Audit Risk and Rent Extraction: Evidence from a Randomized Evaluation in Brazil*

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Abstract

We report results from a randomized experiment designed to test whether increased audit risk deters rent extraction in local public procurement and public service delivery. Our estimates suggest that temporarily increasing annual audit risk by about 20 percentage points reduced the proportion of local procurement processes involving mismanagement or corruption by about 17 percentage points. Higher audit risk also reduced the proportion of restricted procurement modalities adopted by local managers. In contrast, we find no evidence that increased audit risk affected the quality of publicly provided preventive and primary health care services, measured using client satisfaction surveys. We also find no evidence that higher audit risk had an effect on local compliance with national guidelines of the conditional cash transfer program *Bolsa Família*, measured in terms of appropriate inclusion of beneficiaries into the program or their compliance with health and education conditionalities.

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1 Introduction

Waste and corruption are two key determinants of the cost of public service provision. Keeping waste and corruption—rent extraction for short—low is important in its own right and is also widely believed to be a driver of economic development (Rose-Ackermann 1999, 2004). However, measuring rent extraction objectively is notoriously challenging.[1] It is even more challenging to assess whether rent extraction is responsive to policy intervention because top-down monitoring policies in particular are only rarely truly or "as if" randomly assigned.

In this paper we report results from a randomized evaluation designed to test whether higher audit risk deters corruption and waste in local public procurement and improves provision of public services in Brazil. Following the economic approach to crime (Becker 1968), an official will shirk or steal if and only if the expected utility from doing so exceeds utility under the person's best alternative. Expected utility depends on the magnitude of sanctions if caught and the probability of their application. While higher audit risk should lower the expected utility from shirking or stealing and hence deter rent extraction, the magnitude of this effect depends on the probability that sanctions are applied conditional on detection. In the Brazilian setting analyzed here, as in many other countries, the probability that local officials are punished through fines, loss of mandate or prison time is typically considered to be very low (Arantes 2004). To what extent higher audit risk deters waste and corruption in such environments is therefore an open and important empirical question.

Our research design relies on the randomization of 120 municipalities into a treatment group, exposed to a roughly 20 percentage points higher annual probability of being audited than the 5% audit probability in the control group, effectively consisting of the 5'400 remaining municipalities in Brazil.[2] The randomization was designed by the Brazilian federal government internal audit agency (*Controladoria-Geral da União,* CGU) and carried out and publicly an-

[1] Di Tella and Schargrodski (2003) look at prices paid by hospitals for basic supplies before and after a crackdown on corruption. Reinikka and Svensson (2004) examine the difference between funds disbursed by the central government and funds reportedly recieved by schools. Golden and Picci (2005) compare physical public infrastructure to the cumulative amount of government spending on that infrastructure. Olken (2007) computes "missing" expenditures in road construction using independent cost estimates provided by engineers. Ferraz and Finan (2010) construct corruption measures based on audit findings. Litschig and Zamboni (2010) also use audit results to measure rents, but without distinguishing between waste and corruption.

[2] Municipalities are the lowest level of government in Brazil (below the federal and state governments).

nounced in May 2009.[3] In order to ensure that municipalities were aware of their treatment status, mayors in treatment group municipalities also received a letter from CGU, stating that they were part of a group of 120 municipalities, 30 out of which would be audited one year later.[4] In May 2010, CGU sampled 30 treatment as well as 30 control municipalities as part of the regular random auditing process. From May 2010 onwards, treatment group municipalities were again exposed to a roughly 5% annual audit probability.[5] The treatment thus consisted of a temporary increase in audit risk of about 20 percentage points. In order to increase sample size, we supplement the 60 municipalities sampled for an audit in May 2010 with 60 control group municipalities that were sampled two months earlier in March 2010.[6]

We measure rents as irregularities in local public procurement and service delivery uncovered by CGU auditors. If compliance with homogeneous national regulations is socially beneficial, irregularities in procurement or service delivery uncovered by auditors provide an objective measure of rent extraction by local executive officials, either through outright corruption or low effort on the job.[7] For the vast majority of the regulations considered by auditors in Brazil, compliance is likely to be socially beneficial although typically privately costly.[8] For example, procurement regulations are designed to ensure that the public pays the lowest price available for a given good or service required, yet implementing a competitive procurement procedure, such as a (reverse) auction, is privately costly for the local manager. Similarly, health ministry regulations require medical staff to provide certain service hours, which is again privately costly, yet beneficial for service users.

Our data on public procurement and service delivery irregularities are non-public and serve as the basis for the published audit reports used in Ferraz and Finan (2010), Brollo, Nannicini, Perotti, and Tabellini (2012), and Litschig and Zamboni (2012). The procurement data are at

[3]We introduced the idea of conducting a randomized evaluation to CGU staff and were involved in the early design stage of the project.

[4]This implies that we cannot disentangle the effect of simply receiving a letter from CGU from the effect of exposure to a higher audit probability. However, the effect of the letter "treatment" is likely to be orders of magnitude smaller than the effect of exposure to an objectively higher audit risk.

[5]Treatment group municipalities were therefore never exposed to lower audit risk than those in the control group.

[6]Our relatively small sample size precludes meaningful subgroup analysis. We have investigated, for example, whether higher audit risk has a different effect on rent extraction for first- or second-term mayors and found no economically or statistically significant difference there. Results are available on request.

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

[8]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.

the individual process level and span the entire range of locally provided public services in Brazil, including preventive and primary health care, elementary education, housing and urban infrastructure, and transportation. The service delivery data are based on locally representative household surveys conducted by CGU auditors as part of their standard field work. We focus on two nation-wide programs, the family and preventive health program (*Saúde da Família*) and the conditional cash transfer program (*Bolsa Família*).[9]

While we distinguish irregularities indicating mismanagement or corruption from what we call procedural irregularities—where the connection to inefficiency is only indirect—we do not attempt to identify proper corruption episodes.[10] Our reasons for doing so are twofold. First, irregularities are based on objectively verifiable facts, while identification of corruption inevitably requires judgment since few cases are clear-cut in practice. CGU auditors themselves explicitly abstain from making such judgments and leave it to prosecutors to decide whether to further investigate certain irregularities and potentially press charges against particular individuals. Our second reason 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 higher audit risk should operate on both corruption and administrative misconduct, a comprehensive measure of rents is more appropriate for our purposes.

Our main empirical result provides clear evidence in favor of the prediction that local officials reduce rent extraction in procurement in response to higher audit risk. Our estimates suggest that increasing annual audit risk by about 20 percentage points reduced the proportion of local procurement processes involving mismanagement or corruption by about 17 percentage points. Higher audit risk also reduced the proportion of restricted (and privately less costly to execute) procurement modalities adopted by local managers. Whether these impacts reflect a net reduction in rent extraction or merely a substitution over time—with high audit risk municipalities "making up" at least some lost rents in subsequent periods—we cannot say. In either

[9]There are other major programs, in education for example, as well as programs and projects that run only in a subset of municipalities, for which we do not have the survey data.

[10]Our mismanagement or corruption coding is almost identical to the "Broad corruption" coding in Brollo, Nannicini, Perotti, and Tabellini (2012), yet considerably broader than the corruption coding in Ferraz and Finan (2010). See Table 6 and Section 5 for a more detailed comparison.

case, however, the results provide strong empirical support for the economic approach to crime (Becker 1968).

In contrast, we find no evidence that increased audit risk affected the quality of preventive and primary health care services provided under the *Saúde da Família* program. Since potential punishments for serious irregularities in procurement include jail, while for service delivery they only include fines or loss of the job, differences in potential punishments might drive the difference in results. A complementary interpretation is that irregularities in service provision cannot be identified with the same precision as irregularities in procurement and so higher audit risk might matter less to service providers, compared to procurement officials. Irregularities in procurement are relatively easy to identify because local officials are required to document each step of the process. In contrast, the behavior of local service providers is much harder to verify through a CGU audit. For example, while health facility users might complain about infrequent opening hours of the health post, health staffers could easily dispute this fact and auditors would have a hard time verifying any of these competing claims.

We also find no evidence that higher audit risk had an effect on local compliance with national guidelines of the conditional cash transfer program *Bolsa Família*, measured in terms of appropriate inclusion of beneficiaries into the program or their compliance with health and education conditionalities. Again, differences in punishment are likely to be part of the explanation for the zero effect since the punishment for overstating the number of kids in the household or for not sending them to school, for example, is at most the loss of the benefit. Administrative consequences for the local program managers are similarly limited. Another interpretation, which is supported by our data, is that most *Bolsa Família* recipients were appropriately included in the program—they were poor enough—and they already complied with health and education conditionalities to a large extent.[11] Thus, they could not respond to higher audit risk because they were doing nothing wrong in the first place.

To our knowledge the only antecedent to our study using a (field) experimental research design is Olken (2007), who examines the effect of a higher audit probability on corruption in road

[11]While household visits allow auditors to assess inclusion errors into *Bolsa Família* fairly accurately, compliance with education and health conditionalities might of course be overstated by local officials.

construction in Indonesia. He finds that an increased probability of a government audit, from a baseline of 4 percent to 100 percent, reduces "missing" expenditures by 8 percentage points relative to total project expenditures. As in our case, Olken's research design essentially evaluates the effect of a temporary (and project-specific) increase in audit risk. Compared to the proportion of local procurement processes involving waste or corruption used in our study, Olken's measure of corruption is clearly more precise. The advantages of our procurement irregularities are that they measure rents more broadly, encompassing both waste and corruption, and that they are available for government procurement across the entire range of locally provided public services, not just for road construction. Moreover, the survey data on user satisfaction allow us to go beyond input measures, such as "missing" expenditures, and examine potential effects on outputs, such as quality of public services.

The paper is organized as follows. In Section 2 we describe the audits program and give institutional background on potential judicial, administrative, and political punishments that may arise from the detection of irregularities in the local public administration. Section 3 presents theoretical predictions regarding the effect of higher audit risk on shirking or stealing by local officials. We discuss the experimental design in Section 4. In Section 5 we present the data on irregularities in local public procurement and service delivery. In Section 6 we describe our estimation approach. Results are presented in Section 7. 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.[12] 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

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

national (money) lottery and results are broadcast on television and through other media. Sampling is geographically stratified by state. As of July 2010, 33 rounds have been carried out with 60 municipalities sampled in recent rounds.

