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

Stephan Litschig
Yves Zamboni

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

Yves Zamboni[†]   Stephan Litschig[‡]

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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 government procurement and service delivery in Brazil. Our estimates suggest that temporarily increasing annual audit risk by about 20 percentage points reduced the share of audited resources involved in corruption by about 10 percentage points and the proportion of procurement processes with evidence of corruption by about 15 percentage points. The corruption reduction is entirely driven by procurement modalities that restrict competition and afford discretion to procurement officials in their choice of suppliers. 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 eligibility requirements for the conditional cash transfer program *Bolsa Família* - measured through household visits by auditors. We provide a simple model that rationalizes these findings and discuss implications for audit design.

Keywords: Government Audit, Corruption, Rents, Cash Transfer Program, Primary Health Care, Service Delivery

JEL: D73, D78, H41, H83, K42

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 difficult (Olken and Pande 2012). 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. Moreover, even well-identified studies find that in some combinations of context and type of rent extraction top-down monitoring works, while in other settings it does not.

This paper reports results from a randomized policy experiment that we designed jointly with the Brazilian federal government audit agency (*Controladoria-Geral da União,* CGU) in order to test whether - holding the context constant - higher audit risk deters rent extraction in local government procurement as well as in service delivery. We adapt Allingham and Sandmo's (1972) classic model of tax evasion to the case of rent extraction to show that one should expect substantial heterogeneity in responses to the same increase in audit risk depending on the parameters of the rent-extraction problem faced by public or private agents. For example, we show that an increase in audit risk alone may fail to substantively reduce rent-taking when sanctions and the probability that they are applied are very low, as is the case in public service delivery in many developing countries (Chaudhury et al. 2006). And although in local public procurement both potential sanctions and their likelihood of materializing are higher, the levels of these two key parameters might still be too low to deter corrupt officials even when audit risk goes up. Moreover, 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).

Our research design relies on the randomization of 120 municipalities into a high audit risk group, exposed to a 25 percent annual probability of being audited and a control group, effectively consisting of the 5,400 remaining municipalities in Brazil that were exposed to an annual audit risk of roughly 5 percent.[1] The randomization was carried out by CGU 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

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[1]Municipalities are the third tier of government in Brazil (below the federal and state governments).

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. In order to increase power, we sometimes pool the 30 control municipalities sampled for an audit in May 2010 with 60 control municipalities that were sampled two months earlier in March 2010. 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, the intervention consisted of a temporary increase in audit risk of about 20 percentage points.

We measure rents as irregularities in local public procurement and service delivery as determined by CGU auditors, judged against a uniform national standard. Irregularities in procurement or service delivery provide an objective measure of rent extraction by local executive officials, either through outright embezzlement or low effort on the job as in Persson and Tabellini (2000) for example, as long as compliance with regulations is socially beneficial. For the vast majority of the regulations considered by auditors in Brazil, compliance is likely to be socially beneficial although typically privately costly. In the terminology of Bandiera, Prat and Valletti (2009) irregularities uncovered by auditors therefore constitute a measure of active waste in government spending.[2] 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 modality is privately costly for the local manager.[3] Similarly, health ministry regulations require medical staff to provide certain service hours, which is again privately costly, yet beneficial for service users. For cash transfer program beneficiaries, we measure rents as excessive payments given the level of income and number of children in the household as determined by CGU field visits.

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 Zamboni (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 infrastruc-

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[2]It is worth noting that the regulations pertaining to public procurement reflect international best practices as laid out in the WTO's Agreement on Government Procurement.

[3]Auriol, Straub and Flochel (2016) provide evidence on the excess costs for taxpayers associated with restricted procurement modalities, such as "exceptional" procedures by which regular public tenders are disregarded.

ture, agriculture 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*). While CGU also considers other major programs in education for example, we do not have the corresponding microdata on audit findings.

In addition to the procurement process-level and survey data we analyze, 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 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 that local officials reduce rent extraction in procurement in response to higher audit risk. The estimates suggest that temporarily increasing annual audit risk by about 20 percentage points reduced the share of audited resources involved in corruption by about 10 percentage points and the proportion of local procurement processes with evidence of corruption by about 15 percentage points. The corruption reduction is entirely driven by procurement modalities that restrict competition and afford discretion to procurement officials in their choice of suppliers. Among unrestricted modalities, which consist of different types of procurement auctions, the incidence of corruption is lower overall and not affected by increased audit risk. We find that these results are invariant to alternative corruption codings used in prior literature and estimate that increasing audit risk benefited taxpayers more than it cost, even under conservative assumptions about the actual amount of money diverted.

In contrast to the impacts 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. Yet quality was reported to be substandard in at least some dimensions, with respondents reporting not being attended by a doctor or dentist when needed, or finding the public health post closed during stipulated opening hours. We show theoretically that both the non-response to higher audit risk and the evidence of shirking under normal monitoring conditions may arise under a relatively low probability of being sanctioned for irregularities conditional on detection. The sanctioning probability in service delivery is likely low because irregularities in service provision cannot be unambiguously identified 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 claim and auditors would have a hard time verifying which of these competing claims is true. In contrast, irregularities in procurement are relatively easy to prove because local officials are required to document each step of the purchasing process. Avis et al. (2016) provide systematic evidence that mayors are being prosecuted and convicted in response to CGU audits. We are not aware of any disciplinary actions being taken against local public health care providers.

In addition to a difference in the probability of sanction conditional on detection of an irregularity, there is also a difference in potential punishments in procurement vs. service delivery, which might account for the differential impacts we find. For the mayor and his direct subordinates who tend to run procurement, potential sanctions are relatively high as they include fines, loss of the job, jail, as well as administrative and electoral punishments. 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 2008). Again, this type of punishment is swift and potentially costly for mayors on election day and electoral incentives matter for corruption as shown in Ferraz and Finan (2011). For service delivery irregularities in contrast, sanctions only include fines or loss of the job. Our model formalizes these two simple rationales for the differential response to audit risk in procurement vs. service delivery. Taken together, our theoretical and empirical results question the deterrence value of having auditors conduct household surveys designed to assess service quality because no hard evidence is

being generated.

As with health service delivery, we find no evidence that higher audit risk had an effect on inclusion errors and overpayments to beneficiaries of the cash transfer program *Bolsa Família* or their compliance with health and education conditionalities. In contrast to health service delivery however, households and local administrators who run and oversee the program were already compliant with eligibility and conditionality requirements in the absence of extra scrutiny. Our model suggests that both the non-response to higher audit risk and the high compliance with program requirements under normal monitoring conditions may arise when the probability of being sanctioned for irregularities even in the absence of an audit is relatively high. Intuitively this is because the level of income - which determines program eligibility - and the number of children - which determines program generosity - are relatively easy to observe for program managers and the public at large. In contrast, public officials involved in procurement or health service delivery can hide their actions (or lack thereof) more easily and irregularities typically require an audit to be revealed, thus making zero rent-taking less likely. Our theoretical and empirical results call into question the rationale for auditor household visits designed to assess compliance with cash transfer program requirements because not much new information is being generated.

The closest antecedent to our study is Olken (2007) who examines the effect of a higher audit probability on corruption in the execution of road construction projects in Indonesia. As in our case, Olken's randomized research design essentially evaluates the effect of a temporary (and in his case 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. Another directly related study by Di Tella and Schargrodski (2003) investigates prices paid for basic supplies by hospitals in the city of Buenos Aires once the city government starts to monitor prices more closely. They find that prices fall by about 15 percent in the short-run and by about 10 percent nine months into the crackdown.[4]

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[4]More tangentially related is the recent work by Avis et al. (2016), who exploit the random sampling of municipalities to investigate the impact of an actual audit on subsequent corruption, as well as the paper by Lichand et al. (2016), who use a difference-in-differences approach to study effects of the introduction of the Brazilian random audits program.

