PLAYING WHAC-A-MOLE IN THE FIGHT AGAINST CORRUPTION: EVIDENCE FROM RANDOM AUDITS IN BRAZIL

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Playing *Whac-A-Mole* in the Fight against Corruption: Evidence from Random Audits in Brazil

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Abstract

Audits can reduce corruption, but corrupt officials may be able to substitute to alternate forms of corruption when the anticorruption policy controls only certain types of corruption. By exploiting the random assignment of municipalities to a large, successful, audit program in Brazil, we document unintended (and undesirable) consequences of such selective anticorruption monitoring: audited municipalities employ more labor in water provision, and this translates into a more inefficient service. We also provide additional evidence consistent with the idea that local officials may be using their discretion in hiring to substitute between different forms of corruption.

Keywords: corruption, audits, efficiency, development, employment, water & sanitation.

JEL codes: D73, D78, H42, K42, L95.

I. Introduction

Corruption (the misuse of public office for private gain) costs at least 3% of world GDP each year (Svensson, 2005), and constitutes a major impediment to economic development (see, e.g., Mauro, 1995; and Olken and Pande, 2012). Audits are a popular tool in the fight against such corruption. The idea of providing incentives to corruptible bureaucrats to behave honestly through an increased probability of detection and punishment, even if old (Becker, 1968; Becker and Stigler, 1974), is certainly appealing. Plus, evidence tells us that audits are successful in reducing corruption (Olken, 2007; Bobonis et al., 2016; Avis et al., 2018; Zamboni and Litschig, 2018).

But things can go awry: some evidence also exists showing that corrupt officials are able to substitute to alternate forms of corruption and to adapt over time to the audit system by changing other dimensions of their performance—especially when only certain types of corruption are audited. For example, in his audit experiment in Indonesia, Olken (2007) finds that audits reduce direct stealing of project funds but increase nepotism (that is, the hiring of officials' family members).

In the popular arcade game *Whac-A-Mole*, every time the player whacks a mole in a hole (by hitting it in the head with a mallet), another mole pops up randomly from a different hole. Like in the arcade game, when authorities fight corruption in one activity, corruption can pop up elsewhere. Unlike in the arcade game, it does not so randomly, but rather as a rational response of corrupt officials to the selective monitoring system. This paper is an empirical study of the possible unintended consequences of such selective anticorruption monitoring, in the context of a large random audits program in Brazil that has proved successful in reducing the misuse of federal funds at the municipal level (Avis et al., 2018; Zamboni and Litschig, 2018).

We focus here on how auditing municipal governments affects the efficiency of water and sanitation provision at the municipal level. Boyco et al. (1996) emphasize that excess employment

is a politically demanded inefficiency of public firms. Unlike other infrastructure services, like electricity or telecommunications, water and sanitation services are locally (i.e., municipally or regionally) provided. Politicians may value some control over the jobs available in utilities (e.g., to reward loyal party members, or to place in positions of authority within utilities those who would promote their political agendas – Berg, 2013) – the 1988 Constitution provides Brazilian mayors with the discretion in hiring necessary for such purposes.

Given that the audits under study reduced corruption, and that previous literature has shown that reduced corruption is strongly associated with less inefficiency, one is tempted to jump to the conclusion that audits ought to be associated with increased efficiency at the firm level.[1] One would be wrong: we find that water companies in audited municipalities use about 4.5% more labor than those in non-audited municipalities, even when controlling for output and other inputs—monitoring corruption can thus backfire in the form of increased labor inefficiency.

We show that this result is robust to alternative specifications that control for municipal characteristics and key cost drivers of water provision. To further assess the validity of our results, we conduct a false experiment estimating the effect of the audits on labor efficiency in 2002 (before the audit program began), and we find no effect associated to the treatment indicator—suggesting that the estimated increase in inefficiency induced by the audits is indeed causal.

A potential explanation for the fact that audits increase labor inefficiency is that, since audits increase the probability of detecting direct stealing, audited corrupt officials may opt to substitute for alternative or less traceable forms of corruption. If this was the case in our setting, then the increase in inefficiency should be restricted to public firms, and should be lower in municipalities

[1] Dal Bó and Rossi (2007) find that corruption at the country level has a negative impact on the efficiency of electricity distribution, in the sense that firms employ less inputs to produce a given level of output.

served by a regional—rather than a municipal—operator (in which it would be more difficult for the mayor to exercise discretion in hiring). We provide evidence that this is indeed the case.

The possibility that imposing controls on certain types of corruption may lead to increases in other types has been "widely discussed but rarely tested" (Niehaus and Sukhtankar, 2015: 233). Besides Olken's (2007) experiment, early evidence is provided by Yang (2008), who studies the possible unintended consequences of selective law enforcement in the context of a customs reform in the Philippines that increased enforcement against a specific method of avoiding import duties. The reform reduced the targeted duty-avoidance method, but caused substantial displacement to an alternative method – without affecting total duty avoidance.

