

# Judicial Presence and Rent Extraction\*

Stephan Litschig<sup>†</sup>

Yves Zamboni<sup>‡</sup>

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## Abstract

We estimate the effect of state judiciary presence on rent extraction in local governments. We measure rents as irregularities related to waste or corruption uncovered by auditors. Our unique dataset at the level of individual inspections allows us to separately examine extensive and intensive margins of rent extraction. The identification strategy is based on an institutional rule of state judiciary branches in Brazil according to which prosecutors and judges tend to be assigned to the most populous among contiguous counties forming a judiciary district. Our research design exploits this rule by comparing counties that are largest in their district to counties with identical population size from other districts in the same state, where they are not the most populous. IV estimates suggest that state judiciary presence reduces the share of inspections with irregularities related to waste or corruption by about 10 percent or 0.3 standard deviations. In contrast, we find no effect on the intensive margin of rent extraction. Finally, our estimates suggest that judicial presence reduces rent extraction only when mayors have re-election incentives.

Keywords: Institutions, Corruption, Rents, Local Governments

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<sup>†</sup>(corresponding author) Universitat Pompeu Fabra and Barcelona GSE, stephan.litschig@upf.edu.

<sup>‡</sup>Fundação Getulio Vargas São Paulo, yves.zamboni@gmail.com.

# 1 Introduction

There is a fair amount of consensus among both academics and policy-makers that institutions that constrain executive power are beneficial for economic development.<sup>1</sup> An important example is the judicial check on executive (and legislative) power, enshrined in constitutions around the world, which has been shown to be a determinant of political and economic freedom (La Porta, López-de-Silanes, Pop-Eleches and Shleifer, 2004), while the evidence on economic growth is mixed (Glaeser, La Porta, López-de-Silanes and Shleifer, 2004; Feld and Voigt, 2003).<sup>2</sup> However, open and contentious questions remain. For example, ideal measures of checks on the executive should reflect permanent constraints, rather than policies or constraints that may exist only on paper. Moreover, there is considerable debate about econometric identification of the causal link between various outcomes and institutions more generally (since institutions themselves likely reflect collective choices). In part, the existing debates are inherent to the nature of cross-country comparisons, which typically rely on aggregated measures of institutions and in which identification of causal effects is notoriously difficult.<sup>3</sup> A complementary approach, advocated and summarized in Pande and Udry (2006), is to analyze institutions in a within-country context where measurement and identification issues can be more easily addressed.

Our paper provides the first evidence on the role of the judiciary in constraining executive power based on micro-data, focusing on rent extraction by the executive branch. Rather than evaluating independence of the judiciary as in the macro literature, we focus on physical *presence* of state judicial institutions in the local community.<sup>4</sup> In the case of the local governments in Brazil considered here, there is no variation in the extent of judicial independence across municipalities because state-level prosecutors and judges are the ones who provide the checks on local officials.<sup>5</sup>

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<sup>1</sup>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 (2006) provide a comprehensive survey of the entire literature on institutions and development.

<sup>2</sup>Glaeser et al. (2004) measure independence of the judiciary based on tenure of judges while Feld and Voigt use an index based on 23 characteristics of the judiciary. Feld and Voigt also construct an index of *de facto* judicial independence, which turns out to be positively correlated with economic growth, while *de iure* judicial independence is not.

<sup>3</sup>See Acemoglu (2005) for a review of empirical challenges in comparative political economy.

<sup>4</sup>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".

<sup>5</sup>Municipalities are the lowest level of government in Brazil (below the federal and state governments). The discussion refers to local governments, counties, communities or municipalities interchangeably.

Less than half of municipalities in Brazil have a local judicial presence and for the vast majority of them, judicial presence 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 Brazil's state judiciary systems to identify the causal effect of state judicial presence (clearly itself a choice variable) on rent extraction by local officials. We measure rent extraction (including low effort on the job) as infractions of public management regulations by the local executive branch as revealed by auditors.<sup>6</sup> Our micro-data thus allow us to shed light on a key policy decision by the executive branch on which the judicial check might operate.

Theoretically, we think of judicial presence as a factor that affects the cost of rent extraction by local incumbent politicians and public servants.<sup>7</sup> Local officials might be exposed to a higher probability of detection in counties with local judicial presence compared to counties without such presence, because the general public faces lower transaction costs to report irregularities. Similarly, local officials may perceive a higher probability of punishment when the state prosecutor lives in town because he faces lower transaction costs for his investigations.<sup>8</sup> Alternatively, local elites might find it easier to capture state judiciary officials when they reside in the same county, which would presumably lower the probability of punishment and increase the incidence of infractions.<sup>9</sup> The effect of judicial presence on rent extraction by local public agents may thus work through a multitude of channels, and the net effect is *a priori* ambiguous.

We address potential endogeneity of local judicial presence by exploiting an institutional feature of state judiciary systems in Brazil: 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-specific laws specify necessary—although not sufficient—conditions for the creation of judiciary districts in terms of population size and typ-

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<sup>6</sup>In our context, local legislators play a minor role compared to the local executive (mayors and program administrators).

<sup>7</sup>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.

<sup>8</sup>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.

<sup>9</sup>See Bardhan and Mookherjee (2000) for the trade-off between local information and capture under centralized vs. decentralized delivery of public services. See Stigler (1971) on state capture by interest groups. See Ríos-Figueroa (2007) for an argument linking judiciary effectiveness to political fragmentation.

ically a subset of other characteristics, such as geographical area, size of the electorate, county fiscal revenue and judicial caseload. Roughly 75% of all counties do *not* become their own judiciary district. These counties are grouped together with contiguous neighbors by the judiciary, and only one of them becomes the local judiciary seat (*sede da comarca*) and gets the physical presence of prosecutors and judges.<sup>10</sup> Although state laws typically do not specify which county should be the seat of the judiciary district in these cases,<sup>11</sup> 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.<sup>12</sup>

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.<sup>13</sup> Essentially, our reduced form compares counties that are largest in their district to counties with identical population size from other districts in the same state, where they are not the most populous. Our instrumental variable (IV) approach explicitly allows for the possibility that judicial presence is correlated with unobserved factors that also affect outcomes—even conditional on population—since we only use variation in judicial presence induced by district-specific population rank.<sup>14</sup> The approach relies on three main identifying assumptions. First, conditional on population, district maximum population is mean independent of unobserved factors that affect outcomes (conditional independence). Second, district maximum population affects rent extraction only through local judicial presence, not through other channels (exclusion restriction). Third, the probability of having a judiciary presence in the municipality jumps when the municipality is largest within its district, conditional on population (first stage). In Section 4 below we discuss how we test these assumptions (to the extent this is possible).

Our measure of rent extraction in local governments is based on audit reports stemming from a policy of randomly selecting Brazilian municipalities for an inspection of federal transfers, which we refer to as the random audits program. Following the terminology of the federal internal au-

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<sup>10</sup>We have no information about *how* the judiciary groups counties into judiciary districts.

<sup>11</sup>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 Judiciária, Art. 8 and Art. 11, respectively).

<sup>12</sup>Private correspondence with judges and technical judiciary staff in various states.

<sup>13</sup>See Andrabi, Das, and Khwaja (2007) for a similar approach used to identify the effect of establishing government girls' schools on subsequent supply of private education.