Our study is the first to show that corruption in public procurement is susceptible to increased audit risk. In contrast to Olken's study on the execution of road construction projects, our corruption measure also includes the bidding and awarding stages that are typical of public procurement and it covers a wide range of locally provided public services. Our procurement process-level data also allow us to document for the first time that the higher discretion afforded to procurement officials under restricted procurement modalities is more frequently abused to facilitate corruption compared to procurement auctions and that the corruption reduction is entirely driven by discretionary modalities. Our findings thus suggest that there is a trade-off between rules and discretion in public procurement in Brazil, contrary to what Bandiera et al. (2009) find for Italy, where public bodies with more autonomous managers do not exhibit higher levels of active waste. An additional advantage of our research design is that all dimensions of procurement and service delivery in the municipality were subject to higher audit risk. In prior studies in contrast, the additional scrutiny was specific to road construction or a limited number of basic hospital supplies, respectively, and village heads and local public managers might therefore have increased corruption in unmonitored activities. As documented in Yang's (2008) study of increased enforcement against a specific method of avoiding import duties, crime displacement can be substantial.

There are also two relevant randomized studies from different Indian states that investigate impacts of top-down monitoring of public-sector health care workers through policy tools other than audits. Banerjee, Glennerster and Duflo (2008) study an intervention where a nongovernmental organization recorded the presence of nurses at randomly selected public health facilities. They find that although absenteeism was reduced in the short run, eighteen months after its inception the program had become completely ineffective because it was undermined from the inside by program administrators. Dhaliwal and Hanna (2013) study a technological solution that reduced the cost of monitoring attendance of health care workers. They also find that absenteeism was reduced essentially over the entire duration of the pilot program. In addition, Dhaliwal and Hanna find that health outcomes, such as low birth weight, improved. Since attendance monitoring in both of these studies was much more objective than what auditors could accomplish only based on user satisfaction surveys, it is not surprising in light of our model that these studies find impacts of increased monitoring on service delivery outcomes while our study does not.

The high levels of local compliance with eligibility requirements for the conditional cash trans-fer 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 the intervention 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 suggest that corruption in cash or in-kind transfer programs is not a first-order concern in developing countries and that the threat of audit in such programs is ineffective. Our model suggests that these findings can be explained by a relatively high probability of being sanctioned for irregularities even in the absence of an audit.

The paper is organized as follows. In Section 2 we describe the audits program and give insti-tutional background on potential judicial, administrative, and electoral punishments that may arise from the detection of irregularities in the local public administration. Section 3 presents theoreti-cal predictions regarding the effect of higher audit risk on stealing and shirking by local officials, service providers, and cash transfer recipients. 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 the main results in light of our model.

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 spend-

ing. 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. Importantly, auditors are paid to execute these inspections and there is no performance bonus for detecting irregularities. Teams of auditors that are based in the state CGU 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.

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

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

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. Final audit reports are sent to local legislatures, the federal ministries remitting the transfers, external audit institutions at state and federal levels, state and federal prosecutors, as well as 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.[6] In addition to these potential judicial sanctions, administrative and electoral sanctions are also possible as shown in Brollo (2012) and Ferraz and Finan (2008), respectively.

3 Theoretical predictions

This section presents a simple model to show that an increase in audit risk alone does not necessarily reduce rent-taking in environments where sanctions and the probability that they are applied are too low. The model is tailored to our setting where audit risk increased for all agents in treatment municipalities, including the mayor and his direct subordinates, health service providers, as well as *Bolsa Família* beneficiaries. The nature of rent-taking varies across agents, as do some of the parameters in the agents' optimization problems, such as the type of sanction they face if caught and the probabilities of sanction with and without an audit. The model is adapted from Allingham and Sandmo's (1972) classic analysis of tax evasion and delivers more realistic and nuanced predictions than the Becker (1968) model of crime, which assumes that the gain from committing an offense is fixed. Nonetheless, the results that higher audit risk does not unambiguously deter rent extraction and that the magnitude of the effect depends on the probability that sanctions are applied conditional on detection also obtain in the Becker model.

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[6]See Arantes (2004, 2007) on the organization and legal instruments at the disposal of the Brazilian *Ministerio Público*.

Let rent extraction Y be a continuous variable with support $[0, \overline{Y}]$, where $\overline{Y}$ denotes a natural upper bound. In procurement this would be some fraction of the total amount of federal transfers received in a given period that the mayor and his subordinates could appropriate. In service delivery, $\overline{Y}$ denotes the nominal number of working hours of doctors and other medical personnel and Y is the hours spent shirking on the job. For cash transfer recipients, $\overline{Y}$ represents the maximum excess payment that could be achieved by over-reporting household size or under-reporting income. $\overline{Y}$ may vary across agents and is likely largest in procurement, followed by service delivery, followed by cash transfer or other welfare programs.

Utility is assumed increasing and concave in rents, $U'(Y) > 0, U''(Y) < 0$. Expected utility depends on two key parameters: the magnitude of sanctions if caught, πY, which we assume are linearly increasing in Y, $\pi > 0$, and the probability that sanctions are applied p. Sanctions could be judicial, administrative, or electoral, or a combination of these, depending on the agent. For example, the mayor and his direct subordinates likely care about electoral sanctions, while lower level and field staff might be relatively insulated from electoral pressure and might care little if program funds are cut (as long as their salaries are not touched). For welfare programs the most likely sanction consists in loss of the entire benefit and perhaps some form of social sanction.

In each period the agent will choose Y so as to maximize

$$E(U) = pU(Y - \pi Y) + (1 - p)U(Y). \tag{1}$$

Our model is static, which we believe is appropriate in our context as intertemporal linkages are limited. For example, stealing less today does not imply that more can be stolen tomorrow. This is because federal transfers for current spending cannot be saved for later periods. Federal transfers for budgeted capital projects can only be postponed in duly justified and authorized cases.[7] Empirically we find no evidence that the composition of purchases, the level of federal transfers or the amount audited were affected by the treatment. Likewise, the number of nominal working hours or the maximum cash transfer payment are fixed each period.

Stealing less today may, admittedly, increase the probability to be in office or in the job or in the program in future periods, which our model implicitly takes for granted. One could therefore

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[7]Public procurement law N° 8666, Art. 8 and Art. 26.

imagine that agents in high audit risk municipalities reduce rent extraction during the period of extra scrutiny even more than they would if they knew that they will have the opportunity to extract rents for sure in the next period, a "golden goose" effect in the terminology of Niehaus and Sukhtankar (2013). Nonetheless, the decision problem remains essentially static: at the beginning of the post-monitoring period, the audit risk and other parameters are exactly the same as they were before the monitoring hike and therefore rent extraction should be back to the level that would have prevailed in the absence of the intervention. It is therefore theoretically unlikely that lower rents during the period of extra scrutiny are offset by higher rents in future periods.

The first-order condition for an interior maximum of (1) is

$$\frac{\partial E(U)}{\partial Y} = p(1 - \pi)U'(Y - \pi Y) + (1 - p)U'(Y) = 0. \tag{2}$$

The second-order condition

$$p(1 - \pi)^2 U''(Y - \pi Y) + (1 - p)U''(Y) < 0, \tag{3}$$

is satisfied as long as utility is concave in rents.

