-squared increases by about 5 percentage points,

going from 19 percent to 24 percent in most cases.[35] Together, the unchanged estimates and increased fit with covariates beyond population imply both that the covariates that are correlated with the instrument (such as judiciary district size or distance to the state capital) are not relevant predictors of the outcome at the margin and that those that are relevant (such as mayor party affiliation dummies) are not correlated with the instrument, conditional on population. Overall, these results thus provide supporting evidence for the conditional mean independence assumption.

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 3 to 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 10 out of 14 bins where both top- and not-top-ranked municipalities are observed. Figure 1 in the online appendix presents a similar picture of the reduced form relationship between population rank and the share of inspections with irregularities related to waste or corruption, conditional on population.

6.4 Visible versus nonvisible irregularities

Panel A of Table 6 shows impact estimates for the share of inspections with visible management irregularities, such as diversion of project resources or substandard project execution. The quadratic, cubic, and nonparametric specifications for population suggest that judicial presence reduces the share of inspections with visible management irregularities by about 4 to 5 percentage points. The estimates are statistically different form zero at 5 or 10 percent in 5 out of 8 specifications. In contrast, the estimates in Panel B show that judicial presence has no effect on the share of inspections with nonvisible irregularities, such as unjustified or excessive payments for goods and services or favouritism in contract awarding. Point estimates are all negative but close to zero and none are

[35] 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.

statistically significant. These results are consistent with the view that judicial presence deters rent-taking through reduced transaction costs for the general public to report irregularities in local public service delivery to prosecutors.

6.5 Falsification test of the exclusion restriction

Table 7 reports estimates of a falsification test that compares our measures of rent extraction between municipalities that are second-ranked in their district and those that are lower-ranked, conditional on population. 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. Results in Panel A show that the share of inspections with irregularities is no different between municipalities that are second-ranked in their district and those that are lower-ranked, conditional on population. Most of the estimates are an order of magnitude smaller than the effect estimates of judicial presence discussed above and they are nowhere near statistical significance. The same is true for management and procedural irregularities, shown in Panels B and C, respectively. These result increase our confidence in the exclusion restriction, that is, other than through judicial presence, top population rank within the district has no effect on rent extraction.

6.6 Impacts by mayoral term

In Table 8 we test whether the effect of local presence of the judiciary on rent extraction depends on the mayor's term in office. In order to account for differences in average income or education of the population or the presence of media across municipalities with first- versus second-term mayors, we run instrumental variable regressions with covariate interactions. Since we are interested in the differential effect of judicial presence by mayoral term, we only report estimates of the average effect and the relevant interaction term. The average effect of judicial presence is slightly larger than in Table 5 but because standard errors increase substantially, none of the estimates are significant. More importantly however, the estimates on the interaction term with a dummy for first-term mayors range between -5 to -6 percentage points and are significant at 10 percent in 4 out of 8 specifications. Since judicial presence reduces the share of inspections with irregularities

more strongly for first-term mayors compared to second-term mayors, these results suggest that judicial and political accountability complement each other. In fact, if we focus on the nonparametric estimates with controls in column (8), the estimated effect of judicial presence on the share of irregular inspections is about $-.038 - 0.064 \times 0.4 = -6.4$ percentage points among first-time mayors. In contrast, for mayors in their second term, local judicial presence does not seem to matter at all ($-.038 + 0.064 \times 0.6 = 0$). Overall, these results indicate that judicial presence operates through an increased probability of detection and prosecution of irregularities, which disciplines incumbents with re-election incentives, rather than an increased probability of conviction, which should also discipline incumbents without re-election incentives.

6.7 Additional robustness checks

Table 1 in the online appendix presents results for the share of irregular inspections based on corruption and mismanagement codings by Ferraz and Finan (2011) and Brollo, Nannicini, Perotti and Tabellini (2013). Effect estimates of judicial presence are negative but attenuated compared to our coding and not statistically different from zero. As discussed in more detail in the online appendix, the effect attenuation comes from the fact that these other codings exclude certain management irregularities, such as project delays or partial and delayed remittance of cash transfers to eligible individuals, which in fact do respond to judicial presence. Moreover, these codings include irregularities in procurement, which we argue are not easily observed by voters and thus are not expected to respond to the presence of the judiciary apparatus. Table 2 in the online appendix shows that judicial presence also reduces the share of audited funds with at least one irregularity although impact estimates are again not statistically different from zero. Table 3 in the online appendix confirms that the irregularity reduction is robust to using population rank in different years—rather than rank in 2005—as the instrument for judicial presence in 2005. Finally, online appendix Table 4 shows that when we replace judicial presence with travel distance to the judiciary seat, the results are qualitatively unchanged. Specifically, we estimate that a travel reduction of 50 kilometers reduces the share of inspections with irregularities by about 5 percentage points and that this reduction is again driven exclusively by management, rather than procedural irregularities.

7 Conclusion

This paper provides evidence on the role of the territorial organization of the judiciary in constraining rent extraction by the local 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. A further breakdown among management irregularities shows that the effect is entirely driven by those that are visible to the general public, such as substandard or delayed project execution, rather than unjustified or excessive payments for goods and services for example. In addition, our results suggest that judicial presence operates through an increased probability of detection and prosecution, which disciplines incumbents with re-election incentives, rather than an increased probability of conviction, which should also discipline incumbents without re-election incentives.

Given that about 75 percent 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) 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).

