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

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Abstract

We report results from a randomized policy experiment designed to test whether increased audit risk deters rent extraction in local public procurement and service delivery in Brazil. Our estimates suggest that temporarily increasing annual audit risk by about 20 percentage points reduced the proportion of procurement processes with evidence of corruption by about 15 percentage points and the share of audited resources involved in corruption by about 10 percentage points. We show that these results are invariant to alternative corruption codings that have been used in prior literature. In contrast, we find no evidence that increased audit risk affected the quality of publicly provided preventive and primary health care services - measured based on user satisfaction surveys - or compliance with national regulations of the conditional cash transfer program *Bolsa Família*.

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1 Introduction

Waste and corruption are two key determinants of the cost of public service provision. However, measuring objectively whether public officials extract rents - either through shirking on the job or outright embezzlement of public funds - 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 policy experiment designed to test whether higher audit risk deters waste and corruption in local public procurement and improves public service provision at the municipality (*município*) level in Brazil.[2] 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. While higher audit risk should lower the expected gain 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 or service providers are punished through fines, loss of mandate or prison time is typically considered to be very low (Arantes 2004; Chaudhury, Hammer, Kremer, Muralidharan, and Rogers 2006). As a result, incumbent politicians might actually decide to increase corruption and forego the chance of re-election in response to higher audit risk, a theoretical possibility proposed by Besley and Smart (2007). To what extent higher audit risk deters waste and corruption in environments of low judicial accountability is therefore an open and important empirical question (Olken and Pande 2011).

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 percent audit risk in the control group, effectively consisting of the 5,400 remaining municipal-

[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 (2011) construct corruption measures based on Brazilian local government audit reports. Litschig and Zamboni (2012) use the same audit reports to measure rents more broadly, without distinguishing between waste and corruption. Niehaus and Sukhtankar (2012) compare official micro-records of a large Indian workfare program to household survey data.

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

ities in Brazil. The randomization was designed by the Brazilian federal government internal audit agency (*Controladoria-Geral da União*, CGU) and carried out and publicly announced in May 2009. 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. 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 percent annual audit probability. Since treatment group municipalities were never exposed to lower audit risk than those in the control group, treatment 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.

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 as in Barro (1973) or Persson and Tabellini (2000). For the vast majority of the regulations considered by auditors in Brazil, compliance is likely to be socially beneficial although typically privately costly.[3] 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.[4] 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 (2011), Litschig and Zam-

[3] 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. 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.

[4] Auriol, Straub and Flochel (2011) provide evidence on the excess costs for taxpayers associated with restricted procurement modalities, such as "exceptional" procedures by which regular public tenders are disregarded.

boni (2012), and Brollo, Nannicini, Perotti, and Tabellini (2013). The procurement data are at 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*).

In addition, we also code mismanagement and corruption episodes from the published audit reports. Previous papers that have used CGU reports all adopt their own definition of corruption and we explore the sensitivity of our results to existing alternative coding choices. Our broadest measure of corruption includes what could be considered instances of mismanagement, following the approach in Litschig and Zamboni (2012) and Brollo, Nannicini, Perotti, and Tabellini (2013). Such a comprehensive measure of rents is perhaps most appropriate for our purposes since the law is not limited to penalizing corruption - which requires a relatively high standard of proof - but allows prosecutors to charge individuals with the lesser offense of "acts of administrative misconduct". In addition, we use the "narrow" definition of corruption introduced by Brollo et al., as well as the even more stringent corruption coding in Ferraz and Finan (2011).

Our main empirical results provide clear evidence in favor of the classical prediction that local officials reduce rent extraction in procurement in response to higher audit risk, rather than not responding or even increasing rent-taking as in Besley and Smart (2007). Our estimates suggest that temporarily increasing annual audit risk by about 20 percentage points reduced the proportion of local procurement processes with evidence of corruption by about 15 percentage points and the share of audited resources involved in corruption by about 10 percentage points. We find that these results are invariant to alternative corruption codings used in prior literature. Higher audit risk did not affect the value of local purchases made during the study period or the amount audited.

Since the increase in audit risk was only temporary, we cannot say whether these impacts reflect a net reduction in rent extraction or merely a substitution over time - with managers in

high audit risk municipalities sitting out the period of increased scrutiny and making up at least some lost rents in subsequent periods, a "golden goose" effect in the terminology of Niehaus and Sukhtankar (2012). Nonetheless, we estimate that increasing audit risk benefited taxpayers more than it cost, even under conservative assumptions about the actual amount of money diverted and the extent of intertemporal substitution of corruption. Although these results are encouraging, it would take a permanent variation in audit risk to assess whether scaling up is advisable, since local officials might find ways to adapt to increased audit risk over time.

In contrast to the impacts we find in procurement, 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. Another interpretation is that there simply was not that much shirking on the job in preventive and basic health care delivery. We consider this possibility less likely since many control group respondents indicated that they did not get medical attention when they needed it, either at home or in the health facility.

We also find no evidence that higher audit risk had an effect on local compliance with national regulations 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. Similarly, administrative consequences of oversight failures by local program managers are limited. Another interpretation, which is supported in our data, is that local compliance with *Bolsa Família* requirements could not respond to higher audit risk because families and local administrators were already compliant to a large extent.

To our knowledge the only antecedent to our study is Olken (2007), who examines the effect of a higher audit probability on corruption in road construction in Indonesia. As in our case, Olken's randomized research design essentially evaluates the effect of a temporary (and project-specific) increase in audit risk. 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. Importantly for our study, he also finds that administrative irregularities in road construction detected by central government auditors are positively correlated with missing expenditures as determined by independent engineers. The main advantage of the corruption measures used here is that they are available for government procurement across the entire range of locally provided public services in Brazil, not only for road construction. Moreover, the survey data on user satisfaction allow us to go beyond input measures and examine potential effects on public service delivery.

The high levels of local compliance with eligibility requirements for the conditional cash transfer program and the zero effect of higher audit risk we document for Brazil are in line with recent evidence on in-kind transfers from sub-Saharan Africa. Dizon-Ross, Dupas, and Robinson (2014) use audits and survey data from bed net distribution programs in Ghana, Kenya, and Uganda to measure health facility-level compliance with targeting rules. In Ghana they also use a randomized research design where treatment consists of informing the facility that it would be monitored and the program potentially shut down in case of irregularities. They find unexpectedly high levels of compliance with targeting rules in all three countries and that the threat of audit did not affect performance in Ghana. Together with our evidence on Brazil, these results paint a more nuanced picture of the state of service delivery in developing countries than what available data and commonly held wisdom suggest.

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 or service providers. We discuss the experimental design in Section 4. In Section 5 we present the non-public data on irregularities in local public procurement and service delivery, as well as the data from published audit reports, and we discuss alternative corruption codings that have been used in prior literature. In Section 6 we describe our estimation approach and discuss potential measurement error bias. Results are presented in Section 7, along with a rough cost-benefit analysis. We conclude by summarizing results.

