



Columbia University

*Department of Economics
Discussion Paper Series*

**The Effect of Judicial Institutions on Local Governance and
Corruption**

*Stephan Litschig
Yves Zamboni*

Stephan Litschig is a PhD student in the Department of Economics, Columbia University. Faculty members W. Bentley MacLeod and Wojciech Kopczuk have recommended the inclusion of this paper in the Discussion Paper Series.

Discussion Paper No.: 0607-15

*Department of Economics
Columbia University
New York, NY 10027*

August 2007

The effect of judicial institutions on local governance and corruption*

Stephan Litschig[†]

Yves Zamboni[‡]

August 8, 2007

Abstract

This paper estimates the effect of judicial institutions on governance and corruption in local governments. Our estimation strategy exploits a unique institutional feature of state judiciary branches in Brazil which assigns prosecutors and judges to the most populous among contiguous counties forming a judiciary district. As a result of this assignment mechanism there are counties with nearly identical populations, some with and some without local judicial presence, which we exploit to impute counterfactual outcomes. Conditional on observable county characteristics, offenses per civil servant are about 35% lower in counties that have a local seat of the state judiciary. The lower incidence of infractions stems mostly from fewer violations of financial management regulations by local administrators, fewer instances of problems in project execution and project management, fewer cases of non-existent or ineffective civil society oversight and fewer cases of improper handling of remittances to local residents.

Keywords: Law enforcement, corruption, local governance, institutions

JEL: H75 K42 M42

*We are grateful to Rajeev Dehejia, Albert Fishlow, Ray Fisman, Wojciech Kopczuk, David Lee, Leigh Linden, Bentley MacLeod, Alex Marsh, Kiki Pop-Eleches, Bernard Salanié, Joseph Stiglitz and Eric Verhoogen, for their generous comments and support. We have also received helpful comments from seminar participants at Columbia University, IPEA Brasilia, SMYE Seville, NEUCD Cornell and the NSEA conference in Lausanne. We wish to thank Elaine Faustino for clarifications on the implementation of the random audit program and Francisco Ramos and his team for compiling the database. Litschig acknowledges financial support from PER and ILAS at Columbia University. The views expressed in this paper are those of the authors and not necessarily those of the Controladoria-Geral da Uniao. All errors are our own.

[†](corresponding author) Columbia University, Department of Economics, 1022 International Affairs Building 420 West 118th Street, New York, NY 10027, USA, sl2189@columbia.edu, phone: 1-646-321-5963

[‡]Controladoria-Geral da Uniao, Setor de Autarquias Sul, Quadra 01, Bloco A, Edifício Darcy Ribeiro Brasília/DF - CEP: 70070-905, Brasil, yves.zamboni@gmail.com

1 Introduction

There is a growing consensus among both academics and policy-makers that institutions matter for economic development. However, open and contentious questions remain regarding both econometric identification of the causal link between institutions and development and the mechanisms through which this link operates¹. In part, the existing debates are inherent to the nature of cross-country comparisons, which necessarily rely on highly aggregated measures of institutions and in which identification is notoriously difficult². This paper contributes to a recent strand of the literature by providing evidence from more disaggregated units of analysis (municipalities within Brazil) and by using detailed knowledge about the institutional design of Brazil's state judiciary systems to measure and identify the effect of state judicial presence (our institutional measure) on local governance (our development outcome)³.

Our paper studies a within-country counterpart of the macro literature on institutions that examines the role of the judiciary in providing a check on executive and legislative power. This literature finds that the extent of judicial independence is an important determinant of political and economic freedom [La Porta et al., 2004], and the quality of state courts [Berkowitz and Clay, 2006], while the evidence on economic growth is mixed [Glaeser et al., 2004, Feld and Voigt, 2003]. In the case of local governments in Brazil, the extent of judicial independence is constitutionally guaranteed because it is state prosecutors and judges who provide the checks on local executive and legislative officials. Rather than evaluating formal independence of the judiciary as in the macro literature, our contribution is to highlight the effect of local *presence* of state judicial

¹For the view that institutions cause growth see the work of Knack and Keefer [1995], Mauro [1995], Hall and Jones [1999], Acemoglu, Johnson and Robinson [2001, 2002, 2004] among others. This wave of research pointing to the primacy of institutions and governance in the development process has recently been challenged mostly on empirical grounds. For the alternative view that economic growth causes institutional improvement see Barro [1999], Alvarez et al., [2000], Djankov et al. [2003] Glaeser et al. [2004] and Glaeser and Saks [2006].

²See Pande and Udry [2006] for a thorough review of the institutions and growth literature and a call for research on institutions based on micro data.

³Governance is good when corruption and other arbitrary government interventions are kept low and public services are delivered efficiently [IPD, forthcoming]. See Rose-Ackermann [1999, 2004] for a review of the empirical literature on governance and development. See Aidt [2003], Jain [2001] and Bowles [1999] for a review of the theoretical literature. See Glaeser and Goldin [2004] for an overview of corruption and reform in US history.

institutions (local judicial presence for short) on governance and corruption⁴.

We address potential endogeneity of local judicial presence by exploiting a unique institutional feature of state judiciary systems in Brazil: although state judiciary branches provide services to all counties in the state, only the most populous among contiguous counties in a given judiciary district (*comarca* in Portuguese) have a local presence of state prosecutors and judges⁵. Because the judiciary district-specific relative population rank determines treatment assignment, there are counties with nearly identical populations, some with and some without local judicial presence, which we exploit to impute counterfactual outcomes⁶. Given that the territorial organization in terms of judiciary districts is distinct from the territorial organization of local and state governments, we consider it unlikely that confounding factors such as other public services like government banks or tax authorities systematically locate in counties with judicial presence. The quasi-random assignment of local judicial presence is reflected in the fact that after matching on population, counties with and without local judicial presence are remarkably similar in observable characteristics, including those that are potentially correlated with local governance, such as average education of the local population [Glaeser and Saks 2006], and ease of access to information [Svensson 2005a, Ferraz and Finan, 2005a].

Theoretically, the effect of judicial presence on agents' behavior may work through a multitude of channels and the net effect is a priori ambiguous⁷. Executive and legislative 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

⁴Governance is good when corruption and other arbitrary government interventions are kept low and public services are delivered efficiently [IPD, forthcoming]. See Rose-Ackermann [1999, 2004] for a review of the empirical literature on governance and development. See Aidt [2003], Jain [2001] and Bowles [1999] for a review of the theoretical literature. See Glaeser and Goldin [2004] for an overview of corruption and reform in US history.

⁵For simplicity we usually refer to "local judicial presence" rather than "local presence of prosecutors and judges" although in Brazil the procuracy is in effect a 4th branch of government with strong factual independence from the other branches.

⁶See Andrabi et al. [2006] for a similar approach used to identify the effect of establishing government girls' schools on subsequent supply of education.

⁷We follow the economic approach to crime formalized by Becker [1968], according to which a person commits an offense if and only if the expected utility from the offense exceeds utility under the person's best alternative. The expected utility from committing an offense depends on the magnitude of sanctions and the probability of their application.

higher probability of punishment when the state prosecutor lives in town because he faces lower transaction costs for his investigations. Because transaction costs cut both ways, however, an alternative mechanism is that 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 infractions⁸.

The outcome measure we use consists of violations of public management regulations revealed by federal government random audits of transfers to local governments. The intuition for this measure is that the extent of local management deviations from a homogeneous national standard reflects an aspect of local government inefficiency, assuming that compliance with the standard is socially beneficial. We think that for the vast majority of the regulations considered by auditors in Brazil this assumption holds, not least because many of these standards reflect international best practices in public financial management [PEFA 2006].

Following the terminology of the federal internal audit agency (Controladoria-Geral da Uniao, CGU) we refer to these violations or offenses as irregularities in public management. The types and incidence of irregularities are representative of problems in the local public sector in Brazil because counties are randomly selected for federal audits through a public lottery. The offenses reported by auditors range from improper financial reporting to lack of oversight in project implementation to waste and actual theft of public resources. From the auditors' reports we obtain an offense rate at the county level by scaling the incidence of irregularities by the number of civil servants in the county administration. Because the reported violations include both corrupt and simply wasteful practices we interpret the offense rate as a measure of local governance, a higher rate indicating worse governance. Our paper refines existing governance measures based on audit findings by using the full audit reports rather than summaries⁹ and by deflating the number of irregularities by amounts audited to control for monitoring intensity. We elaborate on our measure and

⁸See 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 Rios-Figuero [2007] for an argument linking judiciary effectiveness to political fragmentation.

⁹Ferraz and Finan [2005a, 2005b].

its interpretation below.