In order to assess under what parameter values there is an interior solution, we evaluate expected marginal utility (2) at $Y = 0$ and $Y = \overline{Y}$. In particular, we must have that

$$\frac{\partial E(U)}{\partial Y}|_{Y=0} = p(1 - \pi)U'(0) + (1 - p)U'(0) > 0$$

and

$$\frac{\partial E(U)}{\partial Y}|_{Y=\overline{Y}} = p(1 - \pi)U'((1 - \pi)\overline{Y}) + (1 - p)U'(\overline{Y}) < 0.$$

We can rewrite these conditions as

$$1 > (\pi - 1)\frac{p}{(1 - p)} \tag{4}$$

and

$$(\pi - 1)\frac{p}{(1 - p)} > \frac{U'(\overline{Y})}{U'((1 - \pi)\overline{Y})}. \tag{5}$$

From (4) we get that if the combination of π and p is too high then it is optimal not to shirk or steal

at all. And from (5) we can see that if the combination of π and p is too low then it is optimal to extract as much rents as possible, i.e. $\overline{Y}$. Specifically, we need $\pi > 1$, otherwise the level of p does not matter at all and it is optimal to extract maximum rents even as p approaches 1. Intuitively we need $\pi > 1$ because the marginal sanction if caught has to be higher than the marginal gain from taking rents. Even when $\pi > 1$, an increase in p might not be large enough to increase the left-hand side of (5) above the right-hand side and reduce Y below $\overline{Y}$. Because the right-hand side of (5) is smaller than 1 as long as there are some rents to be had ($\overline{Y} > 0$), there are parameter combinations that satisfy both inequalities so that an interior solution may exist.

Our experiment did not vary p directly, but rather p_a, the probability of a central government audit. Let $p_{s|a}$ denote the probability of sanctions conditional on receiving an audit and $p_{s|na}$ the probability of sanctions in the absence of an audit, so that $p = (p_{s|a} - p_{s|na}) \times p_a + p_{s|na}$. We assume throughout that $(p_{s|a} - p_{s|na}) \geq 0$, i.e. sanctions are more likely with than without an audit. Differentiating (2) with respect to p_a and Y we get

$$\frac{\partial Y}{\partial p_a} = -\frac{(1 - \pi)U'(Y - \pi Y) - U'(Y)}{\left[\frac{1 - p_{s|na}}{p_{s|a} - p_{s|na}} - p_a\right]U''(Y) + \left[p_a + \frac{p_{s|na}}{p_{s|a} - p_{s|na}}\right](1 - \pi)^2 U''(Y - \pi Y)} < 0. \quad (6)$$

This derivative is negative; if there is an interior solution, rent extraction is decreasing in p_a.

The first prediction from this simple model is that an increase in p_a does not necessarily reduce rent-taking in environments where sanctions and the probability that they are applied are too low. The smaller is $p_{s|a}$ the smaller the increase in p when p_a goes up and the less likely it is that the left-hand side of (5) becomes larger than the right-hand side if the level of sanctions and the initial probability that they are applied are too low. Put differently, in an environment of impunity rent-taking may stay at $\overline{Y}$ despite the increase in p_a. In the Brazilian setting analyzed here - as in many other countries - the risk of sanction in the absence of an audit, the risk of audit, and the probability that local officials, service providers or cash transfer recipients are punished conditional on an audit are typically considered to be very low. It is thus not obvious that increased audit risk on its own would deter rent extraction. Moreover, even if there is an interior solution under normal monitoring conditions, inspection of (6) suggests that the magnitude of the derivative approaches zero when $p_{s|a}$ decreases. As a result, an increase in audit risk alone may not curb rent extraction much if the link between detection through an audit and eventual sanction is weak, i.e. $p_{s|a}$ is low.

Our simple model also suggests an intuitive interpretation of the differential impacts we find across procurement, health service delivery, and cash transfer sectors. The null result for the cash transfer program together with close to zero inclusion errors and overpayments in the control group is consistent with a high probability of sanction even in the absence of an audit. In the model, a high $p_{s|na}$ means a high p even without increased p_a and hence $1 < (\pi - 1)\frac{p}{(1-p)}$ which yields a corner solution at zero. This makes intuitive sense because the number of children or the level of income are relatively easy to observe for program managers or the public at large even in the absence of an audit. In contrast, $p_{s|na}$ is likely lower in procurement or service delivery because public officials can hide their actions more easily, thus making corners at zero less likely.

Moreover, $p_{s|a}$ is likely lower in service delivery than in procurement because shirking in service provision cannot be identified with the same precision as irregularities in procurement. In the limiting case we have $p_{s|a} = p_{s|na} = 0$ in service delivery, in which case higher audit risk would not matter to service providers and they would continue to shirk to the extent possible. In terms of the model, p would not increase if $p_{s|a} = p_{s|na}$ and so despite the increase in p_a the corner at $\overline{Y}$ would remain unchanged. In procurement in contrast, $(p_{s|a} - p_{s|na}) > 0$ is likely because procurement irregularities cannot be argued with (at least not to the same extent as reports of shirking on the job). As a result, increased audit risk might reduce rent-taking in procurement if the left-hand side of (5) gets pushed above the right-hand side.

The prediction that increased audit risk should reduce rent extraction more in procurement compared to service delivery also arises through two additional channels. First, π is likely higher for the mayor and his direct subordinates who tend to run procurement because in addition to potential judicial or administrative sanctions, they may also worry about electoral punishments. From (5), a higher π makes it more likely that a given increase in p lifts the left-hand side above the right-hand side and breaks the corner at $\overline{Y}$. Similarly, a higher π makes it more likely that rent extraction is initially not at its upper bound and thus responsive to higher audit risk as shown in (6). Finally, the prediction that audit risk should reduce rent extraction more in procurement compared to service delivery also arises because the upper bound is likely higher in procurement. Again from (5), a higher level of $\overline{Y}$ leads to a smaller fraction on the right-hand side of the inequality, making it more likely that a given increase in p breaks the corner at $\overline{Y}$.

4   Experimental design

We designed the experiment jointly with the Brazilian federal government audit agency in order to test whether - holding the context constant - higher audit risk deters rent extraction in local government procurement as well as in service delivery. Our key idea was to build on the existing random sampling of municipalities that had been going on since 2003 and create a randomized treatment group exposed to temporarily higher audit risk. The randomization of treatment status was carried out publicly 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 high audit risk group was stratified by state as shown in Table 1 in the online Appendix.

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.[8] In order to ensure that municipalities were aware of their treatment status, mayors in treatment group municipalities also received a letter from CGU containing this information. 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. At the time of the randomization it was also announced that the high audit risk municipalities 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.[9]

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 in the online Appendix presents audit probabilities that municipalities from different states faced in the 29th lottery. For most states, audit probabilities per round of the lot-

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[8]Portaria N° 930, May 8 2009.

[9]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° 930, May 8 2009).

tery - P(Draw) - were about 1 or 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.[10] Table 1 shows that ex post audit probabilities in the treatment group varied between 16.7 percent and 50 percent because sampling was stratified by state. The modal sampling probability in the treatment group was 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 31st lotteries 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 1 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 power, we sometimes pool the 30 control municipalities sampled for an audit in May 2010 with 60 control municipalities that were sampled two months earlier in March 2010. These extra control municipalities were exposed to exactly the same annual audit risk as those that were sampled in May 2010 (see Figure 1).

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[10]Portaria N° 862, April 30 2010.