Using the same audit program in Brazil as we do, Zamboni and Litschig (2018) discuss the possibility of substitution of corruption across federal vs. non-federal transfers or over time. Lack of appropriate data precluded a direct assessment of that possibility, but the authors argue that the available evidence seems inconsistent with substantial corruption displacement across transfers and over time. The closest antecedent to our study is Lichand et al. (2017), who show another unintended consequence of the same audit program on public service delivery: in spite of reduced corruption in health expenditures, public spending fell by so much after the program that corruption per dollar spent may have actually increased – worsening health indicators, such as hospital beds and immunization coverage. Gerardino et al. (2017) investigate the effects of procurement audits on public entities' choice of purchase procedures in Chile, and find that audits have the unintended consequence of reducing the use of more transparent and competitive auctions in favor of discretionary direct contracts for selection of suppliers—the behavior the audits were trying to discourage.

Other studies adding to the scarce evidence on how potentially corrupt bureaucrats and politicians respond to incentives include Burgess et al. (2012), and Niehaus and Sukhtankar (2015). Both

papers document more generally that different sources of rent extraction are substitutes. Burgess et al. (2012) find evidence of substitution between rents from illegal logging and potential rents from oil and gas revenue sharing in the short run among local officials in Indonesia—a pattern that tends to fade over time as new, higher rent-extraction political coalitions form. Niehaus and Sukhtankar (2015), on the other hand, focus on the dynamic trade-off between extracting rents today and improving one's chances of surviving to extract rents tomorrow (a phenomenon they dub the "golden goose" effect). The authors find evidence of substantial golden goose effects: as permanent opportunities to extract rent increase, the value of continuing in office increases and this induces agents to act more cautiously—i.e., to extract fewer rents today.

II. Background and data

The random audits program

In 2003 the Brazilian federal government audit agency (*Controladoria Geral da União,* CGU) launched a large scale anti-corruption program (*Programa de Fiscalização de Municípios a partir de Sorteios Públicos*), aimed at reducing corruption and misuse of public funds by public administrators.[2] The program consists of random audits of municipal governments' use of federal funds transferred to them, and it was probably the first of its kind in the world. The main findings for each audited municipality are made publicly available to reduce information asymmetries between voters and public officials.

The program randomly selects a fixed number of municipalities per state (*unidad federativa*) through lotteries held by the *Caixa Econômica Federal* in Brasília, drawn in conjunction with the

[2] This description draws heavily from CGU (2004, 2016) and Lopes Gomes (2013). For further description of the audit program see Ferraz and Finan (2008), where the random assignment was first used to analyze how exposing government corruption affected the electoral performance of incumbent politicians.

national lotteries.[3] To ensure a transparent process, members of the civil society, political parties, and the press are all invited to witness the lottery.

Once the municipalities are selected in a lottery, a team of CGU auditors (on average, 10 agents) collects information on the use of federal funds transferred to each municipality's government by examining accounts and documents, and visiting the existing works and services.[4] These auditors also meet with members of the local community and municipal councils to receive direct complaints. The auditors spend approximately one week inspecting, and then send a report to the central CGU office detailing all the irregularities found. These reports are sent to the different agencies in charge of corrective actions (public prosecutors, the legislative branch of each municipality, and the *Tribunal de Contas da União*). Drafted municipalities become eligible again for an audit after one year of the lottery.

The first lottery was carried out in April 2003, and by the end of that year 281 municipalities had been drafted through 7 lotteries, with about 2 billion reais of federal funds involved in the audits. Among the most common irregularities detected by the audit teams were incomplete public works (which had already been paid for), use of fake receipts, rigged and irregular bidding processes, and over-invoicing of goods and services.

The program underwent some modifications, especially in its early years, in terms of the total number of municipalities to be drafted, the number of municipalities per state, the eligibility criteria for municipalities, and the frequency of the lotteries. From the third lottery on, the program randomly selected 50 municipalities to audit from those municipalities with less than 300,000 inhabitants; it then expanded to selecting 60 municipalities per lottery (as of lottery #10) out of those

[3] There are 27 *unidades federativas* in Brazil, including Brasília, the federal capital, which is not eligible for the audit program. The sampling procedure is stratified by state. Because they have few municipalities, the smaller states in the Northern Region (*Região Norte*) are usually grouped together in a single stratum for the draft.
[4] The main areas under scrutiny are education, health and social services.

with less than 500,000 inhabitants.[5] At the very beginning of the program the lotteries were drawn on a monthly basis, but a reference periodicity was never established and the number of lotteries per year has steadily declined along the years, with just one per year in 2013-2015. Further details are provided in the Appendix.

Up until the 40th lottery, which took place in February 2015, almost 2,000 municipalities (some of them, more than once)—which correspond to almost 40% of Brazilian municipalities—and over 20 billion reais in federal funds have been audited by the program. After the 40th lottery the program was seriously redesigned, changed its name (*Programa de Fiscalização em Entes Federativos*), and though it kept the lottery system, this was combined with other criteria for selecting municipalities.