Our results indicate that local judicial presence matters. In particular, we find that conditional on observable county characteristics, the overall incidence of offenses is 35% lower in counties that have a local presence of state judicial institutions. The lower incidence of infractions stems mostly from fewer violations of financial management regulations by local administrators, fewer instances of problems in project execution and project management, fewer cases of non-existent or ineffective civil society oversight and fewer cases of improper handling of remittances to local residents.

In addition to contributing to the literature on the role of institutions in development, our paper also complements recent work examining the role of social and political accountability mechanisms in promoting good governance¹⁰. In concurrent work, Ferraz and Finan [2005a] show that in municipalities where mayors are in their second and final term, there is significantly more corruption compared to similar municipalities where mayors are in their first term. They also find that the presence of a judge and local radio stations reduces this corruption differential, i.e. second-term mayors in counties with judicial presence are less corrupt than those in counties without such presence and similarly for the presence of radio stations. In contrast, we find little evidence that mayoral incumbency status matters for local governance¹¹. Our findings strongly suggest, however, that local judicial presence reduces irregularities in public management irrespective of whether the mayor faces a binding term limit or not. Our results also suggest that the presence of radio stations reduces certain categories of irregularities in public management such as those related to project execution and project management as well as county financial management.

¹⁰Our findings are also consistent with results in recent papers on deterrence. See Corman and Mocan [2000] for an approach using high frequency time series data. See Levitt [2002] for an approach using the number of firefighters as an instrument for the number of police officers. See Di Tella and Schargrodsky [2004] for an approach using geographical allocation of police forces. See Marvell and Moody [1996] and Eck and Maguire [2000] for surveys of earlier empirical work on police presence and crime.

¹¹The main difference between Ferraz and Finan's work and ours is that they use publicly available summaries of the audit reports while we use the full reports. They also attempt to distinguish corruption from mismanagement and show results broken down in this way while we focus on the total incidence of irregularities in public management as well as the incidence in successive stages of public service delivery (procurement, project execution, financial reporting, civil society oversight, etc.). As a result of these differences, the findings, though not directly comparable, are complementary.

Other related papers in the empirical anti-corruption literature investigate the effectiveness of access to information and political and social accountability mechanisms. Olken [2007] conducts a field experiment in Indonesia and finds that an increased probability of government audit reduces corruption by 8 percentage points of total project expenditures on road construction. Di Tella and Schargrodski [2003] find that prices paid by public hospitals of the city of Buenos Aires for basic, homogeneous inputs decrease by 15 percent during the first 9 months of a crackdown on corruption. Reinikka and Svensson [2005a] show that head teachers in schools closer to a newspaper outlet are more knowledgeable of the rules governing the education grant program and that these schools managed to claim a significantly larger part of their entitlement after a newspaper information campaign had been initiated by the central government. Ferraz and Finan [2005b] find that the disclosure of federal government audit results about local governments has a significant impact on the reelection probability of mayors that were found to be corrupt.

A common feature of this recent literature is that the anti-corruption mechanism under consideration works through political and social accountability of public officials rather than through judicial enforcement [Reinikka and Svensson 2005a]. The focus of our paper is instead on judicial accountability of both public officials and politicians¹². Our approach complements this existing literature in that it sheds light on the effectiveness of the prosecution stage of law enforcement¹³. Examining the enforcement stage is important because auditors typically have no formal sanctioning power¹⁴. Political players may be sensitive to the mere disclosure of corruption because they can be held accountable through political and social mechanisms. Whether a public manager is held accountable depends on whether his case is investigated and whether a judge finds him guilty. While social sanctions may also play a role in holding public managers accountable, the relative importance of judicial enforcement is likely to be higher for public officials compared

¹²As noted above, Ferraz and Finan [2005a] find that judiciary presence matters in interaction with incumbency status but they do not attempt to identify its causal impact.

¹³See Van Aaken, Salzberger and Voigt [2004] and Rios-Figueroa [2006] for a conceptual overview of the procuracy's relationship with the other powers of government.

¹⁴The CGU can open an administrative procedure against federal civil servants. But it is state and federal prosecutors who have the power to initiate civil or penal procedures against local government officials.

to politicians. More generally, assessing the costs and benefits of judicial enforcement relative to political and social accountability may allow for a more effective approach to promoting good governance.

The paper is organized as follows. In section 2 we describe the audits program and institutional setting in more detail. We then present the data on irregularities in local public management revealed by the audits program in section 3. We discuss our estimation approach in section 4 and present results in section 5. We conclude with a discussion of extensions.

2 Audits program and institutional setting

Our measure of governance at the local level is based on audit reports stemming from a policy of randomly selecting Brazilian municipalities for an audit of federal transfers, which we refer to as the random audit program. The program was initiated under the government of Luiz Inácio Lula da Silva in March 2003 with the explicit objective of fighting corruption. Audits are carried out approximately every 2 months. In each round, counties are sampled for a federal audit through a public lottery. The machinery used for the selection of counties is the same as that used for a popular national (money) lottery. As of July 2007, 24 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% in recent rounds. Eligibility is based on a county population threshold, which was successively raised from 20,000 to 500,000. Today, essentially all counties are eligible except for state capitals. There is little doubt that county sampling is random. Table I gives a comparison of average county characteristics for audited and non-audited counties and confirms that these are balanced. The table also shows that audits are balanced across mayors' political affiliations.

The random audits 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 headquarter in Brasilia determines the specific aspects of programs and projects that are audited and issues detailed inspection orders to state CGU branches. Teams of auditors that are based in the state offices of the CGU are then sent to the sampled county. Transfers eligible for audit include those that are earmarked to carry out national health and education policies (*legais*), direct transfers to citizens (*diretas*) as well as other negotiated transfers (*voluntarias*). Inspections occur for a subset of eligible federal transfers made during the preceding 2 years¹⁵.

The number of auditors dispatched depends on county size (area and population), the proportion of rural and urban areas and the number of items to be audited. 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 2 weeks in the county in order to carry out an inspection of financial reporting and of project execution in the field. 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 local prosecutors. County mayors are given the possibility to comment on the draft report within 5 business days. Auditors in turn explain whether or not they accept the mayor's justification of problems found.

Final 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 irregular-

¹⁵Exceptions to this rule are possible if warranted by the program under inspection.

ities uncovered by auditors and whether to press charges against particular individuals. If convicted of corruption, defendants may be imprisoned for 1 to 8 years, in addition to losing their mandate and incurring fines¹⁶. Because the judiciary cannot initiate proceedings on its own, prosecutors play a key role in the criminal justice system¹⁷. 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.

Having described some key features of the Brazilian control system, we now present our measure of local governance in more detail.

3 Data on local governance

The violations reported by auditors range from improper financial reporting to lack of oversight in project implementation to waste and actual theft of public resources. (See Appendix I for our translation of an extract from an audit report). The following quotes, translated from actual audit reports, illustrate the types of irregularities encountered by auditors.

In order to circumvent a more formal procedure required for procurement amounts above the legal threshold, the manager fragmented the purchase of medication into a series of smaller amounts. We also found that there exists no inventory control at the healthcare center and that expired medication has been purchased.

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

¹⁶See Arantes [2004] on the organization and legal instruments at the disposal of the Brazilian "Ministerio Publico".

¹⁷Prosecutors 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.

health ministry regulation which explicitly prohibits the use of federal transfers to this end.

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.

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.

Audit report findings were compiled into a database by a team of researchers directed by Francisco Ramos at the federal university of Pernambuco. Because of a data processing lag, our empirical analysis is based on a sample of 561 counties that have been audited through round 12.

Following the practice of the comptroller general's office, we refer to the reported violations of public sector rules and regulations as irregularities in public administration. It is worth emphasizing that each reported irregularity constitutes a breach of a specific legal norm. Although many irregularities would qualify as corrupt practices, in the sense of indicating abuse of public office for private (material) gain, most instances mainly reflect bad public management. Indeed, none of the examples above appear to unambiguously involve corruption. In all examples, however, managers were not exerting enough effort on their job, i.e. 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, neglected inventory control, diverted public funds from their intended use and failed to oversee project implementation by contractors.

While these practices for the most part reflect inefficient management of public resources they may also open the door for corruption and are thus undesirable on both accounts. They illustrate that distinguishing corruption from bad management more generally is very difficult in practice. Indeed, existing objective measures typically capture corruption together with more general forms of government inefficiency. This problem 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, however, 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-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].