<sup>14</sup>Note that our approach requires weaker identifying assumptions than assuming that judicial presence is exogenous conditional on population.

dit agency (*Controladoria-Geral da União, CGU*), we usually refer to the infractions of public management regulations by local government officials revealed in these reports as irregularities in public management. The irregularities reported by auditors range from improper financial reporting, over lack of oversight in project implementation, to waste and actual theft of public resources. Because of the random sampling, the types and incidence of irregularities are representative of problems in the local public sector in Brazil. If compliance with homogeneous national regulations is socially beneficial, deviations from the standard provide an objective measure of rent extraction by local executive officials, either through outright corruption or low effort on the job.<sup>15</sup> For the vast majority of the regulations considered by auditors in Brazil, compliance is likely to be socially beneficial because many of these standards reflect international best practices in public financial management (PEFA, 2006).<sup>16</sup>

Our measure of rents is based on the same reports as the corruption measures in Ferraz and Finan (2008, 2010), with two important differences. The first difference is that we focus on irregularities overall, rather than likely instances of corruption.<sup>17</sup> After all, corruption is only one type of rent extraction.<sup>18</sup> Moreover, corruption represents only a small fraction of irregularities uncovered by auditors as shown in Ferraz and Finan (2010).<sup>19</sup> In addition to inevitable ambiguities in the identification of corruption episodes, our main reason for focusing on overall irregularities is that the law is not limited to penalizing corruption, which requires a relatively high standard of proof because individuals can go to jail if convicted, but allows prosecutors to charge individuals with the lesser offense of "acts of administrative misconduct". Since the judicial check should operate on both waste and corruption, a comprehensive measure of rents is more appropriate for our purposes. However, we 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

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<sup>15</sup>Effort can be seen as negative rents as in Barro (1973) and Persson and Tabellini (2000).

<sup>16</sup>In the terminology of Bandiera, Prat and Valletti (2009) we think of 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.

<sup>17</sup>Ferraz and Finan code an irregularity as an instance of corruption in the following cases: funds disappear from municipal bank accounts, there is no proof of purchase *and* suppliers confirm that goods or services were not delivered, auditors determine that there was over-invoicing, public contracts were awarded to friends *and* the public good was not provided.

<sup>18</sup>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.

<sup>19</sup>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 (unconditional) average number of mismanagement episodes per audited item in their data is about 1.647.

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.<sup>20</sup>

The second difference with other codings of the Brazilian municipal audit reports is that our unique micro-data at the level of individual inspections allow us to separately examine the extensive margin of rent extraction (share of inspections with at least one irregularity) and the intensive margin (number of irregularities per inspections with at least one irregularity). This decomposition is new and important because it allows us to distinguish a situation in which there are irregularities in most or all inspections from a situation in which many irregularities are concentrated in just a few inspections. The decomposition also allows us to test not just whether but *how* judicial presence (or any other institution or policy) affects rent extraction by the local executive branch. In Section 3 below we show that from a descriptive standpoint, the distinction between extensive and intensive margins of rent extraction matters a great deal: 35 percent of all inspections came up entirely clean—without any evidence of procedural or management irregularities—while a full 55 to 61 percent showed no direct evidence that public resources were wasted or stolen.

Our main empirical result is that local presence of state judicial institutions reduces the share of inspections with irregularities related to waste or corruption (the extensive margin) by about 10 percent or 0.3 standard deviations. While we show that judicial presence also reduces irregularities overall, the effect turns out to be driven exclusively by a reduction in irregularities indicating waste or corruption, rather than procedural irregularities. This result is consistent with the intuition that less serious infractions are less likely to be detected by the public and prosecuted by the judiciary. Consequently, such infractions should respond less to judicial presence or not at all. The result also suggests that the reduction in irregularities is unlikely to be driven by a better understanding of public management regulations and hence better compliance in counties with local judicial presence, rather than a deterrence effect, since better understanding would presumably affect procedural irregularities more than those indicating waste or corruption. As further discussed below, these results are robust to the inclusion of detailed municipality and mayor characteristics as in Ferraz and Finan (2010) and to alternative definitions of mismanagement vs. procedural irregularities.

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<sup>20</sup>Our management irregularities roughly correspond to the "mismanagement" category in Ferraz and Finan (2010). The sample average of management irregularities per inspections across municipalities in our data is about 1 (our Table 2).

In contrast, there is no evidence of an effect when we use as dependent variable the total number of irregularities divided by either the number of inspections with at least one irregularity (the intensive margin) or by the number of total inspections (the product of extensive and intensive margins). We omit the latter results to save space. One interpretation of these findings is that the effect of judicial presence is heterogeneous across agents and some of them are simply not deterred. An alternative interpretation is that there might be significant measurement error in the number of irregularities as a result of non-standardized reporting and the interaction between the discovery of irregularities and further inspections, leading to noisier estimates when the number of irregularities is used in the numerator. Indeed, some of the reported irregularities may simply describe various aspects of the same underlying problem that different auditors report in different ways. In contrast, the share of inspections with at least one irregularity should be measured more accurately since auditor discretion plays no role in determining irregularities on the intensive margin.

We also investigate whether the effect of local presence of the judiciary on rent extraction depends on the mayor's re-election incentives, the main focus of Ferraz and Finan (2010). They show that mayors in their first term in office (those with re-election incentives) are less corrupt compared to mayors in their second term (who cannot run again immediately because of weak term limits). Although these authors do not attempt to identify the causal effect of local judiciary presence (they call it judiciary district), they find that in counties with judicial presence the effect of re-election incentives on corruption is reduced. Using our broader measure of rents, in contrast, we find just the opposite: re-election incentives increase the effect of local judiciary presence.<sup>21</sup> 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 when mayors have re-election incentives. This might indicate that judicial presence operates mostly through an increased probability of detection, which disciplines incumbents with re-election incentives, rather than an increased probability of prosecution or conviction, which should also discipline incumbents without re-election incentives. This interpretation is consistent with the fact that actual convictions of majors and other local officials are very rare events in Brazil (Arantes, 2004).

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<sup>21</sup>There is no reason to expect our results to be similar to those reported in Ferraz and Finan (2010) 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.

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 present our data on irregularities in local public management and describe how it relates to existing datasets that are also based on the audit reports. We discuss the territorial organization of the judiciary, identification and our estimation approach in Section 4. Results are presented in Section 5. We conclude with a discussion of limitations and extensions.

## 2 Audits program and institutional background

### 2.1 The random audits program

The random audits program was initiated under the government of Luiz Inácio Lula da Silva in March 2003 with the explicit objective of fighting corruption and waste in local public spending. Most municipalities were eligible for federal audit from the start of the program with the exception of state capitals.<sup>22</sup> 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. As of July 2010, 33 rounds have been carried out with 60 counties sampled in recent rounds. 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%. There is little doubt that county sampling is random.

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 these state 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

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<sup>22</sup>More specifically, eligibility for federal audit is based on a population threshold which was successively increased from 20'000 to 500'000.



(*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.<sup>23</sup>

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

Within a week of the county sampling, auditors spend about two weeks in the county in order to carry out their inspection orders. The quality of public services is assessed through interviews with the local population and service staff members. Auditors then write a report which details all the irregularities encountered during their mission. Reports include the amounts of resources audited, and if possible, any fraction that was diverted, wasted or stolen. This fraction is just a preliminary estimate, however. The exact amount diverted can only be assessed through a more detailed inspection which occurs only if it is subsequently deemed appropriate by the prosecutor in charge of the municipality. County mayors are given the possibility to comment on the draft report within five business days. Auditors in turn explain whether or not they accept the mayor's justification of problems found.

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

Final audit reports are sent to local legislatures, the federal ministries which 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.

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<sup>23</sup>Exceptions to this rule are possible if warranted by the program under inspection.

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 "improbability", 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.<sup>24</sup> Because the judiciary cannot initiate proceedings on its own, prosecutors play a key role in the criminal justice system.<sup>25</sup>

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 by public examination which are highly competitive. 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 Data**

#### **3.1 Data on irregularities in local public management**

Having described some key features of the Brazilian control system, we now present our measures of rent extraction in local governments in more detail. 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 1064 counties (about 20% of all counties in 2000) that have been audited through June 2006.<sup>26</sup> Following the practice of the comptroller general's office, we refer to the reported infractions of public sector management regulations as irregularities in public administration. It is worth emphasizing that each reported irregularity constitutes a breach of a specific legal norm by a local official and is potentially subject to prosecution

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<sup>24</sup>See Arantes (2004) on the organization and legal instruments at the disposal of the Brazilian "Ministerio Publico".