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.[5] Several rounds of sampling occur each year through a public lottery. The machinery used for the selection of municipalities is the same as that used for a popular national (money) lottery and results are broadcast on television and through other media. 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

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

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.[6]

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 report the results of their inspections back to CGU headquarters. Auditors also write a report, detailing the irregularities encountered during their mission. 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 adminis-

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

trative 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.[7]

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 mayor (Ferraz and Finan 2011).[8] Again, this type of punishment is swift and potentially costly for mayors on election day.

3 Theoretical predictions

Following the economic approach to crime, a procurement official or service provider 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. 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

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

[8] 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.

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 $p = p_c \times p_a$.[9] 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 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.[10] 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

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

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

municipalities in the treatment group, 30 would be sampled for a regular CGU audit one year later in May 2010.[11] 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.[12] 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.[13]

While the initially announced (ex ante) probability of an audit for treatment group municipalities was thus 25 percent, 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 municipalities were drawn from the treatment group and 30 from the control group.[14] 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 percent and 50 percent, with a modal probability of 25 percent. 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 percent.

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 municipali-

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

[12] 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).

[13] 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.

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

ties can be approximated as follows:

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

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).

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 Non-public local public procurement data

In contrast to the publicly available audit reports used in prior work, our procurement data are at the level of the individual procurement process. The procurements 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.[15] For each procurement process we know what was acquired, through which modality, and the most serious audit finding. Total purchase amounts, unit prices and amounts affected by irregularities are not routinely collected by auditors.[16] 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 municipalities from the 31st and 32nd lotteries.[17] 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 or that there were differences in the amount audited, as shown in the online Appendix, Tables 1 and 2, respectively.

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

[15] 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.

[16] To be precise, auditors are not required to report this information back to headquarters. In the published CGU audit reports, auditors typically report total purchase amounts and may report unit prices and an estimate of resources affected by irregularities. The exact amount diverted can only be assessed through a more detailed inspection which occurs only if it is subsequently deemed appropriate by the prosecutor in charge of the municipality.

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

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.[18] 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",[19] and the modality "only pre-registered bidders" (*tomada de preços*), which restricts competition to pre-registered suppliers.[20] 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 the control group from the 32nd lottery but only 44 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.

Table 3 in the online Appendix shows impact estimates for the municipality-level proportion of restricted procurement modalities. The fact that in the high audit risk group there are fewer restricted modalities 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".

5.2 Alternative corruption codings

Table 6 presents CGU auditors' classification of irregularities in procurement, as well as corruption and mismanagement codings by ourselves in prior work (LZ, 2012), Ferraz and Finan

[18]This distinction between procurement modalities 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.

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

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

(FF, 2011), and Brollo, Nannicini, Perotti, and Tabellini (BNPT, 2013).

The first procurement-related corruption category in Ferraz and Finan is 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". This corresponds to a subset of the "simulated tender process" and "evidence of favoritism" categories in the CGU classification, where non-provision of the good or service was somehow confirmed, which we do not distinguish in our data. Another procurement-related type of corruption is what they call "over-invoicing", in which "auditors determined that the goods and services were purchased at a value above market price", or "there is no proof of purchase and community member confirm that goods were not delivered", which corresponds to a subset of the "unjustified or excessive payments for goods and services" type of audit finding in the CGU classification. Another corruption case distinguished in Ferraz and Finan is not related to procurement, namely when resources "disappear" from municipal bank accounts. According to Ferraz and Finan (2011) a mismanagement episode in procurement occurs when "less than three firms bid for a public contract", corresponding approximately to the irregularity "invitation for bids to less than three firms" in the CGU classification.

Brollo et al. (BNPT, 2013) also use the CGU audit reports to construct a narrow and a broad corruption measure. Table 6 shows that their narrow corruption coding is broader than the corruption measure constructed by Ferraz and Finan. Specifically, Brollo et al.'s narrow corruption measure includes cases of "limited competition", corresponding roughly to the CGU "evidence of favoritism" category, "fraud", corresponding to the "simulated tender process" category, as well as "over-invoicing", which amounts to CGU's "unjustified or excessive payments for goods and services" category. The main difference with Ferraz and Finan's coding is the addition of "manipulation of the bid value", which CGU refers to as "fractionalizing of procurement amounts", that is, deliberate division of a purchase into smaller amounts in order to avoid unrestricted procurement modalities. Another difference with Ferraz and Finan is that corruption episodes are not restricted to those instances where non-provision of the good or service was somehow confirmed.

Brollo et al.'s broad corruption coding essentially corresponds to the management irregularities in Litschig and Zamboni (2012). Specifically, in their broad measure of corruption Brollo et al. also include "an irregular firm wins the bid process", corresponding roughly to "participating ineligible firm" in CGU terminology, "the minimum number of bids is not attained", which CGU labels "invitation for bids to less than three firms", as well as "the required procurement procedure is not executed", which roughly corresponds to CGU's "procurement modality too restricted".

Table 7 presents the distribution of audit results by the level of audit risk - high vs. low - and lottery. Corruption is coded as in Brollo et al.'s narrow measure. Management irregularities correspond to those considered in Brollo et al.'s broad measure, as well as the remaining mismanagement categories from Litschig and Zamboni. 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, the difference in the share of irregular procurement processes between high and low audit risk groups is essentially driven by corruption, rather than mismanagement, procedural or other irregularities. The shares of procurement processes indicating evidence of corruption in the two control groups are very close, 0.32 for the 32nd and 0.35 for the 31st lottery, respectively, while the corresponding share in the high audit risk group is 0.16.

5.3 Published audit reports

In addition to the process-level procurement data, we also use the published audit reports for the 60 + 60 municipalities that have been audited in March and May 2010, respectively. Our dataset is at the level of the inspection order and contains the year when the audited transaction was made, the amount audited, as well as detailed audit findings which we code in the same way as we did with the process-level procurement data. We follow the approach in Ferraz and Finan (2011) and Brollo et al. (2013) and impute the amount involved in corruption or mismanagement as the amount audited in a given inspection if at least one of the audit findings

indicate a corruption or mismanagement irregularity.[21] We compute the share of audited resources involved in corruption or mismanagement by aggregating across inspections within a given municipality.

5.4 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.[22] For the preventive and basic health care program (*Saúde da Família*), auditors first check the compliance of service units with ministry of health regulations, 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. 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.

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.[23] Auditors conduct field visits to check whether transfer recipient families are of a size and income level compatible with program eligibility rules and whether children's vaccinations are done regularly as required under the program. Auditors also check school and local program management records to assess compliance with enrollment and attendance conditionalities for obtaining the cash transfer.[24]

[21] Note that the amount involved or *valor envolvido* - which is routinely reported by CGU auditors - corresponds to the amount involved in the audited program or project, not the amount involved in corruption.

[22] There are other major programs than those considered here - 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.

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

[24] 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.

