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Evidence from a Randomized Evaluation  
in Brazil**

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**This version: March 2018  
(April 2011)**

*Barcelona GSE Working Paper Series*

*Working Paper n° 554*

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

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March 2018

## Abstract

We report results from a randomized policy experiment designed to test whether increased audit risk deters rent extraction in three areas of local government activity in Brazil: procurement, health service delivery and cash transfer targeting. Our estimates suggest that temporarily increasing annual audit risk by about 20 percentage points reduced the share of audited resources involved in corruption in procurement by about 10 percentage points and the proportion of procurement processes with evidence of corruption by about 15 percentage points. In contrast, we find no evidence that increased audit risk affected the quality of publicly provided preventive and primary health care services - measured through user satisfaction surveys - or compliance with eligibility requirements for the conditional cash transfer program - measured through household inspections. The observed impact heterogeneity across activities is consistent with differences in potential sanctions and in the probability that a sanction is applied, conditional on detection.

Keywords: government audit, corruption, procurement, cash transfer program, health service delivery

JEL: D73, D78, H41, H83, K42

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\*We are grateful for comments from Emmanuelle Auriol, Martina Björkman, Antonio Ciccone, Denis Cogneau, Gabrielle Fack, Patricia Funk, Scott Desposato, Miguel de Figueiredo, Albrecht Glitz, Jorge Hage, James Hines, Yinghua He, Maksym Ivanyna, Yuya Kudo, George Musser Jr., Sylvie Lambert, Gianmarco León, Karthik Muralidharan, Hannes Müller, Luiz Navarro, Rosella Nicolini, Per Pettersson-Lidbom, Giacomo Ponzetto and Anh Tran. We also received helpful comments from seminar participants at GRIPS Tokyo, University of Michigan, the Fiscal Federalism Workshop at IEB, the Political Economy Workshop at Erasmus University in Rotterdam, NEUDC Yale, Universitat Pompeu Fabra, Universitat Autònoma de Barcelona, the Barcelona Development Economics Workshop, University of Namur, SAEe Vigo, Paris School of Economics, Toulouse School of Economics, SEA Lucerne and the ASSA meetings in San Diego. Bruno Sousa provided excellent research assistance. Litschig acknowledges financial support from the Spanish Ministry of Economy and Competitiveness, through the Severo Ochoa Programme for Centres of Excellence in R&D (SEV-2011-0075). The views expressed in this paper are those of the authors and not necessarily those of the Controladoria-Geral da União. All errors are our own.

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# 1 Introduction

Governments around the world use external audits to monitor whether public officials extract rents through shirking on the job or outright embezzlement of public funds. Available evidence on whether such top-down monitoring works is both scant and mixed. While the threat of an audit reduced corruption in local road construction projects in Indonesia (Olken 2007), it had no measurable impact on health worker performance in Ghana (Dizon-Ross et al. 2017). Other top-down monitoring approaches focusing on technological solutions to combat absenteeism in health service delivery have shown similarly mixed results (Banerjee et al. 2008, Dhaliwal and Hanna 2017). Interpretation of these mixed findings is complicated in part because each study evaluates a different intervention in a different context. For example, the government audits in Indonesia might have been effective because prior experience showed that these audits indeed uncovered corruption, while the audits in Ghana were carried out by a nongovernmental organization without established track record. And even identical interventions might have produced different results due to differences in the normative and institutional setting. Yet it is crucial for policymakers to know under what circumstances top-down monitoring can be effective.

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 three key areas of local government activity: procurement, health service delivery and cash transfer targeting. Annual audit risk increased from the same baseline level of about 5 percent to about 25 percent across the entire range of local government activities. It follows by design that impact heterogeneity across activities in this setting cannot be driven by differences in the intervention itself or by differences in the broader socioeconomic or institutional context. We argue that one should nevertheless expect substantial heterogeneity in response to the same increase in audit risk depending on the parameters of the rent-extraction problem faced by public and private agents involved in these activities. Intuitively, 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. Increased audit risk might also not be effective if potential sanctions and their likelihood of materializing are too high even in the absence of increased top-down scrutiny.

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.<sup>1</sup> 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 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, health service delivery and cash transfer targeting as determined by CGU auditors, judged against a uniform national standard. Irregularities in procurement or health service delivery provide an objective measure of rent extraction by local government 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.<sup>2</sup> For example, procurement regulations are designed to ensure that the public pays the lowest price available for a given good or service required, yet

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<sup>1</sup>Municipalities are the third tier of government in Brazil (below the federal and state governments).

<sup>2</sup>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.

implementing a competitive procurement modality is privately costly for the local manager.<sup>3</sup> 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 household inspections.

Our data on public procurement, health service delivery and cash transfer program 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, cover most purchases made with federal transfers, and 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. The health service delivery and cash transfer program data are based on locally representative household surveys and inspections that are 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, health service delivery and cash transfer microdata, we also analyze mismanagement and corruption episodes from the published audit reports as in prior work.

Previous papers adopt their own definitions of corruption and we explore the sensitivity of our results to existing alternative coding choices. Our preferred measure of rent-taking in procurement 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 attractive 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 mayors with the lesser offense of "acts of administrative misconduct". Similarly, public officials also face jail time and fines if they are convicted of negligence or fraud in public procurement. As additional measures we use the "broad" definition of corruption introduced by Brollo et al. (2013),

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<sup>3</sup>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.

as well as the more stringent corruption coding in Ferraz and Finan (2011).

Our 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 in procurement 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 mostly 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. The main caveat is that we cannot rule out substitution of corruption across federal vs. non-federal transfers or over time since this would require audits data on non-federal transfers as well as from post-scrutiny time periods.

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 many dimensions, as substantial numbers of respondents reported not being attended by a doctor or dentist when needed, or finding the public health post closed during stipulated opening hours. We argue that there are two simple rationales for the differential response to audit risk in procurement vs. health service delivery, the first being a marked difference in punishments. For service delivery irregularities, sanctions include at most the loss of the job. For public officials in charge of procurement in contrast, potential sanctions are relatively high as they include not only job termination but also fines as well as jail time.

Second, the differential impacts in procurement vs. health service delivery together with evidence of shirking under normal monitoring conditions are also consistent with 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 are not in a position to verify 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. Although we lack data on follow-up investigations and sanctions of municipal government personnel, Avis et al. (2017) show that the likelihood of a mayor being prosecuted and convicted in response to a CGU audit increases with the number of corruption irregularities (including, but not limited to procurement) but is unaffected by the number of mismanagement irregularities (again including, but not limited to health service delivery).

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 even in the absence of extra scrutiny. We argue that both the non-response to higher audit risk and the low level of cash transfer program fraud under normal monitoring conditions may be due a relatively high probability of being sanctioned for irregularities even in the absence of an audit. 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 results suggest that sending auditors for household inspections in order to assess compliance with cash transfer program requirements is not very useful 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. Finally, Bobonis et al. (2016) consider pre-established and foreseeable audits in Puerto Rico and compare corruption in municipalities that are audited in the period leading up to an election to municipalities that are audited shortly after elections. They find that "timely" audits right before elections reduce corruption by about 67 percent in the short-run, while in subsequent audits corruption is the same across "timely" and "untimely" audited municipalities.<sup>4</sup>

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 (2017) study a technological solution that reduced the cost of monitoring attendance of health care workers. They 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 that these studies find at least temporary 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 transfer program and the zero effect of higher audit risk we document for Brazil are in line with recent evidence on in-kind transfers from sub-Saharan Africa. Dizon-Ross, Dupas, and Robinson (2017) 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 audited

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<sup>4</sup>More tangentially related is the recent work by Avis et al. (2017), 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.

and the program potentially shut down in case of irregularities. They find 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. A plausible interpretation of these findings is that the probability of being sanctioned for irregularities even in the absence of an audit is relatively high because inclusion errors are easily observed by other members of the community.

In addition to testing whether in a given economic and institutional context higher audit risk deters rent extraction in three key areas of local government activity, our study makes two further contributions. First, in contrast to Olken's study on the execution of road construction projects, our study also includes the bidding and awarding stages that are typical of public procurement and it covers a wide range of locally provided public services. Second, 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. Our findings thus suggest that there is a trade-off between rules and discretion in public procurement in Brazil. In contrast, two studies from Italy find that public bodies with more autonomous managers do not exhibit higher levels of active waste (Bandiera et al. 2009) and that discretion does not deteriorate (and may improve) some procurement outcomes (Coviello et al. 2017).