This study considers 5,570 municipalities that were eligible for the program, 1,920 of which were randomly selected through the 40 lotteries. The list of the municipalities selected in each of the 40 lotteries (and eventually audited) was obtained directly from the CGU website.[6] With this information we built a dummy variable (*Drafted*) that takes value 1 if the municipality was drafted at least once in the period 2003-2015 (40 lotteries), and constitutes our treatment indicator.[7]

We restrict our sample to municipalities with a single water and sanitation provider.[8] After dropping municipalities with missing values in our main dependent variable, we are left with 4,516 municipalities, 1,564 of which were audited at least once.

Brazilian municipalities

[5] In spite of the population threshold, over 99% of Brazil's 5,570 municipalities were eligible for the lotteries.

[6] Retrieved from http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores/municipios on 9/27/2018.

[7] We use the random assignment as our treatment variable. The estimates from the reduced form provide a lower bound on the true effect of the audit treatment (Angrist and Pischke, 2009). The novel econometric technique we are using prevents us from using the actual audits (instrumented with the lottery results) as treatment, but given that non-compliance is only 2% in our sample, the effect of treatment on the population of compliers would not differ much from the reduced-form estimates.

[8] Including these 506 municipalities has no effect on our results.

Brazil is a federal presidential representative democracy, whose government is made up of a Federal District (Brasília) and three administrative tiers: the central government, 26 state governments and 5,570 municipal governments.[9] Municipalities are run by a mayor (*prefeito*), a vice-mayor (*vice-prefeito*), and members of the city council (*Câmara de Vereadores*), who are simultaneously elected every four years.

Article 30 of the 1988 Constitution charges municipalities with providing and managing essential public services of local interest—like public transport, early childhood and primary education programs, public health services, and, directly relevant for our purposes, water distribution, sewer collection, solid waste collection and micro-drainage. Revenues available to municipalities to meet their responsibilities can be grouped into three categories: intergovernmental transfers (defined as transfers received from revenues generated by other government entities), locally generated revenues (defined as revenues collected from a municipality's citizens) and other miscellaneous sources of revenue. Intergovernmental transfers account for the vast majority of municipalities' receipts (93% on average in 2011), and are mostly unconditional in nature. Therefore, the mayor and the members of the city council enjoy significant discretion over how to spend these resources.

Data on municipal socioeconomic and demographic characteristics come from the Brazilian statistics office (*Instituto Brasileiro de Geografia e Estatística*, IBGE). To capture underlying differences in municipal characteristics prior to the audits, we include key variables similar to those used in Ferraz and Finan (2008): income per capita (*GDP per capita*), income inequality (*Gini*), total population (*Population*), share of urban population (*Urban*), and share of the population over 25 that completed secondary education (*Education*).[10] To evaluate the validity of our research

[9] This account is mostly based on Gardner (2013), where further details can be found.
[10] The Gini coefficient and the education data are from the 2000 Census. Population estimates are for 2002 and were obtained from https://www.ibge.gov.br/estatisticas-novoportal/sociais/populacao/9103-estimativas-de-populacao.html?edicao=17283&t=downloads. The share of urban population is computed as urban population over total

design, we present in Table 1 summary statistics of these pretreatment (before the program started in 2003) characteristics for treated and control municipalities, and also the differences between both groups.

[Table 1 about here]

As expected from the random assignment, only one out of 6 differences is significant at the 10% level, and a test of the joint significance of all municipal characteristics fails to reject the null of no significance (F-test = 1.02; p-value = .41). Overall, the results from Table 1 suggest that the lottery used by the CGU yielded a balanced experiment.

Water and sanitation

The water and sanitation sector in Brazil is structured mainly around a set of state (regional operators) and municipal (local operators) public firms. The 27 regional operators serve around 70% of the population in 4,602 municipalities, while the rest of the market is composed of 1,607 local operators. Private firms serve only 9% of the population (14% if mixed companies under private management are included).[11] Service operators are local monopolies within their operation areas.

Service coverage has improved over the years. About 97% of the population was served with piped, drinking water on their premises, against 84% in 2000. Coverage of the sewer system also improved, but is much lower: 63.6% in 2015, up from just 41.6% in 2000. Besides, only 27% of the wastewater collected was treated in 2015. Regional differences in coverage in both services are persistent, and a rather regressive pattern is observed when service coverage is analyzed by income levels.[12]

population in 2002. GDP per capita is computed as municipal GDP/population in 2002. Municipal GDP data was obtained from https://sidra.ibge.gov.br/pesquisa/pib-munic/tabelas.

[11] Mixed companies are firms in which the State has a majority shareholding, but is not the sole owner. Public firms are those in which 100% of shares are owned by the State.