The program is implemented by the general comptroller's office (CGU), the internal audit institution of the federal government. When a municipality 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 municipality. Transfers eligible for audit include those that are earmarked to carry out national health and education policies (*legais*), direct transfers to citizens (*diretas*), as well as other negotiated transfers (*voluntarias*), but exclude revenue-sharing transfers. Inspections occur for a subset of eligible federal transfers made during the preceding two to three years.[13]

The number of auditors dispatched depends on municipality 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 municipality 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 municipality 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 municipality sampling, auditors spend about two weeks in the municipality 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

[13]Exceptions to this rule are possible if warranted by the program under inspection.

appropriate by the prosecutor in charge of the municipality. Municipality 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 Potential judicial, administrative and political punishments

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. Reports are also released to the media.

Potential judicial punishments depend on prosecutors who decide whether to further investigate the irregularities uncovered by auditors and whether and what charges to press against particular individuals. If convicted of corruption, defendants may be imprisoned for 1 to 8 years, in addition to losing their mandate and incurring fines. If convicted of "acts of administrative misconduct" or "improbity", punishments include the loss of mandate, the suspension of political rights for 8 to 10 years, prohibition from entering into public contracts for 10 years as well as the obligation to reimburse public coffers.[14]

In addition to these potential judicial punishments, administrative and political punishments are also possible. For example, line ministries can stop transferring funds to the municipal administration if central government program managers deem the uncovered irregularities serious enough. This type of punishment is swift and potentially costly for the mayor in terms of electoral prospects, as emphasized in Brollo (2012). Even if funds are not reduced, voters may react to the mere release and local dissemination of audit findings by updating their views on the quality of the incumbent (Ferraz and Finan 2010). Again, this type of punishment is swift and potentially costly for mayors on election day.

3 Theoretical predictions

Following the economic approach to crime, an official will shirk or steal if and only if the expected utility from doing so exceeds utility under the person's best alternative. Expected

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

utility depends on the magnitude of sanctions if caught and the probability of their application. Using Becker's (1968) notation, let Y denote the income or monetary equivalent of committing an irregularity, f the fine or monetary equivalent of the punishment, p the probability that the punishment is applied and $U_i(Y)$ person i's utility function, which is assumed increasing in Y. The expected utility from shirking or stealing is then as follows:

$$E(U_i) = pU_i(Y - f) + (1 - p)U_i(Y)$$

In this simple framework, the person will shirk or steal if and only if $E(U_i) \geq \mu_i$, where μ_i denotes i's best alternative. It is clear that if higher audit risk increases p—thereby lowering the expected utility from shirking or stealing—some people will be deterred from committing an irregularity:

$$\frac{\partial E(U_i)}{\partial p} = U_i(Y - f) - U_i(Y) < 0$$

But the magnitude of this effect depends on the probability that sanctions are applied conditional on being audited. Let p_c denote the probability of sanctions conditional on receiving an audit and p_a the probability of a central government audit, so that $p = p_c \times p_a$.[15] Then:

$$\frac{\partial E(U_i)}{\partial p_a} = p_c \left[U_i(Y - f) - U_i(Y)\right] < 0$$

This equation makes it clear that the same variation in audit risk affects expected utility differently, depending on the probability that sanctions are applied conditional on being audited and depending on the severity of sanctions. Specifically, the predicted reduction of irregularities due to higher audit risk is stronger, the more likely it is that sanctions are applied conditional on detection and the more severe the punishment. Since in our case potential punishments for serious irregularities in procurement include jail, while for service delivery they only include fines or loss of the job, the economic approach to crime provides a simple interpretation of our differential results for procurement and service delivery. A complementary interpretation is that irregularities in service provision cannot be identified with the same precision as irregularities in procurement—p_c is likely lower in service delivery—and so higher audit risk should matter

[15]For simplicity we assume that the probability of detection of the irregularity conditional on being audited is 1.

less to service providers, compared to procurement officials.

4 Experimental design

The randomization was designed by the Brazilian federal government internal audit agency (*Controladoria-Geral da União,* CGU) and carried out on May 12 2009. The machinery used for the selection of treatment group municipalities was the same as that used for regular CGU audits and the results were later broadcast on television and through other media. The randomization of 120 municipalities into the treatment group was stratified by state as shown in Table 1. At the time of the randomization it was publicly announced that out of the 120 municipalities in the treatment group, 30 would be sampled for a regular CGU audit one year later in May 2010.[16] It was also announced that the 120 municipalities in the treatment group were not eligible for regular CGU audits until May 2010, while the control group, consisting of the remaining 5'400 municipalities, could be sampled during regular lotteries as usual.[17] In order to ensure that municipalities were aware of their treatment status, mayors in treatment group municipalities also received a letter from CGU containing the above information.

While the initially announced (ex ante) probability of an audit for treatment group municipalities was thus 25%, the corresponding annual audit risk for control municipalities depended on the number of lotteries and the probability of being sampled in each of these. From May 2009 to May 2010 there were four regular lotteries, namely the 29th, 30th, 31st and 32nd, as illustrated in Figure 1. Table 2 presents the audit probabilities that municipalities from different states faced in the 29th lottery. For most states, audit probabilities per round of the lottery—P(Draw)—were between 1 and 2 percent. These probabilities were essentially unchanged from previous rounds because setting aside 120 municipalities for the treatment group only marginally reduced the sample of municipalities eligible for audit in the rest of Brazil.

In the 32nd regular lottery, the details of which were announced on April 30 2010, 30 munic-

[16] Portaria N^{o} 930, May 8 2009.

[17] As mentioned above, state capitals and municipalities with population size above 500'000 are exempt from the random audits program. A few other municipalities had received special audits recently and were also exempt from the experiment (Portaria N^{o} 930, May 8 2009).

ipalities were drawn from the treatment group and 30 from the control group.[18] Table 3 shows that, because sampling in both groups was stratified by state, ex post audit probabilities in the treatment group varied between 16.7% and 50%, with a modal probability of 25%. Since the details of the actual sampling scheme used in May 2010 were unknown to the public until a few days before the 32nd lottery, the relevant annual audit risk for treatment group municipalities that could have affected the behavior of local officials likely was 25%.

Under the assumption that the probabilities of being drawn in the 29th, 30th, and 31stlotteries were the same as in the 29th lottery, the corresponding annual audit risk for control municipalities can be approximated as follows:

$$\begin{aligned} P(\text{Audit}|\text{Control}) &= 1 - P(\text{No Audit in any of lotteries 29 through 32}) \\ &= 1 - [1 - P(\text{Draw } 29^{th})] \times [1 - P(\text{Draw } 30^{th})] \\ &\quad \times [1 - P(\text{Draw } 31^{st})] \times [1 - P(\text{Draw } 32^{nd})] \\ &\simeq 1 - [1 - P(\text{Draw } 29^{th})]^3 \times [1 - P(\text{Draw } 32^{nd})] \end{aligned}$$

Table 3 shows that annual audit probabilities in the control group fell mostly in the range of 3 to 6 percent. Ex ante, that is from May 12 2009 to April 30 2010, treatment group municipalities were thus exposed to a roughly 20 percentage points higher annual probability of being audited than control group municipalities. From May 2010 onwards, treatment and control group municipalities were again exposed to the same audit risks they had been exposed to prior to May 2009. The treatment thus consisted of a temporary increase in audit risk of about 20 percentage points. In order to increase sample size, we supplement the 60 municipalities sampled for an audit in May 2010 with 60 control group municipalities that were sampled two months earlier, in March 2010. Note that these municipalities were exposed to exactly the same annual audit risk as the control group municipalities that were sampled in May 2010 (see Figure 1).

[18] Portaria N^{o} 862, April 30 2010.

5 Data

Having described some key features of the Brazilian control system and the experimental design, we now present our micro-data on irregularities in local public procurement and public service delivery in more detail. Our empirical analysis is based on a random sample of 60 + 60 municipalities that have been audited in March and May 2010, respectively. Audit findings for each municipality were compiled into a database by CGU staff. 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 or service provider and is potentially subject to prosecution by state procuracies.

5.1 Local public procurement data

Our procurement data are at the level of individual procurement processes and cover all purchases made with federal funds during the audit period, from January 2009 to May 2010 for the 32nd lottery and from January 2008 to December 2009 for the 31st lottery as illustrated in Figure 1.[19] The procurement data span the entire range of locally provided public services in Brazil, including preventive and primary health care, elementary education, housing and urban infrastructure, and transportation.

Table 4 presents the distribution of goods and services purchased by local governments for the two levels of audit risk—high vs. low—and by lottery. The unit of observation is an individual procurement process. Staple foods, used for a public school meal program, for example, are the most frequently acquired items. Other commonly purchased items are medications for the basic health care program, as well as other non-durable goods. Public works and contracted-out services also constitute a large fraction of local public procurements. Table 4 also shows that for most items there are no obvious differences between treatment and control municipalities in terms of the types of goods and services bought, nor are there difference between control

[19] Because the date of each procurement process is not given in our data, only the year, we cannot exclude processes that were completed prior to May 2009. The inclusion of these processes—which could not have been affected by higher audit risk by construction—will bias our estimates towards zero.

municipalities from the 31st and 32nd lotteries.[20] While the total number of processes is lower in the high audit risk group, there is no evidence that these municipalities received less funding from the central government, as shown in Section 7 below.

Table 5 presents the distribution of procurement modalities by the level of audit risk—high vs. low—and lottery. The unit of observation is again an individual procurement process. There are six modalities in total, three of which restrict the number of competitors and are legal only below certain purchase amounts, and another three modalities without restrictions on the number of competitors.[21] We refer to restricted procurement modalities as direct purchases by the local administration, "bids only by invitation" (*convite*), a modality which leaves it at the total discretion of the local administration whom to "invite",[22] and the modality "only pre-registered bidders" (*tomada de preços*), which restricts competition to pre-registered suppliers.[23] Unrestricted modalities are the "sealed-bid (reverse) auction" (*concorrência*), "on-site (reverse) auction" (*pregão presencial*) and "electronic (reverse) auction" (*pregão eletrõnico*).

A noteworthy feature of the data in Table 5 is that in the control group from the 32nd lottery, there were 189 procurement processes of the restricted modality "bids only by invitation", but there were only 98 processes using this modality in the treatment group. Similarly, of the modality "only pre-registered bidders", there were 66 processes in control group frm the 32nd lottery but only 44 of them in the treatment group. For the unrestricted modalities, "sealed-bid (reverse) auction", "on-site (reverse) auction" and "electronic (reverse) auction", the numbers of processes in treatment and control groups are essentially equal. It is also interesting to note that there are some differences in the proportions of procurement modalities between control municipalities from the 31st and 32nd lotteries, suggesting that pooling across lotteries may not be appropriate for these outcomes. The fact that in the high audit risk group there are fewer restricted modalities is consistent with the observation above that the number of procurement

[20]Nevertheless, from a statistical perspective, the three distributions are different according to Pearson's chi-square test.