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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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: Summary statistics and covariate balance

	Comparison by judiciary seat status		Comparison by population rank within judiciary district		
	Non-seat	Diff.	Not-top	Diff.	Adj. Diff.
	(1)	(2)	(3)	(4)	(5)
Municipality population (,000)	7.82	10.53***	7.72	10.76***	0.12
	[5.85]	(0.65)	[5.91]	(0.63)	(0.08)
Judiciary district population (,000)	71.59	-40.60***	71.62	-40.37***	-77.11***
	[116.80]	(5.46)	[117.03]	(5.49)	(16.89)
Income per capita	155.50	22.49***	156.88	18.20***	13.97***
	[81.80]	(6.95)	[81.55]	(6.98)	(5.22)
Average years of schooling	3.75	0.47***	3.78	0.38***	0.35***
	[1.11]	(0.09)	[1.11]	(0.09)	(0.07)
Proportion living in urban areas	0.52	0.14***	0.53	0.12***	0.06***
	[0.23]	(0.02)	[0.23]	(0.02)	(0.02)
Income Gini coefficient	0.55	0.02***	0.55	0.02***	0.01
	[0.06]	(0.00)	[0.06]	(0.00)	(0.01)
Size of electorate (,000)	5.32	6.94***	5.29	6.96***	0.50***
	[3.54]	(0.42)	[3.65]	(0.41)	(0.17)
Turnout	0.88	-0.02***	0.88	-0.02***	0.01
	[0.06]	(0.00)	[0.06]	(0.00)	(0.01)
Mayor age	48.60	1.41**	48.88	0.55	-1.19
	[9.38]	(0.69)	[9.38]	(0.69)	(1.02)
First-term mayor	0.59	0.04	0.58	0.05	0.07
	[0.49]	(0.04)	[0.49]	(0.04)	(0.05)
Distance to state capital (km)	267.82	-10.43	266.46	-6.30	43.90***
	[163.95]	(12.24)	[162.64]	(12.34)	(16.16)
Radio station	0.26	0.39***	0.26	0.37***	0.17***
	[0.44]	(0.04)	[0.44]	(0.04)	(0.05)
Internet access	0.08	0.28***	0.07	0.30***	0.13***
	[0.27]	(0.03)	[0.25]	(0.03)	(0.04)
Number of municipal civil servants	274.84	279.36***	272.60	283.70***	26.90*
	[192.24]	(22.68)	[189.13]	(22.54)	(14.49)
Digital record of municipal assets	0.48	-0.01	0.47	-0.01	0.06
	[0.50]	(0.04)	[0.50]	(0.04)	(0.05)
Computerized accounting	0.86	0.02	0.87	0.00	-0.01
	[0.34]	(0.03)	[0.34]	(0.03)	(0.03)
Computerized budget execution	0.77	0.03	0.78	-0.01	-0.03
	[0.42]	(0.03)	[0.41]	(0.03)	(0.04)
Own revenue per capita	49.27	-23.62***	46.43	-14.99**	-19.29
	[136.16]	(6.67)	[130.19]	(7.46)	(12.19)
Federal transfers per capita	485.35	-256.59**	481.97	-244.44**	-45.77
	[2,355.07]	(107.64)	[2,359.50]	(108.23)	(75.41)
State transfers per capita	239.78	-101.85***	233.23	-81.56***	-13.43
	[541.79]	(25.81)	[538.12]	(26.67)	(25.30)
Total spending per capita	835.68	-402.83***	820.55	-354.62***	-72.66
	[2,892.09]	(132.70)	[2,892.50]	(134.33)	(101.06)

Notes: OLS estimations based on 721 municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Variables are measured in 2000. Columns (1) and (3) show sample means and in brackets standard deviations for non-judiciary seat and not-top-ranked municipalities, respectively. Columns (2) and (4) show the corresponding difference in mean estimates compared to judiciary seat and top-ranked municipalities, respectively. Column (5) shows the adjusted difference estimate that controls for state fixed effects and dummies for population within bins of width 2,500. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 4: First stage effect of district top-population rank on judicial presence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Panel A: full sample (721 municipalities); dependent variable is judiciary seat (0/1)								
Maximum population (0/1)	0.797***	0.746***	0.796***	0.739***	0.797***	0.739***	0.799***	0.740***
	(0.034)	(0.037)	(0.034)	(0.039)	(0.034)	(0.039)	(0.034)	(0.040)
R-squared	0.77	0.79	0.77	0.79	0.77	0.79	0.77	0.80
Panel B: first-term mayors (433 municipalities); dependent variable is judiciary seat (0/1)								
Maximum population (0/1)	0.787***	0.727***	0.790***	0.721***	0.790***	0.720***	0.803***	0.737***
	(0.043)	(0.050)	(0.043)	(0.053)	(0.043)	(0.054)	(0.043)	(0.054)
R-squared	0.78	0.81	0.78	0.81	0.78	0.81	0.79	0.82
Panel C: second-term mayors (288 municipalities); dependent variable is judiciary seat (0/1)								
Maximum population (0/1)	0.831***	0.788***	0.826***	0.776***	0.827***	0.780***	0.820***	0.777***
	(0.050)	(0.057)	(0.051)	(0.061)	(0.051)	(0.061)	(0.054)	(0.064)
R-squared	0.76	0.79	0.76	0.79	0.76	0.80	0.77	0.80