[12] This brief characterization is based on Da Motta and Moreira (2006) with updated figures. Coverage figures were obtained from the WHO/UNICEF Joint Monitoring Program on Water Supply, Sanitation and Hygiene (WASH) at https://washdata.org/data. Other figures come from the SNIS database to be described momentarily.

Efficiency in water utilities is largely driven by management and efficient labor use (Estache and Rossi, 2005). Brazilian providers have on average 3.1 employees per 1,000 connections—below the Latin American average of 3.8, but well above the international benchmark of 2 employees per 1,000 connections (CAF, 2011, 2012).[13]

Data on water and sanitation by municipality was collected from the *Sistema Nacional de Informações sobre Saneamento* (SNIS).[14] The SNIS database collects information on the operation of water and sanitation services all over Brazil through annual surveys applied to operators since 1995, and contains operational, managerial, financial, and service-quality data.[15] As of 2016, it covers essentially the whole population of the country.

We restrict our analysis to the year 2016, the first complete year after the original program ended. The SNIS database provides us with our main dependent variable, number of employees (*Employees*). According to our 2016 data, approximately 148,000 people were directly employed in the water and sanitation sector.

Given our focus on how auditing municipal governments' expenditure affects efficiency in water provision, we also estimate a parametric labor requirement function, which requires controlling for outputs and other inputs. In line with the literature on water efficiency that began with the seminal work by Stewart (1993), we control for the extension of the water network in kilometers (*Network*, a key capital input), the number of active water connections (*Connections*, the main output), and the

[13] Employees per 1,000 connections is a common measure of labor efficiency in the water sector, in spite of the well-known difficulties of partial productivity indices. See, for example, Lentini (2015), and the regional benchmarking system of ADERASA, the Association of Regulators of Water and Sanitation of the Americas (http://www.aderasa.org/v1/en/grupos-de-trabajo/benchmarking/). Below we will consider a more precise definition of labor efficiency.

[14] http://app3.cidades.gov.br/serieHistorica/, accessed on 12/4/2018 and 1/25/2019.

[15] The SNIS is part of the Modernization Program of the Sanitation Sector (PMSS). Further details on the database can be found in the SNIS webpage: http://www.snis.gov.br/institucional-snis.

number of clients served (which we proxy by population).[16] The SNIS database contains, for each water municipality, also information on provider ownership (public, private, mixed), coverage (local, regional), and type of service provided (water only, water and sewerage). Table 2 presents summary statistics for our SNIS variables.

[Table 2 about here]

III. Econometric methodology

Given the randomized experimental design, estimation is a straightforward comparison of sample mean outcomes for treated and control municipalities. Since treatment probabilities vary by state due to the stratified randomization, the approach typically followed in the literature to estimate average treatment effects (ATEs) in such a context consists on running an ordinary least squares regression of the outcome of interest on a treatment indicator, while including fixed effects for randomization strata (sometimes referred to as the "strata fixed effects" estimator). The ATE is then given by the coefficient on the treatment indicator.

Previous papers that have explored the effects of the CGU random audits program have implemented methodologies along these lines (Ferraz and Finan, 2008; Ferraz and Finan, 2011; Avis et al., 2018; Zamboni and Litschig, 2018). Imbens and Rubin (2015, ch. 9) and Bugni et al. (2019), however, note that the "strata fixed effects" estimator need not even be consistent for the ATE if the target proportion of units assigned to treatment varies across strata, which is the case in the CGU audit program, as stressed before and also shown in the Appendix.

In this paper, we therefore follow the novel approach proposed by Bugni et al. (2019). This approach consists of two steps. In the first step we run a fully saturated regression including

[16] See, for example, Estache and Rossi (2002, 2005) and, for the case of Brazil, Tupper and Resende (2004), Da Motta and Moreira (2006), and Sabioni (2008).

indicators for randomization strata and interactions between each of these indicators and the treatment variable (*Drafted*). Next, in a second step we calculate the overall ATE through a weighted average of the intra-strata ATEs, weighing each intra-strata ATE by the proportion of the sample size corresponding to each strata. Although this estimator is consistent for the ATE, Bugni et al. (2019) show that homoskedasticity-only and heteroskedasticity-robust estimators of the asymptotic variance are inconsistent, and tests using those estimates may be subject to over-rejection. To address this, they propose a new estimator of the asymptotic variance and show that tests using it produce valid inference, having rejection probability equal to the nominal level. Thus, we estimate the ATE through a fully saturated regression and use their novel method of estimating the asymptotic variance to conduct inference.[17]

IV. Results

Main results

Table 3 reports our results on the effect of the audit program on efficiency.[18] Column (1) reports the result of estimating a fully saturated regression of the outcome on the treatment indicator, with no further controls, and shows a statistically significant and large positive effect: being selected for an audit increases employment in the water company at the municipal level by almost 10%. The magnitude of the coefficient is somewhat reduced (+4.8%) when we control for municipal pre-treatment characteristics in column (2), but is still highly significant (and not statistically different from the coefficient in column (1)).