5 Data

This section presents our microdata 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 were audited in March and May 2010, respectively. Audit findings for each municipality were compiled into a database by CGU staff. Following CGU practice, we refer to the reported infractions of public sector management regulations as irregularities. It is worth emphasizing that each reported irregularity constitutes a breach of a specific legal norm by a local official, service provider or cash transfer recipient and is potentially subject to a range of administrative, judicial and electoral sanctions.

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 purchasing process. For the 75% of municipalities with population below 20,000 in our sample, 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, agriculture and transportation. For larger municipalities only a subset of sectors is covered depending on the lottery. Within a sector, the procurement data 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.[11] Because only the year, not the date, of each procurement process is given in our data, we cannot exclude processes that were completed prior to May 2009. Because these purchases could not have been affected by higher audit risk by construction, the inclusion of these processes will bias our estimates towards zero. 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 reported back to headquarters.

There are six procurement modalities in total, three of which restrict the number of competitors and are legal only below certain purchase amounts, and another three modalities without restric-

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[11]In the early years of the program, CGU used a sampling scheme to select only a subset of purchases for audit.

tions on the number of competitors.[12] 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", and the modality "only pre-registered bidders" (*tomada de preços*), which restricts competition to pre-registered suppliers. Unrestricted modalities consist of different types of procurement auctions, namely the "sealed-bid (reverse) auction" (*concorrência*), the "on-site (reverse) auction" (*pregão presencial*) and the "electronic (reverse) auction" (*pregão eletrõnico*).

5.2 Alternative corruption codings

Table 2 presents CGU auditors' classification of irregularities in procurement, as well as corruption and mismanagement codings by Ferraz and Finan (FF, 2011), ourselves in prior work (LZ, 2012), and Brollo, Nannicini, Perotti, and Tabellini (BNPT, 2013). A more detailed description is contained in the online Appendix. The most serious and uncontroversial instances of corruption arise when auditors detect evidence that the tender process was entirely simulated, such as when the winning firm in fact did not exist. More subtle forms of corruption involve steering the public contract to favored firms or paying more than what the winning bid in the auction had been. In addition, there are irregularities such as fractionalizing a purchase or simply choosing a modality that is too restricted given the amount of the purchase. These constitute breaches of procurement regulations designed to ensure that the public pays the lowest price available for a given good or service required. Even if there was no corruption involved, these irregularities imply that procurement agents were shirking on the job - because implementing a competitive procurement procedure, such as a (reverse) auction, is privately costly for the local manager - and that the public most likely overpaid.

5.3 Published audit reports

In addition to the process-level procurement data, we also use the published audit reports as in prior studies. Our dataset is at the level of the inspection order (ordem de serviço) and contains the year when the audited transaction was made, the amount audited, as well as detailed audit

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[12]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 (AGP) in Article VII.3. Brazil is not formally a member of the Agreement.

findings which we code in the same way as we did for the process-level procurement data. The amount involved in each irregularity is only occasionally reported in the audit reports from the two rounds of lotteries we consider. For example, out of 981 procurement-related irregularities, 770 or 78% do not specify an amount. Similarly, out of 61 irregularities related to simulated tender processes, 46 or 74% do not specify an amount. In order to get an "amount of funds involved" in the irregularity, we impute the amount investigated in a given inspection if at least one of the audit findings indicate a corruption or mismanagement irregularity. We compute the share of audited resources involved in corruption or mismanagement by aggregating across inspections within a given municipality. This imputation likely overstates the actual amount of money wasted or stolen. Based on those irregularities where auditors state an amount affected, we compute that this represents on average about 40% of the total amount investigated. Our best estimate of the actual amount wasted or stolen is therefore about 40% of the amount involved in corruption. And because 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 - our cost-benefit analysis below will assume that only 10% of the amount involved in corruption is diverted.

5.4 Survey data

As part of their standard inspections, CGU auditors conduct interviews and field visits at both household and service-unit levels that are designed to assess public service quality. 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. Since survey responses are not made publicly available, we have little reason to believe that there is systematic under-reporting of substandard performance.

For the conditional cash transfer program *Bolsa Família*, the CGU headquarters provides auditors in the field with a list of typically 30 randomly sampled transfer recipient households based on a national sampling frame, although the exact number of respondents can vary depending on conditions in the field. Auditors conduct household visits to check whether transfer recipient families are of a size and income level compatible with program eligibility and generosity regulations, 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. While household visits allow auditors to assess inclusion errors and overpayments to beneficiaries of the *Bolsa Família* program fairly accurately, compliance with education and health conditionalities might be overstated by local officials if they collude with program beneficiaries.

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 3 gives sample means for a host of pre-treatment covariates broken down by level of audit risk and by whether the municipality was actually audited in rounds 31 or 32. As is evident from Table 3, there are no important differences between these four groups along observable dimensions. Moreover, the differences in means between high and low audit risk groups among municipalities that were actually audited are never statistically significant at five percent and only once at 10 percent. Table 3 also provides a joint test of the null hypotheses that the population means are equal across treatment and control groups for municipalities that were actually audited. 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, α the mean outcome in the low audit risk (control) group, β the (constant) treatment effect, D_m the high audit risk (treatment) group indicator and U_{mi} the influence of other unobserved factors that affect the outcome. The data generating process can then be written as:

$$Y_{mi} = \alpha + \beta D_m + U_{mi} \tag{7}$$

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 process level we also use OLS and cluster standard errors at the municipality level. For individual survey responses, we estimate equation (7) with WLS using municipality level averages and weights equal to the number of survey respondents, which vary depending on the program and whether the survey was at the household or service-unit level.

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 stratified randomization, we also present specifications with state fixed effects. We provide a check on small sample bias by in-

cluding 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.[13]

Unfortunately, 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.

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 benefits of committing the offense (Becker 1968). Second, Olken's (2007) study finds that administrative irregularities in road construction detected by central government auditors are positively correlated with missing expenditures as determined by independent engineers. And 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. As a result, available evidence suggests that administrative irregularities detected by auditors do capture at least part of the true level of rent extraction.

A related caveat is that we need to assume that auditors themselves were not bribed into manipulating audit findings. 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

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[13]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.

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 Results

7.1 Impact on the share of audited resources involved in corruption

Table 4 panel A presents impact estimates on the share of audited resources involved in narrow 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 pooled sample, including control municipalities from the 31st lottery and with cumulative controls. The estimates fluctuate around -0.10, down from a control group mean of 23 percent, and are highly significant statistically. Although the estimates are quite variable, the confidence intervals show substantial overlap. For example, the 95 percent confidence interval for the true impact ranges from -0.18 to 0.00 in the first column and from –0.24 to -0.04 in the last column of panel A. Figure 2 shows that higher audit risk shifted the entire distribution to the left.

Table 4 panel B presents impact estimates on the share of audited resources involved in broad corruption (BNPT 2013). Point estimates and significance are similar to the narrow corruption measure above. Figure 1 in the online Appendix shows again that the entire distribution is shifted to the left under increased audit risk. Panel C in Table 4 presents impact estimates on the share of audited resources involved in corruption using the coding form Ferraz and Finan (2011). Point

estimates are somewhat smaller and statistical significance is a bit reduced compared to the corruption codings in panels A and B. Figure 2 in the online Appendix shows that the entire distribution is again shifted to the left with higher audit risk. Overall, we conclude that the reduction in the share of audited resources involved in corruption is invariant to alternative samples, specifications, and corruption codings.

