

AUDITS AND GOVERNMENT HIRING PRACTICES

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Abstract

We exploit the random assignment of Brazilian municipalities to an audit program to explore the link between audits and government hiring practices. We find that audited municipalities employ less labour to provide a given level of public services, and change the way in which they screen their employees—relying less on discretion and more on merit. Given that audits reduce corruption in the activities under scrutiny, our results suggest that different types of corruption can be complements rather than substitutes.

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Embezzlement, graft, influence peddling, bribery, kickbacks, fraud, favouritism, cronyism, nepotism, parochialism, patronage, capture, collusion, extortion—call it what you like, corruption (the misuse of public office for private gain) matters. Even though obtaining reliable statistics on the magnitude of corruption is difficult, the World Bank Institute estimates corruption to be around \$1 trillion per year, a significant 3 percent of world GDP—and that just in bribes.¹ There is a growing consensus that corruption is a major impediment to economic development (see, e.g., Mauro, 1995; and Olken and Pande, 2012), and, non-surprisingly, fighting corruption has become a prime concern in countries all over the world.²

The topic has attracted increasing interest in recent years from the media, academics, policymakers, and the public at large, and few seem to doubt that reducing corruption would be beneficial (Jain, 2001). How best to achieve such a goal (through, for example, top-down monitoring, grassroots participation, information dissemination, or political, judicial, and prosecutorial institutions that constrain rent-seeking), however, remains a disputed issue (Olken, 2007) in spite of abounding suggestions (see, e.g., Eigen, 1996; Leiken, 1997; and Klitgaard, 1998).

In practice, audits (a form of top-down monitoring) are a key policy instrument in the fight against corruption. The idea of providing incentives to corruptible politicians or bureaucrats to behave honestly through an increased probability of detection and punishment is hardly new (Becker, 1968; Becker and Stigler, 1974), and the evidence shows that the basic economic intuition is correct: audits can reduce corruption—at least in the particular activities being audited (Olken, 2007; Bobonis et al., 2016; Avis et al., 2018; Zamboni and Litschig, 2018).³ However, some evidence also exists on monitoring systems that are fouled up in that corrupt officials are able to substitute to alternate forms of corruption and to adapt over time to the new system in place (e.g.,

by changing other dimensions of their performance)—especially when the anticorruption policy imposes controls only on certain types of corruption (see, e.g., Olken, 2007; Burgess et al., 2012; and Niehaus and Sukhtankar, 2015).

Since audits increase the probability of detecting direct stealing, audited corrupt officials may opt to substitute for alternative or less traceable forms of corruption, like hiring, which are not directly audited. Employment in the public sector can be used for the benefit of office holders to the extent that the latter can use their discretion in the hiring process (Brollo et al., 2017). Discretion, while providing more flexibility, can also lead to corruption and patronage (Finan et al., 2015).

It is an open question whether audits generate only negative changes in behaviour in alternative, nonaudited, margins of public officials' performance, or whether good behaviour from fear of the audit might spill over to other dimensions. This paper is an empirical investigation of the possible consequences of selective anticorruption monitoring on government hiring practices, 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 mayors' subsequent use of their discretion in hiring bureaucrats and the efficiency in the provision of municipal services.

Bureaucracies are a key component of state capacity: they implement policies and deliver public services (Ornaghi, 2019; Xu, 2019). A high-quality bureaucracy is characterized by “meritocratic recruitment through competitive examinations, civil service procedures for hiring and firing rather than political appointments and dismissals, and filling higher levels of the hierarchy through internal promotion” (Rauch and Evans, 2000: 50-51), and competitive salaries. Such a ‘Weberian’ bureaucracy (Evans, 1992) has been associated with enhanced prospects for economic growth (Evans and Rauch, 1999), reduced corruption (Dahlström et al., 2012), and improved performance

(Gilmour and Lewis, 2006; Xu, 2019). The empirical evidence shows that merit-based recruitment is the single most important component of a Weberian bureaucracy driving these outcomes (Rauch and Evans, 2000; Dahlström et al., 2012).