5.5 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 and potential measurement error bias

6.1 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 the (constant) 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:

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

Randomization ensures that, in expectation, D_m is uncorrelated with U_{mi}, so $\widehat{\beta}^{OLS}$ provides an unbiased and consistent estimator of β. For municipality-level outcomes, such as the share of audited resources involved in corruption we use OLS. For outcomes at the procurement processes level or for individual survey responses, we estimate equation (1) with WLS using municipality level averages and weights equal to the number of procurement processes or survey

respondents.

For the sake of transparency, we present results separately for the sample from the 32nd lottery and for the pooled sample including the 31st lottery, which we add to increase the precision of our estimates. It is worth emphasizing that including municipalities from the 31st lottery might lead to bias if outcomes were systematically different from one year to the next because the audit periods do not completely overlap as illustrated in Figure 1. Fortunately this turns out to be a minor issue for most outcomes as evidenced by the fact that point estimates vary only slightly across the 32nd lottery and pooled estimation samples. As a further robustness check, we restrict the sample of procurement processes to those that occurred in 2009 or 2010 - excluding 2008 - and again find similar results (available on request).

Since treatment probabilities vary somewhat by state due to the conditional randomization, we also present specifications with state fixed effects. 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 sample from the 32nd lottery we present impact estimates separately for each set of included pre-treatment covariates because this provides the most transparent assessment of small sample bias. For the pooled sample with 120 municipalities we present impact estimates with cumulative controls.[25]

6.2 Potential measurement error bias

A concern with our results - and indeed of any results based on audit reports - is that we cannot rule out that at least part of the estimated impact is due to fewer cases of corruption and mismanagement being detected in the high audit risk group; that is, perhaps local officials simply try harder (and sometimes succeed) to hide mismanagement and corruption episodes in response to increased audit risk. While this might be part of the story, there are two main reasons why reporting differences are unlikely to account for the entire estimated impact. First, hiding malfeasance is costly, so there will be instances where this extra cost exceeds the expected

[25]For the sample with 60 municipalities from the 32nd lottery the degrees of freedom become very small when we include all controls (24 state dummies, 13 party dummies, 8 municipality characteristics and 9 mayor characteristics). Results are available on request.

benefits of committing the offense (Becker 1968). Second, there is likely less underdetection of corruption based on an unexpected type of audit as conducted by engineers in Indonesia, compared to irregularities reported in routine audits. If missing expenditures and administrative irregularities are positively correlated not only in the Indonesian but also in the Brazilian setting, then at least part of the impact we find reflects a real reduction in rent extraction.

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

7 Estimation results

7.1 Impact on the share of procurement processes with evidence of corruption

Table 9 presents impact estimates on the proportion of procurement processes with evidence of broad corruption (BNPT 2013). 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 and cumulative controls. The estimates fluctuate around the -0.15 mark. Although they are quite variable, the confidence intervals show sub-

stantial overlap. Essentially all estimates are highly significant statistically. Figure 2 shows that higher audit risk shifted the entire distribution to the left.

Table 10 presents impact estimates on the proportion of procurement processes with evidence of narrow corruption (BNPT 2013). Point estimates and significance are similar to the broad corruption measure above. Figure 3 shows again that the entire distribution is shifted to the left under increased audit risk. Table 11 presents impact estimates on the proportion of procurement processes with evidence of corruption using the coding form Ferraz and Finan (2011). Point estimates are somewhat smaller and statistical significance is reduced compared to the corruption codings above. Figure 4 shows that the entire distribution is shifted to the left with higher audit risk.

7.2 Impact on the share of audited resources involving corruption

Table 12 presents impact estimates on the share of audited resources involved in broad corruption (BNPT 2013). The estimates fluctuate around -0.10 and are highly significant statistically. Figure 5 shows that higher audit risk shifted the entire distribution to the left. Table 13 presents impact estimates on the share of audited resources involved in narrow corruption (BNPT 2013). Point estimates and significance are similar to the broad corruption measure above. Figure 6 shows again that the entire distribution is shifted to the left with higher audit risk. Table 14 presents impact estimates on the proportion of procurement processes with evidence of corruption using the coding form Ferraz and Finan (2011). Point estimates are somewhat smaller and statistical significance is reduced compared to the corruption codings above. Figure 7 shows that the entire distribution is again shifted to the left with higher audit risk.

7.3 Cost-benefit analysis

Since the average amount audited was about 12 million Reais, the corruption reduction amounts to about 1.2 million Reais or roughly 0.5 million US$. 120 municipalities were exposed to higher audit risk so the potential cost saving amounts to US$ 60 million. In order to increase audit risk by 20 percentage points for these 120 municipalities, 24 extra audits were necessary, each costing about 50,000 US$. The marginal cost of the policy therefore amounts to about US$

1.2 million. Even if only 10 percent of the amount involved in corruption was actually wasted or stolen, the cost saving would still amount to US$ 6 million. And even if corruption increased somewhat in subsequent periods, the net benefit of increasing audit risk was likely positive.

7.4 Impacts on health service delivery

The top part of Table 15 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 15 shows no evidence that increased audit risk affected the quality of health care services provided by local governments. For example, the share of respondents who say they receive regular visits from community health staff - as required under the preventive health program - is essentially 93 percent in both treatment and control groups. The proportion of respondents who say they receive health care at home when needed is about 70 percent 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.

7.5 Impacts on compliance with *Bolsa Família* regulations

The bottom of Table 15 shows that higher audit risk did not seem to affect local compliance with national regulations 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. The high compliance rates evident in Table 15 suggest that the vast majority of *Bolsa Família* recipients were appropriately included in the program - they were poor enough - and that they fulfilled the health and education conditionalities to a large extent.

8 Conclusion

Our main empirical results provide clear evidence in favor of the classical prediction that local officials reduce rent extraction in procurement in response to higher audit risk, rather than not responding or even increasing rent-taking as in Besley and Smart (2007). Specifically, we show that temporarily increasing audit risk at the municipality level in Brazil by about 20 percentage points reduced the proportion of local procurement processes with evidence of corruption by about 15 percentage points and the share of audited resources involved in corruption by about 10 percentage points. We find that these results are invariant to alternative corruption codings that have been used in prior literature.

Whether the impacts on irregularities reflect a net reduction in rent extraction or merely a substitution over time - with managers in high audit risk municipalities sitting out the period of increased scrutiny and making up at least some lost rents in subsequent periods we cannot say. Nonetheless, we estimate that increasing audit risk benefited taxpayers more than it cost, even under conservative assumptions about the actual amount of money diverted and the extent of intertemporal substitution of corruption. Although these results 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.

In contrast to the impacts we find in local public procurement, 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 regulations 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 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 of oversight failures by local program managers are similarly limited. Another interpretation is that higher audit risk did not matter because families and local administrators were already compliant with *Bolsa Família* requirements to a large extent.

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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 level of audit risk and lottery

	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 level of audit risk and lottery

	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		
CGU classification of irregularities	%	LZ	FF	BNPT
- simulated tender process	6.05	M	C	C
- unjustified or excessive payments for goods and services	3.81	M	C	C
- evidence of favouritism	10.94	M	C	C
- fractionalizing of procurement amounts	9.06	M		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 (2012), FF: Ferraz and Finan (2011), BNPT: Brollo, Nannicini, Perotti, and Tabellini (2013), C: Corruption, M: Management/Mismanagement, P: Procedural. N=1,665 procurement processes. Ferraz and Finan (2011) code an irregularity as a case of corruption only if "the public good was not provided".