[17] All estimates using the methodology in Bugni et al. (2019) are obtained using the STATA command *car_sat*, available for download from Ivan Canay's website: https://bitbucket.org/iacanay/car-stata.
[18] The specifications below include all of the control variables where indicated, although the coefficient estimates are not reported.

Without additional controls, it is daring to conclude that these results imply increased inefficiency. Our concept of efficiency is that of labor efficiency—that is, a firm *i* is efficient relative to another firm *j* if, given capital inputs and output, firm *i* uses less labor than firm *j*. Following the literature on efficiency of water utilities, we estimate a labor-requirement function that includes outputs (number of active water connections and population served, proxied by total population) and capital inputs (extension of the water network) as control variables.

In columns (3) and (4) we report our results with and without municipal pre-treatment characteristics. Water providers in audited municipalities have 4.5% more employees than those in non-audited municipalities, and the effect is significant at the 5% level—even after controlling for output and other inputs. Therefore, these results indicate that the audits significantly increased labor inefficiency in the water sector in Brazil.[19]

[Table 3 about here]

Experiment validity

To further argue the validity of the audit lottery, we conduct a false experiment. If the assignment was truly random, then there should be no effect on the number of employees in 2002 for eventually audited firms. Table 4 replicates the specifications in Table 3, but uses the number of employees per municipality in 2002 as the dependent variable. Since up until 2007 water providers voluntarily joined the SNIS, we only obtained information in 2002 for 1,679 municipalities (642 treated) in our 2016 sample. Using this smaller sample, we are not able to find any significant effect of our treatment variable, as expected.

[Table 4 about here]

[19] All of the results we report in this paper are unchanged if we control for the type of service provided. Results are available upon request.

V. Mechanisms

The fact that audits increase inefficiency comes as a surprise given previous findings in the literature that we discussed at the very beginning. What may be the driving force behind this effect? A potential explanation for the fact that audits increase inefficiency is that, since audits increase the probability of detecting direct stealing, audited corrupt officials may opt to substitute for alternative or less traceable forms of corruption, by exploiting their discretion in hiring. Such a mechanism generates at least two observational consequences that we can check with our data.

First and foremost, one would not expect municipal officials to have much discretion over the hiring decisions of private operators. Put differently, our explanation implies that the increase in inefficiency should be restricted to public firms (which include mixed firms under public management). To test this implication, we first split our sample into two groups, private and public firms, and run our preferred specifications (columns [2] and [4] in Table 3) on these subsamples. The first four columns in Table 5 report the results of this exercise.

[Table 5 about here]

As can be readily seen from Table 5, we find a positive and statistically significant impact of the audits on the number of employees and labor efficiency, similar to the one in Table 3, only for municipalities served by public operators. In the case of private firms the estimated coefficients are substantially smaller and nonsignificant. These results suggest that the increase in the number of employees induced by the audits is restricted to public firms – consistent with our substitution hypothesis.[20]

A second implication of the proposed explanation is that, within public providers, local officials should exert higher discretion over hiring decisions in local providers than in regional providers

[20] Results remain unchanged if we exclude mixed firms under private management from the private subsample.

(which serve several municipalities and depend primarily on state governments).[21] The last four columns of Table 5 provide some additional evidence consistent with the substitution hypothesis: the coefficient on employment and labor efficiency in regional providers is only one half of the coefficient for local providers, although we cannot reject the null that both coefficients are equal.

VI. Concluding remarks

In this paper we document the existence of unintended (and undesirable) consequences in one of the largest anticorruption programs in the world, by exploiting the random assignment of Brazilian municipalities to an audit program: audited municipalities employ more labor in water provision, and this translates into a more inefficient service, in the sense that providers in audited municipalities use more labor for a given level of output and other inputs. We also provide additional evidence consistent with the idea that local officials may be using their discretion in hiring to substitute between different forms of corruption, as in Olken (2007).

Translating the increase in inefficiency into a money figure is difficult, but back-of-the envelope calculations show that the redundant labor caused by the audits would cost roughly 103,000 per year per municipality.[22] This implies that almost 20% of the savings from reduced corruption estimated by Avis et al. (2018) for the same audit program could be lost due to public officials adjusting other margins of their behavior to the program. Failure to anticipate this type of effects when designing an anticorruption strategy could be damaging.

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[21] All of the results go through if we include the private providers in the regressions.

[22] Employment increases by 4.5% on average—an extra 0.95 employees per firm. Multiplying this number by the average annual wage (computed as total wages over employees, from the SNIS database) in 2016 of around 108,000 reais, yields our estimate.