Rather than attempting to disentangle corruption from mismanagement, we focus on the overall level of violations as well as major categories of irregularities committed in a given county. We believe that our broader focus on governance rather than corruption is justified since corruption is only one type of government failure determining overall government performance [Rose-Ackermann, 2004]. Similarly, distinguishing between irregularities that are committed knowingly and those that occur due to lack of capacity or ignorance of the appropriate financial management rules is exceedingly difficult in practice. Again, however, what matters for government performance is whether regulations are followed or not, and not so much the exact reasons for (non)compliance.

It is also worth noting that not all problems reported by auditors are under the control of county officials. We exclude those instances from our governance measures where auditors report on state or federal government failures or where reported irregularities are otherwise beyond local government control. We group irregularities according to whether they relate to procurement, program/project execution, financial reporting, program/project management, civil society oversight, remittance management, payments or other. See Appendix II for a more detailed description of these categories and relative frequencies of irregularities. The examples above reflect irregularities in procurement and program management, program management, procurement and project management respectively. We obtain offense rates by scaling the incidence of a given type of irregularity

by the number of civil servants in the county administration¹⁸. See Table II for summary statistics of the above categories scaled by the number of civil servants and broken down by judiciary seat status. We interpret these offense rates as measures of governance in local governments, a higher rate indicating worse governance.

While scaling by the potential number of offenders seems appropriate in general, there is the possibility that in our particular application we are scaling by an endogenous variable, which would complicate the interpretation of an estimated effect. Indeed one of the allegations of corruption and bad management at the local level that is often made is precisely the hiring of friends and relatives for government jobs. All else equal, this would result in a lower offense rate which is precisely the opposite of what our governance measure is supposed to capture. This problem is somewhat mitigated by the fact that auditors routinely check on civil servants' qualifications, which implies that the number of reported irregularities increases with unqualified nepotistic hiring. In order to test whether scaling by number of civil servants is crucial for our results we also use resident population, which is presumably exogenous. Using resident population as the denominator for the governance measure does not qualitatively alter the results in terms of sign and significance and so we focus our discussion on offenses per civil servant which is more appealing on theoretical grounds.

Our governance measure is closely related to the "government effectiveness" component of the World Bank's governance indicators [Kaufmann et al. 2003], which is focused on the "inputs required for the government to be able to produce and implement good policies and deliver public goods." The level of offenses committed by civil servants measures to what extent these inputs are either diverted, wasted or otherwise used suboptimally from a social point of view. We think that this governance measure is more reliable than existing measures as it is based on facts rather than perceptions¹⁹.

Despite this relative advantage, there are two major caveats worth mentioning. First, we assume that existing rules and regulations which define irregularities make sense, i.e.

¹⁸Scaling by the number of potential offenders is standard practice in the literature on the economics of crime. See, for example, Rubin, Shepherd and Dezhbakhsh [2003]

¹⁹See Kaufmann et al. [2006] for a discussion of current corruption measures.

they serve a legitimate purpose in a reasonable way²⁰. 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 calculations. 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, such as procurement and financial management and reporting procedures, reflect international best practices²¹. In order to partly address the worry about inefficient regulations, we break irregularities down into categories related to different stages of public service delivery. This allows for a more nuanced view of irregularities and their desirability from a social point of view.

The second major caveat to our governance measure is that we need to assume that reported irregularities are a fixed proportion of actual irregularities. This assumption would not hold, for instance, if auditors were themselves corrupt and could be bribed into manipulating audit findings [Mookherjee and Png, 1995]. If this manipulation were for some reason more likely in counties with judicial presence, it would invalidate our approach. 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 up to 10 people. This makes it hard to sustain collusion at any significant scale because the whole team has to be bribed in order to conceal irregularities. Third, the interaction between auditors and their "customers" is at a single point in time, which again makes it harder to sustain collusion.

²⁰Without this assumption we are still evaluating compliance. Evaluating local government efficiency is arguably more important, however.

²¹See PEFA [2006] for an overview of international standards in public financial management.

It could be argued, however, that even if auditors were incorruptible, the local elite might somehow manage to manipulate what gets uncovered and what remains unnoticed. While this scenario is plausible in general, it is unlikely in our case because local elites play no direct role in carrying out the audit. Auditors go into a county with specific orders to investigate particular programs and projects and the items on their list are not subject to local review. Neither is it likely that local managers succeed in systematically concealing irregular transactions such that auditors fail to uncover them. The audit is simply too thorough for this to happen as it involves both financial auditing and detailed inspection of public works and services.

The main potential reason why reported irregularities would not be a fixed proportion of actual irregularities is if auditors in the field spent disproportionate amounts of time and effort in a given county or if the CGU headquarter exercised discretion in the types of programs and amounts of resources included in the inspection orders issued to state CGU branches. As mentioned above, we know that the CGU headquarters sometimes uses discretion in allocating auditing resources to "high-risk" counties. It is thus not clear whether a higher reported offense rate reflects a higher propensity of county officials to commit irregularities or whether it simply reflects more intensive reporting. Ideally, we would scale reported irregularities by the number of audit man-hours spent in a given county in order to identify underlying propensities to commit irregularities. Unfortunately the audit man-hour data per county is not readily available so we use the amount of Reais (R\$) audited as a proxy measure. The dependent variable in all of the specifications below is therefore the incidence of irregularities per R\$ audited per civil servant²². As long as any remaining potential measurement error is not correlated with local judicial presence, our estimation approach will yield unbiased estimates of the treatment effect.

Table III gives summary statistics of offenses and scaling variables broken down by counties that were seats of the judiciary in 1999 and those that did not have such judicial presence. It appears that in counties without judicial presence there are on average 19

²²Results are significant at conventional levels and the sign is quantitatively similar (about -30%) when we deflate irregularities only by the amount audited and qualitatively similar (about -15%) when we deflate by the number of civil servants only. Results are available upon request.

offenses reported per 100 civil servants over a two year period. This is about twice the rate reported for judiciary seat counties. This result carries through when we scale by population.

Data on county characteristics are obtained from several sources. Information on county infrastructure, including the indicator for local judicial presence is taken from two surveys entitled “Perfil dos Municípios Brasileiros: Gestão Pública” for the years 1999 and 2001, conducted by the Instituto Brasileiro de Geografia e Estatística (IBGE). Population data are also from IBGE. Data on local income distribution, schooling and health outcomes and distance from state capitals are from the Instituto de Pesquisa Econômica Aplicada (IPEA) based on the 2000 census. Political participation data is from the Tribunal Superior Eleitoral (TSE). County total revenue is from the Ministério da Fazenda (MF). In the following section we discuss our estimation approach in more detail.

4 Estimation approach

We are interested in estimating the causal effect of judicial presence on the quality of local governance. The empirical challenge is that a correlation between judicial presence and irregularities does not imply causation because state judiciary officials might choose the location of the local judiciary seat at least partly in response to local governance conditions²³. In order to address reverse causality, we exploit a unique feature of the organization of state judiciary systems in Brazil: prosecutors and judges must reside in the county which serves as the seat of the judiciary district. The creation of judiciary districts, which typically encompass several counties, is based on county area, population size, electorate, county fiscal revenue and caseload of the judiciary as determined by national law²⁴. State laws specify necessary conditions for the creation of judiciary districts in terms of these observable characteristics with the explicit objective of facilitating citizens’ access to the judicial system. Although most state laws do not specify which county

²³See Eide [1998] for a review of the empirical literature on the economics of crime. See Polinsky and Shavell [2000] for a review of the economic theory of enforcement of law.

²⁴Lei Complementar N° 35, de 14 de Marco de 1979, Art. 95-97.

should be the seat of the judiciary district, empirically, the judiciary seat is located in the largest county in terms of population. Because the judiciary district-specific relative population rank determines treatment assignment, there are counties with nearly identical populations, some with and some without local judicial presence, which we exploit to impute counterfactual outcomes.

We illustrate the judiciary seat assignment mechanism for the majority of states in Brazil in Table IV below²⁵. The table presents probit estimation results explaining location of the judiciary seat in 2005 in terms of characteristics that determine the creation of judiciary districts and an indicator that equals 1 if the county had the maximum population in its district in 2005 and 0 otherwise. Table IV shows that the maximum population indicator is nearly a sufficient statistic for location of the judiciary seat. The indicator by itself explains 99% of judiciary seat locations and other characteristics that determine judiciary districts have almost no additional explanatory power. We conclude from this table that assignment to treatment is almost exclusively based on population. Because we cannot construct the indicator for maximum population for all states in our sample we present OLS regression results rather than IV results from a non-random subsample. IV results from the subsample are available on request. IV results are marginally larger in absolute value, which is not surprising given that the first stage is very strong.