Evans (1992) suggests using the fraction of bureaucratic positions filled by civil-service exam rather than discretionary hiring as a measure of meritocratic recruitment. We focus here (equivalently) on its complement: the fraction filled without a civil-service exam. For each municipality we measure the number of discretionary appointments per 1,000 residents and the share of discretionary appointments in total municipal employment. We find that being audited causes a reduction in discretionary employment of about 1.4 employees per 1,000 residents (or around 12%, evaluated at the sample mean), and a reduction in the share of discretionary appointments of 1.3 percentage points (almost 5% with respect to the sample mean).

We also consider the provision of essential public goods at the municipal level. Efficiency in this context relates to how a given municipal staff (the main input in the production of municipal services) translates into outputs—essential public services of local interest, such as public transport, early childhood and primary education programs, and public health services.

We first find that being audited reduces total employment in the municipality by more than 2.6 employees per 1,000 residents (about 6% at the sample mean, or around 60 employees). Next, we show that audits have *no* effect on the provision of municipal services, as measured by an index that summarizes how a municipality is doing in the areas of employment and income, education, and health. The estimated effect is largely non-significant, and the point estimate is just 0.17% of the sample mean of the index. We interpret these findings as indicative of a clear increase in (labor) efficiency: audited municipalities seem to be providing exactly the same level of public services with fewer workers.

Monetizing the wage savings is difficult, but we can provide some back-of-the-envelope calculations. At an average annual wage in 2004 of around 24,250 reais (in 2019 reais), the wage savings caused by the audits (60 fewer employees per municipality) would amount to roughly 1,455,000 per year per municipality.⁴ To put this figure in perspective, consider that Avis et al. (2018) estimate savings from reduced corruption from the same audit program of about 567,000 reais (including spillover effects) per municipality per year—hence savings would more than triple.

Regarding the dynamics of the effects we document, whether these effects are permanent or temporary is important for interpretation. On the other hand, it is interesting to assess how long it takes for such effects to show up. We find that the average effects on the level and composition of municipal employment show up as early as 2005 (the first year of the term), and persist at essentially the same level 4-5 years after the audits.

The literature has increasingly studied the consequences of government audits using the Brazilian random audits program. After the seminal work of Ferraz and Finan (2008), which found that exposing corruption among politicians up for re-election reduced their probability of subsequent electoral success, a growing number of papers have found that audits have important effects on outcomes such as overall corruption (Avis et al., 2018), political clientelism (Bobonis et al., 2019), economic activity (Bologna Pavlik and Harger, 2019; Colonnelli and Prem, 2020), health outcomes (Lichand et al., 2017), and rent extraction among local governments (Zamboni and Litschig, 2018). Our paper contributes to this literature by studying how audits affect government hiring practices, and documenting their good behaviour spillovers on this outcome.

Our paper also relates to the literature studying the recruitment of employees in the public sector (see Finan et al., 2015). This growing literature has found that financial incentives (Dal Bó et al.,

2013; Deserranno, 2019) and pro-social preferences (Ashraf et al., 2014) play a significant role in shaping the pool of individuals who self-select into applying for a public sector job. However, there is less evidence on the process through which governments select public employees (Colonnelli et al., 2020), which is a concern given the possibility that politicians use public sector hiring to reward political supporters and collaborators, considering the limited mechanisms available for constituents to punish politicians for bad performance (Finan et al., 2015). We contribute to this literature by showing that audits reduce total employment in the public sector, and change the screening strategy from discretion towards merit.

Given that the audits under study reduced corruption (Avis et al., 2018; Zamboni and Litschig, 2018), our efficiency result is consistent with previous literature that has shown that reduced corruption is strongly associated with less inefficiency (e.g., Dal Bó and Rossi, 2007; Colonnelli and Prem, 2020), and extends that literature's results at the firm level to the public sector.

I. BACKGROUND AND DATA

I.1. 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.⁵ 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. Another novelty of the program is its intention of reducing information asymmetries between voters and public officials by making the main findings for each audited municipality publicly available.