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Appendix

[Table A1 about here]

[Table A2 about here]

[Table A3 about here]

TABLE 1

Balancing of pre-treatment characteristics

Variable	*Control group* (1)	*Audited group* (2)	*Difference* (3)
Ln(*Population*)	9.335	9.439	0.041
	(1.107)	(1.081)	(0.032)
Urban	0.598	0.599	0.012*
	(0.236)	(0.225)	(0.006)
Municipal GDP (in million reais)	176.36	165.10	-6.28
	(586.60)	(496.45)	(17.11)
Ln(*GDP per capita in reais*)	8.359	8.282	-0.005
	(0.719)	(0.733)	(0.016)
Education	9.221	9.034	0.074
	(4.494)	(4.361)	(0.124)
Gini	0.551	0.559	0.002
	(0.067)	(0.068)	(0.002)
Observations	2,952	1,564	

Notes: The table shows means and standard deviations of municipal characteristics previous to the audits by municipalities that were audited (Treatment) and municipalities that were not audited (Control). The differences and standard errors (in parentheses) in the last column are computed through a regression of treatment status on municipal characteristics and strata fixed effects. *Significant at 10% level

TABLE 2

Water provision

Variable	*Obs.*	*Mean*	*Standard deviation*
Employees	4,516	21.27	54.42
Network	4,516	103.56	209.52
Connections	4,516	7,347.69	15,360.28
Local	4,516	0.242	0.429
Private	4,516	0.105	0.307

Notes: Employees corresponds to the total number of employees in the water and sanitation industry in each municipality. *Network* is extension of the water network in kilometers. *Connections* measures the number of active water connections. *Local* is a dummy that takes value 1 if the municipal provider serves only that municipality. *Private* is a dummy that takes value 1 if the municipal provider is a privately-owned firm or a mixed firm under private management.

TABLE 3

Main results

	Dependent variable: Ln(*Employees*)			
	(1)	(2)	(3)	(4)
Drafted	0.0993**	0.0477**	0.0453**	0.0447**
	(0.0399)	(0.0200)	(0.0187)	(0.0184)
Municipal-level covariates	No	Yes	No	Yes
Water efficiency controls	No	No	Yes	Yes
Observations	4,516	4,456	4,501	4,456

Notes: All models include indicators for randomization strata and interactions between each of these indicators and the treatment variable (*Drafted*), and are estimated with the STATA command *car_sat*. Standard errors computed as in Bugni et al. (2018) are in parentheses. Municipal level covariates are pre-treatment, and include population (in logs), share of urban population, income per capita (in logs), education, and the Gini index. Water efficiency controls are network extension (in logs) and the number of active water connections (in logs). **Significant at 5% level.

TABLE 4

False experiment

	Dependent variable: Ln(*Employees in 2002*)			
	(1)	(2)	(3)	(4)
Drafted	0.0060	0.0305	0.0240	0.0286
	(0.0654)	(0.0333)	(0.0313)	(0.0305)
Municipal-level covariates	No	Yes	No	Yes
Water efficiency controls	No	No	Yes	Yes
Observations	1,679	1,677	1,667	1,665

Notes: All models include indicators for randomization strata and interactions between each of these indicators and the treatment variable (*Drafted*), and are estimated with the STATA command *car_sat*. Standard errors computed as in Bugni et al. (2018) are in parentheses. Municipal level covariates are pre-treatment, and include population (in logs), share of urban population, income per capita (in logs), education, and the Gini index. Water efficiency controls are network extension (in logs) and the number of active water connections (in logs).

TABLE 5

Discretion in hiring

	Dependent variable: Ln(*Employees*)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Drafted	0.0303	0.0143	0.0460**	0.0435**	0.0853**	0.0746*	0.0431**	0.0388**
	(0.0552)	(0.0538)	(0.0210)	(0.0192)	(0.0426)	(0.0411)	(0.0203)	(0.0186)
Municipal-level covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Water efficiency controls	No	Yes	No	Yes	No	Yes	No	Yes
Sample	Private providers		Public providers		Local providers		Regional providers	
Observations	469	469	3,983	3,983	972	972	3,010	3,010

Notes: All models include indicators for randomization strata and interactions between each of these indicators and the treatment variable (*Drafted*), and are estimated with the STATA command *car_sat*. Standard errors computed as in Bugni et al. (2018) are in parentheses. Municipal level covariates are pre-treatment, and include population (in logs), share of urban population, income per capita (in logs), education, and the Gini index. Water efficiency controls are network extension (in logs) and the number of active water connections (in logs). Private providers include privately-owned firms and mixed firms under private management. Local providers are providers that serve a single municipality, whereas regional providers serve many. **Significant at 5% level. *Significant at 10% level.

TABLE A1

Lotteries per year (2003-2015)

Year	*# lotteries*	*cumulative*
2003	7	7
2004	7	14
2005	5	19
2006	3	22
2007	3	25
2008	2	27
2009	3	30
2010	3	33
2011	2	35
2012	2	37
2013	1	38
2014	1	39
2015	1	40

Source: Relatório de Gestão, CGU, several years.