A potentially more serious threat to validity of our strategy is that unobserved factors might be correlated with both judiciary seats and the incidence of irregularities. Given that the territorial organization in terms of judiciary districts is distinct from the territorial organization of local and state governments, however, we consider it unlikely that potentially confounding factors such as other public services like government banks or tax authorities systematically locate in counties with local judicial presence. Other factors have been identified by the recent literature on fighting corruption and we control for those in our estimations.

²⁵The sample includes all counties from the states of Amapa, Bahia, Amazonas, Ceara, Goias, Minas Gerais, Mato Grosso, Mato Grosso do Sul, Paraiba, Pernambuco, Piaui, Parana, Rio Grande do Norte, Rio Grande do Sul, Santa Catarina, Roraima. We are in the process of obtaining data on judiciary districts for the remaining states.

Control variables include measures of political competition at the local level, such as the number of political parties and whether the mayor faces a binding term limit [Ferraz and Finan 2005a] and measures of access to information such as the presence of local radio stations [Ferraz and Finan 2005a] and proximity of newspaper outlets [Reinikka and Svensson 2005a, 2005b]. Instead of proximity of newspaper outlets we use the presence of an internet provider in the county as a proxy measure for ease of access to information. We proxy for the level of social cohesion or social capital in a community using voter turnout [Zingales 2004]. The rationale for this measure is that because there are no economic or local legal incentives to vote, high voter turnout is likely to reflect a shared concern for public affairs²⁶. We also include average education of the local population [Glaeser and Saks 2006] as well as the geometric distance of counties from their respective state capitals because external control agencies such as the CGU or state courts of account are based in state capitals. Finally, we include a set of other control variables as listed in Table V below.

We use a semi-log specification in order to facilitate interpretation of the coefficient associated with judicial presence. The full estimation equation including economic and demographic county characteristics X is as follows:

$$\begin{aligned}
\ln(\text{offenses/R\$ audited/civil servants})_{ijt} &= \alpha_1(\text{seat of judiciary district})_{ijt-1} & (1) \\
&+ \alpha_2(\text{distance from state capital})_{ijt-1} \\
&+ \alpha_3(\text{mayor in second term})_{ijt-1} \\
&+ \alpha_4(\# \text{ of political parties})_{ijt-1} \\
&+ \alpha_5(\text{radio station})_{ijt-1} \\
&+ \alpha_6(\text{internet access})_{ijt-1} \\
&+ \alpha_7(\text{voter turnout})_{ijt-1} \\
&+ \beta_1 X_{ijt-1} + \beta_2 X_{ijt-1}^2 + a_j + cst + \varepsilon_{ijt}
\end{aligned}$$

²⁶Voting is mandatory in Brazil. While this national requirement may affect the level of voter turnout in Brazil, variation in local turnout is likely to be driven by concerns for the public good or social pressure, both of which are commonly associated with social capital. Zingales [2004] uses this measure to study the effect of social capital on financial development.

A final potential threat to the validity of our results is that the judiciary seat indicator may be capturing a non-linear relationship between population and offenses per civil servant²⁷. One way to address this issue would be to specify a more flexible functional form for population, using dummy variables for population brackets for example. Results may then hinge on the specification of the functional form, however. Instead we use the matching technique to estimate the judiciary seat effect non-parametrically. Essentially we calculate the average difference in offense rates for counties that are of similar size in terms of population but either have local judicial presence or not. We check validity of the results by ensuring that after matching, the two groups are comparable in observable characteristics.

More formally let $Y_i = Y_i(W_i)$ denote the outcome of interest, i.e. the incidence of irregularities per civil servant in county i and let $W_i \in \{0, 1\}$ indicate treatment status, i.e. whether the county has judicial presence or not²⁸. For a given county either $Y_i(1)$ or $Y_i(0)$ are observed but never both. We are interested in the effect of local judicial presence on the incidence of irregularities in public management: $\tau_i = Y_i(1) - Y_i(0)$. Since $Y_i(0)$ is not observed, we need to estimate $\hat{Y}_i(0)$, i.e. we want to estimate what the incidence of irregularities would have been had the county not had a local judicial presence. Matching estimators impute the missing potential outcome from average outcomes of counties with similar characteristics. Let $J_i =$ set of indices of units that are close to unit i according to some metric m , i.e. resident population in our case and let $\#J_i =$ number of units in set J_i . Then the missing outcome for treated units is estimated as

$$\hat{Y}_i(0) = \frac{1}{\#J_i} \sum_{l \in J_i} Y_l.$$

Letting N_1 stand for the number of treated (judiciary seat) units in the sample, the

²⁷ A related problem is that a parametric model as in equation 1 may extrapolate over regions of non-overlapping covariate support, which may bias treatment effect estimates. See, e.g., Heckman et al. [1998a].

²⁸The following discussion is based on Heckman et al. 1998b, Dehejia and Wahba [2002] and Abadie and Imbens [2006].

average treatment effect for the treated (ATT) is estimated as follows:

$$\hat{\tau}|_{W=1} = \frac{1}{N_{1i:W_i=1}} \sum (Y_i(1) - \hat{Y}_i(0))$$

For the matching estimator to identify and consistently estimate the ATT we need to assume that assignment to treatment is independent of potential outcomes, conditional on covariates X , and that the probability of assignment to treatment is bounded away from 0 and 1²⁹:

- (1) W is independent of $(Y(0), Y(1))$ conditional on $X = x$;
- (2) $c < P(W = 1|X = x) < 1 - c$, for some $c > 0$.

We have shown in Table IV that assignment to treatment in our case is based on one covariate: county population³⁰. Whether potential outcomes are independent of assignment to treatment depends on the existence of omitted variables that are correlated with local judicial presence and also affect governance. For the probability of assignment to treatment to be bounded away from 0 and 1 for a given pattern of covariates, there needs to be sufficient overlap in covariate support across treatment and comparison groups. Because the largest counties tend to be assigned to treatment there is a region of the covariate support for small counties for which assumption (2) is violated: $P(W = 1|X = x) = 0$. This implies that it is hard to estimate the average treatment effect for the control units (ATC) as there are only few treated units that could serve to estimate counterfactual outcomes. It is for this reason that we focus on the ATT. We show below that for the treated units reasonably similar comparison units do exist³¹.

5 Estimation results

Table VI gives estimation results for the total number of reported offenses per civil servant per R\$ audited and an expanding set of controls for county economic and demographic characteristics listed in Table V. We are mostly interested in the sign, magnitude and

²⁹For details on the regularity conditions see Abadie and Imbens [2006].

³⁰See Rubin [1977] for a discussion of this assignment mechanism.

³¹See Heckman et al. [1998a] for a discussion of this point.

statistical significance of the estimated parameter $\hat{\alpha}$, the coefficient on the judiciary seat dummy. The first row in Table VI shows that the size of this coefficient shrinks in absolute terms from about - 186% to about - 40% as more economic and demographic control variables are added. Larger counties in terms of population tend to have fewer irregularities per civil servant and since judiciary seat counties tend to be larger than non-seat counties, controlling linearly for population in column 2 substantially reduces the estimated coefficient. State and party affiliation effects introduced in column 3 do not appear to alter the point estimate of α_1 much. The introduction of county economic and demographic control variables in column 4 further reduces the effect of judicial presence on governance, although the effect remains highly statistically significant. Finally, the statistical significance of some of the square terms reported in column V points to the importance of appropriate specification of the functional form. The main result from Table VI is that the main coefficient of interest, $\hat{\alpha}_1$, is negative and statistically significant throughout. Taking column 6 in Table VI as the benchmark specification, the point estimate suggests that judicial presence reduces irregularities per civil servant by about 40% on average³².

Table VII shows that this result is very robust to the introduction of control variables more directly related to governance. In Column 1 we add the distance from state capitals which proxies for the extent of external top-down control over local governments. The point estimate of α_1 is almost unchanged and the coefficient on distance from capital is positive throughout although statistically insignificant. The sign of the coefficient is consistent with our argument about transaction costs reducing the probability of prosecution and detection for counties that are further away from enforcement agencies such as the CGU or state courts of account. Measures of political competition are introduced in columns 2 and 3 but these do not seem to be correlated with the location of the judiciary seat, resulting in unchanged point estimates of α_1 . While the number of political parties appears to be positively correlated with irregularities in local public management, the

³²Because the number of counties selected for federal audit varies by state and over time we also weigh observations by the inverse of their sampling probability. Results are quantitatively very similar to those obtained with unweighted observations.

mayor’s incumbency status does not seem to matter much. The indicator for existence of radio stations (column 4) and access to internet (column 5) have some predictive power as does our measure of social capital (column 6). None of these are correlated with judiciary seat location, however, resulting in unchanged point estimates of α_1 . Using the most conservative estimate, we find that the incidence of offenses is about 39% lower in counties with judicial presence. While we cannot rule out the possibility that unobserved omitted variables are driving our results we consider it unlikely, given our extended set of controls and the underlying quasi-random assignment of judiciary seats conditional on population.