Monetizing the marginal benefit of the intervention in terms of cost savings for the taxpayer is difficult because it is unlikely that the entire amount involved in corruption is actually wasted or stolen. We nonetheless provide a rough cost-benefit analysis based on a conservative assumption about the actual cost saving. Since the average amount audited was about 12 million Reais and assuming an effect size of about -10 percentage points, the reduction of the amount involved in corruption amounts to about 1.2 million Reais or roughly 0.5 million US$. 120 municipalities were exposed to higher audit risk so the potential cost saving amounts to about US$ 60 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. In order to increase audit risk by 20 percentage points for the 120 treatment group municipalities, 24 extra audits were necessary, each costing about US$ 50,000. The marginal cost of the policy therefore amounts to about US$ 1.2 million, yielding a net benefit of US$ 4.8 million.

7.2   Impact on the distribution of goods and services purchased

Table 3 in the online Appendix 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 3 in the online Appendix also shows that there are no important differences in terms of goods and services bought between treatment and control municipalities from the 32nd lottery, suggesting that the treatment did not affect what was being bought. Nor are there marked differences between control municipalities from the 31st and 32nd lotteries, with the exception of public works, which represent

a larger proportion of processes in the 31st lottery.[14] 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 discussed below.

7.3 Impacts on the number of procurement processes, transfers and audited amounts

Table 4 in the online Appendix shows impact estimates on the municipality-level aggregate number of restricted and unrestricted procurement processes. Panel A shows that higher audit risk reduced the number of restricted procurement processes from a level of about 10 in the control group by about 4 processes. Panel B shows that the number of unrestricted processes was unaffected at a level of about 4. While the total number of processes is therefore 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 online Appendix, Tables 5 and 6, respectively. These results suggest therefore that treatment group municipalities were making fewer and larger purchases.

7.4 Impact on the distribution of procurement modalities

Table 5 presents the distribution of procurement modalities by level of audit risk and lottery. The unit of observation is again an individual procurement process. A noteworthy result 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. 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 auction, is to fractionalize the purchase (break it up into pieces)

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[14]From a purely statistical perspective, the three distributions are different according to Pearson's chi-square test.

and conduct a series of restricted procurement processes, such as "bids only by invitation".

7.5 Impacts on the distribution of audit findings

Table 7 in the online Appendix presents the distribution of audit findings by level of audit risk 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 all the 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 distribution of audit findings is remarkably similar across control municipalities, irrespective of whether they were sampled in the 31st or 32nd lottery. This suggests that pooling across lotteries is appropriate, despite the fact that control municipalities from 31st lottery were doing more public works than control municipalities from the 32nd lottery. Third, 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.

7.6 Impact on the proportion of procurement processes involved in corruption

Table 6 panel A presents impact estimates on the proportion of procurement processes with evidence of narrow corruption (BNPT 2013). The estimates fluctuate around the -0.15 mark and are all statistically significant. Although the estimates are quite variable, the confidence intervals show substantial overlap. For example, the 95 percent confidence interval for the true impact ranges from -0.29 to -0.01 in the first column and from -0.23 to -0.05 in the last column of panel A. Figure 3 shows that higher audit risk shifted the entire distribution of corruption to the left.

Table 6 panel B shows that the corruption reduction was exclusively driven by restricted processes. Impact estimates among restricted processes are around -0.20, down from an incidence of 0.39 in the control group. Panel C shows that the incidence of corruption in the control group is lower

in unrestricted processes compared to restricted process (0.39 vs. 0.19) and that increased audit risk had a small and statistically insignificant effect on the incidence of corruption among unrestricted modalities. A plausible interpretation of these findings is that in the control group, 4 out of 10 restricted processes were involved in corruption on average and that higher audit risk pushed procurement officials to run 4 fewer restricted processes, 3 of which would have involved corruption in the absence of increased monitoring, leaving only 1 out of 6, or roughly 17% of restricted processes corrupt under increased monitoring. Overall, these results suggest that the higher discretion afforded by restricted modalities is in practice often abused to strike corrupt deals with favored suppliers.

Table 8 in the online Appendix presents impact estimates on the proportion of procurement processes with evidence of broad corruption (BNPT 2013). Point estimates and significance are similar to the narrow corruption measure above. Figure 3 in the online Appendix shows that the entire distribution of the broad corruption measure is shifted to the left under increased audit risk. Table 9 in the online Appendix 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 but the results are qualitatively unchanged. Figure 4 in the online Appendix shows that the entire distribution is again shifted to the left with higher audit risk.

7.7 Impacts on health service delivery

Table 7 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 7 shows no evidence that increased audit risk affected the quality of health care services provided by local governments. Out of the eleven outcomes considered here, none are statistically different on average between treatment and control groups and the impact estimates are generally small. For example, the share of respondents who say they receive regular visits from community health workers - as required under the preventive health program - is essentially 93 percent in both treatment and control groups. This proportion is precisely estimated, with a 95% confidence interval ranging from about 90 percent to 96 percent, suggesting that community health workers are es-

sentially doing their job in Brazil. The same is not true for more specialized medical personnel, however. For example, the proportion of respondents being attended by a doctor or dentist when needed is only about 75 percent with a confidence interval ranging from about 67 percent to 83 percent. Similarly, the proportion reporting that the public health post is open exactly as required is only 46 or 37 percent, depending on whether control municipalities from the 31st lottery are included or not. Overall, there is thus no evidence that increased audit risk affected the quality of preventive and primary health care services provided under the *Saúde da Família* program, yet quality was reported to be substandard in at least some dimensions.

7.8 Impacts on compliance with *Bolsa Família* regulations

Table 8 shows that higher audit risk did not seem to affect local compliance with national regulations of the conditional cash transfer program *Bolsa Família*. The first two outcomes show that the proportion of families receiving a level of cash transfers that is appropriate given the age and number of children and income is negligibly (and statistically insignificantly) different between treatment and control municipalities. For example, the share of recipient families that get the appropriate cash transfer given the number and age composition of the children is essentially 95 percent in both treatment and control groups with a 95% confidence interval ranging from 93% to 97%. The last three outcomes show that compliance with health and education conditionalities is generally high and no different between treatment and control groups respondents. For example, compliance with vaccination requirements is almost perfect, while school enrollment is about 80 percent. Overall, the high compliance rates in Table 8 suggest that the vast majority of *Bolsa Família* recipients were appropriately included in the program, received the correct level of cash given the age number of children, and fulfilled the health and education conditionalities to a large extent.

8 Conclusion

This study provides new theory and evidence on audit risk and rent extraction in local public procurement as well as in service delivery. We adapt Allingham and Sandmo's (1972) classic model of tax evasion to the case of rent extraction to show that an increase in audit risk does not

necessarily reduce rent-taking in environments where sanctions and the probability that they are applied are too low. In line with our model, we find substantial heterogeneity in responses to higher audit risk depending on the parameters of the rent-extraction problem for public or private agents.

For public officials who run procurement, we find clear evidence of a reduction in rent-taking in response to higher audit risk. The corruption reduction is entirely driven by procurement modalities that restrict competition and afford discretion to procurement officials in their choice of suppliers. In light of our model, the reduced rent-taking in procurement arises because audit findings constitute hard evidence and because mayors and procurement officials are subject to a variety of administrative, judicial, and electoral sanctions in the event that an audit reveals important irregularities. 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.

For public service providers in the preventive and primary health care *Saúde da Família* program in contrast, we find no evidence that increased audit risk affected the extent of shirking on the job. Yet service quality was reported to be substandard in at least some dimensions, with respondents reporting not being attended by a doctor or dentist when needed, or finding the public health post closed during stipulated opening hours. Our model shows that both the non-response to higher audit risk and the evidence of shirking under normal monitoring conditions may arise under a relatively low probability of being sanctioned for irregularities conditional on detection through a standard audit. Irregularities in procurement on the other hand constitute hard evidence and are therefore more likely to lead to sanctions.