Notes: OLS estimations. The sample consists of municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. 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. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 5: Impact on the share of inspections with management or procedural irregularities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Panel A: dependent variable is the share of inspections with at least one irregularity; mean 0.67, std 0.19								
Judiciary seat (0/1)	-0.039*	-0.035	-0.052**	-0.053*	-0.052**	-0.053*	-0.039*	-0.039
	(0.023)	(0.027)	(0.024)	(0.029)	(0.024)	(0.029)	(0.024)	(0.028)
R-squared	0.19	0.24	0.19	0.24	0.19	0.24	0.23	0.28
Panel B: dependent variable is the share of inspections with at least one management irregularity; mean 0.45, std 0.17								
Judiciary seat (0/1)	-0.029	-0.020	-0.046**	-0.042*	-0.046**	-0.041	-0.037*	-0.031
	(0.021)	(0.024)	(0.021)	(0.025)	(0.021)	(0.025)	(0.022)	(0.026)
R-squared	0.18	0.24	0.19	0.24	0.19	0.24	0.23	0.28
Panel C: dependent variable is the share of inspections with at least one procedural irregularity; mean 0.49, std 0.21								
Judiciary seat (0/1)	-0.028	-0.018	-0.016	-0.003	-0.017	-0.004	-0.001	0.015
	(0.028)	(0.032)	(0.028)	(0.033)	(0.028)	(0.033)	(0.028)	(0.033)
R-squared	0.15	0.22	0.15	0.22	0.16	0.22	0.19	0.26

Notes: IV estimations. The instrument for judiciary seat is an indicator for judiciary-district-specific maximum population. The sample consists of 721 municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. See appendix I for the definitions of management and procedural irregularities. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 6: Impact on the share of inspections with visible versus nonvisible management irregularities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Panel A: dependent variable is the share of inspections with at least one visible management irregularity; mean 0.36, std 0.16								
Judiciary seat (0/1)	-0.031	-0.024	-0.051**	-0.046*	-0.050**	-0.046*	-0.047**	-0.042
	(0.021)	(0.025)	(0.022)	(0.026)	(0.022)	(0.026)	(0.022)	(0.026)
R-squared	0.15	0.20	0.15	0.20	0.15	0.20	0.18	0.23
Panel B: dependent variable is the share of inspections with at least one nonvisible management irregularity; mean 0.19. std 0.13								
Judiciary seat (0/1)	-0.027	-0.019	-0.020	-0.014	-0.021	-0.014	-0.010	-0.001
	(0.017)	(0.019)	(0.018)	(0.020)	(0.018)	(0.020)	(0.018)	(0.020)
R-squared	0.13	0.21	0.13	0.21	0.13	0.21	0.17	0.24

Notes: IV estimations. The instrument for judiciary seat is an indicator for judiciary-district-specific maximum population. The sample consists of 721 municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. See appendix I for the definitions of visible and nonvisible management irregularities. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. 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, falsification test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Panel A: dependent variable is the share of inspections with at least one irregularity; mean 0.67, std 0.19								
Second population rank (0/1)	-0.000	-0.002	0.001	-0.002	-0.001	-0.004	-0.001	-0.004
	(0.019)	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)
R-squared	0.18	0.24	0.18	0.24	0.18	0.24	0.22	0.27
Panel B: dependent variable is the share of inspections with at least one management irregularity; mean 0.44, std 0.16								
Second population rank (0/1)	0.006	0.010	0.006	0.010	0.005	0.008	0.006	0.008
	(0.015)	(0.016)	(0.016)	(0.017)	(0.016)	(0.017)	(0.016)	(0.017)
R-squared	0.19	0.25	0.19	0.25	0.20	0.26	0.22	0.28
Panel C: dependent variable is the share of inspections with at least one procedural irregularity; mean 0.50, std 0.20								
Second population rank (0/1)	-0.006	-0.008	-0.002	-0.004	-0.004	-0.006	-0.005	-0.008
	(0.020)	(0.020)	(0.020)	(0.021)	(0.020)	(0.021)	(0.019)	(0.020)
R-squared	0.14	0.21	0.15	0.21	0.15	0.22	0.20	0.27