[21]This distinction between procurement procedures that are open to all interested suppliers and those that are not is made in the Agreement on Government Procurement in Article VII.3. Brazil is not formally a member of the Agreement.

[22]This corresponds to a *limited tendering procedure* under the Agreement on Government Procurement, Article VII:3(c).

[23]This corresponds to a *selective tendering procedure* under the Agreement on Government Procurement, Article VII:3(b).

processes is also lower in this group.

Table 6 presents CGU auditors' classification of irregularities in procurement as well as corruption and mismanagement codings by ourselves (LZ), Ferraz and Finan (FF, 2010), and Brollo, Nannicini, Perotti, and Tabellini (BNPT, 2012). For example, one of the corruption categories in Ferraz and Finan, which they call "over-invoicing", in which "auditors determined that the goods and services were purchased at a value above market price", corresponds to our "unjustified or excessive payments for goods and services" type. Their "irregular public procurement", which is when "there is an illegal call-for-bids where the contract was awarded to a "friendly firm" and the public good was not provided" corresponds to a subset of our "simulated tender process", and "evidence of favoritism" types, where non-provision of the good or service was somehow confirmed, which we do not distinguish in our data. In procurement, a mismanagement episode occurs when "less than three firms bid for a public contract", corresponding to our "invitation for bids to less than three firms".

Brollo et al. (BNPT, 2012) also use the CGU audit reports to construct a narrow and a broad corruption measure. Table 6 shows that their broad corruption coding essentially corresponds to our mismanagement or corruption irregularities. Their narrow corruption measure includes cases of "limited competition", corresponding roughly to our "evidence of favoritism" category, "fraud", corresponding to our "simulated tender process", and "manipulation of the bid value", which we label "fractionalizing of procurement amounts", that is, division of a purchase into smaller amounts in order to avoid unrestricted procurement modalities. Their narrow definition of corruption also includes cases of "favoritism in the good receipt", which we do not distinguish in our data, as well as "over-invoicing", which amounts to our "unjustified or excessive payments for goods and services" category. In their broad measure of corruption, Brollo et al. include "an irregular firm wins the bid process", corresponding roughly to our "participating ineligible firm", "the minimum number of bids is not attained", which we label "invitation for bids to less than three firms", as well as "the required procurement procedure is not executed", which is our "procurement modality too restricted".

Table 7 presents the distribution of audit results for procurement by the level of audit risk—

high vs. low—and lottery. Several features of the data stand out. First, the share of irregular processes, that is, those that were found to be non-compliant with procurement regulations in one way or another is about 0.62 and 0.64 in the control groups from the 32nd and 31st lotteries, respectively, but only about 0.46 in the high audit risk group. Second, a comparison of audit findings across control municipalities from the 31st and 32nd lotteries reveals that the proportions are somewhat dissimilar for many categories. Since we are primarily interested in capturing evidence of mismanagement or corruption, rather than identifying proper corruption episodes, these differences are without consequence for our study. The important fact is that the share of procurement processes indicating mismanagement or corruption in the two control groups is very close, 0.44 for the 32nd and 0.49 for the 31st lottery, respectively, while the corresponding share in the high audit risk group is 0.27.

5.2 Survey data

As part of their standard service orders, CGU auditors conduct interviews and field visits that are designed to assess public service quality at both the household and service-unit level. For the preventive and basic health care program (*Saúde da Família*), auditors first check the compliance of service units with ministry of health guidelines, for example regarding adequacy of the number of service personnel for their assigned service area and adequacy of the team composition (e.g. one doctor, one nurse, 12 technical assistants). Auditors then sample households at random from locally provided sampling frames of potential service users. In our data, the auditors interviewed 22 families on average per municipality in order to assess whether respondents receive adequate quality of care. For example, auditors ask whether the family receives regular visits from community health workers and whether care is provided at the health post if needed.

For the conditional cash transfer program (*Bolsa Família*), CGU headquarters provides auditors in the field with a list of typically 30 randomly sampled transfer recipient households based on a national sampling frame.[24] Auditors conduct field visits to check whether transfer recipient families are of a size and income level compatible with program guidelines and

[24]The exact number of respondents can vary depending on conditions in the field.

whether children's vaccinations are done regularly as required under the program. Auditors also check school records to assess compliance with enrollment and attendance conditionalities for obtaining the cash transfer.

Most of the survey responses are either yes, no, or not applicable, if the household required no health services over the preceding year, for example. In the empirical analysis below we aggregate the household-level data to the municipality level by computing the share that responded yes to a particular question out of the total of respondents who responded either yes or no.

5.3 Caveats

There are three caveats worth pointing out regarding our measures of rent extraction.[25] First, we assume that existing regulations on procurement and service delivery—which define irregularities—make sense, that is, they serve a legitimate purpose in a reasonable way. 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, managers and service providers 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 procurement reflect international best practices as laid out in the WTO's Agreement on Government Procurement.

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 correlated with treatment status, it would bias our estimates. However, we believe that the institutional setup makes it very unlikely that auditors are corrupt. First, auditors are paid

[25] Only the first caveat is genuine to our study. The other two apply to measures of waste and corruption more generally.

by the federal government, not by local governments, which makes it less likely that they are captured by local special interests. Second, auditors are relatively well paid, and therefore have a lot to loose in case collusion gets detected. Third, auditors work in teams of about 10 people on average. This makes it hard to sustain collusion on any significant scale because the whole team has to be bribed in order to conceal irregularities. Fourth, the interaction between auditors and local officials is at a single point in time (unknown ex ante), which again makes it harder to sustain collusion. Finally, CGU auditors' work is itself subject to periodic inspection from the external audit agency of the central government, the *Tribunal de Contas da União* and we are not aware of any reported cases of collusion between CGU auditors and local administrations.

The third caveat is that even if auditors were incorruptible, the local elite might somehow manage to manipulate what gets uncovered and what remains unnoticed. While this scenario is plausible in general, it is unlikely in our case because local elites play no direct role in carrying out the audit. Auditors go into a municipality 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 any trace of them since the audit is very thorough, involving both financial auditing and detailed inspection of public works and services.

5.4 Municipality and mayor characteristics

Data on municipality characteristics are obtained from several sources. Official local population data for the year 2007 are from the population count conducted by the *Instituto Brasileiro de Geografia e Estatística* (IBGE). Data on local income distribution, schooling, and federal transfers are from the *Instituto de Pesquisa Economica Aplicada* (IPEA) based on the 2000 census. Mayor characteristics and party affiliations are from the *Tribunal Superior Eleitoral* (TSE). Table 8 gives difference in means tests for a host of pre-treatment covariates. With the exception of one party affiliation dummy, none of these differences are statistically significant and the magnitudes are generally small. Table 8 also provides a joint test of the null hypotheses that the population means of these covariates are equal across treatment and control groups. The F-

statistic suggests that the randomization worked, that is, it fails to reject the null at conventional levels of significance (p-value=0.44).

6 Estimation approach

Given the randomized experimental design, estimation is a straightforward comparison of sample mean outcomes from treatment and comparison groups. Let Y_{mi} denote the outcome variable for procurement process or individual i in municipality m, β_{mi} the (heterogeneous) treatment effect, D_m the treatment (high audit risk) indicator and U_{mi} other unobserved factors that affect the outcome. The data generating process can then be described as:

$$Y_{mi} = \alpha + \beta_{mi} D_m + U_{mi} \tag{1}$$

Randomization ensures that, in expectation, D_m is uncorrelated with U_{mi}, so $\widehat{\beta}^{OLS}$ provides an unbiased and consistent estimator of the average treatment effect $E(\beta_{mi})$. Rather than estimating equation (1) using OLS at the individual level and clustering standard errors at the municipality level, we estimate equation (1) with WLS using municipality level averages and weights equal to the number of procurement process or individuals in the survey.

Since treatment probabilities vary somewhat by state due to the conditional randomization, we also present specifications with state fixed effects c_s. We provide a check on small sample bias by including pre-treatment municipality characteristics and mayor's characteristics, such as age, gender and education, as well as the mayor's party affiliation into the regression. For the sake of transparency, we present results separately for the sample from the 32$^{\text{nd}}$ lottery and for the pooled sample including the 31$^{\text{st}}$ lottery, which we add to increase the precision of our estimates. It is worth emphasizing that including municipalities from the 31$^{\text{st}}$ lottery might lead to bias if outcomes were systematically different from one year to the next. Fortunately this turns out to be a minor issue for our main result as evidenced by the fact that point estimates vary only slightly across the 32$^{\text{nd}}$ lottery and pooled estimation samples. As a final robustness check, we restrict the sample of procurement processes to those that occurred in 2009 or 2010.

7 Estimation results

Table 9 presents impact estimates on the municipality-level number of local procurement processes. Columns 1 through 5 are based solely on the 32nd lottery and provide the raw difference in means and estimates with state intercepts, mayor party affiliation dummies, municipality characteristics, and mayor's characteristics, respectively. Columns 6 through 10 show estimates from the same five specifications but for the pooled sample, including control municipalities from the 31st lottery. The estimates fluctuate around -3 to -4 processes and are statistically significant at 10% throughout and at 5% in the pooled sample, even before adding control variables. Given that the control group mean number of processes is about 14, the effect corresponds to a 20% to 30% reduction approximately. Figure 2 shows that higher audit risk shifted the right-hand tail of distribution of the number of procurement processes to the left. The reduction in the number of procurements is entirely driven by fewer restricted modalities (results omitted to save space).

Table 10 presents impact estimates for the municipality-level proportion of restricted procurement modalities. Impact estimates are all negative but they vary considerably across specifications and across the 32nd lottery and pooled estimation samples. Figure 3 shows that higher audit risk shifted the right-hand tail of the distribution of restricted procurement modalities to the left. This result is consistent with the finding on the number of procurement processes above since a typical way of circumventing more competitive procedures, such as a sealed-bid (reverse) auction, is to fractionalize the purchase (break it up into pieces) and conduct a series of restricted procurement processes, such as "bids only by invitation".