We proceed as follows. Section 2 describes the audits program and gives institutional background on administrative, judicial and electoral punishments that may arise from the detection of irregularities. Section 3 presents a simple conceptual framework to analyze under what conditions an increase in audit risk reduces rent-taking. We discuss the experimental design in Section 3. Section 4 presents the non-public data on irregularities in local public procurement, health service delivery and cash transfer management, as well as the data from published audit reports. In Section 5 we describe our estimation approach. We present results in Section 6. Section 7 discusses alternative interpretations, including awareness of treatment status, measurement error and corruption substitution. The conclusion presents a rough cost-benefit analysis and evaluates the external validity of our results.

## 2 Audits program and institutional background

The random audits program was initiated under the government of Luiz Inácio Lula da Silva in March 2003 with the explicit objective of fighting corruption and waste in local public spending. Most municipalities were eligible for federal audit from the start of the program with the exception of state capitals.<sup>5</sup> Several rounds of sampling occur each year through a public lottery. The machinery used for the selection of municipalities is the same as that used for a popular national (money) lottery and results are broadcast on television and through other media. 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. Expenditures eligible for audit include those made with federal transfers 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 purchases made with revenue-sharing transfers from the federal or state governments, as well as purchases financed through municipality own revenues. Inspections typically occur for all eligible federal transfers made during the preceding two to three years and amount to about one-third of total municipal revenue on average.

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

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

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. In procurement, auditors analyze procurement documentation and conduct field work to determine whether public contracts were awarded competitively. Responsibility lies primarily with members of the local procurement commission who are typically appointed by the mayor. Auditors also routinely assess the quality of public health services using short surveys in a locally representative sample of households. The main objective of these surveys is to determine whether locally appointed community health workers and medical staff are doing their jobs. Compliance with the cash transfer program eligibility requirements and conditionalities is assessed through household visits and inspection of health and school records, respectively. The main goal is to detect cash transfer program fraud. Responsibility lies with recipients themselves and potentially also with local officials who select beneficiaries and manage the program. As the head of the local government executive, the mayor is ultimately responsible for irregularities committed under his watch.

At the end of their field work, auditors report the results of their inspections back to CGU headquarters. Auditors also write a report, detailing the irregularities encountered during their mission. Municipality mayors are given the possibility to comment on the draft report within five business days. Auditors in turn explain whether or not they accept the mayor's justification of problems found. 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, mayors may be imprisoned for 1 to 8 years, in addition to losing their mandate and incurring fines. If convicted of "acts of administrative misconduct" or "improbability", punishments include the loss of mandate, the suspension of political rights for 8 to 10 years, prohibition from entering into public contracts for 10 years as well as the obligation

to reimburse public coffers.<sup>6</sup> Similarly, procurement officials may lose their job, pay a fine and go to jail if they are convicted of negligence or fraud in public procurement as further discussed in Section 5.<sup>7</sup> For public service providers in contrast, the law only contemplates job termination in case of absenteeism or shirking, not fines or jail time. Similarly, defrauding the cash transfer program only results in loss of the benefit and perhaps some administrative sanction if a public official was involved.

In addition to potential judicial sanctions, mayors also face electoral and other 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 (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).

### **3 Conceptual framework**

This section presents a conceptual framework in the spirit of Becker (1968) to analyze under what conditions an increase in audit risk reduces rent-taking. The framework is tailored to our setting in which audit risk increased for all agents in treatment municipalities, including the mayor, members of the procurement commission, health service providers, as well as *Bolsa Família* beneficiaries and program managers. What varies is the nature of rent-taking across activities, as well as 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. Please see our online appendix for a formal model adapted from Allingham and Sandmo's (1972) analysis of tax evasion.

Rents in procurement consist of kickbacks for steering the contract to a particular supplier or insufficient effort to identify the cheapest available supplier. In service delivery by doctors and medical personnel, rents take the form of shirking on the job, mainly through absenteeism. For

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<sup>6</sup>See Arantes (2004, 2007) on the organization and legal instruments at the disposal of the Brazilian *Ministerio Público*.

<sup>7</sup>Procurement Law 8,666, Art. 89-98.

cash transfer recipients, rents consist of extra payments received by over-reporting household size or under-reporting income. The upper bound of rent-taking varies across activities 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. Expected utility depends on two key parameters: the magnitude of sanctions if caught and the probability that an infraction is detected and sanctions are applied. Sanctions could be administrative, judicial or electoral, or a combination of these, depending on the activity. For example, the mayor and his team likely care about all types of sanctions while procurement officials worry mostly about jail time, fines and loss of the job and perhaps care little if program funds are cut (as long as their salaries are not touched). Service providers can at worst lose their jobs, while for welfare programs the most severe sanction is loss of the benefit and perhaps some form of social sanction.

Our experiment did not vary the probability of sanction directly but it varied the probability of a central government audit and hence the probability that an infraction gets detected. In some activities the chance of being sanctioned even in the absence of an audit might already be high and one would therefore expect increased audit risk to be ineffective. For example, the number of children or the level of income are relatively easy to observe for program managers or the public at large and so we would expect little rent extraction and a limited effect of increased audit risk in cash transfer management. In contrast, detecting irregularities in procurement or service delivery is difficult without an audit because public officials can hide their actions more easily than cash transfer recipients.

Moreover, 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. For example, the sanction probability conditional on detection is likely low in service delivery because shirking in service provision is difficult to prove based only on an audit. In procurement in contrast, the audit generates information that can be used in court because procurement officials are required to document each step of the procurement process. This logic therefore suggests that rent-taking in procurement should be more responsive to increased audit risk than shirking in service delivery. Similarly, increased audit risk might reduce rent extraction more in procurement compared to service delivery because

the sanction if caught is higher for members of the procurement commission. In addition to losing their jobs, procurement officers may have to pay a fine or end up in jail for mismanaging public funds, while service delivery personnel only have their jobs on the line.

To sum up, increased audit risk should not curb rent-taking in activities characterized by a high probability of detection and sanction even in the absence of an audit, such as cash transfer program fraud. Moreover, increased audit risk may deter rent extraction only to the extent that the audit generates information that can be used in court, as is typically the case in procurement but not in service delivery. Finally, increased audit risk is more likely to be effective the larger is the sanction conditional on detection and sanctions are typically more severe for procurement fraud than for service provider absenteeism.

## **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 three areas of local government activity: procurement, health service delivery and cash transfer targeting. 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.<sup>8</sup> 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

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<sup>8</sup>Portaria N° 930, May 8 2009.

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.

While the initially announced (ex ante) probability of an audit for treatment group municipalities was  $30/120 = 25$  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 29<sup>th</sup>, 30<sup>th</sup>, 31<sup>st</sup> and 32<sup>nd</sup>, as illustrated in Figure 1. Table 2 in the online appendix presents audit probabilities that municipalities from different states faced in the 29<sup>th</sup> lottery. For most states, audit probabilities per round of the lottery -  $P(\text{Draw})$  - 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 32<sup>nd</sup> 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.<sup>9</sup> 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 32<sup>nd</sup> lottery, the relevant annual audit risk for treatment group municipalities that could have affected behavior likely was 25 percent.

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

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

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<sup>9</sup>Portaria N<sup>o</sup> 862, April 30 2010.

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).

## **5 Data**

This section presents our microdata on irregularities in local public procurement, public service delivery and cash transfer program targeting 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 government 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 and were provided to the auditors by local governments. For the 75 percent 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 and the selection of sectors was determined at the time of the lottery. Within a sector, the procure-

ment data cover all purchases made with eligible federal funds during the main audit period, from January 2009 to May 2010 for the 32<sup>nd</sup> lottery and from January 2008 to December 2009 for the 31<sup>st</sup> lottery as illustrated in Figure 1.<sup>10</sup> We drop procurement data from 2008 in order to focus measurement on the period of increased audit risk. But 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. These purchases could not have been affected by higher audit risk by construction and their inclusion 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.

A key advantage of our procurement process-level data is that we can examine which procurement modalities are more prone to corruption. 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 restrictions on the number of competitors.<sup>11</sup> 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 eletrônico*).

## 5.2 Coding corruption in procurement

Table 2 presents CGU auditors' classification of irregularities in procurement, as well as corruption codings by Ferraz and Finan (FF, 2011), ourselves in prior work (LZ, 2012), and Brollo, Nannicini, Perotti, and Tabellini (BNPT, 2013). Our preferred measure is what BNPT call narrow corruption, which includes four of the most serious irregularities in procurement according to CGU's classification. In fact, all of these irregularities constitute criminal offenses under procurement law 8,666, punishable with jail time up to 6 years and fines, in addition to loss of the job.

<sup>10</sup>In the early years of the program, CGU used a sampling scheme to select only a subset of purchases for audit.

<sup>11</sup>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.