<sup>25</sup>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.

<sup>26</sup>The number of municipal audits carried out through round 21 is 1091. 21 municipalities were audited twice, and for 6 municipalities we lack census characteristics because they were installed after the year 2000.

by state procuracies.<sup>27</sup>

The violations reported by auditors range from improper financial reporting, over lack of oversight in project implementation, to waste and actual theft of public resources. The following 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 healthcare center. This situation is contrary to health ministry regulation which explicitly prohibits the use of federal transfers to this end.<sup>28</sup>

2) The mayor's office failed to organize a competitive tender for the procurement of school textbooks under the pretext that these books were unique although equivalent alternative textbooks were in fact available. The same administration had purchased different textbooks in the past.<sup>29</sup>

3) Our inspection of the project execution for two sanitary units reveals that they were constructed in smaller dimensions than projected. We also found that the height of the ceramic masonry in the bathroom was constructed below project specifications.<sup>30</sup>

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

While distinguishing corruption from bad management is very difficult in practice—even for prosecutors—,<sup>31</sup> it is also clearly the case that not all irregularities are equally serious. In line

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<sup>27</sup>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.

<sup>28</sup>9th lottery, Salgado de São Félix municipality, Paraíba state, Primary and Preventive Health Care Program.

<sup>29</sup>11th lottery, Abaetetuba municipality, Pará state, Programa Brasil Alfabetizado.

<sup>30</sup>10th lottery, Farias Brito municipality, Ceará state, Programa Esporte Solidário.

<sup>31</sup>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 book-

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.

Appendix I gives a detailed description of the incidence of various kinds of irregularities and whether we consider them of the management or procedural type. We also indicate the types of irregularities we think could be reasonably classified either way. In the examples above, 1) would be a diversion of project resources, which we classify as a management irregularity. 2) would be an inappropriate procurement modality, which we think could be classified as either a management or a procedural irregularity. 3) would be an example of substandard project execution, which we deem a management irregularity.

An important challenge for any measure of rents is how to deal with issues of scale and potential reporting bias. The raw reported number of irregularities 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). Moreover, there will be measurement error bias to the extent that the regressor of interest (judicial presence in our case) is correlated with local government size or the number of inspections. In order to address these issues, we construct a unique dataset at the level of the inspection 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.<sup>32</sup>

Another key feature of our micro-data is that it allows us to separately examine the extensive margin of rent extraction (share of inspections with at least one irregularity) and the intensive margin (number of irregularities per inspections with at least one irregularity). This decomposition

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keeping, 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).

<sup>32</sup>There could still remain some measurement error bias if auditors in the field spent disproportionate amounts of time or effort in counties with judicial presence or if the CGU headquarters exercised discretion in the inspection orders issued to state CGU branches.

is important because it allows us to distinguish a situation in which there are irregularities in most or all inspections from a situation in which many irregularities are concentrated in just a few inspections. The decomposition also allows us to test not just whether but *how* judicial presence (or any other institution or policy) affects rent extraction by the local executive branch.

From a descriptive standpoint, the distinction between extensive and intensive margins of rent extraction matters a great deal. Table 1 presents the distribution of irregularities per inspection (technically per service order). The first striking finding is that 35 percent of all inspections in the sample came up entirely clean, while the median is one irregularity per inspection. This is in stark contrast with the mean number of irregularities per inspection (not shown) which is about 2. The mean is more than twice the median because of many irregularities in just a few inspections: 10% of inspections turn up six or more irregularities. Table 1 also gives summary statistics broken down by type of irregularity for two definitions of management vs. procedural irregularities (see Appendix I for the types of irregularities we think could be reasonably classified either way). Under our two definitions of management irregularities, 55 percent and 61 percent of all inspections, respectively, came up clean, that is, without direct evidence that public resources were wasted or stolen. Again, the average of management irregularities per inspection is about 1 (not shown), the median is zero, and the difference is largely driven by two and one percent of inspections, respectively, which turned up six or more management irregularities.

Table 2 presents summary statistics for the extensive and intensive margins of rent extraction and the number of irregularities per inspection (the product of extensive and intensive margins), aggregated by municipality. The average of total irregularities divided by total inspections across municipalities in our data is 2.2 (median 1.9), while the mean (and median) share of inspections that turn up at least one irregularity is 67%, and the mean number of irregularities per inspections with at least one irregularity is 3.2 (median 2.8). This means that in the "typical" municipality a full 33% of inspections came up entirely clean. Looking only at irregularities that indicate waste or corruption, the average total of such irregularities per total inspections is about 1 (median 0.8), while the mean (and median) share of inspections involving such irregularities is about 0.45, and the conditional-on-positive mean number of such irregularities per inspections is 2.1 (median 1.9). That is, in the typical municipality, 55% of all inspections give no indication that public resources

were wasted. This number increases to about 65% when we drop irregularities that we consider could be classified as either of the management or of the procedural type.

### **3.2 Caveats**

There are three caveats worth pointing out regarding our measures of rent extraction. First, we assume that existing rules and regulations which define both management and procedural irregularities make sense, that is, they serve a legitimate purpose in a reasonable way.<sup>33</sup> Put differently, we take irregularities to be generally detrimental to public service delivery, rather than reflecting attempts by well-meaning officials to circumvent inefficient red tape. As mentioned above, mayors and managers have the possibility to comment on the audit report. Sometimes auditors concede that there are valid arguments for non-compliance and we exclude these instances from our measures. Based on our reading of the regulations considered here, we believe that reported irregularities are for the most part undesirable from a social point of view because they either involve a direct waste or loss of public resources or complicate the detection of such mismanagement. It is also worth noting that the regulations pertaining to public financial management reflect international best practices.<sup>34</sup>

The second caveat is that we need to assume that auditors themselves were not bribed into manipulating audit findings (Mookherjee and Png, 1995). 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 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. Third, 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.

The third caveat is that even if auditors were incorruptible, the local elite might somehow manage to manipulate what gets uncovered and what remains unnoticed. While this scenario is plausi-

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<sup>33</sup>Without this assumption we are still evaluating compliance.

<sup>34</sup>See PEFA (2006) for an overview of international standards in public financial management.

ble 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 irregular transactions such that auditors fail to uncover them since the audit is very thorough, involving both financial auditing and detailed inspection of public works and services.

### **3.3 Data on county characteristics**

Data on county characteristics are obtained from several sources. Data on judiciary districts and the indicator for local judicial presence were obtained from each state's law on the organization and territorial division of the judiciary branch (Código da Organização e Divisão Judiciária). 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). Official local population data for the years 2000 and 2005 are also from IBGE.<sup>35</sup> Data on local income distribution, schooling and health outcomes, and distance to state capitals are from the Instituto de Pesquisa Economica Aplicada (IPEA) based on the 2000 census. Mayor characteristics and political participation data are from the Tribunal Superior Eleitoral (TSE).

Table 3 presents sample means of the covariates used in the empirical analysis. Column 1 gives sample means of all 4442 municipalities in Brazil for which we do *not* have audits data. Columns 2 through 4 give sample means for mutually exclusive subsamples of municipalities for which we have audits data available. Column 2 gives sample means for the 275 single-county judiciary districts in our dataset, column 3 for the 304 municipalities with judicial presence in multi-county districts and column 4 for the 485 municipalities without judicial presence. Columns 5 and 6 present summary statistics for our main estimation sample: multi-county districts with

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<sup>35</sup>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).

and without judicial presence that were no larger than 40'000 inhabitants according to year 2000 census figures. Counties with and without judicial presence show sizeable differences in the raw sample means for many municipality characteristics, making it clear that controlling for population is crucial for our approach.