Table 7: Distribution of audit findings by level of audit risk and lottery

	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	38	10.76	60	12.37	119	14.57
Evidence of corruption	58	16.43	153	31.55	283	34.64
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	222	27.17
Formal error	67	18.98	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 share of procurement processes with evidence of broad corruption (BNPT 2013)

Dependent variable: proportion of procurement processes with evidence of broad corruption (BNPT 2013); control group mean 0.44, std. 0.25										
Treatment (0/1)	-0.157**	-0.104	-0.206***	-0.130*	-0.182**	-0.190***	-0.159***	-0.167***	-0.117*	-0.134**
	(0.074)	(0.069)	(0.074)	(0.073)	(0.070)	(0.053)	(0.051)	(0.054)	(0.060)	(0.062)
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	Y	Y
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	Y
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	0.091	0.609	0.336	0.304	0.222	0.091	0.499	0.609	0.696	0.720

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, as well as cases of fractionalized procurement amounts. Management irregularities correspond to instances where less than three firms were invited to submit bids or procurement modalities were too restricted or a participating firm was ineligible. 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, poverty gap, 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 share of procurement processes with evidence of narrow corruption (BNPT 2013)

Dependent variable: proportion of procurement processes with evidence of narrow corruption (BNPT 2013); control group mean 0.33, std. 0.25

Treatment (0/1)	-0.151**	-0.105*	-0.157**	-0.134*	-0.190***	-0.171***	-0.140***	-0.126**	-0.122**	-0.141***
	(0.072)	(0.062)	(0.073)	(0.073)	(0.057)	(0.046)	(0.047)	(0.056)	(0.065)	(0.063)
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	Y	Y
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	Y
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	0.097	0.661	0.283	0.290	0.362	0.080	0.518	0.590	0.615	0.641

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, as well as cases of fractionalized procurement amounts. See Table 6 for details. 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, poverty gap, 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 share of procurement processes with evidence of corruption (FF 2011)

Dependent variable: proportion of procurement processes with evidence of corruption (FF 2011); control group mean 0.23, std. 0.25

Treatment (0/1)	-0.052	-0.062	-0.048	-0.037	-0.054	-0.113**	-0.116**	-0.125**	-0.117*	-0.128**
	(0.073)	(0.070)	(0.080)	(0.059)	(0.076)	(0.045)	(0.046)	(0.060)	(0.062)	(0.060)
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	Y	Y
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	Y
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	0.014	0.573	0.157	0.387	0.284	0.038	0.482	0.530	0.569	0.620

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. 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, poverty gap, 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 share of audited resources involving broad corruption (BNPT 2013)

Dependent variable: share of audited resources involving broad corruption (BNPT 2013); control group mean 0.24, std. 0.25

Treatment (0/1)	-0.099**	-0.082*	-0.174***	-0.116**	-0.123**	-0.145***	-0.122***	-0.126***	-0.145***	-0.152***
	(0.047)	(0.041)	(0.061)	(0.043)	(0.050)	(0.035)	(0.036)	(0.035)	(0.040)	(0.052)
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	Y	Y
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	Y
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	0.072	0.695	0.389	0.335	0.263	0.074	0.562	0.634	0.732	0.761

Notes: OLS estimations. 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, as well as cases of fractionalized procurement amounts. Management irregularities correspond to instances where less than three firms were invited to submit bids or procurement modalities were too restricted or a participating firm was ineligible. 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, poverty gap, 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 share of audited resources involving narrow corruption (BNPT 2013)

Dependent variable: share of audited resources involving narrow corruption (BNPT 2013); control group mean 0.23, std. 0.25

Treatment (0/1)	-0.091*	-0.072*	-0.166***	-0.108**	-0.113**	-0.133***	-0.108***	-0.114***	-0.132***	-0.137***
	(0.047)	(0.040)	(0.061)	(0.043)	(0.050)	(0.036)	(0.036)	(0.035)	(0.040)	(0.051)
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	Y	Y
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	Y
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	0.060	0.698	0.370	0.345	0.252	0.061	0.566	0.629	0.727	0.750

Notes: OLS estimations. 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, as well as cases of fractionalized procurement amounts. 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, poverty gap, 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: Impact on share of audited resources involving corruption (FF 2013)

Dependent variable: share of audited resources involving corruption (FF 2011); control group mean 0.20, std. 0.24

Treatment (0/1)	-0.069	-0.070*	-0.128*	-0.087**	-0.084	-0.109***	-0.104***	-0.114***	-0.139***	-0.146***
	(0.046)	(0.041)	(0.066)	(0.042)	(0.051)	(0.035)	(0.036)	(0.033)	(0.039)	(0.052)
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party affiliation	N	N	Y	N	N	N	N	Y	Y	Y
Municipality characteristics	N	N	N	Y	N	N	N	N	Y	Y
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	0.037	0.672	0.245	0.348	0.194	0.044	0.462	0.551	0.657	0.696