The first instance of narrow corruption arises when auditors detect evidence that the tender process was entirely simulated (Art. 96 § V), such as when the winning firm in fact did not exist. The second and third type of irregularity include more subtle forms of corruption such as steering the public contract to favored firms (Art. 91) or paying more than what the winning bid in the auction had been (Art. 96 § I). The fourth irregularity considered in the narrow corruption measure is when auditors determine that a purchase was fractionalized, i.e. intentionally reduced in value to avoid a more competitive modality (Art. 90). Even if there was no theft involved, this irregularity implies 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.

As a robustness check we also use FF's more stringent corruption measure, which drops fractionalized purchase amounts, as well as BNPT's broad measure, which codes additional CGU irregularities as corruption episodes, such as when the procurement modality is too restricted given the size of the purchase (Art. 89) or when an ineligible firm is participating in the awarding process (Art. 97). Importantly, these irregularities are also punishable by job termination, fines and prison time. In order to keep corruption measures based on audit reports comparable to the procurement-level analysis, we exclude two types of irregularities that are not related to procurement: BNPT's "diversion of funds" (e.g. earmarked transfers for supplies are used for salaries instead) and FF's "disappeared funds" (resources disappear from municipal bank accounts). We give a more detailed description of alternative corruption codings in the online appendix.

### **5.3 Published audit reports**

In addition to the process-level procurement data, we also use the published audit reports as in prior studies. This serves as a robustness check on the procurement-level analysis and also allows us to roughly estimate the amount of federal funds involved in corruption in procurement. Due to data limitations we cannot match the procurement process-level data with the audit report findings however. Our initial 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 findings which we code in the same way as we did for the process-level procurement data. In line

with prior studies we aggregate the inspection order-level data to the municipality level. And as with the procurement-level data, we use transfers that were done in 2009 or 2010 to construct our outcome measures. Although the main audit period for the 32<sup>nd</sup> lottery ran from January 2009 to May 2010, the published reports (but not the procurement process-level data) also have some information on irregularities in 2008 transfers, which we use to construct a pre-treatment outcome measure.

The denominator of our corruption measures is the total amount of transfers audited in 2009-2010 and 2008 respectively. The numerator is the "amount of funds involved" in any of the corruption irregularities discussed above. The exact 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 658 procurement-related irregularities in 2009 or 2010, 503 or 76 percent do not specify an amount. Similarly, out of 41 irregularities related to simulated tender processes in 2009-2010, 30 or 73 percent do not specify an amount. In order to get an "amount of funds involved" in these irregularities, we impute the amount investigated in a given inspection if at least one of the audit findings indicate corruption according to a given definition. For a given municipality, the share of the audited amount involved in corruption in procurement is computed as:

$$\frac{\sum_i \text{amount audited}_i \times I \{ \text{corruption}_i \}}{\sum_i \text{amount audited}_i},$$

where  $i$  denotes an inspection order and  $I \{ \text{corruption}_i \}$  indicates detection of corruption in a given inspection order. The imputation above likely overstates the actual amount of money wasted or stolen.<sup>12</sup> Based on those irregularities where auditors do state an amount affected, we compute that this represents on average about 40 percent of the total amount investigated. Our best estimate of the actual amount wasted or stolen is therefore about 40 percent of the amount involved in corruption. And because the exact amount of leakage 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 percent of the amount involved in corruption is actually wasted or stolen.

<sup>12</sup>Results without imputation yield no statistically significant difference between high and low audit risk groups but the magnitude of the difference is similar in relative terms with and without imputation. Results are available on request.

Columns (1) through (5) in Table 3 give sample means and standard deviations 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. Panel A uses CGU audit reports data on 2008 federal funds that were transferred prior to the experiment. The per capita amount of 2008 transfers audited in the high audit risk group was about 82 Reais on average while in the low audit risk group that was audited in the same lottery that amount was about 95 Reais. The difference is economically small and not statistically different from zero as shown in column (6). The average per capita amount of 2008 transfers audited in the 31<sup>st</sup> lottery was about 336 Reais, which is much higher because 2008 was part of the main audit period in the 31<sup>st</sup> but not in the 32<sup>nd</sup> lottery. Column (6) of panel A also shows difference in means tests for three corruption measures based on codings in Ferraz and Finan (2011) and Brollo et al. (2013), respectively. Focusing again on the 32<sup>nd</sup> lottery, the difference in average share of amount audited involved in corruption in procurement between high and low audit risk groups is statistically insignificant and small, ranging from 2.6 to 6.4 percentage points across the three measures.

#### **5.4 Household survey and household inspections data**

As part of their standard field work, CGU auditors conduct household interviews 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 the two lotteries we consider combined, the auditors interviewed 2,373 families from 112 municipalities 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. If the family reports on an irregularity this gets included in the audit report. Survey responses are not made publicly available and program managers do not know in advance who will be surveyed. We

therefore have little reason to believe that there is systematic under- or over-reporting of performance.

For the conditional cash transfer program *Bolsa Família*, the CGU headquarter provides auditors in the field with a list of typically 30 randomly sampled transfer recipient households per municipality based on a national sampling frame. Local program managers are not informed which households are sampled for inspection. About 75 percent of sampled households - a total of 2,723 - ended up being successfully inspected in the two lotteries combined. Auditors 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 inspections 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 and schooling are from the *Instituto de Pesquisa Economica Aplicada* (IPEA) based on the 2000 census. Mayor characteristics, party affiliation and win margin in the 2008 mayoral elections are from the *Tribunal Superior Eleitoral* (TSE). Finally, we extract the main categories of municipal spending and federal transfers received by the municipality from the *Finanças do Brasil*, a database compiled by the Brazilian national treasury.

Panel B of Table 3 shows conditional means and standard deviations of municipality characteristics that are available for all municipalities, irrespective of whether they were audited in rounds 31 or 32. Aside from a few exceptions, there are no important differences along observable dimensions between the five groups defined by treatment group and audit status. Moreover, out of the 10 differences in means between high and low audit risk groups among municipalities that were

audited in the 32<sup>nd</sup> lottery shown in column (6), only one difference is marginally significant at 5 percent. Similarly, the F-statistic for the joint hypotheses that none of the covariates in panels A or B can predict whether a municipality is in the high audit risk group is 1 with p-value 0.44.

## 6 Estimation approach

Given the randomized experimental design, estimation is a straightforward comparison of sample mean outcomes from treatment and control groups. Let  $Y_{mh}$  denote the outcome variable for procurement process or household  $h$  in municipality  $m$ ,  $\alpha$  the mean outcome in the low audit risk (control) condition,  $\beta$  the (constant) treatment effect,  $D_m$  the high audit risk (treatment) group indicator and  $U_{mh}$  the influence of other unobserved factors that affect the outcome. The data generating process can then be written as:

$$Y_{mh} = \alpha + \beta D_m + U_{mh}. \quad (1)$$

Randomization ensures that in expectation  $D_m$  is uncorrelated with  $U_{mh}$ , so Ordinary Least Squares (OLS) provides an unbiased and consistent estimator of  $\alpha$  and  $\beta$ . For municipality-level outcomes, such as the share of audited resources involved in corruption in procurement, we use OLS. For outcomes at the procurement process- or household-level we also use OLS and cluster standard errors at the municipality level. For the cash transfer program, household- or individual-level school and health record inspection results are not available. We estimate equation (1) using municipality-level averages and weights equal to the number of households visited or student records checked.

For the sake of transparency we present results separately for the sample of municipalities from the 32<sup>nd</sup> lottery and for the pooled sample including the 31<sup>st</sup> lottery. While including the 31<sup>st</sup> lottery typically reduces standard errors by about 30 percent, it is worth emphasizing that it might also lead to bias if outcomes were systematically different from one year to the next. 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 32<sup>nd</sup> lottery and pooled estimation samples.

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

also including the respective pre-treatment corruption outcome measure from Table 3 Panel A, all the pre-treatment municipality characteristics from Panel B, as well as mayor characteristics, such as age, gender and education and dummies for the mayor's party affiliation. Since the number of covariates is large (24 state dummies, 13 party dummies, 10 municipality characteristics and 9 mayor characteristics) we only show adjusted estimates for the pooled sample with 120 municipalities. 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.

## **7 Results**

This section presents evidence on the level of rent extraction under normal monitoring conditions and its response to higher audit risk in three areas of local government activity and for corresponding types of agents. We start with local public procurement, where the main agents are the members of the procurement commission and rent-taking consists of outright fraud or negligence. We then consider health service delivery, where agents are community health workers and medical staff at public health posts and rents mainly take the form of absenteeism. Finally we look at cash transfer recipients and their compliance with program eligibility and conditionality requirements.