In Table VIII we show estimation results broken down by offense category. For most categories there is a significant number of counties for which no offenses were reported. In order to account for this corner solution outcome at 0 we estimate Tobit models for the various categories of irregularities and only report OLS estimates when results are qualitatively the same as Tobit results. Breaking down the overall effect of judicial presence by offense category is useful as it facilitates and sharpens the interpretation of our results. Table VIII shows that the overall reduction in offenses per civil servant induced by judicial presence stems mostly from fewer instances of problems in project execution and project management, fewer violations of financial reporting and management regulations, fewer cases of non-existent or ineffective civil society oversight and fewer cases of improper handling of remittances to local residents. Our results thus suggest that judicial presence plays an important role in promoting selected aspects of good governance at the local level. Results also show that the distance from state capitals is mostly associated with more problems in local procurement. Access to information appears to reduce problems in local governance related to project execution and management as well as financial management. Finally, there is some evidence that there are more irregularities in project management in counties where the mayor is in his second and final term.

In order to address concerns about functional form specification, Table IX shows estimation results from matching on county population. Only treated units that have a

"close" (within 1000) match in terms of population are considered. At -36.8%, the estimated ATT is very close to the -39% estimate obtained in the full sample above and statistically significant. Table IX also reports sample averages for treatment and comparison group characteristics after single nearest-neighbor matching³³. Panel B shows that relative to the full sample, the comparison group is on average much closer to the treatment group after matching on population. After matching, the two groups are also quite similar in absolute terms although there are some exceptions. Counties with judicial presence are more likely to have local radio stations, their voter turnout is slightly higher, they have smaller budgets, lower poverty gaps and higher gini coefficients³⁴.

Ideally, none of these differences should be statistically significant. The fact that they are raises the possibility that the estimated ATT is biased. Rather than judicial presence, differences in access to information, for example, may account for the observed average difference in irregularities per civil servant.

We attempt to remove some of the potential bias associated with these imbalances by estimating equation 1 for the matched sample, i.e. excluding those comparison group observations that were not used as matches for any treated unit. The estimate of α_1 reported in the second column of Table IX Panel A is very close to the estimate obtained in the full sample, suggesting that covariate imbalance is not fundamentally biasing the results. As a second robustness check we attempt to balance covariates across treatment and comparison groups more precisely by forcing a closer match between treated and comparison units³⁵.

Table X shows estimation results from matching on county population where only treated units that have a "neighbor" within 50 people are retained in the estimation sample. Panel B shows that closer matching eliminates the imbalance in sample means for all covariates except urbanization and the poverty gap measure, none of which are significant

³³Estimated ATT's are robust to higher number of matches but covariate balance is slightly worse due to the inclusion of more distant matches. Results are available upon request.

³⁴Although we do not try to match counties within state borders, a visual inspection of the geographical location of comparison counties suggests that they are not clustered in a given region, which reduces the potential for bias due to regional variation in the quality of governance.

³⁵Matching on the propensity score yields quantitatively similar results. See Heckman et al. [1998b] and Dehejia and Wahba [2002] for details of the propensity score matching procedure.

predictors of irregularities (Table VI). The estimated ATT is -35% and significant. The estimate of α_1 from the matched sample is -37%, which is again close to earlier results. Alternative specifications of the required closeness of each match yield quantitatively similar results. We conclude from this exercise that the observed reduction in irregularities for counties with judicial presence is very robust to specifications of functional form.

6 Conclusion

Our results support the hypothesis that judicial presence promotes good governance at the local level. We find that the incidence of offenses is about 35% lower in counties that serve as local seat of the judiciary branch. Showing the effectiveness of law enforcement in deterring public sector offenses is our main contribution to the empirical literature on ways to improve governance. Given the random sampling process, the result is likely to generalize to all counties with judicial presence in Brazil, not just those in our estimation sample. Although we were unable to estimate the effect of judicial presence for counties without such presence directly, it seems likely that scaling up judicial presence at the local level would reduce irregularities in public management.

Whether scaling up is advisable depends on the net benefits of such a policy. While the costs of an expansion of judicial presence to all local governments 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. A first step in this direction would be to quantify the cost savings and service delivery improvements stemming from judicial presence. As noted earlier, audit reports sometimes include an estimated amount of funds that were diverted, wasted or stolen. Because this amount is a preliminary estimate and not systematically reported by auditors it was not included in our database. By going back to the underlying audit reports we may be able to derive a rough estimate of the cost savings associated with judicial presence.

7 References

- Abadie A. and G. Imbens, 2006, "Large sample properties of matching estimators for treatment effects", *Econometrica*, 74, 1, 235-267
- Acemoglu, D., S. Johnson and J. A. Robinson, 2001, "The Colonial Origins of Comparative Development: An Empirical Investigation," *American Economic Review*, 91, 5, 1369-1401
- , 2002, "Reversal of Fortune: Geography and Development in the Making of the Modern World Income Distribution", *Quarterly Journal of Economics*, 117, 4, 1231-1294
- , 2005, "Institutions as the Fundamental Cause of Long-Run Growth", *Handbook of Economic Growth*, P. Aghion and S. Durlauf, eds., North Holland
- Acemoglu, D., and S. Johnson, 2005, "Unbundling Institutions", *Journal of Political Economy*, 113, 5, 949-995
- Aidt, T., 2003, "Economic Analysis of Corruption: A survey", *The Economic Journal*, 113, 632-652
- Alvarez, M., J. A. Cheibub, F. Limongi and A. Przeworski, 2000, *Democracy and Development: Political Institutions and Material Well-Being in the World, 1950-1990*, Cambridge: Cambridge University Press
- Andrabi T., J. Das and A. I. Khwaja, 2007, "Students Today, Teachers Tomorrow? Identifying Constraints on the provision of Education", unpublished manuscript
- Arantes, R. B., 2004, *The Brazilian "Ministerio Publico" and political corruption in Brazil*, Centre for Brazilian Studies, University of Oxford, Working Paper 50-04
- Bardhan P. and D. Mookherjee, 2000, "Capture and Governance at Local and National Levels", *American Economic Review*, 90, 2, 135-139

- Barro, R. J., 1999, "Determinants of Democracy", *Journal of Political Economy*, 107, S6, 158-183
- Becker, G., 1968, "Crime and Punishment," *Journal of Political Economy*, 76, 2, 169-217
- Berkowitz D. and K. Clay, 2006, "The Effect of Judicial Independence on Courts: Evidence from the American States", *Journal of Legal Studies*, 35, 399-440
- Bowles, R., 1999, "Corruption" in B. Bouckaert and G. de Geest (eds.) *Encyclopedia of Law and Economics, Volume V, Chapter VIII*, Cheltenham, Edward Elgar, 2000
- Corman, H. and H. N. Mocan, 2000, "A Time-Series Analysis of Crime, Deterrence, and Drug Abuse in New York City", *American Economic Review*, June, 90, 3, 584-604
- Di Tella, R., and E. Schargrotsky, 2003, "The Role of Wages and Auditing During a Crackdown on Corruption in the City of Buenos Aires." *Journal of Law and Economics*, 46, 269-292
- , 2004, "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces after a Terrorist Attack", *American Economic Review*, 94, 1, 115-133
- Dehejia R. H. and S. Wahba, 2002, "Propensity Score Matching Methods for Non-Experimental Causal Studies", *Review of Economics and Statistics*, 84, 1, 151-161
- Dezhbakhsh H., Rubin P. H. and J. M. Shepherd, 2003, "Does Capital Punishment Have a Deterrent Effect? New Evidence from Postmoratorium Panel Data", *American Law and Economics Review*, 5, 2, 344-376
- Djankov, S., R. La Porta, F. Lopez-de-Silanes and A. Shleifer, 2003, "The New Comparative Economics," *Journal of Comparative Economics*, 31, 4, 595-619
- Eck, J. and E. Maguire, 2000, "Have Changes in Policing Reduced Violent Crime? An Assessment of the Evidence," in A. Blumstein and J. Wallman, eds., *The crime drop in America*. New York: Cambridge University Press, 207-65