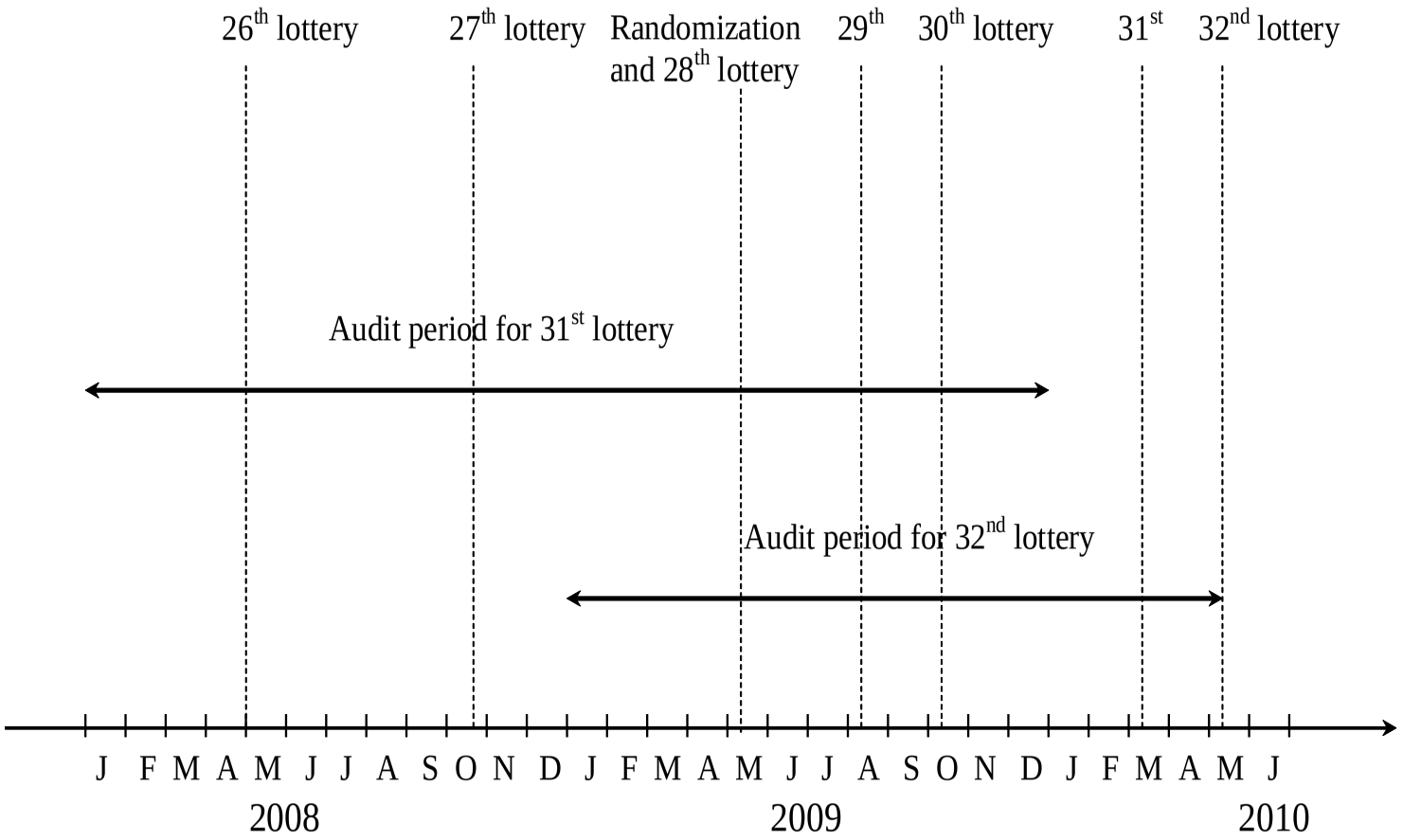
Notes: OLS estimations. Corruption corresponds to cases of simulated (fake) tender processes, cases of favouritism, or when auditors determine that there were unjustified or excessive payments for goods or services. See Table 6 for details. Sample consists of municipalities from the 32nd and 31st lotteries. 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, poverty gap, 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 15: 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)
	32nd lottery		31st and 32nd lottery	
	Control mean	Difference	Control mean	Difference
Proportion of *Bolsa Família* 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 Família* 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 Família* 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 Família* 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)

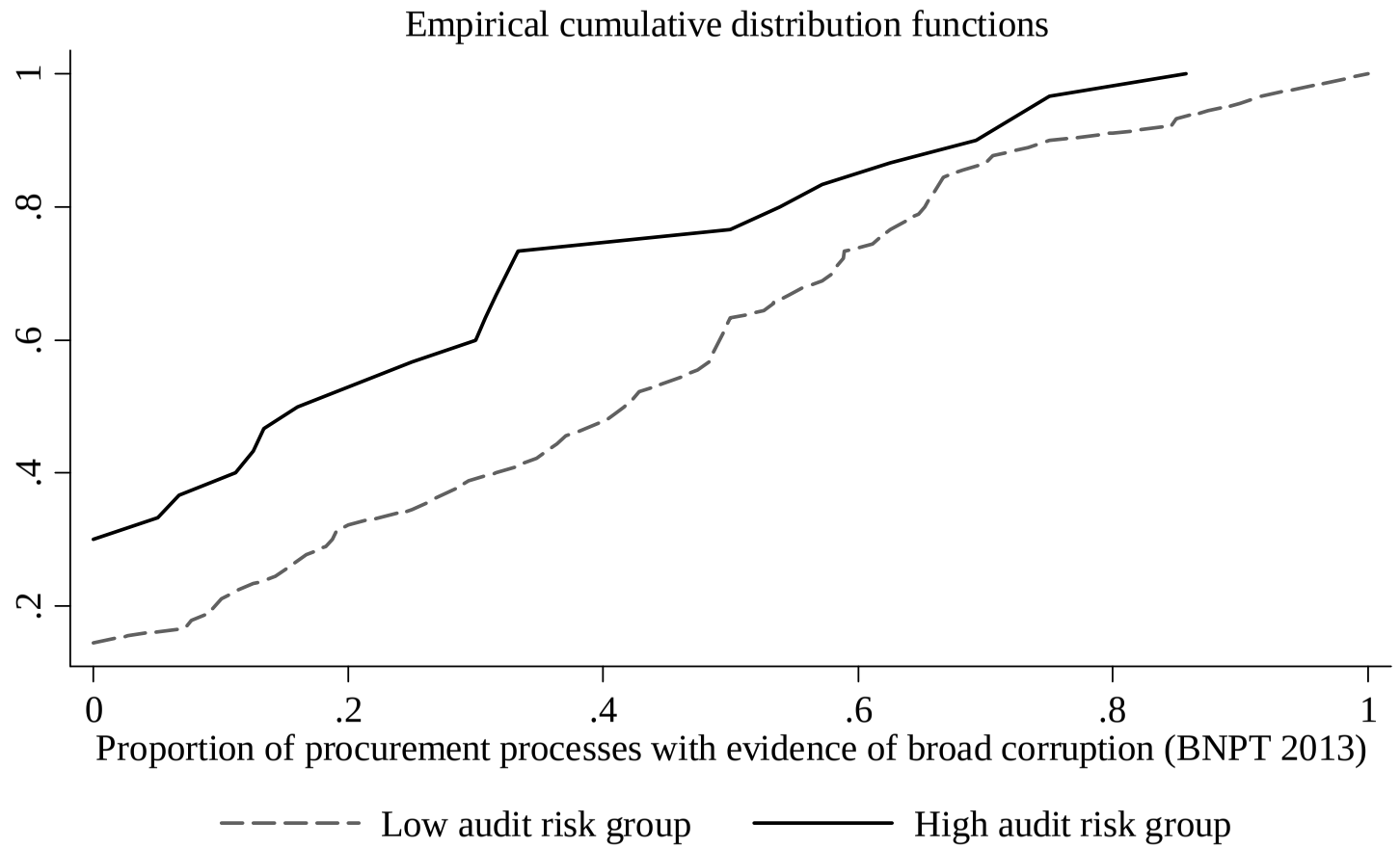
Notes: WLS estimations with weights equal to the number of survey respondents. The unit of observation is the municipality. Robust standard errors in parentheses. N varies by outcome. F-statistics are for the joint hypotheses that all differences in outcomes are zero.