The program randomly selects a fixed number of municipalities per state (*unidade federativa*) through lotteries held by the *Caixa Econômica Federal* in Brasília, drawn in conjunction with the national lotteries.⁶ 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 in the past three years, by examining accounts and documents, and visiting the existing works and services.⁷ 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 public prosecutors, the legislative branch of each municipality, and to the *Tribunal de Contas da União*—the different agencies in charge of corrective actions. For each municipality, a summary of the main findings is uploaded to the CGU website (www.cgu.gov.br) and made available to the media.

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 with less than 500,000 inhabitants.⁸ At the very beginning of the program the lotteries were drawn on a monthly basis, but a reference periodicity was never established. Hence mayors faced uncertainty about audit probabilities, and experiencing an audit was likely to increase perceived probabilities by making audit risk more salient, potentially affecting mayors' subsequent actions.⁹ This study considers 5,476 municipalities that were eligible for the program, 496 of which were randomly selected through the first 11 lotteries, and audited in the electoral term 2001-2004.¹⁰ The list of the municipalities selected in each of the lotteries (and eventually audited) was obtained directly from the CGU website.¹¹ With this information we build a dummy variable (*Audited*) that takes value 1 if the municipality was randomly audited, and constitutes our treatment indicator.

1.2. Brazilian municipalities

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.¹² 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, and public health services. 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 (99.6% on average in 2000, down to 93% 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 previous studies: income per capita (*Income*), income inequality as measured by the Gini coefficient (*Income inequality*), total population (*Population*), share of urban population (*Urban population (%)*), education, proxied by the literacy rate (*Literacy*) and the fraction of the population with a college degree (*Population with college degree (%)*), fraction of households owning a radio (*Has radio*) or a television (*Has TV*), and fraction of households with a water (*Water connection (%)*), sewerage (*Sewerage connection (%)*), or electricity (*Electricity connection (%)*) connection.¹³ To evaluate the validity of our research design, we present in Table 1 summary statistics of these pretreatment characteristics for treated and control municipalities, and also the differences between both groups.

[Table 1 about here]

Only one out of 11 differences is significant at the 10 percent level, and a test of the joint significance of all municipal characteristics fails to reject the null of no significance (F-test = 1.18; p-value = .29). Overall, the results from Table 1 suggest that the lottery used by the CGU yielded a balanced experiment.

1.3. Municipal employment

Public employees in all levels of the administration represented roughly 5% of the Brazilian population and 27% of all formal employment in 2003 – municipal employment accounted for

49% of that total, up from 38% in 1995 (De Mattos, 2011). A commonly used way to compare jurisdictions is by looking at how many employees that jurisdiction has relative to the population. We therefore compute here the total number of public employees per 1,000 residents (*Employees per capita*) in each municipality in the electoral term 2005-08.¹⁴ Following usual practice, we include employees in both the direct and indirect administration.¹⁵

Personnel practices in the public sector are different from those in the private sector. One key feature in this distinction is the reduced set of contracts that can be offered to public employees, coupled with the lack of discipline from the profit motive in the state (Finan et al., 2015). This implies that politicians may use their discretion to allocate public sector jobs to reward followers and to cement political and personal relationships – the very definition of political patronage (Grindle, 2010).

To limit this discretion in hiring, most governments have implemented career civil service systems, in which the majority of “non-elected public sector jobs are filled through a process of credentialing based on education, examination, or some other test of merit; in which a career ladder exists and is accessed through regularized demonstration of credentials of education, examination, tenure in office, or other form of assessing merit; in which tenure is secure barring malfeasance in office; and in which movement in and out (through retirement, for example) is regulated and compensated” (Grindle, 2010: 4).

In Latin America, Brazil was the first country to implement a civil service system with extensive merit-based access (through a formal examination or *Concurso Público*), widely considered to be the most successful in the region (Evans and Rauch, 1999; Gaetani and Heredia, 2002; Iacoviello, 2006).¹⁶ About 83% of municipal employees were career civil servants in 2004.