Another potential explanation for the reduction in restricted procurement modalities is that local managers were actually doing less procurement, not just different modalities, perhaps in order to "sit out" the high audit risk year. However, Table 11 shows that there is no evidence that high audit risk group municipalities were receiving less transfers from the central government during 2009. Figure 4 shows this result graphically. Since federal funds typically must be used during the fiscal year or else returned to the federal government, they cannot be saved for later periods. As a result, no funding differential implies no local spending differential. The zero

effect on local spending also makes sense from a practical point of view since for many goods, such as staple foods, medications or contracted-out cleaning services, local governments hold few or no inventories at all and so they need to make purchases to keep the administration running.

Table 12 presents impact estimates on the proportion of irregular procurement processes. A process is deemed irregular if the audit result from Table 7 is anything other than regular or only a formal error. Impact estimates are remarkably close -0.16 across specifications, are statistically significant at 5% in the sample from the 32nd lottery, and become statistically significant at 1% in the pooled sample, even without controls. Given that the control group mean proportion of irregular processes is 0.63, the effect corresponds to a 25% reduction approximately. Figure 5 shows that higher audit risk shifted the entire distribution of the proportion of irregular procurement processes to the left.

Table 13 presents impact estimates on the proportion of procurement processes with evidence of mismanagement or corruption. Impact estimates fluctuate around the -0.17 mark although they are more variable than for irregular processes. Nevertheless, the confidence intervals show substantial overlap. In fact, all estimates fall within the confidence intervals around all other estimates. As for irregular processes above, all estimates are highly significant statistically. Figure 6 shows that higher audit risk shifted the entire distribution of the proportion of procurement processes with evidence of mismanagement or corruption to the left.

The top part of Table 14 presents impact estimates for a range of outcomes related to the preventive and basic health care program (*Saúde da Família*). In contrast to the effects found for procurement, Table 14 shows no evidence that increased audit risk affected the quality of health care services provided by local governments. For example, the average share of respondents who say they receive regular visits from community health staff—as required under the preventive health program—is essentially 93% in both treatment and control groups. The proportion of respondents who say they receive health care at home when needed is about 70% in the control group and about 6 to 7 percentage points higher in the high audit risk group, but the difference is not statistically significant.

Overall, out of the eleven outcomes considered here, none are statistically different between treatment and control groups. Moreover, the size of the differences is typically small and often the sign of the difference is the opposite of what theory would suggest. Since potential punishments for serious irregularities in procurement include jail, while for service delivery they only include fines or loss of the job, differences in potential punishments might drive the difference in results. A complementary interpretation is that irregularities in service provision cannot be identified with the same precision as irregularities in procurement and so higher audit risk might matter less to service providers, compared to procurement officials. For example, while health facility users might complain about infrequent opening hours of the health post, health staffers could easily dispute this fact and auditors would have a hard time verifying any of these competing claims.[26]

The bottom of Table 14 shows that higher audit risk did not seem to affect local compliance with national guidelines of the conditional cash transfer program *Bolsa Família* either. The first two outcomes show that targeting of beneficiaries was unaffected since the proportion of appropriately included beneficiaries is negligibly (and statistically insignificantly) different between treatment and control respondents. The last three outcomes show the same qualitative result for compliance with health and education conditionalities. Again, differences in punishment are likely to be part of the explanation for the zero effect since the punishment for overstating the number of kids in the household or for not sending them to school, for example, is at most the loss of the benefit. Another interpretation, which is supported by the high compliance rate evident in Table 14, is that most *Bolsa Família* recipients were appropriately included in the program—they were poor enough—and they already complied with health and education conditionalities to a large extent. Thus, they could not respond to higher audit risk because they were doing nothing wrong in the first place.

[26]Another interpretation is that there simply was not that much shirking on the job going on in preventive and basic health care delivery. We consider this possibility less likely since substantial numbers of health service users in our data do in fact indicate that health posts are not always open exactly as required by ministry of health regulations.

8 Conclusion

This paper provides experimental evidence that temporarily increasing annual audit risk by about 20 percentage points reduced the proportion of local procurement processes involving mismanagement or corruption by about 17 percentage points. Higher audit risk also reduced the proportion of restricted procurement modalities adopted by local managers. As in Olken (2007), we cannot say whether these effects reflect a net reduction in rent extraction or merely a substitution over time—with high audit risk municipalities "making up" at least some lost rents in subsequent periods. In either case, our estimates provide clear evidence in favor of the prediction that local officials reduce rent extraction in response to higher audit risk (Becker 1968).

In contrast, we find no evidence that increased audit risk affected the quality of preventive and primary health care services, measured using client satisfaction surveys conducted by auditors. Since potential punishments for serious irregularities in procurement include jail, while for service delivery they only include fines or loss of the job, differences in potential punishments might drive the difference in results. A complementary interpretation is that irregularities in service provision cannot be identified with the same precision as irregularities in procurement and so higher audit risk might matter less to service providers, compared to procurement officials.

We also find no evidence that higher audit risk had an effect on local compliance with national guidelines of the conditional cash transfer program *Bolsa Família*, measured in terms of appropriate inclusion of beneficiaries into the program or their compliance with health and education conditionalities. Again, differences in punishment are likely to be part of the explanation for the zero effect since the punishment for overstating the number of kids in the household or for not sending them to school, for example, is at most the loss of the benefit. Another interpretation, which is supported by our data, is that most *Bolsa Família* recipients were appropriately included in the program—they were poor enough—and they already complied with health and education conditionalities to a large extent.

Audit intensity should be scaled up permanently if and only if the net benefits of such a policy are positive. Although the results from increasing audit risk temporarily are encouraging, it

would take a permanent variation in audit risk to assess whether scaling up is indeed advisable, since local officials might find ways to adapt to increased audit risk over time. Another complication is that assessing the benefits of higher audit risk in monetary terms requires an estimate of the value of a marginal increase in compliance with existing procurement regulations. A necessary first step in this direction would be to quantify the cost savings from lower procurement costs. Unfortunately, however, audit findings currently do not systematically report the price at which local goods and services were purchased. More detailed data is therefore required to better quantify the benefits of higher audit intensity in terms of cost savings.

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Table 1: Randomization lottery May 12 2009

State	N	Draws	P(Treatment) %
Acre (AC)	21		4.0
Amapá (AP)	15	2	4.0
Roraima (RR)	14		4.0
Alagoas (AL)	101	2	2.0
Amazonas (AM)	61	2	3.3
Bahia (BA)	415	10	2.4
Ceará (CE)	183	6	3.3
Espírito Santo (ES)	77	2	2.6
Goiás (GO)	245	6	2.4
Maranhão (MA)	216	6	2.8
Minas Gerais (MG)	849	14	1.6
Mato Grosso do Sul (MS)	77	2	2.6
Mato Grosso (MT)	140	2	1.4
Pará (PA)	142	4	2.8
Paraíba (PB)	222	6	2.7
Pernambuco (PE)	182	4	2.2
Piauí (PI)	223	6	2.7
Paraná (PR)	397	8	2.0
Rio de Janeiro (RJ)	88	2	2.3
Rio Grande do Norte (RN)	166	4	2.4
Rondônia (RO)	51	2	3.9
Rio Grande do Sul (RS)	495	10	2.0
Santa Catarina (SC)	292	6	2.1
Sergipe (SE)	74	2	2.7
São Paulo (SP)	636	10	1.6
Tocantins (TO)	138	2	1.4
Total	5,520	120	

Notes: Source: Portaria Nº 930, May 8 2009. N is the number of municipalities from a given state that are eligible for sampling in the lottery. Draws is the number of municipalities from a given state that are sampled in the lottery. P(Treatment) is the probability of assignment to the high audit risk group, given in percentage points. Municipalities from Acre, Amapá and Roraima states are grouped together for this lottery.

Table 2: 29th lottery August 17 2009

State	N	Draws	P(Draw) %
Acre (AC)	18		2.3
Amapá (AP)	12	1	2.3
Roraima (RR)	13		2.3
Alagoas (AL)	82	2	2.4
Amazonas (AM)	53	1	1.9
Bahia (BA)	389	5	1.3
Ceará (CE)	166	3	1.8
Espírito Santo (ES)	71	1	1.4
Goiás (GO)	230	2	0.9
Maranhão (MA)	189	3	1.6
Minas Gerais (MG)	812	7	0.9
Mato Grosso do Sul (MS)	71	1	1.4
Mato Grosso (MT)	132	1	0.8
Pará (PA)	127	3	2.4
Paraíba (PB)	207	3	1.4
Pernambuco (PE)	159	3	1.9
Piauí (PI)	205	3	1.5
Paraná (PR)	378	3	0.8
Rio de Janeiro (RJ)	83	1	1.2
Rio Grande do Norte (RN)	153	3	2.0
Rondônia (RO)	46	1	2.2
Rio Grande do Sul (RS)	472	4	0.8
Santa Catarina (SC)	280	2	0.7
Sergipe (SE)	66	1	1.5
São Paulo (SP)	609	5	0.8
Tocantins (TO)	132	1	0.8
Total	5,155	60	

Notes: Source: Portaria Nº 1581, August 11 2009. N is the number of municipalities from a given state that are eligible for sampling in the lottery. Draws is the number of municipalities from a given state that are sampled in the lottery. P(Draw) is the sampling probability. Municipalities from Acre, Amapá and Roraima states are grouped together for this lottery.