## 4 Background on judiciary districts, identification and estimation

### 4.1 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.<sup>36</sup> We address potential endogeneity of local judicial presence by exploiting an institutional feature of state judiciary systems in Brazil: 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.<sup>37</sup> State-specific 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 and judicial caseload. Roughly 75% of all counties do *not* become their own judiciary district.<sup>38</sup> These counties are grouped together with contiguous neighbors by the judiciary, and only one of them becomes the local judiciary seat (*sede da comarca*) and gets the physical presence of prosecutors and judges.

Table 4 summarizes the territorial organization of the judiciary across Brazilian states at two points in time, 1999 and 2005. The table shows that there are slightly more than two counties per judiciary district on average. Because of a substantial number of single-county districts, the average district size for districts that group more than one county together is about three. The table also shows that the number of judiciary districts in Brazil has increased only little over the course of these seven years. Although not shown in the table, the vast majority (95%) of counties that had

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<sup>36</sup>This is what Becker's (1968) model of crime and punishment would suggest. In addition to reverse causality, omitted variable bias is also likely. See Eide (1998) for a review of the empirical literature on the economics of crime.

<sup>37</sup>Lei Complementar N<sup>o</sup> 35, de 14 de Março de 1979, Art. 95-97.

<sup>38</sup>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<sup>o</sup> 35, de 14 de Março de 1979, Art. 97, 2nd paragraph).



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.

## 4.2 Identification

Although state laws typically do not specify which of the contiguous counties forming a multi-county judiciary district gets the physical presence of prosecutors and judges, the internally used assignment rule is to locate the judiciary seat in the most populous county. Our research design exploits this rule by using an indicator for judiciary-district-specific maximum population as an instrument for local judicial presence. Note that this research design is necessarily silent on the causal effect of judicial presence for single-county districts since we lack information about the assignment rule in these cases.

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

More formally, let  $Y$  denote the outcome variable (share of inspections with at least one 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,  $U$  unobservables that affect  $Y$  and  $V$  unobservables that affect  $D$ . Assuming that the treatment effect is constant, we can write the outcome and first stage equations as follows:

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

Note that assuming linear specifications for  $X$  in the outcome and first state equations is without

loss of generality as one could always include polynomial terms in  $X$  to flexibly control for population.<sup>39</sup> 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  $\beta_D$ . 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  $\beta_Z = 0$ . Together with the first stage assumption,  $\pi_Z > 0$ , it can easily be shown that the ratio of reduced form coefficients on  $Z$  identifies  $\beta_D$ :

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

In what follows, we assess the plausibility of the three main identifying assumptions that lead to this result.

### 4.3 Assessing conditional mean independence

The key threat to the conditional independence assumption is that unobserved factors are correlated with population rank and that they also have an effect on outcomes, even conditional on population. Note that both of these conditions must hold for conditional mean independence to fail. 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. But conditional mean independence only fails if district size also has a direct effect on outcomes, conditional on population.

More formally, let  $W$  denote district size,  $\beta_W$  the effect of  $W$  on  $Y$ , and  $U'$  other unobserved factors that affect outcomes so that we can write  $E(U|Z, X) = E(\beta_W W + U'|Z, X) =$

<sup>39</sup>See Stock and Watson (2007), Appendix 13.3, for a textbook discussion of conditional mean independence.

<sup>40</sup>To see this, substitute the equation for  $D$  into the equation for  $Y$  and evaluate the conditional expectations on the left-hand side of equation (1). For example, we have  $E(Y|Z = 1, X = x) = (\beta_D \pi_Z + \beta_Z) + (\beta_D \pi_X + \beta_X)x + E(\beta_D V + U|Z = 1, X = x) = \beta_D \pi_Z + (\beta_D \pi_X + \beta_X)x + \beta_D E(V|X = x) + E(U|X = x)$ , where the second equality follows from the exclusion restriction and the conditional mean independence of  $Z$  given  $X$ . Proceeding analogously with the other three conditional expectations produces the right-hand side of equation (1).

$\beta_W E(W|Z, X) + E(U'|Z, X)$ . Even if  $E(W|Z, X) \neq E(W|X)$ , conditional mean independence will hold as long as  $\beta_W \approx 0$  and  $E(U'|Z, X) = E(U'|X)$ . Empirically, it turns out that the effect of district size on outcomes,  $\beta_W$ , is indeed close to zero, once we control for municipality population. The more general point, however, is that a correlation between unobserved factors and population rank by itself does not invalidate the conditional independence assumption, as long as these factors do not affect outcomes, conditional on population. Consequently, there is no point in showing estimates of the correlation between  $W$  and  $Z$ , conditional on  $X$  (and estimates of  $\beta_W$ ) separately, although these results are available on request.

Instead, we show in Section 5 below that—conditional on population—our estimates of  $\beta_D$  are very robust to the inclusion of standard potential confounders such as income per capita and the poverty rate, average education of the local population (Glaeser and Saks, 2006), ease of access to information (Reinikka and Svensson, 2005), proxied by the presence of a local radio station and internet access, and voter turnout (Zingales, 2004). We also prove our results robust to the inclusion of mayor party affiliation indicators and other mayor characteristics, such as incumbency status, education level, age, and gender as in Ferraz and Finan (2010). Another possibility we consider is that second-ranked municipalities might be geographically closer to their respective state capitals, and proximity itself might be related to outcomes through agglomeration effects for example. While empirically it is indeed true that second-ranked municipalities are closer to state capitals, distance to the state capital turns out not to affect rent-extraction, conditional on population.

In addition to these standard controls we also test for confounding factors that might be specific to our research design. For example, as noted above, state laws specify necessary conditions for the creation of judiciary districts in terms of observable characteristics such as geographical area, size of the electorate and county fiscal revenue, in addition to population.<sup>41</sup> Top-ranked municipalities might be systematically different along some or all of these dimensions and these characteristics might be correlated with rent extraction even conditional on population. It turns out however, that none of these potential confounders affect our results once we control for population (estimates available on request).

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<sup>41</sup>Judicial caseload is another criterion that is sometimes used but we do not have this data.

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

Of course there might be other unobserved factors that are correlated with population rank and that also have an effect on outcomes, even conditional on population. For example, better managed counties might successfully pressure the judiciary 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, it is reassuring that our estimates are essentially unchanged with the inclusion of the long list of observables discussed above, once we condition on population.

#### **4.4 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 through other channels. To our knowledge, the territorial organization in terms of districts is an institutional feature of state judiciary systems only—unique and distinct from the territorial organization of local and state governments, for example. Note that other public or private institutions might of course use the same travel cost minimization logic to allocate headquarters among a set of contiguous counties as the one used for determining judicial presence. But there is no intrinsic reason for other institutions to rank municipalities in terms of population *within judiciary districts*, unless of course these institutions are related to the functioning of the judiciary.

So what if other institutions, such as state or federal police, also used population rank within each district to determine whether or not to establish a local presence? We do not have data on police presence at the local level to evaluate this channel.<sup>42</sup> Note, however, that as long as

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<sup>42</sup>If required, we could of course attempt to get this data.

state or federal police presence is a direct consequence of judicial presence—because enforcement activities in the local public sector are complementary with judicial presence—police presence would not violate the exclusion restriction. It would simply mean that one of the channels through which judicial presence operates is through the presence of state or federal police.

In addition to the arguments above, our research design allows us to assess the validity of the exclusion restriction empirically, using a placebo 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 placebo test we perform therefore compares our measures of rent extraction between municipalities that are second-ranked in their district and those that are lower-ranked, conditional on population. As shown in Section 5 below, we find no effect of this placebo 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.