Ample room for discretion in hiring still remains, however, as mayors can hire employees without a civil service examination in prespecified cases: commissioned posts (*cargos comissionados*), positions of trust (*função de confiança*), and temporary jobs (*emprego temporario*).¹⁷ Temporary appointments account for the majority of discretionary hires. We group all these categories in a single measure (*Discretionary per capita*) that represents the average total number of discretionary public employees per 1,000 residents in each municipality in 2005-08. We also compute the average fraction of discretionary employees in each municipality in the same period (*Discretionary share*).¹⁸

After dropping municipalities with missing values in our main dependent variable (18 municipalities) and our covariates (49 municipalities), we are left with an estimation sample of 5,409 municipalities, 471 of which were audited.¹⁹ Table 2 presents summary statistics for our main variables.

[Table 2 about here]

II. ESTIMATION APPROACH

Given the randomized experimental design, estimation is a straightforward comparison of sample mean outcomes for treated and control municipalities. Let y_{ms} denote the different employment and performance outcomes for municipality m in state s . To estimate the average effect of being audited on those outcomes, we estimate different versions of the following reduced-form model through Ordinary Least Squares (OLS):

$$(1) \quad y_{ms} = \alpha + \beta * Audited_{ms} + \mu_s + \gamma * X_{ms} + \varepsilon_{ms}$$

where $Audited_{ms}$ is an indicator for whether municipality m in state s was audited, X_{ms} is a vector of pre-treatment municipal characteristics, and ε_{ms} is a random term, uncorrelated (in expectation) with $Audited_{ms}$ because of randomization.

Since treatment probabilities vary by state due to the stratified randomization we include state fixed effects μ_s for randomization strata (sometimes referred to as the “strata fixed effects” estimator).²⁰ The coefficient of interest is β .

III. RESULTS

This section presents evidence on the impact of audits on municipal governments hiring practices and performance. We begin by presenting estimates of the average effect of an audit on discretionary municipal employment. We then consider the impact of audits on municipal efficiency. Finally, we take a brief look at the timing of the effects.

III.1. Audits and discretionary employment

Discretion in hiring in the public sector has long been regarded as a problem, since it can lead to corruption and patronage. We have argued above that ample room for discretion in hiring remains in Brazilian municipalities, and Brollo et al. (2017) and Colonnelli et al. (2020) provide evidence that this discretion is indeed used for political patronage. To assess whether the audit program affects the extent to which mayors use their discretion in hiring, we report in Table 3 OLS estimates of equation (1) using the number of discretionary public employees per 1,000 residents – columns (1) and (2) – and the share of discretionary employees – columns (3) and (4) – as our outcome variables. The specification in the odd columns control only for state fixed effects, whereas the even columns extend the specification to include municipal pre-treatment characteristics.

[Table 3 about here]

We find that being audited causes a reduction in discretionary employment of about 1.4 employees per 1,000 residents (or about 12%, evaluated at the sample mean), and a change in the composition of public employees, with the share of discretionary appointments reduced by 1.3 percentage points (5% at the sample mean).²¹ A long tradition, associated with the works of Wilson (1887) and Weber (1922) has associated reduced discretion with a more competent and efficient government, and the empirical evidence shows that civil services with access largely based on merit correlate with higher economic growth, reduced corruption, and better bureaucratic performance (see Brollo et al., 2017, and the references therein). In this view, our findings would suggest that, on top of their explicit goal of reducing corruption in the use of federal funds, the audits have generated positive spillovers on this dimension.

III.2. Audits and efficiency

The reduction in discretionary appointments documented in Table 3 may be compensated by an increase in other types of appointments. To show that this is not the case, the first two columns of Table 4 report OLS estimates of equation (1), using total employees per 1,000 residents as our employment outcome, with and without municipal covariates.

Both specifications show a statistically significant and large negative effect: being audited in the electoral term 2001-04 reduces total employment in the municipality by more than 2.6 employees per 1,000 residents in the following term. This represents a reduction in the main input used in the production of municipal services of about 6% at the sample mean (or around 60 employees).