Table 3: 32nd lottery May 10 2010

	Treatment Group			Control Group				Ex post	Ex ante
State	N	Draws	P(Audit)	N	Draws	P(Draw)	P(Audit)	dP	dP
Acre	0	1	50.0	21	1	1.1	7.8	42.2	17.2
Mato Grosso do Sul	2		50.0	72		1.1	5.2	44.8	19.8
Alagoas	2	1	25.0	92	1	0.6	7.7	17.3	17.3
Sergipe	2		25.0	66		0.6	5.1	19.9	19.9
Amazonas	2	1	25.0	56	1	1.0	6.5	18.5	18.5
Rondônia	2		25.0	46		1.0	7.3	17.7	17.7
Amapá	1	1	50.0	12	1	4.3	10.9	39.1	14.1
Roraima	1		50.0	11		4.3	10.9	39.1	14.1
Espírito Santo	2	1	25.0	72	1	0.7	4.8	20.2	20.2
Rio de Janeiro	2		25.0	80		0.7	4.2	20.8	20.8
Bahia	10	2	20.0	385	2	0.5	4.3	15.7	20.7
Ceará	6	1	16.7	162	1	0.6	5.9	10.8	19.1
Goiás	6	1	16.7	230	1	0.4	3.0	13.7	22.0
Maranhão	6	1	16.7	200	1	0.5	5.2	11.5	19.8
Minas Gerais	14	4	28.6	813	4	0.5	3.0	25.5	22.0
Mato Grosso	2	1	50.0	131	1	0.8	4.9	45.1	20.1
Pará	4	1	25.0	125	1	0.8	7.7	17.3	17.3
Paraíba	6	1	16.7	206	1	0.5	4.7	11.9	20.3
Pernambuco	4	1	25.0	168	1	0.6	6.1	18.9	18.9
Piauí	6	1	16.7	200	1	0.5	4.8	11.9	20.2
Paraná	8	2	25.0	379	2	0.5	2.9	22.1	22.1
Rio Grande do Norte	4	1	25.0	153	1	0.7	0.7	24.3	24.3
Rio Grande do Sul	10	2	20.0	472	2	0.4	2.9	17.1	22.1
Santa Catarina	6	2	33.3	280	2	0.7	2.8	30.5	22.2
São Paulo	10	3	30.0	610	3	0.5	2.9	27.1	22.1
Tocantins	2	1	50.0	133	1	0.8	3.0	47.0	22.0
Total	120	30		5,175	30				

Notes: The audit risk calculations in this table are based on Portaria Nº 1581 from August 11 2009 for the 29th lottery, and Portaria Nº 862 from April 30 2010 for the 32nd lottery. N is the number of municipalities from a given state that are eligible for sampling in the lottery. Draws is the number of municipalities from a given state that are sampled in the lottery. P(Draw) is the sampling probability. P(Draw), P(Audit) and dP are given as percentages. For the treatment group, the probability of being drawn in the 32nd lottery equals the probability of receiving a CGU audit between May 2009 and May 2010, P(Draw) = P(Audit). Ex ante (From May 8 2009 to the publication of Portaria Nº 862 on April 30 2010) this probability was 30/120 = 25%. Ex post, it is given above in column 3. For the control group, the probability of receiving a CGU audit between May 2009 and May 2010 depends on the probabilities of being drawn in the 29th, 30th, 31st, and 32nd lotteries. Under the assumption that the probabilities of being drawn in the first three lotteries were the same as in the 29th lottery, P(Audit) for the control group is calculated according to the following approximation: $P(\text{Audit}) = 1-[1-P(\text{Draw } 29^{th})]^3 \times [1-P(\text{Draw } 32^{nd})]$. dP gives the ex ante and ex post difference in audit probabilities between treatment and control groups by state.

Table 4: Distribution of procurement objects by municipality audit risk

	32nd lottery				31st lottery	
	High audit risk		Low audit risk		Low audit risk	
Procurement object	Freq.	Percent	Freq.	Percent	Freq.	Percent
Staple foods	85	24.08	117	24.12	184	22.52
Medication	50	14.16	49	10.10	81	9.91
Other non-durable goods	43	12.18	70	14.43	115	14.08
Medical equipment	5	1.42	9	1.86	33	4.04
IT equipment	6	1.70	12	2.47	8	0.98
Agricultural equipment	10	2.83	7	1.44	21	2.57
Other durable goods	11	3.12	11	2.27	26	3.18
Public works	25	7.08	42	8.66	135	16.52
Contracted-out services	48	13.60	46	9.48	92	11.26
Other objects	70	19.83	122	25.15	122	14.93
Total	353	100.00	485	100.00	817	100.00

Notes: The unit of observation is an individual procurement process. The three distributions are statistically different from each other according to Pearson's chi-square test.

Table 5: Distribution of procurement modalities by municipality audit risk

	32nd lottery				31st lottery	
	High audit risk		Low audit risk		Low audit risk	
Procurement modality	Freq.	Percent	Freq.	Percent	Freq.	Percent
Direct purchase	69	19.55	75	15.46	80	9.79
Bids only by invitation	98	27.76	189	38.97	367	44.92
Only pre-registered bidders	44	12.46	66	13.61	160	19.58
Restricted modalities	211	59.77	330	68.04	607	74.29
Sealed-bid auction	7	1.98	10	2.06	10	1.22
On-site auction	105	29.75	109	22.47	180	22.03
Electronic auction	30	8.50	36	7.43	20	2.46
Total	353	100.00	485	100.00	817	100.00

Notes: The unit of observation is an individual procurement process. The three distributions are statistically different from each other according to Pearson's chi-square test.

Table 6: Auditor classification of irregularities and corruption codings

		Corruption codings		
Auditor classification of irregularities	%	LZ	FF	BNPT
- simulated tender process	6.05	C	C	C
- unjustified or excessive payments for goods and services	3.81	C	C	C
- evidence of favouritism	10.94	C	C	C
- fractionalizing of procurement amounts	9.06	C		C
- invitation for bids to less than three firms	1.27	M	M	M
- procurement modality too restricted	8.52	M		M
- participating ineligible firm	0.24	M		M
- non-selection of the lowest bid	0.48	M		
- other management irregularities	2.60	M		
- absence of preliminary price survey	3.63	P		
- inadequate publication of the call	1.63	P		
- incomplete specification of the call	0.97	P		
- inadequate publication of results	0.91	P		
- other procedural irregularities	1.69	P		
- other irregularities	7.67			
- formal errors	12.87			
- regular process	27.67			

Notes: LZ: Litschig and Zamboni, FF: Ferraz and Finan, BNPT: Brollo, Nannicini, Perotti, and Tabellini, C: Corruption, M: Management/Mismanagement, P: Procedural. N=1,665 procurement processes. Ferraz and Finan (2010) code an irregularity as a case of corruption only if "the public good was not provided".

Table 7: Distribution of audit findings by municipality audit risk

	32nd lottery				31st lottery	
	High audit risk		Low audit risk		Low audit risk	
Audit result	Freq.	Percent	Freq.	Percent	Freq.	Percent
Procedural irregularity	37	10.48	36	7.42	73	8.94
Management irregularity	54	15.30	130	26.80	183	22.40
Evidence of corruption	42	11.90	83	17.11	219	26.81
Other irregularities	29	8.22	53	10.94	45	5.50
Irregular process	162	45.90	302	62.27	520	63.65
Regular process	124	35.13	112	23.09	239	27.17
Formal error	67	18.97	71	14.64	75	9.18
Total	353	100.00	485	100.00	817	100.00

Notes: The unit of observation is an individual procurement process. The three distributions are statistically different from each other according to Pearson's chi-square test.

Table 8: Difference in means tests for pre-treatment covariates

	Treatment group	Control group	Difference	P-value
Population	21'512	18'653	2'858	0.69
	(6'822)	(2'580)	(7'294)	
Income per capita	162.5	157	5.5	0.76
	(15.6)	(8.5)	(17.8)	
Average years of schooling	3.86	3.89	-0.03	0.88
	(0.25)	(0.12)	(0.27)	
Urbanization	0.57	0.59	-0.02	0.62
	(0.04)	(0.02)	(0.05)	
Poverty headcount ratio	0.26	0.26	0.00	0.97
	(0.04)	(0.02)	(0.04)	
Poverty gap	0.52	0.49	0.03	0.18
	(0.04)	(0.02)	(0.02)	
Gini coefficient	0.56	0.56	0.00	0.76
	(0.01)	(0.00)	(0.01)	
Radio station	0.46	0.45	0.01	0.62
	(0.09)	(0.05)	(0.05)	
PMDB	0.20	0.25	-0.05	0.52
	(0.07)	(0.05)	(0.09)	
PSDB	0.13	0.17	-0.04	0.56
	(0.06)	(0.04)	(0.07)	
PTB	0.03	0.10	-0.07	0.15
	(0.03)	(0.03)	(0.05)	
PT	0.10	0.09	0.01	0.86
	(0.06)	(0.03)	(0.06)	
PSB	0.10	0.08	0.02	0.72
	(0.06)	(0.03)	(0.06)	
PR	0.10	0.08	0.02	0.72
	(0.06)	(0.03)	(0.06)	
PP	0.16	0.03	0.13	0.07
	(0.07)	(0.02)	(0.07)	
PDT	0.06	0.02	0.04	0.37
	(0.05)	(0.02)	(0.05)	
F-statistic for the joint hypotheses that all differences are zero				1.02
(p-value)				(0.44)
N	30	90		

Notes : The first three columns give sample means, the difference in means and (standard errors). Municipality characteristics are from the 2000 census, except population, which is from the 2007 population count. Mayor's party affiliation is for the 2009-2012 term.

Table 9: Impact on the number of procurement processes

Dependent variable: number of procurement processes; control group mean 14.4, std. 9.5										
Treatment (0/1)	-4.400*	-4.385*	-6.914*	-5.090*	-5.479*	-2.700*	-2.740**	-3.050*	-2.839*	-4.204***
	(2.563)	(2.423)	(3.610)	(2.686)	(2.953)	(1.402)	(1.327)	(1.675)	(1.477)	(1.582)
State intercepts	N	Y	N	N	N	N	Y	N	N	N
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	N	N
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	N
Mayor's characteristics	N	N	N	N	Y	N	N	N	N	Y
Observations	60	60	60	60	60	120	120	120	120	120
R-squared	0.048	0.626	0.277	0.145	0.228	0.018	0.290	0.111	0.053	0.149

Notes: OLS estimations. Sample consists of municipalities from the 32nd and 31st lotteries. Treatment indicates whether the municipality was in the high audit probability group during the year leading up to the 32nd lottery. Municipality characteristics: year 2007 population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, all measured in 2000. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 10: Impact on the proportion of restricted procurement modalities

Dependent variable: proportion of restricted procurement modalities; control group mean 0.72, std. 0.28

Treatment (0/1)	-0.083	-0.066	-0.158*	-0.048	-0.124	-0.122**	-0.119*	-0.174***	-0.126**	-0.169***
	(0.093)	(0.089)	(0.083)	(0.092)	(0.082)	(0.060)	(0.062)	(0.059)	(0.063)	(0.060)
State intercepts	N	Y	N	N	N	N	Y	N	N	N
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	N	N
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	N
Mayor's characteristics	N	N	N	N	Y	N	N	N	N	Y
Observations	60	60	60	60	60	120	120	120	120	120
R-squared	0.019	0.573	0.338	0.209	0.280	0.031	0.462	0.168	0.165	0.139

Notes: WLS estimations with weights equal to the number of procurement processes in the municipality. Restricted procurement modalities refer to direct purchases by the local administration, bids only by invitation and the modality where only pre-registered bidders can compete for the contract. Sample consists of municipalities from the 32nd and 31st lotteries. Treatment indicates whether the municipality was in the high audit probability group during the year leading up to the 32nd lottery. Municipality characteristics: year 2007 population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, all measured in 2000. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 11: Impact on federal transfers per capita in 2009