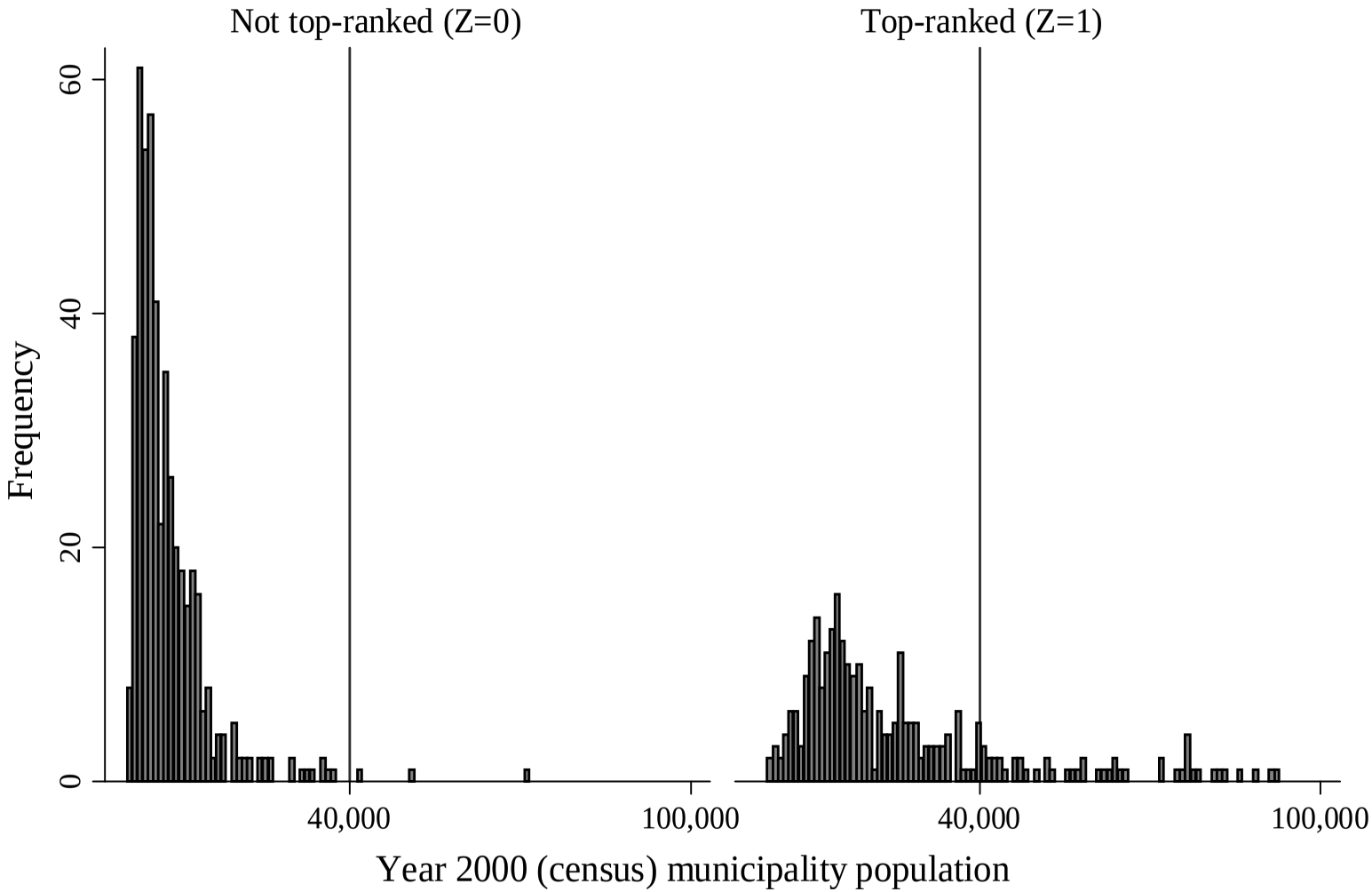
Notes: OLS estimations. Second population rank indicates that the municipality has the second-largest population in its judiciary district based on 2005 district borders and population. The sample consists of 478 municipalities that were audited through round 21, excluding single-municipality judiciary districts, top-ranked municipalities in multi-county districts, as well as municipalities with population above 40,000. See appendix I for the definitions of management and procedural irregularities. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. 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 irregularities, first- vs. second-term mayors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Dependent variable is the share of inspections with at least one irregularity; mean 0.69, std 0.19								
Judiciary seat (0/1)	-0.070	-0.062	-0.068	-0.066	-0.068	-0.066	-0.042	-0.038
	(0.044)	(0.045)	(0.043)	(0.045)	(0.043)	(0.045)	(0.040)	(0.041)
Judiciary seat (0/1) ×	-0.052	-0.057	-0.052	-0.057*	-0.052	-0.057*	-0.057*	-0.064*
[First-term mayor (0/1) – 0.6]	(0.033)	(0.035)	(0.034)	(0.035)	(0.034)	(0.035)	(0.034)	(0.034)
R-squared	0.20	0.24	0.20	0.24	0.20	0.24	0.24	0.28

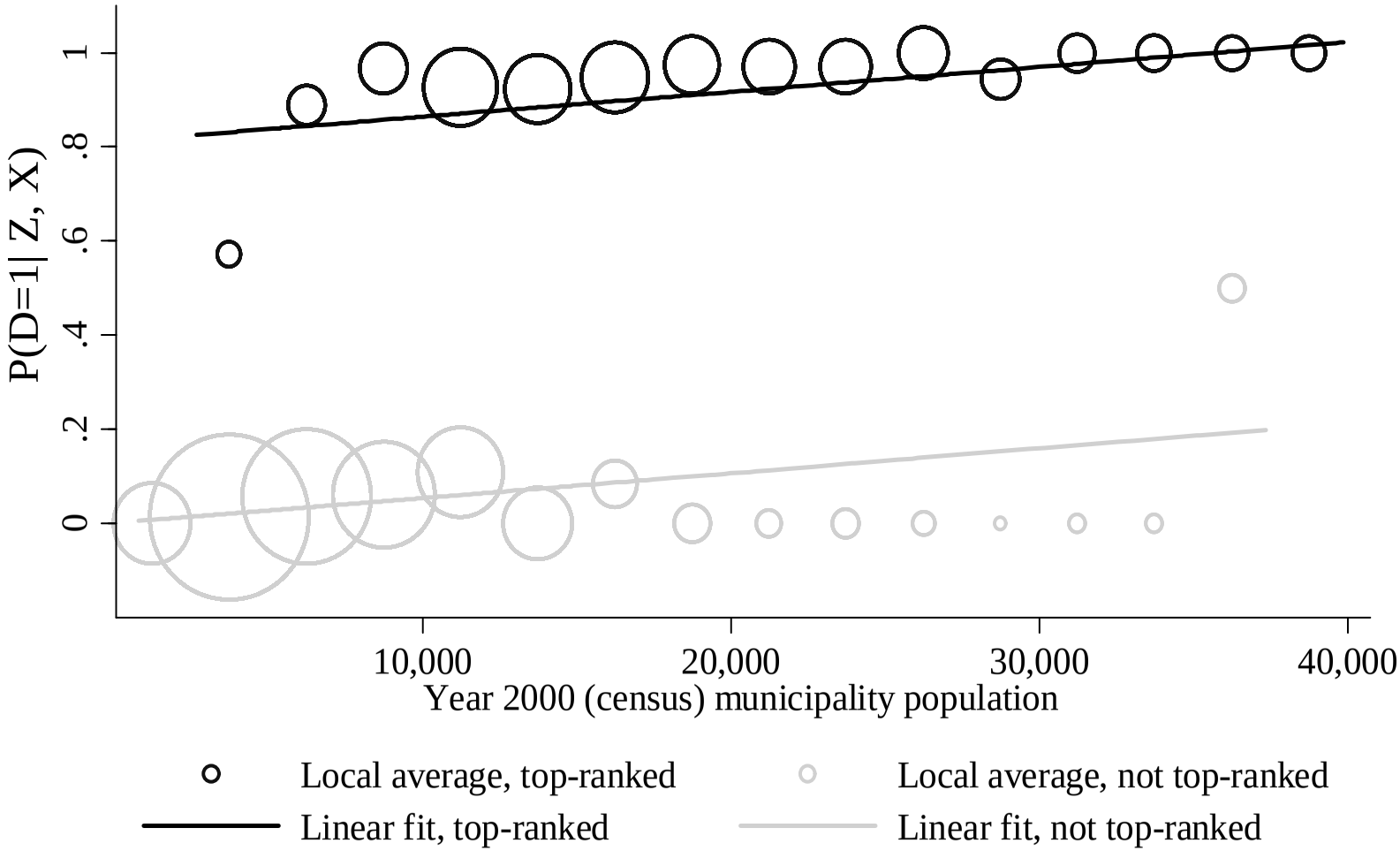
Notes: IV estimations interacted with demeaned covariates. The instrument for judiciary seat is an indicator for judiciary-district-specific maximum population. Demeaned covariates are interacted with the instrument in the first stage and with judiciary seat in the second stage. Demeaned covariates: first-term mayor indicator, judiciary district population, income per capita, average years of schooling, radio station, internet access and number of civil servants. The sample consists of 721 municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Controls include all additional variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. 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