### **7.1 Corruption in procurement**

Table 4 presents evidence on the level of corruption in procurement and its response to increased audit risk. Columns (1) through (3) are based solely on the 32<sup>nd</sup> lottery and show the mean of corruption in the low audit risk group, the difference in means between high and low audit risk groups and the number of observations, respectively. Columns (4) through (6) show the same statistics for the pooled sample, including control municipalities from the 31<sup>st</sup> lottery. Column (7) shows adjusted estimates and column (8) the corresponding number of observations.

Panel A shows results for the share of audited resources involved in narrow corruption in procurement (BNPT 2013) based on CGU audit reports data. Both simple difference and adjusted impact estimates are close to  $-0.10$ , down from a control group mean of about 18 percent, and all

are significantly different from zero at least at 10 percent. The 95 percent confidence interval for the true impact ranges from  $-0.19$  to  $-0.00$  using only the 32<sup>nd</sup> lottery and from  $-0.17$  to  $-0.04$  using the pooled lotteries.

Panel B presents results at the procurement process-level overall and separately for restricted and unrestricted procurement modalities using the same corruption coding as above. Overall impact estimates are about  $-0.15$  in columns (2) and (5) and about  $-0.12$  in column (7) when controls are included, down from a proportion of corrupt processes of about 32 percent in the control group. All impact estimates are statistically different from zero. The 95 percent confidence interval for the true overall impact ranges from  $-0.29$  to  $-0.01$  using only the 32<sup>nd</sup> lottery and from  $-0.25$  to  $-0.05$  using both lotteries together.

Panel B of Table 4 also shows that the corruption reduction was mostly driven by restricted processes. Impact estimates among restricted processes are around  $-0.19$ , down from an incidence of about 0.37 in the control group, while increased audit risk had a much smaller and statistically insignificant effect on the likelihood of corruption among unrestricted modalities. The incidence of corruption in the control group is also lower in unrestricted processes compared to restricted processes (0.20 vs. 0.37), suggesting that in our data the higher discretion afforded by restricted modalities is in practice often abused to strike corrupt deals with favored suppliers.

Table 3 in the online appendix presents results that use the broad BNPT (2013) corruption coding, which includes additional irregularities such as when the procurement modality is too restricted given the size of the purchase or when an ineligible firm is participating in the awarding process. Point estimates and statistical significance are similar to the narrow corruption measure above. For example, the share of audited resources involved in broad corruption is reduced by about 11 percentage points, down from about 20 percent in the control group. The 95 percent confidence interval for the true impact on the share of audited resources involved in corruption ranges from  $-0.18$  to  $-0.05$  and from  $-0.28$  to  $-0.06$  for the likelihood of a corrupt procurement process. Table 4 in the online appendix presents results based on the stricter coding from Ferraz and Finan (2011), excluding cases of fractionalization. Results are again quantitatively similar to the two BNPT codings. Unadjusted impact estimates are about  $-0.08$  and about  $-0.10$  with controls. The 95 percent confidence interval for the impact on the share of audited resources

involved in corruption goes from  $-0.14$  to  $-0.01$  in the pooled sample. Overall, this evidence suggests that the corruption reduction in procurement is invariant to alternative measures, samples, specifications, and corruption codings.

## 7.2 Absenteeism in health service delivery

Table 5 presents evidence on absenteeism by preventive and basic health care program (*Saúde da Família*) staff based on user reports. Columns (1) through (3) are again based solely on the 32<sup>nd</sup> lottery and show the proportion of households reporting a given irregularity in the low audit risk group, the difference in means between high and low audit risk groups and the number of municipalities and respondents, respectively. Columns (4) through (6) show the corresponding statistics for the pooled sample. Adjusted estimates are not shown here to save space but are similar to the simple difference estimates and are available on request.

In contrast to the corruption reduction in procurement, Table 5 shows no evidence that increased audit risk reduced rent-taking in local public health service delivery. Out of the eight outcomes considered, none are statistically different on average between treatment and control groups, irrespective of the sample considered. The p-value for the joint test is 0.99 in the 32<sup>nd</sup> lottery sample and 0.94 in the pooled sample. Impact estimates are also generally small. For example, the likelihood that a household reports receiving visits from community health workers less often than once a month - once per month being required under the preventive health program - is about 50 percent in the low audit risk group in column (4) and virtually identical in the high audit risk group. For some outcomes the impact estimate is even positive, such as for the likelihood that a household member was not attended by a doctor at the health post when needed, which is 4 percentage points higher in the high audit risk group as shown in column (5). Admittedly, however, the confidence intervals are rather large. For example, the 95 percent confidence interval for the true impact on the likelihood of not doing the required monthly visits ranges from  $-0.21$  to  $0.21$  using the pooled sample. Nonetheless, the overall picture that emerges is in stark contrast to the procurement case.

One potential concern with these results is that being in need of a doctor or nurse may itself be influenced by absenteeism and thus by increased audit risk. But Table 5 in the online appendix shows that impact estimates on the likelihood of requiring medical attention are uniformly small,

of inconsistent sign and statistically insignificant. For example, about half the respondents required to see a doctor at the health post in both the low and high audit risk groups.

An a priori plausible explanation for this null effect in health service delivery is that service providers were simply doing their jobs. But the evidence says otherwise. As mentioned above, the likelihood that a household reports receiving visits from community health workers less often than once a month is about 50 percent. And this probability is much more precisely estimated, with the 95 percent confidence interval ranging from 0.39 to 0.59 based on the pooled sample. Since municipalities and households were randomly sampled, this result suggests that community health workers in Brazil are shirking on the job to a considerable extent. The same is also true for more specialized medical personnel. For example, the proportion of households reporting not being attended by a doctor when needed is about 22 percent with a confidence interval ranging from about 15 percent to 29 percent. Similarly, the proportion reporting that the public health post is not usually open during required hours is 63 percent with confidence interval [0.49, 0.76]. Overall, there is thus clear evidence of shirking in the provision of preventive and primary health care services but no evidence that increased audit risk had any deterrent effect.

### **7.3 Compliance with *Bolsa Família* regulations**

Table 6 presents evidence on compliance with eligibility and conditionality requirements of the conditional cash transfer program *Bolsa Família* based on CGU household and school and health record inspections. Similar to the health service delivery results above, impact estimates for the cash transfer program suggest that higher audit risk did not deter irregularities. Out of the 15 impact estimates (5 outcomes and 3 specifications) considered, only one is statistically different from zero at 10 percent. The p-value for the joint test is 0.47 in the 32<sup>nd</sup> lottery sample and 0.36 in the pooled sample. Only the adjusted estimates are jointly significant at 10 percent. More importantly though, impact estimates are all small and equally split between positive and negative signs. In contrast to service delivery however, irregularities in the cash transfer program are close to negligible in the low audit risk group.

The first outcome in Table 6 shows that the likelihood of actual household composition differing from registered composition is only about 5 percent in the low audit risk group and 4 percentage

points higher in the high audit risk group. Similarly, the proportion of households receiving a level of cash transfers that is inappropriate given household income is about 14 percent in the control group and virtually identical in the high audit risk group. Both level and impact estimates are quite precise. The 95 percent confidence interval for the population proportion of households receiving a level of cash transfers that is inappropriate given household income ranges from 11 percent to 18 percent. The impact of increased audit risk on this outcome is between  $-0.06$  and  $0.07$  at 95 percent confidence, clearly more modest than the impact found in procurement. The last three outcomes show that compliance with health and education conditionalities is generally high and no different between treatment and control group respondents. For example, compliance with vaccination requirements is almost perfect, while school enrollment is about 80 percent.

As mentioned above, only about 75 percent of randomly selected households were actually visited and in principle this percentage could be systematically lower in low audit risk municipalities if cash transfer recipients avoided to be at home in order to dodge the inspection. In practice however, the likelihood of inspection was not affected by increased audit risk as shown in Table 6 in the online appendix. For example, the likelihood that household income per capita was assessed by CGU auditors was 77 percent in the low audit risk group and 79 percent in the high audit risk group. Overall, the low non-compliance rates in Table 6 suggest that the vast majority of *Bolsa Familia* recipients were appropriately included in the program, received the correct level of cash given the age and number of children, and fulfilled the health and education conditionalities to a large extent.

## **8 Discussion**

The previous section has documented substantial heterogeneity in both the level of rent-extraction and its response to increased audit risk across procurement, health service delivery and cash transfer targeting. This heterogeneity is genuine in the sense that the risk of a CGU audit increased by the same amount from the same baseline across activities and that the Brazilian local government context is also held constant. We interpret these results in light of the two standard parameters of the rent extraction problem: potential sanctions and the probability that a given sanction is applied, conditional on detection. We then discuss alternative interpretations and available supporting

evidence.