Dependent variable: per capita federal transfers in 2009; control group mean 884, std. 521

Treatment (0/1)	-119.8	-177.4	-126.0	-11.98	-190.1	-32.92	-92.15	-56.69	-4.038	-31.30
	(140.2)	(187.8)	(167.5)	(128.6)	(156.8)	(105.7)	(113.8)	(97.96)	(86.69)	(103.5)
State intercepts	N	Y	N	N	N	N	Y	N	N	N
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	N	N
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	N
Mayor's characteristics	N	N	N	N	Y	N	N	N	N	Y
Observations	57	57	57	57	57	115	115	115	115	115
R-squared	0.013	0.273	0.406	0.457	0.206	0.001	0.253	0.214	0.401	0.105

Notes: OLS estimations. Sample consists of municipalities from the 32nd and 31st lotteries. Treatment indicates whether the municipality was in the high audit probability group during the year leading up to the 32nd lottery. Municipality characteristics: year 2007 population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, all measured in 2000. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 12: Impact on irregular procurement processes

Dependent variable: proportion of irregular procurement processes; control group mean 0.63, std. 0.25

Treatment (0/1)	-0.164**	-0.146***	-0.163**	-0.165**	-0.168**	-0.172***	-0.159***	-0.179***	-0.141**	-0.192***
	(0.073)	(0.053)	(0.077)	(0.076)	(0.072)	(0.052)	(0.045)	(0.051)	(0.056)	(0.052)
State intercepts	N	Y	N	N	N	N	Y	N	N	N
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	N	N
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	N
Mayor's characteristics	N	N	N	N	Y	N	N	N	N	Y
Observations	60	60	60	60	60	120	120	120	120	120
R-squared	0.099	0.757	0.249	0.339	0.283	0.077	0.494	0.163	0.289	0.162

Notes: WLS estimations with weights equal to the number of procurement processes in the municipality. A procurement process is coded irregular if the audit result from Table 6 is anything other than regular or only a formal error. Sample consists of municipalities from the 32nd and 31st lotteries. Treatment indicates whether the municipality was in the high audit probability group during the year leading up to the 32nd lottery. Municipality characteristics: year 2007 population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, all measured in 2000. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 13: Impact on mismanagement or corruption irregularities

Dependent variable: proportion of procurement processes with evidence of mismanagement or corruption; control group mean 0.47, std. 0.24

Treatment (0/1)	-0.167**	-0.114*	-0.215***	-0.133*	-0.178***	-0.200***	-0.177***	-0.225***	-0.170***	-0.241***
	(0.071)	(0.065)	(0.073)	(0.072)	(0.066)	(0.051)	(0.049)	(0.050)	(0.054)	(0.049)
State intercepts	N	Y	N	N	N	N	Y	N	N	N
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	N	N
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	N
Mayor's characteristics	N	N	N	N	Y	N	N	N	N	Y
Observations	60	60	60	60	60	120	120	120	120	120
R-squared	0.106	0.637	0.320	0.333	0.271	0.105	0.505	0.231	0.296	0.191

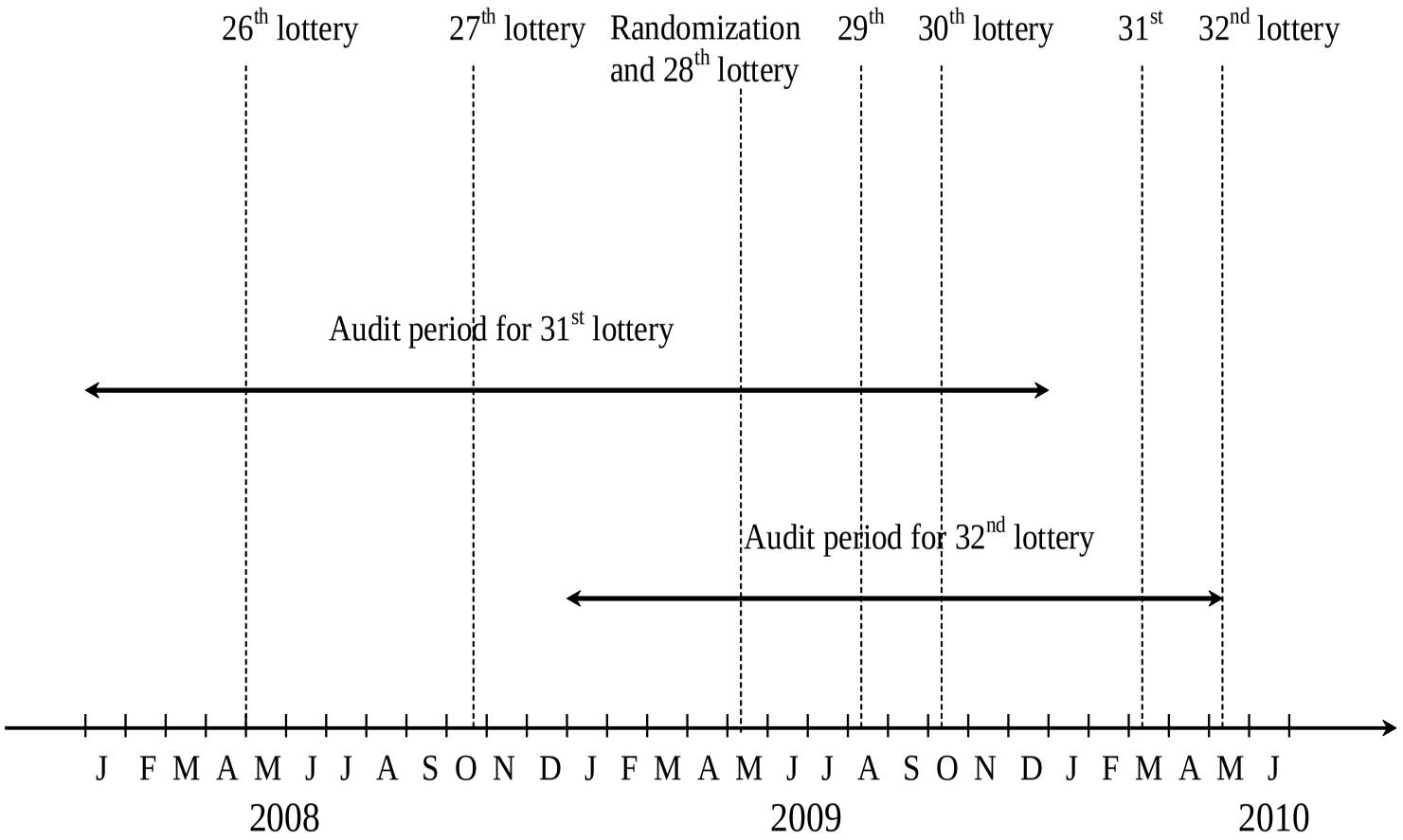
Notes: WLS estimations with weights equal to the number of procurement processes in the municipality. Corruption corresponds to cases of simulated (fake) tender processes, cases of favouritism, or when auditors determine that there were unjustified or excessive payments for goods or services. Management irregularities correspond to fractionalized procurement amounts, procurement modalities that are too restricted, as well as other irregularities. See Table 6 for details. Sample consists of municipalities from the 32nd and 31st lotteries. Treatment indicates whether the municipality was in the high audit probability group during the year leading up to the 32nd lottery. Municipality characteristics: year 2007 population, income per capita, average years of schooling, urbanization, poverty headcount ratio, povertygap, gini coefficient, radio station, all measured in 2000. Mayor's characteristics: first-term mayor indicator, education level indicators, male dummy and age. Robust standard errors are given in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 14: Impacts on health and conditional cash transfer programs

	32nd lottery		31st and 32nd lottery	
	Control mean	Difference	Control mean	Difference
Proportion of adequately staffed teams of community health workers	0.821*** (0.075)	-0.097 (0.114)	0.867*** (0.038)	-0.143 (0.092)
Proportion of respondents that receive visits from community health workers	0.929*** (0.016)	0.018 (0.022)	0.926*** (0.013)	0.022 (0.019)
Proportion of respondents that receive regular visits from community health staff	0.911*** (0.028)	0.016 (0.041)	0.902*** (0.020)	0.024 (0.034)
Proportion of adequately staffed teams of the family health program	0.828*** (0.072)	0.000 (0.102)	0.809*** (0.043)	0.018 (0.084)
Proportion of regularly composed teams of the family health program	0.758*** (0.082)	0.138 (0.101)	0.845*** (0.040)	0.051 (0.07)
Proportion of respondents that received health services at home when needed	0.692*** (0.094)	0.076 (0.128)	0.711*** (0.046)	0.058 (0.097)
Proportion of respondents that were attended by a doctor when needed	0.732*** (0.081)	0.009 (0.119)	0.762*** (0.041)	-0.020 (0.095)
Proportion of respondents that were attended by a nurse when needed	0.932*** (0.032)	0.011 (0.040)	0.951*** (0.013)	-0.007 (0.027)
Proportion of respondents that were attended by a dentist when needed	0.758*** (0.086)	0.063 (0.110)	0.756*** (0.043)	0.064 (0.079)
Proportion of respondents indicating that the health post is open exactly as required	0.457*** (0.123)	-0.072 (0.166)	0.366*** (0.066)	0.020 (0.129)
Proportion of respondents indicating that they were asked to pay a fee for service	0.005 (0.004)	-0.001 (0.005)	0.016 (0.013)	-0.013 (0.014)
F-statistic (p-value)		0.47 (0.91)		0.41 (0.84)
Proportion of Bolsa Familia recipient families with program compatible household size	0.956*** (0.014)	-0.031 (0.026)	0.953*** (0.01)	-0.028 (0.023)
Proportion of Bolsa Familia recipient families with program compatible income	0.856*** (0.024)	-0.009 (0.039)	0.853*** (0.015)	-0.007 (0.033)
Proportion of Bolsa Familia recipient families compliant with required regular vaccinations	0.986*** (0.009)	0.005 (0.012)	0.988*** (0.004)	0.003 (0.009)
Proportion of Bolsa Familia recipient adolescents not enrolled at school	0.218*** (0.033)	-0.018 (0.052)	0.172*** (0.016)	0.028 (0.042)
Proportion of BF recipient and enrolled adolescents attending school infrequently	0.053*** (0.019)	-0.007 (0.022)	0.091*** (0.012)	-0.044*** (0.016)
F-statistic (p-value)		0.47 (0.79)		2.29 (0.05)