[Table 4 about here]

On the output side, municipalities provide a host of services of local interest to which labour is a key input—like public transport, early childhood and primary education programs, and public health services. To measure their performance along such lines, we use an index (*Municipal*

performance) that captures how a municipality is doing in the areas of employment and income, education, and health by summarizing official administrative data on several indicators in a single number between zero and one (with higher numbers indicating better performance).²² Table 4, columns (3) and (4), presents evidence on the effect of the audits on municipal performance thus measured. The effect is largely non-significant (the corresponding p-value is 0.70 in column (4)), and economically negligible (the point estimate in column (4) is just 0.17% of the sample mean of the index).

To be clear, our measure of municipal performance is an ensemble of several different outcomes, which (1) municipal governments can affect to different extents, (2) might be affected by audits over different horizons, and (3) might be affected by audits in opposite ways. Public employment is merely one of the possible mechanism through which audits can affect municipal performance, and audits might affect public service delivery through other mechanisms unrelated to the level and composition of public employment (see, e.g., Lichand and Fernandes, 2019).

Whatever the particular mechanisms at play, however, the results in Table 4 show that the net effect of audits on municipal performance has been null on average. Given the reduction in total municipal employment also documented in that table, audited municipalities seem to be providing exactly the same level of public services with fewer workers—a clear increase in (labor) efficiency.²³ The evidence is suggestive, but we cannot rule out the possibility that improved efficiency comes from other aspects of the anticorruption audits, with no contribution at all from the changes in employment.

III.3. Dynamics

Whether the effects on the bureaucracy documented thus far are permanent or temporary is important for interpretation. If the level and composition of public employment change only

temporarily in response to audits making audit probabilities more salient, but resume to their baseline levels after a few years, then it becomes harder to interpret this effect as an increase in bureaucratic efficiency – especially since it might take some time for changes in public employment to translate into impacts on downstream outcomes. On the other hand, it is interesting to assess how long it takes for the effects to show up.

To address these questions, we re-run our preferred specification (with municipal covariates) with all the dependent variables in turn, but limiting the sample to the first year of the electoral term (2005) and to the last year (2008). The results in Table 5 show that the average effects on employment show up as early as 2005, and persist at essentially the same level 4-5 years after the audits. The magnitudes of the coefficients are similar to those in Tables 3 and 4, and remarkably stable. The coefficients on the share of discretionary employment are estimated somewhat imprecisely, and become marginally nonsignificant (p-values around .12).

[Table 5 about here]

IV. CONCLUDING REMARKS

By exploiting the random assignment of Brazilian municipalities to an audit program aimed at reducing corruption in the use of federal funds, we document the existence of broader consequences in one of the largest anticorruption programs in the world: audited municipalities employ less labour to provide a given level of public services, and change the way in which they screen their employees—relying less on discretion and more on merit. As emphasized by Finan et al. (2015), different screening approaches have important implications for the quality and performance of the bureaucracy.

Two characteristics of a well-functioning government often considered are a high quality of the bureaucracy and an efficient provision of essential public goods, like education, health and infrastructure (La Porta et al., 1999).²⁴ In this view, it is tempting to interpret the improvements in bureaucratic efficiency and professionalization, which take place right away and persist four to five years after the audits, as an increase in the quality of municipal governments. Our evidence, however, does not necessarily map into government quality. Discretion, for instance, may also be put to good use—e.g., to improve selection or mitigate agency problems (check Brollo et al., 2017, and Voth and Xu, 2020, for a discussion and some evidence)—and increase performance.²⁵

To the extent that reduced discretion can be interpreted as reduced patronage (e.g., Brollo and Troiano, 2016), and given that audits reduce corruption in the activities under scrutiny, our results suggest that different types of corruption can be *complements* rather than substitutes. Further exploration of this possibility is left for future research.

While it is beyond the scope of this paper to identify why these audits produce positive spillovers across margins of corrupt behaviour, two key features stand out. First, the audits are conducted rigorously, with potentially severe consequences, and are perceived as such by officials (as seen in existing work on these audits as well). Second, as discussed above, the audits are potentially broad in their scope, involving interviews with members of the local community and members of municipal councils. Thus, officials may reduce corrupt behaviour along a range of margins in order to avoid detection and potential punishment. Less rigorous enforcement or enforcement with an explicitly narrower scope of investigation may well produce different behavioural responses.