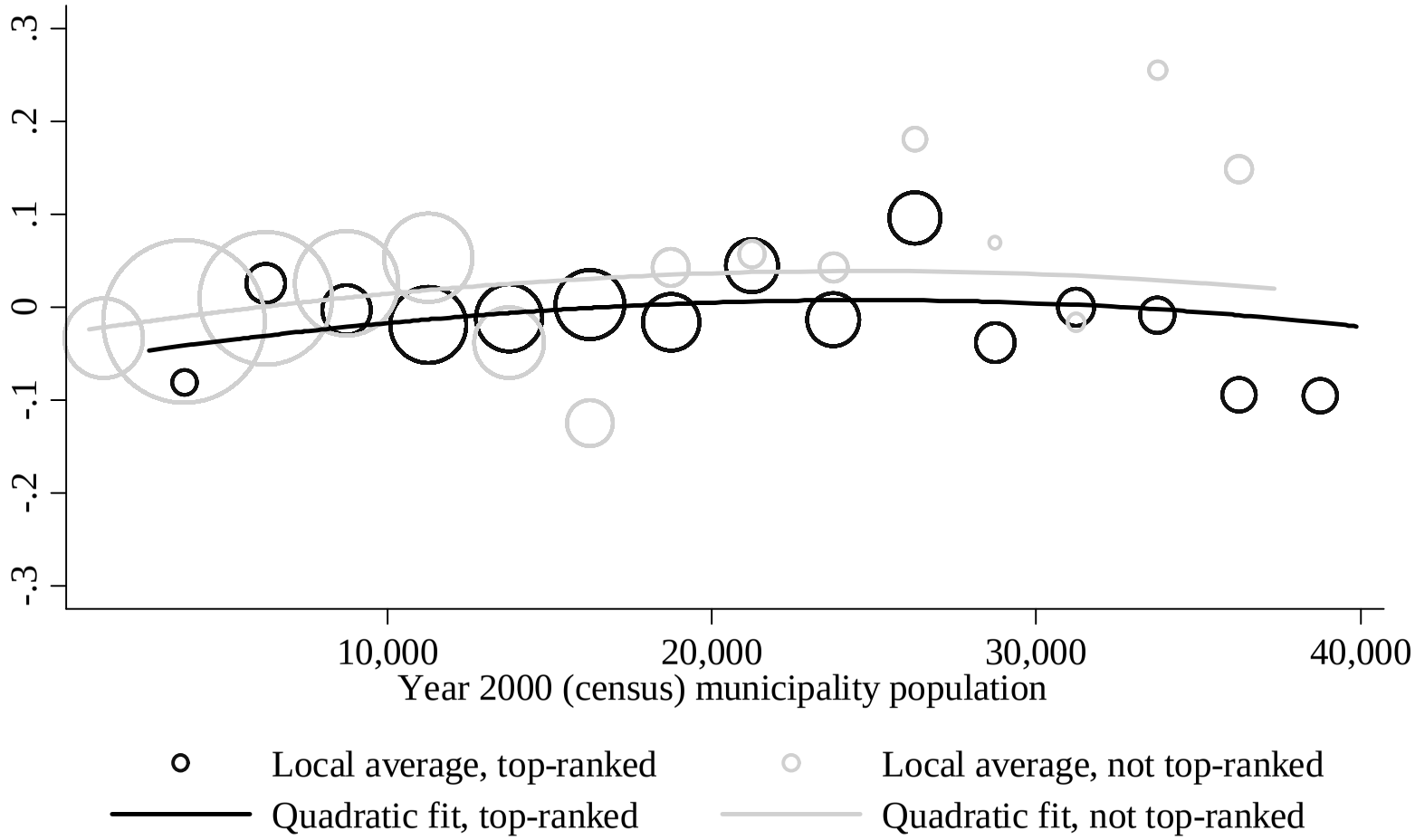
Notes: Z indicates judiciary-district-specific top rank in terms of official 2005 population. The sample consists of municipalities audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 100,000.