For public officials who run procurement, we find clear evidence of a reduction in rent-taking in response to higher audit risk. A plausible interpretation of this result is that the agents in charge of procurement react rationally to a reduced expected utility from engaging in corruption because they are subject to relatively harsh administrative and judicial punishments and because audit findings constitute hard evidence and thus a high probability of sanction conditional on detection.

For public service providers in the preventive and primary health care 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 substantial numbers of respondents reporting not being attended by a doctor or dentist when needed, or finding the public health post closed during stipulated opening hours. 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 because the audit does not generate hard evidence that could be directly used to impose sanctions. Similarly, sanctions in health service delivery are lower since service providers only have their jobs on the line.

We also find no evidence that higher audit risk had an effect on inclusion errors and overpayments to beneficiaries of the cash transfer program or their compliance with health and education conditionalities. In contrast to health service delivery however, households were already compliant with *Bolsa Família* requirements to a large extent even in the absence of increased top-down scrutiny. While sanctions are limited as in health service delivery, defrauding the cash transfer program is more easily observable by program administrators and the public at large than shirking on the job. Thus both the high compliance with eligibility requirements and conditionalities under normal monitoring conditions and the non-response to higher audit risk are consistent with a relatively high probability of getting administrative or social sanctions for defrauding the program even in the absence of an audit.

## **8.1 Awareness of increased audit risk**

We now consider three alternative interpretations of the above results. The first is differential awareness of treatment status. While the randomization lottery was done publicly and in the presence of news media representatives, we have found little evidence of press coverage in local news media. So perhaps procurement officials were simply aware of heightened audit risk while health service providers and cash transfer recipients were not. We acknowledge this possibility but do not think that differential awareness is the whole story. One reason is that the CGU letter informing high audit risk municipalities of their 25 percent audit risk for the upcoming year went only to the mayor, not to the procurement commission or to other municipal staff. And while the mayor typically plays a role in appointing the procurement commission, he also does so for the hiring of community health workers and local medical personnel. The mayor also oversees local management of the cash transfer program. And there is ample anecdotal evidence of nepotism in personnel decisions and of favoritism in cash transfer awards, not only of procurement fraud. Another reason we do not believe that differential awareness of increased audit risk drives our findings is that procurement officials might have been more likely to be informed of the extra scrutiny precisely because expected sanctions for procurement irregularities are higher than for absenteeism. As such, the mechanism underlying the response heterogeneity we find would still originate in differences in punishments and the likelihood that they are applied, although perhaps in part mediated by differential awareness of heightened audit risk.

## **8.2 Systematic measurement error**

A second and more worrisome interpretation of our results - and indeed of any results based on standard audit reports - is differential measurement error: perhaps procurement officials simply tried harder (and sometimes succeeded) to hide corruption in response to increased audit risk. While we cannot rule out this possibility, there are good reasons why measurement error is unlikely to account for the entire estimated impact in procurement. 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, the evidence in Olken's (2007) study suggests that administrative irregularities detected by auditors do capture at least part of the true level of rent extraction, at

least in the Indonesian setting. Missing road construction expenditures in that study are probably measured with little differential error across treatment and control groups because the type of audit conducted by engineers was unexpected. And administrative irregularities detected by central government auditors in the same road projects were positively correlated with missing expenditures. Third, not all irregularities are equally prone to underdetection. While it is conceivable for example that procurement officials found less detectable ways to steer the contract to favored firms in response to increased audit risk, it is nearly impossible to hide an unduly discretionary procurement modality. And yet we also find a substantial and statistically significant corruption reduction in the procurement process-level data when we focus only on irregularities that are difficult to conceal, as shown in online appendix Table 7.

Another caveat related to measurement is that we need to assume that auditors themselves were not bribed into manipulating audit findings. If this manipulation were higher in the high audit risk group this would bias our estimates towards finding a corruption reduction when in fact there was none. However, we believe that the institutional setup makes it very unlikely that auditors are corrupt. First, auditors are paid by the federal government, not by local governments, which makes it less likely that they are captured by local special interests. Second, auditors are relatively well paid, and therefore have a lot to lose in case collusion gets detected. Third, auditors work in teams of about 10 people on average. This makes it hard to sustain collusion on any significant scale because the whole team has to be bribed in order to conceal irregularities. Fourth, the interaction between auditors and local officials is at a single point in time (unknown *ex ante*), which again makes it harder to sustain collusion. Finally, CGU auditors' work is itself subject to periodic inspection from the external audit agency of the central government, the *Tribunal de Contas da União* and we are not aware of any reported cases of collusion between CGU auditors and local administrations.

### **8.3 Corruption substitution**

A third interpretation relates to substitution of corruption across types of transfers and purchases as well as over time. Although the CGU audit covered all sectors in 75 percent of the municipalities in our sample and sector coverage was not disclosed *ex ante* in the remaining 25 percent,

the audit only covered eligible federal transfers, excluding federal revenue-sharing transfers and state transfers for example. So perhaps corruption went down in audited federal transfers but up in other transfers. Moreover, even if corruption was actually reduced during the period of increased scrutiny, perhaps procurement officials compensated lost rents in subsequent periods. We acknowledge these possibilities. Fully addressing these concerns would require audits data on non-federal transfers as well as over time. Unfortunately we cannot directly measure corruption in subsequent periods because too few municipalities got audited again shortly after the period of increased audit risk. Moreover, getting audits data on non-federal transfers is complicated because these do not fall under CGU jurisdiction.

Nonetheless, there are several reasons why we think that corruption displacement across types of transfers and purchases or over time is likely incomplete at best. For one, displacement across types of transfers is difficult because projects are often financed with several sources of funds, including federal transfers that are eligible for CGU audit. And displacement over time is complicated by the fact that federal transfers cannot be saved for later periods. Moreover, Bobonis et al. (2016) find no evidence of intertemporal corruption substitution in response to audits that are predictably carried out shortly before elections.

Furthermore, three pieces of evidence are inconsistent with substantial corruption displacement across transfers and purchases or over time. First, if officials in high audit risk municipalities were trying to hide or postpone corruption they might have tried to alter the level and composition of federal transfers received. But there is no evidence that this was happening. Table 7 uses the universe of municipalities irrespective of whether they were audited or not (only excluding state capitals and very large municipalities) in order to maximize precision. Column (1) shows average per capita federal transfers in the low audit risk group in the pre-treatment year 2008 broken down by sector. Column (2) shows the difference in means compared to the high audit risk group. As expected given the randomization, the difference estimates are all small and of inconsistent signs in 2008. For example, average federal education transfers per capita in 2008 in the control group were 247 Reais and only 8 Reais higher on average in the high audit risk group. More importantly, however, the differences also remain small in 2009 and 2010 during the period of increased audit risk, as well as in the first post-monitoring year 2011. Results with only audited municipalities are

quantitatively similar, as shown in online appendix Table 8.

Second, if officials in high audit risk municipalities were trying to steal less during the period of increased scrutiny and save resources to steal more in future periods, this should show up in the level of municipal spending. Similarly, there should be expenditure shifts across sectors during the period of increased audit risk if mayors attempted to substitute towards those types of expenditures where detection is more difficult. But both level and composition of municipal spending remained unaffected by increased audit risk. Table 8 again uses the universe of municipalities, irrespective of whether they were audited or not. Columns (1), (3), (5) and (7) show average per capita municipal spending for the major spending categories in the low audit risk group for years 2008 through 2011, respectively. Columns (2), (4), (6) and (8) show the respective difference in means compared to the high audit risk group. For example, average municipal education spending per capita in 2008 in the control group stood at 444 Reais and only about 3 Reais higher on average in the high audit risk group. And as with federal transfers above, the spending differences remained small from 2009 through 2011 across all spending categories. Results with only audited municipalities are again quantitatively similar, as shown in online appendix Table 9.

Third, if officials in high audit risk municipalities were trying to postpone corruption through specific purchases, this should show up in the composition of acquisitions. Table 9 shows the distribution of goods and services purchased by audited local governments during the period of increased scrutiny 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. Table 9 shows that there are no marked differences in the distribution of goods and services bought between treatment and control municipalities from the 32<sup>nd</sup> lottery, suggesting that the treatment did not affect what was being bought. 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 any spending differences as discussed above. Instead, treatment group municipalities were making fewer and larger purchases because they reduced the number of restricted purchases in response to increased audit risk as shown in online appendix Table 10. Taken to-

gether, the available evidence seems inconsistent with substantial corruption displacement across transfers and purchases or over time. Nonetheless, data limitations prevent us from fully ruling out such substitutions. Quantifying their extent is a priority for future work on the effectiveness of top-down monitoring policies.

## **9 Conclusion**

External audits are used by many governments to monitor public officials and service providers and deter various forms of rent extraction. Available evidence on the effectiveness of such top-down monitoring is mixed however and interpretation of these findings is complicated in part because each study evaluates a different intervention in a different context. Yet it is of first-order importance for policymakers to know under what circumstances increased top-down monitoring is effective.