#### **4.5 Assessing the first stage**

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

If the effect of local judicial presence on rent extraction is heterogeneous, we estimate a local average treatment effect for small- to medium-sized municipalities in multi-county districts.<sup>43</sup> 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.<sup>44</sup> 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 73 percentage points—the estimated local effect might be fairly representative of the average effect among small- to medium-sized

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<sup>43</sup>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.

<sup>44</sup>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 (Angrist and Imbens 1994; Angrist and Pischke 2009).

municipalities.

#### 4.6 Estimation approach

We use the IV estimator to estimate  $\beta_D$  (rather than indirect least squares (ILS) as in the discussion above) because in addition to providing a point estimate (numerically identical to the ILS estimate), the IV estimator also directly calculates the correct (robust) standard errors.<sup>45</sup> Because the population size of the municipality has a direct effect on rent extraction, conditioning on population is crucial for our approach. Figure 1 shows that there are essentially no municipalities ranked second or lower in terms of population in their district ( $Z=0$ ) that have a population above 40'000 in our sample. In order to ensure a common support we therefore drop treatment group municipalities ( $Z=1$ ) with population above 40'000 from the sample.<sup>46</sup> We also drop single-county judiciary districts, which satisfy all requirements by themselves and are therefore intrinsically different from those that do not.<sup>47</sup> Note that these two sample restrictions are dictated by our knowledge of the assignment rule for multi-county judiciary districts and the fact that we lack such institutional information about single-county judiciary districts.

We control for the direct effect of population on outcomes using polynomial terms in actual year 2000 census population.<sup>48</sup> We use year 2000 census population as a control—rather than predicted 2005 population—in order to be consistent with the measurement of other municipality characteristics which are all from the year 2000.<sup>49</sup> All estimations below include state fixed effects because the probability of having a local judicial presence varies systematically across states (as evident from Table 4) as does our measure of rent extraction. Note that we cannot include judiciary district fixed effects since our research design necessarily compares counties from different districts once population is held constant.

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<sup>45</sup>Reduced form estimates are available on request.

<sup>46</sup>We only trim from the top because at the bottom the two supports overlap as is evident from Figure 1.

<sup>47</sup>Conceptually, our instrument is also not well defined in single-county judiciary districts because maximum population requires a comparison of at least two municipalities per district.

<sup>48</sup>Results are quantitatively very similar when we use a set of indicators for bins of  $X$  as controls.

<sup>49</sup>Results are quantitatively very similar when we use year 2005 predicted population as a control.

## 5 Estimation results

Table 5 presents linear probability model (OLS) estimates of the first stage relationship between local judicial presence (judiciary seat) and the indicator for judiciary-district-specific maximum population (maximum population), conditional on population. The estimates of  $\pi_Z$  in the first three columns of Table 5 with linear, quadratic, and cubic population controls, respectively, suggest that the probability of having a local presence of the judicial apparatus increases by a highly significant 80 percentage points if the county is largest in its district, for counties with the same population size. Columns four, five and six consecutively add municipality characteristics, a set of mayor party affiliation dummies, and other mayor characteristics. Municipality characteristics are the only set of covariates that jointly predict judicial presence (p-value=0.00) and some of these seem to be somewhat correlated with the instrument since the point estimates of  $\pi_Z$  decreases to about 73 percentage points. The last two columns of Table 5 show that the first stage is similar in magnitude and statistical significance, irrespective of whether the municipality is run by a first- or second-term mayor. Overall, these results suggest that, controlling for population and other covariates, the first stage estimate is about 73 percentage points.

Table 6 gives IV estimates of  $\beta_D$  using the share of inspections that turn up at least one irregularity as the dependent variable. The point estimates of  $\beta_D$  in the first three columns of Table 6 are decreasing from the linear population control to the quadratic and then stabilize (no change when the cubic control term is added) at  $-0.051$ , significant at the 5 percent level. This suggests that while the linear specification of population yields an upward biased estimate of  $\beta_D$ , the quadratic and cubic specifications control for any direct effect of population size on the incidence of irregularities. Columns four through eight show that, consistent with the assumption of independence of the instrument conditional on population, the effect size of about  $-0.05$  is remarkably robust to the inclusion of municipality characteristics, mayor party affiliation dummies, as well as other mayor characteristics. While these tests of the conditional independence assumption are reassuring, one should keep in mind that much of the variation in the share of irregular inspections remains unaccounted for even after controlling for all available covariates (R-squared only reaches about 0.24).

Table 7 presents the main empirical result, which is that local presence of state judicial institutions reduces the share of inspections with irregularities related to waste or corruption (the extensive margin of rent extraction) by about 10 percent or 0.3 standard deviations. As in Table 6, the IV estimate of  $\beta_D$  in the first three columns of Table 7 decreases as the quadratic population term is added but stabilizes at about  $-0.046$ , again significant at the 5 percent level. And again, the effect size is remarkably robust to the inclusion of municipality characteristics, mayor party affiliation dummies, as well as other mayor characteristics. Compared to the mean share of inspections with management irregularities of 0.44 and standard deviation of 0.16, the effect amounts to about  $-10\%$  or 0.3 standard deviations.

In contrast, there is no evidence of an effect when we use as dependent variables the total number of irregularities divided by either the number of inspections with at least one irregularity (the intensive margin) or by the number of total inspections (the product of extensive and intensive margins). Table 8 shows results for total irregularities divided by the number of "dirty" inspections. The point estimates change sign several times across specifications and they are nowhere near statistical significance, mainly because the standard errors are an order of magnitude larger compared to Tables 6 and 7 above. We omit the results for total irregularities divided by total inspections to save space (they are available on request).

Table 9 shows that the lower incidence of infractions is driven exclusively by a reduction in management irregularities, rather than procedural irregularities. This result is consistent with the intuition that less serious infractions are less likely to be detected by the public and prosecuted by the judiciary. The result also suggests that the reduction in management irregularities is unlikely to be driven by a better understanding of public management regulations and hence better compliance in counties with local judicial presence, since this would presumably affect procedural irregularities more than irregularities indicating waste or corruption.

Table 10 shows that the results are robust to an alternative cutoff values for trimming the population support. The cutoff value above 51'000 residents is chosen because this includes the next two municipalities which are second- or lower-ranked in their district, as evident from Figure 1. While the choice of cutoff value is clearly arbitrary, we have found quantitatively similar results for a range of cutoffs and these results are available on request. Tables 11 and 12 show that the



results are also robust to an alternative definition of mismanagement vs. procedural irregularities.

The placebo test in Table 13 shows that the incidence of irregularities is no different between municipalities that are second-ranked in their district and those that are lower-ranked, conditional on population. Although most of the estimates are negative, they are an order of magnitude smaller than the estimates of  $\beta_D$  discussed above and they are nowhere near statistical significance. The placebo test result therefore increases our confidence in the exclusion restriction, that is, were it not through judicial presence, population rank per se would have no effect on rent extraction.

In Tables 14 and 15 we test whether the effect of local presence of the judiciary on rent extraction depends on the mayor's re-election incentives. Our estimates suggest that judicial and political accountability complement each other, with judicial presence reducing the share of inspections with irregularities more strongly for first-term mayors compared to second-term mayors. In fact, for mayors in their second term, local judicial presence does not seem to matter at all. The estimate of the differential effect in Table 14 becomes a bit larger and statistically significant at 5 percent when covariates are added.<sup>50</sup>

Table 15 shows that the differential effect is again driven by a reduction in irregularities related to waste or corruption. For mayors in their first term, the point estimate is about  $-0.07$  (sum of the first two coefficients), or roughly  $-16\%$ , significant at 5% throughout (p-value on F-statistic). For mayors in their second term in contrast, local judicial presence seems to have no effect. These results suggest that judicial presence operates mostly through an increased probability of detection of irregularities, which disciplines incumbents with re-election incentives, rather than an increased probability of prosecution or conviction, which should also discipline incumbents without re-election incentives. This interpretation is consistent with the fact that actual convictions of majors and other local officials are very rare events in Brazil (Arantes, 2004).