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



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

		Corruption codings[a]		
Auditor classification of irregularities	%	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/N	C	
- excess cash holdings (opportunity cost)	43.71	M/N		
- unjustified payment of bank fees	1.40	M/N		
- 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	17.86			
- unjustified or excessive payments for goods and services	15.99	M/N	C	C
- simulated tender process	2.56	M/N	C[c]	C
- evidence of favouritism	5.59	M/N	C[c]	C
- fractionalizing of procurement amounts	3.45	M/N		C
- invitation for bids to less than three firms	6.54	M/N	M	M
- procurement modality too restricted	3.86	M/N		M
- participating ineligible firm	7.66	M/N		M
- non-selection of the lowest bid	1.74	M/N		
- evidence of price collusion	0.52	M/N		
- unjustified direct purchase (no competition)	6.30	M/N		
- irregular composition of the procurement commission	1.60	M/N		
- absence of preliminary price survey	5.52	P		
- inadequate publication of the call	3.68	P		
- incomplete specification of the call	3.28	P		
- inadequate publication of results	1.72	P		
- tender process without funding	0.06	P		
- formal errors	29.92	P		
Social security contribution collection	1.74	P		

[a] LZ: Litschig and Zamboni, FF: Ferraz and Finan, BNPT: Brollo, Nannicini, Perotti, and Tabellini, P: Procedural, M/N: Mismanagement/Nonvisible, M/V: Mismanagement/Visible, 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	33.68			
- project not implemented	10.08	M/V	C	
- partial project execution	5.18	M/V	C	
- substandard project execution	28.22	M/V	C	
- diversion of project resources	10.51	M/V	M	M
- time overruns	0.54	M/V		
- project delays	2.17	M/V		
- inadequate project inputs	1.84	M/V	M	
- project on hold	1.14	M/V		
- inadequate infrastructure to run program	5.74	P		
- lacking oversight of project implementation	6.20	P		
- irregular sub-contracting	0.13	P		
- irregular change of work plan	2.20	P		
- irregular project documentation	13.07	P		
- matching grant requirements not met by local governments	3.26	P		
- staff members have inadequate training	4.07	P		
- irregular contract	0.51	P		
- late payment to suppliers	0.15	P		
- failure to notify community of resource receipt	3.01	P		
- formal errors	1.71	P		
Inventory and equipment management	13.56			
- inventory or equipment unaccounted for	49.43	M/V		
- irregular sale of inventory or equipment	1.28	M/V		
- unusable or only partially usable inventory or equipment	6.91	M/V		
- 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/V		
- remittance to ineligible individuals	17.84	M/V		
- benefit not remitted	4.65	M/V		
- partial remittance	1.06	M/V		
- delayed remittances	5.17	M/V		
- duplication of remittance	5.00	M/N		
- program beneficiary not found	10.34	M/N		
- non-existent school attendance verification	7.27	M/N	M	
- number of beneficiaries below target	2.83	M/N		
- 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			

Online appendix for the paper “Judicial Presence and Rent Extraction”

Stephan Litschig and Yves Zamboni

October 9, 2019

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1 Details on alternative corruption codings

This section describes alternative codings of CGU auditors' classification of irregularities in more detail. In addition to our own coding, we discuss those by Ferraz and Finan (FF, 2011), and Brollo, Nannicini, Perotti, and Tabellini (BNPT, 2013). Please refer to appendix I of the paper for the discussion below.

Ferraz and Finan define a corruption and a mismanagement measure, which essentially correspond to mutually exclusive subsets of our management irregularities. Importantly however, they argue that corruption irregularities are potentially observed by voters, while management irregularities are not. Our own assessment is often somewhat different. 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" type of irregularity, which we consider nonvisible to voters. 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. Since we do not have this information in our data we code "irregular/non-existent receipts" as a procedural irregularity. Other examples of "diversion of funds" include the partial construction of roads and classrooms, or the construction of dams and wells on politicians' private farms, which correspond to our "project not implemented", "partial project execution", "substandard project execution" and "diversion of project resources". We agree with their assessment that these types of irregularities are likely visible to voters. Another type of corruption they distinguish is "over-invoicing", in which "auditors determined that the goods and services were purchased at a value above market price", which corresponds to our "unjustified or excessive payments for goods and services" type. In contrast to Ferraz and Finan's assessment we consider this irregularity nonvisible to voters. 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. Our own

assessment is again that these irregularities are essentially nonvisible to voters, absent revelation through an external audit.

The mismanagement measure in Ferraz and Finan is based on separate types of irregularities, which they argue are all not easily observed by voters. 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", a nonvisible management irregularity. 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", and "non-existent school attendance verification" categories, respectively. We agree with their assessment that these irregularities are hard to detect by voters.