We also find no evidence that higher audit risk had an effect on inclusion errors and overpayments to beneficiaries of the *Bolsa Família* cash transfer program or their compliance with health and education conditionalities. In contrast to health service delivery however, households and local administrators were already compliant with *Bolsa Família* requirements to a large extent even in the absence of increased scrutiny. Our model suggests that both the non-response to higher audit risk and the high compliance with eligibility requirements and conditionalities under normal monitoring conditions can be explained with a relatively high probability of getting administrative or social sanctions for defrauding the program even in the absence of an audit.

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Table 1: Sampling probabilities in the 32nd lottery and annual audit risk

State	N	Draws	P(Audit)	N	Draws	P(Draw)	P(Audit)	Ex post dP	Ex ante dP
		Treatment Group			**Control Group**				
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 N° 1581 from August 11 2009 for the 29th lottery, and Portaria N° 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 N° 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: $P(Audit) = 1 - [1 - P(Draw\ 29^{th})]^3 \times [1 - P(Draw\ 32^{nd})]$. dP gives the ex ante and ex post difference in audit probabilities between treatment and control groups by state.

Table 2: Auditor classification of irregularities and corruption codings

CGU classification of irregularities	%	LZ	FF	BNPT
			Corruption codings	
- 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	C
- invitation for bids to less than three firms	1.27	M	M	C
- procurement modality too restricted	8.52	M		M
- participating ineligible firm	0.24	M		M
- non-selection of the lowest bid	0.48	M		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 3: Difference in means for pre-treatment covariates

Audit risk	High	Low	High	Low	
Audited in 31st or 32nd lottery	No	No	Yes	Yes	
Population	25,460	23,610	21,512	18,653	2,858
	[48,043]	[45,554]	[37,366]	[24,482]	(7,294)
Income per capita	151.3	169.8	162.5	157.0	5.5
	[88.3]	[94.5]	[85.8]	[80.8]	(17.8)
Average years of schooling	3.90	4.02	3.86	3.89	-0.03
	[1.36]	[1.26]	[1.35]	[1.13]	(0.27)
Urbanization	0.59	0.59	0.57	0.59	-0.02
	[0.23]	[0.23]	[0.24]	[0.21]	(0.05)
Poverty headcount ratio	0.28	0.25	0.26	0.26	0.00
	[0.18]	[0.18]	[0.20]	[0.18]	(0.04)
Poverty gap	0.51	0.49	0.52	0.49	0.03
	[0.09]	[0.11]	[0.12]	[0.10]	(0.02)
Gini coefficient	0.56	0.56	0.56	0.56	0.00
	[0.05]	[0.06]	[0.07]	[0.06]	(0.01)
Radio station	0.46	0.45	0.46	0.45	0.01
	[0.50]	[0.50]	[0.51]	[0.50]	(0.05)
PMDB	0.19	0.21	0.20	0.25	-0.05
	[0.39]	[0.41]	[0.41]	[0.4]	(0.09)
PSDB	0.12	0.14	0.13	0.17	-0.04
	[0.33]	[0.35]	[0.35]	[0.38]	(0.07)
PTB	0.09	0.07	0.03	0.1	-0.07
	[0.29]	[0.26]	[0.18]	[0.30]	(0.05)
PT	0.08	0.1	0.10	0.09	0.01
	[0.27]	[0.30]	[0.31]	[0.29]	(0.06)
PSB	0.09	0.05	0.10	0.08	0.02
	[0.29]	[0.23]	[0.31]	[0.27]	(0.06)
PR	0.12	0.07	0.10	0.08	0.02
	[0.33]	[0.25]	[0.31]	[0.27]	(0.06)
PP	0.06	0.1	0.16	0.03	0.13
	[0.23]	[0.30]	[0.38]	[0.18]	(0.07)
PDT	0.09	0.06	0.07	0.02	0.05
	[0.29]	[0.24]	[0.25]	[0.15]	(0.05)
N	90	5318	30	90	

Notes: The first four columns give sample means and in brackets standard deviations. The last column gives the difference in means between the third and fourth column and in parentheses the corresponding standard error. 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. The F-statistic for the joint hypotheses of zero differences in all rows of column five is 1.02 with p-value 0.44.

Table 4: Impact on the share of audited resources involved in corruption

Panel A, narrow corruption (BNPT 2013)

Dependent variable: share of audited resources involved in narrow corruption; 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)
R-squared	0.060	0.698	0.370	0.345	0.252	0.061	0.566	0.629	0.727	0.750

Panel B, broad corruption (BNPT 2013)

Dependent variable: share of audited resources involved in broad corruption; 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)
R-squared	0.072	0.695	0.389	0.335	0.263	0.074	0.562	0.634	0.732	0.761

Panel C, corruption (FF 2011)

Dependent variable: share of audited resources involved in corruption; 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)
R-squared	0.037	0.672	0.245	0.348	0.194	0.044	0.462	0.551	0.657	0.696
Municipalities	60	60	60	60	60	120	120	120	120	120
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party dummies	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

Notes: OLS estimations. See Table 2 for details on corruption codings. 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 5: Distribution of procurement modalities by level of audit risk and lottery

Procurement modality	32nd lottery				31st lottery	
	High audit risk		Low audit risk		Low audit risk	
	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
Unrestricted modalities	142	40.23	155	31.96	210	26.71
Total	353	100.00	485	100.00	817	100.00

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

Table 6: Impact on the likelihood of narrow corruption (BNPT 2013) at the procurement process level

Panel A, restricted and unrestricted processes

Dependent variable (0/1): procurement process with evidence of narrow corruption (BNPT 2013); control group mean 0.33, std. 0.47

Treatment (0/1)	-0.151**	-0.105*	-0.157**	-0.134*	-0.190***	-0.171***	-0.140***	-0.126**	-0.122**	-0.141***
	(0.071)	(0.048)	(0.064)	(0.067)	(0.053)	(0.046)	(0.041)	(0.046)	(0.049)	(0.047)
R-squared	0.097	0.661	0.283	0.290	0.362	0.080	0.518	0.590	0.615	0.641
Procurement processes	838	838	838	838	838	1,655	1,655	1,655	1,655	1,655

Panel B, only restricted processes

Dependent variable (0/1): procurement process with evidence of narrow corruption (BNPT 2013); control group mean 0.39, std. 0.49

Treatment (0/1)	-0.194***	-0.137***	-0.196***	-0.173**	-0.229***	-0.214***	-0.180***	-0.170***	-0.154***	-0.149***
	(0.071)	(0.049)	(0.068)	(0.071)	(0.055)	(0.053)	(0.046)	(0.053)	(0.056)	(0.053)
R-squared	0.043	0.445	0.361	0.093	0.117	0.030	0.450	0.463	0.189	0.478
Procurement processes	541	541	541	541	541	1,148	1,148	1,148	1,148	1,148

Panel C, only unrestricted processes

Dependent variable (0/1): procurement process with evidence of narrow corruption (BNPT 2013); control group mean 0.19, std. 0.40

Treatment (0/1)	-0.052	0.016	-0.036	0.003	0.048	-0.047	-0.026	-0.002	-0.034	-0.021
	(0.122)	(0.055)	(0.123)	(0.075)	(0.069)	(0.068)	(0.052)	(0.060)	(0.056)	(0.045)
R-squared	0.005	0.399	0.234	0.163	0.198	0.003	0.331	0.360	0.256	0.437
Procurement processes	297	297	297	297	297	507	507	507	507	507
Municipalities	60	60	60	60	60	120	120	120	120	120
State intercepts	N	Y	N	N	N	N	Y	Y	Y	Y
Mayor's party dummies	N	N	Y	N	N	N	N	N	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

Notes: OLS estimations at the level of the procurement process. See Table 2 for details on corruption codings. 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. Unrestricted modalities are the sealed-bid (reverse) auction, on-site (reverse) auction, and electronic (reverse) auction. 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. Clustered standard errors are in parentheses. *, **, and *** indicate significance at 10 percent, 5 percent and 1 percent levels respectively.