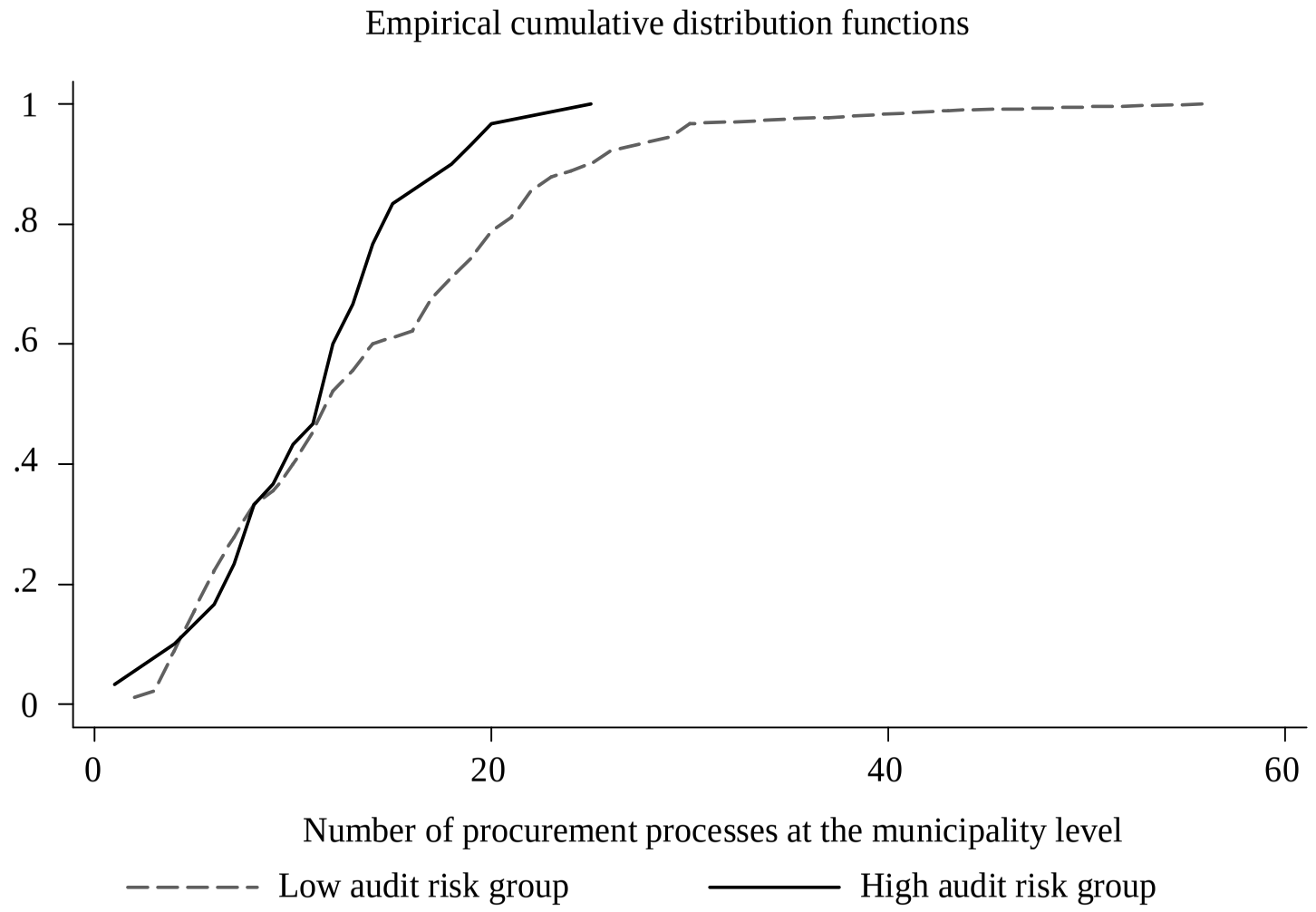
Notes : WLS estimations with weights equal to the number of survey respondents. The unit of observation is the municipality. Robust standard errors in parentheses. N varies by outcome. F-statistics are for the joint hypotheses that all differences in outcomes are zero.

Figure 1: Timeline



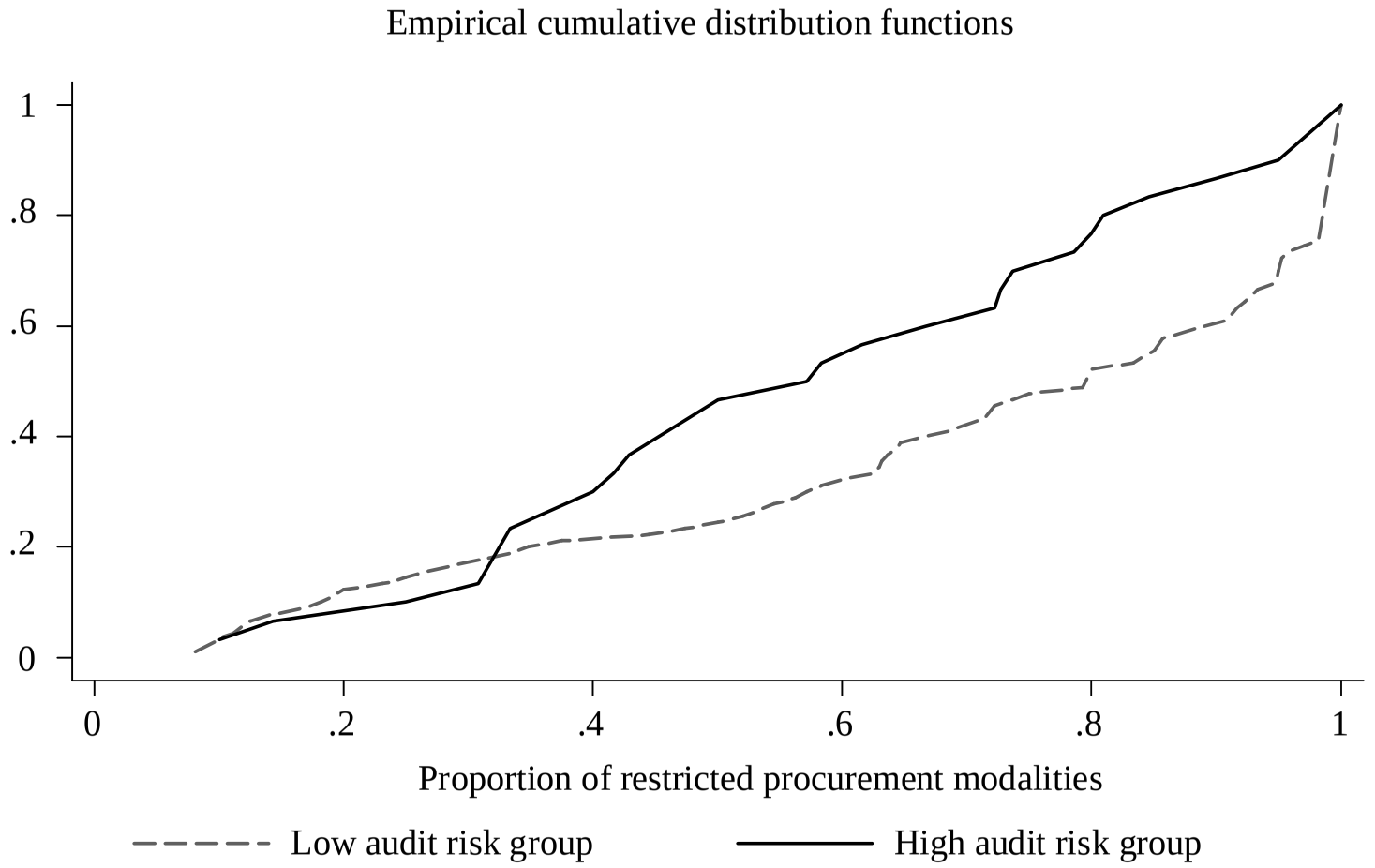
Notes: The 26th through 32nd were regular audit lotteries. 60 municipalities were sampled in each round. The randomization lottery coincided with the 28th lottery and randomly assigned 120 municipalities to the high audit risk group for the upcoming year. In the 32nd lottery 30 municipalities were drawn from the high audit risk group and another 30 from all other municipalities. All lotteries used the same sampling technology. For the 32nd lottery the audit period extended back to January 2009. For the 31st lottery the audit period extended from January 2008 until December 2009.

Figure 2: Impact on the distribution of the number of procurement processes



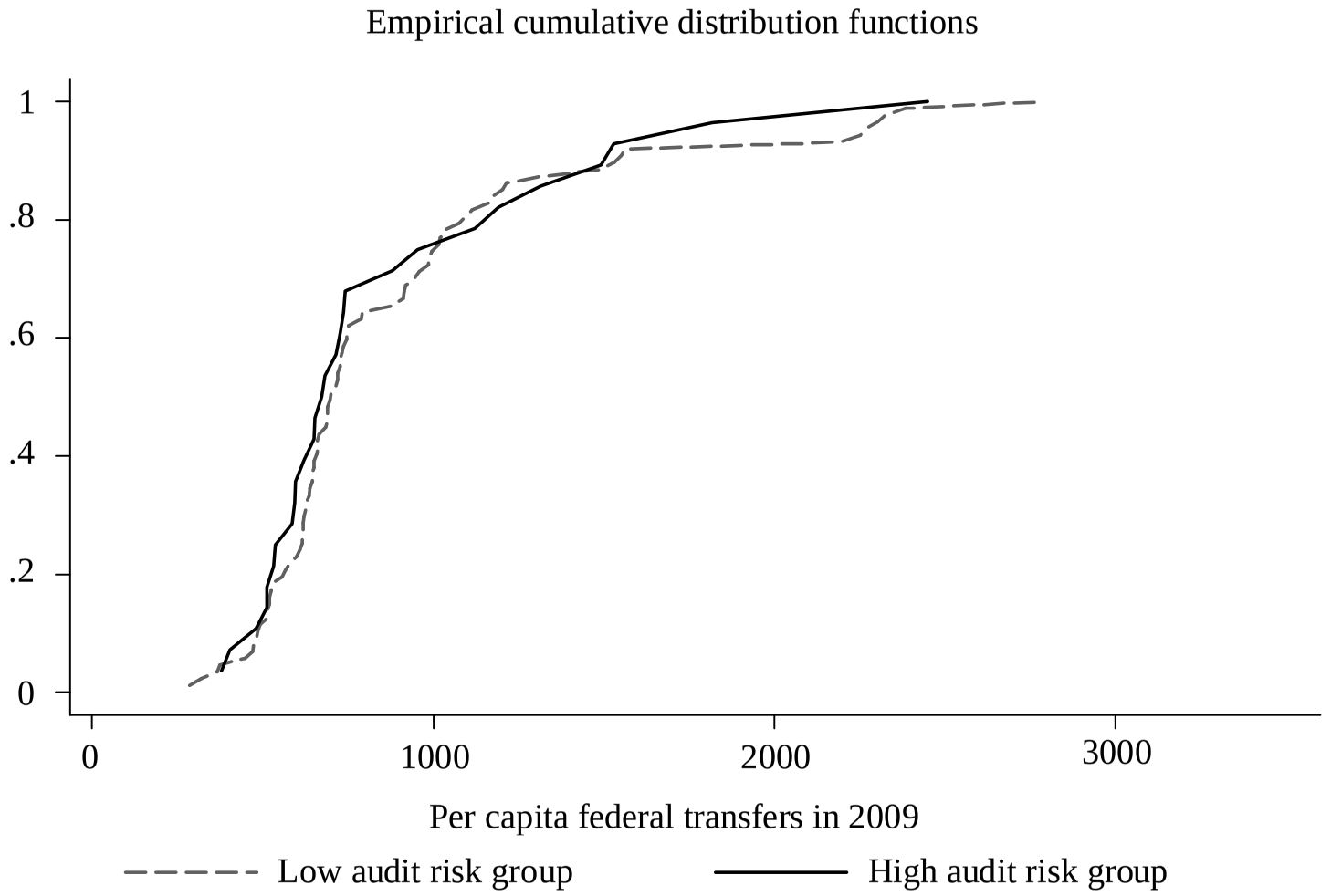
Notes: Sample consists of municipalities from the 32nd and 31st lotteries.