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¹ Figures quoted in Svensson (2005). Check also Shah (2006).

² Although corruption is a rampant in much of the developing world, developed countries are also affected. For instance, in the United States federal prosecutors have convicted more than 10,000 government officials of acts of official corruption in the period 1990-2002 (Glaeser and Saks, 2006).

³ Olken (2007) finds that increasing government audits significantly reduces corruption in a field experiment in Indonesian villages. Similarly, Bobonis et al. (2016) find that timely and foreseeable audits induce a significant short-term reduction in municipal corruption levels in Puerto Rico. Using the same program we study in this paper, Avis et al. (2018) show that audits significantly reduce future corruption in Brazilian municipalities, while increasing the probability of facing subsequent legal action; and Zamboni and Litschig (2018) estimate that temporarily increasing audit risk reduced the share of audited resources involved in corruption in procurement.

⁴ Wage data correspond to employees in the public administration, and was obtained from <http://dataviva.info/en/data/>. This may include public employees at other levels of government. We could not find the corresponding figure for just municipal employees. Wages were converted to 2019 reais through the consumer price index of the IBGE (<https://www.ibge.gov.br/estatisticas/economicas/precos-e-custos/9256-indice-nacional-de-precos-ao-consumidor-amplo.html?=&t=series-historicas>).

⁵ 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.

⁶ 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.

⁷ The main areas under scrutiny are education, health and social services.

⁸ In spite of the population threshold, over 98% of Brazil's 5,570 municipalities were eligible for the lotteries.

⁹ A similar learning argument is invoked by Avis et al. (2018) and Colonnelli and Prem (2020) for the Brazilian audits, and by Kleven et al. (2011), Malmendier and Nagel (2011), and Gallagher (2014) in other contexts.

¹⁰ These are the same lotteries considered by Ferraz and Finan (2011), and roughly the same as in Ferraz and Finan (2008). Moreover, the learning-through-experience argument above is more likely to hold at the very beginning of the program, when uncertainty was greatest.

¹¹ Retrieved from <http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores/municipios> on 9/27/2018.

¹² This account is mostly based on Gardner (2013), where further details can be found.

¹³ The Gini coefficient, the education data, the share of urban population, and the access to services figures are all from the 2000 Census (<http://tabnet.datasus.gov.br/cgi/ibge/censo/cnv/ginibr.def>, <https://sidra.ibge.gov.br/tabela/200>, <https://www.ibge.gov.br/estatisticas/multidominio/cultura-recreacao-e-esporte/9663-censo-demografico-2000.html?edicao=9859&t=resultados>, <https://sidra.ibge.gov.br/tabela/1453>, <https://sidra.ibge.gov.br/tabela/1442>, <https://sidra.ibge.gov.br/Tabela/2413>). Population estimates are for 2000 and were obtained from <https://www.ibge.gov.br/estatisticas/sociais/populacao/9103-estimativas-de-populacao.html?=&t=downloads>. Income is measured through GDP per capita, computed as municipal GDP/population in 2002. Municipal GDP data was obtained from <https://www.ibge.gov.br/estatisticas/economicas/contas-nacionais/9088-produto-interno-bruto-dos-municipios.html?edicao=18021&t=downloads>.

¹⁴ All employee figures were obtained from the website of the Instituto Brasileiro de Geografia e Estatística: <https://www.ibge.gov.br/estatisticas/sociais/protecao-social/10586-pesquisa-de-informacoes-basicas-municipais.html?=&t=downloads>. We transform the raw figures into per capita figures by dividing them by population (in thousands).

¹⁵ The direct administration is directly connected to the municipal executive power (the mayor), has no separate legal status, and has no administrative autonomy. The indirect administration is characterized by entities (e.g., autarchies, public foundations, public enterprises and mixed-capital companies) that have their own separate legal status, having therefore, administrative autonomy and specific budgets for their purposes.

¹⁶ Check Grindle (2010) and Iacoviello and Rodríguez-Gustá (2006) for characterizations of the evolution of civil service in Brazil.

¹⁷ Check Colonnelli et al. (2020) and IBGE (2017) for further details.