Table 7: Impacts on user satisfaction with the family health program

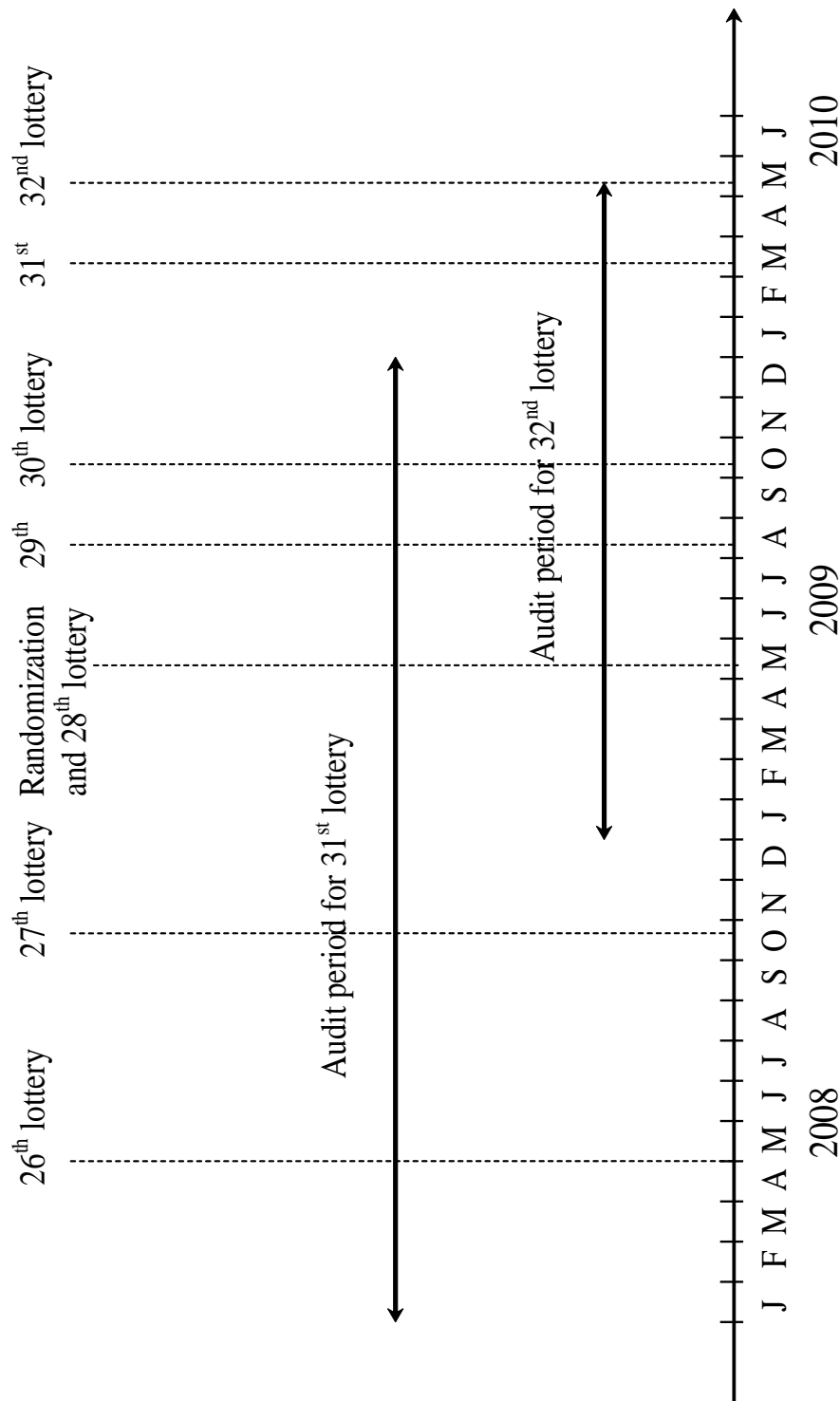|	32nd lottery		31st and 32nd lottery	
	Control mean	Difference	Control mean	Difference
Proportion of adequately staffed teams of community health workers	0.821*** (0.075)	-0.097 (0.114)	0.867*** (0.038)	-0.143 (0.092)
Proportion of respondents that receive visits from community health workers	0.929*** (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		0.47		0.41
(p-value)		(0.91)		(0.84)

Notes: WLS estimations with weights equal to the number of survey respondents. N varies by outcome. The 'Control mean' column gives the sample average in the low audit risk group. The 'Difference' column gives the difference in means between high and low audit risk groups. Robust standard errors in parentheses. F-statistics are for the joint hypotheses that all differences in outcomes are zero.