Figure 1: Timeline



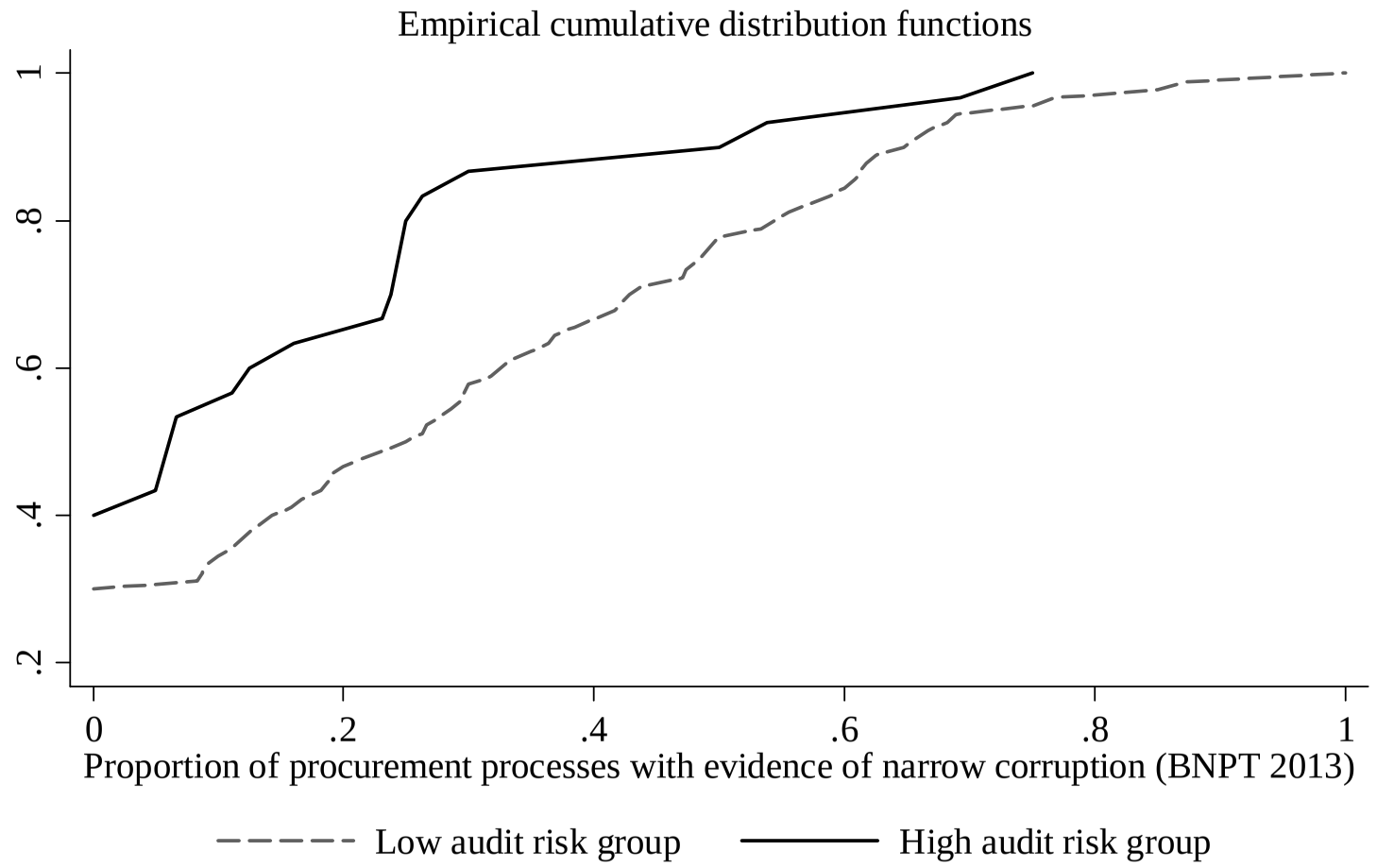
Notes: The 26th through 32nd were regular audit lotteries. 60 municipalities were sampled in each round. The randomization lottery coincided with the 28th lottery and randomly assigned 120 municipalities to the high audit risk group for the upcoming year. In the 32nd lottery 30 municipalities were drawn 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 broad corruption (BNPT 2013), proportion of processes



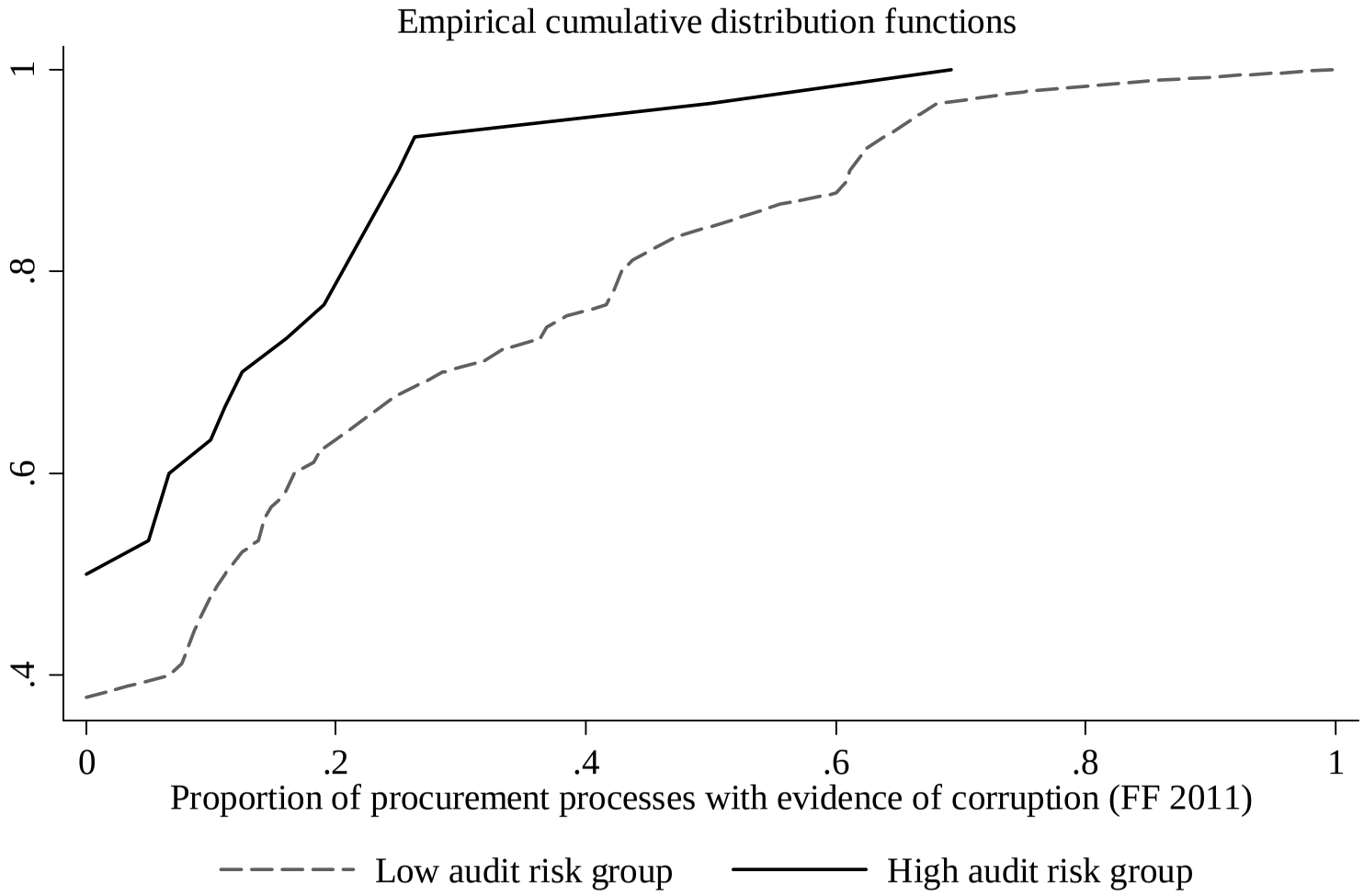
Notes: Corruption corresponds to cases of simulated (fake) tender processes, cases of favouritism, or when auditors determine that there were unjustified or excessive payments for goods or services, as well as cases of fractionalized procurement amounts. Management irregularities correspond to instances where less than three firms were invited to submit bids or procurement modalities were too restricted or a participating firm was ineligible. See Table 6 for details.