## 6 Conclusion

Our paper provides the first evidence on the role of the judiciary in constraining executive power based on micro-data, focusing on rent extraction by the executive branch. In particular, we show

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<sup>50</sup>We find quantitatively similar results when we restrict the sample to municipalities that were audited during 2003 or 2004, the last two years of the 2001-2004 term, to which our first-term indicator corresponds. Since auditors inspect transfers that were made during the two to three preceding years, audit results from 2005 and 2006 correspond at least in part to management practices in the 2001-2004 term.

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

Although this evidence is suggestive of a particular mechanism, ultimately we cannot say precisely *how* judicial presence leads to a reduction in rent extraction by local government officials. The results are nonetheless of interest from the perspective of evaluating whether to scale up judicial presence at the local level, since pinpointing exact channels is typically not viewed as necessary (nor sufficient) for such an exercise (Ludwig, Kling and Mullainathan, 2011).<sup>51</sup> Given that about 75% of all municipalities belong to multi-county districts and that the vast majority of them is of small to medium size, and given the high proportion of municipalities that followed the assignment rule, the (local average) treatment effect we identify in this study is in fact fairly general. From a policy perspective, our results therefore suggest that scaling up judicial presence at the local level in Brazil would likely reduce irregularities related to waste or corruption in the local public sector.

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

Whether judicial presence reduces rent extraction in other countries and institutional contexts

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<sup>51</sup>Learning more about mechanisms would of course be desirable in itself in order to interpret the results and to help assess whether they would generalize to other settings (Deaton, 2010; Ludwig, Kling and Mullainathan, 2011).

as well is an open and important question. We speculate, however, that our results help explain the fact that state district attorneys, the U.S. institutional equivalent of Brazilian state prosecutors, today are present in every county 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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## 8 Appendix I

### Incidence and classification of irregularities

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



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

Table 1: Distribution of irregularities per service order

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

Notes: N=30874. Unit of observation is the *service order*, which is a standardized set of inspections on programs and projects issued by the CGU headquarters. Management irregularities include those classified as Mgmt./Proc. in Appendix I, while clear management irregularities exclude those. Procedural irregularities exclude those classified as Mgmt./Proc. in Appendix I, while serious procedural irregularities include those.

Table 2: Irregularities and inspections, aggregated by municipality

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

Notes: N=1064. Unit of observation is the municipality. Management irregularities include those classified as Mgmt./Proc. in Appendix I, while clear management irregularities exclude those. Procedural irregularities exclude those classified as Mgmt./Proc. in Appendix I, while serious procedural irregularities include those.

Table 3: Municipality summary statistics (sample means)

Audited	No		Yes			
Judiciary seat in 2005		Yes	Yes	No	Yes	No
Single-município judiciary district in 2005		Yes	No	No	No	No
Year 2000 population support ('000)		Full	Full	Full	<40	<40
Number of municipalities	4'442	275	304	485	240	481
<u>Year 2000 municipality characteristics</u>						
Municipality population	31'876	41'896	34'744	8'390	18'389	7'823
Judiciary district population	68'150	41'896	50'490	73'865	32'600	71'100
Income per capita	171.8	154.2	191.3	156.4	178.1	156.1
Average years of schooling (25 years and older)	4.05	3.97	4.41	3.76	4.2	3.76
Percentage of residents living in urban areas (%)	58.6	61.9	68.9	52.4	66.1	52.3
Poverty headcount ratio (national poverty line, %)	24.6	29.7	22.7	26.1	24.4	26.1
Poverty gap (%)	49.5	51.6	50.1	50.2	49.7	50.1
Gini coefficient	0.56	0.58	0.58	0.55	0.58	0.55
Radio station (0/1)	0.44	0.59	0.69	0.26	0.65	0.26
Internet access (0/1)	0.22	0.33	0.45	0.08	0.36	0.08
Size of electorate	20'593	26'259	22'514	5'629	12'292	5'315
Electoral turnout	0.87	0.83	0.85	0.87	0.86	0.87
Log distance to state capital	5.29	5.00	5.31	5.34	5.36	5.35
<u>Mayor's party affiliation during 2001-2004 term</u>						
Partido do Movimento Democrático Brasileiro	21.2	19.2	23.3	19.6	24.6	19.8
Partido da Social Democracia Brasileira	19.3	19.5	16.7	19.6	15.0	19.8
Partido do Frente Liberal	18.1	16.2	16.4	19.0	17.5	19.0
Partido Progressista Brasileiro	10.5	5.6	11.0	14.1	11.3	14.0
Partido Trabalhista Brasileiro	7.7	6.7	6.3	5.8	5.3	5.8
<u>Major characteristics during 2001-2004 term</u>						
Some higher education (completed or not) (0/1)	0.47	0.48	0.62	0.36	0.58	0.36
Some secondary education (completed or not) (0/1)	0.29	0.35	0.22	0.33	0.24	0.34
Some primary education (completed or not) (0/1)	0.23	0.17	0.15	0.29	0.17	0.29
No formal education (0/1)	0.01	0.00	0.01	0.02	0.01	0.01
Age	49.6	50.9	50.2	48.7	50.0	48.6
Male (0/1)	0.93	0.93	0.93	0.95	0.93	0.95
First-term (0/1)	-	0.65	0.63	0.59	0.63	0.58

Notes: See Section 3.3 for data sources. Only the most important political parties are given in the table.

Table 4: Judiciary districts (comarcas) in Brazil, 1999 and 2005

State	Region	# of Counties		# of Comarcas		Counties per Comarca 2005			
		2000	2005	1999	2005	Mean	Std. Dev.	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	146	1.23	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		5506	5563	2415	2605	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 da Organização e Divisão Judiciária of each state. See Section 3.3 for more information. Regions are North, Northeast, Center-west, South-east and South.

Table 5: First stage

Dependent variable: judiciary seat (0/1)		Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Population specification:		Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Maximum population (0/1)	0.80*** (0.03)	N	N	0.80*** (0.03)	0.73*** (0.04)	0.79*** (0.03)	0.73*** (0.04)	0.79*** (0.03)	0.73*** (0.04)	0.73*** (0.04)	0.73*** (0.04)	0.73*** (0.04)	0.72*** (0.06)
Municipality characteristics		N	N	N	Y	N	Y	N	Y	Y	Y	Y	Y
F-statistic (p-value)				2.83 (0.00)			2.84 (0.00)		2.85 (0.00)	2.85 (0.00)	2.03 (0.00)	2.03 (0.00)	1.29 (0.23)
Mayor's party affiliation		N	N	N	N	Y	Y	N	Y	Y	Y	Y	Y
F-statistic (p-value)				1.37 (0.13)			1.37 (0.13)		1.02 (0.43)	1.02 (0.43)	0.93 (0.54)	0.93 (0.54)	0.53 (0.92)
Mayor's characteristics		N	N	N	N	Y	N	Y	Y	N	Y	Y	Y
F-statistic (p-value)				0.91 (0.52)			0.91 (0.52)		0.86 (0.56)	0.86 (0.56)	0.43 (0.90)	0.43 (0.90)	1.04 (0.41)
Mayor's term		Any	Any	Any	Any	Any	Any	Any	Any	Any	Any	First	Second
Observations	721	721	721	721	721	713	721	713	713	721	713	426	287
Percent predicted	94	94	94	94	94	94	94	94	94	94	94	94	94

Notes: OLS estimations. Dependent variable equals 1 if the municipality served as the local seat of the state judiciary branch in 2005 and 0 otherwise. Maximum population equals 1 if the municipality had the largest population in its district based on 2005 estimated population and 0 otherwise. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Major's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 6: Impact on total irregularities