Table 8: Impacts on eligibility and conditionality compliance

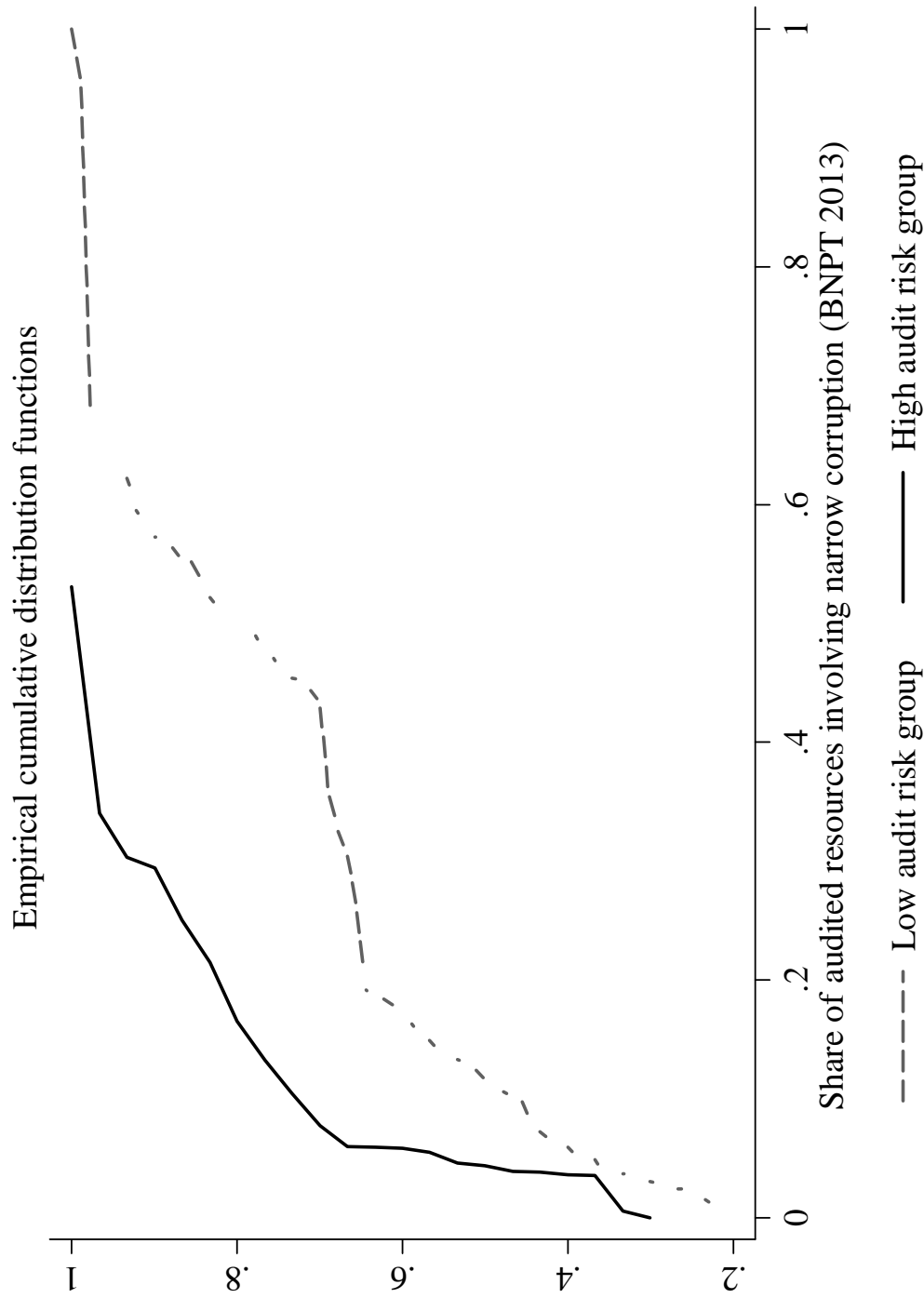|	32nd lottery		31st and 32nd lottery	
	Control mean	Difference	Control mean	Difference
Bolsa Família recipient family with appropriate benefit given number and age of children	0.956*** (0.014)	-0.031 (0.026)	0.953*** (0.010)	-0.028 (0.023)
Bolsa Família recipient family with appropriate benefit given family income	0.856*** (0.024)	-0.009 (0.039)	0.853*** (0.015)	-0.007 (0.033)
Bolsa Família recipient family compliant with required regular vaccinations	0.986*** (0.009)	0.005 (0.012)	0.988*** (0.004)	0.003 (0.009)
Bolsa Família recipient adolescent enrolled at school	0.782*** (0.033)	0.018 (0.052)	0.828*** (0.016)	-0.028 (0.042)
Bolsa Família recipient and enrolled adolescent attending school frequently	0.947*** (0.019)	0.007 (0.022)	0.909*** (0.012)	0.044*** (0.016)
F-statistic		0.41		2.29
(p-value)		(0.84)		(0.05)

Notes: WLS estimations with weights equal to the number of survey respondents. N varies by outcome. The 'Control mean' column gives the sample average in the low audit risk group. The 'Difference' column gives the difference in means between high and low audit risk groups. Robust standard errors in parentheses. F-statistics are for the joint hypotheses that all differences in outcomes are zero.
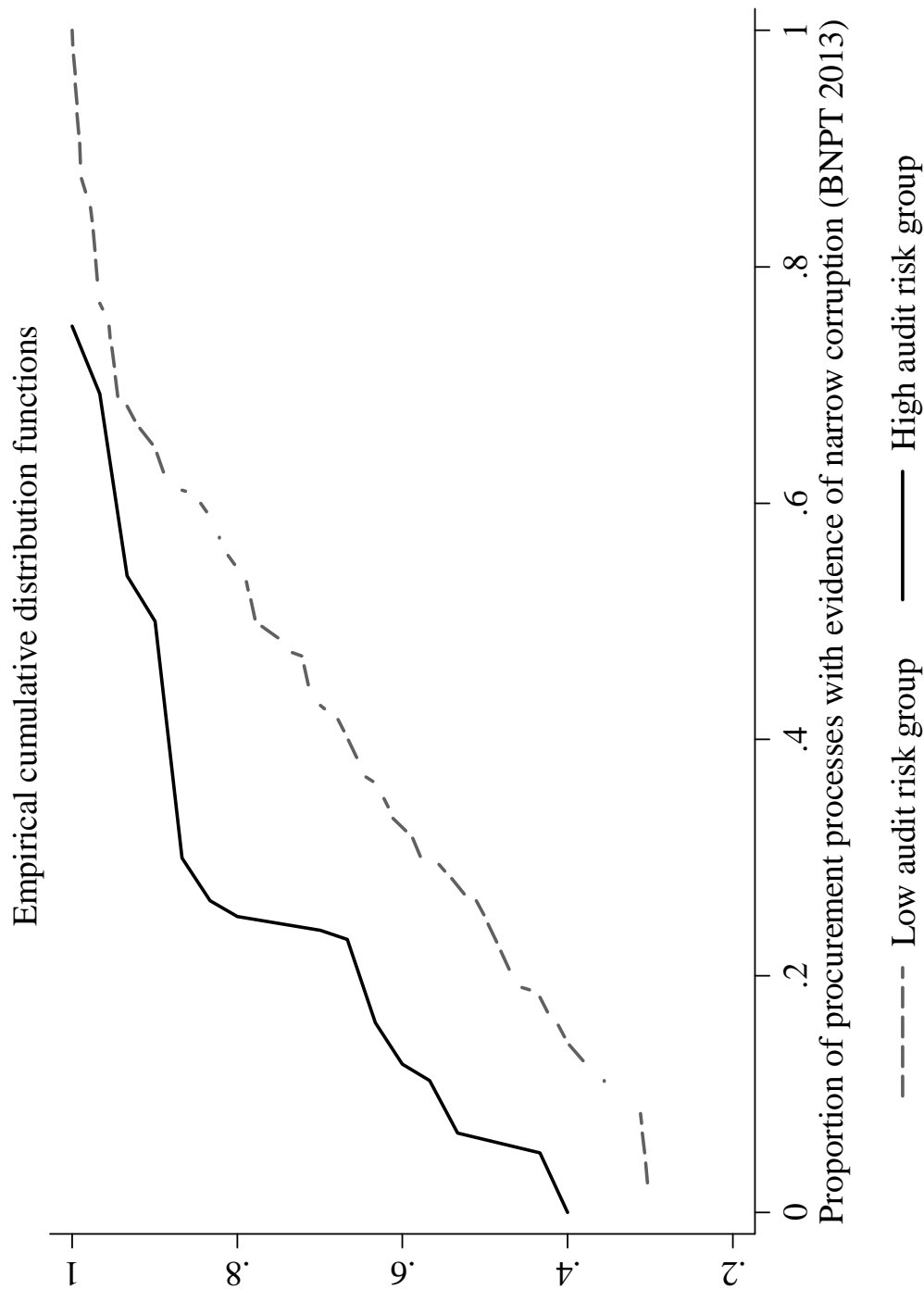
Figure 1: Timeline

26th lottery

27th lottery   Randomization and 28th lottery   29th   30th lottery   31st   32nd lottery

Audit period for 31st lottery

Audit period for 32nd lottery

J F M A M J J A S O N D J F M A M J J A S O N D J F M A M J

2008   2009   2010

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 narrow corruption (BNPT 2013), share of audited amount

Empirical cumulative distribution functions

Share of audited resources involving narrow corruption (BNPT 2013)

- - - Low audit risk group ——— High audit risk group

Notes: Narrow 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 2 for details.

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

Empirical cumulative distribution functions

Proportion of procurement processes with evidence of narrow corruption (BNPT 2013)

- - - - Low audit risk group   ——— High audit risk group

Notes: Narrow 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 2 for details.