¹⁸ All employee figures were obtained from the website of the IBGE: <https://www.ibge.gov.br/estatisticas/sociais/protecao-social/10586-pesquisa-de-informacoes-basicas-municipais.html?=&t =downloads>. We transform the raw figures into per capita figures by dividing them by population (in thousands). The share of discretionary employees is calculated as the number of discretionary employees over the total number of municipal employees.

¹⁹ Including municipalities with missing values for the covariates does not affect our estimates of the unadjusted relationship between audits and our dependent variables.

²⁰ 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).

²¹ The reduction is present in all types of discretionary appointments, and the total effect is not driven by any particular category. These results are available upon request.

²² We use the *Índice FIRJAN de Desenvolvimento Municipal* (IFDM), obtained from <https://www.firjan.com.br/ifdm/>. Some variables considered in the index are primary school dropout, average classroom-hours, prenatal care, and child mortality. Full details can be found at the IFDM website (<https://www.firjan.com.br/ifdm/>).

²³ For studies of the efficiency of the local public sector check, e.g., De Borger et al. (1994) and Balaguer-Coll et al. (2007). De Sousa and Stosic (2005) analyze the efficiency of Brazilian municipalities.

²⁴ The empirical evidence shows that good government is ultimately good for growth (see La Porta et al., 1999, and the references therein), which underscores the importance of looking at the quality of government.

²⁵ Restrictions on discretionary appointments are usually well-regarded in practice. Already in 1883, the Pendleton Civil Service Act established a merit-based public service system in the federal U.S. government to reduce the discretion of party bosses (Grindle, 2010).

Table 1. Mean comparisons between audited and nonaudited municipalities

Variable	Control		Treatment		Difference
	Mean	Standard deviation	Mean	Standard deviation	
<i>Population</i>	22,320.14	43,192.38	25,328.24	40,528.67	909.40 (1976.99)
<i>Urban population (%)</i>	58.46	23.11	59.98	22.99	2.607*** (0.972)
<i>Income</i>	8.29	.80	8.26	.84	0.044 (0.029)
<i>Income inequality</i>	0.554	0.068	0.564	0.065	0.001 (0.003)
<i>Literacy</i>	81.14	11.14	79.76	11.32	0.157 (0.301)
<i>Population with college degree (%)</i>	0.029	0.026	0.026	0.025	-0.000 (0.001)
<i>Has radio</i>	81.67	21.35	80.27	21.64	0.432 (0.661)
<i>Has TV</i>	87.63	17.01	86.14	17.97	0.434 (0.635)
<i>Water connection (%)</i>	87.80	15.57	86.01	16.46	0.164 (0.558)
<i>Sewerage connection (%)</i>	76.16	18.31	74.04	18.59	0.283 (0.619)
<i>Electricity connection (%)</i>	82.29	12.58	79.94	13.36	0.212 (0.386)
Observations	4,938		471		

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*). *Population* is the total number of residents in the municipality. *Urban population (%)* is the share of the population living in urban areas. *Income* is proxied by municipal GDP per capita (in logs). *Income inequality* is measured through the Gini coefficient of per capita household income for each municipality in. *Literacy* is the share of the population that is literate in 2000. *Has radio* is the fraction of households owning a radio. *Has TV* is the fraction of households owning a television. *Water connection (%)* is the fraction of households with a water connection. *Sewerage connection (%)* is the fraction of households with a sewerage connection. *Electricity connection (%)* is the fraction of households with an electricity connection. All characteristics correspond to the year 2000, except for *Income*, which is measured in 2002. The differences and standard errors (in parentheses) in the last column are computed through a regression of treatment status on municipal characteristics and state fixed effects.