- Eide, E., 1998, "Economics of criminal behavior", in B. Bouckaert and G. de Geest (eds.), *Encyclopedia of Law and Economics, Volume V, Chapter VIII*, Cheltenham, Edward Elgar, 2000
- Ferraz, C. and Finan F., 2005a, "Reelection Incentives and Political Corruption: Evidence from Brazilian Audit Reports", unpublished manuscript, University of California at Berkeley
- , 2005b, "Exposing corrupt politicians: the effect of Brazil's publicly released audits on election outcomes", unpublished manuscript, University of California at Berkeley
- Glaeser, E. L. and C. Goldin, 2004, "Corruption and Reform: An Introduction", NBER Working Paper No. 10775
- Glaeser, E. L. and R. Saks, 2006, "Corruption in America", *Journal of Public Economics*, 90, 1053-1072
- Glaeser, E. L., R. La Porta, F. Lopez-de-Silanes and A. Shleifer, 2004, "Do Institutions Cause Growth?", *Journal of Economic Growth*, 3, 271-303
- Hall, R. E. and C. I. Jones, 1999, "Why Do Some Countries Produce so Much More Output per Worker than Others?", *Quarterly Journal of Economics*, 114, 1, 83-116
- Golden, M. A. and L. Picci, 2005, "Proposal for a New Measure of Corruption, Illustrated with Italian Data", *Economics and Politics*, 17, 37-75
- Heckman, J., Ichimura, H., Smith J. and Todd P., 1998a, "Characterizing Selection Bias Using Experimental Data", *Econometrica*, 66, 5, 1017-1098
- Heckman, J., Ichimura, H., and Todd P., 1998b, "Matching As An Econometric Evaluation Estimator", *Review of Economic Studies*, 65, 261-294
- Hellman, J., Jones, G. and Kaufmann, D., 2000, "“Seize the State, Seize the Day” State Capture, Corruption and Influence in Transition Policy" *Research Working Paper - The World Bank*

- Initiative for Policy Dialogue, *Decentralization and Intergovernmental Relations*, forthcoming task force report
- Kaufmann D., A. Kraay and M. Mastruzzi, 2003, "Governance Matters III: Governance Indicators for 1996–2002", World Bank Research Paper
- , 2006, "Measuring Corruption: Myths and Realities", *Development Outreach*, September
- Knack, S. and P. Keefer, 1995, "Institutions and Economic Performance: Cross-Country Tests Using Alternative Measures ", *Economics and Politics*, 7, 3, 207-27
- Levitt, Steven, 2002, "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Reply", *American Economic Review*, 92, 4, 1244–50
- La Porta, R., F.Lopez-de-Silanes, C. Pop-Eleches and A. Shleifer, 2004, "Judicial Checks and Balances" *Journal of Political Economy*, 112, 2, 445-470
- Mauro, P., 1995, "Corruption and Growth," *Quarterly Journal of Economics*, 110, 681-712
- Marvell, T. and C. Moody, 1996, "Specification Problems, Police Levels, and Crime Rates", *Criminology*, November, 34, 4, 609–46
- Mookherjee D. and I.P.L.Png, 1995, "Corruptible Enforcers: How Should They Be Compensated?", *The Economic Journal*, 105, 428, 145-159
- Olken, B. A., 2007, "Monitoring corruption", *Journal of Political Economy*, forthcoming
- Pande, R. and C. Udry, 2006, "Institutions and Development: A View from Below", unpublished manuscript
- Public Expenditure and Financial Accountability partnership program, 2006, *Public Financial Management Performance Measurement Framework*, Washington DC

- Polinsky, A. & Steven S., 2000, "Public Enforcement of Law", in B. Bouckaert and G. de Geest (eds.), *Encyclopedia of Law and Economics, Volume V, Chapter VIII*, Cheltenham, Edward Elgar, 2000
- Przeworski, A., 2004a, "The Last Instance: Are Institutions the Primary Cause of Economic Development?", *European Journal of Sociology*, 45, 2, 165-188
- , 2004b, "Geography vs Institutions Revisited: Were Fortunes Reversed?", Mimeo, New York University
- Reinikka, R and J. Svensson, 2004, "Local Capture: Evidence from a Central Government Transfer Program in Uganda", *Quarterly Journal of Economics*, 2, 679-706
- , 2005a, "The Power of Information: Evidence from a Newspaper Campaign to Reduce Capture." Working paper, IIES, Stockholm University
- , 2005b, "Fighting Corruption to Improve Schooling: Evidence from a Newspaper Campaign in Uganda", *Journal of the European Economic Association*, 3, 2-3, 259-267
- Ríos-Figueroa, J., 2006, *Judicial Independence: Definition, Measurement, and Its Effects on Corruption. An Analysis of Latin America*, PhD Dissertation, New York University
- , 2007, "Fragmentation of Power and the Emergence of an Effective Judiciary in Mexico, 1994-2002", *Latin American Politics and Society*; 49, 1, 31
- Rodrik, D., A. Subramanian and F. Trebbi, 2002, "Institutions Rule: The Primacy of Institutions over Geography and Integration in Economic Development", NBER Working Paper 9305, National Bureau of Economic Research (October)
- Rose-Ackerman, S. 1999, *Corruption and Government, Causes, Consequences and Reform*, Cambridge University Press
- , 2004, "Governance and Corruption" in *Global Crises, Global Solutions*, B. Lomborg, ed. Cambridge: Cambridge University Press, chapter 6.

- Rubin, D., 1977, "Assignment to a Treatment Group on the Basis of a Covariate," *Journal of Educational Statistics*, 2, 1, 1-26
- Svensson, J., 2005, "8 Questions about Corruption", *Journal of Economic Perspectives*, 19, 3, 19-42
- Zingales L., Guiso L. and P. Sapienza, 2004, "The Role of Social Capital in Financial Development", *The American Economic Review*, 94, 526-556

8 Appendix I

National program for agricultural development

Activity: financial assistance for municipal infrastructure and public service projects.

Objective: financial support to municipal governments with a view to implement, modernize, amplify, rationalize and reallocate infrastructure related to small scale agricultural development.

Inspection orders: 149529 and 149532

Objects inspected: Rural electrification, sewage and transportation systems

Local executing agent: government executive branch

Type of transfer: Contract, n.º 105034-13.

Financial resources transferred: R\$ 163.127,92

Scope of inspection: Total amount

3.1) Irregularity in procurement related to the electrification project

Fact:

Out of three firms that participated in the tendering, two of them were represented by the same engineer who also wrote the project specifications. This is contrary to art. 9, of law n.º 8.666/93 – which prohibits the participation of the author of the project in the tendering- and reduces effective competition, which is contrary to § 1.º, art. 3.º, of the same law.

Evidence: Analysis of related procurement documentation.

Mayor's justification: No comment.

Auditors' conclusion: Irregularity maintained.

9 Appendix II

Procurement (15.2% of 28'727 irregularities in total)

Irregularities in this category relate mostly to limited competition for contracts and attempts to obstruct oversight by both auditors and stakeholders in the procurement process. The following are the types of irregularities included in this category: absence of preliminary price survey (7.7% of procurement related irregularities), missing/incomplete procurement documentation (23%), irregular composition/capture of the procurement commission (10.2%), invitation for bids to less than three firms (9.1%), fractionalizing of procurement amounts (4.5%), non-selection of the lowest bid among those that meet project requirements (2.8%), evidence of price collusion (1%), inappropriate procurement modality (less competition) (11.2%), inadequate/incomplete publication of tender information (14%), participating ineligible firm (11.7%), other (4.6%)

Program/project execution (27.3%)

This category includes irregularities related to the execution of new programs and capital projects. Irregularities include the following: diversion of project resources (10.8%), partial project execution (11.2%), substandard project execution (9.1%), project not executed (9.6%), lacking oversight of project implementation (13.6%), irregular project documentation (14%), inadequate project inputs (3%), time overruns (3.4%), other (4%). Also included in this category are instances where program execution is impaired by a lack of infrastructure (8.1%), matching grant requirements are not met by local governments (5.8%), or staff members have inadequate training (7.2%)

Financial reporting (14.5%)

This category includes irregularities related to the quality and timeliness of financial reporting by local governments. Public officials involved in committing irregularities are for the most part financial managers. Irregularities include the following: irregular/non-existent financial report (64.6%), irregular/non-existent receipts (27.9%), untimely reporting (5.9%), other (1.5%)

Program/project management (9.9%)

Irregularities in this category are related to management of existing public capital projects and services. Irregularities are as follows: irregular sale of equipment (2.5%), non-existent equipment utilization control (13.6%), non-existent inventory control (24.2%), inadequate equipment/inventory maintenance (24.7%), completed but unused projects (13.8%), inappropriate use of equipment (16.5%), non-existent project/service (3.7%), other (1%)

County financial management (4.1%)

Irregularities in this category are related to county financial management. Irregularities are as follows: excess cash holdings (opportunity cost) (8.5%), emission of checks without justification (37.8), irregular account management (36.7), spending without appropriation (10.7%) and other (6.2%)

Civil society oversight (6%)

Irregularities in this category are related to civil society oversight. Irregularities are as follows: non-existent civil society council (13.5%), inefficient/non-existent oversight (76.2%), irregular council composition (1.7%), evidence of council capture by mayor (6.4%), other (2.3%)

Remittance management (11.5%)

Irregularities in this category are related to management of remittances to individuals. Irregularities are as follows: remittance to ineligible individuals (16.9%), partial or non remittance (19.8%), non-existent school attendance verification (12%), number of beneficiaries below target (6.8%), incomplete register of beneficiaries (15%), delayed remittances (3.8%), duplication of remittance (2.2%), other (23.6%)

Irregular payments (3.2%)

Irregularities in this category are related to unjustified/undocumented or excessive payments for goods and services.