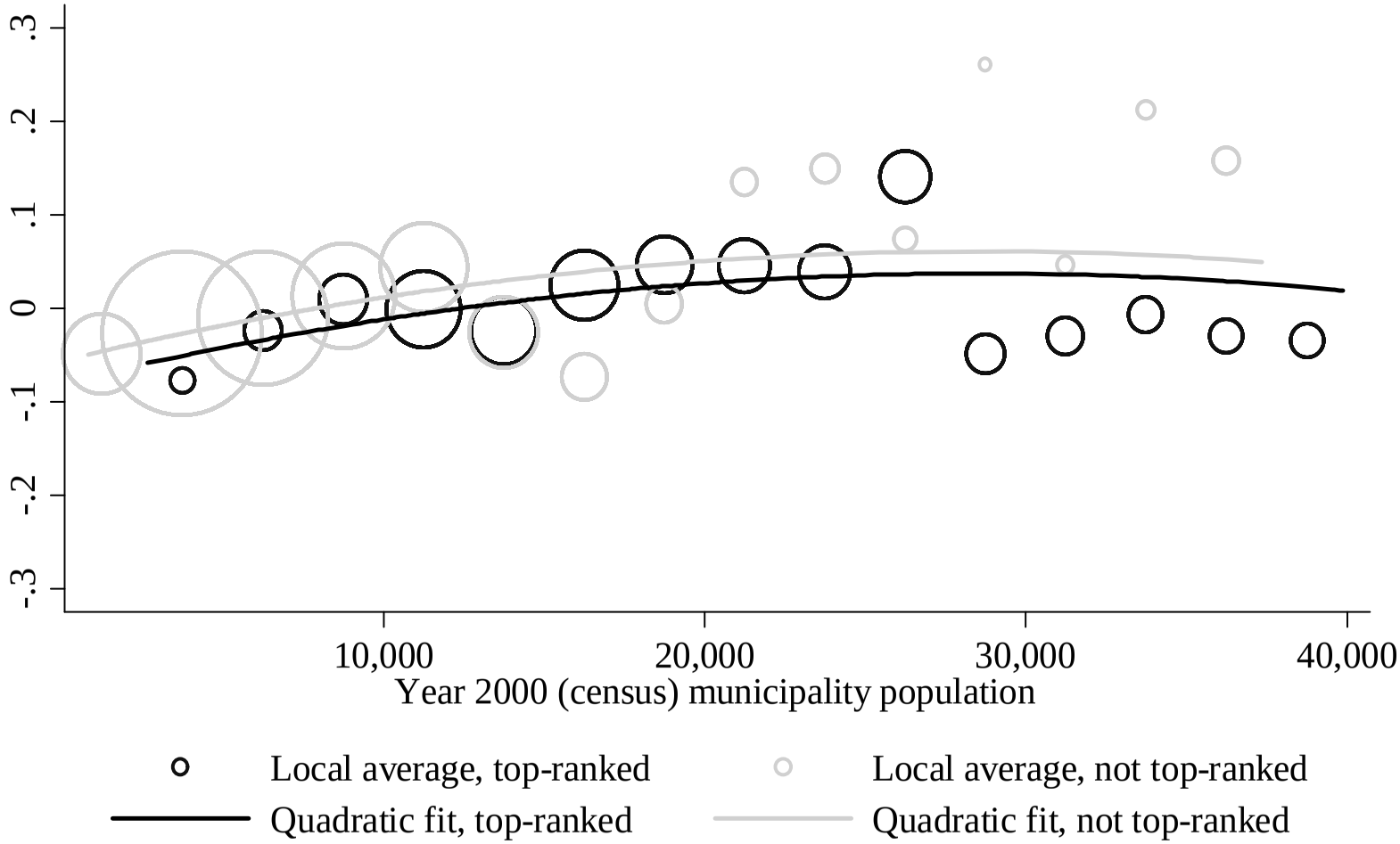
Brollo et al. also use the CGU audit reports to construct a narrow and a broad corruption measure, both of which basically correspond to a subset of our management irregularities as shown in appendix I in our paper. Their narrow corruption measure includes cases of "limited competition", corresponding 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", corresponding 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".

Table 1 below shows estimates of the effect of judicial presence on corruption based on the codings by Ferraz and Finan (2011) and Brollo et al. (2013). Please refer to the paper for details of the specifications. In Panel A the dependent variable is the share of inspections with at least

one corruption irregularity. While all estimates are negative, the magnitudes are of the order of -1 to -2 percentage points and none are statistically different from zero. In contrast, our results for management irregularities in Panel B of Table 5 in the paper suggest a reduction of about 4 percentage points and four out of eight estimates are significant at 5 or 10 percent. This difference in results comes from the fact that the Ferraz and Finan (2011) coding excludes certain management irregularities, such as project delays or partial and delayed remittance of cash transfers to eligible individuals, which do in fact respond to judicial presence. The mean share of inspections with at least one corruption irregularity is only 0.32, while the corresponding mean for management irregularities is 0.45.

In Panel B of Table 1 below the dependent variable is the share of inspections with at least one broad corruption irregularity according to the Brollo et al. coding. Despite its name, this coding includes even fewer types of irregularities since the mean share affected is only 0.19. In particular, the Brollo et al. coding excludes any irregularities related to project execution. Results indicate that judicial presence has zero impact on this corruption measure. This is not surprising, since their broad corruption measure essentially captures irregularities in procurement, which we argue are not easily observed by voters and thus are not expected to respond to the presence of the judiciary apparatus.

Figure 1: Impact on the share of inspections with management irregularities, reduced form



Notes: The dependent variable on the vertical axis is the share of inspections with at least one management 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.