TABLE A2

Lotteries' characteristics

Year	*Lottery no.*	*Lottery date*	*# municipalities to be drafted*	*cumulative*	*# eligible municipalities*	*min population*	*max population*	*# strata*
2003	1	03/04/2003	5	5	NA	NA	NA	5
2003	2	12/05/2003	26	31	NA	NA	NA	26
2003	3	18/06/2003	50	81	2778	10,000	250,000	26
2003	4	30/07/2003	50	131	5400	0	300,000	26
2003	5	03/09/2003	50	181	5354	0	300,000	26
2003	6	15/10/2003	50	231	5324	0	300,000	26
2003	7	12/11/2003	50	281	NA	0	300,000	26
2004	8	30/03/2004	50	331	5196	0	300,000	26
2004	9	29/04/2004	50	381	2635	10,000	500,000	22
2004	10	26/05/2004	60	441	5207	0	500,000	23
2004	11	30/06/2004	60	501	2637	10,000	500,000	23
2004	12	01/08/2004	60	561	5192	0	500,000	23
2004	13	27/10/2004	60	621	2616	10,000	500,000	23
2004	14	17/11/2004	60	681	4887	0	500,000	23
2005	15	14/04/2005	60	741	4856	0	500,000	24
2005	16	09/06/2005	60	801	4835	0	500,000	24
2005	17	16/08/2005	60	861	4823	0	500,000	24
2005	18	27/09/2005	60	921	4819	0	500,000	24
2005	19	07/11/2005	60	981	4813	0	500,000	24
2006	20	23/03/2006	60	1041	4801	0	500,000	24
2006	21	02/06/2006	60	1101	4787	0	500,000	24
2006	22	19/07/2006	60	1161	4777	0	500,000	24
2007	23	09/05/2007	60	1221	4770	0	500,000	24
2007	24	24/07/2007	60	1281	4752	0	500,000	24
2007	25	09/10/2007	60	1341	4757	0	500,000	24
2008	26	30/04/2008	60	1401	4748	0	500,000	24
2008	27	29/10/2008	60	1461	4741	0	500,000	24
2009	28	12/05/2009	60	1521	5318	0	500,000	24
2009	29	17/08/2009	60	1581	5155	0	500,000	24
2009	30	05/10/2009	60	1641	5148	0	500,000	24
2010	31	01/03/2010	60	1701	5186	0	500,000	24
2010	32	10/05/2010	60	1761	5175	0	500,000	21
2010	33	26/07/2010	60	1821	5281	0	500,000	24
2011	34	15/08/2011	60	1881	5302	0	500,000	22
2011	35	03/10/2011	60	1941	5245	0	500,000	22
2012	36	23/07/2012	60	2001	5265	0	500,000	22

Year	*Lottery no.*	*Lottery date*	*# municipalities to be drafted*	*cumulative*	*# eligible municipalities*	*min population*	*max population*	*# strata*
2012	37	08/10/2012	60	2061	5292	0	500,000	22
2013	38	04/03/2013	60	2121	5301	0	500,000	22
2014	39	17/02/2014	60	2181	5300	0	500,000	22
2015	40	02/02/2015	60	2241	5020	0	100,000	24

Source: Retrieved from http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores/legislacao on 09/24/2018.

TABLE A3

Stratification per lottery (part 1)

Stratum \ Lottery no.	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**
ACRE		1	1	1	1	1	1	1											
ALAGOAS		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
AMAPÁ		1	1	1	1	1	1	1											
AMAZONAS	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1
BAHIA		1	3	3	3	3	3	3	4	5	5	5	5	5	5	5	5	5	5
CEARÁ		1	3	3	3	3	3	3	3	2	2	2	2	2	2	2	2	2	2
ESPÍRITO SANTO		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
GOIÁS	1	1	2	2	2	2	2	3	3	3	3	3	3	3	3	3	3	3	3
MARANHÃO		1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
MATO GROSSO		1	2	1	2	1	2	1	2	2	2	2	2	2	2	2	2	2	2
MATO GROSSO DO SUL		1	1	2	1	2	1	2	1	1	1	1	1	1	1	1	1	1	1
MINAS GERAIS		1	4	4	4	4	4	4	6	8	8	8	8	8	7	7	7	7	7
PARÁ		1	3	3	3	3	3	2	2	2	2	2	2	2	2	2	2	2	2
PARAÍBA		1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
PARANÁ		1	2	2	2	2	2	2	2	4	4	4	4	4	4	4	4	4	4
PERNAMBUCO		1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
PIAUÍ	1	1	3	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
RIO DE JANEIRO		1	3	3	3	3	3	3	2	2	2	2	2	2	2	2	2	2	2
RIO GRANDE DO NORTE		1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
RIO GRANDE DO SUL		1	3	3	3	3	3	3	4	5	5	5	5	5	5	5	5	5	5
RONDÔNIA		1	1	1	1	1	1	1											
RORAIMA		1	1	1	1	1	1	1											
SANTA CATARINA	1	1	1	2	2	2	2	2	2	3	3	3	3	3	3	3	3	3	3
SÃO PAULO	1	1	3	3	3	3	3	3	4	6	6	6	6	6	6	6	6	6	6
SERGIPE		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
TOCANTINS		1	1	1	1	1	1	1	1	2	2	2	2	2	2	2	2	2	2
ACRE, AMAPA, AMAZONAS, RONDÔNIA, RORAIMA									1										
ACRE, RONDÔNIA															1	1	1	1	1
AMAPA, RORAIMA															1	1	1	1	1
AMAPA, RONDÔNIA, RORAIMA																			
ACRE, RONDÔNIA, RORAIMA																			
ACRE, AMAPA, RORAIMA																			
ACRE, AMAPA, AMAZONAS																			
ACRE, AMAPA, RONDÔNIA, RORAIMA										1	1	1	1	1					