We report results from a randomized policy experiment designed to test whether in the Brazilian local government context higher audit risk deters rent extraction in procurement, health service delivery and cash transfer targeting. Since audit risk increased from the same baseline level of about 5 percent to about 25 percent for all agents in the municipality, any impact heterogeneity across areas of local government activity cannot be driven by differences in the intervention itself or by differences in the broader socioeconomic or institutional context.

We find substantial heterogeneity in both the level of rent-extraction and its response to increased audit risk. While there is substantial corruption in public procurement as well as widespread absenteeism among health workers, only corruption in procurement is responsive to increased audit risk. Cash transfer program fraud is almost negligible even without increased monitoring. These results suggest that increasing the likelihood of an audit alone is not sufficient to deter rent-taking if potential sanctions and the probability of sanction conditional on detection are too low. Moreover, increasing the likelihood of an audit may also be unnecessary for programs that are targeted based on easily observed individual or household characteristics.

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. Moreover, the extent of corruption substitution across federal vs. non-federal transfers as well as over time is unknown. We nonetheless provide a rough cost-benefit analysis in order to

illustrate the magnitudes involved. 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.<sup>13</sup> The marginal cost of the policy therefore amounts to about US\$ 1.2 million, yielding a net benefit of US\$ 4.8 million.

Should audit intensity therefore be scaled up across Brazil? Even though the corruption reduction in procurement is encouraging, it would probably 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. Whether such a permanent increase in audit risk would be feasible to engineer is another question. While additional studies are required to assess the external validity of our findings, we believe that many of the key features of the Brazilian setting - such as stiff penalties for procurement fraud, difficulties pinning down absenteeism of service providers, and welfare benefit targeting based on observables - are common in many other settings and so our results might be fairly general.

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<sup>13</sup>This cost estimate is based on a typical audit involving 2 cars, 10 auditors, 2 weeks in the field and a 500 km round trip from the CGU state branch to the municipality. The estimate includes salaries, per diem, car rental and fuel costs.

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

State	High audit risk			Low audit risk				Ex post	Ex ante
	G	Draws	P(Audit)	G	Draws	P(Draw)	P(Audit)	dP	dP
Acre	0	1	50.0	21	1	1.1	7.8	42.2	17.2
Mato Grosso do Sul	2	1	50.0	72	1	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	1	25.0	66	1	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	1	25.0	46	1	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	1	50.0	11	1	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	1	25.0	80	1	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 29<sup>th</sup> lottery, and Portaria N° 862 from April 30 2010 for the 32<sup>nd</sup> lottery. G 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 high audit risk group, the probability of being drawn in the 32<sup>nd</sup> lottery equals the probability of receiving a CGU audit between May 2009 and May 2010,  $P(\text{Draw}) = P(\text{Audit})$ . 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 low audit risk group, the probability of receiving a CGU audit between May 2009 and May 2010 depends on the probabilities of being drawn in the 29<sup>th</sup>, 30<sup>th</sup>, 31<sup>st</sup>, and 32<sup>nd</sup> lotteries. Under the assumption that the probabilities of being drawn in lotteries 29, 30 and 31 were the same as in lottery 29,  $P(\text{Audit})$  for the control group is calculated according to the following approximation:  $P(\text{Audit}) = 1 - [1 - P(\text{Draw } 29^{\text{th}})]^3 \times [1 - P(\text{Draw } 32^{\text{nd}})]$ . dP gives the ex ante and ex post difference in audit probabilities between treatment and control groups by state.

Table 2: CGU classification of audit findings and corruption codings in procurement

<u>CGU classification of audit findings</u>	%	Corruption codings			
		LZ	BNPT broad	BNPT narrow	FF
- simulated tender process	5.90	X	X	X	X
- unjustified or excessive payments for goods and services	3.45	X	X	X	X
- evidence of favouritism	9.74	X	X	X	X
- fractionalizing of procurement amounts	8.36	X	X	X	
- invitation for bids to less than three firms	1.30	X	X		
- procurement modality too restricted	8.36	X	X		
- participating ineligible firm	0.15	X	X		
- non-selection of the lowest bid	0.31	X			
- other management irregularities	3.37	X			
- absence of preliminary price survey	3.68				
- inadequate publication of the call	1.61				
- incomplete specification of the call	0.97				
- inadequate publication of results	1.15				
- other procedural irregularities	0.77				
- other irregularities	8.28				
- formal errors	13.96				
- regular process	28.60				

*Notes:* LZ: Litschig and Zamboni (2012), FF: Ferraz and Finan (2011), BNPT: Brolo, Nannicini, Perotti, and Tabellini (2013). N=1,304 procurement processes from 2009 or 2010. Ferraz and Finan (2011) code an irregularity as a case of corruption only if “the public good was not provided”.

Table 3: Pre-treatment covariate balance

Audit risk	High	Low	Low	Low	High	Difference
Audited in 31 <sup>st</sup> or 32 <sup>nd</sup> lotteries	No	No	31 <sup>st</sup>	32 <sup>nd</sup>	32 <sup>nd</sup>	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: CGU audit reports data, 2008 transfers						
Per capita amount of federal transfers audited	-	-	335.9 [315.8]	95.4 [117.3]	81.8 [101.5]	-13.6 (30.4)
Share of audited amount involved in corruption in procurement	-	-	0.299 [0.350]	0.129 [0.232]	0.193 [0.351]	0.064 (0.083)
Share of audited amount involved in narrow corruption in procurement	-	-	0.362 [0.377]	0.167 [0.287]	0.193 [0.351]	0.026 (0.089)
Share of audited amount involved in broad corruption in procurement	-	-	0.374 [0.374]	0.219 [0.326]	0.245 [0.362]	0.026 (0.096)
Number of municipalities	-	-	59	26	26	
Panel B: CGU, TSE, and IBGE data						
Audited prior to 28th lottery	0.32 [0.47]	0.25 [0.43]	0.38 [0.49]	0.27 [0.45]	0.27 [0.45]	0.000 (0.116)
Mayor win margin in 2008 < 5%	0.26 [0.44]	0.24 [0.43]	0.30 [0.46]	0.10 [0.30]	0.30 [0.47]	0.200 (0.102)
Population (in thousands)	25.5 [48.0]	23.2 [44.3]	21.1 [28.3]	13.8 [13.1]	21.5 [37.4]	7.742 (7.227)
Income per capita	151.3 [88.3]	169.5 [94.0]	157.2 [85.7]	156.5 [71.4]	162.5 [85.8]	6.028 (20.382)
Average years of schooling	3.90 [1.36]	4.02 [1.26]	3.95 [1.17]	3.80 [1.06]	3.86 [1.35]	0.062 (0.313)
Urbanization	0.59 [0.23]	0.59 [0.23]	0.61 [0.22]	0.57 [0.19]	0.57 [0.24]	0.001 (0.055)
Poverty headcount ratio	0.28 [0.18]	0.25 [0.18]	0.27 [0.19]	0.25 [0.17]	0.26 [0.20]	0.017 (0.048)
Poverty gap	0.51 [0.09]	0.49 [0.11]	0.50 [0.10]	0.48 [0.10]	0.52 [0.12]	0.046 (0.029)
Gini coefficient	0.56 [0.05]	0.56 [0.06]	0.57 [0.06]	0.56 [0.05]	0.56 [0.07]	0.005 (0.016)
Radio station	0.47 [0.50]	0.45 [0.50]	0.45 [0.50]	0.47 [0.51]	0.47 [0.51]	0.000 (0.131)
Number of municipalities	90	5,311	60	30	30	

*Notes:* The first five columns give sample means and in brackets standard deviations. Column (2) excludes state capitals and municipalities with 2007 population larger than 500,000. The last column gives the difference in means between columns (4) and (5) and in parentheses the corresponding standard error. The share of audited amount involved in corruption is based on federal resources that were transferred during 2008, prior to the experiment. See Table 2 for details of the corruption coding. Audited prior to 28th lottery indicates whether the municipality was audited by CGU at least once in lotteries 2 through 27. Mayor win margin in 2008 < 5% indicates whether the win margin between the elected mayor and the runner-up candidate in 2008 was smaller than 5%. Municipality characteristics are from the 2000 census, except for population, which is from the 2007 population count. The F-statistic for the joint hypotheses that none of the covariates in panels A or B predict whether a municipality is in the high audit risk group is 1 with p-value 0.44.