Figure 3: Impact on the distribution of restricted procurement modalities



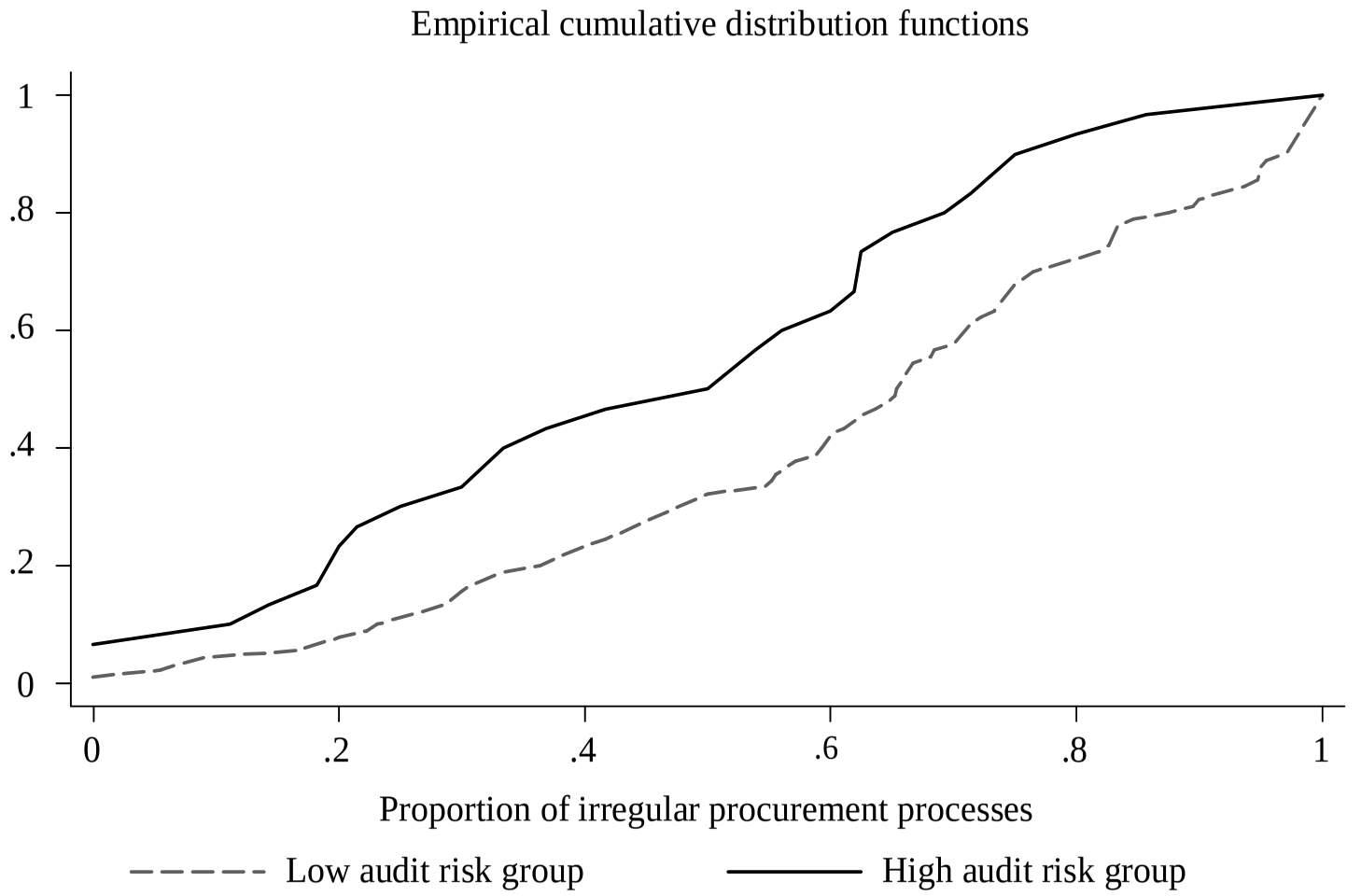
Notes: Restricted procurement modalities refer to direct purchases by the local administration, bids only by invitation and the modality where only pre-registered bidders can compete for the contract. Sample consists of municipalities from the 32nd and 31st lotteries.

Figure 4: Impact on the distribution of federal transfers per capita in 2009



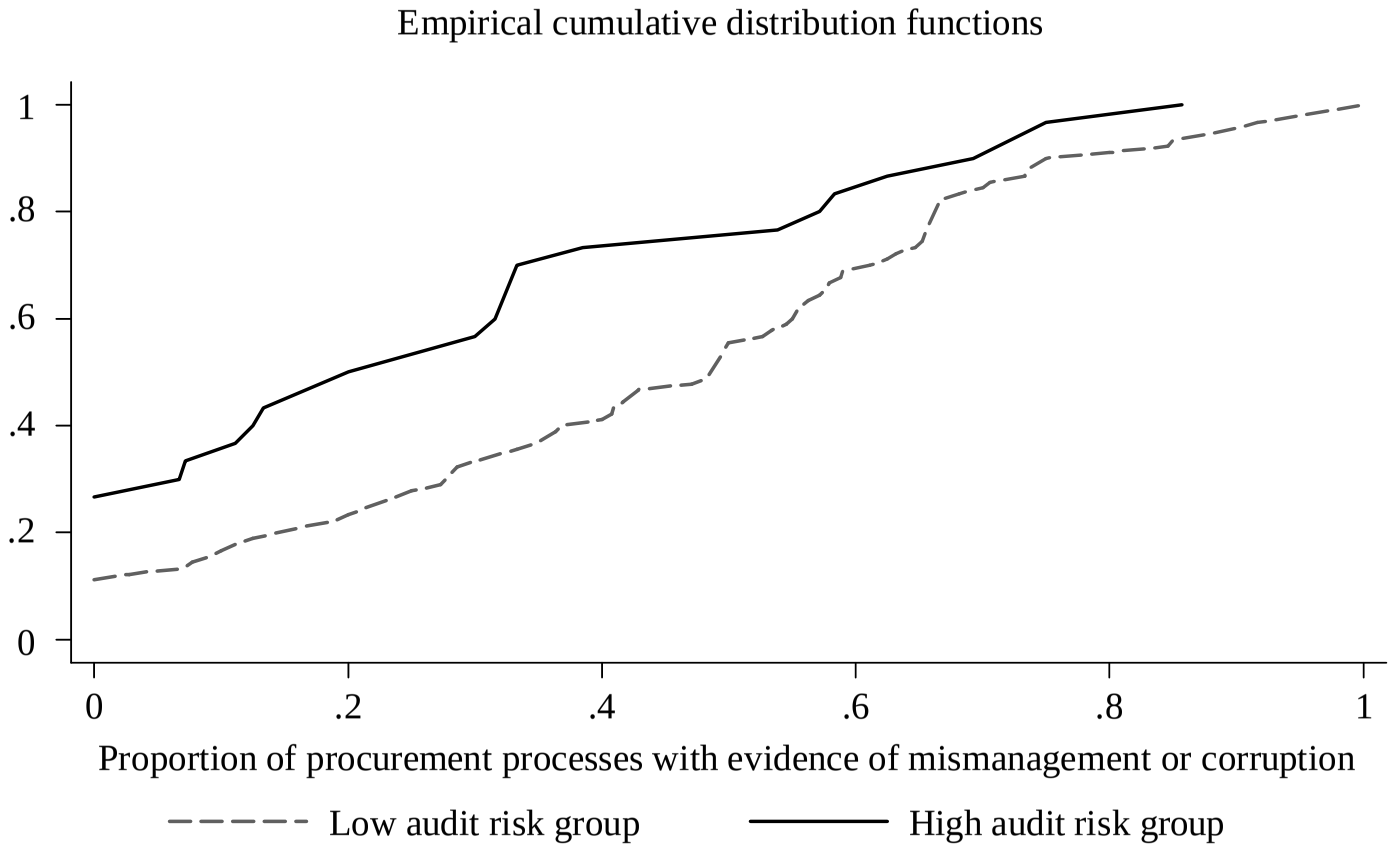
Notes: Sample consists of municipalities from the 32nd and 31st lotteries.

Figure 5: Impact on the distribution of irregular procurement processes



Notes: A procurement process is coded irregular if the audit result from Table 6 is anything other than regular or only a formal error. Sample consists of municipalities from the 32nd and 31st lotteries.

Figure 6: Impact on the distribution of mismanagement or corruption irregularities



Notes: Corruption corresponds to cases of simulated (fake) tender processes, cases of favouritism, or when auditors determine that there were unjustified or excessive payments for goods or services. Management irregularities correspond to fractionalized procurement amounts, procurement modalities that are too restricted, as well as other irregularities. See Table 6 for details. Sample consists of municipalities from the 32nd and 31st lotteries.