Stratum \ Lottery no.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
ACRE, MATO GROSSO DO SUL																			
ALAGOAS, SERGIPE																			
AMAPA, RORAIMA																			
AMAZONAS, RONDÔNIA																			
ESPIRITO SANTO, RIO DE JANEIRO																			
	5	26	50	50	50	50	50	50	50	60	60	60	60	60	60	60	60	60	60

Source: Retrieved from http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores/legislacao on 09/24/2018.

TABLE A3

Stratification per lottery (part 2)

Stratum \ Lottery no.	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
ACRE																					
ALAGOAS	1	1	1	2	2	2	2	2	2	2	2	2		2	1	1	1	1	1	1	1
AMAPÁ																					1
AMAZONAS	1	1	1	1	1	1	1	1	1	1	1	1		1							1
BAHIA	5	5	5	5	5	5	5	5	5	5	5	5	2	5	5	5	5	5	5	5	5
CEARÁ	2	2	2	3	3	3	3	3	3	3	3	3	1	3	4	4	4	4	4	4	4
ESPÍRITO SANTO	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1
GOIÁS	3	3	3	2	2	2	2	2	2	2	2	2	1	2	3	3	3	3	3	3	4
MARANHÃO	2	2	2	3	3	3	3	3	3	3	3	3	1	3	2	2	2	2	2	2	2
MATO GROSSO	2	2	2	1	1	1	1	2	1	1	1	1	1	1	2	2	2	2	2	2	1
MATO GROSSO DO SUL	1	1	1	1	1	1	1	2	1	1	1	1		1	1	1	1	1	1	1	1
MINAS GERAIS	7	7	7	7	7	7	7	4	7	7	7	7	4	7	7	7	7	7	7	7	7
PARÁ	2	2	2	3	3	3	3	3	3	3	3	3	1	3	3	3	3	3	3	3	2
PARAÍBA	2	2	2	3	3	3	3	3	3	3	3	3	1	3	2	2	2	2	2	2	2
PARANÁ	4	4	4	3	3	3	3	3	3	3	3	3	2	3	3	3	3	3	3	3	3
PERNAMBUCO	2	2	2	3	3	3	3	3	3	3	3	3	1	3	3	3	3	3	3	3	3
PIAUÍ	2	2	2	3	3	3	3	3	3	3	3	3	1	3	2	2	2	2	2	2	2
RIO DE JANEIRO	2	2	2	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1
RIO GRANDE DO NORTE	2	2	2	3	3	3	3	3	3	3	3	3	1	3	3	3	3	3	3	3	3
RIO GRANDE DO SUL	5	5	5	4	4	4	4	4	4	4	4	4	2	4	4	4	4	4	4	4	4
RONDÔNIA	1	1	1	1	1	1	1	1	1	1	1	1		1							
RORAIMA																					1
SANTA CATARINA	3	3	3	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
SÃO PAULO	6	6	6	5	5	5	5	5	5	5	5	5	3	5	6	6	6	6	6	6	6
SERGIPE	1	1	1	1	1	1	1	2	1	1	1	1		1	2	2	2	2	2	2	2
TOCANTINS	2	2	2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1

Stratum \ Lottery no.	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
ACRE, AMAPA, AMAZONAS, RONDÔNIA, RORAIMA															2						
ACRE, RONDÔNIA																					
AMAPA, RORAIMA																					
AMAPA, RONDÔNIA, RORAIMA																2					
ACRE, RONDÔNIA, RORAIMA																		2		2	
ACRE, AMAPA, RORAIMA	1	1	1	1	1	1	1	1	1	1	1	1		1							
ACRE, AMAPA, AMAZONAS																	2		2		
ACRE, AMAPA, RONDÔNIA, RORAIMA																					
ACRE, MATO GROSSO DO SUL													1								
ALAGOAS, SERGIPE													1								
AMAPA, RORAIMA													1								
AMAZONAS, RONDÔNIA													1								
ESPIRITO SANTO, RIO DE JANEIRO													1								
	60	**60**	**60**	**60**	**60**	**60**	**60**	**60**	**60**	**60**	**60**	**60**	**30**	**60**	**60**	**60**	**60**	**60**	**60**	**60**	**60**

Source: Retrieved from http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores/legislacao on 09/24/2018.