Table 4: Corruption in procurement, BNPT (2013) narrow measure

Dependent variable	32nd lottery			31st and 32nd lottery		
	Control mean	Simple difference	N [G]	Control mean	Simple difference	Adjusted N [G] difference
	(1)	(2)	(3)	(4)	(5)	(6) (7) (8)
Panel A: CGU audit reports data, 2009 and 2010 transfers						
Share of audited amount involved in narrow corruption in procurement	0.181 (0.042)	-0.098 (0.048)	[60]	0.187 (0.027)	-0.105 (0.035)	-0.095 (0.056) [111]
Panel B: CGU procurement process-level data, 2009 and 2010 transfers						
Procurement process with evidence of narrow corruption	0.315 (0.061)	-0.153 (0.071)	834 [60]	0.316 (0.037)	-0.154 (0.051)	1,304 [117] -0.118 (0.051) [117]
Restricted procurement process with evidence of narrow corruption	0.369 (0.057)	-0.194 (0.071)	539 [60]	0.365 (0.037)	-0.189 (0.056)	880 [113] -0.145 (0.063) [113]
Unrestricted procurement process with evidence of narrow corruption	0.201 (0.115)	-0.060 (0.112)	295 [49]	0.201 (0.066)	-0.060 (0.078)	424 [87] -0.026 (0.068) [87]

*Notes*: Panel A: Municipality-level OLS estimations with robust standard errors. Panel B: OLS estimations at the procurement-process level with standard errors clustered at the municipality level. Narrow corruption corresponds to cases of simulated tender processes, cases of favoritism, or when auditors determine that there were unjustified or excessive payments for goods or services, as well as cases of fractionalized procurement amounts. The 'Control mean' columns (1) and (4) give the sample average in the low audit risk group. The 'Simple difference' columns (2) and (5) give the difference in means between high and low audit risk groups. In columns (3), (6) and (8) G is the number of municipalities and N is the number of procurement processes used to estimate columns (2), (5) and (7), respectively. The 'Adjusted difference' column (7) reports estimates on the high audit risk group dummy from a regression that also includes state intercepts, mayor's party dummies and mayor characteristics in 2008, and municipality characteristics from 2000 and 2007. In panel A the regression additionally includes the share of audited resources involved in narrow corruption in procurement in 2008. Mayor characteristics: first-term mayor indicator, indicator for mayor win margin < 5%, mayor education level indicators, male dummy and age. Municipality characteristics: indicator for CGU audit at least once in lotteries 2 through 27, population, income per capita, average years of schooling, urbanization, poverty headcount ratio, poverty gap, gini coefficient and indicator for local radio station. 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.

Table 5: Absenteeism in health service delivery

Dependent variable	32nd lottery			31st and 32nd lottery		
	Control mean (1)	Simple difference (2)	N [G] (3)	Control mean (4)	Simple difference (5)	N [G] (6)
Household receives no visits from community health worker	0.071 (0.016)	-0.019 (0.022)	1,099 [58]	0.074 (0.013)	-0.023 (0.019)	2,373 [112]
HH receives visits from community health worker less often than once a month	0.533 (0.090)	-0.040 (0.131)	1,099 [58]	0.495 (0.053)	-0.002 (0.108)	2,373 [112]
Household member did not receive a visit from medical staff at home when needed	0.375 (0.107)	-0.050 (0.179)	248 [29]	0.336 (0.053)	-0.011 (0.152)	522 [61]
Household member was not attended by a doctor at the health post when needed	0.227 (0.060)	0.034 (0.105)	607 [31]	0.223 (0.036)	0.038 (0.093)	1,374 [63]
Household member was not attended by a nurse at the health post when needed	0.070 (0.032)	-0.014 (0.040)	599 [29]	0.050 (0.013)	0.007 (0.027)	1,361 [61]
Household member was not attended by a dentist at the health post when needed	0.247 (0.096)	-0.051 (0.124)	447 [27]	0.244 (0.047)	-0.048 (0.090)	1,003 [58]
HH indicates that the health post is not usually open during required hours	0.500 (0.127)	0.087 (0.171)	527 [30]	0.628 (0.067)	-0.041 (0.132)	1,144 [63]
HH indicates being asked to pay a fee for service from medical staff	0.005 (0.004)	-0.001 (0.005)	1,082 [58]	0.016 (0.013)	-0.013 (0.013)	2,343 [113]
F-statistic		0.180			0.350	
(p-value)		(0.992)			(0.943)	

*Notes:* OLS estimations at the household (HH) level. Standard errors are clustered at the municipality level. The 'Control mean' columns (1) and (4) give the sample average in the low audit risk group. The 'Simple difference' columns (2) and (5) give the difference in means between high and low audit risk groups. In columns (3) and (6) G is the number of municipalities and N is the number of households used to estimate columns (2) and (5) respectively. F-statistics are for the joint hypotheses that all coefficients in a given column are zero.

Table 6: Compliance with cash transfer program eligibility and conditionality requirements

Dependent variable	32nd lottery			31st and 32nd lottery				
	Control mean difference (1)	Simple (2)	N[G] (3)	Control mean difference (4)	Simple N[G] difference (5)	N[G] (6)	Adjusted N[G] difference (7)	(8)
Actual household composition differs from registered composition	0.046 (0.014)	0.037 (0.028)	1,333 [59]	0.048 (0.009)	0.036 (0.026)	2,723 [117]	0.044 (0.024)	2,723 [117]
Cash transfer amount is inappropriate given household income	0.140 (0.024)	0.008 (0.038)	1,346 [60]	0.147 (0.015)	0.002 (0.033)	2,750 [118]	-0.009 (0.036)	2,750 [118]
Children have inappropriate vaccination records	0.018 (0.011)	-0.011 (0.013)	573 [60]	0.013 (0.004)	-0.006 (0.008)	1,209 [118]	-0.014 (0.011)	1,209 [118]
Bolsa Família recipient not enrolled at school	0.208 (0.031)	-0.019 (0.050)	3,352 [60]	0.156 (0.015)	0.033 (0.040)	6,630 [118]	0.025 (0.041)	6,630 [118]
Enrolled Bolsa Família recipient attending school infrequently	0.039 (0.013)	0.022 (0.020)	2,686 [60]	0.081 (0.010)	-0.021 (0.018)	5,543 [118]	-0.045 (0.032)	5,543 [118]
F-statistic		0.920			1.110		2.190	
(p-value)		(0.473)			(0.357)		(0.060)	

*Notes:* WLS estimations at the municipality level with weights corresponding to the number of households or individual students investigated in each municipality. Robust standard errors in parentheses. The 'Control mean' columns (1) and (4) give the sample average in the low audit risk group. The 'Simple difference' columns (2) and (5) give the difference in means between high and low audit risk groups. In columns (3), (6) and (8) G is the number of municipalities and N is the number of respondent households or students to whom a given inspection applies that are used to estimate columns (2), (5) and (7), respectively. The 'Adjusted difference' column (7) reports estimates on the high audit risk group dummy from a regression that also includes state intercepts, mayor's party dummies and mayor characteristics in 2008, and municipality characteristics from 2000 and 2007. Mayor characteristics: first-term mayor indicator, indicator for mayor win margin < 5%, mayor education level indicators, male dummy and age. Municipality characteristics: indicator for CGU audit at least once in lotteries 2 through 27, population, income per capita, average years of schooling, urbanization, poverty headcount ratio, poverty gap, gini coefficient and indicator for local radio station. F-statistics are for the joint hypotheses that all coefficients in a given column are zero.

Table 7: Federal transfers per capita before, during, and after increased audit risk

Dependent variable	2008		2009		2010		2011	
	Control	Simple	Control	Simple	Control	Simple	Control	Simple
	Mean difference	(1)	Mean difference	(2)	Mean difference	(3)	Mean difference	(4)
Federal education transfers per capita	246.9 (1.5)	8.3 (9.3)	276.9 (1.6)	11.6 (10.7)	330.2 (1.8)	8.1 (11.9)	400.8 (11.5)	7.0 (18.9)
Federal health transfers per capita	85.4 (0.7)	-1.5 (5.1)	99.5 (0.7)	-4.1 (4.9)	109.1 (0.8)	-2.2 (5.7)	131.8 (4.9)	-0.6 (8.2)
Federal welfare transfers per capita	10.4 (0.2)	2.8 (1.5)	11.9 (0.2)	0.9 (1.1)	14.6 (0.18)	0.9 (1.2)	19.5 (0.7)	2.1 (1.6)
Other federal transfers per capita	29.7 (0.9)	-8.8 (2.8)	49.5 (1.0)	-1.4 (6.8)	39.1 (1.2)	-1.4 (5.3)	32.9 (1.5)	-6.3 (4.1)
Federal capital transfers per capita	48.7 (1.2)	6.6 (10.1)	38.4 (1.1)	-6.6 (5.1)	63.6 (1.6)	-1.4 (9.4)	66.6 (1.3)	65.8 (61.1)
Number of municipalities	5,320	5,440	5,358	5,477	5,334	5,452	5,227	5,341
F-statistic		2.550		0.960		0.230		1.340
(p-value)		(0.026)		(0.443)		(0.950)		(0.242)

*Notes:* OLS estimations at the municipality level. Robust standard errors in parentheses. The sample consists of the universe of municipalities with available data on federal transfers, excluding state capitals and municipalities with 2007 population larger than 500,000. The dependent variable is the per capita transfer of a given type and in a given year. Transfers are in current Reals. The 'Control mean' columns (1), (3), (5) and (7) give the sample average for a given type of revenue in a given year in the low audit risk group. The 'Simple difference' columns (2), (4), (6) and (8) give the difference in means between high and low audit risk groups for a given type of transfer in a given year. F-statistics are for the joint hypotheses that all differences in outcomes in a given year are zero.