Other irregularities (8.3%)

Table I Panel A: Means and standard errors of county characteristics

Variable	Audited counties	NOT audited counties
	Mean (Std. Err.)	Mean (Std. Err.)
Resident population ('000)	29.2 (3.0)	31.4 (2.8)
County total revenue ('000'000)	17.9 (1.9)	24.0 (3.1)
Average years of schooling (years) (people 25 and older)	3.9 (0.05)	4.0 (0.02)
Civil servants (#)	658.0 (36.8)	683.0 (35.1)
Income per capita (R\$)	119.7 (3.0)	123.2 (1.0)
Poverty (%) (National poverty line R\$ 37.75/month)	26.1 (0.8)	24.7 (0.3)
Life expectancy (years)	67.4 (0.2)	67.7 (0.1)
Infant mortality (%)	46.2 (1.2)	44.5 (0.4)
Sample size	550	4956

Note: all data for year 2000

Panel B: Party affiliations of mayors in Brazil and in audited counties

Mayor's Party	Brazil	%	Audited counties	%
PMDB	1254	22.69	127	22.64
PFL	1025	18.55	102	18.18
PSDB	988	17.88	105	18.72
PP (Ex-PPB)	617	11.16	60	10.7
PTB	397	7.18	29	5.16
PDT	287	5.19	33	5.88
PL	233	4.22	25	4.46
PT	174	3.15	17	3.03
PPS	163	2.95	16	2.85
PSB	131	2.37	16	2.85

Note: only largest parties listed in this table

Table II : Summary statistics of irregularities by category

Variable	Judiciary Seat	Mean	Std. Dev.	Min	Max
Procurement	N	0.027	0.039	0	0.413
	Y	0.016	0.020	0	0.155
Program/project execution	N	0.052	0.042	0	0.231
	Y	0.027	0.023	0	0.132
Financial reporting	N	0.030	0.030	0	0.217
	Y	0.014	0.015	0	0.132
Program/project management	N	0.020	0.021	0	0.188
	Y	0.009	0.009	0	0.061
Financial management	N	0.008	0.012	0	0.070
	Y	0.004	0.005	0	0.045
Civil society oversight	N	0.015	0.013	0	0.073
	Y	0.006	0.006	0	0.036
Remittance management	N	0.025	0.023	0	0.122
	Y	0.011	0.011	0	0.059
Irregular payments	N	0.007	0.009	0	0.055
	Y	0.003	0.004	0	0.033
Irregular service charge	N	0.001	0.003	0	0.018
	Y	0.001	0.002	0	0.016
Other	N	0.011	0.016	0	0.098
	Y	0.008	0.012	0	0.083

Note: 299 out of 550 counties in our estimation sample serve as seat of the judiciary district.
All categories scaled by the number of civil servants in the county administration.

Table III : Summary statistics of offenses and scaling variables

Variable	Judiciary Seat	Mean	Std. Dev.	Min	Max
Number of offenses	N	44	25	2	174
	Y	56	32	1	244
Number of civil servants	N	317	248	55	1801
	Y	976	1120	118	11696
Amount audited ('000)	N	2473	3545	129	38200
	Y	10200	35000	311	584000
Offenses per R\$ 1m audited	N	30.15	23.74	1.82	149
	Y	15.53	20.52	0.07	280
Offenses per R\$ 1m audited per civil servant	N	0.165	0.217	0.003	1.87
	Y	0.036	0.073	0.00	1.00
Offenses per civil servant	N	0.190	0.140	0.008	0.817
	Y	0.095	0.075	0.001	0.615
County population ('000)	N	10.03	11.78	1.27	142.38
	Y	37.34	49.07	3.56	449.48
Offenses per county resident	N	0.007	0.005	0.000	0.033
	Y	0.003	0.003	0.000	0.025
Civil servants per capita	N	0.038	0.016	0.006	0.112
	Y	0.030	0.012	0.007	0.089
Civil servants without entry examination (%)	N	0.33	0.32	0.00	1.00
	Y	0.40	0.34	0.00	1.00

Note: 299 out of 550 counties in our estimation sample serve as seat of the judiciary district.

Table IV: determinants of judiciary seat location

Dependent Variable: Judiciary seat					
Max population	0.95 (0.005)**	0.95 (0.005)**	0.85 (0.028)**	0.85 (0.028)**	0.85 (0.031)**
Area ('000)		17.75 (7.24)*	11.82 (6.67)	14.2 (6.39)*	17.85 (7.46)*
Population ('000)			0.01 (0.001)**	-0.011 (0.011)	-0.018 (0.011)*
Electorate ('000)				0.043 (0.015)**	0.055 (0.015)**
Total revenue (‘000’000’000)					-4.82 (4.52)
Observations	3874	3874	3874	3872	3551
Percent predicted	99	99	99	97	97

Notes: Table gives marginal effects after probit estimations. Dependent variable equals 1 if county was the local judiciary seat in 2005 and 0 otherwise. Max population equals 1 if the county had the largest population in its district in 2005 and 0 otherwise. Standard errors in parentheses. *, ** indicate significance at 5 percent and 1 percent levels respectively.

Table V : Summary statistics of covariates

Variable	Judiciary Seat	Mean	Std. Dev.	Min	Max
Distance from state capital (km)	N	264	162	17	736
	Y	216	158	12	821
Mayor's second term (0/1)	N	0.43	0.50	0	1
	Y	0.40	0.49	0	1
Political parties (#)	N	2.50	0.86	1	7
	Y	3.00	1.05	1	7
Radio station (0/1)	N	0.28	0.45	0	1
	Y	0.68	0.47	0	1
Internet access (0/1)	N	0.08	0.28	0	1
	Y	0.40	0.49	0	1
Voter turnout (%)	N	0.87	0.06	0.62	0.99
	Y	0.85	0.06	0.58	0.97
Resident population ('000)	N	10.0	11.8	12.7	142.4
	Y	37.3	49.1	3.6	449.5
Electorate ('000)	N	6.5	6.5	1.2	74.7
	Y	23.8	30.8	2.5	313.2
Total county revenue ('000)	N	5627	10800	3	137000
	Y	16200	27200	1629	273000
Income per capita	N	147	80	41	440
	Y	174	99	42	834
Average years of schooling (people 25 and older)	N	3.68	1.15	0.81	7.59
	Y	4.21	1.29	1.44	8.75
Life expectancy	N	66.99	4.86	55.91	77
	Y	67.55	4.07	55.91	78
Urban population (%)	N	0.53	0.24	0.12	1
	Y	0.65	0.21	0.15	1
Poverty (%) (National poverty line R\$ 37.75/month)	N	0.28	0.18	0.01	0.71
	Y	0.26	0.17	0.01	0.75
Poverty gap (%)	N	0.51	0.10	0.25	0.78
	Y	0.51	0.09	0.20	0.79
Gini coefficient	N	0.56	0.06	0.39	0.73
	Y	0.58	0.06	0.45	0.78

Note: 299 out of 550 counties in our estimation sample served as seat of the judiciary district in 1999.
All data for years 1999 or 2000.