Figure 3: Impact on the distribution of narrow corruption (BNPT 2013), proportion of processes



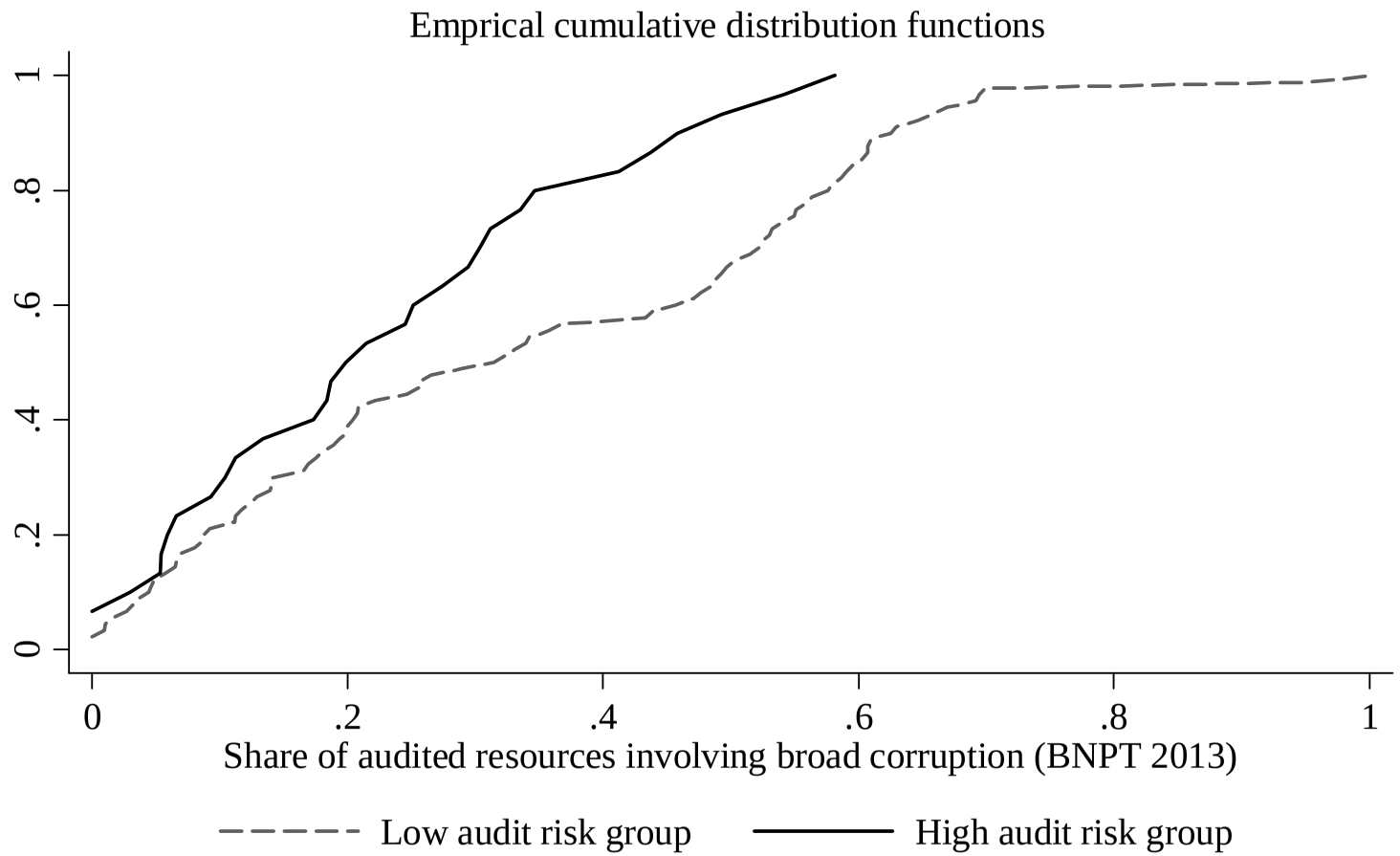
Notes: Corruption corresponds to cases of simulated (fake) tender processes, cases of favouritism, or when auditors determine that there were unjustified or excessive payments for goods or services, as well as cases of fractionalized procurement amounts. See Table 6 for details.

Figure 4: Impact on the distribution of corruption (FF 2011), proportion of processes