Table 8: Municipal expenditure per capita before, during, and after increased audit risk

Dependent variable	2008		2009		2010		2011	
	Control	Simple difference						
Education expenditure per capita	443.8 (2.6)	2.6 (16.3)	469.8 (2.5)	10.1 (16.5)	546.3 (3.1)	7.8 (20.4)	640.6 (3.8)	13.0 (22.3)
Health expenditure per capita	361.8 (2.6)	-13.5 (16.7)	380.0 (2.6)	-23.4 (16.7)	436.5 (3.0)	-21.1 (19.0)	495.1 (3.5)	-12.5 (24.1)
Welfare expenditure per capita	91.4 (1.1)	1.9 (6.7)	95.1 (1.1)	0.6 (6.8)	109.8 (1.2)	-1.8 (7.6)	127.5 (1.5)	4.9 (9.3)
Transportation expenditure per capita	94.5 (2.1)	-1.2 (10.9)	78.9 (1.8)	3.2 (10.9)	100.4 (2.3)	-10.1 (12.6)	104.2 (2.5)	12.8 (17.5)
Housing expenditure per capita	170.0 (2.2)	10.3 (16.1)	138.8 (1.7)	3.4 (11.8)	183.7 (2.3)	0.5 (15.3)	198.2 (2.5)	-5.4 (16.0)
Number of municipalities	4,899	5,008	5,278	5,395	5,060	5,169	4,667	4,772
F-statistic		0.810		1.800		0.650		0.560
(p-value)		(0.543)		(0.109)		(0.662)		(0.730)

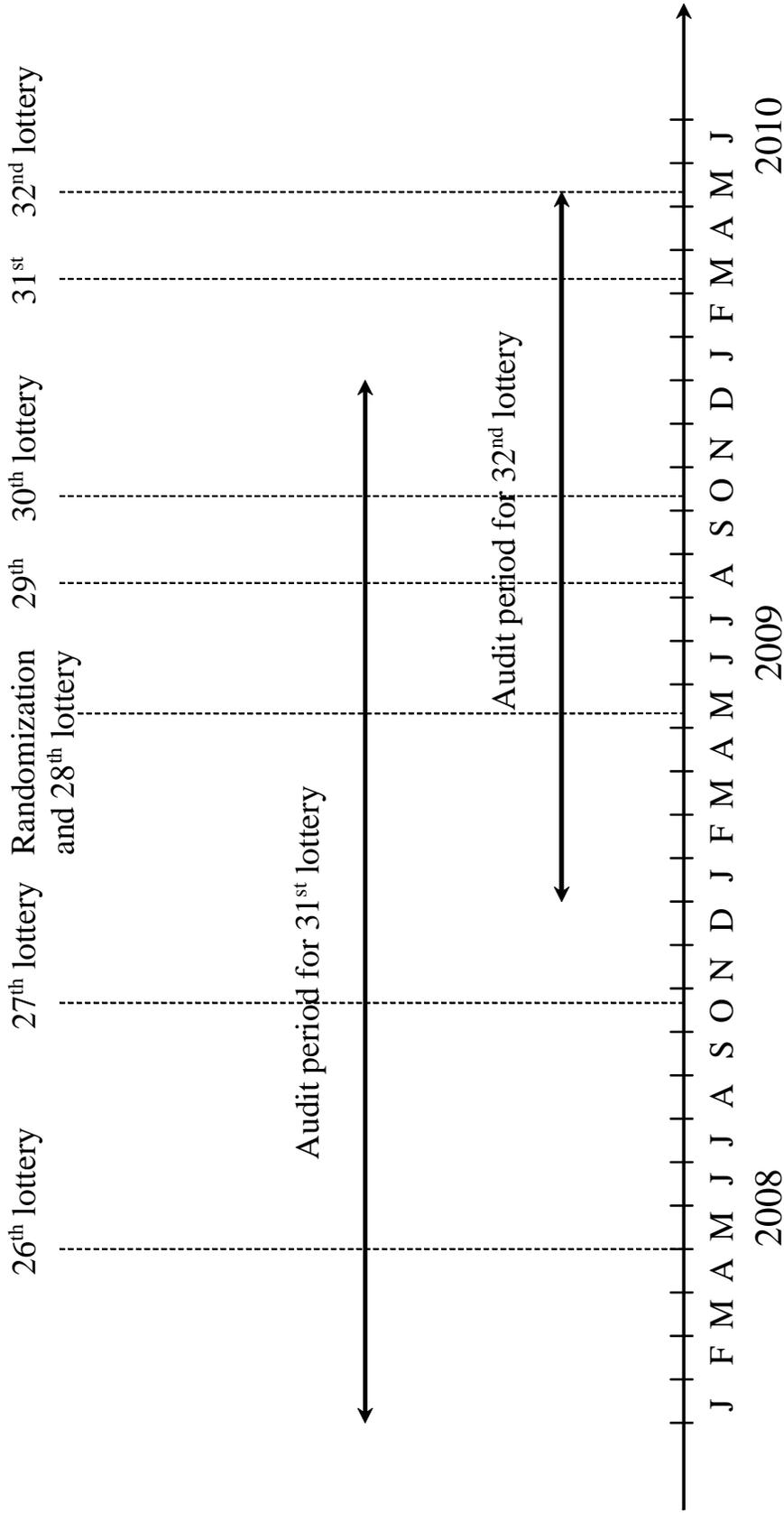
*Notes:* OLS estimations at the municipality level. Robust standard errors in parentheses. The sample consists of the universe of municipalities with available budget data, excluding state capitals and municipalities with 2007 population larger than 500,000. The dependent variable is the per capita municipal expenditure in a given budget category and year. The 'Control mean' columns (1), (3), (5) and (7) give the sample average for a given budget category in a given year in the low audit risk group. The 'Simple difference' columns (2), (4), (6) and (8) give the difference in means between high and low audit risk groups for a given budget category in a given year. F-statistics are for the joint hypotheses that all differences in outcomes in a given year are zero.

Table 9: Distribution of purchases by level of audit risk and lottery

<u>Procurement object</u>	32 <sup>nd</sup> lottery			31 <sup>st</sup> lottery		
	High audit risk	Low audit risk	Percent	High audit risk	Low audit risk	Percent
Staple foods	Freq. 85	Freq. 117	24.15	Freq. 110	Freq. 110	23.40
Medication	50	49	14.20	49	49	10.43
Other non-durable goods	43	70	12.22	74	74	15.74
Medical equipment	5	9	1.42	16	16	3.40
IT equipment	6	12	1.70	5	5	1.06
Agricultural equipment	10	6	2.84	8	8	1.70
Other durable goods	11	11	3.13	16	16	3.40
Public works	24	41	6.82	59	59	12.55
Contracted-out services	48	46	13.64	67	67	14.26
Other objects	70	121	19.89	66	66	14.04
Total	352	482	100.00	470	470	100.00

*Notes:* The unit of observation is an individual procurement process. For the 32<sup>nd</sup> lottery the data are from 2009 or 2010, while for the 31<sup>st</sup> lottery the procurement processes are almost entirely from 2009. The high vs low audit risk distributions from the 32<sup>nd</sup> lottery are not statistically different from each other according to Pearson's chi-square test (p-value 0.128).

Figure 1: Timeline and audit periods for the 31st and 32nd lotteries



*Notes:* The 26<sup>th</sup> through 32<sup>nd</sup> were regular audit lotteries. 60 municipalities were sampled in each round. The randomization lottery coincided with the 28<sup>th</sup> lottery and randomly assigned 120 municipalities to the high audit risk group for the upcoming year. In the 32<sup>nd</sup> 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 32<sup>nd</sup> lottery the main audit period extended back to January 2009. For the 31<sup>st</sup> lottery the main audit period extended from January 2008 until December 2009. In both lotteries some earlier federal transfers were audited as well.