Dependent variable: share of inspections with at least one irregularity; mean 0.67, std 0.19		Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Population specification:									
Judiciary seat (0/1)		-0.038 (0.023)	-0.051** (0.024)	-0.051** (0.024)	-0.055* (0.029)	-0.054** (0.025)	-0.049** (0.025)	-0.058* (0.030)	-0.059* (0.030)
Municipality characteristics		N	N	N	Y	N	N	Y	Y
F-statistic					0.71 (0.74)			1.04 (0.41)	0.96 (0.49)
Mayor's party affiliation		N	N	N	N	Y	N	Y	Y
F-statistic						24.6 (0.00)		3.75 (0.00)	2.91 (0.00)
Mayor's characteristics		N	N	N	N	N	Y	N	Y
F-statistic							0.50 (0.87)		0.62 (0.78)
Observations		721	721	721	721	721	713	721	713
R-squared		0.193	0.191	0.191	0.201	0.217	0.197	0.232	0.236

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 7: Impact on management irregularities

		Dependent variable: share of inspections with at least one management irregularity; mean 0.44, std 0.16					
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic
Judiciary seat (0/1)	-0.029 (0.021)	-0.046** (0.022)	-0.046** (0.021)	-0.049* (0.026)	-0.045** (0.022)	-0.044** (0.022)	-0.049* (0.026)
Municipality characteristics F-statistic (p-value)	N	N	N	Y 0.84 (0.61)	N	N	Y 1.01 (0.43)
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y 103.8 (0.00)	N	Y 7.35 (0.00)
Mayor's characteristics F-statistic (p-value)	N	N	N	N	N	Y 0.53 (0.85)	N 0.75 (0.66)
Observations	721	721	721	721	721	713	713
R-squared	0.184	0.188	0.188	0.200	0.216	0.196	0.230

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.



Table 8: Impact on total irregularities, intensive margin

Dependent variable: total irregularities / number of inspections with at least one irregularity; mean 3.05, std 1.28								
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	
Judiciary seat (0/1)	-0.065 (0.201)	-0.009 (0.222)	-0.013 (0.223)	0.259 (0.238)	-0.051 (0.218)	-0.034 (0.214)	0.193 (0.232)	0.165 (0.229)
Municipality characteristics F-statistic (p-value)	N	N	N	Y 0.91 (0.54)	N	N	Y 1.00 (0.45)	Y 0.84 (0.61)
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y 465 (0.00)	N	Y 53 (0.00)	Y 23.9 (0.00)
Mayor's characteristics F-statistic (p-value)	N	N	N	N	N	Y 0.58 (0.81)	N	Y 0.68 (0.73)
Observations	721	721	721	721	721	713	721	713
R-squared	0.182	0.184	0.186	0.206	0.239	0.191	0.259	0.266

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Major's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 9: Impact on procedural irregularities

Dependent variable: share of inspections with at least one procedural irregularity; mean 0.49, std 0.20								
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic		
Judiciary seat (0/1)	-0.027 (0.028)	-0.015 (0.029)	-0.016 (0.029)	0.006 (0.033)	-0.018 (0.029)	-0.021 (0.029)	0.004 (0.034)	0.001 (0.035)
Municipality characteristics F-statistic (p-value)	N	N	N	Y 1.37 (0.18)	N	N	Y 1.78 (0.05)	Y 1.72 (0.06)
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y 4.35 (0.00)	N	Y 3.98 (0.00)	Y 3.25 (0.00)
Mayor's characteristics F-statistic (p-value)	N	N	N	N	N	Y 0.97 (0.46)	N	Y 1.18 (0.31)
Observations	721	721	721	721	721	713	721	713
R-squared	0.149	0.154	0.156	0.179	0.184	0.162	0.213	0.219

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Major's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 10: Impact on total irregularities, broader support

Dependent variable: share of inspections with at least one irregularity; mean 0.67, std 0.19								
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic		
Judiciary seat (0/1)	-0.044** (0.022)	-0.047** (0.024)	-0.049** (0.024)	-0.054* (0.029)	-0.053** (0.025)	-0.049** (0.024)	-0.058* (0.030)	-0.061** (0.030)
Municipality characteristics F-statistic (p-value)	N	N	N	Y	N	N	Y	Y
				0.65 (0.80)			1.03 (0.41)	0.97 (0.48)
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y	N	Y	Y
					27.4 (0.00)		3.75 (0.00)	2.93 (0.00)
Mayor's characteristics F-statistic (p-value)	N	N	N	N	N	Y	N	Y
						0.56 (0.83)		0.70 (0.70)
Observations	739	739	739	739	739	731	739	731
R-squared	0.193	0.192	0.196	0.205	0.223	0.203	0.237	0.242

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 51'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 11: Impact on clear management irregularities

Dependent variable: share of inspections with at least one clear management irregularity; mean 0.38, std 0.16								
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic		
Judiciary seat (0/1)	-0.026 (0.021)	-0.042* (0.022)	-0.042* (0.022)	-0.045* (0.026)	-0.042* (0.022)	-0.038* (0.022)	-0.047* (0.026)	-0.045* (0.027)
Municipality characteristics F-statistic (p-value)	N	N	N	Y	N	N	Y	Y
				0.89 (0.55)			0.94 (0.51)	1.02 (0.43)
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y	N	Y	Y
					92.1 (0.00)		6.24 (0.00)	4.74 (0.00)
Mayor's characteristics F-statistic (p-value)	N	N	N	N	N	Y	N	Y
						0.81 (0.60)		1.07 (0.38)
Observations	721	721	721	721	721	713	721	713
R-squared	0.151	0.155	0.157	0.170	0.176	0.170	0.190	0.206

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Major's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 12: Impact on serious procedural irregularities

Dependent variable: share of inspections with at least one serious procedural irregularity; mean 0.52, std 0.20							
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	
Judiciary seat (0/1)	-0.041 (0.029)	-0.030 (0.029)	-0.031 (0.029)	-0.006 (0.034)	-0.033 (0.030)	-0.007 (0.034)	-0.012 (0.035)
Municipality characteristics F-statistic (p-value)	N	N	N	Y 1.43 (0.15)	N	Y 1.90 (0.03)	Y 1.82 (0.04)
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y 6.54 (0.00)	Y 3.46 (0.00)	Y 2.80 (0.00)
Mayor's characteristics F-statistic (p-value)	N	N	N	N	N	Y 0.99 (0.44)	Y 1.23 (0.27)
Observations	721	721	721	721	721	721	713
R-squared	0.143	0.148	0.150	0.176	0.179	0.212	0.184

Notes: IV estimations. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 13: Placebo test of the exclusion restriction

Dependent variable: share of inspections with at least one irregularity; mean 0.66, std 0.19						
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic
Second population rank (0/1)	-0.001 (0.019)	0.001 (0.019)	-0.002 (0.019)	-0.005 (0.020)	-0.003 (0.020)	-0.008 (0.020)
Municipality characteristics F-statistic (p-value)	N	N	N	Y 0.73 (0.73)	N	Y 0.80 (0.65)
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y 1.32 (0.18)	Y 1.53 (0.09)
Mayor's characteristics F-statistic (p-value)	N	N	N	N	Y 0.91 (0.52)	Y 1.06 (0.39)
Observations	479	479	479	479	476	476
R-squared	0.179	0.179	0.184	0.200	0.212	0.248

Notes: OLS estimations. Second population rank indicates that the municipality has the second-largest population in its judiciary district based on 2005 estimated population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Major's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, top-ranked municipalities in multi-county districts, well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 14: Impact on total irregularities for first- and second-term mayors