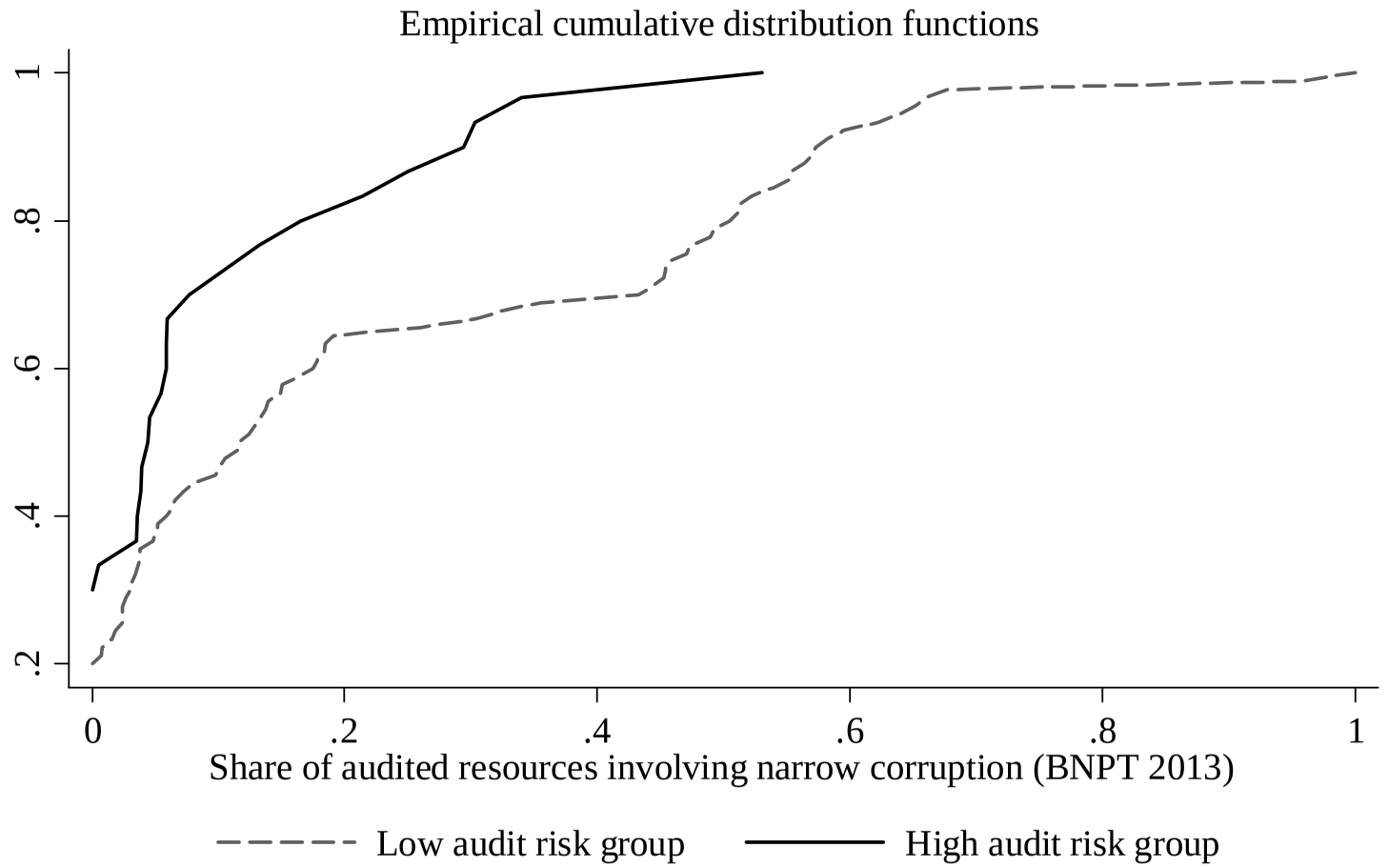
Notes: Corruption corresponds to cases of simulated (fake) tender processes, cases of favouritism, or when auditors determine that there were unjustified or excessive payments for goods or services. See Table 6 for details.

Figure 5: Impact on the distribution of broad corruption (BNPT 2013), share of audited amount



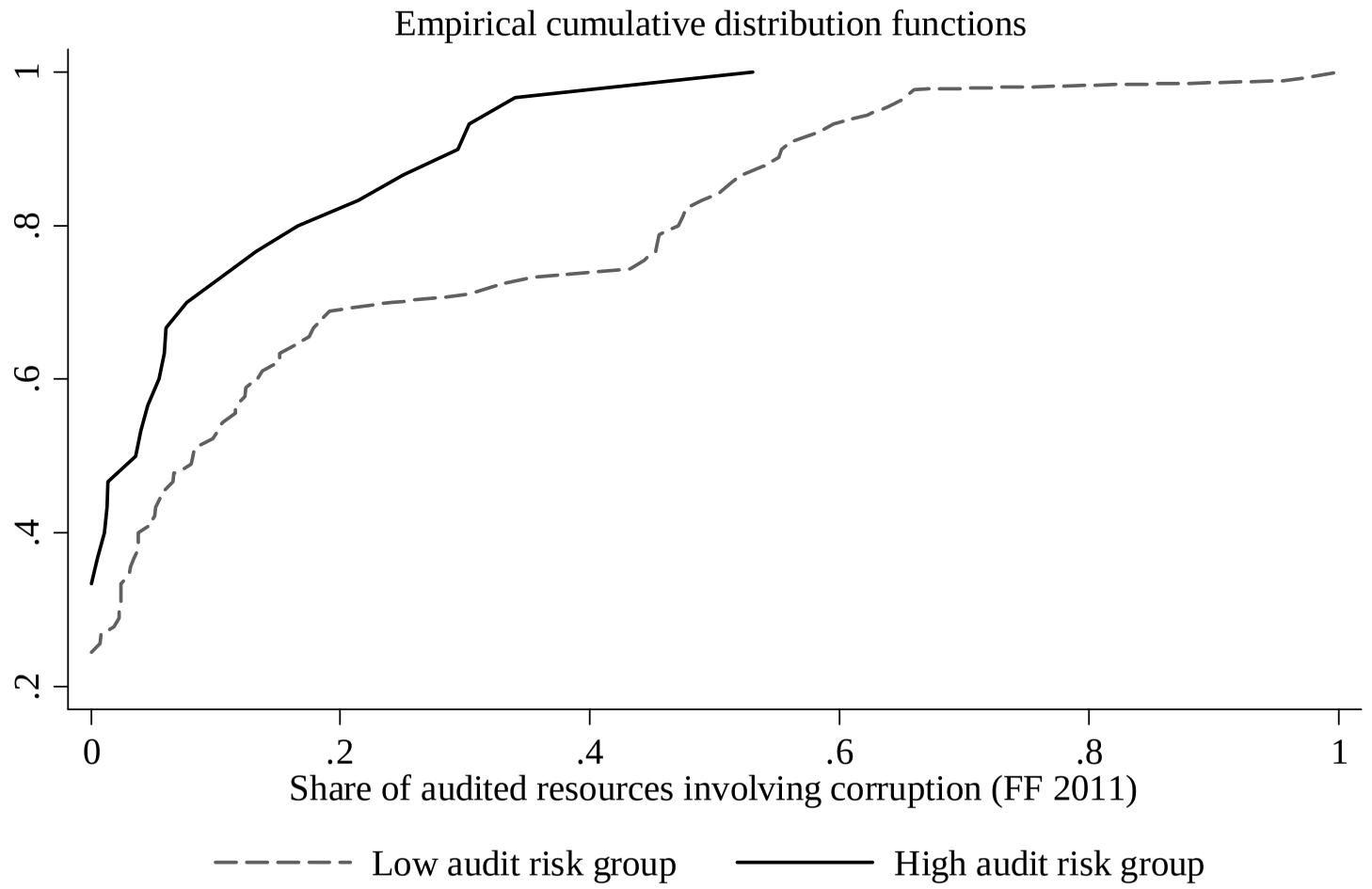
Notes: Corruption corresponds to cases of simulated (fake) tender processes, cases of favouritism, or when auditors determine that there were unjustified or excessive payments for goods or services, as well as cases of fractionalized procurement amounts. Management irregularities correspond to instances where less than three firms were invited to submit bids or procurement modalities were too restricted or a participating firm was ineligible. See Table 6 for details.

Figure 6: Impact on the distribution of narrow corruption (BNPT 2013), share of audited amount



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, as well as cases of fractionalized procurement amounts. See Table 6 for details.

Figure 7: Impact on the distribution of corruption (FF 2013), share of audited amount



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. See Table 6 for details.