Table 2. Summary statistics

Variable	Obs.	Mean	Standard deviation	Minimum	Maximum
<i>Employees per capita</i>	5,409	44.32	18.72	10.01	220.46
<i>Discretionary per capita</i>	5,409	12.24	10.64	0	187.09
<i>Discretionary (share)</i>	5,409	0.263	0.162	0	1
<i>Municipal performance</i>	5,385	0.583	0.127	0.268	0.901
<i>Audited</i>	5,409	0.087	0.282	0	1

Notes: *Employees per capita* and *Discretionary per capita* represent the average total number of public employees per 1,000 residents and the total number of discretionary public employees per 1,000 residents in each municipality in 2005-08. *Discretionary (share)* is the average fraction of discretionary employees in each municipality in 2005-08. *Municipal performance* is measured through the IFDM index of municipal development. *Audited* is a dummy that takes value 1 if the municipality was audited in 2003-04.

Table 3. Audits and discretionary employment

Dependent variable:	<i>Discretionary per capita</i>		<i>Discretionary (share)</i>	
	(1)	(2)	(3)	(4)
<i>Audited</i>	-1.416*** (0.412)	-1.445*** (0.403)	-0.013* (0.007)	-0.013* (0.007)
Mean of dep. variable	12.24		0.263	
Municipal characteristics	No	Yes	No	Yes
Observations	5,409	5,409	5,409	5,409

Notes: All models include state fixed effects. Robust standard errors are in parentheses. *Discretionary per capita* represents the average total number of discretionary public employees per 1,000 residents in each municipality in 2005-08. *Discretionary (share)* is the average fraction of discretionary employees in each municipality in 2005-08. *Audited* is a dummy that takes value 1 if the municipality was audited in 2003-04. Municipal characteristics are pre-treatment, and include income per capita (in logs), the Gini index, share of urban population, education, fraction of households owning a radio or a television, and fraction of households with a water, sewerage, or electricity connection. ***Significant at 1 percent level. *Significant at 10 percent level.

Table 4. Audits and efficiency

Dependent variable:	<i>Employees per capita</i>		<i>Municipal performance</i>	
	(1)	(2)	(3)	(4)
<i>Audited</i>	-2.588*** (0.806)	-2.620*** (0.729)	0.0040 (0.0035)	-0.0010 (0.0026)
Mean of dep. variable	44.32		0.5827	
Municipal characteristics	No	Yes	No	Yes
Observations	5,409	5,409	5,385	5,385

Notes: All models include state fixed effects. Robust standard errors are in parentheses. *Employees per capita* represents the average total number of public employees per 1,000 residents in each municipality in 2005-08. *Municipal performance* is measured through the IFDM index of municipal development. *Audited* is a dummy that takes value 1 if the municipality was audited in 2003-04. Municipal characteristics are pre-treatment, and include income per capita (in logs), the Gini index, share of urban population, education, fraction of households owning a radio or a television, and fraction of households with a water, sewerage, or electricity connection. ***Significant at 1 percent level.

Table 5. Short- and medium-run effects of audits

Dependent variable:	<i>Discretionary per capita</i>		<i>Discretionary (share)</i>		<i>Employees per capita</i>		<i>Municipal performance</i>	
	1 st year	Last year	1 st year	Last year	1 st year	Last year	1 st year	Last year
<i>Audited</i>	-1.499*** (0.464)	-1.550*** (0.500)	-0.014 (0.009)	-0.012 (0.008)	-2.847*** (0.730)	-2.611*** (0.848)	0.0003 (0.0028)	0.0013 (0.0028)
Mean of dep. variable	11.15	12.30	0.256	0.243	41.30	47.30	0.558	0.603
Municipal characteristics	Yes		Yes		Yes		Yes	
Observations	5,409		5,409		5,409		5,163	5,335

Notes: All models include state fixed effects. Robust standard errors are in parentheses. *Discretionary per capita* represents the average total number of discretionary public employees per 1,000 residents in each municipality. *Discretionary (share)* is the average fraction of discretionary employees in each municipality. *Employees per capita* represents the average total number of public employees per 1,000 residents in each municipality. *Municipal performance* is measured through the IFDM index of municipal development. *Audited* is a dummy that takes value 1 if the municipality was audited in 2003-04. The first year of term is 2005, and the last year is 2008. Municipal characteristics are pre-treatment, and include income per capita (in logs), the Gini index, share of urban population, education, fraction of households owning a radio or a television, and fraction of households with a water, sewerage, or electricity connection. ***Significant at 1 percent level.