Table VI: Determinants of local governance

Dependent Variable: ln(offenses/R\$ audited/civil servants)						
Judiciary seat	-1.865 (0.119)***	-1.241 (0.132)***	-1.155 (0.133)***	-0.925 (0.109)***	-0.674 (0.102)***	-0.399 (0.103)***
Resident population		-0.023 (0.004)***	-0.022 (0.004)***	-0.059 (0.009)***	-0.075 (0.014)***	-0.062 (0.014)***
County electorate				0.064 (0.018)***	0.050 (0.021)**	-0.027 (0.024)
Income per capita				-0.002 (0.001)*	-0.005 (0.002)***	-0.004 (0.004)
Average years of schooling				-0.090 (0.098)	0.234 (0.267)	0.311 (0.261)
Urbanization				-0.846 (0.266)***	-1.227 (0.952)	-1.136 (0.918)
Poverty gap				0.053 (0.589)	-0.964 (3.280)	-0.332 (3.020)
Gini coefficient				-3.398 (1.000)***	-20.835 (9.999)**	-17.448 (9.147)*
(Resident population)^2					0.000 (0.000)***	0.000 (0.000)***
(County electorate)^2					-0.000 (0.000)	0.000 (0.000)***
(Income per capita)^2					0.000 (0.000)***	0.000 (0.000)
(Average years of schooling)^2					-0.021 (0.037)	-0.034 (0.034)
(Urbanization)^2					0.586 (0.878)	0.695 (0.838)
(Poverty gap)^2					0.883 (3.258)	0.394 (3.004)
(Gini coefficient)^2					15.736 (8.688)*	13.346 (7.951)*
(Resident population)^3						-0.000 (0.000)***
(Income per capita)^3						0.000 (0.000)
State and Party affiliation effects	N	N	Y	Y	Y	Y
Observations	547	547	547	547	547	547
R-squared	0.30	0.55	0.66	0.71	0.76	0.78

Notes: OLS regressions. RHS variables for years 1999 or 2000. Robust standard errors in parentheses. *, ** and *** indicate significance at 10, 5 and 1 percent levels respectively.

Table VII: Determinants of local governance

Dependent Variable: ln(offenses/R\$ audited/civil servants)						
Judiciary seat	-0.396 (0.102)***	-0.396 (0.103)***	-0.409 (0.102)***	-0.389 (0.101)***	-0.399 (0.100)***	-0.395 (0.100)***
Distance from capital	0.042 (0.057)	0.043 (0.057)	0.058 (0.059)	0.066 (0.060)	0.052 (0.059)	0.067 (0.060)
Mayor's second term		0.018 (0.079)	0.040 (0.082)	0.042 (0.083)	0.059 (0.083)	0.056 (0.082)
# of political parties			0.098 (0.053)*	0.098 (0.053)*	0.104 (0.053)*	0.103 (0.053)*
Radio station				-0.152 (0.099)	-0.186 (0.099)*	-0.183 (0.099)*
Internet access					0.295 (0.147)**	0.269 (0.148)*
Voter turnout						1.886 (0.735)**
Observations	547	547	547	547	547	547
R-squared	0.78	0.78	0.79	0.79	0.79	0.79

Notes: OLS regressions. RHS variables for years 1999 or 2000. Additional control variables (not shown) include state and party affiliation effects as well as the following variables and their squares: resident population, electorate, income per capita, average years of schooling, urbanization, povertygap, gini coefficient. Robust standard errors in parentheses. *, ** and *** indicate significance at 10, 5 and 1 percent levels respectively.

Table VIII Panel A: Determinants of governance by category

Dependent Variable: Offenses/R\$ audited/civil servants in	Procurement	Project execution	Financial reporting	Project management	Financial management
Judiciary seat	-1.28 (2.40)	-0.38 (0.11)***	-0.56 (0.12)***	-3.99 (1.55)***	-1.91 (0.84)**
ln(distance from state capital)	2.85 (1.31)**	0.05 (0.06)	0.07 (0.07)	0.54 (0.84)	0.22 (0.46)
# of political parties	-0.56 (1.05)	0.07 (0.05)	0.12 (0.05)**	-0.84 (0.67)	-0.07 (0.37)
Mayor's second term	2.75 (1.88)	0.01 (0.08)	0.070 (0.10)	2.72 (1.21)*	-0.47 (0.64)
Internet access	0.33 (3.15)	0.21 (0.17)	0.22 (0.16)	1.34 (2.01)	1.44 (1.11)
Radio station	2.00 (2.25)	-0.29 (0.11)***	-0.11 (0.11)	-2.69 (1.44)*	-1.52 (0.002)**
Voter turnout	31.42 (19.10)	1.59 (0.80)**	2.49 (0.88)***	11.27 (12.25)	13.20 (6.70)**
Observations	547	542	529	547	547
R-squared		0.75	0.74		

Notes: OLS regressions for columns 2 and 3 (ln(Y)), Tobit model for columns 1, 4 and 5 (unconditional marginal effects). Additional control variables (not shown) as in column 6 Table VI.

Panel B: Determinants of governance by category continued

Dependent Variable: Offenses/R\$ audited/civil servants in	Civil society oversight	Remittance management	Irregular payments	Irregular service charge	Other
Judiciary seat	-2.99 (1.38)**	-4.96 (2.19)**	-0.98 (0.78)	0.06 (0.14)	-1.19 (1.05)
ln(distance from state capital)	0.26 (0.75)	1.07 (1.19)	0.53 (0.42)	-0.07 (0.08)	0.09 (0.57)
# of political parties	-0.59 (0.60)	-0.60 (0.95)	-0.41 (0.34)	0.00 (0.06)	0.08 (0.45)
Mayor's second term	1.04 (1.08)	-0.82 (1.71)	-0.54 (0.60)	-0.13 (0.10)	-0.25 (0.82)
Internet access	2.00 (1.80)	3.90 (2.85)	0.27 (1.04)	0.17 (0.17)	1.10 (1.37)
Radio station	-1.99 (1.29)	-2.96 (2.04)	-0.44 (0.72)	-0.09 (0.13)	1.12 (0.98)
Voter turnout	15.53 (10.91)	18.20 (17.38)	10.30 (6.13)*	0.50 (1.14)	9.50 (8.23)
Observations	547	547	547	547	547

Notes: Tobit models for all columns (unconditional marginal effects). RHS variables for years 1999 or 2000. Additional control variables (not shown) as in column 6 Table VI.

Table IX Panel A: Estimation results

	Matching estimator	OLS restricted to matched sample
ln(offenses/ R\$ audited/ civil servant)	ATT -0.368 0.112	α_1 -0.336 0.094

Notes: Single nearest neighbor matching with replacement. 86 treated units that have no comparison units within a 1000 people radius are dropped from the sample. Comparison group observations are weighted by their matching frequency. OLS model is equation 1 in the text without state fixed effects.

Panel B: Mean differences in covariates

Variable	Treated	Comparison	t-stat.	p-value
ln(distance from state capital)	5.156	5.144	0.15	0.883
Second term (0/1)	0.429	0.458	-0.59	0.559
Political parties (#)	2.820	2.707	1.35	0.178
Radio station (0/1)	0.594	0.467	2.64	0.009
Internet access (0/1)	0.259	0.235	0.56	0.575
Voter turnout (%)	0.845	0.834	1.70	0.090
Population ('000)	18.999	19.013	-0.01	0.989
Electorate ('000)	12.648	11.642	1.52	0.129
Revenue ('000)	8081	9865	-2.25	0.025
Income per capita	156	156	-0.02	0.982
Schooling	3.874	3.805	0.54	0.588
Urbanization (%)	0.597	0.580	0.77	0.442
Poverty gap (%)	0.501	0.534	-3.70	0.000
Gini coefficient	0.580	0.558	4.32	0.000
N	212			

Table X Panel A: Estimation results

	Matching estimator	OLS restricted to matched sample
ln(offenses/ R\$ audited/ civil servant)	ATT -0.350 0.179	α_1 -0.369 0.188

Notes: Single nearest neighbor matching with replacement. 224 treated units that have no comparison units within a 50 people radius are dropped from the sample. Comparison group observations are weighted by their matching frequency. OLS model is equation 1 in the text without state fixed effects.

Panel B: Mean differences in covariates

Variable	Treated	Comparison	t-stat.	p-value
ln(distance from state capital)	5.127	5.085	0.34	0.735
Second term (0/1)	0.405	0.418	-0.17	0.868
Political parties (#)	2.635	2.689	-0.38	0.707
Radio station (0/1)	0.459	0.337	1.51	0.133
Internet access (0/1)	0.148	0.094	1.00	0.318
Voter turnout (%)	0.855	0.845	1.04	0.301
Population ('000)	12.681	12.678	-0.00	0.996
Electorate ('000)	8.889	8.025	1.64	0.103
Revenue ('000)	5995	5870	0.13	0.894
Income per capita	149	134	1.11	0.270
Schooling	3.793	3.639	0.79	0.432
Urbanization (%)	0.589	0.518	1.91	0.058
Poverty gap (%)	0.492	0.526	-2.18	0.031
Gini coefficient	0.580	0.566	1.56	0.121
N	74			