Dependent variable: share of inspections with at least one irregularity; mean 0.67, std 0.19		Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Population specification:									
Judiciary seat (0/1)		0.002 (0.035)	-0.007 (0.036)	-0.006 (0.036)	0.004 (0.041)	0.006 (0.038)	-0.010 (0.036)	0.019 (0.044)	0.013 (0.044)
Judiciary seat (0/1) × First-term mayor (0/1)		-0.066 (0.047)	-0.071 (0.049)	-0.071 (0.049)	-0.103* (0.058)	-0.089* (0.051)	-0.062 (0.049)	-0.126*** (0.062)	-0.122* (0.063)
F-statistic for $H_0: \beta_1 + \beta_2 = 0$ (p-value)		4.27 (0.04)	5.54 (0.02)	5.62 (0.02)	5.76 (0.02)	5.82 (0.02)	4.70 (0.03)	6.06 (0.01)	5.83 (0.02)
Municipality characteristics F-statistic (p-value)		N	N	N	Y	N	N	Y	Y
Mayor's party affiliation F-statistic (p-value)		N	N	N	N	Y	N	Y	Y
Mayor's characteristics F-statistic (p-value)		N	N	N	N	N	Y	N	Y
Observations		721	721	721	721	721	713	721	713
R-squared		0.202	0.200	0.200	0.219	0.244	0.210	0.262	0.270

Notes: Separate IV estimations in the samples with first-term mayors and second-term mayors. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

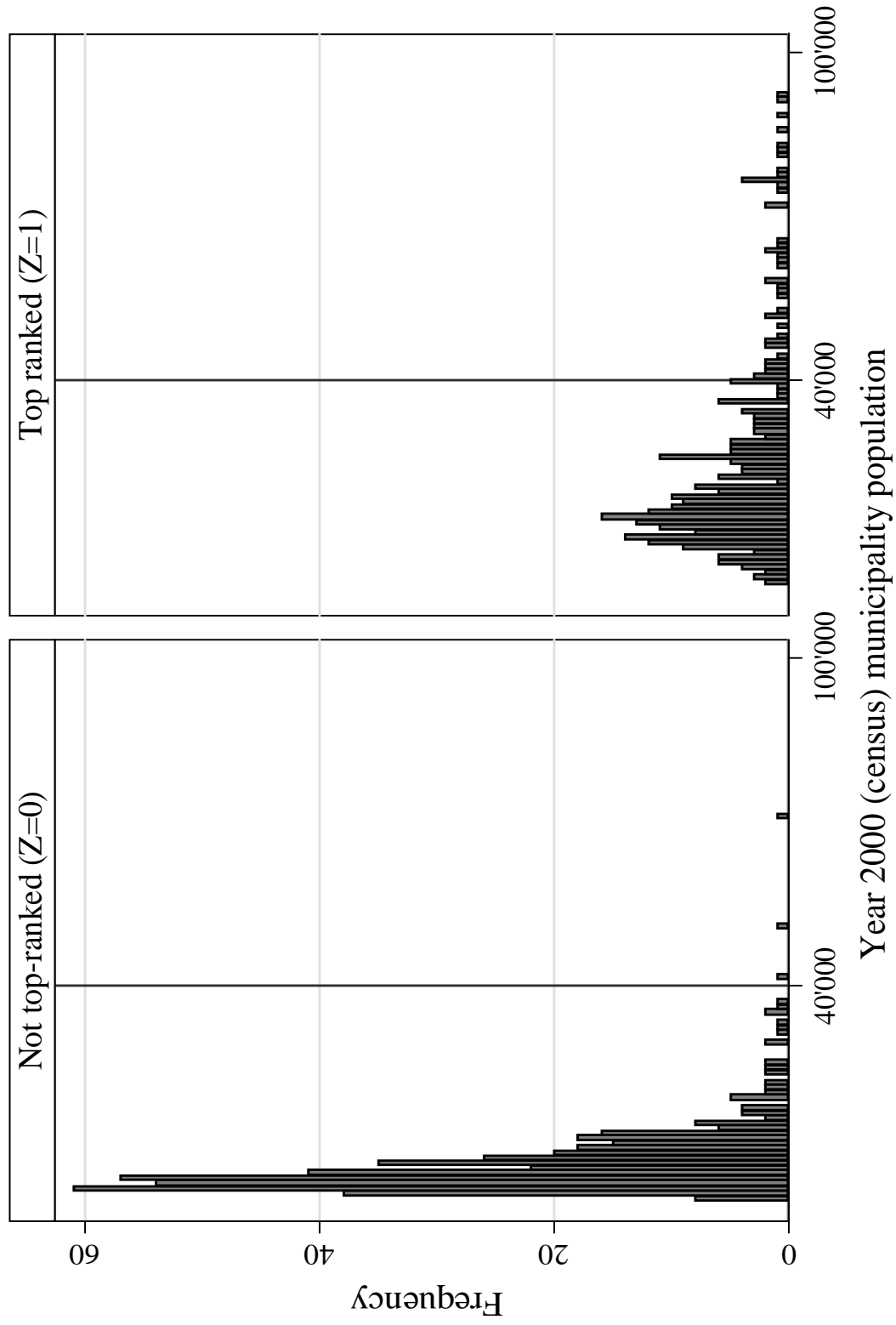
Table 15: Impact on management irregularities for first- and second-term mayors

Dependent variable: share of inspections with at least one management irregularity; mean 0.44, std 0.16									
Population specification:	Linear	Quadratic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Judiciary seat (0/1)	0.011 (0.033)	-0.002 (0.035)	-0.001 (0.035)	0.023 (0.039)	0.014 (0.035)	-0.005 (0.034)	0.038 (0.041)	0.033 (0.040)	
Judiciary seat (0/1) × First-term mayor (0/1)	-0.067 (0.043)	-0.071 (0.045)	-0.072 (0.045)	-0.112** (0.052)	-0.088* (0.045)	-0.066 (0.045)	-0.132** (0.054)	-0.128** (0.054)	
F-statistic for $H_0: \beta_1 + \beta_2 = 0$ (p-value)	4.00 (0.05)	6.58 (0.01)	6.67 (0.01)	6.79 (0.01)	6.30 (0.01)	5.95 (0.02)	7.09 (0.01)	6.98 (0.01)	
Municipality characteristics F-statistic (p-value)	N	N	N	Y	N	N	Y	Y	Y
				1.37 (0.12)			1.25 (0.19)	1.22 (0.21)	
Mayor's party affiliation F-statistic (p-value)	N	N	N	N	Y	N	Y	Y	Y
					43.7 (0.00)		4.26 (0.00)	3.16 (0.00)	
Mayor's characteristics F-statistic (p-value)	N	N	N	N	N	Y	N	Y	Y
						0.88 (0.60)		0.81 (0.65)	
Observations	721	721	721	721	721	713	721	713	713
R-squared	0.197	0.200	0.201	0.232	0.244	0.220	0.273	0.289	

Notes: Separate IV estimations in the samples with first-term mayors and second-term mayors. The instrument for judiciary seat is the indicator for judiciary-district-specific maximum population. Municipality characteristics, all measured in 2000: judiciary district population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, internet access, electorate and voter turnout, distance to state capital. Mayor's party affiliation are a set of dummy variables for each political party. Major's characteristics: first-term mayor indicator, education level indicators, male dummy and age. F-statistics test the joint null hypotheses that none of the municipality characteristics / party affiliation dummies / mayor's characteristics predict the outcome variable. All specifications include state fixed effects. Sample restricted to municipalities audited through round 21. Sample excludes single-municipality judiciary districts, as well as municipalities with population above 40'000. Robust standard errors are given in parentheses. \*, \*\*, and \*\*\* indicate significance at 10 percent, 5 percent and 1 percent levels respectively.



Figure 1: Histograms of municipality population by population rank



Notes: Z indicates judiciary-district-specific top rank in terms of official 2005 population. Sample is restricted to multi-county judiciary districts and municipalities for which audit data are available.