Table 1: Impact on the share of inspections with evidence of corruption and broad corruption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Panel A: dependent variable is the share of inspections with at least one corruption irregularity; mean 0.32, std 0.16								
Judiciary seat (0/1)	-0.032	-0.021	-0.024	-0.012	-0.024	-0.012	-0.015	-0.002
	(0.021)	(0.024)	(0.022)	(0.024)	(0.022)	(0.024)	(0.022)	(0.024)
R-squared	0.18	0.24	0.18	0.24	0.18	0.24	0.20	0.26
Panel B: dependent variable is the share of inspections with at least one broad corruption irregularity; mean 0.19, std 0.13								
Judiciary seat (0/1)	-0.015	-0.014	-0.003	-0.002	-0.004	-0.002	0.008	0.009
	(0.018)	(0.020)	(0.018)	(0.020)	(0.018)	(0.020)	(0.018)	(0.021)
R-squared	0.14	0.21	0.15	0.22	0.15	0.22	0.18	0.24

Notes: IV estimations. The instrument for judiciary seat is an indicator for judiciary-district-specific maximum population. The sample consists of 721 municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. See appendix I in the paper for the definitions of corruption and broad corruption irregularities. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 2: Impact on the share of audited funds with at least one irregularity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Dependent variable is the share of audited funds with at least one irregularity; mean 0.80, standard deviation 0.21								
Judiciary seat (0/1)	-0.020	-0.030	-0.026	-0.037	-0.026	-0.037	-0.019	-0.029
	(0.026)	(0.030)	(0.028)	(0.033)	(0.028)	(0.033)	(0.029)	(0.033)
R-squared	0.11	0.17	0.11	0.17	0.11	0.17	0.14	0.20

Notes: IV estimations. The instrument for judiciary seat is an indicator for judiciary-district-specific maximum population. The sample consists of 721 municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 3: Population rank in different years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Panel A: instrument for judiciary seat is an indicator for judiciary-district-specific maximum population in 2004								
Judiciary seat (0/1)	-0.040*	-0.036	-0.053**	-0.052*	-0.053**	-0.052*	-0.042*	-0.041
	(0.024)	(0.027)	(0.025)	(0.029)	(0.025)	(0.029)	(0.024)	(0.029)
R-squared	0.19	0.24	0.19	0.24	0.19	0.24	0.22	0.28
Panel B: instrument for judiciary seat is an indicator for judiciary-district-specific maximum population in 2003								
Judiciary seat (0/1)	-0.034	-0.032	-0.045*	-0.047	-0.045*	-0.047	-0.035	-0.036
	(0.023)	(0.027)	(0.025)	(0.029)	(0.024)	(0.029)	(0.024)	(0.029)
R-squared	0.19	0.25	0.19	0.24	0.19	0.24	0.23	0.28
Panel C: instrument for judiciary seat is an indicator for judiciary-district-specific maximum population in 2002								
Judiciary seat (0/1)	-0.033	-0.031	-0.045*	-0.046	-0.045*	-0.046	-0.035	-0.035
	(0.024)	(0.027)	(0.025)	(0.029)	(0.025)	(0.029)	(0.024)	(0.029)
R-squared	0.19	0.25	0.19	0.24	0.19	0.24	0.23	0.28
Panel D: instrument for judiciary seat is an indicator for judiciary-district-specific maximum population in 2001								
Judiciary seat (0/1)	-0.037	-0.033	-0.049*	-0.049	-0.049*	-0.049	-0.038	-0.037
	(0.024)	(0.028)	(0.025)	(0.031)	(0.025)	(0.030)	(0.025)	(0.030)
R-squared	0.19	0.24	0.19	0.24	0.19	0.24	0.23	0.28
Panel E: instrument for judiciary seat is an indicator for judiciary-district-specific maximum population in 2000								
Judiciary seat (0/1)	-0.021	-0.016	-0.031	-0.030	-0.031	-0.030	-0.020	-0.016
	(0.023)	(0.026)	(0.024)	(0.028)	(0.024)	(0.028)	(0.023)	(0.028)
R-squared	0.20	0.25	0.20	0.25	0.20	0.25	0.23	0.28

Notes: IV estimations. The dependent variable in all panels is the share of inspections with at least one irregularity. The sample consists of 721 municipalities. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 4: Impact of travel distance to judiciary seat on share of inspections with irregularities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population specification:	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic	Nonparam.	Nonparam.
Control variables:	N	Y	N	Y	N	Y	N	Y
Panel A: dependent variable is the share of inspections with at least one irregularity; mean 0.67, std 0.19								
Distance to judiciary seat	0.091*	0.092	0.114**	0.129*	0.113**	0.129*	0.088*	0.099
(100km)	(0.054)	(0.068)	(0.053)	(0.070)	(0.053)	(0.070)	(0.054)	(0.072)
R-squared	0.19	0.25	0.19	0.24	0.19	0.24	0.23	0.28
Panel B: dependent variable is the share of inspections with at least one management irregularity; mean 0.45, std 0.17								
Distance to judiciary seat	0.068	0.053	0.102**	0.101*	0.102**	0.101*	0.084*	0.080
(100km)	(0.048)	(0.061)	(0.047)	(0.061)	(0.047)	(0.061)	(0.049)	(0.065)
R-squared	0.19	0.25	0.19	0.24	0.19	0.24	0.23	0.28
Panel C: dependent variable is the share of inspections with at least one procedural irregularity; mean 0.49, std 0.21								
Distance to judiciary seat	0.065	0.046	0.035	0.008	0.036	0.009	0.001	-0.040
(100km)	(0.066)	(0.083)	(0.063)	(0.080)	(0.063)	(0.080)	(0.064)	(0.085)
R-squared	0.15	0.22	0.15	0.22	0.16	0.22	0.19	0.25

Notes: IV estimations. The endogenous variable is the travel distance from the municipality town hall to the judiciary seat, expressed in 100 kilometers. The instrument for travel distance is an indicator for judiciary-district-specific maximum population. The sample consists of 721 municipalities that were audited through round 21, excluding single-municipality judiciary districts, as well as municipalities with population above 40,000. See Appendix I for the definitions of management and procedural irregularities. Controls include all variables shown in Table 3, a dummy for whether at least one covariate was imputed, as well as a set of dummy variables for mayoral party affiliations. Nonparametric indicates that the specification includes a set of dummy variables for population within bins of width 2,500. All specifications include state fixed effects. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.